THE IMPACT OF WOMEN'S HEALTH CLINIC CLOSURES ON FERTILITY

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ABSTRACT

In recent years, the government of Texas has enacted multiple restrictions and funding limitations on women's health organizations affiliated with the provision of abortion services. These policies have caused numerous clinic closures throughout the state, drastically reducing access to reproductive health care. We study the impact of these clinic closures on fertility rates by combining quarterly snapshots of health center addresses from a network of women's health centers with restricted geotagged data of all Texas birth certificates for 2008–13. We calculate the driving distance to the nearest clinic for each zip code and quarter, and find that an increase of 100 miles to the nearest clinic results in a 1.2 percent increase in the fertility rate. This increase is driven by a 2.4 percent increase in the fertility rate for unmarried women, while there is no statistically significant change for married women.

KEYWORDS: family planning, contraception, fertility rate, birth rate, law, Texas
JEL CLASSIFICATION: H75, I18, J13

I. Introduction

Women's health care, particularly access to reproductive health care and abortion services, provokes a wide range of reactions across the political spectrum. For example, the 2016 Democratic Party platform supported "access to quality reproductive health-care services, including safe and legal abortion," as part of Democrats' commitment to "protecting and advancing reproductive health, rights, and justice" (Democratic National Committee 2016). In contrast, the Republican Party platform (Republican National Committee 2016) pledged to "oppose the use of public funds to perform or promote abortion" and asserted "the sanctity of human life." Since January 2017, the current presidential administration and majority-Republican Congress have repeatedly sought to cut funding for women's health organizations that provide abortion services or are associated with organizations that do.[1] This consensus among the president and Congressional leadership has increased

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1 For example, the eight-page Health Care Freedom Act spent two and a half pages legislating that certain health-care organizations that provide for abortions could not receive federal funds (see the Health Care Freedom Act 2017, https://www.budget.senate.gov/imo/media/doc/HealthCareFreedomAct.pdf). This sentiment was also mentioned in the summary of the White House's fiscal year 2018 budget (see "The President's

doi:10.1162/ajhe_a_00123
American Journal of Health Economics 5(3): 334–359

the possibility that previous state-level funding cuts to women's health and family planning services could now happen nationwide.

To illuminate the potential consequences of such a national policy, this paper studies one set of previous funding cuts and restrictions that led to clinic closures, namely those of the state of Texas. In particular, we investigate the following question: How does ease of access to women's health and family planning clinics affect the fertility rate? These specialized clinics offer contraceptive and sometimes abortion services, in addition to often serving as a primary point of access to the health-care system (Frost, Gold, and Bucek 2012). Therefore, we hypothesize that clinic closures and the resulting increases in driving distance lead to a higher fertility rate, and in this paper we seek to identify and to quantify that effect.

Given a change in the fertility rate, we also test whether particular demographic subgroups are driving this overall change. Based on the demographic composition of abortion patients—who may face higher barriers to reproductive health services and therefore approximate the "marginal" patients affected by clinic closures—we hypothesize that any increase in the fertility rate resulting from lack of access to family planning services is concentrated among unmarried women (Jerman, Jones, and Onda 2016). In addition, we predict that decreased access to family planning services affects both white and Hispanic women, affects women of both low and high educational attainment, has some effect on births beyond the first child, and lowers the mean maternal age at birth.[2]

We test our hypotheses using a recent series of politically motivated public policy changes in Texas. First, in 2011, Texas cut its two-year family planning budget from $111 million to $38 million, and gave funding priority to primary care. Consequently, by 2012, 146 clinics had lost state funds, 53 clinics had closed, and 38 clinics had reduced their hours of operation (White et al. 2012).[3] As a result, there were almost 50 percent fewer organizations to help poor women plan their pregnancies (Culp-Ressler 2012), with many basic contraceptive services now out of reach (Jones 2012).

Furthermore, in 2013, Texas excluded provider networks affiliated with abortion providers from the Women's Health Program. This program was largely Medicaid-funded, with the federal government contributing about $30 million per year, or 90 percent of program costs. Because of Texas's action, Texas lost substantial federal funding at the end of 2012 (Smith 2013). Given that our birth record data extend only through 2013, we observe the effect of clinic closures occurring no later than in the first quarter of 2013. Therefore, our analysis is primarily informative for quantifying the effects of the first Texas policy.

Fiscal Year 2018 Budget: Overview," https://www.whitehouse.gov/sites/whitehouse.gov/files/omb/budget/fy2018/fact_sheets/2018%20Budget%20Fact%20Sheet_Budget%20Overview.pdf).

2 The Guttmacher Institute report (Jerman, Jones, and Onda 2016) notes that approximately 85 percent of abortion patients nationwide are unmarried. They also report that, in 2008, 37 percent of abortion patients nationwide are white, 25 percent are Hispanic, 39 percent have a high school diploma or less, 61 percent have had at least one previous birth, and 58 percent are in their 20s.

3 Because of our nondisclosure agreement with the data provider, we are unable to say how many of these overall closures were associated with the provider network we study.

These policy changes and the resulting clinic closures allow us to test the impact of ease of access to care on fertility rates. We follow the approach of Lu and Slusky (2016) by using health center addresses from a particular network of women's health centers to calculate the driving distance to the nearest clinic in that network for each zip code. We also calculate fertility rates for each zip code. Then, using a within estimator, we estimate the impact of a relative change in driving distance on the relative fertility rate. We find that an increase in driving distance to the nearest clinic leads to a statistically significant increase in fertility rates, and that the effect is concentrated among unmarried women.[4]

Clinic closures could affect fertility rates through two complementary mechanisms: (1) lack of access to contraception, with more women having unplanned pregnancies due to using a less effective or no method of contraception; and (2) lack of access to abortion.[5] While we are unable to directly disentangle these two mechanisms in this paper, it is important to note that our results are largely driven by changes in access to contraception because the vast majority of clinics that closed during our time period of analysis only provided nonabortion family planning services.[6]

Our paper contributes to and draws on a well-established literature that investigates the impact of proximity to health-care providers (or other entities) on health and health-care outcomes (e.g., Goodman et al. 1997; Buchmueller, Jacobson, and Wold 2006; Currie et al. 2010; Anderson and Matsa 2011; Currie, Greenstone, and Moretti 2011; Currie and Walker 2011; Hill 2014, 2018; Rossin-Slater 2013; and Lu and Slusky 2016). This literature validates our methodological approach of estimating the impact of geographic access to a women's health-care provider on local behavior, while controlling for time-invariant differences across granular regions.

