

# Exhibit A

NO. \_\_\_\_\_

JEFFERSON CIRCUIT COURT  
DIVISION \_\_\_\_\_ ( )  
JUDGE \_\_\_\_\_

**EMW WOMEN'S SURGICAL CENTER,  
P.S.C., *et al.*,**

PLAINTIFFS

v.

**DANIEL CAMERON, *et al.*,**

DEFENDANTS.

**AFFIDAVIT OF JASON LINDO, Ph.D.**

I, Jason Lindo, Ph.D., declare the following:

1. State bans on abortion impose substantial costs. A large body of literature shows that abortion is reduced and childbearing is increased when states enact such laws. Individuals affected in this manner are disproportionately women of color and disproportionately disadvantaged relative to the general population in terms of their economic circumstances.

2. Research resoundingly indicates these individuals are made *more* disadvantaged when they are impeded from accessing abortion. They become increasingly disadvantaged in part because of the substantial monetary costs associated with having and raising a child. As a result of these costs, adding a child to a household without expanding its resources will thrust poor families deeper into poverty and non-poor families closer to the poverty line. Moreover, research shows that household resources are *not* sufficiently expanded to prevent such increases in poverty when a child is added to a family. Indeed, household resources are typically *reduced* overall. In addition, educational attainment is reduced for younger women who have restricted access to abortion.

3. Bans on abortion also impose substantial costs even on individuals who are able to travel to other states to obtain abortions. These individuals are likely to face additional travel expenses (including childcare and/or lost wages), delays, a more-limited set of procedures, and additional medical risks and medical expenses.

4. If there is no access to abortion in Kentucky, it will impose these—and other—costs on its residents. Individuals obtaining abortions in Kentucky are more likely to be Black, more likely to be Hispanic, more likely to be unmarried, and more likely to have no more than a high school education than the general population of Kentucky residents. Many of these individuals would typically be considered of schooling age and/or early in their careers in the labor market.

5. Based on historical data, a majority of individuals seeking abortion in Kentucky have previously given birth, and many will have children later in their lives after having had an abortion. Naturally, the effects described above imply that these children will grow up in households with more limited resources and reduced parental education. Given a large body of reliable and rigorous research showing that household resources and parental education have a causal effect on a wide array of children's outcomes, we can expect the deleterious effects of restricted abortion access to extend to these children in many ways. In particular, this body of work indicates that the effects are likely to manifest in poorer health at birth, increased infant mortality, lower test scores, more behavioral and social problems, reduced educational attainment, and poorer adult economic outcomes. These conclusions are supported by a large number of rigorous studies of causal effects.

## **I. Professional Credentials and Experience**

6. I provide the following facts and opinions as an expert in the field of economics and policy evaluation. I am a Professor of Economics and the Ray A. Rothrock '77 Senior Fellow at Texas A&M University. Prior to my appointment as full professor on September 1, 2018, I was an Associate Professor of Economics at Texas A&M beginning in 2013.

7. I have been a Research Associate at the National Bureau of Economic Research (NBER) since 2014, and before that, I was a Faculty Research Fellow at NBER beginning in 2011. NBER is the nation's leading nonprofit economic research organization, studying a wide range of topics, including the effects of various public policies.

8. I received a B.A. in economics in 2004, an M.A. in economics in 2005, and a Ph.D. in economics in 2009—all from the University of California, Davis.

9. I have published 28 research articles in peer-reviewed journals and books. I am a Specialized Co-editor of *Economic Inquiry*, where I determine whether the journal should publish submitted papers in the areas of health economics, public economics, and policy evaluation.

10. My research interests include health economics and issues concerning youth, including the economic effects of abortion and contraceptive policies.

11. I have taught courses on empirical research methods at the undergraduate and Ph.D. levels for 14 years. These courses focus on the quantitative methods that economists use to evaluate the causal effects of government programs and other interventions, how these methods overcome problems that often plague correlational analyses, and the conditions under which these methods are appropriate.

12. A copy of my curriculum vitae setting forth my experience, education, and credentials in greater detail is attached as **Attachment 1**.

## **II. Credibly Evaluating *Causal* Effects**

13. It is generally extremely important to distinguish between correlational studies and rigorous studies of causal effects. The opinions I offer in this declaration are based primarily on the body of evidence on the *causal* effects of laws restricting access to abortion and on the *causal* effects of having children. These opinions are consistent with those described in the Economists' Amicus Brief in *Dobbs v. Jackson Women's Health*, which also emphasized credible studies of causal effects and which I signed along with 153 other distinguished economists.

14. It is common for introductory courses in statistics or the social sciences to explain that "correlation does not imply causation." This is very useful knowledge to convey to students because it cautions them against interpreting all correlations as if they reflect a causal relationship. A little knowledge can be a dangerous thing in this instance, however, because it leads some people to incorrectly believe that it is impossible for researchers to quantify causal effects. For this reason, in all the courses I teach—all of which focus on how to conduct empirical research to quantify causal effects—I explain to my students that correlation does not generally imply causation, but correlation does imply causation under some conditions. Randomized-control-trial experiments, in which researchers randomly assign participants to a treatment group or control group, illustrate this point in an intuitive manner. It is broadly accepted, by the Food and Drug Administration for example, that a correlation between treatment and outcomes *in this particular type of setting* indicates a causal effect.

15. A randomized-control-trial experiment is one approach among many for

evaluating causal effects. To be clear, it is a very powerful approach because it involves a researcher creating conditions such that a correlation between treatment and outcomes (or a difference in average outcomes across the two groups) provides compelling evidence on whether there is a causal effect of treatment on outcomes. This approach can identify a causal effect if the outcomes observed in the control group provide a good counterfactual for the outcomes that would have been observed in the treatment group in the absence of treatment. The act of random assignment (and a large number of participants) ensures that this condition will be met.<sup>1</sup> In such circumstances, we expect the outcomes of the two groups to be extremely similar in the absence of treatment. This is the logic implicit in the widely accepted idea that causal effects are credibly quantified by comparisons of treatment and control groups in researcher-conducted randomized-control-trial experiments.

16. As I mentioned above, there are many other approaches that can also be used to estimate causal effects. These tools are commonly used to evaluate “natural experiments” whereby chance, forces of nature, institutions, or policymakers determine who is treated and who is not treated. These tools have been developed extensively over the past 30 years by econometricians, such that there has been a “credibility revolution” in empirical research aiming to quantify causal effects.<sup>2</sup> Along these lines, the 2021 Nobel Prize in Economics was awarded for “methodological contributions to the analysis of causal relationships.”<sup>3</sup> These methods were also discussed in the Economists’ Amicus Brief. While these tools do not generally recover causal effects, they do under specific conditions.

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<sup>1</sup> Indeed, differences are expected to be zero in expectation and they are expected to shrink to zero in larger and larger samples.

<sup>2</sup> Joshua D. Angrist & Jörn-Steffen Pischke, *The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics*, 24 J. OF ECON. PERSP. 3, 4 (2010).

<sup>3</sup> *The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 2021*, THE NOBEL PRIZE (June 25, 2022), <https://www.nobelprize.org/prizes/economic-sciences/2021/summary/>.

17. The difference-in-differences research design features extensively amongst the causal studies I highlight below. Difference-in-differences research designs are one of the most routinely used approaches to estimating causal effects in the social sciences. This methodological approach is the focus of one of the three chapters in a section titled “The Core” of the popular and seminal Ph.D. level econometrics textbook *Mostly Harmless Econometrics*.<sup>4</sup> In the typical application, this empirical approach involves the comparison of *changes over time* between some treatment group (e.g., a state enacting some new policy regarding abortion) and some comparison group (e.g., a state not changing abortion policies). As such, instead of needing the treatment group and comparison group to have the same outcome *levels* in the absence of treatment, this research design requires that they would have the same *changes over time* in the absence of treatment. Researchers using this methodology in a convincing manner, such that their results can be considered credible estimates of causal effects, provide evidence that this assumption is credible in their particular context.

### **III. Background on Individuals Seeking Abortion**

18. To provide context for the causal studies I review in the next section and what they demonstrate in terms of the consequences of eliminating access to abortion in Kentucky, in this section I discuss the characteristics of individuals seeking abortions across the United States and in Kentucky.

19. Based on 2014 abortion rates, 23.7 percent of women aged 15–44 years in 2014 would be expected to have an abortion by the time they are 45 years old (assuming 2014 abortion rates continue through the time they are 45 years old).<sup>5</sup> 12 percent of women obtaining abortions

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<sup>4</sup> Joshua D. Angrist & Jörn Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion*, PRINCETON UNIV. PRESS 169–182 (2008).

<sup>5</sup> Rachel K. Jones & Jenna Jerman, *Population Group Abortion Rates and Lifetime Incidence of Abortion: United States, 2008–2014*, 107 AM. J. PUB. HEALTH 1904, 1907 (2017).

are less than 20 years old and 60 percent are in their 20s.<sup>6</sup> Women of color are disproportionately represented among American women obtaining abortions. In terms of race, 27.6 percent of women obtaining abortions in 2014 were Black, even though only 14.9 percent of US women aged 15-44 were Black.<sup>7</sup> In terms of ethnicity, 24.8 percent of individuals obtaining abortions in 2014 were Hispanic, even though only 20 percent of US residents were Hispanic.<sup>8</sup>

20. A substantial majority of American women seeking abortions have relatively low incomes.<sup>9</sup> In 2014, half had incomes less than the federal poverty line and three-quarters had incomes less than 200 percent of the poverty line.<sup>10,11</sup> Compounding their financial difficulties, 59 percent had previously given birth and 55 percent were neither married nor cohabiting.<sup>12</sup> Moreover, 55 percent reported having experienced at least one “disruptive life event” during the preceding 12 months, where disruptive life events include the death of a close friend or family member, having a family member with a serious health problem, having a baby, separating from a partner, having a partner arrested or incarcerated, being unemployed for at least one month, falling behind on rent or a mortgage, or moving two or more times.<sup>13</sup>

21. Women’s ability to obtain abortions depends on many factors beyond their control, including the availability of care, the amount of travel required, affordability, and state requirements such as waiting periods.<sup>14</sup> Survey data shows that among women who would have

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<sup>6</sup> *Id.* at 1906.

<sup>7</sup> *Id.*

<sup>8</sup> *Id.*

<sup>9</sup> *Id.* at 1906–1907.

<sup>10</sup> In 2014, the Federal Poverty line was \$12,316 for a single adult, \$16,317 for a family with one adult and one child, and \$19,073 for a family with one adult and two children. The Federal Poverty line was \$15,853 for family of two adults, \$19,055 for a family with two adults and one child, and \$24,008 for a family with two adults and two children. CARMEN DENAVAS-WALT & BERNADETTE D. PROCTOR, U.S. CENSUS BUREAU, INCOME AND POVERTY IN THE UNITED STATES: 2014 43 (2015).

<sup>11</sup> Jones, *supra* note 5, at 1906.

<sup>12</sup> *Id.*

<sup>13</sup> Rachel K. Jones & Jenna Jerman, *Characteristics and Circumstances of U.S. Women Who Obtain Very Early and Second Trimester Abortions*, 12 PLOS ONE 1, 3–4 (2017).

<sup>14</sup> NAT’L ACAD. SCI., THE SAFETY AND QUALITY OF ABORTION CARE IN THE UNITED STATES 12 (2018).



preferred to have obtained their abortions sooner in time, 59 percent report that delays occurred because it took time for them to make arrangements.<sup>15</sup> Consistent with this statistic, empirical evidence indicates that regulations that substantially increase the financial, travel, and/or logistical burdens of obtaining an abortion have a significant effect on abortion access. I discuss this evidence in greater detail below.

