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Economic Essays On Health and Location

Joseph Garuccio
jgaruccio1@student.gsu.edu

ABSTRACT

ECONOMIC ESSAYS ON HEALTH AND LOCATION

By

JOSEPH ANTHONY GARUCCIO

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Committee Chair: Dr. Charles Courtemanche

Major Department: Economics

This dissertation is composed of three chapters that focus on the effect of one's environment on one's health and healthcare decisions. Specifically, this work focuses on how various policies and physical environments affect one's potential access to care, one's probability of acquiring preventive care, and the spread of the novel coronavirus 2019 (COVID-19). In my first chapter, I examine if Medicaid expansions induced new physicians to locate closer to poor populations. I use precise physician location data and American Community Survey data at the census block group level to identify the extent to which the expansions induced new physicians to locate closer to poor populations. A goal of the Affordable Care Act and Medicaid expansions was to increase healthcare access for low-income adults. I show that new physicians in expansion states located increasingly closer to poor populations after expansion, arguably increasing their healthcare access.

In my second chapter, I estimate the effect increases in urban sprawl in metropolitan statistical areas (MSA) have on the probability individuals acquire timely preventive care. I make use of Behavioral Risk Factor Surveillance System data, an index of urban sprawl at the MSA level, and the 1947 Interstate Highway Construction Plan to estimate the effect of increased sprawl.

In an instrumental variable design, I find that a standard deviation increase in sprawl lowers the probability that individuals have various important cancer screenings and are more likely to be obese. Such an increase also increases the probability of individuals obtaining flu shots.

In my third chapter, my coauthors and I estimate the effect social distancing policies had on reducing the growth rate of COVID-19. We make use of daily, county-level confirmed case and intervention data from Johns Hopkins University as well as state-level testing data to estimate the effect of four key social distancing policies. We make use of an event-study design to separately estimate the effect of shelter-in-place orders (SIPOs), bans on large gatherings, public school closures, and restaurant and entertainment venue closures. We find that SIPOs and the closure of restaurant and entertainment venues significantly reduced the growth rate. We found no significant evidence that gathering bans nor school closures had a mitigating effect.

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ACCEPTANCE PAGE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

Dissertation Chair: Dr. Charles Courtemanche

Committee: Dr. Carlianne Patrick
Dr. Glenn Landers
Dr. James Marton

Electronic Version Approved:

Sally Wallace, Dean
Andrew Young School of Policy Studies
Georgia State University
May 2021

DEDICATION

This work is dedicated to the men and women who graciously invested in my growth and development as a person and professional. I am eternally grateful.

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I suspect my parents would rather that I have produced grandchildren than a doctorate; nevertheless, I hope this accomplishment can bring them pride in some small way. More seriously, my memory is insufficient to provide the names of all those worthy of acknowledgment and gratitude for their efforts in support of me. I am grateful beyond words for the level of kindness and commitment I have been shown and of which I may never feel worthy.

To my parents, Richard and Beverly, and siblings, Shannon, Mark, Leslie, Rebecca, and Ricky, this accomplishment could not have been possible without your support and belief in me. I hope this serves in some small way to bring me closer to the son and brother you deserve.

To my committee, your kindness and commitment to me have meant more than I will ever be able to tell you. Without your support, I do not know that I could have completed this journey nor start the next. I hope I will provide a former mentee and new colleague worthy of pride as I move to the next step in my career.

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INTRODUCTION

This body of work is comprised of three independent economic essays related to the effect one's environment has on one's health and healthcare decisions. The goal of this dissertation is to provide insight into the effects one's physical and policy environment can have on one's health. I aim to shed light on how state and local policy can influence one's health and healthcare decisions as well as the effect urban environments can have on healthcare decisions. Specifically, I examine the impacts of Medicaid expansions on new physician locations, urban sprawl on preventive care use, and social distancing policies on the COVID-19 growth rate.

In my first chapter, I estimate the extent to which Medicaid expansions under the Affordable Care Act (ACA) led new physicians to locate closer to poor populations. Gains in access to healthcare for low-income adults were a goal of the ACA. I approach access from a spatial perspective and estimate the change in the proportion of poor individuals per 1,000 residents near new physician locations. The argument is, if new physicians locate closer to poor populations, then those populations experience an increase in healthcare access. I make use of precise physician location data and population data from the American Community Survey to examine the changes in populations that new physicians locate near due to Medicaid expansions. I find that within expansion states, new physicians located increasingly closer to poor populations from 2014 to 2016. I do not find, however, any evidence that new physicians elected to enter expansion states over non-expansion states and thereby reduce access in non-expansion states. This suggests an increase in access for poor populations in expansion states without an accompanying reduction for similar populations in non-expansion states.

In my second chapter, I estimate the effect increases in metropolitan statistical area (MSA) urban sprawl has on preventive care use. Acquiring preventive care services can become more inconvenient and costly as urban areas become more spread out, difficult to traverse, segregated along residential and commercial lines, and have their economic activity more dispersed. To examine this, I use preventive care use data from the 2012 Behavioral Risk Factor Surveillance System (BRFSS) and a 2010 index of MSA level urban sprawl. I use the 1947 Highway Construction Plan to instrument for levels of sprawl and estimate the effect of sprawl on the probability of timely preventive care acquisition. I find that a one standard deviation increase in sprawl tends to reduce the probability of individuals acquiring important preventive care, particularly among cancer screening services. My results also suggest that greater sprawl increases the probability of acquiring a flu shot and being obese.

In my third chapter, my coauthors and I estimate the effect social distancing policies had on the novel coronavirus 2019 (COVID-19) growth rate from March 1, 2020, to April 27, 2020. States and localities imposed various social distancing measures within this time frame to combat the spread of COVID-19 in the early stages of the pandemic. We used an event-study design to examine the reducing effects of shelter-in-place orders (SIPOs), bans on large gatherings, public school closures, and closures of restaurants and entertainment venues separately. We found significant evidence that SIPOs and closures of restaurants and entertainment venues reduced the COVID-19 growth rate. We found no significant evidence that either bans on large gatherings or public-school closures affected the COVID-19 growth rate. Our results suggest the more imposing social distancing measures had clear reducing effects. Alternatively, the results for gathering bans and school closures were imprecise though possibly meaningful effects could not be ruled out.

CHAPTER I: MEDICAID EXPANSIONS AND NEW PHYSICIAN LOCATIONS

1.1 INTRODUCTION

Among the primary goals of the Patient Protection and Affordable Care Act (ACA) was to create near-universal health insurance coverage in the United States (Gruber, 2011). The pursuit of this goal involved a combination of mandates, public insurance expansions, and subsidies for the purchase of private insurance. These various components were to work toward extending coverage among underserved and largely uninsured populations in the U.S. The various policies, implemented mostly in 2014, sought to improve “accessibility, affordability, and quality of care,” particularly among the very sick as well as uninsured, low-income adults (Obama, 2016).

Historically, gaining health insurance has been perceived as gaining increased access to healthcare. This may come through a combination of facing a reduced price for healthcare services due to being insured and being perceived as a reliable payer by health care providers. All insurance types, however, may not be considered equally appealing by physicians as compensation rates vary, sometimes substantially, across insurance types (Berman et al., 2002, Zuckerman et al., 2012; 2014; 2017). Such differences have been thought to historically limit access to health care for Medicaid enrollees, especially to physicians with established practices and patient rosters.

Physicians, even if willing to see new Medicaid patients, may only be able to accept a limited number or provide them services at the cost of providing services to other patient types. This would mean a tradeoff of access between patients of different insurance types. Additionally, the location of physicians affects access. The further away a doctor is, the more difficult she is to see. If established physicians are time-constrained or inconvenient to reach, then it is important to understand how newly entering physicians respond to changes in public insurance coverage. If physicians are unable or unwilling to make themselves more available to newly insured Medicaid enrollees, then the enrollees' access to care may be far less than one would hope.

There is relatively little causal research examining the supply-side response to the ACA and its effect on access. This paper's primary contribution is to utilize rich, national data on physicians that are particularly well suited to studying this issue from a geographic perspective. I use difference-in-differences and event-study models to estimate the effect of Medicaid expansions on physician entrants per 100,000 state population and on the population under the federal poverty line (FPL) per 1,000 people near new physician locations. I use these two outcomes as spatial measures of changes in access. Physicians being drawn to expansion states could indicate either a desire to capture the pool of new customers or a hiring response from established practices facing increased demand for their services. It would also suggest a potential loss of access if those physicians would have served similar populations in non-expansion states. If new physicians are willing to locate closer to lower-income populations post-expansion within states, then those populations have arguably greater healthcare access.

The advantages of the data I use, which come from the National Plan and Provider Enumeration System (NPPES), are the ability to precisely locate physicians and to focus exclusively on gross entry. The latter advantage is an improvement over the use of public data which provides net counts of physicians that combines new entrants with recent exits and makes disentangling policy effects on either type difficult. I focus on the location decisions of post-residency (i.e. new) physicians across and within state lines.

In general, I do not find evidence that Medicaid expansions impacted new entrants per 100,000 state population among newly entering physicians. Rather, I find that doctors choose to locate closer to low-income populations within expansion states. Pre-treatment coefficient estimates from event study regressions generally support a causal interpretation of the results. Taken together, these results suggest an increase in access for low-income adult populations in expansion states that did not come at the expense of non-expansion states.

1.2 BACKGROUND

ACA Medicaid Expansion

With the implementation of the ACA, there were significant gains in health insurance for the previously uninsured (Courtemanche et al., 2017; Frean et al., 2017; Courtemanche et al., 2018a; Courtemanche et al., 2018b) and expansions of public health insurance led to increases in health care demand and utilization (Baicker et al., 2013; Ghosh et al., 2017; Finkelstein et al., 2012; Kolstad and Kowalski, 2012; Miller, 2012a; Miller, 2012b; Simon et al., 2017; Wherry and Miller, 2016). Simon et al. (2017) found increases in the probability that poor adults had a personal physician due to Medicaid expansions. Ghosh et al.'s (2017) findings suggest greater prescription drug access for chronic conditions among new Medicaid enrollees. These findings as well as others point toward greater access to healthcare for the newly insured (Rhodes et al., 2017; Mazurenko et al., 2017; Antonisse et al., 2018). The American Medical Association (AMA) reported statistically significant increases in Medicaid patients as a share of average physicians' patient mix in expansion states in its Physician Practice Benchmark Surveys (Unlisted Staff Writer, 2017). Neprash et al. (2018), however, found little to no increase in physician Medicaid participation due to the Medicaid expansions and that Medicaid patients remained concentrated among relatively few physicians. Additionally, mixed positive and null findings of the expansions' effect on preventive care usage suggest some limitations on access gains (Finkelstein et al., 2012; Courtemanche et al. 2018b).

Other research and a survey of Michigan doctors by the University of Michigan suggests that the ACA insurance expansions led to longer initial wait times (Benitez et al., 2019) and less time spent by physicians with patients (Garthwaite, 2012; Slowery et al., 2018). Rhodes et al. (2017), however, did find wait time for appointments for the privately insured to be stable across 10 states in mid-2014 despite increased Medicaid enrollment. This suggests that established physicians were not at their capacity constraints at this time, the tradeoff was being made with Medicare patients, or that the tradeoff was in time spent with patients. Tipirneni et al.'s (2019) post-Medicaid expansion survey of Michigan primary care providers (PCPs), however, did list capacity as the most commonly reported factor influencing the acceptance of new Medicaid patients. Those PCPs accepting new Medicaid patients tended to be female, minorities, nonphysician providers, specialized in internal medicine, paid by salary, or working in practices with Medicaid-predominant payer mixes (Tipirneni et al., 2019). Broadly, the evidence suggests that there were tradeoffs in access made by time-constrained physicians. Such constraints and the lack of a substantial change in Medicaid participation by practicing physicians means that the decisions of newly entering physicians, who are less location-constrained than established physicians, could be vital to ensuring access for newly insured populations.

On June 28, 2012, the United States Supreme Court ruled that the expansion of Medicaid programs was at the discretion of the states (KFF, 2012). This introduced the potential for significant variation in state expansion decisions. Twenty-six states and the District of Columbia expanded Medicaid in 2014. However, the ACA allowed states to expand Medicaid before and after 2014 and multiple states did so to some degree (Courtemanche et al., 2017). As noted earlier, the expansions of public insurance brought significant gains in insurance coverage. According to the Kaiser Family Foundation, before the implementation of the ACA, Medicaid and the

Children's Health Insurance Program (CHIP) had just over 56.8 million enrollees across the United States, and by the end of 2016, this number had swelled to just under 75 million (KFF, 2020).

The ACA not only generated a large increase in new enrollment via state Medicaid expansions but also tried to incentivize physicians to be more willing to accept Medicaid enrollees. The federal government fully sponsored a notable increase in Medicaid compensation for 146 primary care services (Maclean et al., 2018). Physicians who specialized in primary care or for whom these services constituted a certain majority percentage of the services they provided qualified for the increased compensation. This "fee bump" was a temporary, nationwide measure lasting for the years 2013 and 2014. The federal government ultimately did not elect to continue paying for this fee increase, and funding for it ended after December 2014 with an estimated cost between 7 to 12 billion dollars (Medicaid and CHIP Payment and Access Commission, 2015). As of July 2016, 19 states had decided to continue funding the fee increase fully or partially or extend it to other specialties beyond primary care (Zuckerman et al., 2017).

Some evidence suggests that the fee bump increased access to healthcare (Polsky et al., 2015; MACPAC, 2015; Rhodes et al., 2017; Alexander and Schnell, 2017), though other research found little change in physicians' acceptance of Medicaid (Decker, 2016; 2018). This picture is one of increased access for Medicaid enrollees among physicians who already participated in Medicaid (Neprash, 2018; Tipirneni 2019), with the primary care fee bump providing little incentive for additional participation (Decker, 2016; 2018). This could lead to participating providers hiring more physicians to address the additional demand which would be faced disproportionately by them. Additionally, while established physicians may not be willing to accept the costs of participating in Medicaid (Timbie et al., 2017), newly entering physicians may be drawn to Medicaid as an arena in which there is less established provider competition.

Zukerman and Goin (2012) show Medicaid-Medicare compensation ratios for various medical services. A large majority of Medicaid programs compensate physicians less than Medicare for their services and compensate primary care and other specialty services at different relative rates. Excluding Tennessee, 34 of 49 states' fee-for-service Medicaid programs compensated other, non-obstetric services relatively higher than primary care. Among the other states, 12 compensated primary care services relatively higher than other services, and four compensated them at an equal ratio. These ratios do not reveal what specialties are paid more; however, they show how close compensation for one insurance type is to another for various services. All else equal, this shows if the two insurance types are close or remote substitutes in compensation.

Similar data from 2016 showed that between 26 and 30 states fee-for-service programs compensated other, non-obstetric services relatively higher (KFF 2016). The range arises from certain states electing to continue the primary care fee bump in some fashion. These differences suggest that primary care physicians may find new Medicaid enrollees less appealing than other specialists during the sample period from 2011 to 2016. Primary care physicians may also face greater competition from non-physician providers such as nurse practitioners and physician assistants who can provide many of the same basic services (Van Vleet and Paradise 2015). To explore potential response differences, I perform a subsample analysis for primary care physicians and other specialists.

Existing Literature

There is a host of literature examining the location decisions of physicians. The broad finding in this literature is that physician supply tends to respond to policy changes. Research focusing on physician responses to tort reform, such as the capping of non-economic damages for malpractice, finds that the implementation of these caps increases physician supply, particularly in specialties most at risk of malpractice claims (Kessler, 2005; Klick and Stratmann, 2007; Matsa, 2007; Chou and Lo Sasso, 2009; Malani and Reif, 2015; Pesko et al., 2017; Chatterji et al., 2018). However, there are dissenting opinions that find no effect of malpractice reform on supply (Paik et al., 2016; Hyman et al., 2015). Some research suggests that the riskiest physicians in states may sort undesirably into neighboring reform states (Leiber, 2014).

Research focusing on physicians' urban-rural decisions finds that student loan forgiveness programs increase the supply of physicians in rural counties (Kulka and McWeeny, 2017; Falcetone, 2017). Within this literature, however, is evidence that physicians are somewhat resistant to moving across state lines. Falcetone (2017) found that physicians prefer to locate near their place of residency and relays the fact that 54 percent of physicians remain in their state of residency for their first job. Taken together these literatures motivate my investigation of the effect of the ACA Medicaid expansion on physician location decisions. On one hand, physicians seem to be responsive to policy changes when it comes to location decisions. On the other hand, the seeming distaste of physicians for Medicaid implies that incentives for relocating with respect to this specific policy may not be particularly strong. Additionally, since physicians have a preference for remaining within their state of residency, it is important to examine within-state location decisions, not just cross-state decisions.

The Pathway to Becoming a Physician

The first step for future physicians after medical school is their residency training. Residency lengths vary among specialties and can be as short as 3 years or as long as 7 years. If a physician wants to sub-specialize, then they will need to apply for and accept sub-specialty training in what is called a fellowship. Most fellowships are an additional 1 to 2 years, however, some may be 3 or 4 more years.[1]

To practice medicine independently, physicians in the U.S. must acquire a medical license for their specialty in the state in which they practice. While medical licensure for physicians occurs at the state level, there is a required national exam component. The other requirements can vary but all states require applicants to have some amount of post-graduate training (residency), pass their national exams, provide information about malpractice suits, and pay a fee to the state for initial licensure and license renewal (Kocher, 2014). When a physician must acquire a license varies. California requires licensure during residency; however, other states have not codified such a requirement. In Georgia, at least some residents are given a grace period at the end of residency to pursue licensure (Albano 2020). Following licensure, physicians pursue board certification. Physicians cannot become board certified before completing residency. Residents typically search for their first post-residency job during their final year of residency and most will start in their new position mere weeks after completing their residency training (Darves, 2014). As residency years typically end in June, this suggests there are few opportunities to adapt location decisions in the six months leading up to the bulk of Medicaid expansions which occur in January.

1.3 DATA

Sources and Outcome Construction

I ask two questions in my analysis. The first is, did the Medicaid expansions change the level of new physician entry in expansion states relative to non-expansion states? The second is, did the Medicaid expansions induce new physicians to locate er to poor populations? To address

[1] See: https://education.uwmedicine.org/pages/specialties-subspecialties/

the first question, I construct a count of new physicians per 100,000 state population from a sample of new physicians. This sample was extracted from the monthly publications of the National Plan and Provider Enumeration System (NPPES), which contains the near universe of physicians, from April 2011 to December 2016.[2]

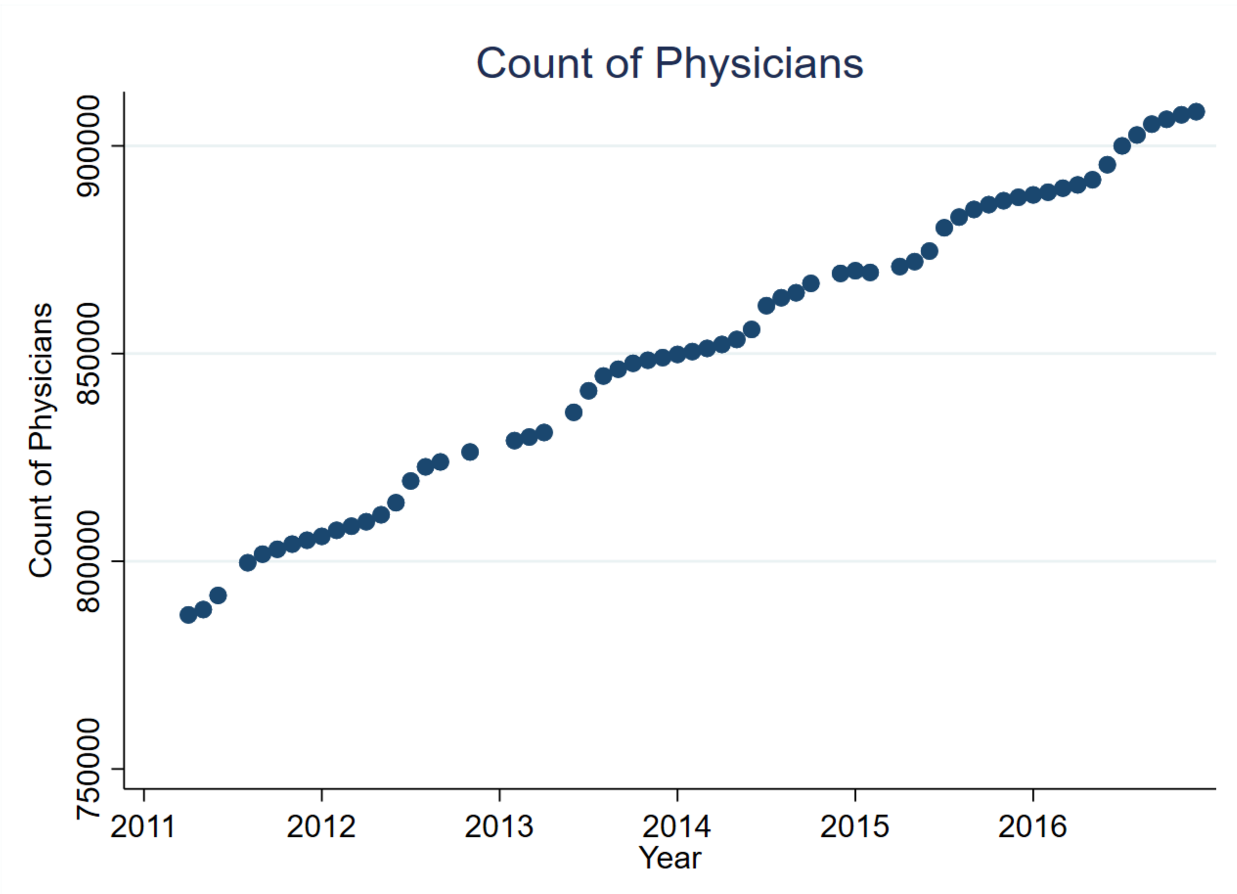
To bill insurance and transmit health information protected under the Health Insurance Portability and Accountability Act (HIPAA), physicians in the US were required to obtain a unique numeric identifier known as a national provider identifier (NPI) by May 23, 2008. Registration has no monetary cost and is compulsory for insurance reimbursement. Therefore, the NPI registry contains the near universe of licensed physicians and other entities that directly bill insurance or transmit protected data. Individuals and organizations have separate NPIs that allow for unique identification. In 2013, the CMS began requiring the use of an NPI when writing prescriptions, making it even more difficult for a physician to avoid having one.

