Essays in Public and Health Economics

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Essays in Public and Health Economics

Presented by Karen Shen

candidate for the degree of Doctor of Philosophy and hereby certify that it is worthy of acceptance.

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Date: ___April 30, 2021___

Essays in Public and Health Economics

A dissertation presented
by

Karen Shen

to

The Department of Economics

in partial fulfillment of the requirements
for the degree of
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Essays in Public and Health Economics

Abstract

This dissertation contains three chapters, each with the goal of providing evidence to better inform policymaking around challenges facing a different segment of the health care system in the United States.

In Chapter 1, I study the home care industry for seniors with significant functional difficulties. This industry has seen dramatic growth over the past few decades, spurred in part by increased public financing through Medicaid. I investigate the effects of a policy that increased the use of formal home care among Medicaid-eligible seniors by more than 50%. I show that this expansion of formal home care primarily serves to replace informal care from family members, particularly spouses and daughters. For daughters, the decrease in care supplied is accompanied by an increase in labor supply: for every 2.4-3 women whose parent receives formal home care as a result of this policy, one additional daughter works full-time.

In Chapter 2 (with Eric Barrette and Leemore Dafny). I investigate the effects of two policies intended to increase access to and quality of treatment for people with opioid use disorder (OUD): insurance parity requirements, and supply-side policy increasing the number of medication-assisted treatment (MAT) providers. Neither policy significantly increases the probability that patients with OUD receive any treatment, but both policies shift patients' choices of treatment, in opposing directions. While parity causes substitution away from MAT, the standard of care, increases in the county-level share of physicians able to prescribe agonists are associated with substitution toward medication-assisted treatment.

In Chapter 3, I study facility-level determinants of infection and mortality at nursing homes during the first wave of the COVID-19 pandemic. I find that staff neighborhood characteristics are strongly associated with facility deaths per bed: one standard deviation increases in average staff neighborhood population density, public transportation use, and non-white share were associated with 1.3, 1.4, and 0.9 additional deaths per 100 beds, respectively. These effect sizes exceed the effect of all facility management variables and even facility size, suggesting the importance of controlling staff community transmission to lessen the devastation in nursing homes.

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To my parents

Chapter 1

Who Benefits From Public Financing of Home Care for Low-Income Seniors?

1.1 Introduction

Most governments of advanced economies spend a significant share of their budgets on basic care for young children, the disabled, and the frail elderly. One motivation for this spending is to help ensure the health and well-being of these vulnerable populations. However, a second motivation for these programs is often to reduce the burden on unpaid caregivers (who are disproportionately women) and potentially to lessen the well-documented negative effect of caregiving obligations on labor force participation and labor supply. While this benefit has been studied extensively in the case of childcare (summarized in Section 1.1.1), there have been significantly fewer studies on the effect of public spending on eldercare on caregiver labor supply, particularly in the United States. This paper aims to fill that gap.

An estimated 70% of people who turn sixty-five will need long-term care at some point in their lifetime (Johnson, 2017). In the United States, this care has historically been provided mostly by unpaid family members, with nursing homes serving as a last resort for those with severe needs or without access to other sources of care. The government's role has traditionally been limited to to paying for nursing home stays through Medicaid once a patient's other resources have been exhausted (roughly 60% of all nursing home stays are covered by Medicaid). However, the past few decades have seen the emergence of a formal home care industry that offers an alternative to both institutional care and family care. Today, this industry employs over two million aides and generates $135 billion in revenue each year.[1] This growth reflects at least in part Medicaid policy reforms that have dramatically expanded Medicaid coverage of home and community-based long-term care. Compared to just $7.5 billion in 1990, Medicaid spent more than $86 billion on home and community-based long-term care in FY2015 (constant 2015 dollars; Eiken *et al.*, 2017; Wenzlow *et al.*, 2016).

This paper considers the impacts of this public spending. By using a source of Medicaid policy variation that generates large variation in rates of home care use across states among Medicaid-eligible seniors, I estimate the impact of expanded access to home care on seniors, their families, and government revenues. The incidence of such a policy will depend crucially on how seniors and their families respond to the availability of home care—whether they use the program to delay or avoid nursing home use, to supplement care from informal caregivers, or to substitute for informal care. To explore these issues, I use data from the Health and Retirement Study that allows me to observe the care choices, income, health and functioning of a representative sample of seniors from 2000-2016. I leverage two main sources of variation in this data: (1) policy variation across state Medicaid programs in whether they have chosen to amend their Medicaid State Plan to include personal care as an offered benefit (described in more detail in Section 1.3), and, (2) income variation across seniors which affects their likelihood of being eligible for Medicaid. These two sources of

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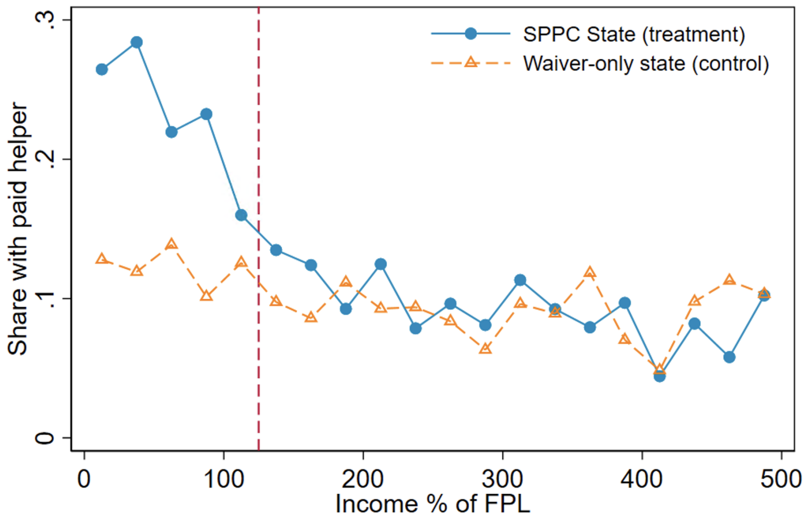
[1] I define the home care industry using two NAICS codes: 6216.00 (Home Health Care Services) and 6241.20 (Services for the Elderly and Disabled) and compute aide employment using the Occupational Employment Statistics and revenue using the 2017 Economic Census. Total revenue for these two industries is $135B, but some of this revenue is associated with skilled home health care, which is usually provided in a post-acute setting, rather than long-term care. We can use the Service Annual Survey to get a sense of the size of the post-acute home health care industry, which is mostly paid for by Medicare ($35B) and private health insurance ($15B).

Figure 1.1: *Share of functionally-impaired seniors who report having a paid helper by state plan PC policy and income*



Notes: Seniors who reported having difficulty with at least one activity of daily living are grouped into 25 percentage point buckets by their countable income using Medicaid rules as a percentage of the federal poverty line (FPL). Seniors with income above 500% FPL are not shown in this graph. The x-coordinate represents the midpoint of each bucket's income range, and the y-coordinate represents the share of seniors in this bucket who reported having a paid home helper.

variation allow me to estimate the effect of adopting the State Plan Personal Care (SPPC) benefit using a cross-sectional difference-in-differences design that compares outcomes for low-income (high Medicaid eligibility) seniors in "treatment" states (states with the SPPC benefit) to high-income (low Medicaid eligibility) seniors in those same states and to other low-income seniors in control states (states without the benefit).

Figure 1.1 previews this identification strategy. The figure graphs average utilization rates of paid home care among functionally impaired seniors as a function of income and whether they live in a state that adopted the SPPC benefit. Among seniors whose incomes are low enough that they are likely eligible for Medicaid (left of the vertical dashed red line), those living in states adopting the SPPC benefit have significantly higher levels of paid home care utilization than those living in states that did not adopt the SPPC benefit. This difference across states becomes negligible at higher income levels. My preferred estimate of this effect—which also controls for individual characteristics such as marital status, education, chronic conditions, and the senior's degree of functional difficulty—suggests that the SPPC benefit causes a seven percentage point increase in the probability that a Medicaid-eligible senior with functional impairments uses paid home care, compared to a baseline average of 12 percentage points, translating to more than a 50% increase in the formal home care utilization.

In order to understand the welfare effects of this increase, it is of first-order importance to understand who the marginal recipients of paid home care are, and what type of care they would have been receiving in the absence of the policy. I show that most of the marginal recipients of care for this expansion would have otherwise been living in the community and receiving at least some informal care in the absence of this policy. Thus, the formal home care expansion only slightly reduces the share of seniors who receive no care or nursing home care (and neither of these reductions are statistically significant). However, the policy appears to induce a significant shift in *who* provides care to seniors living at home, causing eligible seniors to replace care from relatives with care from non-relatives.

These results suggest that unpaid family care has a fairly large elasticity with respect to the availability of paid home care. However, I also find that this elasticity varies by the type of caregiver: although wives, husbands, and daughters provide the majority of unpaid family care, I find that most of the decrease in care hours is concentrated among husbands and daughters, while care provided by wives appears to be relatively inelastic. I then study whether the policy affects the labor supply of these caregivers. I find no effect of the policy on the labor supply of spouses or sons (unsurprising because most spouses are above the usual age of retirement and I do not find much of a decrease in caregiving from sons), but large effects for daughters. Depending on the specification used, I estimate that one of every 2.4-3 daughters whose parent receives paid home care as a result of the policy works full-time as a result of the SPPC benefit.

I also evaluate the effect of the SPPC benefit on where seniors live, their health and well-being, and the health and well-being of their potential caregivers. While I find that seniors are more likely to live on their own as a result of the policy, I find no significant impacts on the health and well-being of seniors, their children, or their partners, though the evidence for daughters is more suggestive of potentially positive impacts. For seniors, I discuss a couple reasons one might not expect to see significant impacts: one possibility is that this reflects heterogeneous treatment effects of switching from nursing home or informal care to formal home care that are dependent on the care quality of the different options and the health of the senior, and another possibility is that because most people are simply substituting home care from formal providers for home care from informal providers, the health impacts for all marginal seniors are small.

A significant concern with this identification is that there may be omitted variables at the state-level that may affect outcomes for low-income seniors that are statistically correlated with states' adoption of the SPPC benefit. Where the data allow, I also estimate a triple difference specification that expands the sample to include all seniors with and without ADL impairments and uses a third level of variation, whether or not a senior has an ADL impairment, to identify the treatment group. This allows me to check that the effects I estimate are specific to low-income seniors with ADL impairments in SPPC states, rather than low-income seniors in SPPC states at large, thus narrowing the set of plausible omitted variables threats to the identification. I also show that the effects are robust to both parametric tests (where I show that the effect of interest is robust to including other state-level controls such as average Medicare spending or Democratic vote share) and non-parametric tests (where I randomly simulate treatment states) of potential omitted variable bias.

The finding that daughters of frail seniors have higher labor supply in states with the SPPC benefit has implications for the policy's impact on both equity and efficiency. From an equity perspective, this finding is consistent with other work that suggests that providing in-kind care benefits may be an effective tool to combat gender differences in labor force participation and pay (Blau and Kahn, 2017; Olivetti and Petrongolo, 2017). From an efficiency perspective, the finding of a large labor supply effect implies the existence of a significant fiscal externality from financing home care for seniors in the increased income tax revenues from their daughters.

I elaborate on this further with a back-of-the-envelope calculation of the net cost to the government of a policy that would expand access to home care in control (waiver-only) states to the levels in treatment (SPPC) states. I estimate that the fiscal externality could lead to a savings of roughly 15% of the program cost. However, as has been noted by many policymakers and analysts, the larger potential for savings to the government would be if the policy could substantially reduce nursing home use. As I discuss in the next section, the literature's estimates of the size of this substitution effect vary widely; unfortunately, my own estimate is also imprecise. The point estimate falls within the range in the literature, and implies one deferred nursing home admission for every five new paid home care users which would imply savings of roughly 40% of the program cost, but this estimate is not statistically significant at conventional levels and should therefore be treated with caution.

1.1.1 Related literature

There is a large literature that has studied the relationship between elder caregiving and labor supply (Ettner, 1996; Johnson and Lo Sasso, 2006; Van Houtven *et al.*, 2013; Truskinovsky and Maestas, 2018; Fahle and McGarry, 2017). There have been fewer studies that investigate how long-term care policy may mediate the relationship between caregiving for parents and daughter labor supply (the focus of this paper). A few recent papers investigating this question include Coe *et al.* (2015), who use quasi-experimental variation in the use of long-term care insurance (LTCI) generated from tax subsidies to show that LTCI reduces informal care and increases child labor supply, Fu *et al.* (2017) who use policy variation in the provision of long-term care in Japan to show that public funding for long-term care increases the labor force participation of female caregivers, and Løken *et al.* (2017) who study a long-term care reform in Norway.

There is a much larger literature on the impact of providing childcare on maternal labor supply (Baker *et al.*, 2008; Gelbach, 2002; Havnes and Mogstad, 2011; Bauernschuster and

Schlotter, 2015). Many of these studies have found large positive effects on labor supply, while others (e.g. Havnes and Mogstad, 2011) have found no effect–these differences likely stem from differences in the baseline level of labor force participation, who is targeted by the policy, and whether parents have alternative private sources of childcare. More broadly, several researchers have argued that cross-country differences in pro-family policies and spending on work complements (such as childcare and eldercare) may explain differences across countries in female labor supply (Kleven, 2014; Blau and Kahn, 2013; Kleven *et al.*, 2020).

This paper is also related to the literature on the cost-effectiveness of providing home and community-based care through Medicaid. Early demonstration projects sponsored by the federal government often highlighted the "woodwork effect" (essentially a moral hazard extensive margin effect), where greater availability of home care would result in people "coming out of the woodwork" to claim the new benefit and thus increase rather than decrease total costs to the government (Doty, 2000; Kemper *et al.*, 1987; Berkeley Planning Associates, 1984). These early studies estimated a wide range of woodwork effects depending on the program being studied, ranging from one deferred nursing home admission for every 1.6-16 recipients of home care. More recent studies of state-level spending trends have been more optimistic as to the potential for home- and community-based care to achieve cost savings (Grabowski, 2006; Kaye *et al.*, 2009; Eiken *et al.*, 2013). One limitation of the approach taken in these studies is that they generally compare states based on their realized spending on home- and community-based care, which may be endogenous to other factors that may contribute to a reduction in nursing home use. This paper uses a specific policy choice for identification—the adoption of the SPPC benefit—which should be subject to fewer endogeneity concerns. In addition, my data includes seniors who are likely and unlikely to be eligible for Medicaid, allowing me to control for other state-level factors better than studies that only use Medicaid data. My findings are consistent with other studies that have shown significant elasticities of supply of informal home care (e.g. Bolin *et al.*, 2008; Van Houtven and Norton, 2004; Golberstein *et al.*, 2009) and small elasticities of demand for nursing home care (e.g. Grabowski and Gruber, 2007) with respect to public program generosity and formal care availability.

1.2 Conceptual Framework

What are the potential effects of offering formal home care to low-income seniors? To help think through the possibilities, we consider a model where a family unit consisting of a functionally impaired senior and any potential caregivers (e.g. a spouse and children) jointly maximizes the total utility of each of its members. Our thought experiment involves comparing two environments: the control environment in which seniors have only two options for care (a nursing home or unpaid family care), and the treatment environment in which a third option is introduced (paid home care). Because the context for this thought experiment is Medicaid long-term care policy, we assume that all care is provided at no cost to the families, except a hassle cost associated with applying for Medicaid (which may be significant, since families may need to deplete their assets).

In the control environment, each family evaluates the relative utility of using nursing home care or unpaid family care for the impaired senior. Families can also choose for the senior to go without care. The total family unit's utility of these three options will depend on the seniors' health status, and the caregiver's opportunity cost of caregiving. In the treatment environment, there are thus three types of people who may take up formal home care: (1) people who previously chose nursing home care, (2) people who previously chose unpaid family care, and (3) people who previously chose no care. The sum of these groups will be the people who "take up" the SPPC benefit in my study (the compliers), and the first order empirical question that I attempt to answer is how big each of these groups of compliers is.

Both with and without the paid home care option, there are several places where the private choices of individuals may diverge from the socially optimal choices. First, patients do not pay the monetary cost of nursing home care or paid home care, so one may expect greater consumption of both of these options compared to the optimum unless the hassle costs of applying to Medicaid are high enough. However, if a choice of care has health benefits on the margin relative the other choices, some of the benefits of this choice may accrue to the government rather than to the individual in the form of lower health care spending, so there may also be under-consumption of the beneficial care option. Finally, if care choices affect other economic behavior, such as labor supply decisions, private choices will not account for potential fiscal externalities from changes in income tax revenues, again leading to suboptimal care consumption.

The question posed by this paper is who benefits (and by how much) from public financing for home care for low-income seniors. Considering only fiscal benefits, those seniors who are moved from choosing nursing home care to choosing paid home care are likely to have lower Medicaid spending, since the cost of nursing home care generally exceeds the cost of paid home care, leading to lower costs for the government. For the seniors who are moved from choosing family care to choosing paid home care, the expectation should be that costs to the government should increase, since these seniors are now receiving formal care that was previously provided for free by family members. However, if the people who make this switch are switching because they had a high opportunity cost of providing family care due to other outside labor market options, there is the potential of a positive fiscal externality from this switch resulting from more family caregivers being in the labor force.

1.3 Background

1.3.1 Long-term care for the elderly and Medicaid

Need for long-term care services is often defined in terms of having difficulty with activities of daily living (ADLs), usually defined as bathing, dressing, toileting, eating, transferring, walking. Using the 2007 American Community Survey, Kaye *et al.* (2010) estimate that roughly five million people over the age of 65 have difficulty with one or more of these activities. People can receive long term care in a variety of settings and ways, but the two most common in the US are: (1) nursing homes, which are highly regulated and provide 24-hour skilled care and are used by 1.3 million people with ADL difficulties, and (2) informal care (Mudrazija and Johnson, 2020).

There are at least two trends over the past few decades that have begun to offer alternatives for people who may not need the high level of care provided by nursing homes, but need more support than their potential informal caregivers may be able or willing to provide. First, many researchers have noted the expansion of alternative senior housing arrangements such as assisted and independent living communities. However, many of these facilities primarily serve private-pay residents and people with less severe needs, and the ACS estimates that only 150,000 people with ADL difficulties live in these types of residences. Nonetheless, the evidence suggests that the growth of these housing options has reduced the use of nursing home care (Grabowski *et al.*, 2012). The second trend is the subject of this paper: the growth of paid home care, where an aide is hired (usually through an agency, but sometimes as an independent provider) to help with basic personal care or household tasks. Figure 1.1 suggests that this type of care is commonly used by low-income seniors in particular, which I hypothesize to be due to these seniors being eligible for the Medicaid benefits studied in this paper.[2]

While Medicare does not pay for long-term care,[3] Medicaid has been required to cover nursing home care since its creation in 1965 and currently funds over 60% of nursing home stays. However, Medicaid is not similarly required to cover other forms of long-term care such as home care. Policymakers at the time were worried about the moral hazard (woodwork) effect discussed above (Smith and Feng, 2010). Many advocates have since argued that this has led to an “institutional bias” in Medicaid long-term care spending, and have argued in support of reforms that increase access to home and community-based services (HCBS). These reforms are commonly referred to as “rebalancing” reforms. These

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[2] Medicaid is generally prohibited from covering room and board except in nursing homes, thus limiting its ability to cover other sources of residential care such as assisted living.

[3] Nursing home stays or home health visits are only covered for short periods following a hospitalization.

reforms were first passed beginning in the 1970s and 1980s, but Doty (2010) notes that growth of these programs was slow until the 1990s.[4]

Figure 1.2 shows how the allocation of Medicaid long-term care spending on the aged and physically disabled changed from 2000 to the present.[5] Per-enrollee spending on home and community-based care more than tripled between 1990 and 2016. On the other hand, nursing home spending increased over the 1990-2000 decade, but peaked in 2002 and is now substantially below 1990 levels. As a result, the share of long-term care Medicaid spending going toward home and community-based care was less than 15% percent in 1990, and exceeded 40% by 2016.

1.3.2 State variation in Medicaid provision of personal care

Medicaid spending on home and community-based care can include spending on a variety of services, including personal care, chore services, case management, adult day care, respite care for family caregivers, residential care (excluding room and board), home modifications, home-delivered meals. In this paper, I focus on one of the largest components of HCBS spending: personal care services, i.e. assistance from an aide with ADLs.

All states cover personal care, but there are two primary mechanisms they can use to do so. The first is that states have the option to add personal care as a benefit in their Medicaid State Plan. This option was first made available to states in 1975, and means that personal care becomes essentially like an entitlement benefit: as long as a Medicaid enrollee meets the need criteria for services, the state must provide them. The second primary mechanism used by states are 1915(c) waivers. Waiver programs have several features that allow states more flexibility in designing their HCBS benefits: instead of having to design a one-size-fits-all

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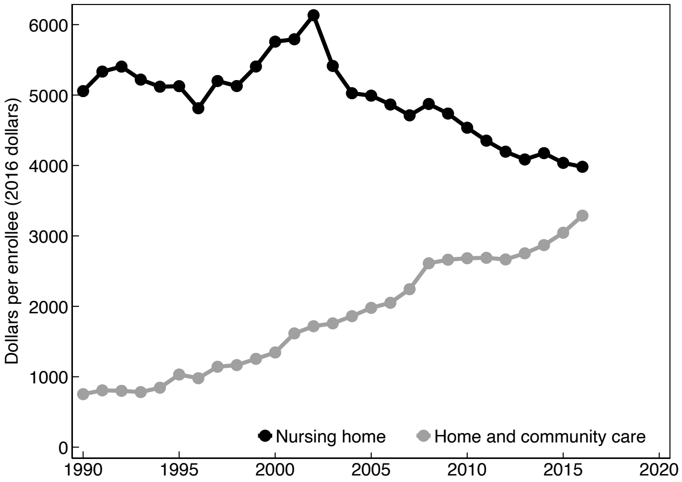
[4] Two factors that accelerated their growth were reforms during the Clinton administration and the Supreme Court's 1999 Olmstead decision, which asserted that states had an obligation to serve people with disabilities in the community if possible.

[5] Most Medicaid long-term care data groups beneficiaries into three broad categories: (1) the aged and physically disabled, (2) intellectually and developmentally disabled, and (3) individuals with serious mental illness. Although this paper is focused specifically on the aged, it is unfortunately quite difficult to isolate spending on the aged from spending on the physically disabled in the available data.

Figure 1.2: *National Medicaid spending on long-term care for aged and physically disabled adults per enrollee (2017 dollars)*



Notes: Spending numbers come from CMS-64 reports analyzed in Wenzlow *et al.* (2016) and adjusted for inflation using CPI-U. Spending is scaled by total aged and disabled enrollment from the Brief Summaries of Medicare and Medicaid reports from CMS, subtracting counts of intellectually and developmentally disabled LTSS recipients from the Residential Information Systems Project. In 2016, there were roughly 14.6 million aged and disabled enrollees.

benefit, states can create several waiver programs covering different services for different groups of people (e.g. people with intellectual disabilities vs. older adults). Additionally, the 1915(c) program also allows states to set enrollment and/or cost caps. Ex-ante, it is not obvious whether these two mechanisms should induce different rates of personal care utilization among ADL-impaired seniors covered by Medicaid. However, as I previewed earlier in Figure 1.1, the data indicates that SPPC states have far higher rates of paid home care utilization than waiver-only states, suggesting that on balance, the different features of the 1915(c) program relative to an entitlement benefit has led to slower growth of these programs.

When the Personal Care benefit was introduced, many states were quick to adopt the benefit because they had already begun to subsidize home care for low-income seniors and the Medicaid benefit was a way to secure federal funds for these programs (Kennedy and Litvak, 1991). Twenty-five states adopted the Personal Care benefit in the late 1970s and early 1980s and are shown in blue in Figure 1.3. These states will be the main "treatment" states in the paper. Once the 1915(c) waiver program passed, state adoption of the SPPC benefit essentially stopped until the late 1990s, but none of the early adoption states retracted the benefit. The light orange states in Figure 1.3 never adopted the SPPC benefit and all cover personal care through waiver programs (I thus refer to these states, which are the main control states in the paper as "waiver-only" states[6]). Finally, five states adopted the Personal Care benefit between 1999 and 2007 and are shown in white.[7] For simplicity, I drop the late adoption states for the main analysis in the paper, Because these programs grow gradually, they are likely to be different from both control and treatment states. In Section 1.7, I present a version that includes these states as control states before the passage of the benefit, and treatment states after. The gradual, rather than sharp, of these programs is also the reason that it is necessary to conduct a cross-sectional, rather than time-series analysis.

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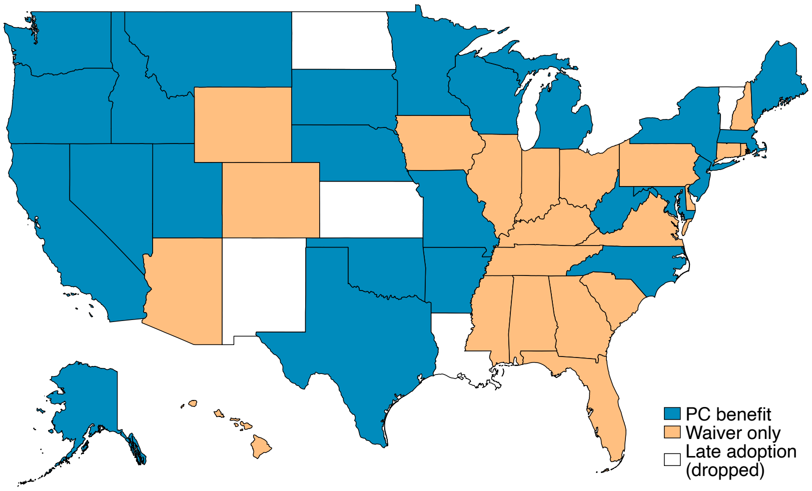
[6] Some states offer both state plan personal care services and waiver services, where the waiver services are generally used to supplement the SPPC benefit.

[7] A few things likely contributed to the resurgent interest in the SPPC benefit after 1999 including the Olmstead decision, and federal incentives through the 1915(i) program to adopt the benefit.

Figure 1.3: *State variation in inclusion of personal care in the Medicaid State Plan during the analysis period (2000-2016)*



Notes: The map shows whether states have adopted the Personal Care optional benefit in their state plans. The states in blue adopted the option in the 1970s or 1980s, while the late adoption states adopted the option between 1999 and 2007. Two states, FL and NH, adopted the option but in a very limited fashion (FL adopted the option only for 24-hour care and NH adopted it only for people who are wheelchair-bound) and so are coded as waiver-only states for this analysis.

One significant trend in Medicaid's provision of home care is in the increasing use of "consumer direction," where enrollees can hire their own aide (often a family member), rather than use an aide from a Medicaid-contracted agency. The use of consumer direction also varies widely by state Medicaid programs, but I do not exploit this variation in my analysis.

1.3.3 Medicaid eligibility for seniors

There are two main ways seniors can qualify for Medicaid. First, anyone over 65 who has income and assets below the state thresholds is eligible for Medicaid. Both income and assets are calculated after deducting certain exemptions (e.g. half of earned income, a primary residence). In general, states must set their income threshold between the federal SSI eligibility level (74% of the federal poverty level, or FPL) and 100% FPL to receive federal matching funds. In most states, the asset threshold is usually \$2,000 for individuals and \$3,000 for couples.[8] Roughly 70% of aged Medicaid enrollees qualify through this pathway (De Nardi *et al.*, 2012). The remaining qualify through special eligibility pathways such as medically needy programs, which allow people to count medical costs against their income, and thus allow people with higher incomes but high medical costs to enroll in Medicaid (they still must deplete their assets). This is commonly referred to as "spending down" onto Medicaid, and is used by many long-term nursing home residents.[9]

Figure 1.4 shows how these rules translate into actual coverage rates in my analysis sample of ADL-impaired seniors. I group seniors by their countable income (gross income minus allowed exemptions) expressed as a percentage of the FPL, and plot the share of seniors in this income group who report being covered by Medicaid. I find that Medicaid coverage is high and relatively flat for incomes between 0 and 75% FPL, and then gradually falls between roughly 75% and 200% FPL before flattening out again (though it remains

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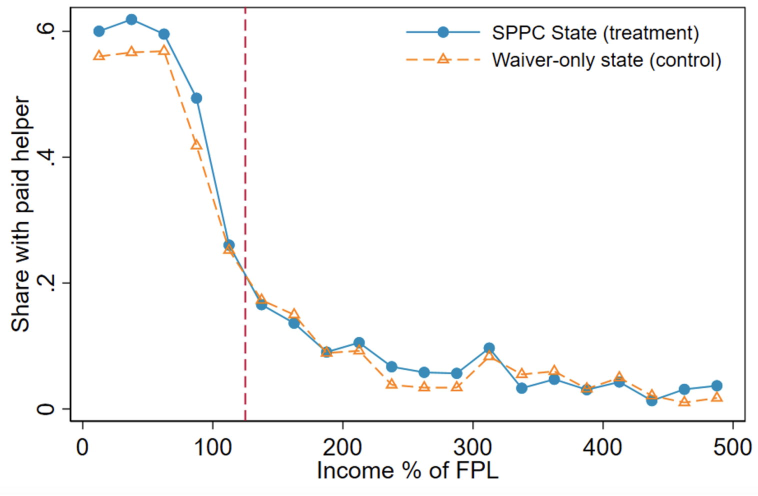
[8] See Musumeci *et al.* (2019) for a complete description of state eligibility thresholds.

[9] While not all states have medically needy programs, all states without medically needy programs allow potential enrollees to put extra income in a trust in order to qualify for Medicaid if they are in an institution, and all except three states (AL, NV, and WY) also allow trusts to be used for home care (Musumeci *et al.*, 2019).