22. The economic circumstances in Kentucky relative to the United States suggest that an even larger share of its women would face financial challenges in attempting to obtain an abortion than we would expect based on the statistics described above, which are based on U.S. averages. To put Kentucky's economic conditions in context, in the table below I report 2020 poverty rates calculated by the United States Census Bureau for Kentucky and for the United States as a whole.<sup>16</sup> These statistics highlight both the degree to which Kentucky has a high poverty rate relative to the U.S. average and also the high poverty rate for those in female-headed households with children and no spouse present. In Kentucky, 37.2 percent of people in such households were in poverty. These statistics suggest that individuals seeking abortions in Kentucky may be even more disadvantaged than those seeking abortions nationwide.

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<sup>15</sup> Lawrence B. Finer et al., *Timing of Steps and Reasons for Delays in Obtaining Abortions in the United States*, 74 *CONTRACEPTION* 334, 335 (2006).

<sup>16</sup> The statistics I show were drawn from data sets posted on the Census Bureau website. U.S. CENSUS BUREAU, CURRENT POPULATION SURVEY: DETAILED TABLES FOR POVERTY, [https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46\\_weight\\_10050\\_1.xlsx](https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46_weight_10050_1.xlsx) (last visited June 6, 2022); U.S. CENSUS BUREAU, CURRENT POPULATION SURVEY: DETAILED TABLES FOR POVERTY, [https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46\\_weight\\_10050\\_5.xlsx](https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46_weight_10050_5.xlsx) (last visited June 6, 2022); U.S. CENSUS BUREAU, CURRENT POPULATION SURVEY: DETAILED TABLES FOR POVERTY, [https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46\\_weight\\_10050\\_8.xlsx](https://www2.census.gov/programs-surveys/cps/tables/pov-46/2021/pov46_weight_10050_8.xlsx) (last visited June 6, 2022).

**2020 Poverty Rates (Percent of Population)**

	<u>Kentucky</u>	<u>U.S. Average</u>
Overall	13.9	11.4
Adult non-elderly (18-64)	12.4	10.4
Female-headed household w/ children and no spouse	37.2	33.4

23. Data from the Kentucky Annual Abortion Report for 2020,<sup>17</sup> produced by Kentucky's Department for Public Health and Office of Vital Statistics, confirm that women seeking abortions in Kentucky disproportionately come from groups that are typically economically disadvantaged, as measured by many different characteristics that are strong predictors of poverty. Compared to the broader set of Kentucky residents, they are more likely to be Black, more likely to be Hispanic, more likely to be unmarried, and more likely to have no more than a high school education.

24. 87.2 percent of women obtaining abortions in Kentucky in 2020 were unmarried.<sup>18</sup> This is an extremely large share compared to the share of Kentucky residents over 18 who are unmarried (49.4 percent) and it is even larger compared to the share of Kentucky residents who reported giving birth in the past year who are unmarried (34.5 percent).<sup>19</sup>

25. In terms of race, 34.5 percent of individuals obtaining abortions in Kentucky in 2020 were Black,<sup>20</sup> even though only 8.5 percent of Kentucky residents are Black. In terms of ethnicity, 7.6 percent of individuals obtaining abortions in Kentucky in 2020 were Hispanic,<sup>21</sup> even though only 3.9 percent of Kentucky residents are Hispanic.

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<sup>17</sup> KY. PUB. HEALTH, KY. ANNUAL ABORTION REP. FOR 2020 (2021), <https://chfs.ky.gov/agencies/dph/dehp/vsb/Forms/2020KYAbortionAnnualReport.pdf>.

<sup>18</sup> *Id.* at 4.

<sup>19</sup> Statistics for Kentucky residents are authors calculations based on the 2020: ACS 5-Year Estimates produced by the United States Census Bureau.

<sup>20</sup> *Supra* note 17, at 6.

<sup>21</sup> *Id.* at 5.

26. In 2020, 4,104 people obtained abortions in Kentucky, including 3,487 Kentucky residents.<sup>22</sup> Many of these individuals would typically be considered of schooling age and/or early in their careers in the labor market. Specifically, 366 were under 20 years old, 1,119 were 20-24 years old, and 1,229 were 25-29 years old.<sup>23</sup> As such, many of these individuals are at a stage in their lives such that accessing abortion may determine whether they continue in school or make other early-career investments, both of which affect individuals' economic circumstances throughout their lives and their children's lives.

27. The same statistical report indicates that 66.3 percent of the people who obtained abortions in Kentucky in 2020 had previously given birth.<sup>24</sup> Thus, there is substantial potential for the existing children of individuals seeking abortions to be affected by policies that limit their parents' access to abortion.

**IV. How changes in abortion access for Kentucky residents will translate into fewer abortions and increased childbearing**

28. There is substantial evidence on the causal effects of abortion restrictions on abortion rates and childbearing. Consistent with what we would expect based on economic theory, this evidence routinely shows that abortion is reduced, and childbearing is increased, in circumstances in which abortion access is restricted. This has been demonstrated repeatedly in rigorous studies of causal effects and by many different research teams studying many different contexts. This evidence is also consistent with the broader evidence base on the causal effects of access to health care on health care utilization.

29. Regarding research on laws making abortion illegal altogether, there are rigorous

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<sup>22</sup> *Id.* at 2-3.

<sup>23</sup> *Id.* at 4.

<sup>24</sup> *Id.* at 9.

studies documenting the causal effects of changes that took place in the United States in the 1970s. These studies typically use a difference-in-differences research design to evaluate the effects of altered access, quantifying how outcomes changed over time in states where abortion became legal relative to how outcomes changed over the same period of time in states where the prevailing law did not change. Several research teams have used some version of this methodology using a variety of data sets and a variety of statistical refinements, repeatedly finding that abortion legalization has significant effects on childbearing.<sup>25</sup>

30. These effects are clear in the figure below, which is reproduced from the Economists' Amicus Brief in *Dobbs v. Jackson Women's Health* based on results from Levine et al. 1999.<sup>26</sup> It shows the difference in birth rates between a set of "repeal states" (i.e., five states where abortion became legal in 1970) and the rest of the United States from 1965 to 1980.<sup>27</sup> In so doing, it captures two state-level "natural experiments" on the effects of abortion legalization on birth rates. The first occurred in 1970, when abortion became legal in the five repeal states, while it remained illegal in the rest of the United States, which can thus be used for comparison to evaluate this first natural experiment. The second natural experiment occurred in 1973, when *Roe v. Wade* made abortion legal in the rest of the United States, while it remained legal in the repeal states, which can thus be used for comparison.

31. Reading the evidence in the figure from the earliest years to the latest years depicted, it first demonstrates that the difference in birth rates between the repeal states and the

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<sup>25</sup> See, e.g., Phillip B. Levine et al., *Roe v Wade and American Fertility*, 89 AM. J. OF PUB. HEALTH 199 (1999); Jonathan Gruber et al., *Abortion Legalization and Child Living Circumstances: Who Is the 'Marginal Child'?*, 114 Q. J. OF ECON. 263 (1999); Caitlin Knowles Myers, *The Power of Abortion Policy: Reexamining the Effects of Young Women's Access to Reproductive Control*, 125 J. OF POL. ECON. 2178 (2017); Kelly Jones, *At a Crossroads: The Impact of Abortion Access on Future Economic Outcomes* (AM. U. WORKING PAPER, 2021), <https://doi.org/10.17606/0Q51-0R11>.

<sup>26</sup> Levine et al, *supra* note 25.

<sup>27</sup> Consistent with the difference-in-differences design, the 1970 difference is subtracted from the difference observed in all years. As such, the figure shows differences in all years relative to the difference observed in 1970.

rest of the United States was fairly constant from 1965 until 1970, a time period in which abortion was not legal in any state. Then, after abortion became legal in the repeal states, birth rates fell substantially in those states relative to other states, such that their birth rate was 5 percent lower from 1971 to 1973 (relative to the 1970 difference). As such, the first natural experiment indicates that making abortion legal in the repeal states reduced birth rates in those states. Alternatively, the evidence can be thought of as indicating that birth rates are increased if abortion is illegal.

32. Then after *Roe v. Wade* made abortion legal in the other states, their birth rates fell relative to the repeal states, such that repeal-states-minus-other-states difference that emerged from 1971-1973 had vanished by 1976. As such, the second natural experiment indicates that making abortion legal in the rest of the United States decreased birth rates in those states. Alternatively, the evidence can be thought of as indicating that birth rates are increased if abortion is illegal.

**Figure 1: Trends in birth rates in repeal states relative to the rest of the country**



33. Studies examining abortion legalization during this era also show that the effects of childbearing are especially large for non-white women.<sup>28</sup> Moreover, researchers have repeatedly documented significant effects on childbearing among teenagers and women in their early twenties.<sup>29</sup> Estimates from Myers indicate that legalizing abortion and allowing young women to obtain an abortion without parental consent reduced teen motherhood by 34 percent and reduced teen marriage by 20 percent.<sup>30</sup>

34. The aforementioned studies documenting causal effects of state bans on abortion are also consistent with rigorous research documenting the causal effects of changes in access that have taken place more recently.

35. The most exhaustive research on recent changes in access to abortion providers comes from studies that have investigated Texas's regulatory environment, in which a 2013 law (Texas HB-2) caused nearly half the clinics in the state to stop providing abortions. This scenario offered an ideal setting for research because of the sheer magnitude of the "natural experiment," and the large population that it affected, both of which are helpful in obtaining more precise estimates of the effects of abortion regulations on abortion rates and associated outcomes, such as delays in ability to obtain care and associated births. It can be thought of as a natural experiment because it was similar to a clinical trial in the sense that a "treatment" (abortion clinic access) was altered by an external force (i.e., a regulation leading to certain clinics being unable to provide abortions). Studying Texas HB-2 has allowed researchers to learn about the effects of diminished abortion clinic access by comparing counties experiencing

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<sup>28</sup> See, e.g., Levine et al., *supra* note 25; Joshua D. Angrist & William N. Evans, *Schooling and Labor Market Consequences of the 1970 State Abortion Reforms*, NAT'L BUREAU OF ECON. RESEARCH WORKING PAPER 18 (1996); Gruber et al., *supra* note 25; Myers, *supra* note 25; Jones, *supra* note 25.

<sup>29</sup> See, e.g., Levine, *supra* note 25; Angrist & Evans, *supra* note 28; Myers, *supra* note 25; Jones, *supra* note 25.

<sup>30</sup> Myers, *supra* note 25.

large changes in abortion clinic access to counties experiencing smaller (or no) changes in clinic access.

36. Researchers frequently measure access to abortion clinics based on the distance to the nearest clinic. Naturally, the nearest abortion clinic may not be able to serve all women because it may not provide all types of abortions and may not have the capacity to meet demand. Moreover, some women seeking abortion may opt for more distant clinics because of other considerations, e.g., proximity to family, access via public transportation, etc. As such, distance to the nearest clinic is thought of by researchers as a “proxy variable” that provides a useful measure of abortion clinic access.

37. Three separate research teams have rigorously evaluated how the clinic closures precipitated by Texas HB-2 affected travel distance and how these impacts on travel distance affected abortion rates: Quast, Gonzalez, and Ziemba (2017),<sup>31</sup> Fischer, Royer, and White (2018),<sup>32</sup> and Lindo et al. (2020).<sup>33</sup> The credibility of this body of research is bolstered by the fact that each of the independent research teams chose to use similar (though not identical) research methods and similar (though not identical) data—and all reached very similar conclusions.<sup>34</sup> All three determined that increases in distance to the nearest clinic caused by regulation-induced clinic closures caused significant reductions in abortions obtained from medical professionals.