Each month of NPPES data contains physicians' unique identifier, their primary practice location at the street level, and their current, precise specialty (taxonomy code). This data does not contain demographic nor other individual information outside of sex and sole proprietor status. By CMS guidelines, resident physicians are only to change their taxonomy code from that of a student trainee to that of a physician after they are licensed. Therefore, those with physician taxonomy codes in the NPPES represent the near universe of licensed physicians. I observe the point of licensure for those who make this change during my sample period; however, the completion of residency is not provided. Figure 1. shows the national level count of licensed allopathic and osteopathic physicians observed in this data. The December 2016 count of these physicians in

[2] January to March of 2011 was not available from the data source and May 2013 was also missing.

Figure 1 is just over 908,000. For contrast, Young et al. (2017) counted 953,695 allopathic and osteopathic physicians by the end of 2016 using data from the Federation of State Medical Boards. My count makes up over 95% of the physician count found by Young et al. (2017). The doctors used in my analysis also include podiatrists and optometrists. Their inclusion brings the count of doctors to just over 978,000 by the end of 2016.

FIGURE 1: COUNT OF ALLOPATHIC AND OSTEOPATHIC PHYSICIANS IN NPPES



Note: May 2013 is missing from my dataset.

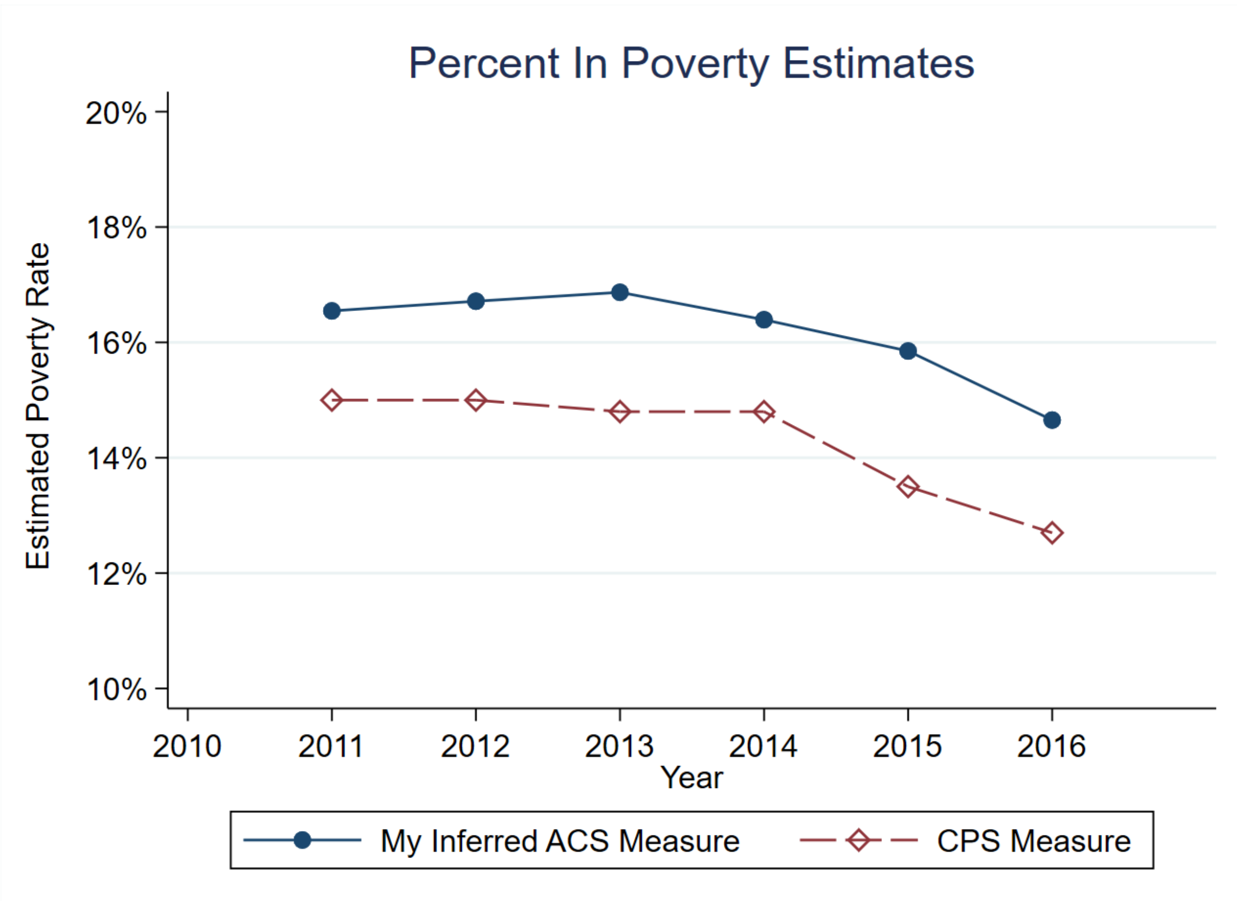
Given that licensure can occur during residency, the date of licensure cannot reliably be used to identify new physician entry. To identify the date of entry, I follow Falcetone (2017) and utilize the CMS's Medicare Physician Compare. Medicare Physician Compare provides information on physicians and medical groups that participate in Medicare. While this data set does not contain all physicians, it does contain participating physicians' NPI and their year of graduation from medical school. The Year of entry can, therefore, be identified by adding the years of required training for a specialty to the year of graduation. Employing this method, I constructed my sample's annual state-level count of entering physicians per 100,000 state population. I aggregate entries to the year level as sparsely populated states do not always have a physician enter every month. As a precaution, I examined my prospective entrants years later in the NPPES' publication for June of 2020. If an individual identified as a potential entrant did not have a physician's taxonomy code in 2020 or became a sub-specialist after my sample period, then I removed them from my sample. This avoids conflating post-training entrants with those who did not remain a physician or did not complete their fellowship training during the sample period. This removed less than 4.5 percent of potential entrants.

To examine if the Medicaid expansions induced doctors to locate closer to poor populations, I estimate the number of individuals under the federal poverty line (FPL) per 1000 population residing within twenty, ten, and five miles of a new entrant's location. The maximum size of this radius comes from research in the states of Kentucky and Washington. One paper found that about 82% of patients traveled less than 20 miles for their healthcare visit (Cashion et al., 2013). The other found that surveyed adults would be willing to travel just over 20 miles on average for routine care, though average trips at the time were considerably shorter (Yin, 2013).

I infer the general population and those under the federal poverty line near physician locations using American Community Survey (ACS) data at the census block group level; which is the lowest level geography publicly available. I utilize the five-year files for the ACS, which are one percent national samples for each year and the only files which publish census block group data. I assume the data best represents the middle year of each five-year period from 2009-2018.

There is limited information offered at the census block group level given that some groups have very small population sizes. I infer the number of individuals under the FPL living in each block group using block group population, number of households in each block group, and the number of households in various income categories. Taking the average household size and assuming households are uniformly distributed within income categories, I estimate the population under or at the poverty line in each block group. Figure 2. plots the annual poverty rate I infer alongside that reported by the Census Bureau using Current Population Survey's (CPS) data (Semega et al., 2017).

FIGURE 2: INFERRED PERCENT IN POVERTY



My inferred percentage is consistently about one percentage point higher than the CPS but tracks it very well. Assuming each census block group's population lives in its centroid, I construct the number of individuals under the federal poverty line per 1000 population living within twenty, ten, and five miles of each entering physician's location. Table 1. displays a table of summary statistics for the aggregate state and individual level outcomes by pre and post-expansion periods.

TABLE 1: CHAPTER I SUMMARY STATISTICS

State Entries Per 100K Population	**Mean (2012-2013)**	**Mean (2014-2016)**
All Doctors	2.60	2.77
Primary Care	1.10	1.16
Other Specialties	1.50	1.60

Population Under FPL Per 1,000 Near Physician Location	**Mean (2012-2013)**	**Mean (2014-2016)**
Within 20 Miles		
All Doctors	167.04	154.40
Primary Care	167.71	155.86
Other Specialties	166.57	153.39
Within 10 Miles		
All Doctors	184.75	169.85
Primary Care	185.73	171.11
Other Specialties	184.07	168.99
Within 5 Miles		
All Doctors	204.02	188.13
Primary Care	208.38	190.83
Other Specialties	200.99	186.27

Note: 2011 is excluded here as the physician data begins in April of that year. Its inclusion would reduce the pre-period state entry means due to this. For consistency, 2011 is excluded in the doctor level means as well.

Data Limitations

There are limitations to using Medicare Physician Compare to identify entry and the NPPES's primary practice location for physicians. Pediatricians, returning physicians, and foreign-born physicians are likely underrepresented in my sample. Pediatricians do not tend to participate in Medicare and relatively few appear in the Physician Compare data. Physicians who return from an extended break from practice or who are foreign-born enter on non-traditional timelines. Both types require additional training to be licensed and basing entry on graduation year likely excludes the majority of these physicians. The Medicaid expansions were designed, however, to increase healthcare access for poor adults and the sample's lack of pediatricians is less concerning than it might be in other circumstances. I exclude those that do appear in Physician Compare from my sample as my concern is about poor adult access to care.

The effect of omitting returning physicians is ambiguous as there is little research on returning physicians. It has been estimated that around 10,000 physicians could return to practice each year; however, there is little information on how many do return and in what specialties (AMA Reentry Fact Sheet, 2011).[3] The omission of foreign-born physicians, on the other hand, likely leads to conservative results. Around twenty-five percent of physicians practicing in the US have medical degrees from foreign countries, and evidence suggests that most of these physicians are not US citizens (AIC, 2018). This report finds that foreign-trained physicians constitute nearly one-third of doctors practicing in areas where at least 30 percent of the population are at the federal poverty level. This suggests the omission of these doctors' location choices will lead to understated levels of low-income individuals near entering physician locations.

The benefit of having a precise location for each physician's primary practice location is limited by the fact that physicians may practice at multiple locations. The NPPES does not track nor require physicians to report all locations of practice. The effect this may have is ambiguous and depends on where else physicians may practice. If a physician's additional practice locations are in higher-income areas, then results implying increased access for low-income populations would be overstated. A similar argument could be made for an understated or unaffected result.

[3] The year is not listed on the sheet, however another source mentions that the study providing this number is from 2011, see: https://khn.org/news/for-doctors-who-take-a-break-from-practice-coming-back-can-be-tough/
Last accessed: 7/31/2020

There is not an obvious means to address this limitation and I rely on the assumption that the majority of each physician's time is spent at their primary practice location.

1.4 METHODS

I employ difference-in-differences (DiD) and event-study specifications in both my cross and within-state choice analyses. The examination of cross-state location choices determines if the Medicaid expansions induced differential sorting. If the composition of state entrants changed after expansions, then the interpretation of within-state results needs to acknowledge this change. My preferred specification uses only the 40 states which expanded in January 2014 or did not expand before 2017. I exclude those states which expanded earlier or later in my sample period. I do not have pre-expansion data for early expansion states. My sample period from April 2011 to December 2016 and I cannot examine any response to these early expansions. Further, the late expansion states have long pre-expansion periods and short post-expansion periods (one as short as six months). This introduces potentially unwanted variation in pre and post-expansion results due to changes in number and type of contributing states. There is a growing literature that expresses concerns about the legitimacy of event-study results and pre-trend tests when the timing of treatment is heterogeneous (Sun and Abraham 2020). My preferred specification avoids this concern and creates balance in the periods before and after expansion supporting a more causal interpretation of results. My cross-state decision DiD specification is as follows

$$Entries\_Per\_100K\_Pop_{it} = \beta_0 + \beta_1 Medicaid\_Exp_{it} + \boldsymbol{X_{it}}\boldsymbol{\lambda} + \boldsymbol{\tau}_t + \boldsymbol{\gamma}_i + \mu_{it} \qquad (1)$$

where $Entries\_Per\_100K\_Pop_{it}$ is the count of all entering physicians or a specific group of physicians in state *i* in year *t* per 100,000 state population, $Medicaid\_Exp_{it}$ is an indicator equal to 1 if state *i* has expanded its Medicaid program in year *t* or years prior and 0 otherwise. $\boldsymbol{X_{it}}$ is a vector of state-level controls[4] for state i in year t, $\boldsymbol{\tau}_t$ and $\boldsymbol{\gamma}_i$ are year and state fixed effects respectively, and μ_{it} is the error term.

The event study specification closely resembles equation (1) and is as follows

$$Entries\ Per\ 100K\ Pop_{it} = \beta_0 + \sum_k (Ever\_Expanded_i \text{ x } Year_k)\boldsymbol{\phi} + \boldsymbol{X_{it}}\boldsymbol{\lambda} + \boldsymbol{\tau}_t + \boldsymbol{\gamma}_i + \mu_{it} \quad (2)$$

$Ever\_expanded_i$ is an indicator equal to 1 if state *i* expanded Medicaid in January of 2014. $Year_k$ is an indicator for a given year such that k ϵ {2011, 2012, 2014, 2015, 2016}, leaving 2013 as the comparison year.

My within-state decision specification is very similar to that of my cross-state and is as follows

$$Pop\ Under\ FPL\ Per\ 1000\ Pop\ Near\ doc_{ijkt} = \alpha_0 + \alpha_1 Medicaid\_Exp_{kt} + \boldsymbol{X_{kt}}\boldsymbol{\lambda} + \delta_1 female_{ijkt} + \boldsymbol{\tau}_{jt} + \boldsymbol{\gamma}_{jk} + \boldsymbol{\varepsilon}_{ijkt} \quad (3)$$

Where $Pop\ Under\ FPL\ Per\ 1000\ Pop\ Near\ doc_{ijkt}$ is the population under the federal poverty line per 1000 population living within 20, 10, or 5 miles of doctor *i* of type *j* in state *k* in year *t*. $\boldsymbol{X_{kt}}$ remains a vector of state-level controls, $female_{ijkt}$ indicates if the entrant is female, the fixed effects are now year by doctor type (primary care, surgery, and other specialties) and state by doctor type respectively.

The event study specification for states which expanded in 2014 is as follows

[4] These controls include state-level means of race, education, insured levels, income, and population. Additionally, I control for whether states kept the primary care fee pump in any fashion and had any policy changes affecting malpractice.

$$Pop\ Under\ FPL\ Per\ 250\ Pop\ Near\ doc_{ijkt} = \alpha_0 + \sum_k (Ever\_Expanded_i \text{ x } Year_k)\boldsymbol{\pi} + \boldsymbol{X_{kt}\lambda} + \boldsymbol{Z_{it}\delta} + \boldsymbol{\tau}_{jt} + \boldsymbol{\gamma}_{jk} + \boldsymbol{\varepsilon}_{ijkt} \qquad (4)$$

where the year before expansion is again used as the reference year.

1.5 RESULTS

Results

Table 2 displays the difference-in-differences estimates for my preferred cross-state decision specification. Figure 3 displays the cross-state event study for all physician types, primary care, and other specialties. Tables for these event studies can be found in Appendix A in Table A.1. I find little evidence that the Medicaid expansions induced changes in physician entries per 100,000 state population. The 95 percent confidence intervals for the difference-in-differences results, however, do not rule out potentially meaningful effects. The interval for all physician types includes effects ranging from a 13.7 percent decrease in physicians entering expansion states to an 11.1 percent increase. The event-study results in Figure 3 also do not suggest meaningful changes in the state-level entry for any doctor type, however, the possibility of such changes cannot be entirely ruled out.

Table 3 shows the difference-in-differences estimates for my preferred within-state specification. Figure 4 displays the within-state event studies for radii of twenty, ten, and five miles respectively. The table of these results can be found in Appendix B, Table A.2. As the radius tightens around those living nearest physicians, a clear pattern emerges. I find significant evidence that all physician types chose to locate closer to poor populations in expansion states. The five-mile radius results for all physician types suggest the population under the federal poverty line per 1,000 residents near new physician locations increased by 3.6 percent relative to the pre-expansion mean in the first year of expansion, 4.9 percent in the second year, and 7.9 percent in the third year. Figure 5 displays the sub-sample event studies for primary care and other specialties. The results for primary care are insignificant in the first two years but suggest an increase of 7.3 percent in the third year. The results for the other specialties suggest increases of 4.3 percent in the first year, 4.9 percent in the second, and 8.3 percent in the third. These results imply that newly entering physicians located increasingly closer to poor populations in expansion states over time.

TABLE 2: CROSS-STATE DID - ENTRANTS PER 100K STATE POPULATION

Variables	All Doctors	Primary Care	Other Specialties
Medicaid Expansion	-0.0339	-0.0348	0.0010
	(0.1597)	(0.0983)	(0.0930)
State FE	x	x	x
Year FE	x	x	x
Observations	240	240	240

Note: The outcome of interest is the state-level count of new entries per 100,000 state population. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform and if primary care fee bump was kept, and state and year fixed effects. Standard errors in parentheses are robust and clustered at the state level (+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001).

Robustness

Figures 6, and 7 examine the sensitivity of results to the exclusion of states by expansion timing. They explore the potential concern that excluding populous states like California, in which many physicians begin practice, may significantly influence results. Excluding the early and late expanding states reduced the sample by nearly 9,000 entrants which is almost 25 percent of all entrants in the sample. Just over 3,000 of those excluded entrants started practice in California. Figure 6 displays three event studies for all physician types which include the addition of early expanding states to the preferred set, of later expanding states, and the use of all states. Consistent with the preferred set, the inclusion of early expanders, late expanders, or all states in the cross-state analysis does not result in any statistically significant findings. This suggests that cross-state results are not driven by state exclusions.

Figure 7 displays similar event studies for the within-state analysis. The within-state results are largely robust to the inclusion of early and late expansion states. Following Courtemanche et al. (2017) I assume that the full expansion for early expansion states occurred in January of 2014. Figure 7 displays the five-mile, event-study results for all doctor types with the inclusion of early expanders, late expanders, all states, and all states except Michigan. The inclusion of early expansion states does little to change the post-expansion results, however, a significant pre-trend appears in 2011. This trend does not persist in 2012 but could suggest a response to early expansions in 2010 and 2011. A lack of data before 2011 prohibits further exploration.

TABLE 3: WITHIN-STATE DID

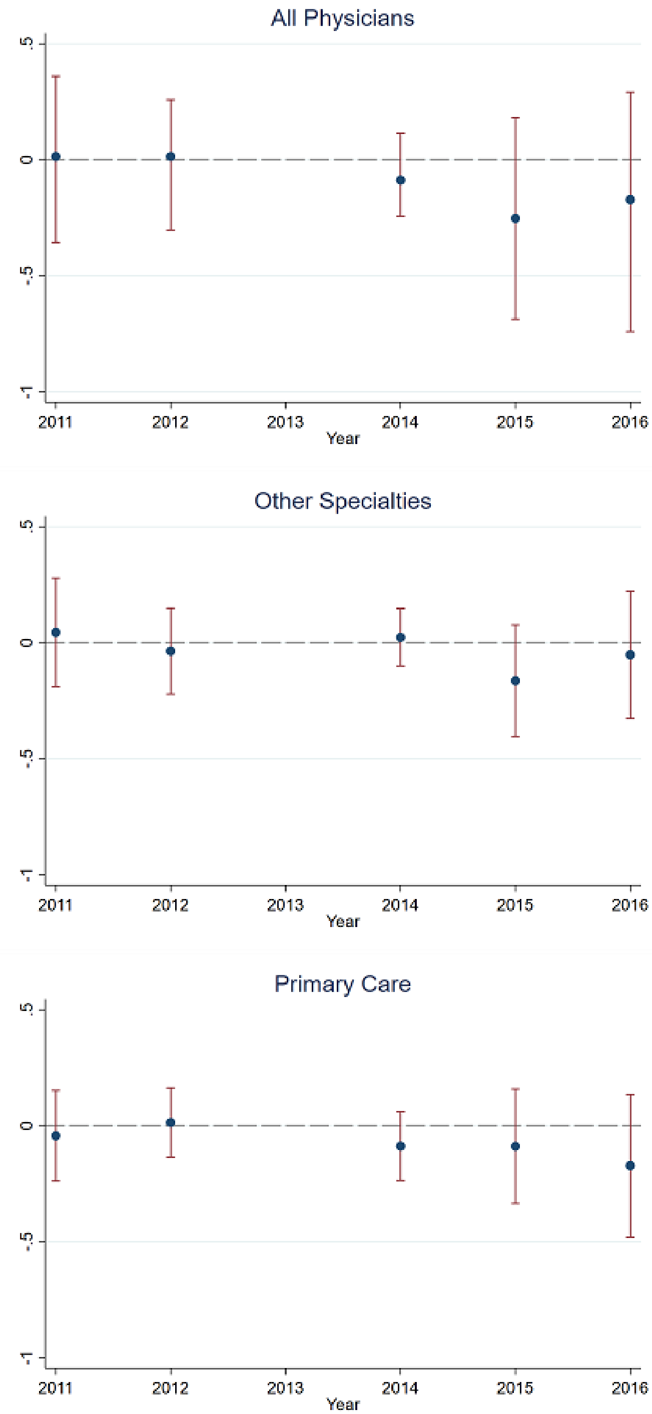
Panel 1. 5 Mile Radius	All Docs (5 mi.)	Primary Care (5 mi.)	Other. Spec. (5 mi.)
Medicaid Expansion	6.5248**	4.6286	8.6457*
	(2.1100)	(4.7646)	(3.4059)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30243	12353	17890

Panel 2. 10 Mile Radius	All Docs (10 mi.)	Primary Care (10 mi.)	Other. Spec. (10 mi.)
Medicaid Expansion	3.1119+	-0.4109	5.9858**
	(1.6557)	(3.8391)	(1.7873)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30254	12360	17894

Panel 3. 20 Mile Radius	All Docs (20 mi.)	Primary Care (20 mi.)	Other. Spec. (20 mi.)
Medicaid Expansion	1.8718	0.2873	3.1118+
	(1.9714)	(3.1234)	(1.7254)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30255	12361	17894

