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ABSTRACT

Measuring the Burden: The Effect of Travel Distance on Abortions and Births*

I compile and disseminate novel panel data sets measuring county-level travel distances to abortion facilities and resident abortion rates. Using these data and exploiting temporal and spatial variation in distances, I implement difference-in-difference research designs measuring the causal effects of distance to the nearest abortion facility. The results indicate large and non-linear effect: An increase in travel distance from 0 to 100 miles—a level that courts have generally treated as not unduly burdensome for women seeking abortions—is estimated to prevent 20.5% of women seeking an abortion from reaching a provider, and in turn to increase births by 2.4%.

JEL Classification: I11, I12, J13

Keywords: travel distance, abortions, births

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1 Introduction

A wave of legislation restricting abortion access has swept the United States over the past decade. Between 2011 and 2019, 33 states enacted 479 new restrictions, representing nearly 40% of all restrictions enacted since *Roe v. Wade* legalized abortion nationwide in 1973 (Nash, 2019). This trend is on track to continue and perhaps accelerate: in the first two months of 2021 lawmakers in 43 states introduced new bills with 384 new abortion restrictions (Nash, 2021). The slate of recent restrictions is marked by decreasing emphasis on demand-side regulations such as mandatory waiting periods and increasing emphasis on supply-side regulations targeting providers such as admitting privileges requirements, hospital transfer policies, facility requirements, and outright bans (Joyce, 2011; Nash, 2021). As supply-side restrictions have expanded, the number of abortion clinics has declined in regions with the most legislative activity, by 27% in the Midwest and 20% in the South between 2011 and 2017 (Jones and Jerman, 2017a; Jones et al., 2019; Nash et al., 2019).

Approximately 1 in 5 pregnancies are aborted and current age-specific abortion rates suggest 1 in 4 U.S. women will have an abortion in their reproductive lifetimes (Jones et al., 2019). The sheer incidence and prevalence of abortion suggests the effects of abortion restrictions and abortion access is of fundamental interest to social scientists. These questions also are of fundamental interest to courts, which evaluate the constitutionality of abortion restrictions under the undue burden standard established in *Planned Parenthood v. Casey* (1992). In this landmark ruling, affirmed and clarified in *Whole Woman's Health v. Hellerstedt* (2016) and *June Medical Services, LLC v. Russo* (2020), the Supreme Court held that states may regulate abortions so long as these regulations do not impose an "undue burden," defined as a "substantial obstacle in the path of a woman seeking an abortion." The Court considered the potential burdens of travel distance in this ruling, obliquely suggesting that travel distances of as much as 150 miles do not constitute a substantial obstacle, a finding that has been cited in subsequent rulings (e.g., *Whole Woman's Health v. Hellerstedt*, 2014).

While an economist might rightly avoid the question of whether a demand response is "undue," our methodological toolkit is well-suited to the task of identifying and measuring the "burdens" of abortion restrictions and abortion access, which is fundamentally a question of measuring demand elasticities. An extensive economics literature credibly exploiting natural experiments indicates that the liberalization of abortion policies in the 1960s and 1970s had profound effects in reducing fertility and delaying family formation (Angrist and Evans, 2001; Levine et al., 1999; Myers, 2017) and likely influenced socioeconomic outcomes for women and children (Gruber et al., 1999; Angrist and Evans, 2001; Kalist, 2004; Ananat et al., 2007). The economics literature indicates that in the fifty years since *Roe*, abortion and birth outcomes have been influenced by demand-side restrictions requiring parental involvement for minors seeking abortions (Levine, 2003; Joyce and Kaestner, 2020; Myers and Ladd, 2020) and mandatory waiting periods for abortions (Joyce et al., 1997; Joyce and Kaestner, 2001; Joyce et al., 2006; Lindo and Pineda-Torres, 2019; Myers, 2021).

More recently, supply-side restrictions that closed abortion facilities in Texas and Wisconsin afforded a credible natural experiment to directly identify and estimate the causal effect of travel distance. Exploiting the sudden closures of approximately half of Texas' abortion facilities in 2013, three independent teams of authors found that even modest increases in travel distances in Texas caused large reductions in abortions, and that the effect of travel distance on abortion rates is nonlinear, with initial increases in distance having the largest effects (Quast et al., 2017; Fischer et al., 2018; Lindo et al., 2020). Venator and Fletcher (2020) adopt a similar difference-in-difference approach in a different state, exploiting shocks to travel distances caused by facility closures in Wisconsin, and find similar effects. Comparing results from the two states, Lindo et al. (2020) estimate that an increase in travel distance from 0 to 100 miles causes a 25.8% decline in abortions using data from Texas while Venator and Fletcher (2020) estimate a 24.9% decrease using data from Wisconsin.

In this paper I expand both the temporal and spatial scope of these analyses while also introducing and disseminating two new data sets. I assemble a database identifying the location and operation dates of every publicly-identifiable abortion facility in the United

States since 2009, and use these data to construct a county-by-year panel of travel distances to the nearest abortion provider. Figure 1 illustrates county-level travel distance changes between 2009 and 2019.[1] There is rich variation over this period to exploit in a difference-in-difference research design: 568 counties experience increases in travel distance ranging from 5 to 307 miles and 355 experienced decreases in travel distance ranging from 5 to 168 miles.[2] To implement a difference-in-difference research design at a national level, I compile an additional panel of county-level resident abortion rate outcomes for the 32 states whose health departments publish these data. Both the travel distance and abortion counts panels are posted, documented, and visualized in a new website.[3]

The results of a difference-in-difference analysis indicate that the effects of travel distance observed in Texas and Wisconsin generalize to the entire country. An increase in travel distance from 0 to 100 miles is estimated to reduce abortions by 20.5% and increase births by 2.4%, while the next 100 miles of travel distance (from 100 to 200 miles) is estimated to reduce abortions by 12.7% and increase births by 1.6%. All age and ethnic groups are responsive to travel distances, but the effects on births are particularly pronounced for young women and non-Hispanic black women. These estimated effects are robust to implementing a triple-difference model with state-by-year fixed effects, and a distributed lead model does not indicate substantial pre-trends, observations which suport a causal interpretation of the results.

[1]The provider coordinates are randomly perturbed by 0.005 to 0.014 degrees (approximately 1/3 to 1 mile) to prevent identifying provider addresses based on the map.

[2]The increases in travel distances due to facility closures largely occur in states the Guttmacher Institute regards as "hostile" or "very hostile to abortion rights," including Alabama, Indiana, Louisiana, Mississippi, Missouri, North Carolina, Ohio, Oklahoma, Texas, and Wisconsin. Correspondingly, the decreases in travel distance due to facility openings largely occur in states the Guttmacher Institute regards as "supportive" or "very supportive of abortion rights." These include Maine and Washington, where two large networks of providers began offering telemedicine medication abortions during this period.

[3]Website domain will be provided at time of publication.