38. A graphic summarizing the estimated effects of regulation-induced increases in

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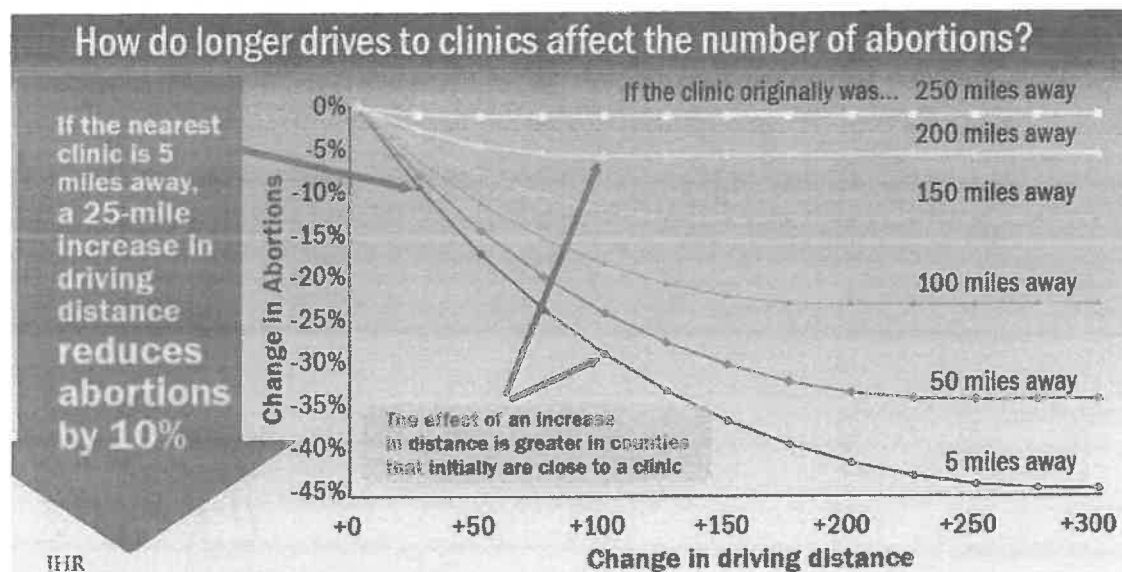
<sup>31</sup> See generally Troy Quast et al., *Abortion Facility Closings and Abortion Rates in Texas*, 54 J. HEALTH CARE ORG., PROVISION, FIN. 1 (2017)..

<sup>32</sup> See generally Stefanie Fischer et al., *The Impacts of Reduced Access to Abortion and Family Planning Services on Abortions, Births, and Contraceptive Purchases*, 167 J. PUB. ECON. 43 (2018).

<sup>33</sup> Jason Lindo et al., *How Far is Too Far? New Evidence on Abortion Clinic Closures, Access and Abortions*, 55 J. HUM. RESOURCES 1137 (2020).

<sup>34</sup> Some of the differences include the way that clinic operations were measured, the years of data that were used to measure outcomes, and the specific statistical adjustments that were made for changes in county characteristics over time.

travel distance from Lindo et al. is provided below. It demonstrates that increases in travel distance have significant effects for women initially living within 200 miles of a clinic, and that the largest effect is on those initially nearest to clinics for whom a 25-mile increase (one-way) reduces abortion rates by ten percent.



39. The estimated effects reported in Fischer et al. also indicate substantial effects of travel distance on abortion rates, though their estimates are not directly comparable to those reported in Lindo et al.<sup>35</sup> The estimated effects in Quast et al. are smaller in magnitude than

<sup>35</sup> Fischer et al. report estimates of the effects of having versus not having a clinic within 25, 50, and 100 miles in Panel A of Table 3. The estimates reported in their table, when correctly converted into percent effects, find abortions fall by 15.2–19.7 percent for counties that move from having a clinic within 25 miles to none within 25 miles; by 15.4–20.1 percent for counties that move from having a clinic within 50 miles to none within 50 miles; and by 19.8–30.2 percent for counties that move from having a clinic within 100 miles to none within 100 miles. However, the percent estimates described in the text of Fischer et al. are incorrect, because the authors have calculated percent effects from their model coefficients by multiplying them by 100, when percent effects from a Poisson regression model should be calculated by exponentiating the coefficient, subtracting one, and then multiplying by 100. That is, they calculate percent effects as  $100 \times \text{coefficient}$  when they should be calculated as  $100 \times (e^{\text{coefficient}} - 1)$ . I also note Fischer et al. describe their preferred estimates as derived from a model that controls for regional trends. Given that increases in distance are almost certainly affected by regional trends, it is inappropriate to control for such trends. In particular, controlling for such trends makes it such that the distance variables will not fully capture the effects of increases in distance. For this reason, their “preferred estimates” are likely to understate the true effects of increases in travel distance on abortion access. Fischer, *supra* note 32 at 51.



those in Lindo et al., but Quast et al. notably foreshadowed that subsequent studies using better data would find larger effects.<sup>36</sup>

40. Researchers have also documented significant effects of travel distance using a similar research design applied to evaluate the effects of a regulation in Wisconsin, which caused two out of the five clinics in the state to close and increased the distance that individuals had to travel to reach their nearest clinic. Venator and Fletcher<sup>37</sup> found that a one-hundred-mile increase in distance to the nearest clinic led to 31 percent fewer abortions and three percent more births.<sup>38</sup>

41. Moreover, research has also documented significant effects of travel distance using a similar research design to evaluate changes in travel distances occurring across all U.S. counties from 2009 to 2020 resulting from changes in provider operations. In particular, Myers<sup>39</sup> finds that a hundred-mile increase in distance to the nearest clinic reduces abortions by 20.5 percent and increases births by 2.4 percent.

42. Combined with the aforementioned research on abortion legalization in the 1970s, this research highlights that abortion rates and births are significantly affected by abortion restrictions, even in circumstances where some individuals are able to access out-of-state abortion providers. Based on Guttmacher Institute analyses of their regulatory environments,

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<sup>36</sup> In Quast et al.'s own words, in their study "[a] facility was classified as operating in a given year if its license was effective for at least 6 months. Using license dates may overstate the period during which a facility was in operation. Specifically, a clinic may have ceased performing abortions even though its license was in effect. *These instances would attenuate the regression coefficients we estimate*" (emphasis added). Quast, *supra* note 31 at 2. Lindo et al. and Fischer et al. both used improved data on clinic operations that account for the fact that many clinics were forced to close before their licenses were set to expire.

<sup>37</sup> Joanna Venator & Jason Fletcher, *Undue Burden Beyond Texas: An Analysis of Abortion Clinic Closures, Births, and Abortions in Wisconsin*, 278 J. POLICY ANALYSIS AND MANAGEMENT (2021).

<sup>38</sup> A smaller percent effect on births than on abortions is expected due to the fact that a relatively large share of pregnancies are carried to birth versus ending via an abortion. For example, suppose there are 100 pregnancies, 20 of which will end in abortion and 80 will end with childbirth. If restricted abortion access causes a 20 percent reduction in abortions, that would correspond to four fewer abortions (20 percent times 20 abortions initially) and four additional births. Four additional births represents a 5 percent increase (4 more births divided by 80 births initially times 100 percent).

<sup>39</sup> Caitlin Myers, *Measuring the Burden: The Effect of Travel Distance on Abortions and Births*, IZA Working Paper 14556 (2021).

most of the states around Kentucky have restrictive abortion policies in place<sup>40</sup> and most of the states around Kentucky are likely to ban or restrict access to abortion in the near future.<sup>41</sup> This information is depicted in the maps below.



<sup>40</sup> *Abortion after Roe: New Comprehensive Map Tracks Abortion Policies and Statistics for each State, June 9, 2022*, GUTTMACHER INST., <https://www.guttmacher.org/news-release/2022/abortion-after-ro-e-new-comprehensive-map-tracks-abortion-policies-and-statistics>.

<sup>41</sup> *Id.* at 1.



nearest hospital, Buchmeuller, Jacobson, and Wold<sup>44</sup> found that increases in distance resulting from hospital closures shifted care away from emergency rooms and outpatient clinics to doctors' offices, leading to significant increases in deaths from unintentional injuries and heart attacks.

46. In a study of children with varying degrees of hospital access, Currie and Reagan found that distance to a hospital has significant effects on medical checkups for Black children.<sup>45</sup> They found that each additional mile a child must travel to access medical care reduced the probability of that child having had a checkup in the past year by three percentage points (from a mean baseline of 74 percent).<sup>46</sup>

47. In another study examining the role of travel distance, Kelly, Lindo, and Packham found that the Colorado Family Planning Initiative, which expanded women's access to intrauterine devices (IUDs) and contraceptive implants at family planning clinics, significantly reduced teen birth rates, but only for those living close to a clinic.<sup>47</sup> Specifically, the effects were concentrated among women living in zip codes within 7 miles of a clinic, with only some modest evidence of the program having effects on women living 7 to 12 miles from a clinic, and no evidence of effects on women living more than 12 miles from a clinic.<sup>48</sup> The study's results highlight that expanding access to readily available health care can increase health benefits for individuals, but shows how those benefits may be limited by travel distance.<sup>49</sup>

48. Recent research conducted by the Kentucky Department for Public Health

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<sup>44</sup> Thomas C. Buchmueller et al., *How Far to the Hospital?: The Effect of Hospital Closures on Access to Care*, 25 J. HEALTH ECON. 740, 759 (2006).

<sup>45</sup> Janet Currie & Patricia B. Reagan, *Distance to Hospital and Children's Use of Preventative Care: Is Being Closer Better, and for Whom?*, 41 ECON. INQUIRY 378, 378–79 (2003).

<sup>46</sup> *Id.*

<sup>47</sup> Andrea M. Kelly et al., *The Power of the IUD: Effects of Expanding Access to Contraception Through Title X Clinics*, 192 J. PUBLIC ECON. 1, 2 (2020).

<sup>48</sup> *Id.* at 27.

<sup>49</sup> *Id.*

indicates that a lack of transportation and low incomes are also important barriers to health care access in Kentucky. Specifically, the Kentucky Department for Public Health's Kentucky Primary Care Office administered The Primary Care Needs Survey from December 2020 to January 2021 as a part of its research process for producing its 2021 Needs Assessment Report.<sup>50</sup> In so doing, they collected information from 261 individuals from state and local partners, health departments, and other groups involved in health care.<sup>51</sup> As described in the 2021 Needs Assessment Report: "[d]ata collected include[d] perceptions about primary care needs, populations facing health disparities, health care access, and workforce concerns in Kentucky."<sup>52</sup>

49. In response to a question about the greatest barriers that patients face when accessing care in the communities where they work, respondents reported transportation far more than anything else: 64 percent of respondents reported that transportation was among the greatest barriers for patients accessing care<sup>53</sup>. The second most cited barrier was that patients could not afford care (31 percent).<sup>54</sup>

50. The same survey also highlights that low-income populations and racial/ethnic minorities face the greatest health disparities. Respondents reported that low income populations face health disparities more than any other group (28 percent).<sup>55</sup> The second most cited group was racial/ethnic minorities (21 percent).<sup>56</sup> As such, these statistics indicate that the same groups of individuals who will be disproportionately affected by Kentucky's abortion ban are already disadvantaged in terms of health disparities.

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<sup>50</sup> 2021 NEEDS ASSESSMENT REPORT, Kentucky Department for Public Health, Kentucky Primary Care Office (2021).

<sup>51</sup> *Id.* at 14.

<sup>52</sup> *Id.* at 6.

<sup>53</sup> *Id.* at 19.

<sup>54</sup> *Id.*

<sup>55</sup> *Id.* at 18.

<sup>56</sup> *Id.*

## V. Economic (and other) effects of restricted access to abortion and childbearing

51. In discussing the effects of Kentucky's ban, I consider three categories of individuals who would obtain abortions in Kentucky in the absence of a ban: (i) those who still have an abortion, travel farther to do so out of state, and do not experience any delays; (ii) those who still have an abortion, travel farther to do so out of state, and experience delays as a result; and (iii) those who are prevented from having an abortion.<sup>57</sup>

52. The first group (those who still have an abortion around the same time they would otherwise, but they travel farther to do so) will suffer economic harm because of financial costs associated with additional travel, including transportation costs, and possibly including lodging costs, lost wages, and/or childcare costs.

