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Abstract

Did Medicaid expansion under the Affordable Care Act affect the course of the COVID-19 pandemic? We answer this question using a regression discontinuity design for counties near the borders of states that expanded Medicaid with states that did not. Relevant covariates change continuously across the Medicaid expansion frontier. We find that 1) health insurance changes discontinuously at the frontier, 2) COVID-19 cases do not change discontinuously at the frontier but the precision of this estimate is low, 3) COVID-19 deaths do not change discontinuously at the frontier and the confidence intervals exclude large declines in deaths in Medicaid expansion areas, 4) smart thermometer readings of fever rates from Kinsa, Inc. do not change discontinuously at the frontier, and 5) COVID-19-related doctor visits discontinuously increase in Medicaid expansion areas.

Key words: Affordable Care Act, COVID-19, Medicaid, regression discontinuity

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1 Introduction

In this paper we investigate whether Medicaid expansion in accordance with the Affordable Care Act has had an effect on the spread and intensity of Covid-19 across the U.S. Lack of universal health insurance has been noted as a major difficulty in the U.S. response to the pandemic, and in particular, for the large disparities in Covid-19 case and death rates for minorities (The Economist 2020). Having health insurance can motivate individuals to get examined by medical professionals earlier in the cycle of the disease without worrying about the resulting expenditures, which may lead them to be rapidly diagnosed and reduce their spread of the virus. Moreover, having health insurance may allow individuals to access health care at an earlier stage of the disease, increasing the chance of survival, or to access higher quality healthcare along multiple margins[1]. On the other hand, having one's health expenditures covered may have perverse effects of subsidizing potential exposure to the virus by incentivizing individuals to visit health care facilities, where they may catch or spread the virus to others, or to behave less cautiously in daily life outside the health care sector. Having health insurance may also lead to the patient receiving more, and potentially iatrogenic, procedures. Health insurance may also have no effect on virus transmission if neither individual behavior nor treatment patterns are responsive to considerations of cost for health care. Therefore, the potential net effects of insurance coverage expansion are ambiguous. This ambiguity is in line with the state of the current literature on whether health insurance improves health, and is not surprising.

To answer this question, we exploit stark variation in the availability of public insurance created by the Affordable Care Act's expansion of Medicaid to cover all individuals in households earning up to 133% of the poverty line and the Supreme Court's decision making this expansion optional for each state (NFIB v. Sebelius 2012). By January 2020, 37 states and Washington D.C. have adopted the expansion, whereas the remaining states had their own, lower, Medicaid eligibility thresholds. As a rule, the thresholds in these remaining states excluded a large number of individuals who would have gotten Medicaid with the expansion, such as childless adults and many individuals earning considerably less than the poverty line.

We perform a regression discontinuity analysis at the county level at the borders of the Medicaid-expanding states with Medicaid-nonexpanding states. As Covid-19 case and death rate data in the U.S. is at the county level with no other covariates consistently available, we cannot match infected or deceased individuals to their insurance status as of this time;

[1] EMTALA requires hospitals to offer emergency stabilization care for individuals without regard for their ability to pay or insurance coverage. However, it may be the case that EMTALA patients receive lower quality care by being assigned to different medical professionals, experiencing longer wait times and facing more crowded facilities.

however, we document that there are sizeable differences in insurance rates that appear discontinuously at the borders, confirming that crowdout from the ACA Medicaid expansions was minimal and that the expansions increased insurance rates (see e.g. Frean et al. 2019 among others). We also document that a large number of covariates, including population density, income per capita, demographics including racial composition and health variables such as pre-Covid mortality and prevalence of Covid-19 preconditions, remain continuous across these borders. Hence, it is likely that our analysis obtains the causal effect of Medicaid expansion on reported outcomes related to Covid-19.

We find that there are no statistically significant discontinuities in Covid-19 reported cases or deaths per capita across the Medicaid expansion borders, with the point estimates indicating, if anything, that Covid-19 intensity is higher on the Medicaid-expanding side of the borders. We observe a statistically significant and positive discontinuity in Covid-19 related doctor visits as measured by Carnegie Mellon's Covid Tracker.

The results based on reported Covid-19 cases and deaths are consistent with two hypotheses. First, Medicaid expansion could have no effect on the course of the epidemic (or the positive and negative effects may be canceling out). Second, Medicaid expansion could have a positive effect on the probability of Covid-19 cases being reported (or deaths being classified as Covid-19 related) and a negative effect on the underlying course of the pandemic. To disentangle which of these hypotheses is correct, we consider data on temperature readings from smart phone-connected personal thermometers (smart thermometers) distributed by Kinsa, Inc. Multiple studies, including Avery et al. (2020), Harris (2020) and Liautaud et al. (2020) have cited Kinsa smart thermometer data as a way of proxying for the spread of Covid-19. The advantage of these data is that it provides a measure of Covid-19 intensity that is plausibly independent of any reporting biases created by Medicaid. Using the Kinsa smart thermometer reading data, we do not find discontinuities in the percent of readings reporting fevers, with standard errors narrow enough to reject large effects.

Our paper is part of a large literature investigating whether health insurance improves health. This question is still open in the literature, and it is likely that different health insurance reforms may affect some dimensions of health but not others. Finkelstein and McKnight (2008) find that life expectancy for the elderly did not rise following the introduction of Medicare in states with low as opposed to high previous insurance coverage for the elderly, while Baicker et al. (2012) observe mental and self-reported health benefits but not improved blood pressure or other health metrics for a group of individuals randomly assigned to receive Medicaid in Oregon. On the other hand, Card et al. (2009) find that individuals admitted to a hospital right after turning 65 (and qualifying for Medicare) are less likely to require readmission and more likely to survive than individuals admitted just before they turn 65. More recently, Miller et al. (2019) has matched ACS insurance data with death records from the vital statistics to find that individuals who qualified for and obtained Medicaid after the ACA insurance expansions had a lower death rate than similar individuals living in states that did not pass the expansions.

Our paper also contributes to the literature on measuring the impacts of the Affordable Care Act. While there is a substantial body of work confirming that the ACA causally increased insurance coverage (Sommers et al. 2014, 2015, Courtemanche et al. 2017, Duggan et al. 2019, Frean et al. 2019, Kaestner et al. 2017), we are not aware of other work that has used a regression discontinuity design to identify this effect.

While multiple papers have noted correlations between lack of health insurance and Covid-19 incidence (see e.g. Benitez et al. 2020), the only paper of which we are aware that looks at causal effects with regard to Covid-19 clinical outcomes and the Affordable Care Act is Sanchez (2020), which considers the causal effect of Covid-19 on ACA exchange enrollment in Washington. Clemens et al. (2020) considers the long-run effects of the Affordable Care Act on economic outcomes during the recession induced by the Covid-19 pandemic, but does not consider effects on clinical outcomes. Our paper is most closely related to Schmidt et al. (2019), who use a regression discontinuity design to investigate the effect of the Affordable Care Act on participation in other social programs.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 describes the empirical strategy. Section 4 presents the results. Section 5 concludes.