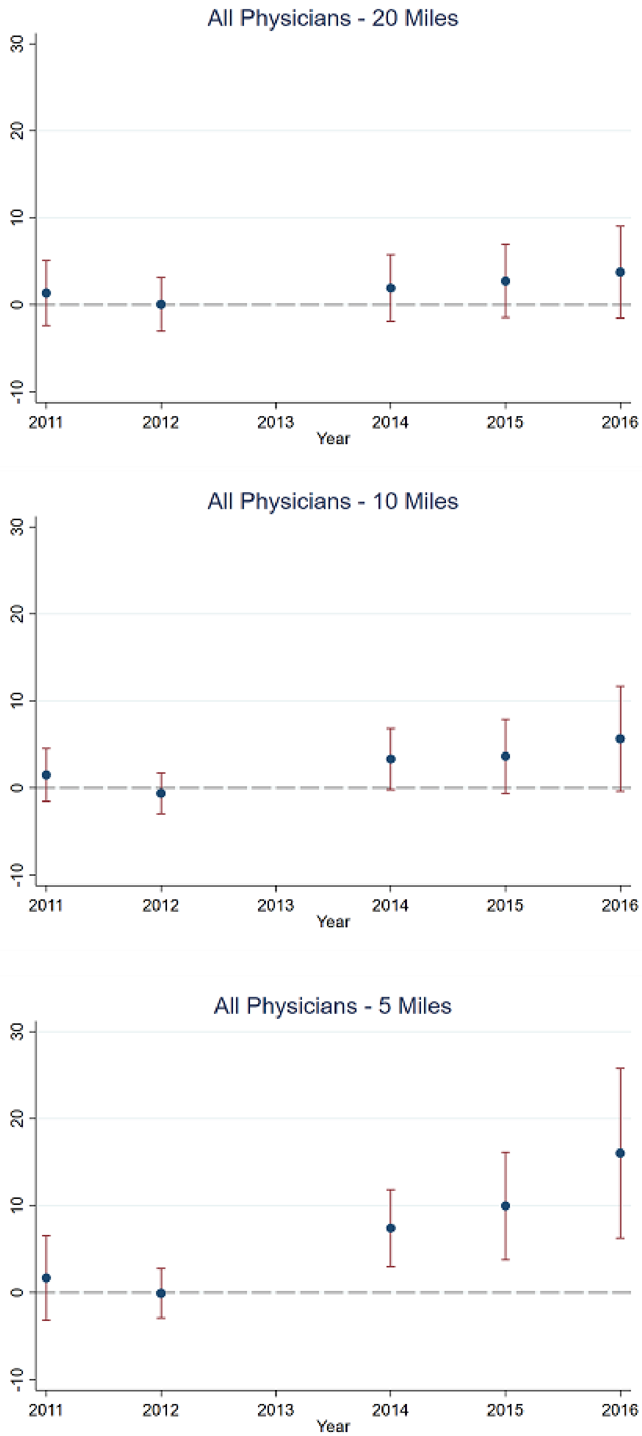
Note: The outcome of interest is the population under the federal poverty line per 1000 population living within 20, 10, and 5 miles of newly entering physicians. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform and if primary care fee bump was kept, and state by physician type and year by physician type fixed effects. Standard errors in parentheses are robust and clustered at the state level (+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001)

FIGURE 3: CROSS-STATE EVENT STUDY – ENTRIES PER 100K POPULATION



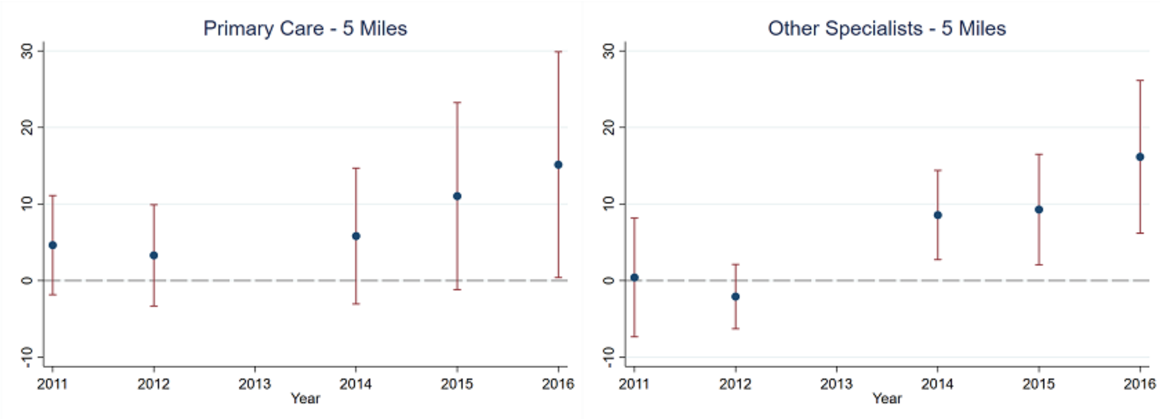
Note: The outcome of interest is the state-level count of new entries per 100,000 state population. Explanatory variables of interest are interactions between Medicaid expansion status and year. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform, and if primary care fee bump was kept, and state and year fixed effects. 2013 is the comparison year and graphs display 90% confidence intervals. Standard errors are robust and clustered at the state level.

FIGURE 4: WITHIN-STATE EVENT STUDY: 20, 10, AND 5 MILE RADIUS



Note: The outcome of interest is the population under the federal poverty line per 1000 population living within 20, 10, and 5 miles of newly entering physicians. Explanatory variables of interest are interactions between Medicaid expansion status and year. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform, and if primary care fee bump was kept, and state by doctor group (primary care or other specialists) and year by doctor group type fixed effects. 2013 is the comparison year and graphs display 90% confidence intervals. Standard errors are robust and clustered at the state level.

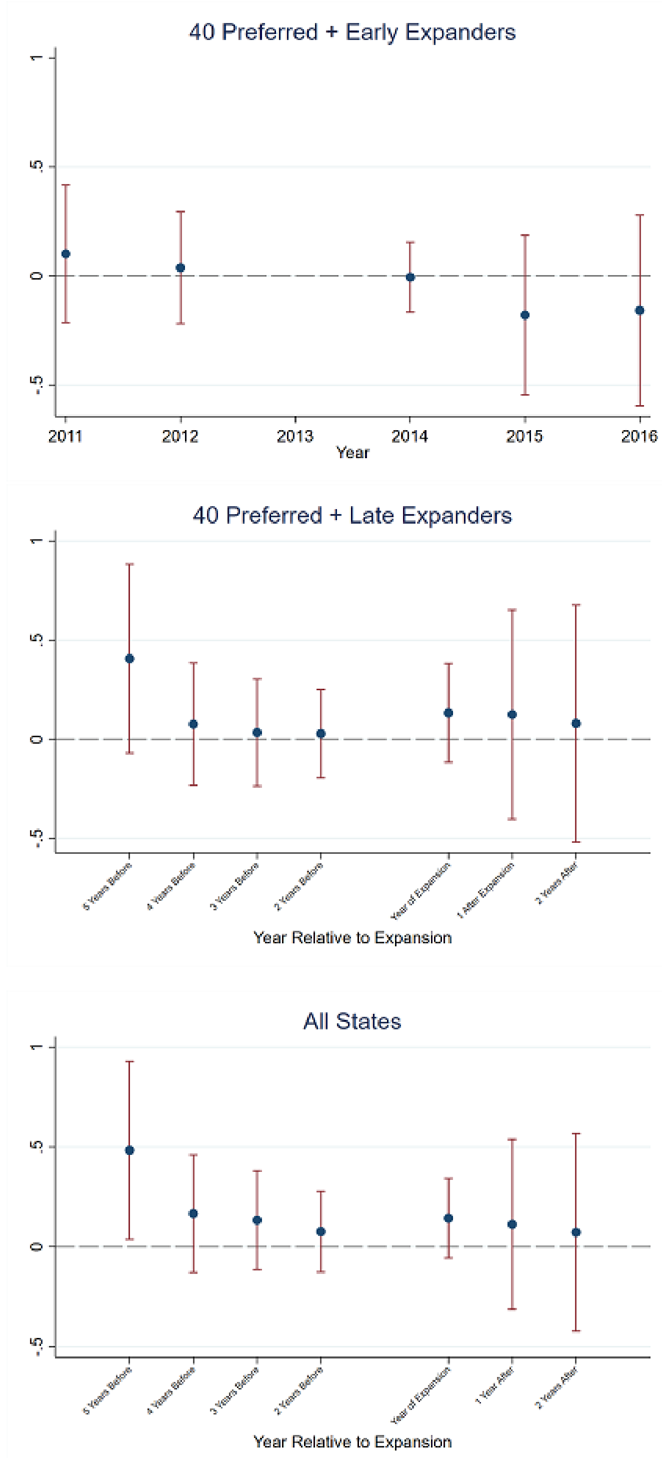
FIGURE 5: SUB-SAMPLE ANALYSIS – PRIMARY CARE AND OTHER SPECIALISTS



Note: The outcome of interest is the population under the federal poverty line per 1000 population living within 5 miles of newly entering physicians. Explanatory variables of interest are interactions between Medicaid expansion status and year. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform, and if primary care fee bump was kept, and state by doctor group (primary care or other specialists) and year by doctor group type fixed effects. 2013 is the comparison year and graphs display 90% confidence intervals. Standard errors are robust and clustered at the state level.

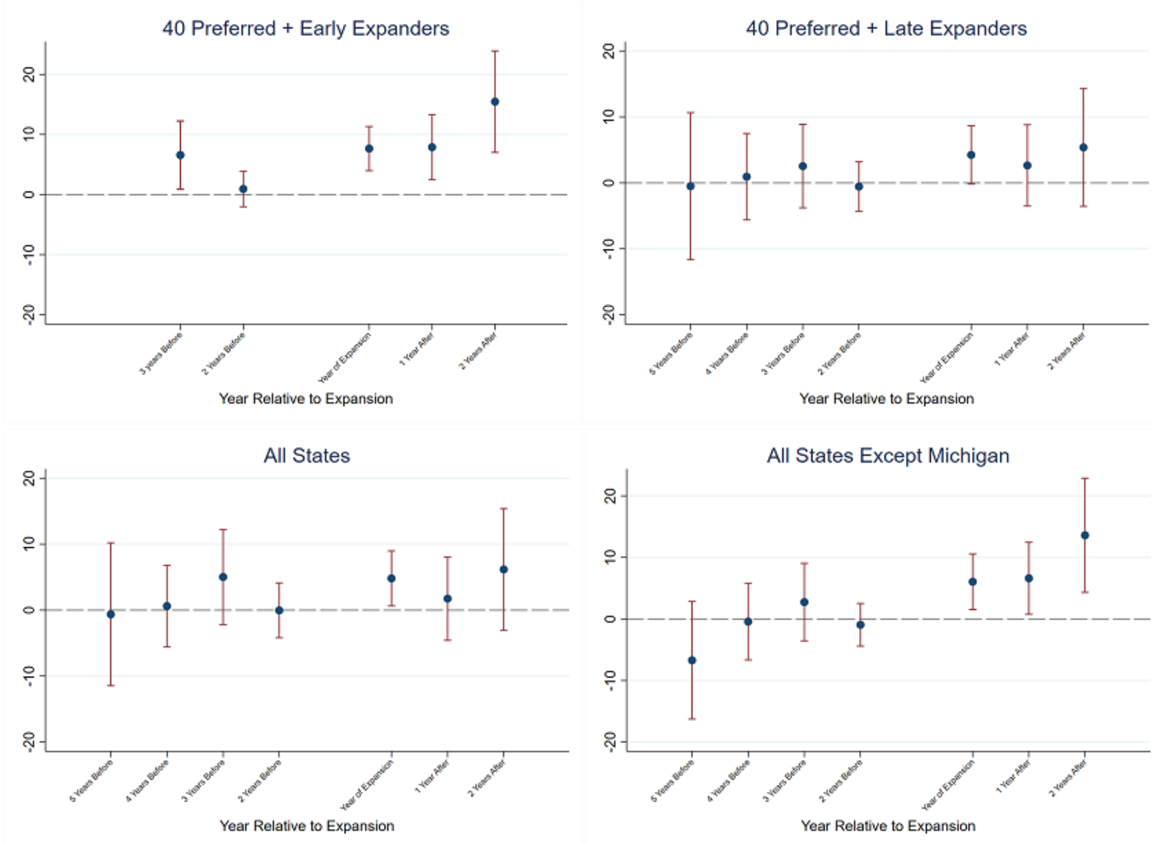
The inclusion of late expansion states introduces more heterogenous timing in expansions and produces noisier results. The results are similar to my preferred results for those years shared by all included states (three years prior through the year of expansion). In the shared periods, there are no significant pre-trends and there are significant increases in the year of expansion. The results for the subsequent expansion years are suggestive of increases but are insignificant. The use of all states presents a similar story, suggesting that the noisiness of post-expansion results is driven by the inclusion of late-expanding states. The exclusion of Michigan, which expanded in April of 2014, addresses this lack of precision and provides results similar to my preferred specification. While it is reassuring that statistical imprecision is not systemic to all late expanding states, the source of it in Michigan cannot be explained by this analysis.

FIGURE 6: CROSS-STATE EVENT STUDIES – VARYING STATE INCLUSIONS



Note: The outcome of interest is the state-level count of new entries per 100,000 state population. Explanatory variables of interest are interactions between Medicaid expansion status years relative to expansion. For early expansion states, January of 2014 is assumed to be the official expansion date. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform, and if primary care fee bump was kept, and state and year fixed effects. 2013 is the comparison year and graphs display 90% confidence intervals. Standard errors are robust and clustered at the state level.

FIGURE 7: WITHIN-STATE EVENT STUDIES – VARYING STATE INCLUSIONS



Note: The outcome of interest is the population under the federal poverty line per 1000 population living within 5 miles of newly entering physicians. Explanatory variables of interest are interactions between Medicaid expansion status and year. Other controls include state-level demographics, education, unemployment, indicators for malpractice reform, and if primary care fee bump was kept, and state by doctor group (primary care or other specialists) and year by doctor group type fixed effects. 2013 is the comparison year and graphs display 90% confidence intervals. Standard errors are robust and clustered at the state level.

1.6 DISCUSSION

My findings suggest healthcare access increased for low-income populations within expansion states without reducing access in non-expansion states. The broad ACA literature suggests access increased to those physicians already participating in Medicaid but physician participation did not change. This places increased importance on new physician decisions. If Medicaid expansions had induced new physicians to enter expansion states over non-expansion states, then it likely would have been those predisposed to serving Medicaid patients. This could have led to undesirable access tradeoffs among low-income populations. In such a case, the gain in the probability of low-income adults in expansion states having a personal physician (Simon et al., 2017), might have come at the expense of similar populations in non-expansion states. Finding only within-state effects suggests expansion states increased access without negatively impacting their non-expansion neighbors. Therefore, findings of increased prescription drug access for chronic conditions (Ghosh et al., 2017) would not be diminished by accompanying access tradeoffs in non-expansion states.

I find somewhat smaller and less precise estimates for primary care physicians relative to other specialists. This could be due to a somewhat smaller sample or to differences in relative compensation rates. For a majority of states non-primary care, non-obstetric services are compensated at a higher relative rate (Zukerman and Goin 2012; KFF 2016). The weaker results among newly entering primary care physicians could be due to a relatively weaker financial incentive to serve Medicaid enrollees. It could also be due to greater competition for these populations from non-physician PCPs (Tiperneni et al. 2019).

I find physicians' location decisions are responsive to changes in Medicaid, but only within their chosen state of practice. This may be because physicians prefer to stay within their state of residency (Falcetone, 2017) and the relatively low compensation offered by Medicaid was unable to overcome this preference (Zuckerman et al.; 2012, 2014, 2017). The malpractice reform literature finds state-level effects on physician supply, sometimes only for at-risk specialties, using area-level counts or post-residency decisions from a single state (Chatterji et al., 2018). My sample, while unlikely to be representative, is national and uses only entrants. This reduces the risk of results being influenced by physician exit or being highly localized. Figure 1 shows a declining poverty rate in the US from 2014 to 2016 for both my inferred rate and the CPS measure. This suggests my results are not driven by changes in poverty but by changes in location decisions.

This work contributes to a sparse literature on supply-side responses to the ACA and the wider literature on physician location decisions. My results support demand-side research suggesting increased healthcare access. They imply the supply-side response was to reallocate entrants within states to accommodate the increased demand from expanded Medicaid. It also demonstrates a need for additional supply-side research as estimates of access changes require a fuller understanding of both supply and demand responses.

This paper brings a novel, national dataset to bear on an underexplored area of research and indicates valuable future work to be done. My results are suggestive of increased access but do not address physician persistence in their post-residency location. If physicians remain in their post-residency location for extended periods, then my results suggest increasing access over time for low-income populations. However, if they move on quickly to serve high-income areas and are merely being replaced by new entrants, then access increased in a more limited fashion.

My work is policy informative and provides insight into the effect of Medicaid expansions. However, a limitation is that it does not comment on the cost-effectiveness of increases in access. It is beyond the scope of this paper to evaluate the costs and benefits of changes in access. Nevertheless, research on insurance expansions in the US remains relevant as the national debate on the form health insurance and healthcare should take is ongoing. My work suggests that the observed gains in access in expansion states came in the most preferred form. Expansion states increased healthcare access for low-income populations without evidence of damaging their non-expansion neighbors. While I do not suggest that physicians will never be induced to locate across state lines by changes in Medicaid policy, my results suggest that Medicaid policy may be a means of addressing access disparities within states without damaging one's neighbors. Policy has changed with administrations over time and is likely to change again in the future. This creates a need for continued, causal research to inform the decisions of policymakers.

CHAPTER II: URBAN SPRAWL AND PREVENTIVE CARE

2.1 INTRODUCTION

Common aspects of the United States' metropolitan areas that affect their populations, affect a majority of the nation as urban areas contain nearly eighty percent of the country's population (U.S. Census Bureau 2016). As urban areas expand, so too do the activities performed in environments almost entirely manmade. The layout of cities' systems of roads, the location of businesses and residences, the existence and routes of public transportation, and all other amenities come together to give an urban area its form. Given the population living within urban areas, understanding their form's influence on residents' health, labor markets, socioeconomic distributions, and other outcomes are of interest to researchers and policymakers. This work contributes to the urban form and health-related research by estimating the impact of urban sprawl on the probability one obtains certain, recommended preventive care services.

As a city expands and shopping centers, subdivisions, commercial centers, and other types of development are built, they are often constructed with their own parking lots, parking structures, driveways, lawns, offices, living space, and more paved roads and black-topped areas. The added space used for living, working, and entertaining tends to reduce the population per given square unit of distance in an area. If the urban form is significantly spread out, then it is often called sprawling. In a broad sense, urban sprawl can be thought of as "the process in which the spread of development across the landscape far outpaces population growth" (Ewing et al 2014). In this case, people and places are further apart and travel between locations may require additional time and costs on average. The degrees to which metropolitan areas are traversable, allocated for particular uses, have concentrated economic centers, and populations are distributed across space provide the common measures of sprawl. How traversable (grid-like) a metropolitan area may be is also used as a measure of sprawl, though, perhaps less commonly than the other three.

In areas that are sprawled, one’s ability to travel by other means than by car may become more inhibited and inconvenient. People who live in cities and depend on public transportation may not be offered a route that takes them to suburban areas where their doctor may be located or they may find that a bus or train traveling to that area runs infrequently. Even if the travel time remains unchanged to a preferred health facility's location, switching from taking the train to a car can impose higher costs. Among other scenarios, higher costs arise when an equidistant train trip is cheaper than driving, when one does not own a car and must hire one, or when one has purchased an unlimited train pass and using a car is an additive cost. As costs and inconvenience rise, individuals may forgo certain trips including those for medical services that do not seem urgent.

Ewing et al. (2014) found that urban sprawl is associated with greater amounts of driving, pollution, vehicle accidents, and reduced use of alternatives to driving such as walking. Other research found that reduced population and housing density are correlated with increased driving times to primary care providers in Orlando (Bejleri et al. 2016). This suggests that sprawl likely imposes higher average travel costs. The question this paper asks is, does this greater urban sprawl decrease the probability of acquiring preventive care?

2.2 THEORETICAL CONSIDERATIONS

Preventive care may be the area of medical care expected to respond most strongly to sprawl levels. Preventive care may be thought of as optional care by an average individual. These are services that people receive proactively while feeling healthy to head off potential, future health problems and remain healthy. These services are unlikely to be viewed as being as much of a necessity when compared to services that one seeks for an illness. Their probability of use may, therefore, be more sensitive to how burdensome a service is to acquire. Sprawl's expected effect on medical care usage when one is ill is more ambiguous. On one hand, it seems plausible for increased inconvenience to reduce the usage of care for prevention and the treatment of illness. On the other hand, care for illnesses may be utilized more if there is a decline in preventive care usage that translates into a higher rate of manifested illnesses requiring treatment. For this reason, I focus on preventive care, as it is plausibly affected in only a single direction.

The decision to forgo services rests on how preventive care is valued relative to its cost of acquisition. The value of preventive care can be discussed from two points of view. One point of view evaluates preventive care in terms of cost-effectiveness (Maciosek, 2006; Maciosek 2017). From this perspective, the value of a given service is based on whether the estimated cost of the prevention is cheaper than treating the illness itself. This is an important consideration for those who bear the cost of medical bills such as health insurers. This manner for valuating preventive care, however, is unlikely to be shared by individuals. Grossman (1972) relates that there is value in and of one's health and, to that extent, there is value in investing to improve or maintain one's health. All other things equal, people prefer to be well rather than ill, which necessitates a value difference. Pure cost-effectiveness at the individual level disregards the value of one's health in the well and sick state. If preventive treatment is cost-neutral, people are unlikely to be indifferent between the well state which preventive care preserves and the sick state which may arise in its absence.

The urban form for metropolitan residents influences the costs they face in obtaining preventive care. For some suburban residents, care may less costly if facilities have located near them. On average, however, the more separated and spread out things are, the more costly and inconvenient obtaining care becomes. It seems sensible to expect that as the cost of obtaining preventive services rises, the probability of forgoing them increases. One study found that elderly people in sprawling suburban areas are more likely to be treated for ambulatory care sensitive conditions that are considered preventable with preventive care (Mobley et al. 2006a). Other research has found that added distance to a hospital does reduce the probability that central city black children have a checkup by about three percent (Currie and Reagen 2003). These findings suggest that at least for some, the cost of preventive care is affected by one's environment and provides motivation for additional research.

2.3 RELEVANT LITERATURE: SPRAWL AND HEALTH

The majority of urban form and health research focuses on areas other than preventive care and finds largely negative health effects of sprawl. One study found that sprawl is associated with higher risks of traffic fatalities, particularly for pedestrians (Ewing et al. 2003b). Another claimed that increased urban sprawl is associated with longer emergency medical service response times and delayed ambulance arrivals (Trowbridge et al. 2009). These studies imply health risks are part of the higher costs that sprawl can impose through greater car use. This may be especially higher among the elderly as they are more likely to cause traffic accidents than non-elderly adults and use ambulances (Loughran et al. 2007; Albert et al. 2013).