2 Data

I compile and disseminate two panel data sets for this analysis: county-level travel distances to abortion facilities based on a new database of abortion providers and county resident abortion rates based on a new compilation of abortion surveillance reports published by state health departments. These data are included in the replication package with this paper.

2.1 Travel distance to the nearest abortion facility

The Guttmacher Institute, a pro-choice reproductive research organization based in New York, has tracked U.S. abortion facilities periodically since 1973. In its most recent census of providers, the Guttmacher Institute identified 1,587 facilities that provided abortions in 2017. Of these facilities, 808 clinics provided 95% of abortions, while 779 hospitals and physician offices provided the remaining 5% of the national total (Jones et al., 2019). Because roughly half of the facilities provide very few abortions, in their publications Guttmacher researchers distinguish between clinics where services are provided to the general public and those physicians and hospitals that provide small numbers of abortions on a limited basis (Bearak et al., 2017). This approach is mirrored by the second source of information on provider locations: the ANSIRH Abortion Facility Database (2021), a survey of abortion facilities conducted since 2017 by scholars at the University of California San Francisco. The ANSIRH facility databse is based on publicly available sources and intended to catalog facilities that women seeking abortions can readily identify in the phone book or an internet search.

While the Guttmacher and ANSIRH provider databases are rich and useful sources for many purposes, neither are readily adapted to a difference-in-difference research design because the ANSIRH database is not a panel and the Guttmacher database is intermittent. Moreover, the limited county-level facility counts provided by Guttmacher under strict data use agreements to outside researchers do not allow researchers to distinguish clinics from hospitals or physicians. These limitations in the Guttmacher counts make it impossible to distinguish openings and closures from infrequent providers that may provide no abortions in some years.[4]

I fill this void by compiling a database of the locations of United States abortion facilities operating between January 1, 2009 and December 31, 2020. I use a variety of sources including state licensing databases, current and archived facility websites, current and archived directories of Planned Parenthood health centers, current and historical directories of providers that are members of the National Abortion Federal (NAF), and accounts of provider operations published in the press. My goal is to identify all facilities—including private physician offices, hospitals, and freestanding clinics—that have publicly advertised the provision of abortion services or are otherwise likely to be readily found by a woman seeking an abortion.

I geocode the locations of providers using their street addresses. Based on the facility operations dates, I use the Stata Geonear (Picard, 2010) and Georoute modules to calculate the travel distance from the population centroid (United States Census Bureau, 2017) of each county in the continental U.S.[5] to the nearest operating abortion facility on the 15th day of each month from January 2009 through January 2021.[6] I average monthly travel distances to generate a county-by-year panel of average travel distance to the nearest abortion facility,

[4]Despite these limitations, Brown et al. (2020) use the Guttmacher provider data to estimate a difference-in-difference specification of the relationship between county travel distance and county abortion rates in 18 states. As in the Texas and Wisconsin literatures (Quast et al., 2017; Fischer et al., 2018; Lindo et al., 2020; Venator and Fletcher, 2020), they observe a negative relationship between travel distance and abortions, with a 100 mile increase in travel distance estimated to reduce the abortion rate by 1.1 per 1,000 women. This estimate is likely biased downward by the substantial noise in the Guttmacher provider data. The authors estimate an additional model instrumenting for distance with proximity to a large population of college-enrolled women under age 25. The instrumented estimates of travel distance are 5 times larger in magnitude than the un-instrumented ones. However, the authors offer little justification for this choice of instrument, the relevance of which seems questionable given that 80% of abortion patients are not college students (Jerman et al., 2016).

[5]I exclude Alaska and Hawaii from the analyses because driving distances cannot be calculate for many counties and county-equivalents in these states due to a lack of road networks.

[6]The Georoute module relies on the HERE API to calculate travel distances under typical travel conditions. To reduce computing time, I first used the Geonear module to identify the 30 nearest providers to each county population centroid as measured by Euclidean distance. I then calculated travel distances to each of these providers and identified the nearest provider by the minimum travel distance.

the key explanatory variable for the analyses.[7]

2.2 Abortions and Births

The National Center for Health Statistics (NCHS), an agency of the Centers for Disease Control (CDC), develops and recommends standard forms and procedures for recording U.S. births, and all states cooperate with the federally-mandated reporting of birth certificate data. I use individual birth records from the NCHS with restricted-use county identifiers to compile a complete county-by-year panel of births by county of residence (NCHS, 2021).[8] To construct rates, I use estimates of county populations by sex, race, and individual year of age published by SEER (2021).

Unlike births and other vital statistics, the federal government does not mandate that state health authorities collect and report information on abortions. The CDC does encourage abortion surveillance by issuing technical guidelines and suggested standards, and collects aggregated counts voluntarily supported by most states. But these aggregate counts are not broken down by county of residence. Similarly, the Guttmacher Institute compiles abortion counts using CDC aggregate counts which Guttmacher augments with information from providers, but these counts do not provide county-level detail.

I fill this second void in United States abortion data by reviewing and compiling county resident abortion rates published by state health agencies during the sample period of 2009 to 2019. Of 48 continental states, I identify 33 which publish or release county-level resident abortion counts.[9] I compile a panel of county-level resident abortion counts for these states

[7] Abortion providers in the United States are subject to disruption and violence. In 2019, for instance, NAF documented 1,507 instances of trespassing, 24 of instances of assault and battery, 19 invasions, 92 threats of harm, and 3,123 instances of hate mail or harassing calls targeting abortion providers (National Abortion Federation (NAF), 2020). To reduce risks of harm to providers, I do not include the facility-level database in the replication package, but do provide a mechanism to apply for and use this database under a data use agreement. The replication package does include county-level travel distances.

[8] These data can be obtained by completing an application form for NCHS restricted vital statistics data sets. On approval and excecution of a data use agreement, the NCHS shares these data at no cost via secure FTP protocols.

[9] These states are Alabama, Arizona, Colorado, Delaware, Florida (beginning in 2017), Georgia, Idaho, Illinois, Indiana, Kansas, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada,

in all available years and include this panel and detailed documentation in the replication package accompanying this paper. The panel of county-level abortion counts is slightly imbalanced because Florida did not begin reporting abortion counts until 2017 and 12 states have not released counts through 2019.[10]

Figure 1 illustrates that changes in travel distance are strongly associated with changes in abortion rates. Counties where travel distances decreased exhibited a 12.3% *increase* in resident abortion rates, a striking observation when compared to the 11.3% decrease in counties where access was not changing and the overall secular decline in U.S. abortion rates. Similarly, counties where travel distances increased experienced even larger decreases in abortion rates—27.3%—than the rest of the country. Birth rates have also exhibited a secular decline during this period, and the average decline of birth rates in counties with decreasing travel distances are similar to those in counties with no changes. However, birth rates have not fallen as fast in counties where abortion access is decreasing—births fell by 9.3% over this period in counties with increasing travel distance to an abortion provider versus by 11.6% in counties where travel distance did not change.