Figure 1.4: *Rates of reported Medicaid coverage by income among seniors with an ADL difficulty (2000-2016 HRS)*



Notes: See notes from Figure 1.1. The x-coordinate represents the midpoint of each bucket's income range, and the y-coordinate represents the share of seniors in this bucket who reported having Medicaid coverage.

nonzero through the end of the graph at 500% FPL). The lack of a discrete drop in Medicaid coverage can likely be attributed to the asset requirement and spend-down pathways, which should mean under reasonable assumptions that people with incomes above the income threshold will face declining probabilities of qualifying for Medicaid. Another thing to note is that even among people with incomes below 75% FPL, Figure 1.4 shows that only 60% of respondents report having Medicaid coverage. This likely reflects a combination of both incomplete take-up of Medicaid (Bitler and Zavodny, 2017), and measurement error (either mismeasurement of income or misreporting of coverage, see Boudreaux *et al.*, 2015). For the main results in the paper, I use a threshold of 125% FPL to define the treatment and control groups; however, given that this choice is somewhat arbitrary, I perform sensitivity tests to this choice and report the results in Section 1.7 and in the Appendix.

1.4 Data

The data for this study comes from the 2000-2016 waves of the Health and Retirement Study (HRS), a longitudinal panel study that interviews people in the US who are over the age of 50 every two years from sample entry (usually when they are between 50-56) until death. The survey includes a broad range of questions related to aging, health, and financial well-being. The first cohorts were interviewed in 1992 and 1993, and new cohorts are added periodically to refresh the sample. A natural starting year for analysis is 1998, the first year where the HRS contained a representative sample of the over 50 population in the US; however, I start my sample in 2000 because the questions about caregiving change slightly in this year.[10]

My main analysis sample will be all respondent-years where the respondent is 65 or older and reports any difficulty with an ADL because of a health or memory problem. For some specifications, I will include the population over 65 with no reported ADL difficulties as a control group. On average, there are approximately 10,000 people per year over 65 in the HRS, and 21% report an ADL difficulty, resulting in a sample size of 21,918 respondent-years across the nine years in the sample. Table A.1 compares the demographic, family, and health characteristics of seniors with and without ADL difficulties in the HRS. Seniors with an ADL difficulty are older (the two samples have average ages of 79 and 75, respectively), and more likely to be female (65% compared to 55%) and single (58% compared to 38%) than seniors without ADL difficulties. They are also more likely to have income below 125% FPL (36% compared to 16%).

If a respondent reports having difficulty with any ADL(s), they are first asked to identify who helps them with ADL(s) (up to 15 helpers, beginning with the person who helps them the “most”). They are then asked to provide details on each of these helpers, including the helper’s relationship to them, the number of days and hours per day the helper provided help (over the past two months), whether the helper was paid, and if so, an estimate of the

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[10] Before 2000, respondents were not asked detailed questions about caregiving by spouses.

out-of-pocket costs the respondent paid.

Table 1.1 summarizes the living and care arrangements of seniors in the ADL-impaired sample. Panel A splits the sample based on whether the impaired senior is single or has a partner (married or living with a partner as if married). Just 15% of ADL-impaired seniors are in residential care. Residential care is particularly uncommon for people who have a partner, accounting for only 8% of partnered seniors with ADL impairments. By contrast, almost 60% of ADL-impaired seniors are living on their own (defined as alone or with only their spouse), and 20% of seniors are living with a child. The last few rows summarize sources of care for those living at home. Almost half of seniors living at home do not receive any help at all (true of both singles and people with a partner). Of those receiving help, most receive help from unpaid helpers only, but a non-trivial share of ADL-impaired seniors do receive some paid help: 16% of singles and 8% of people with a partner.

Panel B restricts the sample to ADL-impaired seniors living in private housing and receiving some help, and summarizes who their caregivers are, and how many hours of care they provide. The average senior receiving help at home has 1.8 helpers, who provide 41 hours of care a week. In Appendix Figure A.1 , I show that this distribution of hours of care is actually bimodal: most seniors in my sample receive between 0-2 hours per day (less than 15 hours per week), but roughly 10% receive 24-hour care every day. For this reason, I focus many of my results on care provision along the extensive margin, rather than using hours, which are likely to be heavily influenced by people receiving 24-hour care. Daughters provide almost 30% of all care hours, followed by non-relatives and wives, who each provide roughly 20% of care hours. Panel B also reveals the importance of unpaid and co-resident helpers, who provide 79% and 70% of total hours, respectively.

In addition to information about seniors who receive care, the structure of the HRS is advantageous because it allows the researcher to construct samples of the children and partners of every respondent. This is because spouses are interviewed for the HRS sample (regardless of age), and respondents are asked to provide fairly rich detail on each of their children, such as the child's age, education level, where they live, and whether the child

Table 1.1: *Summary of care received and living arrangements of seniors with ADL impairments*

Panel A. Place of residence and source of care for seniors with ADL impairments (N=21,918)

	All	Single	Partnered
% of sample	100	57	43
Live in residential care	0.15	0.23	0.08
Nursing home	0.14	0.21	0.07
Other residential	0.01	0.02	0.01
Live in private housing	0.85	0.77	0.91
Residence			
Living on own	0.58	0.50	0.70
Living w/ kid	0.20	0.33	0.23
Other	0.06	0.07	0.05
Care			
No help	0.40	0.38	0.42
Unpaid only	0.32	0.24	0.44
Paid help	0.13	0.16	0.08

Panel B. Types of caregivers and weekly hours of care for ADL-impaired seniors receiving some help at home (N=10,089)

		Hours per week				
	Number of helpers	Total	Unpaid	Paid	Co-res	Non co-res
Total	1.80	41.0	32.5	9.3	28.5	13.9
Wife	0.24	8.7	8.7	–	8.7	–
Husband	0.18	6.2	6.2	–	6.2	–
Daughter	0.40	11.9	10.6	1.3	7.9	4.0
Son	0.21	4.3	4.0	0.3	2.9	1.4
Other rel.	0.18	3.9	3.3	0.6	2.3	1.6
Non-rel	0.30	8.3	1.1	7.2	1.1	7.2

Note: I censor all hours values at 24 hours per day (144 hours per week) so the rows do not always sum perfectly. In the raw data, a person may have more than 24 hours of care per day if they report two caregivers who each help for 24 hours per day.

provides help (e.g. help with ADLs, financial help) or receives help (e.g. help with childcare, financial help) from the respondent. I summarize these samples in Table A.2 and Table A.3 in the Appendix.

To obtain even richer detail on child caregivers, I supplement my main analysis with a secondary analysis that samples HRS respondents who are potential caregivers, rather than care recipients. Specifically, I use data provided by respondents between the ages of 50 (the youngest age at which people are eligible to be sampled as a respondent for the HRS) and 65 about their parents (whether each parent is alive, and whether they have personal care needs) and any care that they provide to their parents.

1.5 Empirical Strategy

I employ a few different specifications to study the effects of the SPPC benefit on seniors and their children. In general, these specifications use variation across (1) states, (2) income, and (3) presence of an ADL impairment, but which types of variation are used depends on the outcome and group being studied. Section 1.5.1, outlines my strategies for studying outcomes for ADL-impaired seniors in the HRS, while Section 1.5.2 outlines the strategies I use to study the children and partners of these seniors.

1.5.1 Effects on seniors

My main empirical strategy estimates a difference-in-differences specification on the sample of ADL-impaired seniors, where the policy variable of interest is whether or not a state has adopted the State Plan Personal Care benefit, and the treatment group is seniors who have low enough income to likely qualify them for Medicaid. For an ADL-impaired senior i residing in state s and year t, this equates to estimating the following equation:

$$Y_{it} = \beta \text{SPPC}_s \text{IncElig}_{it} + \gamma \text{IncElig}_{it} + \delta \lambda_{it} + \tau_s + \mu_t + \epsilon_{it} \tag{1.1}$$

where SPPC_s is a state-level variable that indicates whether or not the state has offers personal care through its state plan and IncElig_{it} is an indicator variable that is equal to 1 if the senior's income is below 125% FPL. The coefficient β, which multiplies the interaction of these two variables, is the main coefficient of interest. I also include the effect of IncElig_{it} by itself and state fixed effects τ_s (which would absorb the effect of SPPC_s by itself), as well as year fixed effects μ_t, and a vector of controls that vary at the individual or individual-year level λ_{it}. These variables include demographics (age, sex, race/ethnicity, foreign-born, education), family supports (whether the senior has a partner and/or kids), and measures of health and functioning (numbers of functional difficulties, chronic conditions). Summary statistics for all of these variables are presented in Table A.1 in the Appendix. I use income eligibility, rather than Medicaid coverage, because the decision to enroll in Medicaid may be endogenous to the availability of public home care.[11]

Dependent variables Y_{it} include whether seniors receive different types of care, the number of hours of care they receive, living arrangements, and measures of health and well-being. While care outcomes are only asked for people with ADL impairments, living arrangements and health and well-being measures are asked for all respondents. For these outcomes, I also estimate a triple-difference specification using the sample of all respondents i over 65:

$$\begin{aligned} Y_{it} = &\beta \text{SPPC}_s \times \text{IncElig}_{it} \times \text{ADL}_{it} + \delta_1 \text{IncElig}_{it} \times \text{ADL}_{it} + \delta_2 \text{SPPC}_s \times \text{IncElig}_{it} \\ &+ \delta_3 \text{SPPC}_s \times \text{ADL}_{it} + \gamma \text{IncElig}_{it} + \tau_s + \mu_t + \lambda_{it} + \epsilon_{it}. \end{aligned} \tag{1.2}$$

Again, β is the coefficient of interest, and now multiplies a variable which is equal to 1 if a senior lives in a SPPC state, is low-income, and has an ADL impairment (ADL_{it}). All one-way and two-way interactions are also included. Standard errors for all specifications are clustered at the state-level.

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[11] Indeed, the primary source of income for seniors in my sample is Social Security retirement benefits, which are unlikely to be significantly affected by one's desire to obtain paid home care.

1.5.2 Effects on children and partners

The Medicaid SPPC benefit may also affect the potential caregivers of seniors with ADL impairments. In this section, I describe my strategies for studying these effects. I first use the HRS to construct samples containing all children and partners of the seniors used above, and estimate parallel strategies to identify the effect of the policy on caregiving and labor supply for these two groups. I then describe my strategy for studying the health and well-being of children and partners, which requires using a different sample of children for which these outcomes are observed.

A. Caregiving and labor supply among children and partners

Every HRS respondent's spouse is automatically included in the HRS sample, regardless of age, and we thus have detailed data on spouse outcomes such as labor supply and health and well-being, in addition to knowing how much care they are providing. Respondents also give basic information about each of their children, including whether the child is working, and whether or not that child helps them with ADLs and IADLs, as well as their age, education, marital status, and number of kids.

I again make use of both a difference-in-differences and triple difference specification. For the difference-in-differences specification, the sample is either all spouses j or all children j of an ADL-impaired senior HRS respondent. For dependent variables Y_{ijt} such as whether the spouse or child helps the senior and whether or not the spouse or child is working, I estimate:

$$Y_{ijt} = \beta_g \text{SPPC}_s \text{IncElig}_{it} + \gamma_g \text{IncElig}_{it} + \delta \lambda_{it} + \pi \eta_{jt} + \tau_{sg} + \mu_t + \epsilon_{it} \tag{1.3}$$

This is the same equation as Equation (2.3), except I have added controls for demographic characteristics of each spouse or child j η_{jt} (for spouses: I use age, gender, education; for children: I use age, gender, education, marital status, and whether they have any children and any young children). I also estimate separate coefficients for each of the main effects by gender, since there are large gender differences in both labor supply and caregiving. The triple difference specification is likewise akin to Equation (1.2) except for the inclusion of the same controls η_{jt}.

B. Health and well-being of children and partners

To estimate effects on spouse health and well-being, we can make use of the same specifications as above. However, HRS respondents provide only basic demographic information about their children, and so we do not have any information on child health or well-being. To estimate the effects of Medicaid policy on these outcomes, I invert the sampling process for identifying potential caregivers: instead of using the children of HRS respondents with ADL difficulties, I use the sample of HRS respondents who may provide care to a parent, exploiting the fact that the HRS also asks respondents if their parents are alive and if they need help with personal care tasks. While this means that I now have detailed data on child health and well-being, a drawback to using this data is that I do not have the granular data on parent income that enabled me to compute a fairly accurate measure of Medicaid income eligibility in the main sample. I thus employ a slightly different strategy to study the effect of the Medicaid SPPC benefit on child health and well-being: rather than estimating differences among low- and high-income individuals in treatment and control states, I restrict my sample to people for whom the policy is likely to bind, and estimate the difference between the labor supply of children who have or do not have a parent with personal care needs in the two types of states.

My main analysis sample is the sample of all HRS respondents under the age of 65 who reported that their family financial situation was poor growing up (given the choices of pretty well off, about average, and poor). This sample differs from the child sample above in two ways: (1) it only includes people over the age of 50,[12] (2) it includes people whose parents have passed away, and (3) the family financial situation restriction. Again, I make this last restriction to limit the sample to people who are likely to be affected by the policy of interest.

My preferred specification to estimate the impact of the SPPC benefit is again a difference-

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[12] From the earlier analysis, I estimate that approximately half of the children of ADL-impaired seniors are over 50.

in-differences specification, but where the two sources of variation are (1) if they livs in a SPPC state, and (2) if they have any parent who needs help with personal care needs (ParentDiff_{jt}):

$$\begin{aligned} Y_{jt} =& \beta_1 \text{SPPC}_s \times \text{ParentDiff}_{jt} + \beta_2 \text{SPPC}_s \times \text{ParentAlive}_{jt} + \\ & \gamma_1 \text{ParentDiff}_{it} + \gamma_2 \text{ParentAlive}_{it} + \delta \lambda_{it} + \zeta \pi_{jt} + \tau_s + \mu_t + \epsilon_{it} \end{aligned} \tag{1.4}$$

The coefficient of interest above is β_1, and the specification also includes an estimate of the effect of having any living parent ParentAlive_{jt}, alone and also interacted with living in a SPPC state, as well as the same controls and state and year fixed effects as above.

1.6 Results

1.6.1 Care patterns and living arrangements for seniors

Table 1.2 begins by exploring the impact of the SPPC benefit on what type of care seniors with ADL difficulties receive. The sample is all senior-year observations where the senior reports having at least one ADL difficulty. The first three columns explore care provided to seniors in private housing, while the fourth and fifth columns look at residential care.

Starting with the third row (the control variables), the table shows that the strongest demographic determinants of receiving care at home vs. being in residential care are race/ethnicity, with minority patients significantly less likely to be in residential care and more likely to receive care at home. These differences by race and ethnicity have been noted previously in the literature. The table also shows that family supports are strongly associated with receiving only unpaid help at home, and negatively associated with being in residential care. Finally, worse health and functioning measures all decrease the probability that a senior receives no help (column 3), but have different effects on which type of care seniors choose: for example, having dementia significantly increases the probability a senior chooses nursing home care over other options, while having difficulties with more instrumental activities of daily living (cooking, grocery shopping, managing medication) has almost no effect on the probability a senior is in residential care, but a large effect on the probability that a senior receives paid help at home.

The top row shows the main coefficients of interest: the effect of the SPPC benefit on seniors' care choices. I find that high-eligibility seniors in SPPC states are 7.1 percentage points more likely to have a paid home helper, more than a 50% increase over the overall average utilization rate of 13 percentage points (column 1). Most of this effect comes from people who would otherwise have lived at home and received only unpaid help (column 2). The Medicaid SPPC benefit also has a smaller negative effect on the share of people living at home and receiving no help (column 3), and living in a nursing home (column 4), but neither effect is statistically significant at conventional levels. These results suggest that for the marginal seniors in this sample, paid home care is mostly used to supplement or replace unpaid home care, rather than reaching people who would be without help or living in a nursing home.

Table 1.3 turns to a continuous measure of care, the number of hours of care seniors report receiving at home each week. The first column shows that the Medicaid SPPC benefit increases the number of paid hours of care among the eligible population by 2.8 hours per week. If we assumed that this increase was entirely due to the extensive margin change in Table 1.2, this would mean that the average new recipient of paid home care receives 40 hours per week of paid care, which would be roughly in line with the findings in Table 1.1. The second column shows that the benefit also caused a reduction in unpaid hours equal to roughly 65% of the additional paid hours.[13] Thus, only 35% of the hours paid for by the program accrue as "new" hours of care to the impaired senior.

One possibility is that this effect is because the SPPC benefit simply converted unpaid caregivers into paid caregivers by compensating family members for their care provision through consumer direction programs, so there is no real effect on who is providing care. Another possibility is that the policy causes (paid) non-relative caregivers to take over some

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[13] These estimates should be interpreted with some caution because respondents are only asked whether each caregiver was paid or not, but it is possible that some caregivers provided both paid and unpaid care hours.

Table 1.2: *Effect of Medicaid SPPC benefit on probabilities of receiving help and hours of help received per day among ADL-impaired seniors*

	Private Housing			Residential Care	
	(1) Paid Help	(2) Unpaid Only	(3) None	(4) Nursing Home	(5) Other
$\text{PC}_s \times \text{IncElig}_{it}$	0.071***	-0.050***	-0.009	-0.015	-0.000
	[0.018]	[0.016]	[0.016]	[0.014]	[0.005]
IncElig_{it}	-0.011	0.035***	-0.042***	0.019*	0.002
	[0.015]	[0.012]	[0.012]	[0.010]	[0.004]
Demographics					
Age	0.003***	0.000	-0.008***	0.004***	0.001***
	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]
Female	0.008	0.025*	-0.027**	-0.005	-0.001
	[0.016]	[0.014]	[0.012]	[0.013]	[0.004]
Black	0.036***	0.037**	0.006	-0.075***	-0.006***
	[0.009]	[0.016]	[0.011]	[0.009]	[0.005]
Hisp/Latino	0.070***	0.034**	0.013	-0.114***	-0.006
	[0.015]	[0.015]	[0.019]	[0.009]	[0.005]
Some college	0.028***	-0.055***	0.016	0.010**	0.003
	[0.009]	[0.012]	[0.010]	[0.005]	[0.003]
Family					
Has male spouse/partner	-0.038***	0.182***	-0.081***	-0.056***	-0.004*
	[0.011]	[0.011]	[0.011]	[0.009]	[0.002]
Has female spouse/partner	-0.049***	0.246***	-0.109***	-0.081***	-0.007
	[0.013]	[0.0014]	[0.013]	[0.009]	[0.004]
Has son	0.001	0.041***	-0.0123**	-0.019**	0.000
	[0.007]	[0.012]	[0.010]	[0.007]	[0.003]
Has daughter	0.004	0.042***	-0.014	-0.031***	-0.002
	[0.006]	[0.007]	[0.009]	[0.008]	[0.002]
Health and functioning					
Dementia	-0.042**	-0.085***	-0.059***	0.187***	0.003
	[0.016]	[0.014]	[0.014]	[0.012]	[0.003]
Other Cog Impairment	0.007	0.024**	-0.084***	0.051***	0.003*
	[0.008]	[0.011]	[0.011]	[0.006]	[0.002]
Stroke History	0.033***	0.006	-0.069***	0.031***	0.003
	[0.011]	[0.011]	[0.008]	[0.008]	[0.003]
Lung disease	0.026***	0.018	-0.008	-0.035***	-0.001
	[0.008]	[0.011]	[0.010]	[0.008]	[0.002]
Heart problem	0.003	0.032***	-0.015**	-0.021***	0.000
	[0.006]	[0.007]	[0.007]	[0.005]	[0.002]
Diabetes	0.002	0.033***	-0.037***	0.002	0.000
	[0.006]	[0.010]	[0.008]	[0.006]	[0.002]
# Mobility Diffs	0.005***	0.029***	-0.028***	-0.006***	0.001
	[0.001]	][0.002]	[0.002]	[0.001]	[0.000]
# ADL Diffs	0.022***	-0.036***	-0.051***	0.066***	-0.000
	[0.003]	[0.003]	[0.003]	[0.004]	[0.001]
# IADL Diffs	0.043***	0.082***	-0.130***	0.007*	-0.002
	[0.004]	[0.004]	[0.003]	[0.004]	[0.001]
Constant	-0.260***	-0.035	1.590***	-0.227***	-0.078***
	[0.046]	[0.058]	[0.045]	[0.037]	[0.016]
Depvar Mean	0.129	0.324	0.396	0.142	0.012
N	21918	21918	21918	21918	21918

Notes: Sample includes all senior-year observations where senior reports having at least one ADL difficulty in 2000-2016 panels of HRS. Coefficients are from OLS estimation of the difference-in-differences specification with sample survey weights. State fixed effects were included but omitted from the table. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.3: *Effect of Medicaid SPPC benefit on hours of care received per day by ADL-impaired seniors, by whether caregiver is paid and relationship of caregiver to senior*

				Relatives					
	(1) Paid	(2) Unpaid	(3) Non-rel	(4) All	(5) Wife	(6) Husband	(7) Daughter	(8) Son	(9) Other
$PC_s \times IncElig_{it}$	2.844***	-1.882*	2.241***	-1.321	-0.050	-0.484	-0.581	-0.135	-0.507
	[0.677]	[1.036]	[0.680]	[1.090]	[0.621]	[0.443]	[0.094]	[0.047]	[0.062]
$IncElig_{it}$	-2.193***	0.918	-1.968***	0.776	-0.672	-0.160	1.166***	-0.330	1.044**
	[0.560]	[0.811]	[0.556]	0.850]	[0.421]	[0.200]	[0.351]	[0.234]	[0.445]
Dep. Var. Mean	4.355	13.778	3.885	14.231	3.695	2.874	4.328	1.556	2.323
N	21918	21918	21918	21918	21918	21918	21918	21918	21918

Notes: Sample includes all senior-year observations where senior reports having at least one ADL difficulty in 2000-2016 panels of HRS. Coefficients are from OLS estimation of the difference-in-differences specification with sample survey weights. Regressions also contain state fixed effects and demographic and health controls shown in Table 1.2. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of the care that would otherwise be provided by relatives. In columns 3 and 4, I show that the latter appears to be the case: the policy raises non-relative hours by 2.2 hours per day, and decreases relative hours by 1.3 hours per day. These estimates suggest that the policy significantly changes who is providing care to seniors with ADL impairments, replacing care from (unpaid) relatives with care from (paid) non-relatives. Finally, columns 5-9 attempt to allocate the reduction in relative care across different types of relatives. These estimates are all noisy, but the point estimates suggest that that husbands and daughters can account for most of the reduction in care from relatives.

Table 1.4 investigates whether the availability of paid home care through the Medicaid SPPC benefit alters the living arrangements of ADL-impaired seniors. Panel A presents results from the same difference-in-differences specification shown above, where each of the columns represents a mutually exclusive category of living arrangement. The results suggest that likely-Medicaid eligible seniors are more than four percentage points more likely to live on their own (column 1) as a result of the SPPC benefit. This increased rate of living independently appears to be due to reductions in the rates of living with one or more of their children (column 2), living with a non-child household member (column 3), and living in a nursing home (column 4), though none of these effects is statistically significant on their own. Panel B expands the analysis sample to include all HRS respondents over 65

Table 1.4: *Effect of Medicaid SPPC benefit on living arrangements of seniors with ADL impairments*

	Private housing			Residential care	
	(1) Alone or w/ partner	(2) With child	(3) Other	(4) Nursing Home	(5) Other
Panel A. Seniors with ADL impairments					
$\text{PC}_s \times \text{IncElig}_{it}$	0.048***	-0.021	-0.012	-0.015	-0.000
	[0.014]	[0.013]	[0.011]	[0.014]	[0.005]
IncElig_{it}	-0.065***	0.035***	0.007	0.019*	0.002
	[0.010]	[0.010]	[0.010]	[0.010]	[0.004]
Dep. Var Mean	0.583	0.203	0.061	0.142	0.012
N	21918	21918	21918	21918	21918
Panel B. All seniors					
$\text{PC}_s \times \text{IncElig}_{it} \times \text{ADL}_{it}$	0.064***	-0.036*	0.001	-0.027	-0.000
	[0.024]	[0.019]	[0.013]	[0.019]	[0.007]
$\text{PC}_s \times \text{IncElig}_{it}$	-0.013	0.018	-0.015**	0.007**	0.004
	[0.017]	[0.017]	[0.007]	[0.0003]	[0.004]
$\text{PC}_s \times \text{ADL}_{it}$	0.000	0.002	-0.007	0.002	0.002
	[0.015]	[0.016]	[0.010]	[0.010]	[0.006]
$\text{IncElig}_{it} \times \text{ADL}_{it}$	-0.017	0.007	-0.011	0.022**	-0.002
	[0.015]	[0.016]	[0.010]	[0.010]	[0.006]
IncElig_{it}	-0.043***	0.030***	0.018***	-0.003	-0.001
	[0.013]	[0.013]	[0.005]	[0.002]	[0.002]
Dep. Var. Mean	0.721	0.171	0.054	0.037	0.017
N Seniors	95891	95891	95891	95891	95891

Notes: This table presents two separate regressions, a DD regression using the sample of ADL-impaired seniors, and a DDD regression using the sample of all seniors. Both regressions contain controls for senior age, gender, race/ethnicity, education, marital status, whether they have a son and/or a daughter, cognitive status, history of stroke, and counts of mobility difficulties, ADL difficulties, IADL difficulties, and chronic conditions, as in Table 1.2. Standard errors are clustered at the state level and reported in brackets. Significance: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

(with and without ADL impairments) and runs the triple difference specification outlined by Equation (1.2) above. The estimates are all similar in both sign and magnitude; if anything, they are slightly larger in absolute size; for example, the estimated effect on the probability that seniors are living independently using this specification is 6.5 percentage points. This suggests that the differences observed in Panel A are not due to overall differences in the residence patterns of low-income seniors in SPPC states relative to waiver-only states, but rather are specific to low-income seniors with ADL impairments, making it more plausible that they are related to the existence of the SPPC benefit.

Finally, Table 1.5 investigates whether the policy's effect on care choices differs based on the type of family support the senior has. This table reproduces the regressions in Table 1.2, but interacts the main effects with an indicator for one of three potential family situations: if the senior is partnered, single with kids, or single without kids. The signs of the coefficients in the first two rows (partnered seniors, and seniors with kids) generally follow the same pattern as Table 1.2, where the largest negative effect is on the probability that a senior is receiving only informal care at home. However, the third row displays a different pattern: there is no evidence of substitution away from informal care at home (column 2), and larger negative effects on the probability that a senior is receiving no help (column 3), or in residential care (columns 4 and 5), though the residential care effects are not statistically significant. While this group accounts for only ten percent of the ADL-impaired senior population, column 1 shows that they are very likely to take up paid home care as a result of the SPPC benefit, suggesting that if a policymaker is concerned with care substitution, it may be possible to target seniors who are unlikely to have other sources of care.

1.6.2 Caregiving and labor force participation among children and partners of ADL-impaired seniors

The evidence thus far indicates that offering the SPPC benefit to seniors with ADL impairments reduces the amount of care they receive from family members. In this section, I study the effects of this reduction on the activities of the caregivers by focusing on the children and partners of seniors with ADL impairments. I focus on these two groups for two reasons: (1) they provide the majority of unpaid care to seniors in my sample, (2) as discussed in Section 1.4, it is possible to use the HRS to construct samples of all partners and all children of HRS respondents, allowing for the estimation of treatment effects on these two groups.

Table 1.6 begins with children. Panel A shows the result of estimating Equation (1.3) using the sample of all children of ADL-impaired seniors in the HRS. As discussed in Section 2.4, this is the same difference-in-differences specification as above, except for the addition of controls for the demographic characteristics of the child (age, marital status,

Table 1.5: *Heterogeneity of effect of Medicaid SPPC benefit on care choices by whether senior has a partner and/or children*

	Private Housing			Residential Care	
	(1) Paid Help	(2) Unpaid Only	(3) None	(4) Nursing Home	(5) Other
$\text{PC}_s \times \text{IncElig}_{it}$					
× Partnered	0.030	-0.051*	0.023	-0.017	0.006
	[0.028]	[0.026]	[0.024]	[0.019]	[0.008]
× Single with kids	0.056***	-0.061***	0.005	0.000	-0.002
	[0.021]	[0.019]	[0.020]	[0.016]	[0.005]
× Single no kids	0.160***	0.052	-0.120***	-0.058	-0.028
	[0.061]	[0.064]	[0.059]	[0.006]	[0.020]
IncElig_{it}					
× Partnered	0.007	0.006	-0.025	0.017	0.003
	[0.024]	[0.021]	[0.018]	[0.016]	[0.004]
× Single with kids	-0.010	0.065***	-0.056***	0.002	0.001
	[0.016]	[0.014]	[0.014]	[0.012]	[0.003]
× Single no kids	0.005	-0.053	-0.061	0.092***	0.016
	[0.037]	[0.046]	[0.051]	[0.037]	[0.018]
Dep. Var. Mean	0.129	0.324	0.396	0.142	0.012
N	21918	21918	21918	21918	21918

Notes: Sample includes all senior-year observations where senior reports having at least one ADL difficulty in 2000-2016 panels of HRS. Coefficients are from OLS estimation of the difference-in-differences specification with sample survey weights. Regressions contain state-by-partner/kid status fixed effects and demographic and health controls shown in Table 1.2. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

having young kids). The regressions also interact the main effects with the child's gender, given the stark differences in caregiving behavior by gender noted earlier. To help rescale the rest of the estimates in the table as treatment-on-the-treated effects, column 1 reproduces the first column from Table 1.2 of the effect of the SPPC benefit on the probability that a parent of a daughter or parent of a son receives paid home help as a result of the policy. For parents of daughters, the effect of the SPPC benefit (8.0 percentage points) is larger than the overall average effect in Table 1.2, while for parents of sons, the effect is smaller (5.5 percentage points), despite the baseline probabilities being quite similar.

