

## The Impact of Women's Health Clinic Closures on Preventive Care<sup>†</sup>

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*We examine the impact of women's health clinic closures on women's preventive care use in Texas and Wisconsin using a unique policy context, data on clinic street addresses, and confidential respondent ZIP codes from the Behavioral Risk Factor Surveillance System. From a within-ZIP-code analysis, we conclude that an increase of 100 miles to the nearest clinic results in a decrease in the annual utilization rate of a clinical breast exam by 11 percent, a mammogram by 18 percent, and a Pap test by 14 percent. These estimates are generally larger for women of lower educational attainment. (JEL H75, I11, I18, J13, J16)*

Many women rely on publicly funded family planning and women's health clinics as their only recent source of care, including preventive care (Frost, Gold, and Bucek 2012; Guttmacher Institute 2016). Some of these organizations provide abortion services in addition to family planning and other reproductive health services. Consequently, such organizations may face politically motivated funding cuts, which may also impact important non-abortion outcomes and related services.

One primary cause of women's health facility closures is the loss of public funding. In the past few years, several states have attempted and in some cases succeeded in cutting public funding for women's health organizations that provide (or are affiliated with provision of) abortion services. These policies are exogenous to our outcomes of interest: women's cancer screenings and routine checkups. This is clearly stated by one of the state legislators responsible for recent funding changes in Texas,

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who said, "I don't think anybody is against providing health care for women. What we're opposed to are abortions."<sup>1</sup>

The exogeneity of these politically motivated funding cuts to women's preventive health care provides an ideal setting for studying the consequences of clinic closures. Previous estimates of the price elasticity of demand for health care services suggest that clinic closures should decrease utilization due to the increased opportunity cost of obtaining care (Manning et al. 1987, Ringel et al. 2002, Baicker and Goldman 2011). The magnitude of such a decrease, however, is ex ante unclear in this context since preventive care services may be available from numerous alternative providers, including primary care clinics and community health centers. Therefore, quantifying these effects is important both for this particular policy context and for better understanding the role of specialized women's health clinics in providing (primary) preventive care.

This paper is the first to quantify the impact of clinic closures on the incidence of preventive care, especially those closures resulting from exogenous policy factors such as funding cuts. By combining health center addresses from a national provider network and confidential respondent ZIP codes from the Behavioral Risk Factor Surveillance System (BRFSS), we are able to study the relationship between survey respondents' distance to the nearest clinic and their preventive care behavior over the 2007–2012 period. Our within-ZIP-code analysis reveals that increases in distance to the nearest clinic lead to statistically significant reductions in women's annual utilization of clinical breast exams and Pap tests. Additionally, we find that women of lower educational attainment tend to be particularly affected by clinic closures.

We contribute to and draw on two literatures. The first investigates the impact of proximity to health care providers on individual health and health care outcomes. Previous work has studied the effects of distance to medical care on the likelihood of hospitalization and mortality (Goodman et al. 1997), hospital closures on access to care (Buchmueller, Jacobson, and Wold 2006), family planning programs on fertility (Bailey 2012), and proximity to a Women, Infants, and Children clinic on a variety of mother and infant outcomes (Rossin-Slater 2013).

A separate literature focuses on abortion and contraceptives (e.g., Gruber, Levine, and Staiger 1999; Goldin and Katz 2002; Jacobson and Royer 2011). In particular, this literature studies the impacts of various types of abortion-related legislation on non-abortion outcomes like sexual behavior (Klick, Neelsen, and Stratmann 2012), socioeconomic outcomes of children (Pop-Eleches 2006), crime (Donohue and Levitt 2001), and fertility (Lahey 2014), but so far has not investigated the impacts on preventive health care.

These literatures as a whole validate our approach of estimating the impact of geographic access to a health care provider on individual-level behavior, while controlling for time-invariant differences across granular regions. However, prior work in this area lacks an explicit analysis of the relationship between proximity to a

<sup>1</sup> Belluck, Pam, and Emily Ramshaw. 2012. "Women in Texas Losing Options for Health Care in Abortion Fight." *New York Times*, March 7. <http://www.nytimes.com/2012/03/08/us/texas-womens-clinics-retreat-as-finances-are-cut.html>.

family planning or women's health clinic and preventive care use, especially in the context of politically motivated funding cuts and resulting facility closures. We hope that our analysis will fill this gap.

### I. Legislative Background

In this paper, we focus on two of the states, Texas and Wisconsin, where funding cuts have been the most impactful.<sup>2</sup> These two states are also geographically and culturally distinct from each other, so that using both in our analysis with the appropriate state-level controls should help to mitigate any state-specific trends.<sup>3</sup>

In 2011, Texas enacted severe funding cuts through two legislative channels. First, the Texas legislature cut the Department of State Health Services (DSHS) family planning budget by two-thirds (from a 2-year total of \$111 million to \$37.9 million). The remaining funds were allocated through a system that gave priority to organizations providing comprehensive primary care over those providing family planning services only. By 2012, out of 240 pre-legislation clinics included in the DSHS Family Planning Program, 53 had closed outright and another 38 had reduced their hours (White et al. 2012).

In addition, Texas passed in 2011 and subsequently implemented in 2013 a rule excluding provider networks affiliated with abortion providers from the Women's Health Program (WHP), which served low-income women.<sup>4</sup> This exclusion applies to an entire provider network, even if not all of the facilities in that network provide abortion services. Prior to this, the WHP was a Medicaid program, with the federal government contributing about \$30 million a year and paying 90 percent of the reimbursements (Texas Women's Healthcare Coalition 2013). However, upon implementation of the new rule, the federal government ceased its contribution because the rule violated federal law, which does not permit discrimination among qualified providers. Texas Governor Rick Perry said that the state would "go it alone" without the federal funding, despite not being able to reach as many women as under the original program.<sup>5</sup>

The impacts of these recent funding cuts on clinics and women throughout Texas have received extensive media coverage. For instance, a review of state records by *The Texas Observer* found that in the year following Texas's deep family planning

<sup>2</sup>In other states, funding cuts have been largely ineffective due to injunctions or to organizations reapplying for federal funds. (This information is based on a conversation with a representative of the organization providing our clinic locations database.)

<sup>3</sup>Using more than one state in our analysis allows us to control for state-specific trends and also is consistent with the fact that state-level funding cuts are part of a national politically motivated strategy. Separately running our main specification (without state-specific trends) by state provides extremely comparable results for Texas; the Wisconsin results are somewhat comparable, but it is difficult to draw precise conclusions due to factors such as the small sample size and a relatively large number of fixed effects. Fitting a model with separate variables and then testing whether the distance coefficients are equal for Texas and Wisconsin (i.e., a Chow test) reveals that we cannot reject equality in a majority of cases.

<sup>4</sup>While the implementation of this legislation (Senate Bill 7) occurred after our study period, it is mentioned because there may have been anticipatory closings and because it is further evidence of the political environment surrounding women's health clinics in Texas.

<sup>5</sup>Smith, Jordan. 2013. "Fewer Women Served Under Texas Women's Health Program." *Austin Chronicle*, July 31. <http://www.austinchronicle.com/daily/news/2013-07-31/number-of-women-served-under-texas-womens-health-program-drops/>.

budget cuts, “146 clinics have lost state funds, clumped mainly in the Panhandle, Central Texas, and on the border with Mexico. More than 60 of those clinics have closed their doors forever. The number of organizations that help poor women plan pregnancy has shrunk by almost half.”<sup>6</sup>

The funding cuts described above have had a direct impact on the provider network that we study. This paper uses a confidential database of health center street addresses for a particularly large national provider network (see Section II for more details).