Other studies look at a broad set of links between sprawl and health, including heart health. A small number of studies have attempted to link between urban sprawl and coronary heart disease. These studies, however, have mixed results and generally seem to find no effect. Of those papers, two find an association between urban sprawl and coronary heart disease in women (Mobley et al. 2006b; Griffin et al. 2012); while Ewing et al. (2003a) found sprawl be associated with higher rates of hypertension.

The research focused on sprawl's impact on food access and obesity has found stronger results. Christian (2010) found a negative correlation between sprawl and food insecurity. Ewing et al. (2003a) found that increases in sprawl are associated with less leisure time walking and higher rates of obesity. Other studies found a similar association between sprawl and obesity (Vandegraft and Yorked 2004), while another found that increased driving time was associated with an increased probability of obesity (Frank et al. 2004).

The findings on obesity, however, were somewhat disputed and then reaffirmed by later literature. Some research suggested that people with high body max index measures tend to choose to live in high sprawl areas, which casts doubt on a causal link between sprawl and obesity (Plantinga and Bernell 2007). Similar correlational research claimed that there was no link at all between sprawl and obesity once time-invariant, unobserved characteristics were accounted for (Eid et al. 2008). Later causal research, however, found that had the proportion of the population living in dense areas not declined in the average metropolitan area from 1970-2000, then obesity would have been reduced by about 13% (Zhao and Kaestner 2010).

2.4 DATA

Preventive Care Outcomes

To estimate the effect greater MSA sprawl has on acquiring certain preventive care services within medically recommended time windows, I use Behavioral Risk Factor Surveillance System (BRFSS) data from 2012 and an MSA sprawl index from 2010. The BRFSS contains survey data on respondents' use of preventive care services. The examined services such as breast, cervical, and colorectal cancer screenings and influenza vaccinations, are among those prioritized by the National Commission on Preventive Priorities (Maciosek et al. 2017). Other outcomes include biannual checkups and whether respondents have a personal doctor. The 2012 survey is used as questions are reflective of services received in past years and would include 2010 sprawl levels. The BRFSS data is used to create binary variables that take the value one if respondents acquired a given service within the recommended time window and zero otherwise. Table 4 displays the timing recommendations for the various services.

TABLE 4: RECOMMENDATIONS FOR PREVENTIVE CARE SERVICES

Service	Timing	Recommending Body
Routine Checkups	1-2 Years	American Medical Association*[5]
Flu Shots	Yearly	Center for Disease Control
Mammograms	1-2 Years (Ages 45+)	American Cancer Society
Breast Exams	Yearly (40+), 1-3 Years(20-39)	American Cancer Society
Pap Smears	Every 3-5 years (21+)	Mayo Clinic
Colon/Sigmoidoscopies	Every 10 years (50+)	American Cancer Society

While the BRFSS is a nationally representative survey, the full BRFSS is not necessarily MSA representative. This means that decisions concerning data usage and weighting needed to be made carefully. An alternative that was considered in place of the full BRFSS was the BRFSS SMART. The BRFSS SMART is another dataset offered by the CDC that focuses on MSAs. Unfortunately, it has a minimum response requirement to include data on a particular MSA in a given year. This excludes considerable MSAs that are less populous than others. These exclusions leave a restrictive subset of very similar MSAs in terms of sprawl characteristics and greatly reduced the variation off of which I aim to identify the effect of sprawl. I ultimately use the full

[5] The American Medical Association has not endorsed yearly checkups in more recent years.

BRFSS given the restrictiveness in the SMART data and apply MSA population weights in place of the BRFSS weights. Table 5 provides summary statistics for the BRFSS data I use as control variables or to construct control variables. There are negligible differences between my unweighted subsample and the full, unweighted BRFSS.

TABLE 5: SUMMARY STATISTICS FOR SUB-SAMPLE

Variable	Full BRFSS Sample	MSA Sub-Sample
Age	54.54	53.88
White	73.63%	76.68%
Black	11.66%	11.64%
Asian (Non-Pacific Isl.)	1.97%	1.96%
Female	59.65%	58.30%
Married	51.80%	52.54%
Highschool Diploma	26.21%	25.34%
Some College	27.14%	27.28%
4+ Years of College	38.87%	40.21%
10k < Income < 15k	5.59%	5.53%
15k < Income < 20k	7.45%	7.40%
20k < Income < 25k	8.97%	8.94%
25k < Income < 35k	10.65%	10.63%
35k < Income < 50k	14.07%	14.10%
50k < Income < 75k	15.79%	15.85%
Income > 75k	32.09%	32.23%
Employed for Wages	43.37%	45.88%
Self-Employed	7.69%	7.94%
Retired	27.15%	25.40%
Has Any Health Insurance	88.61%	88.60%

Measure of Sprawl

The measures of urban sprawl come from a 2010 index of urban sprawl made available by Ewing et al. (2014)[6] [7]. An older version of this index was used by Christian (2010). I use this sprawl index at the metropolitan statistical area (MSA) level and have measures for 205 MSAs in

[6] Index can be obtained at: https://gis.cancer.gov/tools/urban-sprawl/
[7] Due to data collection difficulties, measures for Massachusetts are not included in this sprawl index.

44 states[8]. Ewing et al. (2014) calculate their measures of sprawl using four factors. Theses factors are in turn, the composition of multiple variables that can be associated with each factor. The factors are population density, mixed land use, economic centering, and street accessibility. A more sprawled MSA is associated with lower population densities, more segregated land use, more dispersed economic activity, and more convoluted street systems. Greater detail on what variables are used to construct the values of each factor can be found in Ewing et al (2014) or to a lesser degree in this chapter's appendix discussion.

The measure of each factor was constructed at the county level for around 1000 counties and was standardized to have a mean of 100 and a standard deviation of 25. This mean and standard deviation largely holds when aggregating to the MSA level. The index also provides a composite score that is the equally-weighted average of the pre-standardized factors. As the individual factor measures are standardized and rounded, I cannot replicate the composite measure.

The Instrumental Variable: The 1947 Highway Construction Plan

To address the concern that contemporaneous use of preventive care services and the level of sprawl of one's home MSA may be correlated with unobserved preferences, I instrument for sprawl using the 1947 highway construction plan. I use a similar method to that used by Baum-Snow (2007) and Zhao and Kaestner (2010), though in a static setting. Baum-Snow (2007) examined the effect highways had on suburbanization; a term sometimes used in place of urban sprawl. To address the endogeneity of contemporaneous choices of where governments build roads and where people elect to live, Baum-Snow used the 1947 federal interstate highway plan as an instrument for contemporaneous highway construction. The plan was designed to connect distant,

[8] States for which I do not have obesevations or do not have sprawl measures are: AK, DE, DC, HI, MA, MT, WY

major United States cities for the sake of national defense. The building of the interstate highway system was approved by the federal government in the Federal-Aid Highway Act of 1944. The legislation required the plan to have highways "…so located as to connect by routes as direct as practicable, the principal metropolitan areas, cities, and industrial centers, to serve the national defense, and to connect at suitable border points with routes of continental importance in the Dominion of Canada and the Republic of Mexico…."[9]

The argument for the instrument is that the planned location of highways is correlated with the location of current highways but is uncorrelated with unobserved contemporaneous preferences for highway location. Baum-Snow found that in the decades from the 1950s to the 1990s the population in central cities diminished by 17%, despite population growth in metropolitan areas of 72%. The key factor that drove this suburbanization was the construction of interstate highways through important American cities (Baum-Snow 2007). The plan predates my first year of data by 63 years and was meant to link cities for the sake of national defense and not for people to move to the suburbs nor enable (inhibit) their getting colonoscopies in the mid to late 2000s. On the surface, it seems likely that this instrument can address the endogeneity concern embedded in the selection of modern highway locations and their effect on sprawl and health care choices.

I construct my instrument using a map of the 1947 plan which can be seen in Figure B.1 of Appendix C. Using the map, I created a count of highway rays running through the cities plotted on the map and those cities were then associated with the MSA in which they reside. A highway passing through a city is counted as having two rays. If a highway terminates in a city, then it

[9] Federal Highway Administration website, https://www.fhwa.dot.gov/programadmin/interstate.cfm. Last accessed 9/25/2018

contributes one ray. The number of highway rays in an MSA is the sum of all highways passing through or terminating in constituent cities.

Validity challenges arise, however, in cross-sectional analysis using the 1947 highway plan as an instrument. Static, cross-MSA sprawl comparisons must carefully account for ray allocation. An initial concern is that the highway ray assignment is not as good as random. Cities included in the 1947 plan were large population centers. Unaccounted for, this alone serves as a violation of the independence assumption for instrumental variables. Further, variation in population in the 1940s is not a desirable instrument as it is likely correlated with unobserved MSA and other characteristics unrelated to sprawl that affect health care utilization. Its inclusion in the instrument induces a violation of the exclusion restriction as planned highway rays could influence outcomes via pathways other than sprawl.

Conditional on MSA population in the 1940s, however, the number of rays is largely determined by a city's geographic convenience. The total number of planned rays does not depend on the population; rather, it largely depends on a city's geographic location relative to other cities included in the highway plan. To illustrate, consider the cities of Los Angeles, California, Indianapolis, Indiana, and Chattanooga, Tennessee. In 1940, Los Angeles had a population of around 1.5 million, Indianapolis had a population of under 400,000, and Chattanooga had a population of just over 125,000. Los Angeles and Chattanooga were assigned four highway rays. Indianapolis, however, was assigned seven.

Chattanooga is mostly centered between Nashville, Tennessee, Atlanta, Georgia, Knoxville, Tennessee, and Birmingham, Alabama. It is due to this centrality that it receives as many highway rays as Los Angeles, which is a hub connecting Modesto, San Diego, Santa Monica, and San Bernardino. Indianapolis is relatively central to many populous midwestern cities such as

Columbus and Cincinnati, Ohio, St. Louis, Missouri, Louisville, Kentucky, and Chicago, Illinois among others. This happenstance led Indianapolis to be assigned more rays. It is the variation in total rays from some MSAs having more or fewer MSAs arrayed around them that this paper seeks to exploit. Once conditioned on the 1940 population and regional fixed effects, which account for regional differences in total populous cities, this number is largely random and does not rely on 1940 characteristics of an MSA or its population that are likely correlated with health care utilization.

Turning from the independence assumption, the relevance and monotonicity assumptions are simpler to argue. I use a vector of indicators for planned highway rays for each MSA to allow for non-linear effects and to check monotonicity. The first-stage F-statistics hold steady at ten for all outcomes and the coefficient on each indicator is positive. This implies that sprawl increases with the number of rays in agreement with Baum-Snow (2007) and Zhao and Kaestner (2010) and that the monotonicity assumption is not violated.

The final assumption to be discussed is the exclusion restriction. The number of rays assigned to an MSA may be conditionally free of undesirable correlations with 1940 MSA and population characteristics, however, it may have concerning correlations with these characteristics in 2010. If constructed rays led to changes in MSA and population characteristics unrelated to sprawl but correlated with preventive care utilization, then the instrument fails the exclusion restriction. For example, if additional highway ray construction led to a greater prevalence of billboards advertising preventive care and in turn greater awareness of such care's value, then highway rays could affect utilization through population awareness independently of sprawl. While many such violations would be difficult to test, some possibilities may be more feasible for testing.

If differences in highway rays led to differences in per capita park acreage and the use of parks led to greater overall health from exercise, then the cost of becoming ill could rise to make preventive care more appealing. If the gain in overall health requires exercise to maintain and illness prevents exercise, then, while being healthier might reduce the likelihood and loss of health in illness, there is an added cost of deterioration absent maintenance. If the combined health loss due to illness and deterioration is greater than the loss in the absence of gains from exercise, then preventive care is more valuable. This could lead to a change in utilization and a violation of the exclusion restriction all else equal. I assume that such influences are at most minimal, but their potential existence should be acknowledged.

2.5 METHODS

To push toward a causal interpretation of results, I use an instrumental variable specification rather than simple ordinary least squares (OLS). The concern in running a naïve OLS regression is that some unobserved factors or preferences could influence both where one decides to live and one's health care behaviors. Any naively observed relationship between sprawl and preventive care, therefore, could be the result of an unobserved factor or preference and not reveal a true causal relationship. The likely endogenous naïve model is as follows,

$$\Pr(PC = 1)_{ijk} = \beta_0 + \beta_1 sprawl_{jk} + \boldsymbol{\gamma X_{ijk}} + \boldsymbol{\eta Z_{jk}} + \upsilon_k + \mu_{\mathbf{ijk}} \qquad (1)$$

where $\Pr(PC = 1)_{ijk}$ is the probability that individual i in MSA j in census region k has a given preventive care service in a recommended time frame. $Sprawl_{jk}$ is the composite measure of the sprawl of MSA j in region k. $\boldsymbol{X_{ijk}}$ is a vector of individual controls that contains indicators for any health insurance, gender, race, marital status, employment, income, and education category.

Similarly, $\boldsymbol{Z_{jk}}$ is a vector containing controls for doctors per 100,000 MSA residents in 2010[10], MSA level unemployment, and MSA population in 1940. υ_k is a vector of census region fixed effects and $\mu_{\mathbf{ij}}$ is the error term.

My preferred specification is a two-stage least squares instrumental variable specification that is as follows

$$Stage\ 1: Sprawl_j = \boldsymbol{PlannedHwyRays_{jk}\alpha} + \boldsymbol{\gamma X_{ijk}} + \boldsymbol{\eta Z_{jk}} + \upsilon_k + \eta_{\boldsymbol{jk}} \quad (2)$$

$$Stage\ 2: \Pr(PC = 1)_{ij} = \widehat{Sprawl}_{jk} + \boldsymbol{\gamma X_{ijk}} + \boldsymbol{\eta Z_{jk}} + \upsilon_k + \mu_{\mathrm{j}} \quad (3)$$

where ***PlannedHwyRays***$_{jk}$ is the vector of indicators for planned highway rays for MSA j in region k in 1947. $\widehat{Sprawl}_{jk}$ is the predicted composite sprawl measure. This approach avoids concerns of endogenous contemporaneous highway placement and pushes nearer causality as the intent of the highway plan was not to enable health care consumption.

2.6 RESULTS

Table 6 contains the results for the naïve OLS. They largely suggest a standard deviation increase in sprawl reduces the probability of obtaining timely preventive care; though, only significantly for pap smears and mammograms. The OLS results also suggest that increased sprawl significantly reduces the probability of having a personal doctor. They show no significant effect of sprawl on the probability of being obese; however, the coefficient does have an intuitive sign. All of the effects are small in size and represent less than two percent of the respective sample mean.

[10] This comes from the Area Health Resource File for 2010.

Table 7 shows the first stage for the preferred two-stage-least-squares specification. The instrument is strong with an F-statistic just over ten and shows monotonicity. Table 8 displays the results for the IV portion of my preferred specification. The effects are similar in direction to the OLS's results though mostly larger and significant for flu vaccines, pap smears, sigmoid and colonoscopies, and obesity. They suggest that, relative to the mean, a standard deviation increase in sprawl reduces the probability of having a timely colonoscopy by 0.5 percent, a pap smear by 2.1 percent, and increases the probability of having an annual flu vaccine and being obese by 4.8 and 4.9 percent respectively. Significance aside, the results for the OLS and IV specifications consistently suggest that increased sprawl results in less preventive care use and greater obesity.

TABLE 6: SIMPLE OLS REGRESSIONS

VARIABLES	Has Doctor	Checkup	Flu Vaccine	Pap Smear	Breast Exam	Mammogram	Sigmoid/ Colonoscopy	Obese
Sprawl (SD)	-0.00672**	-0.00527	0.00697	-0.0137***	-0.00101	-0.00757*	-0.00255	0.00437
	(0.00337)	(0.00338)	(0.00520)	(0.00456)	(0.00453)	(0.00418)	(0.00193)	(0.00339)
Census Region FE	x	x	x	x	x	x	x	x
Observations	199,784	198,362	195,050	106,204	102,610	76,536	84,863	192,985
Mean	0.857	0.856	0.468	0.833	0.72	0.821	0.952	0.283

Note: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1) clustered at the MSA level. Control variables include indicators for any health insurance, gender, race, marital status, employment, income, education category, doctors per 100,000 MSA residents, MSA level unemployment, MSA population in 1940, and are census region fixed effects.

TABLE 7: FIRST STAGE OF IV

VARIABLES	Sprawl (SD)
1st Ray	0.451
	(0.383)
2nd Ray	0.172
	(0.211)
3rd Ray	1.204***
	(0.236)
4th Ray	0.681***
	(0.182)
5th Ray	1.485***
	(0.282)
6th Ray	1.284**
	(0.539)
7th Ray	0.453
	(0.290)
8th Ray	1.225***
	(0.277)
10th Ray	3.825***
	(0.736)
13th Ray	0.644***
	(0.223)
Census Region 2	0.118
	(0.187)
Census Region 3	0.488**
	(0.188)
Census Region 4	-0.651***
	(0.208)
MSA Pop 1940	-3.49e-07***
	(6.11e-08)
F-Stat	10.12
Observations	199,784

TABLE 8: 2SLS IV REGRESSIONS

VARIABLES	Has Doctor	Checkup	Flu Vaccine	Pap Smear	Breast Exam	Mammogram	Sigmoid/ Colonoscopy	Obese
Sprawl (SD)	-0.00998	-0.00803	0.0226*	-0.0176*	0.000780	-0.00683	-0.00476*	0.0139**
	(0.00644)	(0.00771)	(0.0118)	(0.00924)	(0.00934)	(0.0110)	(0.00287)	(0.00663)
Census Region FE	x	x	x	x	x	x	x	x
Observations	199,784	198,362	195,050	106,204	102,610	76,536	84,863	192,985
Mean	0.857	0.856	0.468	0.833	0.72	0.821	0.952	0.283
F-Stat	10.12	10.09	10.19	10.19	10.31	10.29	10.54	10.13

Note: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1) clustered at the MSA level. Control variables include indicators for any health insurance, gender, race, marital status, employment, income, education category, doctors per 100,000 MSA residents, MSA level unemployment, MSA population in 1940, and are census region fixed effects.

Robustness

In the data section, changes in MSA amenities due to sprawl were mentioned as possible pathways to violations of the exclusion restriction. Such changes could also lead to an endogeneity concern. For example, a change in park acreage could lead to population sorting in which more health-conscious people choose to live in areas with greater per capita park acreage. These populations would likely use more preventive care and differences across MSAs could be driven by population composition. Similarly, if health-conscious individuals prefer sprawled or compact areas, then population sorting over time could create disparities in health care utilization due to preference rather than sprawl.

To examine this possibility, I incorporate a specification similar to one used to examine sorting in Combes et al. (2007). In my specification, two parallel stages lend predicted values to a subsequent final stage. The first part of the parallel step employs essentially the OLS regression in which MSA fixed effects are included in the place of sprawl. The second part of the parallel step is essentially the first stage of the IV which only controls for 1940 MSA population and census region fixed effects.

The goal of the first parallel stage is to extract the common effect of living in an MSA on outcomes, embedded in which is the causal effect of MSA sprawl. By focusing on the estimated effect of sprawl through the MSA fixed effect, I avoid potential unobserved characteristics, such as individual fixed effects, that could bias results in the simple two-stage IV. Within the individual fixed effect could be preferences to live in sprawled areas or in areas conducive to exercise or other healthy behaviors. This specification aims to minimize the influence of such unobserved characteristics since to be in the MSA fixed effect a characteristic or propensity must be shared by all residents. It also preserves the easy interpretation of results for a linear probability model.

The final stage uses predicted MSA fixed effects and sprawl from the two parallel stages to examine the causal relationship between sprawl and preventive care. The specification is as follows

$Stage\ 1_a\colon \Pr(PC = 1)_{ij} = \boldsymbol{X_{ij}\gamma} + \boldsymbol{\eta Z_j} + MSA\_FE_j + \varepsilon_{ij}$ (4)

$Stage\ 1_b\colon Sprawl_{jk} = \boldsymbol{PlannedHwyRays_{jk}\alpha} + MSA\_pop1940_{jk} + \upsilon_k + \mu_{jk}$ (5)

$Stage\ 2\colon \widehat{MSA\_FE}_{jk} = \widehat{Sprawl}_{jk} + MSA\_pop1940_{jk} + \upsilon_k + \eta_{jk}$ (6)

Where $\widehat{MSA\_FE}_j$ is the predicted MSA fixed effect for MSA j in census region k. **PlannedHwyRays**$_j$ is the vector of highway ray indicators and $\widehat{Sprawl}_j$ is the predicted value of sprawl. In stage 1_a MSA population weights are used and robust standard errors are clustered at the MSA level. In stage 1_b and stage 2 the unit of observations is the MSA and robust standard errors. Table 9 shows the results for this specification. The results are similar to the main specification, however, the instrument is somewhat weaker. While the results may be reassuring, they do not comprehensively rule out potential concerns.

Further exploration of the endogeneity concern is limited but does also provide a brief check of the exclusion restriction as well. Using park acreage data extracted from a 2011 publication by the Center for City Park Excellence and The Trust for Public Land, I examine if planned highway rays led to changes in park acreage per 1,000 city residents (City Park Facts 2011). If more planned rays led to changes in park acreage, then this could induce either unwanted sorting or the mentioned violation of the exclusion restriction. Unfortunately, only the 100 largest cities had their park acreage published and many MSAs have multiple constituent cities. Without knowing the acreage of each constituent city, I could not aggregate this data to the MSA level and my analysis was limited at the city level.

Table 10 shows the simple OLS results where park acres per 1,000 city population in 2010 is the outcome and planned rays are the explanatory variable of interest. I control for 1940s population and census region fixed effects. The results show no significant association between planned rays and future park acreage. This is somewhat reassuring; though, all possible violations of the exclusion restriction and endogeneity concerns cannot be ruled out similarly. Nevertheless, this does lend support to a causal interpretation of the preferred specification.

In a companion check of the sorting concern and the independence assumption, I examine whether mean 1940 MSA temperature is predictive of planned highway ray allocation. If warmer or colder MSAs were assigned rays based on how easy or arduous it would be to construct highways in such temperatures, then rays are not assigned randomly even conditional on 1940 MSA population and census region. Further, given historic temperatures' correlation with current temperatures, if planned rays are associated with MSA temperatures, then the sorting concern arises again if populations sort based on weather. Table 11 shows the results for this OLS regression. Mean MSA temperature in 1940 is the outcome and I control for 1940 MSA population and region fixed effects. I do not find mean temperature to be a significant predictor of planned highway rays. This lends additional support to a causal interpretation of the preferred specification.