53. The second group (those who still have an abortion, but they travel farther and are delayed in so doing) will suffer the same economic harms associated with travel in addition to harms associated with delaying their abortions. Delays can limit the set of clinics that can serve an individual, the types of procedures available to them, and the costs of the procedure. A one-week delay can, for example, increase the cost of obtaining an abortion by up to \$502.<sup>58</sup>

54. Health risks also tend to be higher for women obtaining later abortions. Though the major-complication rate (where major complications are defined as those requiring hospital admission, surgery, or blood transfusion) remains low throughout pregnancy, it increases over time. It is 0.16 percent for first-trimester aspiration abortion, and 0.41 percent for second-

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<sup>57</sup> Note that other individuals seeking abortion may be affected as well. In particular, individuals who would typically have abortions in other states may face limited appointment availability as a result of increased pressure due to a lack of service provision in Kentucky.

<sup>58</sup> Jason M. Lindo & Mayra Pineda-Torres, *New Evidence on the Effects of Mandatory Waiting Periods for Abortion*, 80 J. HEALTH ECONOMICS (2021).

trimester or later procedures.<sup>59</sup>

55. Regarding both the first and second groups of individuals, survey data also indicate that delays and additional travel requirements impose financial *and emotional burdens*. For example, among abortion patients surveyed in Texas, 31 percent of women reported that the state's 24-hour mandatory waiting period and two-trip requirement had a negative effect on their emotional well-being.<sup>60</sup> Among abortion patients in Utah, 62 percent reported that the 72-hour waiting period and two-trip requirement affected them negatively in some way, including 47 percent reporting lost wages from needing to take extra time off work, 30 percent reporting increased transportation costs, 27 percent reporting lost wages by family or friends, and 33 percent reporting that they had to disclose their abortion to someone who they would not have told if there were no waiting period.<sup>61</sup> Women in Louisiana reported similar challenges associated with travel, highlighting concerns about missing work, encountering traffic or bad weather, thinking their car would not be able to make the trip, and having to lie about their absence to their parents or partners.<sup>62</sup> Some of these women also reported that challenges making arrangements, combined with the mandatory delay and two-trip requirement, resulted in them being unable to obtain their preferred abortion procedure and/or made them worry that they would have to continue an unwanted pregnancy. Notably, these surveys do not include individuals who were unable to obtain abortions and, thus, likely understate the burdens imposed on individuals interested in abortion.

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<sup>59</sup> Ushma D. Upadhyay et al., *Incidence of Emergency Department Visits and Complications After Abortion*, 125 OBSTETRICS & GYNECOLOGY 175, 181 (2015).

<sup>60</sup> TEX. POLICY EVALUATION PROJECT, IMPACT OF ABORTION RESTRICTIONS IN TEXAS 1 (2014).

<sup>61</sup> Jessica N. Sanders et al., *The Longest Wait: Examining the Impact of Utah's 72-Hour Waiting Period for Abortion*, 26 WOMEN HEALTH ISSUES 483, 485 (2016).

<sup>62</sup> Erin Carroll & Kari White, *Abortion patients' preferences for care and experiences accessing services in Louisiana*, 2 CONTRACEPTION: X 1, 3 (2020) [hereinafter, Carroll, *Abortion patients' preferences*].

56. The third group of individuals (those who are prevented from having an abortion altogether) are likely to be the most disadvantaged. Being prevented from having an abortion for these individuals can mean having a child earlier than they otherwise would and or having more children than they otherwise would. Each of these consequences of impaired abortion access involve substantial costs.

57. It is also well established that continuing a pregnancy to childbirth poses greater short-term health risks than having an abortion.<sup>63</sup> Thus, individuals who are prevented from having an abortion due to restricted access also face greater health risks as a result.

58. In terms of the overall economic costs of having a child, some are obvious because they involve monetary expenditures, and some are less obvious because they involve lost earnings or impaired earnings potential due to the fact that having a child is time consuming.

59. Expenditures associated with pregnancy and delivery can include medical costs for some individuals (e.g., those who are uninsured who are disproportionately likely to be people of color) that can be substantial. Indeed, the risk of catastrophic health expenditures (spending greater than 10% of family income in a year) is significantly higher for those giving birth than it is for similar non-pregnant reproductive-aged individuals.<sup>64</sup> And this risk is particularly high for low-income individuals giving birth.<sup>65</sup> Other costs besides direct medical expenses include transportation costs and childcare costs associated with medical care and other activities typically done in advance of having a child (such as parenting classes and shopping). These costs—particularly at a time when a new member is being added to the household—can

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<sup>63</sup> Elizabeth G. Raymond & David A. Grimes, *The Comparative Safety of Legal Induced Abortion and Childbirth in the United States*, 119 *OBSTETRICS & GYNECOLOGY* 215, 216–17 (2012).

<sup>64</sup> Jessica Peterson et. al., *Catastrophic Health Expenditures With Pregnancy and Delivery in the United States*, 139 *OBSTETRICS & GYNECOLOGY* 509–520 (2022).

<sup>65</sup> *Id.*



push individuals further into poverty.

60. Child-rearing expenses include housing, food, transportation, clothing, health care, childcare, and many miscellaneous expenses. Lino et al.<sup>66</sup> estimates that average household expenditures on a first child exceed \$11,000 annually for middle-income married-couple families, for low-income married-couple families, and for low-income single-parent families.<sup>67</sup> Lino et al. estimates that average household expenditures on a *second* child total over \$170,000 from the birth of that child through age 17 for low-income households.<sup>68</sup> Moreover, these expenditures are extremely similar for single-parent households and married-couple households, even though single-parent households have one fewer potential earner and much lower income on average. As a result, child-rearing expenses consume a greater percentage of income for single-parent families and, thus, an additional child for such a family will have an especially large impact on the proportion of income that remains available to meet the needs of other family members. As I described above, a substantial share of individuals seeking abortion are already in poverty. Adding a child to such a household without substantially expanding their resources will thrust such an individual deeper into poverty. Given the highly persistent nature of economic circumstances, this is likely to affect the individual for their entire life.

61. In addition, pregnancy, childbearing, and childrearing are extremely time consuming. This can make it difficult for people to continue in school, to make other investments in their careers, to work as many hours as they would like, to maintain jobs, to look for work, etc.

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<sup>66</sup> Mark Lino et al. "Expenditures on Children by Families, 2015" UNITED STATES DEPARTMENT OF AGRICULTURE, CENTER FOR NUTRITION POLICY AND PROMOTION MISCELLANEOUS REPORT NO. 1528-2015 (2017).

<sup>67</sup> Lino et al. define the middle-income group as those in the middle tercile of the before-tax income distribution, or those with income between \$59,200 and \$107,400. The low-income group is comprised of households in the lowest tercile of this income distribution, or those with income less than \$59,200. All numbers referenced in this paragraph are in 2015 dollars. Prices have risen substantially since 2015 due to inflation, especially childcare and housing prices.

<sup>68</sup> *Id.*

Thus, the time costs associated with pregnancy, childbearing, and childrearing can affect an individual's financial resources in the short run and in the long run. As a result of these costs and childrearing costs, having a child earlier than planned or having a child that was not planned can cause irreparable economic harm by putting an individual on an entirely different life course in which they have more limited resources (possibly on top of having another child to provide for).

62. Many carefully designed studies have quantified such effects. Miller et al<sup>69</sup> used data from the Turnaway Study, which collected data on individuals seeking abortions at 30 abortion providers across the United States from 2008 to 2010, including individuals who were (i) no more than two weeks below the gestational age limit (who were thus able to have an abortion at that clinic), and (ii) individuals who were up to three weeks past the gestational age limit of the clinic (who were thus *unable* to obtain an abortion at that clinic). Notably, all of those in category (i) obtained an abortion and thus did not carry the pregnancy to childbirth, whereas 68 percent of those in category (ii) carried the pregnancy through childbirth. The other 32 percent of these individuals either obtained an abortion elsewhere or had a miscarriage.

63. Several studies have reported how outcomes differed across these two groups of individuals in the Turnaway Study.<sup>70</sup> Miller et al combines data from the Turnaway Study with Experian credit report data from 2006 to 2016. These data made it possible for Miller et al. to use cutting-edge methods for estimating causal effects, which account for systematic differences

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<sup>69</sup> Sarah Miller et. al., *Economic Consequences of Being Denied an Abortion*, Am. ECON. J.: ECON. POL'Y, (Forthcoming) 1, 5 (2021).

<sup>70</sup> BM. Antonia Biggs, Ushma D. Upadhyay, Charles E. McCulloch & Diana G. Foster, *Women's Mental Health and Well-being 5 Years After Receiving or Being Denied an Abortion: A Prospective, Longitudinal Cohort Study*, 74 JAMA PSYCHIATRY 169, 169-178 (2017); Diana Greene Foster, M. Antonia Biggs, Lauren Ralph, Caitlin Gerdtz, Sarah Roberts & M. Maria Glymour, *Socioeconomic Outcomes of Women Who Receive and Women Who Are Denied Wanted Abortions in the United States*, 108 AM. J. OF PUB. HEALTH 407, 407-413 (2018); Diana G. Foster, Sarah E. Raifman, Jessica D. Gipson, Corinne H. Rocca & M. Antonia Biggs, *Effects of Carrying an Unwanted Pregnancy to Term on Women's Existing Children*, 205 THE J. OF PEDIATRICS 183, 183-89 (2019).

between the two groups (besides their ability to obtain an abortion) that might cause their outcomes to differ. Moreover, Miller et al. present strong evidence that these methods are appropriate for the population and outcomes considered.

64. Miller et al. use multiple approaches to estimating the causal effect of being denied an abortion (i.e., being unable to obtain an abortion at the clinic at which they presented for care). They focus primarily on difference-in-differences estimates, which capture how outcomes change over time for denied individuals relative to those who were able to obtain abortions at the clinic they presented to.<sup>71</sup> The economic outcomes are measured using Experian credit report data from 2006 to 2016.<sup>72</sup> To measure financial distress, they examine: the amount of debt sent to a third-party collection agency; delinquent debt (i.e., debt that is 30 or more days past due on open accounts); the number of public records from courts, including bankruptcies, tax liens, and evictions; and whether the individual has a credit score at or below 600, which is considered “subprime” and thus reflects a poor credit history.<sup>73</sup> They use standard methodology to combine these data into a summary measure of financial distress, which they refer to as a “financial distress index.”<sup>74</sup>

65. As I noted above, Miller et al. compare outcomes for the two groups over time. To account for the fact that individuals in the data set presented at clinics at different times between 2008 and 2010, they harmonize the data by defining an “event time” for each person.<sup>75</sup> For those continuing their pregnancy and delivering a child, event time is 0 during the month of

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<sup>71</sup> In the paper, they refer to “event study” estimates as well as “difference-in-differences” estimates. The estimates they describe as “event-study estimates” are a *set* of difference-in-differences estimates capturing the effects over time. The estimates they describe type of difference-in-differences capture the average effect across the five years individuals are observed after a childbirth would have been expected if they carried the pregnancy to term. Miller, *supra* note 69.

<sup>72</sup> *Id.* at 14.

<sup>73</sup> *Id.* at 17.

<sup>74</sup> *Id.* at 18.