Figure 1 shows this network's clinic locations in Texas and in the surrounding states at the beginning (panel A) and end (panel B) of the dataset, which correspond to before and after most of the funding cuts were enacted.<sup>7</sup> Qualitatively, it is clear that the number of clinics decreased substantially during this time, especially in Texas. Some of the clinic closures, particularly those in neighboring states or those occurring prior to 2011, may reflect the influence of non-policy factors as well, but it is clear from the political background that Texas legislation played a critical role in clinic closures during this overall period. We have also included clinics located in neighboring states because their closure may affect residents of Texas (or Wisconsin) due to proximity, and sometimes (as we will discuss for Wisconsin below) those closures are also strongly influenced by exogenous policy factors.

In Wisconsin, in 2011, Governor Scott Walker signed a budget that eliminated state and federal funding to several women's health centers. These cuts disproportionately affected organizations affiliated with abortion, despite the fact that many if not all of the defunded centers did not actually provide abortion services themselves.<sup>8</sup> Furthermore, the language of the budget is written in such a way that defunded clinics also cannot work with public (state) labs to read the tests that are part of their cancer screening process.<sup>9</sup>

Figure 2, analogous to Figure 1, shows the clinic locations in Wisconsin and the surrounding states at the beginning (panel A) and end (panel B) of the dataset.<sup>10</sup> While less stark than the before-and-after pictures of Texas, one can see clinic closures in Wisconsin and also in the bordering areas of Minnesota.<sup>11</sup>

Although Wisconsin and Texas are the focus of our analysis, some facility closures in neighboring states can also help us to determine the impact of

<sup>6</sup>Jones, Carolyn. 2012. “One Year Later, Cuts to Women's Health Have Hurt More Than Just Planned Parenthood.” *Texas Observer*, August 15. <http://www.texasobserver.org/one-year-later-cuts-to-womens-health-have-hurt-more-than-just-planned-parenthood/>.

The slight discrepancy from the clinic numbers reported in White et al. (2012) is most likely due to differences in the time periods reviewed.

<sup>7</sup>Note that the Medicaid WHP ended on December 31, 2012, so the impact of Senate Bill 7 may be greater in 2013 and subsequent years.

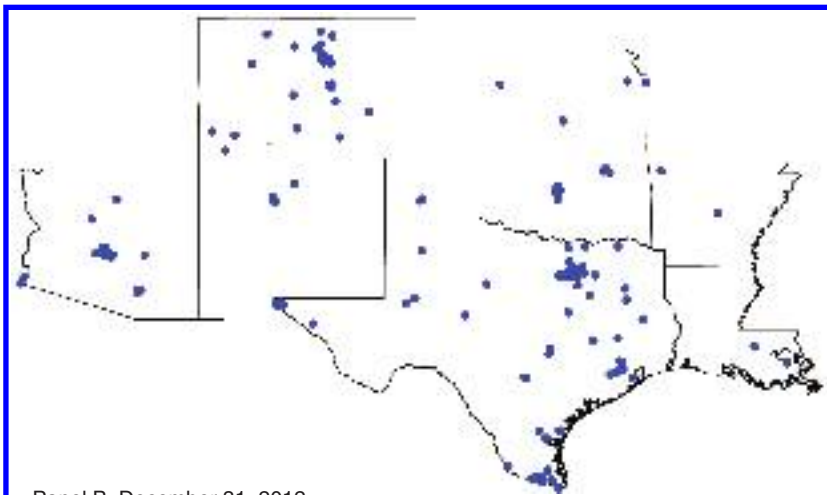
<sup>8</sup>For example: Laasby, Gitte. 2013. “Planned Parenthood, Citing Budget Cut, to Close Four Rural Wisconsin Centers.” *Journal Sentinel*, February 18. <http://www.jsonline.com/news/wisconsin/planned-parenthood-citing-budget-cut-to-close-four-rural-wisconsin-centers-ns8qp79-191710011.html>.

<sup>9</sup>Bassett, Laura. 2011. “Wisconsin Set to Defund Planned Parenthood, Slash Family Planning.” *Huffington Post*, August 21. [http://www.huffingtonpost.com/2011/06/21/wisconsin-fourth-state-to-defund-planned-parenthood\\_n\\_881504.html](http://www.huffingtonpost.com/2011/06/21/wisconsin-fourth-state-to-defund-planned-parenthood_n_881504.html).

<sup>10</sup>Clinics in the Lower Peninsula of Michigan would not be the nearest clinics to any Wisconsin ZIP code, so we exclude them from our analysis.

<sup>11</sup>The new clinics in southern Illinois that are included for completeness are unlikely to be the nearest clinics to any Wisconsin ZIP code and so will have minimal, if any, impact on our results.

Panel A. October 1, 2007



Panel B. December 31, 2012

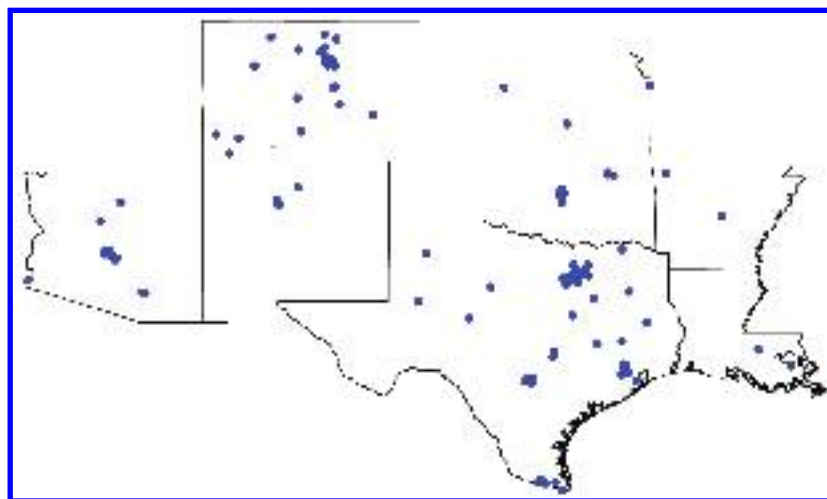
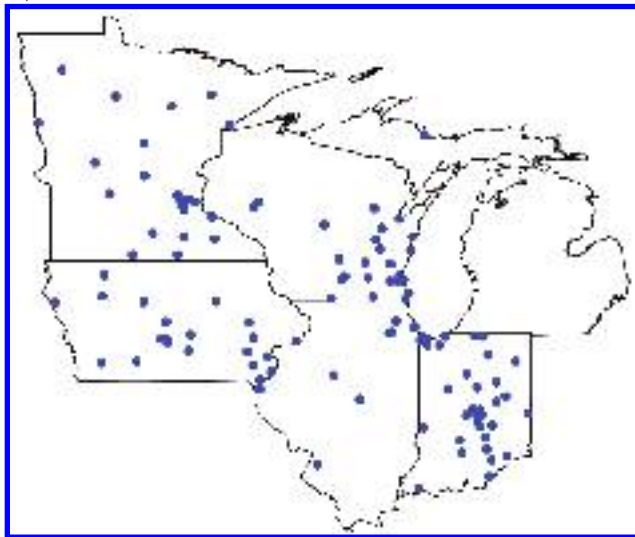


FIGURE 1. CLINIC LOCATIONS IN TEXAS AND SURROUNDING STATES

*Notes:* Each point on the map represents a clinic that was open as of the date listed. The states surrounding Texas are Arizona, Arkansas, Colorado, Kansas, Louisiana, New Mexico, and Oklahoma.

closures—especially those motivated by exogenous, political factors—on preventive care. Specifically, the Minnesota closures (e.g., the clinic in Red Wing) in 2011 were the result of cuts to the federal Title X Family Planning program, which grants funds to local clinics. Title X funds are not used to pay for abortions, but this program has faced political opposition in recent years because critics do not want any funding going to organizations affiliated with abortion providers. As a result, despite the fact that none of the Minnesota clinics that closed in 2011 provided abortion services, their closures were brought about through exogenous, abortion-related policies and therefore are of interest to our analysis.