We also contribute to a literature on public policy and fertility (e.g., Amuedo-Dorantes, Averett, and Banksak 2016; Kroeger and La Mattina 2017; Rau, Sarzosa, and Urzúa 2017) and, more specifically, family planning programs and fertility. Focusing on increases rather than decreases in access to care, Bailey (2012) finds that the introduction of US family planning programs in the 1960s and 1970s was associated with "significant and persistent reductions in fertility" at the county level. Our paper complements and builds on these findings by examining whether reductions in access to care have the opposite effect on fertility rates as increases in access to care. Our findings suggest that the effects are indeed symmetric. We show that even in a much more recent time period, when publicly funded family planning programs have been established for decades and contraceptive use is much more widespread, reduced geographic access to care is an important factor in family planning, especially for unmarried women.

Our results are also consistent with a broader literature on abortion and fertility, including with Coleman and Joyce (2011) and Grossman, Baum, et al. (2014), who find that

4 Our results are robust to using driving time instead of driving distance.

5 The abortion mechanism is consistent with Grossman et al. (2017), who find that an increase in distance to the nearest facility providing abortion services was associated with a decline in abortions between 2012 and 2014 in Texas.

6 For completeness, we generally refer to access to both contraceptive and abortion services throughout this paper, but our discussion focuses more on access to contraception.

new stringent abortion requirements and restrictions in 2004 and 2013–14, respectively, reduced abortions in Texas; with Girma and Paton (2013), who find that the 2003 parental consent law had no effect on underage pregnancies; and with Cintina (2017), who finds that increased access to emergency contraception in Washington State reduced the abortion rate. Additionally, Musse (2017) finds that in Nepal, living closer to a clinic that offers pregnancy tests decreased the proportion of women who did not know they were pregnant.

Finally, this paper complements recent work by Stevenson et al. (2016) studying the impact of Texas funding restrictions on contraception and childbirth covered by Medicaid; work by Packham (2017) on family planning funding cuts and teen fertility rates; work by Cunningham et al. (2019) and Quast, Gonzalez, and Ziemba (2017) on abortion clinic closures and abortion rates; work by Fischer, Royer, and White (2018) on the impact of reduced access to abortion and family planning services on abortion, births, and contraceptive purchases; and older work by Kearney and Levine (2012). In particular, a complementary study by Stevenson et al. (2016) finds changes in contraceptive use, and Cunningham et al. (2019) and Quast, Gonzalez, and Ziemba (2017) find reductions in abortion. While our results are broadly consistent with these findings, our empirical approach is different. For example, Stevenson et al. (2016) define a binary "treatment" based on the presence of a clinic in a county since the funding restrictions more strongly affected these counties compared with those without a clinic. Packham (2017), Cunningham et al. (2019), Quast, Gonzalez, and Ziemba (2017), and Fischer, Royer, and White (2018) also all use a county-level approach to study the impact of clinic closures.

Our approach, on the other hand, is substantially more granular because we calculate changes in driving distance to the nearest clinic at the zip code level. This level of granularity is important for examining changes along the intensive margin—that is, changes in driving distance—for urban and rural residents alike. In particular, our measure is likely to capture more variation in clinic access, both for residents of urban areas, where there may be multiple clinic locations with only some closing over time, and for residents of rural areas, where there may not be a single clinic within an entire county during the time period of analysis.

Additionally, we study the marriage margin and find substantially different effects for married and unmarried women. Among the studies mentioned above, the only one that examines this particular margin is the supplemental analysis of Fischer, Royer, and White (2018), albeit with a less granular approach. Examining heterogeneous outcomes by marital status is important since unplanned children born to unmarried mothers are more likely to experience worse economic outcomes.[7] Specifically, children born to unmarried mothers are more likely to grow up in unstable living arrangements, be in poverty, receive less education, and have sex at early ages (McLanahan and Sandefur 1994; Demo and Cox 2000; Haveman, Wolfe, and Pence 2001; Thomas and Sawhill 2005). As adults, they are more likely to be idle (neither in school nor employed) and if employed have lower

7 This result has been established using a variety of econometric techniques, such as comparing unmarried mothers who have twins with those who have singletons (Bronars and Grogger 1994) and using availability of funded abortion services as an instrumental variable (Korenman, Kaestner, and Joyce 2001).

income (McLanahan and Sandefur 1994; Aquilino 1996; Carlson and Corcoran 2001; Musick 2002; Amato 2005). We therefore need to understand whether the policy changes in Texas and the resulting clinic closures have increased the number of children born into adverse economic circumstances.

Our overall finding of a negative relationship between the fertility rate and access to contraceptive and abortion services is also consistent with the mechanisms underlying changes in education, career, and fertility trends among women (see, e.g., Goldin 2014, 2015) and specifically increased maternal age (Matthews and Hamilton 2016). However, the effects that we find for this particular policy context potentially represent backtracking from the general progress in women's empowerment that has resulted from family planning.

II. Data

This paper uses two primary data sources: (1) quarterly snapshots of clinic addresses from a network of women's health and family planning clinics, and (2) birth certificates from Texas. These data sets are supplemented with several other sources, including the coordinates of zip code centroids, a zip code to Zip Code Tabulation Area (ZCTA) mapping, and total population and population by demographic subgroups at the ZCTA level.

The primary exogenous variable—driving distance to the nearest clinic—is calculated from end-of-quarter snapshots of clinic locations between Q3 2007 and Q1 2013, from a network of women's health and family planning clinics. This nonprofit network was one of the largest recipients of funding from both the Texas Department of State Health Services (DSHS 2016) Family Planning Program and the Women's Health Program. Its clinics provide a range of family planning and reproductive health-care services, and its patient mix is very similar to that of clinics receiving federal funding under Title X.[8]

As described in further detail below, we use these end-of-quarter snapshots to calculate the driving distance from each zip code centroid to the nearest clinic at the end of each quarter.[9] These driving distances are then assigned to the period of time three to four quarters before each birth to approximate a mother's access to care before and in the early phases of her pregnancy.

The primary outcome variable—the general fertility rate[10] (hereafter referred to as simply the "fertility rate") in each quarter in each zip code—is calculated from a restricted version of all administrative birth certificates from the DSHS's Vital Statistics office for

8 Fowler et al. (2011) report that Title X patients are approximately 90% female, 25% ages 15–19, 30% ages 20–24, 20% ages 25–29, and 25% ages 30 and over.

9 The network that we study includes clinics throughout the United States. Therefore, the nearest clinic may be located in a neighboring state (i.e., Texas, New Mexico, Colorado, Kansas, Oklahoma, Arkansas, Louisiana, or Arizona) for some women.

10 The general fertility rate (GFR) is defined as the number of children born to women ages 15–49 over a given time period, divided by the number of women in the population ages 15–49, and then multiplied by 1,000 to scale it to be per 1,000 women. The GFR is typically calculated on an annual basis. In this study, we multiply our quarterly fertility rates by 4 in order to make them comparable to other GFRs.

