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The Affordable Care Act and the COVID-19 Pandemic: A Regression Discontinuity Analysis

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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 testing is discontinuously larger in Medicaid-expanding states, and (3) the fraction of beds occupied in ICUs is discontinuously smaller in Medicaid-expanding states. We also find that (4) COVID-19 cases and deaths do not change discontinuously at the frontier, with the precision of these estimates being low, but the null result on deaths being general across demographic groups. Finally, we find that (5) smart thermometer readings of fever rates from Kinsa, Inc. do not change discontinuously at the Medicaid expansion frontier.

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. and on the ensuing medical response. 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 on the local intensity of the Covid-19 pandemic is ambiguous.

To address 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, we cannot match infected or deceased individuals to their insurance status as of this time; however, we document that there are sizeable differences in insurance rates that appear discontinuously at the borders, confirming that

[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.

the ACA 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 age and racial composition and social infrastructure such as the number of persons per room and the intensity of public transit use, remain continuous across these borders. Controlling for state policies during the pandemic also does not affect our results. Hence, it is likely that our analysis obtains the causal effect of Medicaid expansion on reported outcomes related to Covid-19. We conduct our analysis for the time period between March 2020 (the start of the pandemic) and July 2021 (the point at which a large fraction of the U.S. population was vaccinated, and right before the Delta and Omicron waves).

We find moderate and frequently significant positive effects of Medicaid expansion on Covid-19 related utilization. First, we observe a robust and significant increase in log Covid-19 tests per million just on the Medicaid expansion side of the boundary. Additionally, we find that during the third (fall 2020 - spring 2021) wave of the pandemic, both ICU bed occupancy ratios and Covid-19 related ICU hospitalizations were discontinuously lower on the Medicaid-expanding side of the border, which is consistent with a greater ability to deploy additional resources to the ICU and a better management of Covid-19 cases in areas that expanded Medicaid.

However, while we find significant effects of Medicaid expansion on utilization, we do not see statistically significant or large effects of Medicaid expansion on measurable pandemic outcomes. We find that there are no statistically significant discontinuities in Covid-19 reported cases or deaths per million across the Medicaid expansion borders, with the point estimates being small in magnitude compared to the cross-sectional variation in these measures. We also do not find statistically significant discontinuities for deaths per million when separately looking at the population aged 15-64, the population aged 65+, as well as when separately considering non-Hispanic white, Black and Hispanic Americans. While our results do not exclude the possibility that some subpopulations for which data on Covid-19 outcomes is not systematically available (e.g. near-elderly low-income Americans) may have experienced significant effects of Medicaid, and while the confidence intervals of our null results generally do not exclude sizeable negative as well as positive effects, our results suggest that it is unlikely that ACA Medicaid expansion explains a large share of the overall geographic or demographic pattern of Covid-19 outcomes.

We also consider the possibility that 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 assess this interpretation, we consider data on temperature readings from smartphone-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 potentially 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. (2021) 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. Their results are supported by Borgschulte and Vogler (2020), who find that the ACA Medicaid expansion decreased mortality based on a propensity score matching analysis of counties with different Medicaid expansion decisions, and by the randomized experiment of Goldin et al. (2021), who find that informing people of their violation of the mandate penalty causes them to sign up for health insurance and decreases their mortality rate.

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 and other health effects of the ACA.

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 main results. Section 5 explores the robustness checks. Section 6 concludes

2 Data

2.1 Medicaid Expansion Definitions

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). The set of Medicaid-expanding states changed during the pandemic, with Nebraska implementing its expansion in October 2020, and Oklahoma and Missouri adopting but not implementing their expansions. We consider robustness to including Nebraska in the set of nonexpansion states.

2.2 Data on Main Outcome Variables

We use county-level data on cumulative Covid-19 reported cases, deaths and tests per million from the Opportunity Tracker (Chetty et al. 2020) as of April 6. 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 2021), so measurement error in reporting is likely to be both systematic and large. We also use county-level data on Covid-19 deaths per million by age or racial and ethnic category for 2020 alone from CDC Wonder.

We utilize data on beds and hospitalizations by hospital facility from the Department of Health and Human Services (2020). This data was released to the public in the fall of 2020 and provides weekly counts of total and Covid-19 related ordinary and ICU hospitalizations for over 4,000 facilities in the U.S., each of which is matched to the county in which it is located. As this dataset starts in August 2020, we use it to analyze only the third (fall) wave of the pandemic, which peaked in early January 2021. It is worth noting that many counties do not have hospitals and even more counties lack ICUs, so our sample of counties for the analysis of hospitalization data differs from the full sample of counties. We confirm that covariates for these smaller samples are balanced as they are for the full sample, and that our other results remain qualitatively unchanged if the hospital and ICU samples are used.

To validate that uninsurance rates fall at the Medicaid expansion border, and that the change is due to a decrease in Medicaid coverage rather than insurance coverage from another source, we use data from the 2015-19 5-year ACS. We also supplement this data with 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 Opportunity Insights) 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.

Finally, we 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.

[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

2.2.1 Data on Smart Thermometer Readings from Kinsa

To address the potential confounding of the real and reporting responses in measuring Covid-19 related outcomes, 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 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]

Smart thermometer data is useful to address the concern that an individual with Covid-19 is more likely to be a reported case if they have access to Medicaid than if they do 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

3.1 Regression Discontinuity Design

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

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

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 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 after the pandemic began). 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.

A concern about our identification assumption may be that other factors systematically change at the borders of Medicaid-expanding states with nonexpanding ones. It is well known that in general there are discontinuities in economic outcomes at U.S. state borders (e.g. Holmes 1998) coming from differential state policies. While we cannot rule out this concern completely, we attempt to address this concern by including indices of state policies during the Covid-19 pandemic provided by Hallas et al. (2020) as controls in equation (1) in robustness checks. We also take comfort that most of the outcomes that we consider in the placebo checks, some of which (e.g. log income per capita) can be discontinuous across state boundaries, do not show evidence of discontinuities across the Medicaid expansion boundary. Intuitively, many states on the Medicaid expansion boundary have expanded Medicaid only recently, and given the spread of Medicaid expansion, the boundary generally divides states that are similar to each other rather than states with very divergent economic and political identities.

As our design is a geographic regression discontinuity design, we also present robustness checks including polynomials in latitude and longitude as additional controls in the spirit of Dell (2010) and Dell and Querubin (2018). Our estimates remain essentially unaffected.

3.2 Choice of Bandwidth

We present our main results using the bandwidth selection procedures suggested by Calonico et al. (2014, 2017, 2020) that allow for different bandwidths on different sides of the Medicaid expansion border to minimize the mean squared error (MSE) of the estimate of the discontinuity. We always use a uniform (rectangular) kernel for our analysis. As robustness checks, we present estimates using bandwidths that minimize the coverage error rate (CER), and we also present estimates that employ corrections for the bias of the regression discontinuity estimator, taking the correction into account when computing optimal bandwidth size. We report the bandwidths used for every main result or placebo estimate in units of one thousand kilometers; it is worth noting that a bandwidth of approximately 1330 kilometers includes the entire continental United States. Owing to their noncontiguity with the Medicaid expansion boundary, we exclude all areas in Alaska, Hawaii, Puerto Rico or other U.S. island territories from our estimates.

Figure 1 provides a map of the continental United States showing counties that fall within 100, 150 and 200 miles of the Medicaid expansion border (roughly the first, second and third quartiles of estimated bandwidths in our analysis). It is clear that the largest bandwidths involve including most of the counties in the states that directly lie on the Medicaid expansion border, while the typical bandwidths involve including fewer counties. It is rare that counties from states away from the Medicaid expansion border receive positive weight from the bandwidth.

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). We standardize all dependent variables to have unit variance across counties, allowing us to interpret the discontinuity that we estimate in terms of standard deviations of the variable. We first present discontinuity estimates for variables that we would not expect to be discontinuous across the Medicaid expansion border. Some of these are basic demographic and economic variables such as log median household income per capita, log population density, the percentages of county population that are white, Black and above the age of 80, whether the county is urban (in an MSA) and the 2019 unemployment rate. Others are more specifically tied to the way that different counties entered the pandemic and to potential determinants of Covid-19 intensity, such as the extent to which individuals spent time away from home in the three weeks before the 10th Covid case in their county (using data from SafeGraph), the fraction of housing stock that is owner occupied, the number of rooms per person (a measure of urban crowding), the fraction of individuals using public transit, and the spending decline during March 2020 as the pandemic was beginning in the U.S. as a whole. Given the likely importance of political attitudes to behavior during the pandemic, we consider two political variables. First, we investigate potential discontinuities in Donald Trump's vote share in the county in the 2016 election. Second, bearing in mind that voters who supported Trump specifically may have systematically different attitudes to Covid-19 than voters who support the Republican party more generally, we consider the change between 2012 Republican candidate Mitt Romney's vote share and Donald Trump's vote share. Reassuringly for the validity of our empirical approach, none of these 14 variables show any statistically significant discontinuities across the Medicaid expansion border, and the point estimates on all differences are small, generally within 0.2 standard deviations.[5] 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, which are predominantly "red".

The second group of variables that we consider are baseline indicators of population health before the beginning of the pandemic. These are the county's death rate from heart disease, the fraction of the county population that is obese, the rate of hypertension, the percent of population with diabetes, the death rate from cancer, the death rate from respiratory diseases, the smoking rate, the age-adjusted mortality rates for the whole county population as well as, separately, for Black Americans aged 15-64, the log hospital expenditures, and the numbers of ICU and hospital beds. All of these variables could have been affected by the Affordable Care Act before the pandemic, and all of them could subsequently affect the intensity of the pandemic. Therefore, it may not be surprising to find discontinuities in these variables suggesting better population health and more health resources on the Medicaid-expanding side of the border, and if such discontinuities are found, they may be the mechanism for any differentially better Covid-19 outcomes found on that side of the border.

[5] The only exception is the unemployment rate in 2019, which was 0.219 standard deviations lower in the Medicaid-expanding counties.

Many of the point estimates on the discontinuities in the health and mortality outcomes indeed show a negative sign, consistent with Medicaid expansion improving baseline health. The point estimate on the age-adjusted mortality rate of Black Americans aged 15-64 is small but negative, consistent with Sommers et al. 2015 and Miller et al. 2021, as Black Americans in this age group are more likely to be "compliers" of Medicaid expansion than most other demographic groups. However, of the 12 health outcomes we consider, only one is statistically significantly different on different sides of the border (fraction obese).

Panel 1 of Figure 2 presents the regression discontinuity plot for log median household income per capita, omitting border fixed effects and using an ad hoc bandwidth of 100 (0.1 thousand) kilometers. We see that this variable is remarkably continuous at the Medicaid expansion border. More generally, Appendix Figure AI presents regression discontinuity plots for all the placebo variables; 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 in general, although the point estimates on many of these variables are negative, consistent with the ACA having some small positive effects on underlying health. 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 Validation of Insurance Discontinuity

The bottom four rows of Table I confirm that there is a large discontinuity in insurance rates at the Medicaid expansion border and that it is driven by differences in Medicaid coverage rather than other types of insurance coverage (e.g. private employer-based or individually purchased coverage). First, we confirm that there is a very large (0.78 standard deviation) drop in uninsurance for individuals aged between 18 and 65 at the Medicaid expansion boundary using the 2018 SAHIE data. 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 about 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.