2.3 Additional controls

Table 1 summarizes the additional controls used in the difference-in-differences analyses that follow. These include county-by-year demographic controls for the racial and ethnic composition of women of childbearing age (SEER, 2019), the educational attainment of women of childbearing age (Manson et al., 2020), and population-wide urbanization rates (Manson et al., 2020). I also control for economic conditions associated with fertility using annual county-level estimates of unemployment published by the Bureau of Labor Statistics (2020) and annual county-level estimates of poverty rates and median household income published by the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program (2020).

New York, North Carolina, North Dakota, Ohio, Oregon, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, and Wisconsin.

[10] Appendix Table A3 illustrates that the estimated abortion effects are robust to using a balanced panel for the subset of counties reporting annually from 2009 through 2019.

Additional controls capture time-varying state policies: contraceptive mandates for private insurers through state action and the Affordable Care Act (Yordán, 2014); one and two-trip mandatory waiting periods for abortions (Myers, 2021); Medicaid family planning expansions; Medicaid expansions under the Affordable Care Act; and welfare generosity and family cap policies constructed primarily with data published in Congressional Greenbooks and by the Urban Institute.[11]

3 Econometric Model

I estimate the effect of travel distance on county resident abortions and births using a difference-in-difference research design that exploits spatial and temporal variation in travel distance arising from abortion facility openings and closures (Figure 1). Because the outcomes are discrete and occasionally equal to zero, I implement this strategy using a Poisson model with an exposure for the population of women aged 15 to 44, the standard denominator for abortion and birth rates.[12] The model takes the following form:

$$E[Y_{c,s,t}|\text{distance}_{c,s,t}, \boldsymbol{\beta X_{cst}}, \boldsymbol{\upsilon_c}, \boldsymbol{\upsilon_t}] = exp(f(distance_{c,s,t}) + \boldsymbol{\beta X_{c,s,t}} + \upsilon_c + \upsilon_t) \tag{1}$$

where $Y_{c,s,t}$ is either the resident abortion rate in county c in state s in time t or the county resident birth rate in year time $t+1$, where a lead is used for birth outcomes to allow for gestation. The explanatory variable of interest, $distance_{c,s,t}$, measures the travel distance to the nearest abortion facility, which is specified as a linear or quadratic function in the models. The vector $\boldsymbol{X}_{c,s,t}$ includes an intercept, county demographics and economic conditions, and

[11] Variables compiled by the author rely on multiple sources. These are described and documented in the replication package accompanying this paper.

[12] Like linear models, the Poisson model is not subject to the incidental parameters problem associated with fixed effects (Cameron and Trivedi, 2013). While the possibility of overdispersion is the main theoretical argument that might favor an alternative count model like the negative binomial, the conditional fixed effects negative binomial model is not a true fixed effects model as it does not control for all stable covariates (Allison and Waterman, 2002). I correct for potential overdispersion in the Poisson model by calculating sandwiched standard errors (Cameron and Trivedi, 2013). I also demonstrate that the results are robust to using estimating a weighted least squares model with log rates as outcomes, dropping observations with 0 counts. (Compare Panels A and B of Appendix Table A1.)

state-level policy controls, all of which are listed and summarized in Table 1; υ_c includes county fixed effects, which control for unobserved county characteristics with time-invariant effects on abortion or birth rates; υ_t includes year fixed effects, which control for national shocks affecting abortion or birth rates similarly across all counties.[13]

The internal validity of this difference-in-difference approach relies crucially on the common trends assumption, which in this context is that absent a change in travel distance, birth and abortion rates would have *trended* similarly across counties, conditional on the control variables. As summarized in Table 1, I include a rich set of controls for time-varying demographic and economic conditions at the county level, including age and ethnic composition interacted with quadratic time trends, educational attainment, urbanization, poverty rates, median household income, and unemployment rates. I also control for state-level policies including mandatory waiting periods for abortions, Medicaid family-planning expansions, insurance mandates for contraception coverage, Medicaid expansions following the Affordable Care Act, the presence of a family cap to receive welfare benefits, and the maximum welfare benefits for a family of 3.

A series of robustness exercises reported in the Appendix further support the common trends assumption and a causal interpretation of the difference-in-difference estimates. A triple-difference specification with state-by-year fixed effects to control for annual state-level shocks yields similar results to the double difference specification (Appendix Table A1 Panel C). Additionally, a model with distributed lags and leads, which is equivalent to an event study research design implemented for a continuous treatment variable (Schmidheiny and Siegloch, 2020), supports the conclusion that there are neither substantial pre-trends in abortions or births in advance of travel distance changes nor dynamic treatment effects (Goodman-Bacon, 2018). These results support a causal interpretation of the difference-in-difference estimates of the effects of travel distance. See Appendix Table A4 and Figure A1 and the associated discussion for further information.

[13] Appendix Table A2 demonstrates that the results are robust to excluding various combinations of controls.

4 Results

Table 2 presents the results of difference-in-difference models corresponding to Equation 1. The estimates in Column 1 based on the linear specifications of travel distance indicate that a 100-mile increase in travel distance reduces abortion rates by 16.4% ($p < 0.01$).[14] Column 2 presents estimates based on a quadratic specification of distance to allow for non-linear effects. To facilitate interpretation, Panel A of Figure 2 visually depicts these estimates, illustrating the predicted effect of increases in travel distance ranging from 0 to 200 miles (x-axis) from initial travel distance values of 0, 50, or 100 miles. The results confirm prior evidence from Texas (Fischer et al., 2018; Lindo et al., 2020) and Wisconsin (Venator and Fletcher, 2020) of a diminishing marginal effect of travel distance on abortions: an increase in distance from 0 to 100 miles is estimated to reduce abortions by 20.5% ($p < 0.01$), while an increase from 100 to 200 is estimated to reduce abortions by 12.7% ($p < 0.01$).

Evaluating at the sample mean abortion rate of 11.92 (Table 1), the predicted 20.5% reduction in abortions due to an increase in travel distance from 0 to 100 miles corresponds to 2.4 fewer abortions per 1,000 women aged 15-44. While these reductions in abortions may correspond to increases in births, it also is theoretically possible that they represent response in sexual behavior and reduction in unintended pregnancies or that they represent substitution from abortions in formal medical settings to self-induced or "self-managed" abortions not captured by abortion surveillance (Ralph et al., 2020).[15] As a bounding exercise, if the entirety of the observed reduction in abortions is explained by reductions in unintended

[14] Throughout the discussion of the results as well as in all figures depicting marginal effects, I calculate exact percent effects as $100 \times (exp(\beta \Delta X) - 1)$ and use the delta method to construct standard errors and p-values.