2008–13.[11] The restricted version contains two variables essential to our analysis: the mother's zip code and the child's birthdate, a combination of which allows each birth to be matched to the appropriate driving distance to the nearest clinic. We also observe demographic variables, including the mother's age, race, ethnicity, educational attainment, marital status, and number of prior live births.

We supplement these two primary data sets with four other data sets. To calculate the distance from each zip code to the nearest clinic, we use zip code centroid coordinates from SAS (2013). To calculate the fertility rate in each zip code in each quarter per population subgroup, we first map the mother's zip code to a ZCTA[12] using the crosswalk for 2011 (i.e., the midpoint of our data set) from UDS Mapper (2011),[13] and then match each ZCTA with its population (total and by subgroup) from 2008 to 2012 from the US Census's five-year estimates (US Census Bureau 2016a).[14]

We also include the county-level unemployment rate as a control in our regressions, since there is a strong negative link between unemployment rates and fertility (Ananat, Gassman-Pines, and Gibson-Davis 2013; Currie and Schwandt 2014; Buckles, Hungerman, and Lugauer 2017). Analogous to how we assign driving distance to each ZCTA-quarter, we calculate the mean monthly unemployment rate for 9 and 12 months before the last month of each quarter.[15]

III. Methodology

The methodology in this paper is analogous to that of Lu and Slusky (2016), which uses a within estimator to study the impact of changes in driving distance to the nearest clinic on the incidence of preventive care. More broadly, by constructing a continuous measure of driving distance that proxies for access to care, our analysis fits within a longstanding literature on the effects of the time price of health care (e.g., Acton 1975; Coffey 1983; Vistnes and Hamilton 1995; Clarke 1998; Lourenço and Ferreira 2005; Jeuland et al. 2010; Brent 2017). Our results are consistent with the broad consensus in this literature that demand for health care is indeed sensitive to nonmonetary costs, such as travel time.

We first construct several key variables. For each Texas zip code and quarter, we calculate the geodesic (i.e., crow-flies) distance from the zip code centroid to each clinic in

11 We do not expect the finding of Krieger et al. (2016) of an inverse relationship between state funding for abortion and infant deaths to affect our results. Specifically, we use births (regardless of how long the newborn lives) to calculate the birth rate, while they focus on infant mortality before age one. A change in the infant death rate should not have any effect on the fertility rate as we are calculating it.

12 This is necessary because some zip codes such as post office boxes have official populations of zero.

13 The US Census does not provide a formal crosswalk.

14 Our results are robust to using quarterly population levels for each ZCTA, using a linear interpolation between the 2000 Census and the midpoints of the Census's five-year population estimates for 2007–11 through 2012–16 (US Census Bureau 2016a).

15 Our results are robust to using alternative measures to control for local labor market conditions, such as the annual county-level employment-to-population ratio from the BEA (2006–14) and the annual ZCTA-level share of tax returns that report receiving unemployment benefits.

our primary clinic location data set using the Haversine formula. Then, for the clinic that has the shortest geodesic distance from a given zip code centroid, we calculate the driving distance, using Google Maps.[16]

We map each mother's zip code of residence to the corresponding ZCTA because some zip codes have no official population. We then aggregate the number of births by ZCTA and quarter, match each ZCTA with its population from the US Census's five-year estimates (US Census Bureau 2016a), and calculate the quarterly fertility rate for each ZCTA. Our measures are generally consistent with the literature, and the five-year population estimates are a reasonable proxy for midperiod population since the population remained relatively stable between 2008 and 2012. Additionally, our results are robust to using other count-based specifications that do not require population estimates.

For each ZCTA, we then use the driving distance data from the zip code of the same name.[17] We estimate driving distance around the time of conception as follows: first we assign each birth to the end-of-quarter date (e.g., February 12 is assigned to March 31), and then we calculate the mean of driving distance lagged three quarters (e.g., June 30 of the previous year) and driving distance lagged four quarters (e.g., March 31 of the previous year). This approach provides a reasonable estimate of the mean driving distance during the period shortly before (e.g., when a woman may be seeking contraceptives) and after conception (e.g., when a woman may be seeking an abortion). While we use the mean of driving distance lagged three and four quarters as our main measure of driving distance around the time of conception, our results are also robust to using the driving distance lagged four quarters.

Our primary econometric specification is within-ZCTA, over time:

$$y_{zt} = \beta_0 + \beta_1 \frac{dist_{z,t-3} + dist_{z,t-4}}{2} + \beta_2 \frac{UR_{z,t-3} + UR_{z,t-4}}{2} + \boldsymbol{\beta}_3 \boldsymbol{\zeta}_z + \boldsymbol{\beta}_4 \mathbf{q}_t + \boldsymbol{\beta}_5 \boldsymbol{r}_t + \varepsilon_{zt},$$

where the unit of analysis is ZCTA z in quarter-year t. y is a measure of the fertility rate, and *dist* is the driving distance from a ZCTA to the nearest clinic. *UR* refers to the county-level unemployment rate (BLS 2014), which we use to control for the effects of the regional labor market on the fertility rate. Similar to our calculation of driving distance around the time of conception, this labor market measure is incorporated as the mean of the unemployment rate lagged three and four quarters.[18]

$\boldsymbol{\zeta}$, $\mathbf{q}$, and $\boldsymbol{r}$ are ZCTA, quarter, and year fixed effects. We cluster standard errors by county, which is generally more conservative than clustering by more granular geographies and because there are likely across-ZCTA, within-county correlations that

16 Lu and Slusky (2016) looked at the impact of driving distance increases on preventive care rates and used several alternative measures of clinic proximity, and found comparable results.

17 This is partly out of convenience and partly because zip codes always map to ZCTAs of the same name if those ZCTAs exist. That is, there is never a case where X → Y but Y → A. If X → Y, then Y → Y.

18 Since the unemployment rate is reported on a monthly basis, we assign the unemployment rate in the last month of a quarter as the end-of-quarter unemployment rate.

should be accounted for. Our main sample is all ZCTAs with a population greater than zero.

This empirical approach is then applied to demographic subgroups, including by age group, marital status, ethnicity, and educational attainment.[19] We also examine fertility rates by birth parity and quartiles of the unemployment rate. These subgroup analyses supplement the main analysis by providing a clearer picture of who is most affected by clinic closures that lead to higher fertility.

We interpret our estimates as causal for two primary reasons. First, our use of ZCTA fixed effects controls for time-invariant differences across ZCTAs in unobservable characteristics, such as attitudes toward family planning that could correlate with clinic closures. Second, based on the policy context and empirical evidence, we argue that the changes in driving distance over time are sufficiently exogenous to produce robust estimates. Specifically, we observe that the majority of clinic closures occurred during the quarter in which the 2011 funding cut took effect. This timing strongly suggests that most of the clinic closures we study were largely affected by the funding cut. Furthermore, following Lahey's (2014) approach, we confirm that we cannot predict whether a clinic will experience a subsequent closure using 2007 fertility rates, ZCTA population for females ages 15–49, or the pre-policy-change trend in fertility rates. Based on these analyses, we conclude that the closures from politically motivated funding cuts during our time frame are sufficiently uncorrelated with previous values of our outcome variables and with population that we can consider them exogenous and move forward with our analysis.