The remaining columns examine the behavior of the children, beginning with whether or not they help the impaired parent with ADLs or IADLs. Overall, 11.4% of daughters whose parents have an ADL difficulty provide unpaid help to that parent, but the SPPC benefit reduces this rate by 3.1 percentage points, more than a 25% reduction (column 2). The share of sons who provide unpaid help is significantly lower on average (5.5%), and I estimate a small negative decline of 1.3 percentage points that is not statistically significant. As shown in column 3, a much smaller share of children provide paid help to their parents (0.8 percent of daughters, and 0.2 percent of sons of seniors with ADL difficulties), but the SPPC benefit is associated with an increase of 0.7 percentage points in the probability that a daughter is a paid helper. Column 4 shows that there is also a reduction in the share of daughters who live with the impaired parent of 1.8 percentage points, from a baseline of 7.4 percentage points, which is consistent with the earlier result that the SPPC benefit reduces co-residence with children in Table 1.4.

Finally, columns 5 and 6 look at whether the child is reported to be working by the parent. I find that daughters are 3.7 percentage points more likely to be working as a result of the SPPC benefit (column 6), but that the labor supply of sons is unaffected by the policy (in fact, the point estimate is slightly negative). I find no effect of the policy on the share of daughters or sons working part-time. The survey does not specify whether providing paid care is considered work; however, comparing the sizes of the coefficients in column 3 with column 6 suggests that even if daughters are reported as working when they are paid helpers but not when they are unpaid helpers, this effect cannot explain the entirety of the labor supply effect. Together with column 1, this result implies a treatment-on-the-treated (ToT) effect of one additional daughter working full-time for every 2.4 daughters whose parent receives paid help as a result of the SPPC benefit. This is quite a large effect, potentially suggesting that the marginal seniors who use paid home care as a result of the SPPC benefit are people whose daughters may be on a labor supply margin.

Panel B re-estimates the co-residence and labor supply effects using the triple-difference specification and the larger sample of all children of seniors in the HRS. Unlike the DD specification in Panel A, this specification is able to account for unobserved factors that might lead to lower co-residence or higher labor force participation among lower-income women in SPPC states. I do not find much evidence of omitted variable bias along this dimension (which would show up in the effect of $\text{SPPC}_s \times \text{IncElig}_{it}$), and the DDD estimates of the effect of the SPPC benefit are similar to the DD estimates in Panel A. Using this specification, I estimate a ToT effect of one additional daughter working full-time per three daughters whose parents are new recipients of paid home care, a slightly smaller but still quite large and significant effect. Together, these results suggest that the SPPC benefit causes an increase in labor supply among daughters of low-income seniors with ADL difficulties, likely by reducing their caregiving burden. On the other hand, sons experience a slight reduction in caregiving, but no increase in labor supply, suggesting that unlike daughters, sons at the margin of caregiving do not appear to also be at a labor supply margin.

Table 1.7 turns to the effect on spouses and partners, again interacting the main effects by the gender of the potential caregiver (in this case, the spouse or partner). The first column reproduces the effect on the probability that an senior with ADL impairments is receiving paid help, and finds substantial heterogeneity based on the gender of their partner. While ADL-impaired seniors with male partners experience a statistically significant increase in their use of paid home care, ADL-impaired seniors with female partners do not, despite similar baseline probabilities of using paid help. Column 2 looks at whether a senior's spouse is their "primary" ADL helper (the person who they report helps them the most).

Table 1.6: *Effect of Medicaid SPPC benefit on co-residence, caregiving, and labor supply of children of seniors with ADL impairments*

	Respondent	Child of respondent				
	(1) Receives Paid Help	(2) Provides Unpaid Help	(3) Provides Paid Help	(4) Lives w/ parent	(5) Works PT	(6) Works FT
Panel A. Parent-child pairs where parent is ADL-impaired senior						
$PC_s \times IncElig_{it}$						
× Daughter	0.080***	-0.031***	0.007***	-0.018***	0.003	0.037***
	[0.021]	[0.012]	[0.003]	[0.007]	[0.008]	[0.014]
× Son	0.055***	-0.013	-0.001	-0.005	0.000	-0.026
	[0.025]	[0.008]	[0.001]	[0.010]	[0.008]	[0.019]
$IncElig_{it}$						
× Daughter	-0.018	0.036***	-0.003	0.006	-0.006	-0.049***
	[0.018]	[0.009]	[0.002]	[0.006]	[0.006]	[0.011]
× Son	0.003	-0.010	-0.001	0.005	0.003	-0.041**
	[0.020]	[0.008]	[0.001]	[0.007]	[0.006]	[0.018]
Daughter Mean	0.142	0.114	0.008	0.074	0.090	0.554
Son Mean	0.140	0.055	0.002	0.073	0.049	0.673
N	75757	75757	75757	75757	75757	71600
Panel B. Parent-child pairs where parent is any senior						
$PC_s \times IncElig_{it} \times ADL_{it}$						
× Daughter				-0.020***	0.001	0.031**
				[0.007]	[0.010]	[0.015]
× Son				-0.005	0.003	-0.020
				[0.010]	[0.008]	[0.020]
$PC_s \times IncElig_{it}$						
× Daughter				0.000	0.000	0.011
				[0.006]	[0.008]	[0.010]
× Son				0.004	-0.002	-0.009
				[0.008]	[0.005]	[0.011]
$PC_s \times ADL_{it}$						
× Daughter				0.003	0.008	-0.013
				[0.006]	[0.006]	[0.013]
× Son				0.003	-0.001	0.005
				[0.005]	[0.004]	[0.009]
$IncElig_{it} \times ADL_{it}$						
× Daughter				0.001	0.013*	-0.051***
				[0.005]	[0.007]	[0.012]
× Son				-0.001	-0.009	0.000
				[0.008]	[0.006]	[0.017]
$IncElig_{it}$						
× Daughter				0.004	-0.016***	-0.010*
				[0.004]	[0.006]	[0.006]
× Son				0.001	0.014***	-0.051***
				[0.007]	[0.003]	[0.009]
Daughter Mean				0.057	0.101	0.612
Son Mean				0.065	0.044	0.764
N				325709	325709	310446

Notes: This table presents two separate regressions, a DD regression using the sample of children of ADL-impaired seniors, and a DDD regression using the sample of children of seniors with and without ADL difficulties. Both regressions contain controls for senior demographics, family, and health, as in Table 1.2, and also controls for the child's age, sex, marital status, education, and whether they have young kids. The regressions also contain fixed effects at the state-by-child-gender level. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.7: *Effect of Medicaid SPPC benefit on use of paid care, care from spouses, and spouse labor supply for partnered ADL-impaired seniors, by gender of impaired senior's partner*

	Impaired senior	Spouse		
	(1) Receives paid help	(2) Is primary helper	(3) Works part-time	(4) Works full-time
$PC_s \times IncElig_{it}$				
× Wife	0.014	0.002	-0.010	0.001
	[0.031]	[0.043]	[0.025]	[0.026]
× Husband	0.070^{*}	-0.066^{*}	-0.011	0.010
	[0.036]	[0.035]	[0.025]	[0.034]
$IncElig_{it}$				
× Wife	0.0005	0.046	-0.010	-0.066^{***}
	[0.026]	[0.030]	[0.012]	[0.023]
× Husband	-0.006	-0.019	-0.018	-0.021
	[0.026]	[0.026]	[0.021]	[0.022]
Wife Mean	0.096	0.443	0.055	0.113
Husband Mean	0.074	0.379	0.042	0.089
N	9302	9302	9022	9302

Notes: All regressions contain State X Gender fixed effects. Coefficients are from OLS estimation of the difference-in-differences specification on the sample of partnered ADL-impaired seniors with state fixed effects. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Consistent with the small take-up of the benefit shown in column 1 among seniors with wives, the SPPC benefit does not cause a significant reduction in primary caregiving. On the other hand, there is a substantial decrease in primary caregiving among husbands. This suggests that while the supply of care by husbands is fairly "elastic" to the availability of paid home care, the supply of care by wives is less so. Columns 3 and 4 then show that in contrast to the effects on daughters, the reduction in husband caregiving does not seem to be related to their labor market behavior, which is unsurprising given that most spouses in this sample are past the usual age of retirement and not working. Together, Tables 1.6 and 1.7 show that the SPPC benefit causes the most increased use of paid home care among seniors with daughters and/or husbands, and that both daughters and husbands significantly reduce their caregiving. However, daughters also increase their labor supply as a result of the policy, while the labor supply of husbands is not affected.

1.6.3 Health and well-being effects

The previous two sections have shown that eligibility for the Medicaid SPPC benefit has a significant effect on where and from whom seniors receive their care, how much care their family members (particularly husbands and daughters) provide, and whether their daughters are working. This section assesses whether these changes affect the health and well-being of seniors or their family members.

Beginning with ADL-impaired seniors, the switch toward more paid home care and less unpaid home care and nursing home care could plausibly have both positive or negative implications for senior health and well-being. On the one hand, one might expect that seniors who are able to live independently are happier and healthier than those who rely on family members or nursing homes for care. If paid care is more skilled than care from informal care providers, one might also expect fewer adverse health events or lower mortality as a result of the SPPC benefit. On the other hand, if paid home care is lower quality, or if they prefer receiving care from and living with their children, they may experience negative health and well-being effects. Table 1.8 tests these different hypotheses. The measures we use are how they self-rate their health (poor, fair, good, very good, or excellent), their score on the Center of Epidemiological Studies Depression Scale (column 2), and their Diener life satisfaction score (column 3).[14] We convert these scores into binary measures using natural or conventional thresholds. Columns 4-6 focus on adverse event measures of health measured over the course of the two years following the survey: whether the senior experiences a decline in functioning, has a hospital stay, or dies. I find no statistically significant impacts of the benefit on any of these outcomes, and the estimates are fairly noisy, so unfortunately, it is not possible to rule out fairly large positive or negative ToT effects. These results could either reflect the presence of heterogeneous treatment effects, where some seniors experienced positive effects and others experienced negative effects, or they may indicate that most of the treated population does not experience any significant

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[14] This survey is an optional module that was only fielded beginning in 2004. It is completed by roughly 28% of respondents and this sample is non-random (may be subject to selection bias) and the results should therefore be interpreted with caution.

Table 1.8: *Effect of SPPC benefit on health and well-being of seniors with ADL impairments*

	Ratings and assessments			Adverse health event in next two years		
	(1) Good health	(2) Depressed	(3) High life sat.	(4) Func. decline	(5) Hospitaliz.	(6) Mortality
Panel A. Seniors with ADL impairments						
$PC_s \times IncElig_{it}$	-0.008	0.011	0.020	0.002	-0.002	-0.010
	[0.024]	[0.022]	[0.036]	[0.015]	[0.015]	[0.017]
$IncElig_{it}$	-0.049**	0.039**	-0.005	0.015	-0.004	-0.001
	[0.021]	[0.019]	[0.026]	[0.012]	[0.014]	[0.011]
Dep. Var. Mean	0.375	0.315	0.402	0.333	0.467	0.207
N	21967	16917	5110	15690	15690	20086
Panel B. All seniors						
$PC_s \times IncElig_{it} \times ADL_{it}$	-0.010	0.029	0.010	0.010	0.010	-0.017
	[0.027]	[0.024]	[0.043]	[0.016]	[0.020]	[0.018]
$PC_s \times IncElig_{it}$	0.002	-0.014	0.012	-0.005	-0.010	0.001
	[0.013]	[0.010]	[0.022]	[0.011]	[0.011]	[0.005]
$PC_s \times ADL_{it}$	0.024*	-0.017	-0.012	-0.002	-0.018	0.012
	[0.012]	[0.012]	[0.022]	[0.009]	[0.016]	[0.009]
$IncElig_{it} \times ADL_{it}$	0.048**	0.019	0.086***	-0.033***	-0.026	-0.014
	[0.021]	[0.019]	[0.022]	[0.012]	[0.016]	[0.013]
$IncElig_{it}$	-0.083***	0.039***	-0.069***	0.037***	0.014	0.006**
	[0.008]	[0.008]	[0.014]	[0.010]	[0.008]	[0.003]
Dep. Var. Mean	0.705	0.134	0.583	0.186	0.317	0.080
N	95891	86873	26799	81452	81452	89277

Notes: Standard errors are clustered at the state level and reported in brackets. Significance: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

health effects, which is not too surprising given that most seniors only experience a change in who provides them care, rather than a change in whether they are receiving care or how much care they are receiving.

Table 1.9 looks at the effects on the health and well-being of children and spouses, using the same subjective well-being measures as above, and replacing the adverse event measures with two additional questions: first, whether or not the child or spouse reports ever being diagnosed with a psychiatric issue such as anxiety or depression, and second, whether or not the child or spouse reports being troubled with pain. Panel A begins with children, using the limited sample and alternative specification outlined by Equation 1.4. The first two rows show that there are again no statistically significant impacts on health and well-being for spouses or daughters, although the signs and point estimates for daughters are suggestive of potentially positive effects. The second two rows show that having a parent who has personal care needs are associated with significantly worse health and well-being outcomes, particularly for daughters. Panel B looks at effects of spouses, using the preferred DDD

Table 1.9: *Effect of SPPC benefit on health and well-being of children and partners of seniors with ADL impairments*

	(1) Good health	(2) Depressed	(3) High life sat.	(4) Psych problem	(5) Troubled w/ pain
Panel A. Effects on children between 50-65					
$PC_s \times ParentDiff_{it}$					
× Daughter	0.041	-0.029	-0.037	-0.046	-0.089
	[0.06]	[0.04]	[0.09]	[0.05]	[0.06]
× Son	-0.037	0.009	-0.041	0.013	0.037
	[0.06]	[0.04]	[0.09]	[0.04]	[0.06]
$ParentDiff_{it}$					
× Daughter	-0.103^{***}	0.135^{***}	0.030	0.088^{**}	0.098^{*}
	[0.04]	[0.03]	[0.06]	[0.04]	[0.05]
× Son	-0.075^{*}	0.094^{***}	-0.018	0.026	0.096^{*}
	[0.04]	[0.03]	[0.07]	[0.03]	[0.05]
Dep. Var. Mean	0.66	0.23	0.43	0.24	0.44
N	16591	15754	4028	16591	16591
Panel B. Effects on spouses					
$PC_s \times IncElig_{it} \times ADL_{it}$					
× Wife	-0.060	-0.007	-0.013	-0.048	0.009
	[0.054]	[0.042]	[0.081]	[0.041]	[0.047]
× Husband	-0.069	0.026	-0.111	-0.015	-0.029
	[0.047]	[0.047]	[0.106]	[0.042]	[0.043]
Dep. Var. Mean	0.63	0.18	0.49	0.20	0.31
N	53791	49589	15928	47547	53791

Notes: Sample in Panel A contains all respondents between the ages of 50 and 65. Sample in Panel B contains all partners of an HRS respondent over 65. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

specification for these outcomes, but again finds no statistically significant effects on wives or husbands.

1.7 Robustness Checks

This section probes the robustness of the findings in the paper to potential sources of bias and alternative empirical specifications. In general, the focus will be on three results: (1) the large and positive effect of the SPPC policy on paid home care utilization, (2) the negative effect on nursing home use (which has important policy relevance, but is not statistically significant), and (3) the positive effect on daughter labor supply.

1.7.1 Omitted variable bias

One threat to the identification strategy in this paper is that other factors may drive differences between treatment and control states in the outcomes of low-income ADL-impaired seniors and their daughters. For example, SPPC states might be richer states that spend more on all kinds of health care, thus enabling more seniors to live independently and their daughters to work more. While it is not possible to completely rule all confounders, Table 1.10 summarizes some potential confounders to help us understand the likelihood of this type of bias.

The first row uses data from the Dartmouth Atlas to show that SPPC states have slightly lower adjusted Medicare spending per enrollee than waiver-only states, but the difference is small and not statistically significant. I also specifically look at Medicare spending on home health care, which may reflect the propensity of providers to recommend home care over inpatient care or the robustness of the supply of aides in a state. Again, I find slightly lower adjusted home health spending in SPPC states, but the difference is not statistically significant. More broadly, higher income states or states with larger safety nets for low-income individuals may have better outcomes for low-income ADL-impaired seniors and their daughters. The next two rows of Table 1.10 show that SPPC states are slightly wealthier than waiver-only states, but have roughly equivalent Democratic voting shares in the past few presidential elections (both of which may proxy for the size of the safety net). Again, these differences are not statistically significant. Finally, the fifth row looks at a measure of cultural attitudes toward female labor force participation “outside the home” from the General Social Survey, and does not find significant differences between treatment and control states. On the other hand, the last two rows look at Medicaid spending on nursing homes and home and community-based care. SPPC states have almost double as much Medicaid spending per senior on home and community-based care as waiver-only states, and lower nursing home spending (though the second difference is not statistically significant). These differences are likely at least due in part to the adoption of the SPPC benefit, though they may pluasibly reflect other Medicaid long-term care policies as well. For this reason, a more conservative interpretation of the results is that the estimates above reflect the effect of more generous home and community-based care policies in Medicaid.

Taken collectively, Table 1.10 suggests that there are not large differences between SPPC

Table 1.10: *Average characteristics of SPPC states and waiver-only states*

	SPPC states	Waiver-only states	Difference in means
Medicare spending	9,854	10,205	-352
Medicare home health spending	462	500	-38
Median income	31,115	29,757	1,358
Dem vote share (2000-2016)	.47	.47	0.002
Share pop favorable twd. female LFP	0.68	0.66	0.02
Medicaid NH spending	3,282	3,627	-346
Medicaid HCBS spending	2,214	1,046	1,169*
N	25	21	

Notes: This table calculates state-level averages of various state characteristics for SPPC states, waiver-only states, and then calculates the difference-in-means for each variable. Significant differences from a t-test of this difference are marked with an asterisk (*).

states and waiver-only states in many plausible confounders, making it unlikely that these variables are driving the results. Table A.4 in the Appendix tests this formally, by running a "horse race" regression that includes the interaction of being in the top half of states for each of these potential confounders with the individual variation in income eligibility and ADL impairments to see if the main effect disappears once these controls are added. I find that including these additional variables does not substantially change the estimates of the main effects.

1.7.2 Sensitivity analyses

This section probes the robustness of the result to two of the design decisions I made as the researcher by (1) including late adoption states in the sample, and (2) using alternative definitions of the treatment and control income groups.

The main specifications dropped the five late adoption states from the sample, because the programs in those states are likely to be less established, and so the states might look like a mixture of treatment and control states and be harder to interpret. However, to show that the results are not driven by this decision, Table A.6 in the Appendix re-estimates the main results in a sample that includes the five late adoption states, coding them as treated in any years following their adoption of the SPPC benefit and control before, and shows that this alternative design does not substantially affect the results.

One might also be concerned that the results are sensitive to the particular income threshold I chose to define the treatment group of likely-Medicaid-eligibles. Figure 1.4 shows that Medicaid coverage appears to decline slowly with income above 75% FPL, which would be fairly consistent with the eligibility rules. If the results are driven entirely by people whose incomes fall between 75-125% FPL, one might worry that the results are caused by something other than differences in state Medicaid policy. Table 1.11 drops people from the sample whose incomes are in the "fuzzy" range of potential Medicaid eligibility (75%-150% FPL), thus defining the treatment income group as anyone whose income is below 75% FPL and the control group as anyone whose income is above 150% FPL. In effect, this compares people who are very poor and likely to automatically qualify for Medicaid to people who are almost definitely not eligible for Medicaid. The main results are robust to this specification, and the magnitudes of the estimates are actually larger, consistent with the hypothesis that only some individuals under the looser definition of treatment actually have access to paid home care. In this sample, the nursing home effect is statistically significant, and implies a reduction of one fewer nursing home residents per 2.8 additional people receiving home care.

Table A.5 in the Appendix presents an alternative specification that drops people whose incomes are above 300% FPL. I do this because one might worry that people with high incomes are not a reasonable control group for people whose incomes are below 0-125% FPL, and may be more likely to participate in the private pay home care market, which could also be affected by geography. The results in this sample are somewhat smaller and less precisely estimated, but still tell the same story.

1.7.3 Permutation tests

Finally, we may be concerned that non-homoskedasticity in the errors will lead to over-rejection of the null hypothesis that there is no effect of state Medicaid policy on the use of paid home care, nursing home residence, or female labor supply. While I cluster standard

Table 1.11: *Main results estimated on sample that excludes people with incomes between 75-150% FPL*

	(1) Paid Helper	(2) NH Resident	(3) Daughter Works FT
$\text{PC}_s \times \text{IncElig}_{it}$	0.084***	-0.029*	0.043*
	[0.024]	[0.023]	[0.024]
IncElig_{it}	0.005	0.026	-0.091***
	[0.021]	[0.018]	[0.022]
Dep. Var Mean	0.113	0.128	0.571
N	14830	14830	24248

Notes: Sample includes all senior-year observations where senior reports having at least one ADL difficulty in 2000-2016 panels of HRS and whose incomes are not between 75-150% FPL. Coefficients are from OLS estimation of the difference-in-differences specification with sample survey weights. Regressions also contain state fixed effects and demographic and health controls shown in Table 1.2. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

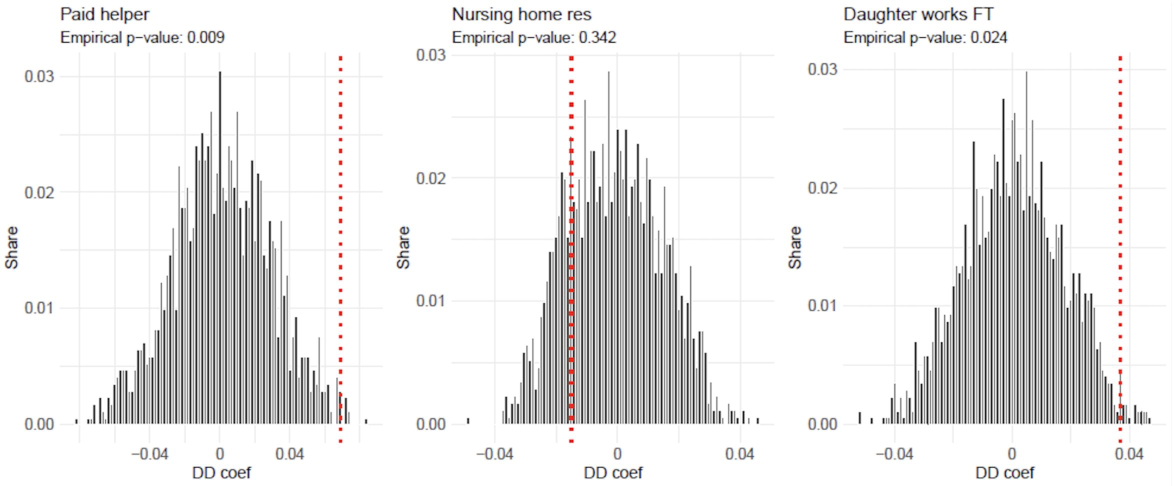
errors at the state level in order to account for this, I also do a randomization test of the main results of the paper to test this possibility non-parametrically. I run a series of regressions equivalent to my baseline difference-in-differences specification, except in each run, I randomly sample 25 states to be "treatment" states. The estimated coefficients from 2,000 of these regressions are shown in Figure 1.5, where the red line indicates the coefficient from the regressions using the true assignment of treatment and control states. Each graph also reports the empirical two-sided p-value for the true coefficient: these p-values provide fairly reassuring evidence that the effects on having a paid helper and daughter labor supply are meaningful, but as expected, we cannot reject a null effect on nursing home use.

In Appendix Figure A.2, I also provide a non-parametric test of robustness by reporting the results of 46 estimations of the baseline specification on the sample of states excluding each state in turn. This test shows that the results are not driven by any individual state.

1.8 Implications for government revenues

This paper evaluates the effect of expanding access to home care by exploiting state-level variation in whether or not personal care is an entitlement in the state's Medicaid program. Roughly, this natural experiment is similar to a policy proposed during the incoming presidential administration's campaign to end waitlists for HCBS (Gleckman, 2020). In this

Figure 1.5: *Distribution of placebo β coefficients from permutation test*



Notes: N=2,000 simulations of difference-in-differences specification where $SPPC_s$ is randomly set to be 1 for 25 treatment states in each simulation. True coefficient is marked by the red dashed line, and the empirical two-sided p-value for the true coefficient is given in the subtitle of each graph.

section, I perform a back-of-the-envelope calculation to illustrate how the estimates of this paper could be used to evaluate such a policy.

There are two main ways that the population receiving services may change if access to home care is expanded: (1) people who would otherwise use institutional care may opt to receive formal care at home instead, (2) people who otherwise would not receive any formal care may begin to receive care (the "woodwork" effect). Both populations are substantial at the policy margin that I consider in this paper. At the mean estimates, the SPPC benefit is associated with 1.5 percent fewer nursing home users per eligible senior, but 7.1 percentage points more home care users, meaning that roughly 20% of the marginal population falls into the first category, and the remaining 80% are part of the "woodwork effect" population. We can map this into government costs by using estimates from the Genworth Cost of Care Survey that put the average price of home care at $23 per hour and the average price of a semi-private room in a nursing home at $90,000 per year. Using the estimate that the SPPC benefit increases paid care by 2.8 hours per week per eligible senior, this would mean that Medicaid would pay an additional $3,335 per eligible senior for home care services and save $1,350 in nursing home costs each year. This estimate should be viewed more as an illustration, given the noise in the nursing home coefficient, but shows that even with a fairly substantial woodwork effect (80%), the savings from nursing home use can be substantial (more than 40%), given the stark difference in costs per user.

The second source of savings to the government is the fiscal externality from increased labor supply from daughters. To estimate these externalities, I re-weight average earnings among women working full-time in the ACS using the age and education levels of daughters of income-eligible ADL-impaired seniors in my sample. This produces an estimate of $50,000 in annual full-time earnings for the average woman of a similar age and education level to the potential group of affected daughters, which would result in additional federal and state income tax revenues of roughly $10,000. Taking the more conservative DDD estimate of 3.1 percent more daughters working full-time and scaling the estimate by the average number of daughters per eligible senior (1.65), I estimate additional income tax revenues of $515 per eligible senior. The final cost of the program is thus roughly $3,335-$1,350-$515=$1,470 per eligible senior, meaning that the government is able to recover roughly 56% of the program's cost due to these two offsets. Expressed in terms of participants rather than the eligible population, this means that the program's cost would be roughly $47,000 per participant before taking into account these savings, but only $20,700 per participant after taking into account these savings.

1.9 Conclusion

Medicare was created to address the lack of health insurance among many seniors at the time, leading to both limited access to health care and the potential for health events to cause significant financial distress. Today, lack of comprehensive long-term care insurance poses a similar problem, and Medicaid and families appear to bear a significant portion of the costs.

This paper identifies a source of state variation in Medicaid long-term care policy to evaluate the impact of financing home care for low-income seniors with functional difficulties. States that passed this policy have rates of paid home care utilization that are more than 50% higher than the baseline average. Notably, the total amount of care that seniors receive does not increase by much as a result of this increased access to home care. Rather, I find that this care is often used to replace care from unpaid family members, suggesting that family members may be a significant beneficiary of policies to expand home care. Focusing on daughters, who provide the most care to seniors in my sample, I find that providing low-income seniors access to paid care reduces the probability that a daughter is caring for their parent, and increases the probability that they are working full-time. For every 2.4-3 daughters of ADL-impaired parents who receive home care as a result of the policy, I estimate that one works full-time as a result of the policy.

From a policy design perspective, these results offer a few insights. First, despite their popularity, I find that waiver programs result in significantly reduced utilization of home care, relative to offering a state plan benefit. Second, my results suggest that care substitution is likely to be a significant effect of policies that expand access to home care for seniors unless the program is targeted very narrowly to people without other family support. However, this care substitution may result in positive social externalities, such as increased daughter labor supply, as in the case of the policy studied in this paper. Finally, the results follow other literature (summarized in Currie and Gahvari, 2008) in emphasizing the importance of considering how take-up of an in-kind benefit will affect the targeting of that benefit toward potential beneficiaries who have the highest marginal social benefit of program participation. In this case, my results suggest that the take-up of in-kind home care may be concentrated among seniors whose potential caregivers are on a labor supply margin, more so than seniors on the margin of entering a nursing home. However, one would imagine that take-up would be significantly different for other potential long-term care reforms, such as respite payments for family caregivers or paid family leave or a cash benefit (for example, Lieber and Lockwood (2019) compare the targeting properties of an in-kind home care benefit to a cash benefit), so a comparison of these reforms should take this into account.

Finally, this paper suggests the need to provide long-term care to a parent may be a significant uninsured risk in the population, particularly for women. Low-income seniors are particularly likely to have functional difficulties, and I find that Medicaid's provision of home care can significantly increase the labor force participation of their daughters. Future research is needed to examine whether this effect is similarly large for higher-income women, and how these policies ultimately impact women's lifetime earnings, given that parental caregiving needs often occur during peak earnings years.

Chapter 2

Do Policies to Increase Access to Treatment for Opioid Use Disorder Work?[1]

2.1 Introduction

Since the 1996 introduction of OxyContin, the extended-release oxycodone preparation marketed to treat acute or chronic pain, the rate of opioid dependence and abuse has skyrocketed in the U.S.[2] Deaths from overdoses involving opioids have climbed annually, rising from just over 10,000 per year in 2000 to 49,860 in 2019, far exceeding deaths attributed to car accidents in recent years (CDC/NCHS, 2018). The growth in fatal overdoses since 2013 has been particularly steep, owing in part to increasing consumption of synthetic opioids such as fentanyl, which is 50-100 times more potent than morphine.[3]

To date, most policy interventions and academic studies have focused on curbing prescriptions for opioids and physician prescribing behavior (e.g. Schnell, 2017; Alpert *et al.*,

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[1] Co-authored with Eric Barrette and Leemore Dafny

[2] https://www.hhs.gov/opioids/about-the-epidemic/index.html

[3] https://www.drugabuse.gov/publications/drugfacts/fentanyl

2020). There is evidence these efforts have reduced both the volume of prescriptions as well as the quantities prescribed (Bao *et al.*, 2016; Buchmueller and Carey, 2018; Sacks *et al.*, 2021)s. Given that an estimated 8-12 percent of patients prescribed opioids for chronic pain have historically developed an opioid use disorder (OUD) (Cicero *et al.*, 2014), these efforts should lead to a reduction in OUD prevalence. However, an estimated 1.6 million Americans already suffer from OUD, and there is a pressing need for additional research on how to increase access to, and utilization of, treatment—particularly medication-assisted treatment, for which there is the most robust clinical support.