2 Data

We define the set of Medicaid-expanding states as the ones that adopted Medicaid expansion on or before Jan. 1, 2020 according to the Kaiser Family Foundation (KFF 2020). We use county-level data on cumulative Covid-19 reported cases and deaths from the New York Times as of June 30 and as of September 18. It is well-known that data on Covid-19 cases is likely very inaccurate (with true case counts as much as ten times more than reported case counts, and actual deaths exceeding reported deaths by 30-50%, see e.g. New York Times 2020a), so measurement error in reporting is likely to be both systematic and large.

To validate that uninsurance rates fall at the Medicaid expansion border, we use the Census Bureau's Small Area Health Insurance Estimates (SAHIE) from 2018. This is the latest set of county-level estimates of uninsurance, although it is not completely current as several additional states have adopted the Medicaid expansion since then (these are Virginia, Maine, Utah, Idaho and Nebraska).

We combine this data with several sets of correlates of Covid-19 spread to ensure that they are balanced across the Medicaid expansion boundary. We obtain data on median household income, fraction of people who own their homes, the fraction of individuals commuting with public transit and the number of rooms per capita from the 2014-2018 5-year ACS, and the 2019 unemployment rate from the Local Area Unemployment Statistics (LAUS). We also use high-frequency consumption data from Affinity (via Chetty et al. 2020) to measure the depth of the 2020 recession by county. We obtain a variety of data on demographics, mortality rates and the prevalence of various diseases from the CDC via the Institute of Health Metrics and Evaluation (IHME). We also use the Healthcare Cost Report Information System (HCRIS) to obtain data on hospital bed counts and types, as well as overall hospital utilization.[2] We obtain data on voting patterns by county as reported by The Guardian.[3]

We use anonymized data on social distancing behavior from SafeGraph, aggregated to the county level (www.safegraph.com, the "Social Distancing Metrics"). Cellphone-based data on individual mobility have been used in recent work on the Covid-19 pandemic to infer the extent to which individuals are restricting their movements to avoid infecting themselves and others, see e.g. Allcott et al. (2020) and Farboodi et. al. (2020). While there are multiple ways in which cellphone-based data may have nonrandom measurement error, in our case, our only concern would be for the measurement error to be the same on opposite sides of the Medicaid expansion boundary, which is likely to be the case.

We also use data on "Doctor Visits for Covid-19 Related Symptoms" from Carnegie Mellon University's Delphi Research Group's CovidCast tracker. This variable is defined as the percentage of daily doctor visits falling in particular ICD-10 codes. We compute the average of this variable as our measure of Covid-related doctor visit intensity. We consider doctor visit intensity in three periods: a placebo period (February 1 - March 1), the first wave (March 1 - June 30) and the second wave (July 1 - September 18). Unlike all the other variables used in this paper, this variable is missing for a large number of sparsely populated counties (it is available for about 2465 out of more than 3000 counties), but offers reasonable coverage around the state borders forming the Medicaid expansion boundary.

2.0.1 Data on Smart Thermometer Readings from Kinsa

We use data on the number of active users and the number of individuals recording fever readings by county and day as provided by Kinsa, Inc.'s smart thermometer network. Kinsa, Inc. provides smart thermometers both for sale and for free (to public elementary school

[2] We use the version of HCRIS data provided by Adam Sacarny on his website sacarny.com/data. We are very grateful to Adam Sacarny for making this resource public.

[3] These data were obtained from https://github.com/tonmcg/US_County_Level_Election_Results_08-16

students) and uses the resulting readings for research, including to forecast flu season patterns. Kinsa has made county-level maps of the percentage of active users recording feverish readings by county and day available to the wider research community at healthweather.us. While the readings in these maps have been smoothed to optimize predictive accuracy by county, we use the unsmoothed versions of these data to avoid data smoothing across the Medicaid expansion boundary.[4] We use county-day data for 2020 for the main outcomes as well as for 2019 to improve the precision of our estimates.

Smart thermometer data is useful to address the concern that an individual with Covid-19 is more likely to be a reported case if she has access to Medicaid than if she does not. As the marginal cost of taking one's temperature is very low, it is plausible that, conditional on thermometer availability, an individual's decision to measure temperature is unrelated to access to Medicaid. A concern could be if Kinsa thermometers are differently distributed in Medicaid-expanding vs. Medicaid-nonexpanding states. However, the availability of data on the number of active users allows us to test this hypothesis.

3 Empirical Strategy

We run a standard regression discontinuity design (Hahn, Todd and van der Klaauw 2001), with the running variable being distance to the closest border between a Medicaid-expanding state and a Medicaid-nonexpanding state.

$$y_c = \alpha + \beta MCD_{s(c)} + \gamma_1 d_c + \gamma_2 MCD_{s(c)} \times d_c + \eta_{b(c)} + \varepsilon_c, \ |d_c| \leq b \tag{1}$$

where c indexes counties, y_c is the outcome variable, $s(c)$ is the state of county c, $MCD_{s(c)}$ is an indicator for whether state $s(c)$ adopted Medicaid expansion by January 2020, d_c is the distance of the county to the nearest point on the Medicaid expansion border, with d_c being negative if $s(c)$ did not expand Medicaid and positive otherwise, and $\eta_{b(c)}$ being a fixed effect for the pair of states defining the border segment to which county c is closest. The parameter b is the bandwidth, which defines the sample over which the linear regression is a good local approximation to the underlying (possibly nonlinear) expected value of y_c conditional on d_c. The choice of bandwidth is a crucial component of the regression discontinuity design and we will explore the consequences of choosing our bandwidth according to several optimality criteria proposed in the literature.

The regression discontinuity methodology should identify the local average treatment effect of the Medicaid expansion on the outcomes of interest as long as the error term ε_i

[4] Results using smoothed data are qualitatively similar and are available on request.

is continuous across the Medicaid expansion boundary. We will present extensive placebo checks in Section 4 to show that there do not appear to be relevant covariates that are discontinuous across the Medicaid expansion boundary, except possibly ones that are additional effects of Medicaid expansion. It is also unlikely that there are other policy changes at the Medicaid expansion boundary (or at a similar set of state boundaries) as the boundary had been in considerable flux until late 2018 (and has experienced further changes this year). It is also unlikely that a discontinuity in ε_i could be generated by selective sorting, as it is states rather than counties that choose whether to proceed with Medicaid expansion.

3.1 Choice of Bandwidth

We consider several bandwidth choices. First, we use an ad hoc bandwidth of 200 kilometers, which tends to include the counties in the states that form the Medicaid expansion border. We will see that this bandwidth will be intermediate to our other choices. Second, we use the Imbens-Kalyanaraman (2012) bandwidth (IK), which optimally trades off bias and variance of the regression discontinuity estimate. We will see later that the IK bandwidth tends to be very large, sometimes including the entire continental United States, the intuition for this being the linearity of the conditional expectation function for much of the domain of the running variable. Third, we use bandwidth selection procedures suggested by Calonico et al. (2014, 2017, 2020) that allow for different bandwidths on different sides of the Medicaid expansion border and that explicitly correct for the bias of the regression discontinuity estimator, taking the correction into account when computing optimal bandwidth size. The first of these procedures selects bandwidths to minimize the mean squared error of the estimator, while the second selects bandwidths to minimize coverage error. In our application, these bandwidths tend to be similar.