<sup>75</sup> *Id.* at 13.

birth and the following 11 months, it is 1 in the following 12 months, it is 2 in the subsequent 12 months, etc.<sup>76</sup> And similarly, event time -1 represents the 12 months leading up to the delivery, -2 represents the preceding 12 months, etc. For those who do not deliver a child, event time is 0 in the month they would have been expected to have a child assuming a 40-week pregnancy and in the following 11 months, and the other event times are constructed in reference to this time period.<sup>77</sup> Given this construction, event time -1 corresponds to the year in which these individuals presented at the clinic intending to have an abortion.<sup>78</sup>

66. Miller et al.'s analyses demonstrate that the two groups of individuals had very similar levels of financial distress up to the year in which they presented at the clinic intending to have an abortion (event times -3, -2, and -1).<sup>79</sup> The outcomes then diverge the following year, with an increase in financial distress for those who were denied abortions at the clinic.<sup>80</sup> This difference in financial distress continues to be evident for the entire five years for which the individuals are observed. A graphic from the paper showing this pattern of estimates is presented below.<sup>81</sup>

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<sup>76</sup> *Id.* at 14.

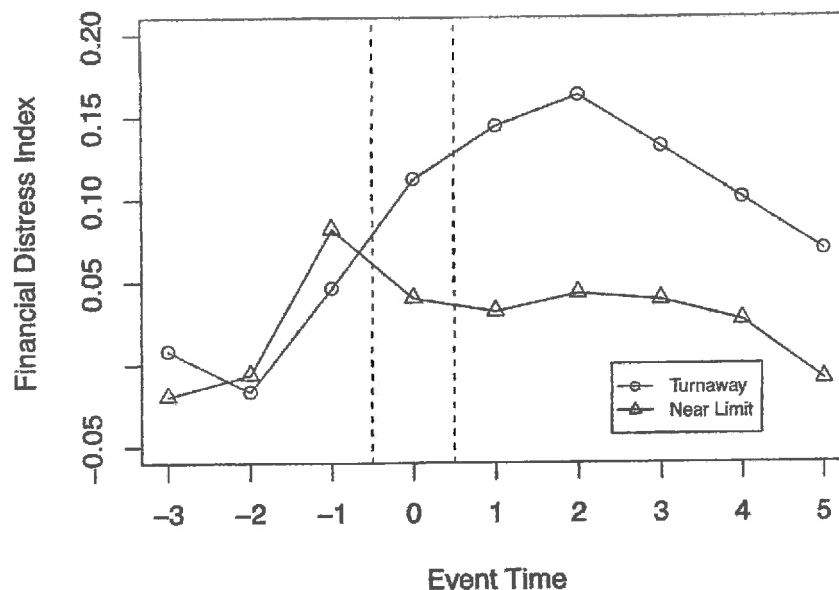
<sup>77</sup> *Id.*

<sup>78</sup> *See id.*

<sup>79</sup> *Id.* at 19.

<sup>80</sup> *Id.*

<sup>81</sup> *Id.* at 40.



67. Miller et al.'s difference-in-differences estimates similarly indicate that the abortion denial significantly increased the financial distress index. Analyses of the subcategories contributing to this index indicate that the abortion denial increased past-due debt by an average of \$1,750 and increased the number of negative public records on credit reports (such as bankruptcy, evictions, and tax liens) by an average of 0.07 over five years.<sup>82</sup> Miller et al. also examine measures of credit access and self-sufficiency. They report that their estimates for these outcomes suggest that being denied an abortion reduces credit access and self-sufficiency, particularly in the years immediately following the denial, but note that these estimates are not always statistically significant.<sup>83</sup>

68. To put the magnitude of these estimated effects into context, Miller et al. compare their findings to rigorous studies of causal effects. In so doing, they report that: "[t]he impact of being denied an abortion on collections is as large as the effect of being evicted (Humphries et

<sup>82</sup> *Id.* at 4.

<sup>83</sup> *Id.* at 4-5.

al., 2019) and the impact on unpaid bills is several times larger than the effect of losing health insurance (Argys et al., 2019). Although imprecisely estimated in our setting, it appears that denying a woman an abortion reduces her credit score by more than the impact of a health shock resulting in a hospitalization (Dobkin et al., 2018) or being exposed to high levels of flooding following Hurricane Harvey (Billings, Gallagher and Ricketts, 2019).”<sup>84</sup>

69. Miller et al. also report estimates based on a regression discontinuity design. This approach to estimating causal inference leverages the idea that the “treatment group” (those denied an abortion because they were past the clinic’s gestational age limit) and the comparison group (those *not* denied an abortion because they were not past the clinic’s gestational age limit) are more and more similar as one restricts attention to individuals who are closer and closer to the gestational age limit.<sup>85</sup> In the limiting case, this involves a comparison of individuals presenting at the clinic on the last day on which the clinic can provide an abortion versus individuals who arrive one day later and past its gestational age limit.<sup>86</sup> The results from the regression-discontinuity-design analyses are consistent with the results from the difference-in-differences analyses.<sup>87</sup>

70. Miller et al. also use a difference-in-differences approach to analyze survey data collected as a part of the Turnaway Study.<sup>88</sup> The nature of these data is such that they cannot examine outcomes before individuals presented for abortion care. As such, they cannot construct difference-in-differences estimates that compare how outcomes change following this encounter relative to *before* the encounter. Instead, they examine changes in survey outcomes over time

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<sup>84</sup> *Id.* at 36.

<sup>85</sup> *Id.* at 28.

<sup>86</sup> *Id.*

<sup>87</sup> *Id.* at 31.

<sup>88</sup> *Id.*

from an initial survey, approximately one week after individuals presented for care. The results from these analyses indicate that the abortion denial led to increases in the number of children in the household without any increase in personal or household income. Indeed, the estimates indicate that the abortion denial reduced monthly personal income by 6.7 percent and reduced household income by 5.5 percent.<sup>89</sup> Though these estimates are not statistically significant, they are consistent with a broader literature comprised of rigorous studies of causal effects that has repeatedly documented large and persistent reductions in earnings caused by childbearing.<sup>90</sup>

71. To measure financial strain, it is necessary to account for needs as well as income. For this reason, researchers typically construct a measure of household's resources *relative to its needs* using federal poverty levels produced by the Department of Health and Human Services, which vary based on the number of adults and children in the household. Miller et al. find that being denied an abortion reduced income relative to the federal poverty level by 28 percentage points on average.<sup>91</sup> This estimate was statistically significant.<sup>92</sup>

72. The Miller et al. study is an extremely impressive work, made possible by combining a unique data set on individuals seeking abortion with credit report data, which provides important insights into the effects of access to abortion. Even though common-sense logic implies that impaired access to abortion will strain resources, this study sheds light on how that happens and by how much. Nonetheless, it is important to keep in mind that the sample of individuals is not representative of all individuals seeking abortion because of its focus on individuals presenting at abortion clinics near its gestational age limit who have prior credit

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<sup>89</sup> *Id.* at 90.

<sup>90</sup> *Id.* at 2 (citing Aguero and Marks, 2008; Adda, Dustmann and Stevens, 2017; Kleven, Landaïs and Sogaard, 2019; Sandler and Szembrot, 2019).

<sup>91</sup> *Id.* at 32.

<sup>92</sup> *Id.* at 75.

histories. Indeed, Miller et al. excludes young individuals from their sample (i.e., those who would have been less than 20 years old at childbirth if the pregnancy was carried to term) “to avoid including [in] the selected group of individuals who were teenagers in the pre-period and thus less likely to appear in credit report data.”<sup>93</sup> Naturally, this means that this work does not capture the effects on individuals without credit histories when they were seeking an abortion and individuals who were still in high school. Notably, 11.9 percent of U.S. women obtaining abortions in 2014 were under age 20.<sup>94</sup>

73. Another strand of literature has examined how state policy changes altering abortion access affected the socioeconomic outcomes for the general population of women in the state, which can be measured using very large data sets. These studies typically use a difference-in-differences research design to evaluate the effects of altered access, focusing on access measured when individuals were teenagers. Specifically, studies in this literature examine how outcomes change across *cohorts* of women in response to changes in abortion access across cohorts. To do so, they evaluate how outcomes changed across birth cohorts living in areas where abortion access was altered during the time under consideration (such that different birth cohorts had different access to abortion) relative to how outcomes changed across the same birth cohorts in other areas where abortion access was not altered during the time under consideration (such that different birth cohorts had the same access to abortion). The power of this approach is that it accounts for changes in outcomes that are expected to occur across cohorts in the absence of changes in abortion access, based on the how outcomes change across cohorts in places where abortion access does not change.

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<sup>93</sup> *Id.* at 25.

<sup>94</sup> Jenna Jerman, Rachel K. Jones & Tsuyoshi Onda, *Characteristics of U.S. Abortion Patients in 2014 and Changes Since 2008*, GUTTMACHER INSTITUTE, N.Y., 6 (2016).



74. Three separate research teams have used this general approach to examine the effects of state abortion bans (in place in the 1960s and early 1970s) on women's educational and economic outcomes: Angrist and Evans,<sup>95</sup> Lindo et al.,<sup>96</sup> and Jones.<sup>97</sup> All three of these studies find that a state ban on abortion has deleterious effects on residents' education and economic outcomes. Specifically, all three studies find that legal access to abortion in an individual's state of residence during youth significantly increases educational attainment among Black women.<sup>98</sup> Angrist and Evans and Jones also find that it increases subsequent employment among Black women.<sup>99</sup> Jones additionally finds that it increases the probability that an individual ends up in a professional career or managerial role, it increases individual earnings and family income, and it decreases poverty and receipt of public assistance for Black women.<sup>100</sup> The credibility of this body of research generally is bolstered by the fact that each of the independent research teams chose to use similar (though not identical) research methods, similar (though not identical) data, and all reached very similar conclusions.<sup>101</sup>

75. Recent work has also used this type of methodology to investigate the effects of more-recent state laws that have altered access to abortion. In particular, Jones and Pineda-Torres<sup>102</sup> examine the effects of impaired access to abortion during youth resulting from state targeted-regulations on abortion providers ("TRAP Laws"), implemented by twenty-one states

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<sup>95</sup> Joshua D. Angrist & William N. Evans, *Schooling and Labor Market Consequences of the 1970 State Abortion Reforms*, 18 RSCH. IN LAB. ECON. 75, 75-113 (2000).

<sup>96</sup> Jason M. Lindo et al., *Legal Access to Reproductive Control Technology, Women's Education, and Earnings Approaching Retirement*, 110 AEA PAPERS & PROC. 231, 234 (2020).

<sup>97</sup> Kelly Jones, *At a Crossroads: The Impact of Abortion Access on Future Economic Outcomes*, (Am. Univ., Working Paper No. 2021-02, 2021), <https://doi.org/10.17606/0Q51-0R11>.

<sup>98</sup> *Id.*

<sup>99</sup> *Id.*

<sup>100</sup> Jones, *supra* note 97.

<sup>101</sup> See Angrist & Evans, *supra* note 95; Lindo et al., *supra* note 96; Jones, *supra* note 97.

<sup>102</sup> Kelly M. Jones & Mayra Pineda-Torres, *TRAP'd Teens: Impacts of Abortion Provider Regulations on Fertility & Education*, (IZA INSTITUTE OF LABOR ECONOMICS DP NO. 14837, 2021).

since 1994. They find that impaired access resulting from these laws caused a significant increase in births and reduction in educational attainment among Black women.<sup>103</sup> There is also evidence that these laws cause increases in violence against women.<sup>104</sup> This finding is consistent with prior work showing that the participants in the Turnaways study who went on to give birth after being denied an abortion were more likely to be a victim of physical violence from the man involved in the pregnancy 24-30 months after seeking an abortion (relative to other groups of women) despite being less likely to be a victim of such violence 6 months prior to seeking an abortion.<sup>105</sup> This finding suggests that continuing an unwanted pregnancy can put an individual at greater risk by tethering them to a potential abuser. It is also consistent with surveys in which respondents indicate “having an abusive partner” as a reason for seeking an abortion.<sup>106</sup>

76. An important limitation of this strand of literature is that it abstracts from the effects on individuals whose access to abortion is actually affected by the state policy changes. Studies taking this approach typically find stronger evidence that abortion legalization affects socioeconomic outcomes for Black women than for white women. This does not imply that being unable to have an abortion is more detrimental to Black women’s socioeconomic outcomes. Instead, it reflects the fact that legalization had a larger impact on abortion access for Black women, as I discussed in the prior section. As such, it would be inappropriate to conclude from this strand of the literature that the socioeconomic outcomes of white women prevented from obtaining abortions are not meaningfully unaffected.