[15] State reciprocal reporting agreements vary, and some states do not account for abortions their residents obtain out of state. As a result, an additional possibility is that the observed reduction in abortions is explained by increases in abortions obtained from out-of-state providers. To assess this possibility, I limit the sample to counties for which the nearest abortion facility is in-state throughout the entirety of the sample period. If out-of-state travel is an important factor driving the results for the full sample, then the estimated effect of travel distance should be much smaller for this sample. The results, reported in Columns 5 and 6 of Appendix Table A3 are substantively the same as those in Table 2, however, suggesting that interstate travel is not a major driver of the observed effect of distance. The estimated reductions in births further support this view.

pregnancies and/or increases in self-managed abortions, then increasing travel distances reduced observed abortions with 0 corresponding effect on birth rates. At the other extreme, if the entirety of the reduction in abortions results in pregnancies carried to term, then the estimated 2.4 reduction in the abortion rate would translate to a 2.4 increase in the birth rate, a 3.9% increase in births relative to the sample mean.

Table 2 presents estimated effects on births, which are well within these theoretical bounds and suggest that in fact a large fraction of the observed reduction in abortions translates to increased births. Columns 3-4 present results estimated for the sample of counties for which abortion statistics also are available, which are extremely similar to those estimated for all counties in the continental U.S. presented in Columns 5-6. Based on the estimates of the quadratic model for the full set of counties (Column 6), an increase in distance from 0 to 100 miles is estimated to increase births by 2.4% ($p < 0.01$) and an increase in distance from 100 to 200 miles is estimated to increase births by 1.6% ($p < 0.01$). These results, depicted visually in Panel B of Figure 2, support the conclusion that observed reductions in abortions due to travel distance represent women who would have obtained an abortion but for the distance, and who give birth as a result of not reaching a provider.

Figure 3 explores the possibility that these effects vary for different demographic groups. This figure plots the estimated effect of an increase in travel distance from 0 to 100 miles on births by race and age group.[16] The first marginal effect for the full sample at the top of the plot corresponds to the estimated 2.4% reduction in births based on Column 6 of Table 2 for all women. The remaining estimated effects are based on corresponding models estimated separately by race and ethnicity and age. The results suggest that all racial, ethnic, and age groups are responsive to travel distance, but that non-Hispanic Black women and women aged 15 to 24 are particularly so. The estimated effect of an increase in travel distance from 0 to 100 miles is 2.1% ($p < 0.01$) for white women versus 3.3% ($p < 0.01$) for black women. It is 5.0% ($p < 0.01$) for women aged 15-19 and 3.4% ($p < 0.01$) for women aged 20-24 versus

[16] Such an exercise is only possible for birth outcomes because most state health agencies do not report county resident abortions by demographic group.

1.4% ($p < 0.01$) for women aged 25-29.

5 Conclusion

The modal woman seeking an abortion in the United States is a low-income mother in difficult life circumstances. In a large survey conducted in 2014, an estimated 97% of patients seeking abortions were older than age 18; 59% had previously given birth; and 75% live below 200% of the poverty line; and 55% report a recent disruptive life event including the death of a close friend or family member, losing a job, breaking up with a partner, or falling behind on rent or a mortgage (Jones and Jerman, 2017c,b). The evidence afforded by new county-level panel data measuring abortion access and outcomes indicates that for many of these women travel distance is an important dimension of the cost of obtaining abortions. An increase in travel distances from 0 to 100 miles is estimated to prevent 1 in 5 women seeking abortions from reaching a provider, the majority of whom give birth as a result.

This new evidence based on a national approach confirms previous findings estimated in the specific contexts of Texas (Fischer et al., 2018; Lindo et al., 2020) and Wisconsin (Venator and Fletcher, 2020), and indicates that the causal effects of travel distances afforded by natural experiments in those two states generalize to the rest of the country. For many women seeking abortions, travel distances that may seem modest to some observers in fact pose insurmountable obstacles.

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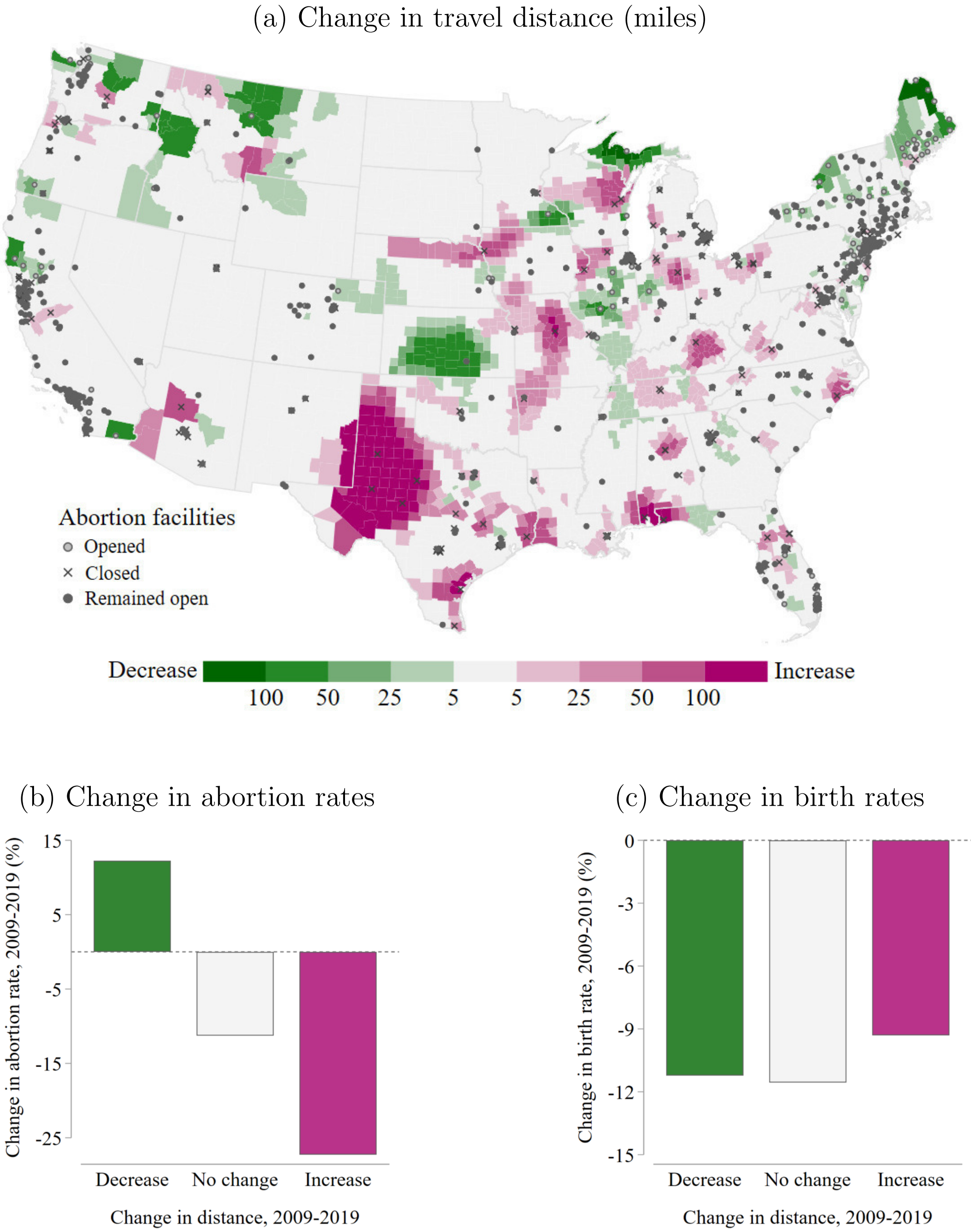
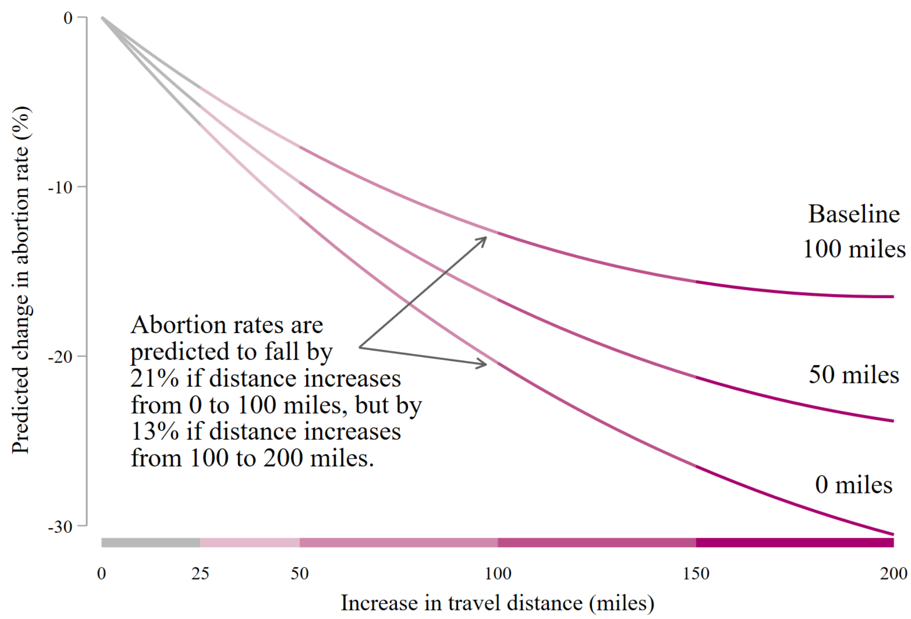
Figure 1
Change in travel distance to the nearest abortion facility and associated changes in abortion and birth rates, 2009-2019