Figure 1 provides three maps of the United States showing counties that fall within 1) a typical bandwidth computed with the procedures of Calonico et al. 2017, approximately 0.12 thousand kilometers, 2) an ad hoc bandwidth of 0.2 thousand kilometers, and 3) a typical bandwith computed according to the procedure of Imbens and Kalyanaraman (2012), approximately 0.45 thousand kilometers. We see that the Calonico et al. bandwidths include the narrowest range of counties, the IK bandwidth a much wider range and the ad hoc bandwidth is in the middle. It is worth noting that a bandwidth of approximately 1330 kilometers includes the entire continental United States.

4 Results

4.1 Covariate Balance

The validity of our regression discontinuity design rests on the idea that relevant covariates that are captured by ε_c are continuous across the Medicaid expansion border. This assumption is not innocuous, as institutional environments change discontinuously at state borders, with possible implications for economic variables. For example, Holmes (1998) finds discontinuities in employment rates and other economic variables at borders of states that have passed right-to-work laws with states that have not done so. Therefore, it is important to check that relevant covariates indeed are balanced so that we do not attribute the effect of these or correlated covariates to Medicaid.

Table I presents local linear estimates of discontinuities in a variety of potential confounders, together with standard errors and bandwidths (expressed in thousands of kilometers). These are as follows:

1. Economic covariates such as log median household income per capita, the county unemployment rate in 2019 and the decline in spending that the county experienced during the pandemic, as measured by Affinity (Chetty et al. 2020).

2. Demographic and mortality covariates, such as log population density, an indicator for whether the county is part of a metropolitan statistical area (MSA), the fraction of the population that is white, and the age-adjusted mortality rates for the population of the whole and for African-Americans aged 15-64 (prime aged adults who may be eligible for Medicaid under the ACA but were not eligible for Medicaid before the ACA unless they had children).

3. Health status covariates such as the fraction of individuals in the county who have hypertension, who are obese, who have diabetes or who die from cancer or heart disease, with the health variables chosen to reflect risk factors for Covid-19.

4. Hospital resources covariates such as the log of total hospital expenditures per capita, and the number of total beds as well as ICU beds. It is well known that ICU space was a binding constraint in multiple areas that were the first to be affected during the pandemic.

5. Other behavioral indicators that may be associated with Covid-19 intensity, such as the number of rooms per person, the fraction of people using public transit to get to work, and the fraction of individuals living in homes that they own. Among these factors

we include a measure of social distancing at the time that the pandemic arrives in the county based on SafeGraph data. We obtain this measure as the fraction of mobile devices remaining completely at home in the three weeks before a county reports its 10th Covid-19 case. We also include the vote for Donald Trump in 2016, as previous research has indicated that individuals describing themselves as Republicans tended to take fewer precautions in the face of the pandemic than did individuals describing themselves as Democrats (Allcott et al. 2020).

We standardize all variables to have zero mean and unit variance across counties, allowing us to interpret the discontinuity that we estimate in terms of standard deviations of the variable.

We find that the balance tests overwhelmingly fail to find evidence of discontinuities in the potential confounders at the Medicaid expansion boundary. Out of the 96 discontinuity estimates that we obtain, only two are significant at the 1% level and the 5% level, and only three are significant at the 10% level, all consistent with the null hypothesis of there being no discontinuities in the covariates. Moreover, only in eight cases do the magnitudes of the estimated discontinuities exceed 0.2 standard deviations, suggesting that any potential imbalances are small. The covariates accounting for the two rejections at 1% are the death rate from cancer (which seems to be higher on the Medicaid expansion side of the boundary) and the mortality rate of African Americans aged 15-64. The latter finding is consistent with Sommers et al. 2015 and Miller et al. 2019, as African Americans in this age group are more likely to be "compliers" of Medicaid expansion than most other demographic groups. Some intuition for why the balance tests overwhelmingly pass is that by January 2020, the Medicaid expansion boundary did not separate different regions of the country (or "blue" from "red" states) but instead lay inside the South and the West.

Figure 2 presents regression discontinuity plots for several key potential confounders: log median household income per capita, fraction white, population density, overall mortality rate, percentage with diabetes and rooms per capita, all for the ad hoc bandwidth. We see that in all of these plots, any potential discontinuities at the Medicaid expansion border do not stand out relative to the variation in the data.

Our placebo exercise not only convinces us that our regression discontinuity estimates of the effects of Medicaid expansion on the Covid-19 pandemic are likely to be causal, but rules out certain mechanisms through which Medicaid might affect the pandemic. One could have expected that in the years preceding the pandemic, Medicaid expansion might have led to a lower prevalence of chronic conditions, such as heart disease, obesity or diabetes, which are risk factors for Covid-19; our balance test show that this is not the case. We also see that the presence of Medicaid did not affect income or consumption levels before the pandemic and also did not affect the severe drop in consumption that took place nationwide during the pandemic (from our analysis of the Affinity variable).

4.2 Main Results

We now proceed to our main results. First, we confirm that health insurance rates do change discontinuously at the Medicaid expansion border. To do this, we use data from the 2018 Small Area Health Insurance Estimates, which are ACS-based health insurance estimates, supplemented by a model for small counties, created by the Census Bureau. In panel 1 of Table II we find a strong and significant negative discontinuity in the fraction uninsured at the Medicaid expansion boundary of about 0.8 standard deviations of this variable, a large effect. This estimate is statistically significant regardless of the choice of bandwidth. As the standard deviation of the fraction uninsured from SAHIE is about 5 percentage points, this estimate suggests that the Medicaid expansion reduced uninsurance rates by 4 percentage points (on a base of 11 percentage points averaging over all counties), which is consistent with e.g. Sommers et al. (2015) and Frean et al. (2019). Hence, insurance coverage does change discontinuously at the Medicaid expansion border, suggesting that there is indeed a difference in treatment at the border.

However, we do not find any discontinuities in the course of the Covid-19 pandemic at the border. The next three panels of Table II present discontinuity estimates for the log of reported Covid-19 cases per million (offset by 1 to avoid the argument of the logarithm ever becoming zero) at the end of the first wave (March 1 - June 30), at the end of the second wave (by September 18) and just for the second wave (July 1 - September 18). For all three variables and all four bandwidths, the discontinuity estimates are statistically insignificant, small (less than 0.2 standard deviations in magnitude) and take on both positive and negative values. The following three panels present discontinuity estimates for the log of reported Covid-19 deaths per million for the same time periods. All these estimates are statistically insignificant and small, with the exception of those for log Covid-19 deaths per million during the first wave, which are between 0.1 and 0.2 standard deviations higher on the Medicaid-expanding side of the boundary for three of the four bandwidth choices, with one of these positive estimates being statistically singificantly different from zero at 10%.