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<sup>103</sup> *Id.*

<sup>104</sup> Caterina Muratori, *The Impact of Abortion Access on Violence Against Women*, (Department of Economics, University of Reading, Working Paper No. 2021-03, 2021).

<sup>105</sup> Sarah C. M. Roberts, M. Antonia Biggs, Karuna S. Chibber *et al.*, *Risk of violence from the man involved in the pregnancy after receiving or being denied an abortion*, 12 BMC MED. 144 (2014).

<sup>106</sup> See, e.g., Karuna S. Chibber, M Antonia Biggs, Sarah C. M. Roberts & Diana Greene Foster, *The role of intimate partners in women's reasons for seeking abortion*, WOMENS HEALTH ISSUES, (2014); M Antonia Biggs, H. Gould & Diana Greene Foster, *Understanding why women seek abortions in the US*, 13 BMC WOMEN'S HEALTH 29 (2013).

77. In any case, this research is broadly consistent with other strands of the economics literature. As I noted above, rigorous studies of causal effects have repeatedly documented large and persistent reductions in earnings caused by childbearing. Rigorous studies of causal effects have also shown that educational attainment is increased when teenagers delay childbearing.<sup>107</sup>

78. There is also a sizeable literature on the causal effects of state laws altering access to contraception. While these laws are obviously different from laws altering abortion access and we would not expect them to have the exact same effects, they are similar in that they have the potential to affect childbearing which may in turn affect other outcomes. In any case, several studies have examined the effects of state-level restrictions on contraceptive access for unmarried, younger women who were teenagers in the 1960s and 1970s using difference-in-differences research designs. As described in Lindo and Bailey's review of these studies, legal access to the pill "had broad effects on women's and men's education, career investments, and lifetime wage earnings. (Goldin and Katz 2002, Bailey 2006, Guldi 2008, Hock 2008, Bailey 2009, Bailey et al. 2011, Guldi 2011). Affected women and men were more likely to enroll in and complete college. Women were more likely to work for pay, invest in on-the-job training, and pursue nontraditional professional occupations. And as women aged, these investments paid off. Thirty percent of the convergence of the gender wage gap in the 1990s can be attributed to these changing investments made possible by the Pill (Bailey, Hershbein, and Miller 2012)."<sup>108</sup>

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<sup>107</sup> See, e.g., Daniel Klepinger, Shelly Lundberg & Robert Plotnick, *How Does Adolescent Fertility Affect the Human Capital and Wages of Young Women?*, 34 THE J. OF HUM. RES. 421, 421–48 (1999); Jason M. Fletcher & Barbara L. Wolfe, *Education and Labor Market Consequences of Teenage Childbearing Evidence Using the Timing of Pregnancy Outcomes and Community Fixed Effects*, 44 J. OF HUM. RES. 303, 303–25 (2009); Adam Ashcraft, Ivan Fernandez-Val, & Kevin Lang, *The Consequences of Teenage Childbearing: Consistent Estimates When Abortion Makes Miscarriage Non-random*, 123 THE ECON. J. 875, 875–905 (2013); Lisa Schulkind & Danielle H. Sandler, *The Timing of Teenage Births: Estimating the Effect on High School Graduation and Later-Life Outcomes*, 56 DEMOGRAPHY 345, 345–65 (2019).

<sup>108</sup> Martha J. Bailey & Jason M. Lindo, *Access and Use of Contraception and its Effects on Women's Outcomes in the U.S.*, (Nat'l Bureau of Econ. Rsch., Working Paper No. 23465, 2017), <http://people.tamu.edu/~jlindo/ReproductiveTechnologyNBERwp.pdf>.

As such, the studies in this literature provide strong support for the argument that policies altering childbearing can have substantial educational and economic impacts.

79. To put the estimated effects on educational attainment into context, it is important to keep in mind that the benefits of education are likely to go well beyond wages. As Oreopoulos and Salvanes write in their summary of the literature on the non-pecuniary benefits of education: “Gains from school occur from being in a job that not only pays more but also offers more opportunities for self-accomplishment, social interaction, and independence. Schooling generates occupational prestige. It reduces the chance of ending up on welfare or unemployed. It improves success in the labor market and the marriage market. Better decision-making skills learned in school also lead to better health, happier marriages, and more successful children. School also lead to better health, happier marriages, and more successful children. Schooling also encourages patience and long-term thinking. Teen fertility, criminal activity, and other risky behaviors decrease with it. Schooling promotes trust and civic participation. It teaches students how to enjoy a good book and manage money. And for many, schooling has consumption value too.”<sup>109</sup> As I discuss in the next section, an individual’s education has important implications for their children as well.

#### **VI. Expected Effects on the children of individuals facing restricted access to abortion**

80. As noted above, a majority of those obtaining abortions have previously given birth. In addition, many individuals will go on to have children later in their lives after they have had an abortion. As such, the lives of these children will also be altered by the impacts on their parents described above.

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<sup>109</sup> Philip Oreopoulos & Kjell G. Salvanes, *Priceless: The Nonpecuniary Benefits of Schooling*, 25 J. OF ECON. PERSP. 159, 159-84 (2011).

81. Economists highlight that parents can invest in children's outcomes through monetary expenditures and time inputs.<sup>110</sup> As I described in the prior section, restricted abortion access and increased childbearing strain both of these resources. As a result, affected children suffer due to their parents' more limited resources.

82. A large literature with many high-quality studies of causal effects documents how more-limited economic resources has detrimental effects on children. Studies of this type have repeatedly found significant effects of economic resources on test scores,<sup>111</sup> which are strongly correlated with subsequent socioeconomic outcomes, and behavioral and emotional issues.<sup>112</sup> Researchers have also examined the effects on children's outcomes in adulthood. Along these lines, a recent review of the causal effects of expanding resources available to poor households on economic outcomes concludes that there are "large benefits...to children over the long run."<sup>113</sup> Recently released works have provided even more evidence of these benefits, in studies that measure causal effects on test scores, educational attainment, and adult earnings<sup>114</sup> in addition to measures of earnings capacity, economic self-sufficiency, neighborhood quality, and

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<sup>110</sup> Douglas Almond, Janet Currie & Valentina Duque, *Childhood Circumstances and Adult Outcomes: Act II*, 56 J. OF ECON. LITERATURE 1360, 1360-1446 (2018).

<sup>111</sup> See, e.g., Sandra E. Black, Paul J. Devereux, Katrina V. Løken & Kjell G. Salvanes, *Care or Cash? The Effect of Child Care Subsidies on Student Performance*, 96 REV. OF ECON. AND STAT. 824, 824-37 (2014); Gordon B. Dahl & Lance Lochner, *The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit*, 102 AM. ECON. REV. 1927, 1927-56 (2012); Kevin Milligan, & Mark Stabile, *Do Child Tax Benefits Affect the Well-Being of Children? Evidence from Canadian Child Benefit Expansions*, 3 AM. ECON. J.: ECON. POL'Y 175, 175-205 (2011).

<sup>112</sup> See, e.g., Randall Akee, William Copeland, E. Jane Costello, & Emilia Simeonova, *How Does Household Income Affect Child Personality Traits and Behaviors?*, 108 AM. ECON. REV. 775, 775-827 (2018); Kevin Milligan & Mark Stabile, *Do Child Tax Benefits Affect the Well-Being of Children? Evidence from Canadian Child Benefit Expansions*, 3 AM. ECON. J.: ECON. POL'Y 175, 175-205 (2011).

<sup>113</sup> Anna Aizer, Hilary Hoynes & Adriana Lleras-Muney, *Children and the US Social Safety Net: Balancing Disincentives for Adults and Benefits for Children*, 36 J. OF ECON. PERSPS. 149, 149-74 (2022).

<sup>114</sup> Andrew Barr, Jonathan Eggleston & Alexander A. Smith, *Investing in Infants: The Lasting Effects of Cash Transfers to New Families*, THE Q. J. OF ECON., (2022).

life expectancy.<sup>115</sup> These works are consistent with a much broader literature documenting strong correlations between parents' incomes and their children's adult incomes.<sup>116</sup>

83. Researchers have also shown that an individuals' economic circumstances *prior to birth* significantly affects health at birth,<sup>117</sup> which appears to translate into impacts on infant mortality, educational attainment, and adult earnings.<sup>118</sup> This evidence thus provides further evidence that restricted abortion access will have deleterious effects on children (i.e., children born after a parent has been prevented from obtaining an abortion and has impaired economic outcomes as a result).

84. Another strand of literature examines the causal effects of parental education. Researchers studying this topic have found that parental education significantly affects children's health at birth,<sup>119</sup> cognitive skills and behavioral problems in childhood,<sup>120</sup> the probability that children repeat a grade,<sup>121</sup> and involvement in crime.<sup>122</sup> This is relevant, given that restricted abortion access and childbearing reduces educational attainment.

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<sup>115</sup> Martha J. Bailey, Hilary Hoynes, Maya Rossin-Slater & Reed Walker, *Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence from the Food Stamps Program*, (Nat'l Bureau of Econ. Rsch., Working Paper No. 26942, 2020).

<sup>116</sup> See, e.g., Raj Chetty, Nathaniel Hendren, Patrick Kline, Emmanuel Saez & Nicholas Turner, *Is the United States Still a Land of Opportunity? Recent Trends in Intergenerational Mobility*, 104 AM. ECON. REV. 141, 141-47 (2014).

<sup>117</sup> See, e.g., Douglas Almond, Hilary W. Hoynes & Diane Whitmore Schanzenbach, *Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes*, 93 REV. OF ECON. AND STAT. 387, 387-403 (2011); Hilary Hoynes, Doug Miller & David Simon, *Income, the Earned Income Tax Credit, and Infant Health*, 7 REV. OF ECON. AND STAT. AM. ECON. J.: ECON. POL'Y 172, 172-211 (2015); Jason M. Lindo, *Parental Job Loss and Infant Health*, 30 J. OF HEALTH ECON. 869, 869-79 (2011).

<sup>118</sup> Philip Oreopoulos, Mark Stabile, Leslie Roos & Randy Walld, *The Short, Medium, and Long Term Effects of Poor Infant Health*, 43 J. OF HUMAN RES. 88, 88-138 (2008), <http://ideas.repec.org/a/uwp/jhriss/v43y2008i1p88-138.html>.

<sup>119</sup> Janet Currie & Enrico Moretti, *Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings*, 118 Q. J. OF ECON. 1495, 1495-532 (2003).

<sup>120</sup> Pedro Carneiro, Costas Meghir & Matthias Parey, *Maternal Education, Home Environments, and the Development of Children and Adolescents*, 11 J. OF THE EUR. ECON. ASS'N 123, 123-60 (2013).

<sup>121</sup> Philip Oreopoulos, Marianne E. Page & Ann Huff Stevens, *The Intergenerational Effects of Compulsory Schooling*, 24 J. OF LABOR ECON. 729, 729-60 (2006).

<sup>122</sup> Aaron Chalfin & Monica Deza, *The intergenerational effects of education on delinquency*, 159 J. OF ECON. BEHAV. & ORG. 553, 553-71, (2019).

I have reviewed the facts contained in this affidavit and they are true and correct to the best of my knowledge.

Jason Lindo

Jason Lindo, Ph.D.

COMMONWEALTH OF KENTUCKY     )  
                                                           )  
COUNTY OF JEFFERSON             )

Subscribed, sworn, and acknowledged before me by Jason Lindo this 26th day of June, 2022.