IV. Results

A. SUMMARY STATISTICS: DEMOGRAPHIC CHARACTERISTICS

Table 1 shows summary statistics at the ZCTA-quarter level.[20] The mean male population count is approximately equal to the mean female population count.[21] Among females, nearly half are ages 15–49. Among these women of reproductive age, about half are married, and about one-third are Hispanic and one-third are non-Hispanic white. Of the female adult population (ages 18 and over), roughly comparable shares do not have a high school diploma, only have a high school diploma, have some college but not a bachelor's degree, and have at least a bachelor's degree (panel a).

19 Hicks-Courant and Schwartz's (2016) fascinating result that family planning clinics are associated with a lower high school dropout rate does not directly relate to our analysis, since we study women ages 18 and over, for whom educational attainment is relatively stable.

20 There are 22 quarters (Q3 2008–Q4 2013) of data and 1,870 ZCTAs with nonzero population. This yields $22 \times 1{,}870 = 41{,}140$ observations for the primary regressions below.

21 The male population is relevant for calculating the crude birth rate (which is defined as births per total population) and ensuring our results are robust to this measure. If the population were substantially imbalanced (e.g., in North Dakota), then the results may not be robust. In our case, however, as shown in the Online Appendix (http://www.mitpressjournals.org/doi/suppl/10.1162/ajhe_a_00123), the results are robust to using the crude birth rate.

TABLE 1. Summary statistics

	(1)	(2)	(3)
	Mean	SD	% of total
	($N = 41{,}140$ ZCTA-quarters)		
Panel a: Count of population that is …			
Female	6,771	8,347	50%
Female & age 15–19	486	670	4%
Female & age 15–49	3,315	4,291	25%
Female & age 15–49 & married	1,630	2,176	12%
Female & age 15–49 & not married	1,684	2,256	13%
Female & age 18+ & <HS diploma	1,081	1,539	8%
Female & age 18+ & HS diploma	1,395	1,518	10%
Female & age 18+ & some college	1,752	2,083	13%
Female & age 18+ & ≥bachelor's	1,263	1,956	9%
Female & age 15–44 & Hispanic	1,164	2,176	9%
Female & age 15–44 & non-Hispanic white	1,144	1,636	9%
Panel b: Count of births to mothers who are …			
Age 15–19	6.04	9.85	12%
Age 15–49	51.74	71.23	99.8%
Age 15–49 & married	29.79	42.60	57%
Age 15–49 & not married	21.95	33.81	42%
Age 18+ & <HS diploma	12.56	23.91	24%
Age 18+ & HS diploma	13.70	20.88	26%
Age 18+ & some college	13.79	19.73	27%
Age 18+ & ≥bachelor's	11.29	21.62	22%
Age 15–44 & Hispanic	25.31	49.72	49%
Age 15–44 & non-Hispanic white	17.72	25.49	34%
Having their 1st child	20.01	27.49	39%
Having their 2nd child	15.98	22.08	31%
Having their 3rd+ child	15.74	23.60	30%
Panel c: Fertility rate for mothers who are …	**Mean**	**ZCTAs**	**Comparison**
Age 15–49	62.44	1,870	15–49: 62.44
Age 15–19	49.67	1,725	15–49: 62.45
Age 15–44	72.59	1,862	15–49: 62.44
Age 15–49 & married	73.08	1,785	15–49: 62.50
Age 15–49 & not married	52.23	1,785	15–49: 62.50

TABLE 1. *Continued*

	(1) Mean	(2) SD	(3) % of total	
	(*N* = 41,140 ZCTA-quarters)			
Age 18+ & <HS diploma	50.43	1,662	18+: 39.97	
Age 18+ & HS diploma	42.62	1,662	18+: 39.97	
Age 18+ & some college	34.14	1,662	18+: 39.97	
Age 18+ & ≥bachelor's	38.82	1,662	18+: 39.97	
Age 15–44 & Hispanic or non-Hispanic white	74.43	1,621	15–44: 72.46	
Age 15–44 & Hispanic	86.79	1,621	15–44: 72.46	
Age 15–44 & non-Hispanic white	61.88	1,621	15–44: 72.46	
Panel d: Other (weighted by population)	**Mean**	**SD**	**Min**	**Max**
Mother's age (years)	26.5	2.9	15.0	47.0
Unemployment rate (%, 9–12 months ago)	6.9	2.1	1.8	19.5
Driving distance (miles, 9–12 months ago)	42.7	44.6	0.3	289.9
Change in driving distance (miles)	15.3	43.4	−17.3	276.9

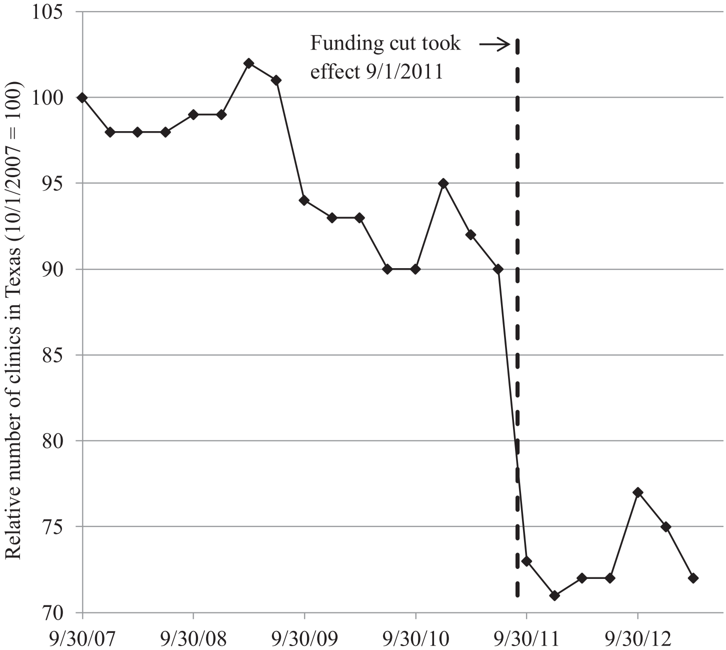
Notes: The unit of analysis in this table is a ZCTA-quarter. Each mean corresponds to a particular subgroup's population count or fertility rate, across all ZCTAs and quarters of data. Panel d is weighted by the population of females ages 15–49 in each ZCTA. For the unemployment rate and the driving distance to the nearest clinic, the mean is taken across ZCTAs and quarters of the mean 9 and 12 months before the birth quarter, as described in the methodology section above.