Given the imprecision of place-level data without information on who exactly benefited from Medicaid (Miller et al. 2019) we examine the range of estimates that could be consistent with our confidence intervals. The standard errors associated with the estimates for log cases per million for the first wave (likely the variable measured the worst) are quite large and consistent with declines (as well as increases) of 0.6-0.7 standard deviations at the

Medicaid expansion boundary. The standard errors associated with the estimates for log deaths per million in the first wave are smaller, and given that these estimates are positive for most bandwidth choices, are consistent with declines of less than 0.2 standard deviations at the Medicaid expansion boundary. The standard errors associated with the estimates for log cases and deaths per million in subsequent waves and cumulatively are consistent with declines of 0.2-0.4 standard deviations.

While we find null effects of various precision for reported cases and reported deaths, we do find moderate positive effects on Covid-19 related utilization in the Medicaid expanding counties at the start of the pandemic. We consider measures of Covid-19 related doctor visits from the Delphi Group's CovidCast database hosted by Carnegie Mellon for the first wave, the second wave and a placebo period in February 2020 in the last four panels of Table II. Looking at doctor visits in the placebo period, we do not see significant differences on different sides of the Medicaid expansion boundary, giving us some confidence that this is indeed a measure of Covid-19 related utilization rather than a measure of utilization as a whole. However, doctor visits during the first wave of the pandemic (March 1 - June 30) increase by about 0.3 standard deviations as one crosses to the Medicaid expanding side of the boundary, which is statistically significant at 5% if the ad hoc or the IK bandwidth is used, and at 10% if one of the CCT bandwidths is used.

The first four panels of Figure 3 provide the regression discontinuity plots for the ad hoc bandwidth accompanying the uninsurance, cases, deaths and doctor visits results for the first wave period. We see that, if anything, the discontinuity in deaths is positive, suggesting more deaths per million on the Medicaid-expanding side of the border, and that the discontinuity in doctor visits is pronounced.

The increase in Covid-19 related doctor visits in Medicaid expanding counties appears to be confined to the first wave. Visits during the second wave (July-September) seem to experience an insignificant discontinuity across the Medicaid expansion boundary, with the estimates less than 0.2 standard deviations in magnitude. The discontinuity in cumulative Covid-19 related doctor visits is positive but statistically insignificant and lower in magnitude than the discontinuity for the first wave only. Our results could be consistent with Medicaid relaxing barriers to access to care (or incentivizing the discovery of positive Covid-19 cases) only during the first wave of the pandemic, perhaps when the medical system was particularly stressed.

4.3 Results for Kinsa Smart Thermometer Readings

As we have noted earlier, the lack of a discontinuity in Covid-19 intensity may be coming from either Medicaid not having an effect on true Covid-19 intensity, or from Medicaid decreasing true numbers of cases and deaths but simultaneously increasing the rate at which Covid-19 cases are reported or the rate at which deaths are attributed to Covid-19.[5] To disentangle these two explanations, we turn to data on fever readings from smart thermometers distributed by Kinsa, Inc. We use two variables in our analysis: the percent of active users reporting feverish readings ("percent ill") and the number of active users (individuals who have used their thermometer over the course of the past year). The former variable is our proxy for the true intensity of Covid-19, as fever is a common symptom of the disease, and as Kinsa has provided evidence that its data are predictive of Covid-19 outbreaks (Chamberlain et al. 2020). The latter variable allows us to check that the distribution of thermometers is continuous across the Medicaid expansion boundary, and therefore, that there should not be differences in thermometer usage by individuals on different sides of the boundary apart from differential prevailing rates of fever-inducing illnesses such as Covid-19.

We also take advantage of the fact that we have 2019 values of the "percent ill" variable, which allows us to improve precision of our estimates by controlling for this variable on the right hand-side of the regression. The corresponding falsification exercise for continuity of active users across the Medicaid expansion boundary then takes the form of controlling for the 2019 number of active users on the right hand-side of the equation in which we look at discontinuities in the number of active users in 2020. Therefore, we modify our estimating equation to read

$$y_{c,2019} = \alpha + \beta MCD_{s(c)} + \zeta y_{c,2019} + \gamma_1 d_c + \gamma_2 MCD_{s(c)} \times d_c + \eta_{b(c)} + \varepsilon_c, \ |d_c| \leq b \tag{2}$$

In results available on request, we have estimated versions of this equation setting $\zeta = 0$ (excluding the 2019 lagged value) or $\zeta = 1$ (looking at discontinuities in the difference between the 2020 and 2019 outcome) as well as performing a difference in regression discontinuity analysis (Deshpande 2016). Our results are very similar to the results we present in the paper, although with larger standard errors. As all the unsmoothed Kinsa variables are highly positively skewed, we transform them with the log transformation, offsetting each variable by either 1 (for counts of active users) or 1% (for the percent ill).

Table III presents our estimates of the parameter β for the Kinsa "percent ill" measure

[5] Even though testing is available to everyone free of charge, often times the first contact of a sick patient is her physician who advises testing based on symptoms. So easy access to a physician for an individual in Medicaid state may lead to more doctor visits and testing.

and the number of active users. We consider four time periods during which to measure our dependent variables: a pre-period (January and February 2020), the first wave of the pandemic (March 1 - June 30), the second wave (July 1 - September 18) and the cumulative two-wave period (March 1 - September 18). Each panel presenting discontinuity estimates for the "percent ill" measure is followed by a panel presenting the corresponding "active users" measure to show that there is no discontinuity in the change in the distribution of active users over time and between Medicaid expanding and non-expanding states. We see that for every period considered – the first wave, the second wave and the cumulative total – there is no discontinuity in log percent ill at the Medicaid expansion border. There also is no discontinuity in the log number of active users across the Medicaid expansion boundary, suggesting that Medicaid is not creating differential incentives for takeup of the Kinsa thermometers. The same is true for the discontinuity estimates in the pre-period, before Covid-19 begins. Moreover, the null effects for both measures are estimated precisely, with declines in the log percent ill or absolute discontinuities in log active users of greater than 0.2 standard deviations being rejectable for almost every time period or bandwidth.

The last two panels of Figure 3 provide regression discontinuity plots for the log percent ill and log active users Kinsa outcomes for the first wave. We see that these series are cleanly continuous through the Medicaid expansion boundary. Based on the results in Table III we conclude that it is unlikely that Medicaid decreased true Covid-19 incidence while boosting its reporting rate. Rather, it appears that Medicaid expansion did not have a statistically distinguishable effect on underlyng clinical outcomes, with the data sufficiently precise to reject effects of moderate size.

4.4 Heterogeneity in the Effects of Medicaid on Doctor Visits

Having noted the greater utilization of medical services related to Covid-19 in Medicaid expanding counties, we now investigate heterogeneity in this difference. Particularly interesting dimensions of heterogeneity are county income and minority composition, as it is well-known that Covid-19 has disproportionately affected low-income and majority-minority areas (Knittel and Ozaltun 2020, McLaren 2020) and may have had differential effects in areas with different political affiliations (Allcott et al. 2020). We define counties as low income if their mean income is below the bottom quartile of the population-weighted county mean income distribution, and as majority-minority if the fraction of individuals identifying as non-Hispanic White is less than 50%. We also consider splitting counties by the median of the Trump vote share in 2016. For each sample, we present discontinuity estimates for two variables: the Covid-19 related doctor visits from the first wave, and the uninsurance rate to show that Medicaid expansion had a "first stage" effect in each of the county samples as well as in the main sample.

