

**UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION**

**CHELSEY NELSON PHOTOGRAPHY
LLC and CHELSEY NELSON,**

Plaintiffs,

v.

**LOUISVILLE/JEFFERSON COUNTY
METRO GOVERNMENT, et al.,**

Defendants.

Case No. 3:19-cv-851-BJB-CHL

**DEFENDANTS’ RESPONSE TO PLAINTIFFS’
MOTION TO EXCLUDE TESTIMONY OF NETTA BARAK-CORREN**

Defendants Louisville/Jefferson County Metro Government (“Louisville Metro”), 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, 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 (collectively, “Defendants”), by counsel, in response to the Motion to Exclude Testimony of Netta Barak-Corren [Doc. 90] (“Motion to Exclude”) filed by Plaintiffs Chelsey Nelson Photography and Chelsey Nelson (collectively, “Chelsey” or “Plaintiffs”) state as follows:

INTRODUCTION

Consideration of social science data by federal courts dates back over a century to the “Brandeis brief” filed in *Muller v. State of Oregon*, 208 U.S. 412, 419 (1908), which relied more on social science data than legal precedent. The U.S. Supreme Court famously embraced social

science evidence in *Brown v. Board of Ed. of Topeka, Shawnee County, Kan.*, 347 U.S. 483, 494 n.11 (1954), when it held that the doctrine of “separate but equal” has no place in public education. Following in this tradition, there is nothing unusual or improper about Defendants introducing the expert opinion of Professor Netta Barak-Corren, a distinguished scholar and scientist whose credentials are not challenged by Plaintiffs.

Professor Barak-Corren opines that granting Chelsey Nelson a religious exemption from the application of Louisville Metro’s antidiscrimination law in this case could significantly increase the likelihood that same-sex couples attempting to hire wedding vendors in Louisville Metro will experience discrimination. Professor Barak-Corren’s opinion is largely based on an experiment she conducted, not for purposes of this litigation, but as a natural progression of her education, expertise, and scholarly interests, which is a hallmark of reliability and admissibility in *Daubert* jurisprudence. Professor Barak-Corren applied what she learned from this experiment to the facts presented by this case to offer an opinion that will assist the Court in understanding the implications of the religious exemption to Louisville Metro’s antidiscrimination law that Chelsey seeks. Plaintiffs offer meager critiques of Professor Barak-Corren’s methodology, which are appropriately addressed to the weight rather than the admissibility of the opinion. This is particularly so because Plaintiffs’ claims will be resolved by the Court, rather than a jury, thus the Court need not exercise a “gatekeeping” role. There is no basis to exclude Professor Barak-Corren’s opinion, which is relevant, reliable, and admissible.

PROFESSOR BARAK-CORREN’S EXPERTISE AND OPINION

I. Professor Barak-Corren’s Expert Qualifications

Professor Netta Barak-Corren is an accomplished legal scholar and cognitive scientist focused on empirical and behavioral analysis of constitutional law. *See* Professor Barak-Corren’s Curriculum Vitae, attached hereto as Exhibit 1. She graduated as the valedictorian of Hebrew

University with an LL.B and B.A. in Law and Cognitive Science before matriculating to Harvard Law School, where she earned Master of Laws and Doctor of Juridical Science degrees. *Id.* The three professors on the committee that conferred Professor Barak-Corren's Doctor of Juridical Science degree were Cass Sunstein (behavioral analysis of constitutional law), Martha Minow (law and religion and multiculturalism), and Tom Tyler (empirical methods and psychology in law). Transcript of Deposition of Netta Barak-Corren, Aug. 4, 2021 ("Barak-Corren Tr."), attached as Exhibit 2, 23:10-21. Professor Barak-Corren's dissertation was a behavioral examination titled "When Law and Religion Conflict." See Barak-Corren Curriculum-Vitae. She is currently a professor at Hebrew University Law School, where she serves as Head of the Law & Psychology Program, the Academic Director of the Center for the Study of Multiculturalism and Diversity, and Member of The Federmann Center for the Study of Rationality, among other distinctions. *Id.* She has authored numerous papers regarding the intersection of law and religion, received numerous grants and awards, and is widely respected in her field. *Id.*

Professor Barak-Corren has extensive education and experience in designing and conducting field experiments. Barak-Corren Tr. 25:11-26:14 (Professor Barak-Corren's doctoral work was empirical and experimental and, since she received her D.J.S., she has done significant work designing and running lab experiments and survey experiments in Israel and the United States); *id.* 26:15-25 (Professor Barak-Corren conducted lab research and survey experiments as part of the Behavioral Insights Group run by Harvard Business School Professor Max Bazerman); *id.* 30:25-31:12 (Professor Barak-Corren worked on field experiments with the Behavioural Insights Team, which consulted with the British government, the World Bank, and the International Monetary Fund); *id.* 61:3-64:25 (describing an online survey experiment Professor Barak-Corren conducted during graduate school).

II. The *Masterpiece* Experiment and Professor Barak-Corren’s Opinion in this Case

Professor Barak-Corren conducted an experiment during the pendency of the U.S. Supreme Court’s decision in *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Com’n*, 138 S. Ct. 1719 (2018) which was designed to measure the impact of the Court’s decision on wedding vendors’ willingness to provide services to same-sex couples (the “*Masterpiece* experiment”). Defendants’ Expert Report, attached as Exhibit 3, ¶ 13. Professor Barak-Corren combined methods from pseudo-experiments (studies that examine the impact of a reform by focusing on events occurring shortly before and after the reform) and field-experiments (studies that randomly allocate different treatments to subjects in the field). JLS Paper, *infra*, p. 3. Over 900 wedding vendors (bakers, photographers, and florists) were contacted shortly before and after the *Masterpiece* decision by fictitious same-sex and opposite-sex couples inquiring about wedding services. *Id.* Each business was contacted by the two types of couples both before and after the decision, resulting in four observations per business and a rich dataset that allowed for both within-business and across-businesses comparisons. *Id.* A “control” group of 251 businesses was contacted for the first time after *Masterpiece*, by both couple types, following the same procedures. *Id.*

As a result of this experiment, Professor Barak-Corren found that the *Masterpiece* ruling, which was decided in favor of a baker that refused to create a wedding cake for a same-sex couple, significantly reduced the agreement to serve same-sex couples as compared with opposite-sex couples, even among previously willing vendors. Defendants’ Expert Report, ¶ 14. The *Masterpiece* effect was equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and did not vary with the political conservativeness of the county. *Id.* However, the effect varies with the religiosity of the environment, such that businesses in areas dense with religious congregations, and particularly Evangelical congregations, were more likely to change their behavior to same-sex couples post-*Masterpiece* (even as before *Masterpiece*,

businesses in religiously-dense areas did not differ from other areas in how they responded to same-sex and opposite-sex couples). *Id.*

The results of the *Masterpiece* experiment have been published in the Journal of Legal Studies and will also appear in a forthcoming edition of the Harvard Civil Rights-Civil Liberties Law Review. See Netta Barak-Corren, *Religious Exemptions Increase Discrimination Towards Same-Sex Couples: Evidence from Masterpiece Cakeshop*, Journal of Legal Studies, 50(1) (2021) (“JLS Paper”); Netta Barak-Corren, *A License to Discriminate? The Empirical Consequences and Normative Implications of Religious Exemptions*, 56(2), Harvard Civil Rights-Civil Liberties Law Review (forthcoming 2021) (“Harvard Paper”). (Both papers are attached to Defendants’ Expert Report, which is submitted as Exhibit 3 to this brief). The Journal of Legal Studies is sponsored by the University of Chicago Law School and publishes interdisciplinary academic research that tests or develops a particular legal or social scientific theory about law. See <https://www.journals.uchicago.edu/journals/jls/about>. Professor Barak-Corren sought to have her study published by the Journal of Legal Studies because it is the best peer reviewed journal in law. Barak-Corren Tr. 49:5-23. Professor Barak-Corren consulted with experts in field studies regarding the design of the study as she was conducting the experiment. Barak-Corren Tr. 117:16-119:21. Professor Barak-Corren’s work on the *Masterpiece* experiment was peer-reviewed by numerous distinguished scholars, discussed at numerous conferences, and reviewed by the editors of the Journal of Legal Studies and the Harvard Civil Rights-Civil Liberties Law Review. See JLS Paper, *footnote on p. 1; Harvard Paper, *footnote on p. 1.

Based on the *Masterpiece* experiment and her analysis of Louisville Metro’s antidiscrimination law, Kentucky’s Religious Freedom Restoration Act (“RFRA”), and evidence regarding religiosity in Louisville Metro and Kentucky more generally, Professor Barak-Corren

opines that granting Chelsey Nelson a religious exemption from the application of Louisville Metro's antidiscrimination law in this case could significantly increase the likelihood that same-sex couples attempting to hire wedding vendors in Louisville Metro will experience discrimination resulting in the denial of equal access to goods and services. Defendants' Expert Report, ¶¶ 12-23.

ARGUMENT

III. Plaintiffs' Disagreements with Professor Barak-Corren's Methodology Are No Basis to Exclude Her Expert Opinion

Plaintiffs do not contest Professor Barak-Corren's expertise, but argue that her opinion should be excluded because her methodology is "flawed." Plaintiffs' criticisms of Professor Barak-Corren's opinion are not well-taken for the reasons set forth below. But the Court need not resolve the parties' disputes regarding Professor Barak-Corren's methodology to determine that her opinion is admissible. "Admissibility under Rule 702 does not require perfect methodology." *Best v. Lowe's Home Centers, Inc.*, 563 F.3d 171, 181 (6th Cir. 2009). Weaknesses in an expert's methodology "affect the weight that [her] opinion is given at trial, but not its threshold admissibility." *Id.* (citations omitted). "[R]ejection of expert testimony is the exception rather than the rule." *W. Tenn. Chapter of Associated Builders & Contractors, Inc. v. City of Memphis*, 300 F. Supp. 2d 600, 602 (W.D. Tenn. 2004) (citations omitted).

Plaintiffs emphasize that the Court has a gatekeeping role with respect to admissibility of expert testimony, but Plaintiffs' claims will be resolved by the Court without the assistance of a jury. In the context of a bench trial (or resolution of Plaintiffs' claims on the parties' cross-motions for summary judgment), the "gatekeeper" doctrine, which was "designed to protect juries," is "largely irrelevant." *Deal v. Hamilton County Bd. of Educ.*, 392 F.3d 840, 852 (6th Cir. 2004); *see also Douglas v. United States*, 2011 WL 2633612, at *6 (E.D. Ky. July 5, 2011) (in a bench trial, there is no jury to be protected "from being bamboozled by technical evidence of dubious merit")

(quoting *SmithKline Beecham Corp. v. Apotex Corp.*, 247 F.Supp.2d 1011, 1042 (N.D.Ill. 2003) (Posner, J.)). Because the Court’s “ability to assess the reliability of expert testimony . . . is somewhat better than that of twelve lay jurors,” *Douglas*, 2011 WL 2633612, at *6, the Court can consider the proper weight to accord Professor Barak-Corren’s opinion without excluding it from evidence.

Even in cases where a jury will hear testimony from an expert, cross-examination and presentation of contrary evidence “are the traditional and appropriate means of attacking shaky but admissible evidence.” *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 596 (1993). Exclusion of expert testimony under Rule 702 “is not intended to serve as a replacement for the adversary system.” Adv. Comm. Note to Rule 702 (quoting *United States v. 14.38 Acres of Land*, 80 F.3d 1074, 1078 (5th Cir.1996)). Plaintiffs had a full and fair opportunity to cross-examine Professor Barak-Corren at her deposition and designated a rebuttal expert who offers critiques of her methodology. Those are the appropriate means to challenge her opinion. There is no basis for Plaintiffs’ motion to exclude Professor Barak-Corren’s opinion under Rule 702.

IV. Plaintiffs’ Criticisms of Professor Barak-Corren’s Methodology Are Not Well-Taken

Plaintiffs’ scattershot critiques of the *Masterpiece* experiment and Professor Barak-Corren’s opinion in this case are unfounded and misplaced. They certainly do not warrant the draconian remedy of excluding her opinion from evidence.

A. Media Coverage of the *Masterpiece* Decision

Plaintiffs argue that Professor Barak-Corren’s conclusion that the *Masterpiece* decision caused a behavioral change in wedding vendors is speculative because she did not measure the participants’ exposure to media reports regarding the *Masterpiece* decision. As a practical matter, because this was an actual field study, rather than a survey of respondents who knew they were

participating in an experiment, it was not possible to examine what media sources the vendors consumed before or after the *Masterpiece* decision. Moreover, prior studies have demonstrated that court decisions shape public opinion indirectly even among individuals who did not read the decision, consume any media regarding the decision, or otherwise became specifically aware of the decision. Barak-Corren Tr. 91:16-93:9; Margaret E. Tankard & Elizabeth Levy Paluck, The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes, 28 PSYCH. SCI. 1334, 1340 (2017), attached as Exhibit 4 (finding that Supreme Court's decision in *Obergefell* affected public opinion regarding same-sex marriage in a way that was unrelated to frequency of media consumption).

Plaintiffs are preoccupied with how media sources reported on *Masterpiece* and whether and how Professor Barak-Corren categorized various media sources as mainstream, conservative, or progressive. Whatever bias exists in media reporting regarding court decisions is irrelevant to the *Masterpiece* experiment, which is not measuring the impact of any media bias but rather the effect of the decision itself. The media is a constant. Plaintiffs do not suggest that there is any material difference in the media bias that existed at the time the *Masterpiece* decision was released and the bias that will exist whenever this Court decides Plaintiffs' claims. Moreover, prior studies have demonstrated that court decisions affect public opinion, even among those who consume two-sided media coverage of the decision. See Katerina Linos & Kimberly Twist, The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods, 45 J. LEGAL STUD. 223, 237-47 (2016) ("Linus & Twist"), attached as Exhibit 5.

Plaintiffs also argue that Professor Barak-Corren is relying on a mischaracterization of the *Masterpiece* decision as granting a religious exemption to enforcement of an antidiscrimination law, when the case's holding was actually limited to finding that enforcement authorities were

hostile to religion, without reaching the question of whether the vendor in that case was entitled to an exemption. These nuances are irrelevant to the experiment because the *Masterpiece* decision was broadly reported and understood as a victory for a baker who asserted a religious objection to making a cake for a same-sex wedding. Barak-Corren Tr. 89:2-13, 189:17-25. To the extent the *Masterpiece* decision was characterized narrowly (as applying only in cases of hostility to religion or otherwise limited to the particular facts presented), that would have worked against the *Masterpiece* effect. In other words, if vendors understood the *Masterpiece* decision as a narrow one, they should have been less affected by the decision with respect to their willingness to provide services for same-sex weddings. The fact that Professor Barak-Corren still found a significant *Masterpiece* effect demonstrates the significant impact of the ruling on behavior among wedding vendors.

B. Analysis of Legal Regimes

Plaintiffs argue that Professor Barak-Corren's findings with respect to legal regimes undermine her conclusion that Louisville Metro would experience the *Masterpiece* effect. The negative effect of *Masterpiece* was found in all legal regimes except for counties in Texas and Indiana (which have state RFRA laws) that enacted local antidiscrimination rules. But RFRA laws differ substantively across states, with Texas and Indiana each adopting a specific type of RFRA that is not the law in all RFRA states. Professor Barak-Corren compared Kentucky's RFRA with the RFRA laws in Texas and Indiana and found material differences. Defendants' Expert Report, ¶ 15. Specifically, unlike the RFRA laws in Indiana and Texas, which both include carve-outs for anti-discrimination laws, Kentucky's law does not have any such carve out. *Id.*

Kentucky's law has only one provision, which states that "Government shall not substantially burden a person's freedom of religion" unless the government satisfies the strict scrutiny standard. See KRS 446.350. The Indiana and Texas laws both contain similar language

prohibiting the government from substantially burdening free exercise of religion except where the government can satisfy the strict scrutiny standard, but these laws carve-out civil rights laws. See Ind. Code Ann. § 34-13-9-0.7 (“This chapter does not (1) authorize a provider to refuse to offer or provide services . . . on the basis of . . . sexual orientation; (2) establish a defense to a civil action or criminal prosecution for refusal by a provider to offer or provide services . . . on the basis of . . . sexual orientation . . .”); TX CIV PRAC & REM § 110.011 (Except for religious non-profits, “this chapter does not establish or eliminate a defense to a civil action or criminal prosecution under a federal or state civil rights law.”). The Indiana carve-out was added to the law after passage of Indiana’s initial RFRA (which contained no such carve-out) resulted in significant public blow-back and loss of convention/events revenue. Defendants’ Expert Report, ¶ 15.

Professor Barak-Corren believes that different RFRA designs could have different impacts on discrimination, especially as these designs interact with existing or inexistent antidiscrimination laws. Defendants’ Expert Report, ¶ 16. Because of the differences between Kentucky’s RFRA, on the one hand, and Texas and Indiana’s RFRA, on the other hand, particularly the differences in the laws’ interaction with antidiscrimination laws, Professor Barak-Corren does not expect the Louisville Metro area to be immune from the statistically significant *Masterpiece* effect observed in the *Masterpiece* experiment. *Id.* That conclusion is bolstered by the high degree of religiosity of Louisville/Kentucky, which Professor Barak-Corren found increased the *Masterpiece* effect in jurisdictions studied as part of the *Masterpiece* experiment. *Id.*

Plaintiffs argue that the carve-out in Texas’ RFRA law applies only to federal and state antidiscrimination laws¹ and that, like Kentucky’s RFRA, the Texas RFRA does provide a defense

¹ Plaintiffs’ authority for this argument is dubious, but the Court need not resolve this issue of Texas law to resolve Plaintiffs’ motion to exclude. Plaintiffs cite two cases in which courts applied Texas RFRA against local laws, but these local laws were not civil rights laws, thus the

to local antidiscrimination laws. The fact remains that a carve-out for antidiscrimination laws is apparent on the face of the language in Texas and Indiana’s RFRA laws, which is not the case for Kentucky’s RFRA. Moreover, Plaintiffs’ argument seems to hinge on an assumption that wedding vendors would be aware of this distinction, while Plaintiffs at the same time generally resist the notion that wedding vendors are aware of judicial decisions regarding religious exemptions.

It is important to understand that Professor Barak-Corren does not offer an opinion or conclude in her papers that a particular legal regime *causes* a particular impact on behavior among wedding vendors. JLS Paper, p. 14. What she observed in the *Masterpiece* experiment was a *correlation* between a particular legal regime and a reduction in the *Masterpiece* effect. Barak-Corren Tr. 109:2-17. Her observations regarding legal regimes are “preliminary” and “require[] more research” due to the variations in RFRA laws enacted in different states. Barak-Corren Tr. 171:5-172:7. The principal finding of the *Masterpiece* experiment remains that granting religious exemptions may increase real-world discrimination experienced by same-sex couples.

C. Design of the *Masterpiece* Experiment

The *Masterpiece* experiment was conducted in four “Waves,” two before the Supreme Court announced its decision in *Masterpiece* and two after the decision was announced. Barak-Corren Tr. 110:4-111:20. In Wave 1, vendors were contacted by fictitious same-sex couples and in Wave 2 vendors were contacted by fictitious opposite-sex couples. *Id.* In the post-*Masterpiece* Wave 3, fictitious same- and opposite-sex couples were allocated randomly to contact the vendors,

enforcement action did not trigger the carve-out set forth in TX CIV PRAC & REM § 110.011 (carving out civil actions and prosecutions under “federal or state *civil rights* law” (emphasis added)). See *Merced v. Kasson*, 577 F.3d 578 (5th Cir. 2009) (city ordinance prohibiting animal slaughter); *Barr v. City of Sinton*, 295 S.W.3d 287 (Tex. 2009) (city zoning ordinance). The only other authority cited by Plaintiffs is the fact that the city of Austin failed to invoke the carve-out in a motion for dismissal of a pre-enforcement challenge to a local ordinance prohibiting employment discrimination. However, parties in litigation do not always raise all available arguments in a motion.

and then in Wave 4 that allocation was reversed, such that each vendor was contacted by both a same- and opposite-sex couple both before and after the *Masterpiece* ruling. *Id.*

There was significant attrition between Wave 1 and Wave 2 of the study, which led Professor Barak-Corren, in consultation with other experts in field studies, to adjust her strategy for Waves 3 and 4 to focus on vendors who had indicated their agreement to serve same-sex couples in Wave 1, to randomize the sequence of whether the vendor was first contacted by a same- or opposite-sex couple, and to make slight adjustments to the language and specificity with respect to wedding date used to make the request for services. Barak-Corren Tr. 116:4-119:21; JLS Paper, p. 22. Plaintiffs criticize various aspects of the design of the *Masterpiece* experiment, none of which diminish the results of the experiment or Professor Barak-Corren's opinion in this case.

Plaintiffs argue that focusing on vendors willing to provide services to same-sex couples in Wave 1 may have created a regression fallacy. But Professor Barak-Corren tested her conclusion against different data sets (a control group of vendors, businesses that indicated willingness to serve both types of couples pre-*Masterpiece*, and businesses that indicated willingness to serve same-sex couples pre-*Masterpiece*) and all data sets produced the same result, i.e. a statistically significant increase in discrimination against same-sex couples. Barak-Corren Tr. 139:2-18, 143:5-10. Professor Barak-Corren's conclusions were also bolstered by her examination of within business transitions, i.e., an examination of how an individual business changed its response from Wave-to-Wave, which demonstrated that same-sex couples were much more likely to experience negative transitions. Barak-Corren Tr. 143:11-23; JLS Paper, pp. 29-31. If Professor Barak-Corren had tested only responses to same-sex couples in Waves 3/4, then Plaintiffs' argument with respect to regression fallacy may have some merit, but that is not what Professor Barak-Corren did. Her

conclusions are not drawn from changes in responses to same-sex couples from Waves 1/2 to Waves 3/4, but rather from a comparison of responses to identical inquiries from opposite- and same-sex couples in Waves 3/4. Absent the *Masterpiece* effect, the response rates would have been the same for both couple types. Professor Barak-Corren's conclusion that post-*Masterpiece* responses reflects an increase in discrimination against same-sex couples does not depend on any comparison to pre-*Masterpiece* responses.

Plaintiffs criticize that in Wave 2 of the *Masterpiece* experiment, non-responses were assumed to be attrition, whereas non-responses in Waves 3 and 4 were coded as negative responses. Plaintiffs point out that a vendor could have many reasons for failing to respond to the inquiries in the *Masterpiece* experiment and argue that Professor Barak-Corren should not assume those reasons are discriminatory. Professor Barak-Corren acknowledges that non-responses could be due to a variety of non-discriminatory reasons, which is why she does not interpret a non-response by any individual vendor as an indication of discrimination. Barak-Corren Tr. 133:7-137:14. Professor Barak-Corren's conclusions with respect the *Masterpiece* effect are drawn from systemic differences she observed in how wedding vendors responded to identical inquiries from same- and opposite-sex couples, not isolated instances of non-responses.

Professor Barak-Corren acknowledges the possibility that attrition from Wave 1 to Wave 2 of the experiment could mean that there was reverse discrimination against opposite-sex couples before *Masterpiece*, but believes it is far more likely that other factors caused the attrition. Barak-Corren Tr. 134:17-135:16; JLS Paper, p. 21-22 (attrition is very common in studies that repeatedly measure the same respondents over time; Professor Barak-Corren investigated the attrition by conducting a random telephone survey, which suggested that businesses that provided no response

in Wave 2 were generally less responsive than other businesses, rather than suspicious or email fatigued).

As Professor Barak-Corren explained at her deposition, field experiments very commonly code non-responses as a negative response to a request for services because a non-response is the most common way to provide a negative response, since writing an explicitly negative response is time costly and can be awkward. Barak-Corren Tr. 133:7-134:15 (citing work by Marianne Bertrand, Sendhil Mullainathan, and Nobel Laureate Esther Duflo). It is more difficult to interpret non-responses when inquiries are not randomized, as was the case in Waves 1 and 2 of the *Masterpiece* experiment, which is why Professor Barak-Corren draws no inferences from non-responses in Waves 1 and 2. Barak-Corren Tr. 133:7-137:14. But that did not hinder her ability to investigate systematic differences in Waves 3 and 4, where the inquiries were randomized as between same- and opposite-sex couples. *Id.* 136:11-14 (“whatever reasons . . . businesses have for not responding, they should spread randomly and therefore equally across couple types”). The fact that non-responses did not distribute equally across couple types supports Professor Barak-Corren’s finding of the *Masterpiece* effect. *See also* JLS Paper, p. 26; Harvard Paper, pp. 38-39 (noting that same-sex couples also experienced less positive responses than opposite-sex couples and a larger increase in explicitly negative responses from before to after *Masterpiece*).

Plaintiffs speculate that differences between tone, specificity, and requests for in-person meetings in Waves 1/2 inquiries compared to Waves 3/4 may have depressed response rates. Professor Barak-Corren explained the reasons for these changes at her deposition, including that she was attempting to avoid arousing suspicion on the part of the vendors, which would have been a greater risk if the emails had been identical between Waves 1/2 and Waves 3/4. Barak-Corren Tr. 153:16-154:24; *see also* Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More*

Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination, 94 AM. ECON. REV. 991 (2004). Those differences are not material to the results of the experiment, which did not look only at how vendors changed their response to same-sex couples before and after *Masterpiece*. Barak-Corren Tr. 145:23-146:18 (If Professor Barak-Corren had looked only at that, her inference of discrimination “would have been much larger, but . . . less precise.”). Instead, Professor Barak-Corren examined how vendors’ responses differed to identical inquiries from same- and opposite-sex couples after the *Masterpiece* decision was rendered. Barak-Corren Tr. 146:19-147:5. Changes to the inquiries from Waves 1/2 to Waves 3/4 do not impact the results of the experiment because those differences were distributed equally across couple types. Barak-Corren Tr. 147:22-148:22, 155:12-17.

Plaintiffs’ critiques regarding the design of the *Masterpiece* experiment are no basis to exclude Professor Barak-Corren’s opinion. See, e.g., *Crouch v. John Jewell Aircraft, Inc.*, 2016 WL 157464, at *21 (W.D. Ky. Jan. 12, 2016) (“This argument must fail because a party’s disagreement with inputs into a methodology is generally not enough to have the methodology excluded under Rule 702. Rather, any perceived deficiencies as to inputs are generally proper issues of weight to be considered by the jury and attacked on cross-examination.”). *United States v. Sullivan*, 246 F. Supp. 2d 696, 698 (E.D. Ky. 2003) (“While it is true that there are scientists who disagree with those theories, *Daubert* does not require universal acceptance—only general acceptance. That some scientists in a field disagree with an expert’s theories or conclusions does not render those theories or conclusions unreliable under *Daubert*.”).

D. Application of the *Masterpiece* Experiment to this Case

The *Masterpiece* experiment was conducted in four states: Texas, North Carolina, Indiana, and Iowa. Plaintiffs argue that Professor Barak-Corren’s opinion should be excluded as unreliable because she has not conducted an experiment in Louisville Metro. Requirements for the

admissibility of expert testimony are not as strict as Plaintiffs suggest. There is nothing that prevents an expert from extrapolating an opinion for a particular case based on prior studies conducted in different circumstances. Indeed, that is commonplace. *See, e.g., Gen. Elec. Co. v. Joiner*, 522 U.S. 136, 146 (1997) (“Trained experts commonly extrapolate from existing data.”). Courts should not admit evidence that “is connected to existing data only by the *ipse dixit* of the expert.” *Id.* But that is clearly not the case here where Professor Barak-Corren has applied her expertise to compare Louisville Metro to the jurisdictions in her study by religiosity and legal regime. Indeed, the fact that Professor Barak-Corren’s opinion in this case flows naturally and directly out of research she conducted independent of this litigation is a hallmark of reliability that weighs in favor of admitting her testimony. *See Johnson v. Manitowoc Boom Trucks, Inc.*, 484 F.3d 426, 434-35 (6th Cir. 2007) (citing *Daubert v. Merrell Dow Pharmaceuticals*, 43 F.3d 1311, 1317-18 (9th Cir.1995)).

Plaintiffs argue that Professor Barak-Corren’s conclusions with respect to religious density in Louisville Metro are unreliable because they rely in part on statistics from Kentucky and “Louisville is not Kentucky.” As a practical matter, the sources of data regarding religiosity that Professor Barak-Corren consulted for purposes of evaluating the jurisdictions studied in the *Masterpiece* experiment did not offer data regarding religious density in Louisville Metro as compared to Kentucky more generally. Barak-Corren Tr. 161:5-19. But that does not weaken Professor Barak-Corren’s conclusion that Louisville Metro is religiously dense, particularly in light of the significant degree to which data from the Pew Institute suggests that there is a higher degree of religiosity in Kentucky than the jurisdictions sampled as part of the *Masterpiece* experiment. *See Defendants’ Expert Report*, ¶¶ 17-22.

The Pew Institute ranks all U.S. states by religiosity based on data from 2014. Kentucky is placed 13th with 63% of adults who are highly religious (a combined index score composed of belief in God, importance of religion, frequency of prayer, and worship attendance). *Id.*, ¶ 18. This result is almost identical to that of Texas (64%) and North Carolina (65%) and higher than Indiana (54%, 22th place) and Iowa (55%, 19th place). *Id.* Kentucky ranks high also on importance of religion; Kentucky is tied with Texas (86% who say religion is somewhat or very important in their lives), slightly above North Carolina (84%) and ahead of Iowa (79%) and Indiana (78%). *Id.*, ¶ 19. According to the Pew study, 49% of Kentucky's population are Evangelicals, much higher than that of all four states in the *Masterpiece* experiment (28%-35%), the same as Alabama and second only to Tennessee (52%). *Id.*, ¶ 20. Louisville, Kentucky is also home to the Southern Baptist Theological Seminary and some of the largest evangelical megachurches in the country (Southeast Christian Church and St. Stephen Baptist Church). *Id.* According to the Pew study, Kentucky also has stronger views against homosexuality and same-sex marriage than any of the states in the *Masterpiece* experiment (44% who say that homosexuality should be discouraged, as compared to 36-37% in the states in the experiment, and 52% who are opposed or strongly opposed to same-sex marriage, as compared to 41-46% in the states in the experiment). *Id.*, ¶ 21. It cannot seriously be contended that Kentucky generally and Louisville Metro, in particular, do not have a high degree of religiosity based on the metrics Professor Barak-Corren used in the *Masterpiece* experiment.

Plaintiffs also point out that U.S. Supreme Court opinions receive more publicity than federal district court opinions and argue that Professor Barak-Corren's opinion is unreliable because she has not conducted an experiment regarding a federal district court decision (as opposed to a U.S. Supreme Court decision). But local media outlets have been covering this litigation and

should certainly be expected to publicize the Court’s ultimate decision regarding Plaintiffs’ claims. *See, e.g.*, Articles from *The Courier-Journal*, collectively attached hereto as Exhibit 6. Chelsey and her counsel (Alliance Defending Freedom (“ADF”)) will also give statements to the press and otherwise publicize the result. *See, e.g.*, Opinion Pieces authored by Chelsey Nelson and Jonathan Scruggs (ADF) and published in *The Courier-Journal*, collectively attached as Exhibit 7. Indeed, ADF’s website already advertises this litigation and touts its potential implications. *See* <https://adflegal.org/blog/big-win-court-rules-photographer-chelsey-nelson-has-right-choose-which-events-celebrate>; <https://adflegal.org/chelsey-nelson-photography-story/donate>.

Plaintiffs place undue emphasis on testimony from HRC witnesses to the effect that they have not observed an increase in discrimination against same-sex couples since Plaintiffs were granted a preliminary injunction in August 2020. Of course, not all instances of discrimination are reported to HRC. Moreover, that testimony was not based on any rigorous or comprehensive analysis of the quantity of discrimination complaints and, even if correct, could be caused by numerous factors, including the Covid-19 pandemic, which has led to a reduction in use of public accommodations and may have dissuaded some complainants from initiating a complaint with HRC, which typically involves an in-person meeting with case investigators. Once again, Plaintiffs’ criticisms of Professor Barak-Corren’s opinion are properly addressed to the weight of the evidence rather than the admissibility of her opinion.

E. Long-Term Effects

Plaintiffs claim that Professor Barak-Corren’s opinion is unreliable because the *Masterpiece* experiment does not study the long-term effects of court decisions granting religious exemptions to antidiscrimination laws. Professor Barak-Corren acknowledges that her study does not purport to find that the impact of any particular court decision is “permanent,” but has explained that there are numerous studies which suggest that the impact of judicial decisions is

prolonged and significant. Barak-Corren Tr. 176:19-181:5 (citing: Linos & Twist, which found that the effect persisted nine months after a court decision and even slightly increased; Eugene K. Ofofu et al., *Same-Sex Marriage Legalization Associated With Reduced Implicit And Explicit Antigay Bias*, 116 PROC. NAT'L ACAD. SCI. U.S. 8846 (2019), which found a consistent and prolonged an effect over a 12-year period; Julia Raifman et al., *Difference-in-Differences Analysis of the Association Between State Same-Sex Marriage Policies and Adolescent Suicide Attempts*, JAMA Pediatr. 171(4): 350-56 (2017), which found an effect 7-18 months after the passage of legislation). More to the point, the *Masterpiece* effect is harmful to same-sex couples even if it is short in duration.

V. Professor Barak-Corren's Opinion is Relevant

Plaintiffs argue that Professor Barak-Corren's opinion is irrelevant because she assumes her conclusion, i.e. that all declines or non-responses are discriminatory, while Plaintiffs argue that asserting a "message-based objection" to providing services to same-sex couples is not discriminatory. Professor Barak-Corren *had no assumptions* about what she would find when she embarked on the *Masterpiece* experiment. See JLS Paper, pp. 2-3; Barak-Corren Tr. 58:3-61:2. Indeed, she thought that arguments on both sides of the debate were potentially plausible. *Id.* The opponents of exemptions were arguing that exemptions will expand discrimination towards same-sex couples, and this squares with classic law and economics theories. *Id.* But the proponents of exemptions had an argument that looked plausible as well, a-priori, which is that support for same-sex marriage and homosexuality is rising and that the whole issue of religious objection applies to a negligible minority of businesses and individuals. *Id.* The goal of the *Masterpiece* experiment was to "bring more empirical clarity to normative debates that often have . . . assumptions about consequences . . . that are often just not based on actual evidence." Barak-Corren Tr. 59:1-6.

Professor Barak-Corren hoped to explore the distinction Plaintiffs highlight, i.e. refusals to provide a service to a same-sex couple due to a “message-based objection” rather than discriminatory animus towards same-sex couples. To her disappointment, vendors did not make this distinction in their responses to inquires sent during the *Masterpiece* experiment, for example by offering to provide off-the-shelf products to same-sex couples instead of a custom wedding cake. Barak-Corren Tr. 193:11-198:13. More importantly, there is no difference in the impact to same-sex couples between a “message-based objection” and a refusal of service due to discriminatory animus.

Plaintiffs have argued that Louisville Metro must prove a compelling “interest in denying an exception to just Chelsey” (see Doc. 90, p. 30 (citing *Fulton v. City of Philadelphia, Pennsylvania*, 141 S. Ct. 1868, 1881 (2021))), while at the same time arguing that Professor Barak-Corren’s opinion, which offers empirical evidence regarding the harm to same-sex couples associated with granting “just Chelsey” a religious objection, is irrelevant. The Court need not spend long analyzing Plaintiffs’ relevance arguments to see through the inconsistency in Plaintiffs’ position. What Plaintiffs really want is for the Court to consider only Chelsey’s interests, and not the interests of real-world individuals who experience discrimination on the basis of their sexual orientation. The Court is under no such constraint. The Court can properly consider Professor Barak-Corren’s opinion and the *Masterpiece* experiment, which shows that even an individual exemption can have a significant and robust impact on a market and its customers.

CONCLUSION

For the foregoing reasons, Defendants respectfully request that the Court deny Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren.

Respectfully submitted,

/s/ Casey L. Hinkle

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Counsel for Defendants

CERTIFICATE OF SERVICE

I hereby certify that on September 29, 2021, the foregoing was filed via the Court's electronic filing system, which will automatically send notice of such filing to all counsel of record.

/s/ Casey L. Hinkle

Counsel for Defendants

Exhibit 1

Netta Barak-Corren

Hebrew University Law School | Mt. Scopus, Jerusalem 91905, Israel | netta@huj.ac.il

ACADEMIC POSITIONS

Member, The Federmann Center for the Study of Rationality, Hebrew University	2021 – Present
Academic Director, Center for the Study of Multiculturalism and Diversity	2020 – Present
Nootbaar Religious Freedom Fellow, Pepperdine University School of Law	2020 – Present
Associate Professor (tenured), Hebrew University Law School	2020 – Present
Head of the Law & Psychology Program	2017 – Present
Assistant Professor, Hebrew University Law School	2017 – 2020

EDUCATION

S.J.D. Harvard Law School <i>Dissertation:</i> When Law and Religion Conflict: A Behavioral Examination <i>Committee:</i> Cass Sunstein, Martha Minow, Tom Tyler (Yale)	2016
LL.M. Harvard Law School (waived on admission to the S.J.D.) <i>Dean's Prize in Law and Economics</i> <i>Dissertation awarded the Fisher-Sanders Award</i>	2013
LL.B. and B.A. in Law and Cognitive Science, Hebrew University of Jerusalem <i>Valedictorian, Summa cum laude with highest honors</i>	2012

RESEARCH INTERESTS

Constitutional Law, Law and Religion, Law and Social Norms, Empirical Legal Studies, Behavioral Economics, Moral Psychology, Conflict Resolution.

PUBLICATIONS

20. Netta Barak-Corren, *Religious Freedom in Israel*, in HANDBOOK ON ISRAELI CONSTITUTIONAL LAW (forthcoming, Barak A., Medina, B., and Roznai, Y. Eds., forthcoming, Oxford University Press)
19. Netta Barak-Corren and Yael Kariv-Teitelbaum, *Behavioral Responsive Regulation*, REGULATION AND GOVERNANCE (forthcoming 2021).
 - ✧ Invited presentation at Israeli Ministry of Justice, Department of Regulation.
18. Netta Barak-Corren and Lotem Perry-Hazan, *Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations*, JOURNAL OF SOCIAL ISSUES (2021) (special issue for the 50th anniversary of legal socialization)
 - ✧ Press coverage: [Ha'aretz](#), [The Marker](#), [Haredim10](#), [Seder Yom \(IDF radio\)](#)
 - ✧ Invited presentations at Israeli Knesset, Ministry of Education, Ministry of Justice, and Israeli Democracy Institute.
 - ✧ Non-technical summary written and circulated by [Chief Scientist, Ministry of Education](#).

17. Yuval Barak-Corren, Netta Barak-Corren, Alex Gileles-Hillel, and Eyal Heiman, *The Effect of C-Reactive Protein on Chest X-Ray Interpretation: A Decision-Making Experiment Among Pediatricians*, [Pediatric Pulmonology](#).
16. Netta Barak-Corren, *The Empirical Consequences and Normative Implications of Religious Exemptions*, 56(1) [HARVARD CIVIL RIGHTS-CIVIL LIBERTIES LAW REVIEW](#) (forthcoming 2021)
15. Netta Barak-Corren, *Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from Masterpiece Cakeshop*, 50(1) [JOURNAL OF LEGAL STUDIES](#) 75-110 (2021)
 - ✧ Invited presentations at Chicago Law School, Harvard Law School, Stanford Law School, Texas Law School, Virginia Law School, and Yale Law School.
 - ✧ Press coverage: [Boston Globe](#), [SCOTUSblog](#), [National Constitution Center](#)
 - ✧ Summarized for [the Atlantic](#).
14. Netta Barak-Corren, *Regulating for Social Integration by Behavioral Design: An Evidence-Based Approach for Culturally Responsive Regulation*, [REGULATION AND GOVERNANCE](#) (forthcoming 2021)
13. Karen Huang, Regan Bernhard, Netta Barak-Corren, Max H. Bazerman, and Joshua D. Greene, *Veil-of-Ignorance Reasoning Mitigates Self-Serving Bias in Resource Allocation During the COVID-19 Crisis*, *JUDGMENT AND DECISION MAKING* (2021)
 - ✧ Summarized for [HARVARD BUSINESS REVIEW](#).
12. Netta Barak-Corren, *Taking Conflicting Rights Seriously*, 65(2) *VILLANOVA LAW REVIEW* 295 (2020)
 - ✧ Lead article of the issue
 - ✧ Selected for the 2018 Stanford International Junior Faculty Forum (~5% acceptance rate)
 - ✧ Selected for CELS 2018 at Michigan Law School, Ann Arbor
 - ✧ Invited presentations at Chicago Law School, Harvard Law School, and Toronto Law School.
 - ✧ Recipient of the Gorni Junior Faculty Award for Outstanding Research in Public Law.
11. Netta Barak-Corren & Daphna Lewinsohn-Zamir, *What's in a name? The asymmetric effects of identifiability on offenders and victims of sexual harassment*, 16(4) *JOURNAL OF EMPIRICAL LEGAL STUDIES* 955 (2019)
 - ✧ Selected for CELS 2018 at Michigan Law School, Ann Arbor
 - ✧ Selected for SJDM 2018 at New Orleans (%20 acceptance rate)
10. Netta Barak-Corren, *Reexamining the evidence on Ultra-Orthodox attitudes and gender separation in academia*, 49 *MISHPATIM—HEBREW U. LAW REVIEW* (2019) [in Hebrew].
 - ✧ Recipient of the Gorni Junior Faculty Award for Outstanding Research in Public Law.
 - ✧ Cited by four of five Supreme Court Justices in HCJ 8016/16 Berzon v. State of Israel (2021)
 - ✧ Hebrew summary: [ICON-S-IL Blog](#) 2019; Analysis post-Berzon: [ICON-S-IL Blog](#) 2021.
 - ✧ Press coverage: [Ha'aretz](#), [Globes](#), [Ynet](#)
9. Netta Barak-Corren & Max H. Bazerman, *Indecision and decision making in moral conflicts*, *ORG. DYNAMICS* (2019).
8. Netta Barak-Corren, Yuval Feldman, & Noam Gidron. *The Provocative Effect of Law: Majority Nationalism*

[*and Minority Discrimination*](#), 15(4) JOURNAL OF EMPIRICAL LEGAL STUDIES 951-986 (2018).

7. Netta Barak-Corren & Max H. Bazerman, [*Saving lives your task or God's? The nuanced relationship between religiosity and moral judgment*](#), 12(3) JUDGMENT AND DECISION MAKING 280-296 (2017).
 6. Netta Barak-Corren, [*Beyond dissent and compliance: Religious decision-makers and secular law*](#), 6(2) OXFORD JOURNAL OF LAW AND RELIGION 293-322 (2017).
 ✧ Translated and reprinted in 4 [BIFRAT U'BICHLAL](#) 17-53 (2019) [in Hebrew].
 5. Netta Barak-Corren, Chia-Jung Tsay, Fiery A. Cushman, & Max H. Bazerman, [*If you're going to do wrong, at least do it right: Moral conflict promotes moral consistency*](#), 64(4) MANAGEMENT SCIENCE 1528-1540 (2017).
 4. Netta Barak-Corren, [*Does Antidiscrimination Law Influence Religious Behavior? An Empirical Examination*](#), 67(4) HASTINGS LAW JOURNAL 957 (2016).
 3. Netta Barak-Corren, [*Antidiscrimination Law and the Religious Workplace*](#), LGBT LAW IN ISRAEL (2016) [in Hebrew].
 2. Netta Barak-Corren*, Edy Glozman*, & Ilan Yaniv, [*False Negotiations: The Art and Science of Not Reaching an Agreement*](#), 59(4) JOURNAL OF CONFLICT RESOLUTION 671 (2015). (* equal contribution)
 ✧ Recipient of the Howard Raiffa Doctoral Student Paper Award.
 ✧ Reviewed in Harvard's Program on Negotiation Magazine.
 1. Netta Barak-Corren, [*Property Rights and Workers Rights: Protecting the Wage of Independent Workers*](#), 42(3) MISHPATIM—HEBREW U. LAW REVIEW 973 (2012) [in Hebrew]
 ✧ Recipient of the M. Goldberg Prize for the best paper on labor law in Israel in 2011.
- Netta Barak-Corren, *Compliance with the law under religion-based normative conflicts: A behavioral analysis and preliminary prescriptions* (Unpublished master thesis)
 ✧ Recipient of the Fisher-Sanders Award for the best paper written on conflict resolution in Harvard, MIT and Tufts, 2013

SHORTER PIECES

Netta Barak-Corren, [*The HCJ Decision on Gender Segregation in Israeli ultra-Orthodox Academia: Towards Empirical Constitutional Law*](#) ICON-S-IL BLOG (2021) [Hebrew]

Netta Barak-Corren, [*On the Lethal Cocktail of Israeli Democracy and the End Problem of the HCJ wars*](#), 45 TEL AVIV UNIVERSITY LAW REVIEW FORUM (2021) [Hebrew]

Netta Barak-Corren, [*How One Supreme Court Decision Increased Discrimination Against LGBTQ Couples*](#), THE ATLANTIC (2021)

Netta Barak-Corren, [*Shared Fate, Unshared Faith: Israel and the Haredi Society in the Current Corona Moment*](#), THE JEWISH QUARTERLY REVIEW FORUM

David Enoch, Netta Barak-Corren, Michal Shur-Ofri, Ofer Malcai, and David Heyd, *Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management*, HEBREW UNIVERSITY LAW JOURNAL FORUM (2021) [[Hebrew](#)] [English synopsis](#)

Max H. Bazerman, Regan Bernhard, Joshua Greene, Karen Huang, and Netta Barak-Corren, [How Should We Allocate Scarce Medical Resources?](#) HARVARD BUSINESS REVIEW (2020).

Netta Barak-Corren, [Gender Segregation in Israeli ultra-Orthodox Academia is not a Zero-Sum Game](#), ICON-S-IL BLOG (2019) [Hebrew]

SELECTED WORKS IN PROGRESS

Religion-equality conflicts and the war within religion, R&R, AMERICAN JOURNAL OF COMPARATIVE LAW (A*)

Majority Nationalism Laws and the Equal Protection of Minorities: Experimental and Observational Evidence from Israel, R&R, JOURNAL OF LEGAL STUDIES (A*) (with Noam Gidron and Yuval Feldman)

SELECTED AWARDS, GRANTS, AND HONORS

Elected member, The Federmann Center for the Study of Rationality	2021
Cornell University Center for Social Sciences Grant Competition Award (\$12,000, w/ Nelson Tebbe)	2021
Nootbaar Religious Freedom Fellow, Pepperdine University School of Law	2020-22
Gorni Prize for an Outstanding Junior Scholar in Public Law	2019
Israeli Science Foundation Research Grant No. 1487/19 (\$152,676 for four years, w/ Lotem Perry-Hazan)	2019
Sacker Prize for Paper Accepted to <i>Mishpatim</i> (\$1500)	2019
Selected for the Seventh WZB Migration and Diversity Conference (~3% acceptance rate)	2019
Selected for the Stanford International Junior Faculty Forum (~5% acceptance rate)	2018
Birk Prize for Excellence in Legal Research	2018
Alicia Fund, Cherrick Center for the Study of Zionism Research Grant (\$2000)	2018
Minerva Center for Human Rights Research Grant (\$2000)	2018
Barak Center for Interdisciplinary Research Grant (\$3500, \$2500, \$4500)	2016-19
Empirical Legal Research Group Inaugural Fellow, Harvard Law School	2015-16
Graduate Research Fellowship, Program on Negotiation, Harvard Law School	2015-16
Sinclair Kennedy Travelling Fellowship, Society and Fellows of Harvard College	2014-15
Human Rights and Judaism Fellowship, Israeli Democracy Institute	2014-17
Next Generation Grant, Program on Negotiation, Harvard Law School	2014
Harvard's Interfaculty Initiative on Mind, Brain, and Behavior Student Award	2014
Cravath International Grant, Harvard Law School	2014
Student Empirical Research Grant, Program on the Legal Profession	2013
Harvard Law School Summer Academic Fellowship	2013
Dean's Prize in Law and Economics, Harvard Law School	2013
Gammon Fellowship, Harvard Law School	2012
Shapiro Scholarship, Harvard Law School	2012-13

International Peace Scholar, P.E.O.	2012-13
Pearlman Scholarship, Hebrew University of Jerusalem	2012-13
Valedictorian (1 of 230), Hebrew University of Jerusalem School of Law	2012
Lord Wolf Award for Leadership and Social Contribution, Hebrew University	2012
Albert Einstein Award	2009-11
✧ Three-time recipient of HUJI's highest honor for academic excellence	
Rector Prize & Dean's List	2009-11
✧ Ranked 1 of 900 students in the law school for three consecutive years	
The Cognitive Science Excellence Program Award	2011
✧ Ranked at the top of the interdisciplinary excellence program	
Oberlander Prize for excellence in international Moot Court Competitions	2011
Quarter-Finalist in the Jessup International Moot Court Competition	2011
✧ Reached for the first time in the history of Israel	
Top Oralist and Memorial Honors in the Jessup International Moot Court Competition	2010
Best Oralist, Hebrew University's Moot Court competition	2009
Best Speaker, The European Model United Nations, the Hague, Netherlands	2009
<i>Miscellaneous:</i>	
Head of Intelligence Creativity Award for Contribution to Israeli National Security	2007
Israeli Young Scientist Competition, second prize (during high school)	2002

SELECTED OTHER ACTIVITY

Coauthored white papers as part of the COVID-Collective Impact Academia-IL initiative	2020
✧ Ethics Guidelines during the COVID-19 pandemic	
✧ How to harness public cooperation with the COVID-19 regulations	
Academic advisor to JDC Israel on designing behaviorally informed intervention programs	2019
Founded and organized the Empirical Legal Research Group in Harvard Law School	2013-16
Behavioural Insights Team (BIT), London, UK, Research Fellow	2014
Behavioral Insights Group, Harvard University, Undergraduate research mentor	2013-14
Israeli Supreme Court, Chambers of Chief Justice Dorit Beinisch, Law Clerk	2011-12
Mishpatim—Hebrew University Law Review, Editor-in-law	2008-10

SELECTED TALKS AND PRESENTATIONS

* Only talks and presentations are included, no posters.

1. **2014, Harvard Law School**, Program on the legal Profession Speaker Series (invited). Paper titled: "Not in My School: The Conflict between Law and Religion from the Eyes of Religious Educators";
2. **2014, Stanford Law School**, Inaugural Conference for Junior Researchers, the Program in Law and Society. Paper titled: "Not in My School: The Conflict between Law and Religion from the Eyes of Religious Educators";
3. **2014, Harvard Business School**, the Behavioral Insights Group Lab Meeting. Paper titled: "If You're Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under Joint Evaluation";
4. **2014, Hebrew University**, International Workshop on Behavioral Legal Studies (invited). Paper titled: "If You're Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under

Joint Evaluation”;

5. **2014, Behavioral Insights Team, London, UK** (invited). Paper titled: “If You’re Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under Joint Evaluation”;
6. **2015, Israeli Democracy Institute**, Human Rights and Judaism Colloquium. Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
7. **2015, Yale Law School**, Conference on Law, Religion and Politics, doctoral workshop. Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
8. **2015, Harvard Psychology Department**, Boston Area Moral Cognition Group (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
9. **2015, Washington University St. Louis**, Annual Conference on Empirical Legal Studies. Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
10. **2015, Hebrew University Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
11. **2015, Bar Ilan Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
12. **2015, Haifa Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
13. **2015, IDC Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
14. **2016, Israeli Democracy Institute**, Human Rights and Judaism Colloquium. Paper titled: “Social Impact Discrimination”;
15. **2016, Harvard Business School**, the International Behavioral Exchange 2016 conference (invited). Presentation titled: “Law, Values, and Norm Conflicts”;
16. **2016, Hebrew University Law School**, the Israeli ICON-S conference. Paper titled: “Social Impact Discrimination”;
17. **2016, McGill Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Social Impact Discrimination”;
18. **2016, Israeli Democracy Institute**, the weekly roundtable (invited guest speaker). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
19. **2016, Bar Ilan Law School**, International Conference on Decision-making and Law (invited). Paper titled: “The Nation Law and Minority Discrimination”;
20. **2017, Jerusalem Institute**, Conference on Ultra-Orthodox Education (invited keynote speaker). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
21. **2017, Ben Gurion University**, Decision Making and Economic Psychology (DMEP) Seminar (invited). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
22. **2017, Tel Aviv Law School**, Law and Economics Workshop (invited). Paper titled: “Social Impact Regulation”;
23. **2017, Hebrew University Law School**, Public Law Workshop. Paper titled: “Social Impact Regulation”;
24. **2017, Bar Ilan Law School**, Empirical Legal Studies workshop (invited). Paper titled: “Social Impact Regulation”;
25. **2017, Notre Dame Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
26. **2017, Cornell Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “The

- Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
27. **2017, Virginia Law School**, Annual Comparative Constitutional Law Roundtable (invited). Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
 28. **2017, Ben Gurion University**, joint workshop of the Hebrew University Federman Center for the Study of Rationality & Ben Gurion Center on Decision Making and Economic Psychology (invited). “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 29. **2018, Hebrew University Law School**, Law and Economics Workshop. Paper titled: “Social Impact Regulation”;
 30. **2018, Hebrew University Federman Center for the Study of Rationality**, Annual Retreat (invited guest speaker). Presentation titled: “Conflicts Between Law and Religion: An Empirical Examination”;
 31. **2018, American Law and Economics Annual Meeting (ALEA)**. Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
 32. **2018, Hebrew University Law School**, Public Law Workshop. Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
 33. **2018, Hong Kong University Law School**, ICON-S Conference. Paper titled: “The War Within”;
 34. **2018, Harvard Psychology Department**, Moral Psychology Lab (invited speaker). Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 35. **2018, Harvard-MIT Program on Negotiation Seminar Series** (invited speaker). Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 36. **2018, Harvard Law School**, Empirical Legal Studies Series (invited). Paper titled: “The War Within”;
 37. **2018, Chicago Law School**, Public Law and Legal Theory Workshop (invited). Paper titled: “The War Within”;
 38. **2018, Stanford Law School**, International Junior Faculty Forum (invited). Paper titled: “The War Within”;
 39. **2018, Michigan Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “The War Within”;
 40. **2018, Michigan Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “What’s in a Name?: The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 41. **2018, University of Toronto Law School**, Law and Religion Workshop (invited). Paper titled: “The War Within”;
 42. **2018, Society on Judgment and Decision-Making Annual Conference**, New Orleans. Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 43. **2018, IDC Law School**, Justice M. Cheshin Memorial Conference (invited). Paper titled: “Reexamining the Evidence on Ultra-Orthodox Attitudes and Gender Separation in Academia”;
 44. **2019, Tel Aviv Law School**, Israeli Law and Society Conference. Paper titled: “Reexamining the Evidence on Ultra-Orthodox Attitudes and Gender Separation in Academia”;
 45. **2019, Tel Aviv Law School**, faculty workshop (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 46. **2019, WZB Berlin**, Seventh WZB Migration and Diversity Conference (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 47. **2019, Bar Ilan Law School**, Conference on the Interpretation of the Nation Law (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies:

- Experimental and Observational Evidence from Israel”;
48. **2019, Federman Center for the Study of Rationality**, joint workshop of the Hebrew University Federman Center & Ben Gurion Center on Decision Making and Economic Psychology (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 49. **2019, Hebrew University Law School**, public law workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 50. **2019, Hebrew University Law School**, Humboldt-Minerva Human Rights Under Pressure joint seminar. “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 51. **2019, Hebrew University Law School**, International Workshop on Behavioral Legal Studies. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 52. **2019, Hebrew University Law School**, faculty workshop. Paper titled: “What’s in a Name?: The Disparate Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 53. **2019, University of Toronto Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 54. **2019, Claremont McKenna College**, Annual Conference on Empirical Legal Studies (waived). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 55. **2020, Bar Ilan Law School**, Faculty Seminar (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 56. **2020, Hebrew University School**, Private Law Workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 57. **2020, Law, Society, and Psychological Science CRN Summer Research Series**, Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 58. **2020, International Forum on the Future of Constitutionalism**, Works-in-Progress Comparative Constitutional Law Summer Roundtable (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 59. **2020, Chicago Law School**, Measuring Impact in Constitutional Law Conference (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 60. **2020, Hebrew University**, Jewish and Israeli Law Student Club. Talk titled: Prayers and Demonstrations during COVID-19.
 61. **2020, Ministry of Justice**, Regulation Forum. Paper titled: “Behavioral Responsive Regulation”.
 62. **2020, Bar Ilan University**, Jewish and Democratic Law Center. Talk titled: “Religion in the Public Sphere”.
 63. **2020, Tel Aviv University**, The Wars of the High Court of Justice Conference. TBA.
 64. **2020, Israeli Democracy Institute**, Ultra-Orthodox Researchers Forum. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
 65. **2020, UCLA Law School**, Private Law Workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 66. **2020, Haifa University School of Education**, Law, Religion, and Education webinar. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
 67. **2020, Pepperdine Law School**, Nootbarr Fellows Workshop. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.

68. **2021, Hebrew University of Jerusalem Law School**, Public Law Workshop. Paper titled: "Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations".
69. **2021, University of Virginia Law School**, Family Law Center Symposium, Paper titled: "The Effects on Children of Equality Rules for Religious Placement Agencies".
70. **2021, University of Virginia Law School**, Law and Economics Workshop (invited). Paper titled: "Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*".
71. **March 2021, Tel Aviv University, School of Government, Political Science and International Relations**, faculty seminar. "The day after the elections: how to fix Israeli democracy?"
72. **March 2021, Philosophy 360**, Paper titled: "Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management".
73. **March 2021, Rutgers University, Center for Population-Level Bioethics** seminar, Paper titled: "Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management".
74. **March 2021, Israeli National Academy of Arts and Sciences**, (invited) special webinar, "Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management".
75. **April 2021, Hebrew University of Jerusalem, School of Public Policy**, faculty seminar (invited). Paper titled: "Behavioral Responsive Regulation."
76. **April 2021, IDC Herzliya Law School**, Constitutional Law Workshop (invited). Paper titled: "Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management".
77. **April 2021, Bar Ilan Law School**, Constitutional Law Workshop (invited). Paper titled: "Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management".
78. **April 2021, Yale Law School**, Religious Freedom Clinic (invited). Paper titled: "Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*".
79. **April 2021, Israeli Ministry of Justice**, Department of Consulting and Legislation Advanced Seminar in Constitutional law, (keynote speaker). "Constitutional Law: A General Perspective and Current Trends".
80. **May 2021, Law and Society Association Annual Meeting**, virtual conference. Paper titled: "Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations".
81. **May 2021, Law and Society Association Annual Meeting**, virtual conference. Paper titled: "Behavioral Responsive Regulation."
82. **May 2021, Hebrew University Center for Interdisciplinary Data Science Research workshop**. Paper titled: "Examining the Effect of Anti-Discrimination Legislation on Outcomes of Children in Adoption and Foster Care."
83. **June 2021, Haifa University Law School**, faculty seminar (invited). Paper titled: "Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*".
84. **June 2021, Annual Law and Religion Roundtable** (invited). Paper titled: "Examining the Effect of Anti-Discrimination Legislation on Outcomes of Children in Adoption and Foster Care.".
85. **June 2021, Compliance-Net Annual Meeting**, virtual conference. Paper titled: "Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations".
86. **July 2021, International Society of Constitutional Law Annual Meeting**, virtual conference.

- Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
87. **July 2021, International Society of Constitutional Law Annual Meeting**, virtual conference. Paper titled: “Behavioral Responsive Regulation”.
 88. **July 2021, International Society of Constitutional Law Annual Meeting**, virtual conference. Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”.
 89. **July 2021, International Society of Constitutional Law Annual Meeting**, virtual conference. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
 90. **July 2021, Retreat Rationality**, virtual conference. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
 91. **November 2021, Hebrew University Psychology Department**, faculty seminar (scheduled)
 92. **November 2021, Chicago Law School**, Coase-Sandor Center on Law and Economics (scheduled)
 93. **January 2022, Notre Dame Law School**, Empirical, Behavioral, and Experimental Analysis of the Law International Conference (scheduled)
 94. **January 2022, American Association of Law Schools Law & Religion Section** (scheduled)
 95. **March 2022, Haifa University Political Science Department**, faculty seminar (scheduled)
 96. **March 2022, Israeli Judiciary**. Talk titled: Behavioral Analysis of Constitutional Law (scheduled)

~ last update: August 2021

Exhibit 2

Professor Netta Barak-Corren

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1 UNITED STATES DISTRICT COURT WESTERN DISTRICT OF KENTUCKY LOUISVILLE DIVISION Case No. 3-19-CV-00851-BJB-CHL CHELSEY NELSON PHOTOGRAPHY, LLC and CHELSEY NELSON, PLAINTIFFS v. LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT, et al., DEFENDANTS DEPONENT: PROFESSOR NETTA BARAK-CORREN DATE: AUGUST 4, 2021 COURT REPORTER: JESSICA TAYLOR ROSS TAYLOR COURT REPORTING KENTUCKY 2901 SIX MILE LANE LOUISVILLE, KENTUCKY 40220	3 1 2 A P P E A R A N C E S 3 4 COUNSEL FOR PLAINTIFFS: 5 Ryan Bangert, Esq. 6 Bryan D. Neihart, Esq., AZ Bar No. 035937 7 (Via Zoom videoconference) 8 ALLIANCE DEFENDING FREEDOM 9 15100 N. 90th Street 10 Scottsdale, Arizona 85260 11 Telephone: (480)444-0020 12 Email: rbangert@adflegal.org 13 bneihart@adflegal.org 14 15 COUNSEL FOR DEFENDANT, LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT: 16 17 Casey L. Hinkle, Esq. 18 Rick Adams, Esq. 19 (Via Zoom videoconference) 20 KAPLAN JOHNSON ABATE AND BIRD, LLP 21 710 W. Main Street, 4th Floor 22 Louisville, Kentucky 40202 23 Telephone: (502) 416-1630 24 Email: chinkle@kaplanjohnsonlaw.com 25 radams@kaplanjohnsonlaw.com
2 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 PROFESSOR NETTA BARAK-CORREN 16 AUGUST 4, 2021 17 18 19 20 21 22 23 24 25	4 1 2 A P P E A R A N C E S 3 4 COUNSEL FOR DEFENDANT, LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT: 5 6 Jason D. Fowler, Esq. 7 John Carroll, Esq. 8 (Via Zoom videoconference) 9 ASSISTANT JEFFERSON COUNTY ATTORNEYS 10 531 Court Place, Suite 900 11 Louisville, Kentucky 40202 12 Telephone: (502)574-6321 13 Email: jason.fowler@louisvilleky.gov 14 john.carroll2@louisvilleky.gov 15 16 17 18 19 20 21 22 23 24 25

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9 1 2 INTRODUCTION 3 4 THE COURT REPORTER: We're on the 5 record. My name is Jessie Ross, the court 6 reporter. I'm reporting remotely today from 7 Louisville, Kentucky via Zoom videoconference. 8 Counsel, please state your name and whom you 9 represent and let us know if there is anyone 10 else present with you. 11 MR. BANGERT: Ryan Bangert, 12 representing Plaintiff, and I have no one with 13 me in the room. 14 MS. HINKLE: Casey Hinkle is here 15 for Defendants, and also on the Zoom call are 16 John Carroll, Jason Fowler, and Rick Adams, all 17 for the defendants. 18 MR. NEIHART: And Bryan Neihart is 19 also here representing the plaintiffs. No one 20 else is with me. 21 THE COURT REPORTER: Okay. And I 22 think that is everyone. I will now ask the 23 deponent. Ma'am, can you please state your full 24 name for the record? 25 THE WITNESS: Sure. My name is	11 1 2 PROCEEDINGS 3 4 PROFESSOR NETTA BARAK-CORREN, the 5 said witness, called by the Plaintiffs, Chelsey 6 Nelson Photography, LLC, and Chelsey Nelson, by 7 and through counsel, having been first duly 8 sworn, was examined and deposed as follows: 9 10 * * * 11 EXAMINATION 12 BY MR. BANGERT: 13 Q. Thank you, Professor. Thank you 14 for being here today. I know you've already 15 done this, but would you please state your name 16 for the record one more time? 17 A. Sure. Netta Barak-Corren. 18 Q. And Professor, if it's okay with 19 you, I'm just going to refer to you as Professor 20 today. Is that acceptable? 21 A. Yes, please Counsel. 22 Q. Very good. Professor, have you 23 testified before in a court proceeding? 24 A. No, Counsel. 25 Q. This is your first time?
10 1 Netta Barak-Corren. Thank you. 2 3 * * * 4 5 THE COURT REPORTER: If you raise 6 your right hand for me I'll get you sworn in. 7 THE WITNESS: (Complying.) 8 THE COURT REPORTER: Do you 9 solemnly swear or affirm the testimony you are 10 about to give will be the truth, the whole 11 truth, and nothing but the truth? 12 THE WITNESS: Yes. 13 THE COURT REPORTER: Thank you. 14 Counsel, you may proceed. 15 16 * * * 17 18 19 20 21 22 23 24 25	12 1 A. Yes. 2 Q. Is there any reason that you're 3 aware of today that you won't be able to testify 4 accurately and truthfully? 5 A. No. 6 Q. Now, is English your first 7 language? 8 A. No. 9 Q. What is your first language? 10 A. Hebrew. 11 Q. How many languages do you speak? 12 A. Well, I speak Hebrew and English, 13 and also a little bit of Arabic and Italian. 14 Q. Wonderful. If there's something 15 that I say today that is not well said or is not 16 understandable, would you please ask me to 17 repeat myself? I wanted to make sure that we're 18 communicating well. 19 Is that -- is that fair? 20 A. That's wonderful. 21 Q. And if you need a break at any 22 time, please be sure to ask me. This is not an 23 endurance test, nor is it a -- a sprint. So if 24 you do need a break, please just raise your hand 25 or say so and we can do so. My only request is

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13 1 that if there's a take -- that the question is 2 pending, that you answer the question and then 3 we can take a break. 4 Is that fair? 5 A. Yes. 6 Q. Very good. Now, I know that we 7 sent some documents over to your counsel earlier 8 today. I wanted to know if -- if you received 9 them and if you have them available. The first 10 would be a copy of your expert report in this 11 case, and that would include your report, as 12 well as the three exhibits that accompany that 13 report. Do you have that document available to 14 you? 15 A. Yes, you mean by that -- my report 16 -- my two papers and the Online Appendix, right? 17 Q. And I -- and I apologize, I was not 18 referring to your Online Appendix. I was -- I 19 was going to mention that just a moment. What I 20 was referring to you is your expert report dated 21 June 30, 2021 that you've signed and that was 22 submitted in this case. 23 A. Yes, I have it. 24 Q. And attached to that -- you have 25 that.	15 1 unpublished paper that -- and I don't know if 2 its been published yet or not, but it was going 3 to be -- looking for the name of the journal. 4 The Journal of Legal -- 5 A. The Journal of Legal Studies. Yes. 6 It's actually been published -- 7 Q. Okay. So it has been published 8 now? 9 A. -- in the past week or two. Yeah. 10 Q. Very good. Okay. So though -- 11 that package of documents is what I'm referring 12 to as -- as your report. 13 Is that fair? 14 A. Yes. Yes. 15 Q. And then there's a second document 16 that you mentioned and that is your Online 17 Appendix? 18 A. Yes. I also have that with me as 19 well. 20 Q. Very good. Do you consider your 21 Online Appendix to be part of your report as 22 well? 23 A. Well, it's part of my papers. 24 Q. So is that yes, your Online 25 Appendix is considered to be part of your
14 1 A. Yeah. 2 Q. Is that correct? 3 A. Uh-huh. (Witness answers in the 4 affirmative?) 5 Q. And when you -- when you answer my 6 questions, I would ask that you simply say yes 7 or no. Sometimes uh-huhs and uh-uhs, even 8 though that's normal conversation it's hard for 9 the court reporter to pick up. So that's -- 10 that's just a helpful reminder for all of us 11 during these depositions. 12 But you -- you do have a copy of 13 the June 30th expert report? 14 A. Yes, I do. 15 Q. And attached to that at least the 16 document that I have as three exhibits. The 17 first is your CV, the second is a copy of a -- a 18 journal article. And I believe that has not yet 19 been published but it's in one of the Harvard 20 Law Journals. 21 A. Uh-huh. (Witness answers in the 22 affirmative.) 23 Q. Is that correct? 24 A. Yes. 25 Q. And then the third is an	16 1 report? 2 A. I would think so. 3 Q. Okay. And the final document that 4 I believe you should have is our fifth set of 5 requests for production. Have you seen that 6 document? 7 A. I did. You referred to the 8 document where you asked for additional 9 disclosure, right? 10 Q. That is correct. 11 A. Yes, I have it. 12 Q. Very good. Now, I want to briefly 13 cover off some of your education and your 14 experience. Where did you grow up? 15 A. So I was born and raised in Haifa, 16 which is in Northern Israel. And then as most 17 Israelis I was off to the military and served -- 18 served for nearly five years in the 19 intelligence. And then I went to university in 20 Israel, to the Hebrew University and I studied 21 both law and cognitive science. 22 Q. Okay. And I'll stop you right 23 there. I appreciate the information. Where did 24 you -- did you attend high school in Israel? 25 A. Yes, I did.