Next, we verify that this change in insurance rates also exists in the 2015-2019 5-year ACS. The 5-year ACS implicitly differentially weights states that adopted the ACA Medicaid expansion at different dates and is therefore less current, but it includes information for states that expanded Medicaid in 2019, does not rely on model-based approximations for small counties and allows disaggregation of health insurance by type. We find a 0.6 standard deviation increase in the fraction insured in the 5-year ACS, which is significant at 1%. Finally, in the last two rows of Table I, we show that the entire increase in insurance in the 2015-2019 5-year ACS comes from a sharp increase in the fraction of individuals reporting to be insured with Medicaid (0.7 standard deviations). The fraction of individuals reporting to have insurance other than Medicaid, on the other hand, remains continuous across the Medicaid expansion boundary. This is consistent with the absence of crowdout of non-Medicaid insurance into Medicaid reported by, e.g., Frean et al. (2019). Panels 2 and 3 of Figure 2 present regression discontinuity plots confirming the large, discontinuous increase in Medicaid insurance at the Medicaid expansion boundary, and the absence of change in other types of insurance.

4.3 Main Results

We now proceed to our main results, which we present in Table II. As different variables that we consider are defined over different time periods, and as we wish to give a sense of the heterogeneity in the effects (or non-effects) we find over time, we present both cumulative estimates and estimates by wave of Covid-19 in the U.S. for the variables available since the start of the pandemic (cases, deaths, tests, doctor visits and Kinsa outcomes). On the other hand, we present the hospitalization-related variables forthree separate components of the "third" wave of the virus, which was also the most severe in the U.S. on average in terms of clinical outcomes before the advent of mass vaccination. We present results for the time period before the peak (August to late November 2020), the time period around the peak (December through February) and the period after the peak (February to April).

As our outcome variables vary along radically different scales and as their dispersion changed markedly over the course of the pandemic, each cell presents the standardized coefficient of the variable as the main estimate and the regular (unstandardized) coefficient next to it in brackets. On the second line of the cell, we present the standard error of the standardized coefficient (in parentheses) as well as the bandwidths in thousands of kilometers to the Medicaid-nonexpanding and the Medicaid-expanding sides of the cutoff respectively.

We focus discussion on the standardized coefficients for ease of comparison of the sizes of the effects.

4.3.1 Results for Covid-19 Cases and Deaths per Million

We do not find any discontinuities in the intensity of the Covid-19 pandemic, measured by log cases or deaths per million, at the Medicaid expansion border. (Logged variables are offset by unity to avoid the argument of the logarithm ever becoming zero). The first two panels present cumulative and wave-specific estimates of discontinuities in these variables, finding no estimates that are both negative and statistically significant. In fact, the discontinuities in cases and deaths at the border during the first wave of the virus (March 1 - July 4) are positive, suggesting that Medicaid-expanding areas may have experienced more cases and deaths per million than did Medicaid-nonexpanding areas. The other point estimates (for the second and third wave, as well as for the pandemic period as a whole) are negative but small (below 0.1 standard deviation in magnitude).

It is important to consider the width of our confidence intervals on these null results. For the first wave (during which our point estimates are positive) we can reject declines greater than 0.14 standard deviations for cases and 0.05 standard deviations for deaths. However, for the other waves and for the cumulative pandemic period, our confidence intervals include declines between 0.2-0.4 standard deviations. One reason for the relative imprecision of our estimates for deaths may be that data for small, rural counties are noisy. In panels 10 and 11 of Table II, we narrow down to samples of larger counties that have hospitals or ICUs. Here, we can rule out decreases of log deaths per million that are larger than 0.3 standard deviations for the hospital sample and 0.2 standard deviations for the ICU sample for the pandemic as a whole. Considering the unstandardized coefficients, for the ICU sample, we can rule out a larger than 0.12 log point discontinuous decrease in deaths per million in Medicaid-expanding areas relative to nonexpanding ones.

We corroborate our estimates by presenting graphical evidence. First, panels 4 and 5 of Figure 2 present the regression discontinuity plots for log cases and deaths per million over the course of the entire pandemic. No discontinuities are evident at the boundary, especially when compared with the scope of the variation of the graph and with the prominent discontinuity in Medicaid coverage that we observed in Panel 2. Second, we present week-by-week estimates of discontinuities in log cases and deaths per million in Panels 1 and 2 of Figure 3. For these figures, we run our regression discontinuity analysis for every week of the data (weeks beginning Friday) and plot the resulting estimates and standard errors as a time series. We also present dashed lines denoting the range of effect sizes of 0.2 standard deviations in absolute magnitude. We see no tendency for the estimates to be systematically below zero in any period of the data (and we do see some marginally significant tendency for the week-by-week estimates to be above zero during the weeks corresponding to the first wave), confirming that our choice of subperiods was not masking any time when the Medicaid-expanding areas may have had significantly lower case or death rates than Medicaid-nonexpanding areas. We also see that the vast majority of the week-by-week estimates are less than 0.2 standard deviations in absolute magnitude.

Results on Deaths by Demographic Categories Using CDC Wonder Data We use the county-level crude rate data on deaths per million for the population as a whole that was released by the CDC continuously through the pandemic as our baseline measure of death rates because of its comparability to the cases, tests and hospitalizations data and because of its availability through the summer of 2021. However, we take advantage of the release of CDC Wonder data for 2020 in January 2022 to investigate the patterns of the effects of Medicaid expansion on Covid-19 death rates by age and racial groups. The ACA affected Medicaid eligibility only for individuals under 65, thus any effects on Medicaid expansion on Covid-19 related outcomes on the 65+ population would have to work indirectly through increased funding for hospitals thanks to receipt of Medicaid reimbursement for younger patients. On the other hand, effects for the under-65 population would measure both the direct effect of being insured and the indirect effect of more generous funding of health resources. The effects on Black and Hispanic Americans are also important to estimate separately because these populations have historically had higher uninsurance rates and saw greater rises in insurance coverage after the ACA Medicaid expansion (KFF 2021).

The fifth through eighth columns of Table III present estimates of the effect of Medicaid expansion on age-adjusted Covid-19 death rates for the 15-64 population (directly affected by expanded Medicaid eligibility), for the 65+ population (which accounted for the overwhelming fraction of Covid-19 deaths during the pandemic), and for the Black and Hispanic populations. The first row of this table presents the baseline estimates, and the subsequent rows present robustness checks, which will be discussed in the next section. We see that neither the 15-64 population nor Black and Hispanic Americans experienced statistically significant decreases in Covid-19 deaths per million during 2020, with the 15-64 and 65+ populations actually experiencing an increase, while Black and Hispanic Americans saw decreases of 0.11 and 0.03 standard deviations respectively. The confidence intervals on these estimates are, once again, large, and exclude decreases in deaths per million larger than only 0.2 standard deviations for the 15-64 population, 0.12 standard deviations for the 65+ population, 0.35 standard deviations for Black Americans and 0.17 standard deviations for Hispanic Americans. However, moving to the sample of large counties with ICUs (row 18 of

Table III) we find that, if anything, Covid-19 mortality was higher on the Medicaid-expanding side of the border for these three groups, with the lower confidence bounds excluding large mortality declines for the 15-64 population and Hispanic Americans, while allowing a 0.08 standard deviation decline for Black Americans and a 0.12 standard deviation decline for the 65+ population. Panels 7 through 9 of Figure 2 present regression discontinuity plots for the Covid-19 death rates of the 15-64 population and of Black and Hispanic Americans respectively. It is clear that there is no evidence of a discontinuity at the Medicaid expansion boundary in any of these variables. We therefore do not find precise evidence that Medicaid expansion may have decreased Covid-19 mortality for the Black and Hispanic populations, or that either Medicaid's direct effect of providing coverage or its indirect effect of funding hospitals acted to decrease Covid-19 mortality.

Finally, in order to address the concern that the Covid-19 pandemic may have exacerbated death rates from other causes (for example, because of overcrowded ICUs or fears of accessing the medical system) the last column of Table III provides estimates of the effect of the Medicaid expansion on total deaths per million in 2020, controlling for total deaths per million in 2019. The estimates are positive for the overall population as well as for white non-Hispanic and Black Americans and small in magnitude. We conclude that our regression discontinuity analysis does not provide strong evidence that Medicaid expansion reduced Covid-19-related mortality either on average or for specific demographic groups.

Interpretation of Null Results on Covid-19 Mortality Our null results for the effect of the ACA Medicaid expansion on Covid-19 mortality should be interpreted in light of the existing literature. They are most similar to Finkelstein et al. (2012), which found insignificant effects of Medicaid on mortality, with large standard errors. Subsequent studies have found significant negative effects of Medicaid on all-cause mortality in subsamples of the U.S. population that were particularly likely to be affected by insurance expansion and that have relatively high mortality rates, such as the low-income near-elderly (Miller et al. 2021) and the middle-aged uninsured (Goldin et al. 2021). As our analysis cannot consider Covid-19 outcomes separately for these groups, our results do not preclude significant negative effects of Medicaid on mortality for their members and are therefore not inconsistent with the above findings. However, our results do suggest that differential access to the ACA Medicaid expansion is not a major explanatory factor behind disparities in Covid-19 mortality across racial and ethnic (rather than income) groups. More generally, our results suggest that had the ACA Medicaid expansion been extended to the nonexpanding counties (perhaps, to the nonexpanding states) at the Medicaid expansion boundary, these areas would not have seen significant decreases in cases and deaths per million, and would not have seen differential decreases in deaths per million between different demographic groups. Such a conclusion is plausible also because the ACA Medicaid expansion directly affected a relatively small fraction of the overall population, which was also at a relatively low risk from Covid because of age.

4.4 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. 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 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

$$y_{c,2020} = \alpha + \beta MCD_{s(c)} + \zeta y_{c,2019} + \gamma_1 d_c + \gamma_2 MCD_{s(c)} \times d_c + \eta_{b(c)} + \varepsilon_c, \;\; |d_c| \leq b \qquad (2)$$

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). 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).

Panels 3 and 4 of Table II present our estimates of the discontinuities in the Kinsa "percent ill" measure and the number of active users for the entire pandemic as well as for each wave separately. We see that, for every period considered, 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 every time period.

Panel 6 of Figure 2 provides a regression discontinuity plot for the log percent ill Kinsa variable for the cumulative pandemic period. We see that this series is cleanly continuous through the Medicaid expansion boundary. Panel 4 of Figure 3 further shows that this variable is statistically indistinguishable from being continuous through the boundary in almost every week of the data. 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.