Table IV presents our heterogeneity results. First, we observe that for each of these five groups of counties, uninsurance rates are substantially and statistically significantly lower on the Medicaid expanding-side of the boundary. However, the decline in uninsurance rates is much larger for the low-income and majority-minority counties than it is for counties that are neither, highlighting that Medicaid expansion increased insurance much more in areas that used to have high uninsurance rates before the Affordable Care Act. Counties in which Trump had a below-median vote share in 2016 have a modestly smaller decline in uninsurance across the Medicaid expansion boundary than do counties with an above-median Trump share. Second, we see that counties that are neither low-income nor majority-minority do not see statistically significant or economically large discontinuous increases in Covid-19-related doctor visits, and, if anything, may have discontinuous decreases in such doctor visits, although the standard errors on the discontinuity estimates are large and typically include the population estimates from Table II. Lastly, we find that low-income counties and majority-minority counties do have the pattern of discontinuities for doctor visits in the first wave and the change from the first wave to the second that we found in the overall sample. The discontinuities in doctor visits during the first wave are positive and statistically significantly different from zero for low-income counties and are positive, not statistically significant, but quantitatively large (over one standard deviation as compared to 0.2 standard deviations for the overall sample) for the majority-minority counties. Given the small number of majority-minority counties, the lack of statistical significance is not surprising. The discontinuities in doctor visits are slightly smaller in counties with below-median Trump shares than in counties with above-median Trump shares, although both are close in magnitude to the overall population estimates.

Our main conclusion from the heterogeneity analysis is that the effects of Medicaid expansion on doctor visits are driven by low-income and majority-minority areas, which had disproportionately higher uninsurance rates before the ACA and which saw disproportionate effects of Medicaid expansion on insurance coverage. Observing this heterogeneity pattern strengthens our interpretation of the main results as suggesting that Medicaid may have relaxed the temporary resource constraints during the first wave of Covid-19.

5 Conclusion

Our analysis has shown that 1) there are no discontinuous jumps of reported Covid-19 cases and deaths per million across the Medicaid expansion boundary, 2) there are no discontinuous jumps in feverish temperatures recorded by smart thermometers across the same boundary, 3) there is a moderate discontinuous increase in Covid-19 related medical utilization at the boundary, 4) there is a large, discontinuous decrease in the fraction of the population uninsured at that boundary and 5) other covariates that may affect Covid-19 transmission appear to be continuous across the boundary, validating that our analysis is appropriate. Our null results for Covid-19 outcomes and feverish temperatures are precise, rejecting even moderately small effects of Medicaid. Based on these five pieces of evidence, we can conclude that while Medicaid may increase utilization of medical services in relation to Covid-19, it does not seem to affect the course of reported or underlying clinical outcomes during the pandemic.

Our finding is consistent with the findings of Finkelstein (2007) and Finkelstein and McKnight (2008) on Medicare, as well as Baicker et al. (2012) on Medicaid, which show that health insurance expansion increases utilization but does not affect physical measures of health. This finding does not imply that health insurance is not useful, as health insurance may provide benefits for financial health, as well as for self-reported health. Instead, it suggests that the primary purpose of health insurance is to provide financial security rather than access to health resources.

As next steps, we will conduct a more in-depth investigation of heterogeneity in effects of Medicaid by income, race, education, democrat/republican vote share and others. We will look for heterogeneous effects on Covid incidence, doctor visits as well as measures from Kinsa. These will help us pin down whether the null effect that we find here is generalizable to these various groups.

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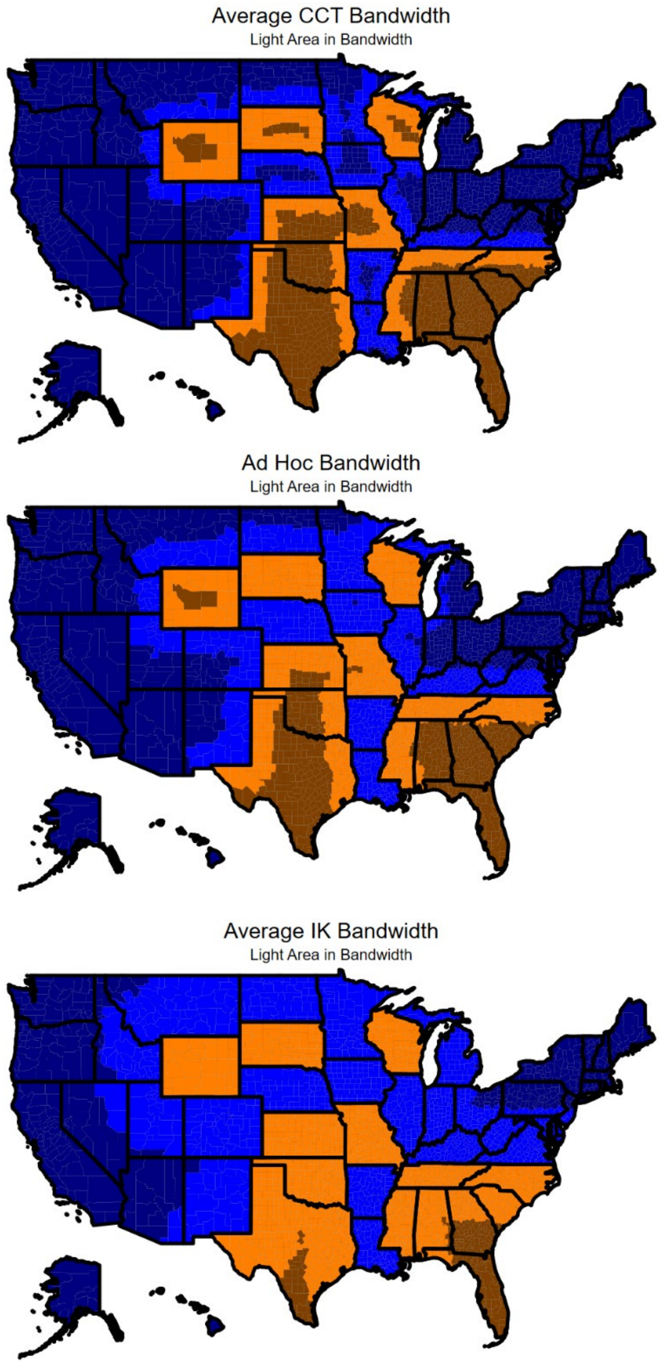
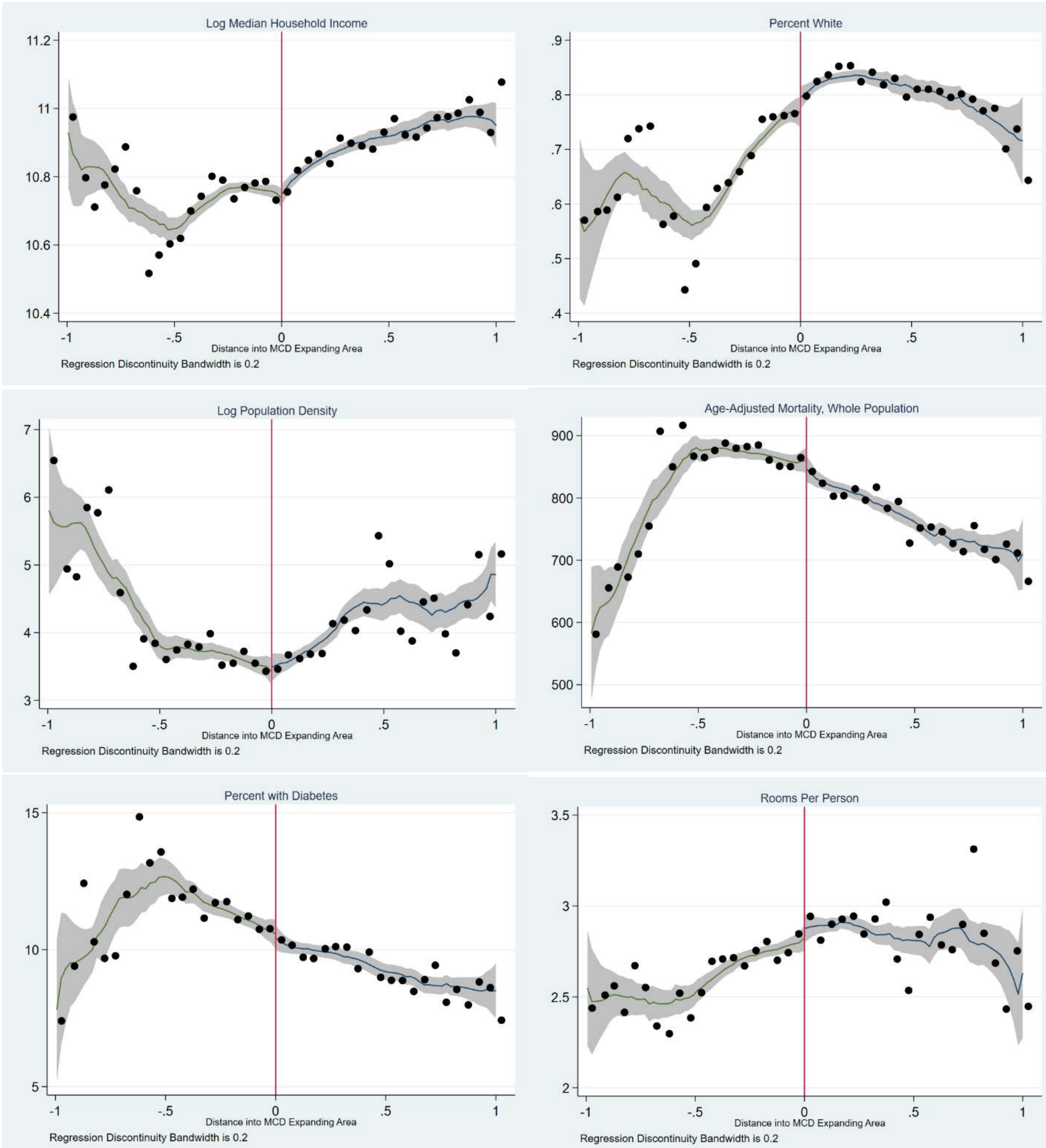
6 Figures