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17 1 Q. Where was that? 2 A. The Hebrew Reali School in Haifa. 3 Q. And then after high school, you 4 spent five years in the Israeli military. 5 Is that correct? 6 A. I think four-and-a-half or five. 7 Yeah. It -- it was long ago. 8 Q. Okay. Very good. But the military 9 service came between high school and college. 10 Is that correct? 11 A. Right. Israelis do military sort 12 of instead of college. 13 Q. Very good. Not a bad idea 14 actually. So you received your B.A. and L.L.B. 15 from Hebrew University. 16 Is that correct? 17 A. Yes. 18 Q. And was that a joint degree? 19 A. Yes. 20 Q. Okay. And what did you study while 21 at Hebrew University? 22 A. You mean more specifically? 23 Courses? 24 Q. Yes. Yeah. Well, what did -- what 25 were the -- what were the majors? In the United	19 1 cover? 2 A. Yes, that's a really good question. 3 Cognitive science is in the intersection of 4 several different disciplines. So you study -- 5 if you're doing Cognitive Sciences in your first 6 degree, you study both neuroscience, behavioral 7 economics, psychology, cognitive -- both 8 cognitive psychology and social psychology, 9 linguistics, and philosophy. 10 Q. And after your time of obtaining 11 your L.L.B. and B.A. from Hebrew, you went onto 12 Harvard University. 13 Is that correct? 14 A. Went onto -- I didn't hear you. 15 Q. Harvard University? 16 A. Yes. 17 Q. In Cambridge, Massachusetts? 18 A. Yes. I mean, before that, after I 19 finished with my first degree at Hebrew U, then 20 I also -- so I also interned -- clerked in U.S. 21 terms with the United -- with the Israeli 22 Supreme Court Chief Justice, who was then Dorit 23 Beinisch. So I spent a year at the Supreme 24 Court, clerking. And then I also did the bar 25 exams in Israel. And then I went to the United
18 1 States we have majors, you'll major in history 2 or science or biology. Did you have any majors 3 or emphases or focus of study when you were at 4 Hebrew University -- 5 A. So our -- 6 Q. -- for your B.A.? 7 A. -- so our system is different. So 8 whereas Americans first do at general B.A. 9 where they have concentrations as you mentioned, 10 and then they go on to pursue a J.D. if they go 11 in to law school. In Israel your J.D. 12 equivalent, the L.L.B., you can do it as your 13 first-degree. It's more similar to the European 14 system. So, if you will, your major is law, and 15 then I did, in sense, two degrees in one which 16 is law and cognitive science, which was a bit 17 unusual because it's two really independent 18 degrees. 19 Q. How many years did you spend 20 obtaining your degrees in law and cognitive 21 science? 22 A. Three-and-a-half years. That's the 23 -- that's the program. 24 Q. And what exactly is cognitive 25 science? What -- what does that discipline	20 1 States for my first L.L.M. and then doctoral 2 studies. 3 Q. Okay. So that clerkship with the 4 Chief Justice was between 2012 and 2013? 5 A. No. It was between March 2011 and 6 April or May 2012. I stayed with her a little 7 bit longer because she was retiring and needed a 8 little bit more help. Usually clerkships are 9 only one year, but I extended for -- for two 10 months longer. 11 Q. I see. Did you -- were you a 12 clerking for the Chief Justice while you were 13 wrapping up your -- your first-degree at Hebrew 14 University? And I ask only because on your 15 resume or CV it says that you graduated from 16 Hebrew University with your B.A. and L.L.B. in 17 2012. 18 A. Yes, because it's very un- -- 19 Americans will not understand that, but Hebrew 20 University, like many other Israeli 21 universities, confer the degrees one year after 22 you actually conclude your studies. So 23 actually, I mean, you get the -- it's -- I don't 24 know why. I guess it's just because of some 25 logistical issues, but you finish -- you finish

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21 1 all of your coursework, you finish all of your 2 exams. 3 You go on to clerkship or 4 internship or whatever, and then you actually 5 get your degree like the official ceremony and 6 everything. Almost a year, if not more after 7 you've finished. 8 Q. That makes sense. So when did you 9 move to Cambridge to -- I assume you moved to 10 Cambridge to begin your studies at Harvard. 11 A. I did. 12 Q. Is that accurate? 13 A. Yes -- yes. I moved on August 14 2012, if I'm not mistaken. 15 Q. And on your resume, it notes that 16 your L.L.M. at Harvard was waived on admission 17 to the S.J.D. Program? 18 A. Yes. 19 Q. What does that mean? 20 A. That's another good question. So 21 -- so it's a -- okay. It's going to be long. I 22 hope. Well, I don't know. So essentially, 23 Harvard has a rule that you cannot do two 24 degrees, two consecutive degrees on scholarship 25 in Harvard. So -- and typically if you're going	23 1 proceeded to obtain your S.J.D. from Harvard and 2 that degree was conferred in 2016. 3 Is that correct? 4 A. Right. I believe it was May. 5 Q. Did you have any specific focus of 6 study for your S.J.D. program? 7 A. Yes. So the way it's defined is 8 that you first need to choose some fields which 9 you're going to get examined on in your oral 10 exams. And so I -- and -- and I had three 11 professors on my committee, Professor Cass 12 Sunstein with whom I worked on behavioral 13 analysis of constitutional and public law. And 14 Professor Martha Minow with whom I worked in law 15 and religion and notions of multiculturalism, 16 pluralism, and diversity more generally. And 17 then Professor Tom Tyler from Yale Law School, 18 with whom I worked on empirical methods in legal 19 research and particularly psychology -- 20 psychology in law and -- and -- and experimental 21 methods. 22 Q. Did part of your studies either at 23 Harvard or Hebrew University involve the study 24 of statistical methods? 25 A. Yeah -- yeah, I studied quite a lot
22 1 to do a Ph.D. in Harvard, it's not a problem 2 because Ph.D. programs have this built in M.A. 3 in them, which you consider a part of the 4 Ph.D. where you did two years of coursework, you 5 do some oral exams, and then you continue to do 6 your research. 7 Within the Law School, they 8 structured it differently because most students 9 in the L.L.M. cohort do not proceed -- do not 10 continue to pursue graduate work in the 11 classical doctoral sense. So when you get 12 scholarship to attend the L.L.M. program as I 13 did. I got a combination of scholarships what 14 ultimately was close to a full scholarship. 15 Then you -- and you are -- and you are accepted 16 and want to continue to do a proper doctorate, 17 then you need to waive your first degree in 18 order to be able to continue to get funding 19 during your doctoral degree. 20 Q. That -- that makes sense -- that 21 makes sense. Thank you. 22 A. I don't think it makes sense. But 23 thank you for being honest. 24 Q. As lawyers can count dollars and 25 pennies. That makes sense. And then you	24 1 of statistics. So in Hebrew University, I did, 2 either two or three courses in statistics. And 3 then in Harvard I took all of the courses of the 4 graduate school for that for psychology. So -- 5 so -- and they had several statistics courses 6 for our graduate students. So I took all of 7 them. 8 Although I should say that I was 9 not allowed to take them for credit. So I was 10 auditing them and doing all of the, you know, 11 coursework and final -- final task work. But 12 because of this strange combination with the 13 L.L.M. and the S.J.D., I was not allowed to take 14 them for credit. 15 Q. Did -- is part -- as part of your 16 studies in statistical methodology, did you 17 perform any field work or practicums? 18 A. Or what? 19 Q. Or practicums, I suppose it's an 20 American word for like internships or practical 21 application in the statistical studies by going 22 out and doing an actual field study or a pseudo 23 study. 24 A. Yes -- yes. Okay. So I didn't 25 want to --

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25 1 Q. Tell me -- tell me a little bit 2 about that -- that practical work. 3 A. Sure. So first I did quite a lot 4 of research both for my doctoral studies and 5 also for additional academic work outside of my 6 formal doctorate. So I'll start with this and 7 then I'll move on to talk about, I think what 8 you called prac- -- practicals. So it was a 9 summer internship, but I'll say a little bit 10 more about that in a second. 11 So first my -- my entire doctoral 12 work was empirical and experimental. I started 13 out so I was -- I train myself not only in 14 experimental methods and statistics, but also in 15 qualitative methods. And my first work was 16 qualitative, where I was interviewing on the 17 field, both doing field trips and also Zoom. It 18 wasn't Zoom then, I think it was Skype. But I 19 was interviewing religious managers both in 20 Israel, and in different parts of the United 21 States on their experiences with conflicts -- 22 conflicts with the law. So that was the first 23 part where I assume you might want to refer to 24 as actual hands-on experience with empirical 25 research.	27 1 THE WITNESS: I see that there are 2 only 9:55 minutes left, is that right? That 3 popped on my screen. Should I ignore it? 4 THE COURT REPORTER: I'm not sure 5 what's going on. The Zoom is acting really 6 strangely today, which I took a deposition 7 earlier, so I'm not sure why. So if something 8 happens, finish your answer and then maybe we 9 can take a break in a minute to figure it out. 10 THE WITNESS: Okay. 11 THE COURT REPORTER: Sorry about 12 that. Go ahead -- go ahead. 13 BY MR. BANGERT: 14 Q. And Professor Corren, you were -- 15 you were just answering my questions about your 16 -- your experience with respect to conducting 17 field studies. And perhaps I can help summarize 18 it by just asking this question: Do you 19 consider yourself to be an expert in the design 20 and administration of field studies in the -- in 21 the discipline of statistics? 22 A. Yes. 23 Q. Do you consider yourself -- 24 A. And I think I didn't finish because 25 you were asking me also about internships.
26 1 And then I went on to do quite a 2 bit of experimental work both in Israel and in 3 the United States, designing and running lab 4 experiments and online survey experiments, which 5 are quite common, really have become the norm in 6 both psychological research and perhaps even 7 economic research and political science 8 research. So whereas in the past we would -- I 9 mean not we, but researchers would simply 10 experiment with students. Now they aim to get 11 better and larger and more representative 12 samples and they can find them online. So I did 13 that also for my -- for my S.J.D. work. Two big 14 -- two big experiments. 15 And then also I was working with a 16 group of researchers and particularly Professor 17 Max Bazerman at Harvard Business School. And 18 that group was called the Behavioral Insights 19 Group, or BIG. And as part of this group, I was 20 both immersed in other people's research and 21 also conducting my own research with Max 22 Bazerman, Fiery Cushman, and Josh Greene was 23 part of the picture for some time. And we were 24 doing both lab research and online survey 25 experiment research.	28 1 Q. No, I don't believe I asked about 2 -- well, if I said practicum that, I wasn't 3 really thinking the internships, so we can set 4 that to the side for now. Do you consider 5 yourself to be an expert in the practice of law? 6 A. Yes. 7 Q. Have you ever practiced law at a 8 law firm? 9 A. No. 10 Q. Have you ever worked for a 11 corporation as in-house counsel? 12 A. No. 13 Q. You spent one year clerking for the 14 Israeli Supreme Court. 15 Is that correct? 16 A. That's correct. 17 Q. Have you had any other clerkships 18 for any other judges apart from the one that you 19 just mentioned? 20 A. No. 21 Q. Have you ever -- apart from your 22 current teaching position at Hebrew University, 23 have you ever held full-time employment in the 24 field of law? 25 A. Well, I was working during my

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29 1 studies. I'm not sure if that refers to one of 2 the questions you asked me before and if it 3 does, then consider that as a correction. But I 4 did work during my studies for the Attorney 5 General offices in Israel. 6 Q. I apologize. You said Attorney 7 General's offices? 8 A. Yes. Yeah. 9 Q. And what did you do for the 10 Attorney General in Israel? 11 A. So I was writing legal memos, I was 12 working on legal cases, these kind of things. 13 Q. How long did you work with the 14 Attorney General? 15 A. Probably a year. 16 Q. Was that full-time employment or 17 were you also engaged in study at that time? 18 A. Well, I was also engaged in 19 studying at the time. 20 Q. How many hours per week roughly did 21 you work for the Attorney General during that 22 time? 23 A. Probably two days or three days a 24 week and more during summers and breaks. 25 Q. Did you have a formal title?	31 1 fellow with the Behavioral Insights Team in 2 London. And they define themselves as a 3 non-for-profit. But maybe today they would 4 define themselves to also as a consulting firm. 5 They consult the British government and the 6 World Bank and the International Monetary Fund 7 and developing countries. So you may count 8 that, yeah. 9 Q. Okay. 10 A. And while I was there, I was -- I 11 was working on field experiments and so on. 12 Q. Is there any other employment 13 experience in your background that we haven't 14 discussed yet that you would consider pertinent 15 to the opinions that you're rendering in this 16 case? 17 A. Apart from University? I don't 18 think so. 19 Q. So let's cover off your university 20 employment. You've been a professor at Hebrew 21 University since 2017. 22 Is that correct? 23 A. Yeah. I mean, December 2016. But 24 formally, yeah '17. 25 Q. And you received tenure in 2020.
30 1 A. I don't think so. I mean, a 2 student -- a student legal intern or something 3 is probably how I would translate it. 4 Q. Do you consider yourself an expert 5 in the field of behavioral psychology? 6 A. Yes. 7 Q. Are there any other fields that you 8 consider yourself expert in that I have not yet 9 mentioned? 10 A. Law and religion, constitutional 11 law, law and social norms, if that's a field, 12 it's not -- it's not one of the largest. And I 13 would consider myself expert more generally in 14 research methods and empirical legal research. 15 So you specifically addressed experimental 16 studies or field studies. But -- but I work 17 with additional methodologies, not such an 18 irrelevant to what we're talking about today, 19 but... 20 Q. Have you ever done work with a 21 consulting firm as a non-retained consulting 22 expert witness? 23 A. I don't think so. I mean, what I 24 wanted to tell you about before I thought you 25 asked me, was when I was working as a summer	32 1 Is that correct? 2 A. Uh-huh. (Witness answers in the 3 affirmative.) 4 Q. Congratulations by the way. 5 A. Thank you. 6 Q. I know that you hold yourself out 7 in a variety of different capacities. But how 8 would you personally describe your discipline of 9 expertise as a professor at Hebrew University 10 today? 11 A. So I would define myself as a legal 12 scholar and cognitive scientist. 13 Q. And in addition to, I assume you 14 teach classes to students at Hebrew University? 15 A. I do. 16 Q. And you perform the usual academic 17 research that you would associate with a tenured 18 professor? 19 A. Yes. 20 Q. Beyond teaching and academic 21 research, what other activities do you engage in 22 as a tenured professor at Hebrew University? 23 A. So lots, but I also am the Academic 24 Director since -- since this year of the Center 25 for the Study of Multiculturalism and Diversity

33 1 in Hebrew U. As of last week, I'm a member of 2 the Center for the Study of Rationality at the 3 Hebrew University. 4 Q. May I put a pin in your testimony 5 right there just for a moment. I want to give 6 you a chance to finish, but those two centers, 7 very high level, what kind of work are you doing 8 with respect to those two centers and your role 9 therewith? 10 A. So very different roles. So first, 11 the Center for the Study of Multiculturalism and 12 Diversity, I'm the academic director, there's 13 also an executive director. So my goal and role 14 is mainly to direct the academic activity of the 15 Center. That includes giving students and 16 faculty research grants from our limited, but 17 existing, research budget. Throwing, organizing 18 conferences, webinars. Next year we're going to 19 have a very big conference. And also 20 fundraising for the Center. So that's what I do 21 in this capacity. 22 With the Center for the Study of 23 Rationality, it's very different. It's a 24 different center. So it's an inter- -- okay, 25 it's an -- yeah, I just -- I just okayed this	35 1 Q. Do you ever consult with lawmakers 2 about proposed legislation? 3 A. No, but I wasn't then -- I was 4 invited to the Israel Parliament several times 5 to present my research. 6 Q. Are you affiliated with any think 7 tanks or policy organizations separate and apart 8 from the work at Hebrew University? 9 A. No. But I did receive research 10 grants from Israeli Institute of Democracy, to 11 do some Israeli-relevant empirical work. And I 12 am online a Nootbaar Religious Freedom Fellow at 13 Pepperdine Law School, which is, again more 14 similar to the Center for the Study of 15 Rationality, an academic framework, where mostly 16 American scholars, I'm the only Israeli, convene 17 every month or two to discuss academic papers 18 and we're supposed to have an in-person 19 conference this year if the world will allow us 20 to do that. But that's another affiliation. I 21 had it last year and then it continues next 22 year. 23 Q. Have you done any paid or unpaid 24 consulting work for any U.S. policy 25 organizations, think tanks, political parties?
34 1 message. 2 It's an interdisciplinary center 3 that brings together established researchers 4 from Hebrew U, primarily mathematicians and 5 economists and statisticians, but also a limited 6 pool of people that the first group selects from 7 other disciplines. I would say it's -- it's 8 quite prestigious to be selected to the Center. 9 And so the Center brings together these group of 10 -- of scientists from all across the university 11 to think together, to hold seminars and 12 conferences. I don't have any administrative or 13 academic directorship role with respect to it. 14 I'm one of the newest and youngest members of 15 the Center. 16 Q. Is your role with -- either of 17 those centers involve the design or 18 administration of field studies? 19 A. No. This is a -- this is what I do 20 in my capacity as a PI in my own research. And 21 apart from that, you asked what else that I do. 22 So I'm invited to give talks. I'm invited to -- 23 to speak in events, I sometimes write opinion 24 columns for -- for the media or for law blogs, 25 these kinds of things.	36 1 A. No, that's my first paid work 2 outside of academia in the US. 3 Q. When you say that, are you 4 referring to this engagement in this case? 5 A. This engagement, yes. This case, 6 yes -- yes. Of course, when I was in Harvard, I 7 was working in the university. I was TAing, I 8 was research assistants for professors, but 9 yeah, that's my first -- 10 Q. Thank you. 11 A. -- non-academic engagement. 12 Q. Who contacted you about serving as 13 an expert witness in this case? 14 A. Counsel Casey Hinkle. 15 Q. When were you contacted by 16 Ms. Hinkle? 17 A. So I think it was February 2020. 18 So this '20 or '21, this year. So yeah. 19 Q. '21. 20 A. Yeah, about half -- half a year 21 ago, right or less? 22 Q. It's all compressed together thanks 23 to COVID. Yes, 2021. 24 A. I know, it feels like we lost a 25 year of our lives. So yes, so Casey contacted

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37 1 me after I published an essay in the Atlantic 2 about my academic work. She read it and she 3 wanted to reach out. 4 Q. What were you asked to do 5 specifically with respect to this case? 6 A. So Casey reached out because she 7 thought I might be able to give expert opinion 8 in this case. I didn't really know what it 9 meant originally. And so we had a discussion 10 about that to understand what it means. And 11 then I said I'll need to review the materials. 12 I also sent Casey to read the full papers to see 13 whether she still thinks it's a good idea after 14 she reads everything and not just the opinion 15 piece. Maybe Casey remembers other things as 16 well. 17 MS. HINKLE: Professor, yeah. I 18 just want to caution you not to disclose the 19 subject matter of our discussions. I think the 20 question was more, you know what did you 21 understand your task to be in -- in delivering 22 the opinion. So just to caution you that -- 23 that our conversations are not discoverable. 24 THE WITNESS: Sure. So what -- 25	39 1 about it because I previously did some -- in 2 another research of mine it popped up. But then 3 I also needed to read Kentucky's RFRA and to 4 compare it with the kinds of RFRA's I had in my 5 study. 6 And then that was -- you may want 7 to ask me more about that later, but that was a 8 major thing for me to do. And then I wanted to 9 compare also Louisville and Kentucky in general 10 to the states that I sampled in my own study. 11 So that's the second part of my report 12 essentially. It wasn't the simplest to do 13 because not all information was available to the 14 same degree. But that's why I invested some 15 time in it. 16 Q. How much time did you spend, 17 roughly speaking, undertaking those activities? 18 A. So I think in total, I probably 19 worked on this case 30 to 45 hours. 20 Q. And that includes the drafting of 21 your expert report as well as the background 22 research that you just described? 23 A. Yes. Yes. 24 Q. And does not include the time 25 you've spent today in deposition?
38 1 BY MR. BANGERT: 2 Q. That's -- that's correct, Professor 3 and I'm not asking you to recount the contents 4 of your conversations with Ms. Hinkle. What I'm 5 trying to do is understand your understanding of 6 the task you were assigned to perform. So let 7 me just go with that question. What is your 8 understanding, after you had these conversations 9 with Ms. Hinkle, what understanding she -- did 10 you walk away with as to the task you were to 11 perform? 12 A. That I need to review the situation 13 in Louisville with respect to this case and see 14 whether I think that my research applies or do 15 not apply to that situation. And then to decide 16 what opinion to write. 17 Q. And with that understanding what 18 did you go about doing to accomplish that task? 19 A. So first, I did all kinds of 20 research on Louisville, Kentucky. So I was 21 first interested in the legal background of 22 Louisville in that sense I mean, the kinds of 23 laws that apply to -- to Louisville. So I 24 already knew that's the Fair Practice Ordinance 25 was involved. Actually, I knew a little bit	40 1 A. Does not include the time that 2 we're talking now. 3 Q. Does that 30 to 35 hours also 4 include time you spent preparing for this 5 deposition? 6 A. Yes. 7 Q. How much time did you spend 8 preparing for this deposition? 9 A. Probably 15 hours or so. 10 Q. You said 15? 11 A. I think so. 12 Q. So 15 hours preparing for the 13 deposition and between 15 to 20 hours 14 researching and drafting your report. 15 Is that fair? 16 A. Yeah. 17 Q. What did you do to prepare for the 18 deposition today? And again, I'm not asking for 19 conversations, specific conversations that you 20 had with Counsel, but just in generally 21 speaking, what activities did you undertake to 22 prepare for your deposition today? 23 A. So that was primarily reading 24 Dr. Yancey's rebuttal report. And these papers, 25 those that I was able to find. And, you know, I

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41 1 mean, summarizing the -- writing my own -- my 2 own thoughts and responses to these comments, et 3 cetera. 4 Q. Roughly, how much money have you 5 charged to date for your services in this case? 6 A. I'm getting paid \$300 an hour. 7 Q. Have you sent an invoice for your 8 time yet? 9 A. I did. 10 MR. BANGERT: And Casey I'll just 11 asked that that be produced. I know we have not 12 specifically requested it, but I would just like 13 to ask now if we can have a copy of that after 14 the deposition. 15 MS. HINKLE: Okay. 16 BY MR. BANGERT: 17 Q. Okay. So Professor, from our 18 conversation that we just had my understanding 19 of the work that you did -- I'm going to 20 summarize what I understand the work that you've 21 done to be. In response to the task you were 22 given by Ms. Hinkle, you conducted research on 23 the legal background of the City of Louisville 24 and the State of Kentucky, including things such 25 as the anti-discrimination laws present in	43 1 experiment. 2 Is that fair? 3 A. Yes. 4 Q. And those four states were Indiana, 5 Texas. I'm blanking out now -- Iowa and North 6 Carolina? 7 A. That's correct, Counsel. 8 Q. I'm sitting in Austin, Texas right 9 now so I'm guessing you probably surveyed some 10 of my friends around here who are in business, 11 et cetera. 12 A. I don't know -- I don't know when 13 did you marry, but in case you organized the 14 wedding a long time ago then I'm not sure it's 15 the same people. 16 Q. It was a long time ago. I've been 17 married for over 20 years. So very good. 18 Now, you mentioned earlier that you 19 had received at least a copy of our request for 20 production, the documents that we requested. 21 A. Yes. 22 Q. Do you recall that? 23 A. Uh-huh. (Witness answers in the 24 affirmative.) 25 Q. Did you provide any documents to
42 1 Louisville, the RFRA law in Kentucky. And you 2 also looked at the -- the laws that are 3 presently in dispute in this lawsuit. 4 Correct? 5 A. These are -- so to clarify, I was 6 looking at the laws that are at present in 7 dispute in this conflict. I wasn't researching 8 all of the legal background of Kentucky or 9 Louisville. 10 Q. Fair point. Thank you. So you -- 11 you were looking at the laws in dispute in this 12 lawsuit? 13 A. Yes. 14 Q. The second thing that you looked at 15 were the papers and pleadings in this case, the 16 complaint, the court rulings, and some of the 17 briefing. 18 Is that fair? 19 A. Yes, I did. 20 Q. And the third thing that you did 21 was you looked at some information or data to 22 attempt to compare the City of Louisville and 23 the State of Kentucky generally to the four 24 states that you studied, at least the four 25 states that you studied in your Masterpiece	44 1 Counsel in response to that request? 2 A. I did. So you asked for the 3 transcript of the phone survey? Yes, that's 4 that. 5 Q. How about information about the 6 specific respondents to your audit study? 7 A. So I did not provide this 8 information and I'm not going to provide it 9 because providing it would violate my IRB 10 approval to conduct the study. 11 Q. And what specifically about 12 providing those materials would violate your IRB 13 approval? 14 A. So when -- say that again, I didn't 15 hear the last part of the question. 16 Q. Yeah. What exactly about providing 17 those materials to us -- those materials as 18 being information about the specific response to 19 the audit study. What about providing that 20 information would violate your approval? 21 A. So it's a universal rule that when 22 researchers conduct research with human 23 subjects, then the identity of the subjects 24 remain confidential and anonymous. Even if it 25 becomes known to the researchers, it must never

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45 1 become known to outsiders. There are very few 2 exceptions for that rule and they would require 3 the explicit consent of participants to become 4 known. If -- if one -- if a researcher wants to 5 somehow disclose that identity and it's not 6 always possible. 7 But in any event, in my IRB 8 approval and that's really standard practice, I 9 committed -- and that's one of the conditions 10 for the -- for under which the committee 11 approves the request that no one except myself 12 and my research assistant would be exposed to 13 the identity of the businesses that I contacted 14 and that we would deanonymize the data and would 15 not release identified data when the study is 16 published. Which is what I did. I mean, I 17 deanonymized the data. 18 Q. When you say deanonymized, you mean 19 you removed any indication as to the specific 20 identities of the respondents when you published 21 your data in your -- in your journal articles? 22 A. Yes. Yeah. 23 MR. BANGERT: I want to turn to 24 your report, if that's okay. And I believe you 25 have a copy of it there. Casey, just for sake	47 1 dated June 30, 2021, inclusive of Exhibits 1, 2 which is her CV, Exhibit 2, which is a copy of 3 an article entitled, A License to Discriminate? 4 The Market Response to the Masterpiece Cakeshop, 5 for forthcoming publication in the Harvard Civil 6 Rights and Civil Liberties Law Review. And 7 Exhibit 3 which is the draft of an article that 8 I understand has now been published entitled -- 9 give me just a moment -- Religious Exemptions 10 Increase Discrimination Towards Same-Sex 11 Couples: Evidence from Masterpiece Cakeshop, to 12 be published in The Journal of Legal Studies. 13 So and that -- that comprises the total of 129 14 pages in this PDF. So with that understanding, 15 I will mark that entire document as Exhibit 1. 16 (Whereupon, the referred to 17 document was marked as Exhibit 1, 18 and is attached hereto and made a 19 part hereof.) 20 BY MR. BANGERT: 21 Q. So Professor, if you have your 22 report at your fingertips, I'd like to you to at 23 least have it available. It was dated June 30, 24 2021. 25 Is that correct?
46 1 of efficiency, I -- I will refer to the report 2 by looking at the copy I have on my screen. If 3 you would like I will share my screen with you 4 so you can see it, but I would prefer not to if 5 I don't have to, only because that way I can 6 continue the conversation with the witness. So 7 assuming you all have copies of the report -- 8 it's the one that you produced to me and that we 9 sent earlier by e-mail to you, assuming that 10 we're all in agreement that that is that the 11 report I think we can proceed on that basis. 12 Is that -- is that fair? 13 MS. HINKLE: Yes, that's fine. I 14 have it with me. 15 MR. BANGERT: Okay. Good. Good. 16 That's the one that we e-mailed over to you 17 either this morning or last night. And I 18 believe, Jessie, you also got a copy of that as 19 well? 20 THE COURT REPORTER: Yes, I have 21 it. 22 BY MR. BANGERT: Good. Okay. So 23 without objection, I'd like to mark that 24 document -- and when I say that document, that 25 is the Expert Report of Netta Barak-Corren,	48 1 A. Uh-huh. (Witness answers in the 2 affirmative.) 3 Q. It was signed by you? 4 A. Yes. 5 Q. It is nine pages and 23 paragraphs 6 long. 7 Correct? 8 A. I'm going to check that. Yes. 9 Q. And that document is an accurate 10 and complete copy of the document entitled 11 Expert Report of Netta Barak-Corren. 12 Correct? 13 A. That's what I submitted. 14 Q. Yes. Have you -- have you updated, 15 modified, changed, or amended that report since 16 you signed it on June 30, 2021? 17 A. No. 18 Q. Attached to your report as Exhibit 19 1 is your CV. Has -- have you updated, amended, 20 or changed your CV since you submitted a copy of 21 it with your report on June 30th? 22 A. Probably, yes. Would you like an 23 updated copy? Am I allowed to ask a question? 24 Okay. 25 Q. In this case? Yes. And I would

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49 1 certainly like a copy -- an updated copy of your 2 CV. 3 A. Okay. I'll make sure to pass it 4 through Casey. 5 Q. Thank you very much. How would you 6 describe the difference between Exhibits 2 and 7 -- Exhibit 2 and Exhibit 3, these two journal 8 articles that were attached to your report? 9 A. Yeah. That is a wonderful 10 question. So they started out as one paper but 11 it was too big, in a sense, to be published as 12 one. And my first preference was to publish 13 this in the best peer review journal in law, 14 which is the Journal of Law and Legal Studies. 15 And they really asked me to take out all of the 16 more law review kind of content. So I created 17 those two papers. And then the primary piece, 18 the more empirical sort of hard data piece, went 19 really to the Journal of Legal Studies. And 20 then I would -- I published the more -- the 21 longer, more normative and also I think more 22 reader-friendly piece in the Harvard Civil 23 Rights-Civil Liberties Review. 24 Q. Now, I have Exhibit 3 is a draft of 25 the article that was to go to the Journal of	51 1 both papers in, like, a near-finished format 2 after they were -- after they were done with all 3 of the substantive review and revision that 4 academic papers usually go through. 5 Q. Okay. Same question with respect 6 to the Harvard Journal article, has it changed 7 in substance since the time that you provided it 8 to us in June 30th as an exhibit to your report? 9 A. Not in substance. 10 Q. Has it been published? 11 A. That's a really good question. 12 They -- it should have been published. But it's 13 -- I mean, they're in delay in production, but 14 there's nothing -- there is no further action 15 that I should take on my end. I mean it's -- 16 the next thing I'm supposed to get it on the 17 mail. 18 Q. And to be clear, there's nothing 19 about the Harvard Journal article, Exhibit 20 Number 2, that has changed from the time you 21 submitted it with your June 30th report that 22 would impact, in any way, your opinions in this 23 case? 24 A. No. 25 Q. Looking at Exhibit Number 1, and
50 1 Legal Studies. You mentioned earlier today that 2 it has now been published. Has the document -- 3 has that document changed in substance from the 4 time it was in draft form and attached to the 5 report until the time it was published? 6 A. Not in substance, but there were 7 some minor changes. For today, because I 8 thought that that would make more sense, I -- I 9 still brought the draft with me because I 10 thought you're going to refer to it. So but, 11 you know, I mean, the Journal had -- it has been 12 gone through -- through language editing. So, 13 you know, there are some -- and the tables look 14 somewhat different because they wanted to match 15 it with their styles. And there were also a 16 little bit of rounding changes that may slightly 17 change, like, whether it's like a 9.2, or 9.4 in 18 some -- in some of the statistics but nothing 19 major, no. 20 Q. So apart from those minor changes, 21 did any of those changes from the draft form to 22 the published form, do any of those impact, 23 change, or cause you to modify the opinions 24 you're giving in this case? 25 A. No. No. No. You already received	52 1 I'm going to go to the expert report, and in 2 particular, paragraph 4. And you mentioned this 3 earlier, but in paragraph 4, you say, Chelsey 4 Nelson asks the Court -- I'm reading from 5 paragraph 4. I'm sorry, I was looking at the 6 wrong paragraph. 7 You say in paragraph 4, I have been 8 asked to provide an expert opinion regarding the 9 anticipated effects of granting Chelsey Nelson 10 the relief sought by her Complaint on the 11 willingness of other wedding vendors to provide 12 services to same-sex couples. 13 Do you see that? 14 A. Yes. 15 Q. And that accurately summarizes your 16 understanding at the tasks you were given to 17 perform as an expert in this case? 18 A. Yes. 19 Q. If you'll turn with me to paragraph 20 12 of that same report, there is a heading 21 containing the word, Opinion. 22 Do you see that? 23 A. Yes. 24 Q. Does paragraph 12 contain the 25 opinion that you are rendering as an expert

53 1 witness in this case? 2 A. Is that a question? 3 Q. It is. Would you like me to 4 restate it? 5 A. Yes. 6 Q. Does paragraph 12 contain the 7 opinion that you are rendering as an expert 8 witness in this case? 9 A. Yes, but I would also consider all 10 of the rest as part of the opinion. 11 Q. So paragraph 12 states based on my 12 work to date, I have formed the opinion that 13 granting Chelsey Nelson a religious exemption 14 from the application of Louisville's Fairness 15 Ordinance in this case could significantly 16 increase a likelihood that same-sex couples 17 attempting to hire wedding vendors in 18 Louisville, Kentucky will experience 19 discrimination resulting in the denial of equal 20 access to goods and services, period. 21 Is it fair to say that is your 22 top-line opinion in this case? 23 A. Yes. 24 Q. Now, you mentioned just a moment 25 ago that you would also point to the paragraphs	55 1 Q. I agree. So you are not testifying 2 today that you are certain that granting Chelsey 3 Nelson an exemption in this case will increase 4 the likelihood that same-sex couples seeking 5 wedding vendors in the Louisville area will 6 experience discrimination. 7 Correct? 8 A. No. There's nothing certain in 9 life or in science. 10 Q. Are you attaching some sort of 11 probability to the likelihood that such -- such 12 individuals in the Louisville area will 13 experience discrimination 50 percent, 60 14 percent? 15 A. No, I cannot attach a specific 16 probability. And what I wrote in paragraph 12 17 that you cited is that granting Chelsey Nelson a 18 religious exemption from the application of 19 Louisville's Fairness Ordinance in this case 20 could significantly increase the likelihood that 21 same-sex couples attempting to hire wedding 22 vendors will experience discrimination. 23 Q. And you agree with me that it is 24 possible that granting Chelsey Nelson a 25 religious exemption may not significantly
54 1 following that opinion as your opinion in this 2 case. 3 Is that accurate? 4 A. Yeah. I mean, for me it's 5 stylistic kind of distinction between opinion 6 and basis for my opinion, which is sort of 7 accepted with the format. 8 Q. Certainly. But in terms of if 9 someone were to ask you as I am now, what is 10 your opinion in this case, paragraph 12 11 accurately, fairly, and fully summarizes the 12 opinion that you're rendering, whereas the 13 following paragraphs provide the supporting 14 basis for that opinion. 15 Is that a fair statement? 16 A. I think so. I think you would say 17 that that's the bottom line. 18 Q. Now, you note in paragraph 23 of 19 your report that studies like the Masterpiece 20 experiment can never predict future behavior 21 with absolute certainty. 22 Is that fair? 23 A. Yes. That's an important line. 24 Q. I'm sorry? 25 A. I said that's an important line.	56 1 increase the likelihood, that same-sex couples 2 attempting to secure wedding services will have 3 experience discrimination in the Louisville 4 area. 5 Correct? 6 A. I would say that everything is 7 possible and we have empirical research evidence 8 in order to help us understand what's more 9 possible than not. 10 Q. But you can't attach a percentage 11 probability to how much more possible it is that 12 couples seeking same-sex wedding services will 13 experience discrimination versus will not? 14 A. No, I cannot. 15 Q. Now, in your opinion in paragraph 16 12, with all the qualifications that you've 17 given, your opinion in paragraph 12, depends 18 entirely on the Masterpiece experiment that you 19 conducted, and that you document in Exhibits 2 20 and 3 to your report. 21 Correct? 22 A. Not only on that, it's also based 23 on my entire research, and the research of 24 others whom I cite my papers. So I would say 25 that the workload between the opinion and the

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57 1 papers, is that the papers establish the full 2 standing of the literature, including my own 3 work, of course, primarily maybe my own work 4 with respect to this question. And then the 5 report takes the -- takes the question, takes 6 the -- takes the question really of whether this 7 is relevant to Louisville, Kentucky. So -- 8 Q. Go ahead. I'm sorry for 9 interrupting you. Please, go ahead. 10 A. So I would not separate between the 11 report and the papers that refer to additional 12 studies that discuss them. 13 Q. Absent the Masterpiece experiment 14 that you conducted, you would have no empirical 15 basis to say that Chelsey Nelson's exemption 16 that she is seeking, if granted, would 17 substantially likely increase the risk of 18 discrimination to same-sex couples seeking 19 wedding services in Louisville. 20 Correct? 21 A. Well, I do have additional studies 22 that would support this conclusion. I think 23 though, that the Masterpiece experiment is the 24 most straightforward evidence, and that's why 25 it's the focus of the report. But if you want,	59 1 broadly is my goal in my research is to bring 2 more empirical clarity to normative debates that 3 often have some either hidden, or tautological 4 assumptions about consequences, or preferences, 5 or diversity that are often just not based on 6 actual evidence. 7 As -- and as an empiricist this is 8 what I did. And so specifically in this case, 9 as I discuss in my papers, the opponents of 10 religious exemptions were arguing that 11 exemptions are going to increase discrimination. 12 That if lawmakers or courts provide exemptions, 13 then that would increase the demand for 14 exemptions. In the -- so if we can use the 15 terms of supply and demand, then if the state 16 supplies exemptions, then more people would want 17 them or would use them. Even people who maybe 18 would not have used them otherwise. So that's 19 the position, that was the argument of the 20 proponents, but they didn't really have data 21 that they could base that on. 22 And then there was the argument of 23 the -- excuse me -- I was talking before on the 24 opponents of exemptions. And then there was the 25 argument of the proponents of exemptions. So of
58 1 we can talk about those additional studies as 2 well. 3 Q. What was the impetus for conducting 4 the Masterpiece experiment? 5 A. What was the -- 6 Q. What was your motivating -- what 7 was the motivation for you to conduct the 8 Masterpiece experiment? 9 A. So I think I would just more 10 generally point you to the introduction of both 11 the JLS paper, and the Harvard paper, because 12 they really summarize the motivation. But as a 13 researcher who studies religion and equality 14 conflicts for several years now, one of the 15 things that perplexed me is how both proponents 16 and opponents of religious exemptions on both 17 sides of the debate waive empirical arguments, 18 arguments about the potential consequences of 19 exemptions, without having actual empirical 20 basis for these arguments. 21 So it's not my first study on -- on 22 this question, although I was researching a 23 different context before. But what I -- what I 24 think is my main goal in this project, or was my 25 main goal in this project, and still more	60 1 course the proponents of exemptions did not base 2 their entire, or even the core of their 3 arguments for religious exemptions on the 4 argument that exemptions will have no 5 consequences, but that did play a role in their 6 arguments, maybe because they're responding to 7 proponents of exempt -- to opponents of 8 exemptions. Or maybe because it is truly 9 important for at least some of the -- for some 10 of the scholars who support religious 11 exemptions, the assumption that these exemptions 12 are sought by a negligible minority of -- of 13 individuals with deeply held convictions in a 14 world becoming more and more supportive of 15 same-sex weddings, and homosexuality, and -- and 16 -- and queer people in general. And therefore, 17 their consequentialist argument, again, not 18 supported by data, was that there is no way that 19 -- that religious exemptions are going to 20 increase discrimination. Because if you look at 21 society, society has become so much more open 22 and inclusive. 23 And I was, you know, reading the -- 24 reading these arguments as an empiricist, and 25 talking about it with my colleagues. And it

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61 1 just struck me as a question that needed to be 2 researched. And I studied this question in the 3 past. So in a Hastings Law Journal piece that I 4 published during my grad school it was based on 5 two very large survey experiments where I 6 recruited religious people, half of whom 7 identify as Democrats and half of whom identify 8 as Republicans. And it was in a different 9 setting, it was a setting that I wrote a little 10 bit about as well, of the dismissal of teachers 11 for pregnancy out of wedlock, from religious 12 schools. So it's a phenomenon that -- that is 13 quite prevalent both in the United States and in 14 Israel, my home country. And after doing lots 15 of interviews with religious managers over this 16 question, which I found fascinating. Then my 17 first set of experiments was -- were in that 18 question. 19 And as you probably know, there is 20 a conflict there with anti-discrimination laws 21 that really depends -- I mean, that depends also 22 on the court that will -- that will adjudicate 23 the conflict. But -- but at least depends in 24 part on the role of the teacher, et cetera, 25 whether -- whether she's a minister or not. So	63 1 And then afterwards, I asked them would they, if 2 they were in the shoes of the principal, what 3 they would do and then afterwards, ask them 4 several questions. One of them, you know, 5 another case like that comes to you a year 6 after, what would you do now? And there were 7 many findings for this study, but the one that's 8 relevant to what we're going to talk -- what 9 we're talking about today was the one that 10 refers to this last question. 11 So what a found was that after 12 being exposed to a court decision that provides 13 this kind of accommodation to keep the school's 14 autonomous decision. Then -- then decision -- 15 religious decision-makers dismissed the second 16 teacher in turn, more than religious 17 decision-makers that -- that read about the 18 court enforcing anti-discrimination law and -- 19 Q. May ask you a question about that 20 study, Professor? It sounds like that's not a 21 field study necessarily? 22 A. That's right. That's an online 23 survey experiment. 24 Q. Very good. Let me just interrupt 25 you there because I think it's -- we've been
62 1 there are quite a lot of conflicts like that. 2 And I presented my participants with a scenario 3 in which they're asked -- they're given a 4 scenario and their ask what would they do. 5 Would they -- would they keep the teacher, or 6 will they dismiss her? 7 And then after they make their 8 decision, they get what happens next, which is 9 the court decision, and then I vary several 10 types of court outcomes. One is enforcement of 11 anti-discrimination law, so reinstating the 12 teacher. And the other is the -- the more 13 common one which is to not reinstate the teacher 14 but provide her compensations. 15 And the reason why I was interested 16 in this contrast is because the religious 17 managers whom I interviewed both in the United 18 States and in Israel, were very fond of the 19 compensation route. And they viewed -- they 20 viewed compensation as an easy get away from the 21 conflict that would allow them to, essentially 22 buy freedom -- buy their freedom as -- as 23 autonomous institutions. 24 And so participants were randomly 25 assigned to read these different court outcomes.	64 1 going for about an hour, five minutes. And so I 2 always want to offer my witness a break after an 3 hour. If you're good to continue, then please 4 do. 5 A. For now I'm good. Thank you for 6 offering. 7 Q. Very good. If it's okay with you, 8 I'd like to turn to something specific in your 9 report on page -- 10 A. I want -- I want to finish, answer 11 the question, if I can. 12 Q. Okay. Yeah, please then -- please 13 finish answering the question. 14 A. Okay. So what I found in that 15 study was that providing an accommodation 16 increases the rate of participants who would 17 then dismiss the second teacher in turn. And 18 then I went on to try and study the mechanisms 19 driving this, and that study gained lots of 20 interest. But one of the things that were 21 disadvantages of that studies is that it was 22 based on hypothetical scenarios rather than 23 unfilled behavior. And therefore, even though I 24 already studied the question of what exemptions 25 are going to do in a more lab setting, I felt

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65 1 that I should also study a bit in a field 2 setting in order to provide another test for -- 3 for this question. 4 And I also want to say that there 5 are not a lot of opportunities to do these field 6 studies, because you cannot really randomly 7 allocate real people, right, to experience 8 conflicts. So Masterpiece -- the Masterpiece 9 case, the Masterpiece decision provided a really 10 interesting opportunity to try and get at this 11 disagreement about the consequences of religious 12 exemptions. I'm done. 13 Q. Thank you. So with respect to your 14 report, I'm going to turn to page 26 of the PDF, 15 which is Exhibit Number 2, page 6 of the draft 16 Harvard Law Journal article. 17 Do you see that? 18 A. Say again, page 6 of -- 19 Q. Yeah. This is Exhibit 2 to your 20 report -- 21 A. Yeah. 22 Q. -- which is the Harvard Law Journal 23 article draft, page number 6 of that draft, 24 bottom paragraph beginning with the words what 25 were the results of the field experiment.	67 1 well. This is one of the documents we sent to 2 you. So moving over to the Online Appendix. 3 A. What page? 4 Q. Page 19. 5 A. Okay, I'm there. 6 Q. And then there is a table, table 7 OA4.4. Do you see that? 8 A. Yes. Uh-huh. 9 Q. And I'm looking at the column 10 labeled, Total PR. PR stands for, Positive 11 response. 12 Correct? 13 A. Uh-huh. (Witness answers in the 14 affirmative.) 15 Q. And you note in the period before 16 Masterpiece of vendors who were surveyed or 17 audited, I should say. Would you prefer I use 18 the word audited or surveyed? 19 A. Audited. 20 Q. Audited. Vendors who were audited 21 prior to the Masterpiece decision being 22 rendered, who were requested to provide services 23 for a same-sex wedding, of those vendors, 63.6 24 percent gave positive responses to that query. 25 Correct?
66 1 Do you see that? 2 A. Yes. 3 Q. About four lines down you note that 4 while 63.6 percent of businesses were willing to 5 serve same-sex couples before Masterpiece 6 Cakeshop, only 49.2 percent were so willing 7 after the decision was rendered. And then you 8 note in parentheses that is a 14.4 percentage 9 point gap or an approximately 23 percentage 10 decrease in favorable responses. 11 Do you see that? 12 A. Yes. 13 Q. Okay. Now, my understanding is -- 14 I'm going to mark a second exhibit, and that 15 would be your Online Appendix. 16 MR. BANGERT: So Jessie, I believe 17 you have a copy of that as well. 18 THE COURT REPORTER: I do. 19 (Whereupon, the referred to. 20 document was marked as Exhibit 2, 21 and is attached hereto and made a 22 part hereof.) 23 BY MR. BANGERT: 24 Q. And, Professor and Ms. Hinkle, I 25 believe you all both have a copy of that as	68 1 A. Where are you reading from? 2 Q. I'm looking at table OA4.4. 3 A. So you're not reading, you're just 4 interpreting the table, right? 5 Q. That's my understanding of what the 6 table says. 7 A. Yeah. So you will just repeat it 8 because I thought you were reading for something 9 -- from something. 10 Q. So table OA4.4. 11 A. Yes. 12 Q. In the column, Total PR, positive 13 responses. 14 A. Yes. 15 Q. It reports that 63.6 percent of 16 vendors who were audited prior to Masterpiece 17 Cakeshop and requested to provide services for a 18 same-sex wedding ceremony responded favorably to 19 that request? 20 A. Yes. 21 Q. And below that in the row labeled 22 after Masterpiece, that number of positive 23 responses drops to 49.2 percent. 24 Correct? 25 A. Right. That's correct.

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69 1 Q. Okay. I want to do a little bit of 2 unpacking of the data in this table. So let's 3 start with the negative responses. There's a 4 column with the word, Negative or N-E-G. 5 Do you see that column? 6 A. Yes. 7 Q. My understanding is that you coded 8 negative responses as zeros. 9 Correct? 10 A. I think I coded negative responses 11 as minus one. 12 Q. Minus one. Okay -- okay. So 13 negative responses are coded as minus one. You 14 also have a negative with referral response is 15 that also coded with a negative one or was that 16 the negative 0.5? 17 A. I think that was a negative 0.5. 18 But to be sure, I'm just going there to -- 19 Q. Sure. Tell me where you're going 20 as you are doing. 21 A. So you can go to page 29 where you 22 see the coded variables. And you see that there 23 was a response scale of five options. One for 24 an explicit positive response, minus 1 for an 25 explicit negative response. 0 was for no	71 1 Q. So -- so going back to table 0 -- 2 OA4.4 for negative -- negative responses, I'm 3 looking at the column Neg. That records 4 negative responses that would have received a 5 nuanced score of negative 1, and a binary score 6 of 0. 7 Correct? 8 A. Yes. 9 Q. Now, in order for a response to 10 fall in the negative category, I'm going to 11 summarize my understanding here, the vendor had 12 to respond to the outreach from the auditors, 13 and indicate the vendor would not perform the 14 service. 15 Correct? 16 A. Yes. 17 Q. Now, in the response, when the 18 vendor said I will not perform the service, in 19 order to fall into the negative category, the 20 nuanced negative 1, the vendor was not required 21 to say I will not perform the service, because 22 the couple seeking wedding services is a 23 same-sex couple. 24 Correct? 25 A. Not required, no.
70 1 response at all. And there was the implicit 2 positive response which received 0.5 and minus 3 0.5 was, you could think about it as an implicit 4 negative response, but it was when the vendor 5 declined to provide the service, but referred 6 the couple to a colleague or suggested, you 7 know, some other products that the couple didn't 8 want. 9 Q. I recall that. And my 10 understanding though -- 11 A. And then maybe -- maybe your 12 confusion from before is -- is about the 13 variable binary response, which is just that -- 14 Q. That is correct. The variable 15 binary response, negatives were recorded as 0 16 and positives as 1. 17 Correct? 18 A. That's right. But you group -- you 19 group all of the negatives together and they 20 receive 0 and all of the positives together and 21 they received 1. 22 Q. Yes. And I should have been more 23 precise in my question. I was thinking about 24 the binary response, not the nuanced response. 25 A. It's okay, Counsel.	72 1 Q. Not required. A simple no would 2 suffice? 3 A. Sure. 4 Q. Now, in the negative with referral 5 category, same question. In order to fall 6 within the negative with referral category that 7 vendor was not required to provide the reason 8 for the no, and no was sufficient. 9 Correct? 10 A. That's correct. 11 Q. And in order to fall within the 12 negative with referral category, the negative 13 0.5 nuance score, that was then ultimately -- 14 A. You just asked about that before. 15 Are asking the same question again or are you -- 16 Q. I'm restating sort of where I'm at 17 just so for clarity on the record, I want to 18 make sure that everyone knows I'm still within 19 that column negative with referral. So this is 20 the column that is assigned a point -- negative 21 0.5 nuance score, that was rounded to a 0 binary 22 score, to fall within that column a vendor 23 simply needed to provide a no response -- 24 provide a no, an actual affirmative responses no 25 and refer the couple to another vendor.

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73 1 Correct? 2 A. I'm sorry, I'm going to have to ask 3 you to restate because it's getting complicated. 4 Can you rephrase your question? 5 Q. I will certainly do so. I was 6 actually confusing myself. So to fall within 7 the category negative with referral. 8 A. Yes. 9 Q. A vendor would have to respond to 10 the inquiry? 11 A. Yes. 12 Q. He would -- the vendor would have 13 to say I will not provide the service. 14 Correct? 15 A. I'm waiting for you to continue. 16 Q. And the vendor would have to make a 17 referral to another name? 18 A. That's right. 19 Q. Okay. No other criteria were 20 assigned? 21 A. Either referral or as you've seen, 22 I mean if they were saying no, I can do that, 23 but I can give you something else. 24 Q. Okay. And so a -- and when you say 25 that something else, would that have been	75 1 A. Sometimes people say, we cannot do 2 -- we cannot do -- we cannot photograph the 3 event. But, you know, mostly they would say, 4 you know, maybe you could turn to someone else 5 or they would say, I do not offer these services 6 anymore, but -- but I do other things. You 7 know, maybe people have switched from 8 photography to videography or something or these 9 kind of stuff. And we were coding them 10 consistently because sometimes people said that 11 to one person but said something different to 12 another person. So the fact that someone said 13 that they are no longer doing photography, for 14 example, does not mean that they would say that 15 to the -- to another couple that would -- that 16 would contact them. 17 Q. Did you encounter any vendors who 18 responded to you and said, I provide services of 19 this type, whether it be bakery or photographer, 20 but I simply do not provide services for 21 weddings? 22 A. The funniest thing we did -- I 23 don't remember exactly whether we had something 24 so specific, but we did have someone who said, 25 you know, I don't -- I no longer -- I'm no
74 1 something like a non-custom cake? 2 A. No. So -- 3 Q. Okay. What -- what falls in these, 4 something else? 5 A. So for example, if you were asking 6 for -- I was thinking originally that -- that 7 you would -- you could find there. No, I was 8 doing this research on Masterpiece and I was 9 thinking on Jack Phillips's argument that he was 10 willing to provide cupcakes and not wedding 11 cakes. But as you see, that turned out to be a 12 very small category of cases. 13 Q. Yes. And when you were -- and I 14 imagine you probably looked at some of the raw 15 data, when you were looking at the referrals or 16 you were looking at the responses ultimately 17 landed in this column, negative with referral -- 18 A. Yeah. 19 Q. -- did you see any instances of the 20 vendor offering to provide services other than 21 those that were requested by the auditor? 22 A. There were very few of those. Very 23 few. 24 Q. And in those instances, what types 25 of alternate services were being suggested?	76 1 longer in the business of photography, I am 2 going to become an astronaut. So that person 3 ultimately was excluded from the sample because 4 he responded the same thing to all. So, you 5 know, we realized he wasn't supposed to be 6 there. 7 Q. Yes. That's that very common 8 photographer to astronaut career path. 9 A. Right. I thought you would 10 appreciate that. 11 Q. Yes. And that's -- I would expect 12 to hear that answer more often. So -- 13 A. I'm trying to do my best. 14 Q. So then you have a category in the 15 middle there. I'm looking again at table OA4.4. 16 It says, no response. And I assume that just 17 means, as your report says, as your documents 18 say, the vendor simply did not respond to the 19 inquiry. 20 Correct? 21 A. Yes. 22 Q. Now, my understanding, and correct 23 me if I'm wrong, if a vendor reached out after 24 the seven days or a period after the audit 25 questions were provided, after the e-mail was

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77 1 sent, if the vendor reached out late, like 8, 9, 2 10, 11 days after they would either fall in the 3 no response category or be tossed out of the 4 survey? I can't recall which it was. 5 A. No. They would be coded. 6 Q. Oh, they would be coded. Okay. So 7 even if they responded well after -- afterwards, 8 they -- 9 A. I never -- I never code or analyze 10 data before everything is complete. It's a bad 11 practice. 12 Q. So in order to fall within the no 13 response column, a vendor simply had to not 14 respond to the e-mail that was the subject of 15 that particular wave? 16 A. Yep. And I want to -- I mean, I 17 don't know if it's important for your purposes, 18 but I want to be clear that we were not coding 19 automatic responses as responses. So it had to 20 be an active response. 21 Q. Yes. Understood. Like a -- like 22 an automatically generated response from the 23 Microsoft Outlook program, saying -- 24 A. E-mail, I'll return to your later, 25 right.	79 1 the coding because my answer maybe interfered 2 with their judgment. So they remain 0.5. 3 Q. And then positive was obviously an 4 agreement to provide the service on the date 5 given? 6 A. Yeah, like, wonderful, we'd like to 7 work with you. What did you have in mind? 8 Q. Okay. Now, going back to paragraph 9 13 of your report in this case. 10 A. Yep, I'm -- I'm looking at it. 11 Q. Good. You talked about the wedding 12 market? 13 A. Yes. I see that. 14 Q. Yes. You said you were trying to 15 get a -- you were examining the impact of the 16 Masterpiece Cakeshop decision on the wedding 17 market. And I assume that you were now in this 18 report attempting to evaluate the impact of a 19 potential decision in Chelsey Nelson's case on 20 the wedding market in Louisville, Kentucky. 21 Is that a fair inference? 22 A. Yes. 23 Q. And the wedding market, as I 24 understand it, is the collection of vendors who 25 provide wedding-related services to residents of
78 1 Q. That was not a response. Okay. 2 Positive responses, when we 3 mentioned that it was cooperative, describe for 4 me what -- what kinds of specific responses fell 5 within that cooperative column? 6 A. So I think I talk about this in the 7 -- in the nuanced part of the appendix. So I'm 8 just going to read from there and I'm happy to 9 expand if you'd like me to. So for example, 10 they could be asking for additional information 11 about the location of the venue which was not 12 specified in the e-mail. And that strike me, as 13 at least initial willingness. So it's not a 14 negative. So -- but it wasn't a full positive. 15 So that -- that got 0.5. 16 So sometimes it looked like they 17 were willing to consider, but they need more 18 information. And even though every e-mail 19 received a response, then to not taint the study 20 with our responses then we didn't consider what 21 comes after our -- so let's say they asked us, 22 you know, about the venue and, you know, then I 23 give -- I give them a -- I give them a -- a 24 village that's nearby their business, and then 25 they say wonderful, great. I wouldn't correct	80 1 a particular geographical jurisdiction. 2 Correct? 3 A. Yeah. I mean, it could be whatever 4 jurisdiction or a geographical area you have in 5 mind. And of course, you know, there could be 6 -- Louisville is -- is the center of Kentucky. 7 I mean, then it's part of a metropolitan. I 8 mean, it could have additional influences as 9 well, but yes. 10 Q. Yes. And is it fair to say -- I'm 11 looking at page 4 of Exhibit 3 to your report, 12 which was the draft article for the Journal of 13 Legal Studies. At the bottom of Page 4, you 14 listed about ten -- you list ten types of 15 vendors who are participants in the process of 16 organizing a wedding: Reception venues, wedding 17 planners, bakers, florists, photographers, 18 videographers, bridal and groom salons, 19 jewelers, DJs, and calligraphers. 20 Do you see that? 21 A. Yes, I do. 22 Q. Is it fair to say that is -- that 23 that collection of vendors constitutes the 24 wedding market for your purposes? 25 A. Well, I mean, there are lots of

81 1 people providing services for weddings and I 2 would say that these are among them. 3 Q. Yes. Are there others that you 4 have in mind besides these ten? 5 A. These are the -- these are, I 6 think, the most common ones. Of course, couples 7 could use more or less of them. 8 Q. All right. So and you agree that 9 not every couple takes advantage of a DJ for -- 10 for example? 11 A. That's right. But bakers, 12 florists, and photographers are probably the 13 most -- the busiest ones, almost in every 14 wedding, you have them. If not all -- all 15 weddings. 16 Q. Reception venues are pretty common 17 as well? 18 A. Right. Although, I mean, couples 19 can also marry at their parents' home. I mean, 20 that depends that there are lots of -- it's a 21 very complex legal transaction to organize a 22 wedding -- 23 Q. Right. 24 A. Lots of serious transactions. 25 Q. Right. One that I see missing here	83 1 A. No, I didn't. 2 Q. You didn't study -- you didn't 3 study videographers? 4 A. No. Although they might be similar 5 to photographers, but I didn't study this group, 6 no. 7 Q. You didn't study wedding planners? 8 A. No. 9 Q. You didn't study jewelers? 10 A. No. 11 Q. You didn't study DJs? 12 A. No. 13 Q. You did not study calligraphers? 14 A. No, I did not. 15 Q. You did not study bride and groom 16 salons? 17 A. That's correct. 18 Q. Now, did you also study whether 19 same-sex couples are more or less likely to 20 select wedding service providers based on 21 referrals, or are more likely to simply cold 22 call or pick them out of a phonebook? 23 A. So that's an interesting question. 24 That brings me to my background research on the 25 wedding market, which I did in order to begin
82 1 will be the wedding venue, for instance, 2 assuming it's different than the reception? 3 A. Okay. I meant -- I meant the 4 venue. You could add that as well. 5 Q. I see. Okay. Now, did you do 6 anything to evaluate whether the attitudes and 7 the willingness of each of these various members 8 of the wedding market are identical with respect 9 to your willingness to serve same-sex weddings? 10 A. No. We don't have any information 11 on that, I think, anywhere. 12 Q. So you can't tell me whether a 13 wedding venue provider is more or less likely to 14 agree to provide services to a same-sex wedding 15 than a baker? 16 A. So what I can tell you based on my 17 study is that I looked at these differences. I 18 also write about it and I found no differences 19 in the specific sense that we're interested in, 20 which is whether those different business 21 categories would respond differently to same-sex 22 couples following Masterpiece Cakeshop, the 23 decision. 24 Q. But you didn't study reception 25 venues?	84 1 this study. So it seemed fairly common for -- 2 just based on researching what couples do 3 online. And you can see some of this in -- in 4 my studies in the references, but I did a lot of 5 -- I -- you asked me about my field of expertise 6 before. I should -- I forgot to say that I'm 7 also an expert in planning a wedding after this 8 research. 9 And what I found was that it's very 10 common for couples to just search online in, you 11 know, in the surroundings of the place where 12 they want to marry and look for business -- 13 businesses, vendors who would be interested in. 14 And there are also websites. For example, The 15 Knot that just vendors can register there and 16 then you can search there for wedding vendors 17 very conveniently and, you know, based on area 18 and -- and all kinds of stuff. And The Knot is 19 not the only one. So and, of course, I mean, 20 referrals are also an option. But it seems very 21 common for -- for a couple to just search online 22 for -- for vendors. 23 Q. Now, when you say you became an 24 expert wedding planning, are you referring to -- 25 to your own wedding?

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85 1 A. My own wedding was before this 2 study. 3 Q. Okay. I wasn't sure if you were 4 making a reference to that. So but did you -- I 5 hear what you're saying, but did you do anything 6 in terms of quantitative -- or sure qualitative 7 or quantitative analysis to determine how 8 frequently a same-sex couple relies on referrals 9 versus how frequently they rely on simply 10 choosing vendors unknown to them previous out of 11 the phonebook or off of -- off of an online 12 resource? 13 A. No, there is no information on that 14 available to analyze. 15 Q. Sure. 16 A. And I was doing my research on what 17 couples do bases on what's -- what the resource 18 is available for them. Yeah. 19 Q. Sure. You would agree with me 20 though, I mean, the wedding market varies from 21 state to state, geographical location to 22 geographical location. 23 Correct? 24 When you found that, for instance, 25 Iowa had apparently a shortage of bakers and you	87 1 outside of the United States and -- because 2 these cases are recurring all over the world, 3 but there are cases of both types. 4 Q. So you're not telling me though 5 that you're relying on case law to inform your 6 views as to how couples identify wedding 7 vendors, you're not -- that's not what you're 8 telling me. 9 Correct? 10 A. No, I'm just giving you an -- so 11 expanding your views on how kind -- how kind of 12 -- how same-sex couples might come to engage 13 with wedding vendors. 14 Q. Sure. That you would agree case 15 law is actually highly unrepresentative sample 16 of how interactions and conflicts between 17 religion and principles -- 18 A. I would -- I would think you may be 19 citing my research now. 20 Q. I think I'm thinking of your 21 Villanova Law Review article. 22 A. That's correct. 23 MR. BANGERT: Yes. By the way, I 24 am offering to our reporter, are you in need of 25 a break at this time?
86 1 had to turn to florists to supplement your 2 research in Iowa. 3 Correct? 4 A. That's right. 5 Q. All right. And so it's reasonable 6 to believe that the ways in which couples 7 identify and locate wedding vendors will vary 8 from place to place. Wouldn't you agree with me 9 that that's a reasonable assumption? 10 A. No, I don't know that. I mean -- 11 Q. You don't know what -- you don't 12 know either way? 13 A. No. I mean, if we're looking at 14 the examples from the case law, then we can see 15 that conflicts can occur either way. So for 16 example, the couple that entered Jack Phillips's 17 store came there because they got a 18 recommendation on Phillips's cakes from their 19 wedding planner. They didn't know him before. 20 And then with Arlene Flowers, that 21 was a long-time -- a long-time customer. But 22 then with some other cases like the Aloha B&B 23 hotel then that was like a cold call in your 24 terms. And you know, there are some cases also, 25 I mean, in many, you know, I -- I could also go	88 1 THE COURT REPORTER: I'm okay. 2 BY MR. BANGERT: 3 Q. Good. Okay. Let's talk about -- 4 the Masterpiece decision was something that 5 factored heavily in your research. 6 Correct? 7 A. The Masterpiece decision itself? 8 Q. Yes. 9 A. Of course. 10 Q. Of course. Now, the Masterpiece 11 ruling did not yield a religious exemption for 12 Jack Phillips, right? 13 A. I think that depends on how you 14 look at it. That's actually disputed by legal 15 scholars. 16 Q. Right. The -- the upshot of that 17 decision was the Supreme Court found that the 18 Colorado Commission acted with hostility towards 19 Jack's religion and for that reason had violated 20 the free exercise clause. 21 Correct? 22 A. Yes. But then afterwards, the 23 Supreme Court remanded two other cases where 24 there was no allegation of hostility on the 25 record, you know better than I do, and returned

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89 1 them to the lower courts. 2 And I should also say, I mean, that 3 Masterpiece was more vague than clear about the 4 exact basis for the ruling. And that allowed 5 the decision to be portrayed, even by Jack 6 Phillips himself and his lawyers, as a victory 7 for religious freedom, which is not just about 8 the proceeding in Colorado. I mean, Phillips is 9 quoted saying -- I can find that if you'd like 10 -- that, you know, from now on every religious 11 person can feel free to follow their business 12 and to run their business according to their 13 values. So obviously, there is what the court 14 does and what people understand the court to do. 15 And -- 16 Q. Yes. I -- I agree. 17 A. And even legal scholars can 18 disagree about the interpretation of certain 19 court decisions, which is what happened in this 20 case as well. 21 Q. Well, there would be no law review 22 industry without such disagreements. Now, you 23 would -- do agree with me though, the court, 24 because it detected religious hostility in the 25 Colorado Commission's decision, that it did not	91 1 in-person or on social media or online about the 2 decision, but any of those would be a way -- 3 A. And they can also listen to the 4 radio or, you know, watch TV, et cetera, et 5 cetera. 6 Q. Sure. I would -- I would classify 7 all those as media-driven accounts of the 8 content of the decision. 9 A. Yes. 10 Q. But fundamental to your opinion is 11 the assumption that vendors will become aware, 12 one way or another, of the content of the Court 13 decision that you would -- you would argue is 14 driving their behavior or influencing their 15 behavior? 16 A. Well, research -- some research -- 17 research seems to show that they don't have to 18 actually become aware of it. So imagine the -- 19 I don't know if that's of service to you but 20 imagine the following scenario. Say that you 21 and I are both wedding vendors or maybe that 22 you're, you know that you're a friend of mine, 23 okay? And you read about this decision and I'm 24 a wedding vendor and, you know, you come to me 25 and you don't tell me about the decision, but I
90 1 conduct a full-blown, full scrutiny -- strict 2 scrutiny analysis of the Commission's actions 3 but more or less assumed it was a violation per 4 se? 5 A. I would agree with this first part 6 of what you said that he did not conduct a full 7 strict scrutiny analysis, at least not the 8 majority opinion. Of course, we have the 9 minority. 10 Q. Correct. Correct. Now, your -- 11 your analysis of the Masterpiece opinion's 12 effect on the willingness of vendors to supply 13 services to same-sex couples really depends upon 14 their -- depends upon those vendors reaching 15 some understanding about the content of the 16 decision that was rendered by the Supreme Court. 17 Is that fair? 18 A. Either by themselves or by an 19 atmosphere that was surround -- that was -- 20 that evolved out of the decision. 21 Q. Of course, in ways that that can 22 happen. The vendor themselves could read the 23 opinion. The vendor could read media accounts 24 of the opinion, or the vendor could have 25 personal conversations with others, whether by	92 1 get from everything that you're saying that 2 you're in favor of, you know, for example, 3 denying service to same-sex couples to 4 interracial couple to whatever it is. I might 5 be influenced by that, even if you don't tell me 6 that anything has changed. 7 So sometimes court decisions 8 influence how people behave or what they think 9 through their shaping the -- their perceptions 10 of the social norm. So we have several studies 11 that shaped -- that showed that this is what 12 happens with Supreme Court decisions. 13 So I can talk about one. So for 14 example, Obergefell. So with Obergefell, two 15 social psychologists from Princeton, Betsy Levy 16 Paluck and Margaret Tankard, have studied over a 17 series of national opinion surveys, et cetera, 18 that they started with before Obergefell and 19 then followed with after Obergefell a month -- 20 for a month after the decision. 21 And they studied both people's 22 personal views and their perceptions of the 23 social norm towards same-sex marriages. And 24 what they found was that the people's personal 25 views were not changed, but people's perceptions

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93 1 of the social norm regarding the legitimacy of 2 same-sex marriage have changed after the 3 decision. And they also found that this was not 4 affected by the frequency of media consumption. 5 So the -- the -- their finding suggests that 6 there could be other ways, either -- other than 7 consuming media about the decision where message 8 of the court actions spread in society and shape 9 social views. 10 Q. You're an attorney, so you're -- 11 you're familiar with the concept of attenuated 12 causation, proximate cause, and but for 13 causation, the rules governing causation, and it 14 strikes me, you were -- you were attempting to 15 measure the direct actual cause and effect 16 relationship between the Masterpiece decision 17 and decisions by vendors whether or not to 18 provide service to same-sex weddings. 19 That was your objective? 20 A. Right. And since -- okay. Let's 21 -- let's -- let -- let you finish your question. 22 I would not want to preempt your question. 23 Q. No, I was -- I was just trying to 24 confirm that was your objective. 25 Did you do any work to evaluate	95 1 Q. Okay. Can you point me to where in 2 your report, these studies that you're talking 3 about right now are mentioned? 4 A. Do you have it printed or do you 5 want to search it? 6 Q. I have -- I can search it. I can 7 do either. 8 A. So the main study that we can talk 9 about, because I think it did the best job, is 10 the study of Katerina Linos and Kimberly Twist 11 from 2016. And if you're looking at the JLS 12 piece where I'm at, but we can look outside at 13 the other piece, I think it's mentioned there as 14 well. So it's mentioned on page 10 and 8 and 15 twice on page 10, 8, and in page 42 in the 16 references. 17 Q. Okay. I don't need to discuss that 18 article so much as just make sure I knew where 19 to find it. Any other references that would 20 explicate this idea that you were just telling 21 me that -- that knowledge of a decision by a 22 court can filter into the public consciousness, 23 even without a member of the public consuming 24 media about the decision or reading the decision 25 itself.
94 1 whether the vendors you contacted as part of 2 your Masterpiece experiment had any knowledge 3 of, appreciation for, or understanding of the 4 Masterpiece decision after it was rendered? 5 A. No, I couldn't do that. I mean, it 6 wasn't a survey. You asked me before how I'd 7 like to call it. I was contacting them -- 8 contactin them as discerned customers wishing to 9 get service. They're -- these wedding vendors 10 are not -- they're not part of any -- of any 11 survey, company panel or something. 12 So I would say that they -- there 13 were several studies conducted in other Supreme 14 Court decisions before mine that measured 15 attitudinal changes, asked, recruited survey 16 participants and some of them collected 17 information on media consumption. And I can 18 talk about what kind of met -- lessons we 19 learned from those studies. 20 Q. Okay. I don't want to ask you that 21 question, but I do want to ask you this: Are 22 the studies that you're talking about right now 23 mentioned anywhere in your expert report or the 24 exhibits thereto? 25 A. Yeah.	96 1 A. Sorry, I just -- before I mentioned 2 Linos and Twist, I mentioned Tankard and Paluck 3 and they're mentioned on page 44 of my 4 references going by Tankard. 5 Q. Okay. And any other references 6 that you would point me to for that proposition? 7 A. And there was also another, the -- 8 I cite additional two studies that measured the 9 impact of the legalization of gay marriage on 10 public opinion. They're Kazyak and Stange from 11 2018 and Ofosu, O-F-O-S-U, et al., from 2019. 12 These are all studies that are directly relevant 13 to this project and -- but they're also more, I 14 mean -- 15 Q. Now let me ask about one cited in 16 your -- cited in your report. 17 A. Okay. 18 Q. So is that -- have we covered the 19 waterfront of sources that would support the 20 proposition we were just discussing that are 21 cited in your report? 22 A. Yes. 23 Q. Thank you. Now, you mentioned -- I 24 want to go back to -- I want to change gears a 25 little bit and talk about a slightly different

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97 1 topic. And that is the selection of the states 2 that you included in the Masterpiece experiment. 3 So Iowa, North Carolina, Indiana, and Texas. 4 And I'm thinking in particular -- 5 A. And then after this question, I'm 6 asking a break. 7 MR. BANGERT: Okay. Well, you 8 know, instead of me getting into this because 9 it's going to be a series of questions. Why 10 don't we just go ahead and take the break now 11 and then come back. Would that be fair? 12 THE WITNESS: Yeah. 13 MR. BANGERT: Okay. It is -- by my 14 lights it is 2:16 Texas time. I don't even want 15 to ask what time it is Israel right now. 16 THE WITNESS: 10:16. 17 MR. BANGERT: 10:16. Okay. Well, 18 I will -- why don't we just take ten minutes and 19 come back in ten minutes. Is that fair? 20 THE WITNESS: Yes. 21 MR. BANGERT: Okay. I'm going to 22 mute my line and go off video. I'll be back in 23 ten minutes. 24 25	99 1 A. Uh-huh. (Witness answers in the 2 affirmative.) 3 Q. Okay. And the characteristics in 4 -- that are identified here are GDP per capita? 5 A. Uh-huh. (Witness answers in the 6 affirmative.) 7 Q. Importance of religion? 8 A. Uh-huh. (Witness answers in the 9 affirmative.) 10 Q. And then some percentages, 11 percentages of conservatives and percentages of 12 evangelicals. 13 A. Uh-huh. (Witness answers in the 14 affirmative.) 15 Q. Then some attitudes. Attitudinal 16 studies, attitudes towards homosexuals, and 17 attitudes towards same-sex marriage? 18 A. That's correct. 19 Q. And then if you go to the following 20 page, you also identify whether those 21 jurisdictions have either a state RFRA or a 22 state or local anti-discrimination law? 23 A. Uh-huh. (Witness answers in the 24 affirmative.) 25 Q. Are those the characteristics that
98 1 * * * 2 (OFF THE RECORD) 3 * * * 4 5 THE COURT REPORTER: Okay. We are 6 back on the record. 7 8 * * * 9 CONTINUED EXAMINATION 10 BY MR. BANGERT: 11 Q. Professor, we're back from a break 12 and I wanted to turn your attention to page 12 13 of Exhibit Number 2 -- I'm sorry, Exhibit Number 14 3 to your report. I'm sorry, Number 2 -- Number 15 2, your Harvard Law Journal article. 16 A. Are you sure? Because page 12 is 17 where I describing the JLS piece, the sample. 18 Q. Yes, that is correct. Oh, was that 19 the JLS. You're right, that is the JLS piece. 20 I was -- I was misidentifying the format. That 21 is the JLS piece. So page 12 of Exhibit 3. And 22 there you have a table, Table 1, which 23 identifies characteristics of the sampled 24 regimes. 25 Do you see that?	100 1 you looked at when attempting to decide whether 2 or not a state was similar enough to other 3 states to include within your Masterpiece 4 experiment? 5 A. Yes. Yeah. These are those. 6 Q. These are those and there -- were 7 there any other characteristics that you did not 8 include in the table that we just discussed that 9 were determinative for you? 10 A. No. 11 Q. No. Thanks. Let me finish my 12 question for you to make sure I've got it 13 correctly on the record that were determinative 14 of whether or not you included a state within 15 your study? 16 A. No. I was looking at those and 17 that was a hard enough job. 18 Q. Okay. And you ultimately, based on 19 those criteria, you settled on the states of 20 Iowa and North Carolina, Indiana and Texas. 21 A. Right. And I explain in the paper 22 that some of the complexities around this 23 decision. 24 Q. Yes. Understood. Now, I noticed 25 in the table you break out, in addition to the

101 1 four states, two metropolitan areas in Texas: 2 Dallas and Houston? 3 A. Right. I hope you're not 4 disappointed Austin is not there. 5 Q. Oh, not -- not at all. Not at all, 6 Austin would never want to be associated with 7 Dallas or Houston. But in terms of Dallas and 8 Houston, were there any other -- let me stop 9 there. It looks to me like in addition to 10 looking at state-level data, you also looked at 11 metropolitan level data for those two cities in 12 particular. 13 Is that fair? 14 A. So that -- that's to exemplify 15 because my original idea was to sample just 16 states. But then what I found was that the 17 category of both having an anti-discrimination 18 law and having a RFRA is just very, very 19 limited. I mean, I knew that but I -- I -- 20 nobody has compared the characteristics of 21 states before. So it's turned out to be the 22 case that they're not really comparable, those 23 states to the other states. And that would have 24 been unfortunate for a study that wants to clean 25 as many things possible and just focus on	103 1 Pugh Institute survey of -- of primarily states, 2 but they did include statistics for some cities. 3 And that was my only possibility of showing how 4 this might look like at the -- at the city 5 level. 6 So I have to admit that I don't 7 remember whether there are no such cities -- no 8 additional cities because they're not available 9 on the Pugh Institute website database, sorry, 10 or because there -- there wasn't just enough 11 space in the table. But I was just trying to 12 illustrate this local variation of some 13 localities having both a state RFRA and a state 14 anti-discrimination law like Dallas versus 15 Houston that doesn't. 16 Q. Yes. And I know there's -- there's 17 -- a separate measure that you referenced 18 elsewhere in your report called the density of 19 religiosity or religious density. 20 A. Correct. 21 Q. You recall that concept? 22 A. Yeah. 23 Q. And my understanding of that 24 concept, religious density, that's a measure 25 that you and your team devised. And I'm going
102 1 differences in legal backgrounds. 2 So that's when I began to zoom in 3 and look at -- at little counties or cities 4 really. And -- and that -- and then that's -- 5 that guided my choice in Indiana and Texas. As 6 states they were similar in all respects to Iowa 7 and North Carolina. At least more similar than 8 all other -- other united -- states in the 9 United States. But still had this interesting 10 variation inside them of having some localities 11 with anti-discrimination laws and some not. And 12 what I do here is just I exemplify what the data 13 looks like in those localities, just by focusing 14 on Dallas and Houston. 15 Q. Of course. Did you look at the 16 criteria that are listed in Table 1 with respect 17 to any other specific localities, either within 18 the state of Indiana or the state of Texas and 19 just simply didn't report that in the table? 20 A. I don't remember, but my guess 21 reconstructing what I did is -- because I'm sure 22 you know that, but this date of is not available 23 for any locality. So this -- these data on the 24 importance of religion and attitudes towards 25 homosexuals, et cetera, this is taken from the	104 1 to summarize my understanding of how you devised 2 that measure. You looked at populations in 3 certain metropolitan areas and you identified 4 congregations. So evangelical congregations, 5 religious -- 6 A. Can I correct you though? I mean, 7 before you -- 8 Q. Yes. 9 A. So it wasn't a diff- -- so it 10 wasn't anything that I devised. So I was 11 relying -- relying on publicly available data. 12 Essentially, a very large survey of religious 13 congregations in the United States that was 14 conducted in 2010 and was the largest by then, I 15 mean, maybe Pugh Institute is working on 16 something else now. But when I was doing the 17 research that was -- that was the best we had. 18 And what I did was I matched the 19 data -- that publicly available data from -- 20 from that survey of congregations. And I can 21 explain about the method of how they collected 22 the data, if you're interested. And then -- and 23 then I matched that with my data with respect to 24 the localities, the specific counties, where 25 businesses were located. So I was able to -- to

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105 1 match my -- the -- the -- the sort of religious 2 environments where my businesses were located 3 with -- with their data and have that data 4 inside my data set. 5 Q. Yes. And so when I -- when I -- 6 when I said that you devised the measure, you 7 were relying -- I understood that you were 8 relying on publicly available data. But in 9 terms of the concept of religious density, you 10 weren't able to go to a publication and say, 11 ah-ha, the religious density of X metropolitan 12 area is this. You had to make it -- you had to 13 perform a calculation on your own using publicly 14 available data in order to arrive at a measure 15 of religious density. 16 That's -- is that fair? 17 A. Yeah. But both the population and 18 the number of congregations were taken from the 19 same source. So that -- 20 Q. Yes. 21 A. -- available data. Yeah. 22 Q. Yes. 23 A. So that I was really just 24 normalizing the number of congregations with the 25 number of people residing in the area.	107 1 discussed. 2 A. Yes. 3 Q. Setting religious density off to 4 the side, over there, apart from that, there was 5 no other measure that you could look at to 6 determine the religiosity of a particular 7 metropolitan area. 8 Correct? 9 A. Not at the county level, not that I 10 was aware of. 11 Q. All right. So -- so you really had 12 so the -- the density measure is really what you 13 had to look at when you were trying to evaluate 14 the religious context at the county level in 15 which a particular wedding vendor exists or 16 lives in? 17 A. That's right. 18 Q. Now, did you do anything to 19 evaluate whether the wedding vendors who were 20 contacted as part of your audit, understood or 21 knew about the legal regime in which they 22 operate? When I say legal regime, I'm talking 23 specifically about whether it's a RFRA or 24 non-RFRA state and whether or not there's an 25 applicable anti-discrimination law in their
106 1 Q. Yes. Yes. Now, something that I 2 didn't see in your report and I'm curious, did 3 you look at the size of the congregations such 4 that some areas -- I'm sorry. 5 A. Let me -- yes. So I didn't and the 6 reason why I didn't is that -- on that -- on the 7 -- the surv- -- the very large survey that I'm 8 referring to, they explicitly say, the 9 researchers, that this is not reliable. So they 10 collected information both on the number of 11 congregations and on the number of congregants. 12 But they explicitly said that they think this is 13 not reliable. They had several reasons to think 14 that. 15 First, there were some 16 congregations that reported on more people than 17 the people in the county. And there were some 18 mismatches with the numbering of other 19 congregations and they just felt that it's not 20 reliable. So they explicitly, sort of, warn 21 researchers about this measure. So, you know, I 22 wanted to -- to play safe and I just looked at 23 the number of congregations. 24 Q. Of course. So -- so we have this 25 concept of religious density that we've just	108 1 particular jurisdiction? 2 A. No, I don't. And I actually write 3 about that in the paper, so you must have read 4 that. But it's actually very difficult to make 5 these kinds of inferences. So there is a host 6 of research in the field of law in society. You 7 know, what shapes what and whether people know 8 or do not know on the content of laws where 9 they're at. So it would be too strong an 10 assumption to think that people are necessarily 11 aware. Although you might think that 12 businesses are more aware than just, you know, 13 regular individuals about the content of laws 14 that are relevant to them. But still, I mean, 15 it's -- it's too strong an assumption. And for 16 the same reasons that I couldn't survey that -- 17 those businesses that weren't survey respondents 18 about their media consumption habits, then I 19 didn't survey them about whether they know the 20 state of the law. 21 Q. Right. And I think that's probably 22 why you said in your report that you're not 23 arguing for a causal relationship between the 24 specific legal regime within which a vendor 25 operates, and that vendor's particular attitudes

109 1 towards same-sex marriage? 2 A. That is correct. So and what I 3 wrote about that is that that relationship could 4 -- so what's the study could reveal in that 5 respect is correlations -- potential 6 correlations. And -- but we wouldn't know 7 whether it was a specific societal makeup that 8 caused some pieces of legislation to be enacted 9 and others not. Or whether legislation itself 10 shaped what people believe in. And the reason 11 why we cannot answer this question is because 12 these pieces of legislation weren't researched 13 in real times, unlike the Masterpiece decision. 14 So we don't have a clean before and after test 15 for them. That would require another study that 16 would be more limited because it wouldn't be an 17 experiment. 18 Q. Yes. I think that's also why you 19 -- you don't claim that the -- that you can draw 20 a causal relationship between the particular 21 socio-political structures underlying the legal 22 regime of any particular location -- any 23 particular jurisdiction where an event -- 24 wedding is located, and that vendor's attitudes 25 towards same-sex marriage?	111 1 comprised requests that were sent to vendors 2 only on behalf of same-sex couples. 3 Correct? 4 A. That's correct. 5 Q. Hypothetical same-sex couples. 6 A. I think the term is fictitious. 7 Q. Fictitious. Thank you. And then 8 wave 2 comprised of requests sent on behalf of 9 fictitious opposite-sex couples? 10 A. That's correct. 11 Q. And then you -- waves 3 and 4, it 12 was a mix of both? 13 A. Randomly allocated. 14 Q. Randomly allocated to specific 15 vendors. My understanding was -- 16 A. Each business was randomly 17 allocated to receive either a same-sex or an 18 opposite-sex coupling with three. And then -- 19 Q. You reversed it? 20 A. Exactly, on wave 4. 21 Q. Yes. Yes. Now, you noted in -- 22 I'm going to find the exact page. I think it's 23 footnote 21, page 17 of Exhibit 3, which is your 24 Journal of Legal Studies article. 25 A. Which page?
110 1 A. Right. Mapping of the variation in 2 that -- in the study with respect to these 3 features. 4 Q. Now, as I understand your study you 5 conducted it really in four waves, two waves 6 prior to the Masterpiece decision and two waves 7 after the Masterpiece decision. And my 8 understanding of the third and fourth waves was 9 that they were sent in June of 2018. I'm trying 10 to recall when the first and second waves were 11 sent. I thought it was an April, but I was -- I 12 was just looking for that and I couldn't find 13 it. Was it an April of 2018 or May? 14 A. It was in May. 15 Q. May. May. 16 A. Do you want the specific dates? 17 Q. That would be helpful. Yes. 18 A. So May -- May 8th. 19 Q. Was wave 1? 20 A. May 15th. 21 Q. Uh-huh. 22 A. And then June 4 was Masterpiece. 23 And then June 13th and June 20th. 24 Q. Yes. And so four specific -- four 25 different waves, four different dates. Wave 1	112 1 Q. Page 17. 2 A. 17. 3 Q. Footnote 21. 4 A. Yep. 5 Q. Your Journal of Legal Studies 6 article, you note that -- you make a note about 7 the timing -- 8 A. Yeah. 9 Q. -- for contacting prospective 10 vendors. And you noted that for florists and 11 cake shops, roughly six to nine months in 12 advance is appropriate timing to contact them. 13 And for photographers it's a bit longer, 9 to 11 14 months. 15 A. That's correct. 16 Q. Okay. And how did you come up with 17 that? With those figures or numbers? Was that 18 based on the research you did about the wedding 19 market? 20 A. That's correct. 21 Q. Yes. But you're -- you're not -- 22 you don't hold yourself out as an expert on 23 wedding planning, right? 24 A. I do though -- I just -- I 25 corrected myself before that.