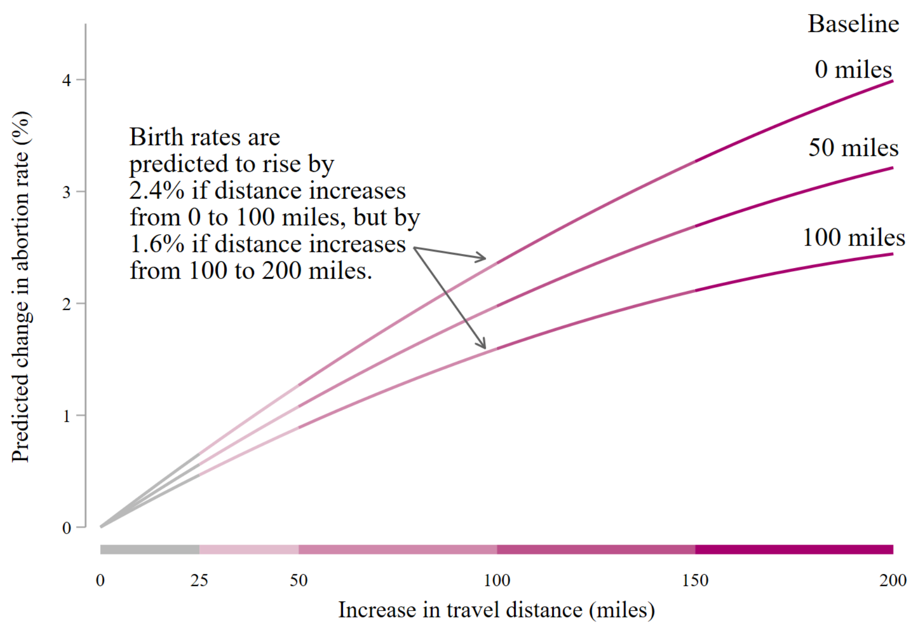
Notes: Figures represent county-level changes in travel distance to the nearest abortion facility between January 1, 2009 and January 1, 2019. Distances are measured between county population centroids and facility coordinates. Bar charts describe growth rates of population-weighted mean resident abortion rates for reporting counties and birth rates for all counties. Sources: NCHS (2020); SEER (2019); data collected by author. See text for full information regarding definitions and sources.

Figure 2
Predicted effect of increasing travel distances on abortion and birth rates

(a) Abortions

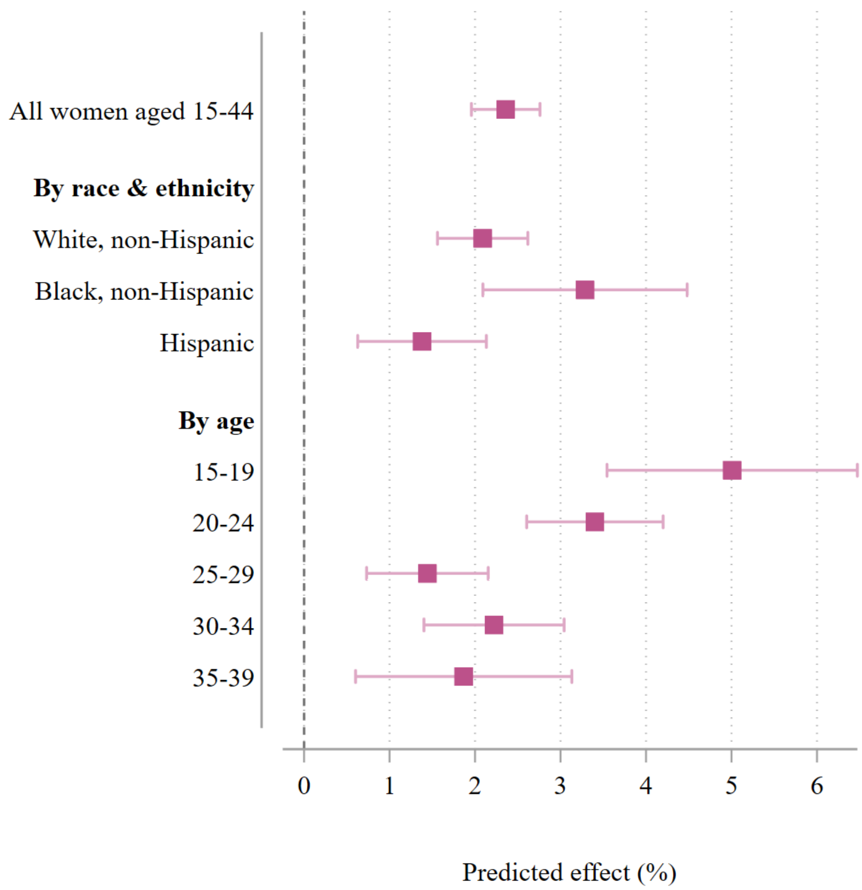


(b) Births



Notes: Estimated effects of travel distance based on difference-in-difference Poisson specifications of abortion and birth counts by county of residence represented in Columns 2 and 6 of Table 2.