4.5 Results for Covid-19 Testing

Notwithstanding that we do not find evidence that Medicaid expansion ameliorated direct pandemic outcomes, we find moderate to strong evidence that Medicaid expansion increased health resource utilization in response to the pandemic. Panel 5 of Table II presents discontinuity estimates for the log of Covid-19 tests per million. In contrast with the case or death evidence, we see a moderately sized discontinuous increase in test rates (0.38 standard deviations) in Medicaid-expanding areas, which is statistically significant at 1% for the pandemic as a whole and for any wave individually. Panel 7 of Figure 2 and Panel 3 of Figure 3 show that not only is there a positive discontinuity in log Covid-19 tests per million during the entire period of the pandemic to the end of the third wave, but also that this discontinuity is present, significant and of roughly constant magnitude in virtually every week since May 2020, before which Covid-19 testing was relatively difficult to access. Hence, we conclude that the ACA Medicaid expansion sizeably increased Covid-19 testing in the areas where it had been implemented to date. The unstandardized coefficient in the cumulative log tests per million regression is consistent with Medicaid expansion increasing testing per million by 0.46 log points, or nearly 60%.

It is worth considering the channels through which the ACA Medicaid expansion could have operated to increase Covid-19 testing per capita. The FFCRA, passed in March 2020 and extended throughout the pandemic, mandated that Medicare, Medicaid and all private insurance cover Covid-19 testing, and allowed states to use Medicaid funding to cover Covid-19 tests among the uninsured. Fewer than half of the states indeed used Medicaid funding for this purpose, with the complier states disproportionately being Medicaid-expanding states. However, it is likely that the uninsured experienced considerably lower certainty that their Covid-19 test would be covered than insured individuals would, especially given their lower attachment to the health care system. Therefore, the uninsurance reducing effects of the Medicaid expansion may have been one channel through which it increased testing per person. Another channel could be that the health system was better resourced through a greater share of individuals having some kind of health insurance (Garthwaite, Gross and Notowidigdo 2018). Unfortunately, our identification strategy does not allow straightforwardly distinguishing between these two mechanisms.

In addition to the result on testing, we find statistically insignificant positive effects on Covid-19 related utilization of physician resources 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 pandemic as a whole and each of the three waves in Panel 7 of Table II. Doctor visits during the first wave of the pandemic (March 1 - July 4) increase by about 0.2 standard deviations as one crosses to the Medicaid expanding side of the boundary, which is statistically insignificant. Doctor visits during the third wave of the pandemic (October 12 - present) show a statistically insignificant discontinuity of the same size. However, doctor visits during the second wave are continuous across the Medicaid expansion boundary (if anything, somewhat lower on the Medicaid-expanding side).

Another respect in which Medicaid expansion may have affected utilization during the pandemic is through vaccination rates. Vaccinations began in the winter of 2021 and were performed freely for everyone, including the uninsured, however, medical providers in Medicaid-expanding areas may have been in a better financial situation relative to non-expanding areas, allowing them to vaccinate individuals faster. Moreover, indivdiuals in Medicaid-expanding areas may have had a stronger relationship with the medical system before. In Panel 9 of Table II we find statistically insignificant but positive effects of Medicaid expansion on vaccination both for the pandemic period as a whole and for every subperiod (roughly a month) before July 2021.

4.6 Results for Mobility

A critical feature of the pandemic has been the decline in mobility as individuals socially distanced to avoid infecting themselves and their families, and as work in some sectors began to be carried out from home. It is therefore interesting to examine how Medicaid expansion affected mobility. The direction of such an effect ex ante is ambiguous. On the one hand, if Medicaid expansion created easier access to testing, then individuals could be more certain of their Covid-19 infection status and less apprehensive of infecting relatives and friends if they were to visit them. On the other hand, if Medicaid expansion increased interactions with medical professionals, people could have obtained more accurate information about both the risks of Covid-19 infection and appropriate avoidance strategies (e.g. that infection is unlikely to happen outdoors but more likely to happen in crowded indoor settings), leading them to decrease mobility. Appendix Table AI presents estimates of our regression-discontinuity specification (1) with mobility measures from Google Mobility (available for 700-1700 counties of the approximately 3,000 total) as the dependent variable. These mobility measures are based on cell phone data, and in combination with high-resolution maps, allow the categorization of mobility by type of economic activity.

We see that Medicaid expansion has negative, moderate to large and generally significant effects on mobility in restaurants and recreation settings, as well as mobility in transit settings (rows 1 and 4 of the table). We also see negative, but insignificant and small effects on mobility in workplace and grocery and pharmacy settings, which are more difficult for individuals to avoid, as well as for mobility away from home more generally. Finally, we see small but statistically insignificant positive effects on mobility within residential settings (consistent with individuals staying at home more), and moderate and occasionally significant positive effects on mobility in parks. These patterns are consistent with the second hypothesized channel, in which greater interaction with the medical system by Medicaid-eligible individuals may lead them to adopt social distancing strategies recommended by the medical profession, such as avoiding restaurants and going to parks.

4.7 Results for Hospital and ICU Data

While data on Covid-19 cases, deaths and tests per million at the county level has been available over the entire course of the pandemic, data on Covid-19 related hospitalizations and admissions to the ICU tended to be available only at the state level. However, in December 2020, HHS publicly released comprehensive data on both regular and ICU hospitalization levels and hospital and ICU bed capacity at the level of the hospital facility, likely in response to growing concerns over the potential of the third wave of Covid-19 to overwhelm

ICUs in especially hard-hit regions. While this data is necessarily imperfect, regular and ICU hospitalizations are likely to be better recorded than cases and deaths during the third wave of the pandemic as medical professionals are, by definition, present with hospitalized patients and able to test them for Covid-19.

We collapse the facility-level data by county of facility location. Many counties do not have hospitals (with individuals from these counties going to hospitals in neighboring counties) and still more lack ICUs. Therefore, our samples for the hospital and ICU outcomes are partial subsets of U.S. counties. We check for covariate balance for these samples in Table III. We do not find violations of the continuity assumption for the covariates that should be pure placebos, although we do find discontinuously lower mortality rates for Black Americans aged 15-64 on the expanding side of the boundary in the ICU sample.

As we have data only for the third wave of the pandemic, we split the sample into three parts: the peak of the wave (approximately December 2020 and January 2021), the period before the peak and the period after the peak. In panels 10 and 11 of Table II, we see that log Covid-19 related hospital admissions per million and the fraction of hospital beds occupied do not have statistically significant discontinuities at the Medicaid expansion border. However, in panels 12 and 13 of Table II, we see statistically significant negative discontinuities of moderate size (greater than 0.2 standard deviations in absolute magnitude) for the fraction of ICU beds occupied and the log Covid-19 related ICU admissions per million. One should note that there are no negative discontinuities in log Covid-19 deaths per million for the counties in the ICU sample, so the negative discontinuity in log Covid-19 ICU admissions per million must be offset by a positive discontinuity in log Covid-19 deaths per ICU admission. However, these discontinuities may be interpreted as evidence that Medicaid-expanding areas use ICUs more efficiently and route only the sickest patients to the ICU. Panels 8 and 9 of Figure 2 present graphical evidence of the discontinuities in the ICU-related outcomes, while Panels 5 and 6 of Figure 3 show that the discontinuities in the fraction of ICU beds occupied occur mainly at and after the peak of the third wave, while the discontinuities in the log of Covid-19 ICU admissions per million are more evenly distributed over time.

5 Robustness Checks

Table III presents a summary of robustness checks of our main results. First, for each robustness check, we verify whether there are any discontinuities in the 14 covariates we expect to be continuous across the Medicaid expansion border. We then investigate the robustness of our null result on log deaths per million, as well as of the discontinuities in the main variables for which we find effects, log tests per million and the fraction ICU beds occupied. Finally, we present supporting estimates for our null result on log deaths per million using CDC Wonder data for 2020 for the 15-64 population as well as Black and Hispanic Americans. For each regression we present the standardized coefficient and its standard error for better assessment of the magnitude of the effect.

Our baseline specification chose different bandwidths on either side of the Medicaid expansion boundary, selecting the bandwidths to minimize the MSE of the discontinuity estimate. Panels 1 through 4 of Table III explore alternative ways to select the bandwidths within the frameworks of CCT (2014) and Calonico et al. (2019). Specifically, Panel 2 investigates using the same bandwidth on both sides of the boundary, Panel 3 selects the bandwidths to minimize the coverage error rate (CER) of the confidence interval rather than the MSE of the estimate, and Panel 4 employs a correction for the bias of the nonparametric estimate proposed in CCT (2014). Neither procedure substantially affects our baseline results. We can also test for robustness to the regression discontinuity methodology by acknowledging the geographic nature of our discontinuity design and adding a polynomial of increasing complexity in latitude and longitude as a control (Dell 2010). Panels 5 through 8 show that adding polynomials in latitude and longitude of order up to 4 has minimal effect on most of our results. When a first-order polynomial in latitude and longitude is added, the discontinuity in Black 15-64 mortality rates becomes marginally significant and negative, which would, if anything, work against our finding of no discontinuity in Covid-19 mortality rates, but this discontinuity stops being statistically significant when higher order polynomials are used. On the other hand, when higher order polynomials are used, the discontinuity in log deaths per million grows to -0.18 (relative to -0.02 in the baseline specification) but remains statistically insignificantly different from zero. Our placebo checks fail to reject the continuity of any of the baseline covariates across the Medicaid expansion border.

Broderick, Giordano and Meager (2021) present evidence on the importance of influential observations to traditional analysis using moments that are functions of sample averages. Motivated by their work, we investigate the robustness of our results to outliers and influential observations by running a median regression version of the regression discontinuity design as proposed by Chiang, Hsu and Sasaki (2019) (CHS). The CHS approach does not accommodate border segment fixed effects, which we will see in Panel 10 as not essential to our qualitative findings. Panel 9 presents the median regression results, which are close to our baseline estimates, although the discontinuity in ICU bed occupancy is no longer statistically significant, though of similar magnitude. It is also worth noting that the null result for log deaths per million is more precise than in the baseline specification (excluding a decline of more than 0.1 standard deviations). We find these results to be consistent with our baseline estimates not being excessively sensitive to outliers.

We also investigate robustness of our results to the covariates that we include in the regression discontinuity design. While we include border segment fixed effects (one for each pair of bordering states) in our baseline specification, the regression discontinuity design should be valid without the inclusion of these fixed effects and their role should largely be to improve precision. Panel 10 of Table III shows that our results are little affected by removing the border segment fixed effects. The discontinuity in the fraction of ICU beds occupied in the county loses significance but remains quantitatively similar to the baseline. A greater concern is that our approach assumes that other factors that are discontinuous at state borders – such as state policies to fight the Covid-19 pandemic – are unrelated to the error term in equation (1). We test this concern by including the Oxford Stringency Index for US states as a covariate in our specification in Panel 11, and by including an expanded version of this index that includes additional government policies in Panel 12. Neither check has large effects on our results or our placebo checks.

As discussed in Section 2, the set of states expanding Medicaid was not constant during the pandemic as Nebraska expanded Medicaid in October 2020. Moreover, while Wisconsin did not officially participate in the ACA Medicaid expansion, it had a very similar expansion of Medicaid on its own, using only a somewhat lower eligibility threshold than employed by the ACA Medicaid expansion. As Wisconsin forms an important part of the Medicaid expansion boundary (it is noncontiguous with the other Medicaid-nonexpanding states), it is reasonable to check robustness to treating Nebraska as a nonexpansion state and to treating Wisconsin as an expansion state. Panels 13 and 14 of Table III show that neither choice affects the statistical significance or qualitative magnitude of our baseline results. If anything, the estimates for log Covid-19 deaths per million are more positive and their confidence intervals exclude large declines.