Tracy Martin-Wright  
Notary Public     TRACY MARTIN-WRIGHT

My commission expires: Oct 16, 2025

Kentucky NP ID: KNP 35554



# Attachment 1



# JASON M. LINDO

jlindo@tamu.edu | (979) 446-3969 | <http://people.tamu.edu/~jlindo/>  
Texas A&M University | Department of Economics | College Station, TX 77843-4228

## CURRENT POSITIONS

Professor of Economics, Texas A&M University, 2018–Present  
Ray. A. Rothrock '77 Senior Fellow, Texas A&M University, 2019–Present  
Distinguished Visiting Scholar, Montana State University, 2020 – Present  
Fellow, Global Labor Organization, 2017–Present  
Research Associate, National Bureau of Economic Research (NBER), 2014–Present  
Research Fellow, Institute for the Study of Labor (IZA), 2010–Present  
Co-Editor, *Economic Inquiry*, 2016–Present  
Associate Editor, *Journal of Population Economics*, 2016–Present

## PREVIOUS POSITIONS

Visiting Research Scholar, Montana State University, 2016 – 2020  
Associate Professor of Economics, Texas A&M University, 2013–2018  
Visiting Principal Fellow, University of Wollongong, 2012–2014  
Faculty Research Fellow, National Bureau of Economic Research (NBER), 2011– 2014  
Assistant Professor of Economics, University of Oregon, 2009–2013

## EDUCATION

Ph.D., Economics, University of California, Davis, 2009  
M.A., Economics, University of California, Davis, 2005  
B.A., Economics, University of California, Davis, 2004

## RESEARCH AND TEACHING INTERESTS

Applied microeconomics, health, issues concerning youth, econometrics

## PUBLICATIONS

### Refereed Publications

Lindo, Jason M., Isaac D. Swensen, and Glen R. Waddell. **"Effects of Violent Media Content: Evidence from the Rise of the UFC,"** *Journal of Health Economics*, forthcoming.

Lindo, Jason M. <sup>Ⓔ</sup> Mayra Pineda-Torres. **"New Evidence on The Effects of Mandatory Delay Laws for Abortion,"** *Journal of Health Economics*, 80, 2021.

Kelly, Andrea, Jason M. Lindo, and Analisa Packham. **"The Power of the IUD: Effects of Expanding Access to Contraception Through Title X Clinics,"** *Journal of Public Economics*, 192, 2020.

Lindo, Jason M., Caitlin Myers, Andrea Schlosser, and Scott Cunningham. **"How Far is Too Far? New Evidence on Abortion Clinic Closures, Access, and Abortions,"** *The Journal of Human Resources*, 55(4), pp. 1137-1160, 2020.

Lindo, Jason M. **"Weighing the Evidence on the Likely Effects of Expanding Access to LARCs on Poverty,"** *American Journal of Obstetrics & Gynecology*, 222(4), pp. S864-S865, 2020.

Lindo, Jason M., Dave Marcotte, Isaac D. Swensen, and Jane Palmer. **"Any Press is Good Press? The Unanticipated Effects of Title IX Investigations on University Outcomes,"** *Economics of Education Review*, 73, 2019.

- Lindo, Jason M., Jessamyn Schaller, and Benjamin Hansen. **"Caution! Men Not At Work: Gender Specific Labor Market Conditions and Child Maltreatment,"** *Journal of Public Economics*, 163, pp. 77-98, 2018.
- Lindo, Jason M. and María Padilla-Romo. **"Kingpin Approaches to Fighting Crime and Violence: Evidence from Mexico's Drug War,"** *Journal of Health Economics*, 58, pp. 253-268, 2018.
- Bondurant, Samuel, Jason M. Lindo, and Isaac D. Swensen. **"Substance Abuse Treatment Centers and Local Crime,"** *Journal of Urban Economics*, 104, pp. 124-133, 2018.
- Lindo, Jason M., Peter Siminski, and Isaac D. Swensen. **"College Party Culture and Sexual Assault,"** *American Economic Journal: Applied Economics*, 10(1), pp. 236-65, 2018.
- Lindo, Jason M. and Analisa Packham. **"How Much Can Expanding Access to Long-Acting Reversible Contraceptives Reduce Teen Birth Rates?"** *American Economic Journal: Economic Policy*, 9(3), pp. 348-76, 2017.
- Barreca, Alan I., Jason M. Lindo, and Glen R. Waddell. **"Heaping-Induced Bias in Regression-Discontinuity Designs,"** *Economic Inquiry*, 54(1), pp. 268-293, 2016.
- Lindo, Jason M., Peter Siminski, and Oleg Yerokhin. **"Breaking the Link Between Legal Access to Alcohol and Motor Vehicle Accidents: Evidence from New South Wales,"** *Health Economics*, 25(7), pp. 908-928, 2015.
- Lindo, Jason M. **"Aggregation and the Estimated Effects of Economic Conditions on Health,"** *Journal of Health Economics*, 40, pp. 83-96, 2015.
- Lindo, Jason M. and Charles Stoecker. **"Drawn into Violence: Evidence on 'What Makes a Criminal' from the Vietnam Draft Lotteries,"** *Economic Inquiry*, 52(1), pp. 239-258, 2014.
- Lindo, Jason M., Isaac D. Swensen, and Glen R. Waddell. **"Alcohol and Student Performance: Estimating the Effect of Legal Access,"** *Journal of Health Economics*, 32(1), pp. 22-32, 2013.
- Lindo, Jason M., Isaac D. Swensen, and Glen R. Waddell. **"Are Big-Time Sports a Threat to Student Achievement?"** *American Economic Journal: Applied Economics*, 4(4), pp. 254-274, 2012.
- Cuffe, Harold, William T. Harbaugh, Jason M. Lindo, Giancarlo Musto, and Glen R. Waddell. **"Evidence on the Efficacy of School-Based Incentives for Healthy Living,"** *Economics of Education Review*, 31(6), pp. 1028-1036, 2012.
- Barreca, Alan I., Melanie Guldi, Jason M. Lindo, and Glen R. Waddell. **"Saving Babies? Revisiting the Effect of Very Low Birth Weight Classification,"** *The Quarterly Journal of Economics*, 126(4), pp. 2117-2123, 2011.
- Lindo, Jason M. **"Parental Job Loss and Infant Health,"** *Journal of Health Economics*, 30(5), pp. 869-879, 2011.
- Lindo, Jason M., Nicholas J. Sanders, and Philip Oreopoulos. **"Ability, Gender, and Performance Standards: Evidence from Academic Probation,"** *American Economic Journal: Applied Economics*, 2(2), pp. 95-117, 2010.
- Lindo, Jason M. **"Are Children Really Inferior Goods? Evidence from Displacement-driven Income Shocks,"** *The Journal of Human Resources*, 45(2), pp. 301-327, 2010.

#### Book Chapters and Other Academic Publications

- Bullinger, Lindsey Rose, Jason M. Lindo, and Jessamyn Schaller. **"Economic Determinants of Child Maltreatment,"** in Alain Marciano and Giovanni Battista Ramello, eds., *Encyclopedia of Law and Economics, Second Edition*, 2021.
- Lindo, Jason M., Mayra Pineda-Torres, David Pritchard, and Hedieh Tajali. **"Legal Access to Reproductive Control Technology, Women's Education, and Earnings Approaching Retirement,"** *AEA Papers and Proceedings*, 110, pp. 231-235, 2020.
- Bailey, Martha J. and Jason M. Lindo. **"Access and Use of Contraceptives and Its Effects on Women's Outcomes in the United States,"** in Susan L. Averett, Laura M. Argys, and Saul D. Hoffman, ed., *Oxford Handbook on the Economics of Women*, New York: Oxford University Press, 2018.
- Lindo, Jason M. and Jessamyn Schaller. **"Economic Determinants of Child Maltreatment,"** in Jürgen Backhaus, ed., *Encyclopedia of Law and Economics*, pp. 1-10, NY: Springer, 2014.
- Lindo, Jason M. and Peter Siminski. **"Should The Legal Age For Buying Alcohol Be Raised to 21 Years?"**

*Medical Journal of Australia*, 201(10), p. 571, 2014.

Page, Marianne, Ann Huff Stevens, and Jason M. Lindo. **"Parental Income Shocks and Outcomes of Disadvantaged Youth in the United States,"** in Jonathan Gruber, ed., *An Economic Perspective on the Problems of Disadvantaged Youth*, pp. 213–235, Chicago: University of Chicago Press, 2009.

#### Policy Briefs and Editorials

Jason M. Lindo, Krishna Regmi, and Isaac D. Swensen, **"Layoffs, Divorce, and the Effect of Unemployment Insurance"** *EconoFact*, October 21, 2020.

Andrea M. Kelly, Jason M. Lindo, and Analisa Packham, **"Could Expanding Access to Contraception Improve Economic Outcomes?"** *EconoFact*, August 20, 2019. Republished by *PBS News Hour*, August 29, 2019.

Jason M. Lindo, Peter Siminski, and Isaac D. Swensen, **"Big game days in college football linked with sexual assault,"** *The Conversation*, September 20, 2018.

Jason M. Lindo, Dave E. Marcotte, Jane E. Palmer, and Isaac D. Swensen, **"Any Press is Good Press? Study Finds Federal Investigations of University Responses to Sexual Misconduct Cases May Help Enrollments,"** *ProMarket: The Blog of the Stigler Center at the University of Chicago Booth School of Business*, August 16, 2018.

Jason M. Lindo, Peter Siminski, and Isaac D. Swensen, **"Football, College Party Culture, and Sexual Assault,"** *EconoFact*, July 19, 2018.

Bondurant, Samuel, Jason M. Lindo, and Isaac D. Swensen, **"Access to Substance Abuse Treatment, Drug Overdose Deaths, and Crime,"** *EconoFact*, March 16, 2018.

Lindo, Jason M. **"Defunding Planned Parenthood Didn't Reduce the Number of Abortions in Texas,"** *Dallas Morning News*, July 6, 2017.

Lindo, Jason M. and Analisa Packham. **"Lowering the Teenage Birthrate,"** *New York Times*, July 13, 2015.

Lindo, Jason M. and Analisa Packham. **"Long-acting Reversible Contraceptives Reduced Teen Pregnancies, Especially in Higher-Poverty Areas,"** *UC Davis Center for Poverty Research Policy Brief*, 4(3), 2015.

Lindo, Jason M. and María Padilla-Romo. **"Kingpin Approaches to Fighting Crime and Violence: Evidence from Mexico's Drug War,"** *Cato Research Briefs in Economic Policy*, No. 31, July 2015.

Lindo, Jason M. **"Gender-Specific Measures of Economic Conditions and Child Abuse,"** *Center for the Study of Women in Society Research Matters*, Spring 2013.

#### Working Papers

Swensen, Isaac <sup>Ⓡ</sup> Jason M. Lindo <sup>Ⓡ</sup> Krishna Regmi. **"Stable Income, Stable Family,"** NBER Working Paper No. 26228.