Figure 1



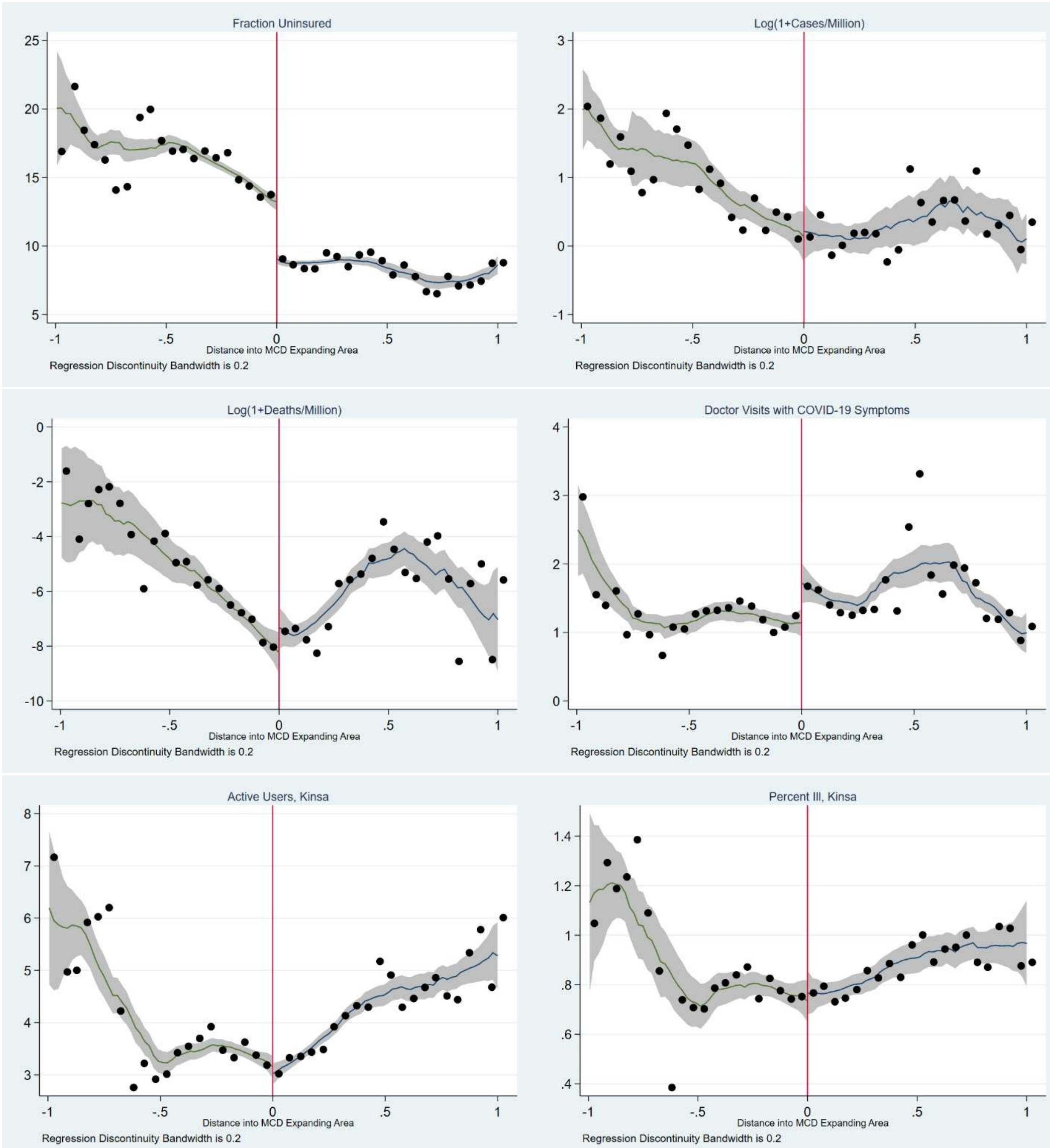
CCT bandwidths computed using the methods of Calonico et al. (2014, 2017, 2020). Ad hoc bandwidth is equal to 0.2 thousand kilometers. IK bandwidth computed according to Imbens and Kalyanaraman (2012).