It is also interesting to examine the sensitivity of our results to weighting each U.S. county by population. In principle, population weighting is not necessary for the regression discontinuity design to be consistent, however, if the variances of measurement errors in county outcomes are inversely proportional to population, then population weighting should deliver improvements in precision. Panel 15 presents our results, which are very similar to the baseline estimates except that the effect of the Medicaid expansion on the 15-64 mortality rate is estimated to be a statistically significant -0.37. This would be a sizeable effect that would challenge our interpretation of the mortality findings. However, it is largely dependent on outliers; as Wisconsin forms part of the Medicaid expansion boundary, Chicago and its environs (containing some of the largest U.S. counties by population) exert disproportionate weight on the estimates. In Panel 16 we recompute our population weighted estimates while counting Wisconsin as an expansion state, and the statistically significant

-.37 estimate for the 15-64 mortality rate turns into a statistically insignificant -0.11. The other estimates are unaffected, except the estimate for log tests per capita loses statistical significance, nevertheless retaining its magnitude. Our placebo checks also continue to show continuity of the baseline covariates across the Medicaid expansion border, even when the sample is substantially reweighted.

We also consider the sensitivity of our estimates to restricting the sample of U.S. counties to various subsamples. First, in panels 17 and 18, we consider the subsamples of counties with hospitals and ICUs in the HHS weekly data on Covid-19 hospital outcomes by facility (see Section 4.6). As discussed in that section, the discontinuities in our outcomes of interest are little affected. We also consider geographic heterogeneity in our results. Panel 19 considers restricting to just the Southern segment of the Medicaid expansion border (the connected stretch along the Mississippi river and then through the Kentucky-Tennessee and Virginia-North Carolina segments). This is an important robustness check to conduct as this represents a connected stretch of the border, with the states along it sharing strong regional commonalities. The discontinuity in testing becomes especially pronounced, and the estimates of the effects of Medicaid on death rates for the 15-64 population as well as for Black and Hispanic Americans become more negative, though they remain statistically insignificant. Panel 20 provides a similar exercise for all segments of the Medicaid expansion boundary that lie outside the Southern segment, with results being similar to the baseline, although two of the 14 placebo checks fail. Finally, Panel 21 considers only the border segments at which the governors of the two bordering states come from the same political party. For example, the Virginia-North Carolina segment is included, as both states had Democratic governors during the pandemic period, while the Louisiana-Mississippi segment is excluded, as Louisiana had a Democratic governor while Mississippi had a Republican one. Allcott et al. (2020) document heterogeneity in Covid-19 outcomes by political identity, suggesting that states with different partisan identities of the governor may have handled the pandemic differently. The discontinuity in the fraction ICU beds occupied is insignificant and small, but the null discontinuities in most of the placebo measures and in Covid-19 deaths per million, as well as the strong discontinuity in testing per million remain.

Finally, we consider how our baseline results differ by performing a regression discontinuity analysis relative to a naive continental U.S.county-wide OLS regression of the outcomes of interest on a Medicaid expansion dummy, performed in Panel 22. We see that five of the 14 baseline covariates have a systematic relation with Medicaid expansion. We also see that log Covid-19 death rates per million are statistically significantly lower (at 1%) in counties with Medicaid expansion, with the magnitude of the standardized coefficient being a moderately sized -0.43 standard deviations, reflecting the widely reported (e.g. The Economist, 2020 article cited at the beginning of the paper) negative correlation between insurance coverage and Covid-19 health outcomes. Effects of this size are outside the confidence intervals of our baseline estimates of the discontinuity of log death rates per million as well as of most of these estimates in our robustness checks. Interestingly, Appendix Table 3 shows that the OLS estimates of the effects of Medicaid expansion on the 15-64 population and on Black and Hispanic Americans are statistically insignificant and frequently positive, with the negative and significant estimate for overall mortality driven by the 65+ population and white, non-Hispanic Americans. On the other hand, the correlations between Medicaid expansion and the log of the Covid-19 test rate, or the ICU occupancy rate, are similar to the baseline.

6 Conclusion

Our analysis has shown that (1) there is a large, discontinuous decrease in the fraction of the population uninsured at the Medicaid expansion boundary, (2) there is a moderate to large and statistically significant discontinuous increase in Covid-19 tests per million at the boundary, and (3) there is a moderate to large and statistically significant discontinuous decrease in Covid-19 ICU hospitalizations per million and in the fraction of ICU beds occupied at the boundary. On the other hand, we also find that (4) there are no significant discontinuous jumps of reported Covid-19 cases, hospitalizations and deaths per million across the Medicaid expansion boundary, with the lack of discontinuity for deaths being general across age and racial and ethnic groups, but with the standard errors being large, and (5) there are no discontinuous jumps in feverish temperatures recorded by smart thermometers. Covariates that may affect Covid-19 transmission appear to be continuous across the boundary, validating that our analysis is appropriate.

It is important to understand what our paper does and does not show. Our findings are consistent with the results of Finkelstein (2007) and Finkelstein and McKnight (2008) on Medicare, as well as Baicker et al. (2013) and Finkelstein et al. (2012) on Medicaid, which show that health insurance expansion increases utilization but does not have strong and precise effects on physical measures of health. Our estimates could be interpreted as suggesting that if Medicaid were expanded to the nonexpanding counties near the boundary, it would likely not have affected pandemic outcomes in these counties, but would have affected testing and ICU occupancy. Effects of Medicaid expansion to counties far away from the expansion boundary could be different, as could be the effects of health insurance provision through a different program than Medicaid, or the effects of Medicaid on a different subpopulation than we analyze (e.g. low-income or uninsured near-elderly Americans as in Miller et al. 2021 and Goldin et al. 2021). Our 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. It should be noted that the effects of health insurance on mortality are likely very heterogeneous by the source of mortality, so our results are very specific to the context of Covid-19, an acute, infectious disease. It is also very likely that the utilization effects of the ACA Medicaid expansion – increased testing and decreased ICU crowding, even conditional on no changes in Covid-19 deaths per million – may be providing unobserved benefits that are not captured by any of the variables we analyze, such as peace of mind, better individual health choices and potentially even effects on non-COVID mortality. Investigating these channels should be an important topic for future research.

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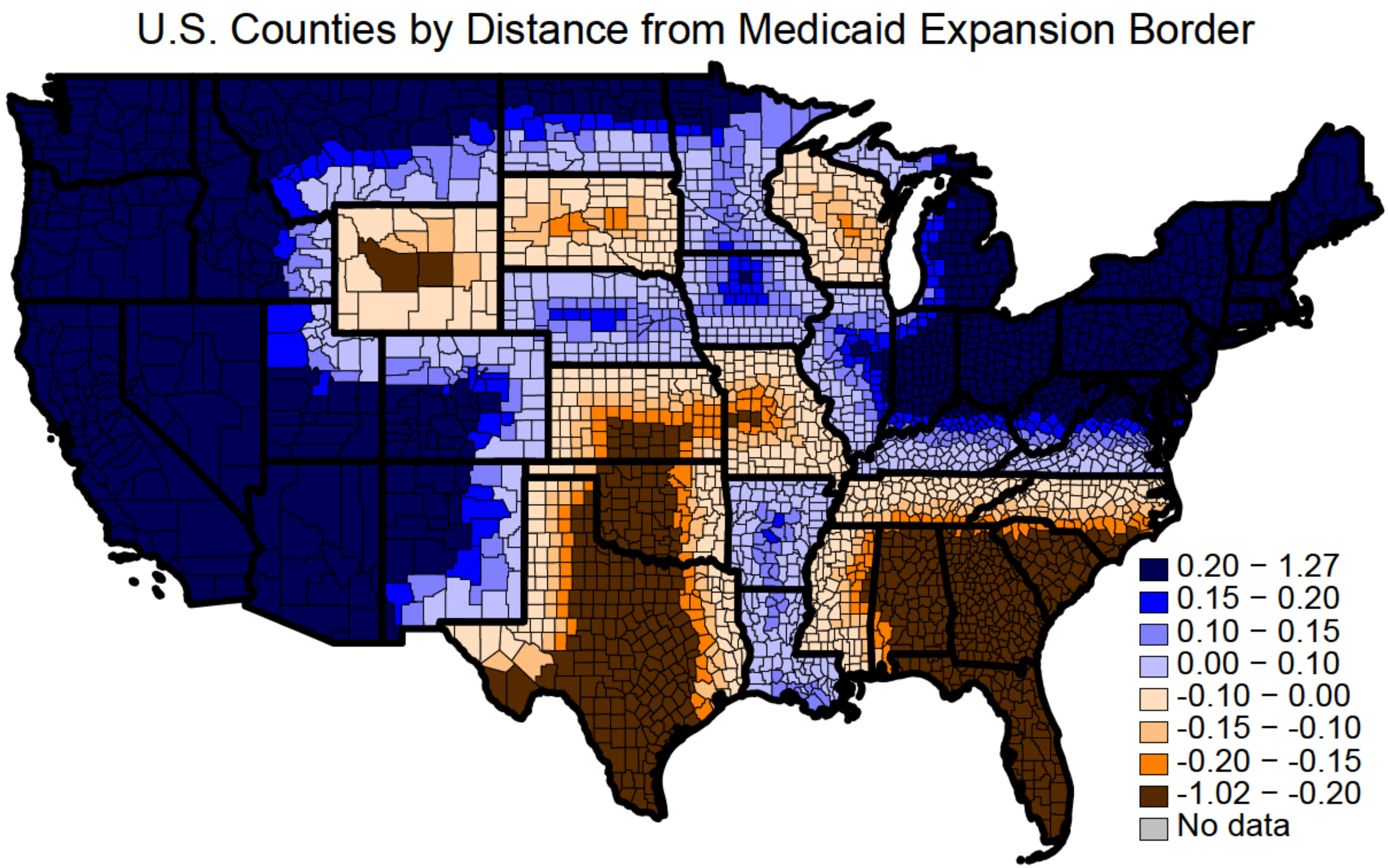
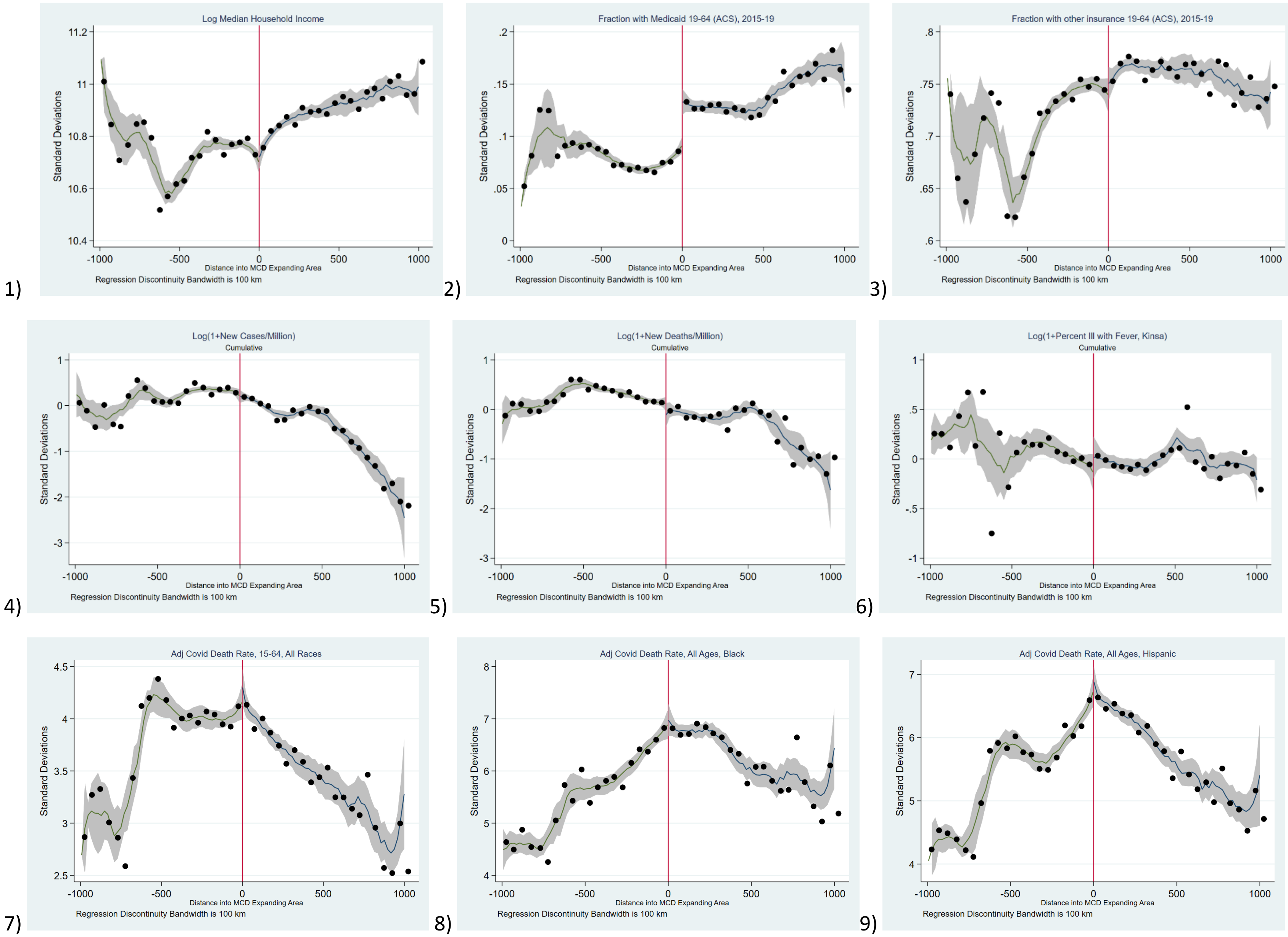
Figure 1