This study helps to address this gap. Using data from a large commercial claims database between 2008-2017, we study treatment utilization, clinical outcomes, and spending among a sample of individuals newly diagnosed with OUD. Just 53 percent of commercially-insured individuals newly diagnosed with OUD received medication within six months of their diagnosis, and this share actually declined over our sample period (Shen *et al.*, 2020).[4] An additional 23 percent of newly diagnosed individuals received treatment that did not include medication. We study the effect of two policies designed to increase access to treatment: (1) improving insurance coverage of treatment and (2) increasing the supply of providers of medication-assisted treatment or MAT (specifically, prescribers of buprenorphine, the most commonly utilized medication).

To examine how insurance coverage affects treatment, we evaluate the effect of insurance parity laws, which require commercial plans to provide equal coverage for substance use treatment as for other medical conditions, on treatment utilization and outcomes. We use a difference-in-differences strategy that compares small group and large group enrollees before and after the Affordable Care Act extended parity requirements to small group plans effective 2014; parity was previously mandated for large group plans as of 2010 via the Mental Health Parity and Addiction Equity Act of 2008. We find that the extension of parity to small groups increased the utilization of residential treatment among small-group

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[4] As we describe below, there are three medications approved to treat OUD; our data primarily capture buprenorphine, by far the most common medication utilized.

enrollees newly diagnosed with OUD, but *decreased* the utilization of MAT, resulting in no net change in the propensity to receive any treatment. We also find no statistically significant impacts on clinical outcomes or medical spending as a result of parity.

Given the decrease in MAT utilization as a result of parity, we next consider the potential impact of MAT supply-side policies that would increase the number of clinicians eligible to prescribe buprenorphine. Examples of such policies include the hub-and-spoke model in Vermont,[5] or lowering training requirements for buprenorphine prescribers.[6] For this analysis, we collected data via a Freedom of Information Act request on the number of providers in each U.S. county who have acquired the requisite credential to prescribe buprenorphine, and created a normalized measure of provider supply, *BP MD Share*, by dividing this number by the number of "frontline" physicians in a county.[7] We estimate models relating patients' treatment decisions to *BP MD Share* in the relevant county and year, controlling for patient and plan characteristics, county and county-year covariates, and state-by-year fixed effects.[8]

We find that increases in *BP MD Share* are associated with greater utilization of MAT in the six months following diagnosis, lower utilization of medication-free outpatient treatment, and modest improvements in clinical outcomes. This analysis relies on the assumption that variation in *BP MD share* over time and across counties (controlling for the initial county *BP MD share*, as well as state-year fixed effects) is orthogonal to other determinants of patients' treatment decisions and outcomes. This assumption could be violated if, for example,

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[5] As described in Brooklyn and Sigmon (2017), the hub-and-spoke model creates centers of addiction expertise ("hubs") to increase the willingness of "spokes" (primary care providers) to provide addiction care by offering support to these providers such as intake, induction, and care for patients who destabilize.

[6] In the final days of the Trump administration, the Department of Health and Human Services issued new addiction treatment guidelines exempting physicians with a narcotics prescribing license from mandatory training before prescribing buprenorphine. The guidelines were withdrawn in early 2021 by the leadership of the Department of Health and Human Services installed by President Biden; officials reportedly cited the need for additional study.

[7] As we discuss in the text, we define "frontline" physicians as primary care physicians and psychiatrists. For all but the final year of our study period, non-physicians were not eligible to prescribe these medications.

[8] As we explain below, the data are too thin to support county fixed effects, but we include as a control variable the initial BP MD Share for each county.

counties with the largest increases in *BP MD Share* may diagnose more patients with OUD, owing to greater physician awareness and engagement. To assess the potential impact of omitted variables such as these, we confirm the results are robust to (1) excluding county-year covariates, which may be correlated with unobserved covariates; and (2) excluding patients who receive treatment on the same day of diagnosis, which (as we discuss below) may arise due to changes in physicians' diagnosing behavior (i.e., the "diagnosis margin.")

Supplementary analyses show the benefits of increasing *BP MD Share* are most pronounced when counties have below-median values of *BP MD Share*.[9] In these county-years, increases in *BP MD Share* are associated with greater utilization of MAT but not with reductions in other treatment modalities, driving an increase in the overall propensity to receive treatment and substantially larger clinical benefits than in the entire sample. In fact, the data suggest that increasing *BP MD Share* in counties at above-median levels is not associated with higher overall treatment rates nor with significant improvements in clinical outcomes, although rates of MAT do increase (at the expense of medication-free regimens).

While most prior studies utilize aggregate data including the Medicaid and uninsured population or focus specifically on the Medicaid population, there are advantages to studying the commercially insured population. First, patient churn in this population is considerably less, enabling the researcher to track outcomes over time, and to study patients conditional on prior health history (e.g., of substance use disorder). Focusing on the newly diagnosed enables us to compare individuals at similar stages of their disease across areas and over time, and isolates changes in the extensive treatment margin. Second, low-income, non-disabled adults were not generally covered by Medicaid until the Affordable Care Act expansions beginning in 2014, limiting the study period for the Medicaid population. Last, although survey data suggests OUD prevalence among commercially-insured adults is only one-third that among the uninsured and publicly insured (SAMHSA, 2017a), due to the sheer size of the commercially-insured population, the total numbers of commercially-

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[9] The national population-weighted median value of (*BP MD Share*) is 7.5 percent over the study period, 2009-2017. In these analyses, counties may shift from below to above median over time.

insured and publicly insured adults suffering from OUD are similar. Hence, any effort to increase treatment rates will need to address the barriers across a range of populations.

Overall, our results suggest that increasing the generosity of commercial insurance coverage for OUD treatment did not increase the utilization of MAT, the clinical standard of care. Rather, we find that more generous coverage increases utilization of a higher-cost care alternative, residential care, whose effectiveness has not been established in the clinical literature. In contrast, we find suggestive evidence that utilization of MAT increases with availability of physicians eligible to prescribe buprenorphine, controlling for other observable and potentially unobservable factors (i.e., via state-year fixed effects). In areas with below-median access to buprenorphine prescribers, increases in buprenorphine prescribers lead to higher treatment rates and improved clinical outcomes. In areas with above-median access, we find patients substitute away from medication-free treatment and toward MAT, meaning that the extensive margin of receiving any treatment is not significantly affected. Additional research is needed to develop and assess robust ways to increase the share of patients who receive treatment for this deadly disorder.

The paper proceeds in five additional sections. Section 2 provides background on OUD, the medications used to treat it, and prior related literature. Section 3 describes our data sources and presents key descriptive findings. Section 4 outlines both empirical analyses, and Section 5 presents the main results, robustness checks, and extensions. Section 6 concludes.

2.2 Background

2.2.1 Prevalence and treatment of opioid use disorder

The most widely cited estimates of the prevalence of substance use disorder in the U.S. derive from the National Survey on Drug Use and Health (NSDUH), which conducts face-to-face interviews with approximately 70,000 individuals each year. Using the NSDUH, the CDC estimated that 1.6 million people met the criteria for an opioid use disorder (OUD) in

2019. A larger population is estimated to have misused opioids (10.1 million) but did not meet the criteria for an OUD.[10]

OUD is treated by a diverse set of providers and therapies. A key distinction between treatment types is whether or not they include medication, often referred to as"medication-assisted treatment," or MAT. Two types of medications are currently approved for the long-term treatment of opioid dependence: opioid agonists and opioid antagonists. Opioid agonists are themselves opioids, but they activate opioid receptors for longer durations and at a lower intensity than analgesic opioids, and thus prevent withdrawal symptoms without producing the same euphoric effect. The most studied agonist is methadone, which was approved for the treatment of opioid dependence in 1972, and is taken orally once per day. Buprenorphine (also sold under brand names such as Suboxone and Zubsolv) was approved by the FDA for treatment of opioid addiction in 2002. Buprenorphine is a partial agonist, meaning that it has a ceiling effect, thus limiting its potential for abuse relative to methadone. In addition, it is commonly delivered in combination form with naloxone, which prevents the opioid from acting if injected, further decreasing its risk of abuse. While methadone can only be dispensed in federally licensed facilities, buprenorphine can be prescribed for take-home use by physicians who obtain a waiver from the Drug Enforcement Administration (DEA).[11] The second type of medication used to treat OUD are opioid antagonists, which block, rather than activate, opioid receptors so that patients who take antagonists and then use an opioid do not experience the opioid's effects. The only FDA-approved antagonist is naltrexone. The extended release version (a monthly injection with the brand name Vivitrol) was approved to treat OUD in 2010. Because it has no abuse potential, Vivitrol is often

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[10] The Diagnostic and Statistical Manual of Mental Disorders (DSM-5), defines a diagnosis of "substance use disorder" as "when recurrent use causes clinically and functionally significant impairment, such as health problems, disability, and failure to meet major responsibilities." Diagnostic criteria include evidence of impaired control, social impairment, risky use, and pharmacological criteria. (see Appendix B.1 for further details) This definition is distinct from other commonly used terms such as "dependence" or "misuse." Misuse is defined as use in any way not directed by a doctor, including use without a prescription of one's own, and use in greater amounts, more often, or for a lengthier period of time than prescribed.

[11] Buprenorphine is available as a sublingual film or tablet (typically taken once or twice daily), and since early 2018, as a monthly arm patch.

favored for use in criminal justice settings.

Most medication-assisted therapy is known as "maintenance therapy," meaning that patients are expected to continue taking the medication for an extended period of time (possibly indefinitely).[12] In contrast, medication-free treatment generally begins with detoxification and is followed by abstinence-supporting care. This care can be delivered in an inpatient or residential setting, or in varying intensities on an outpatient basis.[13]

An interesting feature of substance abuse treatment is that a substantial amount of care is delivered by specialty providers, including dedicated substance abuse treatment facilities. These providers are generally licensed by states, which vary in their licensing requirements. However, in many states, it is not necessary to have a prescribing professional on staff to be a licensed treatment provider. In addition, many facilities also follow an abstinence model of treatment. During our study period, the share of substance abuse treatment facilities offering medication-assisted treatment increased from just 22% in 2009 to a (still low) 38% in 2017 (SAMSHA, 2019).

A significant body of clinical research supports medication-assisted treatment (MAT).[14] In clinical trials that compare patients receiving these medications with a control placebo group, those receiving MAT with agonists experienced significant reductions in other opioid use (as measured through hair or urine tests, or self-reports) and increased retention in treatment programs.[15] There are far fewer studies of extended-release naltrexone, but the

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[12] Methadone and buprenorphine are sometimes used for detoxification but not for maintenance, meaning that patients are given methadone and buprenorphine to manage withdrawal symptoms in either an inpatient or outpatient setting and gradually tapered over the course of the detoxification period. (SAMHSA (2017b) estimates that this treatment accounts for less than 10% of admissions using medication).

[13] Outpatient care includes higher intensity "day treatment" (known as "partial hospitalization" if the program exceeds 20 hours of care per week or "intensive outpatient treatment" for programs of 9-20 hours per week)' as well as standard outpatient programs (<9 hours per week) (American Society of Addiction Medicine, 2018). Many treatment programs take a "step down" approach where higher intensity services are offered to patients with the most severe disorders, who may transition over time to less intense levels of care.

[14] See Mattick *et al.* (2009) and Mattick *et al.* (2014) for a review; we summarize the evidence further in Appendix B.2.

[15] In many trials, the control and treatment groups also received additional services such as psychotherapy, as is generally recommended, but studies comparing MAT with and without psychotherapy generally fail to find a statistically significant difference in outcomes (e.g. Sigmon *et al.*, 2016).

literature thus far generally finds improved outcomes, albeit at lower rates than agonist therapy (Lee *et al.*, 2018; Connery, 2015). On the other hand, there is relatively little evidence of the effectiveness of any treatment regimen that does not include medication.

Clinical studies do not speak directly to the effects of MAT outside of tightly controlled clinical trial settings. There are non-clinical, retrospective studies that use insurance claims data to compare patients receiving MAT to other patients; these have found reductions in spending and improvements in outcomes for patients receiving MAT, but they generally do not control for selection into treatment (e.g., Kessel *et al.*, 2018; Larochelle *et al.*, 2018; Wakeman *et al.*, 2020).

2.2.2 Parity laws

In the private insurance market, so-called “parity laws" have expanded coverage for OUD treatment. Parity laws require private insurance plans to offer equivalently generous coverage (measured by cost sharing, day limits, etc.) for mental health and/or substance abuse services as they do for general medical and surgical services. The first federal parity statute was the Mental Health Parity Act of 1996 (MHPA), but it explicitly exempted substance use disorders. As a result, several states passed parity laws for substance use disorders prior to the second federal parity action, the Mental Health Parity and Addiction Equity Act (MHPAEA), which was passed in 2008 and required parity for substance use disorders effective 2010.[16] However, both MHPAEA and most (if not all) of the earlier state laws excluded individuals and small groups (i.e., groups with fewer than 50 members) (Buchmueller *et al.*, 2007). The Affordable Care Act extended parity to small groups in 2014. Because our data begins in 2008, we study this last parity action, using large group enrollees, as the control group for our difference-in-differences study.[17]

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[16] To be more precise, MHPAEA required parity for policy years beginning after October 9, 2009, but private policy years are typically calendar years. (Source: CMS Fact Sheet: The Mental Health Parity and Addiction Equity Act)

[17] Parity was also extended to individual plans, but we do not consider these enrollees in our study given other large changes to the individual insurance market by the ACA.

Evidence regarding the impact of parity laws on treatment utilization has been mixed (see Peterson and Busch, 2018, for a review). Prior studies generally make use of two different types of data sources: (1) claims data (similar to this paper), and (2) data from treatment facilities. Most studies that use claims data (including this study) find no impact of parity on the overall utilization rate of treatment, although they show that parity laws reduce patient cost-sharing and increase the use of out-of-network services (Busch *et al.*, 2014; Ettner *et al.*, 2016; McGinty *et al.*, 2015). On the other hand, studies using facility-level data generally find that the volume of “treatment admissions" increased as a result of parity (e.g., Wen *et al.*, 2013; Maclean *et al.*, 2017). Because these studies use data on treatment admissions, however, the effects will combine increases in OUD prevalence, extensive margin increases in treatment, and intensive margin increases in treatment. These data are also unlikely to capture most buprenorphine treatment, which is generally prescribed in-office. Our analysis extends prior work on the impact of parity by using a more recent natural experiment, isolating its effects on MAT, and examining health outcomes.

2.2.3 Provider supply

Another potential barrier to treatment is the availability of providers. In the case of medication-assisted treatment, provider supply may be limited by the strict regulation of medications used to treat OUD. Methadone can only be dispensed at federally licensed Opioid Treatment Programs (OTPs, commonly known as methadone clinics). Buprenorphine can be prescribed for at-home use, but only by physicians who have obtained an “X” waiver.[18] Physicians obtain waivers by completing an eight-hour training course or through board certification in addiction medicine. In 2016, nurse practitioners and physician assistants became eligible for waivers with 24 hours of training.[19] See Appendix B.3 for a

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[18] These waivers were created in the Drug Addiction Treatment Act of 2000. However, buprenporphine, the first drug to fall under the Act’s requirements, was approved by the FDA for the treatment of opioid use disorder in 2002.

[19] Waivers are subject to legislated patient limits—originally 30 for all providers; now 100 for prescribers in the first year and 275 for those who apply after their first year for an increase.

broader discussion of regulation regarding controlled substances used to treat substance abuse disorders.

The top panel of Figure 2.1 shows trends in the supply of Opioid Treatment Programs and X waivers from 2004-2017. While the number of OTPs has been fairly stable over the past fifteen years (around 1,300 nationwide), there has been a steady increase in the number of waivered prescribers since the approval of buprenorphine, with a pronounced surge after 2016. The bottom panel graphs estimates of the number of patients using methadone, buprenorphine, and naltrexone, constructed from national, public data sources. It shows that the number of patients using buprenorphine for OUD has climbed in tandem with the increase in waivered providers; we estimate it exceeded 1.2 million unique users in 2016, with significantly fewer patients using methadone (around 320,000) and naltrexone (around 30,000).

There are relatively few studies of the effects of increasing provider supply on treatment utilization and outcomes. Swensen (2015) exploits county-level changes in substance abuse facilities and finds a reduction in fatal overdose rates in counties with increases in treatment facilities. Our study is complementary: we use patient-level data, which enables us to explore the impact of interventions (both demand and supply-side) on specific treatment modalities and clinical outcomes, and allows for a richer set of controls. Another related study, Wen *et al.* (2018), shows an association between buprenorphine provider supply and buprenorphine prescribing in Medicaid at the state-level, but given the unit of observation cannot control for other policies that may vary at the state-year level. Our study includes state-year fixed effects to account for this variation.

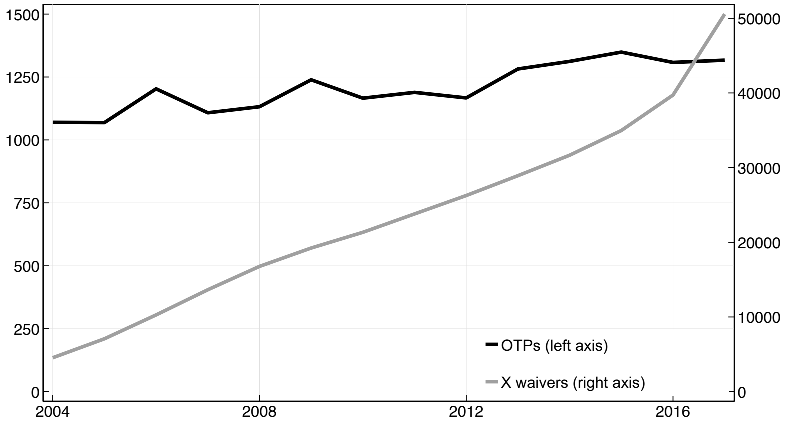
2.3 Data

2.3.1 Private Insurance Claims Data

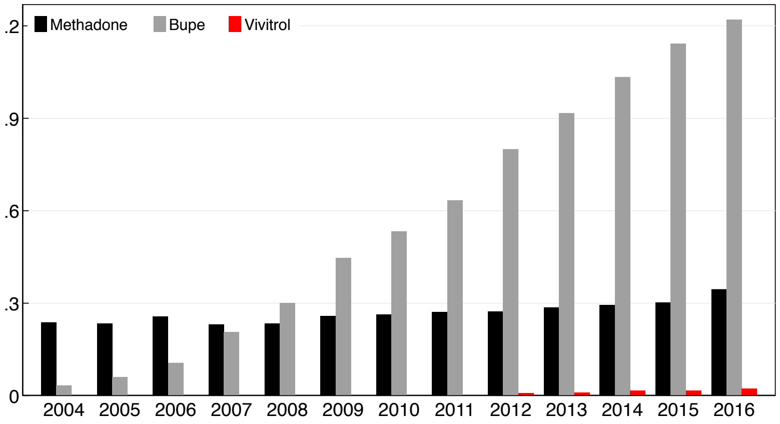
Our primary data are insurance claims from the Health Care Cost Institute (HCCI) dataset for calendar years 2008-2017. HCCI aggregates data from three large national insurers who

Figure 2.1: *Number of Opioid Treatment Programs and Waivered Prescribers, 2004-2017*

(a) *Trends in supply and use of medication-assisted treatment*



(b) *Annual number of patients using MAT, by drug (millions), 2004-2016*



Notes: The top panel plots counts of OTPs from the National Survey of Substance Abuse Treatment Services (black) and counts of X waivers (grey) from SAMHSA public data. The bottom panel shows the average number of patients using methadone (black), buprenorphine (grey), and Vivitrol (ER naltrexone; red) by year for the years 2004 through 2016. Methadone estimates are from the National Survey of Substance Abuse Treatment Services; buprenorphine estimates are from the IMS Total Patient Tracker assuming an average of 8 prescriptions per patient per year; Vivitrol estimates are from Alkermes 10K filings assuming a 50% Medicaid market share and prices of $500 per Medicaid and $1000 per private insurance dose.

jointly cover one-quarter of the non-elderly, commercially insured population and contains data on members from all fifty states and the District of Columbia. Relative to the national commercially insured population, HCCI is more concentrated in urban areas. Cooper *et al.* (2019) note that the HCCI data appears to be more geographically comprehensive than the MarketScan database, the other leading source for commercial claims data. We restrict our analysis sample to enrollees between the ages of 18 and 64 who receive medical, mental health, and prescription drug benefits from their primary insurer.[20] After applying this restriction, our baseline sample contains 12-15 million individuals per month, corresponding to about 15% of the commercially insured 18-64 population.

Identifying patients with opioid use disorder (OUD)

We identify enrollees with OUD in two ways: (1) inclusion of an OUD diagnosis on any non-lab claim;[21] (2) receipt of buprenorphine formulations used for the treatment of OUD.[22] We add the second route because prescription drug claims lack diagnosis codes, so individuals who are treating their OUD solely with buprenorphine (or whose other treatment services are not included in insurance claims, e.g. Narcotics Anonymous meetings) may not have an OUD diagnosis in our data.[23] During the study period, we find OUD prevalence in our sample increased steadily from 1.7 to 3.9 per 1,000 enrollees. By comparison, data from the NSDUH (which relies on self-reports) shows similar prevalence among the privately-insured for 2017, but a smaller increase since 2008—from 4.2 to 4.3 per 1,000 enrollees.

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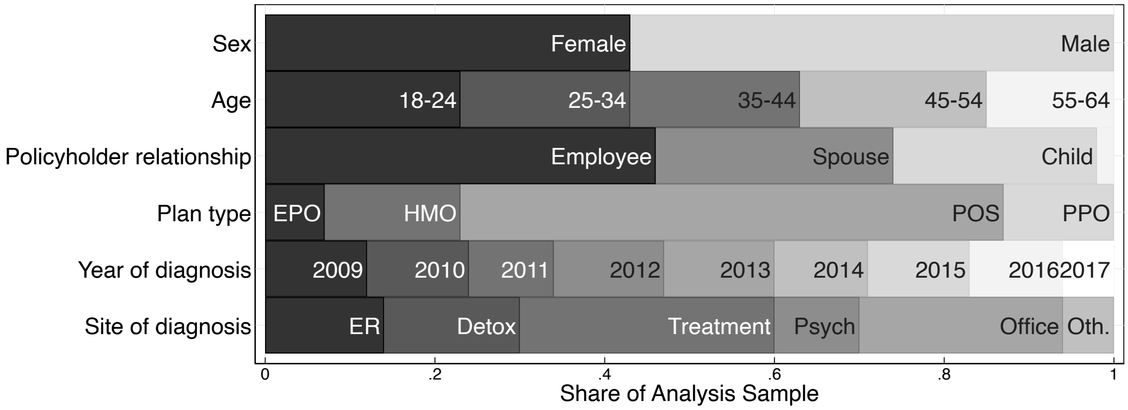
[20] Some large employers choose to “carve out" mental or pharmaceutical benefits to companies other than their insurance carrier, e.g. to a pharmaceutical benefits management company (PBM). In such cases, we lack comprehensive claims information.

[21] We exclude lab claims because diagnoses on lab claims are sometimes listed when testing for the presence of a condition, even if the test was not positive.

[22] As described in Appendix B.1, some formulations of buprenorphine are primarily used for treatment of pain while others are used for treatment of OUD; these can be identified by the specific “NDC codes" included in claims.

[23] 44% of people receiving buprenorphine in a given year do not have an associated OUD diagnosis.

Figure 2.2: *Summary statistics for new diagnosis sample*



Notes: This figure summarizes key individual-level variables for the new diagnosis sample, as defined in the text. The policyholder relationship variable is missing for 2% of the sample and thus does not sum to 1. N=78,222.

Defining a "new diagnosis" analysis sample

We restrict our analysis sample to patients with a "new" diagnosis of OUD, defined as patients whose first OUD-related claim in our data appears after a period of at least 12 months with no OUD-related claims. We further require that these patients be observed for an additional six months after diagnosis, so that we can explore the care they receive and a range of outcomes observable through claims data.[24] Limiting the sample to new diagnoses enables us to study clinical outcomes and spending for a sample of individuals with the same disease state, assuming no variation in physicians' diagnosing behavior,[25] and yields an analysis sample of approximately 9,000 patients per year.

We describe the composition of this sample in Figure 2.2. The figure confirms some of the demographic characteristics of the opioid crisis identified in prior literature: a

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[24] Note that patients need not have claims during the followup period; however, they must continue to have insurance coverage through one of the carriers in HCCI in order to be included in the analysis sample. 1.5% of those included in the new diagnosis sample have no claims during the 6-month period post-diagnosis.

[25] The inclusion of state-year fixed effects in all models should absorb much of this variation, which is also unlikely to be correlated with small versus large-group treatment decisions.

disproportionate share of the sample is male (57%) and young (43% aged 18-34, as compared to 14% of all HCCI enrollment ages 18-64). Given the incidence in the young population, it is unsurprising that a sizeable share of the new diagnosis sample (23%) receives coverage through the policy of a parent or guardian. Figure 2.2 also provides the distribution of the sample by the year and setting of the patient's initial OUD diagnosis.[26] Nearly half the sample is diagnosed in a detox or treatment setting, and 14% are diagnosed in the ER. The remainder are primarily diagnosed during a non-treatment office visit, such as a physical, psychiatric evaluation, or visit for a different medical complaint.

Treatment, spending, and clinical outcomes of patients with new OUD diagnoses

For each newly diagnosed patient, we construct indicator variables for receiving different types of treatment in the six months post-diagnosis, indicator variables for experiencing specific adverse events in the six-months post-diagnosis, and also measure total medical spending in the pre-diagnosis and post-diagnosis periods.

We define three mutually exclusive treatment categories: MAT, Medication-free Residential, and Medication-free Outpatient. Mutually exclusive categories aid us in analyzing substitution across modalities. "MAT" includes patients receiving medication at any point in the 6-month post-diagnosis period. 93% of the MAT sample received buprenorphine.[27] The MAT sample includes patients who received medication as their only source of treatment in our data (67%), as well as patients who receive other treatment services in addition to

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[26] The sample is roughly halved in size for 2017, as our claims data end in 2017 so we only have a 6-month followup period for patients diagnosed during the first half of the year. Setting aside 2017, the number of patients in the analysis sample does not increase over time, notwithstanding the previously documented increase in OUD prevalence among HCCI enrollees, because the share of HCCI enrollees receiving both mental health and prescription drug benefits through their primary insurer—a requirement for inclusion in our sample—is decreasing over time.

[27] We classify patients as receiving buprenorphine if they have at least one buprenorphine prescription drug claim in the six months following their diagnosis. Methadone and naltrexone are identified using procedure codes. Our data include relatively few methadone claims, and it is possible that some patients are paying for it out-of-pocket or receiving it free of charge. However, data from the National Survey of Substance Abuse Treatment Services suggest that only 10% of the 300,000 methadone patients per year in the US have private health insurance. IMS counts of buprenorphine suggests that about 57% of the 1.2 million buprenorphine patients in 2016 paid for their prescription using private insurance, leading to a buprenorphine utilization rate among privately insured patients that is about 20 times the utilization rate of methadone.

Table 2.1: *Mean clinical and spending outcomes by type of treatment received*

	(1) All	(2) MAT	(3) Med-free Res	(4) Med-free OP	(5) No TX
% of Sample	100.0	30.0	3.7	18.7	37.5
Clinical outcomes (%)					
Any OD	4.5	3.0	7.0	5.0	5.2
Drug-related ER	25.8	16.7	37.7	26.4	30.5
Other ER	22.8	18.0	24.1	19.1	27.0
6m Spending ($)					
Before Diagnosis	11,934	7,695	11,087	9,437	14,679
After Diagnosis	17,513	12,479	39,842	20,942	16,466
Treatment	4,921	6,463	26,016	10,686	0
Residential	484	368	9,966	0	0
Outpatient	3,760	3,844	16,050	10,686	0
RX	425	1,413	0	0	0
Non-Treatment	13,220	6,654	18,869	11,576	16,466

Notes: This table reports the mean values of the clinical and spending outcomes (in CPI-U adjusted 2017 constant dollars) for the new diagnosis sample. All patients are included in column 1 (N=78,222). Columns 2-5 subdivide the sample into four groups: column 2 includes patients who received medication-assisted treatment, column 3 includes patients who received residential treatment that did not include the use of medications, column 4 includes patients who received treatment without residential services or medication, and column 5 includes patients who did not receive any form of treatment. Clinical outcomes are expressed as the percentage of patients observed with any of the listed events in the six months following diagnosis. Spending is computed as total allowed amounts in the six months before or after the diagnosis. Details on how events and spending categories are defined are available in Appendix B.1.

medication (33%).[28] Defined this way, nearly 30% of newly diagnosed OUD patients received MAT in the six months following their diagnosis. Another 23% obtain medication-free services, which we subdivide into those receiving residential treatment (4%) and those receiving outpatient services only (19%). Table 2.1 summarizes our clinical and spending outcome measures for the full sample (column 1), and by treatment type (columns 2-5).