Figure 2



Data on median household income and rooms per capita from the 2014-2018 ACS. Data on fraction white and population density from the Census Bureau. Data on mortality from CDC Wonder and data on diabetes prevalence from IHME.

Figure 3



Data on percent uninsured from the Census Bureau's Small Area Health Insurance Estimates (SAHIE). Data on Covid-19 cases and deaths from the New York Times. Data on Covid-19-related doctor visits from Carnegie Mellon's CovidCast. Data on percent ill and active users from Kinsa, Inc.

7 Tables

Table I (I)

Placebo Regression Discontinuity Effects *RD Includes Interacted 1-D Polynomial and Border Segment FE* *All Effects in Standard Deviations of Dependent Variable*				
	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Log Median Household Income	.015	-.051	.114	.116
	(.125)	(.115)	(.104)	(.125)
BW	.2	.477	.076	.062
BWR	.2		.124	.102
Log Population Density	.032	.012	.134	.084
	(.109)	(.076)	(.124)	(.118)
BW	.2	.328	.096	.079
BWR	.2		.170	.139
In MSA	-.061	-.003	.148	.124
	(.120)	(.084)	(.146)	(.143)
BW	.2	.472	.099	.081
BWR	.2		.225	.184
Unemployment Rate (2019 LAUS)	-.159	-.227	-.255	-.243
	(.163)	(.170)	(.183)	(.166)
BW	.2	.340	.091	.075
BWR	.2		.148	.121
Spending Decline During Pandemic (Affinity)	.269	.224	.188	.140
	(.191)	(.182)	(.275)	(.238)
BW	.2	.387	.123	.101
BWR	.2		.194	.159
Percent White	.119	.063	.036	.080
	(.094)	(.090)	(.124)	(.125)
BW	.2	.303	.072	.059
BWR	.2		.147	.121
Age-Adjusted Mortality, Whole Population	-.067	-.047	-.094	-.013
	(.103)	(.120)	(.115)	(.123)
BW	.2	.268	.089	.073
BWR	.2		.133	.109
Age-Adjusted Mortality, Blacks aged 15-64	-.321***	-.171	-.177	-.217
	(.121)	(.118)	(.175)	(.190)
BW	.2	.770	.080	.066
BWR	.2		.128	.105

Table I (cont).

	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Log Hospital Expenditures	.016	.005	.106	.051
	(.093)	(.081)	(.134)	(.107)
BW	.2	.296	.130	.107
BWR	.2		.188	.154
ICU Beds	.014	.006	-.038	-.084
	(.055)	(.053)	(.117)	(.194)
BW	.2	.289	.086	.070
BWR	.2		.094	.077
Hospital Beds	.007	.017	-.066	-.128
	(.057)	(.055)	(.090)	(.172)
BW	.2	.264	.108	.089
BWR	.2		.098	.080
Fraction Owner Occupied	.006	.114	-.043	.028
	(.119)	(.095)	(.164)	(.194)
BW	.2	2.122	.092	.076
BWR	.2		.163	.133
SafeGraph Early Social Distancing	.056	.087	.025	.031
	(.118)	(.095)	(.136)	(.127)
BW	.2	.383	.116	.095
BWR	.2		.203	.167
Rooms Per Person	.116	.080	-.237	-.309
	(.164)	(.098)	(.198)	(.196)
BW	.2	1.229	.066	.054
BWR	.2		.274	.225
Public Transit	.053	-.038	.024	.041
	(.040)	(.090)	(.045)	(.052)
BW	.2	.265	.117	.096
BWR	.2		.064	.053
Pollution (NOAA, NO2 Concentration)	.018	.027	-.038	-.034
	(.089)	(.111)	(.115)	(.112)
BW	.2	.263	.086	.071
BWR	.2		.106	.087
Trump Vote Share 2016	.071	-.018	-.038	-.034
	(.151)	(.116)	(.163)	(.154)
BW	.2	.311	.088	.072
BWR	.2		.145	.119

Table I (cont).

	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Death Rate from Heart Disease	-.037	-.084	.000	.023
	(.127)	(.148)	(.125)	(.118)
BW	.2	.264	.087	.071
BWR	.2		.117	.096
Fraction Obese	-.157	-.102	-.164	-.168*
	(.107)	(.118)	(.101)	(.102)
BW	.2	.268	.083	.068
BWR	.2		.205	.168
Hypertension Rate	-.049	.014	-.137	-.184
	(.093)	(.109)	(.084)	(.113)
BW	.2	.261	.076	.062
BWR	.2		.133	.109
Percent with Diabetes	-.073	-.079	-.037	-.018
	(.110)	(.112)	(.132)	(.158)
BW	.2	.303	.101	.083
BWR	.2		.272	.222
Death Rate from Cancer	.193	.300***	.068	.077
	(.122)	(.116)	(.170)	(.171)
BW	.2	.285	.081	.066
BWR	.2		.205	.168
Respiratory Mortality	-.142	-.134	-.121	-.131
	(.109)	(.134)	(.111)	(.116)
BW	.2	.310	.089	.073
BWR	.2		.099	.081
Smoking Rate	-.076	-.028	-.052	-.016
	(.145)	(.150)	(.129)	(.134)
BW	.2	.537	.092	.075
BWR	.2		.107	.088

Each cell shows the estimate of the coefficient β from equation (1) with standard errors clustered on state in parentheses. Column 2 uses a bandwidth from Imbens and Kalyanaraman (2012), reported on line BW, while Columns 3 and 4 use bandwidths from Calonico et al. (2017) that differ to the left and to the right of the border, reported on lines BW and BWR respectively. Data sources are 2014-2018 ACS (median household income, fraction owner occupied, rooms per persion, fraction using public transit), Census Bureau (population density, fraction white), CDC Wonder (mortality variables), IHME (health status and smoking variables), the Guardian (Trump vote share), HCRIS (hospital facilities variables), NOAA (air pollution), and SafeGraph (social distancing).