All distances in units of 1000 km

Figure 2



1)
Log Median Household Income
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
2)
Fraction with Medicaid 19-64 (ACS), 2015-19
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
3)
Fraction with other insurance 19-64 (ACS), 2015-19
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
4)
Log(1+New Cases/Million)
Cumulative
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
5)
Log(1+New Deaths/Million)
Cumulative
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
6)
Log(1+Percent Ill with Fever, Kinsa)
Cumulative
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
7)
Adj Covid Death Rate, 15-64, All Races
Standard Deviations
Distance into MCD Expanding Area
Regression Discontinuity Bandwidth is 100 km
8)
Adj Covid Death Rate, All Ages, Black
Standard Deviations
Distance into MCD Expanding Area
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9)
Adj Covid Death Rate, All Ages, Hispanic
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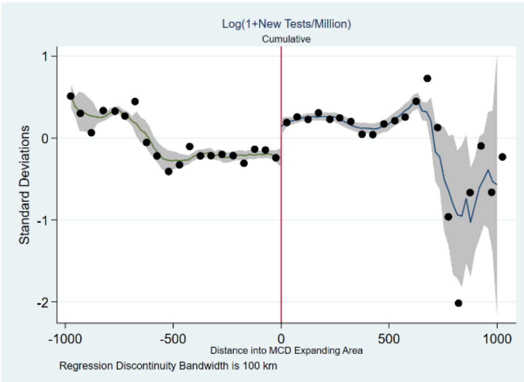
Regression Discontinuity Bandwidth is 100 km



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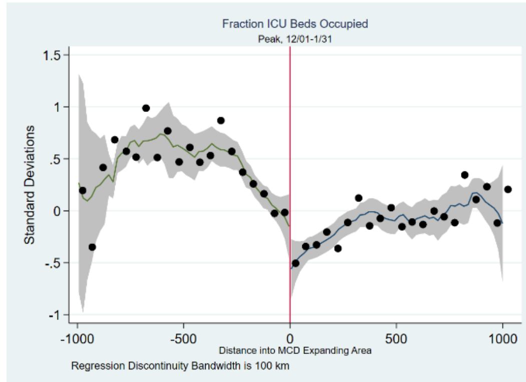

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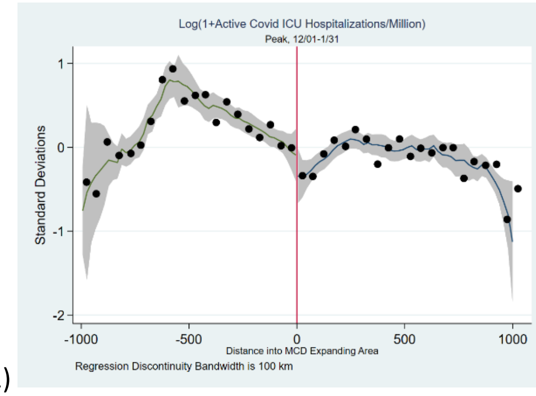

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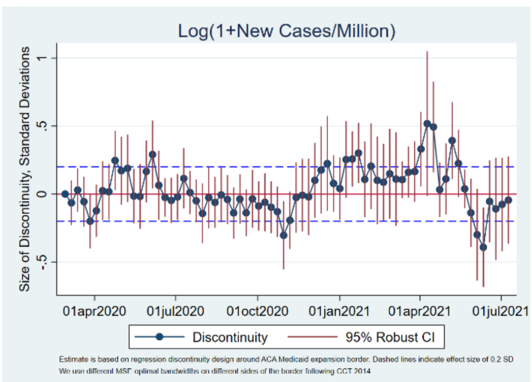

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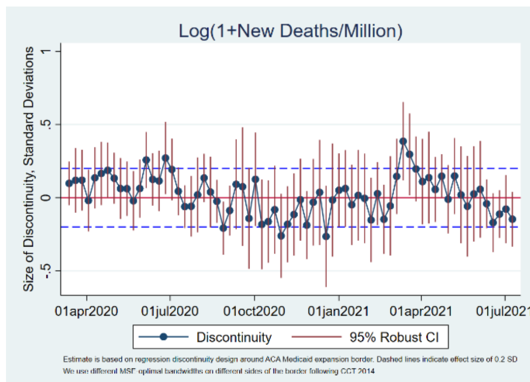

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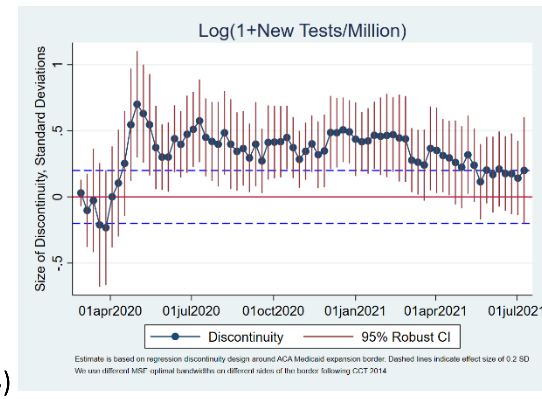

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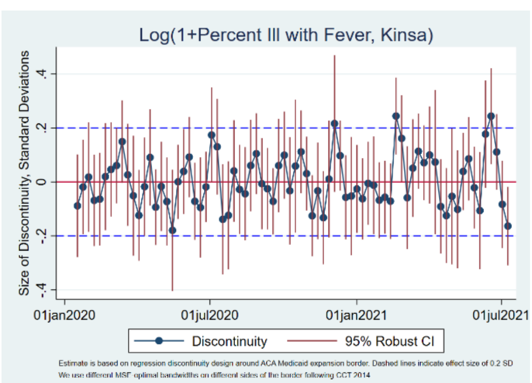

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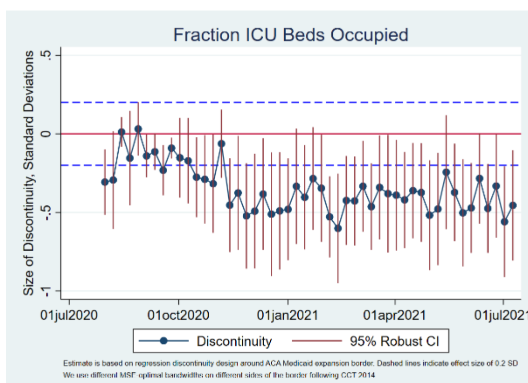

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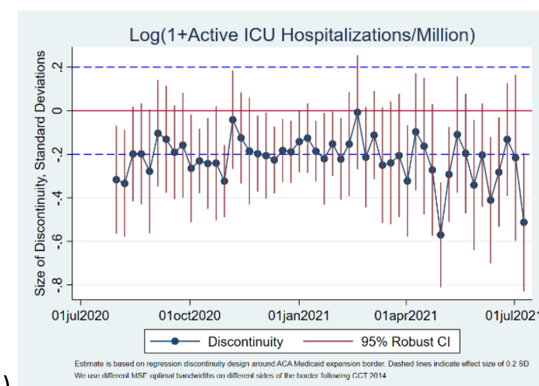


6)

8 Tables

Table I (I)

Placebo Regression Discontinuity Effects			
	Estimate	SE	Bandwidths
Baseline Characteristics: Placebo			
Log Median Household Income	-.006	(.098)	(.081, .099)
Log Population Density	.021	(.084)	(.085, .093)
Percent White (CDC), 2019	-.024	(.121)	(.088, .080)
Percent Black (CDC), 2019	.032	(.132)	(.091, .076)
Percent 80+ (CDC), 2019	.090	(.144)	(.077, .137)
In MSA	-.086	(.151)	(.133, .119)
Social Distancing(first 21 days), SafeGraph	.075	(.125)	(.117, .108)
Fraction Owner Occupied (ACS), 2015-19	.047	(.174)	(.098, .106)
Rooms Per Person (ACS), 2015-19	.013	(.175)	(.062, .123)
Public Transit (ACS), 2015-19	.034	(.029)	(.144, .067)
Unemployment Rate (LAUS), 2019	-.216	(.189)	(.090, .109)
Spending Decline During Pandemic (Affinity)	.101	(.229)	(.112, .146)
Trump Vote Share 2016 (Guardian)	-.043	(.141)	(.105, .070)
Trump Share 2016-Romney Share 2012 (Guardian)	-.106	(.132)	(.087, .097)
Baseline Characteristics: Health			
Death Rate from Heart Disease (CDC)	-.014	(.117)	(.077, .092)
Fraction Obese (CDC)	-.252**	(.120)	(.097, .111)
Hypertension Rate (CDC)	-.130	(.106)	(.064, .097)
Percent with Diabetes (CDC)	.014	(.121)	(.079, .123)
Death Rate from Cancer (CDC)	.166	(.158)	(.110, .162)
Respiratory Mortality (CDC)	-.163	(.099)	(.096, .079)
Smoking Rate (CDC)	-.044	(.098)	(.097, .079)
Age-Adjusted Mortality (CDC), Whole Population	-.032	(.111)	(.090, .100)
Age-Adjusted Mortality (CDC), Blacks aged 15-64	-.094	(.129)	(.073, .094)
Log Hospital Expenditures (HCRIS), 2019	.007	(.110)	(.121, .141)
ICU Beds (HCRIS), 2019	-.067	(.072)	(.109, .103)
Hospital Beds (HCRIS), 2019	-.111	(.109)	(.081, .086)
Insurance Discontinuity			
Fraction Uninsured 18-65 (SAHIE), 2019	-.783***	(.110)	(.072, .069)
Fraction Insured 19-64 (ACS), 2015-19	.614***	(.135)	(.092, .074)
Fraction with Medicaid 19-64 (ACS), 2015-19	.704***	(.137)	(.114, .099)
Fraction with other insurance 19-64 (ACS), 2015-19	.008	(.109)	(.121, .115)

Each cell shows the estimate of the coefficient β from equation (1) with standard errors clustered on state in parentheses. See section 2 for data sources.