#### Articles In Progress

Berthelon, Matias, Diana Kruger, Carolina Molinare León, and Jason M. Lindo. **"Emergency Contraception Effects on High School Drop Out."**

Jason M. Lindo, Mayra Pineda-Torres, David Pritchard, and Hedieh Tajali. **"Effects of Access to Reproduction Control Technology on Women's Retirement-Age Outcomes."**

DiNardi, Michael, Melanie Guldi, and Jason M. Lindo. **"Reassessing the Effects of Emergency Contraception on Population-Level Outcomes."**

Lindo, Jason M. and Emily Zheng. **"Better Economy, More Babies? New Evidence on the Effects of Economic Conditions on Childbearing."**

Pritchard, David, Jonathan Tillinghast, and Jason M. Lindo. **"How Do Students Respond to Historical Course Grade Information? Evidence from a Randomized Control Trial."**

#### GRANTS AND COMPETITIVE EXTERNAL FELLOWSHIPS

Laura and John Arnold Foundation, PI, 2018

National Institute for Health Care Management Research and Education Foundation, PI, 2017  
 Turnovsky Fellowship, 2017  
 US Department of Justice Research Grant, Co-PI with Isaac D. Swensen, Award 2014-R2-CX-0015, 2014

### INTERNAL GRANTS

Texas Census Research Data Center Proposal Development Grant, 2014  
 Texas Census Research Data Center Proposal Development Grant, 2013  
 Center for the Study of Women in Society Faculty Research Grant, University of Oregon, 2012  
 Junior Professorship Development Grant, University of Oregon, College of Arts and Sciences, 2011  
 Junior Professorship Development Grant, University of Oregon, College of Arts and Sciences, 2010  
 Junior Faculty Award, University of Oregon, 2009  
 Graduate Student Travel Award, UC Davis, 2007

### HONORS AND AWARDS

Best Supporter of Graduate Students, Texas A&M Department of Economics, 2020  
 Outstanding Graduate Instructor of the Year, Texas A&M Department of Economics, 2018  
 Best Graduate Advisor, Texas A&M Department of Economics, 2017  
 Outstanding Graduate Instructor of the Year, Texas A&M Department of Economics, 2013  
 Emerging Scholar, Center for Poverty Research, University of Kentucky, 2011  
 Phi Beta Kappa, 2005

### PRESENTATIONS

**2021–2022 (including planned):** Elon University, University of Connecticut, Essen Health Conference (keynote)  
**2020–2021:** Centre for Health Economics–Monash Business School, Monash University Department of Economics, Association for Mentoring & Inclusion in Economics (AMIE)  
**2019–2020:** Miami University, Indiana University, San Diego State University, Society of Family Planning Annual Meeting, American Economic Association Annual Meetings, University of Michigan, University of South Florida  
**2018–2019:** 3rd IZA Workshop on Gender and Family Economics, University of California at Davis, Brookings Conference on Improving Opportunity Through Family Planning  
**2017–2018:** University of Kansas, Stata Texas Empirical Micro Conference, Sam Houston State University, Ifo Institute Workshop on Economic Uncertainty and the Family, 18th Annual Southeastern Health Economics Study Group, University of Tennessee, Texas A&M University (Agricultural Economics), Birdsall House Conference on Women (Center for Global Development), Texas A&M University (School of Public Health), University of South Carolina, Columbia University, American University, NBER Health Economics Program Meetings, University of California at Davis, Montana State University Initiative for Regulation and Applied Economic Analysis Conference on “Economics of Reproductive Health Policies”  
**2016–2017:** Montana State University, University of Colorado at Boulder, West Virginia University, Fall Meetings of the Association for Public Policy Analysis & Management, Annual Meetings of the American Economics Association, University of California at Merced, Southern Methodist University, Victoria University of Wellington  
**2015–2016:** Texas Tech University, Southern Economic Association Annual Meetings, National Institute for Health Care Management Webinar on Adolescent Health and Teen Pregnancy, NBER Children’s Program Meetings, China Meeting of the Econometric Society  
**2014–2015:** Monash University, University of North Carolina at Charlotte, Baylor University, SOLE/EALE World Meetings  
**2013–2014:** Tulane University, University of Texas at Dallas, Dalhousie University, University of Houston and Rice University, University of Wollongong, Victoria University of Wellington, Massey University

**2012–2013:** Labour Econometrics Workshop (Discussant), University of Wollongong, Texas A&M University, University of Illinois at Urbana-Champaign, Louisiana State University, Michigan State University, University of California at Merced, 5th Annual Meeting on the Economics of Risky Behaviors, NBER Children's Program Meetings

**2011–2012:** The Australian National University, University of Wollongong, Australian Labour Econometrics Workshop, University of Notre Dame, Case Western Reserve University, University of Maryland, University of Oregon, SOLE Annual Meetings, IZA/SOLE Transatlantic Meeting of Labor Economists

**2010–2011:** NBER Children's Program Meetings, SOLE Annual Meetings, Public Policy and the Economics of the Family Conference at Mount Holyoke College, University of Kentucky, Portland State University

**2009–2010:** Western Economic Association Annual Meetings, American Economic Association Annual Meetings (Discussant), SOLE/EALE World Meetings, The Economics of Family Policy Conference at the University of Bergen, NBER Children's Program Meetings, Economic Demography Workshop, University of British Columbia

**2008–2009:** NBER Higher Education Program Meetings, RAND Corporation, University of Colorado at Denver, Stanford Institute for Economic Policy Research, University of Oregon, The College of William and Mary, Sonoma State University, California State University at Sacramento, All UC Labor Conference, UC Davis Economy, Justice, and Society Retreat, Western Economic Association Annual Meetings

### **ADDITIONAL PROFESSIONAL ACTIVITIES**

**Co-Director of Mentoring:** Association for Mentoring & Inclusion in Economics (AMIE), 2020–Present

**Referee:** *American Economic Journal: Applied Economics*, *American Economic Journal: Economic Policy*, *American Economic Review*, *American Journal of Health Economics*, *American Journal of Obstetrics and Gynecology*, *The B.E. Journal of Economic Analysis and Policy*, *Children and Youth Services Review*, *Contemporary Economic Policy*, *Contraception*, *Demography*, *Eastern Economic Journal*, *The Economic Journal*, *Economics of Education Review*, *Economic Inquiry*, *Education Evaluation and Policy Analysis*, *Empirical Economics*, *Health Economics*, *Industrial and Labor Relations Review*, *Institute for Women's Policy Research*, *Journal of Applied Econometrics*, *Journal of Family and Economic Issues*, *Journal of Health Economics*, *The Journal of Human Resources*, *Journal of The Japanese and International Economies*, *Journal of Labor Economics*, *Journal of Labor Research*, *Journal of Law Economics and Organization*, *Journal of Policy Analysis and Management*, *Journal of Political Economy*, *Journal of Population Economics*, *Journal of Public Economics*, *Journal of the Royal Statistical Society*, *Labour Economics*, *Proceedings of the National Academy of Sciences*, *Public Choice*, *The Quarterly Journal of Economics*, *Review of Economics of The Household*, *Review of Economic Studies*, *The Southern Economic Journal*, *Women's Health Issues*

**Reviewer:** National Science Foundation, APPAM Program Committee

**Co-organizer or Committee Member:** Montana State University Initiative for Regulation and Applied Economic Analysis Conference on "Economics of Unemployment Insurance" 2020 (Co-organizer), Texas Health Economics Workshop 2019 (Co-organizer), Montana State University Initiative for Regulation and Applied Economic Analysis Conference on "Economics of Reproductive Health Policies" 2018 (Co-organizer), Annual Health Economics Conference 2018 (Committee Member), Economic Demography Workshop 2018 (Committee Member), Midwestern Econometrics Group Meetings 2017 ((Committee Member), Economic Demography Workshop 2017 (Committee Member), 15th Annual Labour Econometrics Workshop 2012 (Committee Member)

**Advisory Board Member:** Michigan Contraceptive Access, Research, and Evaluation Study, 2018–Present

### **TEACHING EXPERIENCE**

#### **Texas A&M University**

PhD-level Applied Microeconometrics (Fall 13, Fall 14, Spr 15, Spr 16, Spr 17, Spr 18, Spr 19, Spr 21)

Program Evaluation (Fall 14, Spr 14, Spr 16, Spr 17, Spr 18, Fall 19, Fall 20, Spr 21)

#### **Shanghai University of Finance and Economics**

Short Course in Econometric Methods for Causal Inference (Summer 16)

#### **University of Oregon**

Graduate Labor Economics (Winter 10, Fall 10, Spr 13)  
 Topics in Labor Economics (Fall 09, Winter 10, Fall 10, Spr 11, Fall 11, Spr 12, Spr 13)  
 Economics of Gender (Spr 11, Fall 11, Spr 12)

**PHD STUDENT ADVISING** (including graduation year and initial placement)

**Texas A&M University**

Jie Zhong (co-chair, in progress)  
 Wesley Miller (in progress)  
 Andre'ay Harris (in progress)  
 Mayra Pineda Torres (chair, 2022), Georgia Tech University  
 David Pritchard (chair, 2022), U.S. Census Bureau  
 Hedieh Tajali (2022), University of Edinburgh  
 Andrea Kelly (chair, 2020), Grinnell College  
 Manuel Hoffman (2020), University of Heidelberg  
 Joshua Witter (2020), Correlation Research Division at the Church of Jesus Christ of Latter-Day Saints  
 Roberto Mosquera (co-chair, 2019), Universidad de las Américas  
 Brittany Street (2019), University of Missouri  
 John Anders (2019), US Census Bureau  
 Ruichao Si (2019), Nankai University  
 Samuel Bondurant (chair, 2018) US Census Bureau  
 Abigail Peralta (2018), Louisiana State University  
 Yongzhi Sun (2018), Southwestern University of Finance and Economics  
 Maria Padilla-Romo (chair, 2017), University of Tennessee  
 Emily Zheng (chair, 2017), Chinese University of Hong Kong - Shenzhen  
 Jaegum Lim (2017), Korean National Assembly  
 Analisa Packham (chair, 2016), Miami University  
 Pierre Mouganie (2015), American University of Beirut  
 Jillian Carr (2015), Purdue University

**University of Oregon**

Kristian Holden (co-chair, 2014), American Institutes for Research (AIR)  
 Harold Cuffe (co-chair, 2013), Victoria University of Wellington  
 Isaac Swensen (co-chair, 2013), Montana State University  
 Brian Vander Naald (2012), University of Alaska, Juneau  
 Eric Duquette (2010), Economic Research Service, USDA

**UNIVERSITY SERVICE**

Faculty Senate, 2014-2016  
 Climate and Diversity Committee, 2015-2016  
 Academic Affairs Committee, 2014-2015

**DEPARTMENTAL SERVICE**

**Texas A&M University**

Econometrics Search Committee, 2019–2021  
 Economics Department Head Search Committee, 2019–2020  
 PERC Applied Microeconomics Workshop Co-organizer, 2019–2020

Organizer, Inaugural Public Labor and Industrial Organization (PLIO) Alumni Conference, 2019  
 Graduate Placement Co-director, 2013–2014, 2015–2016, 2017–2018, 2018–2019  
 Economics Undergraduate Research Opportunities Program Advisor, 2014–2015, 2018–2019  
 PhD Qualifier Exam Committee, 2015–present  
 Executive Committee, 2017–2018  
 Graduate Instruction Committee, 2017  
 Applied Microeconomics Search Committee Chair, 2014–2015  
 Applied Microeconomics Search Committee, 2013–2014

#### University of Oregon

McNair Scholar Advisor, 2012–2013  
 Graduate Placement Co-director, 2010–2012  
 Undergraduate Program Committee, 2009–2013  
 Seminar Committee, 2009–2010  
 Applied Microeconomics Brownbag Co-organizer, 2009–2010

### SELECTED MEDIA APPEARANCES AND COVERAGE OF RESEARCH

#### Television:

"Rape on College Campuses," Not Safe with Nikki Glaser (Comedy Central), 7/12/16  
 "College Football and Campus Sexual Assault," Outside The Lines (ESPN), 2/19/16  
 "College Game Day's Disturbing Trend," Watching the Hawks (RT), 1/11/16

#### Radio/Podcast:

"Episode 33: Persistent Effects of Violent Media Content," Probable Causation, 8/4/20  
 "Persistent Effects of Violent Media Content," Vox's The Weeds, 5/26/20 (46th minute)  
 "The benefits of IUDs," Vox's The Weeds, 3/26/19 (37th minute)  
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*Updated May 9, 2022*