Panel A. October 1, 2007



Panel B. December 31, 2012

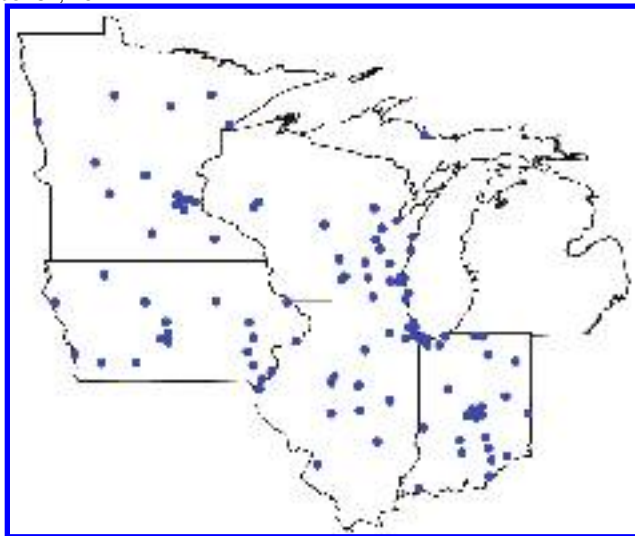


FIGURE 2. CLINIC LOCATIONS IN WISCONSIN AND SURROUNDING STATES

*Notes:* Each point on the map represents a clinic that was open as of the date listed. The states surrounding Wisconsin are Illinois, Indiana, Iowa, Michigan, and Minnesota. We exclude clinics located in the Lower Peninsula of Michigan because none of those clinics would be identified as the “nearest clinic” to any ZIP code in Wisconsin.

The aforementioned clinic closures in Texas, Wisconsin, and their neighboring states, many of which were at least in part the result of politically motivated, exogenous funding changes, should have an impact on the incidence of the preventive care services they provided. In addition, closures might not only have a direct effect through worsened geographic access but also indirect effects, such as overcrowding or increased fees at remaining clinics, which could further discourage preventive



care utilization. While recent studies investigate the prospective impact of facility closures through provider surveys (Ku et al. 2012) or provide anecdotal evidence of negative impacts (Texas Policy Evaluation Project 2013), this paper is the first to perform a retrospective analysis using econometrically robust methods.

## II. Data

### A. Health Center Locations

To identify clinic closures (and openings) and to measure distance to a care provider, we use quarterly snapshots of health center street addresses from a national network of women's health centers with clinics throughout the United States. This network is a specialized women's health provider that received one of the largest shares of funding from the Texas Department of State Health Services (DSHS) Family Planning Program and one of the largest shares of reimbursements from the Women's Health Program during our overall period of analysis.

The snapshots of health center locations represent every end-of-quarter date from October 1, 2007 to December 31, 2012,<sup>12</sup> during which this network saw a substantial decrease in its number of clinics. For example, the number of its clinics in Texas decreased by 25 percent from the beginning of the dataset (October 1, 2007), to the end (December 31, 2012). In addition, this network accepts all types of insurance, including Medicaid, and provides a substantial amount of charity/indigent care, which allows us to test our hypothesis that funding cuts have a larger impact on women of lower educational attainment (a proxy for lower socioeconomic status). One important note for interpreting our results is that this provider network does not perform mammograms; rather, when appropriate, women are referred out.

### B. Preventive Care Use

For individual behaviors, we use restricted<sup>13</sup> survey data from the Centers for Disease Control and Prevention (CDC) Behavioral Risk Factor Surveillance System (BRFSS) that includes the ZIP code of residence for each respondent.<sup>14</sup> The BRFSS is an annual, cross-sectional public health survey conducted monthly in all 50 states, and it includes questions about women's preventive care in the 2008, 2010, and 2012 surveys. We use this data to construct measures of preventive health care utilization, specifically whether a woman has received a clinical breast exam, mammogram, Pap test, or routine checkup in the past year.<sup>15</sup>

<sup>12</sup>End-of-quarter dates for each year are generally March 31, June 30, September 30, and December 31, with the exception of October 1, 2007.

<sup>13</sup>Texas' and Wisconsin's public health departments agreed to perform confidential mergers with the provider data, followed by encryption of the ZIP codes, so that we could use a within-ZIP-code analysis and they could maintain survey confidentiality.

<sup>14</sup>These ZIP codes of residence are for BRFSS respondents only, not patients of the health centers in the provider network. We have no direct information on the patients of the provider network.

<sup>15</sup>The BRFSS describes a routine checkup as "a general physical exam, not an exam for a specific injury, illness or condition."

In 2011, the BRFSS underwent two significant changes that make comparison across years more difficult. First, the CDC added cellular telephone-only households to its sample (previously limited to landline telephone households), and second, the statistical weighting method changed. To maintain as much comparability across survey years as possible, we use only the landline sample in all survey years, along with the corresponding landline-sample weights. This reduces the external validity of any conclusions to only those households with at least one landline telephone, but including cellular telephone-only households would drastically reduce our comparability and make any time varying within analysis impossible.<sup>16</sup>

### *C. Constructing Distance to the Nearest Provider*

Our primary independent variable is the driving distance from a BRFSS respondent's ZIP code centroid to the nearest clinic in the provider database. This variable is calculated through the following steps. First, we geocode every facility location<sup>17</sup> in every end-of-quarter (e.g., June 30, 2008, September 30, 2008) snapshot. Next, using the centroid coordinates for each ZIP code (available from SAS) and the haversine formula, we calculate the great-circle ("crow-flies") distance from each ZIP code to each facility in each time period. Then, for each ZIP code in each time period, we identify its closest crow-flies clinic and calculate the driving distance between the two.<sup>18</sup> The output of this phase of the dataset construction is a list of driving distances to the closest clinic for every ZIP code at the end of every quarter.

The final step is to produce a weighted average over the past year, as most BRFSS questions about preventive care are retrospective. For example, a question might ask, "How long has it been since your last breast exam?" with the shortest timeframe answer being "Within the past year." This is intuitive, as we are generally more interested in whether the respondent had an exam in a normal timeframe (i.e., the past year) than we are in the precise number of days since her most recent exam.

This retrospective timeframe, though, requires us to construct a comparable independent variable. The BRFSS data contains the exact interview date for each respondent. Using the interview date, we calculate average driving distance to the nearest facility using end-of-quarter distances over the past year, where each end-of-quarter observation is weighted by the number of days in the past year that were closest to that quarter. For example, for an individual interviewed on August 16, 2010, the

<sup>16</sup> Another approach is to include the cellular telephone-only respondents with a dummy variable (Barbaresco, Courtemanche, and Qi 2015). Unlike that paper, which studies a law with an exogenous age cutoff, we believe the responses of landline versus cellular telephone-only users to changes in distance differ in a way that is not entirely captured by the controls. For instance, cellular telephone-only respondents represent populations who have more health risk factors and who may respond differently to changes in access to care (Pierannunzi et al. 2012). Including cellular telephone-only respondents even with a dummy control variable would therefore bias our results. Online Appendix B Table B.1 documents the significant demographic differences between these two types of respondents.

<sup>17</sup> For Texas, this is every facility in Texas, New Mexico, Colorado, Kansas, Oklahoma, Arkansas, Louisiana, and Arizona. For Wisconsin, this is every facility in Wisconsin, Minnesota, Iowa, Illinois, Indiana, and the Upper Peninsula of Michigan.

<sup>18</sup> See Bernhard (2013) for details on geocoding and on driving distance calculations.



resulting weighted average distance (all for some ZIP code  $z$ ) would be approximately as in equation (1):

$$(1) \quad \overline{dist}_{8/16/10} = \frac{2}{365} dist_{9/30/10} + \frac{91}{365} dist_{6/30/10} \\ + \frac{91}{365} dist_{3/31/10} + \frac{91}{365} dist_{12/31/09} + \frac{90}{365} dist_{9/30/09}$$

In the equation above, there are 2 days closest to 9/30/2010 (8/16/2010 and 8/15/2010), 91 closest to 6/30/2010 (8/14/2010 to 5/16/2010), 91 closest to 3/31/2010 (5/15/2010 to 2/14/2010), 91 closest to 12/31/2009 (2/13/2010 to 11/15/2009), and 90 closest to 9/30/2009 (11/14/2009 to 8/17/2009).<sup>19</sup>

In order to construct this past-year weighted average distance measure described above, we focus our analysis on BRFSS respondents with interview dates in mid-August 2008 and later, since the clinic location data are only available beginning in October 2007. The survey questions of interest on preventive care are asked only on a biennial basis, so our preferred sample includes all women interviewed in the 2010 and 2012 surveys, as well as those interviewed from mid-August onward in 2008.