Regression Discontinuity Effects on Main Variables				
	(1)	(2)	(3)	(4)
	Cumulative	First Wave 3/1-7/4	Second Wave 7/5-10/11	Third Wave 10/12-7/9/21
1. Log(1+New Cases/Million)	-.09 [-.029]	.12 [.209]	-.03 [-.032]	-.07 [-.023]
(N=3107)	(.11) (.16,.12)	(.13) (.10,.12)	(.11) (.13,.09)	(.10) (.11,.09)
2. Log(1+New Deaths/Million)	-.02 [-.024]	.13 [.353]	-.05 [-.130]	-.10 [-.108]
(N=3107)	(.17) (.09,.15)	(.09) (.09,.10)	(.17) (.12,.11)	(.14) (.10,.13)
3. Log(1+Percent Ill with Fever, Kinsa)	.11 [.049]	.05 [.036]	.03 [.017]	.10 [.051]
(N=3033)	(.08) (.11,.16)	(.09) (.11,.24)	(.08) (.12,.17)	(.07) (.13,.15)
4. Log(1+Number of Active Users, Kinsa)	-.00 [-.008]	-.01 [-.023]	-.01 [-.022]	-.00 [-.001]
(N=3033)	(.02) (.11,.16)	(.03) (.11,.16)	(.03) (.12,.17)	(.02) (.12,.15)
5. Log(1+New Tests/Million)	.38*** [.461]	.46*** [.544]	.29** [.477]	.38*** [.579]
(N=3107)	(.09) (.08,.09)	(.15) (.09,.06)	(.12) (.10,.08)	(.10) (.11,.12)
6. Doctor Visits Measure, CMU CovidCast	.24 [.482]	.20 [.415]	-.10 [-.292]	.24 [.650]
(N=2292)	(.17) (.08,.10)	(.16) (.07,.11)	(.15) (.08,.11)	(.15) (.15,.10)
7. Log(1+New Deaths/Million), Hosp. Sample	.02 [.016]	.22 [.543]	-.18 [-.381]	-.07 [-.053]
(N=2399)	(.16) (.09,.10)	(.15) (.14,.12)	(.17) (.17,.12)	(.13) (.15,.09)
8. Log(1+New Deaths/Million), ICU Sample	.20 [.120]	.28* [.613]	-.01 [-.030]	.00 [.003]
(N=1553)	(.19) (.08,.12)	(.17) (.10,.10)	(.18) (.11,.14)	(.16) (.11,.14)
	Cumulative	By 05/01/21	05/01-06/01	06/01-07/06
9. Log(0.1+Percent Vaccinated)	.11 [.191]	.08 [.139]	.16 [.202]	.15 [.176]
(N=3107)	(.12) (.06,.07)	(.13) (.06,.07)	(.12) (.07,.10)	(.15) (.09,.10)
	Cumulative	Pre-Peak 8/01-11/30	Peak 12/01-1/31	Post-Peak 2/1-7/6
10. Fraction Hospital Beds Occupied	.13 [3.468]	.19 [4.767]	.07 [1.863]	.06 [1.735]
(N=2399)	(.16) (.12,.19)	(.15) (.11,.19)	(.17) (.12,.19)	(.20) (.13,.18)
11. Log(1+Active Covid Hosp./Million)	.02 [.028]	-.07 [-.110]	.05 [.100]	-.09 [-.189]
(N=2399)	(.14) (.12,.14)	(.16) (.12,.11)	(.10) (.14,.16)	(.14) (.18,.12)
12. Fraction ICU Beds Occupied	-.51*** [-12.73]	-.41*** [-10.52]	-.51*** [-13.99]	-.50*** [-12.95]
(N=1553)	(.14) (.20,.10)	(.14) (.17,.11)	(.18) (.13,.11)	(.15) (.17,.10)
13. Log(1+Active Covid ICU Hosp./Million)	-.42** [-.682]	-.41** [-.719]	-.28* [-.553]	-.44** [-.771]
(N=1553)	(.20) (.10,.11)	(.21) (.11,.11)	(.17) (.15,.13)	(.17) (.18,.10)

Each cell shows the estimate of the standardized coefficient β from equation (1) with the unstandardized coefficient in brackets. Standard errors clustered on state are in parentheses on the second line, and bandwidths on both sides are in the second parentheses.

Table III

Robustness Summary

Baseline RD Includes Interacted 1-D Polynomial and Border Segment FE

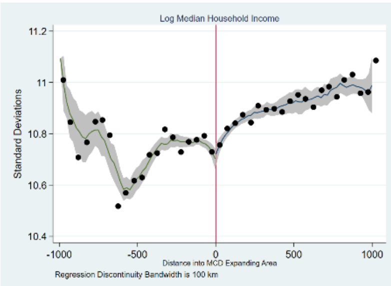
Full Sample, KFF MCD Expansion Definition as of Jan. 2020

Bandwidth is Two-Sided MSE Optimal. Conventional Estimates. Unweighted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number Placebo Rejected	Log OI Crude Covid Death Rate	Log Test Rate	Fraction ICU Beds Occupied	Log CDC AA Covid 15-64 Death Rate	Log CDC AA Covid 65+ Death Rate	Log CDC AA Covid Black Death Rate	Log CDC AA Covid Hispanic Death Rate	Log CDC AA Total Death Rate
1. Baseline	0	-.02 (.17)	.38*** (.09)	-.51*** (.14)	.04 (.12)	.10 (.11)	-.11 (.12)	-.03 (.10)	-.06 (.08)
2. Common Bandwidth	0	-.05 (.17)	.36*** (.10)	-.41*** (.14)	.10 (.10)	.01 (.12)	-.02 (.10)	-.02 (.10)	-.04 (.09)
3. CER Bandwidths	0	-.00 (.16)	.40*** (.09)	-.57*** (.16)	-.01 (.12)	.04 (.13)	-.11 (.09)	-.08 (.12)	-.02 (.09)
4. Bias Correction	0	-.02 (.19)	.38*** (.09)	-.56*** (.16)	.02 (.13)	.10 (.15)	-.09 (.14)	-.05 (.12)	-.06 (.09)
5. 2D 1 Order	0	-.03 (.17)	.40*** (.09)	-.31** (.14)	.04 (.11)	.06 (.12)	-.07 (.12)	-.14 (.09)	-.06 (.09)
6. 2D 2 Order	0	-.03 (.17)	.40*** (.09)	-.35** (.16)	.02 (.10)	.08 (.12)	-.13 (.10)	-.04 (.10)	-.06 (.07)
7. 2D 3 Order	0	-.17 (.22)	.42*** (.10)	-.48*** (.15)	.05 (.11)	.09 (.11)	-.14 (.10)	-.01 (.11)	-.07 (.10)
8. 2D 4 Order	0	-.18 (.23)	.36*** (.09)	-.49*** (.15)	.02 (.11)	.12 (.11)	-.12 (.10)	-.04 (.10)	-.04 (.09)
9. Median Regression	0	.04 (.08)	.31*** (.09)	-.40 (.27)	.03 (.16)	-.00 (.08)	.07 (.27)	.22 (.16)	-.13 (.12)
10. No Segment FE	0	-.10 (.24)	.43** (.17)	-.34 (.28)	-.01 (.25)	.05 (.18)	-.05 (.32)	-.05 (.14)	-.11 (.32)
11. Stringency Control	0	.00 (.16)	.33*** (.10)	-.35** (.17)	.00 (.11)	.16 (.10)	-.01 (.10)	-.07 (.10)	-.05 (.07)
12. State Policies Controls	0	.06 (.15)	.30*** (.10)	-.36** (.14)	-.00 (.12)	.21** (.10)	-.01 (.10)	-.08 (.10)	-.03 (.08)
13. NE Not Expands	0	.19* (.11)	.47*** (.09)	-.41*** (.14)	.08 (.09)	.10 (.12)	-.06 (.10)	.07 (.10)	.02 (.09)
14. WI Expands	1	.19 (.12)	.51*** (.11)	-.59*** (.11)	.10 (.09)	.13 (.13)	.05 (.09)	.07 (.09)	.00 (.09)
15. Population Weighted	0	-.03 (.13)	.39** (.18)	-1.01*** (.35)	-.37** (.18)	-.15 (.14)	-.00 (.17)	.13 (.28)	-.27** (.11)
16. Population Weighted, WI	1	.18 (.16)	.37 (.25)	-1.52*** (.34)	-.11 (.21)	-.08 (.07)	.22 (.15)	-.43 (.34)	-.08 (.13)
17. Hospital Sample	0	.02 (.16)	.43*** (.09)	-.49*** (.15)	.10 (.10)	.05 (.11)	-.05 (.11)	.06 (.10)	-.07 (.10)
18. ICU Sample	0	.20 (.19)	.26** (.11)	-.51*** (.14)	.25** (.12)	.06 (.12)	.10 (.18)	.27* (.16)	.00 (.13)
19. Southern Border	1	.22 (.21)	.71*** (.27)	-.43** (.19)	-.19 (.15)	.18 (.22)	-.13 (.12)	-.15 (.13)	-.04 (.18)
20. Western Borders	2	-.07 (.22)	.26*** (.09)	-.37** (.16)	.11 (.16)	-.22* (.12)	-.10 (.16)	-.04 (.15)	-.10 (.16)
21. Concordant Governors	1	.08 (.16)	.25*** (.08)	-.13 (.22)	-.11 (.16)	.21 (.16)	-.11 (.16)	-.15 (.15)	-.07 (.15)
22. Naive OLS	5	-.43*** (.11)	.33** (.15)	-.39*** (.12)	-.27 (.17)	-.37** (.15)	.18 (.20)	.09 (.17)	-.48*** (.15)

Each cell shows the estimate of the standardized coefficient β from equation (1) with standard errors clustered on state in parentheses. All robustness checks explained in Section 5. There are 14 variables in the placebo group, listed in Table I.