A secondary concern to those just explored is the sign on flu vaccines. It is consistently positive and suggests that increased sprawl leads to a higher probability of vaccinations. Intuitively, one might expect all preventive service use to decline. However, this increase could be due to the spread of development increasing access to newly built drug stores and grocery stores which can administer flu vaccines. If this is the case and sprawl makes it less convenient to visit medical facilities, then the probability of receiving a vaccination in a traditional medical facility such as a doctor's office, hospital, or clinic should decline while the probability of receiving one in a store should rise.

To investigate this, I use data provided by the BRFSS on flu vaccine location to create binary indicators for whether respondents were vaccinated in a traditional medical facility rather than anywhere else and in a store rather than anywhere else. Table 12 displays the results for two separate regressions. Each indicator was used as the outcome variable in the preferred IV specification. Though imprecise, the results suggest that a standard deviation increase in sprawl reduced the probability of receiving a vaccination in a traditional location and increased the probability of receiving it in a store.

2.7 DISCUSSION

This work contributes to the literature by adding to an area of sparsity and reinforcing previous findings. The methods used here have pushed largely correlational literature closer to causality. In pushing closer to causality, the findings here and those they reinforce increase in value to policymakers. My findings suggest that urban sprawl tends to reduce the likelihood of preventive care use, though, very minorly in some cases. Across various specifications, sprawl consistently had a negative impact on preventive care use, except for flu vaccines. My preferred results suggest a standard deviation increase in sprawl led to a statistically significant reduction in the probability of having a timely colonoscopy or pap smear by 0.5 percent and 2.1 percent respectively. Though imprecise in the preferred specification, across all specifications employed, sprawl is suggested to reduce the probability of having a personal doctor, a biannual checkup, and mammograms. The effect sizes are small and consistently negative even if imprecise.

If forgone screening services lead to higher rates of late-stage cancer or chronic diseases, then greater urban sprawl likely worsens the overall health of MSA populations, all else equal. A caveat, however, is that there are many more health care services, including others that are meant to prevent or treat, which are not considered here. My findings suggest a fairly negative impact of sprawl; however, it would be ill-advised to extrapolate beyond the scope of what is presented here, including for flu shots. The preferred results suggest that greater sprawl increases the likelihood of obtaining a flu vaccine by nearly five percent. A higher probability of receiving a flu shot could either be evidence of desirable health behavior if independent of all other types of care or undesirable if it represents unwanted substitution away from other services.

Consistent with other work, I also find sprawl led to increases in the probability of obesity. Ewing et al. (2003) found that the odds of being obese in a county one standard deviation below mean sprawl levels were only ninety percent of those for one living in a county one standard deviation above the mean sprawl level. Vandegraft and Yoke (2004) find that obesity rates rose with the percentage of developed land. Frank et al. (2004) found an association between increased mixed land use (less sprawl) and decreased obesity. Zhao and Kaestner (2010) suggest significant decreases in population density (more sprawl) led to significant increases in obesity. My findings suggest that a standard deviation increase in composite sprawl, which includes measures of mixed land use and population density, led to a 4.8 percent increase in the probability of being obese.

A key takeaway is that I do not find sprawl to be preventive care nor obesity neutral. My findings point to sprawl largely having a consistently negative effect on preventive care. In conjunction with the wider literature, this suggests that urban policymakers should take health effects into account when making urban form decisions. My findings continue a trend suggesting that sprawl has fairly undesirable effects on health. Again, however, I caution against sweeping statements as the direction of my flu vaccination results suggests there is more to be considered. There may be possible benefits of sprawl that will come to light in future research.

TABLE 9. MSA FEREGRESSIONS

VARIABLES	Has Doctor	Checkup	Flu Vaccine	Pap Smear	Breast Exam	Mammogram	Sig/Colonoscopy	Obese
Sprawl (SD)	-0.0129	-0.00454	0.00448	-0.0115	0.00555	-0.0106*	-0.00516**	0.0106***
	(0.0148)	(0.00290)	(0.0141)	(0.0147)	(0.0103)	(0.00617)	(0.00254)	(0.00220)
Census Region FE	x	x	x	x	x	x	x	x
Observations	204	204	204	204	204	204	204	204
F-Stat	6.3	6.3	6.3	6.3	6.3	6.3	6.3	6.3
Mean	0.857	0.856	0.445	0.821	0.712	0.814	0.945	0.277

Note: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1) clustered at the region level. Control variables include indicators for any health insurance, gender, race, marital status, employment, income, education category, doctors per 100,000 MSA residents, MSA level unemployment, MSA population in 1940, and are census region fixed effects.

TABLE 10. CITY PARK ACREAGE REGRESSION

VARIABLES	Acres Per 1,000 Residents
Planned City Rays	-1.428
	(2.574)
MSA Population 1940	-1.20e-06**
	(5.99e-07)
Census Region FE	x
Observations	95

Note: Robust standard errors in parentheses
(*** p<0.01, ** p<0.05, * p<0.1) clustered at the state level.

TABLE 11. 1940 MSA TEMPERATURE REGRESSION

VARIABLES	Planned Rays
Mean MSA Temperature 1940	-0.0589
	(0.0424)
MSA Population 1940	1.01e-06***
	(2.37e-07)
Census Region FE	x
Observations	205

Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1), clustered at state level.

TABLE 12. FLU VACCINE LOCATION

VARIABLES	Traditional Health Facility	Store (Drug Store, Grocery, etc.)
Sprawl (SD)	-0.0317	0.0179
	(0.0237)	(0.0193)
Observations	85,679	85,679
F-Stat	10.42	10.42

Note: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1) clustered at the MSA level. Control variables include indicators for any health insurance, gender, race, marital status, employment, income, education category, doctors per 100,000 MSA residents, MSA level unemployment, MSA population in 1940, and are census region fixed effects. Model is linear probability and results should be interpreted as percentage point change

CHAPTER III: STRONG SOCIAL DISTANCING MEASURES IN THE UNITED STATES REDUCED THE COVID-19 GROWTH RATE[11]

3.1 INTRODUCTION

A critical question during the COVID-19 pandemic is the effectiveness of the social distancing policies adopted by US states and localities in bending the curve. Although these policies take a variety of forms – such as imposing shelter-in-place orders (SIPOs); restricting dine-in at restaurants; closing other non-essential business such as bars, entertainment venues, and gyms; banning large social gatherings; and closing public schools – their effectiveness depends critically on the cooperation of the public. For example, although California's first-in-the-nation SIPO carries threats of fines and incarceration, its effectiveness fundamentally relies on social pressure (Friedson et al., 2020). Compliance with social distancing orders appears to be related to local income, partisanship, and political beliefs in the US; and compliance with self-quarantines is related to potential losses in income in Israel (Bodas and Peleg, 2020; Painter and Qiu, 2020; Wright et al., 2020).

Some epidemiological models forecast the eventual number of COVID-19 cases and fatalities based on untested assumptions about the impact of social distancing policies in contemporary society. The widely cited Imperial College model notes the impact of social distancing measures will likely vary between countries and even communities. In their modeling of social distancing, they assume all households reduce contact outside the household, school, or workplace by 75 percent, school contact rates are unchanged, workplace contact rates fall by 25 percent, and household contact rates rise by 25 percent (Ferguson et al., 2020). Another study

[11] This chapter is coauthered with Charles Courtemanche, Anh Le, Joshua Pinkston, and Aaron Yelowitz

assumes social distancing measures will reduce the average contact rate by 38 percent, based on evidence from the 1918 influenza pandemic (Thunstrom et al., Forthcoming).

At issue is not whether isolation works to limit the spread of disease, but rather whether the particular government restrictions designed to encourage social distancing in the US reduced spread relative to simply providing information and recommendations. Individuals may voluntarily engage in avoidance behavior, such as hand washing or wearing masks, once they fully perceive the risks of contagion (Abaluck et al., 2020; Harris, 2020). Critics of government measures highlight Sweden's less intrusive response to COVID-19, although Sweden's strategy is increasingly questioned (Reuters, 2020).

The literature on the COVID-19 pandemic in the US is evolving rapidly, and at the time of our writing, we were aware of several working papers using quasi-experimental econometric methods to examine the consequences of social distancing policies. Recent work examined mobility and location trends from Google at the state-level and found significant effects of stronger measures (like SIPOs) on movement using difference-in-differences methods.[10] Similar findings have been found in a study with SafeGraph mobility data (Andersen, 2020), although a different study using PlaceIQ and SafeGraph data found strong measures were not important (Gupta et al., 2020). California's SIPO significantly reduced COVID-19 cases, and a broader analysis of all SIPOs found increased rates of staying home full-time and reductions in COVID-19 cases (Friedson et al., 2020; Dave et al., 2020). Other authors used interrupted time-series methods and found that early statewide social distancing measures were associated with decreases in states' COVID-19 growth rates, but later SIPOs did not lead to further reductions (Siedner et al., 2020).

Our work – which leveraged both state and county policy variation and used a flexible event-study method that allowed for effects to vary across measures and over time – estimated the impacts of four types of social distancing measures on confirmed COVID-19 case growth rates through April 27, 2020. The reduced-form approach captures any potential pathways driven by these mandates, including complementary avoidance behaviors that the public may engage in if these orders provide an informational shock in addition to increasing social distancing.

3.2 DATA

The unit of observation was daily US county/county equivalents. Although there are 3,142 counties in the US, official COVID-19 records report New York City as a whole rather than dividing it into five counties, reducing this number to 3,138. Our dataset tracked counties over 58 days from March 1, 2020 to April 27, 2020, leading to a sample size of 182,004. We chose March 1 as the start date because no new cases were reported in the entire U.S. on most days in January and February. The April 27 end date was chosen to coincide with the first removal of one of four types of restrictions we analyzed (the re-opening of restaurants and other entertainment facilities in Georgia) (Georgia 2020). Each county observation was weighted by population using 2018 estimates from the United States Department of Agriculture's Economic Research Service (U.S. Department of Agriculture, 2019).

We examined the daily growth rate in confirmed COVID-19 cases at the county level, which originated from the 2019 Novel Coronavirus COVID-19 Data Repository provided by the Johns Hopkins Center for Systems Science and Engineering. This repository collected data on COVID-19 cases worldwide from a range of sources including government and independent health institutions (John Hopkins University, 2020).

The daily exponential growth rate was calculated as the natural log of cumulative daily COVID-19 cases minus the log of cumulative daily COVID-19 cases on the prior day. We chose this functional form because epidemiological models predict exponential growth in the absence of intervention. Percent growth in cases is identical to percent growth in cases per capita since reported county populations did not vary during the sample period. The growth rate was multiplied by 100 and can be read as percentage point changes. In computing the growth rate, we followed a recent COVID-19 study and added one to the case counts to avoid dropping counties that started with zero cases (Bursztyn et al., 2020).

The data on the timing of state and local government social distancing interventions was gathered from a host of sources and made available by Johns Hopkins University (Killeen et al., 2020). The appendix explains a few corrections we made to the dates and provides a list of state- and county-level policies used in the analysis.

We focused on four government-imposed interventions: SIPOs, public school closures, bans on large social gatherings, and closures of entertainment-related businesses. For large gatherings, we used the date of the first prohibition that was at least as restrictive as 500 people. Most of the bans were much more restrictive: 95 percent of the time (in our population-weighted sample) the prohibition extended to 50 people. For entertainment-related businesses, we used the date of the first closure of either restaurant dining areas (including bars) or gyms/entertainment centers. 96 percent of the time, if one such prohibition was in place, the other was in place as well.

We included control variables related to the availability of COVID-19 tests. The same Data Repository that provides cases also includes daily counts of positive, negative, and pending tests in each state on each day, which we added together. To mirror our measure of cases, we converted this testing variable to the exponential daily growth rate of cumulative tests performed. In the appendix, we show that the results were robust to the use of other functional forms. Since COVID-19 test results are generally not available immediately, we also included the one-day lag of this growth rate. Further lags (out to 10 days) were considered but always statistically insignificant, so we did not include them. Most states did not report any pending tests, meaning that they did not officially record tests until the results were obtained. This likely explains the lack of a longer lag between testing growth and case growth.

3.3 METHODS

We estimated the relationship between social distancing policies and the exponential growth rate of confirmed COVID-19 cases using an event-study regression with multiple treatments. This approach is akin to difference-in-differences but more flexible, as it interacts the treatment variables with multiple indicators of time since implementation, thereby tracing out the evolution of the treatment effects over time (Saloner and Maclean, 2020).

For each of the four policies, we include seven variables: whether it was implemented 1-5, 6-10, 11-15, 16-20, or more than 20 days ago; and whether it will be implemented 5-9 or 10 or more days later. Implementation on the current day through four days from now was, therefore, the reference group. If a county never adopted the policy, each of these variables was set to 0 throughout the sample period. Our econometric specification is as follows

$$\ln(cases_{cd}) - \ln(cases_{c,d-1})$$
$$= \beta_0 + \sum_{i=-2}^{-1} \beta_{1i} SIPO_{cdi} + \sum_{i=1}^{5} \gamma_{1i} SIPO_{cdi} + \sum_{i=-2}^{-1} \beta_{2i} schools_{cdi}$$
$$+ \sum_{i=1}^{5} \gamma_{2i} schools_{cdi} + \sum_{i=-2}^{-1} \beta_{3i} events_{cdi} + \sum_{i=1}^{5} \gamma_{3i} events_{cdi}$$
$$+ \sum_{i=-2}^{-1} \beta_{4i} entertainment_{cdi} + \sum_{i=1}^{5} \gamma_{4i} entertainment_{cdi}$$
$$+ \sum_{i=0}^{1} \alpha_i \left(\ln(tests_{c,d-i}) - \ln(tests_{c,d-1-i})\right) + county_c + day\_by\_cd_{cd} + \varepsilon_{cd}$$

$cases_{cd}$ contains the cumulative confirmed COVID-19 cases in county *c* on day *d* (with 1 added to prevent the variable from being undefined when the county does not yet have a case). $SIPO_{cdi}$ is an indicator for whether a shelter-in-place order was enacted *i* time periods before day *d* in county *c*, where the time periods are defined as

$$i = \left\{ \begin{array}{c} -2 \equiv 10 \; or \; more \; days \; before \; d \\ -1 \equiv 5 \; to \; 9 \; days \; before \; d \\ 1 \equiv 1 \; to \; 5 \; days \; after \; d \\ 2 \equiv 6 \; to \; 10 \; days \; after \; d \\ 3 \equiv 11 \; to \; 15 \; days \; after \; d \\ 4 \equiv 16 \; to \; 20 \; days \; after \; d \\ 5 \equiv 21 \; or \; more \; days \; after \; d \end{array} \right\}$$

, leaving 0 to 4 days before *d* as the omitted reference range; i.e. the coefficients reflect differences between the predicted growth rate under a policy enacted *i* periods ago versus one that will be enacted within the next four days. $Schools_{cdi}$ is an indicator for whether schools were closed starting *i* periods before day *d* in county *c*. $Events_{cdi}$ is an indicator for whether large events were banned (at least as stringent as 500 people) starting *i* periods before day d in county c. $Entertainment_{cdi}$ is an indicator for whether restaurants or gyms/entertainment centers were closed starting *i* periods before day *d* in county c. $Tests_{cd}$ is the cumulative COVID-19 tests recorded in the state containing county c on day d (with 1 added to prevent the variable from being undefined when the state has not yet run a test); the summation term before the log-difference in tests reflects the fact that the lag of the log difference is also included. $County_c$ is the fixed effect for county c; this captures unobserved county-level variables that do not vary during our sample period, which is why we do not control for county characteristics, such as population density, demographics, or voting behavior, that might influence case growth rates. $Day\_by\_cd_{cd}$ is a fixed effect for day d (ranging from March 1 to April 27, 2020) in the Census Division containing county c; this captures unobserved variables that trend over time, and allows these time trends to vary by Census Division. ε_{cd} is the error term for county c on day d.

An event study model is particularly useful to study the impact of social distancing policies on COVID-19 cases for two reasons. First, after accounting for the incubation period and time between the onset of first symptoms and positive test results, such policies likely only affect official cases after a considerable lag (Lauer et al. 2020). Additionally, the inclusion of variables reflecting future implementation allows for an analysis of pretreatment trends. Since it is not plausible for policies that have not yet been implemented to causally affect current cases, finding such associations could suggest misspecification. For instance, one might expect counties with rapidly growing case counts to be the most likely to enact these measures, leading to a reverse-causal relationship between current cases and future policies that would be detected by our model.

Each policy was implemented at least 10 days after the start of the sample period and at least 20 days prior to the end. Therefore, each policy contributes to the identifying variation for all coefficients except those for more than 20 days ago and 10 or more days from now. Since the estimated policy effects at those two "catch-all" time periods could partially reflect compositional changes, they should therefore be interpreted with more caution than the estimates for the other time intervals.

In addition to the testing controls discussed above, the model also included fixed effects for geography and time. County fixed effects accounted for the likelihood that, even aside from differences in policies, case growth rates may have varied due to a number of county characteristics, including population density and residents' education, political orientation, and age (Painter and Qiu, 2020; Wright et al., 2020). Fixed effects for each day in each of the nine U.S. Census Divisions (522 fixed effects in total) allowed for flexible underlying trends in growth rates that could vary in different parts of the country, helping to account for the staggered nature of the outbreak across locations (United States Census Bureau, Undated). We reported 95% confidence intervals, with standard errors robust to heteroskedasticity and clustered by state, the level of most of the policy variation.

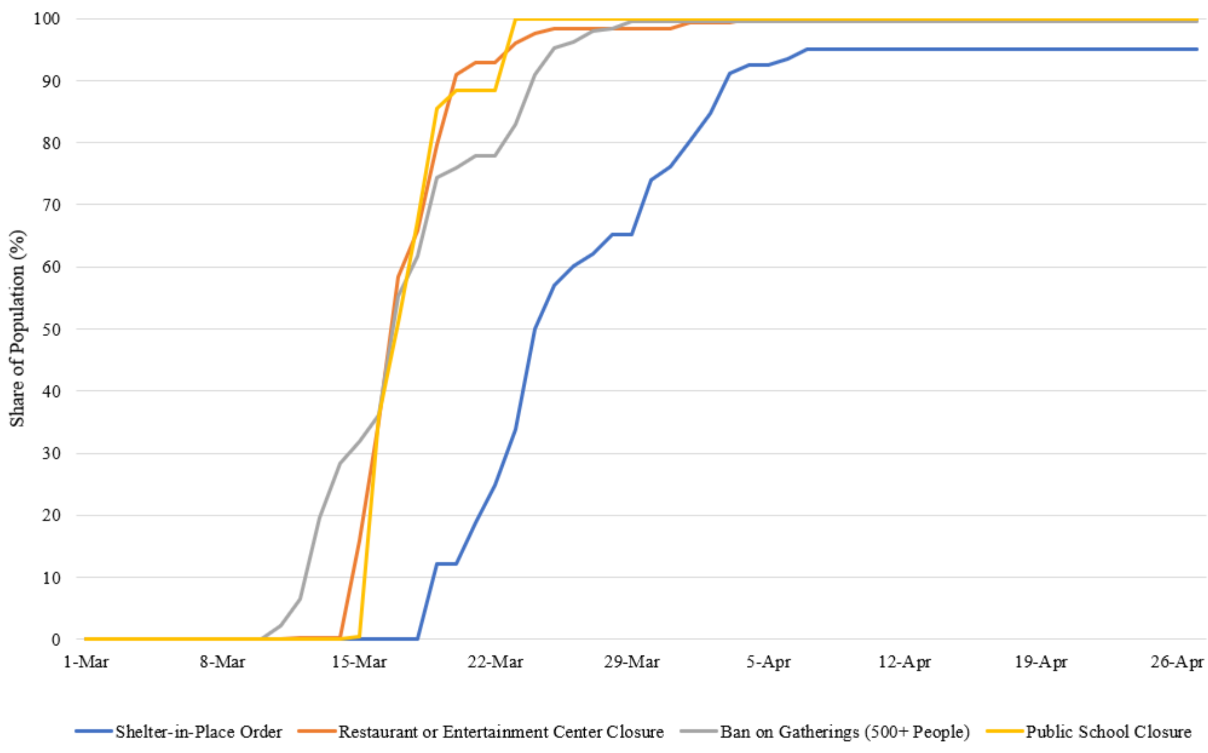
3.4 RESULTS

Descriptive Information

Confirmed COVID-19 cases grew rapidly during the sample period, from just 30 on March 1 to 978,047 on April 27. Appendix D, Figure C.1 shows the number of counties with any COVID-19 cases on each day. On March 1, the vast majority of counties had zero cases, and across all days, 49 percent of unweighted county-by-day observations were zero. However, counties with zero cases tended to have low populations, so our population weights limited the influence of these counties on the results. Moreover, in Appendix J, we showed that our conclusions were not sensitive to handling zeros in different ways other than adding one when computing the logs: using the inverse hyperbolic sine transformation, which is defined at zero, and estimating separate regressions for whether the county had any cases and the exponential growth rate conditional on having cases (Bursztyn et al., 2020).

Figure 8 illustrates the reach of social distancing policies on the US population over time. The SIPO was generally the last policy to be implemented, and adoption was uniformly lower than the other policies. On March 1, no jurisdiction had implemented all four measures. By March 22, nearly 25 percent of the US population was covered by all the measures, growing to approximately 65 percent by March 29 and 95 percent by April 7, when the last SIPO took effect.

FIGURE 8: U.S. POPULATION COVERED BY SOCIAL DISTANCING POLICIES



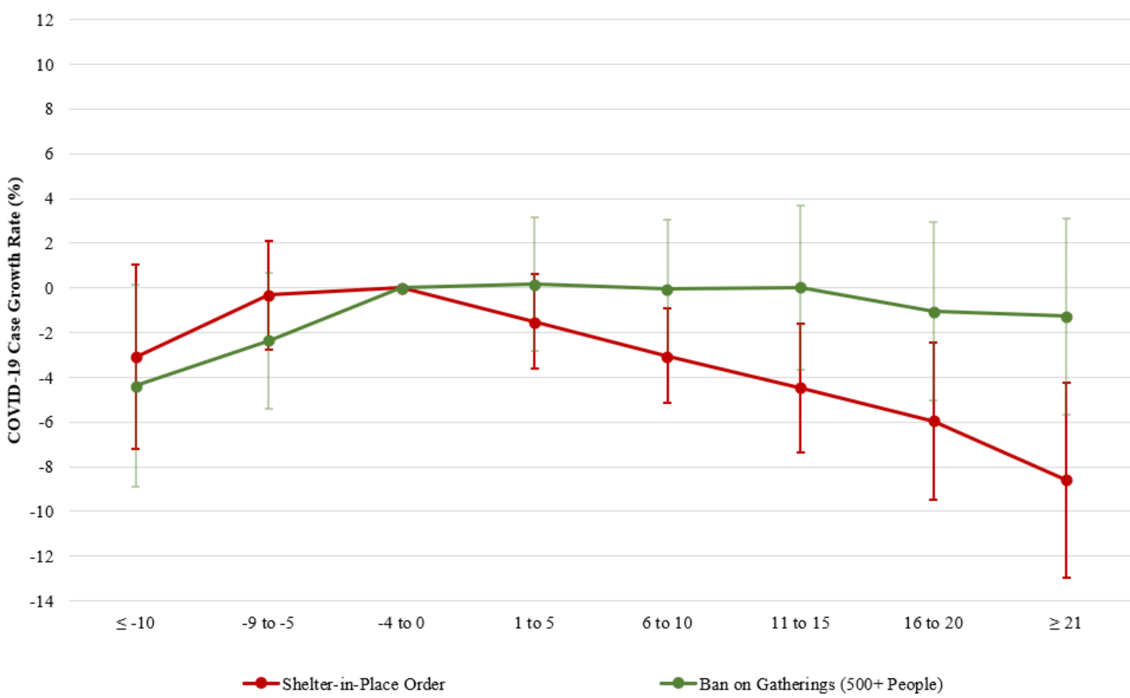
Sources: Authors' calculations from COVID-19 Data by the Johns Hopkins Center for Systems Science and Engineering.
Notes: Estimates are weighted by county population.