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113 1 Q. You do. Okay. So -- 2 A. I mean, I would say that I -- I 3 haven't been researching this field in 2020 and 4 2021. But back then in 2018, I was a real 5 expert on planning weddings in the U.S. 6 Q. Okay, but in terms of I guess -- 7 A. I wouldn't plan your wedding now, 8 maybe the field has changed. 9 Q. I see. So you do not hold yourself 10 out as an expert in wedding planning today in 11 2021? 12 A. Right. I don't know what COVID did 13 to weddings. 14 Q. Fair enough. I don't think it did 15 anything because no one's getting married right 16 now, apparently. Well, actually, I've had 17 several friends get married, so that's not true. 18 It's an unfair statement. Okay. 19 So what -- were there specific 20 sources that you looked to for this information 21 that your report in footnote 21? 22 A. So I see footnote 21 doesn't report 23 sources, but other footnotes report sources of 24 exemplar websites and wedding manuals. So that 25 was -- that was something I researched quite	115 1 me right now, I would say that I don't know. 2 But I could, I mean, Casey said I shouldn't 3 offer to look for stuff, but -- 4 Q. Well, I will comment -- I won't 5 comment on the advice of counsel. I'm not 6 asking you about the advice of counsel. 7 Let me ask you this, let's go to 8 your -- let's go to your Online Appendix, which 9 is Exhibit 2 to your deposition. 10 A. Sure. 11 Q. And in your Online Appendix, you 12 report the script, I won't call it a script, but 13 the text of the e-mails that you sent -- 14 A. Sure. 15 Q. -- to vendors as part of the 16 various waves of your report -- of your study. 17 Is that correct? 18 A. That's correct. 19 Q. There are all of the various -- 20 there are all of the e-mail texts reported here 21 in your online account? 22 A. Yes. 23 Q. Now, who drafted the text of these 24 e-mail scripts? 25 A. I did together with my research
114 1 heavily. 2 Q. Okay. And all of the research that 3 you conducted into the wedding market is 4 referenced in your report, right? At least all 5 the -- all the resources that you're relying 6 upon to inform your expertise in the wedding 7 market? 8 A. Not necessarily. I mean, journals 9 often want you to cut words from footnotes and 10 cuts resources sometimes. And so things that 11 they didn't see material may have been dropped. 12 But enough sources are there in terms of, you 13 know, types of websites and so on, to give you a 14 sense of the sources that I was using. 15 Q. Are there any sources that you can 16 think of right now that directly inform the 17 information that you convey in footnote 21 that 18 are not reported in your -- in your report? 19 A. I don't know. I mean, maybe there 20 are more because I always choose -- I mean, 21 there were so much -- so many of them that maybe 22 I chose just several for the footnotes that were 23 sort of referencing the kind of wedding manuals 24 that I was using. And so maybe there are more 25 than that, right? But I mean, if you're asking	116 1 assistant. And we also had someone who's native 2 English speaker to go over it -- more than one, 3 actually. 4 Q. And the reason I asked that 5 question is I noticed the -- the script that was 6 used for waves 1 and wave 2 -- wave 1 and wave 7 2 -- 8 A. Yeah. 9 Q. -- was very similar. But when you 10 get to the script for waves 3, and again, for 11 wave 4, I'm going to describe the language as 12 much more colloquial in nature. For instance, I 13 see -- I even see a, woo-hoo, in one of them, 14 the photographer e-mail for wave 3. 15 A. That's right. There was a, woo-hoo 16 there. 17 Q. Yes. 18 A. But we had something similar to a 19 woo-hoo, I think, in one of the 1 and 2 e-mails 20 as well. But in any event, you're right to note 21 that there was a change in the style or tone of 22 the e-mails from before and after Masterpiece 23 and that was deliberate. 24 Q. Okay. Why was there a change in 25 style and tone after Masterpiece?

117 1 A. So it wasn't related to Masterpiece 2 itself. It was related to the attrition that I 3 saw after wave 2. So what happened was that I 4 fill in wave 1, and then I fill in wave 2 and 5 even though we didn't code the data so we had 6 indication from the platform that we were using 7 to send the e-mails, that there were less 8 e-mails received back for wave 2. That looked 9 like a significant difference. So I was 10 already -- 11 Q. Right. 12 A. What? 13 Q. I said right, I was just -- I was 14 -- I was encouraging you on in your answer. 15 A. Okay. And so what I did then was 16 to -- and in that -- that point I also consulted 17 lots of colleagues who also do field studies. 18 And the main takeaway that I decided to 19 implement in waves 3 and 4 was for them to be 20 sufficiently different than waves 1 and 2 in 21 order to not generate whatever it was on wave 2 22 whether it was e-mail fatigue or suspicion or 23 anything that caused this attrition to occur in 24 wave 2, which was surprising. So I was 25 expecting attrition. And that is why the order	119 1 set as well. 2 But the -- the main idea was that 3 we're conditioning on those first responses and 4 then comparing what happens after Masterpiece to 5 couples of both types. And then the -- the 6 e-mails on waves 3 and 4 for same-sex and 7 opposite-sex couples may be different in tone 8 than the e-mails on waves 1 and 2, but they're 9 similar to identical in tone among themselves. 10 And note, I also noted in Dr. Yancey's rebuttal 11 report his question about dates. 12 So on waves 1 and 2, I was giving 13 the -- the businesses a month. So I was saying, 14 you know, the wedding will take place on month 15 X. And then again, to distinguish the e-mails 16 from wave 1 and 2 to go to wave 3 and 4, I moved 17 to a specific weekend, but it was the same for 18 same-sex and opposite-sex couples. I chose two 19 consecutive weekends long enough into the future 20 so it wouldn't matter and it was the same 21 treatment for both. 22 Q. I believe they were Fridays, 23 actually, if I'm not mistaken. 24 A. Well, I was talking about a -- a 25 week -- wedding weekend usually with
118 1 of the e-mails was first from same-sex couples 2 and then from opposite-sex couples because I 3 didn't want to conflate discrimination with 4 attrition, but I didn't expect it to be as 5 large. 6 So -- so -- what -- so there were 7 several changes that were implemented in waves 3 8 and 4. And the basic idea was that because on 9 waves 3 and 4, I implement randomization and I'm 10 only comparing the difference between same-sex 11 and opposite-sex couples. Then it's okay that 12 the -- the e-mails differ from waves 1 and 2 13 because I'm focused on what's happening after 14 Masterpiece to same-sex and opposite-sex 15 couples, and also have the control which we 16 might get to in a second. 17 And so the point is not to compare 18 directly the responses in waves 3 and 4 to waves 19 1 and 2, but to condition on wave 1 as you've 20 seen in the paper. So I focus on those -- on 21 those businesses that responded positively to 22 same-sex couples on wave 1, or those businesses 23 who responded positively to both types of 24 couples in waves 1 and 2, and also include very 25 transparently the analysis of the entire data	120 1 photographers. 2 Q. So a couple of things to unpack 3 from your answer there. As I understand you're 4 coding convention, a non-response was coded as 5 0, and a 0 in your binary calculation is a 6 negative response. 7 Correct? 8 A. That's right. 9 Q. And so if you look at waves 1 and 10 waves 2. 11 A. Uh-huh. (Witness answers in the 12 affirmative.) 13 Q. And I am looking in particular at 14 table OA4.4 -- 15 A. Yes. 16 Q. -- in -- which is page 19 of your 17 Online Appendix. If you looked only at the 18 three Masterpiece audit results, it appears that 19 it is more likely that wedding vendors will 20 respond favorably to same-sex weddings than 21 opposite-sex weddings based on the data. 22 A. Before the Masterpiece -- before 23 Masterpiece was decided, do you mean? 24 Q. Yes. 25 A. So can you rephrase your question

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121 1 to -- so I make sure to answer the right one? 2 Q. Of course. I'm looking at table 3 OA4.4. 4 A. Yeah. 5 Q. And only at the data reporting 6 results before the Masterpiece Cakeshop -- 7 Cakeshop decision. 8 A. Yep. 9 Q. The data indicates that wedding 10 vendors who were audited were more likely to 11 respond favorably to an inquiry about a same-sex 12 wedding than an inquiry about an opposite-sex 13 wedding? 14 A. That's right because same-sex 15 inquiries were sent in wave 1 and opposite-sex 16 inquiries were sent on wave 2 and wave 2 is when 17 we saw the ex -- the attrition. 18 Q. Correct. And the attrition, I 19 believe you are referring to as the -- the lack 20 of response because where the -- the -- the 21 greater non-response to the audit. 22 A. Correct. 23 Q. Because in wave 1, no-response 24 rates were 29.1 percent. In wave 2, the 25 non-response rate was 41.3 percent.	123 1 the non-response rate to opposite sex couples 2 decreased from 41.3 percent in waves 1 and 2 to 3 33.7 percent in waves 3 and 4. 4 Correct? 5 A. I think it's odd to say that 6 non-response decreased, but I think that I 7 understand what you say and that you are 8 correct. 9 Q. It may be a double negative, but I 10 think it's the -- the point here is the 11 non-response rate decreased. The response rate 12 increased. 13 A. Yes. Yes. 14 Q. Between the two -- the -- the two 15 groups of waves. And it's fair to say that when 16 you look at the significant decrease as you've 17 reported in positive response rate to same-sex 18 wedding inquiries from prior to Masterpiece to 19 after Masterpiece, it goes from 63.6 down to 20 49.2. 21 A. Uh-huh. (Witness answers in the 22 affirmative.) 23 Q. When you look at that decrease, the 24 vast majority of that can be accounted for in 25 the dramatically increased non-response rate to
122 1 A. Right. So let me explain or maybe 2 you want to ask a question. 3 Q. I am -- I'm -- I'm just -- I'm 4 pointing out that those are the numbers that are 5 reported in OA4.4. 6 Correct? 7 A. Yes, these -- those are the numbers 8 in the table. 9 Q. Right. Right. And, now, I'll let 10 you explain in a moment but I want to complete 11 my thought. And that is, if you turn to the 12 period after Masterpiece, what you see is a 13 reversal of the non-response rate to some 14 degree. Whereas the non-response rate prior to 15 Masterpiece for opposite-sex couples, which were 16 surveyed or audited in wave 2 is 41.3. Now the 17 non-response rate to same-sex couples is 41.1 18 percent, almost identical to the non-response 19 rate to opposite-sex couples in the first two 20 waves. 21 Correct? 22 A. Uh-huh. (Witness answers in the 23 affirmative.) 24 Q. Yes. Whereas the response rate to 25 opposite-sex couples increased from 40 the --	124 1 same-sex wedding inquiries. 2 A. Yes. But of course, I mean, you 3 can -- you choose to look at the half-empty side 4 of the glass, but we can ignore the half-empty 5 side of the glass and just analyze the half-full 6 side of the glass which is -- 7 Q. Can I -- I just stop you for a 8 second there? I don't understand what you mean 9 by the half-empty and the half-full. So you're 10 going to have to tell me what you're talking 11 about there. 12 A. Okay, may -- maybe now linguistic 13 differences separate us. So what I'm saying is 14 that you focus on the negative responses, but -- 15 and -- you -- I -- I'm sorry, you focus on the 16 no responses. So if we're focusing only on the 17 positive responses and disregard what's going on 18 which is not a positive response, simply looking 19 at positive responses, we see the change from 20 63.6 to 49.2 percent. 21 Q. And you would have to agree with me 22 though, that the -- the decrease, the 23 deterioration and the positive response rate 24 from pre-Masterpiece to post-Masterpiece -- 25 A. Uh-huh. (Witness answers in the

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125 1 affirmative.) 2 Q. -- most of those positive responses 3 landed in the no response category, correct? 4 Most of the decrease. 5 A. That not -- not necessarily the 6 case. So if you ask -- if you're looking at 7 within subject, so if you're looking at the same 8 subject over time, then we need to look at a 9 different table. If you are interested to know 10 across all subjects what happened, then you can 11 see the data here. There's also an increase in 12 negative, like explicitly negative response. 13 But it's true that across all 14 subjects, you see that the main form of 15 providing a negative response is to provide a no 16 response. So to not provide a response at all. 17 But when you are looking at within business 18 transitions, then you see something a little bit 19 different. So we can -- we can go to look at 20 this table if you're interested. 21 Q. And I am, but let me just, kind of, 22 finish my thought on this. You mentioned the -- 23 the no negative responses just a moment ago, the 24 actual negative responses. And it's true that 25 negative responses to inquiries into same-sex	127 1 I'm only looking at the data reported in this 2 table, not necessarily how it's normalized 3 below, but I'm looking at -- for opposite-sex 4 couples, negative response rates, actual 5 negative response rates, actual nos, we talked 6 about this earlier. Actual nos, I'm not going 7 to do your wedding, went from 2.9 to 4.8, which 8 is an increase of 1.9 points. 9 Correct? 10 A. So again, both types of no are nos. 11 The question of whether there comes another 12 politeness after the no is I call this as a 13 negative with a referral but it's still a no. 14 Q. Right. And -- so let's get to -- 15 A. Still an actual no in your terms. 16 Q. Yes, a negative with referral, 17 there was an increase from same -- same-sex 18 couples of 0.1 point and for opposite-sex at 1.2 19 points. 20 A. Uh-huh. (Witness answers in the 21 affirmative.) What you're looking at just so 22 make sure -- 23 Q. I'm looking at negative -- negative 24 with referral. 25 A. Negative with referral and would
126 1 weddings increased from 4.0 percent prior to 2 Masterpiece to 6.3 percent after Masterpiece. 3 I'm looking again at the table OA4.4. 4 Do you see that? 5 A. Right. Or you can look -- or you 6 can look as I look below table OA4.4 at combined 7 negative responses. So they increase from 10.28 8 percent negative response before Masterpiece to 9 16.5 percent negative responses after 10 Masterpiece. 11 Q. And that's -- that's a different 12 manipulation of the data here. But I -- I'm 13 also interested by the fact that negative 14 responses to opposite-sex inquiries also 15 increased -- 16 A. Slightly, yes. 17 Q. -- over that same time period. 18 Well, if you look at the opp- -- the negative 19 responses to same-sex inquiries went up 2.3 20 points. 21 A. From -- from about 10 percent to 22 about 13 percent. So about half the -- half the 23 growth of same-sex couples and that's 24 statistically significant. 25 Q. And again, I'm looking -- and again	128 1 you repeat the numbers? 2 Q. Sure. For opposite-sex, actual 3 negative responses with referrals went from 2.8 4 to 4.0 percentage points. 5 A. Uh-huh. (Witness answers in the 6 affirmative.) 7 Q. Whereas for same-sex couples, 3.1 8 to 3.4 percentage points. 9 A. Uh-huh. Yep. 10 Q. If you combine the increase, the 11 actual negative responses to opposite-sex 12 couples increased more percentage points than 13 actual negative responses to same-sex couples 14 from pre to post-Masterpiece. 15 A. Only if you're looking only at 16 negative with the referrals. But if you're 17 looking at the negative responses together, you 18 see the opposite picture. Which is that the 19 percentage of negative responses that same-sex 20 couples received increased 177 percent. The 21 increase of such responses for opposite-sex 22 couples. But as -- again, as I say below the 23 table, these are small numbers. I wouldn't base 24 any, you know, statistical inference only on 25 those numbers.

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129 1 Q. Okay. Let me stop for a moment to 2 make sure when you're saying those numbers, 3 we're referring specifically to the increases 4 that we -- the -- the -- the changes that we've 5 been discussing in actual negative responses or 6 actual negative responses with referrals, those 7 are small numbers is what you're saying? 8 A. So what I'm saying is that each of 9 those in separate are very small numbers. And 10 even when you combine them together, these are 11 not a lot of responses. But when you combine 12 them together, it does make more sense to try 13 and do the exercise that you're doing. 14 Q. Right. But even when you combine 15 them together -- 16 A. And that -- and that exercise is 17 reported below the table. 18 Q. Yes. And I -- and I have seen that 19 report where you note that you went from -- you 20 basically increased a raw total of 22 additional 21 negative responses from 66 to 88. 22 Correct? 23 A. Why -- and that's why I said the 24 numbers are small. But I think that we should 25 look at positive responses, but I'm consistent	131 1 that talk about negative and negative with 2 referral and I think we all agree, those are as 3 you said, small numbers? 4 A. Uh-huh. (Witness answers in the 5 affirmative.) 6 Q. The driver, the thing that drives 7 the fall in positive response rates to same-sex 8 wedding inquiries, pre-Masterpiece and 9 post-Masterpiece, is largely the increase in the 10 non-response rate to same-sex wedding inquiries, 11 pre-Masterpiece and post-Masterpiece. 12 Correct? 13 A. Right. Will I get to explain 14 anytime soon? 15 Q. Of course. I just -- I want to 16 make sure I understand something about that. 17 And you did code non-response as a -- as a 18 negative response. 19 Correct? 20 A. I did. 21 Q. Yes. And you agree it's possible 22 that a non-response from the vendor could have 23 been because the vendor was too busy to offer a 24 service requested, right? 25 A. Absolutely.
130 1 in treating together negative responses and 2 positive responses in both kinds of analysis. 3 So both -- both the cooperative and the positive 4 are analyzed together and the negatives and 5 negatives with referrals are analyzed together. 6 And the reason is really simple because the 7 numbers are so, so small when they're looked at 8 apart. 9 Q. Yes. And I -- I want to -- there's 10 something that I think is constructive here, you 11 -- we talked about this earlier in your 12 deposition, but one of the headlines -- one of 13 the headline findings that you identified as 14 part of your report was that willingness by 15 wedding vendors to serve same-sex couples fell 16 from 63.6 percent positive response rate to 49.2 17 percent positive response rate, pre and 18 post-Masterpiece. 19 Correct? Recall, we talked about 20 that? 21 A. Right. It's there in the 22 introduction where I summarized the findings. 23 Q. Absolutely. And we've been talking 24 about -- we've been, sort of, going 25 back-and-forth about the columns in table OA4.4	132 1 Q. And you agree that a non-response 2 could have meant the vendor was no longer 3 offering the wedding services requested? 4 A. Unless they were trying to be 5 astronauts. 6 Q. Unless they were trying to be 7 astronauts. It's possible that a non-response 8 from a vendor means the vendor was already 9 booked for the dates requested? 10 A. Absolutely. 11 Q. The same can mean the vendor just 12 simply is bad at checking his or her e-mail? 13 A. Absolutely. 14 Q. It could mean that the vendor 15 doesn't know how to use his or her computer? 16 A. Absolutely. But what you're 17 missing is an important assumption. 18 Q. Okay. Before I get to that 19 assumption though, you mean, the vendor could 20 have been on vacation when the e-mail arrived, 21 right? 22 A. Any number of reasons. And that's 23 why we never -- never infer anything particular 24 about a single vendor from a non -- no response. 25 We only look for in these kinds of studies,

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133 1 systematic differences across the categories 2 that we're looking at. 3 Q. And you've been -- you've been 4 pining to give me an explanation. So I -- you 5 may now, what -- what's -- what is the 6 explanation that you would like to provide? 7 A. Okay. Wonderful. So first, just 8 very generally about coding no response as 9 negative response. So I just want to say that 10 in asynchronous forms of communication, where 11 the receiver does not have to respond, 12 non-response is the most common way to provide a 13 negative response. And we've seen that and this 14 is the working assumption of almost any audit 15 correspondence field experiment done with 16 respect to discrimination or just, you know, 17 business service refusal, housing service 18 refusal, employers refusal, anything. So it's a 19 very, very, very common assumption. 20 And there -- there were studies 21 about that as well, and I cite some of them in 22 -- in -- in the paper. The -- the reason for 23 this is simple. Writing a negative response is 24 just time costly and sometimes awkward and the 25 easiest way is simply to ignore the inquiry at	135 1 against before Masterpiece, what I term in the 2 paper, reverse discrimination, or whether there 3 was another reason whether it was fatigue or 4 suspicion or frustration or forgetfulness or 5 something else. 6 Maybe something happened to 7 Gmail's, you know, some kind of transmission of 8 e-mails kind of issue in the cloud. So and it 9 just seems less likely and I -- I think you 10 could agree with me on that, that the -- that 11 something else happened rather than reverse 12 discrimination. So that there was some other 13 reason why businesses dropped communication to 14 larger degree in that wave, okay? And that's 15 actually quite common in field studies, and I 16 write about that as well. So it's common that 17 participants drop between waves even when 18 they're incentivized, so even when they're 19 getting paid to complete more waves. 20 So in order to find out what 21 happened, we might talk about it later. I did 22 several things, but the crucial difference 23 between waves 1 and 2 where we cannot in fair 24 with sufficient certainty, that no response 25 means discrimination because there was no
134 1 all. But of -- and of course, as I said before, 2 non-response can result from any sources beside 3 bias. And that's why we cannot interpret 4 non-reply by any individual vendor as a sign of 5 discrimination. 6 What we can do and do is to 7 investigate systematic differences. Okay. And 8 really that has been recognized by a long list 9 of influential works in economics from the 10 seminal work that I really talk a lot about in 11 the paper of Marianne Bertrand and Sendhil 12 Mullainathan about racial discrimination hiring 13 in 2003, to the work of Marianne Bertrand and 14 Nobel Laureate Esther Duflo in 2016, and really 15 many other studies. 16 Okay. But, and that's a crucial 17 point. It's more difficult to interpret 18 non-response when queries are not randomized, as 19 was the case in waves 1 and 2 of the studies 20 that took place before the Masterpiece decision. 21 So because wave 1 was solely from same-sex 22 couples and wave 2 was solely from opposite-sex 23 couples, it was not possible to infer with 24 sufficient certainty whether opposite-sex 25 couples were simply disfavored and discriminated	136 1 randomization. In waves 3 and 4 where there was 2 discrimination and all businesses are randomly 3 allocated, couple types, such that they are 4 randomly receiving either same-sex or 5 opposite-sex couple. There we can make 6 inference because there we have the strong, the 7 golden tool of social science is randomization. 8 So what -- so let -- let me try to 9 explain what it means. So when we're randomly 10 allocating couples to businesses, what it means 11 is that whatever reasons couple -- businesses 12 have for not responding, they should spread 13 randomly and therefore equally across couple 14 types. So all the things we said before, I'm 15 vacation, I just don't want to take this 16 business, I'm booked on that, I have 17 forgetfulness, fatigue, suspicion. Everything 18 should spread equally across couple types once 19 you implement randomization. 20 And this is what allows for an 21 inference that non-response in waves 3 and 4, is 22 part of the negative responses in a way that 23 allows us to infer that there is differential 24 treatment of couple types. Because all of these 25 reasons should have applied equally to both

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137 1 types of couples. But what I find is that it's 2 not the case. So in waves 3 and 4, opposite-sex 3 couples had a 19.6 percent of receiving no 4 response, whereas same-sex couples has almost 28 5 percent of not receiving a response. 6 So the chance of same-sex couples 7 to not receive a response is 40 -- 42 percent 8 higher, which is of course, highly statistically 9 significant. And as I said before, I mean 10 same-sex couples also experienced an increase in 11 explicitly negative responses from before to 12 after Masterpiece, which was larger than that of 13 opposite-sex couples. But again, that's a -- 14 that's a -- that's a side-finding for me. 15 Q. Thank you. So I -- I -- and I 16 think I understand what you're saying and what 17 your suggestion -- what you're suggesting here 18 is, I'm going to summarize, that waves 3 and 4, 19 because you conducted those waves in such a way 20 that inquiries were made on behalf of both 21 opposite and same-sex couples in both waves. 22 A. At the same time. 23 Q. And then you found in the data that 24 you described, I would argue that, that data 25 suggests at least as you would frame it, that	139 1 Q. Okay. 2 A. So the -- the reason why I can, 3 with confidence argue that there is defini- -- 4 differential treatment post-Masterpiece, is not 5 only due to the randomization of identities to 6 vendors post-Masterpiece, also because I'm 7 drawing on several different types of analysis 8 and they all come out the same way. 9 So whether I'm looking at all of 10 the businesses in the sample, I'm seeing this 11 effect. Whether I'm looking just the control 12 group businesses, so businesses that I didn't 13 e-mail before, I'm seeing this effect. And if 14 I'm focusing only on businesses that served 15 same-sex couples before Masterpiece, I'm seeing 16 this effect. And if I'm focusing on businesses 17 that were willing to serve both types of 18 couples, I'm seeing this effect. 19 And what I write in the paper is, 20 that when we're focusing on this group of 21 businesses that were willing to serve same-sex 22 couples before Masterpiece and we're examining 23 them, this is the relevant group to look at 24 because you wouldn't expect businesses that 25 weren't willing to serve same-sex couples before
138 1 data suggests, that post-Masterpiece same-sex 2 couples are more likely to experience a, what 3 you would term, negative response than 4 opposite-sex couples. 5 Is that a fair statement? 6 A. Or they're just less likely to 7 receive positive responses for -- 8 Q. And that's what I -- 9 A. -- a host of reasons -- for a host 10 of reasons, but those reasons don't spread 11 equally hence, we can infer differential 12 treatment. 13 Let me say something else about 14 that. So -- 15 Q. And before you do -- I didn't mean 16 to ask a question about that. I was just trying 17 to confirm what I understood. And so I mean, 18 you can -- you're certainly free to talk. I 19 just -- I don't want to extend the deposition 20 longer than it needs to be. So -- so let me -- 21 I'm just going to try to kind of get through 22 some of these -- these questions if I may, 23 because I know it's getting late in your end. 24 A. So I do want to complete something 25 else.	140 1 to begin to serve them after Masterpiece. I 2 would expect any effect to come from businesses 3 that prior to Masterpiece were serving 4 businesses and now will not. I do also examine 5 within business transitions, that's another 6 table. And I find -- 7 Q. That's a separate -- that's a 8 separate line of inquiry that I haven't gotten 9 to yet. And I -- and I want to tie a bow though 10 on this. And it is -- they're a couple of 11 elements to what you have been studying and 12 reporting. One is simply an examination of 13 whether a post-Masterpiece, in your view, a 14 same-sex couple is more or less likely to 15 receive a response or a favorable response to an 16 inquiry to -- to a wedding vendor, they are not 17 a same-sex couple. And you've -- you talked 18 about that. 19 So post-Masterpiece in your view, 20 the data that you received from your two from -- 21 from waves 3 and 4 demonstrates that a same-sex 22 couple is more likely to receive a negative or a 23 non-response than an opposite-sex couple. The 24 opinion you're offering in this case though, is 25 that the Masterpiece decision caused to -- there

141 1 to be an increase in the likelihood of 2 discrimination against same-sex couples. 3 A. Uh-huh. (Witness answers in the 4 affirmative.) 5 Q. And so whether or not you've 6 established, but the fact or existence of 7 differential treatment after Masterpiece, you 8 still need to be able to demonstrate that that 9 differential treatment grew or increased because 10 of the Masterpiece decision in order to reach 11 your opinion. 12 Would you agree with me on that? 13 A. And I do that -- and I do that by 14 focusing exactly by the way that I explained to 15 you now. 16 Q. Yes. And what I heard you say was 17 the way that you bridged that gap, the way that 18 you make the connection between the Masterpiece 19 Cakeshop and a hypothesis that discrimination 20 increased is through this same business change 21 or this -- this change in the attitude of 22 businesses that prior to Masterpiece were 23 willing to serve same-sex couples and that after 24 Masterpiece were not. And that is the link that 25 you draw.	143 1 A. Right. Or the way -- 2 Q. Okay. 3 A. -- you can -- you can also think 4 about it from a different perspective if it's 5 easier for you. So one analysis looks at all of 6 the businesses that responded positively in the 7 first wave. The second looks at all of the 8 businesses that responded positively to both 9 waves before Masterpiece. And then we also have 10 the control group. 11 And then the final analysis that I 12 was pointing to you to, is the within business 13 transitions. Where what I find is that it -- 14 that although transition -- transitions occur in 15 all directions, so you could either be a 16 business that before Masterpiece would not serve 17 an opposite-sex couple and now you do or you 18 would not serve a same-sex couple and now you do 19 or the other way around. So I document all of 20 the transitions and I show the same-sex couples 21 are much more likely to experience negative 22 transitions and that change is highly 23 statistically significant. 24 Q. In your business transitions study, 25 you did find that there were actually businesses
142 1 Is that fair? 2 A. That's one way. And then the 3 second way would be to focus on businesses that 4 served both same-sex and opposite-sex couples 5 before that. So that's another analysis that's 6 relegated for space reasons primarily to the 7 appendix, but also there. And then -- 8 Q. Let me stop you. I want to make 9 sure I understand the differential that you're 10 drawing. So when you have businesses that are 11 willing to serve same-sex couples, I'm thinking 12 of businesses that are willing to serve same-sex 13 couples, either by themselves or in addition to 14 opposite-sex couples. It sounds like you're 15 drawing a distinction between those two 16 different groups? 17 A. I do. 18 Q. Okay. And is that the -- is that 19 the what you call the keen? I guess the 20 generally keen groups, the ones that will serve 21 everyone no matter what? 22 A. That seems to be the case, yes. 23 Q. Okay. And then you have another 24 group that seems to be serving same-sex couples, 25 but not necessarily opposite-sex couples?	144 1 that would not serve same-sex couples or did not 2 respond to same-sex couples pre -- prior to 3 Masterpiece, that did agree to serve after 4 Masterpiece? 5 A. Right. 6 Q. There was a certain set that you 7 found. And do you -- do you infer from that 8 Masterpiece Cakeshop decision, in some instances 9 encouraged businesses to become more solicitous 10 in serving same-sex couples? 11 A. So I think that's an excellent 12 question. And in some of my studies I do 13 document an effect that I call the provocative 14 effect of law, which is sort of the opposite of 15 the expressive effect of law or you can simply 16 think about it as some kind of react and sort of 17 backlash. And I think what's interesting in 18 this study is that you see, well, we don't 19 absolutely know whether that's it or not. I 20 mean, in my experiments, in the more survey 21 attitudinal experiments, I can talk about 22 mechanisms with more confidence. So you see 23 that there is some of that here, but it's 24 dwarfed by the transition on the other side. 25 Q. All right. Thank you. You do note

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145 1 that there was tremendous attrition between 2 waves 1 and wave 2. You've noted that several 3 times in your report, and that certainly 4 complicated your ability to analyze changing 5 attitudes post-Masterpiece. Do you agree with 6 me though that in order to evaluate whether 7 Masterpiece, the Masterpiece decision was the 8 causative effect -- and I'm -- I'm going to set 9 this question up this way. 10 You have a business prior to 11 Masterpiece agreeing to serve or gave you a 12 positive response to a same-sex wedding inquiry. 13 A. Uh-huh. (Witness answers in the 14 affirmative.) 15 Q. After Masterpiece, that same 16 business gives a negative response or no 17 response to a same-sex inquiry. So this is one 18 of those businesses that you've been describing 19 as it changed it's mind, so to speak. It went 20 from a 1 on the binary scale to a 0 on the 21 binary scale, right? And you have the 22 Masterpiece Cakeshop decision intervening 23 between those two positions. You would agree 24 with me that it's important in order to isolate 25 the Masterpiece Cakeshop decision as the	147 1 with respect to same-sex couples. I'm just 2 looking at how they behave to -- to identical 3 inquiries from opposite-sex and same-sex couples 4 after the decision is rendered. And that's 5 crucial. 6 Q. Right. But -- but your attrition 7 rate in wave 2 really compromises your ability 8 to understand how those same businesses reacted 9 to both opposite-sex and same-sex couples 10 together prior to Masterpiece. Because you've 11 said that many times. 12 A. Right. So I concede in the paper 13 that I'm not able to know what is the extent of 14 discrimination towards same-sex couples if there 15 is any before Masterpiece. So that I'm not able 16 to know. But by focusing on the businesses that 17 were willing to serve and seeing whether they 18 distinguish between couples of both types after, 19 and these were all businesses that were 100 20 percent willing to serve same-sex couples 21 before. 22 And by the way, I mean, going back 23 to your first line of questions on this, the 24 e-mails that they received afterwards were more 25 business-friendly than the e-mails they received
146 1 causative agent for that change, that you would 2 need to be asking that business virtually the 3 same question, both before and after 4 Masterpiece? 5 A. No, I would not agree with you. 6 Q. Okay. 7 A. And -- and that's -- that's my -- 8 that goes back to my explanation from before. 9 Okay. So what I'm doing is I'm not looking only 10 at the change from, did you serve same-sex 11 couples before to would you serve same-sex 12 couples now. If I would do that, my estimates 13 from discrimination would have been much larger, 14 but I think less precise. Exactly because of 15 the kind of line of questions that you were 16 concerned about of the changes that occurred 17 from before to after Masterpiece. So this is 18 not what I'm doing. 19 What I'm doing is I'm looking at 20 all of the businesses that prior to Masterpiece 21 were willing to provide service to same-sex 22 couples. And what I'm doing now is I'm 23 comparing how they behave to same-sex versus 24 opposite-sex couples after Masterpiece. So I'm 25 not looking at only, at what -- how they changed	148 1 before. They were more precise, they were more 2 action-oriented, they were more specific in 3 terms of date. They -- the profiles had 4 pictures. Lots of things changed for the 5 better. Yet, they were dis -- discriminating 6 now against same-sex couples, which they didn't 7 do before just based on the fact that they were 8 willing to serve them before. 9 Q. Okay. And now, you would agree 10 with me though, that if you introduced in the 11 e-mail communication before Masterpiece versus 12 after, if you introduced a new variable in the 13 e-mail communication after Masterpiece to a 14 vendor that previously was willing to serve the 15 same-sex couple, that that new variable that you 16 introduced could contaminate your results and 17 make them less reliable in measuring the impact 18 of the Masterpiece decision on that vendors 19 willingness to serve? 20 A. No, only if I would have introduced 21 the variable differentially to both couple 22 types, but I didn't do that. 23 Q. Okay. Well, I -- and that's where 24 I want to focus for just a moment in your Online 25 Appendix.

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149 1 A. Yes. 2 Q. You have your scripts. 3 A. Uh-huh. (Witness answers in the 4 affirmative.) Which ones are you looking at? 5 Q. Well, let me just look -- let me 6 kind of scroll up to the top here. So in the 7 first wave, which was a wave that went to 8 same-sex couples. 9 A. Uh-huh. (Witness answers in the 10 affirmative.) 11 Q. I'm looking at the very first one 12 sent to bakeries. You note that John and Adam 13 are engaged to be married on January 2019. I 14 assume that mean -- that was meant -- meant to 15 say in January 2019? 16 A. Uh-huh. (Witness answers in the 17 affirmative.) 18 Q. Okay. So you were asking for an 19 indication from bakeries as to whether they 20 could provide service in the month of January 21 2019. 22 Correct? 23 A. Yes. 24 Q. Okay. And if you go just to the 25 photographer e-mail which is right below that,	151 1 believe is 29.1 percent for same-sex couples in 2 the wave 1 inquiries? 3 A. You are referring to table OA4.4 4 again, right? 5 Q. I am. 6 A. Okay. 7 Q. And so when you move to waves 3 and 8 waves 4, you changed the date parameters? 9 A. Of course, I did. 10 Q. For the -- for the same-sex 11 couples. But it's not -- but -- it's not just 12 changing the month. You actually gave a 13 specific date for the bakery services of 14 February 22nd, which is a Friday, 2019. 15 A. Uh-huh. (Witness answers in the 16 affirmative.) 17 Q. For photographers, you gave two 18 dates, May 17th or 18th of 2019. 19 A. Yeah. 20 Q. And for florists it was February 21 22nd, right? 22 A. Yeah. That's because photographers 23 work on weekends. So they always price for 24 weekends. 25 Q. Sure. And in your report you note
150 1 if -- the date is moved to April of 2019. 2 Correct? 3 A. That's because of the footnote that 4 we were talking about before -- 5 Q. Certainly. 6 A. -- of the time it takes to contact 7 vendors of different businesses. 8 Q. Right. Right. Florists, January 9 2019. Again because of the footnote? 10 A. Yes. 11 Q. And then in the same -- in the wave 12 2 e-mails, you follow roughly the same pattern. 13 You give months of availability, January for 14 bakers, April for photographers, and again 15 January for florists. 16 Do you see that? 17 A. Yeah. 18 Q. And as you record in your Table 4.4 19 responses to the same-sex couple inquires. You 20 -- you -- you only -- you have -- you had less 21 than 30 percent non-response rate to the 22 inquiries that you sent out, the ones that we 23 just reviewed, right? 24 A. Say that again. 25 Q. You -- your non-response rate I	152 1 that wedding vendors are cognizant of the fact 2 that couples who have already established a date 3 for their wedding are very unlikely to change 4 that date for a vendor. 5 A. Uh-huh. (Witness answers in the 6 affirmative.) 7 Q. Is that right? 8 A. Uh-huh. (Witness answers in the 9 affirmative.) 10 Q. Okay. And you would expect that if 11 a wedding vendor received an e-mail inquiring 12 about a specific date -- 13 A. Uh-huh. (Witness answers in the 14 affirmative.) 15 Q. -- that the wedding vendor was 16 already booked for, that the vendor would either 17 say, no, I can't do it or potentially give no 18 response whatsoever. 19 Correct? 20 A. Uh-huh. (Witness answers in the 21 affirmative.) 22 Q. Those are reasonable assumptions, 23 are they not? 24 A. I think so far you're just 25 restating what I wrote if I'm not mistaken.

153 1 Q. Uh-huh. And it's also fair to say 2 that if a wedding vendor declines to provide 3 service or provides no response to an e-mail, 4 requesting service on a date for which the 5 vendor is booked already, that the decline or 6 the non-response has nothing to do with animus 7 or discrimination based on sexual orientation? 8 A. Sure. 9 Q. And -- 10 A. But again as you scroll, I want to 11 explain. 12 Q. Okay, then. 13 A. Okay. 14 Q. You can explain. I just want -- 15 there's no question pending right now. 16 A. Okay. So let me -- let me explain 17 why this change is actually necessary to 18 implement substantive equality between waves 1 19 and 2 and waves 3 and 4. So the timing of the 20 prospective wedding that was noted in the e-mail 21 was determined uniformly within wave and for 22 each couple based on the same market norms. So 23 when you were contacting vendors a month 24 afterwards, if you give them exactly the same 25 day that you gave them a month before that, you	155 1 that -- 2 Q. I wasn't asking you about that 3 actually. 4 A. Okay. So I won't answer. 5 Q. Okay. I do want to know one thing 6 though. You would agree with me that it's more 7 likely you're going to get and -- it's more 8 likely that a wedding photographer is going to 9 be unable to provide service if you only give 10 that person one date to provide the service 11 versus 30 or 31 dates? 12 A. But I did that for both same-sex 13 and opposite-sex couples. So whatever 14 unavailability patterns you'd should -- you'd 15 seen, they should have distributed equally 16 across couple types unless there is 17 discrimination. 18 Q. And for waves 3 and waves 4? 19 A. Yes, that's correct. 20 Q. Yes. And waves 3 and waves 4 both 21 were sent out after Masterpiece decision was 22 rendered? 23 A. That's correct. 24 Q. Right. But it was certainly more 25 likely that when wedding vendors contacted prior
154 1 actually give them less time ahead. So you're 2 giving them less than a fair chance of being 3 able to provide you the service. 4 So whenever you contact vendors, if 5 you want to keep being within the market norm, 6 of contact them, say 11 months ahead -- away for 7 photographers and eight months away for a bakers 8 and florists, you need to change the date. So 9 that's the first thing. Why the date had to be 10 changed after Masterpiece. It would have been 11 necessary either way. 12 In addition, I moved from 13 specifying a month to specifying a weekend or a 14 date specifically. And the reason for that was, 15 as I noted before, that I need to create some 16 change between e-mails 1 and 2 and e-mails 3 and 17 4 that would make them seem sufficiently 18 different so as not to arise e-mail fatigue, 19 suspicion, or all of the other kinds of things 20 that I didn't want to raise. And giving a 21 specific weekend, which again was identical 22 substantively between the two couples, same-sex 23 couple and opposite sex couple after Masterpiece 24 allowed me to achieve that. Now you were asking 25 about how I coded those responses. So I coded	156 1 to Masterpiece, would have had more availability 2 in response to the e-mails that were sent 3 because you gave them more dates to choose from? 4 A. That's possible. But that means 5 that my group is just larger and I'm, you know, 6 more conservative in my test. 7 Q. Right. But you -- you just don't 8 know how to compare the response rates to same 9 and opposite-sex couples pre-Masterpiece because 10 you had such tremendous attrition from waves 1 11 and wave 2? 12 A. So again, what I'm saying in the 13 paper is that I'm not attempting to compare them 14 and to discern anything about discrimination 15 before Masterpiece. 16 Q. Right. 17 A. My objective is -- my objective 18 from the beginning, was to measure whether 19 Masterpiece changed anything. So I was obv- -- 20 and obviously, I would have wanted to be able to 21 also say something about what happened before 22 Masterpiece because there is really no data on 23 that before. 24 Q. Right. 25 A. But my main objective --

157 1 Q. So you mainly can say -- 2 A. But my main objective was to 3 understand whether there was change. And 4 whether there was change that I can still and do 5 examine and record. 6 Q. Certainly there was change. But at 7 the same -- you know, I'll leave it there. 8 A. But I want to emphasize my coding 9 policy was identical. So if someone offered a 10 date that -- that was outside of the month, in 11 waves 1 and 2, they were coded exactly the same 12 as if they would've code -- asked for a 13 different wet -- weekend in waves 3 and 4. 14 Q. Right. So if the -- if the wedding 15 vendor in wave 1 said, I can't do it in January 16 but I can do it in February. 17 A. Yeah. That would have been the 18 same. Yeah. 19 Q. Sure. Is it significant to your 20 analysis that the Texas RFRA exempts local 21 anti-discrimination laws? 22 A. It is. I would have written a 23 different -- 24 Q. And why is that? 25 A. -- I would have written a different	159 1 I wanted to. That's what I'm saying. 2 Q. Cool. I -- I rarely have the 3 opportunity to have a witness who's locked in 4 place. And so this might be one of those -- 5 A. Your opportunity. 6 Q. This is my opportunity apparently. 7 So something that was critical I 8 think to your report was, trying to draw a 9 connection between Louisville, Kentucky and the 10 four states that were studied in the Masterpiece 11 experiment. 12 Is that fair? 13 A. Yeah. 14 Q. Okay. And I know we've already 15 covered this, but just to reset ourselves, you 16 did not actually study wedding vendors in the 17 state of Kentucky? 18 A. That's correct. 19 Q. As part of the Masterpiece 20 experiment? 21 A. That's correct. 22 Q. Nor did you conduct a follow-on 23 study for purposes of this report? 24 A. That's correct. 25 Q. Sorry. I'm getting e-mails from --
158 1 report otherwise. 2 Q. I see. My understanding about the 3 Louisville mark- -- is it that the -- this kind 4 of gets into a critical question that I want to 5 ask you. And -- you can take a look here right 6 now. And I'm going to -- to do something that 7 lawyers are never supposed to do, but I'm going 8 to do it as a courtesy and that is to tell you 9 I'm about 65 percent through my deposition, 10 possibly 70. So I say that to you for timing 11 purposes because I know it is late there. So I 12 certainly would, you know, be willing to keep 13 going and try to wrap this thing up this evening 14 or we can come back for maybe an hour or two 15 tomorrow. I'll -- I'll give -- I'll -- I'll 16 make it your choice. I just -- I was noticing 17 that it is probably 11:36 your time, right? 18 A. That's correct. 19 Q. So -- 20 A. So I -- I think probably the 21 university locked me in here in any event and 22 I'm still up. So let's try to go on until it's 23 enough for today. 24 Q. Okay. 25 A. I'm not sure that I can get out if	160 1 I can -- okay. It looks like you did try to -- 2 or did you -- did you compare Kentucky's GDP 3 against the GDPs in Indiana, Texas, North 4 Carolina, and in Iowa? 5 A. Are you looking at specific -- 6 specific text in my table? No. You are looking 7 at -- at the table on page 9, right? 8 Q. Yes. Of your -- again, we're going 9 to say page 9 of your report which is paragraph 10 number 22. 11 A. Yeah. Yeah. No. I didn't look in 12 Kentucky's GDP. 13 Q. Right. 14 A. If I -- if I had it would've been 15 reported here. 16 Q. Right. Right. And I just wanted 17 to make sure I wasn't -- it wasn't something 18 that you did outside of the table or something 19 that wasn't in your report. 20 A. No. You're right though that it's 21 an omission. 22 Q. Okay. Sure. But you did go ahead 23 and pull the data from the Pugh study for 24 Kentucky and record that in the table? 25 A. Right. And the reason why I

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161 1 focused on the Pugh data is because I was really 2 interested in understanding religiosity. 3 Q. Yes. Yes. Did you conduct a -- 4 and I think -- and I'm asking this because I 5 don't see it reported in the table. Did you 6 conduct an evaluation of the religious density 7 in the Louisville metropolitan area for purposes 8 of your report? 9 A. I tried to, but I didn't succeeded 10 in -- in actually getting the data properly. 11 Q. Okay. And was that -- did you try 12 to use the same sources that you used for 13 purposes of your Masterpiece study? 14 A. I did. 15 Q. Okay. And that those sources 16 didn't provide you with satisfactory data to 17 conduct a religious density evaluation of 18 Louisville? 19 A. Yeah. 20 Q. Okay. Louisville has a local 21 anti-discrimination law, correct? I mean 22 obviously it does, but I'm just asking. 23 A. That's what I've heard, have you? 24 Q. You've heard about that? I -- 25 we've heard about that too. And Kentucky has a	163 1 included those additional cities in that 2 analysis of -- of the immunity of these types of 3 jurisdictions, to impacts by Masterpiece. 4 A. Sure. So if I understand your 5 question correctly, if not, just, you know, 6 correct me. So I want to make sure that you -- 7 you're not somehow confused by the table. The 8 table shows only Houston, Dallas, simply for -- 9 simply for exemplifying and demonstrating what 10 this might look like at the city level. But the 11 analysis is run on all of the counties that by 12 the time of the experiment had these kinds of 13 local laws. Not just Dallas, and Houston, also 14 in Indiana. 15 Q. Right. And -- yes, if I -- I'm 16 looking at jurisdictions in Indiana, you have 17 Indianapolis, Fort Wayne, Evansville, 18 Bloomington, Muncie, South Bend, and Terre 19 Haute? 20 A. Right. 21 Q. And I believe in Texas you were 22 pointing to Dallas, San Antonio, Houston, 23 El Paso, Plano, and Fort Worth. I'm looking at 24 footnote 1 of 2 in your Harvard step, in -- in 25 -- in the Harvard piece?
162 1 RFRA statute? 2 A. That's right. 3 Q. Yes. You recorded in your report 4 that there was one type of jurisdiction that 5 seemingly did not experience significant or any 6 statistically significant change in 7 discrimination toward same-sex weddings 8 post-Masterpiece. And that was jurisdictions 9 that had both a RFRA and an anti-discrimination 10 law. 11 Do you recall that? 12 A. That's right. Yes. 13 Q. And I think it's been noted between 14 78 and 76 percent willingness to serve -- or 15 right around there. And I assume that that 16 really comes down to the cities of Houston and 17 Dallas, in Texas. Is that -- were those the two 18 jurisdictions where you -- you saw that effect? 19 A. No. So the list is longer. Let me 20 find the footnote. So footnotes 13 and 14 in 21 the JLS piece give you all of the counties in 22 both Dallas and Indiana. 23 Q. And I was -- I was curious if those 24 were -- if you included those additional cities, 25 you -- you anticipated my next question. If you	164 1 A. Okay. I'm looking at footnotes 13 2 in the JLS piece, but I -- it's -- it's the same 3 -- it's the same information. 4 Q. Yes, it is, same information. 5 Okay. And so in those particular 6 jurisdictions, the ones that we just discussed, 7 those cities, you did not witness or record a 8 statistically meaningful increase in the level 9 of discrimination against same-sex couples in 10 response to Masterpiece. 11 Is that fair? 12 A. That's correct. That's correct. 13 Q. Okay. And do you attribute that to 14 -- elsewhere in your report, you have a chart 15 that shows religious density versus likelihood 16 of discrimination. And you show that as the 17 density increases, you report of a greater 18 likelihood of discrimination. So do you 19 attribute the immunity of these jurisdictions to 20 the Masterpiece decision in effect to a lesser 21 religious density, or is it something else? 22 A. No, I don't. I want to take you to 23 in the Online Appendix. Let me just find the 24 table. So essentially, as often is the case 25 with empirical pieces the journal wants you to

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165 1 provide a very brief report, and so all of the 2 are meaningful analyses is relegated to the 3 appendix. So a lot is going on there. Okay, so 4 let's go to table OA4.8 on page 28 of the Online 5 Appendix. Are you there? 6 Q. I'm -- I am there. 7 A. Okay. So I hope it's okay to guide 8 you through this regression table. 9 Q. Of course. 10 A. Okay. It's just to answer your 11 question, right? So what the regression table 12 means is that you have here those columns that 13 have numbers one to five, these are all -- each 14 of them stand for a different model. And then 15 wherever you see a coefficient, then that means 16 that that variable was included in the model. 17 And so what you see here essentially is, you see 18 the one table where you have two different 19 analysis. One of the evangelical congregations 20 density and the religious congregations density, 21 that's all groups like no matter what religion 22 you are. 23 And then you see -- so you -- you 24 can see for each of these models what happens 25 when you introduce evangelical congregation	167 1 enough that it can even overcome the impact of 2 religious density, even religious density 3 associated with evangelicals? 4 A. Well, I wouldn't say -- I wouldn't 5 used the word overcome because that -- 6 Q. Negate, perhaps? 7 A. -- that assumes some kind of maybe 8 causal relationship. Which as we said before, I 9 can't assume when we're talking about those 10 differences in legislation. But it does tell 11 you that this -- this is -- the effect of the 12 AD-RFRA interaction does not come from any 13 association between legal regime and religiosity 14 rather than these are two independent effects 15 that there is, if you will, there is enough 16 statistical room for both of them to be highly 17 statistically significant and powerful. And -- 18 and -- and that's it. 19 And I want to qualify a little bit 20 because sometimes analysis relates to one of the 21 points in the rebuttal report. It's actually -- 22 it's actually not advised to add too many 23 factors into -- into statistical analysis. 24 Because some -- sometimes when you actually add 25 too many factors, then you begin to encounter
166 1 density versus not, and you see that in all 2 models, the effect of both AD, they're up there 3 on the top and the interaction between AD and 4 RFRA, which is the AD and then the two dots RFRA 5 semicolon. This is a interaction, it remains 6 statistically significant. 7 So even when you have evangelical 8 congregations density or when you have religious 9 congregations density in the model. And even 10 when you include the interaction between 11 same-sex and evangelical density, which is 12 highly statistically significant. Even in those 13 models, you still see a statistically 14 significant effect for the interaction of AD and 15 RFRA, which means that these variables, this 16 specific interaction remains statistically 17 significant even after you control for the 18 effect of religious density. 19 Was that clear? I can -- I can 20 clarify if not. 21 Q. I'll -- I'll repeat what I think I 22 heard. And here's what I understood, which is 23 that the -- the variables of how -- the variable 24 of having both an anti-discrimination law and a 25 RFRA law in place is sufficiently powerful	168 1 another problem of multicollinearity between 2 variables. And sometimes that also just alludes 3 an obscures interpretation of coefficients. 4 I can give you examples, but you 5 didn't ask me. So I wouldn't for now. But -- 6 but -- but there is a point, what I want to say 7 is there is a limit to how many interactions you 8 can test in a regression model. And you know, 9 and that's -- that's a limit there. So I -- I 10 cannot tell you to the full extent that I would 11 have wanted to what's the full relationship 12 between these variables. What I can tell you 13 that they both have independent and strong 14 effects. 15 Q. Got it. And so and this is going 16 to be -- and part of this is -- is owing to just 17 the fact that I live in Texas, but many of the 18 metropolitan areas that you list, I'm going to 19 get back to the footnote in the report. 20 A. Yep. 21 Q. So Dallas, San Antonio, Austin, 22 El Paso, and Fort Worth, you have five of the 23 six largest metropolitan areas in state right 24 there -- 25 A. Uh-huh. (Witness answers in the

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169 1 affirmative.) 2 Q. -- comprising well over half of the 3 state's population. 4 I don't -- I've never lived in 5 Indiana, but I'm assuming that when you add 6 Indianapolis, Fort Wayne, Bloomington, Muncie, 7 South Bend, and Terre Haute together, you're 8 getting a similar density of population in 9 Indiana as well? 10 A. Yes. 11 Q. What this tells me -- and these are 12 the jurisdictions that have that feature of both 13 the RFRA and an anti-discrimination law that 14 you've described as being significant and 15 meaningful. It looks to me like the areas that 16 were -- 17 A. I want to correct you, they were 18 statistically insignificant. But that's 19 meaningful, potentially. I mean what was 20 meaningful, what -- what -- and what you're 21 aiming at is that there was no statistically 22 significant effect of Masterpiece in those 23 areas. No, they were not significant. 24 Q. Thank you. Yes, you're -- you're 25 correcting my -- my elementary statistics	171 1 would not support that this is what drives this 2 particular effect. 3 Q. Combination of anti-discrimination 4 law plus -- plus a RFRA simultaneous -- 5 A. And as I write in the paper, I mean 6 this -- I treat this as a preliminary finding 7 that's super interesting and requires more 8 research because maybe we have some kind of 9 answer there to these conflicts. But what -- 10 what makes, what -- what needs to curb our 11 potential enthusiasm is that there are -- these 12 are very specific types of RFRA's. 13 So in my preliminary research when 14 I was trying to select states for the 15 Masterpiece study, I -- I -- I just found how 16 diverse the RFRA world is. So the RFRA of a 17 universe is highly diverse. Some states like 18 Kentucky did something very similar to the 19 federal RFRA. But then you have states like 20 Texas and Indiana that created this highly 21 detailed law with lots of, you know, 22 limitations. And specifically the carve-out for 23 local anti-discrimination laws. 24 And that -- that is a limitation of 25 my study that it does not have all of those
170 1 knowledge that I gained about 25 years ago. So 2 that's very -- thank you for that. 3 It looks to me though like the 4 message -- the takeaway if I'm just looking at 5 it simplistically, you know, simplistic while 6 you're here. The takeaway is major metropolitan 7 areas are -- that -- that have those RFRA and 8 anti-discrimination laws are going to be 9 somewhat immune to the effect of Masterpiece? 10 A. So I tested that more directly. So 11 I have an analysis in my regression tables where 12 I limit the analysis only to large urban areas. 13 So only places with more than 80,000 in the 14 population. And I still find the Masterpiece 15 effect. 16 So what you forget, in a sense, is 17 that there are big metropolitan areas also in 18 the other states that I'm examining. And also 19 there are big metropolitan areas also in Texas 20 that are not included in the -- in the AD 21 category. 22 So I wouldn't -- I wouldn't say 23 with confidence what you just say. I would say 24 that's -- that's a nice hypothesis. But what I 25 have currently, the evidence I currently have	172 1 types of RFRA's inside because it would have been 2 impossible to compare states in all of these 3 other dimensions. And therefore, this 4 particular quote/unquote immunity, that I see 5 there, I -- I -- I don't think that I can then 6 transfer it to states or cities that have RFRA's 7 that do not include these carve-outs. 8 And by the way, I do not include a 9 lot of other things. So there are lots of 10 limitations and conditions to using your RFRA 11 rights in -- in Texas and Indiana that are 12 simply not there in Kentucky. It's much simpler 13 to use your RFRA rights in -- in Kentucky, so it 14 seems. 15 Q. And so you didn't feel comfortable 16 for the reasons that you just described, you 17 didn't feel comfortable applying the finding to 18 Louisville, that jurisdictions with 19 anti-discrimination laws and RFRA laws generally 20 don't experience an impact from Masterpiece? 21 A. Right. Right, if Chelsey Nelson 22 would have been based in Austin, the report 23 would have looked very differently. 24 Q. And that's because the legal regime 25 in Austin is, you believe it's different than

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173 1 the legal regime in Louisville? 2 A. Yeah. 3 Q. Well, I can tell you Austin is 4 certainly different than Louisville in a lot of 5 ways. If you've ever been to Austin, you'll 6 know Keep Austin weird is the mantra here, so... 7 MS. HINKLE: I have to correct you, 8 but that's also a mantra here. 9 MR. BANGERT: In Louisville? 10 MS. HINKLE: Yeah. There -- 11 MR. BANGERT: Oh no. 12 MS. HINKLE: Louisville has a lot 13 of, People are weird, bumper stickers. You see 14 all the time. 15 MR. BANGERT: Oh my goodness. 16 MS. HINKLE: Austin doesn't own 17 that phrase. 18 THE WITNESS: I -- I -- I should 19 update my report now. 20 MR. BANGERT: I'm -- I'm -- I'm 21 deeply troubled now. Well, and I -- at least 22 you're not in Tennessee in that case. 23 Tennesseans insufferably claim they founded 24 Texas. And so we -- we take great umbrage in 25 that, but that's it. That's a fight for a	175 1 THE COURT REPORTER: We're back on 2 the record. 3 4 * * * 5 EXAMINATION 6 BY MR. BANGERT: 7 Q. Great. Professor, thanks for 8 coming back. I know it's late there. We'll get 9 you out here as fast as we can. Just a few more 10 questions to clean up the record. 11 One thing that you noted in your 12 report was that you're unable to replicate the 13 Masterpiece study today because the effects of 14 Masterpiece will now be drowned out by other 15 intervening events. 16 Is that fair? 17 A. Yes, it is. 18 Q. And so you don't -- you don't know 19 and you really can't measure how persistent the 20 effect of the Masterpiece decision is or was on 21 any particular event? 22 A. That's correct. But -- but I can 23 learn from other studies in answer to these 24 questions. 25 Q. Okay. But in terms of the study
174 1 different day. 2 I'm just kind of checking off my 3 outline here, Doctor -- Professor, because I 4 want to make sure covering things, but I'm also 5 trying to be respectful of your time. Okay. 6 Okay. Maybe a few things that I 7 can jump over. 8 Professor, can I beg your 9 indulgence? I -- I may be able to wrap this up 10 in relatively quick fashion, but if we can take 11 about a five-minute break, I just want to 12 consult my outline and make sure I've -- I've 13 covered what I needed to cover and there may be 14 a little bit more but I -- let me see if I can 15 -- 16 THE WITNESS: Yeah. Yeah. Let's 17 let's then go resume on like on 12:01 my time, 18 which is... 19 MR. BANGERT: Yes, that's perfectly 20 fine. All right, I'll see you in five minutes. 21 THE WITNESS: Okay. 22 23 * * * 24 (OFF THE RECORD.) 25 * * *	176 1 that you conducted, beyond the snapshot in time 2 that you took in June of 2018, we don't know 3 what those attitudes looked like in July or 4 August of 2019 -- 2018 or thereafter? 5 A. Right. So we do have -- I can talk 6 about one indication in July that's consistent, 7 not from my study from another study. And the 8 reason why I wasn't able to continue testing was 9 the three of the four states, legislation was 10 introduced with respect to the expansion of 11 religious liberty and religious exemptions. And 12 that obviously created a conflation where even 13 if those legislation where provoked or 14 encouraged by Masterpiece itself, as at least 15 some legislators were saying they were, then we 16 can never know whether it's the -- the 17 legislation that influenced or continues to 18 influence or the poor decision itself. 19 Q. And so based on that, you -- you 20 can't tell me based on your study whether 21 Masterpiece had a permanent effect on attitudes 22 of vendors toward their willingness to serve 23 same-sex couples? 24 A. No. But what I can tell you is the 25 following: First, the previous studies of

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177 1 Supreme Court decisions did find that the effect 2 that the Court has is prolonged, the effect that 3 the Court has on the public. So for example, 4 Katerina Linos and Kimberly Twist that I 5 referred to before, their 2016 paper, they 6 studied the effect of the Court's two rulings 7 and the, Show Me Your Papers Act and the -- what 8 was there then the first Affordable Care Act 9 constitutional challenge. So that was back in 10 2012. 11 And their study was identical to my 12 own in the sense that they tested the effect 13 shortly before and after the decision. But 14 their study wasn't behavior, it was about 15 attitudes. So they were trying to understand 16 whether the -- whether the public changes in 17 terms of the level of support it has for those 18 different policies that were contested in court 19 and where affirmed by the Court. 20 But they also had access to a 21 public opinion survey that collected information 22 on public attitudes both before those core 23 decisions and also nine months after the 24 decision. And then they found that the effect 25 that they discovered, which was consistent with	179 1 time trend, but it reduces at sharper rates. It 2 states that legalized gay weddings and even 3 after this -- this reduction, there was still 4 considerable level of bias according to their 5 findings. But these data do show that the 6 effect of legal changes can be substantial and 7 prolonged. 8 And then the final study that I 9 want to talk about is a study that was 10 published. So I was actually exposed to it 11 after I published my own work. And that's a 12 study that was published in JAMA Psychiatry. So 13 that's a medical journal, a very good one by a 14 team of researchers. And that's a utilized data 15 from 2014 through 2016 from adults, aged 18 to 16 64 years, in three states, that implemented laws 17 that permitted denial of services to same-sex 18 couples. 19 So in 2015, both Utah, Michigan, 20 and North Carolina each enacted some kind of law 21 that is essentially a religious exemption law in 22 different fields by the way. And they had six 23 nearby control states which were Idaho and 24 Nevada for Utah, Ohio, and Indiana for Michigan, 25 and Virginia and Delaware for North Carolina.
178 1 my own finding in the sense of the Supreme Court 2 had an effect in the expected direction such 3 that the public shifted in the direction of the 4 majority ruling. So they find that the effect 5 persisted nine months after the decision. And 6 in fact, it even slightly increased. If you're 7 interested in a specific reference, that's page 8 238 of their study. 9 And then in another study that was 10 published in the Proceedings of the National 11 Association of Science, PNAS, one of the best 12 journals in the world, a team of researchers 13 studied whether the gradual legalization of 14 same-sex marriage in the United States by both 15 court decisions and legislative act reduced 16 implicit and explicit bias towards same-sex 17 couples. 18 So the researchers there had one 19 million responses over 12 year period. And 20 their results across five different 21 operationalizations, consistently find that 22 marriage legalization, both at the level of the 23 court and the legislation, reduces bias. And 24 they find specifically that anti-gay's bias 25 reduces over time in all state. So there is a	180 1 And what they find is -- so that's like a three 2 differences analysis. So it's both before and 3 after. That's the first difference. Treatment 4 versus controlled states. That's a second 5 difference. And comparing sexual minority 6 adults with heterosexual adults. 7 And they find that adults that -- 8 individuals that identify as sexual minority, 9 they increase in their mental distress. So they 10 report increased levels of mental distress in 11 the year after their states enacted laws 12 permitting denials of service to same-sex 13 couples as compared with both the level of 14 mental distress before the law was enacted and 15 the level of mental distressing in the 16 controlling states and the level of mental 17 distress among heterosexual couples. So in all 18 three levels. 19 And this finding can be thought of 20 as particularly worrying because what the 21 researchers found was that sexual minority 22 adults had already higher baseline levels of 23 mental distress than heterosexual adults, and 24 they don't find the same sense of change amongst 25 heterosexual adults. And to refer to the -- to

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181 1 the level -- to the -- to the long-term 2 documentation. So this change occurred. They 3 find -- so they find it 7 to 18 months after the 4 implementation of these laws. So that's a 5 considerable length of time after legislation. 6 Q. Sure. And so all the studies that 7 you just mentioned, none of them were studying 8 the wedding market in particular, were they? 9 A. No. That's correct. They were -- 10 these were studies on the effect of Supreme 11 Court decisions or on legislation on how people 12 behave and think. 13 Q. And many of them were looking at 14 relative rates of bias amongst the respondents? 15 A. So the first one was looking at 16 public opinion, the second one was looking at 17 anti-gay bias, and the third one was looking at 18 mental distress among sexual minorities and had 19 hetero -- heterosexual adults. 20 Q. Yes. 21 A. And then that my -- my only debt to 22 you is that there is another study. It -- so 23 it's a public opinion poll that was conducted 24 one month after Masterpiece or one month and a 25 half after I finished collecting data.	183 1 THE WITNESS: What? 2 MS. HINKLE: Objection to form. 3 Professor, you can answer. I was just noting my 4 objection for the record. 5 THE WITNESS: So it doesn't mean 6 that Counsel Bangert need to refer -- 7 MS. HINKLE: No. No. You can 8 answer the question if you understood it. 9 BY MR. BANGERT: 10 Q. I should probably re-ask it now 11 though I think it's probably flown the coop. 12 There's nothing in your report -- 13 let me strike that question. Let me ask a 14 better question. 15 Unlike the studies you just 16 mentioned, your Masterpiece study evaluates the 17 willingness of vendors to provide a service in 18 response to a Supreme Court decision. 19 Correct? 20 A. Yes. 21 Q. None of the studies you just 22 mention, evaluated the willingness of a vendor 23 to provide a service in response to a Supreme 24 Court decision? 25 A. No. The -- the study that comes
182 1 And what it examined is changes in 2 whether the public thinks that businesses should 3 be allowed to refuse service to gay couples for 4 religious reasons and it'd find an increase in 5 the level of -- so the percentage of the public, 6 that think that this should be legal. So it's 7 not a whole lot of time afterwards, but it's 8 still longer than Masterpiece itself. 9 Q. Yes. So -- and I appreciate that 10 information. You didn't do anything in your 11 report though to apply and draw comparisons 12 between the studies you just mentioned and the 13 findings of your report. 14 A. So I do refer to some of these 15 studies in my papers that are annexed when I 16 talk about those changes. 17 Q. Right. But you didn't -- there's 18 nothing -- there's nothing about those studies 19 that would suggest that wedding vendors 20 providing a service to individuals requesting 21 that service, would necessarily respond or 22 persist in their reactions to a Supreme Court 23 case in the same way that attitudes, bias, or 24 feelings would nec -- would persist? 25 MS. HINKLE: Objection to form.	184 1 closest is a study that looks at public opinion 2 regarding wedding service refusal. 3 Q. Right. And independent -- 4 completely independent of one's attitude, one's 5 bias, or one's internal belief system, you've 6 written on something called the social -- social 7 impact regulation. The willingness of religious 8 persons to sublimate their beliefs and attempt 9 to accommodate those with whom they disagree 10 when possible and when it can be squared with 11 their religious priors. 12 Is that fair? 13 A. I wouldn't, I think, phrase it as 14 you do, but let's see what the question is. 15 Q. Sure. But you are obviously 16 familiar with this concept. You've written on 17 it several times. 18 A. And glad to read my -- my work. 19 Q. It was -- it was -- it was 20 fascinating, I will say. And I say that with 21 all honesty, it was. 22 It's true that regardless of 23 whether someone holds religious beliefs that may 24 contradict or be in conflict with certain sexual 25 lifestyles or behaviors, what you have found is

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185 1 that those religious believers will oftentimes 2 try to find ways to reduce conflict or to 3 accommodate those persons when possible? 4 A. I did. 5 Q. And you also, it's true that for 6 every decision like Masterpiece Cakeshop, there 7 are other decisions that may cut in the opposite 8 direction in support of gay rights or same-sex 9 wedding rights. You mentioned one, Obergefell. 10 Correct? 11 A. Uh-huh. (Witness answers in the 12 affirmative.) 13 Q. You also -- you're familiar, I'm 14 sure, with the Bostock decision that was 15 rendered by the court, the term prior to the one 16 they just completed? 17 A. Yes, I am. 18 Q. And you would expect that those 19 decisions would likewise have impacts and 20 effects on public attitudes, perceptions, and 21 beliefs about same-sex -- individuals who are 22 homosexual or same-sex marriage? 23 A. I would expect that, yes. 24 Q. You would expect that? 25 A. I mean, with Obergefell, we have	187 1 So Masterpiece was decided after Obergefell. So 2 whatever effect Obergefell had is already 3 captured within -- within Masterpiece -- that 4 the dual and potentially conflicting effects of 5 Bostock and Fulton, this is less -- this is more 6 difficult because that happened afterwards. 7 Q. All right. But you agree the Court 8 issuing decisions both protective of religious 9 freedom and protective of gay rights, gay 10 individual civil rights, can mitigate the net 11 social impact of this decision in this area? 12 A. I think it's complicated to say 13 that. First, I think, I -- I mean, I know that 14 there was a study conducted on Bostock, but 15 there aren't any results available yet. It's 16 not my own. So we might have -- we may have 17 answers on that but -- 18 Q. But it's at least theoretically 19 possible then? 20 A. You recall better than I do I'm 21 sure that Bostock was really not about religious 22 freedom and actually Justice Gorsuch insisted on 23 keeping that out and not providing any hint that 24 his expansive reading of the word sex in the 25 Civil Rights Act will somehow create limitations
186 1 evidence that it did. Is it that's the Tankard 2 and Paluck study and also the Ofosu study. I 3 mean, we have evidence about that. 4 We also have evidence that there is 5 reactants, I should say. I mean, for 6 Obergefell, there was reactants. But that's the 7 Ofosu study. There was reactants in those 8 states that did not themselves enact legalized 9 gay marriage themselves. People there became 10 more hostile to gays after Obergefell than 11 before. So that's what -- that's what the 12 researchers found. So as you alluded to before, 13 changes can happen in both directions. 14 Q. Both directions. Right. And you 15 don't have -- you're not able to right now as we 16 sit here to net out the effect of multiple 17 Supreme Court decisions on attitude towards 18 same-sex marriage or same-sex individuals. And 19 I'm talking about Obergefell, Bostock, Fulton, 20 Masterpiece, to net out the net effect of all of 21 that over time. 22 You're not able to do that as you 23 sit here today? 24 A. No. But I just want to say that 25 not all of them are relevant in the same way.	188 1 for the practice of religious people. So and -- 2 and I think he follows on that in Fulton. 3 So I think, you know, I wouldn't 4 make any speculations. I would say that these 5 are things that are worth testing as science -- 6 as a scientist, I'm saying that. 7 Q. Okay. The Masterpiece decision was 8 not decided on First Amendment free speech 9 grounds, was it? 10 A. Not by the majority, as you know. 11 Q. Right. And so given the -- of 12 course, the concurrence is talking a little bit 13 about it. But in terms of the holding of the 14 case, it didn't rest on free speech grounds? 15 A. That's correct. 16 Q. Now, you understand, though, that 17 one of Jack Phillips' arguments in that case was 18 that the First Amendment protected his right not 19 to be compelled to speak certain messages with 20 which he disagreed. In this case, that would be 21 the creation of certain cakes that would -- that 22 communicate a message that he disagreed with. 23 You recall that, right? 24 A. Yeah, of course. 25 Q. Right. And you and your

189 1 Masterpiece model, you were looking more at 2 attitudes by vendors as -- strike that question, 3 terrible questions. 4 The question is: You were looking 5 in your Masterpiece study to evaluate the effect 6 of the Masterpiece decision on wedding vendors. 7 And the way that wedding vendors will be 8 impacted by the decision was -- was mediated 9 through and filtered through the media. So the 10 way they understood the decision ultimately 11 would affect their reaction to it. 12 Is that fair? 13 A. Yes. Unfortunately, I think it's 14 true for most decisions. 15 Q. Yes. The social impact really is 16 still remediated by the media. 17 And so in the case of Masterpiece, 18 it was not reported as a free speech decision, 19 clearly. It was a free exercise decision. 20 A. Right. And I would say that it was 21 mostly reported simply as a victory for the 22 baker. 23 Q. Yes. 24 A. But it -- but -- but it was noted, 25 the religious grounds, certainly.	191 1 Q. Yes. And -- and I know Jack 2 personally, and I'll tell you that is absolutely 3 his position; that he will say that to this day. 4 In fact, he was -- he was just to in trial back 5 in March and that's exactly what he testified at 6 trial. I -- in fact, one of the witnesses at 7 trial was a homosexual individual who was a 8 close friend of Jack's who -- 9 A. I read -- I read your piece -- I 10 read your piece at the National Review. 11 Q. Very good. So -- well, you've been 12 reading my stuff too, so not quite as 13 interesting as yours. 14 A. They're interesting. 15 Q. But I'll tell you he -- he 16 certainly is willing to serve anyone, he just 17 won't say -- to communicate a message. 18 And here's why I think that's 19 interesting because I want to go back to your 20 Online Appendix. And we were talking a little 21 bit about changes to the -- the stimuli, changes 22 to the -- the text of the e-mails being sent 23 both before and after. 24 A. Uh-huh. (Witness answers in the 25 affirmative.)
190 1 Q. So that -- I know. And you would 2 expect that the way in which the wedding vendor 3 community responded to that decision would be 4 directly related to the way in which they 5 understood the decision to have been framed or 6 what the decision was? 7 A. As we talked about before, I don't 8 have direct evidence on the mechanisms. The 9 closest I have is to see how the effect expands 10 in religiously-dense environment. 11 Q. Yes. Here's why I'm asking all of 12 these esoteric legal questions, because you 13 understand that Jack Phillips has testified, 14 over and over again, as a baker, that he is 15 willing to serve same-sex couples, he's willing 16 to serve homosexual individuals, he's willing to 17 sell them any product in his cake shop off the 18 shelves. He simply won't create cakes that 19 communicate a message with which he disagrees. 20 You understand that's his position? 21 A. Yes. Although I do note -- I -- I 22 should note that I know that this was factually 23 contested in the case and I do not consider 24 myself able to settle with that. But I -- I did 25 hear him say that in interviews.	192 1 Q. And in the first wave of e-mails 2 being sent to the bakers in particularly, you 3 note something. You say -- I'm looking at 4 page 1 of the Online Appendix: My name is John. 5 And my partner, Adam, and I are engaged to be 6 married in January. We are specifically looking 7 for a traditional tiered cake with a 8 personalized topper. And from what we saw on 9 your website, we think you may be just the 10 perfect one to help us realize that dream. 11 And most of us would assume a 12 traditional tiered cake with personalized topper 13 is going to be a custom wedding cake or 14 personalized really gives that away, right? 15 A. Uh-huh. (Witness answers in the 16 affirmative.) 17 Q. Okay. The next paragraph, Getting 18 down to business, what is the price range we 19 should expect? And are there any additional 20 services you supply that may be of interest to 21 us? In addition, what kind of shelf product 22 alternatives do you bake. 23 So I noticed that you added that in 24 to the bakers query, and I'm assuming that you 25 added that in because of Jack's willingness to

193 1 provide off-the-shelf goods. 2 Is that fair? 3 A. That's correct. 4 Q. Okay. And so -- 5 A. That's also why I quoted the 6 negative with referral to begin with. 7 Q. Yes. 8 A. So I was looking to see originally, 9 I thought and that -- and I think I write this 10 also in the paper. That's also why I have both 11 photographers and bakers. So I was looking to 12 see whether those nuanced differences that could 13 be looked at and -- and would provide thought 14 provoking ways in which vendors deal with this 15 conflict. But that turned out to be, as I -- as 16 you've seen, largely irrelevant. 17 Q. Yeah. Well, but I don't know if it 18 was because you coded as positive, a baker who 19 was willing to provide a service requested. 20 Do you know whether you coded as 21 positive any response from the baker that they 22 would provide shelf products? 23 A. Yes. And there was just -- my 24 disappointment is that there were -- there were 25 none.	195 1 A. But so what is on the net -- 2 Q. So that option -- 3 A. So the net effect of Masterpiece 4 that you see there is an -- that same coding 5 policy, of course, applied to the third and the 6 fourth wave, is it shows you that even when 7 we're not counting those, we're seeing 8 discrimination. 9 Q. As I was -- I thought it was 10 interesting that you didn't provide that option 11 in the third and fourth waves to the bakers. 12 A. So I -- I did, or at least that was 13 -- so I asked to look at the third wave e-mail, 14 so we know that different cakes have different 15 costs, so can you please send us a few different 16 options and we will go from there. So -- and we 17 didn't ask there about like, you know, the 18 custom topping and all of that. 19 So that provided a wide enough 20 window for bakers who wanted to provide some 21 kind of positive response to say whatever they 22 wanted about their potential menu of cakes. 23 So for example, you know, you could 24 get that and you could say, I'm happy to prepare 25 140 cup cakes, but I -- I don't do specialized
194 1 Q. Okay. 2 A. So if someone ask -- if someone 3 offered us a shelf product that -- they were 4 coded as positive. 5 Q. Okay. And you also received 6 inquiries from various bakers where you -- you 7 coded them as, I think, positive as plus 0.5 if 8 it was a request for additional information. 9 Do you recall that? 10 A. Yeah. 11 Q. Okay. And is it possible some of 12 those bakers may have been trying to discern or 13 decipher whether they could provide shelf 14 products? 15 A. Maybe, but they were coded as 16 positive, so I wouldn't count them as 17 discrimination. 18 Q. Absolutely. So even if there was a 19 baker who reached back out to you for more 20 information intending to provide shelf products 21 but having a First Amendment objection to 22 providing a custom product, that would have been 23 a positive baker? 24 A. Under my conservative coding, yes. 25 Q. Yes.	196 1 cakes or I can't do a specialized cake. 2 Q. So that's your -- that's the -- 3 where you -- you -- at least what you testified 4 to was that sentence: We know that different 5 cakes have different costs, so if you could 6 please send us a few different options and go 7 from there, that's where you were trying to 8 imply that shelf products would be acceptable? 9 A. Yes, within the -- within the 10 necessary changes from the pre-waves to the -- 11 to the fourth wave. 12 Q. Is it possible that -- 13 A. The third and fourth wave. 14 Q. Is it possible that bakers 15 receiving the third wave e-mail may have been 16 unaware or confused by the change in text and 17 believe that they were not actually able -- that 18 shelf products were not one of the services 19 being requested? 20 MS. HINKLE: Objection. Form and 21 foundation. 22 THE WITNESS: I'm confused and 23 that's -- I'm not sure what I'm supposed to do 24 now. 25 * * *

197 1 BY MR. BANGERT: 2 Q. Let me ask you this question: You 3 agree that the third wave e-mail to same-sex 4 couples on behalf of -- the third wave e-mail on 5 behalf of same-sex couples to bakers does not 6 include an explicit call to provide shelf items? 7 A. I think it provides a wide enough 8 margin to do that. But again, I want to remind 9 you that what I'm doing is I'm comparing 10 same-sex and opposite-sex couples in the third 11 and the fourth wave. 12 Q. And so your objective actually 13 changed from the first and second wave to the 14 third and fourth wave? 15 A. My objective was still to 16 understand whether there is discrimination, but 17 after Masterpiece, I had to insert changes in 18 the wording of the e-mails and I kept them the 19 same for -- across same-sex and opposite-sex 20 couples, still allowing participants to offer me 21 whatever they want, so creating a wide enough 22 margin for them to be helpful if they wanted to. 23 But again, I mean, I'm -- I'm 24 telling you this because this was a 25 disappointment of mine from the businesses, that	199 1 was communicated over the phone. And there was 2 none over e-mail that would either choose such 3 explicit words as, you know, I'm -- 4 Q. Yeah. And that's what I was trying 5 to get at was that via the e-mail response, you 6 didn't receive an e-mail that explicitly said, I 7 don't do same-sex weddings? 8 A. No. I did get like some rude 9 negative e-mails like, No. But that's it. 10 Q. Well, some vendors might just think 11 they're efficient, right? Just a more, We're 12 busy. 13 A. I mean if they were super 14 efficient, they wouldn't answer, but they felt 15 the need to say no. 16 MR. BANGERT: Very good. Okay. 17 Professor, I have reached the end of my outline. 18 I appreciate your time. 19 THE WITNESS: And I have so many 20 other things I want to say. 21 MR. BANGERT: Well, I -- I -- I -- 22 I'm sure we -- it would be wonderful to grab a 23 cup of coffee and -- and continue the 24 conversations but I fear that -- that you 25 probably have work to do tomorrow.
198 1 this doesn't really happen, not before nor 2 after. So there wasn't -- there wasn't a change 3 in that. 4 If -- if -- you know, if it would 5 have happened before and then it wouldn't have 6 happened afterwards, I would share your concern. 7 But this was not close to being common. I think 8 there may be five or so businesses that chose 9 this option. 10 I thought it's going to be 11 interesting, but it turned out to be very, very, 12 very rare. And I share your excitement about 13 this possibility, but the data disappointed me. 14 Q. So but you did make the change 15 though and in the third wave e-mail -- 16 A. I made it more open. 17 Q. You made it more open, right. 18 A. Yes. But that was still an option 19 for them to propose. 20 Q. Right. But instead of -- would you 21 get any -- do you recall any specific responses, 22 negative responses, that explicitly said, I 23 don't do same-sex weddings? 24 A. Very few. So there is one that you 25 already referred to in the rebuttal report that	200 1 So Casey, I pass the witness to 2 you. 3 MS. HINKLE: I don't have any 4 questions of the witness at this time. 5 Thank you very much for your time 6 today, Professor. 7 MR. BANGERT: All right. Very 8 good. Well, thank you, Professor. I hope 9 you're able to get out of your office safely. 10 11 12 * * * 13 (Witness excused.) 14 * * * 15 16 17 18 19 20 21 22 23 24 25

Professor Netta Barak-Corren

August 4, 2021

201 1 STATE OF KENTUCKY) 2) SS. 3 COUNTY OF JEFFERSON) 4 I, JESSICA TAYLOR ROSS, a Notary 5 Public within and for the State at Large, do 6 hereby certify that the foregoing deposition was 7 taken before me, via Zoom, at the time and for 8 the purpose in the caption stated; that the 9 witness was first duly sworn to tell the truth, 10 the whole truth and nothing but the truth; that 11 the deposition was reduced to digital shorthand 12 and recorded by me in the presence of the 13 witness; that the foregoing is a full, true and 14 correct transcript of my digital notes and 15 recording; that there was no request that the 16 witness read and sign this deposition; that the 17 appearances were as stated in the caption. 18 19 WITNESS MY SIGNATURE this 11th day of 20 August, 2021. 21 My commission expires July 21, 2022. 22 23 /s/ Jessica T. Ross 24 JESSICA TAYLOR ROSS 25 Court Reporter Notary Public, State At Large Notary ID 602031 V/JR	203 1 2 - 2 - 3 4 Once I have received the errata sheet from 5 you, I will then append the errata sheet to the 6 original transcript and forward it to 7 Mr. Bangert for safekeeping. 8 If you have any questions about this 9 errata sheet procedure, please let me know. 10 11 Sincerely, 12 /s/ Jessica Taylor-Ross 13 Jessica Taylor-Ross 14 15 Enclosures (1) 16 17 18 19 20 21 22 23 24 25
202 1 TAYLOR COURT REPORTING KENTUCKY 2 2901 Six Mile Lane 3 Louisville, KY 40220 4 Telephone (502) 671-8110 5 Facsimile (502) 671-1630 6 August 16, 2021 7 8 TO: Casey L. Hinkle, Esq. 9 KAPLAN JOHNSON ABATE AND BIRD, LLP 10 710 W. Main Street, 4th Floor 11 Louisville, Kentucky 40202 12 Telephone: (502) 416-1630 13 Email: chinkle@kaplanjohnsonlaw.com 14 15 RE: UNITED STATES DISTRICT COURT, WESTERN 16 DISTRICT OF KENTUCKY, LOUISVILLE DIVISION 17 Case No. 3-19-CV-00851-BJB-CHL 18 CHELSEY NELSON PHOTOGRAPHY, LLC and 19 CHELSEY NELSON 20 VS. 21 LOUISVILLE/JEFFERSON COUNTY METRO 22 GOVERNMENT, et al. 23 Errata Sheet for PROFESSOR NETTA 24 BARAK-CORREN 25 26 Dear Ms. Hinkle, 27 28 Pursuant to your request for a Read and 29 Sign, I am providing you with the transcript 30 errata sheet for the deposition of Professor 31 Netta Barak-Corren given in the above matter on 32 August 4, 2021. 33 Please advise Professor Barak-Corren she 34 is to read your copy transcript and complete the 35 errata sheet (all three pages) attached hereto, 36 have it notarized and return all three pages to 37 me within thirty days of the date of this 38 letter. If there are no changes to be made, 39 please indicate there are no changes on the 40 errata sheet, have it notarized and return all 41 three pages to me within thirty days of the date 42 of this letter.	204 1 UNITED STATES DISTRICT COURT 2 WESTERN DISTRICT OF KENTUCKY 3 LOUISVILLE DIVISION 4 Case No. 3-19-CV-00851-BJB-CHL 5 6 CHELSEY NELSON PHOTOGRAPHY, LLC 7 and CHELSEY NELSON, PLAINTIFFS 8 9 v. 10 11 12 LOUISVILLE/JEFFERSON COUNTY METRO 13 GOVERNMENT, et al., DEFENDANTS 14 15 DEPONENT: PROFESSOR NETTA BARAK-CORREN 16 DATE: AUGUST 4, 2021 17 18 COURT REPORTER: JESSICA TAYLOR ROSS 19 20 21 22 ERRATA SHEET FOR PROFESSOR NETTA BARAK-CORREN 23 24 TAYLOR COURT REPORTING KENTUCKY 25 2901 SIX MILE LANE LOUISVILLE, KENTUCKY 40220

51 (Pages 201 to 204)

Taylor Court Reporting Kentucky
(502) 671-8110 Fax (502) 671-8116

Exhibit 3

**UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION**

**CHELSEY NELSON PHOTOGRAPHY
LLC and CHELSEY NELSON,**

Plaintiffs,

v.