#### D. Texas Department of State Health Services Family Planning Program Sites

We require additional data to check that our main results are not being driven by out-of-network clinic funding changes. As mentioned in Section I, the Texas Department of State Health Services (DSHS) Family Planning Program helps fund clinic sites across Texas to provide low-cost family planning and reproductive health care services.<sup>20</sup> We use yearly data on DSHS-funded clinic sites from fiscal year 2007 (September 1, 2006, to August 31, 2007) through fiscal year 2013 (September 1, 2012, to August 31, 2013) to determine past-year distance to the nearest DSHS-funded clinic, where distance is calculated in a similar manner as described in the previous subsection.<sup>21</sup>

It is important to note that the funding end and start dates in the DSHS data do not necessarily indicate clinic closures or openings. In Section VI, we further discuss our use of the distance to the nearest DSHS-funded clinic in a falsification test.

#### E. Local Unemployment Rates

We also use county-level unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics program. Controlling for the local economic environment allows us to account for changes in health insurance coverage

<sup>19</sup>The precise formula in our analysis allows for half-day weights (e.g., 90.5 days instead of 90), when the midpoint between two end-of-quarter dates falls between two calendar days.

<sup>20</sup>While the DSHS Family Planning Program first allocates money to specific agencies based on guidelines and funding applications, the agencies can then (mostly) independently determine the funding start/end dates and amount of funds to be allocated to their individual clinic sites.

<sup>21</sup>The DSHS Family Planning Program has occasionally funded clinics in the particular provider network that we consider. When we calculate distance to the nearest DSHS-funded clinic, we only consider out-of-network clinics that received such funding.

rates, which are strongly tied to employment in the United States,<sup>22</sup> and for local economy-driven changes in demand for preventive care. We use the seasonally unadjusted rates, as our dependent variables are not adjusted for seasonality, and we therefore want to control for any fluctuations that follow seasonal business cycles.

### III. Methods

Our econometric approach is a within-ZIP-code, over time analysis at the individual level. This allows for both individual-level demographic controls and higher-level controls that capture differences in local prevalence and access to care.

$$(2) \quad y_{izt} = \beta_0 + \beta_1 \text{dist}_{zt} + \beta_2 \mathbf{X}_{izt} + \beta_3 \mathbf{C}_{zt} + \beta_4 \zeta_z + \beta_5 \tau_t + \varepsilon_{izt}.$$

Equation (2) is the main specification for our regressions. The dimensions are  $i$  for an individual who lives in ZIP code  $z$  and is surveyed in year  $t$ .  $y$  is the outcome of interest.  $\text{dist}$  is the distance in miles (generally in units of 100 miles) from a respondent's ZIP code centroid to the nearest facility in the provider database, averaged over the past year.  $\mathbf{X}$  are individual-level controls, described below.  $\mathbf{C}$  contains the past-year average county-level unemployment rate, including the interview month as well as the 11 preceding months.  $\mathbf{C}$  also contains either linear annual state time trends or state-by-year fixed effects.  $\zeta$  and  $\tau$  are ZIP code and year fixed effects, respectively. Finally, standard errors are clustered at the county level to account for spatial correlation among adjacent ZIP codes that are jointly affected by clinic closures.

We restrict the sample to women aged 18 to 44. The vast majority of the provider network's patients come from this demographic, as this population is the most in need of any kind of family planning. In addition, as explained previously, we improve sample comparability over time by restricting the sample to respondents with a landline telephone, and we use the CDC's survey weights that adjust for non-coverage and nonresponse among landline telephone households.

For observed differences, we include several individual-level controls: age, along with indicators for black, Hispanic, and "other" race/ethnicity categories (with white as the omitted category), marital and employment status, highest level of educational attainment (high school graduate or GED equivalent, some college, or college graduate), annual household income level (1–8 scale), and health care coverage.

The linear annual state time trend or state-by-year fixed effects is important because funding cuts (and many other policies, e.g., public transit funding) occur at the state level. Our attempt in this paper is not to show that the overall cuts changed preventive care utilization rates for the state as a whole, but rather that individual locations whose proximity to care was more drastically affected by the cuts subsequently experienced larger drops in preventive care rates.<sup>23</sup>

<sup>22</sup>We also directly control for individual health insurance coverage status; note that the BRFSS variable does not distinguish between different types of health insurance (e.g., Medicaid, private).

<sup>23</sup>We do not include linear county time trends for the following reason. Since an individual clinic closure likely affects multiple ZIP codes and since there is a clear time trajectory in most of the closures, a county-level time trend would absorb most of the variation of our key independent variable, if not in some cases be collinear with it.

We control for cross-sectional differences and address any unobserved time-invariant differences between ZIP codes by including ZIP code fixed effects in all specifications. Our results are therefore identified within ZIP code.<sup>24</sup>

The crucial assumption of our analysis, which is fundamentally a continuous difference-in-differences, is that had there been no change in distance to the nearest clinic, the average change in the incidence of preventive care would have been consistent across ZIP codes (Bertrand, Duflo, and Mullainathan 2004; Abadie 2005; Slusky 2015). The short time series of clinic locations unfortunately does not allow for a pre-2007 “stable” period to use for a placebo regression.

One other potential methodological concern is that the closed clinics of this particular provider network either are being replaced by new clinics of another network or are coincident with other, unobserved clinic closures nearby. This is not a major concern, however, for several reasons. First, if there were (unobserved by us) new out-of-network clinic openings in the vicinity of recent in-network closures, then *ceteris paribus* we would likely be *underestimating* the impact of closures from the network on preventive care. This is due to the fact that the affected (“change in distance”) ZIP codes did not lose as much access to care as we presume, yet their preventive care rates still declined from trend in comparison to the no-change ZIP codes.

Second, the extent of the funding cuts supply shock was so large that there were likely no new clinics immediately being opened to fill the specific vacuum left by this network (which comprised approximately one-fifth to one-third of funding-related closures in Texas).<sup>25</sup> Furthermore, a substantial number of this network’s closures occurred in poorer, less urban areas, where alternative women’s health care providers are already relatively scarce and where researchers have found that capacity constraints on preexisting providers may be especially binding (Ku et al. 2012).

Third, as described below, our results are robust to incorporating a control for the distance to the nearest out-of-network clinic funded by Texas’s DSHS Family Planning Program.

#### IV. Results

Table 1 shows the weighted averages of several demographic variables. Column 1 shows the means for all respondents. Column 2 shows the means for the subsample of women whose highest level of educational attainment is a high school diploma (or GED equivalent) or less. This is an attempt to proxy for low-socioeconomic-status

<sup>24</sup>Our main results encompass 4,852 observations representing 1,352 ZIP codes. Although we have an unbalanced panel, we do not believe that our results are only identified on a small number of ZIP codes for the following reasons. First, the majority (802) of the ZIP codes in our sample contain multiple observations, with an average of 5.4 respondents per ZIP code. Of these 802 ZIP codes, most (606) have observations in multiple years, and the remaining 196 have multiple observations in one year (important because closures occur throughout the year, and therefore 2 individuals from the same ZIP code in the same survey year may still have different distances to the nearest clinic). In addition, we have 198 ZIP codes with observations in all survey years, each with an average of 9.8 respondents. Furthermore, only 11 percent of observations are the only respondent in their ZIP code. Finally, expanding the geographic unit to county (rather than ZIP code) fixed effects produces largely consistent results, as shown in online Appendix C.

<sup>25</sup>For example, see Texas Women’s Healthcare Coalition (2013).