The first few rows report the incidence of three clinical outcome measures: overdoses, drug-related ER visits, and non-drug-related ER visits. We find that only 4.5% of our sample has an overdose claim in the six months after diagnosis; this rate likely understates the volume of overdoses because some overdoses may not result in any insurance claims (e.g., overdoses handled in the field by first responders). On the other hand, 26% of all newly-diagnosed OUD patients experiences one or more drug-related ER visits, which we define as any ER visits that include an overdose, drug dependence, poisoning, or withdrawal diagnosis code (again, excluding lab claims).[29] Finally, 23% of the sample visits the ER for non-drug-related reasons. Overall, the rate of adverse clinical outcomes post-diagnosis appears to be lowest among those receiving medication-assisted treatment (column 2), and highest among people receiving medication-free residential treatment (column 3). However, these patterns may reflect both selection into treatment as well as the causal effects of treatment.

The bottom rows of Table 2.1 summarizes average spending for each group in the six months before and after a new OUD diagnosis.[30] Average spending in the six months prior to diagnosis is high—nearly \$12,000 (in CPI-adjusted 2017 dollars)—and it increases to more than \$17,000 in the six months following diagnosis. Post-diagnosis spending

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[28] We pooled together all treatment regimens including medications due to the strong clinical evidence for MAT.

[29] We summarize the most frequent primary diagnoses for these visits in Table B.3.

[30] We assign claims to spending categories using a priority order system detailed in the Appendix that generally allocates spending to the "highest intensity" service provided on a given day. Allocating spending by day rather than by service allows us to capture treatment-related spending that is not billed as a "treatment service," such as physician billing for evaluation and management or lab services in a treatment program. However, it may also result in overcounting if patients receive non-treatment related services on the same day as a treatment service. We do not count detox as a treatment service.

for patients receiving medication-free treatment (columns 3 and 4) is significantly higher than post-diagnosis treatment spending for patients receiving MAT (column 2), reflecting both higher treatment spending and higher non-treatment spending for patients receiving medication-free treatment. However, patients receiving MAT also have lower pre-treatment spending, suggesting these raw comparisons are likely to suffer from selection bias. Notably, non-treatment spending increases post-diagnosis for all patients except those receiving MAT.[31]

2.3.2 Prescriber Data

We use the individual's zip code and diagnosis year in the claims data to merge in county-year data on several variables, including our measure of access to buprenorphine, *BP MD Share*. This measure is constructed by taking the number of providers with waivers to prescribe buprenorphine and dividing by the number of primary care physicians and psychiatrists ("frontline providers") in the county-year. The numerator is constructed from data on the practice zipcode (at the time the waiver was granted) and waiver date of every provider granted a waiver to prescribe buprenorphine, which we obtained through a FOIA request to SAMHSA.[32] We use this source to estimate the number of waivered providers in every county and year.[33] The denominator is constructed from the Area Health Resource Files.[34] Normalizing the number of providers with waivers in this way serves two purposes. First, it addresses the fact that some counties have relatively low or high availability of medical professionals. A central county may provide care for residents of outlying counties,

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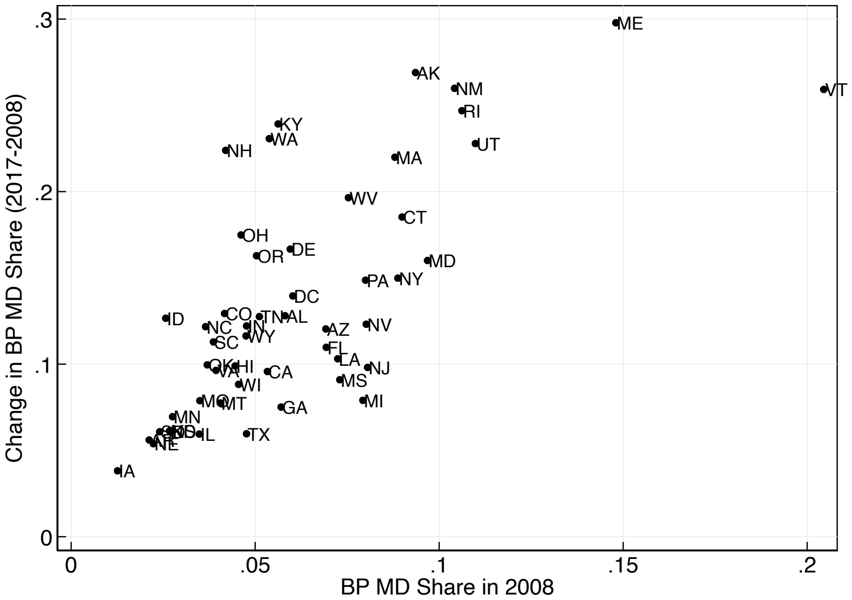
[31] Note that by construction, all pre-diagnosis spending is non-treatment spending.

[32] At present, only limited historical data is publicly available, specifically state-level counts of waivers.

[33] Unfortunately, although the data records provider moves, it does not record the date on which a move occurs. We use the first recorded location to construct *BP MD share*. In unreported results, we confirm that using the last reported location does not alter the results. Approximately 5% of providers experience at least one move in our data. We also do not observe if or when providers stop practicing or prescribing buprenorphine.

[34] The AHRF obtains these measures from the American Medical Association's *Masterfile* and defines primary care physicians as MDs and DOs who provide direct patient care and practice principally in one of the four primary care specialties: general or family practice, general internal medicine, pediatrics, and obstetrics and gynecology.

Figure 2.3: *Relationship between change in state BP MD Share (2017-2008) and initial level in 2008*



Notes: This figure plots initial (2008) *BP MD Share* against the change in *BP MD Share* between 2008 and 2017. State-level estimates are obtained by cumulating SAMHSA's reports of new waivers by state and year and thus are subject to some error due to providers becoming inactive or moving states.

and failing to normalize for the density of medical professionals could yield misleading estimates of access. Second, this normalization is appropriate in light of potential policies to address access, e.g., easing the restrictions for prescribing buprenorphine or abbreviating training. PCPs and psychiatrists are among the likeliest targets of such interventions, as they are the primary frontline physicians encountering patients with OUD.[35]

Nationally, *BP MD Share* increased from 6.2% at the end of 2008 to 18.1% at the end of 2017. There is substantial variation in *BP MD Share* across locations and over time. Figure 2.3 graphs the growth in state-level *BP MD Share* between 2008 and 2017 against the initial

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[35] Addiction medicine specialists are often psychiatrists, therefore many of these specialists are included in the denominator. One exception to the waiver process is that patients can also be dispensed buprenorphine at OTPs (although they cannot receive a prescription for take-home use unless issued by a waivered provider). However, survey data of these facilities suggest that the number of patients receiving buprenorphine at an OTP is relatively small (about 50,000 in 2015, less than 5% of the total estimated number of patients receiving buprenorphine) (Alderks, 2016) and it is likely that these OTPs may employ waivered providers. Thus, we do not make adjustments for OTP locations in our analysis.

state-level *BP MD Share* in 2008. The figure identifies some early adopters (e.g., Vermont, Maine, New Mexico, Rhode Island and Utah) as well as late adopters (e.g., New Hampshire, Idaho, Kentucky, Washington). It also shows that state-level *BP MD Share* does not converge over time: instead, there is a clear positive relationship between growth before and after 2008. This pattern implies attempts to instrument for access to buprenorphine using state-time variation (e.g., state-specific post-ACA Medicaid expansions) are unlikely to be orthogonal to other factors potentially affecting treatment utilization.[36] Thus, our analysis exploits cross-county variation in *BP MD Share*, and controls for state-year effects.

2.4 Empirical Strategy

We pursue two separate analyses of potential barriers to treatment. The first analysis exploits the extension in parity to small groups by the Affordable Care Act in 2014; as noted earlier, the MHPAEA had previously required parity for large groups in 2010. These results reveal the impact of increasing the generosity of insurance coverage (a demand-side intervention) on different types of OUD treatment. The second analysis explores the relationship between access to buprenorphine providers and patient outcomes. This analysis speaks to the impact of a more targeted, supply-side policy intervention.

2.4.1 Effects of Insurance Parity on OUD Treatment and Outcomes

To study the effect of insurance parity, we use a difference-in-differences specification that compares small group to large group enrollees before and after parity was extended to small groups in 2014. For this analysis, we apply three additional restrictions to our analysis sample of newly diagnosed OUD patients. First, we limit the estimation sample to fully-insured enrollees, so as to maximize the comparability of the treatment (small group) and control (large group) samples. Small group enrollees are nearly always enrolled in fully-insured plans, whereas the majority of large group enrollees are in self-insured plans.

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[36] Indeed, such an attempt on our part showed evidence of “pre trends," as expansion states were generally increasing access to buprenorphine both before and after the Medicaid expansion took place.

Sponsors of self-insured plans often influence the type and degree of utilization review, whereas insurers typically set these terms for fully-insured enrollees. Second, we exclude the first year of data (2009) as parity was not required uniformly of all large group plans until 2010. Finally, we also drop patients newly diagnosed during the second half of 2013 in the event their spending (partly in 2014) reflects the policy change. Summary statistics (Appendix Table D1) show that patient characteristics in this smaller sample (which still contains over 28,000 observations) are very similar to those of the full sample. Using this sample, we estimate the following specification:

$$Y_{ist} = \alpha + \beta X_i + \kappa \text{SmallGroup}_i \times \text{Post}_{it} + \omega \text{SmallGroup}_i + \gamma_{st} + \epsilon_{ist} \tag{2.1}$$

for each newly-diagnosed individual i living in state s and diagnosed in year t. The dependent variables Y_{ist} are indicator variables for different treatment modalities, spending measures, and clinical outcome measures.

The coefficient of interest is κ, which captures the differential effect of being enrolled in a small group plan after parity was extended to small groups in 2014. We control for patient and insurance characteristics X_i (sex, age band, plan type, indicator for a high-deductible healthplan), as well as state-year fixed effects γ_{st}. For dependent variables Y_{ist}, we use binary measures reflecting treatment utilization, binary measures of clinical outcomes, and continuous measures of spending. We transform spending measures using $log(x+1)$.

To explore pre-trends, we also plot coefficients κ_t from the expanded specification below:

$$Y_{ist} = \alpha + \beta X_i + \kappa_t \text{SmallGroup}_i \times \text{I(Year = t)} + \gamma_{st} + \epsilon_{ist} \tag{2.2}$$

One threat to a causal interpretation of the results is the possibility of changes in the composition of small and large group members which are also correlated with changes in utilization. For example, in other work, we report that older patients became less likely to utilize buprenorphine over this period (Shen *et al.*, 2020). If small group enrollees are skewing younger or older over time, such a trend could bias the results. Thus, in the Appendix, we also provide estimates of models where we interact each of our controls X_i

with the *Post* indicator.

2.4.2 Effects of Access to Buprenorphine Prescribers on Treatment and Outcomes

Our second analysis estimates the relationship between changes in the local availability of buprenorphine providers and the propensity of newly-diagnosed OUD patients to receive treatment (with or without buprenorphine), as well as clinical and spending outcomes.

For each patient i living in county c and diagnosed in year t, we regress treatment utilization, clinical, and spending outcome measures Y_{ict} on our measure of buprenorphine access using the following specification:

$$Y_{ict} = \alpha + \beta X_i + \delta \text{BP MD Share}_{ct} + \mu \text{BP MD Share}_{c,2008} + \lambda Z_{ct} + \gamma_{st} + \epsilon_{ict}. \tag{2.3}$$

Our main coefficient of interest is δ, which captures the relationship between the dependent variable (e.g., an indicator for receiving MAT) and BP MD Share_{ct}. In addition to the same patient-level controls X_i previously described, we also include county-year covariates Z_{ct} that may impact our outcome measures (average health care spending for commercially-insured adults, to capture variation in local price levels and healthcare utilization, and the unemployment rate, to capture economic conditions which may independently affect health and health-related spending).[37] The unemployment rate has specifically been found to be positively related to adverse opioid-related events (Hollingsworth, Ruhm and Simon, 2017).

We again include state-year fixed effects (γ_{st}) to help control for fixed and time-varying factors at the state level that may affect both *BP MD Share* and the various outcomes of interest. For example, increases in *BP MD Share* may coincide with increases in state funding for substance abuse treatment or other state initiatives that affect MAT (e.g., the state-level Medicaid expansions occurring in 2014+), which could independently affect the propensity to initiate MAT. Finally, to control for county-level unobservables that might bias

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[37] Average county-year spending is calculated using HCCI claims for all non-elderly enrollees with both medical and prescription claims data; the county-year unemployment rate is from the BLS Local Area Unemployment Statistics.

the coefficient of interest, we include the "initial" county-level *BP MD Share* (measured in 2008, the year prior to the start of the study period). We include this lagged measure in lieu of county fixed effects because the median county in our sample contains only six patients across all years. Initial *BP MD Share* will capture relevant county-specific, time-invariant characteristics that may affect both *BP MD Share* and our outcomes of interest, such as pre-existing substance abuse treatment infrastructure. Because our variable of interest (*BP MD Share*) varies at the county-year level, we report standard errors clustered by county-year.

One concern with this empirical strategy is that our sample might be endogenously selected as a response to the variable of interest. For example, if treatment capacity directly influences the diagnosis margin, the population diagnosed with OUD might be systematically different in counties experiencing greater increases in *BP MD Share*. Finkelstein *et al.* (2017) finds that regional variation in "diagnostic intensity" amplifies regional differences in estimated health. Although state-level variation (and changes) in diagnostic intensity in our setting are absorbed through fixed effects, county-level changes in diagnosis margins could pose a problem for identification. We attempt to address this concern in Section 2.5 by further restricting the sample to exclude individuals who receive treatment on the same day they are diagnosed. Around 29% of our sample is diagnosed (per the claims data) on the first day of treatment. If more providers are trained to dispense buprenorphine, they may diagnose more marginal cases of it because they have tools to help treat it; such cases are likelier to result in same-day treatment. As implied by Figure 2.2, the sample excluding these patients will primarily consist of patients diagnosed in ER or non-treatment office-based settings, including psychiatric visits. Summary statistics for this sample, in which treatment rates are quite a bit lower, as expected, are in Appendix Table D1.

A second concern is that omitted variables may bias the coefficient of interest. For example, unobserved economic conditions may both increase the prevalence of OUD and drive physicians to seek waivers, but these conditions might also reduce the likelihood that patients choose to receive treatment for their disease (perhaps due to a heightened need to work, or concerns about cost-sharing for treatment). Alternatively, increases in

BP MD Share may coincide with other county-specific policy changes or investments in treatment accessibility or capacity, in which case any changes in outcomes would also reflect these unobserved reforms. Although we cannot fully address the concerns associated with omitted variables, we present estimates of our baseline specification that exclude county-year covariates and to assess whether the results are sensitive to omitting these controls.

2.5 Results

2.5.1 Effects of Post-ACA Parity for Small Groups on OUD Treatment and Outcomes

In this section, we present results obtained by comparing the post-diagnosis treatment decisions and clinical and spending outcomes of patients with small group coverage relative to patients with large group coverage, before and after the extension of parity requirements to small groups.

Table 2.2 shows the impact of parity on the propensity for patients to receive treatment of any kind as well as specific types of treatment within six months of diagnosis using equation (2.1). While parity may have impacted all types of treatment, our prior is that the effect will be most pronounced for residential treatment, which is particularly expensive and hence likely to be subject to the most restrictive prior authorization or utilization limits before parity was mandated.[38] Column (1) shows that extending parity to small groups did not increase the probability that a newly diagnosed small-group enrollee received any treatment. Columns (2)-(4) show that in fact, parity reduced the likelihood that patients received MAT, while increasing the likelihood that patients received medication-free treatment, in particular medication-free residential treatment. The coefficient estimates on *Small Group* × *Post*, which captures the average effect of parity on small versus large-group employees,

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[38] As previously noted, the specific treatment types are defined to be mutually exclusive. Patients with medication claims at any point during the 6-month post-diagnosis period are classified under"MAT." Of the 1,368 newly diagnosed patients who received residential treatment in the sample used for the parity analysis, 285 are classified in the MAT category. Results are insensitive to adding them to the residential category.

Table 2.2: *Effect of small-group parity on treatment utilization*

	(1)		(2)		(3)		(4)	
	Any		MAT		Med-free Res		Med-free OP	
Small Group × Post	-0.009	[0.012]	-0.027**	[0.011]	0.014***	[0.005]	0.005	[0.010]
Small Group	0.025***	[0.008]	0.029***	[0.007]	0.000	[0.003]	-0.004	[0.006]
Female	-0.075***	[0.006]	-0.051***	[0.006]	-0.001	[0.002]	-0.023***	[0.005]
Age 25-34	-0.071***	[0.010]	0.077***	[0.009]	-0.030***	[0.004]	-0.117***	[0.008]
Age 35-44	-0.181***	[0.010]	-0.004	[0.009]	-0.046***	[0.004]	-0.131***	[0.008]
Age 45-54	-0.298***	[0.009]	-0.088***	[0.009]	-0.049***	[0.004]	-0.162***	[0.008]
Age 55-64	-0.385***	[0.010]	-0.135***	[0.010]	-0.060***	[0.004]	-0.190***	[0.008]
Employee	0.018***	[0.007]	0.067***	[0.006]	-0.015***	[0.003]	-0.034***	[0.006]
POS	0.021***	[0.007]	-0.008	[0.007]	0.025***	[0.003]	0.004	[0.006]
PPO	0.048***	[0.010]	0.029***	[0.010]	-0.006	[0.004]	0.026***	[0.008]
CDHP	-0.009	[0.008]	-0.018**	[0.008]	-0.002	[0.003]	0.011*	[0.007]
Constant	0.695***	[0.008]	0.309***	[0.008]	0.067***	[0.003]	0.319***	[0.007]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.53		0.31		0.04		0.18	
Adj R^2	0.12		0.08		0.04		0.05	
Obs	28325		28325		28325		28325	

Notes: This table reports OLS regression estimates of equation (1) in the text using the sample of newly diagnosed, fully-insured enrollees between 2010 and 2017 who were not diagnosed in the second half of 2013. The independent variable of interest is an indicator for being covered in a small-group plan after parity was passed. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: *Relationship between clinical outcomes and small-group parity*

	(1) Overdose		(2) Drug-related ER		(3) Other ER	
Small Group × Post	0.003	[0.005]	0.010	[0.011]	0.002	[0.010]
Small Group	-0.002	[0.003]	-0.016**	[0.007]	-0.030***	[0.007]
Female	0.005*	[0.003]	0.032***	[0.006]	0.085***	[0.005]
Age 25-34	-0.023***	[0.004]	-0.060***	[0.009]	0.039***	[0.008]
Age 35-44	-0.030***	[0.004]	-0.075***	[0.009]	0.044***	[0.008]
Age 45-54	-0.023***	[0.004]	-0.065***	[0.009]	0.056***	[0.008]
Age 55-64	-0.023***	[0.005]	-0.065***	[0.010]	0.065***	[0.009]
Employee	-0.016***	[0.003]	-0.061***	[0.006]	-0.058***	[0.006]
POS	-0.000	[0.003]	0.001	[0.006]	-0.006	[0.006]
PPO	-0.002	[0.004]	-0.024**	[0.010]	0.008	[0.009]
CDHP	0.005	[0.004]	0.020**	[0.008]	0.013*	[0.007]
Constant	0.069***	[0.004]	0.330***	[0.008]	0.184***	[0.007]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.04		0.25		0.21	
Adj R^2	0.01		0.02		0.03	
Obs	28325		28325		28325	

Notes: This table reports OLS regression estimates of equation (1) in the text using the sample of newly diagnosed, fully-insured enrollees between 2010 and 2017 who were not diagnosed in the second half of 2013. The independent variable of interest is an indicator for being covered in a small-group plan after parity was passed. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: *Relationship between spending outcomes and small-group parity*

	(1)		(2)		(3)	
	Ln(Treatment Spend)		Ln(Non-TX Spend)		Ln(Total Spend)	
Small Group × Post	-0.025	[0.098]	-0.030	[0.071]	0.005	[0.044]
Small Group	0.130**	[0.064]	-0.101**	[0.047]	-0.084***	[0.029]
Female	-0.594***	[0.050]	0.832***	[0.036]	0.373***	[0.022]
Age 25-34	-0.740***	[0.079]	0.137**	[0.058]	0.067*	[0.035]
Age 35-44	-1.685***	[0.079]	0.812***	[0.058]	0.227***	[0.035]
Age 45-54	-2.555***	[0.077]	1.446***	[0.057]	0.422***	[0.035]
Age 55-64	-3.272***	[0.086]	2.017***	[0.063]	0.679***	[0.039]
Employee	0.059	[0.056]	-0.543***	[0.041]	-0.386***	[0.025]
POS	0.334***	[0.058]	0.102**	[0.042]	0.167***	[0.026]
PPO	0.546***	[0.086]	0.027	[0.063]	0.196***	[0.038]
CDHP	-0.054	[0.069]	-0.053	[0.051]	-0.055*	[0.031]
Constant	5.513***	[0.068]	6.298***	[0.050]	8.142***	[0.031]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	4.07		7.15		8.41	
Adj R^2	0.12		0.20		0.16	
Obs	28325		28325		28325	

Notes: This table reports OLS regression estimates of equation (1) in the text using the sample of newly diagnosed, fully-insured enrollees between 2010 and 2017 who were not diagnosed in the second half of 2013. The independent variable of interest is an indicator for being covered in a small-group plan after parity was passed. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

are statistically significant at α=0.01 for residential treatment and α= 0.05 for MAT. The magnitude of the residential effect is quite large: the point estimate implies parity increases utilization of residential care by 0.014 relative to a pre-period small group mean of 0.033, or 48 percent.[39] Proportionally, the impact on medication-assisted treatment (8.7 percent of baseline) is smaller, but MAT has a much higher baseline utilization level, at 0.38 for small-group sample members during the pre-period. The net effect is that parity does not increase the odds of receiving any treatment; rather it appears to lead to substitution away from MAT and toward residential care which rarely includes it.[40]

Many of the included controls are statistically significant and of independent interest, albeit not the focus of this study. Treatment probabilities are generally decreasing with age and are lower for females. We also find that employees are more likely to receive MAT, while dependents are more likely to receive medication-free care (both residential and outpatient), even after controlling for age and gender.

To check the parallel trends assumption, we also estimated models interacting the small group indicator with individual year dummies (as represented in equation 2.2 above). Figure **??** plots the coefficient estimates on these interaction terms, where the omitted year is 2010. There is no evidence of a pre-ACA difference in utilization trends for small versus large-group enrollees. The decline in the use of MAT relative to large groups is pronounced between 2014 and 2016, and lessens in 2017, a year during which *BP MD Share* also surges.

We next examine how clinical outcomes evolved post-parity for newly diagnosed OUD patients in small-group plans relative to large-group plans. Table 2.3 presents the same difference-in-differences specification as in Table 2, but substituting the indicators for treatment with indicators for specific adverse clinical events during each patient's six-month post-diagnosis period. We do not find any significant effects of small-group parity on

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[39] The point estimate (standard error) obtained when defining "residential" to include patients who receive medication as well as residential care is 0.015, which corresponds to an increase of 43 percent relative to the pre-period small group mean of .035 using this definition for residential care.

[40] Given the small sample for residential treatment, we also estimated models using indicators for any residential treatment (i.e., with or without medication), which occurs for 4.8% of the sample. The results are qualitatively similar and available upon request.

the probability of these events, and all of the coefficient estimates are positive, suggesting that the substitution of medication-free care for MAT as a result of parity may have been harmful. Plots of yearly interactions with the small group indicator also show no evidence of pre-trends (see Appendix Figure B.1).

Table 2.4 contains the results for spending outcomes: treatment spending, non-treatment spending, and total spending. We find parity did not result in higher treatment spending among patients newly diagnosed with OUD, a result that may appear surprising in light of the high average costs of residential care. However, residential treatment is fairly rare, and the increase in spending on this category is more than offset by the reduction in MAT. We also do not find statistically significant changes in non-treatment or total spending (treatment and non-treatment combined) as a result of parity. Overall, the estimates are imprecise, however, so it is not possible to rule out sizeable changes in spending.

As discussed above, the estimates of the impact of parity may be biased if small group patients differ from large group patients in ways that are correlated with their insurance coverage, have differential trends in outcomes, and for which we do not control in the main specification. For example, if our sample of small group enrollees skews younger over time, and younger enrollees experienced different trends in utilization and outcomes in the post-parity years of our sample for reasons unrelated to parity, the coefficient estimates may be biased. As a robustness check, we supplement the specifications in Tables 2-4 with interactions between each patient-level observable and a post-parity indicator (Appendix Tables B.5-B.7). Including these controls does not change our finding of significant substitution of treatments as a result of parity, and it has relatively limited impact on any of the coefficient estimates capturing the impact of parity on clinical or spending outcomes, suggesting that such factors are unlikely to be a significant source of bias.

Collectively, these results suggest that coverage limitations present in small group policies pre-ACA constrained patients' ability to access residential care, but not MAT, and that extending parity likely decreased the use of MAT due to substitution of other treatment modalities.

Table 2.5: *Relationship between county-level access to waivered providers and OUD treatment utilization*

	(1) Any		(2) MAT		(3) Med-free Res		(4) Med-free OP	
BP MD Share$_{ct}$	0.040	[0.056]	0.167***	[0.050]	-0.036	[0.023]	-0.092**	[0.040]
BP MD Share$_{c,2008}$	0.181*	[0.095]	0.109	[0.090]	0.020	[0.032]	0.053	[0.059]
Female	-0.077***	[0.004]	-0.057***	[0.003]	-0.004***	[0.001]	-0.016***	[0.003]
Age 25-34	-0.077***	[0.006]	0.071***	[0.006]	-0.032***	[0.003]	-0.117***	[0.005]
Age 35-44	-0.184***	[0.006]	-0.010*	[0.006]	-0.041***	[0.002]	-0.134***	[0.005]
Age 45-54	-0.302***	[0.006]	-0.092***	[0.005]	-0.046***	[0.002]	-0.164***	[0.005]
Age 55-64	-0.390***	[0.006]	-0.144***	[0.005]	-0.055***	[0.003]	-0.192***	[0.005]
Employee	0.020***	[0.004]	0.047***	[0.004]	-0.010***	[0.001]	-0.016***	[0.003]
POS	0.010**	[0.005]	-0.008*	[0.004]	0.011***	[0.002]	0.007***	[0.004]
PPO	0.019**	[0.007]	0.012*	[0.006]	-0.023***	[0.003]	0.014**	[0.006]
CDHP	-0.010**	[0.005]	-0.022***	[0.004]	0.002	[0.002]	0.010**	[0.004]
Ln(AvgSpend$_{ct}$)	0.008	[0.015]	0.035***	[0.014]	0.004	[0.005]	-0.033***	[0.011]
Unemp Rate$_{ct}$	-0.007***	[0.002]	-0.001	[0.002]	-0.000	[0.001]	-0.006***	[0.001]
Constant	0.694***	[0.118]	0.041	[0.111]	0.058	[0.040]	0.624***	[0.088]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.53		0.30		0.04		0.19	
Adj R^2	0.13		0.08		0.03		0.05	
Obs	78222		78222		78222		78222	

Notes: This table reports OLS regression estimates of equation (3) in the text using the sample of newly diagnosed patients between 2009 and 2017. The independent variable is *BP MD Share*, the ratio of the number of practitioners with waivers to prescribe buprenorphine to the number of PCPs and psychiatrists in a county in that year. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c, as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.5.2 Effects of Access to Buprenorphine Prescribers on Treatment and Outcomes

In this section, we examine the relationship between increases in *BP MD Share* in a patient's county and the probability that a patient receives medication-assisted or medication-free treatment. We also explore the implications for treatment and non-treatment spending, as well as adverse health events.