Impact of Social Distancing Policies

Figure 9 illustrates the coefficients (and confidence intervals) for SIPOs and bans on large gatherings derived from the event-study model. Relative to the reference category of 0-4 days before implementation, SIPOs lead to statistically significant reductions in the COVID-19 case growth rate of 3.0 percentage points after 6-10 days, 4.5 after 11-15 days, 5.9 after 16-20 days, and 8.6 from day 21 onward. Because the model held constant the other types of policies, these estimates should be interpreted as the additional effect of SIPOs *beyond* shutting down schools, large gatherings, and entertainment-related businesses. This additional effect may come from either the requirement/strong advisement to shelter-in-place aside from “essential” activities or the accompanying closure of any “non-essential” businesses that remained open. We did not observe any statistically significant “placebo” effects of SIPOs in the periods prior to implementation, giving credence to a causal interpretation of our main results. If anything, the pre-trend appears to point upward, which would make our estimates in the post-treatment period conservative.

We found no evidence that bans on large social gatherings influenced the growth rate. The point estimates for banning gatherings were statistically insignificant. However, the 95% confidence intervals included reductions of up to 3-6 percentage points, so the lack of evidence of an effect should not be misinterpreted as clear evidence of no effect. Also, the lack of a statistically significant reduction in the post-treatment period could potentially be due to an upward (though not statistically significant) pre-treatment trend. However, results from the aforementioned event study with separate variables for each day showed that the pre-trend disappeared four days prior to implementation.

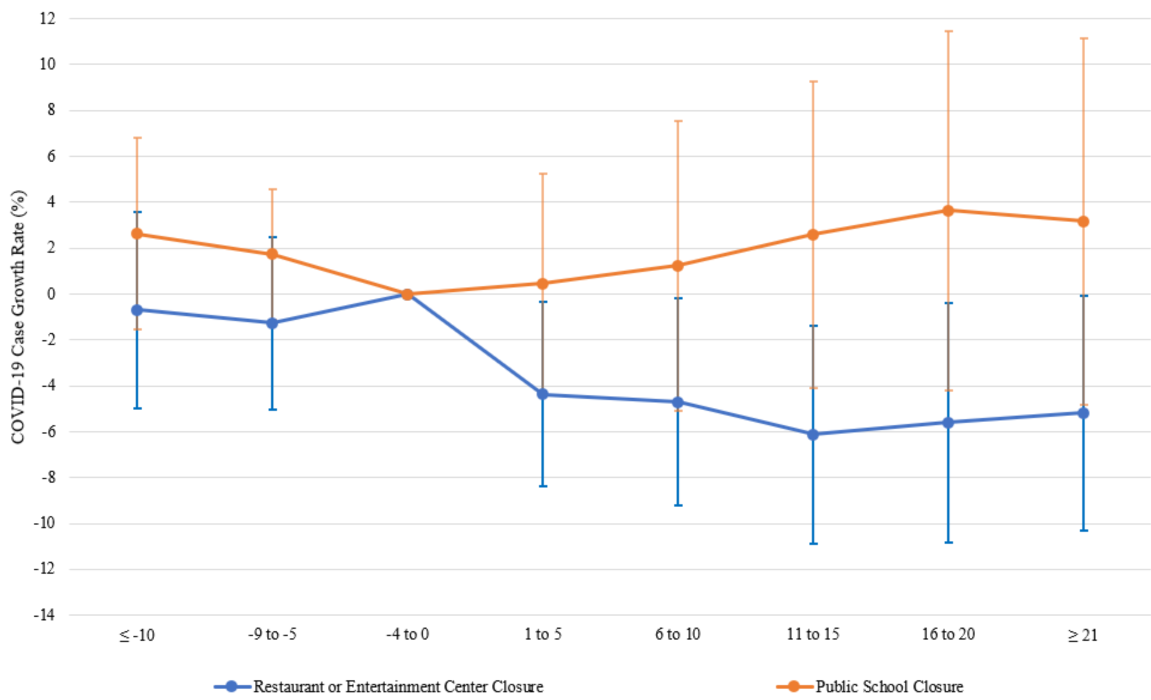
FIGURE 9: EVENT-STUDY MODEL – SIPO AND GATHERING BANS



Sources: Authors' analysis of county-level COVID-19 case data.
Notes: Day, county, and division-by-day fixed effects and testing growth controls were included. Standard errors were heteroskedasticity-robust and clustered by state.

Figure 10 shows estimates for the restaurant-and-entertainment-related businesses and school closings. Closing restaurant dining rooms/bars and/or entertainment centers/gyms led to statistically significant reductions in the growth rate of COVID-19 cases in all time periods after implementation. The estimated effect was 4.4 percentage points after 1-5 days, 4.7 after 6-10 days, 6.1 after 11-15 days, 5.6 after 16-20 days, and 5.2 after 21 or more. Prior to implementation, policies related to businesses showed no effect on the growth rate, again passing the "placebo" test.

FIGURE 10: EVENT-STUDY MODEL – SCHOOL AND ENTERTAINMENT CLOSURES



Sources: Authors' analysis of county-level COVID-19 case data.
Notes: Day, county, and division-by-day fixed effects and testing growth controls were included. Standard errors were heteroskedasticity-robust and clustered by state.

In contrast, we found no evidence that school closures influenced the growth rate. The point estimates were never close to statistically significant, but the 95% confidence intervals meant that we could not rule out reductions of up to 4-5 percentage points.

Adding the coefficient estimates for each policy gives the combined effect of implementing all four social distancing policies. In days 1-5 after implementation, the bundle of restrictions reduced the growth rate of COVID-19 cases by 5.4 percentage points. In days 6-10 after implementation, the growth rate fell by 6.8 percentage points. This reduction grew to 6.8 percentage points after 6-10 days, 8.2 percentage points after 11-15, 9.1 after 16-20, and 12.0 after 21 or more. As discussed previously, the estimate for 21+ days should be viewed with caution, as it did not utilize the same geographic balance of treatments as the estimates for the other time intervals. A conservative interpretation of these results would therefore be that the impact reached 9.1 percentage points after 16-20 days and appeared to remain at least as high after that.

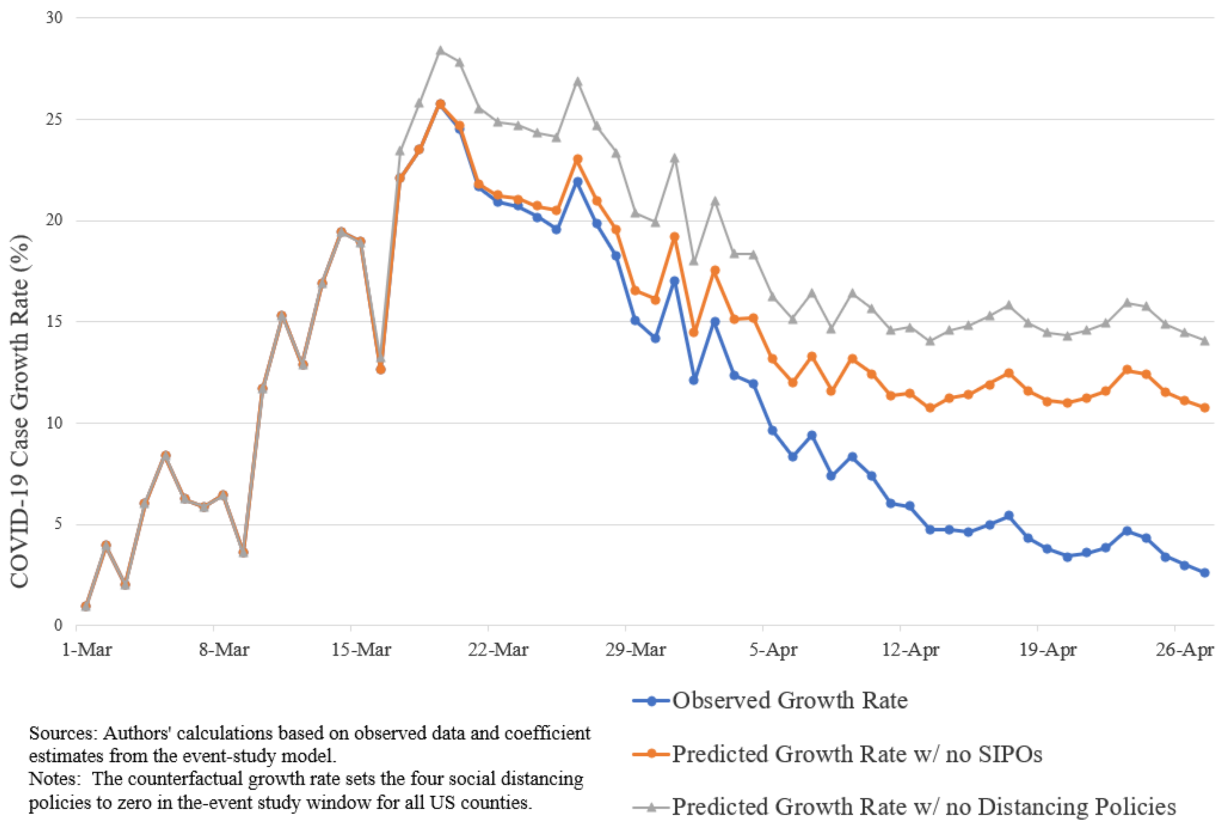
Counterfactual Simulations

Figure 11 compares the observed growth rate of COVID-19 cases to two counterfactuals: 1) none of the four social distancing measures ever being imposed and 2) no SIPO ever being imposed. The process for creating these counterfactuals is described in this chapter's appendix discussion. The mean exponential growth rate without any interventions was 16.2 percent over the full time period. The observed and both counterfactual growth rates peaked on March 19, 2020 at 26-28 percent but started to diverge afterward, eight days after the earliest restriction. Without any social distancing policies, the case growth rate would have stayed similarly high for another week before gradually falling to 14 percent by April 27, 2020. Without SIPOs – but keeping the other restrictions – the growth rate would have fallen to 11 percent. The actual growth rate, which reflects all implemented distancing policies including SIPOs, fell to 3 percent by that date.

Figure 12 compares the reported number of COVID-19 cases over time to the number of cases predicted by our event-study regression under these same two counterfactual scenarios. Again, see this chapter's appendix discussion for details. The graph uses the natural logarithm of nationwide cases (or predicted cases) for the y-axis scale, but with corresponding numbers labeled on the y-axis instead of logs.

In all three scenarios, cases increased roughly linearly on the log scale, as expected under exponential growth, until the last week of March – approximately two weeks after the first restrictions and one week after the first SIPO. The actual curve then began to flatten substantially, eventually leading to 978,047 cases by April 27. In contrast, the two counterfactual curves only

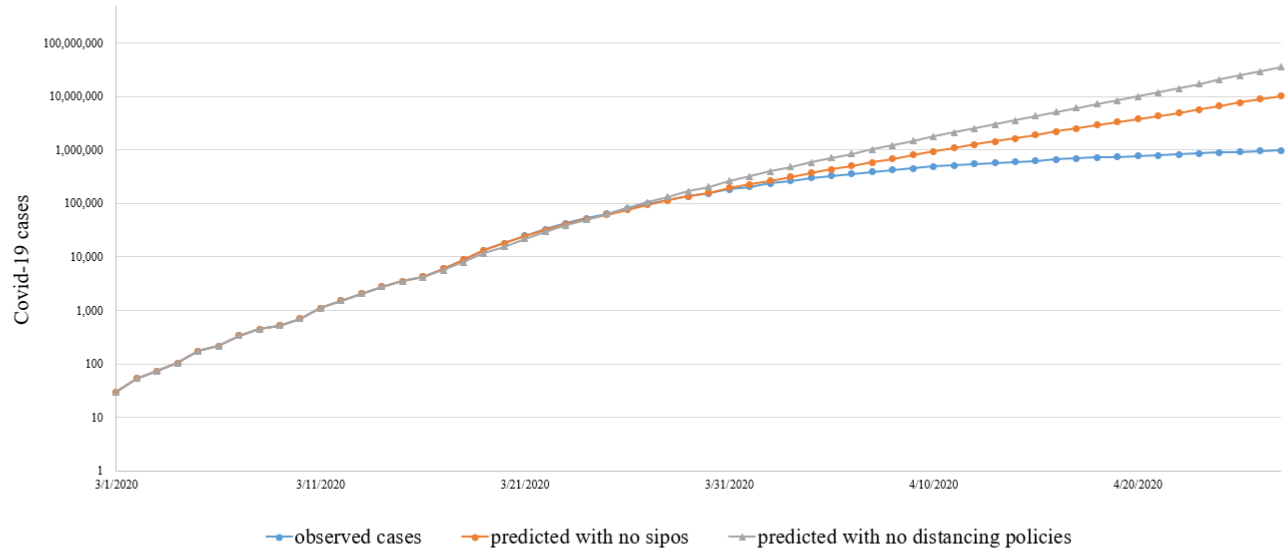
FIGURE 11:OBSERVED GROWTH RATE AND ESTIMATED GROWTH RATES



Sources: Authors' calculations based on observed data and coefficient estimates from the event-study model.
Notes: The counterfactual growth rate sets the four social distancing policies to zero in the-event study window for all US counties.

flattened slightly. By the end of the sample period, the model predicts that cases would have been 10 times higher without SIPOs (10,224,598) and 35 times higher (35,257,098) without any social distancing restrictions. Interestingly, the closures of restaurants/entertainment facilities accounted for a larger share of the reduction in cases than SIPOs, despite SIPOs having larger coefficient estimates. This is because restaurant/entertainment facilities were implemented earlier and in more places than SIPOs. With exponential growth, several days can make a substantial difference.

FIGURE 12: SOCIAL DISTANCING POLICIES FLATTEN THE CURVE (LOG SCALE)



Robustness Checks Related to Multicollinearity

Our event-study specification included 24 different treatment variables (6 for each of the 4 policies) as well as Census-Division-by-day fixed effects, raising the question of whether there was sufficient identifying variation to precisely identify the impacts of the different policies. In particular, this could conceivably have explained the null results for public school closures and large event bans. Our first two robustness checks, therefore, utilized a simpler specification that included only a single variable for each policy type: whether the policy was in effect 10 days ago in the first regression and 20 days ago in the second. (The lag allowed for incubation periods, testing delays, etc.) Also, we included day fixed effects rather than their interactions with each of the nine Census Divisions. This means that policy differences across Census Divisions were allowed to contribute to the identifying variation, whereas the baseline model relied only on within-Census-Division variation.

The results are in Table C.3 of Appendix H . We observed the same pattern of results as we did in our main model: SIPOs had the largest statistically significant effect, restaurant/entertainment closures had the second largest, and large event bans and school closures had no discernable impacts.

Robustness Checks Related to Testing

We performed a series of checks aimed to ensure that the results were not driven by the way in which we controlled for testing. Specifically, we were concerned about two issues. First, the baseline model's functional form for tests was the daily log difference in cumulative tests (as well as its first lag), which mirrored our specification for the outcome variable, effectively assuming that a given percentage increase in testing corresponded to a given percentage increase in cases. However, it is possible that the relationship between testing and cases is better characterized by a different functional form. Second, the number of tests performed depends not only on availability but also on the severity of the outbreak. The coefficient estimates for the testing variables were therefore likely biased, and bias could have spilled over to the coefficient estimates for the policy variables.

Accordingly, we run four checks. The first three use different functional forms for the testing variables: 1) ln(new tests) – i.e. the log of the difference rather than the difference in the logs, 2) new tests (not logged) per 1,000 residents, and 3) cumulative tests run to date. The fourth check simply excludes testing variables completely. The results, presented in Table C.4 of the Appendix I alongside the corresponding estimates from the baseline model, show that testing was indeed positively associated with cases, but our coefficient estimates of interest were remarkably stable across the different specifications. While none of the testing measures are perfect, this stability increased our confidence that differential trends in access to testing across counties did not drive our conclusions.

Robustness Checks Related to Timing

Our next series of robustness checks related to the subjective nature of the start date of the sample. For our baseline results, we began the sample period on March 1. We excluded prior days because there were no new reported cases in the U.S. on most days in January in February. We did not start later in March because using the entire month allowed for ten days prior to the implementation of any policy with which we could examine pre-trends. However, arguments could be made for either earlier or later start dates. On one hand, an earlier start date would utilize more data and allow for a longer pre-treatment period. On the other hand, the earlier the start date, the more counties started the sample with no cases, exacerbating any potential issues created by adding one to the log of cases to prevent the outcome from being undefined.

Our first two checks simply started the sample a half month earlier and later: Feb. 15 and March 15, respectively. The third check used all available data, meaning a Jan. 22 start date. The fourth check used this longer period but avoided adding one to the log of cases by running two separate regressions: 1) one with the full sample but an indicator for any cases as the outcome (linear probability model due to the bias inherent in nonlinear fixed effects models), and 2) one with exponential growth rate as the outcome, but without adding one to the log of cases when creating the variable, thereby restricting the sample to counties with cases on both the current and previous days.[12] Next, we return to the original March 1 start date and avoid adding one to the log

[12] Note that this is not exactly equivalent to the conventional two-part model, which would use a dichotomized version of the continuous outcome as the dependent variable in the first part. In our setting, that would mean using the original outcome variable (with one added to the log of cases), and conditioning the sample for the second part on having a non-zero growth rate, meaning counties with cases but no growth in the previous day would also be

of cases a different way: by using the inverse hyperbolic sine transformation, which is interpretable in the same manner as natural logs but defined at zero.[13]

Appendix J, Table C.5 displays the results. Across all start dates, we continued to find that SIPOs and restaurant/entertainment center closures – but not the other two policies – reduced the growth rate of cases. Interestingly, the results from the two-part specification suggested that SIPOs reduced COVID-19 spread along both the extensive margin (delaying the arrival of the first case in the county) and the intensive margin (slower spread once the first case was recorded), whereas restaurant/entertainment center closures did so only along the intensive margin. The results from the regression using the inverse hyperbolic sine specification showed this more directly, as those results were virtually identical to those from the baseline model.

State Inclusion and Other Miscellaneous Robustness Checks

Our next group of checks asked whether results were robust to dropping particular, unique locations: New York, the hardest-hit state; Washington, the first state to be hit; and California, which had the nation's largest population and earliest shelter-in-place order. Appendix K contains the results, which show that dropping any of these states has little impact on the estimates.

We close with robustness checks related to other miscellaneous issues. First, we estimate a more fine-grained event study model that included indicators for each day from 10 days before a policy took effect to 20 days after, rather than grouping them into 5-day increments. Appendix L Table C.6 displays the results. Not surprisingly, the estimates were less precise than those

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dropped. Given these distinctions, it is not obvious to us how one might compute combined marginal effects that encompass both parts of the model, so we do not do so.

[13] For further detail, see John B. Burbidge, Lonnie Magee and A. Leslie Robb. "Alternative Transformations to Handle Extreme Values of the Dependent Variable." *Journal of the American Statistical Association* Vol. 83, No. 401 (Mar., 1988), pp. 123-127.

obtained using 5-day groupings, and the wider confidence intervals generally made the results less informative. In particular, we no longer observed significant effects for restaurant/entertainment center closures, despite most of the point estimates remaining roughly similar to those obtained using the 5-day intervals. The confidence intervals included reductions as large as 8 or more percentage points, making the lack of statistical significance uninformative. In contrast, the confidence intervals for SIPOs remain smaller, and most of those estimates remain statistically significant. One useful result from this check is that the apparent upward pre-treatment trend for gathering bans was concentrated among the first half of the pre-treatment window and largely disappeared four days before treatment. This increased our confidence that the failure to find a causal effect of these bans was not due to confounding from pre-trends.

3.5 DISCUSSION

It is important that readers view the results in Figure 5 as a means of illustrating the estimated effectiveness of social distancing restrictions at "flattening the curve", as opposed to literal predictions about alternate histories if policymakers had not taken action. Had they not done so and COVID-19 had continued to spread in the manner depicted by our simulations, voluntary social distancing by individuals and businesses would have likely increased as panic over the rising death toll and hospital overcrowding across the country mounted, offsetting some of the additional predicted cases. In technical terms, the Census-Division-by-day fixed effects in the later portion of the sample period would likely have evolved differently. Regardless, avoiding these startling numbers would have required stringent social distancing in one form or another, whether through government restrictions or private choices. Economic theory suggests that private choices would not likely have slowed the spread as much, as individuals' prevention efforts have substantial benefits to others (positive externalities) that are not factored into their decision-making.

Relatedly, testing shortages would likely have prevented official case counts from reaching the numbers presented in our counterfactual simulations. However, this is largely a semantic distinction, as these infections would still be severe enough to warrant testing in the absence of a shortage. If anything, not being confirmed as a COVID-19 case could lead to inadequate treatment.

As striking as our counterfactual estimates are, they still are not worst-case scenarios because they account for at least some voluntary social distancing. Even without any government restrictions, Figure 4 illustrated a 14.3 percentage point drop from the peak growth rate to the end of the sample period. The most plausible explanation is the responses of individuals and businesses to information about the severity of the pandemic and federal guidelines.