Figure 3
Predicted effect of an increase in travel distance on births, by race



Notes: Estimated effects of a 100-mile increase in travel distance from a baseline of 0 miles. All estimates are based on a difference-in-difference Poisson specifications of birth counts by county of residence as a quadratic function of travel distances. These specifications correspond to that in Column 6 of Table 2 for all births, but are estimated separately for the indicated ethnic or age group.

Table 1
Summary Statistics

	source	mean	s.d.
Outcomes			
Abortion rate (per 1,000 women)	Author	11.86	8.30
Birth rate (per 1,000 women)	NCHS, SEER	62.15	9.44
Demographic Controls			
Percent of female population aged 15-44 that is			
Non-Hispanic white and age 15-19	SEER	9.25	4.64
Non-Hispanic white and age 20-24	SEER	9.75	4.99
Non-Hispanic white and age 25-29	SEER	9.94	3.57
Non-Hispanic white and age 30-34	SEER	9.65	3.61
Non-Hispanic white and age 35-39	SEER	9.39	3.87
Non-Hispanic white and age 40-44	SEER	9.78	4.37
Non-Hispanic black and age 15-19	SEER	2.57	2.40
Non-Hispanic black and age 20-24	SEER	2.68	2.61
Non-Hispanic black and age 25-29	SEER	2.56	2.51
Non-Hispanic black and age 30-34	SEER	2.35	2.29
Non-Hispanic black and age 35-39	SEER	2.24	2.19
Non-Hispanic black and age 40-44	SEER	2.21	2.18
Hispanic and age 15-19	SEER	3.64	3.42
Hispanic and age 20-24	SEER	3.52	3.27
Hispanic and age 25-29	SEER	3.41	3.11
Hispanic and age 30-34	SEER	3.31	2.95
Hispanic and age 35-39	SEER	3.19	2.91
Hispanic and age 40-44	SEER	2.96	2.84
Percent of females 18-44 with			
Less than high school degree	Census	9.91	4.95
High school degree	Census	21.96	5.93
Some college	Census	37.71	7.07
College degree	Census	30.41	11.35
Urbanized population (%)	Census	83.16	22.95
Economic controls			
Unemployment rate	BLS	6.56	2.84
Poverty rate	SAIPE	14.75	5.39
Median household income (10,000 $2018)	SAIPE	6.30	1.70
Reproductive Policy Controls			
I(1-Trip mandatory waiting period)	Author	0.26	0.43
I(2-Trip mandatory waiting period)	Author	0.23	0.42
I(Medicaid family planning expansion)	Author	0.78	0.41
I(Insurance mandate for contraception)	Yordan	0.87	0.32
Healthcare Policy Controls			
I(ACA Medicaid Expansion)	Author	0.33	0.47
Welfare Policy Controls			
Max welfare benefit for family of 3 (100 $2018)	Author	4.83	2.00
I(Family cap)	Author	0.39	0.49

Notes: Population-weighted summary statistics calculated for United States counties for 2009-2019. Variables constructed by author rely on multiple sources. See text and replication package for additional information.

Table 2
Difference-in-difference estimates of the effect of travel distance on abortion and birth rates

	Abortions		Births			
			Counties with abortion surveillance		All counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance (100s miles)	-0.179***	-0.275***	0.019***	0.027***	0.019***	0.027***
	(0.005)	(0.010)	(0.001)	(0.003)	(0.001)	(0.003)
Distance^2 (100s miles)		0.046***		-0.004***		-0.004***
		(0.004)		(0.001)		(0.001)
No. of counties	2173	2173	2168	2168	3107	3107
N	22026	22026	20441	20441	31069	31069

Notes: Estimated coefficients for Poisson models of county-level abortion and a one-year lead of birth rates for the population of women aged 15-44 observed from 2009 to 2020. All models include county and year fixed effects as well as the following time-varying county control variables: the fraction of the 15-44 female population that falls into detailed age and ethnicity groups interacted with quadratic time trends to account for trends in fertility outcomes; the unemployment rate, poverty rate, and median household income; educational attainments of the female population aged 15-44; state policies governing mandatory waiting periods for abortions, over-the-counter access to emergency contraception, Medicaid family expansion waivers, and contraceptive mandates for private insurers. These control variables are listed and summarized in Table 1, and all variables and sources are described and documented in the text and replication package. $^*p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$.

Appendix

This appendix presented a series of robustness checks and other models intended to evaluate the internal validity of the difference-in-difference research design and causal interpretation of the results.

Robustness of results to WOLS and alternative controls

Panel A of Table A1 reproduces the results of the Poisson specification reported in Table 2. Panel B produces results using ordinary least squares regression weighted by county population of women of childbearing age in which the outcomes is the log abortion or birth rate. In the weighted OLS models, observations with 0 counts are dropped because the log of 0 is undefined. A comparison of Panels A and B reveals that the weighted OLS results are very similar to the results using a Poisson specification.

Table A2 demonstrates that the results in Table 2 also are robust to alternative sets of controls for county and state time-varying demographics, economic, and policy conditions. Panel A reproduces the results in Table 2 using the full set of controls. Panel B reports results using only the demographic controls, as labeled in Table 1. Panel C reports results with no control variables; these models include only the travel distance explanatory variable of interest and county and year fixed effects.

Robustness of abortion results to alternative sub-samples

Columns 1-2 of Table A3 reproduce the results in Columns 1-2 of Table 2, which are based on an unbalanced sample of counties in states reporting county resident abortion counts in any years between 2009 and 2019. Most of the imbalance arises from the fact that only about half of state health departments have reported 2019 abortion counts at the time of this analysis. Columns 3-4 of Table A3 illustrate that these results are robust to using a balanced panel of only those counties for which abortion counts are available in every year from 2009 to 2019, demonstrating that these results are very similar.

As documented in the replication package, state health department vary in how they track residents' travel to other states to obtain abortions. Ten states—Alabama, Colorado, Georgia, Idaho, Kansas, Mississippi, Missouri, North Carolina, Texas, and Washington—track and account for abortions residents obtain out of state, while the remaining 22 states report only county resident abortion counts for abortions obtaining in state. This raises the possibility that some fraction of the estimated reduction in abortions due to increased travel distances reported in Table 2 reflect increased out-of-state travel. To evaluate this possibility, I limit the sample only to those counties for which the nearest abortion facility is located within the same state in every year of the sample period. The results, reported in Columns 5-6 of Table A3 are very similar to those in Table 2 and support the view that interstate travel is not a primary driver of the estimated abortion effects.