TABLE 1—BRFSS SUMMARY STATISTICS FOR WOMEN AGED 18–44, TEXAS AND WISCONSIN

	Full sample (1)	High school or less (2)	<i>p</i> -value (3)
<i>Panel A. Dependent variables</i>			
Clinical breast exam in last 12 months	0.57	0.48	0.000
Mammogram in last 12 months	0.19	0.13	0.000
Pap test in last 12 months	0.59	0.52	0.000
Routine checkup in last 12 months	0.63	0.56	0.000
<i>Panel B. Individual/other characteristics</i>			
Age (years)	33.1	31.9	0.000
White	0.51	0.33	0.000
Black	0.12	0.12	0.927
Hispanic	0.32	0.52	0.000
High school graduate (or GED equivalent)	0.24	0.62	0.000
Some college (including technical school)	0.31		
College graduate	0.31		
Annual household income level (1–8)	5.4	4.2	0.000
Employed	0.59	0.47	0.000
Married	0.56	0.48	0.000
Has health care coverage	0.70	0.53	0.000
Past-year county unemployment rate (pp)	7.45	7.54	0.007
Driving distance to nearest clinic (mi)	27.2 (44.1)	30.5 (46.2)	0.000
Observations	4,641	1,649	

*Notes:* Weighted means are calculated from the combined sample of nonpregnant females aged 18–44 in the 2008, 2010, and 2012 BRFSS landline datasets for Texas and Wisconsin. The sample includes all women interviewed from mid-August onward in 2008, as well as all women interviewed in the 2010 and 2012 BRFSS surveys. We limit observations to those with no missing values for any of the dependent variables or individual/other characteristics. Column 2 restricts to women with a high school diploma (or GED equivalent) or less. Numbers in columns 1–2 are expressed as a fraction of the total sample, unless otherwise noted. Column 3 refers to the *p*-value for a mean-comparison test between the full sample and restricted sample. Standard deviations in parentheses.

women who may be less likely to have access to another source of care.<sup>26</sup> Column 3 shows the *p*-value for whether the sample mean is different between the full sample and the subsample. The strong statistical significance in all of the mean differences (e.g., lower overall incidence of preventive care, lower income, less employment, less health insurance coverage, lower marriage rates, and farther distance to the nearest clinic), reinforces our decision to repeat our analysis on the lower educational attainment subsample.

Figure 3 shows the change in weighted-average past-year driving distance from the start of our data (August 16, 2008) to the end (December 31, 2012) for each ZIP code tabulation area (ZCTA) in Texas. The distribution is heavily skewed toward an increase in the distance to the nearest clinic in the provider network, with particular ZCTAs in the north, south, and west of Texas experiencing increases of

<sup>26</sup>We split the sample by level of educational attainment because education is a reasonable proxy for socioeconomic status and is relatively stable in this population of women aged 18–44. (The results are also robust to limiting to women aged 26–44.) Other potential stratifying variables either would likely have shifted in composition over these particular years of heavy labor market turmoil or are insufficiently granular in this dataset (e.g., health insurance).

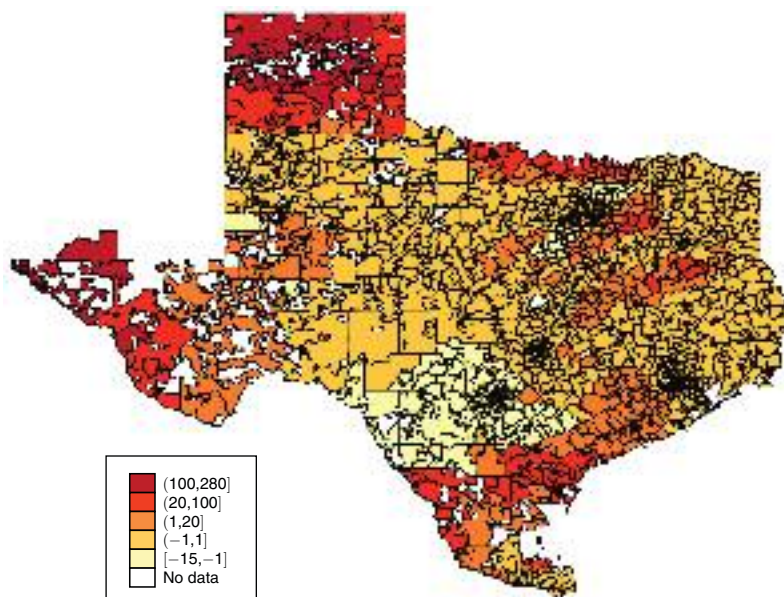


FIGURE 3. CHANGES IN DRIVING DISTANCE TO NEAREST CLINIC, TEXAS

*Notes:* This map shows the distribution of changes in driving distance (mi) to the nearest clinic, between August 16, 2008, and December 31, 2012. Each outlined area is a ZIP code tabulation area (ZCTA). ZCTAs are generalized areal representations of USPS ZIP code service areas. The ZCTA code is the same as the ZIP code for most, but not all, areas. Using quarterly snapshots of clinic locations in Texas and its surrounding states, we calculate driving distance to the nearest clinic as follows: for each quarter, we assign each ZIP code a “nearest clinic” based on crow-flies distance; for each quarter, we then estimate driving distance from each ZIP code centroid to its nearest clinic via the Google Geocoding API; and finally, we calculate a weighted average of driving distance over the past year for each ZIP code and date. The map shows the change in this weighted-average distance measure between August 16, 2008, and December 31, 2012, for each ZIP code.

20 to 100 miles, and some ZCTAs in the north and far west experiencing increases well above 100 miles.

Figure 4 shows the analogous picture for Wisconsin. Here, the changes in distance are much smaller, with most in the range of one to ten miles, and large swaths of the state experiencing minimal (within one mile) or no change. Still, we observe numerous ZCTAs in the south and west of the state experiencing increases of 20 to 45 miles in their driving distance to the nearest clinic.

The primary tables in this paper examine the impact of clinic closures on the incidence of four preventive care outcomes: clinical breast exams, mammograms, Pap tests, and routine checkups. For each outcome, we examine the results with and without state-level controls, in both the full sample and a subsample of women whose highest level of educational attainment is a high school diploma or less.

Table 2, panel A, shows the impact of past-year average driving distance to the nearest clinic on whether an individual had a clinical breast exam in the past year. Since all specifications include ZIP code fixed effects, these coefficients represent the impact of deviations from the mean distance for a given ZIP code across years on deviations from the mean rates of clinical breast exams.



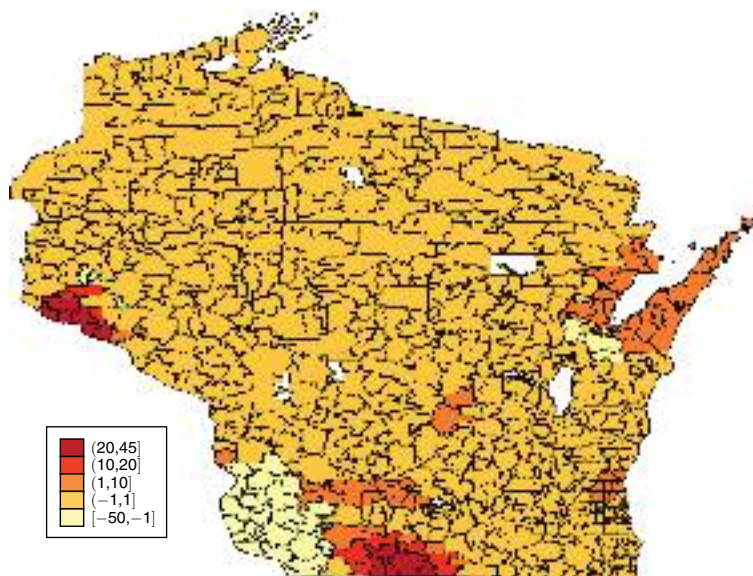


FIGURE 4. CHANGES IN DRIVING DISTANCE TO NEAREST CLINIC, WISCONSIN

Note: See Figure 3.