Appendix Figure I: Placebo Graphs



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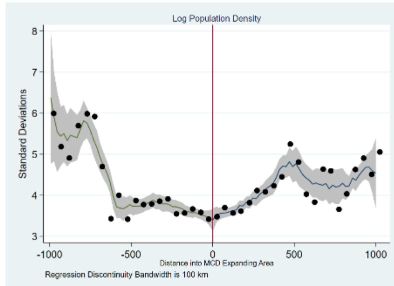

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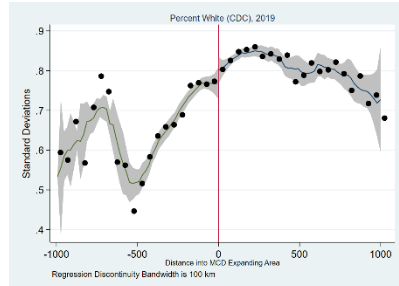

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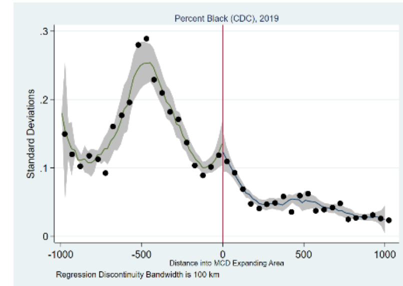

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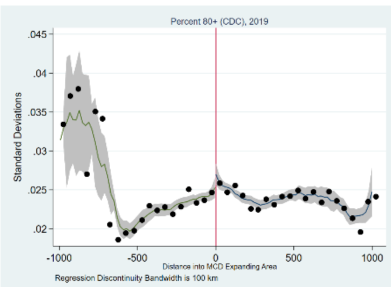

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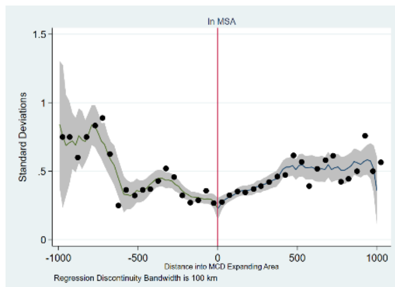

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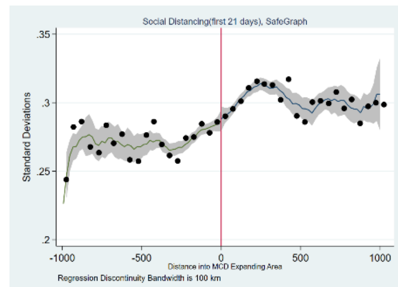

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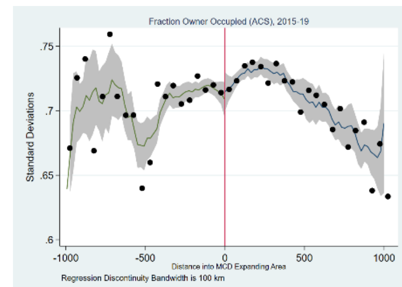

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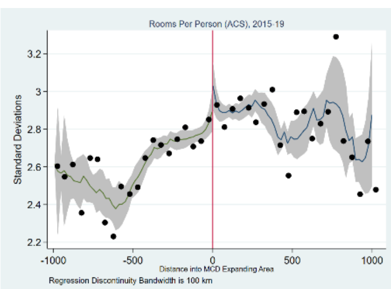

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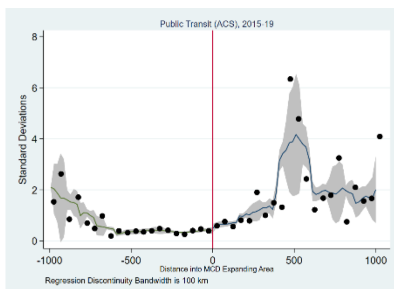

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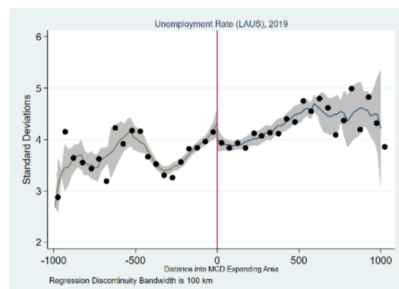

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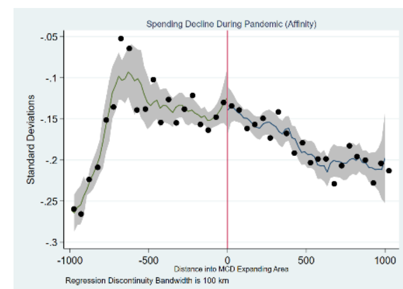

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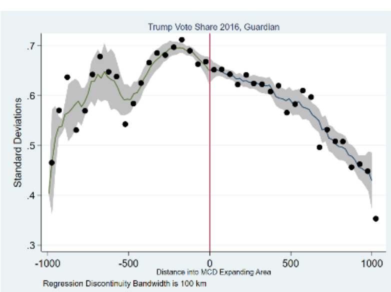

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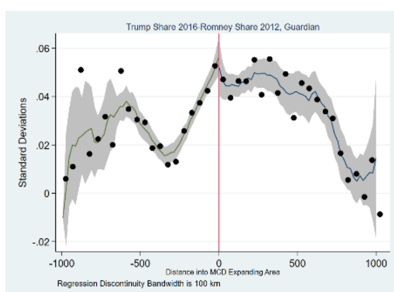

14.

Appendix Figure II: Health Characteristics Graphs

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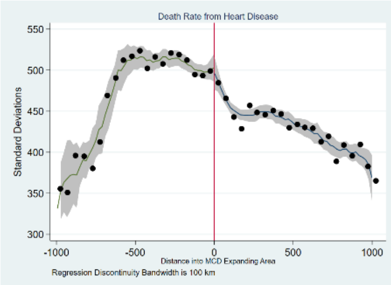

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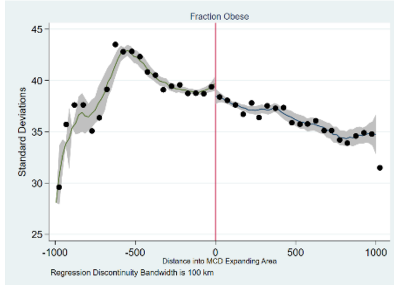

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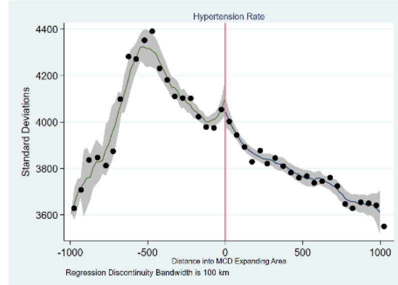

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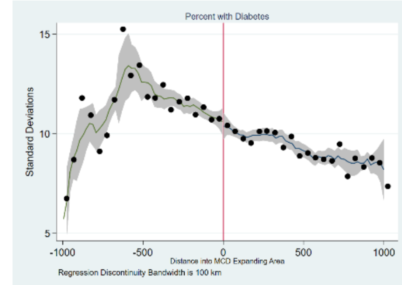

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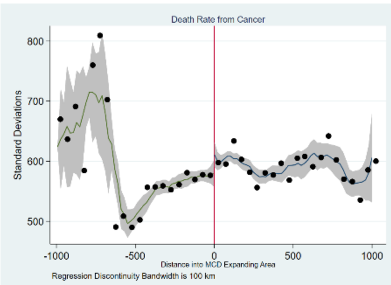

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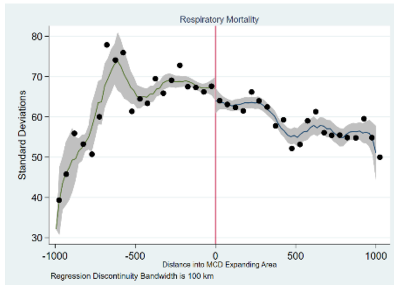

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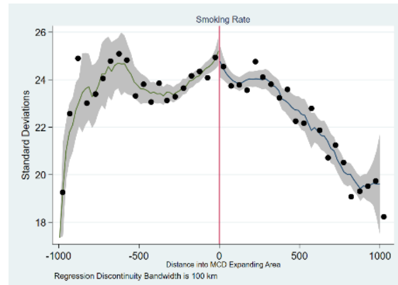

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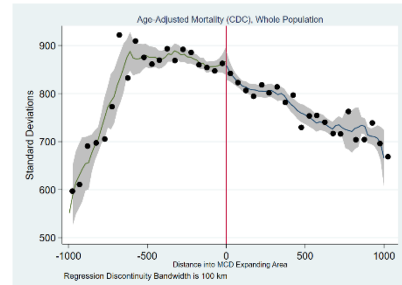

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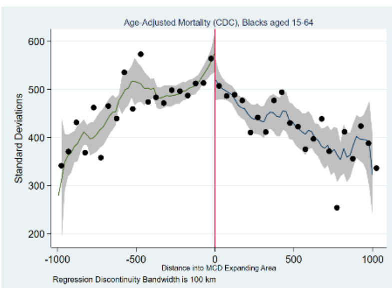

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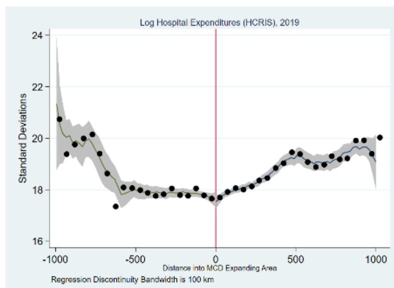

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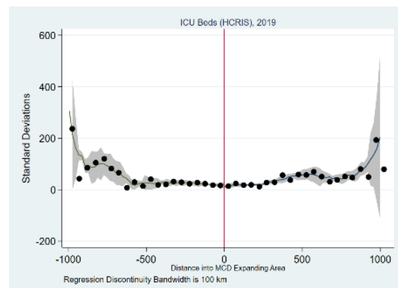
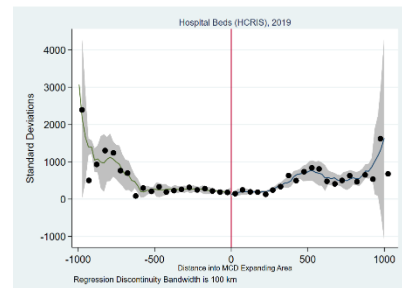
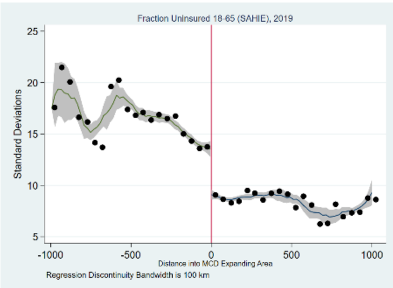
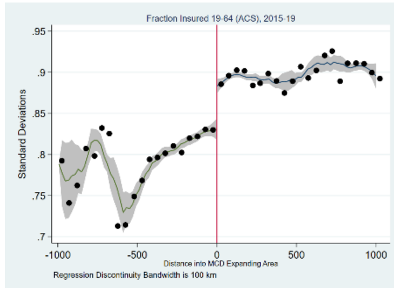


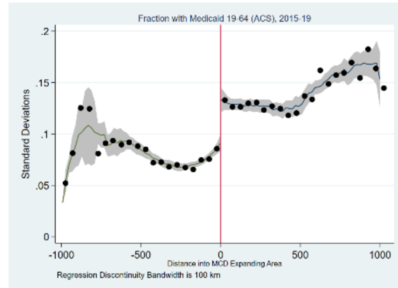
26.