As illustrated in Figure 1, the identifying variation in travel distance in these models is generated by distance changes across the country. However, the distance changes occurring in Texas, which are largely driven by the enforcement of an admission privileges requirement in 2013 that results in the rapid closure of nearly half of the state's abortion facilities, stand out as particularly large. The Texas natural experiment also is the subject of three papers estimating the effects of travel distance (Quast et al., 2017; Fischer et al., 2018; Lindo et al., 2020). To assess whether the Texas closures and resulting travel distance changes are driving the results observed in the present national study, I drop Texas from the sample and report results identified by the remaining variation in travel distance in Columns 7-8 of Table A3. These results indicate similar effects of travel distance, indicating that the observed effects are not driven solely by Texas.

Triple difference specification

In the difference-in-differences specifications in Table 2, the effect of travel distance is identified by within-state variation in distances, with controls for time-varying county and state demographic, economic, and policy conditions. To allow for the possibility of state shocks such as unobserved policies or cultural shifts that are correlated with facility operations and travel distance, I additionally estimate triple difference specifications with state-by-year fixed effects. This demanding specification did not converge using a Poisson model, so I estimated it with weighted OLS using log rates as outcomes. (Recall that Table A1 demonstrates that the difference-in-differences results are robust to selecting a Poisson or WOLS model.) These results, presented in Panel C of Table A1 are very similar to those reported based on the primary difference-in-difference Poisson specification, supporting the credibility of the common trends assumption underlying causal inference.

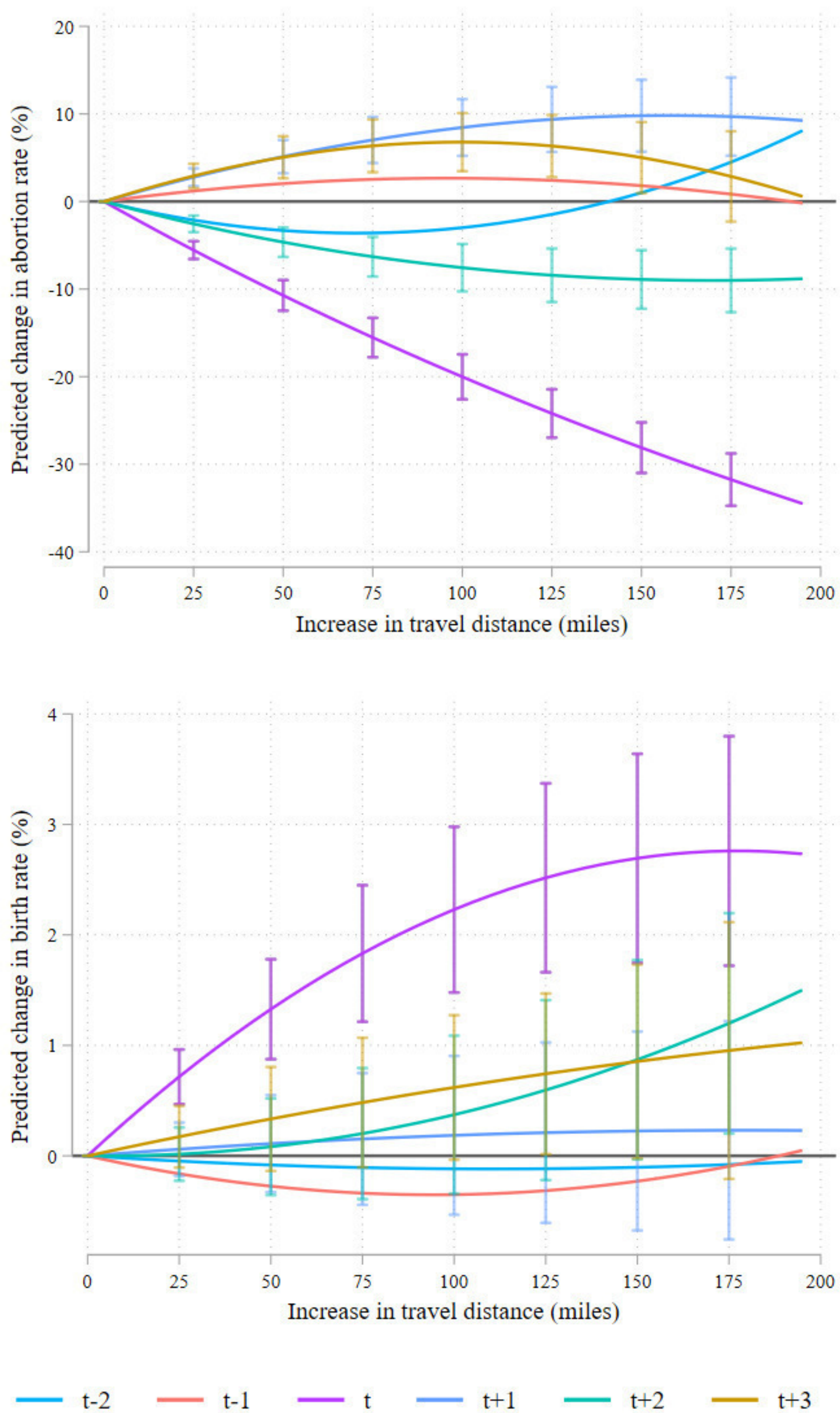
Distributed lead specification

As Goodman-Bacon (2018) shows, difference-in-differences estimates can be sensitive to time period selection and biased in the presence of heterogeneous treatment effects. In the context of the continuous and continuously-varying travel distance treatment in the analyses in these papers, the most straightforward and practical approach to evaluating this potential threat to causal inference is to implement a model with distributed lags and leads, which is equivalent to an event study (Schmidheiny and Siegloch, 2020).

The outcome window in the main analysis covers 11 years from 2009 to 2019. The data include travel distances through 2021, so the use of up to 3 travel distance leads does not reduce the sample used for the analysis. However, the data do not allow for observation of travel distances prior to 2009, so each sequential lag introduced into the model removes an additional year from the lower end of the observation window. I implement a model with 2 lags and 3 leads, which utilizes observations from 2011 to 2019. Table A4 reports the results of these models with distributed lags and leads. For ease of interpretation, Figure A1 depicts the estimated effect of an increase in travel distance ranging from 0 to 200 miles (x-axis) from a baseline of 0 miles in times t-2 through t+3 based on the results in Columns 2 and 4 of Table A4. The results indicate substantial and immediate effects of an increase in travel distance, and provide no evidence of dynamic effects into the following two years whereby the reductions in abortions or increases in births further evolve. Moreover, the results do not indicate that future changes in travel distances affect current outcomes.

Hence the results of a model with distributed lags and leads, which is equivalent to an event study, supports the common trends assumption and offers no evidence of dynamic treatment effects. This supports an interpretation of the difference-in-difference estimates in the primary specifications as causal effects of travel distance which persist over time.