Among women with births, nearly all are ages 15–49, the majority are married, half are Hispanic, and less than one-third are non-Hispanic white women. Across the different educational categories, the mean number of births across ZCTA-quarters is comparable. Finally, about 40 percent of births are first births, and about one-third are second births (panel b).

Mean fertility rates vary across ZCTAs for each subgroup of women. The number of ZCTAs varies by subgroup since we cannot calculate a fertility rate if there are no women of reproductive age in a given subgroup and ZCTA. To facilitate comparisons across related subgroups (e.g., fertility rates among women ages 18-plus with varying levels of educational attainment), the rightmost column recalculates the overall fertility rate for the relevant age range and for the same set of ZCTAs as used in the primary specification. Overall, fertility rates are higher among married women, Hispanic women, and women with lower educational attainment (panel c).

The mean maternal age at birth is 26.5 years, and the mean unemployment rate around the time of conception is 6.9 percent, though there is wide variation in both variables. The mean driving distance to the nearest clinic around the time of conception was 42.7 miles, and the mean change in driving distance over the course of the sample period was an increase of 15.3 miles. Again, as with many of the variables, this measure also varies

FIGURE 1. Relative number of clinics in Texas



Note: Number of clinics is relative to October 1, 2007, which is normalized to 100.

widely, with some ZCTAs experiencing a distance *decrease* by up to 17.3 miles and some experiencing an increase by almost 300 miles (panel d).[22,23]

B. SUMMARY STATISTICS: CLINICAL DATA

Figure 1 shows the relative number of clinics in Texas in this network for each quarter between Q3 2007 and Q1 2013, normalizing the number on October 1, 2007 (the start date of our clinic data set) to 100. While a small share of clinics closed prior to the 2011 budget cut, Figure 1 shows that the overwhelming majority of closures occurred during the quarter in which the funding cut took effect. Specifically, we observe a 19 percent drop in the number of clinics in this network between June 30, 2011, and September 30, 2011. The timing of this precipitous drop is consistent with our expectation that the majority of clinic closures during our study period were largely driven by budget cuts, which took effect on September 1, 2011 (White et al. 2015; Stevenson et al. 2016).

22 While an event study graph would likely be useful here, our data are not conducive. Not only is the treatment continuous as opposed to discrete, but many ZCTAs are affected by multiple closures, which makes it ambiguous when to define "event-time zero." These heavily affected ZCTAs play an important role in our results. Therefore, focusing on ZCTAs only affected by one closure would not be comparable.

23 The mean increase in driving distance is consistent with Gerdts et al. (2016), which surveyed Texas-resident women seeking abortions and found that clinic closures increased driving distance.

Figure 2 provides a visualization of the effect of clinic closures on Texas women's changes in driving distance. The shaded ZCTAs (US Census Bureau 2016b) indicate the change in driving distance to the nearest clinic over time, using the earliest and latest data available to us on clinic addresses. In panel a, we see that while a handful of ZCTAs experienced small decreases in driving distance, the overwhelming majority saw driving distance increase. Some areas, particularly in the west, north, and southeast of Texas, experienced driving distance increases of greater than 100 miles, and as high as 280 miles. Panel b shows the same result but at a larger scale for Texas's five largest metropolitan areas (in decreasing order by population). Here we see variation within cities, where some ZCTAs in Dallas/Fort Worth/Arlington (DFW), Houston, San Antonio, and Austin saw small increases (0–20 miles) while others saw minimal if any change. El Paso, however, saw enormous increases in driving distance (over 100 miles) for all of its ZCTAs, which resulted from closures of all of the El Paso clinics in the network we study.[24]

C. IMPACT OF DRIVING DISTANCE ON FERTILITY RATE

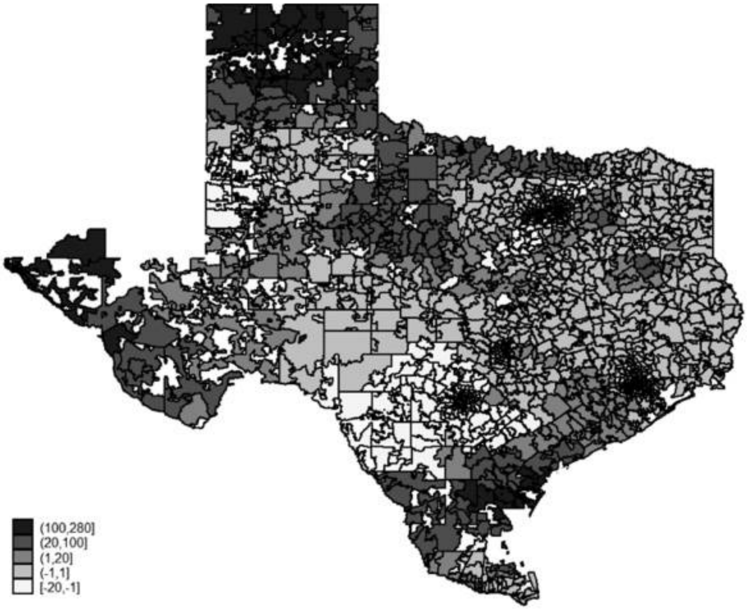
Table 2 presents our main regression results, which restrict the sample to ZCTAs with a nonzero population of women ages 15–49. We gradually add each control and set of fixed effects. First, the pooled ordinary least squares (OLS) specification of fertility rate on driving distance shows a strong, positive causal relationship, with a 100-mile[25] increase in the distance to the nearest clinic raising the fertility rate by approximately 3.2 children per 1,000 women ages 15–49 (column 1). Adding year and quarter fixed effects makes minimal difference (column 2). Adding ZCTA fixed effects in column 3 substantially reduces the magnitude of the coefficient of interest. An increase in distance of 100 miles to the nearest clinic now leads to a statistically significant coefficient of 0.727 children per 1,000 women ages 15–49, but the estimate is substantially more precise. In relative terms, this coefficient represents an increase of approximately 1.2 percent from a sample mean rate of 62.44 children per 1,000 women ages 15–49. This change in the coefficient magnitude is as expected, given the large cross-sectional differences across ZCTAs in Texas, and suggests that ZCTAs that experienced larger increases in driving distance were also those that would have experienced relatively larger increases (or relatively smaller decreases) in fertility even in the absence of clinic closures. For instance, it is plausible that clinics were more likely to close in areas that already had a more negative overall attitude toward family planning. ZCTA fixed effects control for the time-invariant portion of these differences, and therefore will be included in all other estimations in this paper. Overall, the positive coefficient shown in column 3 is consistent with Bailey's (2012) result that there is a negative relationship between the fertility rate and public funding for family planning.