Column 1 of Table 2 corresponds to a specification with ZIP code and year fixed effects, and individual demographic and county-level controls as described in Section III. Here, the statistically significant coefficient of  $-0.074$  can be interpreted as a 100-mile increase in distance leading to a reduction of 7.4 percentage points in an individual's propensity for having received a clinical breast exam in the past year. In relative terms, this is a 13 percent drop from the sample mean of 57 percent. We generally prefer to express distance in units of 100 miles because some areas (as shown in Figure 3) experienced increases of at least 100 miles; these local areas may be sparsely populated (see online Appendix A Figure A.1), but it is important nevertheless from a policy perspective to understand the impacts of a large drop in access to care, even if relatively few people are affected. Furthermore, this simple scaling-up of our main linear specification improves readability of the coefficients in our tables.

Column 2 adds a linear annual state time trend to account for any overall trends that are happening statewide in Texas or Wisconsin. Column 3 replaces the linear annual state time trend with more flexible state-by-year fixed effects. These additional state-level controls have a minimal effect on the coefficient of interest, which remains statistically significant in all three specifications.

Columns 4–6 of Table 2 repeat the analysis, but only on individuals whose highest level of educational attainment is a high school diploma (or GED equivalent) or less.<sup>27</sup> The magnitudes of the coefficients more than double, and the results tell us that increasing the driving distance by 100 miles reduces the incidence of

<sup>27</sup>This subsample analysis includes individual-level and higher-level controls, as in the full sample analysis, but excludes an educational-attainment control because we stratify based on that.



TABLE 2—IMPACT OF CLINIC CLOSURES ON CANCER SCREENING, TEXAS AND WISCONSIN, 2007–2012

	Full sample			High school diploma or less		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Clinical breast exam</i>						
Driving distance	-0.074	-0.064	-0.064	-0.178	-0.177	-0.178
– 100 mi	(0.030)	(0.029)	(0.031)	(0.060)	(0.060)	(0.060)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,702	4,702	4,702	1,677	1,677	1,677
R <sup>2</sup>	0.575	0.576	0.576	0.742	0.742	0.742
Mean	0.57	0.57	0.57	0.48	0.48	0.48
<i>Panel B. Mammogram</i>						
Driving distance	-0.032	-0.030	-0.035	-0.083	-0.074	-0.083
– 100 mi	(0.039)	(0.040)	(0.042)	(0.032)	(0.030)	(0.031)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,709	4,709	4,709	1,682	1,682	1,682
R <sup>2</sup>	0.537	0.537	0.537	0.688	0.689	0.691
Mean	0.19	0.19	0.19	0.13	0.13	0.13
<i>Panel C. Pap test</i>						
Driving distance	-0.087	-0.083	-0.080	-0.087	-0.083	-0.072
– 100 mi	(0.039)	(0.038)	(0.039)	(0.067)	(0.067)	(0.067)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,696	4,696	4,696	1,675	1,675	1,675
R <sup>2</sup>	0.536	0.536	0.536	0.737	0.737	0.738
Mean	0.59	0.59	0.59	0.52	0.52	0.52

Note: See Table 3 for full table notes.

clinical breast exams by 18 percentage points among women of lower educational attainment.

Panel B of Table 2 shows an analogous analysis for having had a mammogram in the past year. Columns 1–3 show no statistically significant impact of an increase in driving distance to the nearest clinic on mammography rates in the full sample. In columns 4–6, however, there is a statistically significant decrease, despite the smaller sample size. For a 100-mile increase in driving distance, mammogram rates among women of lower educational attainment drop by 7–8 percentage points. The magnitude of this coefficient is smaller than for clinical breast exams, but this is a large effect given that the subsample mean is only 13 percent.<sup>28</sup>

<sup>28</sup>Mammograms are generally only recommended for the oldest women in our sample, whereas clinical breast exams are often recommended for all adult women (American Congress of Obstetricians and Gynecologists 2011, American Cancer Society 2014). Additionally, while mammography guidelines underwent significant changes

TABLE 3—IMPACT OF CLINIC CLOSURES ON ROUTINE CHECKUPS, TEXAS AND WISCONSIN, 2007–2012

	Full sample			High school diploma or less		
	(1)	(2)	(3)	(4)	(5)	(6)
Driving distance	–0.028	–0.027	–0.024	–0.093	–0.081	–0.074
– 100 mi	(0.023)	(0.022)	(0.023)	(0.066)	(0.064)	(0.066)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,823	4,823	4,823	1,743	1,743	1,743
R <sup>2</sup>	0.545	0.545	0.545	0.738	0.739	0.739
Mean	0.63	0.63	0.63	0.56	0.56	0.56

*Notes:* Driving distance is in units of 100 miles. Standard errors are reported in parentheses and are clustered by county. The sample includes all women interviewed from mid-August onward in 2008, as well as all women interviewed in the 2010 and 2012 BRFSS surveys. We restrict the sample to nonpregnant females aged 18–44.

All regressions use BRFSS survey weights for landline telephone households and contain ZIP code and year fixed effects, as well as linear annual state time trends, state-by-year fixed effects, and/or individual-level and county-level control variables when indicated. Individual-level controls include age, and indicators for black, Hispanic, and “other” race/ethnicity categories (with white as the omitted category), marital and employment status, highest level of educational attainment (high school graduate or GED equivalent, some college, or college graduate), annual household income level (1–8 scale), and health care coverage. The county-level control is unemployment rate, averaged over the interview month and the 11 preceding months.

Panel C of Table 2 shows the impact on Pap tests.<sup>29</sup> Here, just as for clinical breast exams, the addition of state-level controls has minimal effect on the magnitude of the coefficients. The main results for the full sample are strongly statistically significant, with an 8–9 percentage point drop for a 100-mile increase in distance, corresponding to approximately a 14 percent drop in Pap testing rates from the sample mean of 59 percent. The coefficients are comparable for the subsample of women with lower educational attainment (although the relative effect is slightly larger), suggesting that this particular preventive care measure is not as correlated with education as are the ones discussed above. Since the coefficient magnitude is the same, decreasing the sample size increases the standard errors (see columns 4–6 compared to 1–3), which leads to the coefficients not being statistically significant.

Table 3 shows the impact on routine checkups. Here, the impact is statistically insignificant and smaller than for the women’s health-specific measures discussed above. Although the results for routine checkups are inconclusive, the point estimates are still consistent with our prior reasoning. In particular, columns 4–6 of Table 3 show a much larger impact on women of lower educational attainment. The 7-percentage-point drop in column 6 for a 100-mile move is about a 13 percent drop from a subsample mean of 56 percent.

during our period of analysis, they were announced nationwide and we do not believe that they differentially affected women in changing-distance ZIP codes.

<sup>29</sup>The American Congress of Obstetricians and Gynecologists released new guidelines in late 2009, but we do not believe that individuals in no-change ZIP codes were affected in a systematically different way by these recommendations than women in changing-distance ZIPs and therefore this blanket guideline change should not bias our results.