While our results suggest both SIPO and non-SIPO measures can be effective at averting COVID-19 cases, the lack of evidence of effects of school closures or bans on large social gatherings is noteworthy. We cannot rule out the possibility that these null results are due to statistical imprecision, but it is also possible that both policies may displace social interaction rather than reducing it. For example, school closures may have led families to continue social interactions outside of the school setting, such as at daycare centers or parks. Google mobility data through April 5, 2020 show increases of 10 percent or more in visits to parks in 28 states (Google, 2020). A new study finds that schools are only slightly more dangerous than parks and playgrounds for COVID-19 transmission, supporting this explanation (Benzell et al., 2020). Alternatively, school closures primarily affect children and the vast majority of children experience mild symptoms and therefore may not be included in confirmed cases (Editorial, 2020). While asymptomatic children can pass the virus to adults who become more severely ill, our results imply that the extent to which this led to confirmed cases did not change when schools were closed.

Similarly, official group events may have simply been replaced by informal gatherings. Alternatively, official prohibitions may have been largely redundant since the largest events (such as college and professional sports) were already being cancelled due to CDC guidance or other information. Also note that school closures and large event bans occurred prior to the implementation of SIPOs, meaning substitute types of social gatherings were still allowed. Our results, therefore, should not be interpreted as a forecast about what would happen if schools were reopened or certain large gatherings were allowed while other aspects of SIPOs remained in place.

Limitations

There were several limitations to our analysis. Official COVID-19 case counts are known to understate the true prevalence of the disease, as they do not include asymptomatic carriers, those who are not ill enough to seek medical care, and those who are unable to obtain a test due to supply constraints (Friedson et al, 2020). Nonetheless, confirmed case counts are crucial to the Trump administration's "Opening Up America Again" plan, which proposed a "downward trajectory of documented cases within a 14-day period" or "downward trajectory of positive tests as a percent of total tests within a 14-day period (flat or increasing volume of tests)" as criteria to loosening social distancing measures (The White House, 2020). Moreover, to the extent that testing shortages led to only the sickest individuals receiving them, official case counts can loosely be interpreted as the prevalence of moderate-to-severe illnesses, a relevant metric for policy purposes.

A related caveat is that, ideally, we would like to be able to control more precisely for access to testing. Available data only allowed us to control for the number of tests performed at the state, rather than the county level. However, most of our policy variation is at the state level, so state-level testing should go a long way towards alleviating bias. Additionally, the number of tests performed depends not only on their availability but also the level of illness in the community, making it endogenous. Our estimated policy effects remain virtually identical if we drop the testing variables (see appendix), providing reassurance that this limitation did not meaningfully affect our conclusions.

Also, we might ideally want to estimate a richer econometric model. It would be interesting to trace out the timing of impacts more exactly, study the policies' interactions with each other or county characteristics, and examine the impacts of other social distancing policies such as closing public parks and beaches, the requirement to wear masks in public, restrictions on visitors in nursing homes, informal state announcements of first cases or fatalities, and federal government actions such as prohibiting international travel (Gupta et al., 2020). However, it is difficult to include numerous correlated treatment variables without reducing precision to the point where statistical inference is uninformative. A robustness check in the appendix illustrates this by showing that the estimates generally become much less precise than the ones we report below if we include separate indicators for each day from 10 days before treatment to 20 days after. One could ask if even our five-day windows were too demanding of the available policy variation, but other appendix robustness checks show that simpler specifications do not reveal new insights.

Finally, as is typical of observational data analyses, we cannot rule out all possible threats to causal inference. Numerous possible confounders could vary across time and space, including the other policies mentioned above, informal encouragement by government officials to wear masks or improve hygiene, changing business practices, and social norms regarding distancing. That said, including Census-Division-by-day and county fixed effects in our model and examining pretreatment trends helped us to push in the direction of causality. We show in the appendix that the results were robust to the inclusion of county-specific pre-treatment trends, further supporting a causal interpretation of the results.

3.6 CONCLUSION

We estimated the separate and combined impact of four widely adopted social distancing policies. Both SIPOs and closures of restaurants/bars/entertainment-related businesses substantially slowed the spread of COVID-19. We did not find evidence that bans on large events and closures of public schools also did, though the confidence intervals cannot rule out moderately sized effects. Two recent papers on the effect of the same government social distancing restrictions on mobility found the same pattern of results, suggesting a plausible mechanism (Abouk and Heydari, 2020; Andersen, 2020).

Our contribution was to provide credible empirical evidence on whether US social distancing measures worked as intended in flattening the curve. Estimating other important benefits and costs from social distancing, including the total lives saved and economic harm, was beyond the scope of our study. Other work has attempted to estimate job losses, simulate effects on the overall economy and economic growth, or estimate distributional consequences from current and past pandemics (Friedson et al., 2020; Thunstrom et al., 2020; Scherbina, 2020; Hall et al, 2020l Greenstone and Vishan, 2020; Correia et al., 2020).

Nonetheless, we provided important information about the benefits of social distancing for policymakers to consider as they decide on strategies for restarting economic activity. For instance, our results suggest that returning to partial measures such as school closures and restrictions on large gatherings, while removing the restrictions that prevent the redirection of social activity to other settings, would be ineffective in curbing the spread of the virus. At issue moving forward is whether cases averted simply turn into cases delayed, and a premature return to light measures would make this more likely.

APPENDIX A

TABLE A1: CROSS-STATE EVENT STUDY - COUNT OF ENTRIES PER 100K STATE POPULATION

Variables	All Doctors	Primary Care	Other Specialties
Ever Expanded x 2011	0.0200	-0.0317	0.0365
	(0.2165)	(0.1206)	(0.1097)
Ever Expanded x 2012	-0.0047	0.0219	-0.0274
	(0.1626)	(0.0865)	(0.0926)
Ever Expanded x 2014	-0.0639	-0.0905	-0.0422
	(0.1084)	(0.0887)	(0.0625)
Ever Expanded x 2015	-0.2598	-0.0979	-0.1780+
	(0.2555)	(0.1455)	(0.1054)
Ever Expanded x 2016	-0.2280	-0.1772	-0.0800
	(0.3076)	(0.1857)	(0.1320)
State FE	x	x	x
Year FE	x	x	x
Observations	240	240	240

Standard errors in parentheses
+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$

APPENDIX B

TABLE A2: 5, 10, AND 20 MILE RADII

5 MILE RADIUS

Variables	All Docs (5 mi.)	Primary Care (5 mi.)	Other. Spec. (5 mi.)
Ever Expanded x 2011	1.6806	4.6303	-0.3015
	(2.8880)	(3.8421)	(4.4893)
Ever Expanded x 2012	-0.0858	3.2931	-2.4525
	(1.6811)	(3.9219)	(2.3592)
Ever Expanded x 2014	7.3972**	5.8144	8.7365*
	(2.6298)	(5.2654)	(3.4214)
Ever Expanded x 2015	9.9623**	11.0290	9.7751*
	(3.6631)	(7.2478)	(4.3225)
Ever Expanded x 2016	16.0180**	15.1352+	16.6085**
	(5.7948)	(8.7538)	(5.9260)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30243	12353	17890

10 MILE RADIUS

Variables	All Docs (10 mi.)	Primary Care (10 mi.)	Other. Spec. (10 mi.)
Ever Expanded x 2011	1.4864	1.8702	1.2702
	(1.8002)	(4.1620)	(2.6823)
Ever Expanded x 2012	-0.6244	-0.7464	-0.7631
	(1.3824)	(3.4513)	(1.7984)
Ever Expanded x 2014	3.3059	-0.9717	6.2627**
	(2.0864)	(4.2267)	(1.8855)
Ever Expanded x 2015	3.6282	1.2990	5.3685+
	(2.5247)	(4.8459)	(2.8503)
Ever Expanded x 2016	5.6478	1.1541	8.5811*
	(3.5751)	(5.4068)	(3.3689)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30254	12360	17894

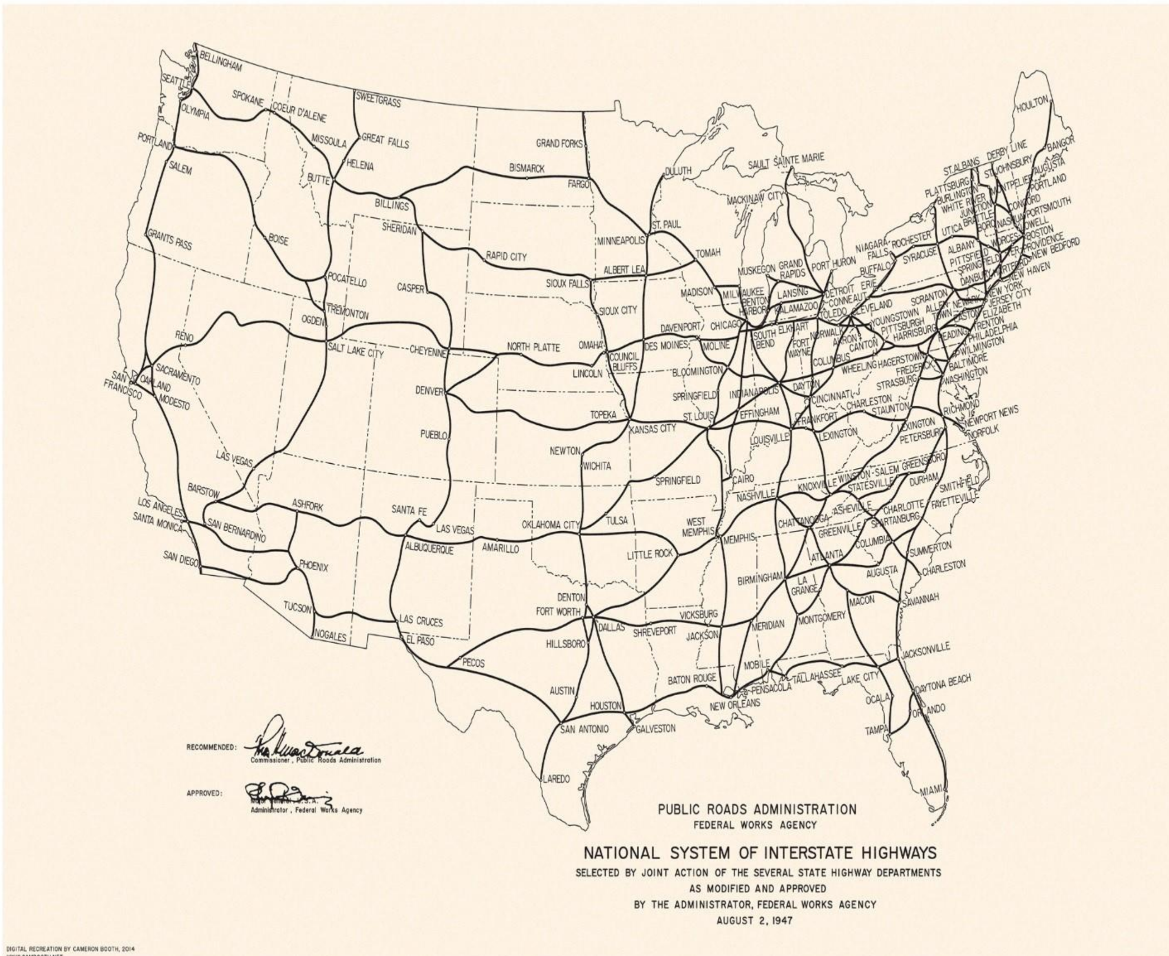
20 MILE RADIUS

Variables	All Docs (20 mi.)	Primary Care (20 mi.)	Other. Spec. (20 mi.)
Ever Expanded x 2011	1.3386	-1.6270	3.2467
	(2.2221)	(3.1000)	(2.2520)
Ever Expanded x 2012	0.0424	0.4133	-0.2586
	(1.8196)	(2.5933)	(1.6895)
Ever Expanded x 2014	1.9127	0.1213	3.2514+
	(2.2648)	(3.6750)	(1.7011)
Ever Expanded x 2015	2.7150	2.7372	2.9532
	(2.4834)	(3.3404)	(2.4928)
Ever Expanded x 2016	3.7445	2.2655	5.1846+
	(3.1365)	(4.0849)	(3.0323)
Year FE		x	x
State FE		x	x
Year x Doctor Type FE	x		
State x Doctor Type FE	x		
Observations	30255	12361	17894

+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001

APPENDIX C

FIGURE B1: MAP OF THE 1947 INTERSTATE HIGHWAY PLAN



Sprawl Index Factor Construction

The density factor is constructed from measures of gross density of urban and suburban census tracts, percentages of populations living in low suburban densities and in medium to high urban densities, urban density based on the National Land Cover Database, and a gross density variable similar to the first but derived from data from the Local Employment Database (LED)[14]. The use of the LED means that Massachusetts is excluded from the index as data for that state was not obtained in the LED. Thus, any conclusion drawn from these measures of sprawl cannot necessarily be generalized to MSAs in Massachusetts.

The mixed-use factor is calculated from three variables: geography-wide, average job-population balance, geography wide degree of job mixing, and a geography-wide, average Walk Score. The first two were calculated at the census block group level using the 2010 Census and LED data at the block level. The first examines the relative ratio of jobs to residents within a given radius. The second looks at variation in job types by industry in a similar fashion. The third was calculated from Walk Score, Inc. data and measures the proximity to various amenities and how accessible they are on foot. Finer detail on the calculations of these variables can be found in the cited report; though these details are for the county level calculation, the same principles were applied to MSAs[15].

The urban centering factor provides a measure of economic and population centeredness within metropolitan areas. In their words "urban centers are concentrations of activity that provide agglomeration economies, support alternative modes and multipurpose trip making, create a sense of place in the urban landscape, and otherwise, differentiate compact urban areas from sprawling ones."[16] The variables constructing the centering measure are variation in population density, variation in employment density[17], percentage of the population in the central business district or

[14] Ewing R. and Hamidi S., "Measuring Sprawl and Validating Sprawl Measures", Report Prepared for the NCI, NIH, Ford Foundation, and Smart Growth America, (2014) p. 12. Available at https://gis.cancer.gov/tools/urban-sprawl/, last accessed 9/25/2018
[15] Ewing and Hamidi, Op. Cit. p. 13-14.
[16] Ewing and Hamidi, Op. Cit. p. 14.
[17] Greater variation around the mean is taken to imply greater centering.

economic sub-centers, and percentage of employment in the central business district or sub-centers.

The street accessibility factor is comprised of measures of the average block size (excluding rural blocks larger than a square mile), percentage of small urban blocks (less than 1/100 if a mile in size), intersection density, and percentage of four or more-way intersections. These variables are calculated with the use of TomTom GPS data.[18] Again, finer detail on the calculation of these variables and factors can be found in the already cited report. The street factor offers a measure of how connected and easy to navigate a metropolitan area is.

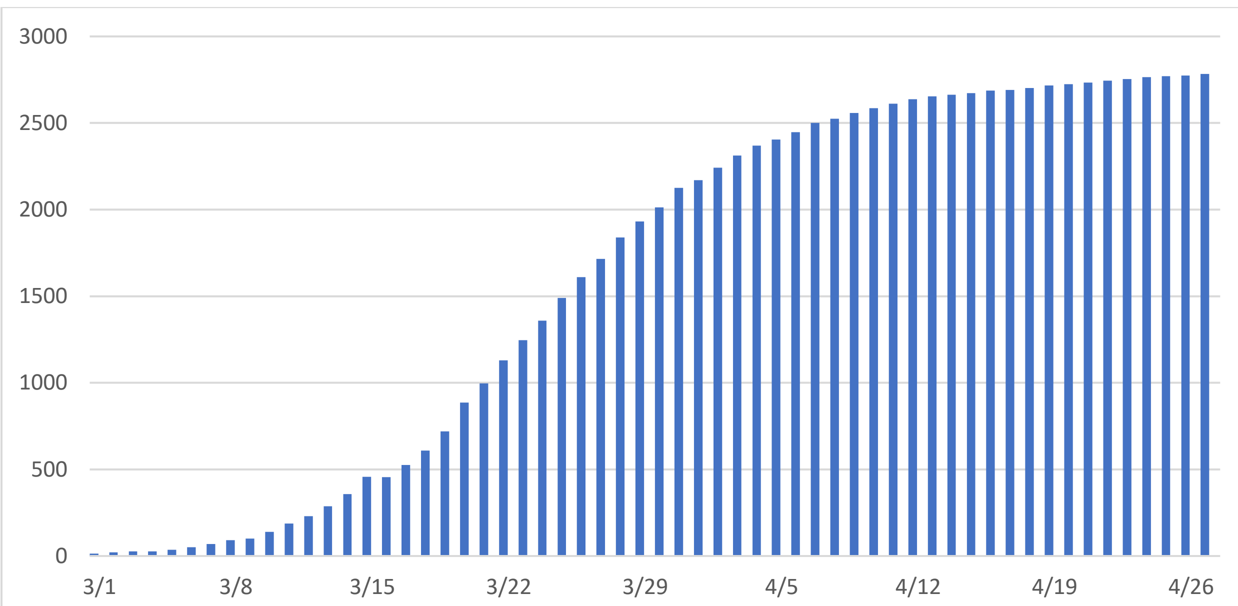
[18] Ewing and Hamidi, Op. Cit. p.17.

APPENDIX D

FIGURE C1: NUMBER OF U.S. COUNTIES WITH CONFIRMED CASES

<u>Diffusion of COVID-19 during Sample Period</u>

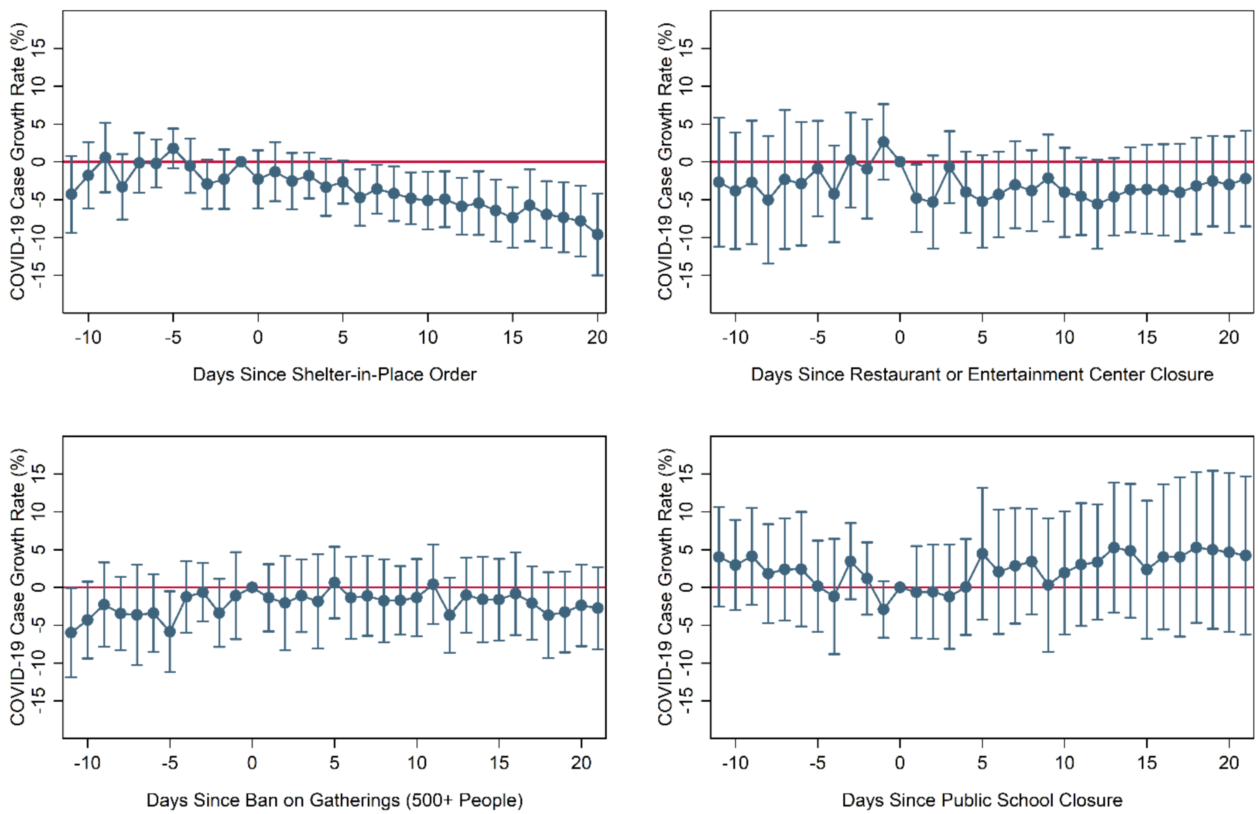
Appendix Figure C.1 below shows the growth in the number of counties with confirmed COVID-19 cases during our sample period of March 1 through April 27, 2020. These are simple counts, not adjusted for population. 0.4% of counties had a COVID-19 case on 3/1, but 11% of the US population lived in those counties. Similarly, more than half of US residents lived in counties with a case by March 13, even though just 9% of counties had a case. By April 1, 97% of residents lived in counties with a case, compared to 71% of the unadjusted number of counties.



Note: All cases for Bronx, Kings, Queens, and Richmond counties in NY were lumped into New York county in the source data.

APPENDIX E

FIGURE C2: EVENT-STUDY MODEL INCLUDING DAY INDICATORS



APPENDIX F

TABLE C1: EFFECTIVE DATES OF STATE SOCIAL DISTANCING MEASURES

State	Shelter in Place	No Gatherings of 500+	Public Schools Closed	Rest./ Ent./Gym Order
AL	4/4	3/13	3/16	3/19
AK	3/28	3/24	3/19	3/17
AZ	3/31	3/17	3/16	3/17
AR		3/26	3/17	3/19
CA	3/19	3/19	3/19	3/15
CO	3/26	3/13	3/23	3/17
CT	3/23	3/23	3/17	3/16
DE	3/24	3/24	3/16	3/12
DC	4/1	3/25	3/16	3/16
FL	4/3	3/17	3/16	3/20
GA	4/3	3/17	3/18	3/20
HI	3/25	3/25	3/23	3/20
ID	3/25	3/25	3/23	3/23
IL	3/21	3/18	3/17	3/18
IN	3/25	3/13	3/19	3/17
IA		3/17	4/2	3/17
KS	3/30	3/17	3/18	3/24
KY	3/26	3/20	3/16	3/16
LA	3/23	3/17	3/16	3/17
ME	4/2	3/18	3/15	3/18
MD	3/30	3/13	3/16	3/16
MA	3/24	3/17	3/17	3/17
MI	3/24	3/24	3/16	3/16
MN	3/28	3/27	3/18	3/17
MS	4/3	3/25	3/20	3/25
MO	4/6	3/21	3/19	3/21
MT	3/28	3/28	3/16	3/20
NE		3/19	3/19	4/4
NV	4/1	3/24	3/16	3/20
NH	3/28	3/24	3/16	3/16
NJ	3/21	3/16	3/18	3/16
NM	3/24	3/18	3/16	3/18
NY	3/22	3/13	3/18	3/16
NC	3/30	3/12	3/16	3/17
ND			3/16	3/19
OH	3/24	3/15	3/17	3/15
OK		3/29	3/17	4/1
OR	3/23	3/16	3/16	3/17
PA	4/1	3/23	3/16	3/17
RI	3/28	3/16	3/16	3/23
SC	4/7	3/18	3/16	3/18
SD			3/16	3/23
TN	4/1	3/25	3/20	3/23
TX	4/2	3/14	3/23	3/19
UT		3/12	3/16	3/18
VT	3/25	3/25	3/18	3/17
VA	3/30	3/24	3/16	3/16
WA	3/23	3/11	3/17	3/17
WV	3/24	3/24	3/16	3/24
WI	3/25	3/17	3/18	3/17
WY		3/20	3/20	3/19

Adjustments for Likely Data Errors

The original county-by-day cases data contained some likely reporting errors. First, the COVID-19 case counts on Sunday, March 15, 2020 were identical to those from March 14, 2020 in all large counties that had cases at the time, even though substantial growth was reported on March 16, 2020. This is almost certainly due to lack of reporting on that particular Sunday rather than a true lack of new cases. We therefore imputed the cases for March 15, 2020 so that growth from March 14, 2020 to March 16, 2020 was evenly split between the two days.