Table 2.6: *Relationship between county-level access to waivered providers and OUD patient clinical outcomes*

	(1)		(2)		(3)	
	Overdose		Drug-related ER		Other ER	
BP MD Share$_{ct}$	0.024	[0.021]	-0.107**	[0.049]	-0.012	[0.044]
BP MD Share$_{c,2008}$	-0.049	[0.032]	-0.014	[0.077]	0.065	[0.074]
Female	0.009***	[0.002]	0.031***	[0.003]	0.086***	[0.003]
Age 25-34	-0.029***	[0.003]	-0.066***	[0.005]	0.050***	[0.005]
Age 35-44	-0.033***	[0.003]	-0.080***	[0.005]	0.061***	[0.005]
Age 45-54	-0.028***	[0.003]	-0.068***	[0.005]	0.059***	[0.005]
Age 55-64	-0.027***	[0.003]	-0.077***	[0.006]	0.067***	[0.006]
Employee	-0.013***	[0.002]	-0.066***	[0.004]	-0.066***	[0.003]
POS	-0.001	[0.002]	-0.004	[0.004]	0.000	[0.004]
PPO	-0.000	[0.003]	-0.013**	[0.006]	0.011	[0.006]
CDHP	0.001	[0.002]	0.008*	[0.004]	0.006	[0.004]
Ln(AvgSpend$_{ct}$)	0.011*	[0.006]	0.015	[0.012]	0.026**	[0.012]
Unemp Rate$_{ct}$	-0.001	[0.001]	0.001	[0.001]	0.002	[0.001]
Constant	-0.012	[0.045]	0.209**	[0.100]	-0.049	[0.093]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.05		0.26		0.23	
Adj. R^2	0.01		0.03		0.02	
Obs	78222		78222		78222	

Notes: This table reports OLS regression estimates of equation (3) in the text using the sample of newly diagnosed patients between 2009 and 2017. The independent variable is *BP MD Share*, the ratio of the number of practitioners with waivers to prescribe buprenorphine to the number of PCPs and psychiatrists in a county in that year. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c, as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: *Relationship between county-level access to waivered providers and OUD post-diagnosis spending*

	(1) Ln(Treatment Spend)		(2) Ln(Non-TX Spend)		(3) Ln(Total Spend)	
BP MD Share$_{ct}$	0.141	[0.465]	-0.558	[0.347]	-0.168	[0.215]
BP MD Share$_{c,2008}$	1.676^{**}	[0.780]	-0.249	[0.533]	0.047	[0.320]
Female	-0.607^{***}	[0.029]	0.846^{***}	[0.020]	0.356^{***}	[0.013]
Age 25-34	-0.821^{***}	[0.050]	0.220^{***}	[0.037]	0.076^{***}	[0.023]
Age 35-44	-1.701^{***}	[0.050]	0.950^{***}	[0.037]	0.295^{***}	[0.022]
Age 45-54	-2.626^{***}	[0.049]	1.526^{***}	[0.035]	0.472^{***}	[0.022]
Age 55-64	-3.363^{***}	[0.053]	2.018^{***}	[0.038]	0.664^{***}	[0.023]
Employee	0.099^{***}	[0.032]	-0.538^{***}	[0.022]	-0.373^{***}	[0.013]
POS	0.239^{***}	[0.037]	0.171^{***}	[0.027]	0.195^{***}	[0.017]
PPO	0.286^{***}	[0.055]	0.166^{***}	[0.040]	0.199^{***}	[0.024]
CDHP	-0.078^{*}	[0.040]	-0.003	[0.029]	-0.027	[0.018]
Ln(AvgSpend$_{ct}$)	0.178	[0.121]	0.184^{**}	[0.078]	0.246^{***}	[0.049]
Unemp Rate$_{ct}$	-0.076^{***}	[0.013]	-0.059^{***}	[0.009]	-0.051^{***}	[0.005]
Constant	4.691^{***}	[0.980]	5.261^{***}	[0.626]	6.532^{***}	[0.397]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	4.08		7.39		8.56	
Adj R^2	0.12		0.19		0.15	
Obs	78222		78222		78222	

Notes: This table reports OLS regression estimates of equation (3) in the text using the sample of newly diagnosed patients between 2009 and 2017. The dependent variables are ln(spending measure + 1). The independent variable is *BP MD Share*, the ratio of the number of practitioners with waivers to prescribe buprenorphine to the number of PCPs and psychiatrists in a county in that year. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c, as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Baseline specification

In Table 2.5, we report estimates of equation (2.3) using indicators for receiving different types of treatment in the post-period (i.e., the six months following a new diagnosis) as dependent variables. Again, we do not find that this policy lever is associated with a significant change in the probability that a newly diagnosed OUD patient receives any treatment (column 1). We again see evidence of substitution of treatments, but in the opposite direction of that observed in the parity analysis: patients are significantly likelier to receive MAT if their local access to it (as measured by *BP MD Share*) increases (column 2), and less likely to receive medication-free outpatient treatment (column 4).

We can interpret the magnitudes of the coefficient estimates by considering the effect of the median county increase *BP MD Share* between 2008-2017, 8.8 percentage points. An increase of this size is associated with an estimated increase of 1.5 percentage points (+/- 0.4 percentage points using the 95% confidence interval) in the probability that a newly diagnosed patient with OUD receives MAT. This increase is small, albeit not trivial, as compared to the overall sample mean of 30 percent. However, it is largely offset by a statistically significant decrease in medication-free outpatient therapy. Thus, while patients are significantly more likely to receive MAT when more providers can prescribe it, they are not, on average, significantly more likely to receive any treatment.

Next, we consider the relationship between an increase in *BP MD Share* and indicators for post-diagnosis clinical outcomes. We find increases in *BP MD Share* correspond to statistically-significant reductions in the share of patients with drug-related ER visits (column 2). The estimates imply a county with the median increase in *BP MD Share* experiences a reduction in drug-related ER visits of one percentage point, relative to a sample mean of 26 percentage points. We find no statistically significant relationships with the other clinical outcomes (overdose and “other" ER visits).

Table 2.7 considers the association between changes in buprenorphine provider supply and health care spending for OUD patients. The coefficient estimates are noisy and none are statistically significant at conventional levels. The point estimates suggest any modest increase in treatment spending is more than offset by reductions in non-treatment spending, yielding a net negative, but statistically insignificant, coefficient estimate for total spending. However, it is possible to rule out a total spending increase of more than 0.8% associated with the median county increase in *BP MD Share* from 2008-2017.[41] The coefficients on the control variables reveal that non-treatment spending and overall spending for newly diagnosed OUD patients are positively correlated with average healthcare spending for the entire commercially insured population in a county-year and that all three measures of healthcare spending are negatively correlated with the unemployment rate in the county. Treatment spending decreases with age, but overall spending increases. In addition, treatment spending is higher for males, but overall spending is lower.

In order to allay concerns that the diagnosis margin may be endogenous to local *BP MD Share*, we re-estimate all specifications after excluding patients who receive treatment on the same day of diagnosis (see Tables B.8-B.10 of the Appendix). Treatment utilization rates are lower for this group by construction, but we continue to find that county-level *BP MD Share* is positively associated with MAT use and negatively associated with residential and outpatient treatment use (Table B.8), although the effect sizes are smaller and less precisely estimated. We also continue to find evidence of a reduction in drug-related ER visits (Table B.9), suggesting that the clinical improvements are not driven by MAT treatment providers diagnosing less severe patients. Finally, we also find slightly stronger suggestive evidence (i.e., coefficient estimate with $p<.10$) in Table B.10 of decreased post-diagnosis total spending as a result of increased *BP MD Share* in this sample, driven by decreases in both treatment-related and non-treatment-related spending.

As a second check on the robustness of these results, we re-estimate these models after dropping the county-year control variables. To the extent that these measures are correlated with unobserved county-year factors that may also be correlated with *BP MD Share*, this exercise provides some suggestive evidence that omitted county-year factors are not driving

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[41] This upper bound is obtained by adding 1.96 standard deviations to the mean estimate (.005), multiplied by the median county increase, 0.088.

the results. The coefficient estimates of interest for all dependent variables, presented in Appendix Tables B.11-B.13 are very similar.

Nonlinear effect of BP MD Share

We hypothesize that the relationship between access to treatment and the share of providers who are waivered to prescribe buprenorphine may diminish at higher levels of *BP MD Share*. We test this hypothesis by interacting our main independent variable, *BP MD Share*, with an indicator that takes a value of 1 if a county's *BP MD Share* has surpassed the national (population-weighted) median for a county during our sample years, 7.5 percent. This effectively allows for two different slopes (one below the median and one above the median) for the relationship between the outcome variables and *BP MD Share*.

Table 2.8 displays the results of this specification for OUD treatment. Column (2) confirms our hypothesis, showing that the relationship between the probability that a newly diagnosed patient receives MAT and *BP MD Share* is three times as large when *BP MD Share* is below rather than above the median. In addition, while there is almost complete substitution toward MAT and away from other treatment regimens at above-median values of *BP MD Share*, increases in MAT in below-median counties appear to translate one-for-one into higher treatment rates. Overall, we estimate that below the national median of 7.5 percent, each increase of 1 percentage point in county *BP MD Share* is associated with a 0.4 percentage point change in the probability a an individual diagnosed with OUD receives MAT, and a similar increase in the probability they receive any treatment at all.

Tables 2.9 and 2.10 presents the same specification estimated using clinical and spending outcomes as dependent variables. In Table 2.9, column (2) shows that the negative effect on drug-related ER visits is only statistically significant below the median BP MD Share, and column (3) shows that in this region of *BP MD Share*, other ER visits decline as well. In Table 2.10, we find that treatment spending increases with *BP MD Share* when it is below the median level (column 1), which is unsurprising given the lack of treatment substitution. Nevertheless, we do not find a significant increase in total spending even in this region

Table 2.8: *Relationship between OUD treatment utilization and county-level access to waivered providers, above and below national median share*

	(1)		(2)		(3)		(4)	
	Any		MAT		Med-free Res		Med-free OP	
BP MD Share$_{ct}$								
× BP Share Below Med$_{ct}$	0.380***	[0.145]	0.389***	[0.137]	0.032	[0.054]	-0.041	[0.109]
× BP Share Above Med$_{ct}$	-0.007	[0.059]	0.110**	[0.052]	-0.037	[0.024]	-0.081*	[0.044]
BP Share Above Med$_{ct}$	0.026***	[0.010]	0.026***	[0.009]	0.003	[0.003]	-0.002	[0.007]
BP MD Share$_{c,2008}$	0.169*	[0.094]	0.098	[0.088]	0.018	[0.032]	0.053	[0.059]
Female	-0.077***	[0.004]	-0.057***	[0.003]	-0.004***	[0.001]	-0.016***	[0.003]
Age 25-34	-0.077***	[0.006]	0.072***	[0.006]	-0.032***	[0.003]	-0.117***	[0.005]
Age 35-44	-0.184***	[0.006]	-0.009*	[0.006]	-0.041***	[0.002]	-0.134***	[0.005]
Age 45-54	-0.302***	[0.006]	-0.092***	[0.005]	-0.046***	[0.002]	-0.164***	[0.005]
Age 55-64	-0.390***	[0.006]	-0.144***	[0.005]	-0.055***	[0.003]	-0.192***	[0.005]
Employee	0.020***	[0.004]	0.047***	[0.004]	-0.010***	[0.001]	-0.016***	[0.003]
POS	0.010**	[0.005]	-0.008*	[0.004]	0.011***	[0.002]	0.007*	[0.004]
PPO	0.019**	[0.007]	0.012**	[0.006]	-0.007***	[0.002]	0.014**	[0.006]
CDHP	-0.010**	[0.005]	-0.022***	[0.004]	0.002	[0.002]	0.010**	[0.004]
Ln(AvgSpend$_{ct}$)	0.012	[0.015]	0.038***	[0.014]	0.006	[0.005]	-0.032***	[0.011]
Unemp Rate$_{ct}$	-0.007***	[0.002]	-0.001	[0.002]	-0.000	[0.001]	-0.006***	[0.004]
Constant	0.641***	[0.119]	0.002	[0.111]	0.021	[0.041]	0.618***	[0.089]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.53		0.30		0.04		0.19	
Adj R^2	0.13		0.08		0.03		0.05	
Obs	78222		78222		78222		78222	

Notes: This table re-estimates equation (2.3), where the independent variable BP MD Share$_{ct}$ is interacted with indicator variables for BP MD Share$_{ct}$ being below or above the national county median across all years in the sample (population-weighted). The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: *Relationship between clinical outcomes and county-level access to waivered providers, above and below national median share*

	(1) Overdose		(2) Drug-related ER		(3) Other ER	
BP MD Share$_{ct}$						
× BP Share Below Med$_{ct}$	0.015	[0.057]	-0.320**	[0.132]	-0.392***	[0.125]
× BP Share Above Med$_{ct}$	0.024	[0.023]	-0.075	[0.051]	0.023	[0.047]
BP Share Above Med$_{ct}$	-0.000	[0.004]	-0.017**	[0.008]	-0.023***	[0.008]
BP MD Share$_{c,2008}$	-0.049	[0.031]	-0.006	[0.077]	0.077	[0.074]
Female	0.009***	[0.002]	0.031***	[0.003]	0.086***	[0.003]
Age 25-34	-0.029***	[0.003]	-0.066***	[0.005]	0.050***	[0.005]
Age 35-44	-0.033***	[0.003]	-0.080***	[0.005]	0.061***	[0.005]
Age 45-54	-0.028***	[0.003]	-0.068***	[0.005]	0.059***	[0.005]
Age 55-64	-0.027***	[0.002]	-0.077***	[0.005]	0.0667***	[0.006]
Employee	-0.013***	[0.002]	-0.066***	[0.006]	-0.066***	[0.003]
POS	-0.001	[0.00]	-0.004**	[0.004]	0.000	[0.004]
PPO	-0.000	[0.002]	-0.013***	[0.006]	0.011**	[0.006]
CDHP	0.001	[0.003]	0.008*	[0.004	0.006	[0.004]
Ln(AvgSpend$_{ct}$)	0.011*	[0.006]	0.013	[0.012]	0.021*	[0.012]
Unemp Rate$_{ct}$	-0.001	[0.001]	0.001	[0.001]	0.001	[0.001]
Constant	-0.011	[0.047]	0.244***	[0.102]	0.010	[0.094]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.05		0.26		0.23	
Adj R^2	0.01		0.03		0.02	
Obs	78222		78222		78222	

Notes: This table re-estimates equation (2.3), where the independent variable BP MD Share$_{ct}$ is interacted with indicator variables for BP MD Share$_{ct}$ being below or above the national county median across all years in the sample (population-weighted). The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Table 2.10: *Relationship between post-diagnosis spending and county-level access to waivered providers, above and below national median share*

	(1) Ln(Treatment Spend)		(2) Ln(Non-TX Spend)		(3) Ln(Total Spend)	
BP MD Share$_{ct}$						
× BP Share Below Med$_{ct}$	3.038**	[1.194]	-0.554	[0.791]	0.187	[0.488]
× BP Share Above Med$_{ct}$	-0.230	[0.495]	-0.436	[0.374]	-0.169	[0.232]
BP Share Above Med$_{ct}$	0.210***	[0.078]	-0.040	[0.052]	0.011	[0.031]
BP MD Share$_{c,2008}$	1.572**	[0.760]	-0.225	[0.527]	0.039	[0.319]
Female	-0.607***	[0.029]	0.846***	[0.020]	0.356***	[0.013]
Age 25-34	-0.821***	[0.050]	0.220***	[0.037]	0.076***	[0.023]
Age 35-44	-1.700***	[0.050]	0.950***	[0.037]	0.295***	[0.022]
Age 45-54	-2.624***	[0.049]	1.526***	[0.035]	0.472***	[0.022]
Age 55-64	-3.362***	[0.053]	2.018***	[0.038]	0.665***	[0.023]
Employee	0.098***	[0.032]	-0.538***	[0.022]	-0.373***	[0.013]
POS	0.240***	[0.037]	0.171***	[0.027]	0.195***	[0.017]
PPO	0.287***	[0.055]	0.165***	[0.040]	0.199***	[0.024]
CDHP	-0.077*	[0.040]	-0.003	[0.029]	-0.027	[0.018]
Ln(AvgSpend$_{ct}$)	0.212*	[0.121]	0.184**	[0.078]	0.250***	[0.049]
Unemp Rate$_{ct}$	-0.070***	[0.013]	-0.050***	[0.009]	-0.050***	[0.005]
Constant	4.257***	[0.992]	5.278***	[0.38]	6.481***	[0.402]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	4.08		7.39		8.56	
Adj R^2	0.12		0.19		0.15	
Obs	78222		78222		78222	

Notes: This table re-estimates equation (2.3), where the independent variables BP MD Share$_{ct}$ interacted with indicator variables for BP MD Share$_{ct}$ being below or above the national county median across all years in the sample (population-weighted). The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the county-year level and reported in brackets.

of values (column 3), presumably due to the noisy estimated reductions in non-treatment spending. These results suggest that for places with below-median shares of waivered providers, increasing this share has the potential to significantly increase MAT utilization and to improve clinical outcomes for patients with OUD, without necessarily leading to increases in total spending.

2.6 Conclusion

As the U.S. opioid crisis continues, policymakers are exploring ways to expand access to treatment. Unfortunately, there is a lack of empirical evidence to guide funding and planning. This study attempts to fill some of the gap by exploring treatment among a large sample of commercially-insured patients newly-diagnosed with OUD during the period 2009-2017.

We focus on two potential barriers to treatment: (1) insurance coverage of substance use treatment, and (2) the availability of local physicians able to prescribe buprenorphine, the OUD treatment with the most robust clinical support. Using a difference-in-differences specification exploiting the extension of insurance parity to small groups beginning in 2014, and assuming treatment utilization for newly diagnosed small and large group enrollees would otherwise have similar trends (controlling for a range of patient and plan observables), we find that parity generated significant increases in residential treatment, and a decline in medication-assisted treatment. We find no net change in the propensity for newly diagnosed patients to receive any form of treatment, nor do we find statistically significant effects on clinical or spending outcomes, although the point estimates are noisy.

To examine the impact of expanding provider supply, we consider the relationship between treatment patterns and local access to providers with waivers to prescribe buprenorphine. We find that increases in *BP MD Share* are associated with greater utilization of MAT, and in counties at below-median levels of *BP MD Share*, this increase is not offset by substitution away from other treatment modalities. In these below-median counties, we also observe clear improvements in clinical outcomes, as measured by drug-related ER visits and other ER visits, when *BP MD Share* increases. Effects on spending in these counties are unclear: treatment spending rises, but appears at least partially offset by reductions in non-medical spending, yielding noisy estimates of the impact on total spending. Overall, these findings suggest that expanding access to prescribers is likely to increase uptake of MAT, but in areas where access is already relatively high, this increase is likely to come at the expense of other treatment modalities.

Together, our findings suggest that policy interventions aimed at expanding access to treatment have impacts on the treatment modality selected, but in many settings there is no net affect on the propensity to receive any treatment. The clinical standard of care, medication-assisted treatment, appears to be more limited by provider access than by insurance coverage, while the most expensive type of care, residential care, appears to be limited by coverage restrictions. These findings likely reflect the segmented nature of substance abuse treatment, particularly with regard to medication. A significant amount of care is provided in specialty facilities, which typically lack waivered providers.[42] Going forward, future research on the determinants of the decision to seek care, and on the effectiveness of a more integrated care system for patients with OUD, would be extremely helpful in developing policy solutions.

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[42] In our sample, 31% of people who received any treatment received treatment from a specialty facility. MAT rates were 26% for those who received some treatment from a specialty facility, and 71% for those who did not receive treatment from a specialty facility.

Chapter 3

Nursing Home COVID-19 Outbreaks and Staff Neighborhood Characteristics

3.1 Introduction

The COVID-19 pandemic's overwhelming impact on nursing homes in the United States and worldwide has been well-documented (Lau-Ng *et al.*, 2020). As of November 22, 2020, of the 256,597 deaths from COVID-19 nationwide, the Center for Medicare and Medicaid Services (CMS) had recorded 72,642 were among nursing home residents. This number would already imply that 28% of deaths have been among nursing home residents; however, due to the fact that federal reporting was optional before May 10, it is likely to be a significant underestimate (Khimm and Strickler, 2020).

Numerous hypotheses have emerged about what factors affected the vulnerability of facilities to infection, with some pointing to poor management or infection control procedures, and others to specific actions and policies such as the timing of when nursing homes became locked down to visitors, state policies governing the transfer of recovering COVID-positive patients to nursing homes, and the supply of personal protective equipment

(PPE) and testing (Rau and Almendraia, 2020; Alonso-Zaldivar, 2020). Another possibility is that outbreaks are unpredictable and unpreventable and that luck and geography, rather than factors under a nursing home operator or policymaker's control, largely determined which facilities saw outbreaks and which did not.

In support of the latter hypothesis, several studies have documented the importance of county infection rates in predicting COVID-19 cases and deaths at nursing homes (Chatterjee *et al.*, 2020; Gorges and Konetzka, 2021), while most studies have not found a relationship of outbreaks with star rating or even infection control violations (Chatterjee *et al.*, 2020; Abrams *et al.*, 2020; Dean *et al.*, 2020). However, there has been considerably less evidence on whether and how much geography may matter at finer granularities, particularly as compared to other facility variables such as ownership or star rating. This type of evidence is important because it may be better able to highlight or explain disparities that exist across facilities in the same area who were thus subject to the same macro-level policies and conditions (e.g. stay-at-home orders, mask mandates, weather, and distance from initial hot spots).

The objective of this study was to examine the relationship between nursing home COVID-19 outbreaks and differences in the characteristics of the residential neighborhoods of each nursing home staff, controlling for the nursing home's county, using an eighteen-state sample and a novel measure of nursing home staff neighborhoods. These results have the potential to support and extend other literature that has documented the importance of staff transmission in COVID-19 outbreaks (Chen *et al.*, 2021), as well as to shed light on potential mechanisms to explain disparities in the impact of COVID-19 on different facilities that have been documented in the media and in other studies (Gorges and Konetzka, 2021; Gebeloff and Parker, 2020).

3.2 Methods

3.2.1 Data and Variables

The universe of study is all Medicare and Medicaid-certified skilled nursing facility (hereafter, ”nursing home”) in the eighteen sample states, defined using the CMS Nursing Home Compare database. This database provides the name and address of every certified nursing home, as well as a unique provider identifier number that allows it to be matched to other data sources.

The main outcome of interest is facility-level COVID-19 deaths during the first wave of the pandemic (before July 10, 2020). This deaths data is compiled from data published by each state’s department of health between July 5 and July 10, 2020. The eighteen states in the sample were selected primarily due to availability of this data. State data is needed because the federal data did not require facility reporting before May 17, 2020, and is therefore missing a significant number of deaths. S1 File in the supplementary material discusses this data in detail, including differences across the sample states in their reporting requirements. Because these data contained facility names, but not facility identifiers, S1 File also describes the fuzzy-matching and geocoding techniques I used to match facilities in the state data to their federal provider identifier. S1 Figure shows the timing of the data relative to the trends in total COVID-19 deaths and COVID-19 deaths among nursing home residents, as well as the timing of the federal data. This figure shows that the data occur around a local minimum in the total deaths trend—thus why we refer to this as the “first wave”—and also reveals the need for state data to study this wave, as the federal data appears to begin after most deaths in the wave had already occurred.

The Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES) data from the U.S. Census Bureau was used to measure which neighborhoods each facility’s staff live in. These data are primarily derived from state administrative records (e.g. unemployment insurance records), and commonly used to study commuting patterns. To the best of my knowledge, this is the first paper to leverage these data to measure the residential neighborhoods of nursing home staff.

The LODES Origin-Destination file provides counts of employment for every work and home census block pair in three large industry groupings (“goods producing”, “trade, transportation, and utilities”, “all other services”). Staff neighborhood measures were constructed using all workers on the nursing home’s census block in the “all other services’” category. The validity of these measures are based on the assumption that the neighborhoods of service employees on a nursing home’s census block will be representative of the neighborhoods of the nursing home’s employees. For many blocks, I hypothesize that this is likely to be true by default because a nursing home may be the only or largest source of service employment on a block. Two checks of the data support this hypothesis. First, using the Workplace Area Characteristics file from the LODES, which provides employment counts for each workplace census block in twenty (rather than three) industry groupings, I estimate that on the median block in my sample, 92% (IQR: [62%, 100%]) of service employment is in the healthcare and social assistance sector. Second, to consider the possibility that there may be other healthcare or social assistance employers that are not the nursing home on the same block (one example of this is nursing homes that are collocated with hospitals), I use an estimate of the ratio of nursing home employment to nursing home residents derived from national estimates (1.23 employees to every 1 resident) to calculate a predicted employment count for each facility. I find that on the median block, this predicted number is 58% (IQR: [37%, 91%]) of the service sector employment on the block.

For blocks where the nursing home does not comprise the majority of service-sector employment, the results will be affected only if the other service sector employees live in different types of neighborhoods than the nursing home employees. There may be reasons that this is true—for example, nursing home workers may have different demographic characteristics than other service sector employees—and reasons that it is unlikely to be true, such as if neighborhood employment patterns are heavily influenced by public transportation routes. Since it is not possible to assess this directly, in the results section, I test the robustness of my findings to excluding facilities whose predicted employment is significantly below the actual service sector employment on the block. A final potential source of error is due to the timing of the LODES data, which is published with a significant lag. The most recent data at the time of writing is a snapshot of all workers on April 1, 2017 and is thus unlikely to capture the exact neighborhoods of nursing home workers because of worker turnover or moves, but is likely to be representative of the type of neighborhoods in which the nursing home's workers are likely to reside.

I match each of the service sector residence census blocks to tract-level estimates (the finest geography available) from the American Community Survey (ACS) 5-year estimates (2014-2018) of population density, poverty, income, and use of public transportation, and employment by industry and occupation. The employment data is used to compute a predicted share of "frontline workers," following other literature (Dingel and Neiman, 2020; Tomer and Kane, 2020). Finally, weighted averages of these characteristics are taken at the facility-level for all tracts where staff live to obtain the final measures of facility staff exposures.

I collect several additional facility-level variables that may be important in explaining COVID-19 deaths to include in my analysis. Data on the most recent star ratings, ownership (for-profit, non-profit, or public), occupancy rate, and prior infection control-related violations are obtained from the Nursing Home Compare database. I also collect data on chain ownership from the Online Survey Certification and Reporting and Certification and Survey Provider Enhanced Reporting (OSCAR/CASPER) data (facility data collected by state survey agencies during annual LTCF certification inspections), resident demographics including the share of residents whose primary source of payment is Medicaid and the share of residents who are non-white from the Brown LTC Focus Database—which aggregates data from the Minimum Data Set—and data on average wages paid to Certified Nursing Assistants (CNAs) and Registered Nurses (RNs) from the Healthcare Cost Report Information System (HCRIS) data from CMS.

Data on county case counts are obtained from the New York Times COVID-19 Database, which compiles data from state and local governments and health departments to produce daily cumulative case counts at the county level since the beginning of the pandemic. In addition, to help investigate the mechanisms through which local staff residence geography may affect facility deaths, I also collect data on population case rates at finer geographies (town, zip or tract) was also collected from a subsample of eight state health departments that released this data.

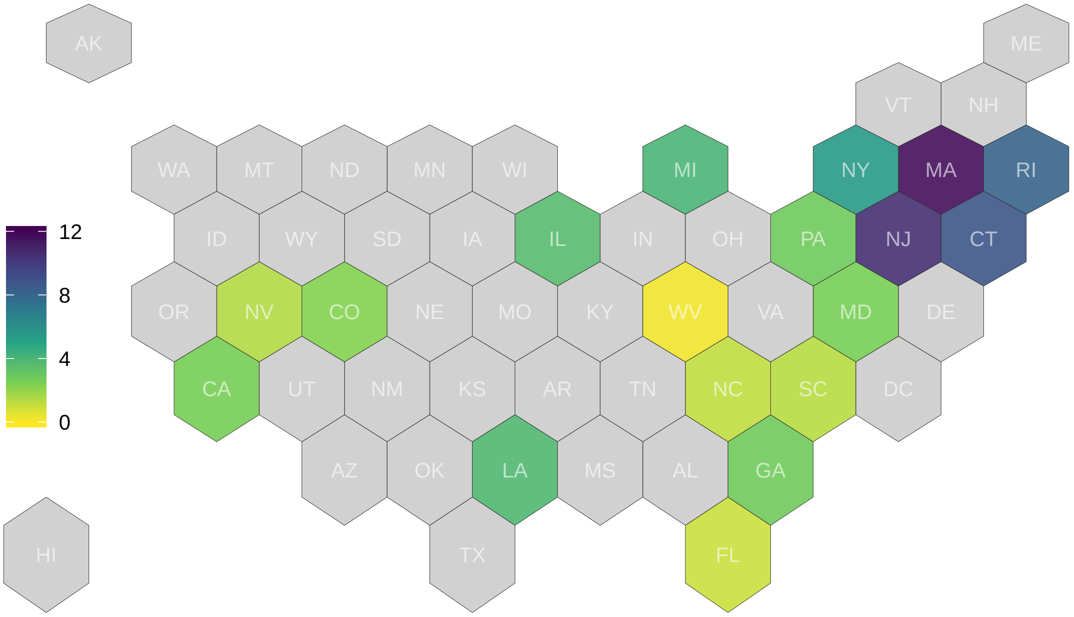
3.2.2 Statistical Analyses

The main analyses are ordinary least squares regressions where the dependent variable is the cumulative number of reported COVID-19 deaths at a facility as of early July per 100 beds. Deaths are used because they are likely to be less dependent on testing and more consistently measured across facilities. Independent continuous variables are scaled to have a standard deviation of 1 to allow for comparisons of effect sizes, while binary variables are unchanged. regressions contain county fixed effects. In general, the independent variables chosen are standard to the literature, and cross-correlations of all independent variables of interest were computed in order to avoid issues of multi-collinearity. The tract characteristics shown represented our pre-analysis hypotheses, so we do not adjust for multiple comparisons. Regression analyses were conducted using Stata 16.1. The facility-level data on deaths and tract-level neighborhood characteristics used in this paper are available at DOI 10.5281/zenodo.4308760.