24 If we exclude El Paso, then the key coefficient of interest is no longer statistically significant. It is, however, close in magnitude to our main result and the same sign, which alleviates potential concerns that clinic closures had a different effect on fertility rates in the rest of Texas than in El Paso.

25 This is the same unit used in Lu and Slusky (2016). The raw coefficient would be for 1 mile, which is not particularly meaningful; 100 miles represents a severe, though not implausible, increase in driving distance resulting from the only clinic in a particular geography closing. Lu and Slusky (2016) also tested multiple nonlinear functions of driving distance and found comparable results.

FIGURE 2. Change in driving distance from October 1, 2007, to March 31, 2013

Panel a



Panel b

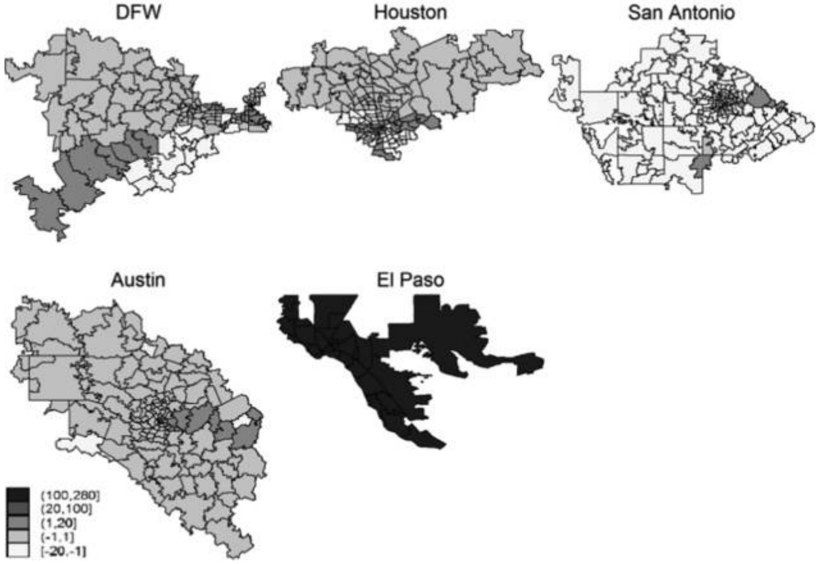


TABLE 2. Main result of impact of driving distance on fertility rate

	(1) Fertility rate 15–49	(2) Fertility rate 15–49	(3) Fertility rate 15–49	(4) Fertility rate 15–49	(5) Fertility rate 15–49	(6) Poisson for births to women 15–49
Driving distance - 100 mi	3.199[b] (1.519)	3.501[b] (1.499)	0.727[a] (0.218)	0.753[a] (0.209)	0.753[a] (0.209)	0.012[a] (0.003)
Unemployment rate				−1.135[a] (0.154)	−1.134[a] (0.154)	−0.017[a] (0.002)
Observations	41,140	41,140	41,140	41,140	41,008	41,008
R^2	0.005	0.035	0.101	0.104	0.104	
Number of ZCTA			1,870	1,870	1,864	1,864
Mean	62.44	62.44	62.44	62.44	62.45	
Year fixed effects		X	X	X	X	X
Quarter fixed effects		X	X	X	X	X
ZCTA fixed effects			X	X	X	X

Notes: Robust standard errors clustered at the county level are in parentheses. OLS results in columns 1–5 are weighted by the population of females ages 15–49. [a]$p < 0.01$, [b]$p < 0.05$, [c]$p < 0.10$.

Our result, though, is from the opposite direction, showing that removing access to family planning services raises the fertility rate.

Column 4 additionally controls for the unemployment rate. While the coefficient on this additional variable is negative and statistically significant (consistent with Currie and Schwandt (2014)), the main "driving distance" coefficient of interest is virtually unchanged. Therefore, for most of the results below, we will always include the local unemployment rate as a control.

Finally, the table includes a count model, specifically a fixed-effect Poisson. This alternative specification is appropriate because the count of births in each ZCTA in each quarter is discrete and nonnegative (see Simcoe 2008). To implement this model, we first limit the sample to ZCTAs that record at least one birth during the sample period, which slightly reduces the total number of ZCTAs and produces OLS results that are comparable to our main finding (column 5). We then apply the fixed-effect Poisson model (average marginal effects shown in log points in column 6) and find that births increase by 1.2 percent for every 100-mile increase, which is identical to the result found from OLS on the fertility rate.[26,27]

26 The Poisson regression does not explicitly use population weights because maximum likelihood estimation of the Poisson regression is already equivalent to generalized weighted least squares (see Charnes, Frome, and Yu 1976).

27 Our results are also robust to not weighting by ZCTA population; to alternative measures of the fertility rate, such as the crude birth rate; and to alternative nonlinear econometric specifications, such as a more

D. PLACEBO TEST

Table 3 checks the validity of our result by including a control for the driving distance to the nearest clinic in the quarter after the quarter of birth. Including all ZCTAs, as shown in column 1, provides only a marginally statistically significant result because driving distance is highly persistent over time, since the majority of ZCTAs were not affected by a clinic closure. Therefore, the other columns of this table consider only the ZCTAs where the driving distance to the nearest clinic changed between 2007 and 2013. For each minimum threshold of driving distance (e.g., changed by more than 0 miles, changed by more than 10 miles, etc.), we first reestimate the main result and then additionally control for the distance after birth. For each threshold, the main coefficient of interest is still statistically significant both alone (even-numbered columns) and when the additional control is added (odd-numbered columns starting with 3). Moreover, the coefficient on driving distance in the quarter after birth is always statistically insignificant.

E. SUBGROUP ANALYSIS

Table 4 shows results from an investigation into whether the fertility increase reported in Table 2 is driven by changes for married or unmarried mothers.[28] We expect the effect to be different based on the substantial literature showing that access to contraception and abortion strongly affects unmarried women (e.g., Goldin and Katz 2002; Bailey 2006; Ananat and Hungerman 2012). Column 1 repeats our main result, excluding the ZCTAs that do not have at least one birth to a married woman and one birth to an unmarried woman at some point during the sample period.[29] Since this restriction applies to only a few low-population ZCTAs, the results are almost identical to our main specification. Columns 2 and 3 then stratify by marital status and show, respectively, that an increase of 100 miles to the nearest clinic results in a statistically insignificant change in the fertility rate for unmarried women and a 2.4 percent increase in the fertility rate for married women. These two coefficients are also statistically significantly different from each other at the 5 percent level.

Our finding of differential effects by marital status—with the effects concentrated among unmarried women—differs from recent literature on the impact of reduced access to abortion and family planning services. Specifically, Fischer, Royer, and White (2018) find no effect for unmarried mothers but an increase in fertility for married mothers. Given that in 2008 and 2014, approximately 85 percent of women obtaining abortions were unmarried, we believe our results to be more plausible (Jerman, Jones, and Onda

general fixed-effect negative binomial specification (see Allison and Waterman 2002). They are also robust to a nonlinear function of driving distance, such as dummy variables for each 50-mile category, a quadratic specification, and an exponential specification.