Figure A1
Marginal effect of a 25-mile increase in travel distance: Distributed Leads Model



Notes: Estimated marginal effect of increase in travel distance on abortions (Panel A) and births (Panel B) from an initial distance of 0 miles. Estimates are based on difference-in-difference Poisson specifications corresponding to those in Columns 2 and 6 of Table 2, but with the addition of distributed lags and leads of travel distance.

Table A1

Robustness of results in Table 2 to weighted OLS and triple-difference specifications

	Abortions		Births			
			Counties with abortion surveillance		All counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Estimates reported in Table 2						
Distance (100s miles)	-0.179***	-0.275***	0.019***	0.027***	0.019***	0.027***
	(0.005)	(0.010)	(0.001)	(0.003)	(0.001)	(0.003)
Distance^2 (100s miles)		0.046***		-0.004***		-0.004***
		(0.004)		(0.001)		(0.001)
No. of counties	2173	2173	2168	2168	3107	3107
N	22026	22026	20441	20441	31069	31069
Panel B: WOLS						
Distance (100s miles)	-0.188***	-0.170***	0.015***	0.013	0.017***	-0.004
	(0.015)	(0.044)	(0.004)	(0.012)	(0.004)	(0.011)
Distance^2 (100s miles)		-0.007		0.001		0.008**
		(0.015)		(0.004)		(0.004)
No. of counties	2209	2209	2206	2206	3108	3108
N	21342	21342	19832	19832	31056	31056
Panel C: WOLS DDD Specification						
Distance (100s miles)	-0.157***	-0.141***	0.010**	-0.002	0.012***	-0.021*
	(0.017)	(0.044)	(0.005)	(0.013)	(0.005)	(0.012)
Distance^2 (100s miles)		-0.006		0.005		0.012***
		(0.015)		(0.004)		(0.004)
No. of counties	2209	2209	2206	2206	3108	3108
N	21342	21342	19832	19832	31056	31056

Notes: Panel A reproduces the estimated effects of travel distance on abortion and birth rates presented in Table 2. Panel B presents alternative estimates based on a weighted ordinary least squares (WOLS) specification in which the outcome variables are the log county abortion rate (Columns 1-2) or log county birth rate (Columns 3-6). Counties with zero counts are excluded from the analyses of log birth rates. Panel C adds state-by-year fixed effects of the WOLS specification to account for year-specific statewide shocks to the outcomes. All models include state and year fixed effects and the full set of control variables.

Table A2
Robustness of results in Table 2 to alternative sets of controls

	Abortions		Births			
			Counties with abortion surveillance		All counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Estimates reported in Table 2						
Distance (100s miles)	-0.179***	-0.275***	0.019***	0.027***	0.019***	0.027***
	(0.005)	(0.010)	(0.001)	(0.003)	(0.001)	(0.003)
Distance^2 (100s miles)		0.046***		-0.004***		-0.004***
		(0.004)		(0.001)		(0.001)
Panel B: Demographic controls only						
Distance (100s miles)	-0.204***	-0.290***	0.022***	0.033***	0.020***	0.031***
	(0.005)	(0.010)	(0.001)	(0.003)	(0.001)	(0.003)
Distance^2 (100s miles)		0.042***		-0.005***		-0.005***
		(0.004)		(0.001)		(0.001)
Panel C: No controls						
Distance (100s miles)	-0.216***	-0.302***	0.009***	0.033***	0.010***	0.037***
	(0.004)	(0.010)	(0.001)	(0.003)	(0.001)	(0.003)
Distance^2 (100s miles)		0.041***		-0.011***		-0.012***
		(0.004)		(0.001)		(0.001)
No. of counties	2173	2173	2168	2168	3107	3107
N	22026	22026	20441	20441	31069	31069

Notes: Panel A reproduces the estimated effects of travel distance on abortion and birth rates presented in Table 2. Panel B presents alternative estimates with demographic controls only. Panel C presents alternative estimates with no controls. All specifications are difference-in-difference Poisson models with county and year fixed effects as described in the text.

Table A3

Robustness of estimated effects on abortions in Columns 1-2 of Table 2 to alternative samples

	Full Sample		Balanced Panel		In-State Only		Drop Texas	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance (100s miles)	-0.179***	-0.275***	-0.188***	-0.290***	-0.154***	-0.303***	-0.225***	-0.239***
	(0.005)	(0.010)	(0.005)	(0.010)	(0.005)	(0.011)	(0.008)	(0.017)
Distance^2 (100s miles)		0.046***		0.049***		0.071***		0.011
		(0.004)		(0.004)		(0.005)		(0.011)
No. of counties	2173	2173	1912	1912	1792	1792	1919	1919
N	22026	22026	20558	20558	17148	17148	19233	19233

Notes: Columns 1-2 reproduce the estimated effects of travel distance on abortion rates presented in Columns 1-2 of Table 2. The remaining columns present estimates based on the same specification but using different samples of counties: Columns 3-4 are estimates using a balanced sample of counties reporting abortions annually from 2009-2018; Columns 5-6 are estimates using only those counties for which the nearest provider is located within the same state in each year from 2009-2018; Columns 7-8 are estimates excluding Texas from the sample. All models correspond to the difference-in-difference Poisson specifications presented in Table 2 and described in the text.

Table A4
Distributed lags and leads

	Abortions		Births	
	(1)	(2)	(3)	(4)
Distance_{t-2}	0.004	0.055***	0.001	-0.007
	(0.010)	(0.020)	(0.002)	(0.005)
Distance^2_{t-2}		-0.029***		0.004**
		(0.008)		(0.002)
Distance_{t-1}	0.051***	-0.104***	-0.002	-0.002
	(0.012)	(0.024)	(0.003)	(0.006)
Distance^2_{t-2}		0.074***		0.001
		(0.010)		(0.002)
Distance_{t}	-0.224***	-0.230***	0.013***	0.031***
	(0.012)	(0.024)	(0.003)	(0.005)
Distance^2_{t}		0.007		-0.009***
		(0.010)		(0.002)
Distance_{t+1}	0.046***	0.119***	0.003	0.003
	(0.011)	(0.022)	(0.003)	(0.005)
Distance^2_{t+1}		-0.038***		-0.001
		(0.009)		(0.002)
Distance_{t+2}	-0.035***	-0.111***	0.007***	-0.000
	(0.011)	(0.022)	(0.003)	(0.005)
Distance^2_{t+2}		0.033***		0.004*
		(0.008)		(0.002)
Distance_{t+3}	0.030**	0.132***	0.005*	0.007
	(0.013)	(0.032)	(0.003)	(0.007)
Distance^2_{t+3}		-0.066***		-0.001
		(0.021)		(0.004)
No. of counties	1439	1439	3107	3107
N	11512	11512	24856	24856

Notes: Reports results of Table 2 estimated with addition of distributed lags and leads. The sample of counties in the models of abortion counts is limited to those counties for which a balanced panel is available. The addition of the lags reduces the observation window from 2009-2019 to 2011-2019. All specifications are difference-in-difference Poisson models with county and year fixed effects as described in the text.