TABLE 4—FALSIFICATION TESTS, TEXAS AND WISCONSIN, 2007–2012

	Full sample			High school diploma or less		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dental visit (women)</i>						
Driving distance	0.009	−0.001	0.001	0.064	0.059	0.053
– 100 mi	(0.026)	(0.028)	(0.029)	(0.043)	(0.046)	(0.047)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,835	4,835	4,835	1,741	1,741	1,741
R <sup>2</sup>	0.590	0.591	0.591	0.747	0.747	0.748
Mean	0.62	0.62	0.62	0.52	0.52	0.52
<i>Panel B. Seasonal flu vaccination (women)</i>						
Driving distance	0.003	−0.006	−0.009	0.011	0.012	0.003
– 100 mi	(0.042)	(0.046)	(0.045)	(0.041)	(0.042)	(0.043)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	4,734	4,734	4,734	1,693	1,693	1,693
R <sup>2</sup>	0.563	0.564	0.564	0.778	0.778	0.780
Mean	0.34	0.34	0.34	0.24	0.24	0.24

*(continued)*

## V. Falsification Tests

This section contains three main falsification tests: testing women's outcomes that are not expected to be affected by women's health and family planning clinics; using men as a control sample; and including data on the distance to the nearest out-of-network family planning clinic funded by the Texas Department of State Health Services (DSHS) to help control for out-of-network changes in access to care during this time period. All three tests strengthen our main findings by alleviating concerns that something else during this period is systematically changing in health care in a way that is correlated with our key independent variable.<sup>30</sup>

In Table 4, panels A and B contain the results of using dental visits and seasonal flu vaccinations as an outcome variable. Both show coefficients that are not statistically significant, and even have the wrong sign. Panels C and D test these same outcomes on men and also do not find any statistical significance. Panel E looks at routine checkups and shows both no statistical significance and even coefficients of the wrong sign for men of lower educational attainment. Given that men represent a small minority of the patients of the provider network considered in this paper, this result is consistent with our intuition.

<sup>30</sup>Statewide Medicaid reimbursement rate changes should not be correlated with the distance to the nearest clinic. Furthermore, we find comparable results when excluding out-of-state closures/openings from the analysis (see Section VI), suggesting that out-of-state reimbursement rate changes are also not driving our main findings.

TABLE 4—FALSIFICATION TESTS, TEXAS AND WISCONSIN, 2007–2012 (*continued*)

	Full sample			High school diploma or less		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel C. Dental visit (men)</i>						
Driving distance	0.025	0.025	0.028	-0.142	-0.154	-0.148
– 100 mi	(0.054)	(0.056)	(0.058)	(0.099)	(0.106)	(0.115)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	2,970	2,970	2,970	1,058	1,058	1,058
R <sup>2</sup>	0.660	0.660	0.660	0.840	0.841	0.841
Mean	0.57	0.57	0.57	0.48	0.48	0.48
<i>Panel D. Seasonal flu vaccination (men)</i>						
Driving distance	0.035	0.038	0.035	-0.124	-0.126	-0.137
– 100 mi	(0.068)	(0.068)	(0.067)	(0.077)	(0.088)	(0.086)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	2,916	2,916	2,916	1,038	1,038	1,038
R <sup>2</sup>	0.650	0.650	0.650	0.839	0.839	0.839
Mean	0.27	0.27	0.27	0.21	0.21	0.21
<i>Panel E. Routine checkup (men)</i>						
Driving distance	-0.068	-0.059	-0.064	0.021	0.025	0.017
– 100 mi	(0.045)	(0.046)	(0.045)	(0.096)	(0.104)	(0.106)
Controls	X	X	X	X	X	X
Time trend		X			X	
State-year FE			X			X
Observations	2,963	2,963	2,963	1,051	1,051	1,051
R <sup>2</sup>	0.624	0.625	0.625	0.840	0.840	0.841
Mean	0.47	0.47	0.47	0.43	0.43	0.43

*Notes:* Driving distance is in units of 100 miles. Standard errors are reported in parentheses and are clustered by county. The sample includes all women interviewed from mid-August onward in 2008, as well as all women interviewed in the 2010 and 2012 BRFSS surveys. We restrict the sample to nonpregnant females aged 18–44 (panels A–B), or to males aged 18–44 (sections C–E).

All regressions use BRFSS survey weights for landline telephone households and contain ZIP code and year fixed effects, as well as linear annual state time trends, state-by-year fixed effects, and/or individual-level and county-level control variables when indicated. Individual-level controls include age, and indicators for black, Hispanic, and “other” race/ethnicity categories (with white as the omitted category), marital and employment status, highest level of educational attainment (high school graduate or GED equivalent, some college, or college graduate), annual household income level (1–8 scale), and health care coverage. The county-level control is unemployment rate, averaged over the interview month and the 11 preceding months.

Another concern that we address in this section is that the particular provider network we study does not allow us to consider the effects of out-of-network changes in access to care that may have been contemporaneous. We therefore supplement our main analysis with additional data from the Texas DSHS Family Planning Program on out-of-network DSHS-funded clinic sites.

Tables 5 and 6 repeat our main analysis for Texas only, while also including the distance to the nearest clinic funded by Texas’s DSHS Family Planning Program. First,

TABLE 5—IMPACT OF IN-NETWORK AND OUT-OF-NETWORK CLINIC CLOSURES ON CANCER SCREENING, TEXAS, 2007–2012

	Full sample		High school or less	
	(1)	(2)	(3)	(4)
<i>Panel A. Clinical breast exam</i>				
Driving distance	-0.0673	-0.0673	-0.1681	-0.1641
– 100 mi	(0.0306)	(0.0302)	(0.0582)	(0.0558)
Distance to DSHS		-0.0916		-0.2803
– 100 mi		(0.3392)		(0.5478)
Controls	X	X	X	X
Observations	3,455	3,455	1,310	1,310
R <sup>2</sup>	0.580	0.580	0.738	0.738
Mean	0.55	0.55	0.46	0.46
<i>Panel B. Mammogram</i>				
Driving distance	-0.0375	-0.0376	-0.0753	-0.0729
– 100 mi	(0.0406)	(0.0409)	(0.0294)	(0.0301)
Distance to DSHS		-0.1021		-0.1718
– 100 mi		(0.2318)		(0.2475)
Controls	X	X	X	X
Observations	3,460	3,460	1,314	1,314
R <sup>2</sup>	0.534	0.534	0.682	0.682
Mean	0.19	0.19	0.13	0.13
<i>Panel C. Pap test</i>				
Driving distance	-0.0790	-0.0790	-0.0639	-0.0547
– 100 mi	(0.0386)	(0.0370)	(0.0609)	(0.0572)
Distance to DSHS		-0.2533		-0.6377
– 100 mi		(0.2476)		(0.4141)
Controls	X	X	X	X
Observations	3,450	3,450	1,307	1,307
R <sup>2</sup>	0.542	0.543	0.735	0.737
Mean	0.59	0.59	0.53	0.53

Note: See Table 6 for full table notes.

in columns 1 and 3, we see that the Texas-only results are extremely comparable to the main results for Texas and Wisconsin combined. Then, by comparing columns 2 and 4 to columns 1 and 3 in Tables 5 and 6, we see that controlling for distance to the nearest DSHS-funded clinic does not substantively affect our main conclusions.

In addition, the coefficients for distance to the nearest DSHS-funded clinic are negative, which is consistent with our intuition that access to these clinics does have an impact on women's preventive health care. The DSHS coefficients are statistically insignificant in general, with much larger standard errors than the main "driving distance" coefficient of interest. The DSHS-funded clinic sites are not necessarily specialized and easily recognizable as women's health providers, and the larger magnitude of the DSHS coefficients suggests that women may be relatively more responsive to the increased cost of visiting a non-specialized provider. Finally, these results are also consistent with analyses of Texas enrollment and claims data,

TABLE 6—IMPACT OF IN-NETWORK AND OUT-OF-NETWORK CLINIC CLOSURES ON ROUTINE CHECKUPS, TEXAS, 2007–2012

	Full sample		High school or less	
	(1)	(2)	(3)	(4)
Driving distance	-0.0215	-0.0221	-0.0756	-0.0673
– 100 mi	(0.0235)	(0.0215)	(0.0665)	(0.0610)
Distance to DSHS		-0.6006		-0.6389
– 100 mi		(0.2562)		(0.4612)
Controls	X	X	X	X
Observations	3,533	3,533	1,357	1,357
R <sup>2</sup>	0.542	0.544	0.727	0.728
Mean	0.60	0.60	0.53	0.53

*Notes:* Driving distance is in units of 100 miles. “Driving distance” refers to the driving distance to the nearest in-network clinic. “Distance to DSHS” refers to the driving distance to the nearest out-of-network Texas Department of State Health Services-funded family planning clinic. Standard errors are reported in parentheses and are clustered by county. The sample includes all women interviewed from mid-August onward in 2008, as well as all women interviewed in the 2010 and 2012 Texas BRFSS surveys. We restrict the sample to nonpregnant females aged 18–44.