28 Unfortunately, we cannot track mothers across births, nor do we know how long mothers are married, so we cannot assess to what degree pregnancy is influencing marriage rates. Still, the results are so stark that we are not concerned overall.

29 We will reestimate our main result for each subset of ZCTAs considered in each table. While this will produce slightly different versions of our primary estimate, each version will be comparable to the rest of the estimates in the table.

TABLE 3. Impact of driving distance on fertility rate, controlling for driving distance after birth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Absolute change in distance > (miles)	−∞	0	0	10	10	25	25	50	50	100	100
Driving distance - 100 mi	0.514[c]	0.907[a]	0.618[c]	0.946[a]	0.807[a]	1.095[a]	1.080[a]	1.282[a]	1.246[a]	0.777[b]	0.783[b]
	(0.310)	(0.264)	(0.342)	(0.244)	(0.286)	(0.261)	(0.265)	(0.408)	(0.407)	(0.339)	(0.363)
Driving distance - 100 mi, quarter after birth	0.514		0.648		0.426		0.073		0.226		−0.070
	(0.534)		(0.544)		(0.548)		(0.377)		(0.442)		(0.352)
Obs.	41,140	26,334	26,334	10,758	10,758	5,830	5,830	3,740	3,740	2,552	2,552
R^2	0.105	0.113	0.114	0.086	0.086	0.099	0.099	0.114	0.114	0.131	0.131
Number of ZCTA	1,870	1,197	1,197	489	489	265	265	170	170	116	116
Mean	66.61	63.83	63.83	66.95	66.95	68.47	68.47	69.35	69.35	66.61	66.61

Notes: The dependent variable in all regressions is the general fertility rate for all women 15–49. Robust standard errors clustered at the county level are in parentheses. All regressions control for the county-level unemployment rate and contain birth year, birth quarter, and ZCTA fixed effects. The absolute change in distance is calculated as the magnitude of the difference in lagged driving distance corresponding to Q3 2008 and Q4 2013. All regressions are weighted by the population of females ages 15–49. $^a p < 0.01$, $^b p < 0.05$, $^c p < 0.10$.

TABLE 4. Impact of driving distance on fertility rate by marital status

	(1) Fertility rate 15–49	(2) Fertility rate 15–49 married	(3) Fertility rate 15–49 not married
Driving distance - 100 mi	0.755[a]	0.177	1.297[a]
	(0.209)	(0.374)	(0.347)
Observations	39,270	39,270	39,270
R^2	0.120	0.072	0.066
Number of ZCTA	1,785	1,785	1,785
Weight	Population female 15–49	Population female 15–49 married	Population female 15–49 not married
Mean	62.50	73.08	52.23

Notes: Robust standard errors clustered at the county level are in parentheses. All regressions control for the county-level unemployment rate and contain birth year, birth quarter, and ZCTA fixed effects. Results are for ZCTAs that have at least one married woman and one unmarried woman between the ages of 15 and 49. [a]$p < 0.01$, [b]$p < 0.05$, [c]$p < 0.10$.

TABLE 5. Impact of driving distance on the teen fertility rate

	(1) Fertility rate 15–49	(2) Fertility rate 15–19	(3) Fertility rate 15–49	(4) Fertility rate 15–19
Restricted to ZCTA with:	Women 15–19	Women 15–19	At least 5 teen (i.e., 15–19) births in each quarter	At least 5 teen (i.e., 15–19) births in each quarter
Driving distance - 100 mi	0.752[a]	−0.626[c]	1.036[a]	0.715[b]
	(0.212)	(0.332)	(0.300)	(0.330)
Observations	37,950	37,950	6,270	6,270
R^2	0.123	0.095	0.336	0.270
Number of ZCTA	1,725	1,725	285	285
Mean	62.45	49.67	71.59	66.94

Notes: Robust standard errors clustered at the county level are in parentheses. All regressions control for the county-level unemployment rate and contain birth year, birth quarter, and ZCTA fixed effects. All regressions are weighted by the population of females ages 15–49. [a]$p < 0.01$, [b]$p < 0.05$, [c]$p < 0.10$.

2016). Additionally, our results potentially have less measurement error than others in the literature since we implement a more granular zip code–level analysis.

Table 5 further unpacks the 1.2 percent increase in the fertility rate shown in Table 2. Here, we look at the teen (or adolescent) fertility rate, which is defined as births to mothers

TABLE 6. Impact of driving distance on mother's age

	(1) Fertility rate 15–49	(2) Mother's age (years)
Driving distance - 100 mi	0.797[a]	−0.091[a]
	(0.212)	(0.017)
Observations	37,290	37,290
R^2	0.123	0.081
Number of ZCTA	1,864	1,864
Mean	62.68	27.25

Notes: Robust standard errors clustered at the county level are in parentheses. All regressions control for the county-level unemployment rate and contain birth year, birth quarter, and ZCTA fixed effects. Results are for ZCTAs that have at least one birth. All regressions are weighted by the population of females ages 15–49. [a]$p < 0.01$, [b]$p < 0.05$, [c]$p < 0.10$.

ages 15–19 divided by the population of women ages 15–19.[30] We limit the analysis here to ZCTAs that have at least one woman between the ages of 15 and 19. Column 1 repeats our main regression on this (large) subset and finds comparable results. Column 2 then looks at the teen fertility rate in OLS. This result is much noisier, having a different sign and only being statistically significant at the 10 percent level.

Columns 3 and 4 therefore reestimate the results using only the 285 ZCTAs that have at least five teen births in each quarter. This is intended to address the concern that perhaps the teen pregnancy results are inconclusive because of the larger amount of left censoring at zero than in the main regressions.

The result in column 3 for these ZCTAs with women ages 15–49 shows a 1.4 percent increase, comparable to our main result. Column 4, focusing on the teen rate, does find a much more consistent result of a relative increase of 1.1 percent, though it is only statistically significant at the 5 percent level, unlike the main findings of this paper. Our results are therefore broadly in agreement with Packham (2017), who found an increase in the teen fertility rate as a result of clinic closures.

Table 6 shows the impact of an increase in distance on the age of the mother. If women were having the same number of children as before but having them earlier, then the age of mothers should be decreasing. Column 1 repeats the main regression for ZCTAs with at least one birth (and therefore at least one data point on the age of the mother). Column 2 shows that an increase of 100 miles in driving distance to the nearest clinic leads to a decrease in the age of the mother by 0.09 years, or approximately one month.[31]

30 See http://data.worldbank.org/indicator/SP.ADO.TFRT.

31 Other stratifications, such as by ethnicity or educational attainment, do not produce results that are statistically significantly different across groups. See Online Appendix A for details.