**LOUISVILLE/JEFFERSON COUNTY
METRO GOVERNMENT, et al.,**

Defendants.

Case No. 3:19-cv-851-BJB

EXPERT REPORT OF NETTA BARAK-CORREN

June 30, 2021

EXPERT WITNESS DISCLOSURE

1. I have been retained as an expert in this matter by Defendants Louisville/Jefferson County Metro Government (“Louisville Metro”), Louisville Metro Human Relations Commission – Enforcement and Louisville Metro Human Relations Commission – Advocacy, Verná Goatley, in her official capacity as Executive Director of the Louisville Metro Human Relations Commission, 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 (collectively, “Defendants”), through their counsel, Kaplan Johnson Abate & Bird LLP (“Counsel”), in connection with litigation brought by Plaintiffs Chelsey Nelson Photography LLC and Chelsey Nelson (collectively, “Chelsey Nelson”).

2. Chelsey Nelson is a wedding photographer in Louisville, Kentucky who filed this litigation to challenge the constitutionality of two provisions in Louisville’s Fairness Ordinance (Louisville Metro Ordinance § 92.01, et seq.), which prohibits discrimination in employment, housing, and the provision of goods and services (public accommodations). Specifically, Ms. Nelson challenges the “Denial Clause,” which makes it “an unlawful practice for a person to deny an individual the full and equal enjoyment of the goods, services, facilities, privileges, advantages, and accommodations of a place of public accommodation . . . on the ground of . . . sexual orientation . . .”, and the “Unwelcome Clause,” which makes it “an unlawful practice for a person, directly or indirectly, to publish, . . . [a] communication, notice, or advertisement, which indicates that the goods, services, facilities, privileges, advantages, and accommodations of a place of public accommodation . . . will be refused, withheld, or denied an individual on account of his [or her] . . . sexual orientation . . .” Louisville Metro Ordinance § 92.05(A) & (B).

3. Chelsey Nelson alleges that her religious beliefs prevent her from providing wedding photography and related services to same-sex couples and wishes to advertise her intent

to deny services to same-sex couples in violation of the Unwelcome Clause in Louisville’s Fairness Ordinance. Chelsey Nelson asks the Court to grant her an exception to application of the challenged provisions in the Fairness Ordinance based on her purported rights to free speech and free exercise of religion under the First Amendment. Chelsey Nelson has also asserted claims for declaratory relief under the First Amendment’s establishment clause, Fourteenth Amendment’s due process clause, and Kentucky’s Religious Freedom Restoration Act (“RFRA”) (KRS 446.350).

4. I have been asked to provide an expert opinion regarding the anticipated effects of granting Chelsey Nelson the relief sought by her Complaint on the willingness of other wedding vendors to provide services to same-sex couples.

5. The opinions expressed in this report are based on information available to me at this time and are subject to supplementation or revision based on additional information that may emerge from depositions, additional document submissions, or other developments, including my ongoing research.

PROFESSIONAL EDUCATION AND BACKGROUND

6. I am a legal scholar and cognitive scientist, focusing on empirical and behavioral analysis of constitutional and public law, with a particular interest in conflicts of rights and the interaction between law and religion and law and social norms.

7. I received my LL.B. in Law and B.A. in Cognitive Science from the Hebrew University (Valedictorian and three-time recipient of the Albert Einstein and Rector awards). I then clerked for the Chief Justice of the Israeli Supreme Court, Hon. Dorit Beinisch, and pursued doctoral studies at Harvard, graduating in 2016. During my time at Harvard, I received the Shapiro scholarship, Gammon fellowship, Sinclair Kennedy Travelling Fellowship, and the Program on Negotiation’s Graduate Research Fellowship, and won the Dean’s Prize in Law and Economics,

and empirical research grants from the Program on the Legal Profession, Harvard's Interfaculty Initiative on Mind, Brain, and Behavior and the Program on Negotiation's Next Generation Grant. I also served as the Inaugural Fellow of the Empirical Legal Research Group in Harvard Law School.

8. Currently, I am an Associate Professor of Law (with tenure) at the Hebrew University of Jerusalem and the Academic Director of the Center for the Study of Multiculturalism and Diversity at the Hebrew University. During 2020-2022 I am also a Nootbaar Religious Freedom Fellow at Pepperdine University School of Law.

9. My academic work was selected to important international fora, including the Stanford International Junior Faculty Forum (~5% acceptance rate) and the WZB Migration and Diversity Conference (~3% acceptance rate) and won several awards, among which Hebrew University's Birk Prize for Excellence in Legal Research, the Israeli Association of Public Law's Gorni Prize for an Outstanding Junior Scholar in Public Law, and the Menachem Goldberg, Howard Raiffa, and Fisher-Sanders Best Paper Awards.

10. My curriculum vitae, which includes any publications I have authored within the last 10 years, is attached hereto as Exhibit 1. This is my first engagement to provide expert services in connection with litigation. I am being compensated for my services in this matter at a rate of \$300/hour. My compensation does not depend in any way on the outcome of this litigation or the opinions stated herein.

INFORMATION REVIEWED AND/OR RELIED UPON

11. In developing my opinions in this matter, I had discussions with Counsel and reviewed and/or relied upon the Complaint, the Court's Order and Opinion granting Chelsey Nelson's motion for preliminary injunction, Louisville's Fairness Ordinance (Louisville Metro Ordinance § 92.01, et seq.), Kentucky's RFRA (KRS 446.350), and the materials cited in my two

forthcoming papers: Netta Barak-Corren, A License to Discriminate? The Market Response to *Masterpiece Cakeshop*, 56(2), Harvard Civil Rights-Civil Liberties Law Review (forthcoming 2021), attached hereto as Exhibit 2; Netta Barak-Corren, Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from *Masterpiece Cakeshop*, Journal of Legal Studies (forthcoming 2021), attached hereto as Exhibit 3.

OPINION

12. Based on my work to date, I have formed the opinion that granting Chelsey Nelson a religious exemption from the application of Louisville’s Fairness Ordinance in this case could significantly increase the likelihood that same-sex couples attempting to hire wedding vendors in Louisville, Kentucky will experience discrimination resulting in the denial of equal access to goods and services.

BASIS FOR MY OPINION

13. The basis for my opinion set forth above is largely the *Masterpiece*¹ experiment, described in detail in my forthcoming papers attached hereto as Exhibit 2 and Exhibit 3. As further described in these papers, I examined the impact of the *Masterpiece Cakeshop* decision on the wedding market by conducting a field experiment before and after the decision was rendered. I fielded the experiment in Iowa, North Carolina, Indiana, and Texas, states that, while sharing similar socio-economic characteristics and social and political attitudes, represent the four types of legal regimes currently in existence in the United States with respect to non-discrimination and religious freedom: regimes that prohibit discrimination on the basis of sexual orientation versus regimes that do not; and regimes that subject restrictions of religious exercise to a stringent test (primarily via state RFRAs) versus regimes that do not. My study audited wedding businesses

¹ See *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Com’n*, 138 S. Ct. 1719 (2018).

(photographers, bakers, and florists) in those regimes both before and after the *Masterpiece* ruling and measured the impact of the decision on the willingness of wedding vendors to provide services to same-sex couples, as compared with opposite-sex couples.

14. I found that the *Masterpiece* ruling – which was decided in favor of a baker that refused to create a wedding cake for a same-sex couple – significantly reduced the agreement to serve same-sex couples as compared with opposite-sex couples, even among previously willing vendors (the “*Masterpiece* effect”). I found that the *Masterpiece* effect was stable and robust to the inclusion of all experimental covariates, including the legal regime, the type of business, and the wave of inquiry. The effect was equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and did not vary with the political conservativeness of the county. However, I found that the effect varies with the religiosity of the environment, such that businesses in areas dense with religious congregations, and particularly Evangelical congregations, were more likely to change their behavior to same-sex couples post-*Masterpiece* (even as before *Masterpiece*, businesses in religiously-dense areas did not differ from other areas in how they responded to same-sex and opposite-sex couples).

15. The negative effect of *Masterpiece* was found in all legal regimes except for counties in Texas and Indiana (RFRA states) that enacted local AD rules. As I discuss in the paper attached hereto as Exhibit 3 (pages 13-18), RFRA states differ substantively across states, with Texas and Indiana belonging in a specific category of RFRA states that is not available in all states. Therefore, for the purpose of preparing this opinion, I compared Kentucky’s RFRA with the RFRA laws in Texas and Indiana. Specifically, unlike the RFRA states in Indiana and Texas, which both include carve-outs for local anti-discrimination laws, Kentucky’s law does not have any such carve out. Kentucky’s law has only one provision, which states that “Government shall not substantially

burden a person's freedom of religion" unless the government satisfies the strict scrutiny standard. *See* KRS 446.350. The Indiana and Texas laws both contain similar language prohibiting the government from substantially burdening free exercise of religion except where the government can satisfy the strict scrutiny standard, but these laws carve-out civil rights laws. *See* Ind. Code Ann. § 34-13-9-0.7 ("This chapter does not (1) authorize a provider to refuse to offer or provide services . . . on the basis of . . . sexual orientation; (2) establish a defense to a civil action or criminal prosecution for refusal by a provider to offer or provide services . . . on the basis of . . . sexual orientation . . ."); TX CIV PRAC & REM § 110.011 (Except for religious non-profits, "this chapter does not establish or eliminate a defense to a civil action or criminal prosecution under a federal or state civil rights law."). The Indiana carve-out was added to the law after passage of Indiana's initial RFRA (which contained no such carve-out) resulted in significant public blow-back and loss of convention/events revenue.

16. As I write in the paper attached hereto as Exhibit 3 (p. 51), different RFRA designs could have different impact on discrimination, especially as these designs interact with existing or inexistent AD laws. Because of these differences between Kentucky's RFRA, on the one hand, and Texas and Indiana's RFRAs, on the other hand, with particular notice to the differences in the laws' interaction with local AD laws, I would not expect the Louisville Metro area to be immune from the statistically significant *Masterpiece* effect I observed in the *Masterpiece* experiment. That conclusion is bolstered by the high degree of religiosity of Louisville/Kentucky, which I found increased the *Masterpiece* effect in jurisdictions studied as part of the *Masterpiece* experiment.

17. To analyze religiosity, I analyzed data from the Pew Institute (Religious Landscape Data (2014), <https://www.pewforum.org/religious-landscape-study/#religions>) and compared that data to the same metrics observed in my paper for the jurisdictions studied as part

of the *Masterpiece* experiment.

18. The Pew Institute ranks all U.S. states by religiosity based on data from 2014. Kentucky is placed 13th with 63% of adults who are highly religious (a combined index score composed of belief in God, importance of religion, frequency of prayer, and worship attendance). This result is close to tied to that of Texas (64%) and North Carolina (65%) and higher than Indiana (54%, 22th place) and Iowa (55%, 19th place).

19. Kentucky ranks high also on importance of religion, the measure I focus on in my papers. Here, Kentucky is tied with Texas (86% who say religion is somewhat or very important in their lives), slightly above North Carolina (84%) and ahead of Iowa (79%) and Indiana (78%).

20. According to the Pew study, 49% of Kentucky's population are Evangelicals, much higher than that of all four states in my study (28%-35%), the same as Alabama and second only to Tennessee (52%). I also observed that Louisville, Kentucky is home to the Southern Baptist Theological Seminary² and some of the largest evangelical megachurches in the country (Southeast Christian Church and St. Stephen Baptist Church).³

21. According to the Pew study, Kentucky also has stronger views against homosexuality and same-sex marriage than any of the states in the *Masterpiece* experiment (44% who say that homosexuality should be discouraged, as compared to 36-37% in the states in the experiment, and 52% who are opposed or strongly opposed to same-sex marriage, as compared to 41-46% in the statues in the experiment).

22. A comparison of religiosity metrics in Kentucky and the *Masterpiece* experiment

² See: <https://www.sbts.edu/>.

³ See: https://en.wikipedia.org/wiki/Religion_in_Louisville,_Kentucky; Southeast Christian Church website (<https://www.southeastchristian.org/home>); St. Stephens Baptist Church website (<https://www.ssclive.org/church-history/>).

states is below:

Criterion	Definition	Kentucky	Iowa	North Carolina	Indiana	Texas
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	86%	79%	84%	78%	86%
% Conservatives	(National average: 36%)	42%	41%	40%	41%	39%
% Evangelicals	(National average: 25%)	49%	28%	35%	31%	31%
Attitudes Towards Homosexuals	“Homosexuality should be discouraged” (National average: 31%)	44%	36%	36%	37%	36%
Attitudes Towards Same-Sex Marriage	Opposing/Strongly Opposing Same-Sex Marriage (National average: 39%)	52%	41%	45%	45%	46%

Source: PEW RESEARCH CENTER, RELIGIOUS LANDSCAPE STUDY (2014).

23. In closing, it is important to note that studies like the *Masterpiece* experiment can never predict future behavior with absolute certainty. The *Masterpiece* study provides evidence that judicial decisions favoring religious objectors to non-discrimination laws can have statistically significant and robust consequences, particularly in very religious areas. In the present case, due to the high degree of religiosity in the area, I would expect to observe the *Masterpiece* effect in Louisville Metro if Chelsey Nelson is granted a religious exemption to application of the Fairness Ordinance in this litigation.


 Netta Barak-Corren

Exhibit 1

Netta Barak-Corren

Hebrew University Law School | Mt. Scopus, Jerusalem 91905, Israel | netta@huji.ac.il

ACADEMIC POSITIONS

Associate Professor (tenured), Hebrew University Law School	2020 – Present
Assistant Professor, Hebrew University Law School	2017 – 2020
Head of the Law & Psychology Program	2017 – Present

EDUCATION

S.J.D. Harvard Law School <i>Dissertation:</i> When Law and Religion Conflict: A Behavioral Examination <i>Committee:</i> Cass Sunstein, Martha Minow, Tom Tyler (Yale)	2016
LL.M. Harvard Law School (waived on admission to the S.J.D.) <i>Dean's Prize in Law and Economics</i> <i>Dissertation awarded the Fisher-Sanders Award</i>	2013
LL.B. and B.A. in Law and Cognitive Science, Hebrew University of Jerusalem <i>Valedictorian, Summa cum laude with highest honors</i>	2012

RESEARCH INTERESTS

Constitutional Law, Law and Religion, Law and Social Norms, Empirical Legal Studies, Behavioral Economics, Moral Psychology, Conflict Resolution.

PUBLICATIONS

20. Netta Barak-Corren, *Religious Freedom in Israel*, in HANDBOOK ON ISRAELI CONSTITUTIONAL LAW (forthcoming, Barak A., Medina, B., and Roznai, Y. Eds., forthcoming, Oxford University Press)
19. Netta Barak-Corren and Lotem Perry-Hazan, *Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations*, JOURNAL OF SOCIAL ISSUES (special issue for the 50th anniversary of legal socialization)
 - ✧ Press coverage: [Ha'aretz](#), [The Marker](#), [Haredim10](#), [Seder Yom \(IDF radio\)](#)
 - ✧ Invited presentations at Israeli Knesset, Ministry of Education, Ministry of Justice, and Israeli Democracy Institute.
 - ✧ Non-technical summary written and circulated by [Chief Scientist, Ministry of Education](#).
18. Netta Barak-Corren and Yael Kariv-Teitelbaum, *Behavioral Responsive Regulation*, Minor Revision, REGULATION AND GOVERNANCE.
 - ✧ Invited presentation at Israeli Ministry of Justice, Department of Regulation.
17. Yuval Barak-Corren, Netta Barak-Corren, Alex Gileles-Hillel, and Eyal Heiman, *The Effect of C-Reactive Protein on Chest X-Ray Interpretation: A Decision-Making Experiment Among Pediatricians*,

Pediatric Pulmonology.

16. Netta Barak-Corren, *The Empirical Consequences and Normative Implications of Religious Exemptions*, 56(1) [HARVARD CIVIL RIGHTS-CIVIL LIBERTIES LAW REVIEW](#) (forthcoming 2021)
15. Netta Barak-Corren, *Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from Masterpiece Cakeshop*, [JOURNAL OF LEGAL STUDIES](#) (forthcoming 2021)
 - ✧ Invited presentations at Chicago Law School, Harvard Law School, Stanford Law School, Texas Law School, Virginia Law School, and Yale Law School.
 - ✧ Press coverage: [Boston Globe](#), [SCOTUSblog](#), [National Constitution Center](#)
 - ✧ Summarized for [the Atlantic](#).
14. Netta Barak-Corren, *Regulating for Social Integration by Behavioral Design: An Evidence-Based Approach for Culturally Responsive Regulation*, [REGULATION AND GOVERNANCE](#) (forthcoming 2021)
13. Karen Huang, Regan Bernhard, Netta Barak-Corren, Max H. Bazerman, and Joshua D. Greene, *Veil-of-Ignorance Reasoning Mitigates Self-Serving Bias in Resource Allocation During the COVID-19 Crisis*, JUDGMENT AND DECISION MAKING (2021)
 - ✧ Summarized for [HARVARD BUSINESS REVIEW](#).
12. Netta Barak-Corren, [Taking Conflicting Rights Seriously](#), 65(2) VILLANOVA LAW REVIEW 295 (2020)
 - ✧ Lead article of the issue
 - ✧ Selected for the 2018 Stanford International Junior Faculty Forum (~5% acceptance rate)
 - ✧ Selected for CELS 2018 at Michigan Law School, Ann Arbor
 - ✧ Invited presentations at Chicago Law School, Harvard Law School, and Toronto Law School.
 - ✧ Recipient of the Gorni Junior Faculty Award for Outstanding Research in Public Law.
11. Netta Barak-Corren & Daphna Lewinsohn-Zamir, [What's in a name? The asymmetric effects of identifiability on offenders and victims of sexual harassment](#), 16(4) JOURNAL OF EMPIRICAL LEGAL STUDIES 955 (2019)
 - ✧ Selected for CELS 2018 at Michigan Law School, Ann Arbor
 - ✧ Selected for SJDM 2018 at New Orleans (%20 acceptance rate)
10. Netta Barak-Corren, [Reexamining the evidence on Ultra-Orthodox attitudes and gender separation in academia](#), 49 MISHPATIM—HEBREW U. LAW REVIEW (2019) [in Hebrew].
 - ✧ Summarized for [the ICON-S-IL Blog](#), 2019.
 - ✧ Press coverage: [Ha'aretz](#), [Globes](#), [Ynet](#)
 - ✧ Recipient of the Gorni Junior Faculty Award for Outstanding Research in Public Law.
9. Netta Barak-Corren & Max H. Bazerman, [Indecision and decision making in moral conflicts](#), ORG. DYNAMICS (2019).
8. Netta Barak-Corren, Yuval Feldman, & Noam Gidron. [The Provocative Effect of Law: Majority Nationalism and Minority Discrimination](#), 15(4) JOURNAL OF EMPIRICAL LEGAL STUDIES 951-986 (2018).
7. Netta Barak-Corren & Max H. Bazerman, [Saving lives your task or God's? The nuanced relationship between religiosity and moral judgment](#), 12(3) JUDGMENT AND DECISION MAKING 280-296 (2017).

6. Netta Barak-Corren, [*Beyond dissent and compliance: Religious decision-makers and secular law*](#), 6(2) OXFORD JOURNAL OF LAW AND RELIGION 293-322 (2017).
✧ Translated and reprinted in 4 [*BIFRAT U'BICHLAL*](#) 17-53 (2019) [in Hebrew].
 5. Netta Barak-Corren, Chia-Jung Tsay, Fiery A. Cushman, & Max H. Bazerman, [*If you're going to do wrong, at least do it right: Moral conflict promotes moral consistency*](#), 64(4) MANAGEMENT SCIENCE 1528-1540 (2017).
 4. Netta Barak-Corren, [*Does Antidiscrimination Law Influence Religious Behavior? An Empirical Examination*](#), 67(4) HASTINGS LAW JOURNAL 957 (2016).
 3. Netta Barak-Corren, [*Antidiscrimination Law and the Religious Workplace*](#), LGBT LAW IN ISRAEL (2016) [in Hebrew].
 2. Netta Barak-Corren*, Edy Glozman*, & Ilan Yaniv, [*False Negotiations: The Art and Science of Not Reaching an Agreement*](#), 59(4) JOURNAL OF CONFLICT RESOLUTION 671 (2015). (* equal contribution)
✧ Recipient of the Howard Raiffa Doctoral Student Paper Award.
✧ Reviewed in Harvard's Program on Negotiation Magazine.
 1. Netta Barak-Corren, [*Property Rights and Workers Rights: Protecting the Wage of Independent Workers*](#), 42(3) MISHPATIM—HEBREW U. LAW REVIEW 973 (2012) [in Hebrew]
✧ Recipient of the M. Goldberg Prize for the best paper on labor law in Israel in 2011.
- Netta Barak-Corren, *Compliance with the law under religion-based normative conflicts: A behavioral analysis and preliminary prescriptions* (Unpublished master thesis)
✧ Recipient of the Fisher-Sanders Award for the best paper written on conflict resolution in Harvard, MIT and Tufts, 2013

SHORTER PIECES

Netta Barak-Corren, *On the Lethal Cocktail of Israeli Democracy and the End Problem of the HCJ wars*, TEL AVIV UNIVERSITY LAW REVIEW FORUM (forthcoming 2021, Hebrew)

Netta Barak-Corren, [*How One Supreme Court Decision Increased Discrimination Against LGBTQ Couples*](#), THE ATLANTIC (2021)

Netta Barak-Corren, [*Shared Fate, Unshared Faith: Israel and the Haredi Society in the Current Corona Moment*](#), THE JEWISH QUARTERLY REVIEW (JQR) FORUM

David Enoch, Netta Barak-Corren, Michal Shur-Ofri, Ofer Malcai, and David Heyd, *Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management*, HEBREW UNIVERSITY LAW JOURNAL FORUM (2021, [Hebrew](#), [English synopsis](#))

Max H. Bazerman, Regan Bernhard, Joshua Greene, Karen Huang, and Netta Barak-Corren, [*How Should We Allocate Scarce Medical Resources?*](#) HARVARD BUSINESS REVIEW (2020).

Netta Barak-Corren, [Gender Segregation in Israeli ultra-Orthodox Academia is not a Zero-Sum Game](#) [Hebrew], ICON-S-IL BLOG (2019)

SELECTED WORKS IN PROGRESS

Religion-equality conflicts and the war within religion, R&R, AMERICAN JOURNAL OF COMPARATIVE LAW (A*)

Majority Nationalism Laws and the Equal Protection of Minorities: Experimental and Observational Evidence from Israel, R&R, JOURNAL OF LEGAL STUDIES (A*) (with Noam Gidron and Yuval Feldman)

SELECTED AWARDS, GRANTS, AND HONORS

Cornell University Center for Social Sciences Grant Competition Award (\$12,000, w/ Nelson Tebbe)	2021
Nootbaar Religious Freedom Fellow, Pepperdine University School of Law	2020-21
Gorni Prize for an Outstanding Junior Scholar in Public Law	2019
Israeli Science Foundation Research Grant No. 1487/19 (\$152,676 for four years, w/ Lotem Perry-Hazan)	2019
Sacker Prize for Paper Accepted to <i>Mishpatim</i> (\$1500)	2019
Selected for the Seventh WZB Migration and Diversity Conference (~3% acceptance rate)	2019
Selected for the Stanford International Junior Faculty Forum (~5% acceptance rate)	2018
Birk Prize for Excellence in Legal Research	2018
Alicia Fund, Cherrick Center for the Study of Zionism Research Grant (\$2000)	2018
Minerva Center for Human Rights Research Grant (\$2000)	2018
Barak Center for Interdisciplinary Research Grant (\$3500, \$2500, \$4500)	2016-19
Empirical Legal Research Group Inaugural Fellow, Harvard Law School	2015-16
Graduate Research Fellowship, Program on Negotiation, Harvard Law School	2015-16
Sinclair Kennedy Travelling Fellowship, Society and Fellows of Harvard College	2014-15
Human Rights and Judaism Fellowship, Israeli Democracy Institute	2014-17
Next Generation Grant, Program on Negotiation, Harvard Law School	2014
Harvard's Interfaculty Initiative on Mind, Brain, and Behavior Student Award	2014
Cravath International Grant, Harvard Law School	2014
Student Empirical Research Grant, Program on the Legal Profession	2013
Harvard Law School Summer Academic Fellowship	2013
Dean's Prize in Law and Economics, Harvard Law School	2013
Gammon Fellowship, Harvard Law School	2012
Shapiro Scholarship, Harvard Law School	2012-13
International Peace Scholar, P.E.O.	2012-13
Pearlman Scholarship, Hebrew University of Jerusalem	2012-13
Valedictorian (1 of 230), Hebrew University of Jerusalem School of Law	2012
Lord Wolf Award for Leadership and Social Contribution, Hebrew University	2012
Albert Einstein Award	2009-11
✧ Three-time recipient of HUJI's highest honor for academic excellence	
Rector Prize & Dean's List	2009-11

✧ Ranked 1 of 900 students in the law school for three consecutive years	
The Cognitive Science Excellence Program Award	2011
✧ Ranked at the top of the interdisciplinary excellence program	
Oberlander Prize for excellence in international Moot Court Competitions	2011
Quarter-Finalist in the Jessup International Moot Court Competition	2011
✧ Reached for the first time in the history of Israel	
Top Oralist and Memorial Honors in the Jessup International Moot Court Competition	2010
Best Oralist, Hebrew University's Moot Court competition	2009
Best Speaker, The European Model United Nations, the Hague, Netherlands	2009

Miscellaneous:

Head of Intelligence Creativity Award for Contribution to Israeli National Security	2007
Israeli Young Scientist Competition, second prize (during high school)	2002

SELECTED OTHER ACTIVITY

Coauthored white papers as part of the COVID-Collective Impact Academia-IL initiative	2020
✧ Ethics Guidelines during the COVID-19 pandemic	
✧ How to harness public cooperation with the COVID-19 regulations	
Academic advisor to JDC Israel on designing behaviorally informed intervention programs	2019
Founded and organized the Empirical Legal Research Group in Harvard Law School	2013-16
Behavioural Insights Team (BIT), London, UK, Research Fellow	2014
Behavioral Insights Group, Harvard University, Undergraduate research mentor	2013-14
Israeli Supreme Court, Chambers of Chief Justice Dorit Beinisch, Law Clerk	2011-12
Mishpatim—Hebrew University Law Review, Editor-in-law	2008-10

SELECTED TALKS AND PRESENTATIONS

* Only talks and presentations are included, no posters.

1. **2014, Harvard Law School**, Program on the legal Profession Speaker Series (invited). Paper titled: "Not in My School: The Conflict between Law and Religion from the Eyes of Religious Educators";
2. **2014, Stanford Law School**, Inaugural Conference for Junior Researchers, the Program in Law and Society. Paper titled: "Not in My School: The Conflict between Law and Religion from the Eyes of Religious Educators";
3. **2014, Harvard Business School**, the Behavioral Insights Group Lab Meeting. Paper titled: "If You're Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under Joint Evaluation";
4. **2014, Hebrew University**, International Workshop on Behavioral Legal Studies (invited). Paper titled: "If You're Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under Joint Evaluation";
5. **2014, Behavioral Insights Team, London, UK** (invited). Paper titled: "If You're Going to Do Wrong, at Least Do It Right: Reconciling Moral Concerns under Joint Evaluation";
6. **2015, Israeli Democracy Institute**, Human Rights and Judaism Colloquium. Paper titled: "Does Antidiscrimination Law Influence Religious Behavior?";
7. **2015, Yale Law School**, Conference on Law, Religion and Politics, doctoral workshop. Paper titled: "Does Antidiscrimination Law Influence Religious Behavior?";

8. **2015, Harvard Psychology Department**, Boston Area Moral Cognition Group (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
9. **2015, Washington University St. Louis**, Annual Conference on Empirical Legal Studies. Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
10. **2015, Hebrew University Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
11. **2015, Bar Ilan Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
12. **2015, Haifa Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
13. **2015, IDC Law School**, Faculty workshop (invited). Paper titled: “Does Antidiscrimination Law Influence Religious Behavior?”;
14. **2016, Israeli Democracy Institute**, Human Rights and Judaism Colloquium. Paper titled: “Social Impact Discrimination”;
15. **2016, Harvard Business School**, the International Behavioral Exchange 2016 conference (invited). Presentation titled: “Law, Values, and Norm Conflicts”;
16. **2016, Hebrew University Law School**, the Israeli ICON-S conference. Paper titled: “Social Impact Discrimination”;
17. **2016, McGill Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Social Impact Discrimination”;
18. **2016, Israeli Democracy Institute**, the weekly roundtable (invited guest speaker). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
19. **2016, Bar Ilan Law School**, International Conference on Decision-making and Law (invited). Paper titled: “The Nation Law and Minority Discrimination”;
20. **2017, Jerusalem Institute**, Conference on Ultra-Orthodox Education (invited keynote speaker). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
21. **2017, Ben Gurion University**, Decision Making and Economic Psychology (DMEP) Seminar (invited). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
22. **2017, Tel Aviv Law School**, Law and Economics Workshop (invited). Paper titled: “Social Impact Regulation”;
23. **2017, Hebrew University Law School**, Public Law Workshop. Paper titled: “Social Impact Regulation”;
24. **2017, Bar Ilan Law School**, Empirical Legal Studies workshop (invited). Paper titled: “Social Impact Regulation”;
25. **2017, Notre Dame Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Mitigating Norm Conflicts by Affirming Identity: Evidence from the Core Curriculum Conflict”;
26. **2017, Cornell Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
27. **2017, Virginia Law School**, Annual Comparative Constitutional Law Roundtable (invited). Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
28. **2017, Ben Gurion University**, joint workshop of the Hebrew University Federman Center for the Study of Rationality & Ben Gurion Center on Decision Making and Economic Psychology (invited). “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
29. **2018, Hebrew University Law School**, Law and Economics Workshop. Paper titled: “Social

- Impact Regulation”;
30. **2018, Hebrew University Federman Center for the Study of Rationality**, Annual Retreat (invited guest speaker). Presentation titled: “Conflicts Between Law and Religion: An Empirical Examination”;
 31. **2018, American Law and Economics Annual Meeting (ALEA)**. Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
 32. **2018, Hebrew University Law School**, Public Law Workshop. Paper titled: “The Provocative Effect of Law: Majority Nationalism and Minority Discrimination”;
 33. **2018, Hong Kong University Law School**, ICON-S Conference. Paper titled: “The War Within”;
 34. **2018, Harvard Psychology Department**, Moral Psychology Lab (invited speaker). Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 35. **2018, Harvard-MIT Program on Negotiation Seminar Series** (invited speaker). Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 36. **2018, Harvard Law School**, Empirical Legal Studies Series (invited). Paper titled: “The War Within”;
 37. **2018, Chicago Law School**, Public Law and Legal Theory Workshop (invited). Paper titled: “The War Within”;
 38. **2018, Stanford Law School**, International Junior Faculty Forum (invited). Paper titled: “The War Within”;
 39. **2018, Michigan Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “The War Within”;
 40. **2018, Michigan Law School**, Annual Conference on Empirical Legal Studies. Paper titled: “What’s in a Name?: The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 41. **2018, University of Toronto Law School**, Law and Religion Workshop (invited). Paper titled: “The War Within”;
 42. **2018, Society on Judgment and Decision-Making Annual Conference**, New Orleans. Paper titled: “The Asymmetric Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
 43. **2018, IDC Law School**, Justice M. Cheshin Memorial Conference (invited). Paper titled: “Reexamining the Evidence on Ultra-Orthodox Attitudes and Gender Separation in Academia”;
 44. **2019, Tel Aviv Law School**, Israeli Law and Society Conference. Paper titled: “Reexamining the Evidence on Ultra-Orthodox Attitudes and Gender Separation in Academia”;
 45. **2019, Tel Aviv Law School**, faculty workshop (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
 46. **2019, WZB Berlin**, Seventh WZB Migration and Diversity Conference (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 47. **2019, Bar Ilan Law School**, Conference on the Interpretation of the Nation Law (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 48. **2019, Federman Center for the Study of Rationality**, joint workshop of the Hebrew University Federman Center & Ben Gurion Center on Decision Making and Economic Psychology (invited). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
 49. **2019, Hebrew University Law School**, public law workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;

50. **2019, Hebrew University Law School**, Humboldt-Minerva Human Rights Under Pressure joint seminar. “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
51. **2019, Hebrew University Law School**, International Workshop on Behavioral Legal Studies. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
52. **2019, Hebrew University Law School**, faculty workshop. Paper titled: “What’s in a Name?: The Disparate Effects of Identifiability on Offenders and Victims of Sexual Harassment”;
53. **2019, University of Toronto Law School**, Annual Law and Religion Roundtable (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
54. **2019, Claremont McKenna College**, Annual Conference on Empirical Legal Studies (waived). Paper titled: “How Majority Nationalism Laws Shape Intergroup Relations in Ethnically Diverse Societies: Experimental and Observational Evidence from Israel”;
55. **2020, Bar Ilan Law School**, Faculty Seminar (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”;
56. **2020, Hebrew University School**, Private Law Workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
57. **2020, Law, Society, and Psychological Science CRN Summer Research Series**, Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
58. **2020, International Forum on the Future of Constitutionalism**, Works-in-Progress Comparative Constitutional Law Summer Roundtable (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
59. **2020, Chicago Law School**, Measuring Impact in Constitutional Law Conference (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
60. **2020, Hebrew University**, Jewish and Israeli Law Student Club. Talk titled: Prayers and Demonstrations during COVID-19.
61. **2020, Ministry of Justice**, Regulation Forum. Paper titled: “Behavioral Responsive Regulation”.
62. **2020, Bar Ilan University**, Jewish and Democratic Law Center. Talk titled: “Religion in the Public Sphere”.
63. **2020, Tel Aviv University**, The Wars of the High Court of Justice Conference. TBA.
64. **2020, Israeli Democracy Institute**, Ultra-Orthodox Researchers Forum. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
65. **2020, UCLA Law School**, Private Law Workshop. Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
66. **2020, Haifa University School of Education**, Law, Religion, and Education webinar. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
67. **2020, Pepperdine Law School**, Nootbarr Fellows Workshop. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
68. **2021, Hebrew University of Jerusalem Law School**, Public Law Workshop. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
69. **2021, University of Virginia Law School**, Family Law Center Symposium, Paper titled: “The Effects on Children of Equality Rules for Religious Placement Agencies”.
70. **2021, University of Virginia Law School**, Law and Economics Workshop (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.

71. **March 2021, Tel Aviv University, School of Government, Political Science and International Relations**, faculty seminar. “The day after the elections: how to fix Israeli democracy?”
72. **March 2021, Philosophy 360**, Paper titled: “Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management”.
73. **March 2021, Rutgers University, Center for Population-Level Bioethics** seminar, Paper titled: “Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management”.
74. **March 2021, Israeli National Academy of Arts and Sciences**, (invited) special webinar, “Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management”.
75. **April 2021, Hebrew University of Jerusalem, School of Public Policy**, faculty seminar (invited). Paper titled: “Behavioral Responsive Regulation.”
76. **April 2021, IDC Herzliya Law School**, Constitutional Law Workshop (invited). Paper titled: “Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management”.
77. **April 2021, Bar Ilan Law School**, Constitutional Law Workshop (invited). Paper titled: “Personal Decisions, Public Consequences: On Distinguishing between the Vaccinated and the Non-Vaccinated in Coronavirus Management”.
78. **April 2021, Yale Law School**, Religious Freedom Clinic (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
79. **April 2021, Israeli Ministry of Justice**, Department of Consulting and Legislation Advanced Seminar in Constitutional law, (keynote speaker). “Constitutional Law: A General Perspective and Current Trends”.
80. **May 2021, Law and Society Association Annual Meeting**, virtual conference. Paper titled: “Bidirectional Legal Socialization and the Boundaries of Law: The Case of Enclave Communities Compliance with COVID-19 Regulations”.
81. **May 2021, Law and Society Association Annual Meeting**, virtual conference. Paper titled: “Behavioral Responsive Regulation.”
82. **May 2021, Hebrew University Center for Interdisciplinary Data Science Research workshop**. Paper titled: “Examining the Effect of Anti-Discrimination Legislation on Outcomes of Children in Adoption and Foster Care.”
83. **June 2021, Haifa University Law School**, faculty seminar (invited). Paper titled: “Do Religious Exemptions Increase Discrimination? Evidence from *Masterpiece*”.
84. **June 2021, Annual Law and Religion Roundtable** (invited). Paper titled: “Examining the Effect of Anti-Discrimination Legislation on Outcomes of Children in Adoption and Foster Care.”.

~ last update: June 2021

Exhibit 2

56(2) HARVARD CIVIL RIGHTS-CIVIL LIBERTIES LAW REVIEW (forth. 2021)

A License to Discriminate?

The Market Response to Masterpiece Cakeshop

Netta Barak-Corren*

What are the consequences of religious exemptions of antidiscrimination laws? And what are the normative implications of these consequences? These questions are currently at the center of a heated debate balancing religious freedom and civil rights. Opponents of religious exemptions from antidiscrimination laws argue that granting exemptions would increase sexual orientation discrimination. Proponents of religious exemptions argue that religious objectors are a small minority and that their exemption would not meaningfully increase discrimination against same-sex couples.

The troubling aspect of this debate is that none of the parties rely on hard data. Particularly missing are data on the effects of exemptions granted in Supreme Court decisions, an issue that the Court has addressed repeatedly in recent years—and is set to do so once again this term, in *Fulton v. City of Philadelphia*.

This Article intervenes in the debate based on the results of a large-scale field experiment that measured the effect of *Masterpiece Cakeshop v. Colorado Civil Rights Commission* on the ability of same-sex couples to receive services in the

* Associate Professor of Law, Hebrew University of Jerusalem. For helpful comments and suggestions I thank Oren Bar Gill, Stephanie Barclay, Ittai Bar-SimanTov, Hanoch Dagan, Rick Garnett, Fred Gedicks, Noam Gidron, Michael Helfand, Ehud Kamar, Vicki Jackson, Kobi Kastiel, Amir Khoury, Ira (Chip) Lupu, Nelson Tebbe, Mila Versteeg, Eyal Zamir, and participants at the 2019 Annual Roundtable on Law and Religion at the University of Toronto, the 2019 Workshop on Behavioral Legal Studies at the Hebrew University, the 2020 Law, Society, and Psychological Science Summer Research Series, the 2020 International Forum on the Future of Constitutionalism Summer Roundtable, the 2020 Measuring Impact in Constitutional Law Conference at Chicago Law School, the Humboldt-Minerva Human Rights Under Pressure Seminar, and faculty and students in Bar Ilan University, the Hebrew University, and Tel Aviv University. Tamir Berkman provided outstanding research assistance. Responsibility for any errors is my own. The online appendix is available at <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>. Anonymized data and code are stored on the Open Science Framework, https://osf.io/ve5yn/?view_only=8853549b0fc248afb793ef41d4e953f8, archived at <https://perma.cc/57DR-CFB2>.

wedding market, as compared to opposite-sex couples. The field experiment revealed that after *Masterpiece Cakeshop*, vendors were less willing to provide wedding services to same-sex couples than to opposite-sex couples. This trend was true even for vendors that provided services to same-sex couples prior to the *Masterpiece Cakeshop* decision. Following *Masterpiece Cakeshop*, the odds that same-sex couples would experience discrimination from wedding vendors are estimated to be between 61% and 85%.

These results have several implications for the debate on religious exemptions. First, they discredit the argument that the effects of religious exemptions are negligible, making clear that exemptions *will* promote further discrimination. Second, the results complicate the conventional portrait of religious objection as fixed (and therefore unyielding to change), showing instead that the demand for discrimination is elastic and shaped by social constructions, even without coercion or sanctions. These negative effects of *Masterpiece Cakeshop* bear on both litigation—showing that antidiscrimination laws are necessary to further states’ compelling interest in securing equality—and in legislation—providing guidance for legislatures on whether and how to enact religious exemptions from antidiscrimination laws.

Finally, the troubling consequences of *Masterpiece Cakeshop* require the Supreme Court to proceed with great care as it sets out to decide *Fulton v. City of Philadelphia* and any other future cases raising ostensible conflicts between religion and anti-discrimination law. However the Court decides to resolve the constitutional issue at hand, it must take into account that even a deliberately narrow and case-specific exemption might have a significant negative impact on the market and its customers.

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INTRODUCTION

The conflict between religious liberty and marriage equality is escalating. This term, the Supreme Court is set to decide *Fulton v. City of Philadelphia*,¹ a case which raises the constitutionality of an antidiscrimination rule that denies religious exemptions for state contractors who refuse to serve same-sex couples. Only two years ago, the Court addressed the conflict in the private market context, in *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Commission*, ruling for a baker who refused to create a wedding cake

¹ *Fulton v. City of Philadelphia*, 922 F.3d 140, 146–47 (3rd Cir. 2019), *cert. granted*, 140 S. Ct. 1104 (2020) (No. 19-123).

for a same-sex couple.² Shortly after the decision, the Court vacated and remanded two similar cases, one involving a florist who would not create flower arrangements for a same-sex wedding³ and another case involving wedding cakes.⁴ An impressive number of similar cases have been making their way through the courts in recent years, involving photographers and video artists,⁵ a web-designer,⁶ a t-shirt store,⁷ a custom wedding invitation studio,⁸ and a bed and breakfast⁹—all of whom object to serving same-sex couples and seek exemptions from antidiscrimination laws.

This state of affairs has caused anxiety and controversy among citizens, lawmakers, and legal scholars. All of these groups are concerned with potential on-the-ground consequences of religious exemptions from antidiscrimination laws. Opponents of religious exemptions warn that granting exemptions will escalate the number and significance of faith claims and could expand sexual orientation discrimination to all facets of public life.¹⁰ Proponents of religious exemptions reject these concerns as factual nonsense, arguing that religious objectors are a negligible minority in a society growing ever more affirming of marriage equality, and that exempting religious objectors will not exacerbate discrimination against same-sex couples.¹¹

The relationship between religious exemptions from antidiscrimination law and the actual consequences for same-sex couples and for religious objectors is thus a central question. Yet there is almost no evidence that could

² Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719, 1720 (2018).

³ Arlene's Flowers, Inc. v. Washington, 138 S. Ct. 2671 (2018), *remanded to State v. Arlene's Flowers, Inc.*, 441 P.3d 1203, 1209 (Wash. 2019). A second petition for certiorari was filed in 2019, and was not decided by the time this article went to press. *See* Petition for Writ of Certiorari, Arlene's Flowers, Inc. v. Washington, No. 19-333 (U.S. Sept. 11, 2019).

⁴ Klein v. Or. Bureau of Lab. & Indus., 139 S. Ct. 2713 (2019).

⁵ Telescope Media Grp. v. Lucero, 936 F.3d 740, 747 (8th Cir. 2019).

⁶ 303 Creative L.L.C. v. Elenis, 405 F. Supp. 3d 907, 912 (D. Colo. 2019), *appeal docketed*, No. 19-01413 (10th Cir. argued Nov. 16, 2020).

⁷ Lexington-Fayette Urban Cnty. Hum. Rts. Comm'n v. Hands On Originals, 592 S.W.3d 291, 294 (Ky. 2019).

⁸ Brush & Nib Studio v. City of Phoenix, 448 P.3d 890, 897 (Ariz. 2019).

⁹ Aloha Bed & Breakfast v. Cervelli, 415 P.3d 919, 923 (Haw. Ct. App. 2018), *cert. denied*, 139 S. Ct. 1319 (2019).

¹⁰ *See infra* notes 66–69.

¹¹ *See infra* notes 76–82.

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help clarify which of the contradictory factual premises is actually true. Such evidence is required to inform legislators debating whether to enact religious exemptions, and courts deliberating whether to grant such exemptions. Underscoring the importance of the consequentialist consideration, Justice Kennedy asked the U.S. Solicitor General¹² during the *Masterpiece Cakeshop* oral arguments, “what would the government's position be if . . . the baker prevails in this case, and then bakers all over the country received urgent requests: Please do not bake cakes for gay weddings. And more and more bakers began to comply. Would the government feel vindicated in its position that it now submits to us?”¹³ The Solicitor General responded that this would make the case for antidiscrimination “much stronger” because states would be able to show “that the application of the law is narrowly tailored to the government’s interest in ensuring access [to public accommodations].”¹⁴ Justice Kennedy was not alone on the bench in considering the consequences of religious exemptions as the key for the decision to grant them. From *Employment Division v. Smith*¹⁵ to *Burwell v. Hobby Lobby Stores, Inc.*,¹⁶ the Court has consistently cited consequentialist concerns (or lack thereof) in rejecting (or granting) requested religious exemptions.

This article contributes to the consequentialist debate on religious exemptions by studying, for the first time, the effects of religious exemptions on sexual orientation discrimination. Part I begins with surveying the relevant legal background on the tension between marriage equality and religious liberty. It addresses the evolution of the conflict and the legislative patchwork of protections across the nation, where some jurisdictions prohibit sexual orientation discrimination in public accommodations (dubbed here “AD Law” regimes, for convenience purposes) and others do not; where some

¹² The U.S. Solicitor General argued as amicus curiae supporting the baker in *Masterpiece Cakeshop*. See Transcript of Oral Argument at 2, *Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm’n*, 138 S. Ct. 1719 (2018) (No. 16-111).

¹³ *Id.* at 45–46.

¹⁴ *Id.* at 46.

¹⁵ 494 U.S. 872, 879 (1990) (noting the concern that permitting an exemption is “in effect to permit every citizen to become a law unto himself”) (quoting *Reynolds v. United States*, 98 U.S. 145, 167 (1879)).

¹⁶ 573 U.S. 682, 692–93 (2014) (“[O]ur holding is very specific. . . . [W]e certainly do not hold or suggest that ‘RFRA demands accommodation of a for-profit corporation’s religious beliefs no matter the impact that accommodation may have’ The effect of the HHS-created accommodation on the women employed by Hobby Lobby and the other companies involved in these cases would be precisely zero.”).

jurisdictions facilitate religious exemptions via a Religious Freedom Restoration Act (“RFRA”) and others do not.¹⁷ This varied setting provides the context for the debate about religious exemptions and for *Masterpiece Cakeshop* itself. Part I concludes with analyzing the opposing consequentialist arguments about religious exemptions, exposing their lack of evidentiary foundations, and the implications of these omissions.

Part II then describes the large-scale field experiment designed to elucidate and inform the consequentialist debate by measuring the impact of the *Masterpiece Cakeshop* decision on sexual orientation discrimination in the wedding vendor market. An extended analysis of the experiment is reported in a separate methodological paper.¹⁸ Wedding vendors (bakers, photographers, and florists; N = 1,155) were sampled from the four types of regimes currently in existence (those with or without antidiscrimination laws; and those with or without religious freedom laws). Each business was contacted via email by four different couples: two shortly before and two shortly after the *Masterpiece Cakeshop* ruling; in each period, one of the test couples was a same-sex couple and the other was an opposite-sex couple. The total dataset includes four observations per business, allowing for both within- and across-businesses comparisons. The question of interest was whether businesses agreed to provide the requested service to the couples.

What were the results of the field experiment? In brief, the field experiment demonstrated that the Court’s decision in *Masterpiece Cakeshop* significantly reduced the willingness of businesses to serve same-sex couples: while 63.6% were willing to serve same-sex couples before *Masterpiece Cakeshop*, only 49.2% were so willing after the decision was rendered (a 14.4 percentage-point gap, or ~23 percent decrease in favorable responses). Zooming in on businesses that, prior to *Masterpiece Cakeshop*, responded positively to same-sex couples, I find that many of these businesses discriminate between opposite-sex and same-sex couples after *Masterpiece Cakeshop*: previously “gay-friendly” businesses that were

¹⁷ These are broad distinctions. Additional nuances are discussed *infra* Part II.B.

¹⁸ Netta Barak-Corren, *Religious Exemptions Increase Discrimination Towards Same-Sex Couples: Evidence from Masterpiece Cakeshop*, J. LEGAL STUD. (forthcoming 2021). Parts of the current article have been adapted from there. The research was approved by the Hebrew University IRB.

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randomly contacted by opposite-sex or same-sex couples responded less favorably to same-sex couples than opposite-sex couples (75.5% vs. 66.3%, a 9 percentage-point gap, or 12 percent fewer favorable responses) after the decision was rendered. This effect is not an artifact of the experiment itself, as it is identically found in the “control” group of businesses that were contacted for the first time after *Masterpiece Cakeshop*. Probing into the differences between the four regime types, I find that the negative *Masterpiece Cakeshop* effect appears in all regimes, including regimes without AD laws and regimes without RFRA, except for those that enacted *both* an AD law and a RFRA. The effect is robust, and remains so when including analyses that control for county-level conservativeness and analyses limited to businesses located in big cities (where, it is often argued, there is considerably less of a discrimination problem). However, the effect of *Masterpiece Cakeshop* is significantly more pronounced in religious environments, as proxied by the density of religious congregations in the county where the business is located.

A back-of-the-envelope calculation demonstrates the broader implications of these results. Provided that couples of all identities typically contract with about ten types of vendors in the process of organizing a wedding (reception venues, wedding planners, bakers, florists, photographers, videographers, bridal/groom salons, jewelers, DJs, and calligraphers—a partial list), that they often inquire with several vendors from each category, and that the average risk of experiencing discrimination per vendor post-*Masterpiece Cakeshop* is about 9%, I estimate the aggregate risk of experiencing at least one instance of discrimination ranges between 61% and 85% for same-sex couples. This means that across the observed differences between businesses, legal regimes, and religious environments, *Masterpiece Cakeshop* had the general effect of exposing same-sex couples to a substantial and heightened risk of discrimination while planning a wedding.

With this novel evidence, Part III returns to the normative debate and considers the implications of the law of religious exemptions. First, the results of the *Masterpiece Cakeshop* experiment discredit the argument that the effect of religious exemptions is negligible and that exemptions will not promote discrimination. Instead, what the *Masterpiece Cakeshop* experiment

shows is that even an intentionally narrow and case-specific exemption can have a substantial impact on an industry and its customers. Second, the results complicate the conventional portrait of religious objection as fixed, showing instead that the demand for discrimination is elastic and shaped by social constructions, even without coercion or sanctions. Third, this evidence makes clear that states and localities have a compelling interest in passing and enforcing anti-discrimination laws, and that such laws are narrowly tailored to that interest. Antidiscrimination laws thus satisfy strict scrutiny (and lower thresholds of judicial review, where applicable).

At the same time, the documented variation between legal regimes tentatively suggests that there is still room for legislatures to explore ways to protect *both* marriage equality and religious freedom, without necessarily increasing discrimination. I suggest specific ways in which legislators could improve the regulation of religion-equality conflicts, by actively seeking to ground policy in data. In particular, I argue that new laws should be experimentally pre-tested to inform lawmakers as to the likely consequences. I demonstrate how such pre-testing could be performed and I explain its advantages.

As is true for any empirical work, this article does not purport to exhaust or conclude the debate about the consequences of religious exemptions. Indeed, this would be impossible. The article is a snapshot of reality at a specific point in time and place and is limited in what such a snapshot can reveal about society—particularly when it comes to complex phenomena such as the relationship between law and behavior. Notwithstanding these important limitations, the centrality of empirical assumptions to the resolution of the debates in constitutional law requires us to grapple with the empirical questions rather than treating them as axioms. The current debate illustrates this need well. Opponents and proponents of religious exemptions rely on conflicting assumptions regarding the consequences of exemptions, largely talking past each other. While there is no assurance that the opposing camps will digest empirical evidence willingly and without bias, there is always hope that at least some will (indeed, this is the underlying premise of all scientific work). At the very least, disagreements about the relevance of the data could increase the sophistication of legal arguments and generate new questions for debate and empirical investigation. For now, the troubling

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effects of *Masterpiece Cakeshop* suggest that the Supreme Court should question empirical arguments that do not base themselves on relevant data and should carefully consider the probable consequences of its impending decision in *Fulton v. City of Philadelphia* and in any other religion-equality conflict that will come before the Court in the future.

I. THE TENSION BETWEEN MARRIAGE EQUALITY AND RELIGIOUS LIBERTY

The tension between sexual orientation equality and religious liberty has been present from the inception of the movement for marriage equality. When Massachusetts became the first State to recognize same-sex marriage in 2004, the Supreme Judicial Court recognized the possibility of such a conflict when it asserted that its “decision [to uphold same-sex marriage laws] in no way limits the rights of individuals to refuse to marry persons of the same-sex for religious or any other reasons. It in no way limits the personal freedom to disapprove of, or to encourage others to disapprove of, same-sex marriage.”¹⁹ Similarly, when the Iowa Supreme Court recognized same-sex marriage—the fourth high court to follow this route, after Massachusetts, California and Connecticut—it stated that “[r]eligious doctrine and views contrary to this principle of law are unaffected, and people can continue to associate with the religion that best reflects their views.”²⁰ In 2015, when the U.S. Supreme Court legalized same-sex marriage across the nation in *Obergefell v. Hodges*, it emphasized that “religions, and those who adhere to religious doctrines, may continue to advocate with utmost, sincere conviction that, by divine precepts, same-sex marriage should not be condoned.”²¹

Other courts expressed reservations about the possibility of mitigating the tension between religion and sexual orientation equality. When the Connecticut Supreme Court recognized same-sex marriage in 2008, it dedicated a lengthy paragraph to describe the religious condemnation of homosexuality and to present it as one of the roots of discrimination towards

¹⁹ *Goodridge v. Dep’t of Pub. Health*, 798 N.E.2d 941, 965 n.29 (Mass. 2003).

²⁰ *Varnum v. Brien*, 763 N.W.2d 862, 906 (Iowa 2009).

²¹ 576 U.S. 644, 679 (2015). Notably, any reference to a potential tension between religious liberty and marriage equality was omitted from a previous marriage equality decision, *United States v. Windsor*, in which the Court struck down the Defense of Marriage Act, a federal law defining marriage as an act between a man and a woman. 570 U.S. 744, 745 (2013).

gay people in society.²² The court then observed that “[f]eelings and beliefs predicated on such profound religious and moral principles are likely to be enduring, and persons and groups adhering to those views undoubtedly will continue to exert influence over public policy makers.”²³ Several years later, Justice Alito dissented from the United States Supreme Court’s decision in *Obergefell* with the opposite prediction, expressing concern that “those who cling to old beliefs will be able to whisper their thoughts in the recesses of their homes, but if they repeat those views in public, they will risk being labeled as bigots and treated as such by governments, employers, and schools.”²⁴

Whether mitigating this tension is possible or not remains to be seen. What is clearly evident, however, is that religion-equality conflicts are rapidly gaining legal momentum and public attention. As the primary origin of these conflicts has been state law, it is necessary to understand the variation between states to assess the background against which religious exemptions are debated.

A. Antidiscrimination Laws and Claims for Religious Exemptions

At present, federal law does not prohibit discrimination on the basis of sexual orientation in public accommodations. Title II of the Civil Rights Act does not prohibit discrimination on the basis of either sex or sexual orientation;²⁵ even if it did, it limits “public accommodation” to hotels, restaurants, gas stations, and places of exhibition or entertainment.²⁶ This definition does not include most of the businesses currently refusing service to same-sex couples, in particular most wedding vendors.²⁷

²² *Kerrigan v. Comm’r of Pub. Health*, 957 A.2d 407, 444–45 (Conn. 2008).

²³ *Id.* at 445.

²⁴ *Obergefell*, 576 U.S. at 741 (Alito, J., dissenting).

²⁵ 42 U.S.C. § 2000a(a). This omission is in contrast to the prohibition on discrimination on the basis of “sex” in employment in Title VII of the Civil Rights Act, a provision that has been interpreted as also covering sexual orientation discrimination. *Bostock v. Clayton Cnty.*, 140 S. Ct. 1731, 1754 (2020).

²⁶ 42 U.S.C. § 2000a(b).

²⁷ *See, e.g.*, *State v. Arlene's Flowers, Inc.*, 441 P.3d 1203, 1209 (Wash. 2019); *Telescope Media Grp. v. Lucero*, 936 F.3d 740, 747 (8th Cir. 2019); *303 Creative L.L.C. v. Elenis*, 405 F. Supp. 3d 907, 912 (D. Colo. 2019), *appeal docketed*, No. 19-01413 (10th Cir. argued Nov. 16, 2020).

Acting to fill the void, twenty-two states, the District of Columbia, and numerous local governments passed legislation prohibiting discrimination based on sexual orientation and/or gender identity in public accommodations (“AD states,” see Figure 1).²⁸ Most of these laws permit no religious exemptions,²⁹ and their definitions of “public accommodations” are generally much broader than that of federal law,³⁰ covering any business open to the public. These laws are the underpinnings of the lawsuits against wedding vendors that refused to provide service to same-sex commitment ceremonies and weddings, citing religious reasons.³¹ Concomitantly, and particularly after the recognition of marriage equality in *Obergefell*, conservative faith groups began calling for religious exemptions from AD laws.³² On the legislative front, some states took steps to advance these calls.³³ In courts,

²⁸ HUM. RTS. CAMPAIGN FOUND., 2020 STATE EQUALITY INDEX 14 (2020), <https://hrc-prod-requests.s3-us-west-2.amazonaws.com/HRC-SEI20-report-Update-022321-Final.pdf?mtime=20210322114741&focal=none>, archived at <https://perma.cc/YG5Y-35Q2>. Note that Figure 1 presents the state of the law at the time of the study in 2018, before Virginia prohibited discrimination on the basis of sexual orientation in public accommodations.

²⁹ Many states provide exemptions for churches and affiliated religious organizations, but these exemptions mostly do not extend to private for-profit businesses. See Lucien J. Dhooge, *Public Accommodation Statutes and Sexual Orientation: Should There Be a Religious Exemption for Secular Businesses?*, 21 WM. & MARY J. WOMEN & L. 319, 344 (2015).

³⁰ For example, IOWA CODE § 216.2.13(a) defines “public accommodation” as “each and every place, establishment, or facility of whatever kind, nature, or class that caters or offers services, facilities, or goods for a fee or charge to nonmembers of any organization or association utilizing the place, establishment, or facility.”

³¹ Such lawsuits against wedding vendors have been brought in various states. See *Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm’n*, 138 S. Ct. 1719 (2018) (in Colorado); *Arlene’s Flowers*, 441 P.3d at 1209 (in Washington); *Telescope Media Grp.*, 936 F.3d at 740 (in Minnesota); *Brush & Nib Studio v. City of Phoenix*, 448 P.3d 890 (Ariz. 2019) (in Arizona); *Cervelli v. Aloha Bed & Breakfast*, 415 P.3d 919 (Haw. Ct. App. 2018), *cert. denied*, 139 S. Ct. 1319 (2019) (in Hawaii); *Klein v. Or. Bureau of Lab. & Indus.*, 410 P.3d 1051 (Or. Ct. App. 2017) (in Oregon); *Elane Photography, L.L.C. v. Willock*, 309 P.3d 53 (N.M. 2013) (in New Mexico).

³² Erik Eckholm, *Conservative Lawmakers and Faith Groups Seek Exemptions After Same-Sex Ruling*, N.Y. TIMES (June 26, 2015), <https://www.nytimes.com/2015/06/27/us/conservative-lawmakers-and-faith-groups-seek-exemptions-after-same-sex-ruling.html>, archived at <https://perma.cc/N48H-G6WN>; *Religious groups react to Supreme Court ruling on same-sex marriage*, TAMPA BAY TIMES (June 26, 2015), <https://www.tampabay.com/news/courts/religious-groups-react-to-supreme-court-ruling-on-same-sex-marriage/2235233/>, archived at <https://perma.cc/5X7P-X27S>.

³³ See *infra* Part I.C.

most wedding-vendor cases ended in defeat for the vendors.³⁴

Masterpiece Cakeshop was the first case in which the Supreme Court granted a petition for certiorari on the question of whether laws forbidding discrimination on the basis of sexuality violate religious freedom.³⁵ Arising under Colorado's AD law, the case presented a conflict between Jack Phillips—the owner of Masterpiece Cakeshop—and Charlie Craig and David Mullins, a same-sex couple who attempted to purchase a cake from Phillips, unaware of Phillips' beliefs. Phillips declined to make the cake, citing his objection to same-sex marriages. The parties dispute whether Phillips offered to sell other products at his store to the couple: Phillips argues that he “offered to make any other cake for them,”³⁶ but the couple argues that Phillips said that “while the bakery would sell baked goods to gay and lesbian customers for other purposes, it would not sell them baked goods for weddings”³⁷ and that “the bakery has repeatedly refused to provide any baked goods . . . for wedding receptions or commitment ceremonies of same-sex couples.”³⁸

³⁴ See, e.g., *Arlene's Flowers*, 441 P.3d at 1237; *Aloha Bed & Breakfast*, 415 P.3d at 923; *Elane Photography*, 309 P.3d at 59; 303 Creative L.L.C. v. Elenis, 405 F. Supp. 3d 907, 912 (D. Colo. 2019). *Contra Brush & Nib Studio*, 448 P.3d at 926 (holding that Phoenix's public accommodations ordinance unconstitutionally compels speech and violates the vendor's free exercise of religion).

³⁵ Before *Masterpiece Cakeshop*, the Supreme Court denied certiorari in *Elane Photography, L.L.C. v. Willock*, 572 U.S. 1046 (2014). After *Masterpiece Cakeshop*, the Supreme Court granted certiorari in *Arlene's Flowers, Inc. v. Washington*, 138 S. Ct. 2671 (2018), and *Klein v. Or. Bureau of Lab. & Indus.*, 139 S. Ct. 2713 (2019), vacating and remanding both cases for further consideration in light of *Masterpiece Cakeshop*. *Arlene's Flowers*, 138 S. Ct. at 2671; *Klein*, 139 S. Ct. at 2713.

³⁶ Petition for Writ of Certiorari at 6, *Masterpiece Cakeshop*, 138 S. Ct. 1719 (No. 16-111).

³⁷ Brief for Respondents Charlie Craig & David Mullins at 4, *Masterpiece Cakeshop*, 138 S. Ct. 1719 (No. 16-111).

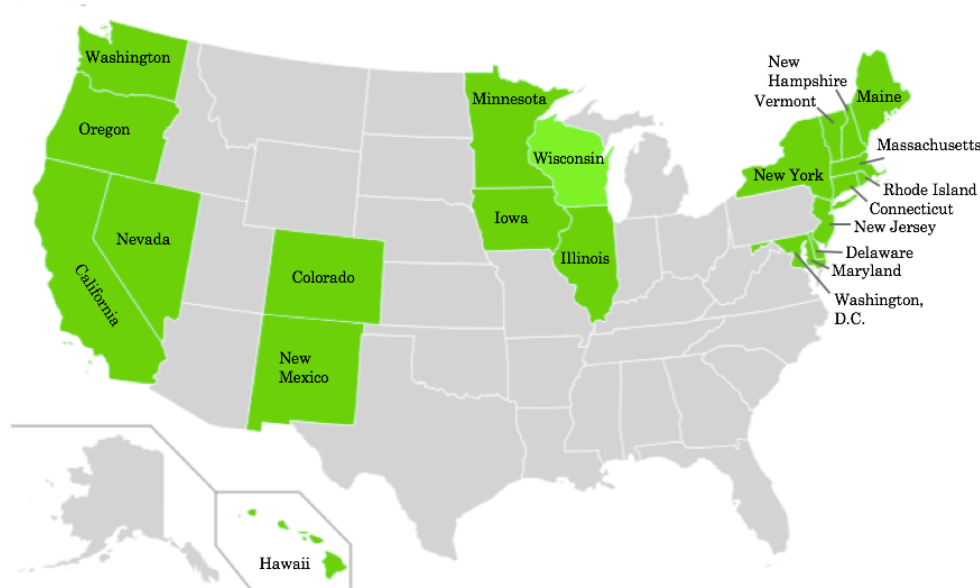
³⁸ *Id.* at 1, 4–5.

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AD states



Source: Human Rights Campaign

Figure 1. States prohibiting sexual orientation and gender identity discrimination in public accommodations as of 2018.³⁹

The Colorado Civil Rights Commission, the administrative body that adjudicates claims under the Colorado Anti-Discrimination Act, found that Phillips discriminated against the couple based on their sexual orientation. During the proceedings, a member of the Commission stated that “to me it is one of the most despicable pieces of rhetoric that people can use to—to use their religion to hurt others.”⁴⁰ Ultimately, these and related comments were among the primary reasons cited by the Supreme Court when it reversed and invalidated the Commission’s decision. The Court noted that the Commission failed to treat Phillips neutrally and fairly and instead showed

³⁹ HUM. RTS. CAMPAIGN FOUND., *supra* note 28, at 14. Wisconsin prohibits only sexual orientation discrimination. *Id.* The map does not include local governments that prohibit discrimination within their boundaries. Note that Figure 1 presents the state of the law at the time of the study in 2018, before Virginia prohibited discrimination on the basis of sexual orientation in public accommodations.

⁴⁰ *Masterpiece Cakeshop*, 138 S. Ct. at 1729.

unconstitutional religious hostility towards his beliefs.⁴¹ In a concurrence joined by Justice Gorsuch, Justice Thomas opined that Phillips should have also prevailed on free speech grounds, stating that creating and designing custom wedding cakes is a form of expressive conduct.⁴²

While Phillips won the case on free exercise grounds, the decision also affirmed the need for AD laws to protect against sexual orientation discrimination in the marketplace. The majority acknowledged that “if [religious] exception[s] were not confined, then a long list of persons who provide goods and services for marriages and weddings might refuse to do so for gay persons, thus resulting in a community-wide stigma inconsistent with the history and dynamics of civil rights laws.”⁴³ For this reason, the Court did not rule out the possibility that Colorado could eventually rule against Phillips and similarly situated vendors on the basis of its AD law if the state guaranteed a neutral and respectful process to all parties. More generally, the majority’s opinion did not expressly resolve the bigger issue of the relationship between religious liberty and sexual orientation equality.

B. Religious Freedom Laws and Claims for Religious Exemptions

Thus far, I have surveyed the tension between marriage equality and religious liberty from the standpoint of AD legislation. Another type of legislation that bears on the legal status of religion-equality conflicts are Religious Freedom Restoration Acts (“RFRAs”).

Congress enacted the first national RFRA in response to *Employment Division v. Smith*, which held that neutral laws of general applicability (i.e., those that do not intentionally target religion) are constitutional even if they substantially burden the free exercise of religion.⁴⁴ Before *Smith*, one of the

⁴¹ *Id.* at 1723. The Court also found another indication of hostility in “the difference in treatment between Phillips’ case and the cases of other bakers who objected to a requested cake on the basis of conscience and prevailed before the Commission.” *Id.* at 1730. These other bakers refused to create cakes with images that conveyed disapproval of same-sex marriage, and the Commission found their refusal legal because the bakers deemed the messages offensive. *Id.* The Court criticized this differential treatment as a showing of hostility towards Phillips’ faith. *See id.*

⁴² *Id.* at 1742 (Thomas, J., concurring).

⁴³ *Id.* at 1727.