3.3 Results

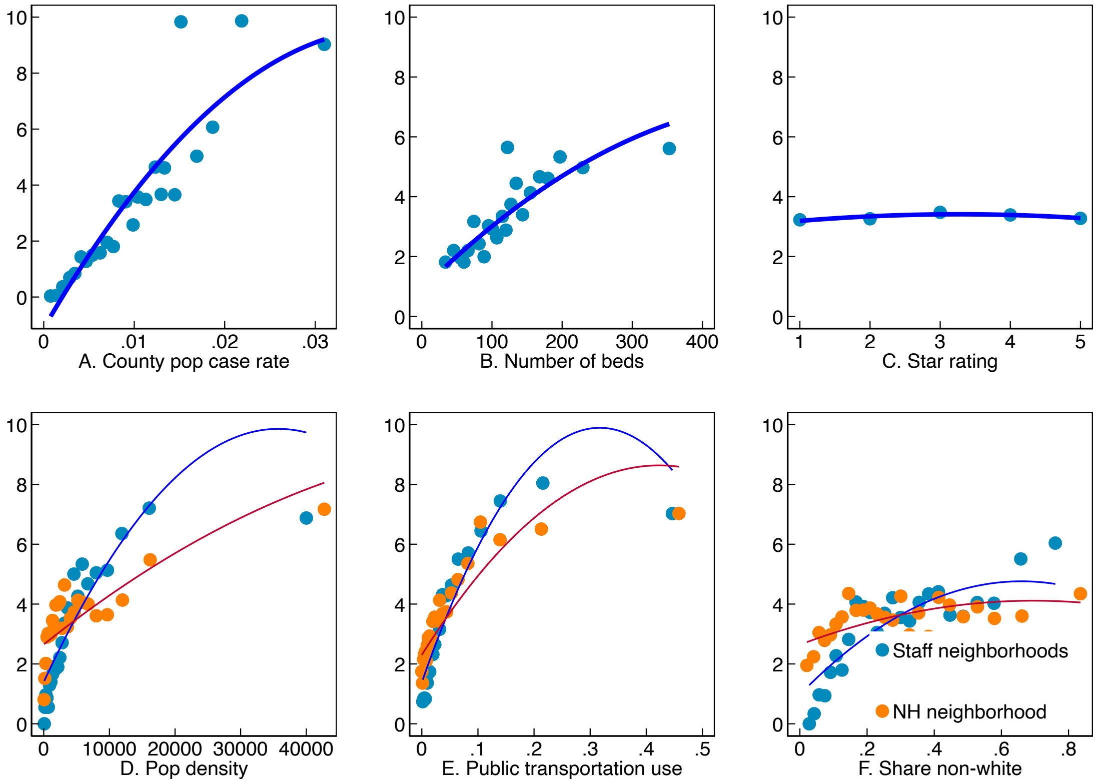
Figure 3.1 introduces the eighteen state sample used in this paper. The sample includes states from each region of the country. The impact of the first wave of the pandemic on these states varied substantially. Fig 2 summarizes the main outcome variable (facility deaths per 100 beds) by state. Overall, at the time of the data, the average nursing home in this sample had experienced 3.7 deaths per bed. For some states in the Northeast (MA, NJ, CT, RI), this number was more than 8 deaths per bed, while some states in other regions (WV, FL, NC, SC, NV) had experienced fewer than 2 deaths per bed. This variation is roughly in

Figure 3.1: *Average of facility-level first-wave COVID-19 deaths per bed in sample states, by state*



Notes: States are shaded according to the average number of deaths per bed at nursing homes in each state. States not included sample are in light grey. N=6,132.

Figure 3.2: *Relationship of facility deaths per bed with staff and nursing home neighborhood characteristics, facility size, and star rating*



Notes: Each panel of the figure shows average nursing home deaths per 100 beds on the y-axis binned by a different variable along the x-axis. Bins are of equal sizes, and the line represents a quadratic fit.

line with variation in total deaths during the first wave of the pandemic, but some of these differences may be because states varied in their reporting requirements. Since the main analysis controls for each facility's county, these differences in reporting should have less impact on the main results.

Figure 3.2 shows how the number of deaths per bed at a facility is related to a set of variables of interest. Nursing home outbreak sizes are strongly related to the county infection rate (Panel A), as well as the number of beds in the facility (Panel B). Panel C plots average death rates by star rating and finds virtually no relationship between the two variables. The bottom row of Figure 3.2 shows the relationship of facility deaths to staff neighborhood characteristics. Nursing homes are grouped by the population density, public transportation use (share of workers who commute to work on public transportation), and non-white share of the census tract where their staff live (in blue) and the tract where they are located (in orange). Facility outbreaks are strongly associated with all three of these characteristics, with steeper slopes for staff neighborhoods than the nursing home neighborhood in each case.

Table 3.1 presents models that jointly estimate the effect of facility characteristics and staff and nursing home neighborhood characteristics on facility deaths per bed. As of early July, there had been an average of 3.7 deaths per 100 beds across all facilities in the sample summary statistics for the independent variables are provided in S1 Table). The model in column (1) does not include any neighborhood characteristics, and shows that for-profit nursing homes are associated with an additional .53 deaths (SE .52, p=.007) and nursing homes that belong to chains are associated with an additional .35 deaths (SE .16, p=.033) per 100 beds. As seen above, even after scaling deaths by the number of beds in a facility, facility size continues to be an important factor in explaining death rates, as are occupancy rates, which may reflect both the mechanical effect of more residents per bed in the facility and potential crowding effects. On the other hand, there is no significant relationship of facility outbreak size with the star rating, prior infection control violations, the Medicaid share, or the non-white share after accounting for the other controls and county fixed effects. Columns (2)-(6) add characteristics of the staff and nursing home neighborhoods. The population density (column 2), public transportation use (column 3), and non-white share (column 4) of staff neighborhoods are all highly and statistically significantly associated with facility deaths per bed. For example, a one standard deviation increase in the average staff neighborhood population density is associated with an additional 1.3 deaths per 100 beds (p<.001, column 2); and comparably sized changes in staff neighborhood public transportation use, nonwhite share, and poverty rate are associated with an additional 1.4 (p<.001), 0.9 (p<.001), and 0.5 (p<.001) deaths per 100 beds, respectively (columns 3-5). In all of these cases, the same characteristic measured for the nursing home's census tract is not

Table 3.1: *Effect of staff tract measures and own tract measures on facility deaths per bed*

	(1)	(2)	(3)	(4)	(5)	(6)
Staff tract pop density		1.314***				
		[0.332]				
NH tract pop density		-0.122				
		[0.141]				
Staff tract pubtrans use			1.396***			
			[0.347]			
NH tract pubtrans use			-0.123			
			[0.177]			
Staff tract share nonwhite				0.863***		
				[0.235]		
NH tract share nonwhite				-0.075		
				[0.127]		
Staff tract pov rate					0.534**	
					[0.168]	
NH tract pov share					-0.156	
					[0.095]	
Staff tract share frontline						0.195
						[0.201]
NH tract share frontline						-0.180
						[0.111]
For-profit	0.553**	0.534**	0.552**	0.550**	0.538**	0.543**
	[0.194]	[0.194]	[0.194]	[0.194]	[0.194]	[0.194]
Chain	0.396*	0.430**	0.419**	0.409*	0.413*	0.394*
	[0.163]	[0.163]	[0.163]	[0.163]	[0.163]	[0.163]
Overall Rating	0.032	0.034	0.035	0.039	0.039	0.032
	[0.092]	[0.091]	[0.091]	[0.092]	[0.091]	[0.092]
No prior infection viol.	0.233	0.236	0.232	0.225	0.227	0.236
	[0.196]	[0.196]	[0.196]	[0.196]	[0.196]	[0.196]
Medicaid share	0.024	0.042	0.039	0.031	0.012	0.031
	[0.091]	[0.091]	[0.090]	[0.090]	[0.091]	[0.091]
Resident share nonwhite	-0.058	-0.164	-0.204	-0.265	-0.154	-0.047
	[0.117]	[0.121]	[0.123]	[0.138]	[0.125]	[0.120]
Avg severity	-0.073	-0.060	-0.047	-0.038	-0.043	-0.068
	[0.085]	[0.085]	[0.085]	[0.086]	[0.086]	[0.085]
Occupancy Rate	0.676***	0.665***	0.670***	0.677***	0.673***	0.676***
	[0.089]	[0.089]	[0.089]	[0.089]	[0.089]	[0.089]
Num beds	0.305***	0.299***	0.306***	0.296***	0.304***	0.301***
	[0.088]	[0.088]	[0.088]	[0.088]	[0.088]	[0.088]
Constant	3.019***	3.041***	3.028***	3.044***	3.025***	3.026***
	[0.180]	[0.180]	[0.180]	[0.180]	[0.180]	[0.180]
Fixed Effects	County	County	County	County	County	County
Depvar mean	3.735	3.735	3.735	3.735	3.735	3.735
Adj R2	0.28	0.29	0.29	0.28	0.28	0.28
N	6132	6132	6132	6132	6132	6132

This table reports OLS regression estimates of facility deaths per 100 beds on a collection of facility characteristics and county fixed effect. All continuous variables are normalized to have a standard deviation of 1. Standard errors are reported in parentheses. Significance: * $p < .05$, ** $p < .01$, *** $p < .001$.

statistically different from zero when the staff neighborhood variable is included. Column 6 shows no effect of the share of workers who are predicted to be frontline workers. The magnitudes of the effects in columns (2)-(4) are larger than any of the effects of other facility characteristics except facility size, suggesting that local staff geography may be an incredibly important factor in determining facility outbreaks. Most of the relationships from column (1) are unchanged by the inclusion of these neighborhood variables: size, for-profit status, and chain status continue to have significant effects on facility outbreaks. The one exception is the resident non-white share, which becomes more negatively correlated with facility deaths once staff neighborhood characteristics are added in columns (2)-(5).

A likely interpretation of the results in Table 1 is that nursing home staff members are an important source of infection, and that the identified neighborhood characteristics are good proxies for the level of community spread in a neighborhood. Unfortunately, few states report infection rates at the neighborhood level (counties are the lowest level of geography that is available nationally). Table 2 uses a subsample of eight states that provide more local data on case rates to test this interpretation. Columns (1) and (3) reproduce columns (3) and (4) (public transportation use and non-white share) from Table 1 for this smaller subsample, since these variables exhibited the strongest associations with facility infection in the full sample (because population density is highly correlated with public transportation use (ρ=.92), it is omitted for the sake of brevity). Both effects are still positive and highly significant predictors of facility death rates in this smaller subsample. Columns (2) and (4) add measures of the population case rate of COVID-19 in staff towns and the nursing home's town (again normalized to have standard deviations of 1). A one standard deviation in the average infection rate of staff towns is associated with an additional 2.2 ($p<.001$, column 2) or 2.4 deaths per 100 beds ($p<.001$, column 4) at a facility. The infection rate of the nursing home's town is associated with a smaller, but still large, increase in deaths. After including these measures, the estimated effects on staff neighborhood public transportation use and staff neighborhood non-white share are significantly reduced, suggesting that it is quite possible that those effects operate through differences in community-level infection.

Table 3.2: *Relationship of nursing home deaths with local infection rates in staff and nursing home neighborhoods*

	(1)	(2)	(3)	(4)
Staff tract pubtrans use	1.259^{***}	0.770^{*}		
	[0.302]	[0.316]		
Staff tract share nonwhite			1.204^{***}	0.349
			[0.350]	[0.376]
Staff town case rate		2.161^{***}		2.353^{***}
		[0.454]		[0.467]
NH town case rate		0.536^{***}		0.519^{**}
		[0.160]		[0.160]
For-profit	0.304	0.442	0.287	0.469
	[0.360]	[0.357]	[0.361]	[0.358]
Chain	0.648^{*}	0.624^{*}	0.471	0.569
	[0.301]	[0.298]	[0.302]	[0.297]
Overall Rating	-0.004	-0.005	0.025	0.002
	[0.172]	[0.170]	[0.173]	[0.171]
No prior infection viol.	0.309	0.343	0.350	0.362
	[0.356]	[0.353]	[0.357]	[0.353]
Medicaid share	-0.010	-0.029	-0.077	-0.042
	[0.180]	[0.178]	[0.180]	[0.178]
Resident share nonwhite	-0.187	-0.432	-0.322	-0.414
	[0.220]	[0.222]	[0.242]	[0.239]
Occupancy Rate	0.849^{***}	0.828^{***}	0.817^{***}	0.841^{***}
	[0.174]	[0.172]	[0.175]	[0.172]
Num beds	0.505^{**}	0.488^{**}		0.482^{**}
	[0.153]	[0.152]		[0.152]
25-50 beds			0.000	
			[.]	
50-100 beds			1.151	
			[0.671]	
100-150 beds			2.256^{***}	
			[0.659]	
150-200 beds			2.615^{***}	
			[0.713]	
200+ beds			2.052^{*}	
			[0.807]	
Constant	3.690^{***}	3.499^{***}	1.961^{**}	3.508^{***}
	[0.336]	[0.334]	[0.683]	[0.335]
Fixed Effects	County	County	County	County
Depvar mean	4.488	4.488	4.488	4.488
Adj R2	0.33	0.35	0.33	0.34
N	2038	2038	2038	2038

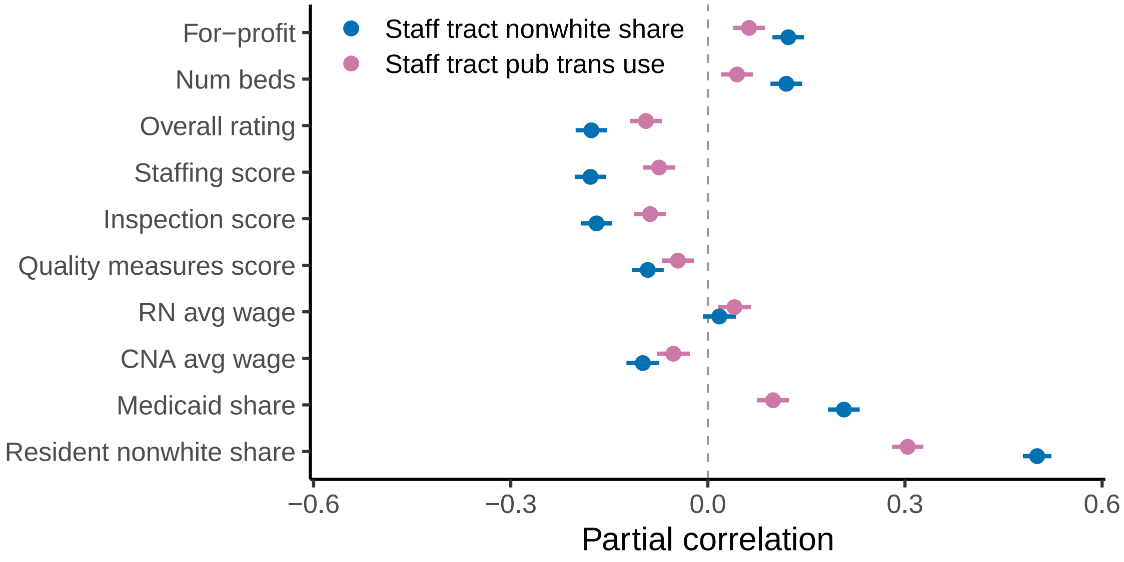
Notes: This table adds measures of town-level infection rates to the models in Table 1. The sample is all nursing homes in the subsample of eight states where this data is reported (CT, FL, IL, LA, MA, MD, RI, SC). All continuous variables have been normalized to have a standard deviation of 1. Standard errors in parentheses* p<.05, ** p<.01, *** p< .001.

Another interpretation is that these results are driven by the individual risk factors of staff members themselves, rather than their neighborhoods. This could have different policy implications, such as suggesting that nursing homes could potentially be protected if we provided staff with non-public transportation options. However, data from the American Community Survey suggests that very few (less than 5%) nursing home workers take public transportation to work in the study states (S2 Table). Furthermore, after controlling for the overall neighborhood measure, there is no effect of a measure of neighborhood public transportation use that is restricted to workers in the education and health care industry (S3 Table, column 1). Likewise, after controlling for the neighborhood racial composition, there is no effect of the share of workers who work on the same block as the nursing home who are non-white, which should be a better proxy for the racial composition of a nursing home's staff (S3 Table, column 2).

It is possible that the initial infection of a facility and the containment or spread of the virus at the facility are affected by different factors. To study this, S4 Table reproduces the main results using a binary indicator for an outbreak, whether or not a facility reported any death, rather than the continuous measure used throughout the paper. Columns (2) and (3) show that the main results apply when investigating the presence of an outbreak: staff neighborhood characteristics continue to be one of the most important predictors of facility infection, and there continues to be a large effect of for-profit status. There are some differences: the effect of chain status is not significant here, and there is a slightly negative effect of star rating on the binary measure, and a positive effect of the resident non-white share, suggesting that lower-rated facilities and facilities with more non-white residents were more likely to experience an outbreak, even though outbreak size was not correlated with these characteristics.

These results beg the question: what types of facilities are likely to have staff who live in more dense and nonwhite neighborhoods with more public transportation? Are lower-quality, for-profit, nursing homes more likely to have higher staff neighborhood exposure? Are the most exposed facilities also the ones with the lowest wages, or the most non-white

Figure 3.3: *Partial correlations between staff neighborhood characteristics and other facility characteristics, controlling for county fixed effects*



Notes: This figure reports partial correlations of different facility characteristics listed along the y-axis with two neighborhood characteristics of interest: public transportation use and non-white share. Correlations control for county fixed effects. Lines represent 95% confidence intervals.

residents? If so, these results could offer a mechanism to explain systematically higher deaths at facilities without unions or facilities with fewer white residents that has been documented in other literature (Abrams *et al.*, 2020; Gebeloff and Parker, 2020). Figure 3.3 shows partial correlations of the staff neighborhood measures with other facility characteristics, controlling for county fixed effects, with a particular focus on measures related to a facility's staffing practices. Both staff neighborhood public transportation use and staff neighborhood non-white share are positively correlated with larger, for-profit, and lower-rated facilities, but the correlations are relatively small. Likewise, there is a small negative correlation of the wage paid to certified nursing assistants (40% of the nursing home workforce, and the occupation likely to have the most contact with patients). On the other hand, staff exposure exhibits much larger correlations with the demographics of the residents: facilities with more Medicaid patients, and especially more nonwhite patients, are more likely to have higher measures of the staff neighborhood exposure measures. These results explain why in Table 1, the resident nonwhite share coefficient becomes insignificant after including the staff neighborhood measures, and suggest a potential channel for observed racial disparities in COVID infection across nursing homes: nursing homes with more non-white residents appear to employ more staff from the most highly exposed neighborhoods.

3.4 Discussion

This study uses a novel approach to measuring staff neighborhood characteristics to provide new evidence that the local geography of where staff lives is a strong predictor of nursing home outbreak sizes, even after controlling for a nursing home's county. Specifically, nursing homes whose staff come from denser, less white neighborhoods with more public transportation use have had significantly larger outbreaks of COVID-19, and that these measures are much more powerful in explaining differences in death rates within a county than many other facility characteristics (such as nursing home rating), and also than the same characteristics of the nursing home's own neighborhood.

While there were early efforts to close nursing homes to visitors and protect nursing home residents, the experience of these homes has indicated that these efforts were not nearly enough, with significant numbers of homes becoming infected after they were "locked down." Because of the close personal contact they have with residents, staff members are a likely source of transmission, and these results lend support to this hypothesis that the spread of COVID-19 in staff communities was an important mechanism driving facility outbreaks in the first wave of the pandemic. However, the small effect of facility management variables compared to the large effect of staff neighborhood characteristics suggests that it may ultimately be necessary to control outbreaks in the community in order to control facility outbreaks. It is possible that these relationships are specific to the first wave of deaths in the spring and early summer of 2020, and that as nursing homes gained experience with controlling outbreaks, other variables became more important in determining deaths from COVID-19. While this study does not investigate deaths in the later months of the pandemic, descriptive evidence from other researchers appears to indicate that community spread remained important after the first wave (Konetzka and Gorges, 2020).

Previous research has documented substantial segregation in long-term care, and how the location of high-quality facilities may exacerbate other inequalities (Fashaw *et al.*, 2020; Mor *et al.*, 2004; Smith *et al.*, 2007; Konetzka and Werner, 2009; Sharma *et al.*, 2019). In the case of COVID-19, even though this study does not find evidence of a significant effect of facility rating on facility outbreaks, it does uncover the concerning finding that the facilities that employ staff from neighborhoods that are more exposed to COVID-19 infection are also the facilities that serve more non-white residents. Early evidence suggests that black and Latino communities have been hit hardest by the pandemic (Millett *et al.*, 2020; Yancy, 2020; Knittel and Ozaltun, 2020; Oppel Jr. and Smith, 2020; Wen and Sadeghi, 2020). The fact that the nursing home industry draws staff disproportionately from these communities in general may explain some of the enormous impact of the pandemic on nursing homes. Finally, the persistent and relatively large effect of for-profit status on COVID-19 outbreaks as well as the consistently small or zero effect of a facility's star rating (after accounting for geography and other facility characteristics) both merit further study, as it suggests that non-profit homes may have responded differently to the pandemic, but that the rating system was not able to predict these differences. In addition, the fact that wages are not highly correlated with high staff exposure also offers an opportunity for future study to understand why certain homes employ more heavily from more exposed communities; it may indicate that these nursing homes are most conveniently located for people living in these communities, or that facilities with more and less non-white residents have different hiring practices.

This study has a few limitations. First, it is important to note that the analysis in this study is correlational and there may therefore be omitted variables that are driving the results. For example, S5 Table shows that the coefficients on staff neighborhood characteristics are reduced if the distance of the nursing home to the central business district of the nearest metropolitan area is included, though they remain relatively large and statistically significant. This could either mean that (1) staff neighborhood characteristics are a true risk factor for facility outbreaks, and nursing homes that are more centrally located are simply likelier to draw staff from more exposed neighborhoods, or (2) centrality affects facility outbreaks through other mechanisms besides staff neighborhoods. However, in this case, the fact that staff neighborhood characteristics continue to be significant after controlling for centrality suggest that the former may be more important, and also offer a lower bound for these effects. A second limitation is that I use the neighborhood characteristics of all service sector employees on the same block as the nursing home to proxy for the neighborhood characteristics of nursing home staff. S6 Table shows that the results are not significantly affected by excluding nursing homes whose blocks contain significantly more service sector employees than my prediction of the nursing home's employment.

Finally, because the analysis is within counties, it cannot offer much insight into the effects of different county- or state-level policies in the COVID-19 response, though these are likely to have been important in determining infection rates and deaths. Additionally, although Table 2 offers suggestive evidence that the relationship between staff neighborhood characteristics and can be explained by higher infection rates in these neighborhoods, it is limited by the lack of data on infection rates by neighborhood (only one state in the sample provides case-level data at the tract-level, the remaining states in Table 2 provided data at the town or zip level, and the other states not included in Table 2 only report case data at the county level). More granular data of this form would help confirm the hypotheses of this study.

3.5 Conclusions

During the first wave of the pandemic, which nursing homes experienced the largest outbreaks of COVID-19 within a county was not random, but it was also not largely determined by other measures of quality commonly cited in the nursing home literature, such as star rating. Instead, a key determinant was the characteristics of the neighborhoods where nursing home staff members lived—facilities whose staff lived in denser, less white neighborhoods with more public transportation use have had significantly more deaths than other facilities in the same county.

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Appendix A

Appendix to Chapter 1

A.1 Supplementary Tables and Figures

Table A.1: *Sample averages of all control variables for analysis sample seniors by functional status, 2000-2016*

	No ADL difficulty	ADL difficulty
Live in SPPC State (PC_s)	0.57	0.58
(Countable) income below 125% FPL (IncElig_{it})	0.18	0.36
Demographics		
Age	74.1	78.7
Female	0.55	0.65
Black	0.08	0.12
Hispanic/Latino	0.05	0.08
Some college	0.45	0.32
Family		
Has male partner	0.27	0.19
Has female partner	0.35	0.23
Has son	0.75	0.73
Has daughter	0.73	0.73
Health		
Dementia	0.06	0.25
Other cog impairment	0.18	0.26
Stroke	0.06	0.19
Lung disease	0.10	0.18
Heart problem	0.28	0.43
Diabetes	0.20	0.25
# Mobility difficulties (Max. 9)	2.0	6.3
# IADL difficulties (Max. 4)	0.1	1.0
# ADL difficulties (Max. 6)	-	2.5
N respondent-years	73,921	21,918
Unique respondents	18,445	9,692

Figure A.1: *Histogram of hours of care per day received by ADL-impaired seniors*

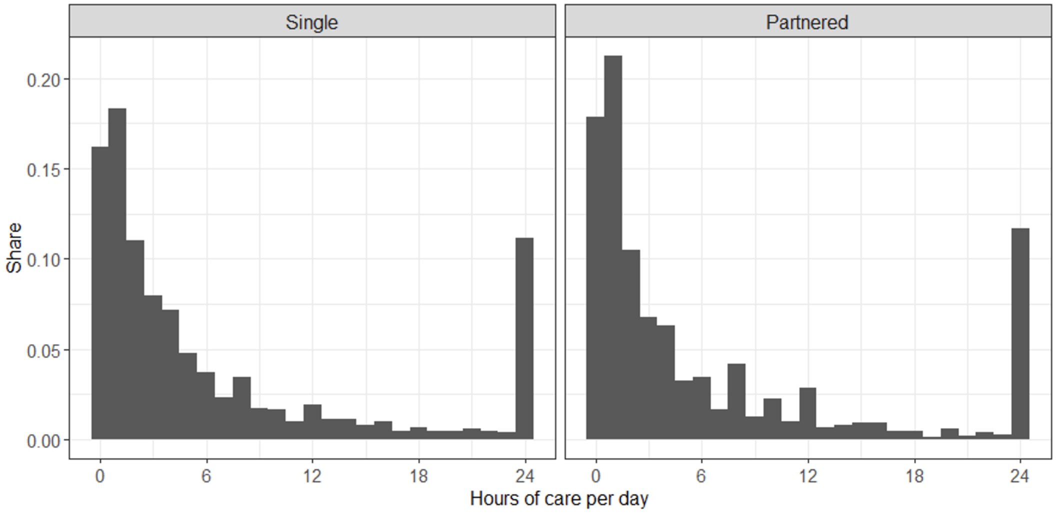

Figure A.2: *DD coefficient on paid home care after leaving out each state individually*

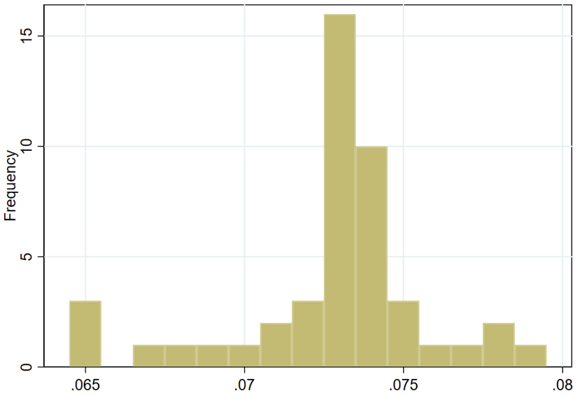

Table A.2: *Sample averages of all control variables for children of HRS respondents*

	Parent has no ADL difficulty	Parent has ADL difficulty
Age	46	50
Female	0.49	0.50
Partnered	0.69	0.66
Any kid	0.82	0.84
Young kid	0.08	0.06
Some college	0.22	0.22
College	0.32	0.30
N	249,952	75,757
Unique individuals	41,347	32,613

Table A.3: *Sample averages of all control variables for spouses and partners of senior HRS respondents*

	Spouse has no ADL difficulty	Spouse has ADL difficulty
Age	72	75
Female	0.55	0.55
Black	0.09	0.13
Hispanic/Latino	0.06	0.09
Some college	0.44	0.35
Dementia	0.06	0.11
Other cog impairment	0.16	0.23
Mobility difficulties (Max. 9)	2.5	3.5
IADL difficulties (Max. 4)	0.2	0.4
ADL difficulties (Max. 6)	0.3	0.7
N	44,724	9,091
Unique individuals	12,177	4,590

Table A.4: *Horse race regression of state-level variables against SPPC benefit*

	(1) Paid Helper	(2) NH Resident	(3) Daughter Works FT
$\text{PC}_s \times \text{IncElig}_{it}$	0.068^{***}	-0.011	0.039^{***}
	[0.018]	[0.013]	[0.013]
$\text{HighMedicare}_s \times \text{IncElig}_{it}$	0.047^{**}	-0.026^{**}	0.008
	[0.023]	[0.013]	[0.023]
$\text{HighHomeHealth}_s \times \text{IncElig}_{it}$	0.001	0.003	0.004
	[0.025]	[0.012]	[0.022]
$\text{HighMedianIncome}_s \times \text{IncElig}_{it}$	0.006	-0.012	0.007
	[0.023]	[0.016]	[0.017]
$\text{HighGSS}_s \times \text{IncElig}_{it}$	0.021	0.022	0.030^{**}
	[0.023]	[0.014]	[0.015]
$\text{HighDem}_s \times \text{IncElig}_{it}$	-0.004	-0.007	-0.037^{**}
	[0.022]	[0.014]	[0.017]
IncElig_{it}	-0.052^{*}	0.037^{**}	-0.047^{***}
	[0.026]	[0.016]	[0.022]
Dep. Var Mean	0.129	0.142	0.554
N	21918	21918	33956

Notes: This table replicates the DDD specification for three main variables of interest. Standard errors are clustered at the state level and reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: *Main results estimated on sample of seniors with income < 300% FPL*

	(1) Paid Helper	(2) NH Resident	(3) Daughter Works FT
$\text{PC}_s \times \text{IncElig}_{it}$	0.060^{***}	-0.015	0.028^{*}
	[0.019]	[0.015]	[0.015]
IncElig_{it}	-0.003	0.019	-0.057^{***}
	[0.014]	[0.010]	[0.010]
Dep. Var. Mean	0.127	0.151	0.542
N	17791	17791	30077

Table A.6: *Main results estimated including late adoption states*

	(1) Paid Helper	(2) NH Resident	(3) Daughter Works FT
$\text{PC}_s \times \text{IncElig}_{it}$	0.071^{***}	-0.020	0.034^{**}
	[0.017]	[0.014]	[0.014]
IncElig_{it}	-0.012	0.023^{**}	-0.059^{***}
	[0.013]	[0.010]	[0.012]
Dep. Var Mean	0.119	0.142	0.554
N	22591	22591	37239

Appendix B

Appendix to Chapter 2

B.1 Data Appendix

B.1.1 Private Health Insurance Claims Data

Sample Definition As described in the text, there are two ways to be included in our sample. First, our sample includes any patient with a diagnosis of OUD (these codes are listed in Table B.1) in the first three positions of any non-lab claim (codes used to identify lab procedures are listed in Table B.2). Lab claims are excluded because these claims sometimes code a diagnosis that was tested for, regardless of whether the test was positive or negative. Note that beginning with ICD-10, up to 5 diagnosis codes can be listed on any given claim instead of 3 under the ICD-9 system.