All regressions use BRFSS survey weights for landline telephone households and contain ZIP code and year fixed effects, as well as individual-level and county-level control variables. Individual-level controls include age, and indicators for black, Hispanic, and “other” race/ethnicity categories (with white as the omitted category), marital and employment status, highest level of educational attainment (high school graduate or GED equivalent, some college, or college graduate), annual household income level (1–8 scale), and health care coverage. The county-level control is unemployment rate, averaged over the interview month and the 11 preceding months.

which report declines in enrollment and services likely due to clinic closures that resulted from budget cuts, and cite challenges in helping women substitute to alternative providers (Pogue 2013).

## VI. Robustness Checks

Our analysis includes four other robustness checks: imputing distances to the nearest clinic for the first three quarters of 2007; using alternative measures of clinic proximity, some of which may better capture the overall availability of care in a region<sup>31</sup>; using alternative survey weights<sup>32</sup>; and excluding out-of-state closures and openings to address the potential endogeneity of closures/openings in other states. All of these checks (which can be found in online Appendices D–G) show that the main results of this paper are robust to our choices regarding time periods

<sup>31</sup> Alternative measures include: “crow-flies” distance to the nearest clinic, average crow-flies distance to the nearest three clinics, driving time to the nearest clinic, the count of clinics within ten crow-flies miles, an indicator for whether there are any clinics within ten crow-flies miles, the natural log of the driving distance variable, and a polynomial (quadratic) function of driving distance.

<sup>32</sup> Rather than using the CDC-determined BRFSS survey weights as in our main set of results, we use alternative weights computed by the Texas DSHS. The DSHS weights take geographic areas into account, which is something the CDC did not consider in the 2008 and 2010 surveys. Our source for this information is an e-mail correspondence with the Texas DSHS, August 2013.



for analysis (i.e., mid-August 2008 onward), specific metrics for distance to the nearest clinic, survey weights, and inclusion of out-of-state closures.

## VII. Discussion

Overall, our main estimates suggest that an increase in distance to the nearest clinic results in decreased preventive care utilization. In particular, our results suggest that reduced access to health care has a greater impact on individuals of lower educational attainment. We interpret this result as being consistent with lower-income, less-educated women having fewer options for care (since they are more likely to be uninsured or underinsured and therefore rely on charity care), and being relatively less likely to seek out preventive care (Rhodes et al. 2012, Garrett and Glover 2014). They may also have less flexible schedules and may lack the means to drive 100 miles each way to the nearest clinic (Enchautegui 2013).

For mammography, the relatively large magnitude of the distance coefficient for women of lower educational attainment is plausible for the following reasons. First, specialized family planning and women's health clinics serve as an entry point to the health care system for many of these women, who might otherwise forgo care due to a lack of alternatives (Frost, Gold, and Bucek 2012). In addition, these women may otherwise receive less mammography screening not only due to socioeconomic disparities in access to care but also in physician recommendation, which is strongly associated with mammography use (O'Malley et al. 2001). The provider network that we consider plays a crucial role in encouraging mammography screening among women of lower educational attainment, by providing referrals based on family history, age, and on-site clinical breast exams. Therefore, clinic closures can substantially decrease the share of these women being referred for and subsequently receiving a mammogram.

We do not find statistically significant impacts on mammography in the full sample or on routine checkups in either sample. The mammogram results are consistent with the fact that the provider network we consider does not provide mammograms but only referrals. In addition, given that there are likely to be more substitutes for provision of general physical exams than for family planning and women's health care, it is not surprising that the magnitude of the coefficients is smaller for routine checkups.

To interpret the economic significance of our estimates, we can compare the impacts of clinic closure to those of other policies. For instance, Kolstad and Kowalski (2012) study the impacts of the Massachusetts health insurance reform and find no overall impact on mammogram utilization. Wherry (2013) finds that state expansions in eligibility for Medicaid family planning services led to an increase in the probability of receiving an annual Pap test by 2.8 percentage points and a clinical breast exam by 1.6 percentage points.

Compared to these policies that seek to increase health insurance coverage or access to affordable health care, our estimates suggest a relatively large impact of geographic access on preventive care utilization, especially among women of lower educational attainment who live in remote areas and experience a large increase in distance to the nearest clinic. Even among less-educated women who

experience an increase in distance of only 10 miles, their annual utilization of clinical breast exams falls by 1.78 percentage points, which is similar in magnitude to the increase that Wherry finds from Medicaid family planning expansions. Given that Americans use preventive services at only about half the recommended rate, it may be an important policy objective to improve geographic access, especially for services that have been found to be cost effective (National Commission on Prevention Priorities (NCPP) 2007, Moyer 2012, United States Department of Health and Human Services (HHS) 2013).

### VIII. Conclusion

Texas and Wisconsin enacted their cuts to funding for family planning and women's health largely due to political motivations, such as to help achieve a government that is consistent with a particular set of moral values.<sup>33</sup> However, as demonstrated in this paper, increasing the opportunity cost of obtaining care has real consequences for the incidence of preventive care, especially for women of lower educational attainment. Additionally, in a broader policy context such as the Affordable Care Act, our findings show that it is equally important to understand nonfinancial as well as financial factors when seeking to increase preventive care utilization.

Reductions in preventive care use can also lead to large costs in the future since prevention can play a significant role in improving health and controlling health care costs (HHS 2011, HHS 2013). More disease (e.g., breast or cervical cancer) cases discovered at a later stage can mean more health care expenditures treating those progressed diseases. For the women of lower educational attainment who are most affected by these funding cuts, the burden of paying for this treatment would fall largely on the state, through both Medicaid and indigent/charity care. These would be additional costs on top of the economic losses resulting from any reductions in labor market participation and productivity and ultimately from premature death. Furthermore, the relatively larger reductions in breast cancer screening that we find among women of lower educational attainment would exacerbate longstanding socioeconomic disparities in screening and survival (Katz and Hofer 1994; Lantz, Weigers, and House 1997; Bradley, Given, and Roberts 2002; Ward et al. 2004; White et al. 2014).

The analysis in this paper covers the period through December 2012. Since women's preventive health questions are only asked every other year, the next year of BRFSS data (covering 2014) was only released in the fall of 2015. Future work can utilize this data to assess the impact of additional cuts and closures. For example, the Women's Health Program in Texas officially forfeited its federal funding at the end of 2012, and media reports already suggest significantly decreased utilization in 2013:

*During the first six months of 2013, there were 38 percent fewer reimbursement claims for birth control than there were during the first six months of 2012, according to a Texas Tribune analysis of data provided by*

<sup>33</sup>For example, Goodwyn, Wade. 2011. "Gov. Perry Cut Funds For Women's Health In Texas." *NPR*, September 20. <http://www.npr.org/2011/09/20/140449957/gov-perry-cut-funds-for-womens-health-in-texas>.

*the Texas Health and Human Services Commission. The number of wellness exams, meanwhile, decreased by 23 percent. Program enrollment figures have also declined, down from 127,000 in January 2012 to 110,900 in May, the most recent month available.*<sup>34</sup>

Overall, our results suggest that women's health and family planning clinic closures negatively impact preventive health care. This paper is the first to retrospectively investigate the impact of such closures—especially those resulting from exogenous policy factors—using econometrically robust methods, unique national time-series data on clinic locations, and restricted BRFSS data on survey respondents' ZIP codes of residence. In doing so, we show that physical proximity to providers plays a significant role in health care utilization, especially among individuals of lower educational attainment, and that specialized women's health organizations play an important role in providing preventive care services.

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<sup>34</sup> Aaronson, Becca. 2013. "Claims Drop Under State-Run Women's Health Program." *Texas Tribune*, December 13. <http://www.texastribune.org/2013/12/13/claims-drop-under-state-run-womens-health-program/>.

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