⁴⁴ 494 U.S. 872, 879 (1990).

tests used by the Court to review substantial burdens on religious freedom required that such burdens be the least restrictive means of serving a compelling government interest (a test known as “strict scrutiny”).⁴⁵ Congress sought to enact that standard through the national RFRA, which provided that the “Government shall not substantially burden a person’s exercise of religion even if the burden results from a rule of general applicability” unless the burden serves “a compelling government interest” and is “the least restrictive means” to further that interest.⁴⁶ But the Supreme Court limited the national RFRA’s scope to the federal government and invalidated it as applied to the states.⁴⁷ Twenty-one states responded by enacting state-level RFRA to ensure that their governments are subject to the same high level of scrutiny as the federal government (see Figure 2).⁴⁸ In ten additional states, courts have interpreted their constitutions to require strict scrutiny.⁴⁹

⁴⁵ See *Sherbert v. Verner*, 374 U.S. 398, 403 (1963); *Wisconsin v. Yoder*, 406 U.S. 205, 219 (1972). Strict scrutiny, however, was not evenly or consistently applied before *Smith*, and the Court sidestepped it in a series of cases. See *O’Lone v. Est. of Shabazz*, 482 U.S. 342, 349 (1987) (declining to apply strict scrutiny to prison policy); *Goldman v. Weinberger*, 475 U.S. 503, 507 (1986) (declining to apply strict scrutiny to military policy); *United States v. Lee*, 455 U.S. 252, 261 (1982) (declining to exempt Amish employers from social security policy); *Bob Jones Univ. v. United States*, 461 U.S. 574, 604 (1983) (declining to reverse the denial of tax benefits of a university, stating that government’s interest in eradicating racial discrimination outweighs the burden on the university’s religious exercise). See generally Ira C. Lupu, *Hobby Lobby and the Dubious Enterprise of Religious Exemptions*, 38 HARV. J. L. & GENDER 35, 51–53 (2015) (exploring the court’s treatment of free exercise claims pre-*Smith*).

⁴⁶ 42 U.S.C. § 2000bb to 2000bb-4 (1993), *invalidated by* *City of Boerne v. Flores*, 521 U.S. 507 (1997) (finding RFRA unconstitutional as applied to the states).

⁴⁷ *City of Boerne*, 521 U.S. at 532. Congress amended RFRA to reflect the holding and removed the words “a State, or a subdivision of a State” from the definition of “government” in the law. See 42 USC § 2000bb-2.

⁴⁸ *State Religious Freedom Restoration Acts*, NAT’L CONF. STATE LEGISLATURES (May 4, 2017), <http://www.ncsl.org/research/civil-and-criminal-justice/state-rfra-statutes.aspx>, archived at <https://perma.cc/QV3E-F67V> [hereinafter NCSL] (providing an up-to-date survey of all RFRA’s).

⁴⁹ Eugene Volokh, 1A. *What Is the Religious Freedom Restoration Act?*, VOLOKH CONSPIRACY (Dec. 2, 2013), <http://volokh.com/2013/12/02/1a-religious-freedom-restoration-act/>, archived at <https://perma.cc/DXZ6-9PLN> (surveying state RFRA’s and state interpretations of their constitutions to require strict scrutiny; since then, AR, IN, and MI also enacted RFRA’s).

RFRA states



Source: National Conference of State Legislatures

www.guttmacher.org

Figure 2. States that have enacted Religious Freedom Restoration Acts as of 2018. Note: The map does not include states that interpret their constitutions to require a RFRA-like protection of religious freedom: AK, MA, ME, MI, MN, MT, NC, OH, WA, and WI.⁵⁰

While RFRA's do not provide absolute guarantees of religious exemptions, conservative legislators in RFRA-less states began pushing for the enactment of RFRA's as a shield (or in some cases, a sword) against potential duties to recognize the validity of same-sex marriage. Mississippi passed a RFRA in 2014; Indiana and Arkansas in 2015.⁵¹ Yet in other states, such as Iowa and Georgia, RFRA bills failed due to public concerns about their implications for LGBTQ rights and fears of commercial boycotts.⁵² In the process, RFRA's came to be viewed as the legislative opposite of AD laws.⁵³

⁵⁰ *Id.*

⁵¹ NCSL, *supra* note 48.

⁵² Kathleen Foody, *Ga. lawmakers leave without vote on religious freedom bill*, WASH. TIMES (Apr. 3, 2015), <https://www.washingtontimes.com/news/2015/apr/3/religious-freedom-measure-focus-of-ga-lawmakers-la/>, archived at <https://perma.cc/77XQ-AUDA>.

⁵³ See, e.g., David Ferguson, *LGBT rights amendment proves to be 'poison pill' for*

C. The Implications of the “Legislative Mismatch”

Figures 1 and 2 show that the distribution of AD laws and RFRA across states is what Professor Lupu has termed a “legislative mismatch” with a relatively narrow overlap. As Professor Lupu notes, the overlap consists of four states that enacted both laws (Connecticut, Illinois, New Mexico, and Rhode Island), a maximum of seven states that have both AD laws and extended protections on religious freedom in their constitutions but no RFRA (Maine, Massachusetts, Minnesota, New York, Vermont, Washington, and Wisconsin),⁵⁴ and a considerable number of local governments in RFRA states that enacted municipal AD laws. This last category includes a number of major cities in conservative states, such as Dallas, Texas, Indianapolis, Indiana, Phoenix, Arizona, and Atlanta, Georgia.⁵⁵

The legal variation that results from the “legislative mismatch” potentially entails very different outcomes for otherwise identical cases. Imagine a photographer refusing to take the engagement photos of a same-

Georgia’s ‘religious freedom’ bill, RAWSTORY (Mar. 27, 2015), <https://www.rawstory.com/2015/03/lgbt-rights-amendment-proves-to-be-poison-pill-for-georgias-religious-freedom-bill/>, archived at <https://perma.cc/39CX-J8J6> (reporting how the passage of an amendment preventing the bill from affecting the state’s civil rights laws collapsed support of the bill).

⁵⁴ Ira C. Lupu, *Moving Targets: Obergefell, Hobby Lobby, and the Future of LGBT Rights*, 7 ALA. C.R. & C.L.L. REV. 1, 45–46 (2015) (classifying states into categories, and since which, no new laws have been enacted to change this classification). Some uncertainty exists as to which states have interpreted their constitutions to require a RFRA-like standard of review. Volokh, *supra* note 49, classifies Hawaii and Vermont as states where courts have explicitly noted uncertainty about whether their constitution entails such a standard, and declined to resolve it, and New York as a state with weak intermediate review. In Hawaii, that uncertainty was recently noted in a case of a wedding service refusal. *Cervelli v. Aloha Bed & Breakfast*, 415 P.3d 919, 934 (Haw. Ct. App. 2018) (“We need not decide whether a higher level of scrutiny should be applied to a free exercise claim under the Hawai’i constitution . . . because we conclude that [Hawaii AD law] satisfies even strict scrutiny as applied to Aloha B&B’s free exercise claim.”).

⁵⁵ Lupu, *supra* note 54, at 46; *Texas’ Equality Profile*, MOVEMENT ADVANCEMENT PROJECT, https://www.lgbtmap.org/equality_maps/profile_state/TX, archived at <https://perma.cc/BC78-VFGU> (last visited Apr. 3, 2021); *Indiana’s Equality Profile*, MOVEMENT ADVANCEMENT PROJECT, https://www.lgbtmap.org/equality_maps/profile_state/IN, archived at <https://perma.cc/A77G-U2A9> (last visited Apr. 3, 2021). No local government in an AD state has enacted a municipal RFRA thus far. Local laws are typically enforceable by complaint to a city agency.

sex couple. In solely AD states, a discrimination claim will likely result in victory for the couple.⁵⁶ In solely RFRA states, such claim will likely fail. In states that enacted neither type of law (e.g., North Carolina), the claim's fate will likely be similar to RFRA states, if only because there is no vehicle to bring an antidiscrimination claim forward. And in the overlap category, where both sexual orientation and religious freedom are afforded legislative protections, the claim's fate would depend on how courts interpret the relationship between the two laws, including their potential application of strict scrutiny to the state's AD law.

Although one may assume that the conflict is strongest in the overlap states, it is not necessarily the case. For example, the four states with both AD laws and RFRAs construed their RFRA to apply only to government agencies, excluding legislatures and courts, or limited relief to be only against the government, excluding private parties.⁵⁷ This structure led the New Mexico Supreme Court to reject the claim that the state's RFRA prevents the application of the state's AD law to a photographer declining service to a same-sex couple.⁵⁸ Courts in Washington⁵⁹ and Hawai'i⁶⁰—states that Lupu

⁵⁶ The analysis in this paragraph assumes the state of the law at the time when the study at the heart of this Article was designed, under which there is no constitutional requirement to exempt religious vendors from generally applicable AD laws. That fact did not change following *Masterpiece Cakeshop*, because the Court found for Phillips on the basis of governmental hostility and did not reach the question of whether Phillips had a right to an exemption from AD laws. 138 S. Ct. 1719, 1723–24, 1732 (2018).

⁵⁷ Rhode Island defines “government” to exclude the legislature and the courts and sets the remedies to be “injunctive and declaratory relief against any governmental authority which commits or proposes to commit a violation of this chapter.” R.I. GEN. LAWS § 42-80.1 (2010). Connecticut defines “state or any political subdivision of the state” to exclude the legislature and the courts and sets the right to relief only against the state. CONN. GEN. STAT. § 52-571b (1993). New Mexico is very similar to both, as explained below. N.M. STAT. ANN. § 28-22 (2000). Illinois defines “government” to include “a branch” but sets the right to appropriate relief in section 20 only “against a government.” 775 ILL. COMP. STAT. 35 (1998).

⁵⁸ *Elane Photography, L.L.C. v. Willock*, 309 P.3d 53, 59, 76 (N.M. 2013) (holding that because the NMRFRA does not apply to the legislator and the courts, and sets remedies only against government agencies, it does not insulate businesses from the legislature's prohibition on discrimination and does not shield them from discrimination lawsuits by private parties, including same-sex couples).

⁵⁹ *State v. Arlene's Flowers, Inc.*, 441 P.3d 1203, 1234 (Wash. 2019) (holding that Washington's AD law survives strict scrutiny).

⁶⁰ *Cervelli v. Aloha Bed & Breakfast*, 415 P.3d 919, 934 (Haw. Ct. App. 2018) (holding that, even if the Hawai'i constitution requires strict scrutiny, the Hawai'i AD law survives it).

classifies as hybrid because of RFRA-like constitutional norms⁶¹—reached a similar result, each ruling that the state's AD law survives strict scrutiny. Overall, a large part of this overlap category appears to be more similar to the AD-only category when it comes to religion-equality conflicts.

The potentially more conflicted overlaps are where RFRA's are construed to apply to state laws (not only executive agencies), without excluding relief against private parties. Such are the Texas and Indiana RFRA's,⁶² and new RFRA bills have followed this model.⁶³ Both the Texas and Indiana RFRA's include language stating that the Act does not authorize or establish a defense for discrimination or breach of civil rights laws for any individual or organization other than religious non-profits.⁶⁴ But, as neither state has AD laws that prohibit discrimination on the basis of sexual orientation, these reservations appear to be relevant only in municipalities within these states that enacted local AD protections.⁶⁵ These clauses are yet to be interpreted by courts as to whether they resolve the tension or not. More generally, RFRA's do not provide a flat guarantee of exemption, only the possibility of securing an exemption subject to certain legal conditions. Therefore, even expansive RFRA regimes do not guarantee religious vendors a right to refuse to serve same-sex couples, although they increase the likelihood that such

⁶¹ Lupu, *supra* note 54, at 45–46.

⁶² TEX. CIV. PRAC. & REM. CODE ANN § 110.014 (1999) (“A person whose free exercise of religion has been substantially burdened . . . may assert that violation . . . without regard to whether the proceeding is brought in the name of the state or by any other person.”); IND. CODE § 34-13-9-7 (2015) (“regardless of whether the state or any other governmental entity is a party to the proceeding”).

⁶³ In addition to the newly enacted Indiana and Mississippi RFRA's, MISS. CODE § 11-61-1 (2014), many recent RFRA bills followed the same structure, including SB 898 in Oklahoma, HB 55 in New Mexico, SB 180 in Kentucky, SB 1062 in Arizona, etc.

⁶⁴ TEX. CIV. PRAC. & REM. CODE ANN § 110.014 (1999); IND. CODE § 34-13-9-7 (2015).

⁶⁵ It seems that this is also how these RFRA provisions have been understood in the public media. David S. Cohen & Leonore Carpenter, *The “Fix” to Indiana's Law Still Doesn't Protect Hoosiers From Anti-Gay Discrimination*, SLATE (Apr. 2, 2015), <https://slate.com/human-interest/2015/04/indiana-religious-freedom-law-the-fix-still-doesnt-protect-gay-hoosiers-from-discrimination.html>, archived at <https://perma.cc/2LSV-G5ZX> (arguing that a suggested fix in Indiana's RFRA is relevant only to the few cities that passed AD bills); Robbie Owens, *Texas Has Its Own Religious Freedom Law*, CBS DFW (Mar. 31, 2015), <https://dfw.cbslocal.com/2015/03/31/fifteen-year-old-texas-law-similar-to-new-indiana-law/>, archived at <https://perma.cc/6FAK-U52N> (claiming the Texas RFRA “can't be misused to disregard civil rights protections.”).

right is granted.

In summary, the contemporary regulation of the tension between sexual orientation equality and religious liberty encompasses four legal categories: (1) regimes (state or local) with both AD laws and RFRAs; (2) regimes that only have AD laws; (3) regimes that only have RFRAs; and (4) regimes that have none. This patchwork is the background against which *Masterpiece Cakeshop* was decided, and against which the debate on religious exemptions is raging.

D. Opposing Arguments About the Consequences of Religious Exemptions

The legislative mismatch and the inconsistent patchwork of protections for same-sex couples and religious objectors across the nation yielded two forceful and opposite responses to religious exemption laws.

In one camp are advocates and scholars that emphatically object to the legislation of new RFRAs and to most types of religious exemptions from AD laws. Much of the concern voiced by this group is about harm and consequences, perhaps most strongly articulated in Mark Stern's argument that if there is any religious accommodation, "inevitably, it will soon stretch to restaurants, hotels, movie theaters—in short, to all facets of public life. A religious right to discriminate against gay people will lead directly to anti-gay segregation."⁶⁶ Professors Douglas NeJaime and Reva Siegel take the view that claims for religious exemptions reflect the same effort to preserve traditional gender norms that characterized the religious objection to enacting these laws in the first place, what they call "preservation through transformation."⁶⁷ Hence, they argue that religious accommodations "may continue democratic conflict in new forms,"⁶⁸ and faith claims would escalate in number and significance.⁶⁹ Law professors also expressed these concerns

⁶⁶ Mark Joseph Stern, *Anti-Gay Segregation May Soon Be Coming to Oregon*, SLATE (Feb. 4, 2014), <https://slate.com/human-interest/2014/02/oregon-anti-gay-referendum-the-initiative-is-homophobic-segregation.html>, archived at <https://perma.cc/SYG8-6PUD>.

⁶⁷ Douglas NeJaime & Reva B. Siegel, *Conscience Wars: Complicity-Based Conscience Claims in Religion and Politics*, 124 YALE L.J. 2516, 2552–54 (2015).

⁶⁸ *Id.* at 2521.

⁶⁹ *Id.* at 2520; see also Douglas NeJaime & Reva Siegel, *Conscience Wars in Transnational Perspective: Religious Liberty, Third-Party Harm, and Pluralism*, in THE CONSCIENCE WARS: RETHINKING THE BALANCE BETWEEN RELIGION, IDENTITY, AND

to legislatures deliberating new RFRA's, urging them to reconsider the bills.⁷⁰

In the opposing camp are advocates and scholars—including some supporters of same-sex marriage⁷¹—who support religious exemptions. This group, which has also been active in communicating with legislators and pushing forward draft proposals for religious exemptions,⁷² rejects the consequential concerns as detached from reality. Professor Andrew Koppelman cites data from polls indicating that a majority of Americans and the vast majority of young Americans now support same-sex marriages.⁷³ Reflecting on the volume of court cases, he then claims that instances of individuals invoking religious exemptions from antidiscrimination laws are extremely rare, “a handful in a country of 300 million people.”⁷⁴ The economic purposes of antidiscrimination law, Koppelman writes, “are a response to pervasive discrimination, and therefore “they are not frustrated by discrimination which is unusual.” Based on the assumption that discrimination against same-sex couples is unusual, he argues that “[i]f gay people are generally protected against discrimination, then a few outliers

EQUALITY 187 (Susanna Mancini & Michel Rosenfeld eds., 2018).

⁷⁰ Letter from Katherine Franke, Isidor & Seville Sulzbacher Prof. of Law, Columbia University, et al., to Ed DeLaney, Rep. of Indiana (Feb. 27, 2015), https://web.law.columbia.edu/sites/default/files/microsites/gender-sexuality/law_professors_letter_on_indiana_rfra.pdf, archived at <https://perma.cc/P5AN-YMX4> (criticizing the original Indiana RFRA); letter from Ira C. Lupo, F. Elwood & Eleanor Davis Prof. of Law Emeritus, George Washington University, et al., to Gov. Nathan Deal (Jan. 21, 2015), <https://georgiaunites.org/wp-content/uploads/2015/01/Georgia-Religious-Freedom-Letter.pdf>, archived at <https://perma.cc/2HB4-LEMP> (criticizing the Georgia RFRA proposal).

⁷¹ See, e.g., Andrew Koppelman, *Gay Rights, Religious Accommodations, and the Purposes of Antidiscrimination Law*, 88 S. CAL. L. REV. 619, 620, 643–44 (2014); Douglas Laycock, *Religious Liberty and the Culture Wars*, 2014 U. ILL. L. REV. 839, 877–80 (2014).

⁷² For a collection of letters to state legislators making these and similar proposals, see Thomas Berg, *ARCHIVE: Memos/Letters on Religious Liberty and Same-Sex Marriage*, MIRROR OF JUSTICE (Aug. 2, 2009), <https://mirrorofjustice.blogspot.com/mirrorofjustice/2009/08/memosletters-on-religious-liberty-and-samesex-marriage.html>, archived at <https://perma.cc/A6MN-M2SW>. For the model exemption law advanced by this group, see Letter from Edward McGlynn Gaffney, Jr., Prof. of Law, Valparaiso Univ. Sch. of Law, et al., to Rosalyn H. Baker, State Sen., Haw. (Oct. 17, 2013), <https://mirrorofjustice.blogspot.com/files/hawaii-special-session-letter-10-17-13-1.pdf>, archived at <https://perma.cc/FUK6-H6KS>.

⁷³ Koppelman, *supra* note 71, at 624.

⁷⁴ *Id.* at 643.

won't make any difference.”⁷⁵ Similarly, Professors Thomas Berg and Douglas Laycock argue that states do not have a compelling interest in enforcing their antidiscrimination laws against religious objectors where “ample alternative providers exist (as they nearly always do).”⁷⁶ *Masterpiece Cakeshop*, in their view, is precisely one such case because other bakers were readily available to provide the service.⁷⁷ Yet, the premise that exemptions should be allowed where market alternatives exist is under-developed in these arguments. How many other bakers would need to be available to justify an exemption? And if a large number of bakers ultimately refused service, would it invalidate an otherwise justified exemption?

The question of what quantity of refusing vendors begins to erode the position of proponents of religious exemptions is left unanswered. Koppelman concedes that, in some areas of the country, many businesses might invoke an exemption; but he immediately dismisses this concern, assuming that these areas do not have antidiscrimination protections in the first place.⁷⁸ With respect to *Masterpiece Cakeshop*, Berg and Laycock simply note that the couple accepted an offer of a free wedding cake after being refused by Phillips.⁷⁹ They do not consider other potential scenarios—for example that a couple would encounter repeated refusals until finally securing a cake—or considerations—for example, that the risk of refusal might be multiplied by the number of vendors a couple typically contracts with for their wedding. Finally, proponents of religious exemptions do not consider the question of how religious exemptions might *themselves* shape market alternatives. If religious exemptions encourage more refusals, or expand to other facets of public life, as Seigel, NeJaime, and others worry,

⁷⁵ *Id.* at 627.

⁷⁶ Douglas Laycock & Thomas C. Berg, *Here's what you missed in the Supreme Court ruling in same-sex wedding cake case*, DALL. NEWS (June 14, 2018), <https://www.dallasnews.com/opinion/commentary/2018/06/14/missed-supreme-court-ruling-sex-wedding-cake-case>, archived as <https://perma.cc/QK9T-FYPH>.

⁷⁷ Thomas C. Berg & Douglas Laycock, *Masterpiece Cakeshop and Reading Smith Carefully: A Reply to Jim Oleske*, TAKE CARE (Oct. 30, 2017), <https://takecareblog.com/blog/masterpiece-cakeshop-and-reading-smith-carefully-a-reply-to-jim-oleske>, archived at <https://perma.cc/59PP-DKYT> (“The case would be different . . . if no other baker were readily available.”).

⁷⁸ Koppelman, *supra* note 71, at 644.

⁷⁹ Brief for Christian Legal Society et al. as Amici Curiae Supporting Petitioners at 30, *Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n*, 138 S. Ct. 1719 (2018) (No. 16-111) [hereinafter Berg & Laycock's Brief].

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then the premise of market alternatives could erode further.⁸⁰

It is possible that the proponents of exemptions are not worried about the potential expansion of faith-based claims because they assume that no religious objector would shy away from expressing their objection under current legal prohibitions, and thus, the only live question is how the authorities choose to treat these inevitable objections. This type of thinking is implicit in Berg and Laycock's description of religious objectors:

Those bakers willing to turn away good business for religious reasons believe that they are being asked to defy God's will, disrupting the most important relationship in their lives, a relationship with an omnipotent being who controls their fates. They believe that they are being asked to do serious wrong that will torment their conscience for a long time after. Petitioner said he would be "dishonoring" and "displeasing" "the sovereign God of the universe."⁸¹

Berg and Laycock further write that "[t]he harm of regulation on the religious side is permanent loss of identity or permanent loss of occupation."⁸² But is the assumption, that religious objection (where there is one) is an inevitable and fixed position, necessarily true? Or might different legal arrangements influence believers to either tolerate or object to same-sex marriage? This, again, is an open empirical question. If religious objection fluctuates in response to the availability of religious exemptions, and individuals who were willing to provide services to same-sex weddings become unwilling to do so once an exemption is announced, it is unclear that the vigor of Berg and Laycock's argument regarding the harm to religious objectors remains intact. In such case, more nuanced questions would need to be explored: What, really, is the magnitude of harm from not being able to refuse service to same-sex weddings? To what extent is refusal the only available religious response? And is it justified to exempt objectors for whom serving same-sex couples would truly disrupt the most important relationship

⁸⁰ NeJaime & Siegel, *supra* note 67, at 2566–74. Koppelman is aware of this concern, but he dismisses such a "cascade" as unlikely given what he considers to be the irreversible trend in social attitudes towards gay couples. Koppelman, *supra* note 71, at 644.

⁸¹ Berg & Laycock's Brief, *supra* note 79, at 31.

⁸² *Id.* at 32.

in their lives, if such exemption also causes many other vendors to refuse service that they would have otherwise provided willingly?⁸³

II. THE MASTERPIECE CAKESHOP EXPERIMENT

A. *The Motivation and Setting for the Experiment*

The primary purpose of the present experiment was to examine the contradicting empirical assumptions regarding the effects of religious exemptions on discrimination towards same-sex couples. These assumptions lie at the heart of the debate on religious exemptions, particularly in the context of weddings, yet neither side has actual data on the consequences of religious exemptions in this market or elsewhere. Even data on the more basic question—the scope of discrimination towards same-sex couples in the wedding industry or any business market—is lacking. These omissions have made it impossible to assess the merits of the opposing positions and have left the debate hanging in the air.

Masterpiece Cakeshop created an opportunity to evaluate these arguments in their most pressing setting. Based on the oral arguments, I anticipated that the Court would grant an exemption, in one format or another.⁸⁴ As “one of the most anticipated decisions of the term,”⁸⁵ the decision was also likely to draw extensive coverage and discussion in the public media (as it did), and thus potentially have an impact on public

⁸³ This is not an exhaustive list of intriguing empirical questions. One question that I do not address in this Article is that of religious same-sex couples, and how harm to religious interests should be weighed when religion is on both sides of the conflict—the vendor *and* the couple. This could be addressed in future articles.

⁸⁴ This expectation was formed based on the comments of Justice Anthony Kennedy, the Court’s swing seat, who hinted that the Court thought that there was “a significant aspect of hostility to a religion in this case.” Transcript of Oral Argument, *supra* note 12, at 54. This became a dominant line of questioning from the conservative judges on the bench. *Id.* at 54–59. Justice Kennedy also said unequivocally, “Counselor, tolerance is essential in a free society. . . . It seems to me that the state in its position here has been neither tolerant nor respectful of Mr. Phillips’ religious beliefs.” *Id.* at 64.

⁸⁵ Amy Howe, *Opinion Analysis: Court Rules (Narrowly) for Baker in Same-Sex Wedding-Cake Case [Updated]*, SCOTUSBLOG (June 4, 2018), <https://www.scotusblog.com/2018/06/opinion-analysis-court-rules-narrowly-for-baker-in-same-sex-wedding-cake-case/>, archived at <https://perma.cc/S7LZ-BVZ2>.

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attitudes and conduct.⁸⁶

When the decision was finally rendered on June 4, 2018, it received broad coverage and mixed responses. National, state, and local news outlets covered the decision and sought comment from local advocacy groups and politicians.⁸⁷ All mainstream outlets, including the New York Times, NBC News, and CNN, titled the decision a victory for the baker; they also called the decision “narrow,” explaining that it did not resolve the big constitutional questions at issue.⁸⁸ At the same time, many conservative leaders and religious liberty advocates hailed the decision as a victory, expressing significantly less reservations about its scope.⁸⁹ Fox News held a supportive

⁸⁶ Katerina Linos & Kimberly Twist, *The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods*, 45 J. LEGAL STUD. 223, 247 (2016).

⁸⁷ See, e.g., Lauren McGaughy, *Supreme Court Sides With Baker Who Refused To Make Wedding Cake For Gay Couple*, DALL. NEWS (June 4, 2018), <https://www.dallasnews.com/news/lgbt/2018/06/04/supreme-court-sides-baker-refused-make-wedding-cake-gay-couple>, archived at <https://perma.cc/T68K-5XXG>; Emma Platoff, *What the U.S. Supreme Court's Masterpiece Cakeshop decision means for religious refusal laws in Texas*, TEX. TRIB. (June 5, 2018), <https://www.texastribune.org/2018/06/05/us-supreme-court-masterpiece-cakeshop-gay-ruling-religious-freedom-tex/>, archived at <https://perma.cc/BW9D-46TF>; Katie Simpson, *New Supreme Court Ruling May Affect Indiana Religious Freedom Lawsuit*, WFYI INDIANAPOLIS (June 4, 2018), <https://www.wfyi.org/news/articles/new-supreme-court-ruling-may-affect-indiana-religious-freedom-lawsuit>, archived at <https://perma.cc/D6YY-DTR5> (describing *Masterpiece* as a victory for religious exemptions which may assist conservative groups to challenge Indiana's “weakening religious freedom protections”).

⁸⁸ Mark Goldfeder, *How the Supreme Court (respectfully) kicked the cake down the road*, CNN (June 6, 2018), <https://edition.cnn.com/2018/06/04/opinions/supreme-court-masterpiece-cakeshop-goldfeder/index.html>, archived at <https://perma.cc/HAG8-UEGC> (“Initial reviews . . . mostly imply that it was a very narrow ruling and is therefore somewhat unremarkable.”); Adam Liptak, *In Narrow Decision the Supreme Court Sides with Baker Who Turned Away Gay Couple*, N.Y. TIMES (June 4, 2018), <https://www.nytimes.com/2018/06/04/us/politics/supreme-court-sides-with-baker-who-turned-away-gay-couple.html>, archived at <https://perma.cc/655G-72YQ> (“The court’s decision was narrow The court passed on an opportunity to either bolster the right to same-sex marriage or explain how far the government can go in regulating businesses run on religious principles.”); Pete Williams, *In narrow ruling, Supreme Court gives victory to Baker Who Refused To Make Cake For Gay Wedding*, NBC NEWS (June 4, 2018), <https://www.nbcnews.com/politics/supreme-court/narrow-ruling-supreme-court-gives-victory-baker-who-refused-make-n872946>, archived at <https://perma.cc/KXG3-74PY> (“[T]he opinion was a narrow one, applying to the specific facts of this case only.”).

⁸⁹ Emilie Kao, *Why the Supreme Court's Ruling for a Christian Baker Was Not 'Narrow'*, DAILY SIGNAL (June 12, 2018), <https://www.dailysignal.com/2018/06/12/why-the-supreme-courts-ruling-for-a-christian-baker-was-not-narrow/>, archived at <https://perma.cc/ECS6-7D72> (“the decision . . . expos[ed] a huge fallacy in the ACLU’s

interview with Phillips, who defined the decision as a “big win.”⁹⁰ Leaders of the U.S. Conference of Catholic Bishops released a joint statement applauding the decision, saying that it “confirms that people of faith should not suffer discrimination on account of their deeply held religious beliefs, but instead should be respected by government officials,” emphasizing the decision’s expression of pluralism and tolerance.⁹¹ The Family Research Council released a statement that the decision “made clear that the government has no authority to discriminate against Jack Phillips because of his religious beliefs” and that the “ruling means Jack will remain free to live according to his beliefs whether he is at work, at home, or in his place of worship.”⁹² These statements do not betray any doubt about the scope of the decision or mention its recognition of the important role of AD laws in

main argument in the case . . . The court’s clear rejection of the discrimination argument has implications for many of the other conflicts currently brewing between religious freedom and sexual orientation.”); *Victory for Colorado Cake Case*, LIBERTY COUNS. (June 4, 2018), <https://www.lc.org/newsroom/details/060418-victory-for-colorado-cake-case>, archived at <https://perma.cc/9M8L-QZ23> (“Though the Court focused on the explicit hostility exhibited by the Colorado Civil Rights Commission in this specific instance, this significant decision will have a wide impact regarding the clash between free speech and the LGBT agenda, including laws that add ‘sexual orientation’ and ‘gender identity.’”).

⁹⁰ *Colorado Baker Reacts to 'Big Win' in Same-Sex Wedding Cake Case*, FOX NEWS INSIDER (June 5, 2018), <https://insider.foxnews.com/2018/06/05/same-sex-wedding-cake-case-colorado-baker-jack-phillips-supreme-court-ruling-was-big-win>, archived at <https://perma.cc/3Z2C-PDRP>; see also Todd Starnes, *A win for Masterpiece Cakeshop but it ain't over yet*, FOX NEWS (June 4, 2018), <https://www.foxnews.com/opinion/todd-starnes-a-win-for-masterpiece-cakeshop-but-it-aint-over-yet>, archived at <https://perma.cc/8STY-5Q5Z> (“Monday’s ruling should give some comfort to Christian business owners who primarily service the wedding industry – gay rights do not necessarily trump everyone else’s rights.”). Other coverage by Fox News was more careful in discussing the limitations of the decision. See, e.g., Bill Mears & Judson Berger, *Supreme Court sides with Colorado baker who refused to make wedding cake for same-sex couple*, FOX NEWS LIVE (June 4, 2018), <https://www.foxnews.com/politics/supreme-court-sides-with-colorado-baker-who-refused-to-make-wedding-cake-for-same-sex-couple>, archived at <https://perma.cc/6YHF-XMS9> (“The narrow ruling here focused on what the court described as anti-religious bias on the Colorado Civil Rights Commission when it ruled against baker Jack Phillips.”).

⁹¹ *Religious freedom groups praise Supreme Court's Masterpiece ruling*, CATH. NEWS AGENCY (June 4, 2018), <https://www.catholicnewsagency.com/news/religious-freedom-groups-praise-supreme-courts-masterpiece-ruling-57089>, archived at <https://perma.cc/NV9W-38UR>.

⁹² *Supreme Court Ruling a Victory for Freedom of Colorado Baker to Live by his Faith, says Family Research Council*, FAM. RSCH. COUNCIL (June 4, 2018), <https://www.frc.org/newsroom/supreme-court-ruling-a-victory-for-freedom-of-colorado-baker-to-live-by-his-faith-says-family-research-council>, archived at <https://perma.cc/4Q7L-Q5FX>.

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protecting against sexual orientation discrimination.

Some progressive commentators observed these enthusiastic responses and voiced concerns that *Masterpiece Cakeshop* will grant objectors a license to discriminate. Gay and Lesbian Alliance Against Defamation (“GLAAD”) President and CEO, Sarah Kate Ellis, said that it “leaves the door wide open for religious exemptions to be used against LGBTQ people.”⁹³ Annise Parker, the President of the LGBTQ Victory Institute, further warned that, “[h]omophobic forces will purposefully over-interpret the ruling and challenge existing non-discrimination laws by refusing service to LGBTQ people in even more situations.”⁹⁴ NBC News columnist, Scott Lemieux, wrote that the decision “presents a serious risk of undermining civil rights law in the name of religious freedom, especially given that it invites yet further suits for the court to consider.”⁹⁵

This combination of factors—a highly anticipated decision, a court that appeared positioned to exempt the religious objector, and the massive coverage that followed the decision and communicated the above messages—created a favorable setting for the empirical test of the effects (or lack thereof) of religious exemptions on sexual orientation discrimination. In a previous study, Professors Katerina Linos and Kimberly Twist found that Supreme Court decisions can increase support for controversial policies that were vindicated by the Court (e.g., the Affordable Care Act), even when the court was divided and the decision was nuanced.⁹⁶ Similarly, three recent studies, measuring the effect of the legalization of same-sex marriage on public attitudes, documented an increase in perceptions that social norms support same-sex marriage⁹⁷ and an increase in personal support for same-

⁹³ Nico Lang, *Hate Groups Want to Exploit Masterpiece Cakeshop Ruling as a License to Discriminate*, INTO (June 4, 2018), <https://www.intomore.com/impact/hate-groups-want-to-exploit-masterpiece-cakeshop-ruling-as-a-license-to-discriminate/>, archived at <https://perma.cc/7DLD-67AR>.

⁹⁴ *Id.*

⁹⁵ Scott Lemieux, *How the 'Narrow' Ruling in Masterpiece Cakeshop Could Undermine Future Civil Rights Cases*, NBC NEWS (June 5, 2018), <https://www.nbcnews.com/think/opinion/how-narrow-ruling-masterpiece-cakeshop-could-undermine-future-civil-rights-ncna879976>, archived at <https://perma.cc/YM2M-9EZ6>.

⁹⁶ Linos & Twist, *supra* note 86, at 247.

⁹⁷ Margaret E. Tankard & Elizabeth Levy Paluck, *The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes*, 28 PSYCH. SCI. 1334, 1339 (2017).

sex marriages⁹⁸ post-*Obergefell*, as well as a sharper decrease in antigay bias in states that legalized same-sex marriage compared with those that did not.⁹⁹ All of these studies were based on attitudinal surveys conducted shortly before and after the decisions or acts of legislation, sometimes with an additional experimental component that randomized the framing of the decision or the information provided on the decision. Yet none of these studies examined the implications of Supreme Court decisions on the behavior of decision-makers pertinent to the subject matter of the decision (in the present case, how wedding vendors are influenced from a decision pertinent to the wedding industry).

In addition, previous studies did not investigate whether effects of Supreme Court decisions vary between socio-legal regimes. As Part II explained, the variation in how states regulate sexual orientation discrimination and religious freedom is potentially important in the present case, as these background regimes yield different expectations about the legal outcomes of otherwise identical cases. These expectations could have directed wedding vendors towards different behaviors and could have differentiated their response to the *Masterpiece Cakeshop* decision. For example, business in regimes that resemble Colorado—with AD laws and without RFRA—might refuse service to same-sex couples to a greater extent post-*Masterpiece Cakeshop* if they believe that *Masterpiece Cakeshop* relaxed their AD obligations. One may also expect this change to be more pronounced in overlap regimes, because the existence of a RFRA could strengthen the impression that businesses are likely to secure an exemption post-*Masterpiece Cakeshop*. In contrast, businesses in regimes that have never enacted AD laws have no legal basis to change their behavior. For these businesses, the law has not changed: they are as free to discriminate after *Masterpiece Cakeshop* as they were before the ruling. All these hypotheses should be couched in the general caveat that businesses are not necessarily well versed in the law. Therefore, it is also possible that businesses in different legal regimes would not respond differently to *Masterpiece*

⁹⁸ Emily Kazyak & Mathew Stange, *Backlash or a Positive Response?: Public Opinion of LGB Issues After Obergefell v. Hodges*, 65 J. HOMOSEXUALITY 2028, 2040 (2018).

⁹⁹ Eugene K. Ofosu et al., *Same-Sex Marriage Legalization Associated With Reduced Implicit And Explicit Antigay Bias*, 116 PROC. NAT'L ACAD. SCI. U.S. 8846, 8849–51 (2019).

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Cakeshop. But then again, laws are not enacted at random, but are the product of certain social and political conditions. These conditions could in turn influence the acceptance and interpretation of the decision, even if businesses are not directly aware of their rights and obligations under the law. In short, investigating differences between socio-legal regimes is vital to understand whether the effect of a national Supreme Court decision is general or varies from one regime to another.

In sum, *Masterpiece Cakeshop* provided a unique opportunity to study the behavioral effect of providing a religious exemption from antidiscrimination laws, a question of which no empirical data exist to date, and which bears heavily on contemporary legal debates. In addition, the present study goes deeper than previous studies in probing the relationship between the national Supreme Court “shock” and the preexisting sub-national legal structures that could vary the effect of the decision between otherwise similar regimes.

B. Research Design

To assess whether *Masterpiece Cakeshop* had an effect on sexual orientation discrimination in the wedding industry, I combined methods from natural (pseudo) experiments and field experiments. As in such experiments, I examined the behavior of wedding businesses in two periods: before (May 8–15, 2018) and after (June 13–20, 2018) the decision (June 4, 2018). As in field experiments, the methods aimed to control for both the setting of the examination and the allocation of sexual orientation treatment between businesses, to allow for causal inference.

Sample construction began with a preliminary comparison of all states, to find those that were most comparable in their overall characteristics yet differed in legal regime. Four states were selected: Indiana, Texas, Iowa, and North Carolina. Table 1 shows that these states have roughly the same attitudinal and economic characteristics yet vary in how they regulate religious freedom and public accommodations. North Carolina has no RFRA and no AD law at any level of government (-RFRA, -AD). Iowa has no RFRA (at any level of government), but has a state AD law (-RFRA, +AD).¹⁰⁰

¹⁰⁰ IOWA CODE § 216.7 (2017). Notably, the Iowa Supreme Court has been a

Indiana and Texas model together the two final categories: both have state RFRA¹⁰¹ and no state AD laws, yet some jurisdictions within these states have local AD laws.¹⁰² Texas and Indiana jurisdictions with AD laws model the +RFRA, +AD category, whereas Texas and Indiana jurisdictions without such laws model the +RFRA, -AD category.

TABLE 1 – CHARACTERISTICS OF SAMPLED REGIMES

Criterion	Definition	IA ¹⁰³	NC ¹⁰⁴	IN ¹⁰⁵	TX ¹⁰⁶	Dallas	Houston
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trailblazer for gay rights, striking down Iowa's anti-sodomy law twenty-seven years before the U.S. Supreme Court did the same in *Lawrence v. Texas*, 539 U.S. 558, 578–79 (2003). *State v. Pilcher*, 242 N.W.2d 348, 359–60 (Iowa 1976) (en banc) (holding Iowa's criminal anti-sodomy law unconstitutional under the Fourteenth Amendment as applied to "adult persons of the opposite sex"). Iowa also became the third state in the nation to allow same-sex couples to marry when the Iowa Supreme Court legalized same-sex marriage in *Varnum v. Brien*, 763 N.W.2d 862, 907 (Iowa 2009). *See also Iowa Supreme Court legalizes gay marriage*, NBC NEWS (Apr. 3, 2009), <https://www.nbcnews.com/id/wbna30027685>, archived at <https://perma.cc/Q6BH-2LUD>. RFRA has been repeatedly proposed and rejected in the state legislature. Barbara Rodriguez, *Controversial 'religious freedom' bill gets another look at Iowa Capitol*, DES MOINES REG. (Feb. 18, 2019), <https://www.desmoinesregister.com/story/news/politics/2019/02/18/iowa-republicans-religious-freedom-restoration-act-capitol-rfra-discrimination-bill/2909230002/>, archived at <https://perma.cc/CT95-6RPZ>.

¹⁰¹ IND. CODE § 34-13-9 (2015); TEX. CIV. PRAC. & REM. CODE ANN § 110.011 (1999).

¹⁰² Jurisdictions in Indiana with AD laws include Indianapolis, Fort Wayne, Evansville, Bloomington, Muncie, South Bend, and Terre Haute. *Indiana's Equality Profile*, *supra* note 55. Jurisdictions in Indiana without AD laws include West Lafayette. *Id.* Jurisdictions in Texas with AD laws include Dallas, San Antonio, Austin, El Paso, Plano, and Fort Worth. *Texas' Equality Profile*, *supra* note 55. Jurisdictions in Texas without AD laws include Houston, Irving, Arlington, Corpus Christi, Lubbock, Garland, Amarillo, Grand Prairie, Brownsville, McKinney, Killeen, McAllen, Waco, Denton, Round Rock, and College Station. *Id.*

¹⁰³ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN IOWA (2015), <https://www.pewforum.org/religious-landscape-study/state/iowa/>, archived at <https://perma.cc/2X9G-YX76>.

¹⁰⁴ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN NORTH CAROLINA (2015), <https://www.pewforum.org/religious-landscape-study/state/north-carolina/>, archived at <https://perma.cc/3NGA-EPKU>.

¹⁰⁵ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN INDIANA (2015), <https://www.pewforum.org/religious-landscape-study/state/indiana/>, archived at <https://perma.cc/TGF4-5LQ5>.

¹⁰⁶ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN TEXAS (2015), <https://www.pewforum.org/religious-landscape-study/state/texas/>, archived at

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						Metro, TX ¹⁰⁷	Metro, TX ¹⁰⁸
GDP (\$) ¹⁰⁹		59,978	54,442	55,173	61,168	--	--
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	79%	84%	78%	86%	85%	83%
% Conservatives	(National average: 36%)	41%	40%	41%	39%	41%	38%
% Evangelicals	(National average: 25%)	28%	35%	31%	31%	38%	30%
Attitudes Towards Homosexuals	“Homosexuality should be discouraged” (National average: 31%)	36%	36%	37%	36%	35%	39%
Attitudes Towards Same- Sex Marriage	Opposing/Strongly Opposing Same- Sex Marriage (National average: 39%)	41%	45%	45%	46%	44%	51%
State RFRA ¹¹⁰		No	No	Yes	Yes	Yes	Yes
State/Local AD law ¹¹¹		Yes	No	Some	Some	Yes	No

Two reasons were responsible for the choice of Texas and Indiana as models of the overlap category (+RFRA, +AD) and the +RFRA, -AD category. As Part II describes, there are three versions of the overlap between RFRA and AD laws: (1) states that enacted both laws; (2) states that enacted an AD law and whose courts interpret the constitution to provide a RFRA-like standard; and (3) local AD laws within RFRA states. The primary reason

<https://perma.cc/DNF2-4PPF>.

¹⁰⁷ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN THE DALLAS METRO AREA (2015), <https://www.pewforum.org/religious-landscape-study/metro-area/dallasfort-worth-metro-area/>, archived at <https://perma.cc/9XQ4-79JH>.

¹⁰⁸ PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN THE HOUSTON METRO AREA (2015), <https://www.pewforum.org/religious-landscape-study/metro-area/houston-metro-area/>, archived at <https://perma.cc/5F7L-Y3ZS>.

¹⁰⁹ GDP per capita was calculated based on data from the second quarter of 2018. BUREAU OF ECON. ANALYSIS, U.S. DEP'T OF COM., GROSS DOMESTIC PRODUCT BY STATE: SECOND QUARTER 2018 (2018).

¹¹⁰ IND. CODE § 34-13-9 (2015); TEX. CIV. PRAC. & REM. CODE ANN § 110.011 (1999).

¹¹¹ This refers to public accommodation laws that apply to private businesses and are enacted at the state or city level.

for choosing the third version to model the overlap category was that the demographic and attitudinal characteristics of the four states that enacted both laws (RI, CN, NM, IL) and the states that had a RFRA without an AD law differed widely from states in the three other categories. Second, as Part II discusses, the particular RFRA design in the first overlap category was not conducive for the examination of the tension between RFRA and AD laws, while the second overlap category raised considerable uncertainty regarding the existence of the same tension. Texas and Indiana provided an adequate demographic and attitudinal comparison to the other legal categories, as well as clarity regarding the classification of their legal regimes.

To be sure, I do not argue that the design is capable of identifying a *causal relationship* between specific regimes and behavioral outcomes (as I will show next, other features of the design allow for the identification of a causal relationship in the entire sample, *across* legal regimes). First, background laws—unlike the experimental treatment—are not randomly assigned. They cannot be easily separated from the underlying political and social climate that produced them.¹¹² In addition, unlike the *Masterpiece Cakeshop* decision, they are not new, so their effect cannot be studied as a pseudo, natural experiment. Second, as discussed above, while different laws provide different behavioral guidance, businesses may not be fully aware of laws' dictates. Nevertheless, it is important to study the variation between legal regimes—if not for the direct impact of law, then for the potential impact of the underlying socio-political structures that the law reflects. Had I only sampled from one regime, important real-world variation would have been masked. Exploring how businesses in different regimes respond to *Masterpiece Cakeshop* is necessary, even if the results are only suggestive and causal inference is limited.

The sample was built by collecting information on photographers, bakers, and florists in each legal regime through a Google search, aiming to include 250 vendors per regime.¹¹³ Only vendors who published an email address

¹¹² In addition, the law is determined not only based on acts of the legislature but also based on judicial decisions and administrative directives that interpret the enacted rule. I attempted to account for those—for example, by not sampling from overlap states where courts interpreted RFRA as providing no protection against AD claims—but it is very difficult to account for all interactions between judge-made law and legislated law.

¹¹³ For an extended discussion of the sample, see Barak-Corren, *supra* note 18, at 12–

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were sampled.¹¹⁴ Vendors were contacted by email, a highly common method for communication in the wedding market. There is ample guidance online on how to write an email to potential vendors, and multiple websites assume that email is the default or best form of communication with vendors.¹¹⁵

Next, sixteen fictitious email profiles were created to facilitate the experiment. In order to assess the baseline discrimination pattern, each business received two emails prior to *Masterpiece Cakeshop* from two different “couples”: a same-sex couple (first wave) and a different-sex couple (second wave). The couples’ sexual orientation was made evident by their names. The name of the sender, appearing in the profile information and the signature, was a generic American male name (John, Robert, Dylan, Scott). The name of the prospective spouse appeared inside the body of the email and was a generic name for an American male or female, depending on the couple’s identity (Adam, Paul, Harry; Ashley, Rebecca, Jessica).¹¹⁶ The

17. The choice of vendors was influenced by recent cases in which businesses refused service to same-sex couples. *See, e.g.*, *State v. Arlene’s Flowers, Inc.*, 441 P.3d 1203 (Wash. 2019) (florists); *Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm’n*, 138 S. Ct. 1719 (2018) (bakers); *Elane Photography, L.L.C. v. Willock*, 309 P.3d 53 (N.M. 2013) (photographers).

¹¹⁴ Vendors that did not publish an email address typically had an online application form on their website, reducing the potential concern that the sample is biased towards technology-oriented vendors.

¹¹⁵ *See, e.g.*, Kelsey Malie, *How to Successfully Communicate With Your Wedding Vendors*, (Mar. 29, 2018), <http://www.kelseymaliecalligraphy.com/blog/2018/3/29/how-to-successfully-communicate-with-your-wedding-vendors>, *archived at* <https://perma.cc/JD6V-M3KY> (“An email is usually the preferred method for inquiries as it allows the vendor to keep track of your conversation, respond in length and from a desktop, and allows them to easily attach files, reference links, and more.”); Kim Forrest, *7 Ways to Effectively Communicate With Wedding Vendors*, WEDDINGWIRE (Feb. 13, 2017), <https://www.weddingwire.com/wedding-ideas/7-ways-to-effectively-communicate-with-wedding-vendors>, *archived at* <https://perma.cc/RNK2-U55G> (assuming at least some communication is done via email); Adair Currie, *How to Email Potential Wedding Vendors*, EVERY LAST DETAIL (Feb. 3, 2015), <https://theeverylastdetail.com/email-potential-wedding-vendors/>, *archived at* <https://perma.cc/32UF-38KE> (providing guidance on how to write emails to potential wedding vendors).

¹¹⁶ These are highly popular names of men and women respectively (and not of the other gender), for people born in the U.S. in the 1980s and 1990s. *See, e.g.*, SOC. SEC. ADMIN., TOP NAMES OF THE 1990S, <https://www.ssa.gov/oact/babynames/decades/names1990s.html>, *archived at* <https://perma.cc/JT2B-XJTW>. Those years are the relevant age cohorts for marriage in 2018, when the experiment was conducted. In 2018, the median age for marriage in the U.S. was 30 for men and 28 for women. *See* A.W. Geiger & Gretchen Livingston, *8 Facts About Love and Marriage in America*, PEW RSCH. CTR. (Feb. 13, 2019),

emails had similar properties, including similar information about the fictitious couple and the service requested from the vendor; they were written in the same level of cordiality. Small, meaningless changes were inserted to diminish suspicion (including variations in font size, font color, signature style, and profile pictures).¹¹⁷ The emails were sent one week apart, about the same time during the week and day, with an intentional hour lag to reduce suspicion.¹¹⁸

A week after *Masterpiece Cakeshop*, on June 13, all businesses were randomized to receive an email from a same-sex or a different-sex couple (third wave); and on the following week, each business received an email from the opposite-orientation couple (fourth wave). In each third and fourth wave, the two emails had similar properties and were different from the two pre-*Masterpiece Cakeshop* emails. Each email was always sent from a profile that has not contacted that business before. Altogether, each business received four different emails from four different profiles. Following the same procedure and schedule, a “control” group of businesses were contacted in the third and fourth waves.¹¹⁹ These businesses were contacted for the first time after the decision, to evaluate the possibility that the repeated measurement of the experimental procedure had an independent effect on business behavior.¹²⁰

C. Strengths and Weaknesses of the Experiment

The experimental design has multiple methodological strengths. First, it combines two of the most powerful methods for causal inference—pseudo-experiments and field experiments—to enable the study of an actual, concrete event—the *Masterpiece Cakeshop* decision—in a controlled setting. Second,

<https://www.pewresearch.org/fact-tank/2019/02/13/8-facts-about-love-and-marriage/>, archived at <https://perma.cc/UL9E-FKVE>.

¹¹⁷ See Barak-Corren, *supra* note 18, online app. § OA1, at 1–10, <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>. All email versions are included in this section of the appendix. *Id.*

¹¹⁸ Barak-Corren, *supra* note 18, at 18. A small group of subjects received each email 24 or 48 hours after the main group, due to logistical issues. *Id.* at 18 n.22.

¹¹⁹ *Id.* at 17. There were 251 vendors. *Id.* Additional details on the composition of the control group can be found at this source. *Id.*

¹²⁰ *Id.* at 17–20. Further details on the procedure and treatment of the data can be found at this source. *Id.*

sending carefully designed materials of fictitious individuals instead of real auditors creates a controlled setting for the study and removes inadvertent auditor biases.¹²¹ The emails enable controlling the couple's identities, how they represent themselves to businesses, the exact content of the inquiry, and the timing of the inquiry. All of these are very difficult to achieve in studies that employ real testers (audit studies). Although testers can be trained to behave similarly, it is impossible to erase the numerous differences between real people, or control for nuances in tone and facial expressions that can disclose the auditors' attitudes or that their search for a job/service is ingenuine.¹²² In addition, while email inquiries do not capture the entire variation in how couples interact with vendors, emails are one of the most common methods of communication between couples and vendors, especially in the inquiry phase.¹²³ To the extent that the process of negotiating with vendors has even moderate friction, one would expect that reduced positive responses to emails would ultimately translate into less market opportunities for same-sex couples. Third, the outcome measure—agreement to provide services to the couple—is less crude than, for example, callbacks in employment experiments that were used in previous prominent studies.¹²⁴ This is because the conflict about discrimination in wedding services focuses on the specific stage of the transaction that is studied here: the initial inquiry about the service.¹²⁵

Alongside these strengths, the experiment also has limitations. First, similar to other studies of discrimination in the field, I study asynchronous communication rather than face to face or phone communication.¹²⁶ As noted, there are good reasons for that. However, how the results translate to additional methods of communication remains an open question and a topic for a future study. In addition, the experiment examines willingness to

¹²¹ Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination*, 94 AM. ECON. REV. 991, 993–94 (2004).

¹²² *Id.*

¹²³ *See, e.g.*, sources cited *supra* note 115.

¹²⁴ Bertrand & Mullainathan, *supra* note 121, at 997.

¹²⁵ *See* Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719, 1724 (2018) (inquiry about purchasing a cake); State v. Arlene's Flowers, Inc., 441 P.3d 1203, 1211 (Wash. 2019) (inquiry about floral arrangements); Elane Photography, L.L.C. v. Willock, 309 P.3d 53, 59–60 (N.M. 2013) (inquiry about wedding photos).

¹²⁶ Bertrand & Mullainathan, *supra* note 121, at 991–93.

provide service to male couples and does not examine impacts on lesbian or non-binary couples or couples with distinctively Black or non-white names, nor does it explore the intersectionality of gender and race. This, too, could be a topic for a future study.¹²⁷

An additional limitation, resulting from the pseudo- and controlled experiment design, is that I examine the effect of *Masterpiece Cakeshop* in a relatively short time span: several weeks after the ruling. While collecting more observations would have been desirable, it was not possible to continue isolating the effect of the decision from intervening political developments beyond that period. I explain this in more length in the discussion.

TABLE 4: OVERALL RESPONSE RATES IN EACH WAVE

Wave	Overall Response Rate
W1	70.8
W2	58.7
W3	63.4
W4	61.9

Finally, I encountered a large attrition of businesses in the second wave of inquiries before *Masterpiece Cakeshop* (see Table 4)—an issue that pervades studies that repeatedly measure the same respondents over time.¹²⁸

¹²⁷ See, e.g., Kathryn M. Kroeper et al., *Marriage Equality: On the Books and on the Ground? An Experimental Audit Study of Beliefs and Behavior towards Same-Sex and Interracial Couples in the Wedding Industry*, 19 ANALYSES OF SOC. ISSUES & PUB. POL'Y 50 (2019). Kroeper's study was conducted before *Masterpiece*, finding that communications from same-sex couples were ignored more than communications from heterosexual and interracial couples. *Id.* at 66–67. The study did not find meaningful differences between gay and lesbian couples. See Kroeper et al.'s online supplement at 3. The comparisons and intersections explored in the study should be revisited, both because the sample size of each couple type was quite small, and because social norms may change, for example, because of decisions such as *Masterpiece Cakeshop*.

¹²⁸ See, e.g., Graham Kalton, *Designs for Surveys Over Time*, in SAMPLE SURVEYS: INFERENCE AND ANALYSIS 89, 101–03 (C. R. Rao & D. Pfeffermann eds., 2009); ALAN S. GERBER & DONALD P. GREEN, *FIELD EXPERIMENTS: DESIGN, ANALYSIS, AND*

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This pattern hindered the ability to detect discrimination in the pre-*Masterpiece Cakeshop* period, as the first wave of emails was from same-sex couples and the second wave of emails was from opposite-sex couples. While the causes for this attrition are not entirely clear (this is common to studies that encounter attrition),¹²⁹ a random phone survey suggested that businesses that provided no response to the second wave of emails were generally less responsive than other businesses (also over the phone), rather than suspicious or email fatigued.¹³⁰ To minimize the impact of attrition on the robustness of the design, I randomized couples' identity within each following wave. In addition, the following waves were designed to increase responsiveness by altering the style and formatting of the emails and the couples' profiles. This effort succeeded in increasing responsiveness to wave three and in reducing attrition between waves three and four. Nevertheless, I concede that the attrition of businesses from wave two prevents the evaluation of the existence and extent of sexual orientation discrimination before *Masterpiece Cakeshop*.¹³¹ To overcome this pitfall and evaluate the effect of *Masterpiece Cakeshop* on the existence and extent of discrimination *after* the decision, I developed several strategies of analysis which I present next.

D. Findings

In this section, I present the core results of the *Masterpiece Cakeshop* field experiment.¹³² The analysis begins by focusing on businesses that agreed to serve same-sex couples before *Masterpiece Cakeshop* and examining their behavior post-*Masterpiece Cakeshop*. The second analysis examines within-business changes of behavior across all businesses over time. I then move to examining differences between legal jurisdictions and between religious environments.

INTERPRETATION 236–40 (Ann Shin ed., 1st ed. 2012).

¹²⁹ John Fitzgerald et al., *An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics*, 33 J. HUM. RES. 251, 252 (1998).

¹³⁰ See Barak-Corren, *supra* note 18, online app. § OA3, at 11–14, <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>.

¹³¹ See *id.* § OA4.1, at 15–20. It is possible to infer that, prior to *Masterpiece Cakeshop*, opposite-sex couples were disfavored relative to same-sex couples (reverse discrimination), but this inference seems tenuous. To the extent it is true, the magnitude of the *Masterpiece Cakeshop* effect is much larger than estimated below.

¹³² For elaborations, robustness checks, and follow up studies, see Barak-Corren, *supra* note 18.

1. Did *Masterpiece Cakeshop* Increase Discrimination Towards Same-Sex Couples?

To answer this question, I evaluate the impact of *Masterpiece Cakeshop* on the 576 businesses that agreed to serve same-sex couples before the decision. Examining their behavior after the decision can provide an answer as to whether *Masterpiece Cakeshop* had a negative effect on the willingness of businesses to provide services to same-sex couples. As all businesses, these previously “gay-friendly” businesses were randomized post-*Masterpiece Cakeshop* to receive an inquiry either from a same-sex or an opposite-sex couple (and then vice versa in the following wave, such that each business was contacted by both couples post-*Masterpiece Cakeshop*). This design allowed me to estimate the effect of *Masterpiece Cakeshop* precisely, using both within and between businesses data.

Overall, post-*Masterpiece Cakeshop* inquiries from a same-sex couple had a 66.3% chance of receiving a positive response. Equivalent inquiries from an opposite-sex couple have a 75.5% chance of being answered positively. This represents a difference of 9.2 percentage points, a 14% change, that can be solely attributed to the identity of the couple;¹³³ these results were stable and significant in both waves following *Masterpiece Cakeshop*.

How do businesses communicate negative responses to couples? The most common form of declining service is simply no response. This result is anticipated, as writing a negative response is both time-intensive and awkward, and the easiest way for a business to proceed is to ignore the inquiry.¹³⁴ While some non-responses may have had other causes—for example, non-receipt of the initial email or simple forgetfulness—we would expect such errors to distribute randomly and therefore equally across couple types. This is not the case. Opposite-sex couples had a 19.6% chance of not receiving a response to their inquiry, while same-sex couples had a 27.8% chance of not receiving a response. That is, the chance of same-sex couples to not receive a response was 42% higher.¹³⁵ In addition, while explicitly negative responses were less common, the increase in such responses for same-sex couples from before to after *Masterpiece Cakeshop* was 177% the

¹³³ $p = .0006$. *Id.* at 24 tbl.5.

¹³⁴ See John F. Dovidio & Samuel L. Gaertner, *Aversive Racism*, 36 ADVANCES IN EXPERIMENTAL SOC. PSYCH. 1, 10 (2004); cf. Bertrand & Mullainathan, *supra* note 121, at 1006.

¹³⁵ $Z = 3.26$, $p = .001$. Barak-Corren, *supra* note 18, at 26.

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increase of such responses for opposite-sex couples.¹³⁶

The negative effect of *Masterpiece Cakeshop* on the willingness to provide services to same-sex couples was consistent across additional analyses. I find the effect in the entire sample of businesses in the experiment (N=906): comparing the rate of positive responses to same-sex couples before and after *Masterpiece Cakeshop* yields a drop of 14.4 percentage points, or about 23% change. I find the negative effect also in the control group, where the gap between couple types after *Masterpiece Cakeshop* was 9.5 percentage points, or about 14% change. I also find this effect among the particularly keen group of businesses that responded positively to both couples before *Masterpiece Cakeshop*. While these businesses remain more responsive than any other group of businesses, they too differentiate significantly between same-sex and opposite-sex couples after *Masterpiece Cakeshop* (~7 percentage point difference, or about 8% change). The summary of these results is presented in Figure 1, which shows that all business cohorts respond to *Masterpiece Cakeshop* with unfavorable treatment of same-sex couples, notwithstanding different baselines of positive response rates that characterize each cohort separately. In all of these analyses, the *Masterpiece Cakeshop* effect is robust to the inclusion of all experimental covariates, such as the type of business, the legal regime, and so on. The effect is equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and does not vary with political conservativeness. However, as I report in the next sub-section, the effect varies with religiosity of the business environment, such that businesses in areas dense with religious congregations are more likely to show substantial discrimination towards same-sex couples post-*Masterpiece Cakeshop*.

¹³⁶ $Z = 2.14$, $p = .03$. Barak-Corren, *supra* note 18, online app. § OA4, at 19, <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>.

Agreement to Serve Same-Sex and Opposite-Sex Couples After *Masterpiece Cakeshop*

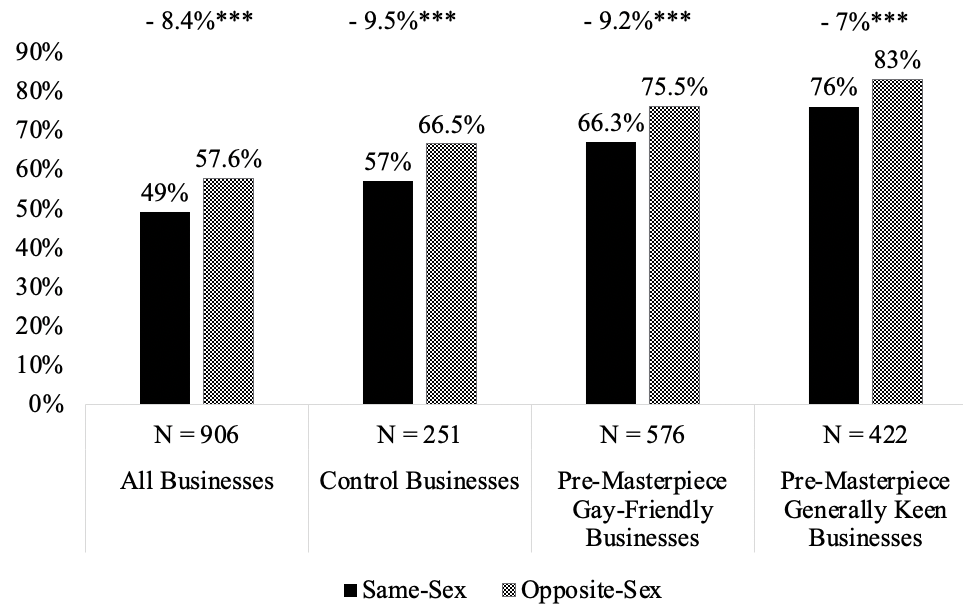


Figure 1.¹³⁷ The average positive response rate to same-sex and opposite-sex couples after the *Masterpiece Cakeshop* decision, by four groups of businesses: all businesses; businesses that were sampled for the first time after *Masterpiece Cakeshop* (control businesses); businesses that, prior to *Masterpiece Cakeshop*, were willing to serve same-sex couples (pre-*Masterpiece Cakeshop* gay-friendly businesses); and businesses that, prior to *Masterpiece Cakeshop*, were generally keen to serve all couples (pre-*Masterpiece Cakeshop* generally keen businesses). Gaps in percentage points are noted. *** $p < .01$.

2. What is the Magnitude of the *Masterpiece Cakeshop* Effect?

How substantial are these effects? Take the average 9% gap in willingness to serve same-sex and opposite-sex couples that was documented in the main analysis, as well as most additional analyses reported above. Now consider the typical couple that contracts with about ten vendors in the process of planning their wedding, including photographers, bakers, florists, videographers, venues, DJs, bridal/groom salons, calligraphers, jewelers, wedding planners, and more.¹³⁸ A conservative estimate of the number of

¹³⁷ *Id.* at 28.

¹³⁸ Photographers were generally less responsive (to all couples) than other businesses, but the negative effect of sexual orientation was robust across business types.

inquiries would be one per each business category, amounting to ten in total. A more liberal (some might say more representative) estimate assumes that each couple inquires with one or two potential vendors in each category, maybe more, amounting to at least 15–20 encounters. As each vendor-couple interaction presents an independent risk of incurring discrimination,¹³⁹ the aggregate risk that same-sex couples would encounter discrimination at least once in their interactions post-*Masterpiece Cakeshop* is a function of the average risk posed by each vendor and the overall number of interactions. This risk ranges from 61% for ten interactions to 85% for twenty interactions,¹⁴⁰ and can go higher (or lower) the more (or fewer) vendors a couple encounters.

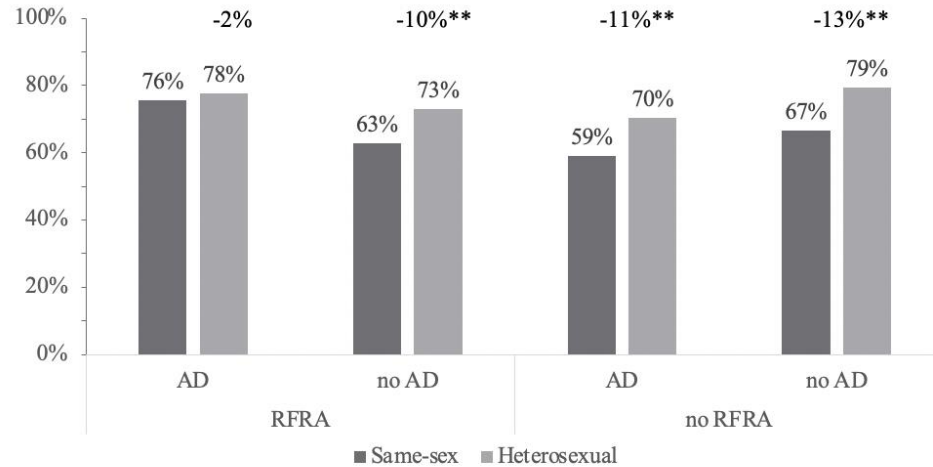
3. Does the Effect of *Masterpiece Cakeshop* Vary Between Legal Regimes?

The results demonstrate a substantial reduction in businesses' willingness to provide services to same-sex couples, as compared with opposite-sex couples, after the *Masterpiece Cakeshop* decision. Next, this section asks how this effect displays in different socio-legal regimes. Because of space limitations, the results are summarized in Figure 2. Briefly, I find that *Masterpiece Cakeshop* had a highly statistically significant negative effect in all regimes, *except* for regimes that enacted both an AD law and a religious freedom law.

¹³⁹ Clearly, independent vendors in one area could be different than independent vendors in another area, as areas differ in their levels of discrimination. In that sense, the risk posed by each vendor is not entirely independent from the risks posed by neighboring vendors. The *Masterpiece Cakeshop* effect was robust to county-level conservativeness and city size but varied with county-level religious density. On some aspects, then, the assumption of independence holds on average, and on other aspects the risk may vary with the environment. In any event, cases of revealed non-independence were rare and were removed from the sample. See Barak-Corren, *supra* note 18, online app. § OA2, at 10–11, <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>.

¹⁴⁰ In probabilistic terms, the question is: what is the probability that at least one of the vendors will discriminate against the couple, given X vendors and that the average vendor poses a 9% discrimination risk? To answer the question, one needs to calculate the odds that *all* X vendors do *not* discriminate (91% per vendor) and subtract that from 1. $P(\text{at least one vendor discriminates}) = 1 - 0.91^X$. This probability is 0.61 for $X=10$ vendors, 0.76 for $X = 15$ vendors, 0.85 for $X = 20$ vendors, and so on.

Impact of *Masterpiece Cakeshop* on Previously Gay-Friendly Businesses, by Legal Regime



Businesses = 576, Observations = 1,152

Figure 2. This figure presents the effect of *Masterpiece Cakeshop* on businesses operating in different legal regimes that prior to the decision agreed to provide services to same-sex couples. +RFRA, +AD regimes are counties in Indiana and Texas that are subject to state RFRA and have enacted local AD laws; +RFRA, -AD regimes are counties in Indiana and Texas that are subject to state RFRA and have *not* enacted local AD laws; the -RFRA, +AD regime comprises all counties in Iowa, a state that has enacted an AD law and no RFRA; the -RFRA, -AD regime comprises all counties in North Carolina, a state that has not enacted a RFRA and has not enacted an AD law, and had no county with such laws at the time of the experiment. See Table 1 for a socio-demographic comparison of the four regimes. ** $p < .05$; *** $p < .01$.

4. Is the Effect of *Masterpiece Cakeshop* Shaped by Religiosity?

Finally, what role does religion play in business behavior? Given that the decision involves a religious exemption and received considerable attention in religious media, one may expect that businesses operating in more religious environments will be more sensitive to *Masterpiece Cakeshop*, and as a result, the effect will be more pronounced in these environments. This hypothesis is particularly plausible with respect to Evangelical-dominant areas, as Evangelical Christians have been involved in a large number of wedding conflicts and are the denomination with the lowest rates of support for same-sex marriage.¹⁴¹ Although individual-level evidence on the

¹⁴¹ PEW RSCH. CTR., U.S. PUBLIC BECOMING LESS RELIGIOUS 108–09 (2015), <https://www.pewforum.org/wp->

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religiosity of the businesses is unavailable in this study, I can examine the impact of the surrounding religious environment by observing the density of Evangelical congregations in the county where the businesses are located.

I explored this hypothesis using public data on county-level density of religious and particularly Evangelical congregations from the U.S. Religion Census.¹⁴² These data help me examine whether religious environment influences previously gay-friendly businesses after *Masterpiece Cakeshop*.

Figure 3 plots the results. The top panel shows that the gap in agreement to serve same-sex and opposite-sex couples varied with the religiosity of the environment of the businesses. All of the businesses in this analysis agreed to serve same-sex couples before *Masterpiece Cakeshop*. After *Masterpiece Cakeshop*, however, businesses in religiously dense areas showed a large gap between same- and opposite-sex couples. In contrast, businesses in areas with few congregations do not significantly distinguish between same-sex and opposite-sex couples. Plotting the results against the density of Evangelical congregations provides very similar results, as the bottom panel of Figure 3 shows. The data for areas with very few congregations is somewhat noisy (only 32 businesses are located in counties where Evangelical density is 0.0004 or below), yet the general trend is the same: the sexual orientation service gap widens with Evangelical density. Notably, the percentage of businesses agreeing to provide services for opposite-sex couples is fairly stable across high- and low-religious/Evangelical density areas. The fluctuation occurs mostly with respect to same-sex couples.¹⁴³

content/uploads/sites/7/2015/11/201.11.03_RLS_II_full_report.pdf, archived at <https://perma.cc/NW2E-998M>.

¹⁴² See generally CLIFFORD GRAMMICH ET AL., 2010 U.S. RELIGION CENSUS: RELIGIOUS CONGREGATIONS AND MEMBERSHIP STUDY (2012).

¹⁴³ For the results of the regression analyses that account for religious and Evangelical density, see Barak-Corren, *supra* note 18, online app. § OA4.6, at 30–32 tbl.OA4.8, <https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf>, archived at <https://perma.cc/GF73-AHXU>.

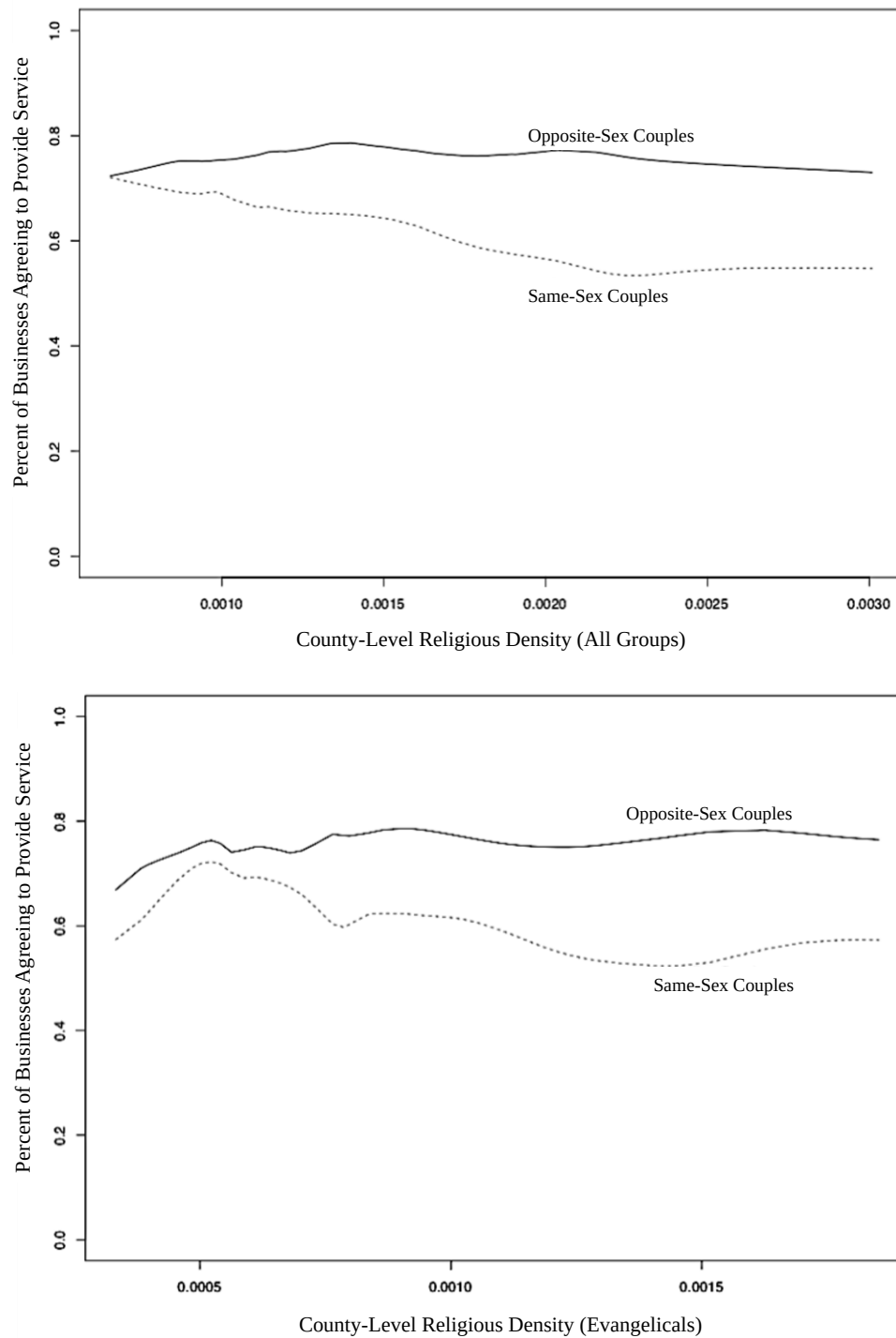


Figure 3. The agreement to serve same-sex and opposite-sex couples post-

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Masterpiece Cakeshop by businesses that provided positive responses to same-sex couples prior to *Masterpiece Cakeshop*, as a function of the religious environment. The top panel illustrates the results as a function of congregations density from all religious groups in the county where the business is located. The bottom panel illustrates the results as a function of the density of Evangelical congregations in the county.

These data demonstrate that the negative effect of *Masterpiece Cakeshop* is significantly concentrated in more religious environments. To get a concrete appreciation of the magnitude of this result, I compared businesses in high versus low Evangelical density environments (top 25% versus bottom 25%). In highly Evangelical environments, previously gay-friendly businesses developed a 20.5 percentage point gap between couples (78 percentage points versus 57.5 percentage points),¹⁴⁴ whereas in slightly Evangelical environments, the gap is 2.7 percentage points (70.6 percentage points versus 67.9 percentage points, and non-statistically significant). The same disparity between high and low religiosity areas is true for general religiosity as well.¹⁴⁵ These results are illustrative, yet it is important to note that the effect is not a binary but a continuum. Not only heavily religious (Evangelical) communities show the effect, but also intermediately religious communities, as Figure 3 demonstrates. These results indicate that businesses in more religious areas updated their behavior after *Masterpiece Cakeshop* significantly more than businesses in less religious areas.

III. THE MASTERPIECE CAKESHOP EFFECT: EXPLANATIONS AND IMPLICATIONS

The field experiment findings exposed the highly consequential effect of law and the Supreme Court in particular on the behavior of the public. A methodological strength of the field experiment is that it tests the effect of *Masterpiece Cakeshop* directly before and after the decision was rendered, while controlling the setting of the study and employing randomization and is therefore able to isolate the decision's causal effect. It would have been desirable to continue examining *Masterpiece Cakeshop*'s effect later in time, but subsequent legal and political developments have severed the causal link between *Masterpiece Cakeshop* and the market, making such examination impossible. Shortly after the decision, legislatures in several states have

¹⁴⁴ $Z = 3.69$, $p = .0002$. Barak-Corren, *supra* note 18, at 36.