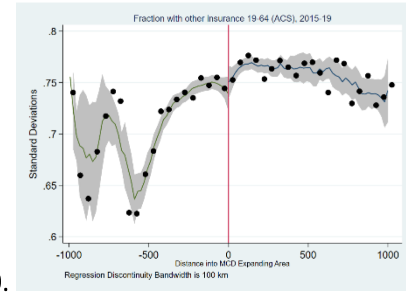


Appendix Figure III: Insurance Discontinuity

27. 

28. 

29. 

30. 

10 Appendix Tables

Table AI (AI)

Regression Discontinuity Effects on Mobility				
	(1)	(2)	(3)	(4)
	Cumulative	First Wave 3/1-7/4	Second Wave 7/5-10/11	Third Wave 10/12-7/9/21
1. Google Mobility Retail+Recrn (N=1662)	-.36*** [-.040] (.12) (.11,.17)	-.19 [-.024] (.12) (.12,.12)	-.43*** [-.066] (.11) (.14,.13)	-.20 [-.024] (.14) (.11,.16)
2. Google Mobility Grocery+Pharm (N=1511)	-.23 [-.021] (.15) (.16,.21)	-.11 [-.011] (.13) (.12,.17)	-.28* [-.035] (.15) (.16,.13)	-.10 [-.009] (.22) (.15,.20)
3. Google Mobility Parks (N=754)	.32 [.128] (.20) (.11,.11)	.26 [.099] (.22) (.23,.12)	.33** [.217] (.15) (.11,.14)	.38* [.127] (.22) (.16,.10)
4. Google Mobility Transit (N=1025)	-.31* [-.069] (.17) (.13,.16)	-.33** [-.061] (.15) (.14,.16)	-.23 [-.057] (.17) (.12,.19)	-.40** [-.099] (.19) (.17,.14)
5. Google Mobility Workplace (N=2139)	-.09 [-.005] (.15) (.10,.11)	.05 [.004] (.15) (.12,.16)	-.18 [-.012] (.12) (.09,.10)	-.16 [-.010] (.14) (.10,.10)
6. Google Mobility Res. (N=1719)	.20 [.005] (.14) (.07,.11)	.04 [.001] (.11) (.09,.11)	.20 [.005] (.18) (.07,.12)	.32* [.007] (.17) (.09,.14)
7. Google Mobility Away From Home (N=1719)	-.28 [-.008] (.17) (.06,.11)	.07 [.003] (.15) (.08,.10)	-.20 [-.006] (.16) (.07,.12)	-.33* [-.009] (.17) (.08,.14)

Each cell shows the estimate of the standardized coefficient β from equation (1) with the unstandardized coefficient in brackets. Standard errors clustered on state are in parentheses on the second line, and bandwidths on both sides are in the second parentheses.

Table 3 (AII)

CDC Mortality and Robustness: Log Covid Deaths per 100,000						
Baseline RD Includes Interacted 1-D Polynomial and Border Segment FE						
Full Sample, KFF MCD Expansion Definition as of Jan. 2020						
Bandwidth is Two-Sided MSE Optimal. Conventional Estimates. Unweighted						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	15-64	65+	White Non Hispanic	Black	Hispanic
1. Baseline	-.05 (.12)	.04 (.12)	.10 (.11)	.00 (.11)	-.11 (.12)	-.03 (.10)
2. Common Bandwidth	-.01 (.12)	.10 (.10)	.01 (.12)	.04 (.11)	-.02 (.10)	-.02 (.10)
3. CER Bandwidths	-.06 (.14)	-.01 (.12)	.04 (.13)	-.04 (.13)	-.11 (.09)	-.08 (.12)
4. Bias Correction	-.08 (.15)	.02 (.13)	.10 (.15)	-.00 (.14)	-.09 (.14)	-.05 (.12)
5. 2D 1 Order	-.04 (.11)	.04 (.11)	.06 (.12)	.02 (.10)	-.07 (.12)	-.14 (.09)
6. 2D 2 Order	-.03 (.12)	.02 (.10)	.08 (.12)	.03 (.11)	-.13 (.10)	-.04 (.10)
7. 2D 3 Order	-.06 (.10)	.05 (.11)	.09 (.11)	-.01 (.09)	-.14 (.10)	-.01 (.11)
8. 2D 4 Order	.00 (.12)	.02 (.11)	.12 (.11)	.02 (.11)	-.12 (.10)	-.04 (.10)
9. Median Regression	-.05 (.13)	.03 (.11)	-.00 (.10)	-.02 (.11)	.07 (.36)	.22 (.15)
10. No Segment FE	-.04 (.21)	-.01 (.25)	.05 (.18)	-.08 (.19)	-.05 (.32)	-.05 (.14)
11. Stringency Control	-.02 (.11)	.00 (.11)	.16 (.10)	-.01 (.11)	-.01 (.10)	-.07 (.10)
12. State Policies Controls	-.00 (.10)	-.00 (.12)	.21** (.10)	.11 (.10)	-.01 (.10)	-.08 (.10)
13. NE Not Expands	-.04 (.09)	.08 (.09)	.10 (.12)	.00 (.10)	-.06 (.10)	.07 (.10)
14. WI Expands	.00 (.11)	.10 (.09)	.13 (.13)	.04 (.12)	.05 (.09)	.07 (.09)
15. Population Weighted	-.24 (.17)	-.37** (.18)	-.15 (.14)	-.16 (.11)	-.00 (.17)	.13 (.28)
16. Population Weighted, WI	-.13 (.11)	-.11 (.21)	-.08 (.07)	-.09 (.14)	.22 (.15)	-.43 (.34)
17. Hospital Sample	-.00 (.11)	.10 (.10)	.05 (.11)	.01 (.13)	-.05 (.11)	.06 (.10)
18. ICU Sample	.16 (.11)	.25** (.12)	.06 (.12)	.28** (.14)	.10 (.18)	.27* (.16)
19. Southern Border	-.10 (.18)	-.19 (.15)	.18 (.22)	.03 (.14)	-.13 (.12)	-.15 (.13)
20. Western Borders	-.15 (.13)	.11 (.16)	-.22* (.12)	-.16 (.19)	-.10 (.16)	-.04 (.15)
21. Concordant Governors	.01 (.15)	-.11 (.16)	.21 (.16)	.08 (.14)	-.11 (.16)	-.15 (.15)
22. Naive OLS	-.43** (.17)	-.27 (.17)	-.37** (.15)	-.39** (.17)	.18 (.20)	.09 (.17)

(3)

Each cell shows the estimate of the standardized coefficient β from equation (1) with standard errors clustered on state in parentheses.

Table 4 (AIII)

CDC Mortality and Robustness: Log Total Deaths per 100,000 *Baseline RD Includes Interacted 1-D Polynomial and Border Segment FE* *Full Sample, KFF MCD Expansion Definition as of Jan. 2020* *Bandwidth is Two-Sided MSE Optimal. Conventional Estimates. Unweighted*						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	15-64	65+	White Non Hispanic	Black	Hispanic
1. Baseline	-.06 (.08)	-.07 (.09)	-.08 (.10)	-.01 (.09)	-.03 (.15)	-.05 (.09)
2. Common Bandwidth	-.04 (.09)	-.03 (.10)	-.07 (.11)	-.03 (.07)	-.03 (.17)	.14 (.12)
3. CER Bandwidths	-.02 (.09)	-.07 (.10)	-.13 (.12)	-.04 (.09)	-.11 (.14)	.01 (.11)
4. Bias Correction	-.06 (.09)	-.10 (.10)	-.08 (.12)	-.00 (.10)	-.05 (.17)	-.09 (.11)
5. 2D 1 Order	-.06 (.09)	-.06 (.10)	-.07 (.11)	-.04 (.09)	-.04 (.15)	-.11 (.10)
6. 2D 2 Order	-.06 (.07)	-.11 (.09)	-.03 (.08)	-.01 (.07)	-.08 (.14)	-.07 (.09)
7. 2D 3 Order	-.07 (.10)	-.06 (.09)	-.06 (.10)	-.02 (.09)	-.18* (.10)	-.05 (.09)
8. 2D 4 Order	-.04 (.09)	-.05 (.09)	-.09 (.12)	-.01 (.09)	-.06 (.12)	.08 (.10)
9. Median Regression	-.13 (.15)	-.04 (.14)	-.12 (.09)	-.04 (.14)	.02 (.06)	.10 (.16)
10. No Segment FE	-.11 (.32)	-.11 (.31)	-.12 (.23)	-.07 (.31)	-.06 (.27)	-.00 (.20)
11. Stringency Control	-.05 (.07)	-.07 (.10)	-.04 (.10)	.00 (.08)	-.05 (.16)	-.03 (.09)
12. State Policies Controls	-.03 (.08)	-.09 (.09)	.00 (.08)	.02 (.09)	-.05 (.16)	-.02 (.09)
13. NE Not Expands	.02 (.09)	-.06 (.09)	.02 (.11)	.06 (.10)	-.05 (.13)	.09 (.11)
14. WI Expands	.00 (.09)	-.13 (.12)	.05 (.10)	.08 (.11)	.06 (.11)	.18* (.09)
15. Population Weighted	-.27** (.11)	-.32* (.18)	-.20* (.12)	-.31 (.20)	-.41*** (.10)	.42 (.33)
16. Population Weighted, WI	-.08 (.13)	-.16 (.13)	-.17 (.15)	-.21 (.21)	-.33*** (.09)	.37 (.27)
17. Hospital Sample	-.07 (.10)	.02 (.10)	-.02 (.10)	-.01 (.09)	.05 (.16)	.19 (.13)
18. ICU Sample	.00 (.13)	.06 (.11)	-.09 (.16)	-.02 (.13)	.06 (.18)	.43** (.18)
19. Southern Border	-.04 (.18)	-.10 (.15)	.11 (.18)	-.10 (.14)	-.11* (.07)	.35** (.15)
20. Western Borders	-.10 (.16)	-.41** (.20)	-.11 (.17)	.00 (.21)	-.08 (.18)	.02 (.18)
21. Concordant Governors	-.07 (.15)	-.27* (.16)	.03 (.16)	.01 (.14)	-.12 (.19)	-.00 (.17)
22. Naive OLS	-.48*** (.15)	-.44*** (.14)	-.40*** (.13)	-.45*** (.15)	-.04 (.15)	.08 (.14)

(4)

Each cell shows the estimate of the standardized coefficient β from equation (1) with standard errors clustered on state in parentheses.