V. Robustness Checks

The Online Appendices (http://www.mitpressjournals.org/doi/suppl/10.1162/ajhe_a_00123) contain results and discussion of several robustness checks. They are discussed here briefly. First, as described above, we unsuccessfully attempt to predict changes in driving distance using predetermined characteristics and trends. We also check our results for potential differences when stratifying by the distance to the Mexican border. This stratification addresses the potential concern that women near the border could access pharmaceuticals in Mexico either for contraception or to induce abortion, whereas those living farther from the border could not (Grossman, White, et al. 2014; Grossman et al. 2015). If so, we would expect a smaller coefficient for ZCTAs near the border and a larger coefficient for those farther from the border. However, we find the opposite to be true, with ZCTAs near the border having a larger and more precise estimate. This is likely because El Paso County, which is a main source of variation in driving distance, is also very close to the border, which makes it difficult to effectively test this question.

In addition, we rerun our analysis for larger levels of geographical aggregation, specifically at the county and commuting zone levels (USDA 2000). As expected, these results are substantially noisier than the results at the ZCTA level, though the coefficients are of a similar sign and magnitude.

Our results are also robust to clustering the standard errors at a greater level of aggregation than counties, including commuting zones (USDA 2000), US Bureau of Economic Analysis (BEA 1999) economic areas, and core-based statistical areas. When we cluster standard errors at a *smaller* level of aggregation, by ZCTA, we find that the standard errors actually increase relative to our main specification. While uncommon, this is possible if standard errors are more negatively correlated at the ZCTA level than at the county level (Cameron and Miller 2015). We continue to cluster the standard errors at the county level in our main specifications because there are likely across-ZCTA, within-county correlations that should be accounted for.

Finally, we implement a robustness check that additionally controls for whether the county had Medicaid Managed Care (MMC), which other researchers have found to affect birth outcomes and fertility (Aizer, Currie, and Moretti 2007; Kuziemko, Meckel, Rossin-Slater 2013).[32] We find no effect of controlling for MMC on our main result. However, this is partially because the rural counties that experienced MMC expansions in 2012 have low populations and therefore have a limited impact on the overall result in our population-weighted regression framework.

32 The names of the counties that changed to MMC before 2007 are from Kuziemko, Meckel, Rossin-Slater (2013). The names of the counties from the Medicaid Rural Service Areas that changed to MMC in 2012 (per https://hhs.texas.gov/sites/default/files//documents/laws-regulations/reports-presentations/2017/medicaid-chip-perspective-11th-edition/11th-edition-complete.pdf) are from page 2 of https://apps.hhs.texas.gov/providers/communications/2014/letters/IL2014-24.pdf and the map on page 13 of https://hhs.texas.gov/sites/default/files/documents/services/health/medicaid-chip/programs/provider-presentation.pdf.

VI. Discussion

Our results show that increases in driving distance to the nearest clinic lead to statistically significant increases in the fertility rate on the order of 1 to 2 percent, and that this increase is robust to a variety of specifications and sample restrictions.

Based on our main results, a back-of-the-envelope calculation indicates that these clinic closures led to an additional 690 births in Texas each year, mostly to unmarried women.[33] Monea and Thomas (2011) estimate that the mean taxpayer cost of a publicly subsidized unintended pregnancy in 2001 was $9,000, or about $12,000 in 2016 dollars. This alone would suggest an additional total public cost of more than $8 million per year. Further considering the approximately $10,000 per year in costs of raising a child for a single mother would suggest that the total direct costs of these extra children are comparable to the approximately $65 million in annual funding cut by Texas or lost in federal matching.[34] In addition, these estimates do not even take into account any indirect costs, such as to women's labor force productivity, the mental well-being of unintended children, and the additional economic consequences as described above of children born to unmarried women. Furthermore, if clinic closures lead to more closely spaced pregnancies, then that can also have adverse consequences since larger spacing between births has a positive effect on outcomes such as educational attainment (Black, Devereux, and Salvanes 2005; Buckles and Munnich 2012).

VII. Conclusion

In recent years, a primary cause of women's health clinic closures is the loss of public funding. Funding-related clinic closures, such as those in Texas, decrease women's ease of access to care—in the current analysis, we focus on increases in their driving distance to the nearest clinic, but closures could have indirect effects as well, such as overcrowding or increased fees at remaining clinics.[35] Our analysis shows that these clinic closures lead to higher fertility rates, likely through the combined effects of reduced access to contraception and abortion services. Furthermore, we find that fertility increases are concentrated among unmarried women.

This paper expands on Lu and Slusky (2016) by using comprehensive administrative data that cover all zip codes in Texas during 2007–13, and by focusing on a direct consequence of family planning clinic closures, namely fertility rates. An increase in fertility rates resulting from decreased access to family planning services can be interpreted as an increase in the number of unplanned pregnancies. When considering the impact of funding cuts, it is important to consider the effects of an increase in the number of unplanned pregnancies. Furthermore, funding cuts may actually lead to increases in future state outlays from decreased tax revenues (e.g., unplanned pregnancies may affect women's

33 690 children = 3,315 women ages 15–49 / ZCTA × 1,870 ZCTAs × 15.3 miles increase in driving distance × (0.727 children / 1,000 women) / "100 miles" × "100 miles" / 100 "1 miles" × "1,000 women" / 1,000 "1 women."

34 See http://www.cnpp.usda.gov/tools/CRC_Calculator/default.aspx.

35 See, for example, Conde (2012), Ku et al. (2012), White et al. (2012), and Zuzek (2013).

educational investments and subsequent earnings) and increased public expenditures (e.g., education and health-care spending) on the additional children born.

ACKNOWLEDGEMENTS

We are grateful to Elizabeth Ananat, Martha Bailey, Janet Currie, Donna Ginther, Bapu Jena, Ted Joyce, Katy Kozhimannil, Jason Lindo, Michael Richards, Analisa Packham, Daria Pelech, Aaron Schwartz, anonymous reviewers, and seminar and conference participants at the University of Kansas, Iowa State University, West Virginia University, NBER Summer Institute Children's Workshop, Yale University, Association for Public Policy Analysis and Management, Southern Economic Association, Texas A&M University, and KORA (the Danish Institute for Local and Regional Government Research) for their help and comments. We would also like to thank Paul Espinosa, Greg Smith, Scott Durham, and Justin Graham at the University of Kansas Information Technology for managing our HIPAA-compliant research server.

FUNDING INFORMATION

Partial funding support for this study is from the Department of Economics at the University of Kansas. We have no financial or other conflicts to disclose. The views presented are those of the authors and do not necessarily reflect those of Analysis Group, Inc. or its clients. All errors are our own.

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