¹⁴⁵ A 17.2 percentage point vs. 3.6 percentage point gap, respectively, $Z = 3.133$, $p = .0017$. *Id.*

proposed or revived new religious liberty bills¹⁴⁶ and two states surveyed in the experiment—Texas and North Carolina—passed legislation related to religious liberty or LGBTQ rights.¹⁴⁷ Given the constantly dynamic legal and political landscape on these issues, whatever has been the conduct of businesses during the intervening period, it can no longer be linked to *Masterpiece Cakeshop*. The *Masterpiece Cakeshop* field experiment therefore provides the cleanest test of the decision's impact and speaks for the consequences directly stemming from the decision itself.

A. Explaining the Masterpiece Cakeshop Discriminatory Effect

What explains the general effect of the *Masterpiece Cakeshop* decision on wedding vendors? Why do they change their behavior after the decision is rendered? Elsewhere, I considered two types of mechanisms that could explain the effect: *cognitive* and *social*.¹⁴⁸

There are two primary cognitive explanations for the results: a law-and-economics-type explanation and an expressive-law-type explanation. The law-and-economics explanation is that *Masterpiece Cakeshop* was interpreted by vendors as a relief of previously-anticipated penalties for discrimination, or as a signal that the Court has little intention to enforce AD laws. In economic terms, *Masterpiece Cakeshop* may have influenced perceptions regarding the probability of sanction and/or the likelihood of enforcement. The problem with this explanation is that it is less plausible in light of the design of the experiment and its findings. First, for *Masterpiece*

¹⁴⁶ This includes Iowa and Texas. See Rodriguez, *supra* note 100; Emma Platoff, *Texas Senate approves occupational licensing bill LGBTQ advocates call a "license to discriminate"*, TEX. TRIB. (Apr. 2, 2019), <https://www.texastribune.org/2019/04/02/texas-senate-religious-refusal-LGBTQ-occupational-licensing/>, archived at <https://perma.cc/J2EQ-GF5H> (reporting on S.B. 17, which would allow occupational license holders to cite sincerely held religious beliefs as a defense for license-threatening conduct or speech).

¹⁴⁷ Emma Platoff, *Texas House passes religious liberty bill amid LGBTQ Caucus' objections*, TEX. TRIB. (May 20, 2019), <https://www.texastribune.org/2019/05/20/texas-religious-liberty-bill-passes-lgbtq-caucus-fear-hateful-rhetoric/>, archived at <https://perma.cc/5LJF-K2J2>; Tim Fitzsimons, *N. Carolina is first in South to ban state funding for conversion therapy*, NBC NEWS (Aug. 3, 2019), <https://www.nbcnews.com/feature/nbc-out/n-carolina-first-south-ban-state-funding-conversion-therapy-n1038846>, archived at <https://perma.cc/2Y27-4SB3>.

¹⁴⁸ Barak-Corren, *supra* note 18, at 36–39.

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Cakeshop to relieve the risk of incurring a penalty, such risk should be present to begin with. Yet, the experiment, by design, eliminated the risk of getting caught (by the couple, by society, and by state officials), as emails allow vendors to entirely avoid the detection of discrimination (whereas in face to face communication, the synchronous nature of communication makes it more difficult). Namely, even before *Masterpiece Cakeshop*, vendors could have opted to ignore emails from same-sex couples or provide excuses; they were under no threat of detection, enforcement, or penalty, and *Masterpiece Cakeshop* did not change that. The reputational risk of being labelled a discriminator (and the potential penalty of losing clients) was also absent, for the same reasons. Hence, the decreased willingness to provide services to same-sex couples cannot be attributed to a relief of risk of penalty.

Furthermore, the negative effect of *Masterpiece Cakeshop* is found even in regimes that have not enacted *any* prohibition on the discrimination of same-sex couples (no-AD law regimes). Businesses in Texas, Indiana, and North Carolina, operating under no obligation to serve same-sex couples, and therefore under no threat of sanction, still adapted their behavior post-*Masterpiece Cakeshop*. Hence, it is unlikely that the negative *Masterpiece Cakeshop* effect is explained by the decision's influence on the legal costs of discrimination, even if these costs indeed dropped. Indeed, by no means do I argue that legal or social penalties are inexistent or uninfluential. Both legal penalties and reputational costs can be very influential. However, their absence from the present setting—a common real-life setting where communication is asynchronous and decisions can be easily masked and remain unknown to the public—makes penalties an unlikely explanation for the effect documented in the present study.

An alternative cognitive explanation is that *Masterpiece Cakeshop* had an expressive effect on wedding vendors, changing their perceptions of the social norm regarding service refusal. The expressive theory of law argues that law can foster change not only or merely by the imposition of costs or benefits, but also by conveying that a certain norm has received a consensual status.¹⁴⁹ The *Masterpiece Cakeshop* decision was a lopsided 7–2 ruling that

¹⁴⁹ See generally Richard H. McAdams, *An Attitudinal Theory of Expressive Law*, 79 OR. L. REV. 339, 339–40 (2000); Cass R. Sunstein, *On the Expressive Function of Law*, 144 U. PA. L. REV. 2021, 2024–25 (1996).

crossed partisan lines and was infused with normative messages. The majority opinion particularly emphasized the importance of tolerance in a free society, the need for pluralism, and respect for the views of religious objectors. These parts of the decision were frequently cited by conservative and religious commentators on the decision.¹⁵⁰ Changes in social norm perceptions and/or personal support of same-sex marriage following the decision could explain why the decision strengthened the impetus of discrimination even if the probability of detection had not changed.¹⁵¹ Another possibility is that personal preferences did not change, but were emboldened by the expressive message of the decision, making decision-makers more likely to act on them.

Moving from cognitive to social mechanisms, the environment in which vendors operate could have also influenced their decisions. I examined several types of environmental factors: urbanism; conservativeness; and religious density (I also examine different legal regimes, but I discuss this separately). All other things being equal, I did not find that the *Masterpiece Cakeshop* effect weakened in more urban environments, nor strengthened in more conservative environments. But I did find an indication of a more specific social mechanism: the religiosity of the surrounding environment. Businesses in religiously-dense areas discriminated against same-sex couples after *Masterpiece Cakeshop* significantly more than businesses in less religious areas.

Before interpreting these results, it is important to note that the religiosity of the environment is a crude proxy for the role of religion in the decision-making process, a proxy that could interact with additional factors in ways that are not controlled for in the study. Therefore, the findings from this analysis should be viewed as suggestive rather than conclusive. With this in mind, one straightforward interpretation of the results is that areas with more religious congregations have more religious businessowners, and that religious owners are adapting their behavior after *Masterpiece Cakeshop*

¹⁵⁰ See *supra* notes 89–92.

¹⁵¹ This explanation is also supported by the evidence on the impact of the Supreme Court on social norms and support of same-sex marriage in *Obergefell v. Hodges*, 576 U.S. 644 (2015). See Tankard & Paluck, *supra* note 97, at 1339; Kazyak & Stange, *supra* note 98, at 2044–45.

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(likely due to a perceived change in the social norm). Another possibility is that *Masterpiece Cakeshop* received greater exposure and favorable attention in religious environments, such that news of the decision (framed by religious and conservative outlets) spread widely and influenced *all* businesses—religious or not. This hypothesis is supported by the work of Linos and Twist, who found that Supreme Court decisions mostly influence attitudes through one-sided media frames, and that these frames have similar influence on people who regularly consume the relevant media versus those who are randomly exposed to them.¹⁵² Either way, a religious environment appears to play a major role in translating *Masterpiece Cakeshop* into negative consequences for same-sex couples. The precise mechanism by which this translation occurs should be addressed in future studies, including survey experiments to measure individuals' religiosity. Such studies could also broaden the investigation to additional mechanisms and legal measures, such as statutory exemptions.

B. Implications for Legislators

The “legislative mismatch” between the protections of sexual-orientation equality and religious freedom across the country should be a cause for concern on both ends of the political spectrum. The two most common regulatory vehicles to afford such protections—AD laws and RFRA—have been mostly stalled in recent years due to heightened anxiety about the consequences of AD laws for religious objectors and of RFRA for LGBTQ people. In May 2019, during a heated debate on the floor of the Texas House about an amendment to the state's RFRA, members of the LGBTQ caucus questioned the bill's sponsors extensively about how the bill might spark discrimination and warned that the bill “perpetuates the rhetoric that leads to discrimination, to hate and ultimately bullying that leads to the consequence of people dying.”¹⁵³ The last states to enact new RFRA until recently were Arkansas and Indiana in 2015;¹⁵⁴ the resulting commercial and public

¹⁵² Linos & Twist, *supra* note 86, at 223.

¹⁵³ Platoff, *supra* note 147.

¹⁵⁴ NCSL, *supra* note 48. In March 2021, right before this Article went to press, South Dakota became the first state in six years to enact a RFRA. S.B. 124, 2021 Leg., 96th Sess. (S.D. 2021).

backlash might have deterred other states from following that route.¹⁵⁵

The situation is similar with respect to AD laws. On April 2020, Virginia expanded its public accommodations law to protect against discrimination based on gender identity and sexual orientation.¹⁵⁶ But prior to that, the last state to enact an AD law prohibiting sexual orientation discrimination in public accommodations was Delaware in 2009.¹⁵⁷ Twenty-seven states have not yet enacted such laws.¹⁵⁸

The *Masterpiece Cakeshop* field experiment conducted a first-of-its-kind examination of the implications of the AD/RFRA mismatch by testing the behavior of wedding vendors from states that are highly similar in terms of their economic, social, and political climates, yet model four different legal regimes: with or without a RFRA, and with or without an AD law. The findings revealed that the introduction of a (perceived) federal religious exemption—in the form of *Masterpiece Cakeshop*—had the same negative impact on same-sex couples in three of the four regimes, but not in regimes that are regulated by both a RFRA *and* an AD law. Intriguingly, the

¹⁵⁵ See, e.g., James Briggs, *RFRA 'Fix' Was Enough to Keep Tourists Coming to Indianapolis*, DES MOINES REG. (Feb. 3, 2017), <https://www.desmoinesregister.com/story/money/2017/02/03/briggs-rfra-fix-enough-keep-tourists-coming-indianapolis/97146094/>, archived at <https://perma.cc/XAS9-QYPT> (describing the backlash in Indiana). In Georgia, Gov. Deal vetoed the state's RFRA bill in response to widespread criticism from LGBTQ groups and supporters, including threats of commercial boycott. Greg Bluestein, *BREAKING: Nathan Deal vetoes Georgia's 'religious liberty' bill*, ATLANTA J.-CONST. (Mar. 28, 2016), <https://www.ajc.com/blog/politics/breaking-nathan-deal-vetoes-georgia-religious-liberty-bill/yVAFf868i7ilsrwT9zpH3L/>, archived at <https://perma.cc/4C4T-Q59N>. The Human Rights Campaign have also made the backlash a salient part of its appeal to states to refrain from enacting RFRA's, for example in its criticism of the recent South Dakota legislation. Wyatt Ronan, *South Dakota Gov. Kristi Noem Signs Religious Refusal Bill, Creating First Major RFRA Law in Six Years*, HUM. RTS. CAMPAIGN (Mar. 13, 2021), <https://www.hrc.org/press-releases/south-dakota-gov-kristi-noem-signs-religious-refusal-bill-creating-first-major-rfra-law-in-six-years>, archived at <https://perma.cc/PF4H-SPT6>.

¹⁵⁶ VA. CODE ANN. § 2.2-3900 (2020).

¹⁵⁷ 77 Del. Laws 90 (2009) (amending 28 sections in the Delaware Code to include sexual orientation).

¹⁵⁸ Out of which, five states—Florida, Kansas, Michigan, North Dakota, and Pennsylvania—recently interpreted the prohibition on “sex” discrimination in their law as including sexual orientation and gender identity. See *State Public Accommodations Nondiscrimination Laws*, MOVEMENT ADVANCEMENT PROJECT (Dec. 10, 2020), <https://www.lgbtmap.org/img/maps/citations-nondisc-public-accom.pdf>, archived at <https://perma.cc/PX77-2RZB>.

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differential effect of *Masterpiece Cakeshop* was partially observed between cities within the same RFRA state that differed on whether they had an AD law or not (e.g., Dallas versus Houston); these differences were associated with significant consequences for discrimination.

Before discussing the potential implications of these results, several caveats are due. To be sure, no causal inferences can be drawn from the legal regime results, as discussed earlier, because legal regimes are not randomly allocated and were impossible to examine in a natural experiment setting in the present context. Therefore, I am not arguing that the (in)existence of one law or the other is the cause for the *Masterpiece Cakeshop* effect. In addition, legal regimes are considerably richer and more nuanced than the letter of the law can reveal; and they are influenced, among other factors, by administrative policies and judicial decisions not captured in this analysis. Furthermore, legal differences between otherwise similar political units could be the result of unobservable variables that could be the actual causes of differences in discrimination. For example, the social and political climate that produced certain legislation might *also* shape the conduct of local businesses; such an explanation is probably more likely than the assumption that wedding businesses are fully familiar with the laws of their political units.

The underlying causes of the findings aside, the results carefully suggest two observations about the implications of the legislative mismatch. First, AD laws do not necessarily safeguard sexual orientation equality or protect against an increase in discrimination. Second, RFRA's are not necessarily themselves detrimental to the operation of equality on the ground.

1. The Push for Federal and State AD Laws Should Not Forsake Local AD Laws.

That AD laws do not necessarily ensure equality is not, on its own, novel. Extensive empirical research has repeatedly exposed and documented the failures of AD laws to prevent and remedy discrimination in practice.¹⁵⁹ Yet it is interesting to observe that regimes that enacted an AD law, but no RFRA,

¹⁵⁹ For a review based on comprehensive data, see ELLEN BERREY ET AL., RIGHTS ON TRIAL: HOW WORKPLACE DISCRIMINATION LAW PERPETUATES INEQUALITY 54–73 (2017).

fare worse than comparable regimes that enacted both laws. Iowa, for example, has a long tradition of protection and advancement of sexual orientation equality. Iowa led the way for other states in invalidating its sodomy law already in 1976 and being one of the first states to recognize same-sex marriage.¹⁶⁰ The state enacted a state-wide ban on sexual orientation discrimination, and efforts to enact a RFRA in Iowa failed several times due to concerns about the potentially detrimental effects of such act on sexual orientation discrimination.¹⁶¹ Against this background, one could expect that the social and political climate that produced Iowa's legal regime would be the most favorable to same-sex couples of all four regimes. Instead, business behavior in Iowa is found to be indistinguishable from regimes that have neither an AD law nor a RFRA (North Carolina), or even from regimes that have no AD law, but do have a RFRA (certain localities in Texas and Indiana). In contrast, regimes that have both an AD and a RFRA (other localities in Texas and Indiana) did not show the negative *Masterpiece Cakeshop* effect.

This pattern raises the question of whether AD laws vary in their effectiveness based on the level of their enactment—namely, whether municipal AD laws are more effective than state variations. This possibility runs counter to the intuition of LGBTQ advocacy groups in many AD-less states. Some of these groups intensified their struggle for state-level AD legislation following *Masterpiece Cakeshop*, claiming that municipal legislative acts are insufficient and “do not carry the force that a state law would.”¹⁶² Clearly, enacting a series of municipal ordinances is less efficient than enacting one law that covers all municipalities and provides legal recourse for the entire state population. Yet there are two potential reasons for why local AD legislation may fare better in reducing discrimination than state-level legislation. First, legislation at the local level may better represent the preferences and behavioral intentions of the political community.¹⁶³

¹⁶⁰ See sources cited *supra* note 100 and accompanying text.

¹⁶¹ See *Iowa Religious Freedom Restoration Act (HF 258)*, REWIRE NEWS GROUP (Feb. 19, 2019), <https://rewire.news/legislative-tracker/law/iowa-religious-freedom-restoration-act-hf-258/>, archived at <https://perma.cc/22MK-GG7M> (documenting the failure of several bills in 2016 and 2018 and the stalling of a 2019 bill).

¹⁶² See Platoff, *supra* note 87.

¹⁶³ For a discussion of how cities promote democratic self-governance and representation better than states, see Yishai Blank, *City Speech*, 54 HARV. C.R.-C.L. L. REV.

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Therefore, the enactment of a municipal AD law by a certain community likely provides a more reliable commitment to equality and nondiscrimination than the enactment of a state AD law. Second, and relatedly, because municipal legislation is closer to home, it could be more successful in persuading residents that have not yet bought into the norm. According to the expressive theory of law, “[a]s long as legislation is positively correlated with popular attitudes or opinions, then it will cause individuals to revise their beliefs about the expected approval or disapproval and to act accordingly.”¹⁶⁴ If this proposition holds in the present case, the fact that a municipal AD law represents the norm of the immediate community increases its ability to persuade individuals from that community to conform. This ability could be compromised the higher a certain legislation “climbs” (namely, a state law might succeed less in revising behavior than municipal law, and federal law might have even less success). This decrease in effectiveness is especially likely in diverse states, where communities that adhere to different norms could respond to AD laws very differently.¹⁶⁵

Given that the findings with respect to state differences are correlational and may therefore reflect a variety of additional factors, these conclusions are tentative and should be further examined in future studies.

One implication for the interim period is not to abandon local initiatives to enact AD laws or prioritize them as less urgent or less important than state-level initiatives. Assuming that equality movements care not only about the law on the books, but also (and perhaps more so) about the law on the ground, including the prevention of actual discrimination and the improvement of people’s lives and opportunities, local AD laws appear to contribute greatly to achieving these goals.

365, 389–96 (2019).

¹⁶⁴ McAdams, *supra* note 149, at 343; *see also* Robert Cooter, *Expressive Law and Economics*, 27 J. LEGAL STUD. 585, 595 (1998) (hypothesizing that enacting a norm can increase the number of people who follow it).

¹⁶⁵ In such cases opposing communities could react against the law. *See* Netta Barak-Corren et al., *The Provocative Effect of Law: Majority Nationalism and Minority Discrimination*, 15 J. EMPIRICAL LEGAL STUD. 951, 956 (2018).

2. RFRA's Are Not Necessarily Recipes for Discrimination and Should Be Pre-Tested to That Effect.

The second important finding that emerges from the comparison of legal regimes is that RFRA's are not necessarily detrimental to the operation of sexual orientation equality. This finding is arguably more surprising and potentially of broad relevance. The enactment of RFRA's and other protections of religious liberty has been the focus of intensive debate in recent years: one of the major concerns being that such laws would encourage greater discrimination against sexual minorities. I already alluded to the levels of anxiety and controversy that characterize this issue. States that enacted or considered enacting RFRA's were threatened with high-impact boycotts. Indiana itself was the subject of such a boycott after passing its RFRA in 2015, losing twelve conventions and \$60 million in revenue.¹⁶⁶ The Indiana legislature quickly passed a "fix" that clarified that the new Act does not trump local AD laws,¹⁶⁷ a provision very similar to the one included in Texas' RFRA from its inception.¹⁶⁸

The results from the *Masterpiece Cakeshop* field experiment indicate that the combination of religious liberty protections of the Texas-Indiana type with AD laws at the local level was resistant to the negative effect of *Masterpiece Cakeshop* on discrimination towards same-sex couples. One potential explanation is that the tension built into these hybrid regimes led businesses to reflect and contemplate their positions in advance—prior to *Masterpiece Cakeshop*—more, perhaps, than businesses in regimes where the tension was less salient. Having already formed a position, businesses in hybrid regimes were possibly more resistant to the influence of *Masterpiece Cakeshop*.¹⁶⁹ Notably, these businesses were not merely more consistent in their behavior; they were also the least discriminatory of same-sex couples post-*Masterpiece Cakeshop* (see Figure 3). Seventy-six percent of businesses in hybrid regimes agreed to provide services to same-sex couples, compared to 59–67% of businesses in other regimes.

As with the findings regarding AD regimes, the relationship between

¹⁶⁶ Briggs, *supra* note 155.

¹⁶⁷ See *supra* notes 64–65.

¹⁶⁸ *Id.*

¹⁶⁹ I thank Stephanie Barclay for proposing this point.

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hybrid regimes and sexual orientation discrimination should be further examined. In particular, RFRA's come in many shapes and forms—e.g., with or without recourse against local governments, private lawsuits, and civil rights law.¹⁷⁰ Different RFRA designs could have different impacts on discrimination, especially as these designs interact with existing or in-existent AD laws. To be sure, businesses in RFRA regimes without AD laws strongly showed the negative *Masterpiece Cakeshop* effect. Caveat is required before enacting a new RFRA or amending an existing act.

Alongside this caveat, the findings regarding hybrid regimes provide tentative hope for scholarly and political efforts—most notably, Professor Robin Fretwell Wilson's work—that marriage equality and religious liberty could be reconciled in legislation somehow, without necessarily exacerbating discrimination.¹⁷¹ How might this goal be achieved?

An important implication of the *Masterpiece Cakeshop* experiment is that such efforts should rely on reliable and robust empirical evidence regarding the likely consequences of the proposal on sexual orientation discrimination. To do that, I propose pre-testing RFRA's (and any other similar mechanism). Lawmakers and law professors must not speculate on the outcomes of their proposals or treat them as self-evident. As the findings of the field experiment teach us, speculations and assumptions that do not rely on directly relevant data are no good. The discipline of empirical legal studies has advanced to offer a variety of methods—including experimental surveys and qualitative in-depth interviews—that could facilitate testing the likely effects of proposed policies before formal implementation.¹⁷²

For example, a state legislature could collect a representative sample of the state population, randomly expose different groups to alternative bills, and examine whether exposure to one bill (compared to the others, or no bill)

¹⁷⁰ See *supra* notes 57–64 and accompanying text.

¹⁷¹ See, e.g., Robin Fretwell Wilson & Anthony Michael Kreis, *Embracing Compromise: Marriage Equality and Religious Liberty in the Political Process*, 15 GEO. J. GENDER & L. 485, 488–89 (2014). Notably, Wilson and her colleague do not promote new RFRA's, but instead specific and clear exemptions for wedding vendors from the duty to provide service to same-sex couples (but not other suspect categories). *Id.*

¹⁷² In general, the field of evidence-based lawmaking has developed both theoretically and practically in recent years. See Ittai Bar-Siman-Tov, *The Dual Meaning of Evidence-based Judicial Review of Legislation*, 4 THEORY PRAC. & LEGIS. 107, 108–11 (2016).

generates more or less anti-gay bias in the population, or produces a more or less accurate understanding of appropriate behavior. Lawmakers could either devise their own decision-making dilemmas to probe citizens' understanding of the proposed law, or they could rely on one of the many measures established in psychological research to capture bias and social norm perceptions.¹⁷³

Clearly, pre-testing laws requires collaboration between lawmakers and empirical legal scholars, or even the establishment of an in-house research department to execute empirical studies for legislatures. Yet the benefits of such approach greatly exceed its costs. First, basing legislation on data, rather than on speculations, is a positive good which improves the quality of the legislative process. Second, the fears and anxiety that accompany the religion-equality conflict prevent the advancement of both AD laws and RFRAs all around the nation and exacerbate cultural divides and political polarization. Were the opposing parties to suspend their assumptions about the consequences of proposed policies and subject them to a rigorous empirical test, they might have been able to approach proposals more openly. In addition, the interim phase of subjecting bills to an a-priori empirical test, before legislating them, will facilitate bipartisan collaboration in research design. Pro-religion and pro-equality legislators will have to sit down and decide what bills they want to test and what measures are needed to capture the consequences they fear or favor, if real. For example, they will need to jointly draft the vignettes (or scenarios) they are interested in probing citizens' reactions to. This deliberation could clarify the stakes for both parties, encourage more reflection about their goals and concerns, and concretize the debate going forward. The results would hopefully resolve the debate in one direction or the other and provide informed ground to base any decision regarding the legislation.

C. Implications for Courts

The findings of the *Masterpiece Cakeshop* field experiment answer several legal questions currently preoccupying the courts.

¹⁷³ See, e.g., Ofosu et al., *supra* note 99, at 8847–48; Tankard & Paluck, *supra* note 97, at 1336–39.

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First, courts are the arbitrators of the debate on the consequences of religious exemptions. Complainants of discrimination and supporting amici frequently warn of increased discrimination towards same-sex couples if religious exemptions are granted. Religious objectors and supporting amici consistently argue that this concern should be dismissed because “ample alternative providers exist.”¹⁷⁴ As a result, courts ask what the consequences of their decisions are likely to be—as did Justice Kennedy, who penned the majority opinion in *Masterpiece Cakeshop*.¹⁷⁵ But, until now, courts have had no relevant data to answer this question.

The *Masterpiece Cakeshop* field experiment provides these data for the first time, documenting the scope of refusals to same-sex couples, as compared with opposite-sex couples, in response to the *Masterpiece Cakeshop* decision. Courts now have concrete evidence from different legal regimes in the U.S., data that were thus far the object of concerns and speculation. Importantly, these data are not drawn from liberal strongholds, but from states that are either at the national average or more conservative. The data show courts that market alternatives *do* exist—there are vendors who will provide services to same-sex couples—and that granting a religious exemption encourages discrimination towards same-sex couples nevertheless, across a wide range of social and legal categories. Indeed, the *Masterpiece Cakeshop* decision generally exposed same-sex couples to a high risk of experiencing discrimination—estimated to be between 65% and 81%, as a function of the number of market interactions in which couples engage. Justice Kennedy’s concern that an exemption would encourage wedding vendors to refuse service to same-sex couples is unfortunately borne out by the data.

These troubling consequences provide the missing piece to the puzzle of applying a strict scrutiny analysis, or RFRA review, to AD laws. Under this doctrine,¹⁷⁶ the court first examines whether the law substantially burdens the

¹⁷⁴ Laycock & Berg, *supra* note 76; *see also* Transcript of Oral Argument, *supra* note 12, at 46 (U.S. Attorney General arguing in support of the baker that “products are widely available from many different sources”).

¹⁷⁵ *Id.* at 44–45.

¹⁷⁶ In 29 states, the standard of scrutiny of governmental burdens on religion is currently lower, encompassing standards such as intermediate scrutiny or even rational basis review. *See supra* Part I.B.

free exercise of religion. To survive strict scrutiny, a law must be the least restrictive means by which to further a compelling governmental interest. This is a high threshold for the government, one “that takes a hard look at the facts and does not accept the ‘government’s bare say-so’ about factual outcomes.”¹⁷⁷ The first part of the justification—proving a compelling governmental interest—is typically uncontroversial when it comes to laws that aim to fight discrimination on the basis of sexual orientation. But the second part—the least restrictive means test—is thornier, especially in cases involving potential religious exemptions. Cannot the compelling governmental interest in eradicating discrimination be achieved through the less restrictive means of a law that permits religious exemptions? To determine whether religious exemptions would undermine a law’s purpose, judges must engage in a factual examination of the consequences of religious exemptions. To know whether a universal enforcement of AD laws is the least restrictive means to ensure access to public accommodations, courts need to know whether religious exemptions detract from this compelling goal. The results of the *Masterpiece Cakeshop* field experiment establish that the decision substantially detracted from this goal in most regimes, by substantially expanding discrimination against same-sex couples. Unfortunately, this was the outcome even though the Court reiterated its commitment to protecting sexual minorities in the marketplace in general, and despite the fact that the exemption crafted for the baker was narrow and case specific. Taken together, these results vindicate states that currently insist on enforcing AD laws without providing exemptions. As I note earlier, it is possible that a different combination of legal means will generate different behavioral outcomes, and such combinations should be tested—or, where relevant, pre-tested—in the appropriate circumstances in the future.

The second implication for courts involves the specific reasoning of the *Masterpiece Cakeshop* decision. The majority Justices—particularly Justice Kennedy—clearly wished to avoid settling the larger tension between religious liberty and sexual orientation equality. Instead, the Justices sought to carve a narrow decision that would not grant wedding vendors a license to discriminate against same-sex couples. This strategy does not seem to have

¹⁷⁷ Stephanie Barclay, *An Economic Approach to Religious Exemptions*, 72 FLA. L. REV. 1211, 1262 (2020) (quoting *Yellowbear v. Lampert*, 741 F.3d 48, 59 (10th Cir. 2014)).

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been successful. *Masterpiece Cakeshop* has ultimately increased discrimination in the wedding industry and bolstered legislators and advocates in their attempts to expand religious protections and narrow the scope of antidiscrimination protections.¹⁷⁸ The two subsequent unreasoned decisions in *Arlene's Flowers*¹⁷⁹ and *Klein*¹⁸⁰ that vacated and remanded other wedding vendor cases, despite very different factual circumstances, might have strengthened the impression that *Masterpiece Cakeshop* was not so narrow after all. Assuming the Court did not intend to expand discrimination against same-sex couples, could other judicial strategies have fared better?

This question is of crucial importance considering the challenge facing the Court in its coming term, and likely future ones. This term, the Court will decide *Fulton v. City of Philadelphia*, which involves a social welfare agency that objects, on religious grounds, to a Philadelphia AD rule. That rule requires the agency to provide services to LGBTQ prospective foster parents, as part of its governmental contract. The agency argues that if the Court rules for the city, the agency will close its doors and children will be harmed. The city argues that same-sex couples should not be excluded solely on the basis of their sexual orientation, and that all children are better off if placement agencies refrain from considering factors other than the best interest of children.

The *Masterpiece Cakeshop* experiment offers two lessons for the *Fulton* Court—and for any court adjudicating religion-equality conflicts in the future. First, the Court should require the parties to present directly relevant data to found arguments about consequences. The findings from the *Masterpiece Cakeshop* field experiment teach us that contrasting arguments about the empirical world can thrive despite the absence of data, as each party to the debate is highly motivated to hold on to their own assumptions and to speculate about the facts. This is a dangerous state of affairs. In constitutional law, as elsewhere, arguments about outcomes should rest on actual data. Not

¹⁷⁸ See Simpson, *supra* note 87 (quoting the head of the American Family Association of Indiana saying that he sees *Masterpiece Cakeshop* as a “signal” to push forward litigation against local AD laws in Indiana); Platoff, *supra* note 147 (describing bills in Texas expanding protections for religious professionals and corporations).

¹⁷⁹ *Arlene's Flowers, Inc. v. Washington*, 138 S. Ct. 2671, 2671–72 (2018).

¹⁸⁰ *Klein v. Or. Bureau of Lab. & Indus.*, 139 S. Ct. 2713, 2713 (2019).

only are data crucial to learn the truth value of consequentialist arguments, they can also nuance and refine legal analysis. In the *Masterpiece Cakeshop* context, the empirical work exposes issues that were not previously considered in the area of religious exemptions—such as the nature of the influence of law on behavior—and creates new and specific questions for lawyers and judges to answer—such as how to factor the aggregate risk of discrimination. It is therefore imperative that courts will develop a more critical approach to consequential arguments and will look for directly relevant data.

The second lesson for the *Fulton* Court flows directly from the poor outcomes resulting from the avoidance strategy used in *Masterpiece Cakeshop*. By now, several different studies—including the present study—have shown that the Court has the power to shape public attitudes and public behavior, thereby producing either less or more bias and discrimination in society.¹⁸¹ It is true that after a decision is handed down by the Court, it takes on a life of its own, and much of its effects depend on how it is communicated by mass media. But the Court is not a helpless statist in this process. A clearer and less ambiguous decision—for example, one that sets a clear rule that is easy to communicate, understand, and follow—is less open to aggrandizement, misstatements, or misinterpretations. The *Fulton* Court should opt for a clear and bright-line decision that provides specific and unambiguous behavioral instructions, including for future cases. This is particularly true if the court decides to grant an exemption in *Fulton*. In such case, the Court should assume, based on the *Masterpiece Cakeshop* effect, that its decision will likely encourage discrimination against sexual minorities. It is the Court's responsibility to minimize this effect to the extent possible. The Justices should not mislead themselves to think that evading the big questions or making a case-specific decision, as in *Masterpiece Cakeshop*, will avoid undesirable outcomes. The Justices should also not mislead themselves to think that their decision will only expand the freedom of a negligible minority of extremely objecting individuals. Rather,

¹⁸¹ Cf. Ofosu et. al, *supra* note 99, at 8849 (finding a sharper decrease in anti-gay bias in states that legalized same-sex marriage compared with those that did not); Tankard & Paluck, *supra* note 97, at 1341–42 (finding that the Supreme Court's decision in *Obergefell v. Hodges*, 576 U.S. 644 (2015), shifted perceived social norms among non-LGBTQIA Americans in support of same-sex marriage).

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exempting religious objectors will likely have a broad impact, including on decision-makers who were willing to provide services before the decision, but will refuse to do so afterwards. These consequences are particularly concerning given the number of wedding conflict cases that have recently resolved in favor of vendors.¹⁸² This trend gives rise to the possibility that the *Masterpiece Cakeshop* effect will repeat and aggregate over time, the more such decisions are made and become known to the public.

Finally, the expansion of discrimination post-*Masterpiece Cakeshop* suggests that courts should develop a better account of the burden that AD laws place on religious objectors. The dominant theory of the relationship between religious exemptions and religious objection put forth in litigation is that religious exemptions relieve the harm that antidiscrimination rules inflict on religious individuals. Among other things, the theory assumes that the only effect of exemptions would be to relieve devout individuals of the societal harm, but that exemptions do not change behavior, because religious objectors would not have provided services to same-sex couples in any event.¹⁸³ Recently, Professor Barclay suggested to formalize this theory in economic terms, arguing that the harms incurred by same-sex couples (as a result of discrimination) should be weighed against the harms incurred by religious objectors (as a result of the AD law), by examining the transaction costs for each party. Barclay argues that the costs to religious objectors are extremely high, because of the idiosyncratic and fixed nature of their beliefs. She reasons that compelling them to act against their faith will increase net societal harm, because religious objectors will either become martyrs, or will end up with a broken conscience.¹⁸⁴ Under this theory of Berg, Laycock, Barclay, and others, the availability of exemptions should not change the scope of religious objection (because they will not enter the transaction in any event), only its consequences for the objectors.

But the results of the *Masterpiece Cakeshop* experiment bely this theory. First, the seeming availability of a religious exemption post-*Masterpiece*

¹⁸² See *Telescope Media Grp. v. Lucero*, 936 F.3d 740, 762 (8th Cir. 2019); *Brush & Nib Studio v. City of Phoenix*, 448 P.3d 890, 926–27 (Ariz. 2019); *Lexington-Fayette Urban Cnty. Hum. Rts. Comm'n v. Hands On Originals*, 592 S.W.3d 291, 298 (Ky. 2019).

¹⁸³ Berg & Laycock's Brief, *supra* note 79, at 32.

¹⁸⁴ Barclay, *supra* note 177, at 27–28 (relying in part on Christopher Lund, *Martyrdom and Religious Freedom*, 50 CONN. L. REV. 959, 965–67 (2018)).

Cakeshop changed the scope of refusal to same-sex couples. To the extent that this effect is due to *Masterpiece Cakeshop*'s encouragement of religiously motivated objection, the data unsettle the theory that religious objection is a result of permanent idiosyncratic features of the objectors' religious identity, features that are unyielding to external influence. Rather, it seems that religious objection is contingent on the seeming availability of an exemption. The demand for objection is not fixed, but elastic. Second, this effect is not related to the imposition or relief of any penalty or enforcement. While it is theoretically possible that prior to *Masterpiece Cakeshop*, some religious objectors in no-exemption regimes caved in to legal pressure because they could not afford the penalties,¹⁸⁵ the experiment shows that wedding vendors changed their behavior in the absence of any state penalty and absent any likelihood of enforcement. First, as discussed above, discrimination increased in regimes that do not prohibit discrimination at all. Second, the option to ignore an email from a same-sex couple was available to all vendors both before and after *Masterpiece Cakeshop*, without anyone ever knowing their reasons for doing so. Wedding vendors changed their behavior not because *Masterpiece Cakeshop* relieved them of a penalty associated with their behavior, but due to other reasons—more likely, the expressive effect of the decision.¹⁸⁶

These findings suggest that transaction costs in religion-equality conflicts are in fact dynamic, and that the religious objection to AD laws can fluctuate as a result of the availability of exemptions in ways that defy the “martyr/broken conscience” dichotomy.¹⁸⁷ These findings require courts to probe deeper into the characteristics of religious objection and explore more carefully the assumptions regarding the magnitude of harm caused to religious objectors from the *unavailability* of exemptions.

Clearly, this is a highly sensitive issue, and posing the question by no means underestimates the possibility that such harm is real and grave for some religious objectors. At the same time, law in general and the Supreme Court, in particular, always navigate two different levels of generality: the

¹⁸⁵ Despite the conscientious harms they experienced. This is the “broken conscience” concern developed in the context of harms by Barclay, *supra* note 177, at 27–28.

¹⁸⁶ See *supra* Part III.A.

¹⁸⁷ Barclay, *supra* note 177, at 27–28.

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specific case and the general rule. In specific cases involving specific objectors, the harm from not providing an exemption could be enormous. Yet, because each decision also contributes to the formation of a general rule, courts cannot ignore how specific decisions eventually create precedents that influence the availability of rights and remedies for everyone, including individuals who do not necessarily share the features of the specific objector. The *Masterpiece Cakeshop* effect indicates that there are wedding vendors in this broader category who are willing to provide services to same-sex weddings in the first instance but become unwilling to do so once an exemption is announced.

There is no doubt that the Court faces an acute dilemma. Both equality before the law and religious liberty are fundamental constitutional rights, and setting their respective boundaries is no simple task. However the Court decides to resolve the constitutional issues at hand, it ought to consider the harms that might result from its decision, and to avoid or mitigate these harms if possible. Courts are often motivated by a desire to provide justice in particular cases without creating inadvertent and unjust consequences across the board. An important first step towards this goal is to follow the general prescription offered in this Article: to rest constitutional analyses of consequentialist arguments directly on relevant empirical evidence.

Exhibit 3

50(1) THE JOURNAL OF LEGAL STUDIES 1 (in press)

Religious Exemptions Increase Discrimination Towards Same-Sex Couples: Evidence from Masterpiece Cakeshop

Netta Barak-Corren*

ABSTRACT

In 2018, the Supreme Court decided *Masterpiece Cakeshop v. Colorado Civil Rights Commission* in favor of a baker who refused service to a same-sex couple due to his religious beliefs. This article examines the behavioral effect of this decision in an experiment (N=1,155 businesses) that measured discrimination towards same-sex couples in wedding services shortly before and after *Masterpiece*. I find that *Masterpiece* significantly reduced the agreement to serve same-sex couples as compared with opposite-sex couples, even among previously willing vendors. Considering the variety of vendors involved in a typical wedding, I estimate the odds that same-sex couples would experience discrimination post-*Masterpiece* between 61% and 85%. These results show that even a narrowly construed exemption can have a significant and robust, even if inadvertent impact on a market and its customers. I discuss the implications of these results for the research on Supreme Court effects on the public.

1. INTRODUCTION

What are the consequences of exempting religious objectors from antidiscrimination law?

* Associate Professor of Law, Hebrew University of Jerusalem. For helpful comments and suggestions I thank Ronen Avraham, Stephanie Barclay, Oren Bar Gill, Ittai Bar-Siman-Tov, Hanoach Dagan, Yuval Feldman, Michael Freedman, Rick Garnett, Noam Gidron, Mark Graber, Michael Helfand, Vicki C. Jackson, Ehud Kamar, Kobi Kastiel, Amir Khoury, Jeff Rachlinski, Alexander Stremitzer, Nelson Tebbe, Mila Versteeg, Eyal Zamir, and participants at the 2019 Annual Roundtable on Law and Religion, the 2019 Workshop on Behavioral Legal Studies: Cognition, Motivation and Moral Judgments, Hebrew University public law workshop, Humboldt-Minerva Human Rights Under Pressure seminar, Tel Aviv faculty workshop, and Bar Ilan faculty workshop. I also benefited tremendously from the dedicated reading and insightful suggestions of JLS editors and two anonymous reviewers. A team of dedicated assistants that included Tamir Berkman, Yechiel Oren, and Tani Shimoff assisted with data collection and coding; Tamir Berkman also provided outstanding research assistance. Responsibility for any errors is my own. This project was approved by the Hebrew University IRB. Materials, data and R code will be available online at JLS's website.

Answering this question is crucial to resolving the escalating tension between religious freedom and sexual orientation and gender equality in the United States. So far, the primary legal tool that has been proposed as a solution has been religious exemptions. But the use of exemptions have also raised considerable objections. Central among those is the concern that granting exemptions would escalate the number and significance of religious claims and extend LGBTQ discrimination (NeJaime 2012; NeJaime and Siegel 2015; Stern 2014). Proponents of religious exemptions reject these concerns as factual nonsense, arguing that religious objectors are a negligible minority in a society growing ever more affirming of LGBTQ equality, and that exempting religious objectors will not expand discrimination against same-sex couples (Koppelman 2014; Berg and Laycock 2017; Laycock and Berg 2018; Laycock 2017, 3:962).

Currently, there is almost no evidence that could clarify which of the contradictory factual arguments is actually true. Not only that such evidence is required to settle and refine theoretical debates, it is also crucial to inform legislators debating whether to enact religious exemptions, and courts deliberating whether to grant such exemptions. From *Reynolds v. United States* (1878), the first case to bring the question of religious exemptions before the Supreme Court, through key decisions such as *Employment Division v. Smith*¹ and *Burwell v. Hobby Lobby Stores, Inc.*,² up to *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Commission*³--the decision at focus in the present study--the Supreme Court has always cited the social consequences of exemptions (or

¹ 292 U.S. 872 (1990) (Scalia, J.) (Citing *Reynolds v. United States* (1878): "To permit this would be to make the professed doctrines of religious belief superior to the law of the land, and in effect to permit every citizen to become a law unto himself.")

² 134 S. Ct. 2751, 2782 (2014) (Alito, J.) (resting the majority opinion on the assumption that the exemption's effect on third-parties "would be precisely zero").

³ 138 S. Ct. 1719 (U.S. 2018).

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lack thereof) when determining whether to reject (or grant) petitions for religious exemptions.

This article elucidates the consequentialist debate on religious exemptions by studying, for the first time, the effects of religious exemptions on sexual orientation discrimination. Part 2 begins by presenting the necessary legal background and the *Masterpiece* decision, in which the Supreme Court ruled in favor of a religious baker who refused service to a same-sex couple. Next, Part 3 describes a large-scale experiment that I designed to measure the impact of *Masterpiece* on sexual orientation discrimination in the wedding services market. To this end, I combined methods from pseudo-experiments (studies that examine the impact of a reform by focusing on events occurring shortly before and after the reform) and field-experiments (studies that randomly allocate different treatments to subjects in the field). Wedding vendors (bakers, photographers, and florists) were sampled to the experiment from the four legal regimes currently existing in the United States that differ based on whether they prohibit sexual orientation discrimination in public accommodations (AD law) or not, and on whether they facilitate religious exemptions via a Religious Freedom Restoration Act (RFRA) or not.⁴ This resulted in a 2 (AD law/no AD law) by 2 (RFRA/no RFRA) matrix from which 906 businesses were sampled to the experiment. Each business was examined shortly before (May 8th-15th, 2018) and after (June 13th-20th, 2018) the *Masterpiece* decision (rendered on June 4th, 2018). In each period, wedding businesses were contacted via email by a same-sex or an opposite-sex couple inquiring about wedding services. Each business was contacted by the two types of couples both before and after the decision, resulting in four observations per business and a rich dataset that allows for both within-business and across-businesses comparisons. A “control” group of 251 businesses was contacted for the first time after

⁴ RFRA is not the only legal vehicle facilitating religious exemptions. Others are addressed in Part 2.

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Masterpiece, by both couple types, following the same procedures. The question of interest was whether businesses agreed to provide the requested service to couples, and specifically whether *Masterpiece* influenced the responses businesses provide to same-sex couples.

Part 4 presents the results of the *Masterpiece* experiment. Briefly, the decision significantly reduced the willingness to serve same-sex couples, from 63.6% before *Masterpiece* to only 49.2% after the decision was rendered (a 14.4 percentage-point gap, or ~23 percent decrease in favorable responses). Zooming in on businesses that, prior to *Masterpiece*, responded positively to same-sex couples, I find that these businesses discriminate between opposite-sex and same-sex couples after *Masterpiece*. Previously “gay-friendly” businesses that are randomly contacted by opposite-sex or same-sex couples after the decision was rendered respond less favorably to same-sex couples (75.5% vs. 66.3%, a 9 percentage-point gap, or 12 percent fewer favorable responses). This effect is not an artifact of the experiment itself, as it is identically found in the “control” group. Probing into the differences between the four legal regimes, I find that the negative *Masterpiece* effect appears in all regimes, except for those that enacted *both* an AD law and a RFRA. The effect is robust, including in analyses that control for county-level conservativeness and analyses limited to businesses located in big cities (where, it is often argued, there is no discrimination problem). However, the effect of *Masterpiece* is significantly more pronounced in religious environments, as proxied by the density of congregations in the county where the business is located.

A back-of-the-envelope calculation demonstrates the broader implications of these results. Provided that couples of all identities typically contract with about ten types of vendors in the process of organizing a wedding (reception venues, wedding planners, bakers, florists, photographers, videographers, bridal/groom salons, jewelers, DJs, and calligraphers—a partial

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list); and that they often inquire with several vendors of each category; and that the average risk of experiencing discrimination post-*Masterpiece* is about 9%; I estimate the aggregate risk of discrimination for same-sex couples between 61% and 85%. This means that, across the observed differences between legal regimes and religious environments, *Masterpiece* has the general effect of exposing same-sex couples to heightened risk of discrimination in the organization of weddings.

Part 5 discusses the results. First, I consider potential explanations for the effect of *Masterpiece* on vendors' behavior, examining both cognitive mechanisms (how the decision is perceived and factored into the decision-making process) and social mechanisms (how social factors, particularly religion, interact with the decision-making process). Moving to implications, I discuss the contributions of the study to the research of Supreme Court effects on the public. In particular, the study sheds new light on the argument that courts are ineffective at spurring social change (Rosenberg 2008), by highlighting the risk that the court will emerge as effective, paradoxically, by spurring inadvertent social change. On the constitutional end, the results discredit the argument that religious exemptions will not expand discrimination. Instead, what the *Masterpiece* experiment shows is that even a narrow exemption can have a significant and robust impact on a market and its customers. Next to these implications, the observed variation between legal regimes and religious environments provides preliminary evidence that discrimination can be minimized under certain conditions. These findings can guide future research on religion-equality conflicts, Supreme Court effects, and the dynamics of discrimination more broadly.

2. LEGAL BACKGROUND

A. The “legislative mismatch” of Antidiscrimination and Religious Freedom Laws

The regulation of sexual orientation discrimination and religious freedom in the U.S. vary at the state and local level. Some jurisdictions prohibit public accommodations (such as wedding businesses) from discriminating on the basis of sexual orientation (hereinafter: “AD laws”), while other jurisdictions do not prohibit such discrimination.⁵ Concomitantly, some jurisdictions enacted rules that facilitate the creation of religious exemptions from rules of general applicability (often these are Religious Freedom Restoration Acts, or RFRAs), while other jurisdictions have not enacted such rules.⁶ With the legalization of same-sex marriage, new RFRAs were enacted in Mississippi (2014), Indiana, and Arkansas (NCSL 2017). In other states, e.g., Iowa and Georgia, RFRA bills failed due to concerns about their implications for LGBTQ rights and fears from commercial boycotts (Foody 2015). In the process, RFRAs became the legislative antonym of AD laws (Ferguson 2015).

The distribution of AD laws and RFRAs across states is a “legislative mismatch” (Lupu 2015) with a narrow overlap. The overlap consists of four states that enacted both laws,⁷ a maximum of seven states that have enacted AD laws and have extended protections on religious freedom in their constitutions,⁸ and a considerable number of local governments in RFRA states that enacted municipal AD laws (Lupu 2015, pp. 48-49). This last category includes a number of major cities

⁵ Twenty-two states, the District of Columbia, and numerous local governments enacted such laws (HRC 2018).

⁶ Twenty-one states enacted RFRAs (NCSL 2017) and ten states interpreted their constitutions to require a RFRA-like standard of review (Volokh 2013). Lupu (2015) and Volokh (2013) classify differently which states have interpreted their constitutions to require a RFRA-like standard. I return to this issue in Section 3.B, where I note that the lack of clarity regarding the legal status in these states was a reason to avoid sampling businesses from them.

⁷ Connecticut, Illinois, New Mexico, and Rhode Island.

⁸ Maine, Massachusetts, Minnesota, New York, Vermont, Washington, and Wisconsin.

in conservative states, such as Dallas, Indianapolis, Phoenix, and Atlanta (Lupu 2015, p. 49). To date, no local government in an AD state has enacted a local RFRA.

In summary, the regulation of the tension between marriage equality and religious liberty divides into four categories: regimes (state or local) with both AD laws and RFRAs; AD-law-only regimes; RFRA-only regimes; and regimes that have enacted none. This is the diverse legal background against which *Masterpiece Cakeshop* was decided.

B. *Masterpiece Cakeshop v. Colorado Commission of Human Rights*

The *Masterpiece* case presented a conflict between Jack Phillips—the owner of Masterpiece Cakeshop—and Charlie Craig and David Mullins, a same-sex couple who entered his cakeshop to inquire about a wedding cake, unaware of Phillips' beliefs. Phillips declined to make the cake citing his objection to same-sex unions.

The Colorado Civil Rights Commission found that the baker discriminated against the couple based on their sexual orientation. The Supreme Court reversed and invalidated the Commission's decision, writing that the Commission showed unconstitutional religious hostility. While the baker won the case on free exercise grounds, the majority acknowledged that "if that [religious] exception were not confined, then a long list of persons who provide goods and services for marriages and weddings might refuse to do so for gay persons, thus resulting in a community-wide stigma inconsistent with the history and dynamics of civil rights laws." (*Masterpiece*, p. 1727). For these reasons, the Court did not rule out the possibility that Colorado could enforce its AD law in similar cases in the future. More generally, the majority's opinion did not expressly solve the tensed relationship between religious liberty and sexual orientation equality

3. THE *MASTERPIECE* EXPERIMENT

A. The motivation and setting for the experiment

The primary purpose of the project is to examine the contradicting empirical assumptions regarding the effects of religious exemptions on discrimination towards same-sex couples. The present contradictions and omissions have made it impossible to assess the merits of the opposing positions and leave the debate hanging in the air.

Masterpiece Cakeshop created an opportunity to evaluate these arguments in their most pressing setting. Based on the oral arguments, I anticipated that the decision would yield an exemption of sorts.⁹ As “one of the most anticipated decisions of the term” (Howe 2018), the decision was likely to draw extensive coverage and discussion in the public media (as it did), thus to potentially have an impact on public attitudes and conduct (Linos and Twist 2016).

When the decision was finally rendered on June 4th, 2018, it received broad coverage and mixed responses. National, state and local news outlets covered the decision and sought comment from local advocacy groups and politicians (Simpson 2018; Platoff 2018; McGaughy 2018). All mainstream outlets, including the New York Times, NBC News, and CNN, titled the decision a victory for the baker; they also called the decision “narrow,” explaining that it did not resolve the big constitutional questions at issue (Liptak 2018; Williams 2018; Goldfeder 2018). At the same time, many conservative leaders and religious liberty advocates hailed the decision as a victory,

⁹ This expectation was based on the comments of Justice Kennedy, then the Court’s swing seat, who hinted that the Court thinks that there was “a significant aspect of hostility to a religion in this case”, Tr. of Oral Arg. 53. This became a dominant line of questions for the conservative judges on the bench, *id.* at 53-59. Justice Kennedy also said unequivocally, “Counselor, tolerance is essential in a free society. [...] It seems to me that the state in its position here has been neither tolerant nor respectful of Mr. Phillips' religious beliefs.” *Id.*, at 63.

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expressing less reservations about its scope.¹⁰ Fox News Insider (2018) held a supportive interview with Phillips, who defined the decision as a “big win.”¹¹ Leaders of the U.S. Conference of Catholic Bishops released a joint statement applauding the decision, saying that it “confirms that people of faith should not suffer discrimination on account of their deeply held religious beliefs, but instead should be respected by government officials” and emphasizing the decision’s expression of pluralism and tolerance (Catholic News Agency 2018). The Family Research Council (2018) released a statement that the decision “made clear that the government has no authority to discriminate against Jack Phillips because of his religious beliefs” and that the “ruling means that Jack will remain free to live according to his beliefs whether he is at work, at home, or in his place of worship.” These statements do not betray any doubt about the scope of the decision or mention the Court’s recognition of the important role of AD laws in protecting LGBTQ people.

Some LGBTQ advocates and progressive commentators observed these enthusiastic responses and voiced concerns that *Masterpiece* will grant objectors a license to discriminate. GLAAD president said that “it leaves the door wide open for religious exemptions to be used against LGBTQ people” (Lang 2018). The president of LGBTQ Victory Institute further warned that “Homophobic forces will purposefully over-interpret the ruling and challenge existing non-

¹⁰ To give the readers a better sense of the conservative and religious framing of the decision, I survey key quotes in the footnotes. Consider Kao (2018): “the decision... [exposed] a huge fallacy in the ACLU’s main argument in the case... The court’s clear rejection of the discrimination argument has implications for many of the other conflicts currently brewing between religious freedom and sexual orientation.”; Liberty Counsel (2018) :“Though the Court focused on the explicit hostility exhibited by the Colorado Civil Rights Commission in this specific instance, this significant decision will have a wide impact regarding the clash between free speech and the LGBT agenda, including laws that add ‘sexual orientation’ and ‘gender identity.’”

¹¹ See also Starnes (2018) (“Monday’s ruling should give some comfort to Christian business owners who primarily service the wedding industry – gay rights do not necessarily trump everyone else’s rights”). Other coverage by Fox News was more careful in discussing the decision, e.g. Mears (2018) (“The narrow ruling here focused on what the court described as anti-religious bias on the Colorado Civil Rights Commission when it ruled against baker Jack Phillips.”).

discrimination laws by refusing service to LGBTQ people in even more situations” (Lang 2018). NBC’s columnist Scott Lemieux (2018) wrote that the decision “presents a serious risk of undermining civil rights law in the name of religious freedom, especially given that it invites yet further suits for the court to consider.”

This combination of factors—a highly anticipated decision, a court that appeared positioned to exempt the religious objector, and the massive coverage that communicated the above messages—created a favorable setting for an empirical test of the behavioral effects (or lack thereof) of religious exemptions. In a previous study, Linos and Twist found that Supreme Court decisions can increase support for controversial policies that were vindicated by the Court (e.g., Obamacare), even when the court was divided and the decision was nuanced (Lincoln and Twist, 2016). Similarly, three recent studies that measured the effect of same-sex marriage legalization on public attitudes documented increases in support for same-sex marriage post-*Obergefell* (Kazyak and Stange 2018); increases in the perception that social norms support same-sex marriage (Tankard and Paluck 2017); and, prior to *Obergefell*, sharper decreases in antigay bias in states that legalized same-sex marriage compared with states that did not (Ofosu et al. 2019). All of these studies were based on attitudinal surveys conducted shortly before and after court decisions or legislative acts, sometimes with an additional experimental component that randomized the framing of the decision. Additional surveys examined public attitudes about LGBTQ discrimination, irrespective of Supreme Court decisions or same-sex marriage legalization (Powell, Schnabel, and Apgar 2017). Yet none of these studies examined the impact of Supreme Court decisions on the behavior of pertinent decision-makers. In the present case, the pertinent decision-makers are wedding vendors and the question is whether they are influenced from a ruling for a vendor who denied

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service to a same-sex couple.

In addition, most previous studies did not investigate whether the effect of Supreme Court decisions varies between socio-legal regimes. Yet different legal regimes can direct wedding vendors towards different behaviors and therefore differentiate their response to *Masterpiece*. For example, businesses in regimes that resemble Colorado—with AD laws and without RFRA—might adopt their response to same-sex couples if *Masterpiece* is perceived as relaxing their AD obligations. One may expect such change to be more pronounced in overlap regimes, because the existence of a RFRA could strengthen the impression that businesses are likely to secure an exemption post-*Masterpiece*. In contrast, businesses in regimes that have never enacted AD laws have no legal basis to change their behavior. For these businesses, the law has not changed: they are as free to discriminate after *Masterpiece* as they were before the ruling.

In sum, *Masterpiece* provided a unique opportunity to study the behavioral effect of religious exemptions from antidiscrimination law, a question of which no empirical data exist to date, and which bears heavily on contemporary legal debates. In addition, the study advances the research of the effects of Supreme Court decisions in several ways: First, the study expands the examination from attitudes to behaviors, and from the general public to pertinent decision-makers. Second, the study goes deeper than previous studies in probing the relationship between the national judicial “shock” and the preexisting legal structures that could vary the effect of the decision between otherwise similar subnational regimes.

B. Research design and methods

To assess whether *Masterpiece* had an effect on sexual orientation discrimination in the

wedding industry, I combined methods from pseudo- and field-experiments. As in pseudo-experiments, I examined the behavior of wedding in businesses in two periods: before (May 8th-15th) and after (June 13th-20th) the decision (June 4th, 2018). As in field experiments, the methods of this examination aimed to control the setting of the examination and the allocation of the sexual orientation treatment between subjects (businesses) to allow for causal inference. The following subsections describe the construction of the sample, materials, and procedure of the experiment.

Sample. Sample construction began with a preliminary comparison of all states, to find those that were most comparable in their overall characteristics yet differed in their legal regime. The comparison included GDP per capita, the importance of religion for state residents, the share of Evangelicals in the state, the share of conservatives, attitudes towards homosexuals, and attitudes towards same-sex marriage. After matching demographic resemblance against legal regime variation, four states were selected for sampling: Indiana, Texas, Iowa and North Carolina. Table 1 shows that these states have roughly the same attitudinal and economic characteristics, that are either at the national average or more conservative.

TABLE 1 – CHARACTERISTICS OF SAMPLED REGIMES

Criterion	Definition	Iowa	North Carolina	Indiana	Texas	Dallas Metro, TX	Houston Metro, TX
GDP per capita (\$)		59,978	54,442	55,173	61,168	--	--
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	79%	84%	78%	86%	85%	83%
% Conservatives	(National average: 36%)	41%	40%	41%	39%	41%	38%
% Evangelicals	(National average: 25%)	28%	35%	31%	31%	38%	30%
Attitudes Towards Homosexuals	“Homosexuality should be discouraged” (National average: 31%)	36%	36%	37%	36%	35%	39%
Attitudes Towards Same-Sex Marriage	Opposing/Strongly Opposing Same-Sex Marriage	41%	45%	45%	46%	44%	51%

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(National average: 39%)

State RFRA	No	No	Yes	Yes	Yes	Yes
State/Local AD law	Yes	No	Some	Some	Yes	No

Notes: GDP per capita is calculated based on data from 2018, Q2. Sources: GDP: THE U.S. BUREAU OF ECONOMIC ANALYSIS, GROSS DOMESTIC PRODUCT BY STATE, SECOND QUARTER 2018 (2018); Population: U.S. CENSUS BUREAU, 2018 NATIONAL AND STATE POPULATION ESTIMATE (2018); All other data: PEW RESEARCH CENTER, RELIGIOUS LANDSCAPE STUDY (2014). State/Local AD Law (row 8) refers to public accommodation laws that apply to private businesses and are enacted at the state or city level.

Alongside their demographic and attitudinal similarity, the sampled states vary in how they regulate religious freedom and public accommodations. North Carolina has no RFRA and no AD law at any level of government.¹² Iowa has no RFRA (at no level of government) but has a state AD law (IOWA CODE § 216.7). Both Indiana (IND. CODE § 34-13-9) and Texas (TEX. CIV. PRAC. & REM. CODE ANN § 110.011) have state RFRAs and no state AD laws, yet some local governments within these States have AD laws. Sampling from all of these regimes produced a 2 (+/- AD) by 2 (+/- RFRA) sampling matrix (Table 2).

Two reasons were responsible for the choice of Texas and Indiana as models of the overlap category (+RFRA,+AD) and the +RFRA-AD category. As Part 2 describes, the overlap between RFRAs and AD laws has three versions: (1) states that enacted both laws; (2) states that enacted an AD law and their courts interpreted their constitution to provide a RFRA-like standard; and (3) local AD laws within RFRA states. The primary reason for choosing the third version to model the category was that the demographic and attitudinal characteristics of the four states that enacted both laws (RI, CN, NM, IL) and the states that only had RFRA, without an AD law, differed too widely than the states populating the other matrix categories. Second, not all RFRA designs raise the same tension with AD laws. RFRAs in the first overlap category are mostly narrow and not

¹² NC has enacted a specific religious exemption allowing magistrates to refuse to perform same-sex marriages, SB 2 (2015), but there is no exemption for private businesses. NC also prohibited cities or counties from passing sexual orientation AD laws, HB 2 (2016), what explains the complete absence of local AD laws in the state.

conducive for the examination of this tension, while the second category raises considerable uncertainty regarding the very existence of the tension (Volkh 2013, cf. Lupu 2015). Texas and Indiana provided an adequate demographic and attitudinal comparison to the other legal categories, as well as clarity regarding the classification of their legal regimes.

TABLE 2 – THE LEGAL REGIME MATRIX AND SAMPLED STATES

	RFRA	No RFRA
AD law	Specific jurisdictions in Indiana and Texas ¹³	Iowa
No AD law	Specific jurisdictions in Indiana and Texas ¹⁴	North Carolina

To be sure, although I invest great efforts in facilitating the comparison between regimes, I do not argue that this design is capable of identifying a causal relationship between specific regimes and behavioral outcomes. First, it is difficult to separate the legal regime from the underlying political and social climate that produced the law.¹⁵ Second, different laws provide different behavioral guidance—as discussed above—but businesses might not be fully aware of law's dictates. Nevertheless, legal regimes are likely to matter, if not for the direct impact of law then for the underlying socio-political structures that it reflects. Had I only sampled from one regime, important variation would have been masked and misleading interpretations might have been construed. Exploring how businesses in different regimes respond to *Masterpiece* is thus

¹³ At the time of the experiment, these included the following: in Indiana, Indianapolis, Fort Wayne, Evansville, Bloomington, Muncie, South Bend, Terre Haute. In Texas, Dallas, San-Antonio, Austin, El-Paso, Plano, Fort Worth.

¹⁴ At the time of the experiment, these included the following: in Indiana, West Lafayette. In Texas: Houston, Irving, Arlington, Corpus Christi, Lubbock, Garland, Amarillo, Grand Prairie, Brownsville, McKinney, Killeen, McAllen, Waco, Denton, Round Rock, College Station.

¹⁵ In addition, the law is determined not only based on acts of the legislature but also based on judicial decisions and administrative directives that interpret the enacted rule. I attempted to account for those—for example, by not sampling from overlap states where courts interpreted RFRA as providing no protection against AD claims—but it is very difficult to account for all interactions between judge-made law and legislated law.

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necessary, even if the results are only suggestive and causal inference is limited.

A power analysis (via G*Power) determined that a sample size of 179 businesses per legal category is needed to detect a medium-size effect (.25) with 80% power (assuming four legal regime groups and two covariates—see below). The detection of within-subject effects (sexual orientation and the effect of *Masterpiece* itself) required a considerably smaller sample. Yet due to pitfalls that could result in sample reduction (e.g., inactive businesses or email addresses; technical failures) the sampling aimed for 250 businesses per legal category.

The wedding industry includes a variety of vendors and services, such as photography, videography, flower arrangements, dresses, suits, wedding cakes, wedding planning, venues, and more. Recent cases in which businesses refused service to same-sex couples involved bakers, photographers, and florists, among others. I was particularly interested in bakers and photographers, because these businesses represent different models of involvement in the wedding: photographers typically spend many hours with the couple, take an active part in the event and are present throughout the wedding, often for 9-10 hours. Typically they also create the couple's wedding album, requiring continued relationship with the couple. In contrast, bakers typically have a more limited interaction with the couple (during tastings and the order), do not play an active role in the event and are not present in the wedding. These differences in personal involvement could bear on vendors' willingness to serve couples. Therefore the sampling focused on these two types of vendors, supplementing them with florists in one legal regime (Iowa) where not enough photographers and bakers were found.¹⁶

¹⁶ Iowa contained smaller populations of bakeries and photographers compared with the other regimes (and particularly bakeries). Florists were chosen to augment the sample because of prior conflicts involving this industry

The sample was built by collecting all vendors in each legal regime that could be found on a simple Google search and published an email address as a form of communication.¹⁷ Contacting wedding vendors via email is a very common, if not the most common method of communication of couples: There is ample guidance online on how to write an email to potential wedding vendors and multiple websites assume that email is the default or best form of communication with vendors.¹⁸ After mapping states and cities/counties that fitted into the legal regime typology, businesses were sampled based on regime size, from large to small. Thus, the sampling gave preference to large political units (e.g., big cities) over small political units (e.g., small cities and rural counties) and ended when the designated sample size was obtained.¹⁹ Each business included in the sample was individually checked and verified to be a relevant business (e.g., a bakery rather than a coffee shop). The final sample includes the entire population of bakeries that met the search criteria in each legal regime, and a large sample of the respective photographers' population.²⁰

(e.g., *Arlene Flowers*, *supra* note 2) and because florists' involvement in the wedding was assumed to be intermediate: florists do not fill an active role in the event and are not present throughout the event, similar to bakers; and they do not appear to be offering shelf products, similar to photographers.

¹⁷ The search words were "[profession] in [jurisdiction]" (e.g., "wedding photographers in Indianapolis, IN").

¹⁸ See, e.g., Forrest (2017) (assuming communication is done via email); Currey (2019) (providing guidance on how to write emails to potential wedding vendors); Malie (2018) ("An email is usually the preferred method for inquiries as it allows the vendor to keep track of your conversation, respond in length and from a desktop, and allows them to easily attach files, reference links, and more."). Vendors that did not publish an email address typically had an online application form on their website, reducing the potential concern that the sample is biased towards technology-oriented vendors.

¹⁹ If the search yielded more results than needed, only the first valid results were included. The rationale for including top results rather than a random sample of search results was based on the Google search algorithm, which prioritizes relevant results, and also on the researchers' experience that first result pages include more relevant results than advanced pages. Top results were typically within the geographic boundaries I searched for, whereas later results were often in suburbs or other cities/counties; in addition, top results typically met the definition of the searched business, whereas later results sometimes belonged to other types of businesses (e.g., a Starbucks coffee shop that came up in a wedding bakeries search).

²⁰ In Iowa (+AD – RFRA), the sample also includes the entire photographer population (Iowa was the only category in which the search was not able to collect 250 businesses and exhausted all business types at 218 businesses). In North Carolina (- RFRA - AD) the sample exhausted 89% of relevant photographers. In the +RFRA -AD regime (Texas and Indiana) the sample exhausted 88% of the population. In the +RFRA +AD regime (Indiana and Texas) our sample exhausted about 50% of the relevant population (the photographer population in this regime was bigger than other regimes).

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In addition to the main experiment sample, I also constructed a “control” group of 251 vendors which was only contacted after *Masterpiece*. The control group was composed of photographers and florists from three of the four legal regimes (as all businesses in Iowa and bakers in all jurisdictions were exhausted in the experimental group). The control exhausted the relevant photographers population in each regime in addition to 45 florists from each regime. As explained in more detail in the procedure, the control group was not designed to test differences between regimes or business types, but to evaluate the effect of the experimental procedure itself.

Procedure and materials. Sixteen fictitious email profiles were created for the experiment. In order to assess the baseline discrimination pattern, each business received two emails prior to *Masterpiece* from two different ‘couples’: a same-sex couple (1st wave) and a different-sex couple (2nd wave). The couples’ sexual orientation was made evident by their names. The name of the sender, appearing in the profile information and the signature, was a generic white American male name (John, Robert, Dylan, Scott). The name of the prospective spouse appeared inside the body of the email and was a generic name for a white American male or female, depending on the couple’s identity (Adam, Paul, Harry; Jessica, Ashley, Rebecca). The emails had similar properties, including similar information about the fictitious couple, the service requested from the vendor, and the prospective timing of that service,²¹ and they were written in the same level of cordiality. Small, meaningless changes were inserted to diminish suspicion (including variations in wording, font size, font color, and signature style). The emails were sent one week apart, during

²¹ The timing of the prospective wedding noted in the email was determined based on market norms about when approximately to contact specific vendors. While the recommended timeline somewhat varies from one wedding portal to another, the emerging norm seems to be that photographers should generally be contacted between 9-11 months in advance, and florists and cakeshops should be contacted 6-9 months in advance (some advices provide a shorter timeframe for cakeshops). Thus, emails to photographers noted a wedding date which was 11 months away and emails to florists and cakeshops noted a date which was 8 months away.

business days and hours, at about the same day of the week and time of the day, with an intentional hour lag to reduce suspicion.²² All email versions are included in the Online Appendix.

A week after *Masterpiece*, on June 13th, all businesses were randomized to receive an email from a same-sex or a different-sex couple (3rd wave); and on the following week, each business received an email from the opposite-orientation couple (4th wave) (Overall, each business received emails from both couple types, one week apart). The emails of each wave had similar properties and were different from the two pre-*Masterpiece* emails. To increase responsiveness and further distinguish the communication from previous waves, post-*Masterpiece* emails included profile pictures and phone numbers, and alternated further the style and formatting of the emails. Each email was always sent from a profile that has not contacted that business before; altogether, each business received four different emails from four different profiles.

Businesses of the control group were contacted for the first time in the 3rd and 4th waves (after the decision was rendered), following the exact same post-*Masterpiece* procedure and during the exact same times. The control group was included to evaluate the possibility that the repeated measurement of businesses in the experiment group could have influenced their behavior, independent from the effect of the decision or couple's identity. More specifically, the goal of the control group was to assess whether the effects of *Masterpiece* were similar among businesses that were contacted both before and after *Masterpiece* and businesses that were freshly-contacted only after *Masterpiece*. This comparison provided an independent reference for response rate and attrition rate, which allowed for an additional robustness check.

²² In each wave, some subjects received the email 24 or 48 hours after the main group, due to logistic issues. All emails were sent during business days and hours.

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In order to reduce suspicion and fatigue, the research team answered each responding business manually once, soon after the response was received, and before the next wave of emails. The answers were personal and varied based on each business' response. Typically, the responses requested more time to think or mentioned a reason for not continuing the correspondence which was unrelated to the details of the offer. Fifty-nine vendors had to be excluded from the sample because of email communication failures (typically, not receiving one of the four emails), and 15 were omitted due to explicit or potential suspicion.²³ The final sample, after exclusions, remained significantly larger than required to detect the minimal effect, based on the power analysis:

TABLE 3 – THE FINAL SAMPLE OF BUSINESSES IN THE EXPERIMENT

	RFRA	No RFRA
	N = 212	N = 210
AD	Photographers: 125	Photographers: 93
	Bakers: 87	Bakers: 35; Florists: 82
	N = 244	N = 238
No AD	Photographers: 179	Photographers: 155
	Bakers: 65	Bakers: 83

For any given inquiry, I measure the response it elicits from the business based on the email communication. Two RAs coded the entire dataset of emails under the close supervision of the PI. The research team conducted numerous meetings throughout the coding process to discuss the coding method, resolve open issues, and fine-tune the coding scales.²⁴ The main outcome of interest in all analyses is whether businesses agreed to provide service to the couple.²⁵

Between the 3rd and 4th email waves a phone survey was conducted with a random sample of

²³ Online Appendix, Section OA2, details reasons for exclusions and the number of vendors excluded.

²⁴ Online Appendix, Section OA5, provides further information regarding the coding process.

²⁵ I had two measures for this outcome, binary and nuanced. The scale for Nuanced Response was: 1 = positive response, 0.5 = cooperative response, e.g., a vendor that asks for more information on the date or the location, 0 = no response, -0.5 = a negative response that includes a referral to other providers/services, -1 = negative response. Binary Response coded all responses > 0 as 1 (positive) and all responses <= 0 as 0 (negative).

wedding vendors to gain insight on non-response patterns observed in waves 1 and 2 (See below). The appendix describes the phone survey's sample, procedure, and results.

C. Strengths and Weaknesses of the Experiment

The experimental design has multiple methodological strengths. First, it combines two of the most powerful methods for causal inference—pseudo-experiments and field experiments—to enable the study of an actual, concrete event—the *Masterpiece* decision—in a controlled setting. Second, sending carefully designed materials of fictitious individuals instead of real auditors creates a controlled setting for the study and removes inadvertent auditor biases (Bertrand and Mullainathan 2003). Third, the outcome measure—agreement to provide services to the couple—is less crude than, for example, callbacks in employment experiments (Bertrand and Mullainathan 2003). This is because the argument about discrimination in wedding services relates to the specific stage of the transaction that is studied here: the initial inquiry about the service.

Alongside these strengths, the experiment also has limitations. First, similar to Bertrand and Mullainathan (2003), I study asynchronous communication rather than face to face or phone communication. There are good reasons for that: Using emails enables the creation of a highly controlled experimental setting, including with respect to the couple's identities, how they represent themselves to businesses, the exact content of the inquiry, and the timing of the inquiry. All of these are very difficult to achieve in studies that employ real testers (audit studies). Although testers/auditors can be trained to behave similarly, it is impossible to erase the numerous differences between real people, or control for nuances in tone and facial expressions that can disclose the auditors' attitudes or that their search for a job/service is ingenuine (Bertrand and Mullainathan, 2003). In addition, while email inquiries do not capture the entire variation in how

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couples interact with vendors, emails are one of the most common methods of communication between couples and vendors, especially in the inquiry phase. To the extent that the process of negotiating with vendors has even moderate friction, one would expect that reduced positive responses to emails would ultimately translate into less market opportunities for same-sex couples. However, how the results translate to additional methods of communication remains an open question. This can be a topic for a future study.

An additional limitation, that flows directly from the method of a pseudo and controlled experiment, is that I examine the effect of *Masterpiece* in a relatively short time span: several weeks after the ruling. While collecting more observations would have been desirable, it was not possible to continue isolating the effect of the decision from intervening political developments. Shortly after the decision, legislatures in Texas, Iowa, and North Carolina have proposed, revived and even passed legislation pertaining to religious liberty and LGBTQ rights (Rodriguez 2019; Platoff 2019a; Platoff 2019b; Fitzsimons 2019). In this dynamic landscape, the conduct of businesses can no longer be linked to *Masterpiece*. Future cases may provide opportunities to examine additional effects of religious exemptions on society.

TABLE 4: OVERALL RESPONSE RATES IN EACH WAVE

Wave	Overall Response Rate
W1	70.8
W2	58.7
W3	63.4
W4	61.9

Finally, and this is an issue pervasive in studies that repeatedly measure the same respondents over time (Kalton, 2009; Gerber and Green 2012, pp. 236-238), I encountered a large attrition of businesses in the second wave of inquiries before *Masterpiece* (See Table 4). This pattern hindered

the ability to detect discrimination in the pre-*Masterpiece* period, as the first wave of emails was from same-sex couples and the second wave of emails was from opposite-sex couples. While the causes for this attrition are not entirely clear (this is common to studies that encounter attrition, Fitzgeralds et. al 1998), a random phone survey suggested that businesses that provided no response to the second wave of emails were generally less responsive than other businesses (also over the phone), rather than suspicious or email fatigued.²⁶ To minimize the impact of attrition on the robustness of the design, in the following waves I randomized couples' identity within each wave. In addition, as described in the procedure, the following waves were designed to increase responsiveness by altering the style and formatting of the emails and the couples' profiles.²⁷ This effort succeeded in increasing responsiveness to wave 3 and in reducing attrition between waves 3 and 4. Nevertheless, I concede that the attrition of businesses from wave 2 prevents the evaluation of the existence and extent of sexual orientation discrimination before *Masterpiece*.²⁸ To overcome this pitfall and evaluate the effect of *Masterpiece* on the existence and extent of discrimination *after* the decision, I developed several strategies of analysis which I present next.

4. RESULTS

This Section is organized as follows: it begins with the main analysis, which focuses on businesses that agreed to serve same-sex couples before *Masterpiece* and examines their behavior post-*Masterpiece*. The second analysis examines within-business changes of behavior across all

²⁶ Online Appendix, Section OA3, reports that vendors who did not respond to the 2nd wave email were also less likely to answer the phone than vendors who replied to that email (36% vs. 52%, respectively). In addition, no 'phone favoritism' was found among email non-responders, *id.*

²⁷ Among these alterations, each profile was allocated a unique phone number, including a personalized outgoing message (identical in content).

²⁸ It is possible to infer that prior to *Masterpiece*, opposite-sex couples were disfavored relative to same-sex couples (reversed discrimination), but this inference seems tenuous. To the extent it is true, the magnitude of the *Masterpiece* effect is much larger than estimated below. Section OA4.1 in the Online Appendix reports these results.

businesses over time. I then move to examining differences between legal jurisdictions and between religious environments. Robustness checks and follow up studies can be found in the Online Appendix and are referenced along the way.