Regression Discontinuity Effects on Main Variables *RD Includes Interacted 1-D Polynomial and Border Segment FE* *All Effects in Standard Deviations of Dependent Variable*				
	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Fraction Uninsured	-.846***	-.847***	-.846***	-.819***
	(.144)	(.145)	(.111)	(.106)
BW	.2	.290	.089	.073
BWR	.2		.111	.091
Log(1+Cases/Million), First Wave	.007	-.086	.134	.108
	(.287)	(.159)	(.328)	(.327)
BW	.2	1.573	.146	.120
BWR	.2		.352	.288
Log(1+Cases/Million), Cumulative	-.065	-.081	-.047	.006
	(.128)	(.098)	(.131)	(.136)
BW	.2	.671	.176	.144
BWR	.2		.188	.154
Log(1+Cases/Million), Second Wave	-.097	-.084	-.052	-.040
	(.102)	(.082)	(.106)	(.098)
BW	.2	.595	.181	.148
BWR	.2		.159	.130
Log(1+Deaths/Million), First Wave	.175*	-.014	.201	.141
	(.106)	(.113)	(.135)	(.128)
BW	.2	.752	.122	.100
BWR	.2		.240	.196
Log(1+Deaths/Million), Cumulative	-.026	-.104	-.063	.001
	(.133)	(.092)	(.148)	(.146)
BW	.2	.480	.172	.141
BWR	.2		.266	.217
Log(1+Deaths/Million), Second Wave	-.038	-.119	-.054	.013
	(.133)	(.106)	(.149)	(.152)
BW	.2	.462	.161	.132
BWR	.2		.298	.244
Doctor Visits with CS, February	.082	.046	.000	-.071
	(.115)	(.088)	(.128)	(.140)
BW	.2	.617	.139	.114
BWR	.2		.174	.142
Doctor Visits with CS, First Wave	.303**	.277***	.318*	.324*
	(.118)	(.097)	(.175)	(.179)
BW	.2	1.554	.091	.075
BWR	.2		.151	.123
Doctor Visits with CS, Cumulative	.193	.221	.122	.179
	(.146)	(.140)	(.198)	(.206)
BW	.2	1.235	.080	.066
BWR	.2		.155	.127
Doctor Visits with CS, Second Wave	-.020	.063	-.158	-.137
	(.158)	(.141)	(.208)	(.219)
BW	.2	3.638	.085	.069
BWR	.2		.257	.211

Each cell shows the estimate of the coefficient β from equation (1) with standard errors clustered on state in parentheses. Column 2 uses a bandwidth from Imbens and Kalyanaraman (2012), reported on line BW, while Columns 3 and 4 use bandwidths from Calonico et al. (2017) that differ to the left and to the right of the border, reported on lines BW and BWR respectively. Data sources are SAHIE (percent uninsured), New York Times (Covid-19 cases and deaths), and Carnegie Mellon's CovidCount (Covid-19 related doctor visits).

Table III (III)

Regression Discontinuity Effects on Kinsa Outcomes *RD Includes Interacted 1-D Polynomial and Border Segment FE* *All Effects in Standard Deviations of Dependent Variable*				
	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Percent Ill, First Wave	.069	-.000	.055	.040
	(.089)	(.059)	(.119)	(.112)
BW	.2	.456	.130	.107
BWR	.2		.321	.263
Active Users, First Wave	-.009	-.002	-.007	.006
	(.034)	(.028)	(.034)	(.038)
BW	.2	.303	.175	.144
BWR	.2		.309	.253
Percent Ill, Second Wave	.038	.009	.048	.035
	(.078)	(.054)	(.086)	(.084)
BW	.2	.575	.138	.113
BWR	.2		.441	.362
Active Users, Second Wave	-.013	-.017	.002	-.009
	(.030)	(.023)	(.035)	(.033)
BW	.2	.293	.126	.104
BWR	.2		.237	.194
Percent Ill, Cumulative	.051	.002	.017	.015
	(.087)	(.042)	(.110)	(.099)
BW	.2	.529	.143	.117
BWR	.2		.325	.266
Active Users, Cumulative	-.011	-.007	-.012	.010
	(.031)	(.025)	(.034)	(.032)
BW	.2	.298	.152	.124
BWR	.2		.284	.233
Percent Ill, Pre-Period	-.095	-.085	-.003	.005
	(.094)	(.053)	(.095)	(.098)
BW	.2	.436	.109	.089
BWR	.2		.281	.230
Active Users, Pre-Period	.005	.007	.012	.031
	(.036)	(.028)	(.035)	(.038)
BW	.2	.321	.175	.143
BWR	.2		.305	.250

Each cell shows the estimate of the coefficient β from equation (1) with standard errors clustered on state in parentheses. Column 2 uses a bandwidth from Imbens and Kalyanaraman (2012), reported on line BW, while Columns 3 and 4 use bandwidths from Calonico et al. (2017) that differ to the left and to the right of the border, reported on lines BW and BWR respectively. Data on percent ill and active users comes from Kinsa, Inc.

Heterogeneity in Doctor Visits by Subsample *RD Includes Interacted 1-D Polynomial and Border Segment FE* *All Effects in Standard Deviations of Dependent Variable*				
	(1)	(2)	(3)	(4)
	BW=0.2	BW=IK	BW=MSE	BW=CER
Fraction Uninsured	-.541***	-.416***	-.513***	-.510***
Neither Low-Income nor Majority-Minority	(.122)	(.126)	(.114)	(.109)
BW	.2	.294	.092	.076
BWR	.2		.134	.110
Doctor Visits with CS, First Wave	.114	.030	-.262	-.202
Neither Low-Income nor Majority-Minority	(.141)	(.179)	(.256)	(.270)
BW	.2	.590	.083	.068
BWR	.2		.131	.107
Fraction Uninsured	-.952***	-1.03***	-.883***	-.856***
Low-Income	(.156)	(.165)	(.126)	(.124)
BW	.2	.388	.083	.068
BWR	.2		.078	.064
Doctor Visits with CS, First Wave	.437**	.299**	.562**	.523*
Low-Income	(.180)	(.134)	(.246)	(.270)
BW	.2	.515	.085	.070
BWR	.2		.177	.146
Fraction Uninsured	-1.01***	-1.38***	-1.48***	-1.28***
Majority-Minority	(.210)	(.235)	(.213)	(.244)
BW	.2	.465	.115	.098
BWR	.2		.083	.071
Doctor Visits with CS, First Wave	1.033	.123	1.533	1.504
Majority-Minority	(.908)	(.547)	(1.066)	(1.104)
BW	.2	.543	.081	.069
BWR	.2		.128	.109
Fraction Uninsured	-.898***	-.854***	-.911***	-.892***
Trump 2016 Share Above Median	(.148)	(.159)	(.143)	(.153)
BW	.2	.297	.076	.063
BWR	.2		.129	.107
Doctor Visits with CS, First Wave	.369**	.397***	.273	.278
Trump 2016 Share Above Median	(.155)	(.116)	(.249)	(.259)
BW	.2	.409	.123	.101
BWR	.2		.136	.113
Fraction Uninsured	-.657***	-.635***	-.707***	-.719***
Trump 2016 Share Below Median	(.163)	(.188)	(.145)	(.154)
BW	.2	.350	.112	.092
BWR	.2		.109	.090
Doctor Visits with CS, First Wave	.250	.225	.318*	.278
Trump 2016 Share Below Median	(.154)	(.138)	(.180)	(.180)
BW	.2	.841	.161	.132
BWR	.2		.156	.128

Each cell shows the estimate of the coefficient β from equation (1) with standard errors clustered on state in parentheses. Column 2 uses a bandwidth from Imbens and Kalyanaraman (2012), reported on line BW, while Columns 3 and 4 use bandwidths from Calonico et al. (2017) that differ to the left and to the right of the border, reported on lines BW and BWR respectively.

Estimation sample indicated under dependent variable label. Data sources are SAHIE (percent uninsured), Carnegie Mellon's CovidCount (Covid-19 related doctor visits), the 2014-2018 ACS (low income and majority-minority designations) and the Guardian (Trump vote share data).