Additionally, 964 of the 182,004 county-by-day observations reported a reduction in cases from the previous day, which is implausible since case counts were cumulative. Approximately half of these were cases where the count increased by one on one day and then decreased by one the next. In such cases, the original increase was likely an error (e.g. presumed case that was later found to be negative, duplicate, or person who lived in a different county/state), so we removed the initial increase so that the case count remained constant over the three days. We set remaining instances of negative growth rates to zero. As shown later in this appendix, our results were virtually unchanged if we simply dropped observations with case reductions as well as all observations on March 15, 2020.

We also corrected some apparent errors in the social distancing policy dates. Many Kansas counties had reported dates for gathering bans that were after the date for the statewide ban, so we changed those dates to match the statewide ban.[19] We removed the reported SIPO in Wyoming, which was listed as March 28, 2020, as sources such as the Kaiser Family

[19] The counties were Allen, Anderson, Atchison, Barber, Barton, Bourbon, Brown, Butler, Chase, Chautauqua, Cherokee, Cheyenne, Clark, Clay, Cloud, Coffey, Comanche, Cowley, Crawford, Decatur, Dickinson, and Doniphan.

Foundation[20] as well as various news articles[21] listed the state as not having any SIPO in place after the date listed. Some school closing dates also appeared incorrect. School closing dates were checked against those listed at edweek.org[22]. If there was more than a day mismatch, then a third source was found to decide the date used, leading to changes for Alabama[23], Alaska[24], Iowa[25], and Wyoming[26]. We changed the SIPO start date for MS[27] and the gathering ban date for KY[28] as they were incorrect in the source data. The SIPOs for IN, MN, NH, OH, and TN had an effective start time of 11:59 pm. We changed the start day for these states to the following day as they were in effect for only one minute on their actual start date. Appendix Tables 2 and 3 below show the state and county (if different from the state) policy dates we ultimately used.

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[20] See: https://www.kff.org/health-costs/issue-brief/state-data-and-policy-actions-to-address-coronavirus/
[21] See: https://www.usnews.com/news/best-states/wyoming/articles/2020-03-30/wyoming-governor-still-no-plan-for-state-stay-at-home-order, https://abcnews.go.com/Health/9-states-issue-formal-stay-home-orders-amid/story?id=69959039
[22] See: https://www.edweek.org/ew/section/multimedia/map-coronavirus-and-school-closures.html
[23] See: https://www.al.com/news/2020/03/alabama-closes-all-k-12-schools.html
[24] See: https://www.adn.com/alaska-news/education/2020/03/16/alaskas-statewide-school-closure-is-about-to-begin-no-one-knows-quite-how-it-will-work/
[25] See: https://www.usnews.com/news/best-states/iowa/articles/2020-03-16/iowa-schools-closing-taking-coronavirus-cue-from-governor
[26] See: https://trib.com/news/local/education/school-districts-across-wyoming-announce-closures-following-governors-recommendation/article_333c5100-c6a6-57f1-b04b-948546b59ffd.html
[27] See: https://www.sos.ms.gov/Content/documents/ed_pubs/Exec%20Orders/1466.pdf
[28] See: https://governor.ky.gov/attachments/20200319_Order_Mass-Gatherings.pdf

APPENDIX G

TABLE C2: EFFECTIVE DATES OF COUNTY SOCIAL DISTANCING MEASURES

State	County	Date Issued	State	County	Date Issued
AL	Jefferson	3/26	MO	Randolph	3/25
AK	Anchorage	3/22	MO	Ray	3/25
CO	Adams	3/24	MO	St. Louis	3/23
CO	Arapahoe	3/24	PA	Allegheny	3/23
CO	Archuleta	3/24	PA	Beaver	3/28
CO	Boulder	3/24	PA	Berks	3/27
CO	Denver	3/24	PA	Bucks	3/23
CO	Douglas	3/24	PA	Butler	3/27
CO	Eagle	3/24	PA	Centre	3/28
CO	Grand	3/24	PA	Chester	3/23
CO	Jefferson	3/24	PA	Delaware	3/23
CO	La Plata	3/24	PA	Erie	3/24
CO	Pitkin	3/24	PA	Lackawanna	3/27
CO	San Miguel	3/24	PA	Lancaster	3/27
FL	Alachua	3/25	PA	Lehigh	3/25
FL	Broward	3/27	PA	Luzerne	3/27
FL	Duval	3/25	PA	Monroe	3/23
FL	Hillsborough	3/27	PA	Montgomery	3/23
FL	Leon	3/25	PA	Northampton	3/25
FL	Miami-Dade	3/25	PA	Philadelphia	3/23
FL	Monroe	3/30	PA	Pike	3/27
FL	Orange	3/26	PA	Washington	3/28
FL	Osceola	3/26	PA	Wayne	3/27
FL	Palm Beach	3/30	PA	Westmoreland	3/27
FL	Pinellas	3/26	PA	York	3/27
GA	Chatham	3/24	TN	Davidson	3/25
GA	Dougherty	3/21	TN	Franklin	3/25
GA	Fulton	3/24	TX	Austin	3/24
KS	Johnson	3/24	TX	Bell	3/24
KS	Leavenworth	3/24	TX	Bexar	3/24
KS	Sedgwick	3/25	TX	Brazos	3/24
KS	Wyandotte	3/24	TX	Collin	3/24
MS	Lafayette	3/22	TX	Dallas	3/23
MS	Lauderdale	3/31	TX	El Paso	3/24
MO	Boone	3/25	TX	Fort Bend	3/24
MO	Cass	3/24	TX	Galveston	3/24
MO	Clay	3/24	TX	Harris	3/24
MO	Cole	3/23	TX	McLennan	3/24
MO	Greene	3/26	TX	Tarrant	3/24
MO	Jackson	3/24	UT	Salt Lake	3/30
MO	Jefferson	3/24	UT	Summit	3/27
MO	Platte	3/24			

APPENDIX H

TABLE C3: ESTIMATED EFFECT OF SOCIAL DISTANCING POLICIES ON THE GROWTH RATE OF COVID-19 CASES IN MODELS WITH A SINGLE VARIABLE FOR EACH POLICY

	10-Day Lag	20-Day Lag
Shelter-in-Place Order in Effect	-3.997 (0.705)*	-4.106 (0.631)*
Restaurant/Gym/Entertainment Closures	-2.417 (0.726)*	-1.951 (0.797)*
No Large Events	-0.486 (1.020)	-0.953 (1.136)
Public Schools Closed	1.705 (1.062)	0.168 (0.903)

Notes: Sample size = 182,004 county-by-day observations. * indicates statistically significant at the 5% level; $^{+}$ 10% level. Standard errors, in parentheses, are heteroskedasticity-robust and clustered by state. Observations are weighted by county population. Regressions include the growth rate in cumulative tests in each of the past two days as well day and county fixed effects.

APPENDIX I

TABLE C4: ROBUSTNESS CHECKS RELATED TO DIFFERENT WAYS OF CONTROLLING FOR TESTING

	Baseline	ln(New Tests)	New Tests per 1,000	ln(Cumula-tive Tests	No Control for Tests
Shelter-in-Place Order					
1-5 Days Ago	-1.514	-1.643	-1.641	-1.639	-1.633
6-10 Days Ago	-3.033*	-3.229*	-3.098*	-3.165*	-3.131*
11-15 Days Ago	-4.482*	-4.671*	-4.530*	-4.658*	-4.630*
16-20 Days Ago	-5.950*	-6.155*	-5.977*	-6.132*	-6.104*
21 or More Days Ago	-8.600*	-8.905*	-8.564*	-8.821*	-8.781*
Restaurant/Gym/Entertainment Closures					
1-5 Days Ago	-4.372*	-4.415*	-4.349*	-4.500*	-4.335*
6-10 Days Ago	-4.710*	-4.805*	-4.751*	-4.878*	-4.713*
11-15 Days Ago	-6.125*	-6.151*	-6.088*	-6.293*	-6.031*
16-20 Days Ago	-5.594*	-5.595*	-5.588*	-5.861*	-5.539*
21 or More Days Ago	-5.177*	-5.149*	-5.156*	-5.500*	-5.135*
No Large Events					
1-5 Days Ago	0.172	0.264	0.258	0.260	0.227
6-10 Days Ago	-0.061	0.033	0.130	-0.010	0.125
11-15 Days Ago	0.013	0.055	0.144	-0.053	0.109
16 or More Days Ago	-1.041	-1.039	-0.888	-1.140	-0.933
21 or More Days Ago	-1.272	-1.269	-1.127	-1.404	-1.164
Public Schools Closed					
1-5 Days Ago	0.304	0.170	0.002	0.298	0.109
6-10 Days Ago	1.004	0.928	0.683	1.149	0.864
11-15 Days Ago	2.443	2.406	2.157	2.751	2.432
16 or More Days Ago	3.465	3.475	3.179	3.868	3.505
21 or More Days Ago	3.095	3.173	2.812	3.591	3.232
Testing Variable for Day t	0.010^{+}	0.291*	0.005*	1.235*	--
Testing Variable for Day t-1	0.012*	-0.064	0.005*	-0.676	--

See notes for Table C.3.

APPENDIX J

TABLE C5: ROBUSTNESS CHECKS RELATED TO SAMPLE START DATES

	Start on 2/15	Start on 3/15	Start on 1/22	Start on 1/22; Any Cases as Outcome	Start on 1/22; Counties with Cases Only	Start on 3/1; Inverse Hyperbolic Sine
Shelter-in-Place Order						
1-5 Days Ago	-1.144	-0.783	-0.994	-0.024*	-1.045	-1.844
6-10 Days Ago	-2.332*	-1.736*	-2.028*	-0.059*	-2.117*	-3.456*
11-15 Days Ago	-3.412*	-2.652*	-2.929*	-0.090*	-2.898*	-4.794*
16-20 Days Ago	-4.517*	-3.626*	-3.847*	-0.112*	-3.666*	-6.052*
21 or More Days Ago	-6.541*	-5.480*	-5.536*	-0.143*	-5.239*	-8.727*
Restaurant/Gym/Entertainment Closures						
1-5 Days Ago	-4.060*	-5.779*	-3.936+	0.011	-6.778*	-4.821*
6-10 Days Ago	-4.164+	-6.406*	-3.952+	0.002	-6.242*	-5.123*
11-15 Days Ago	-5.372*	-8.016*	-5.081*	0.010	-8.085*	-6.556*
16-20 Days Ago	-4.648+	-7.825*	-4.302+	0.027	-7.694*	-5.997*
21 or More Days Ago	-4.067+	-7.849*	-3.683	0.046	-7.549*	-5.556*
No Large Events						
1-5 Days Ago	0.176	0.528	0.213	0.028	-1.713	0.007
6-10 Days Ago	-0.082	0.185	-0.011	0.036	-2.107	-0.509
11-15 Days Ago	-0.012	0.553	0.089	0.035	-2.300	-0.302
16-20 Days Ago	-1.077	-0.317	-0.941	0.033	-3.802	-1.414
21 or More Days Ago	-1.349	-0.300	-1.177	0.030	-4.260	-1.568
Public Schools Closed						
1-5 Days Ago	-0.070	0.260	-0.188	-0.062	1.909	1.033
6-10 Days Ago	0.231	0.334	-0.0004	-0.085	2.656	2.153
11-15 Days Ago	1.335	1.137	1.025	-0.095	3.390	3.956
16-20 Days Ago	2.090	1.786	1.732	-0.107	3.851	5.160
21 or More Days Ago	1.495	1.038	1.116	-0.115	2.774	5.032
Sample Size	229,074	138,072	298,110	298,110	93,245	182,004

See notes for Table C.3.

APPENDIX K

TABLE C6: ROBUSTNESS CHECKS DROPPING STATES

	Drop NY	Drop WA	Drop CA
Shelter-in-Place Order			
1-5 Days Ago	-1.320	-1.564	-1.475
6-10 Days Ago	-2.853*	-2.987*	-2.770*
11-15 Days Ago	-4.248*	-4.616*	-4.531*
16-20 Days Ago	-5.681*	-6.069*	-5.940*
21 or More Days Ago	-8.336*	-8.776*	-8.523*
Restaurant/Gym/Entertainment Closures			
1-5 Days Ago	-4.691*	-4.991*	-5.068*
6-10 Days Ago	-5.189*	-5.293*	-5.220*
11-15 Days Ago	-6.645*	-6.621*	-6.430*
16-20 Days Ago	-6.313*	-6.271*	-6.111*
21 or More Days Ago	-5.947*	-5.875*	-5.603*
No Large Events			
1-5 Days Ago	-0.051	0.483	0.609
6-10 Days Ago	-0.414	0.723	0.719
11-15 Days Ago	0.354	0.850	0.700
16 or More Days Ago	-0.388	-0.004	-0.239
21 or More Days Ago	-0.230	-0.050	-0.356
Public Schools Closed			
1-5 Days Ago	0.923	0.644	0.521
6-10 Days Ago	1.285	2.084	2.368
11-15 Days Ago	2.500	3.614	3.445
16 or More Days Ago	3.442	4.851	4.697
21 or More Days Ago	2.808	4.665	4.398
Sample Size	178,640	179,742	178,640

See notes for Table C.3.

APPENDIX L

TABLE C7: MISCELLANEOUS ROBUSTNESS CHECKS

	Bans on 50 Instead of 500	Restau-rants and Gyms, not or	Drop Ques-tionable Data	Fixed effects for days since first case	Control for Pre-Trend
Shelter-in-Place Order					
1-5 Days Ago	-1.733	-1.393	-1.402	-2.361*	-1.525
6-10 Days Ago	-3.189*	-2.800*	-2.912*	-4.176*	-3.038*
11-15 Days Ago	-4.732*	-4.302*	-4.333*	-5.867*	-4.461*
16-20 Days Ago	-6.262*	-5.780*	-5.775*	-7.47*	-5.888*
21 or More Days Ago	-8.871*	-8.351*	-8.383*	-10.414*	-8.438*
Restaurant/Gym/Entertainment Closures					
1-5 Days Ago	-4.581*	-3.797*	-4.144*	-3.218^{+}	-4.407*
6-10 Days Ago	-4.905*	-3.068	-4.427^{+}	-5.351*	-4.762*
11-15 Days Ago	-6.612*	-4.642*	-5.740*	-6.575*	-6.186*
16-20 Days Ago	-6.330*	-3.988	-5.159^{+}	-7.109*	-5.656*
21 or More Days Ago	-5.970*	-3.357	-4.666^{+}	-7.092*	-5.221*
No Large Events					
1-5 Days Ago	1.971	-0.046	0.188	0.208	0.172
6-10 Days Ago	1.333	-0.787	-0.073	0.488	-0.055
11-15 Days Ago	1.451	-0.684	-0.008	0.643	0.030
16 or More Days Ago	1.085	-1.602	-1.064	-0.358	-1.012
21 or More Days Ago	0.280	-1.800	-1.316	-0.191	-1.218
Public Schools Closed					
1-5 Days Ago	0.649	0.080	0.315	3.207	0.356
6-10 Days Ago	1.921	0.736	0.933	1.717	1.092
11-15 Days Ago	3.521	2.151	2.367	1.396	2.544
16 or More Days Ago	4.882	3.244	3.335	0.641	3.567
21 or More Days Ago	4.798	2.881	2.933	-0.030	3.176
Sample Size	182,004	182,004	182,004	182,004	182,004

See notes for Table C.3.

Creation of Counterfactuals in Figures 11 and 12

In Figure 11, we plot the daily growth rate of COVID-19 cases in the U.S. under three scenarios. The first is actual case growth rates, which reflect the presence of all four social distancing measures when and where they were implemented. The second is counterfactual growth rates when we assume no jurisdictions ever implemented a SIPO but did implement the other measures the same way they did in reality. Formally, this is done by subtracting out the effect of SIPOs from the actual growth rate so that

$$counterfactual1_{cd} = \ln\left(cases_{cd}\right) - \ln\left(cases_{c,d-1}\right) - \widehat{\gamma_1}\sum_{i=1}^{5} SIPO_{cdi}.$$

The third is the counterfactual when we assume no jurisdictions ever implemented any social distancing restriction:

$$\begin{aligned} counterfactual2_{cd} &\\ &= \ln\left(cases_{cd}\right) - \ln\left(cases_{c,d-1}\right) - \widehat{\gamma_1}\sum_{i=1}^{5} SIPO_{cdi} - \widehat{\gamma_2}\sum_{i=1}^{5} schools_{cdi} \\ &- \widehat{\gamma_3}\sum_{i=1}^{5} events_{cdi} - \widehat{\gamma_4}\sum_{i=1}^{5} entertainment_{cdi} \end{aligned}$$

Note that we do *not* also subtract out the "placebo" effects of the future policy implementation variables, since those are intended to capture unobserved confounders rather than part of the causal effect of the policies. After obtaining the counterfactuals, we aggregate all three growth rate variables to the nation-by-day level by weighting each county-by-day observation by county population.

Figure 12 builds on Figure 11 to predict the number of cases under each counterfactual scenario. Note that $\ln\,(cases_{cd}) - \ln\big(cases_{c,d-1}\big) = \ln\,(\frac{cases_{cd}}{cases_{c,d-1}})$ implies that

$$cases_{cd} = cases_{cd-1} \exp\left[\ln\left(\frac{cases_{cd}}{cases_{c,d-1}}\right)\right].$$

With a predicted growth rate, $\ln\,(\frac{\widehat{cases}_{cd}}{cases_{c,d-1}})$, this becomes

$$\widehat{cases}_{cd} = \widehat{cases}_{cd-1} \exp\left[\ln\left(\frac{\widehat{cases}_{cd}}{cases_{c,d-1}}\right)\right] \widehat{\exp(\varepsilon_{cd})}, \tag{A1}$$

where $\widehat{\exp(\varepsilon_{cd})}$ is the average of $\exp(\widehat{\varepsilon_{cd}})$ in the estimation sample.[29]

Therefore, predicting the number of cases under each scenario requires the estimated residual from our regression and predicted values of $\ln\left(\frac{cases_{cd}}{cases_{c,d-1}}\right)$ under the counterfactuals discussed above. Each of these estimates is then plugged into equation (A1) to predict counterfactual cases in county/day observations following the start date of a relevant policy. Prior to a policy beginning in a given county, the "counterfactual" cases are no different from the observed cases. Once the number of cases is predicted in each period under a given counterfactual, we sum the observed and predicted cases by day to create nation-by-day totals for observed and counterfactual cases. When these sums are presented on a logarithmic scale, as they are in Figure 12, the natural log must be calculated again after summing each variable.

[29] Don M. Miller (1984) Reducing Transformation Bias in Curve Fitting, The American Statistician, 38:2, 124-126, DOI: 10.1080/00031305.1984.10483180

This calculation is subject to several technical caveats that together mean the results should be interpreted as rough approximations rather than exact numbers. There is always uncertainly with calculations based on underlying parameters that are estimated, and it is difficult to accurately quantify the amount of this uncertainty. In this particular calculation, errors can compound over time, as $\widehat{cases}_{cd-1}$ in equation (A1) is itself an estimate. Even when $\widehat{cases}_{cd}$ is predicted from observed policies and using observed cases in $d - 1$, the predicted value is larger than $cases_{cd}$ on average in our sample. Additionally, the average difference is always positive later in the sample. To err on the side of caution and avoid having these errors accumulate over time, we discount our counterfactual predictions based on the average ratio of observed to predicted cases whenever the predicted cases (based on observed policies) are greater than observed cases on average.

We also err on the side of caution in one other way. When calculating the predicted case counts for each counterfactual, we set the coefficient on the variables indicating a policy start date 21 or more days in the past ($i = 5$ in the summation above) equal to the corresponding coefficient for 16-20 days. As discussed above and in the manuscript, our sample period includes up to 20 days after each policy. This means there is geographic balance for the variables reflecting up to 20 days, enabling apples-to-apples comparisons. However, the comparability of the estimates for the 21-or-more-day indicators is less clear, as earlier SIPOs remain in that category longer than more recent SIPOs. It is therefore not clear that the additional effect of SIPOs observed after 21+ days relative to 16-20 days is a "true" increase as opposed to simply the artifact of the compositional changes. We therefore make the conservative assumption that the true effect peaked and remained constant after 16 days. Without this adjustment, the estimated cases with no social distancing policies rise from ~35 million to ~50 million.

An assumption that pushes in the direction of overstatement is that the pool of potentially infectable individuals is fixed over time. This ignores immunity from fighting off the virus. By the end of the sample period, with nearly a tenth of the US population predicted to have been sick enough to reach the threshold for a confirmed case, this assumption may not be benign. However, debate continues over the extent to which exposure to the virus conveys immunity, so the exact implications of this assumption are not clear.

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VITA

Joseph Anthony Garuccio was born in Louisville, Kentucky on November 3rd, 1988. He received his Bachelors of Science in Economics from the University of Louisville, graduating with high honors. He earned his Masters of Arts and Doctor of Philosophy in Economics from Georgia State University during which time he was a granted a university fellowship. In his time as a student and to this very day, Joseph has exhibited a strong preference to keep any discussion of himself to a minimum. He is fond of the Henry Kaiser quote “If your work speaks for itself, don’t interrupt.”