A. Has Masterpiece Increased Discrimination Towards Same-Sex Couples?

To answer this question, I evaluate the impact of *Masterpiece* on businesses that agreed to serve same-sex couples before the decision (N=575 businesses * 2 post-*Masterpiece* observations per business, resulting in 1150 observations). The assumption underlying this analysis is that businesses who responded favorably to same-sex couples before *Masterpiece* were equal treatment businesses (for the very least, they were “gay-friendly” businesses). Examining their behavior *after* the decision can provide an answer as to whether *Masterpiece* had a negative effect on the willingness of businesses to provide service to same-sex couples.

Some readers might have expected that a diff-in-diff strategy would be applied to answer the question. But the problem with the diff-in-diff approach is that the attrition between the first and second waves (both of which were pre-*Masterpiece*) meant that a diff-in-diff would be subject to criticism as not having comparable opposite-sex-couple-treated observations. Focusing on businesses who were gay-friendly in the pre-*Masterpiece* period circumvents the need to rely on data from the second wave of emails before *Masterpiece*, where significant attrition occurred. This is possible because all emails from same-sex couples were sent in the first wave of emails and therefore data on responsiveness to same-sex couples were not affected from the subsequent attrition.²⁹ Because assignment to wave in the post-period was randomized (unlike in the pre-

²⁹ Ideally, had attrition not occurred, data from all waves should have been used to simultaneously assess both the existence of discrimination pre-*Masterpiece* and the effect of *Masterpiece* on preexisting discrimination. But because the data from wave 2 suffers from significant attrition, it is not possible to evaluate pre-*Masterpiece* discrimination

period), there is no concern about attrition creating a confound if one conditions on the pre-period behavior and uses post-period outcomes to measure the effect of the decision.

Second, while the sample is smaller than the original sample (N= 906), it is still large enough to detect the effect in question.³⁰ The self-selection of businesses into this sample is not a concern, since these are exactly the businesses that require our focus. Businesses that discriminated against same-sex couples both before and after *Masterpiece* would not influence the results. In contrast, businesses that shifted from positive response to discrimination are precisely the object of the inquiry. Because businesses were randomized post-*Masterpiece* to receive an inquiry from a same-sex or an opposite-sex couple (and the vice versa in the following wave, such that each business was contacted by both couples post-*Masterpiece*), I can estimate the effect of *Masterpiece* precisely, using both within and between businesses data.

TABLE 5 - AVERAGE RESPONSE RATES OF LGBT-SERVING BUSINESSES AFTER *MASTERPIECE*,
BY SEXUAL ORIENTATION OF COUPLES (NUANCED RESPONSE)

Sample	Couple	Negative Responses		No Response	Positive Responses		Total PR	Percent Difference	Total RR	Obs.
		Neg.	Neg. w/ Referral		Coop.	Pos.		in PR		
All	Opposite-sex	2.78	2.08	19.62	2.26	73.26	75.52	9.20	80.38	576
Inquiries	Same-sex	4.17	1.74	27.78	1.74	64.58	66.32	(0.0006)	72.22	576
Wave 3	Opposite-sex	1.39	2.43	19.44	2.43	74.65	77.08	9.38	80.56	289
	Same-sex	3.47	1.74	26.74	2.08	65.63	67.71	(0.011)	73.26	287
Wave 4	Opposite-sex	4.17	1.74	19.79	2.08	71.88	73.96	9.03	80.21	287
	Same-sex	4.86	1.74	28.82	1.39	63.54	64.93	(0.018)	71.18	289

patterns. At the same time, it is possible to answer the second question—what the effect of *Masterpiece* is—by relying exclusively on the first wave of emails, as described in the text.

³⁰ As the effect size exceeded the conservative assumptions of the power analysis, it was detectable also in analyses that relied on smaller samples. This is also true for the robustness analysis reported below.

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Notes: The table reports the response rates, by type of response, for opposite-sex couples and same-sex couples, for businesses that prior to *Masterpiece* provided a positive response to same-sex couples ('gay-friendly'). Response types include: Negative response (column 3), negative response moderated by a referral of the couple to a different vendor (column 4), no response (column 5), cooperative response (column 6), and positive response (column 7). The table also reports total of positive responses per couple type (column 8), as well as the difference in positive responses between couple types (column 9). Column 9 also reports the *p*-value for a test of proportions testing the null hypothesis that the positive response rates are equal across sexual orientation groups. Finally, the table reports the total response rate, positive and negative, per couple type (column 10) and the total number of observations (column 11).

Table 5 tabulates average response rates by the sexual orientation of the couple. Row 1 and 2 present the results for all inquiries collected post-*Masterpiece* by sexual orientation. Inquiries from a same-sex couple have a 66.3 percent chance of receiving a positive response. Equivalent inquiries from an opposite-sex couple have a 75.5 percent chance of being answered positively. This represents a difference of 9.2 pp, or 14 percent, that can be solely attributed to the names manipulation. Column 9 shows that this difference is highly statistically significant.³¹

Rows 3-4 and 4-5 present the same results for each wave of inquiries sent post-*Masterpiece* separately. On the first week after *Masterpiece*, 67.7% of the businesses randomly contacted by same-sex couples responded positively, as compared with 77% who responded positively to opposite-sex couples. This represents a difference in positive response rate of 9.3 pp, or 14 percent. On the second week after *Masterpiece*, the randomization flipped such that each business received an email from the counter-orientation couple. Of the businesses now contacted by opposite-sex couples, ~74% responded favorably, as compared with ~65% who responded favorably to opposite-sex couples. This represents a difference in positive response rate of 9 pp, or 14 percent. These differences are significant in both waves.

Note that this pattern indicates both between-subject differences (in each week, between the

³¹ These statistical tests assume independence of responses. However, as tables OA4.1-3 in the Online Appendix show, the results remain significant when the analysis assumes that the responses are correlated at the business level.

random groups) and within-subject differences (across weeks, within each group). Weekly attrition cannot explain the within-subject pattern, as the rate of response went *up* in in the group that received the first message from a same-sex couple and the second message from an opposite-sex couple. In fact, as column 10, rows 3-5, show, opposite-sex couples experienced no reduction in positive responses from the first to the second wave. Only same-sex couples experienced such reduction. Yet, as Table OA4.1 in the Appendix shows, the effect of wave was not significant.

How do businesses communicate negative responses to couples? As Table 5, Column 5 indicates, the most common form of declining service is simply no response. This result is anticipated, as writing an negative response is both time costly and awkward, and the easiest way is to ignore the inquiry (Dovidio and Gaertner 2004; Bertrand and Mullainathan 2003). While it is possible that failures to respond result from not receiving an email, or forgetting to respond to it, we would expect such errors to distribute randomly, and therefore equally, across couple types. This is not the case. Opposite-sex couples had a 19.6 percent chance of not receiving a response to their inquiry. Same-sex couples had a 27.8 percent of not receiving a response. That is, the chance of same-sex couples to not receive a response was 42 percent higher ($Z=3.26$, $p=.001$).

The additional checks that are reported below and in the Online Appendix indicate that the negative effect of *Masterpiece* on the willingness to provide service to same-sex couples was robust across analyses. I find it in the entire sample of businesses in the experiment ($N=906$): comparing the rate of positive responses to same-sex couples before and after *Masterpiece* yields a drop of 14.4 pp, or about 23 percent. Observing only the responses after *Masterpiece* in the entire sample, the rate of positive responses for same-sex couples was lower than the respective rate for opposite-sex couples in 8.4 pp on average (Section OA4.2 in the Online Appendix); We find the

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same effect in the control group, where the gap between couple types was 9.5 pp (Section OA4.3 in the Online Appendix). We also find this effect among the particularly keen group of businesses that responded positively to both couples before *Masterpiece*. While these businesses remain more responsive than any other group of businesses, they too differentiate significantly between same-sex and opposite-sex couples after *Masterpiece* (~7 pp difference, Section OA4.4 in the Online Appendix). The summary of these results is presented in Figure 1, that shows that all business cohorts respond to *Masterpiece* with unfavorable treatment of same-sex couples, notwithstanding different baselines of positive response rates that characterize each cohort in separate.

In all of these analyses, I find that the *Masterpiece* effect is stable and robust to the inclusion of all experimental covariates, including the legal regime, the type of business, and the wave of inquiry. The effect is equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and does not vary with political conservativeness. However, Sub-Section D finds that the effect varies with the religiosity of the environment, such that businesses in areas dense with religious congregations, and particularly Evangelical congregations, are more likely to adopt their behavior post-*Masterpiece*.

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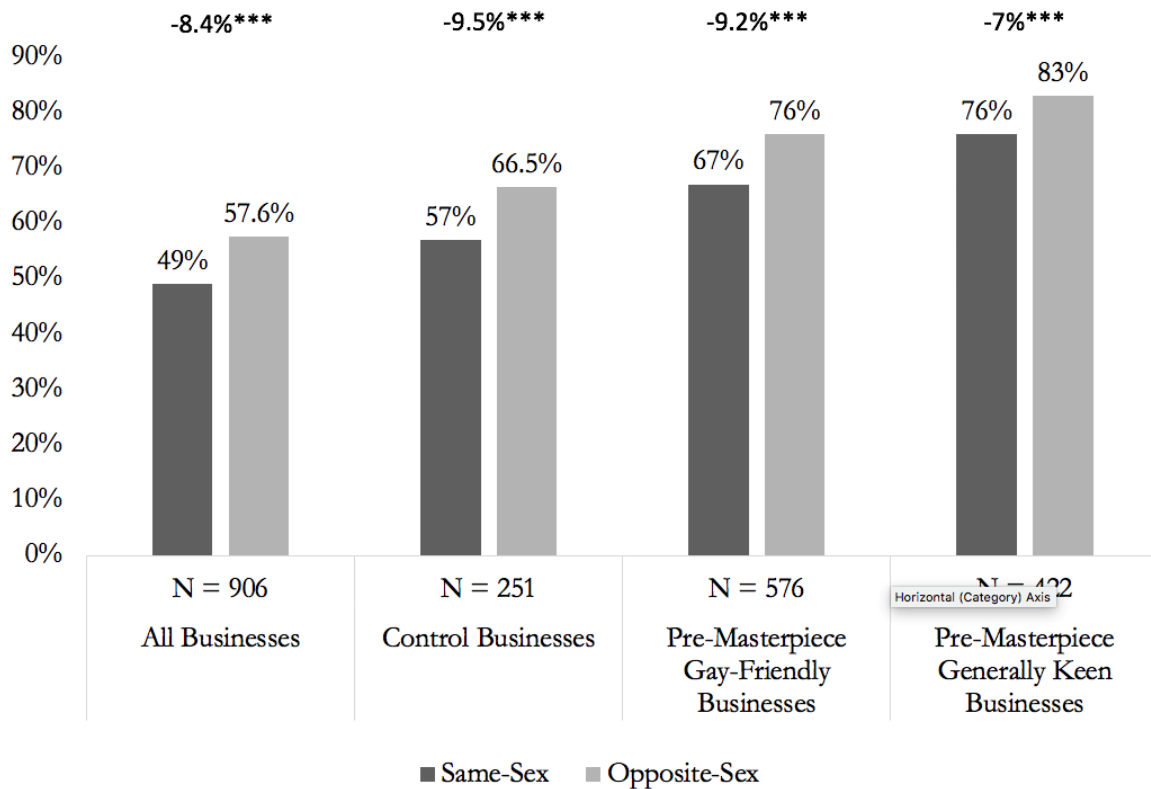


Figure 1. Agreement to serve same-sex and opposite-sex couples after *Masterpiece*

How substantial are these effects? Take the average 9% gap in the willingness to serve same-sex and opposite-sex couples that was documented in most analyses. Now consider the typical couple, that contracts with about 10 vendors in the process of organizing the wedding, including photographers, bakers, florists, videographers, venues, DJs, bridal/groom salons, calligraphers, jewelers, wedding planners, and more.³² A conservative estimate of the number of inquiries would be one per each business category, amounting to ten in total. A more liberal (some might say more representative) estimate assumes that each couple inquires with one or two potential vendors in each category, maybe more, amounting to at least 15-20 encounters. As each vendor-couple

³² Photographers were generally less responsive (to all couples) than other businesses, but the negative effect of sexual orientation was robust across business types.

interaction presents an independent risk of incurring discrimination,³³ the aggregate risk that same-sex couples would encounter discrimination at least once in their interactions post-*Masterpiece* is a function of the average risk posed by each vendor and the overall number of interactions. This risk ranges from 61% for ten interactions to 85% for twenty interactions,³⁴ and can go higher (or lower) the more (less) vendors a couple encounters.

B. Within-Business Transitions

Thus far we studied the effect of *Masterpiece* on businesses that provided favorable responses to same-sex couples prior to the ruling—that is, holding constant the pre-*Masterpiece* behavior, I asked how these businesses respond to random inquiries from same-sex and opposite-sex couples after *Masterpiece*. A complementary approach is to study within-business transitions throughout the duration of the experiment. As each business received four inquiries, two before and two after the decision, one from each couple type in each period, it is possible to study changes not only across businesses but also within businesses over time. While this analysis is compromised by the attrition that occurred in the second wave of inquiries before *Masterpiece*, zooming in on within business transitions alleviates this issue to some extent, as I will show below.

Table 6 tabulates the transitions from no/negative response pre-*Masterpiece* to positive

³³ Clearly, independent vendors in one area could be different than independent vendors in another area, as areas differ in their levels of discrimination. In that sense, the risk posed by each vendor is not entirely independent from the risks posed by neighboring vendors. The *Masterpiece* effect was robust to county-level conservativeness and city size but varied with county-level religious density. On some aspects, then, the assumption of independence holds on average, and on other aspects the risk may vary with the environment. In any event, cases of revealed non-independence were rare and were removed from the sample (Section OA2 in the Online Appendix).

³⁴ In probabilistic terms, the question is: what is the probability that at least one of the vendors will discriminate against the couple, given X vendors and that the average vendor poses a 9% discrimination risk? To answer the question, one needs to calculate the odds that *all* X vendors do *not* discriminate (91% per vendor) and subtract that from 1. $P(\text{at least one vendor discriminates}) = 1 - 0.91^X$. This probability is 0.61 for $X=10$ vendors, 0.76 for $X=15$ vendors, 0.85 for $X=20$ vendors, and so on.

response post-*Masterpiece*, and vice versa, for same-sex and opposite-sex couples. The table indicates that transitions occur in all directions, but overall, they are significantly biased in favor of opposite-sex couples and against same-sex couples.³⁵ Businesses were three times more likely to transition from agreeing to declining service to same-sex couples after *Masterpiece* than transitioning in the opposite direction (row 2, column 4). In contrast, businesses were 42 percent more likely to transition in favor of providing service to opposite-sex couples after *Masterpiece* (row 1, column 4). Studying the same results at the level of the couples indicates that opposite-sex couples were more than twice as likely than same-sex couples to experience a *positive transition*, such that a previously declining business would agree to serve them post-*Masterpiece* (Column 1). In contrast, 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* (Column 2). Notably, the estimates in first cell in Table 5 (row 1, column 1) should have probably been lower, because the general attrition that followed the pre-*Masterpiece* inquiry from opposite-sex couples might have yielded unintended declines of service – ultimately resulting in more positive transitions post-*Masterpiece*. The other cells, however, are not influenced from the attrition issue. In particular, the comparison of the transitions in column 2 and row 2, both showing that same-sex couples are very likely to experience a negative transition after *Masterpiece*, are unaffected.

TABLE 6 – WITHIN-BUSINESS TRANSITIONS BEFORE AND AFTER *MASTERPIECE*
BY SEXUAL ORIENTATION OF COUPLES

<u>Percent of Businesses Transitioning From:</u>		Percent Difference (p-value)	Ratio
<u>Declining to Agreeing</u>	<u>Agreeing to Declining</u>		

³⁵ The occurrence of transitions in all directions is not surprising in and of itself. For example, Bertrand and Mullainathan (2003) found that alongside equal treatment employers (in that study, 88% of all employers) and white-favoring employers (8.4%), there 3.5% of the employers favored black applicants. Transitions in all directions are also expected because of communication failures. Note, however, that omissions to respond were not randomly distributed across couple types, as the p-values of the comparisons in Table 5 indicate.

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Opposite-Sex Couples	15.3% [139]	10.8% [98]	4.5% (0.004)	1.42%
Same-Sex Couples	7.1% [64]	21.4% [194]	-14.3% (0.0000)	0.33%
Percent Difference (p-value)	8.2% (0.0000)	-10.6% (0.0000)		
OS/SS Ratio	2.16%	0.5%		

Notes: The table reports the transition rates within-businesses from declining service (negative or no response) pre-*Masterpiece* to agreeing to provide service (positive response) post-*Masterpiece* (column 1), and vice versa (column 2), for opposite-sex couples (row 1) and same-sex couples (row 2). In brackets in each cell is the number of businesses in that cell. Column 3 reports the percent difference between columns 1 and 2, as well as the p-value of the difference, and Column 4 reports the ratio between Columns 1 and 2 (for each couple type, the ratio of positive/negative transitions). Row 3 reports the percent difference and p-value of the difference for rows 1 and 2, and Row 4 reports the ratio between Rows 1 and 2 (for each transition type, the ratio of opposite-sex vs. same-sex couples experiencing the transition).

These findings complement the main analysis by indicating that the reduction in willingness to provide services to same-sex couples that is evident across businesses post-*Masterpiece* is also evident within-businesses post-*Masterpiece*. This reduction is not offset by businesses that prior to the decision did not serve same-sex couples and transitioned to serving them after the decision (this is also evident from the analysis of the full dataset in Section OA4.2 of the Online Appendix). While transitions from a positive to a negative response occur in all directions, same-sex couples encounter the largest percent of negative directions and the smallest percent of positive transitions.

C. Does the Masterpiece Effect Vary Between Legal Regimes?

The results so far demonstrate a substantial reduction in businesses' willingness to provide service to same-sex couples, as compared with opposite-sex couples, after the *Masterpiece* decision. Next, we would like to learn more about the factors that may influence this gap. More specifically, this Section asks how this effect displays in different socio-legal regimes. Because of space limitations, the results are summarized in Figure 2. Briefly, I find that *Masterpiece* had a highly statistically significant negative effect in all regimes, *except* for regimes that enacted both

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an antidiscrimination law and a religious freedom law. The full analysis is reported in Section OA4.5 of the Online Appendix. I return to these results in the discussion.

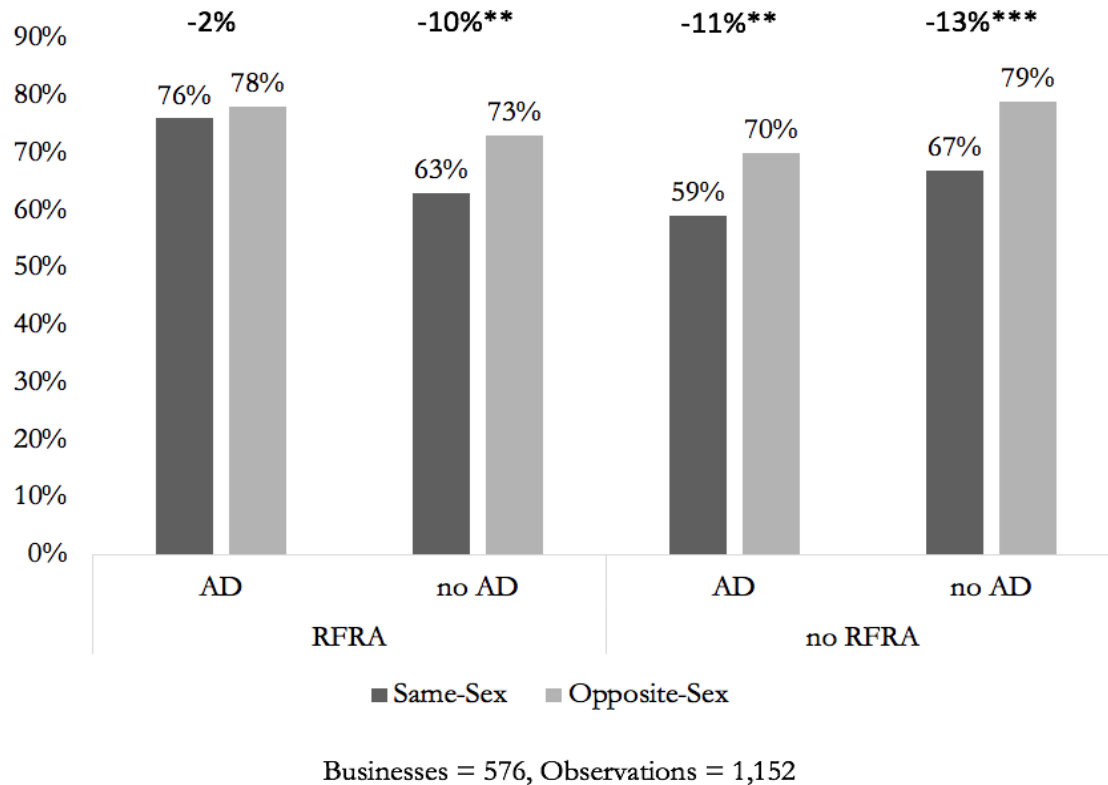


Figure 2. The effect of Masterpiece on previously gay-friendly businesses, by legal regime

D. Is the Masterpiece Effect More Pronounced in Religious Environments?

Finally, what role does religion play in business behavior? Given that the decision involves a religious exemption and received considerable attention in religious media, one may expect that businesses operating in more religious environments will be more sensitive to *Masterpiece*, and as a result, the effect will be more pronounced in these environments. This hypothesis is particularly plausible with respect to Evangelical areas, as Evangelicals have been involved in a large number of wedding conflicts and are the denomination with the lowest rates of support in same-sex

marriage (Pew Research Center 2014). Although individual-level evidence on the religiosity of the businesses is unavailable in this study, I can examine the impact of religious environment by observing the density of Evangelical congregations in the county where businesses are located.

I explored this hypothesis using public data on county-level density of religious and particularly Evangelical congregations from the U.S. Religion Census: Religious Congregations and Membership Study, 2010 (County File), available in the Association of Religion Data Archives (ARDA) (Grammich et. al 2010). The data include county-level counts of the number of congregations and adherents for 236 religious groups, as well as county population. As the number of adherents appeared less reliable than the number of congregations,³⁶ I computed the congregations/population ratio (religious density), for all religious congregations and for Evangelical congregations in particular. I then examine whether religious environment, as proxied by these measures, influences previously gay-friendly businesses after *Masterpiece*.

³⁶ As Grammich et. al (2010) explain, only about 36% of the 236 groups reported data on their adherents and in 31 counties the number of reported adherents exceeded the total population in 2010. While groups can err in counting heads or overstate their actual membership, these biases are less likely in counting congregations and are more likely to be detected by ARDA data checkers.

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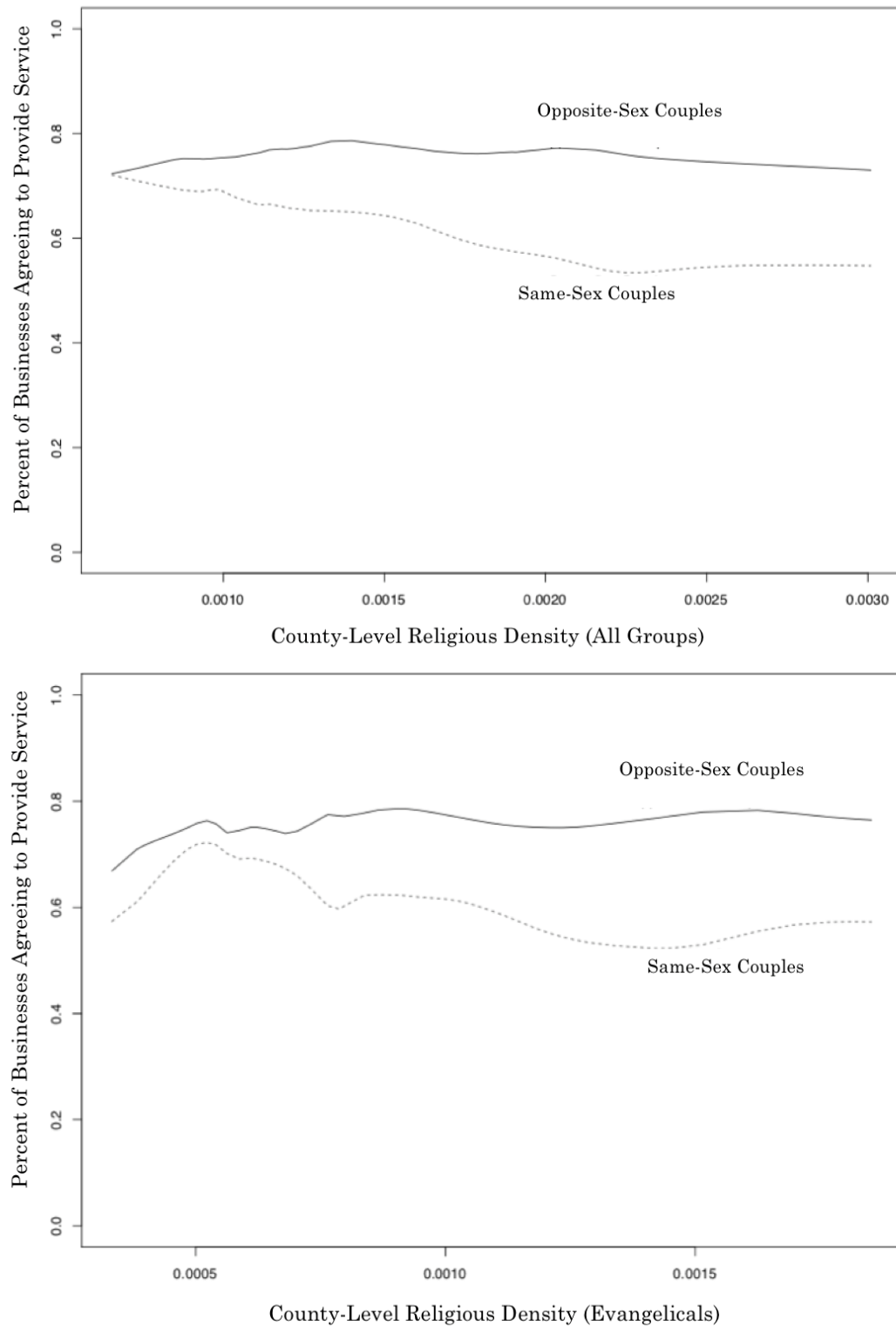


Figure 3. The effect of Masterpiece on previously gay-friendly businesses, by religious environment (top panel - all denominations; bottom panel – Evangelicals).

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Figure 3 plot the results. The top panel shows that the gap in the agreement to serve same-sex and opposite-sex couples varies with the religiosity of the environment of the businesses. All of the businesses in this analysis agreed to serve same-sex couples before *Masterpiece*. After *Masterpiece*, however, businesses in religiously dense areas show a large gap between same- and opposite-sex couples. In contrast, businesses in areas with few congregations do not distinguish between same-sex and opposite-sex couples. Plotting the results against the density of Evangelical congregations provides very similar results, as the bottom panel of Figure 3 shows. The data for areas with very few congregations is somewhat noisy (only 32 businesses are located in counties where Evangelical density is 0.0004 or below), yet the general trend is the same: the sexual orientation gap widens with Evangelical density. Notably, the percent of businesses agreeing to provide service to opposite-sex couples is fairly stable across high- and low-religious/Evangelical density areas. The fluctuation occurs mostly with respect to same-sex couples.

Section OA4.6 in the Online Appendix includes the results of the regression analyses that account for religious and Evangelical density.³⁷ Both are statistically significant, and with the interaction term between sexual orientation and religious/Evangelical density in the model, the effect of sexual orientation is no longer significant. In other words, the negative effect of *Masterpiece* on the agreement to provide service to same-sex couples is significantly concentrated in more religious environments. To get a concrete appreciation of the magnitude of this result, I compared businesses in high vs. low Evangelical density environments (top 25% (N=141) v.

³⁷ One may ask how religious environment influenced behavior before *Masterpiece*. It is not possible to examine this question in full because of the Wave 2 attrition, that affected businesses across different levels of Evangelical density. However, a comparison of each pre-*Masterpiece* wave in separate shows that businesses in highly- and slightly-Evangelical environments did not significantly differ in their agreement to serve opposite-sex ($p=.31$) and same-sex couples ($p=.71$) before *Masterpiece*.

bottom 25% (N=109)). In highly Evangelical environments, previously gay-friendly businesses develop a whopping 20.5 pp gap between couples (78 pp vs. 57.5 pp, $Z=3.69$, $p = .0002$), whereas in slightly Evangelical environments, the gap is 2.7 pp (70.6 pp vs. 67.9 pp, n.s.). Observing high vs. low general religious density areas yields the same results.³⁸ These results indicate that businesses in more religious areas updated their behavior after *Masterpiece* significantly more than businesses in less religious areas.

5. CONCLUSIONS AND IMPLICATIONS

Combining methods from pseudo-experiments and field-experiments, this study finds a robust reduction in the willingness to serve same-sex couples after the Supreme Court *Masterpiece* decision, as compared with opposite-sex couples. The negative effect of *Masterpiece* on same-sex couples is evident in the population of businesses that prior to *Masterpiece* were willing to provide service to same-sex couples, as well as in the entire sample of businesses drawn from four different legal regimes in different US States. We see the causal effect of *Masterpiece* both within businesses, over time, and between businesses randomly contacted by same-sex or opposite-sex couples after the decision was rendered. The negative effect of *Masterpiece* is not an artifact of the experiment, as it is identically found in the control group.

What explains the effect of the *Masterpiece* decision on wedding vendors? The first aspect of this question relates to the *cognitive mechanisms* that translated *Masterpiece* to behavioral change. First, *Masterpiece* may have been interpreted as relieving a previously-anticipated penalty for discrimination or reducing the likelihood of enforcement (Becker 1968). Yet this explanation

³⁸ A 17.2 pp vs. 3.6 pp gap, respectively (High-density N=144; Low-density N=110; $Z=3.133$, $p = .0017$).

appears less plausible given the design of the experiment and the observed data. The experiment eliminated the risk of getting caught by design, as emails allowed vendors –both before and after *Masterpiece* – to entirely avoid detection, by simply ignoring the sender. In addition, vendors changed their behavior even in regimes that do not prohibit sexual orientation discrimination (Figure 2). A more likely possibility is an expressive effect (McAdams 2000; Sunstein 1996), i.e. that the decision led vendors' to perceive the social norm as more permissive of service refusal to same-sex couples. This explanation is supported by Tankard and Paluck (2017) and Kazyak and Stange (2018) who found that the *Obergefell* decision shaped individuals' perceptions of the social norm regarding same-sex marriage. Whereas the *Obergefell* court emphasized marriage equality, the *Masterpiece* court stressed the need to tolerate religious objection to marriage equality. The decisions may have had similar effects on social norm perceptions, only in opposite directions.

The second explanation for the effect could be the *social environment*, and particularly its religiosity, as businesses in religiously-dense areas updated their behavior substantially more than businesses in less religious areas. This may be explained by a greater concentration of religious vendors in these areas, or greater diffusion of conservative frames of the decision that may have influenced also non-religious vendors. The precise mechanism by which religion translated *Masterpiece* to negative consequences should be examined in future studies.

The findings from the *Masterpiece* experiment have several important theoretical implications. First, the experiment indicates that the Court can extend its influence beyond shaping public attitudes to shaping behavior itself. Second, the novel examination of the interaction between national and subnational legal structures highlights that the Court's effects can vary between legal regimes, sometimes in unexpected ways. In the present setting, *Masterpiece* had an effect in

regimes that were not supposed to be influenced from the decision (no AD regimes) and had no effect in regimes where change were expected (AD+RFRA regimes). These results underscore the need to account for subnational variation in future studies. Third, the study sheds new light on the argument that courts are ineffective at spurring social change (Rosenberg 2008), by highlighting the risk that the court will be effective, paradoxically, in spurring *inadvertent* social change. Rosenberg argued that courts are incapable of shaping public opinion but they may produce backlash by mobilizing opponents to block further rulings or reverse existing ones by means of legislation (Rosenberg 2008, pp. 362-370; 418-19). This study adds another risk to this list: that unsuccessful litigation will produce the opposite social consequences, independent from any political mobilization, by influencing market players. The *Masterpiece* Court clearly worried that its decision might increase discrimination against same-sex couples, a concern which may have contributed to the narrow ruling. Were the consequences different had the Court provided a bright line rule rather than “something for everyone”? This question could be explored in future studies.

Last but not least, the present study brings crucial and heretofore unobserved data to bear on a central normative question in constitutional law, one that is essential to the resolution of conflicts between equality and freedom. First, the results discredit the argument that religious exemptions will not expand discrimination. Instead, what the *Masterpiece* experiment shows is that even a narrow exemption can have a significant and robust impact on a market and its customers. These findings advance the debate on the impact of religious exemptions: now that data are available, more nuanced analyses can be performed: We can estimate the actual, aggregate risk of discrimination (which, as I showed above, is likely to be very high); and we can direct research efforts to the factors that are found to aggravate or attenuate the risk (including legal regime and

religious environment), and divest efforts from arguments that have been contradicted in fact (including that discrimination is not an issue in urban or progressive areas). These findings provide concrete guidance for future research.

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Exhibit 4

The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes



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Abstract

We propose that institutions such as the U.S. Supreme Court can lead individuals to update their perceptions of social norms, in contrast to the mixed evidence on whether institutions shape individuals' personal opinions. We studied reactions to the June 2015 U.S. Supreme Court ruling in favor of same-sex marriage. In a controlled experimental setting, we found that a favorable ruling, when presented as likely, shifted perceived norms and personal attitudes toward increased support for gay marriage and gay people. Next, a five-wave longitudinal time-series study using a sample of 1,063 people found an increase in perceived social norms supporting gay marriage after the ruling but no change in personal attitudes. This pattern was replicated in a separate between-subjects data set. These findings provide the first experimental evidence that an institutional decision can change perceptions of social norms, which have been shown to guide behavior, even when individual opinions are unchanged.

Keywords

social influences, social perception, prejudice, attitudes, intergroup dynamics, open data, open materials, preregistered

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After the Supreme Court legalized interracial marriage in the 1967 decision *Loving v. Virginia*, Americans' support for interracial marriage climbed from under 25% to its present-day levels of over 80% (Newport, 2013). Analysts suggested that the Court's decision helped fuel the shift in public opinion (Marshall, 1987; Schacter, 2009). Whether and when institutional decisions actually change individual attitudes are questions that social scientists have long pursued. Studies have suggested that under some conditions, institutions are responsible for changing personal attitudes about social or political issues (e.g., Bartels & Mutz, 2009; Beaman, Chattopadhyay, Duflo, Pande, & Topalova, 2012; Bishin, Hayes, Incantalupo, & Smith, 2016; Clawson, Kegler, & Waltenburg, 2001; Hoekstra, 1995; Mondak, 1992). However, attitudes do not always change easily, particularly when they concern contentious issues. Personal experiences and religious and political views often anchor individuals' attitudes, which makes them less flexible (e.g., Johnson

& Eagly, 1989; Jost, Federico, & Napier, 2009; Prentice, Gerrig, & Bailis, 1997; Sherif & Hovland, 1961).

Questions about the social influence of institutional decisions are nearly always posed one way—does the decision change an individual's personal attitude toward the issue? We hypothesized that institutional decisions may change a different, also consequential viewpoint: an individual's perception of social norms. Norm perceptions are impressions of what opinions or behaviors are common among, or considered desirable by, a group of people (Cialdini & Goldstein, 2004). The perception that “most Americans support interracial marriage” is an example of a perceived social norm (contrasted with “I

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support interracial marriage,” which is a statement of an individual attitude). Extensive research has demonstrated that norm perceptions are consequential because they can guide important individual behaviors, such as energy conservation, voting, and alcohol consumption (Tankard & Paluck, 2016). Individuals use norms as a guide to behavior because they are motivated to be accurate in their social judgment and also because they wish to avoid social rejection (Cialdini & Goldstein, 2004). Thus, social norms may not always align with personal attitudes—individuals may perceive shifts in collective opinion without changing their own mind (Paluck, 2009; Sherif, 1936). In some cases, perceptions of norms motivate changes in attitudes (Stangor, Sechrist, & Jost, 2001). But despite their relevance for understanding social and behavioral change, perceived norms are rarely measured in public opinion polls.

We predicted that institutional decisions could alter perceptions of social norms because these perceptions are known to be dynamically updated over time as individuals take cues from their environment, such as the public behavior of group members or summary information about a group (Miller & Prentice, 1996; Paluck & Shepherd, 2012). We theorized that institutions (defined as entities that “govern, educate, or organize a reference group and their social interactions”; Tankard & Paluck, 2016, p. 192) are an additional cue regarding the content and direction of social norms.

First, because they represent collectives, institutions may be able to change how individuals perceive the group—for example, perceptions of what American people believe now (Tankard & Paluck, 2016). These are perceived norms of the status quo. Although an institution such as the Supreme Court is intended to be insulated from public opinion pressures, individuals may believe that it considers public opinion purposefully, to maintain support, or incidentally, because individual judges are subject to the same social forces as the public (e.g., Friedman, 2009; Mishler & Sheehan, 1996; Rosenberg, 1991). For either reason, if individuals believe that an institution has access to accurate information about public opinion, or if an institution is democratic in nature, they may view an institutional decision as a signal of where the public stands and update their own subjective perception of public opinion.

Likewise, institutional decisions may be understood as an expert projection of the direction in which public opinion or behavior will move (or should move) in the future. At least for institutions that are trusted to determine appropriate behavior and represent a group’s best interests (i.e., “legitimate” institutions; Tyler & Jackson, 2014), we hypothesized that decisions may signal where collective opinion is headed. These perceptions are a type of directional perceived norm, that is, what the collective will be thinking or doing in the future.

Additional factors undoubtedly contribute to institutional influence on perceived social norms. For example, when an institution is highly visible, individuals are aware that many other group members are simultaneously observing its decisions. This awareness of shared attention, reinforced by mass media, may contribute to changes in perceptions of what the group currently believes or will believe (Chwe, 2003; Shteynberg, Bramlett, Fles, & Cameron, 2016). Our goal at present was not to disentangle various explanations for the influence of institutional decisions on social norms, but rather to provide the first experimental test of whether a causal relationship exists between major institutional decisions and perceived social norms.

As we have noted, personal attitudes also respond to institutional decisions under certain conditions. First, institutions seem to influence personal attitudes through the persuasiveness of the reasons behind their decisions and by engaging individuals in deeper thinking—not, by contrast, through a signaling process in which an institutional endorsement is accepted unthinkingly (Bartels & Mutz, 2009). Thus, for issues such as gay marriage on which attitudes are guided less by deliberative reasoning and more by moral instinct (Powell, Quadlin, & Pizmony-Levy, 2015), institutions are less likely to have strong effects on personal attitudes. Second, studies show that attitudes are influenced when the institutional decision is experienced personally by an individual—for example, if the decision affects local community conditions (Hoekstra, 2000). In India, for example, institutional quotas that increased citizens’ exposure to local female politicians changed attitudes toward female leadership (Beaman et al., 2012; see also Flores & Barclay, 2016; Kreitzer, Hamilton, & Tolbert, 2014).

Given that personal attitudes are more strongly tied to ideology and personal experience than to social norm perceptions (e.g., Johnson & Eagly, 1989; Jost et al., 2009; Prentice et al., 1997; Sherif & Hovland, 1961), some of the conditions that limit institutional influence over attitudes may not apply to institutional influence over perceived norms. Thus, in the studies reported here, we sought to identify whether an institutional decision could influence social norm perceptions, even under conditions in which one might not expect attitude change.

Understanding the causal effects of an actual institutional decision is difficult without the ability to randomize exposure to the institutional decision (see Beaman et al., 2012) or without prospective time-series data testing how individuals change prior to and following an institutional decision (Bishin et al., 2016). The present research capitalized on the respective strengths of experimental and time-series strategies, presenting evidence for the causal influence of the U.S. Supreme Court’s 2015 *Obergefell v. Hodges* ruling on

gay marriage. Each study examined whether the Court's decision to allow same-sex couples to marry and to recognize same-sex unions changed individuals' perceptions of American social norms regarding gay marriage. We tested our prediction that this institutional decision would lead Americans who are not lesbian, gay, bisexual, transgender, queer, intersex, or asexual (LGBTQIA) to perceive stronger and increasing public support for gay marriage (present and future social norms), even if personal attitudes toward gay marriage and gay people were left unchanged. We further explored whether behavioral support for gay marriage increased in the same participants following the Supreme Court decision.

Study 1a

Method

Before investigating the relationship between the Supreme Court ruling and perceived social norms and attitudes in the real world, we tested these relationships in a controlled experimental setting. Conducted prior to the ruling, Study 1a isolated the effects of learning about the institutional decision from the effects of other influences, such as news coverage of the ruling and individuals' observation of peer reactions to the ruling. Specifically, we experimentally manipulated participants' perceptions of the likelihood that the Supreme Court would rule in favor of same-sex marriage.

Participants. Participants (52% female, 48% male; mean age = 34.01 years) were recruited online via Amazon Mechanical Turk. The final sample size was 1,673 after the exclusion of 507 participants for failing attention checks and 457 participants for not meeting eligibility requirements. Participants were retained in the sample if they were U.S. citizens, if they did not indicate an LGBTQIA identity, and if they passed a series of attention checks. Including participants who did not pass attention checks does not change any of the study's results. A power analysis indicated that we would need to recruit a sample of approximately 2,600 participants to have 90% power to detect the hypothesized effect, assuming an approximate effect size (Cohen's *d*) of 0.15 and anticipating a need to exclude 29% of the Mechanical Turk respondents, on the basis of a pilot study (see the Supplemental Material available online).

Procedure. Participants were invited to read a brief article about the likely outcome of the upcoming Supreme Court ruling on gay marriage, according to the ostensible in-depth analysis of a panel of Supreme Court scholars. To encourage careful reading, we displayed the text of the

analysis sequentially across a series of pages. Each participant was randomly assigned to read either a version entitled "Supreme Court likely to rule in favor of gay marriage" (*positive-ruling condition*) or a version entitled "Supreme Court unlikely to rule in favor of gay marriage" (*negative-ruling condition*). The following is a sample of how the text was manipulated across these two versions:

A panel of Supreme Court scholars recently conducted in-depth analysis of this issue and determined that it is extremely [likely/unlikely] that the Court will rule in favor of fully legalizing gay marriage. "There's a lot to consider, but based on our extensive analysis, we are confident that gay marriage [will be legalized nationwide by the Supreme Court very soon/will not be legalized nationwide by the Supreme Court anytime soon]," reported a member of the panel.

We then asked participants to imagine the described Supreme Court ruling actually coming to pass (as they might observe it on the news), adapting Carroll's (1978) instructions for imagining a political outcome. Participants were compensated 30¢ for participating.

Finally, as a manipulation check, we asked participants to indicate their "prediction regarding whether the U.S. government will legalize gay marriage at the state level" using a scale from -4 (*certain not to become legal*) to 4 (*certain to become legal*).

Survey items. Participants responded to a series of dependent measures. We concluded with questions about demographics (age, gender, race) and other relevant social views and behaviors, such as engagement in political discussion (see the Supplemental Material for more information).

Perceived status quo norms. Perceptions of status quo norms in support of gay marriage were measured using the mean score of two items ("To what extent do you think Americans oppose or support gay marriage?" and "To what extent do you think Americans oppose or support making gay marriage legal in the United States?"), to which participants responded using a scale from -4 (*strongly oppose*) to 4 (*strongly support*), $\alpha = .90$.

Perceived directional norms. Perceptions of the direction and speed of social-norm change in support of gay marriage were measured using the mean score of two items ($\alpha = .78$). The first was "To what extent do you think that support for gay marriage will or will not increase in the United States in the future?" (answered on a scale from 1, *will not increase at all*, to 9, *will increase greatly*). The second was "How much momentum, if any,

do you think the movement to make gay marriage legal currently has in the United States?” to which participants responded using a scale from 1 (*no momentum at all*) to 9 (*strong momentum*).

Personal attitudes toward gay marriage. Attitudes in support of gay marriage were measured using the mean score of two items (“To what extent do you oppose or support gay marriage?” and “To what extent do you oppose or support making gay marriage legal in the United States?”), to which participants responded using a scale from -4 (*strongly oppose*) to 4 (*strongly support*), $\alpha = .98$.

Rating of gay people on feeling thermometer. Ratings of people who are gay were measured using a single-item feeling thermometer (University of Michigan, 1999; “How warm or cold do you feel toward people who are gay? Please indicate your response by sliding the bar below and leaving it at a particular ‘temperature’”), to which participants responded using a scale from 0 (*cold, unfavorable*) to 100 (*warm, favorable*).

Behavior. As a behavioral measure, we asked participants whether they were interested in being mailed a free sticker to show support for an issue of their choice and told them that a link would be provided to an external Web site containing more information at the end of the study. They indicated whether they did or did not want a sticker, and if they did, whether they wanted one in support of environmental conservation, gay marriage, or a different issue of their choice. We measured the rate of selecting gay marriage as opposed to a different sticker or no sticker.

Trust in and perceived representativeness of the Supreme Court. Respondents’ reported trust in the Supreme Court and belief that the Court represents public opinion was measured using the mean score of two items ($\alpha = .80$). The first was “To what extent do you mistrust or trust the U.S. Supreme Court as an institution?” (answered on a scale from -4 , *strongly mistrust*, to 4, *strongly trust*). The second was “To what extent do you think decisions made by the U.S. Supreme Court do or do not represent public opinion in the United States?” to which participants responded using a scale from -4 (*do not represent at all*) to 4 (*strongly represent*).

Political orientation. Political orientation was measured using a single item (“Where do your political views best fall on the following scale?”), to which participants responded using a scale from 1 (*very liberal*) to 7 (*very conservative*).

Results

Our manipulation check showed that predicted certainty about “whether the U.S. government will legalize

gay marriage at the state level” was greater among participants in the positive-ruling condition ($M = 2.49$, 95% confidence interval, $CI = [2.38, 2.60]$) than among participants in the negative-ruling condition ($M = 1.03$, 95% $CI = [0.87, 1.18]$), $t(1671) = 15.44$, $p < .001$.

As we predicted, participants in the positive-ruling condition perceived the status quo norms regarding Americans’ current support for gay marriage to be significantly higher ($M = 0.96$, 95% $CI = [0.84, 1.07]$) than did participants in the negative-ruling condition ($M = 0.71$, 95% $CI = [0.60, 0.83]$), $t(1671) = 2.95$, $p = .003$, $d = 0.14$ (see Fig. 1). Participants in the positive-ruling condition also projected significantly greater increases in American’s support for gay marriage (perceived directional norms; $M = 7.16$, 95% $CI = [7.06, 7.26]$), compared with participants in the negative-ruling condition ($M = 6.78$, 95% $CI = [6.68, 6.88]$), $t(1671) = 5.23$, $p < .001$, $d = 0.26$ (see Fig. 1).

Less expected, given our prediction that attitudes were less likely to change, we observed that attitudes in support of gay marriage were significantly more positive among participants in the positive-ruling condition ($M = 2.21$, 95% $CI = [2.03, 2.39]$) than among participants in the negative-ruling condition ($M = 1.78$, 95% $CI = [1.59, 1.97]$), $t(1671) = 3.28$, $p = .001$, $d = 0.16$ (Fig. 1). The same pattern was observed for ratings of gay people on a feeling thermometer (positive-ruling condition: $M = 78.64$, 95% $CI = [77.02, 80.26]$; negative-ruling condition: $M = 75.78$, 95% $CI = [74.07, 77.48]$), $t(1671) = 2.39$, $p = .017$, $d = 0.12$ (Fig. 1).

The behavior we measured (selecting a free pro-gay-marriage sticker as opposed to other free issue stickers or no sticker) differed between conditions in the predicted direction but not significantly, an effect that we hypothesized to be less likely than social norm change. Only 17.9% of our sample selected any sticker at all, and 7.9% (66 out of 837) of all participants in the negative-ruling condition selected a pro-gay-marriage sticker, compared with 10.3% (86 out of 836) in the positive-ruling condition, $b = 0.29$, 95% $CI = [-0.40, 0.63]$, $p = .088$. Participants in the positive-ruling condition (20.3%) were more likely to take any sticker, regardless of sticker content, compared with participants in the negative-ruling condition (15.5%), $b = 0.33$, 95% $CI = [0.08, 0.58]$, $p = .011$. Keeping in mind that the behavioral difference was not robust, we found that perceived directional norms correlated with the experimental difference in behavior, which suggests that they play a role in behavioral response (see Section G in the Supplemental Material).

In contrast to our expectations, our findings were not moderated by participants’ political orientation. One exception was that the increase in positive feelings toward gay people was stronger among more conservative participants (see Table S13 in the Supplemental

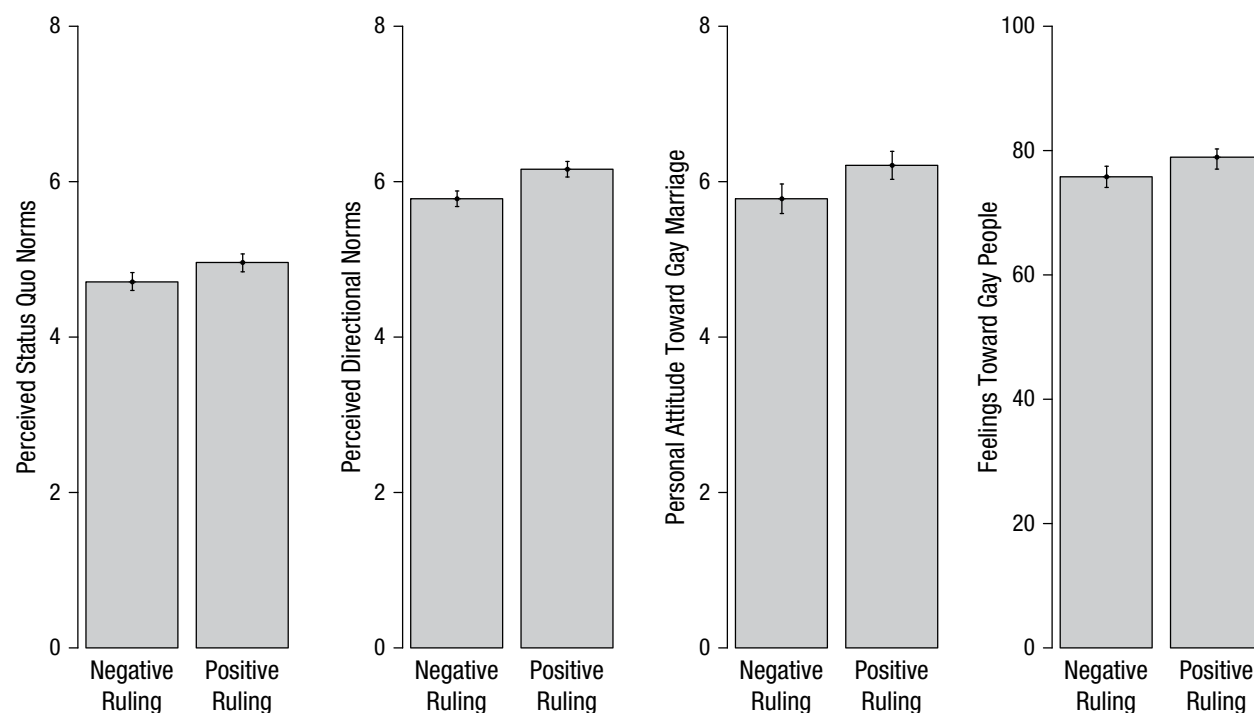


Fig. 1. Results from Study 1a: mean ratings of perceptions of status quo norms, perceptions of the direction and speed of social change in support of gay marriage, personal attitudes toward gay marriage, and feelings toward gay people. Ratings are shown as a function of Supreme Court–ruling condition. The first three measures were rescaled for ease of comparison. Error bars show 95% confidence intervals.

Material). We found evidence of a stronger effect of the anticipated Supreme Court decision among participants who reported more trust in the Court and belief in its representativeness of public opinion in the United States (Table S13). A limitation is that perceptions of the Supreme Court were measured at the end of the study and were affected by the manipulation (see Table S4 in the Supplemental Material). The study manipulated perceptions of the Supreme Court decision without the accompanying real-world influences of media and peer reactions. Participants in both conditions imagined a variety of reactions, and imagined media reactions did not account for the effect of the manipulation on norms or attitudes (see Sections F and H in the Supplemental Material). Thus, our small but statistically significant effects of a positive Supreme Court ruling for legalized gay marriage on social norms perception support the hypothesis that perceived social norms can change in response to the likelihood of the institutional decision itself.

The fact that participants imagined negative and mixed reactions to both positive and negative rulings also suggests that they were not trying to please the experimenters, although this possibility cannot be ruled out. However well-controlled, these types of contrived

survey experiments may induce participants to personally engage with and understand an institutional decision to an extent that they otherwise might not (Egan & Citrin, 2011; Unger, 2008) and can serve only as an approximation of a real-world event. For this reason, we conducted a prospective longitudinal study of the actual Supreme Court decision on gay marriage, to capture any changes in attitudes and perceived norms.

Study 1b

Method

In a five-wave time-series study that followed the same individuals prior to and following the June 2015 U.S. Supreme Court ruling on same-sex marriage, we tested whether the ruling corresponded with a positive shift in perceived social norms and attitudes in support of gay marriage and of gay people. We also collected a separate between-subjects replication data set.

Participants. Participants (43.3% female, 56.7% male; mean age = 33.93 years) were recruited online via Amazon Mechanical Turk for the within-subjects longitudinal component of the study. The final sample consisted of

Table 1. Dates of Longitudinal Survey Waves Surrounding the Announcement of the Supreme Court Ruling on Gay Marriage (Study 1b)

Wave	Launch date	End date
Before announcement of Supreme Court ruling		
1	March 10, 2015	May 28, 2015
2	June 10, 2015	June 14, 2015
3	June 22, 2015	June 24, 2015
After announcement of Supreme Court ruling		
4	June 27, 2015	July 3, 2015
5	July 18, 2015	July 23, 2015

Note: The Supreme Court ruling was announced at 10:00 a.m. on June 26, 2015.

1,063 participants after the exclusion of 269 participants who failed attention checks. As in Study 1a, participants were retained in the sample if they were U.S. citizens, did not indicate an LGBTQIA identity, and passed a series of attention checks. Including participants who did not pass attention checks does not change any of the study's results. The recruited sample size was determined by the power analysis in Study 1a and through additional consideration of the repeated measures design and of attrition rates in a study with similar follow-up methods also conducted on Mechanical Turk (Christenson & Glick, 2013). On average, these participants 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. For the between-subjects time-series data collection, an additional 545 adult participants (45.5% female, 54.5% male; mean age = 32.36 years) were recruited in total across Waves 2 through 5 of data collection (an additional 131 participants were excluded for failing attention checks). This resulted in a combined sample of 1,608 participants in the between-subjects data set.

Within-subjects time-series procedure. Participants for the within-subjects portion of the study were recruited to complete an initial survey (Wave 1) on Amazon Mechanical Turk via an invitation that was posted on the Web site each Wednesday from March 10 to May 28, 2015. The timing of this initial recruitment phase and all subsequent measurement waves was based on the expectation that the Supreme Court ruling would be issued in late June. All participants who were retained in the Wave 1 survey were subsequently invited by e-mail to participate in the two other surveys prior to the ruling (Waves 2 and 3) and the two surveys following the June 26 ruling (Waves 4 and 5; see Table 1 for dates). To maximize participant retention in the two postruling data collections, we sent two identical invitation e-mails on separate days for Waves 4 and 5; otherwise, each invitation to participate in a follow-up wave

was identical (a recruitment strategy adapted from Christenson & Glick, 2013; see Section H in the Supplemental Material). Participants were compensated 30¢ at Wave 1 and 50¢ at each follow-up wave, with a \$1 bonus for completing all waves.

Our rate of retention across Waves 2 through 5 was on average 54.4% of all respondents, a rate comparable with that of other longitudinal time-series studies conducted on Amazon Mechanical Turk (Christenson & Glick, 2013). In Waves 2 through 5, responses were retained if the participant passed that wave's respective single attention-check question. In Wave 5, about 12.3% of the sample guessed the general purpose of the study as it related to the Supreme Court ruling. Their responses are included in all our reported findings; excluding their responses does not affect our results.

Between-subjects time-series procedure. After the conclusion of Wave 1 of the longitudinal component on May 28, 2015, we issued an invitation each Wednesday for a new set of participants to complete the survey, recruiting new respondents weekly through July 22, 2015. The survey was identical to the Wave 1 survey given to the within-subjects sample. This additional sample, combined with respondents from Wave 1 of the within-subjects portion of the study, formed a time-series data set in which new participants were surveyed each week from March 10 through July 22, 2015. We collected this cross-sectional data set to eliminate suspicions about the research hypotheses within one set of respondents and to test whether the effects would be replicated in a repeated panel sample.

Survey items. Participants responded to the same dependent measures as in Study 1a. In the within-subjects data set, the behavioral measure was not repeated in follow-up waves.

Results

Across several specifications that accounted for survey attrition in different ways, we found robust evidence that the Supreme Court ruling was associated with a significant shift in perceived (present and future) social norms in support of gay marriage. We did not find evidence of change in personal attitudes toward gay marriage or in ratings of gay people. This pattern emerged when comparing the survey waves measured immediately before and after the ruling, when implementing a regression-discontinuity design to account for trends over time, when incorporating inverse-probability weighting to address attrition, and when analyzing the separate cross-sectional sample.

First, using paired-samples *t* tests to compare the responses of all participants who completed both the survey wave conducted several days prior to the ruling

(Wave 3) and the wave launched the day after the ruling (Wave 4), we found that perceptions of Americans' current support for gay marriage (perceived status quo norms) were significantly greater in Wave 4 ($M = 1.76$, 95% CI = [1.62, 1.90]) compared with Wave 3 ($M = 1.09$, 95% CI = [0.95, 1.23]), $t(453) = 12.34$, $p < .001$, $d = 0.58$. Perceptions that support for gay marriage in the United States is increasing (perceived directional norms) were significantly greater in Wave 4 ($M = 7.74$, 95% CI = [7.61, 7.87]) compared with Wave 3 ($M = 7.03$, 95% CI = [6.89, 7.17]), $t(453) = 13.07$, $p < .001$, $d = 0.61$. However, attitudes toward gay marriage did not differ significantly in Wave 4 ($M = 1.93$, 95% CI = [1.67, 2.19]) compared with Wave 3 ($M = 1.96$, 95% CI = [1.71, 2.21]), $t(453) = -0.70$, $p > .250$, nor did ratings of gay people on the feeling thermometer (Wave 4: $M = 72.46$, 95% CI = [69.98, 74.93]; Wave 3: $M = 71.75$, 95% CI = [69.40, 74.11]), $t(453) = 1.81$, $p = .071$ (see Table S1 in the Supplemental Material).

Because it is possible that the differences in perceived norms between Waves 3 and 4 are explainable by a larger trend over time, unrelated to the Supreme Court decision, we next fitted all five waves of data to regression models that estimated the size of the discontinuity around the Supreme Court decision (Fig. 2). For each outcome measure, we estimated a series of linear regression-discontinuity models analyzing completed survey responses clustered by subject with robust standard errors. In the models, we included the effect of the Supreme Court ruling (0 = preruling responses, 1 = postruling responses), the number of days between the time of the ruling and the time of individual survey completion (postruling surveys received positive values, preruling responses received negative values), and the interaction between these two variables (see Tables S7–S10 in the Supplemental Material; see also Fig. S1 in the Supplemental Material for a nonparametric model fit). The results were consistent with those of the paired-samples t tests. The ruling had a positive effect on perceived status quo norms in support of gay marriage, $b = 0.77$, 95% CI = [0.65, 0.89], $p < .001$, and on perceived directional norms in support of gay marriage, $b = 0.77$, 95% CI = [0.66, 0.88], $p < .001$. The ruling did not have an effect on personal attitudes toward gay marriage, $b = 0.03$, 95% CI = [-0.11, 0.18], $p > .250$, or on the feeling thermometer, $b = -0.87$, 95% CI = [-2.25, 0.51], $p = .215$ (see Table S3 in the Supplemental Material).

Our results were also substantively unchanged when we used inverse-probability weighting of participant responses, a strategy that directly estimated the average characteristics of individuals who were likely to drop out of the survey on the basis of their baseline covariate values and gave greater weight to the responses of participants who shared characteristics with those who

dropped out (Horvitz & Thompson, 1952; see Table S3). We repeated this strategy using Wave 1 and Wave 2 as the basis for estimating attrition, with and without covariates and with and without case-wise deletion (i.e., only those participants who responded to all five waves). All specifications were consistent with our finding that perceived norms changed in response to the ruling and that attitudes and feelings toward gay marriage and toward gay people did not.

We did not find that these effects were moderated by respondents' reported trust in the Supreme Court, belief that the Court represents public opinion, or frequency of media consumption (see Section F in the Supplemental Material). As in Study 1a, the effects did not differ by the respondents' political orientation, again with the exception that conservatives' feelings toward gay people followed a different pattern than liberals' feelings in reaction to the ruling. Here, however, conservatives rated gay people less warmly as a result of the ruling, while liberals' feelings did not change (see Figs. S4 and S5 and Table S14 in the Supplemental Material).

We replicated this pattern of effects for social norms and attitudes in our between-subjects time-series data set (see Table S1). Perceived status quo norms were significantly greater among a new set of participants surveyed the week after the ruling ($M = 1.31$, 95% CI = [0.93, 1.70]) than among a different set of participants surveyed the week before the ruling ($M = 0.66$, 95% CI = [0.21, 1.11]), $t(124) = 2.17$, $p = .032$, $d = 0.38$. This pattern was the same for perceived directional norms in support of gay marriage (after ruling: $M = 7.47$, 95% CI = [7.13, 7.81]; before ruling: $M = 6.91$, 95% CI = [6.56, 7.26]), $t(124) = 2.28$, $p = .024$, $d = 0.41$. Attitudes in support of gay marriage and ratings of gay people each did not differ among the participants surveyed the week after and the week before the ruling. The pattern also held when we conducted a regression-discontinuity analysis on the full data set across 4 months of weekly data collection, and it held up to the same robustness checks (see Table S12 in the Supplemental Material). Participants in this data set were not more likely to select a gay-marriage sticker following the ruling (Table S1).

The effects are unlikely to have been due to experimenter demand. It is unlikely that participants guessed that experimenters were predicting that social norm change would be greater than attitude change. Moreover, we found the same pattern of effects among participants interviewed just once and those who were interviewed repeatedly (which is the most salient clue that we expected change over time).

Thus, our Study 1b longitudinal findings held across multiple specifications and in a separate between-subjects data collection and showed that the Supreme

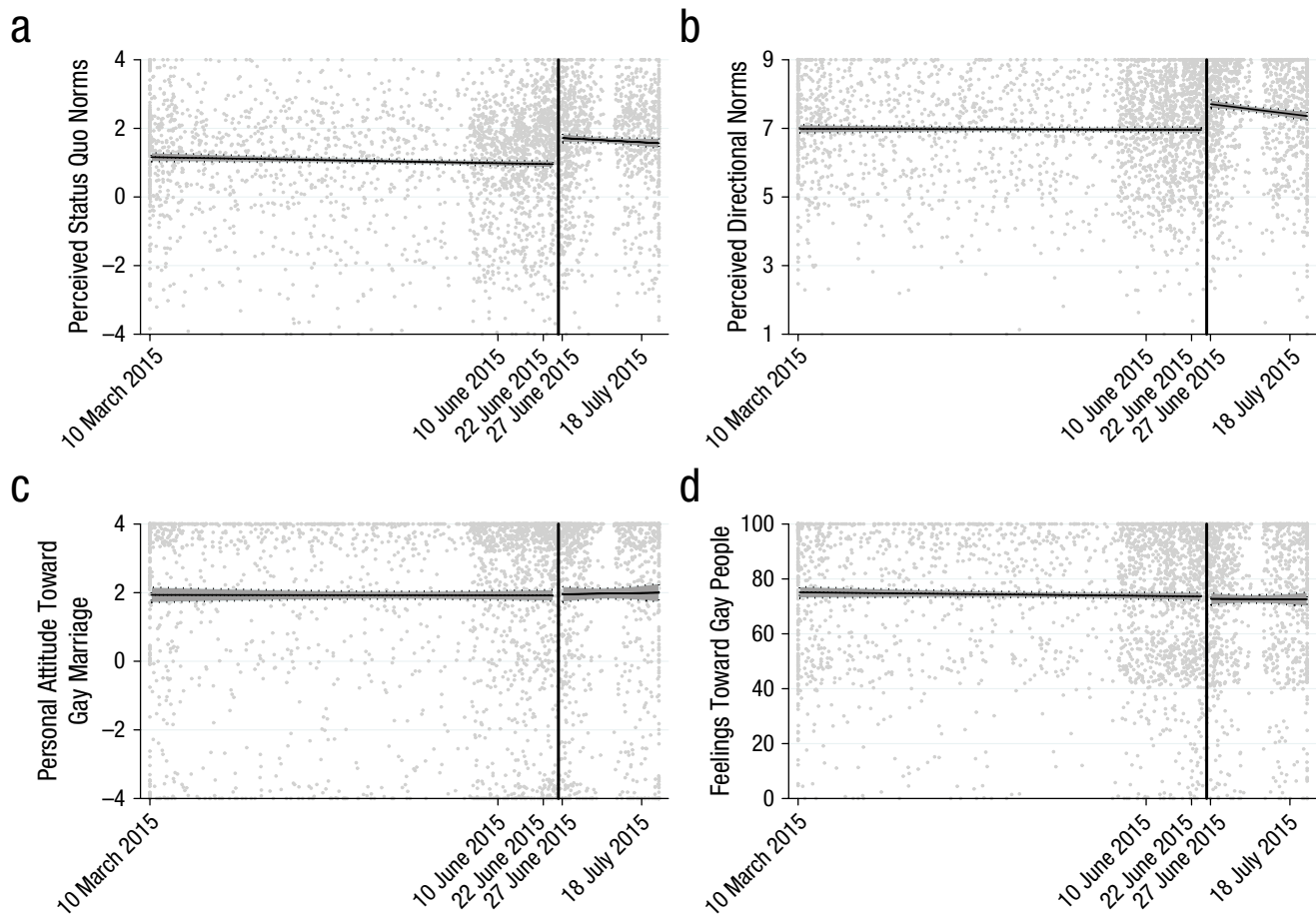


Fig. 2. Results from Study 1b: scatterplots (with best-fitting regression lines) showing the relationship between time and (a) perceptions of status quo norms, (b) perceptions of the direction and speed of social change in support of gay marriage, (c) personal attitudes toward gay marriage, and (d) feelings toward gay people (3,377 repeated measures observations). Separate models were run for the data collection waves before and after the announcement of the Supreme Court ruling on gay marriage (June 26, 2015, 10:00 a.m., indicated by the thick vertical line). Dates along the x-axis indicate the beginning of each of the five waves of data collection. The shaded band around each regression line indicates the 95% confidence interval.

Court *Obergefell v. Hodges* ruling was on average associated with a nearly 1-point increase on a 9-point scale (an average Cohen's *d* of 0.60) in perceived norms supporting gay marriage. As a comparison, the impressive rise in support for gay marriage among baby boomers (people born between 1946 and 1964) from the years 2001 to 2015 was an increase from 32% to 45% (a Cohen's *d* of 0.28; Pew Research Center, 2015). Contrary to our previous laboratory evidence in Study 1a, the results of Study 1b did not reveal evidence of a change in personal attitudes toward gay marriage or ratings of gay people following the Court ruling. Taking the two studies together, we found strong evidence that an institutional decision can change people's social norm perceptions—perceptions of what the public believes and what the public will believe in the future.

Discussion

The two studies reported here suggest that institutions can shift public perceptions of social norms: specifically, perceptions of Americans' current and future support for gay marriage. In a controlled experimental context, we found evidence that the June 2015 Supreme Court ruling on same-sex marriage shifted non-LGBTQIA Americans' perceived social norms in support of gay marriage (Study 1a). A prospective time-series study further showed that the actual Supreme Court ruling in favor of same-sex marriage was indeed associated with an increase in perceived social norms in support of gay marriage (Study 1b). Our research found that, unlike perceived norms, personal attitudes and feelings toward gay marriage and gay people did not

reliably shift in the wake of the ruling, particularly when measured in real-time response to the actual Supreme Court decision.

These changes in perceived social norms matter because they represent shifts in individuals' understanding of their society—where it stands and where it is going. From a behavioral perspective, this kind of understanding matters because individuals often use their perceptions of what is common or accepted in a collective as a guide for their own behavior (Cialdini & Goldstein, 2004; Tankard & Paluck, 2016). In the present research, we were not able to track important behaviors that these perceived norms might influence, such as interactions with the LGBTQIA community or public expression of support for gay marriage. These behaviors are an important extension for future investigation. Additionally, in previous research, shifts in perceived norms have encouraged shifts in personal attitudes (Stangor et al., 2001). Longitudinal research that extends beyond our 5-month period could also test this possibility.

Directional norms that describe what a collective will think or do in the future may capture a perception of momentum toward a new idea, which analysts have described as an important component of social change (Luks & Salamone, 2008). Perceiving a directional norm, such as that American support for gay marriage is on the rise, might otherwise be described as the perception that support for gay marriage is on the right side of history. The present research suggests that institutional decisions may themselves influence this perception of momentum, at least in some cases.

Only in Study 1a (a survey experiment) did we find support for a relationship between institutional decisions and personal attitudes. We know from previous work that institutions cannot always influence attitudes (e.g., Bartels & Mutz, 2009; Clawson et al., 2001; Hoekstra, 1995). Because gay marriage is an issue on which individual attitudes are tied to moral instinct (e.g., Powell et al., 2015), institutional decisions may be less likely to prompt the deliberation that often precedes attitude change (Bartels & Mutz, 2009). Additionally, the differences are myriad between a survey experiment and the real world, where the decision was actually issued. It was more difficult in Study 1a than in Study 1b to rule out the possibility that participants were aware of the research hypotheses. Another notable difference is that in Study 1a, participants were asked to imagine their personal experience after the decision was announced (in Study 1b, the Supreme Court was never mentioned while norms and attitudes were measured). This personalization of the institutional decision is one common denominator of previous

studies that found that attitudes were influenced by institutional decisions (e.g., Beaman et al., 2012; Hoekstra, 2000). Increasing the personal relevance of an institutional decision, either by emphasizing its local relevance or by prompting more personalized thought about it, may increase its influence on attitudes. Previous research has identified a tenuous connection between attitudes and social norms in which they do not reliably correlate or change together (e.g., Paluck & Shepherd, 2012). In these studies, we can emphasize only the robustness of our findings for shifts in perceived social norms, which held up when three methods of data collections were used: experimental, longitudinal, and cross-sectional methods.

It is striking that the Supreme Court decision affected perceived norms similarly for liberals and conservatives in these studies. The lack of moderation by ideology suggests a reduced role of the media, given differences in media consumption by liberals and conservatives (Prior, 2013) and the very likely differing portrayals of the Court's decision by the liberal and conservative media. We also did not find any moderation of our effects according to respondents' trust in the Supreme Court or belief that it represents the public. However, we expect that if a highly untrusted, undemocratic, or illegitimate institution were the source of a decision, it would be less effective in shifting perceived social norms (Tankard & Paluck, 2016). Accumulating studies that exploit variation across institutions, not just variation in individuals' personal attitudes toward the same institution, will advance this research question. Future work can also assess how much movement in perceived norms is due to the mass media attention that accompanies an institutional decision. Our present results were not explained by participants' imagined media responses or regular media consumption. For now, these findings represent the first experimental evidence that institutional decisions can affect norm perception, or perception of collective opinion.

Action Editor

James K. McNulty served as action editor for this article.

Author Contributions

M. E. Tankard and E. L. Paluck designed the studies. M. E. Tankard collected, analyzed, and interpreted the data under the supervision of E. L. Paluck. Both of the authors drafted and approved the final version of the manuscript.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

Supplemental Material

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Open Practices



All data and materials have been made publicly available via the Open Science Framework and can be accessed at <https://osf.io/q4n2b/>. The design, analysis plans, and other materials for the studies were preregistered at the Open Science Framework and can be accessed at <https://osf.io/q4n2b/>. The complete Open Practices Disclosure for this article can be found at <http://journals.sagepub.com/doi/suppl/10.1177/0956797617709594>. This article has received badges for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at <http://www.psychologicalscience.org/publications/badges>.

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Exhibit 5

The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods

Katerina Linos and Kimberly Twist

ABSTRACT

Can Supreme Court rulings change Americans' policy views? Prior experimental and observational studies come to conflicting conclusions because of methodological limitations. We argue that existing studies overlook the media's critical role in communicating Court decisions and theorize that major decisions change Americans' opinions most when the media offer one-sided coverage supportive of the Court majority. We fielded nationally representative surveys shortly before and after two major Supreme Court decisions on health care and immigration and connected our public opinion data with six major television networks' coverage of each decision. We find that Court decisions can influence national opinion and increase support for policies the Court upholds as constitutional. These effects were largest among people who received one-sided information. To address selection concerns, we combined this observational study with an experiment and find that people who first heard about the Court decisions through the media and through the experiment responded in similar ways.

1. INTRODUCTION

Can rulings from the Supreme Court, the most trusted branch of the federal government, change citizens' views so they support its decisions? Can hearing about a Court ruling prompt Americans to believe that particular

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policies are consistent (or inconsistent) with deeply held constitutional values and thus increase (or decrease) overall support for these policies? Multiple observational studies in real-life settings answer no and find that Court rulings have no net impact on national opinion (for example, Marshall 1987; Rosenberg 1991; Hanley 2008; Le and Citrin 2008; Luks and Salamone 2008; Gash and Gonzales 2008) but sometimes polarize opinions (for example, Franklin and Kosaki 1989; Hoekstra and Segal 1996; Johnson and Martin 1998; Brickman and Peterson 2006). In contrast, a few experimental studies answer yes and report that the American public increases its support for policies the Supreme Court upholds (for example, Bartels and Mutz 2009; Hoekstra 1995; Clawson, Kegler, and Waltenburg 2001).

Resolving this quandary is critical both for our understanding of the role of the judiciary in democratic politics and for our understanding of public opinion formation more generally. If Supreme Court rulings can provide a cue for Americans to follow, leading them to accept initially unpopular positions, then the Court can function as a “republican schoolmaster” and as an effective vehicle for social change (Dahl 1957; Caldeira 1986; Franklin and Kosaki 1989; Tushnet 2005). If Court decisions resonate with ordinary Americans, then these decisions are less likely to meet hostility and resistance from officials called on to implement them, which in turn helps guarantee both judicial independence and an effective system of checks and balances more generally (Stephenson 2004; Fox and Stephenson 2011). Finally, if initially unpopular Court rulings gain acceptance over time, this can help counter the major normative challenge to the Court’s legitimacy: that the Court is a countermajoritarian and unresponsive body composed of unelected judges with life tenure (Ackerman 1991; Kramer 2004). In contrast, if Americans do not respond to Court rulings, or are polarized by them, then each of these claims is, at best, a “hollow hope” (Rosenberg 1991).

By improving on measurement techniques that complicate prior studies in real-life settings, we find that Court rulings can change national public opinion, even on controversial issues that have been extensively debated and on which Americans have relatively firm views. But we also explain why our study and many others document a great diversity of

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responses among different population subgroups and why prior experimental and real-life studies come to conflicting conclusions. We argue that previous work misses a critical actor: the media. This happens even though the media, and television in particular, has been called the “most critical conduit” by which Americans learn about the Supreme Court’s actions (Slotnick and Segal 1998, p. 231). In prior experimental studies, researchers assume the role of the media themselves and offer one-sided information about a major Court decision to all respondents (see Chong and Druckman 2007, pp. 638–39). Prior studies in natural settings, in contrast, often group respondents who were not exposed to any media coverage of the Court decision with those exposed to one- and two-sided coverage. We show that the intersection of the Court and the media matters critically for opinion change. The Supreme Court depends more heavily on the media to convey and translate its messages than do other elite actors. Elites in the executive and legislative branches often speak directly to the public; they also buy advertisements and hold frequent press conferences to ensure that their messages are spread widely. In contrast, the nine justices communicate their views through lengthy and complex judicial opinions. As a result, the media’s role is distinctly important to the Court’s influence on public opinion (Davis 1994, p. 16).

We hypothesize that the public tends to follow the Court’s cues when two things occur: the Court rules on a politically salient issue and the national media present their audiences with one-sided coverage supportive of the Court decision. The media are especially likely to cover politically salient cases, as prior studies (for example, Slotnick and Segal 1998) and our data indicate. We expect some of this coverage to be one-sided and positive. We define one-sided positive coverage as messaging supportive of the Court majority; in contrast, two-sided coverage contains both supportive and critical information. Given the influence of elites’ cues on public opinion (for example, Kuklinski and Hurley 1994), exposure to one-sided positive coverage of a trusted actor’s views should cause some Americans to change their views, as prior experimental studies of framing effects show (for example, Nelson, Clawson, and Oxley 1997; Chong and Druckman 2007).

Three research design innovations allow us to test our theory and to add to the literature on how interactions between elites and the media shape public opinion more generally (for example, Iyengar et al. 1984; Page, Shapiro, and Dempsey 1987; Zaller 1992). First, we survey a nationally representative sample of Americans shortly before and shortly af-

ter two major and surprising Court decisions on health care and immigration, asking people for their views on the individual-mandate provision of the Affordable Care Act (Pub L. No. 111-148, 124 Stat. 119) and the “show your papers” provision of Arizona’s immigration law. Prior studies of Court decisions in real-life settings are based on survey questions fielded for other purposes, such as the General Social Survey, and thus share a major limitation: there is a very long time gap, often 1 year or more, between the before sample and the after sample, and events other than the Court decision could influence opinion in this interval (Hoekstra 1995, pp. 112). We are able to show short-term effects because of the proximity of our surveys to the Court rulings, and we document long-run effects by pairing our data with other surveys.

Second, we combine public opinion data with detailed media coverage data and connect individuals’ opinion shifts with the content of television programs they watch. Scholars note that “most published work on media effects does not include measures of media content” (Barabas and Jerit 2009, p. 74), and “most researchers fail to ascertain, let alone content-analyze, the media information that, they assume, their subjects encountered” (Graber 2004, p. 516). Kinder (2007, p. 158) finds it “unnerving . . . that we are still waiting for compelling demonstrations of framing effects in natural settings.” Because the Supreme Court decisions we study received both widespread coverage and coverage that varied dramatically across television programs, we are able to document framing effects in a natural setting and show how Court decisions and media coverage interact to produce national opinion shifts.

Third, we combine our observational study with an experiment to better address selection effects, the key limitation of research in real-life settings. We find that Court decisions influence people who hear about them only through the media they normally use and increase overall support for policies the Court upholds as constitutional. These effects were largest among people who received one-sided information emphasizing the frame chosen by the Court majority. Two-sided coverage, in which a news program emphasizes both the Court majority’s frame and alternative framings of the issue, did not confuse people about what the Court had held but did reduce the impact of the Court decision on opinion change. We also find that people who first heard about Court decisions through the media sources they normally use and people who first heard about Court decisions in the course of our experiment responded in similar ways. These findings contribute both to the literature on courts and

to the literature on public opinion formation, as effects of elites and the media have mostly been studied separately until now.

Although the two cases in our study generated appreciable media attention, they share features that make them hard tests of our theory. In both cases, the Court offered only a weak endorsement of the provisions on which we focus, the show-your-papers provision and the individual health care mandate. Both cases had been extensively debated in the media in the months prior to the rulings. Moreover, the debate tended to follow partisan lines, with Democrats supporting the health care mandate and Republicans supporting the show-your-papers provision. Thus, many respondents likely had firm opinions on immigration and health care before the Court rulings. These features bolster the generalizability of our results, as we expect stronger Court endorsements of novel issues to generate larger public opinion shifts.

2. HOW SUPREME COURT AND MEDIA CHOICES SHAPE NATIONAL OPINION

We argue that the interactions between two actors—Supreme Court justices and the national media—explain when Court decisions lead national public opinion, persuading ordinary Americans to increase (decrease) their support for policies the Court declares constitutional (unconstitutional). Studies have demonstrated the ability of high-credibility elites to lead public opinion through a process of heuristics and cue taking (see, for example, the summary in Bartels and Mutz [2009, p. 251]). Because the Supreme Court has traditionally been the most trusted branch of the federal government (Caldeira and Gibson 1992), we expect that Court rulings have the potential to provide highly influential cues to the American public. For cue taking to occur, three conditions must hold. First, the Supreme Court must decide to review a politically salient issue. Second, journalists must give extensive and one-sided coverage to the decision.¹ Third, many individuals must hear about and understand this news coverage, and some of these must change their views in accordance with the Court ruling.

1. One-sided positive coverage should move opinion in the direction of the Court ruling, while one-sided negative coverage should have the opposite effect.

2.1. Rulings on Politically Salient Issues and News Coverage

Supreme Court justices can sharply reduce the probability that they will shape public opinion by refusing to take on politically salient issues. The Court has significant discretion over the cases it reviews and grants fewer than 1 percent of the petitions it receives.² Prior work establishes that when the Court uses its discretion to deny review, limited news coverage typically follows, even on politically salient and controversial issues (Slotnick and Segal 1998). In turn, limited national coverage leaves national opinion unchanged (Hoekstra 2003).

Conversely, many cases taken on by the Supreme Court receive extensive coverage at the time of the Court ruling. Cases on politically salient topics, especially those involving individual rights, tend to receive disproportionately more media coverage relative to their share of the Court docket (for example, Solimine [1980]; Bowles and Bromley [1992]; see Persily, Metzger, and Morrison [2013] for a discussion of the importance of the Affordable Care Act ruling). In addition, cases that attract many amicus briefs and cases involving multiple dissents garner more coverage, as journalists often consider these important and controversial, and thus newsworthy (Sill, Metzgar, and Rouse 2013, p. 74). However, prior studies cannot tell us why some widely covered cases change Americans' views, while others do not, so we turn to this point next.

2.2. One-Sided Frames and Americans' Views

While extensive news coverage is necessary for Americans to become aware of a Court ruling, we argue that the nature of this coverage determines whether they will take cues from the ruling and update their opinions. More specifically, the arguments that justices develop to support their conclusions and journalists' choices about how to present, add to, or challenge the Court's argumentation are critical. To explain how lengthy and complex legal opinions are translated into short sound bites, we combine general theories about journalism with work specific to journalists covering the Supreme Court.

The Court is distinctive among elite actors in the United States, in that the public hears about its rulings only indirectly. After the Court issues its complex and lengthy opinions, the media decide how to translate those opinions to the public. In contrast, videos of members of Congress or

2. Supreme Court of the United States, Frequently Asked Questions (<http://www.supremecourt.gov/faq.aspx>).

the president speaking are often aired on the news—there is less need for the media to translate what they say, because the public can hear these messages directly. This elevates the media’s role to that of a critical mediator in studies of Court influence on public opinion (Davis 1994, p. 16; Franklin and Kosaki 1995). We focus on television coverage, as television remains the main source of news for most Americans, even in the digital era (Pew Research Center 2011), and, as we note above, is viewed as the “most critical conduit” for information about the Supreme Court (Slotnick and Segal 1998, p. 231). As reporters covering the Court seldom have the time to tell as many stories as they would like (Slotnick and Segal 1998, p. 47), media outlets will pick one or only a few frames likely to resonate with viewers.

Although there are three possible options for coverage—one-sided positive, two-sided, or one-sided negative—we expect that the media will typically choose either one-sided positive or two-sided coverage. Scholarship on journalists covering the Court suggests that they may understand their role as explaining, rather than criticizing, Supreme Court opinions (Slotnick and Segal 1998, p. 21; Davis 1994, p. 20). By reporting on the Court’s actions through the frames chosen by the Court majority, journalists may end up presenting one-sided, largely uncritical coverage. Early studies suggest that television coverage mostly presents the Court majority’s position and does not differ significantly among the networks (Katsch 1983; Davis 1987; Slotnick and Segal 1998).

A different theory of journalism, indexing theory, in turn suggests that journalists seek to avoid acting as mouthpieces for the administration and instead turn to diverse government elites and interest groups to construct frames: they index their reporting to the “magnitude and content of conflicts among key government decision-makers [and other powerful players]” (Bennett 1996, pp. 376–77). Because politicians and interest groups are eager to voice both supportive and critical viewpoints on controversial Court decisions, indexing theory leads us to expect two-sided coverage. We would thus expect to see either one-sided positive coverage or two-sided coverage rather than one-sided negative coverage. That said, it is possible that journalists working for partisan cable networks, namely, Fox News and MSNBC, consistently emphasize frames supportive of their networks’ ideologies (Meador 2013) that could be entirely hostile to the Court, so it is important to study current coverage data. Such variations in media coverage could shape the persuasive effects of Court decisions, as we explain next.

2.3. Extensive One-Sided Media Coverage and Persuasion

Journalists' choices about the frames they select, and the considerations they emphasize, should influence public responses to Court decisions. Many survey experiments indicate that individuals who receive one-sided frames change their opinions accordingly, while individuals who receive two-sided, competing frames are more likely to retain their original views (for example, Druckman 2001; Chong and Druckman 2007, 2013). However, these experimental studies involve fictitious information transmitted to respondents by researchers; as we note above, Kinder (2007, p. 158) says that "we are still waiting for compelling demonstrations of framing effects in natural settings." If Supreme Court decisions receive both extensive coverage and a variety of one-sided and two-sided coverage, we should be able to identify framing effects following actual Supreme Court cases. By separating respondents who receive no information, one-sided information, or two-sided information from the media they regularly use, we should be able to reconcile experimental findings with real-life results.

Hypothesis 1. People who receive one-sided information supportive of the Court majority's ruling should respond more positively than people who receive two-sided information (that also includes critical frames), both in experiments and in natural settings.

3. RESEARCH DESIGN

Prior studies of the Supreme Court's influence in natural settings rely on data generated for other purposes, and long lags often separate the before and after waves. In contrast, the survey research firm YouGov fielded our two-wave, nationally representative survey shortly before and shortly after two major Court decisions. Survey respondents received the first wave of questions in May 2012, about 5 weeks prior to the rulings, and the second wave in the days after the rulings. For each survey, 1,300 respondents completed wave 1; 87.5 percent of these individuals completed wave 2 of the health care survey, and 82.3 percent completed wave 2 of the immigration survey. YouGov reweighted these survey responses to create a nationally representative sample and provided us with complete responses for 2,000 subjects (1,000 per study).

We combined these public opinion data with news coverage data. Scholars note that most published work on media effects does not, in

fact, record or analyze media coverage (Barabas and Jerit 2009, p. 74; Graber 2004, p. 516). The few prior on-point studies find that few Americans respond to events that receive limited national coverage (for example, Barabas and Jerit 2009; Jerit, Barabas, and Bolsen 2006; Nicholson 2003; Price and Czilli 1996), and only the opinions of highly attentive subgroups are likely to change in response to these events (Barabas and Jerit 2010).

By focusing on Supreme Court cases that received moderate to high levels of coverage, we can study how a broader cross section of Americans respond and examine whether factors besides the volume of coverage—such as the frames used by different news sources—influence opinion change. We analyze how six different television networks covered two major Supreme Court cases and connect individuals' opinion shifts to frames to which they were (or were not) exposed, to provide the first comprehensive study of framing effects in a natural setting. Section 4 outlines how we content analyzed news media coverage.

While studies in natural settings have important external-validity advantages and provide “more realistic news scenarios” than what experiments typically offer (Chong and Druckman 2011, p. 254), important concerns about selection arise. The best observational studies, for example, compare people who hear about an event with people who do not. But it is possible that avid news followers care deeply about political issues and current events and thus respond especially strongly to new information about political events. Similarly, we worry that that people who choose to watch Fox News tend to be conservative, or those who watch MSNBC tend to be liberal, and may be more likely than others to respond favorably to ideological messages.

To address selection concerns, we include careful controls, study changes in opinion (to account for individuals' views prior to the Court decision), and combine our observational study with an experiment. In this experiment, we randomly assigned people to receive different information about each case to see whether people who receive information through the media they choose and people randomly assigned to receive similar information respond in similar ways. For each study, 40 percent of respondents were randomly assigned to the no-reminder group ($n = 400$) and did not receive any information about the content of the Court decision from us. The remaining respondents were divided into three equally sized groups and randomly assigned to receive different reminders about the decision ($n = 200$ per group). Our reminders drew on the

major arguments developed by the justices. However, we presented our respondents with short summaries of key points, rather than extensive legal opinions, in an effort to make the information offered through the experiment comparable to the information a respondent might receive through the media she typically uses.

In sum, we expect that people who receive one-sided information that the Supreme Court has judged a law to be constitutional should increase their support for the law, whether they receive the information from the media they regularly use or from a researcher. In contrast, people 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. We also expect that when information is repeated in the course of an experiment to people who have already received it through the media they regularly use, we should not see a further response (Druckman and Leeper 2012, p. 889). Table 1 outlines these predictions.

We expect two-sided frames to have smaller effects than one-sided frames; these attenuated effects could be zero if the two frames are equally strong or even negative if the counterframe is stronger than the main frame. Details about our question wording and study design and our robustness checks are in the online appendix.

4. MEDIA COVERAGE OF THE HEALTH CARE AND IMMIGRATION DECISIONS

Our goal is to examine whether variation in the frames that television programs use influence Americans' responses to Court decisions. In this section, we describe how the news media covered the health care and immigration decisions and provide evidence that media coverage of these decisions varied, with some news programs offering one-sided positive coverage and others offering two-sided coverage.

That said, there are some prerequisites to national opinion change: we expect to see national opinion shifts only when an event receives at least moderately high levels of and moderately clear news coverage. The immigration ruling received a moderately high level of news coverage, while the health care ruling received a high level of news coverage. In fact, media coverage of both health care and immigration issues spiked after the Supreme Court decisions to levels not seen before or since; no other actor or event, including the presidential debates, focused as much media attention on these issues as the Supreme Court. While both rulings

Table 1. Theoretical Predictions

Exposure from Experiment	Exposure from News Media		
	No Information	One-Sided Information	Two-Sided Information
No information	No effect	Positive effect	Attenuated effect
One-sided information	Positive effect	Positive effect	Attenuated effect
Two-sided information	Attenuated effect	Attenuated effect	Attenuated effect

received moderate-to-high levels of attention, coverage of the health care case was more straightforward than coverage of the immigration case. Every major TV evening news program said that the health care law had been upheld, while in the case of the immigration decision, some TV programs said that the law had been upheld, others said that the law had been struck down, and still others suggested that it was a mixed decision.³ Some Americans may have therefore misunderstood the Court ruling, and we take care to identify them.

Figures 1 and 2 show the frames used by evening news programs in covering each decision. Two students coded the evening news transcripts following the procedures outlined in Chong and Druckman (2011), coding for both valence and framing. Six frames were identified for each case; these were derived from the transcripts from morning, afternoon, and late-night news. The bars in Figures 1 and 2 represent the amount of time devoted to each frame as a percentage of total time devoted to the six frames.⁴ Our coders counted the total number of positive, negative, and neutral words belonging to each of the six frames.⁵ We used these

3. While the Court unanimously upheld the controversial show-your-papers provision, it was divided along partisan lines in striking down three other provisions. See the online appendix for more on the news coverage.

4. General news coverage that did not fit into any particular frame took up approximately half of each evening news program. Although the percentages in Figures 1 and 2 change when the denominator becomes total words, rather than words devoted to any frame, the relative amounts of positive, mixed, and negative coverage remain the same.

5. Coders were first trained using transcripts from news programs not airing in the evening. They then evaluated the evening news transcripts, sentence by sentence, classifying each sentence, or portion thereof, in two ways: the valence of the text and the frame it evoked. Valences were either positive (supportive of the Court ruling), negative (opposed to the Court ruling), or neutral (a sentence that is neither, such as “Majority Leader Harry Reid spoke about the ruling today”). Coders were provided with the six frames—previously identified using the morning, afternoon, and late-night news about the rulings—and asked to determine whether the sentence fit one of the six frames or could be classified as general commentary (for example, “The Court ruled today on Arizona’s immigration law”). Our coders then totaled the number of positive, negative, and neutral words belonging to each of the six frames for all transcripts.

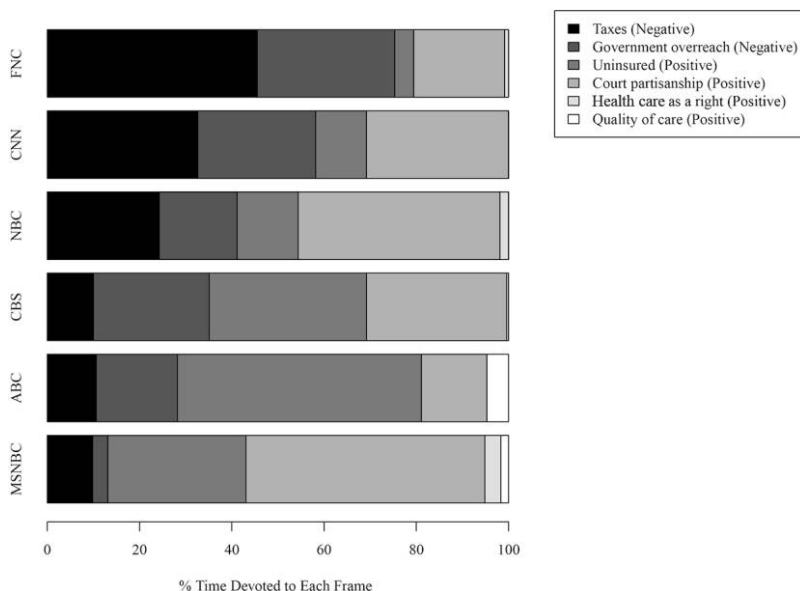


Figure 1. Frames used by six evening news shows in coverage of the health care ruling

totals to determine whether frames were used in a consistently positive or negative way by the six networks or whether usage was mixed.⁶

In the health care case, four frames were consistently used in a positive way, supportive of the Court majority. These included frames presenting health care as a basic right, changes in quality of care, impact on the uninsured, and the argument that the decision on the mandate overcame

6. We summed the total number of positive and negative words identified by our coders for each frame across the six news programs and then calculated the percentage of positive words used (for example, positive words used in racial profiling frame/positive + negative words used in racial profiling frame). If this percentage exceeded 75 percent, we classified the frame as used by the six evening news networks in a positive way, and if it fell below 25 percent, we classified the frame as being used in a negative way. Percentages between 25 and 75 percent were classified as mixed. We followed the procedure used in Chong and Druckman (2011) to determine whether our coders agreed about the presence of frames in a given transcript. We found 93.1 percent agreement about presence (or near total absence) of frames and a Krippendorff's alpha of .85. This measure takes a more conservative estimate of intercoder reliability, allowing some agreement by chance. As we were interested in the amount of negative coverage in each transcript, we also determined the intercoder agreement about the percentage of negative frames, again following Chong and Druckman (2011). Our alpha for the percentage of negative frames in each transcript was .94. These results meet and perhaps exceed typical standards of intercoder reliability. See the online appendix for additional details about the coding process and robustness checks; our results are consistent when we use alternative coding rules.

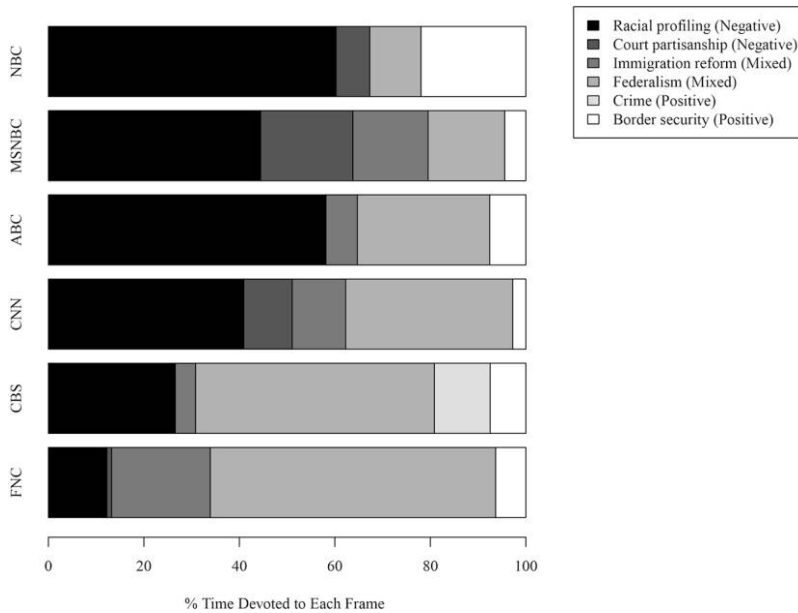


Figure 2. Frames used by six evening news shows in coverage of the immigration ruling

traditional partisanship on the Court (as conservative Chief Justice John Roberts sided with the liberal justices). Two frames, concerning taxes and government overreach, were used in a consistently negative manner. In the immigration case, two frames were used in a consistently positive way: news programs argued that the immigration law could help with both crime and border-security issues in Arizona. Two frames were used in a consistently negative way, raising concerns about racial profiling and about Court partisanship. Two additional frames, concerning federalism and immigration reform, were not used consistently and are thus coded as mixed.

On the basis of prior studies of television networks' partisan leanings and viewers' ideological news preferences (for example, Groeling 2008; Iyengar and Hahn 2009), we would expect to see more conservative coverage on Fox News, more liberal coverage on MSNBC, and relatively centrist coverage on ABC, CBS, NBC, and CNN. Perhaps unsurprisingly, our coding suggests that in the health care case, which upheld a provision championed by Democrats, Fox News allocated the most time to negative frames (frames critical of the Court majority), and MSNBC allocated the least. Conversely, in the immigration case, which upheld a provision

championed by Republicans, Fox News allocated the least time to negative frames, and MSNBC allocated the second-highest amount of time.

What is more surprising to us, however, was that the partisan networks—although critical of decisions that went against their ideological leaning (Fox News on health care and MSNBC on immigration)—explained the rulings in detail and featured some ideologically opposed arguments (for example, benefits for the uninsured on Fox News and aid for border security on MSNBC). Our data suggest that even when a partisan network encounters a Court decision that runs strongly counter to its ideology, it does not devote all of its time to criticisms of the Court. In contrast, when decisions were consistent with the ideology of these partisan networks, the networks opted to present positive one-sided coverage and raise very few competing considerations. Many nonpartisan networks also chose one-sided positive coverage, presenting the Court majority's argument alongside predominantly supportive arguments from other actors.

Our data thus provide tentative support for the second part of our theory, the argument that journalists tend to offer one-sided positive or two-sided coverage of Court decisions. This support is only tentative, because it is based on two Supreme Court cases rather than the universe of cases. Yet prior work suggests that many Court cases are reported in the media as straightforward accounts of what the Court decided and the rationale it provided for its own actions (Davis 1994, p. 20; Spill and Oxley 2003, p. 24). Because the two cases we studied are high profile and controversial, with the controversy closely following partisan lines, it is especially surprising that we do not see exclusively critical coverage.

Extensive and straightforward coverage of the health care decision led 70 percent of our respondents to correctly state that the Court had upheld the individual mandate and 6 percent to state (incorrectly) that the law had been struck down, while another 25 percent did not know. In contrast, the more limited and confusing coverage of the immigration decision led 46 percent of our respondents to correctly note that the Court had upheld the show-your-papers provision and 14 percent to state (incorrectly) that it had been struck down, while another 40 percent did not know which way the Court had ruled. Further analysis of the data suggests that there exists a simple linear relationship between the fraction of the evening news hour devoted to the case and viewers' understanding of the case. That is, even though viewers of Fox News received critical coverage of the health care decision, while viewers of MSNBC received cov-

erage that was much more supportive of the Court majority, this did not seem to confuse respondents; viewers of both stations' evening news programs were very likely to correctly understand the ruling simply because they were exposed to extensive information about it. This finding contributes to a new literature on the effects of partisan media that suggests that simplification along ideological lines may help some viewers process complex political messages (Stroud 2011, pp. 9–10, 108).

That said, closer analysis of our data identifies an important limitation of our measure of knowledge of each case. People who correctly stated that each law had been upheld were disproportionately likely to have supported the law prior to the Court decision in wave 1 of our survey. This effect is particularly pronounced in the immigration case. We believe that a fraction of our respondents interpreted the news coverage they saw in accordance with their prior beliefs; the confusing news coverage of the immigration case magnified this tendency. As a result, we use a measure of exposure rather than one of awareness, as Section 5 explains.

In sum, analysis of news media coverage suggests that both decisions received extensive coverage, much of which was clear, and as a result, large numbers of Americans' understood the Court's ruling. We also observe that television news programs offered one-sided positive coverage or two-sided coverage of each decision and that even ideologically opposed networks devoted time to explanation and frames supportive of the ruling. Whether and how this coverage contributed to opinion change is the question we take up next.

5. NATIONAL PUBLIC OPINION FOLLOWING COURT RULINGS

5.1. Court Decisions in Natural Settings

In the observational portion of our study, we find that support for the health care and immigration provisions we studied increased after the Supreme Court upheld them. We also find that people exposed to one-sided frames (which drew on and supported the majority opinion) through the media they typically use responded more positively to the Court decision than people exposed to two-sided frames (which also emphasized the dissent, or arguments against the ruling).

We first examine aggregate support for the individual mandate over time (Figure 3). Our survey data show that, among people who received no experimental reminder, support for the individual mandate increased

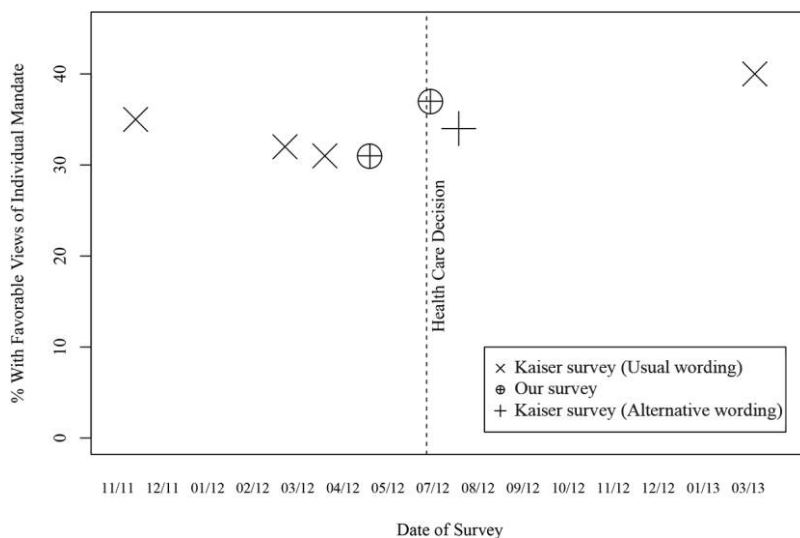


Figure 3. Support for the individual mandate over time

from 29 percent before the decision, in mid-May 2012, to 35 percent in the days after the decision, in late June 2012 ($p < .05$). We tried to field our surveys as close to the Court decision as possible so that our before-and-after comparison reflects responses to the Court decision rather than responses to other intervening events.

We complement our data with data collected by the Kaiser Family Foundation (2013) to illustrate that our short-term effects persisted for many months after the decision. Although the Kaiser Family Foundation used a different sampling technique and phrased its questions differently than we did, it is reassuring to see that its findings are very similar to ours. More specifically, the Kaiser surveys suggest that support for the individual mandate increased from 32 percent in March 2012 to 40 percent in March 2013 and thus that the short-term bump we report persisted for many months after the Court decision. In addition, the Kaiser survey suggests that there was no upward trend in support for the mandate in the months preceding the Court decision, which helps reassure us that the decision constituted a turning point.⁷

We see a bump in support for the show-your-papers provision in our data following the immigration decision, which upheld this provision of

7. Kaiser used distinctive question wording in the survey immediately after the ruling (July 2012), marked with a cross in our graph.

Arizona's immigration law, but it is small and not statistically significant. The discrepancy may well be because coverage of the immigration decision was more limited and confusing than coverage of the health care decision. As a result, only a minority of Americans understood that the Court had upheld the controversial show-your-papers provision, while others did not know, and still others believed that the provision had been struck down.

It is, therefore, important to not only measure aggregate opinion change but distinguish people who heard and understood the decisions from those who did not. Current best practice suggests that questions about exposure to news coverage, rather than questions about knowledge of the content of the news coverage, should be used to distinguish treated from untreated subjects (Druckman and Leeper 2012). Had we used questions measuring knowledge—such as those in our survey asking people whether the provision had been upheld or struck down—as a measure of treatment, we may have introduced bias, because respondents can use their prior beliefs to make sense of news coverage. Severe bias is particularly likely when news coverage is confusing, as in the immigration study. For example, people who strongly supported the show-your-papers provision in wave 1 were far more likely to (correctly) indicate that it had been upheld, relative to people who opposed the provision in wave 1.

While prior studies highlight that exposure, rather than knowledge, is the correct way to distinguish treated from untreated respondents, there is limited research on which measures of exposure are best. Indeed, some of the best prior work is concerned with only whether someone was exposed to a particular media source, rather than to news of a specific event, and thus a simple binary measure of exposure is appropriate (for example, DellaVigna and Kaplan 2007; Gerber, Karlan, and Bergan 2009).

Because we are interested in exposure to specific events, and in general exposure to a news source, we had to construct new measures of exposure. As this part of the coding is novel and somewhat subjective, we present four exposure cutoffs so as to be as transparent as possible about how our coding of exposure influences our results. The health care decision received widespread and straightforward coverage, while the immigration decision received moderate and somewhat confusing coverage.⁸

8. For theoretical reasons, we believe that it is appropriate to use a high threshold of exposure for the health care decision and a moderate threshold of exposure for the immigration decision. We show all thresholds for both decisions in the interest of transparency.

The measures of exposure we present come from a question in our survey that asked respondents to indicate whether they had heard about a series of seven prominent news stories around the time of the decisions. We asked respondents how many of the following headlines they had seen: one about the Court ruling plus six drawn from other events in the news at that time (the Egyptian elections, a Mitt Romney donor retreat, Dick Cheney's daughter's wedding, a Red Sox trade, the Jerry Sandusky trial, and the departure of a *Today* show host).⁹ By combining the headlines respondents indicated having seen in various ways, we were able to identify two relatively low thresholds of exposure (the most attentive 90 percent and 75 percent of our sample) and two moderate-to-high thresholds (the most attentive 25 percent and 35 percent of our sample) to divide people who were likely to know about the Court ruling prior to our experiment—the treated group—from those who were not.¹⁰ Using other questions about attentiveness to politics, we constructed alternative thresholds as well, and we present these robustness checks in the online appendix.

The health care case received widespread and straightforward coverage, so we believe that the first two thresholds (separating out the most attentive 90 and 75 percent of the population) best distinguish people who likely heard and understood this decision from people who did not. In contrast, because the immigration decision received moderate and somewhat confusing coverage, we believe that the second two thresholds (separating out the most attentive 35 and 25 percent of the population) are more appropriate. As a robustness check, we analyze opinions for both cases using each of the four cutoffs.

Because our research examines whether people changed their minds following the Court decisions, our dependent variable takes on three values: 1 for an increase of support for the provision from wave 1 to wave 2, 0 for no change, and -1 for a decrease in support. As the Court upheld

9. The headline about the Supreme Court in our question did not include information about the direction or content of the decision.

10. Using the question about exposure to headlines, we arrived at these thresholds as follows. About 90 percent of our sample had seen at least one of the seven headlines, so we classified these respondents as the most attentive 90 percent. Similarly, about 75 percent of the sample indicated that they had seen the headline about the Court, about 35 percent of the sample indicated seeing at least five of the seven total headlines, and about 25 percent of the sample saw the three political headlines. These four options, we felt, represented four realistic ways of distinguishing those who were likely to have been exposed to information from the Court from those who were likely to not have been exposed to this information prior to our study.

Table 2. Health Care Opinion Change by Type of News Media Exposure

Treatment Cutoff	Overall Effect	One-Sided Information	Two-Sided Information
Main specifications:			
Most attentive 90%	.37** (.13)	.41** (.13)	.28+ (.14)
Most attentive 75%	.13+ (.08)	.19* (.08)	.03 (.09)
Robustness checks:			
Most attentive 35%	.10 (.07)	.16+ (.08)	.01 (.08)
Most attentive 25%	.20** (.07)	.30** (.09)	.07 (.07)

Note. The subjects received no experimental reminder. Standard errors are in parentheses. $N = 400$.

+ $p < .10$.

* $p < .05$.

** $p < .01$.

both laws, a 1 can also be interpreted as a shift toward the Court ruling, while a -1 can be interpreted as a shift away from the Court ruling. This coding follows earlier studies (for example, Egan and Citrin 2011), allowing for comparability. A shift toward the Court ruling (a score of 1) includes people who move from strong disagreement with each provision to mild disagreement, to neutrality, or to support. Thus, we capture more movement than if we study only people who move from opposing to supporting each provision (and vice versa).

Table 2 examines the 400 individuals in the study of the Affordable Care Act who received no reminder from us and heard about the case only from the news media they regularly use. To determine the overall effect, we estimated four ordinary least squares (OLS) models. All models include the same control variables (partisanship, race, education, marital status, employment status, and gender), but each uses a different cutoff value to separate the treated group from the control group.

Results for the overall effect show that however we define the treated group, people who heard about the case were more likely to increase their support for the individual mandate than people who had not. This difference is generally, but not always, statistically significant. However, the magnitude of the effect varies across specifications. When we set lower thresholds for knowledge, we find larger effects. Because coverage of the

decision on the Affordable Care Act was widespread and straightforward across news media outlets, we believe that lower thresholds more accurately capture the true effect of the case; the high thresholds likely include in the control group many people who had heard about the case, which biases our estimates of the treatment effect downward.

We next wanted to compare people who received one-sided coverage of the health care decision from the media they regularly use with people who received two-sided coverage. Figure 1 suggests that over 65 percent of the analysis on Fox News, CNN, and NBC used frames that were critical of the Court majority. Thus, we define viewers of these programs as being exposed to two-sided coverage and viewers of other programs as being exposed to one-sided coverage. Results are very similar when we use other thresholds for one-sided versus two-sided coverage (see the online appendix). We reestimated our OLS models, using two treatment dummies (attentive people who received one-sided coverage and attentive people who received two-sided coverage) and the control variables mentioned above. As shown in Table 2, in every model, people who received one-sided coverage were more likely to increase their support for the mandate than people who received two-sided coverage ($p < .01$). This provides clear evidence of real-world framing effects. Consistent with our expectations in Table 1, one-sided coverage produces larger effects than does two-sided coverage.

Table 3 repeats this analysis for the 400 people who received no reminder from us in the immigration study. We estimate the same models as for Table 2. Again, we find that for all treatment thresholds, attentive people were more likely to increase their support for the show-your-papers provision that the Court upheld, although the difference is not always statistically significant. Because coverage of the immigration decision was less widespread and more confusing than coverage of the health care decision, we believe that a higher threshold (separating out the most informed 35 or 25 percent) for differentiating treated and untreated people is appropriate. The lower thresholds likely include too many people in the treated group who did not understand that the Court had upheld the show-your-papers provision. As with Table 2, we find a larger effect among people who received one-sided coverage from the news media they normally watch than among people who received two-sided coverage.

In the study of the immigration decision, however, the differences between the one- and two-sided groups are not always statistically significant. It is possible that the main negative frame used in the health care

Table 3. Immigration Opinion Change by Type of News Media Exposure

Treatment Cutoff	Overall Effect	One-Sided Information	Two-Sided Information
Robustness checks:			
Most attentive 90%	.07 (.09)	.10 (.09)	.00 (.11)
Most attentive 75%	.17* (.08)	.19* (.08)	.12 (.10)
Main specifications:			
Most attentive 35%	.11 (.07)	.17* (.07)	.02 (.10)
Most attentive 25%	.14* (.07)	.16* (.08)	.10 (.11)

Note. The subjects received no experimental reminder. Standard errors are in parentheses. $N = 400$.

* $p < .05$.

coverage (concerning an increase in taxes) resonated with a broad segment of the population, which resulted in fewer people exposed to two-sided news increasing their support for the individual mandate. The main negative frame used in the immigration case, which concerned racial profiling, may have resonated with a narrower segment of the population. In short, both the presence and the strength of competing frames likely influenced the results, and more research is needed on this point.

The biggest concern with studies in natural settings generally, and thus with the above results, is that people self-select into news viewership. One of these concerns might be that people who choose to watch a partisan channel—such as Fox News or MSNBC—have particularly stable views. While we can never fully dismiss these concerns, we are confident that we are not merely reporting the results of respondents’ media self-selection. For selection effects to bias results in favor of our findings, it would not be enough to have partisan respondents self-select into partisan networks or be more difficult to persuade. It would also have to be true that partisan networks present mostly two-sided coverage, which would result in hard-to-persuade respondents (partisans) being exposed to more mild stimuli (the two-sided news coverage). We found, through our analysis of the media coverage, that partisan networks are likely to present strongly one-sided coverage in favor of Court rulings with which they agree and two-sided coverage of rulings with which they disagree. Thus, for each case, we have one partisan network giving one-sided positive coverage

(MSNBC on health care and Fox News on immigration) and one giving two-sided coverage that includes criticisms of the Court ruling (MSNBC on immigration and Fox News on health care). In both cases, we see that people exposed to one-sided frames respond more positively than people exposed to two-sided frames, and it seems unlikely that selection effects can account for both of these results. Our experiment, described below, helps us better handle a broad range of selection concerns.

5.2. Experimental Reminders

We combine our observational study with an experimental one to better address selection concerns and understand why the results of prior observational and experimental studies often differ. Our main finding is encouraging to experimentalists: people who first hear about an event in the course of an experiment and people who first hear about it from the news media they regularly use respond in similar ways. However, among people who had already received information from the media, mere repetition of this information did not further change their attitudes, though the addition of other information did.

Our main goal in this section is to compare how different treatments and combinations of treatments influenced attitude change. People could receive no information, one-sided information, or two-sided information from the media they normally use. Each of these groups could also receive no information, one-sided information, or two-sided information in the course of the experiment. We classify respondents into nine categories, as illustrated in Tables 1, 4, and 5.¹¹

We then estimated an OLS model with eight dummy variables, one for each of the possible treatment combinations (real-world exposure and experimental exposure). The baseline omitted category is the group that received information neither from the media nor from the experiment. We array the coefficients on these various treatment groups in Table 4 to facilitate comparisons across rows and columns.¹² This model includes the same control variables as before (partisanship, race, education, marital status, employment status, and gender); our results are robust to al-

11. We pooled the two one-sided experimental reminders, reminder 1, which stated the Court's holding, and reminder 2, which also offered an argument in favor of the Court majority's holding.

12. For more details on the coding used in Tables 4 and 5, the full models, and robustness checks, see the online appendix.

Table 4. Health Care Decision Study: Opinion Change by Exposure

Exposure from Experiment	Exposure from News Media		
	No Information	One-Sided Information	Two-Sided Information
No information		.43** (.14)	.32* (.14)
One-sided information	.32* (.14)	.34* (.14)	.39** (.14)
Two-sided information	.01 (.21)	.25+ (.15)	.32* (.14)

Note. Standard errors are in parentheses. $N = 1,000$.

* $p < .05$.

** $p < .01$.

ternative specifications, including other levels of the treatment threshold, ordered logit models, and models with additional control variables.

We first examine the role of the media in leading opinion change and see that how the media covers the Court matters. Table 4 shows that people who received only two-sided real-world reminders responded less positively to the ruling than people who received one-sided reminders from the media they regularly use ($p = .07$). Similarly, people who received only two-sided experimental reminders were less likely to change their opinion than were people who received only one-sided experimental reminders, although this result is just beyond the threshold for statistical significance.¹³

We can, additionally, compare the effects on changes in opinion from exposure to real-world news and from our experiment, as well as whether repetition of information can have an effect on opinions. We see that people who received one-sided reminders from the media they regularly use responded in similar ways to people who did not follow the news but did receive one-sided reminders from us; these coefficients are somewhat similar in size. This is reassuring to experimentalists, as it helps reject the idea that people who do not follow the news in the real world would be indifferent to the news if, in fact, they were exposed to it.

13. We find that people who received two-sided experimental reminders were less likely to change their opinion than people who received real-world two-sided reminders. This discrepancy could be due to the fact that our experimental reminder was delivered shortly before people answered the question about a change in opinion, which made the competing message more salient in the experiment than it would have been in the real world.

Table 5. Immigration Decision Study: Opinion Change by Exposure

Exposure from Experiment	Exposure from News Media		
	No Information	One-Sided Information	Two-Sided Information
No information		.17* (.07)	.03 (.09)
One-sided information	.11* (.05)	.19** (.07)	.22** (.09)
Two-sided information	.12 (.07)	.23** (.09)	.28* (.14)

Note. Standard errors are in parentheses. $N = 1,000$.

* $p < .05$.

** $p < .01$.

When we examine the results for people who received information both from the real world and from the experiment, we find that repetition of the message does not alter results, but the addition of a different message does. If we consider the group that received one-sided real-world information, there is no significant difference in opinions among the group that received no information from our experiment and the group that received a repeated, one-sided message—repetition did not further increase their support. However, for the group that received two-sided information from us—information that was new to them—support for the mandate decreased relative to those who did not receive additional information from us ($p = .07$). These patterns, too, are reassuring for experimentalists, as they suggest true information effects at work.

Table 5 presents the results from our immigration study. Again, we first consider the effect of media coverage and then consider differences between real-world and experimental information. As with the health care study, we estimate an OLS model with eight dummy variables for the various treatments people could have received and our standard set of control variables; alternative models (different treatment thresholds, additional control variables, and ordered logit models) yield similar results. The results from the immigration study are very similar to those of our health care study.

We do not see significant differences in opinion change between people exposed to one- versus two-sided coverage, as we did in the health care case, and we believe that the most likely explanation is the lack of dissent (and presence of a concurrence) in the immigration case. Compet-

ing considerations presented in the context of a nuanced majority opinion may work differently than competing considerations presented in the context of a dissent. Prior research suggests that competing considerations in a majority opinion can sometimes increase, rather than reduce, the persuasive appeal of the majority opinion (Simon and Scurich 2011).

It is also possible, for the individuals who received information from our experiment, that we see evidence of learning rather than repetition. Our experimental reminders were much clearer than much of the news coverage, and reminders 2 and 3 contained information that the show-your-papers provision was unanimously upheld, information that even highly informed respondents were unlikely to have obtained from the media they regularly use. As with the health care study, we find that people who first heard about the case from the experiment respond in ways very similar to those of people who first heard about the case from the media they regularly use.¹⁴

6. CONCLUSIONS AND IMPLICATIONS

We find that the American public takes cues from the Supreme Court, which can result in increased support for controversial laws that have been upheld by the Court. We also find that people who first hear about a decision through the media they regularly use and people who first hear about a decision in the course of an experiment respond in similar ways. However, the ability of Supreme Court rulings to change public opinion depends critically on how the media choose to frame a decision. When the media (or researchers) emphasize frames supportive of the Court majority, the influence of the Court is typically greater than when the media (or researchers) emphasize competing frames. We also find that people who heard from the media that a law had been upheld did not further increase their support for the law when this information was repeated in the course of an experiment.

Our findings differ from the conclusions of prior studies in real-world settings in two main ways. First, we show a modest population-wide increase in support for laws the Supreme Court upholds, whereas prior real-world studies show a null net effect. This difference results, in part,

14. There are no statistically significant differences in opinions between the group hearing only one-sided information from the media and the group hearing only one-sided information from us nor between the group hearing only two-sided information from the media and the group hearing only two-sided information from us.

from our ability to isolate the effect of the Court decision using a short (6-week) window before and after the decision. In contrast, prior studies use a much larger window, of 1 year or more, and thus report on the combined effect of events that preceded and followed the Court decision. The short-term increase in support for the health care law persisted for many months, likely because there was no extensive countermobilization of the type that followed some earlier Supreme Court cases (for example, cases on abortion). Studying a variety of cases is critical, however, because media coverage of Court cases can be idiosyncratic. For example, news coverage of *Roe v. Wade* (410 U.S. 113 [1973]), perhaps the most studied Supreme Court decision, was unusually limited because President Lyndon Johnson died suddenly that day.

Second, we show a large increase in support for laws the Court upholds among people receiving one-sided coverage of the Court decision from the media they regularly use. To our knowledge, this is the first observational study that combines messages from elites and the media in this way and that documents framing effects in a natural setting by connecting individuals' shifts in opinions to the news coverage they received.

This research has significant implications for the literature on the US Supreme Court and for debates on the role of an unelected judiciary in democratic politics more generally. The Court is in an unusual position to focus national attention on particular issues. Whereas much of the news cycle is determined by events out of any individual leader's control, the power of the Court to draw attention to particular issues is especially noteworthy because it has broad discretion in picking the cases that it reviews. We find that the Court can lead public opinion, even on issues that have been extensively debated prior to its decisions and even when the decisions offer only a weak endorsement of controversial policies.

Although the two cases we study generated substantial media coverage, we expect to see similar, if not larger, movement of national opinion following high-profile Court decisions. This is because the cases we study involved a weak endorsement from the Court majority for policies that had been extensively debated along partisan lines before the rulings and on which many respondents already had firm views. Indeed, consistent with our claim that the Supreme Court can lead public opinion, recent polls suggest that support for same-sex marriage reached an all-time high following favorable Supreme Court decisions on this issue in 2013 (Saad

2013).¹⁵ These findings lead us to believe, contrary to the prior observational literature, that the Court offers more than a hollow hope for advocates of social change (Saad 2013). Instead, consistent with constitutional dialogue theory, we find that Court decisions in support of controversial laws increase support for these laws, and this might help quell questions about the normative legitimacy of an unelected judiciary (Ackerman 1991; Kramer 2004).

However, the nature of a Court decision and the way in which it is transmitted through the media moderates the response of the public. Our data suggest that national media seem to treat the Supreme Court with greater deference than they treat the other branches of the federal government and are often willing to offer primarily one-sided coverage supportive of the Court majority. In turn, people who receive this one-sided coverage from the media respond much more positively than people who receive two-sided coverage (with countervailing frames drawn from the dissent or from other sources). That said, the way in which a decision is written might modify its impact. It is possible that countervailing considerations introduced in the context of a dissent reduce the influence of the Court more than countervailing considerations introduced in the context of a nuanced majority opinion or concurrence. Future research along these lines would be very fruitful.

Although the public takes cues from Supreme Court rulings, it may not be the goal of the Court, generally, or the justices, individually, to change public opinion. Upholding a policy as constitutional does not necessarily imply that justices believe that it is the best policy. Indeed, in his opinion for *National Federation of Independent Business v. Sebelius* (132 S. Ct. 603 [2011], p. 6), Chief Justice Roberts suggests that the mandate may be constitutional but may not be good policy: “Members of this Court are vested with the authority to interpret the law; we possess neither the expertise nor the prerogative to make policy judgments. . . . It is not our job to protect the people from the consequences of their political choices.” In short, popular reactions to Supreme Court decisions may not correspond with Supreme Court justices’ political goals.

We have strong theoretical reasons to believe, first, that the combination of elites’ messages and the media coverage of these messages matters greatly and, second, that the Court-media interaction is distinct from the

15. However, it is somewhat more difficult to draw causal inferences from this poll data than from the poll data on health care analyzed here, because support for same-sex marriage had been on an upward trend long before the Court decision, whereas support for the individual mandate was stable or possibly declining.

executive-media or legislature-media interaction. We have known for a long time that both elites and the media can matter for opinion change (see, for example, Iyengar, Peters, and Kinder 1982). Because the Court does so little, relative to elected leaders, to communicate its messages, the role of the media is quite different. Further, scholars have shown that we should expect a more deferential role for the media when they cover the Court than when they cover other elite actors (for example, Slotnick and Segal 1998, pp. 18–21; Sill, Metzgar, and Rouse 2013, p. 61). The media are said to acknowledge their role in helping the justices and the Court maintain institutional legitimacy in the eyes of the public (Davis 1994, p. 20).

Given these distinctive circumstances surrounding the Court and the media, it is essential to consider the two together in a study of opinion change, and ours is one of the first studies to explicitly test interactions of any type between elites and the media. Groeling and Baum (2008) is perhaps the most theoretically similar to this study, and it focuses on how deferential media coverage of the president affects opinion. Yet its theoretical mechanism and empirical design are quite distinct from ours. The authors suggest that when a president declares war, media look for but cannot find competing messages from opposition politicians. In contrast, we suggest that competing messages exist following controversial court decisions but that the media simply spend more time explaining the decision in the Court's terms and using the Court's frames before proceeding to these competing messages. Focusing on the Court specifically, scholars have hypothesized that the way media choose to convey its decisions to the public should matter for opinion change (for example, Clawson and Waltenburg [2003], who experimentally test the impact of media frames about affirmative action, and Stoutenborough, Haider-Markel, and Allen [2006], who note how coverage of gay-rights cases differs between media outlets), but we are the first to test these claims.

These findings also have important and largely reassuring implications for the field of political communication. The scarcity of findings from natural settings in this area is likely because of the difficulty of fielding the appropriate studies. That is, we believe that we find endorsement and framing effects where prior scholars did not because we are able to anticipate major events that received extensive and varied news coverage and connect individual respondents' television exposure to their opinion shifts. We also add to a new literature on pretreatment effects and suggest that, just as repetition of information in the course of an experiment tends not to increase the size of treatment effects, repetition of informa-

tion in two settings (TV coverage and an experiment) does not increase treatment effects.

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Exhibit 6

LOCAL

Photographer who opposes gay marriage sues over Louisville's Fairness Ordinance

Sarah Ladd Courier Journal

Published 5:38 p.m. ET Nov. 20, 2019 | Updated 3:42 p.m. ET Nov. 21, 2019

A Christian photographer in Louisville who opposes same-sex marriage, but has never been forced to photograph a gay wedding, has sued the city, saying its 20-year-old Fairness Ordinance violates her constitutional rights.

Chelsey Nelson, owner of Chelsey Nelson Photography, alleges in her lawsuit that the city is using “the threat of limitless damages” to force her to “create photographs for, blog about, and participate in solemn ceremonies she disagrees with — same-sex wedding ceremonies.”

Her lawyers, including two from Alliance Defending Freedom, a conservative Christian nonprofit that says it defends religious freedom, say the ordinance forces Nelson to “violate the law, forsake her faith or close her business,” and that “these options are unacceptable.”

But Chris Hartman, director of the Fairness Campaign, a Kentucky LGBTQ advocacy group, called the suit “ludicrous” and “a fishing expedition.” He said he thinks Alliance Defending Freedom is using the suit to try to “undermine civil rights laws across the nation.”

Chelsey Nelson: As a Christian, I shouldn't be forced to work same-sex weddings

The suit comes on the heels of a 2018 ruling in which the U.S. Supreme Court said on narrow grounds that Colorado violated the rights of a baker to free exercise of religion when it sanctioned him for refusing to bake a wedding cake for a gay couple.

Similarly, the Kentucky Court of Appeals held in 2017 that a Lexington printer had the free-speech right to refuse to print a T-shirt promoting the city's annual gay pride festival.

In May 2017 the Kentucky Court of Appeals affirmed, holding 2-1, that the Fairness Ordinance didn't trump the company's right to free speech — including its right to refuse to promote a message it disagreed with.

Nelson's suit names Louisville Metro Government and the Louisville Metro Human Relations Commission Enforcement among its defendants.

The suit asks the U.S. District Court in Louisville to block city officials from enforcing the ordinance on the grounds that its stipulations “violate the United States Constitution’s First Amendment protections for speech, for association, for press, for free exercise of religion, and against establishment of religion and the 14th Amendment protections for due process.”

The First Amendment protects the freedoms of religion, speech, assembly and the press. The 14th says government cannot “deprive any person of life, liberty or property, without due process of law.”

“Louisville is using the threat of limitless damages, compliance reports, posting of notices, and court orders to force Chelsey to create photographs for, blog about, and participate in solemn ceremonies she disagrees with — same-sex wedding ceremonies,” the suit says.

The suit was filed as a “pre-enforcement challenge,” meaning that Nelson has not been approached by a same-sex couple, according to the ADF lawyers.

Related news: Wedding cake ruling no help in Kentucky gay rights case

“We don’t force LGBT web designers to create content condemning same-sex marriage for a church,” her lawyers say in the suit. “Or force Muslim printers to design anti-Islam flyers for a synagogue. The First Amendment protects these speakers’ freedom. Louisville should not take the same freedom away from Chelsey just because she wants to speak in favor of one particular view on marriage.”

Nelson's lawyers, Kate Anderson and Jonathan Scruggs with the Alliance Defending Freedom and Joshua Hershberger with Hershberger Law Office, wrote in the suit that the ordinance is forcing Nelson to either “violate the law, forsake her faith or close her business. These options are unacceptable.”

The suit refers people to two directories that list photographers in Kentucky who will photograph same-sex weddings, arguing same-sex couples have other opportunities to get their weddings photographed.

Hartman said the lawsuit is trying to challenge a “provision that has been well-accepted in civil rights statutes across the entire nation for more than 50 years, which says that a business may not post on its business front or I suppose in this case, on its website, that it will not serve protected classes of people.”

He said that same ordinance is there to bar a business from keeping out people of a certain religion or of a certain color, too.

"This has been an instrumental part of civil rights laws since the Civil Rights Act passed in 1964," Hartman said.

"And so to challenge it today, nearly 60 years after the Civil Rights Act passed, and 20 years after the Fairness Ordinance passed, is ludicrous and speaks to how low the Alliance Defending Freedom is willing to go to undermine LGBTQ civil rights and in fact, civil rights for all people."

Read: Fairness Campaign leader arrested in protest of Farm Bureau policies

Hartman said that he doesn't think the lawsuit will get far and that it is "frivolous."

Anderson, one of the lawyers, said in a statement that "artists shouldn't be censored, fined or forced out of business simply for disagreeing with the government's preferred views."

Scruggs added, "Every American, including photographers and writers, should be free to peacefully live and work according to their faith without fear of unjust punishment by the government."

Hartman said, "There is a balance that has been struck in fairness ordinances with religious beliefs" and churches are protected. "But if you have a business that is open to the public, what it says is that you should be serving everyone."

Reach breaking news reporter Sarah Ladd at sladd@courier-journal.com. Follow her on Twitter at [@ladd_sarah](https://twitter.com/ladd_sarah). Support strong local journalism by subscribing today: courier-journal.com/subscribe.

Louisville's Fairness Ordinance

Louisville Metro Ordinance 92.05 states it is an unlawful practice to deny someone "full and equal" enjoyment of goods, services, privileges, advantages and public accommodations based on their race, color, religion, nationality, disability, gender identity or sexual orientation. It furthermore states a person may not publish in any way a communication that suggests a person could be discriminated based on the above criteria. This ordinance excludes restrooms, showers, bath houses and other facilities "which are in their nature distinctly private."

LOCAL

U.S. Justice Department backs Louisville photographer challenging Fairness Ordinance

Dylan Lovan Associated Press

Published 10:32 a.m. ET Feb. 28, 2020

LOUISVILLE, Ky. (AP) — The U.S. Department of Justice has weighed in on a lawsuit filed by a Kentucky wedding photographer who is challenging a city ordinance banning businesses from discriminating against gay customers.

The Justice Department filed a “statement of interest” this week in federal court saying that the photographer, Chelsey Nelson, is likely to succeed on her claim.

Nelson sued Louisville city officials in November, arguing that the city’s ordinance violated the First Amendment. She has written about her business that she “can’t photograph anything that conflicts with my religious conviction that marriage is a covenant relationship before God between one man and one woman,” according to court records.

Background: Photographer who opposes gay marriage sues over Louisville's Fairness Ordinance

Nelson is asking the court to block enforcement of the law, known as a fairness ordinance, because she does not want to photograph same-sex weddings.

“Forcing a photographer, against her conscience, to express her support for a wedding that her faith opposes violates the Constitution,” a release from the Justice Department said Thursday.

Nelson has not, according to court records, been forced to photograph a same-sex wedding.

City of Louisville officials have responded to the lawsuit by arguing that Nelson has no cause to challenge the ordinance. Lawyers defending the city government have asked the judge to dismiss the suit.

Louisville photographer: As a Christian, I shouldn't be forced to work same-sex weddings

The American Civil Liberties Union filed a brief defending the city, arguing that the Nelson’s intent to offer wedding photography only to heterosexual couples violates the city law.

“Nelson Photography must know who a prospective customer is before deciding whether it will refuse to serve that person,” the ACLU brief said, calling it identity-based discrimination.

The Alliance Defending Freedom, meanwhile, is supporting Nelson and the DOJ's decision.

"Every American, including photographers and writers like Chelsey Nelson, should be free to peacefully live and work according to their faith without fear of unjust punishment by the government," said Jonathan Scruggs, the conservative group's senior counsel, in a statement. "We appreciate the DOJ's support of that principle in its statement of interest filed today. Chelsey serves all people. But the Louisville government is trying to compel Chelsey's speech, force her participation in ceremonies she objects to, and eliminate her editorial control over her photographs and blogs."

The suit echoes a 2018 ruling by the Supreme Court that said Colorado violated the rights of a baker to exercise his religion when it sanctioned him for declining to bake a wedding cake for a gay couple.

Billy Kobin with The Courier Journal contributed to this report.

NEWS

Court date for photographer suing over Fairness Ordinance, saying violates rights, approaches

Andrew Wolfson Louisville Courier Journal

Published 6:38 a.m. ET June 19, 2020 | Updated 6:45 a.m. ET June 19, 2020

To foes of same-sex marriage, Chelsey Nelson is Exhibit A for how it can force people to compromise their religious beliefs.

Nelson, 30, a Louisville wedding photographer and blogger who attends Clifton Baptist Church, says she believes God created marriage to be an “exclusive covenant between one man and one woman.”

And she says for that reason, she would refuse a request to photograph or edit pictures from a same-sex wedding or anything else “dishonorable to God.”

Five years after the U.S. Supreme Court declared same-sex marriage a fundamental right, Nelson's federal lawsuit filed last November is drawing national attention, including support from President Trump's administration.

She says she cannot share her views on her website for fear she would be sanctioned for violating Louisville Metro's Fairness Ordinance, the 21-year-old law that says people cannot be denied equal enjoyment of goods and services based on their race, nationality — or sexual orientation.

Her lawsuit claims that the city is using the threat of “limitless damages” and court orders to force her to create photographs, blog about and participate in “solemn ceremonies” she disagrees with.

Also: 5 years after same-sex marriage ruling, Kentucky couples reflect on challenges, triumphs

But the city's lawyers say she hasn't been forced to photograph or write about a single same-sex wedding.

They say no enforcement action has been taken or even threatened against her.

And they say the city has no history of prosecuting such violations.

In fact, the last time this part of the Fairness Ordinance was challenged was 18 years ago, they say, when a doctor said he wouldn't hire gay and lesbian employees. His lawsuit was thrown out, in part, a court said, because his bias was so widely known it was unlikely anyone from the LGBTQ community would apply to work there.

“The plaintiffs in our present litigation may accomplish the same notoriety and arrive at the same result,” the city's lawyers wryly observe in a pleading.

They are seeking to have Nelson's suit dismissed, while she and her company are asking for a restraining order blocking enforcement of the ordinance's accommodation section.

A hearing is set for July 14 before U.S. District Judge Justin Walker, a Sen. Mitch McConnell protege.

The controversy pits two legal principles. The Supreme Court has consistently held that freedom of speech prohibits the government from telling people what they can or can't say. But it has also ruled that laws targeting discriminatory conduct do not violate the First Amendment.

The Supreme Court in 2018 considered a case like Nelson's when it ruled in favor of a Colorado baker who had refused to create a wedding cake for a gay couple. Jack Phillips said decorating cakes is a form of art through which he can honor God and that it would displease God to create cakes for same-sex marriages.

But the court's 7-2 decision was narrow, and it left open the larger question of whether a business can discriminate against members of the LGBTQ community based on rights protected by the First Amendment.

Read more: A Kentucky county in 2004 voted 94% to outlaw same-sex marriage. Few attitudes have changed

That may be why Nelson's suit has attracted such attention.

It has drawn friend-of-the-court briefs from the American Civil Liberties Union and Americans United for Separation of Church and State on the one side and the Center for Religious Expression, the Family Trust Foundation of Kentucky and the U.S. Department of Justice on the other.

The ACLU says if Nelson could refuse service to same-sex couples on First Amendment grounds, another photographer could turn away interracial or interfaith couples, or African Americans or Muslims.

“There is no question that Louisville has the authority to prohibit businesses that choose to operate within its boundaries from discriminating in their sales of goods and services to the public,” the ACLU says in its brief.

But the Memphis, Tennessee-based Center for Religious Expression, which says its mission is to defend the Christian voice and conscience, says that “no one should ever be forced to write, publish, or otherwise create photographic images or other forms of messages that they cannot support in good conscience.”

In a statement to The Courier Journal, Jefferson County Attorney Mike O’Connell said the suit is groundless because Nelson has never been forced to photograph a same-sex wedding. “She has no injury and, so, no standing before the federal court,” he said.

For subscribers: Steve Beshear admits in book he defended same-sex marriage ban to protect son

Chris Hartman, director of the Fairness Campaign, predicted she will lose on the merits if the case is allowed to proceed. “I don’t believe a business that serves the public should have the right to refuse service to a whole class of individuals,” he said.

The suit was filed by the Scottsdale, Arizona-based Alliance Defending Freedom, which says it “advocates for your right to freely live your faith.”

Jon Scruggs, senior counsel and director of its Center for Conscience Initiatives, said, “Chelsey happily serves all people, including those in the LGBT community” — she’ll do photo editing for gay photographers and business owners, he says.

“She just can’t convey certain messages she disagrees with for anyone.”

In an email, Scruggs said she should not any more be forced to photograph a gay wedding than a Muslim should be required “to print pamphlets promoting Judaism for a synagogue” or an LGBTQ artist forced to design banners criticizing same-sex marriage for a church.

Her lawyers say she has lost revenue because she cannot effectively advertise and promote her business “with the threat of this law looming over her.”

In an affidavit, Nelson notes that Louisville wedding photographers who serve same-sex couples may freely advertise that; she cites one who says on his website that “I believe love is love, regardless of your age, race, religion or sexuality.”

Nelson says that as an “expressive business. ... I just want the same freedom.”

Nelson: As a Christian, I shouldn't be forced to work same-sex weddings

She also says she would refuse to photograph anything that devalues God’s creation, condones racism, celebrates pornography or obscenity, praises vulgarity or “contradicts” biblical principles.

For that reason, she says she won't shoot a “cosplay superhero-themed” or “trash-the-dress” wedding or edit photos for a “strip club or Planned Parenthood.”

But in defending the suit, which also names the Human Relations Commission as a defendant, Assistant County Attorneys John Carroll and Jason Fowler say laws must be enforced against a plaintiff before their constitutionality is challenged.

And they say Nelson’s concerns are all “hypothetical.”

The ACLU says Chelsey Nelson Photography seeks a constitutional right to post a statement on its website: “a literal sign in its virtual shop window — proclaiming that it provides wedding photography for heterosexuals only.”

That, it says, would run counter to the basic principle reflected in over a century of public accommodation laws that “all people, regardless of status, should be able to receive equal service in American commercial life.”

As early as 1889, the ACLU notes, the Supreme Court of Nebraska explained in one of the first public accommodation decisions, that a barber, by opening a shop and putting out his sign “thereby invites every orderly and well-behaved person who may desire his services to enter his shop during business hours.”

“The statute will not permit him to say to one: ‘You are a slave, or a son of a slave; therefore I will not shave you.’”

See also: What a town's 20-year struggle with 'Fairness' says about rural LGBTQ rights

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LOCAL

Judge blocks enforcement of Fairness law against photographer who opposes same-sex weddings

Andrew Wolfson Louisville Courier Journal

Published 3:56 p.m. ET Aug. 14, 2020 | Updated 6:02 p.m. ET Aug. 14, 2020

A federal judge has blocked the city of Louisville from enforcing its Fairness Ordinance against a photographer who says she can only shoot pictures of weddings between a man and a woman because of her Christian religious beliefs.

U.S. District Judge Justin Walker ruled Friday that Metro Government may not compel Chelsey Nelson to photograph same-sex weddings or punish her for advertising on her website or social media that she only photographs and blogs about opposite-sex ceremonies.

"America is wide enough for those who applaud same-sex marriage and those who refuse to," Walker wrote in a 27-page opinion. "The Constitution does not require a choice between gay rights and freedom of speech. It demands both."

Walker, a judicial conservative who has been confirmed to the U.S. Court of Appeals for the District of Columbia, also cited the Supreme Court's 2015 opinion establishing the right of same-sex marriage.

"Just as gay and lesbian Americans 'cannot be treated as social outcasts or as inferior in dignity and worth,'" Walker wrote, quoting the opinion in *Obergefell v. Hodges*, "neither can Americans 'with a deep faith that requires them to do things passing legislative majorities might find unseemly or uncouth.'"

Background: Court date for photographer suing over Fairness Ordinance approaches

Walker issued a preliminary injunction for Nelson, saying she is likely to win a lawsuit in which she protested restrictions under the Fairness Ordinance, which protects members of the LGBTQ community from discrimination.

Walker said "her photography is art," that "art is speech" and that the government can't compel speech when it violates the speaker's religious or political principles.

Although photography is wordless, Walker said, "so too is refusing to salute the flag or marching in a parade, both of which the Supreme Court has said are protected forms of speech."

Nelson's lawyer, Jonathan Scruggs, senior counsel for Alliance Defending Freedom, applauded the ruling.

"Just like every American, photographers and writers like Chelsey should be free to peacefully live and work according to their faith without fear of unjust punishment by the government," he said. "'The court was right to halt enforcement of Louisville's law against Chelsey while her case moves forward.'"

The city's lawyers did not immediately respond to questions about the opinion or whether they will appeal.

Its lawyers argued that laws targeting discriminatory conduct do not violate the First Amendment.

Read this: 5 years after same-sex marriage ruling, Kentucky couples reflect

The American Civil Liberties said in a brief that if Nelson could refuse service to same-sex couples on First Amendment grounds, another photographer could turn away interracial or interfaith couples, or Black or Muslim couples.

"There is no question that Louisville has the authority to prohibit businesses that choose to operate within its boundaries from discriminating in their sales of goods and services to the public," it said.

The city also said it had never taken or threatened enforcement action against Nelson.

But Walker said enforcement is a possibility, given that between 2010 and 2017, Louisville investigated 93 businesses for alleged violations of the Fairness Ordinance, and because the city declined to disavow that it might prosecute her.

Nelson also contended that her business suffered because she could not market openly because of the fear of being fined and having to pay legal fees.

Her suit filed last year drew national attention, including support from President Donald Trump's administration.

The Supreme Court in 2018 considered a case like Nelson's when it ruled in favor of a Colorado baker who had refused to create a wedding cake for a gay couple. Jack Phillips said decorating cakes is a form of art through which he can honor God and that it would displease God to create cakes for same-sex marriages.

But the court's decision was narrow, and it left open the larger question of whether a business can discriminate against LGBTQ people based on rights protected by the First Amendment.

Nelson: As a Christian, I shouldn't be forced to work same-sex weddings

In 1999, Louisville passed the Fairness Ordinance, which prohibits discrimination based on sexual orientation and gender identity in housing, public accommodations and employment.

Walker noted that the law requires that companies serve LGBTQ customers and refrain from advertising that they won't serve them. In the two decades since then, 19 Kentucky communities have passed similar laws.

Paying homage to the struggle to enact those laws, Walker said that "a generation ago, when 10 Louisvillians founded the Fairness Campaign, discrimination against gay and lesbian people was legal in every Kentucky city," but that with "extraordinary vision, tenacity, and grit — battling often steep odds in the legislative and judicial arenas, not to mention in their daily lives, the Fairness Campaign and its allies changed that."

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NEWS

Despite ruling for Christian photographer, Louisville will still enforce gay rights law

Andrew Wolfson Louisville Courier Journal

Published 2:13 p.m. ET Aug. 18, 2020 | Updated 6:14 p.m. ET Aug. 22, 2020

Louisville Metro Government will continue to enforce the Fairness Ordinance to prevent discrimination based on sexual orientation, despite a federal judge's preliminary ruling last week blocking punishment of a Christian photographer who says she will shoot only weddings between a man and a woman.

Jefferson County Attorney Mike O'Connell said Tuesday the ruling applies only to Chelsey Nelson, and the city will try to prove at trial its "compelling interest in preventing invidious discrimination against its LGBTQ citizens."

"The Fairness Ordinance remains alive and well," O'Connell said in an email.

The city could have immediately appealed U.S. District Judge Justin Walker's preliminary injunction prohibiting potential enforcement of the law against Nelson for turning away gay couples or advertising that she won't serve them.

Walker agreed with Nelson that photography is speech that is protected by the First Amendment.

Sam Marcossan, a professor at University of Louisville's Brandeis School of Law, said in an email that Walker failed to consider the compelling interest that underlies anti-discrimination laws.

"In effect, he treated her rights as nearly absolute instead of subject to government actions that are necessary to serve the critical goal of fighting discrimination," Marcossan said.

More: Judge blocks enforcement of Fairness law against photographer who opposes same-sex weddings

Corey Shapiro, legal director for the American Civil Liberties Union of Kentucky, which filed a brief in the case, called the decision disappointing.

"Our laws have long said that businesses that are open to the public ... can't pick and choose who they will serve based on who the customer is," he said.

Kenneth Upton, senior litigation counsel for Americans United for Separation of Church and State, said he believes Walker, who has been confirmed to the U.S. Court of Appeals for District of Columbia, "was reaching to make new law because he knows he not going to be on the district court much longer."

Upton noted that the preliminary injunction is not binding on any other court or case.

Nelson's lawyer, Jon Scruggs, senior counsel for Alliance Defending Freedom, hailed the Aug. 14 ruling, saying "photographers and writers like Chelsey should be free to peacefully live and work according to their faith without fear of unjust punishment by the government."

But O'Connell said in his email that the Fairness Ordinance is neutral toward religion and has "never been applied with a bias against religion or speech by Ms. Nelson or anyone else."

Quoting former Justice Anthony Kennedy in the Supreme Court's 2017 Masterpiece Cakeshop decision, O'Connell said that while sincerely held religious beliefs are entitled to respectful consideration by the courts, this must not come at the cost of "subjecting gay persons to indignities when they seek goods and services in an open market."

In that case the Supreme Court passed on the First Amendment question of whether a baker had the free speech right to turn down a same-sex couple.

Marcossan said Nelson's dispute with the city of Louisville "could be the kind of case where the court might consider how to balance expressive and religious rights against anti-discrimination rules."

However, he said the fact that Louisville never enforced the ordinance against her could weigh against it taking the case.

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Exhibit 7

OPINION *This piece expresses the views of its author(s), separate from those of this publication.*

Louisville photographer: As a Christian, I shouldn't be forced to work same-sex weddings

Chelsey Nelson Opinion contributor

Published 12:01 p.m. ET Nov. 21, 2019

Growing up, my very favorite time of the year was Christmas morning. The smell of warm cinnamon rolls filling our living room. The joy of family. The gratitude for what Christmas is really about — a savior for the world. It always left me awestruck. It's an event worth pausing to remember because it was and remains so meaningful.

Some events are just so significant that we treat them differently. They're special. I think this love of special events and of remembering them is why I was drawn to wedding photography.

At a wedding, there are the expected memories to capture — the first look, walking down the aisle, the couple's first kiss as newlyweds. But there are also hidden moments, like when a father fights back an unexpected tear. That's the kind of picture a family will cherish for years to come. And my hope, as I share on my website, "is to give that gift to others — beautiful images that will represent special memories, become family heirlooms, and tell the story of each legacy represented, because it matters."

Background: Photographer who opposes gay marriage sues over Louisville's Fairness Ordinance

I do that by forming close bonds with each couple I photograph. On their wedding day, they probably spend more time with me than anyone else. I even do my initial consultations in my home. At my kitchen table over cookies, I get to hear about them and their dreams for the future as we plan how to capture their big day. Then, we schedule an engagement session to make sure they're comfortable in front of my camera. (Most of us aren't used to a photographer following us around all day, right?)

After their wedding day, I write a blog post about each couple and their wedding, complete with my favorite photos and personal notes celebrating their new marriage and the start of their family.

Why do I care about this so much? Well, I have a passion for marriage and, with it, weddings. As a Christian, I believe that something significant is happening. It's a couple pledging their lives to each other, which the Bible tells us is a picture of the relationship between Jesus Christ and His church.

Because marriage is so important to me, I'm careful to photograph and blog about each of these solemn ceremonies in a way that reflects my views of marriage. In every photograph I take and with each word I pen on my blog, I'm trying to portray this beautiful ceremony and this marriage in the best light possible — to show others that marriage really is worth pursuing.

This means I wouldn't be the best person to photograph every wedding. Of course, I serve everyone regardless of who they are. I just don't photograph every wedding requested of me. For example, I can't celebrate a wedding that devalues how seriously I take marriage — like a heavily themed Halloween or zombie-themed wedding.

My highest aim in life is to honor God, and that informs everything I do, business included. When I look back on my life when I'm older, I want to be able to say that I stayed true to this goal — that I ran the race and finished well.

Yet, as I began expanding my business, I realized a Louisville law threatens me with damages if I stay true to my beliefs about marriage. Actually, the law won't even let me explain some of my religious beliefs about marriage, whether on my studio's website, social media or directly to couples who may want to work with me. I also can't explain how some of my religious beliefs affect which weddings I celebrate through my photography.

I've built a business that I love, working with all people regardless of who they are. But I strongly believe that photographers should be free to align their creative talent with their beliefs. The government shouldn't force artists to express views they disagree with. And that goes for both people who share my particular views and those who don't. I want this freedom to be available to all artists.

I hope that Louisville comes to agree with me. Because no one should have to choose between their faith and their livelihood.

Chelsey Nelson is owner of Chelsey Nelson Photography and has filed suit against Louisville through her attorneys with Alliance Defending Freedom (@AllianceDefends).

OPINION *This piece expresses the views of its author(s), separate from those of this publication.*

Court victory for photographer who opposes same-sex weddings a win for free speech

Jonathan Scruggs Opinion contributor

Published 11:58 a.m. ET Aug. 21, 2020

Americans are fortunate to enjoy several inalienable rights, including the right to control what you say and what you celebrate. That's at the core of the First Amendment. And on Aug. 14, a federal court protected this right for a creative professional in a ruling that will benefit all people of Louisville across the ideological spectrum.

That ruling focused on Chelsey Nelson, a Louisville-based blogger and photographer. She creates photographs and blogs promoting many types of businesses, but her true passion is celebrating one of the most beautiful days in a person's life: their wedding day.

Sadly, Chelsey could not operate her wedding business the way she wanted, because if she did, Louisville would have forced her to create blogs and photographs celebrating same-sex marriage, a view of marriage she disagrees with. Compelling Chelsey to create certain blogs or photographs is unconstitutional, as the federal court said last week.

Courier Journal editor: Our community is changing. Our newsroom must change, too

As a devout Christian, Chelsey believes marriage is a sacred relationship between one man and one woman. At the same time, Chelsey uses her blogging and photography skills to serve all people, including those who identify as LGBT. For example, she would happily use her talents to help an LGBT businessowner promote their yoga studio or restaurant.

But like other creative professionals, Chelsey cannot promote any messages that violate her core convictions. For example, she will decline any request that promotes racism, pornography or any other subject that violates her values, no matter who asks her to do so.

Chelsey's editorial discretion in these matters is not about the client's identity; it is about the content she is asked to create. Because Louisville was denying Chelsey her fundamental freedom to do this — to control what she says — Alliance Defending Freedom filed a federal lawsuit on her behalf.

It must be stressed that Chelsey was not the only person threatened by Louisville's misuse of its law. Far from it. As the ruling noted, Louisville conceded that its legal theory would justify forcing speechwriters and photographers to create content supporting "climate change deniers" even if they want to work only for environmentalist causes. Louisville's defense would allow officials to force a Muslim printer shop to promote anti-Islam messages, or force a Jewish graphic designer to create artwork proclaiming that Jesus rose from the dead.

But freedom of speech should protect both sides in a debate. As the court said, "America is wide enough for those who applaud same-sex marriage and those who refuse to." That's an America worth believing in — a country where no one should have to move across the country to run away from heavy-handed government officials with different religious and political views. Free speech is protected by the highest law in the land, which applies everywhere, for everyone. As the U.S. Supreme Court stated just two years ago, "the people lose when the government is the one deciding which ideas should prevail."

That principle still rings true in our nation's courts. Last year, the U.S. Court of Appeals for the 8th Circuit protected this freedom for Minnesota filmmakers, and the Arizona Supreme Court protected it for a painting and calligraphy studio. The Kentucky federal court has now continued this trend, which is a winning streak not just for those artists but for free speech and for all Americans.

A quest for freedom: The diary of a young Black man raised in Louisville's West End

To be sure, tolerance and diversity of thought are often not popular. In recent months, we've seen the tides of cancel culture attempt to get people fired and harassed. Cancel culture also seems pretty fickle too. J.K. Rowling, the author of the beloved Harry Potter series, supports much of LGBT ideology, but she stated that biological sex is a scientific reality and then some employees at her publisher refused to work on any of her books. Praised today. Ostracized tomorrow.

In stark contrast, the First Amendment has never changed, and its shelter protects all people against government censorship. Your fundamental freedoms are not dependent upon having powerful people in city hall who agree with you. Your protection remains intact, no matter if others disagree.

That is the timeless principle the federal court reaffirmed in ruling for Chelsey. And as we continue navigating the turmoil that is 2020, this principle is perhaps one of the few things Americans can continue to count on no matter what.

Jonathan Scruggs, senior counsel and director of the Center for Conscience Initiatives at Alliance Defending Freedom (@Alliance Defends), represents Chelsey Nelson and her studio, Chelsea Nelson Photography.