Patients receiving take-home buprenorphine treatment for OUD may fill prescriptions without incurring a medical claim and would be excluded by only using diagnosis codes in medical claims. Therefore, we also use prescription drug claims for formulations of buprenorphine used to treat OUD as a second sample inclusion criterion. Some formulations of buprenorphine are primarily used for pain management, while others are used for treating OUD. We identified claims by National Drug Code (NDC), and limited attention to those primarily used for OUD treatment. Specifically, we use the Food and Drug Administration's Orange Book to identify drugs with buprenorphine as an active ingredient, and then follow

Clemans-Cope *et al.* (2017) by excluding Buprenex (an injectable drug), Butrans transdermal patches, Belbuca buccal films, and their therapeutic equivalents. We merge the Orange Book data with the National Drug Code directory to obtain the set of NDC codes to include. These NDCs correspond to Suboxone sublingual tablets and films, Subutex sublingual tablets, Bunavail buccal films, the Probuphine implant, Zubsolv sublingual tablets, and the generic equivalents.

Variable definitions Tables B.2 provide codes we use to identify treatment services in the claims data. Our main utilization variables are indicator variables for having any of these services in the six months following the OUD diagnosis. We do not include detox as a treatment service. Some of the revenue codes specifically indicate that the treatment is for drug treatment; others may be non-specific "residential" or"day treatment" codes. For the latter category, we additionally require a procedure code that specifies drug treatment. "Other treatment" is a catch-all category which includes group counseling, individual counseling, sober homes, and non-specific treatment codes. We also include psychotherapy codes when the primary diagnosis code is a drug-related code.

Our clinical outcome variables are any overdose claim, any drug-related ER claim, and any other ER claim. Drug-related ER claims are identified using the diagnoses in B.1. Again, we use the first three positions on each claim, and exclude lab claims. For the ER claims, we use diagnoses on each day and in the first through third spots on each claim line. Because opioids are often used in combination with other drugs, and because some diagnosis codes are non-specific in their drug type, our results focus on "any overdose" and "drug-related" ER visits. Notably, these diagnosis codes exclude alcohol use disorder or alcohol overdoses. In Table B.3 we summarize the primary diagnoses for drug-related ER visits.

Treatment spending is sometimes charged at a per diem rate, but in other cases, there may be treatment-associated spending that occurs on a different line from the treatment code, such as for drug tests or psychotherapy fees performed as part of a treatment program. Therefore, we assign each day to one type of spending, and allocate all spending on those days to that day type. For days that have claims in multiple categories, we use the priority

Table B.1: *Diagnosis codes*

	ICD-9	ICD-10
Opioid use disorder	304.0x, 304.7x, 305.5x	F11.1x, F11.12x, F11.9x
Drug use disorder	304.x, 305.1-305.9	F11-F19
Opioid overdose	965.0x, E850.0, E850.1, E850.2, E935.0, E935.1	T40.0x T40.1x, T40.2x, T40.3x
Any overdose	965.x, 966.x, 967.x, 970.x, 971.x, 977.9, E850-E858	T39.x, T40.x, T42.x, T43.x, T44.x
Drug withdrawal	292	

order: detox, ER, treatment, inpatient, outpatient, RX spending. Within treatment, we use the priority order: residential, day treatment, other treatment, buprenorphine treatment. Our six-month spending measures use the 180 days of spending before and after diagnosis, where the post-period includes the day of initial diagnosis and the pre-period excludes it.

B.2 Clinical Literature Review

Two Cochrane reviews have evaluated the clinical evidence for opioid agonist therapy. Cochrane reviews are meta-analyses of medical literature.

The first (Mattick *et al.*, 2009) reviews 11 studies that have compared methadone maintenance treatment to different treatments that did not include opioid agonists (e.g. detoxification, psychosocial counseling, placebo medication, no treatment or waitlist). Primary outcomes studied are retention in treatment, mortality, urinalysis, self-reported heroin use, and criminal activity, for durations of a few weeks up to two years. Their meta-analysis concludes that in these studies, patients are more than four times likely to stay in treatment (.684 compared to .154) when given methadone maintenance, and one-third less likely to report or be found to be using heroin (.463 compared to .701). They do not find statistically significant differences in mortality or criminal activity.

The second (Mattick *et al.*, 2014) (first published in 2002, updated in 2008 and 2014) reviews 31 studies that compare buprenorphine treatment to placebo medication and methadone treatment. They conclude that buprenorphine has similar rates of abstinence

Table B.2: *Revenue and procedure codes*

	CPT-4/HCPCS	Revenue Code	ICD9/ICD10 Procedure Codes
ER visit		450, 451, 452, 456, 459	9928x
Detox	H0008, H0009, H0010, H0011, H0012, H0013, H0014	116, 126, 136, 146, 156	94.62, 94.65, 94.68, HZ2ZZZZ
Methadone	H0020, J2315		HZ81ZZZ, HZ91ZZZ
Naltrexone	J2135		HZ84ZZZ, HZ94ZZZ
Residential	H0017, H0018, H0019	1002	
Require both revenue and procedure code		1001, 120, 130, 140, 150, 160, 121, 131, 141, 151, 161, 124, 134, 144, 154, 164, 129, 139, 149, 159, 169	94.64, 94.66, 94.67, 94.69, HZ3x, HZ4x, HZ5x, HZ6x, HZ9x
Day Treatment	H0015	906	
Require both revenue and procedure code		905, 907, 912, 913	94.64, 94.66, 94.67, 94.69, HZ3x, HZ4x, HZ5x, HZ6x, HZ9x
Other Treatment	H0004, H0005, H0006, H0007, H0047, H0050, H2034, H0033, H0016, H2035, H2036, S9475	529, 944, 1003, 1004, 1005	94.45, 94.64, 94.66, 94.67, 94.69, HZ3x, HZ4x, HZ5x, HZ6x, HZ9x
Require primary drug diagnosis	9080x, 9081x, 9082x, 9083x, 9084x, 9085x, 9086x	900, 9811, 914, 915, 916, 918, 919	
Lab	80047-89398, G0477, G0478, G0479, G0431, G0434, G0480, G0481, G0482, G0483, G603x, G604x, G605x		

Table B.3: *Primary diagnosis of drug-related ER visits*

Diagnosis codes	Share	Description
304, 305	17%	Drug use disorder
296	7%	Episodic mood disorders
780	7%	Altered mental status/consciousness, hallucinations, syncope and collapse
965, 977	12%	Poisoning
311	5%	Depressive disorder
786	4%	Respiratory symptoms (chest pain, shortness of breath)
303	4%	Alcohol dependence
300	3%	Anxiety
292	3%	Drug-induced mental disorders
298	2%	Other psychoses
789	2%	Abdominal pain
—	46%	All other

from other opioids as methadone, but slightly lower retention rates, suggesting that like methadone, it should be considered an effective treatment for opioid dependence.

There are reasons to expect that the effectiveness of agonists in clinical trial settings may differ from that in non-trial settings. Both Cochrane reviews note that many of the reviewed clinical trials use a higher and fixed dose regimen than the flexible dosing used in non-trial practice. In addition, many of the trials restrict eligibility to active heroin users, who may differ from the average methadone or buprenorphine patient. Finally, most of the studies pair agonist therapy with psychosocial counseling and other services, which may be more intensive or of higher quality than what patients receive outside clinical trials. (However, a few studies have tested for a differential effect of medication with counseling compared to medication-only treatment and have not found statistically significant differences)

Both reviews rate the quality of the evidence from many of the included studies as high, as many are double-blinded with placebo medication, measure intent-to-treat effects to limit attrition bias, and use objective outcome measures.

B.3 Historical Regulation of MAT

As drugs with the potential for abuse, methadone and buprenorphine are regulated under the Controlled Substances Act (CSA). The CSA typically imposes restrictions on prescriptions (e.g. refill limits) and requires that prescribers be registered with the Drug Enforcement Administration (DEA). When the CSA was written in 1970, it included a portion that prohibited the prescribing of any controlled substance "to a narcotic drug dependent person for the purpose of continuing his dependence upon such drugs." However, methadone had been used for the treatment of OUD since the mid-1960s. In 1974, this prohibition was amended by the Narcotic Addiction Treatment Act to allow prescribing controlled substances for the treatment of dependence at facilities that have fulfilled the requirements to be designated as Opioid Treatment Programs (OTP).

As shown in the top panel of Figure 2.1, there are currently about 1,300 OTPs, commonly known as methadone clinics. This number has been fairly stable since at least the late 1990s. Burdensome regulation and local opposition are often cited as explanations for this lack of growth. Facilities are mostly concentrated in large, urban centers, creating access problems for other populations (Lewis, 1999). Regulation is also burdensome on the patient side: patients at OTPs must generally report daily (or six times per week) to ingest a single dose of methadone under supervision for the first 90 days of treatment, and the number of take-home doses are gradually increased over the course of two years of treatment.[1] In contrast, methadone has been prescribed in primary care clinics in Great Britain, Australia, and Canada since the 1970s and earlier (Samet *et al.*, 2018).

In light of the limited ability of methadone to meet the perceived demand for medication-assisted treatment, the CSA was again amended by the Drug Addiction Treatment Act (DATA 2000) in 2000 to allow physicians to obtain a waiver from DEA/SAMHSA in order

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[1] In the last month of their first year, patients can be given a 6-day supply of take-home medication, and after two years of continuous treatment, they may receive a one-month supply. State regulation can be more restrictive than federal requirements.

to prescribe Schedule III, IV, and V substances that are FDA-approved to treat addiction.[2] Schedule III drugs may therefore be prescribed by physicians from their practice sites, rather than solely in OTPs.

The first FDA-approved drug meeting the DATA 2000 conditions for opioid use disorder was a formulation of buprenorphine introduced in 2002. Because it is a partial rather than full agonist, buprenorphine is regulated as a Schedule III substance (whereas methadone is a Schedule II substance). To qualify for a waiver to prescribe it, physicians must either hold a board certification in addiction medicine or complete an eight-hour training provided by ASAM and other organizations. In 2016, the Comprehensive Addiction and Recovery Act (CARA) allowed nurse practitioners and physician assistants to also qualify for the waiver if they complete 24 hours of training. Under these laws, prescribers are also subject to patient limits that cap the number of patients a prescriber may treat with buprenorphine at any given time. These limits were originally set to 30 patients for all providers, and have been increased over time to 100 for prescribers in their first year following waiver receipt and 275 after. Providers must keep detailed records of all patient prescriptions and these are subject to review by the DEA. In 2018, the SUPPORT for Patients and Communities Act was passed, which included a bill called CARA 2.0. This bill made the authority of PAs and NPs to prescribe MAT permanent, and granted temporary authority to nurse specialists, midwives and anesthetists temporary authority. It also authorized funding for grants for FQHCs and clinics to cover the cost of training providers.

B.4 Results Appendix

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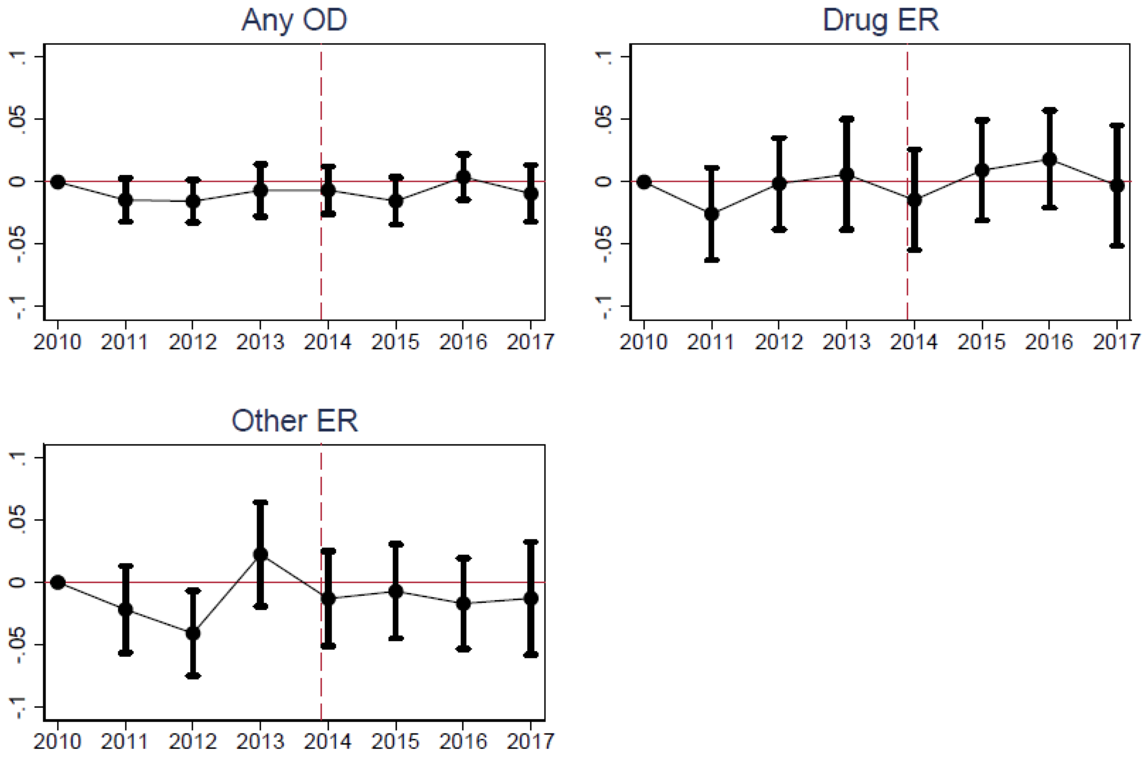
[2] The Drug Enforcement Agency classifies controlled substances by medical use and potential for abuse. Schedule I drugs, such as heroin, have no accepted medical uses. Schedule II drugs may have indications for medical use but have a high potential for abuse resulting severe physical or psychological dependence; examples include oxycodone, morphine, and methadone. Schedule III drugs have lower abuse potential than a Schedule II drug but still may lead to moderate physical dependence or high psychological dependence. Schedule IV and V drugs have low potential for abuse and dependence. https://www.deadiversion.usdoj.gov/schedules/

Table B.4: *Sample composition for baseline, parity, and restricted diagnosis samples*

	Baseline	Parity	Restricted
Controls			
Female	0.43	0.44	0.45
Age 18-24	0.24	0.25	0.22
Age 25-34	0.20	0.19	0.18
Age 35-44	0.20	0.20	0.20
Age 45-54	0.22	0.22	0.24
Age 55-64	0.14	0.15	0.17
Employee	0.46	0.41	0.45
EPO	0.07	0.09	0.07
HMO	0.16	0.03	0.14
POS	0.64	0.76	0.66
PPO	0.13	0.13	0.13
CDHP	0.16	0.22	0.18
Treatment			
Any	0.53	0.53	0.34
MAT	0.30	0.31	0.13
Med-free Res	0.04	0.04	0.04
Med-free OP	0.19	0.18	0.16
Clinical Outcomes			
Overdose	0.05	0.04	0.06
Drug-related ER	0.26	0.25	0.32
Other ER	0.23	0.21	0.25
Spending			
Pre-diagnosis (6m)	11,934	11,159	12,668
Post-diagnosis (6m)	17,513	16,728	18,519
N	78,222	28,325	55,778

Notes: This table compares the demographic composition of our baseline OUD sample (column 1), the sample we use for the parity analysis which excludes self-insured patients, as well as patients diagnosed in 2009 or the second half of 2013 (column 2), and the restricted subsample that excludes patients who received treatment on the day of their diagnosis (column 3)

Figure B.1: *Effect of small-group parity on clinical outcomes*



Notes: This figure shows estimated coefficients and 95 percent confidence intervals on small group × year indicator variables in the sample of newly diagnosed OUD patients who were not diagnosed in the last six months of 2013. Dashed vertical lines indicate the post period begins in 2014.

Table B.5: *Relationship between treatment utilization and small-group parity, controlling for trends by patient characteristics*

	(1)		(2)		(3)		(4)	
	Any		MAT		Med-free Res		Med-free OP	
Small Group × Post	-0.010	[0.012]	-0.027**	[0.011]	0.016***	[0.005]	0.001	[0.010]
Small Group	0.025***	[0.008]	0.027***	[0.007]	0.000	[0.003]	-0.002	[0.006]
Female	-0.071***	[0.008]	-0.049***	[0.008]	-0.002	[0.003]	-0.021***	[0.006]
Age 25-34	-0.062***	[0.012]	0.075***	[0.012]	-0.019***	[0.005]	-0.119***	[0.010]
Age 35-44	-0.159***	[0.012]	-0.022*	[0.012]	-0.024***	[0.005]	-0.113***	[0.010]
Age 45-54	-0.276***	[0.012]	-0.099***	[0.012]	-0.028***	[0.005]	-0.148***	[0.010]
Age 55-64	-0.340***	[0.014]	-0.148***	[0.013]	-0.035***	[0.006]	-0.158***	[0.011]
Employee	0.032***	[0.009]	0.076***	[0.009]	-0.011***	[0.004]	-0.032***	[0.007]
POS	0.017*	[0.009]	-0.002	[0.009]	0.021***	[0.004]	-0.002	[0.007]
PPO	0.016	[0.014]	0.018	[0.014]	-0.005	[0.006]	0.003	[0.012]
CDHP	-0.009	[0.012]	-0.017	[0.011]	-0.001	[0.005]	0.009	[0.010]
Female × Post	-0.009	[0.012]	-0.005	[0.011]	0.001	[0.005]	-0.005	[0.010]
Age 25-34 × Post	-0.026	[0.019]	0.004	[0.018]	-0.031***	[0.008]	0.002	[0.016]
Age 35-44 × Post	-0.059***	[0.019]	0.042**	[0.018]	-0.056***	[0.008]	-0.045***	[0.016]
Age 45-54 × Post	-0.059***	[0.019]	0.029	[0.018]	-0.053***	[0008]	-0.036**	[0.015]
Age 55-64 × Post	-0.098***	[0.021]	0.030	[0.020]	-0.059***	[0.008]	-0.079***	[0.017]
Employee × Post	-0.028**	[0.014]	-0.021	[0.013]	-0.005	[0.006]	-0.002	[0.011]
POS × Post	0.031	[0.020]	-0.003	[0.019]	-0.010	[0.008]	0.044***	[0.016]
PPO × Post	0.088***	[0.025]	0.030	[0.024]	-0.020**	[0.010]	0.078***	[0.021]
CDHP × Post	0.004	[0.017]	-0.001	[0.016]	-0.003	[0.007]	0.009	[0.014]
Constant	0.645***	[0.014]	0.318***	[0.013]	0.051***	[0.006]	0.276***	[0.011]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.53		0.31		0.04		0.18	
Adj R2	0.13		0.08		0.04		0.05	
Obs	28325		28325		28325		28325	

Notes: This table uses the sample of newly diagnosed patients who were not diagnosed in the six months in beginning July 2013. The unit of observation is a patient *i* who received a "new" diagnosis of OUD in year *t*. The independent variable of interest is whether at the time of their diagnosis, the patient was covered in a small-group plan after parity was passed. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the state-year level and reported in brackets.

Table B.6: *Relationship between clinical outcomes and small-group parity, controlling for trends by patient characteristics*

	(1)		(2)		(3)	
	Overdose		Drug-related ER		Other ER	
Small Group × Post	0.002	[0.005]	0.009	[0.011]	0.001	[0.010]
Small Group	-0.001	[0.003]	-0.015**	[0.007]	-0.029***	[0.007]
Female	0.009***	[0.003]	0.037***	[0.007]	0.090***	[0.007]
Age 25-34	-0.025***	[0.005]	-0.049***	[0.011]	0.052***	[0.011]
Age 35-44	-0.030***	[0.005]	-0.052***	[0.011]	0.062***	[0.011]
Age 45-54	-0.015***	[0.005]	-0.035***	[0.011]	0.078***	[0.011]
Age 55-64	-0.019***	[0.006]	-0.032**	[0.013]	0.076***	[0.012]
Employee	-0.021***	[0.004]	-0.063***	[0.008]	-0.062***	[0.008]
POS	0.001	[0.004]	0.005	[0.008]	0.002	[0.008]
PPO	0.005	[0.006]	-0.017	[0.013]	0.020	[0.012]
CDHP	0.004	[0.005]	0.015	[0.011]	0.015	[0.010]
Female × Post	-0.010*	[0.005]	-0.011	[0.011]	-0.011	[0.010]
Age 25-34 × Post	0.005	[0.008]	-0.030*	[0.018]	-0.034**	[0.017]
Age 35-44 × Post	-0.002	[0.008]	-0.057***	[0.018]	-0.045***	[0.016]
Age 45-54 × Post	-0.018**	[0.008]	-0.074***	[0.017]	-0.055***	[0.016]
Age 55-64 × Post	-0.008	[0.009]	-0.075***	[0.019]	-0.028	[0.018]
Employee × Post	0.012**	[0.006]	0.008	[0.013]	0.012	[0.012]
POS × Post	-0.003	[0.006]	-0.010	[0.013]	0.002	[0.008]
PPO × Post	-0.016*	[0.009]	-0.014	[0.019]	-0.026	[0.018]
CDHP × Post	0.000	[0.007]	0.008	[0.015]	-0.005	[0.015]
Constant	0.069***	[0.006]	0.313***	[0.013]	0.185***	[0.012]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.04		0.25		0.21	
Adj R2	0.01		0.03		0.03	
Obs	28325		28325		28325	

Notes: This table uses the sample of newly diagnosed patients who were not diagnosed in the six months in beginning July 2013. The unit of observation is a patient *i* who received a "new" diagnosis of OUD in year *t*. The independent variable of interest is whether at the time of their diagnosis, the patient was covered in a small-group plan after parity was passed. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the state-year level and reported in brackets.

Table B.7: *Relationship between spending outcomes and small-group parity, controlling for trends by patient characteristics*

	(1)		(2)		(3)	
	Ln(Treatment Spend)		Ln(Non-TX Spend)		Ln(Total Spend)	
Small Group × Post	-0.043	[0.099]	-0.030	[0.072]	0.002	[0.044]
Small Group	0.131**	[0.064]	-0.108**	[0.047]	-0.082***	[0.029]
Female	-0.526***	[0.066]	0.800***	[0.048]	0.365***	[0.030]
Age 25-34	-0.620***	[0.102]	0.076	[0.074]	0.089*	[0.046]
Age 35-44	-1.433***	[0.103]	0.689***	[0.075]	0.241***	[0.046]
Age 45-54	-2.268***	[0.101]	1.261***	[0.074]	0.487***	[0.046]
Age 55-64	-2.811***	[0.117]	1.693***	[0.086]	0.707***	[0.053]
Employee	0.185**	[0.075]	-0.603***	[0.055]	-0.396***	[0.034]
POS	0.285***	[0.076]	0.186***	[0.056]	0.138***	[0.034]
PPO	0.251**	[0.119]	0.186**	[0.087]	0.140***	[0.054]
CDHP	-0.067	[0.099]	-0.007	[0.073]	-0.018	[0.045]
Female × Post	-0.143	[0.100]	0.065	[0.073]	0.018	[0.045]
Age 25-34 × Post	-0.359**	[0.0161]	0.186	[0.118]	-0.053	[0.072]
Age 35-44 × Post	-0.660***	[0.160]	0.335***	[0.117]	-0.039	[0.072]
Age 45-54 × Post	-0.742***	[0.157]	0.471***	[0.115]	-0.153**	[0.071]
Age 55-64 × Post	-1.032***	[0.173]	0.719***	[0.127]	-0.071	[0.078]
Employee × Post	-0.248**	[0.113]	0.112	[0.083]	0.023	[0.051]
POS × Post	0.115	[0.116]	-0.198**	[0.085]	0.066	[0.052]
PPO × Post	0.614***	[0.171]	-0.346***	[0.125]	0.115	[0.077]
CDHP × Post	0.065	[0.139]	-0.098	[0.102]	-0.068	[0.063]
Constant	4.992***	[0.113]	6.617***	[0.083]	8.094***	[0.051]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	4.07		7.15		8.41	
Adj R2	0.12		0.20		0.16	
Obs	28325		28325		28325	

Notes: This table uses the sample of newly diagnosed patients who were not diagnosed in the six months in beginning July 2013. The unit of observation is a patient *i* who received a "new" diagnosis of OUD in year *t*. The independent variable of interest is whether at the time of their diagnosis, the patient was covered in a small-group plan after parity was passed. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO/HMO plan type. Standard errors are clustered at the state-year level and reported in brackets.

Table B.8: *Restricted sample analysis of relationship between OUD patient treatment utilization and share of waivered providers*

	(1)		(2)		(3)		(4)	
	Any		MAT		Med-free Res		Med-free OP	
BP MD Share$_{ct}$	0.005	[0.06]	0.076*	[0.04]	-0.035	[0.03]	-0.036	[0.04]
BP MD Share$_{c,2008}$	0.097	[0.10]	0.042	[0.08]	0.019	[0.04]	0.036	[0.07]
Female	-0.057***	[0.00]	-0.028***	[0.00]	-0.005***	[0.00]	-0.024***	[0.00]
Age 25-34	-0.121***	[0.01]	0.001	[0.01]	-0.027***	[0.00]	-0.095***	[0.01]
Age 35-44	-0.226***	[0.01]	-0.055***	[0.01]	-0.040***	[0.00]	-0.131***	[0.01]
Age 45-54	-0.314***	[0.01]	-0.097***	[0.00]	-0.048***	[0.00]	-0.169***	[0.01]
Age 55-64	-0.374***	[0.01]	-0.127***	[0.00]	-0.057***	[0.00]	-0.190***	[0.01]
Employee	-0.008**	[0.00]	0.013***	[0.00]	-0.009***	[0.00]	-0.012***	[0.00]
HMO	0.005	[0.01]	-0.016**	[0.01]	-0.023***	[0.00]	0.044***	[0.01]
POS	0.020***	[0.01]	-0.002	[0.01]	-0.003	[0.00]	0.025***	[0.01]
PPO	0.016*	[0.01]	-0.007	[0.01]	-0.023***	[0.00]	0.046***	[0.01]
CDHP	-0.004	[0.01]	-0.007*	[0.00]	0.001	[0.00]	0.002	[0.00]
Ln(AvgSpend$_{ct}$)	0.009	[0.02]	0.016	[0.01]	0.005	[0.01]	-0.012	[0.01]
Unemp Rate$_{ct}$	-0.007***	[0.00]	-0.002*	[0.00]	-0.001	[0.00]	-0.004***	[0.00]
Constant	0.526***	[0.13]	0.078	[0.10]	0.055	[0.05]	0.393***	[0.10]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.34		0.13		0.04		0.16	
Adj R2	0.12		0.04		0.03		0.06	
Obs	55778		55778		55778		55778	

Notes: This table replicates Table 2.5 in the sample of patients whose did not receive treatment on the same day as their diagnosis. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Table B.9: *Restricted sample analysis of relationship between OUD patient clinical outcomes and share of waivered providers*

	(1) Overdose		(2) Drug-related ER		(3) Other ER	
BP MD Share$_{ct}$	0.036	[0.03]	-0.113*	[0.06]	-0.050	[0.05]
BP MD Share$_{c,2008}$	-0.049	[0.04]	0.032	[0.10]	0.194**	[0.09]
Female	0.008***	[0.00]	0.018***	[0.00]	0.083***	[0.00]
Age 25-34	-0.036***	[0.00]	-0.067***	[0.01]	0.069***	[0.01]
Age 35-44	-0.044***	[0.00]	-0.107***	[0.01]	0.077***	[0.01]
Age 45-54	-0.040***	[0.00]	-0.109***	[0.01]	0.064***	[0.01]
Age 55-64	-0.042***	[0.00]	-0.127***	[0.01]	0.067***	[0.01]
Employee	-0.013***	[0.00]	-0.065***	[0.00]	-0.071***	[0.00]
HMO	-0.011**	[0.00]	-0.026***	[0.01]	-0.014	[0.01]
POS	-0.007	[0.00]	-0.025***	[0.01]	-0.011	[0.01]
PPO	-0.006	[0.01]	-0.031***	[0.01]	0.007	[0.01]
CDHP	0.000	[0.00]	0.005	[0.01]	0.010**	[0.00]
Ln(AvgSpend$_{ct}$)	0.012	[0.01]	0.010	[0.02]	0.021	[0.01]
Unemp Rate$_{ct}$	-0.001	[0.00]	-0.000	[0.00]	0.001	[0.00]
Constant	0.006	[0.06]	0.371***	[0.13]	0.018	[0.12]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.06		0.32		0.25	
Adj R2	0.01		0.04		0.03	
Obs	55778		55778		55778	

Notes: This table replicates Table 2.6 in the sample of patients whose did not receive treatment on the same day as their diagnosis. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Standard errors are clustered at the county-year level and reported in brackets.

Table B.10: *Restricted sample analysis of relationship between OUD patient post-diagnosis spending and share of waivered providers*

	(1) Ln(Treatment Spend)		(2) Ln(Non-TX Spend)		(3) Ln(Total Spend)	
BP MD Share$_{ct}$	-0.309	[0.46]	-0.197	[0.32]	-0.436*	[0.25]
BP MD Share$_{c,2008}$	1.350*	[0.81]	-0.168	[0.49]	0.238	[0.40]
Female	-0.446***	[0.03]	0.571***	[0.02]	0.388***	[0.02]
Age 25-34	-1.104***	[0.06]	0.314***	[0.04]	0.140***	[0.03]
Age 35-44	-1.978***	[0.06]	0.836***	[0.04]	0.367***	[0.03]
Age 45-54	-2.673***	[0.05]	1.181***	[0.03]	0.539***	[0.03]
Age 55-64	-3.184***	[0.06]	1.517***	[0.04]	0.723***	[0.03]
Employee	-0.084**	[0.03]	-0.428***	[0.02]	-0.397***	[0.02]
HMO	0.033	[0.08]	-0.042	[0.04]	-0.033	[0.04]
POS	0.301***	[0.06]	0.101***	[0.04]	0.185***	[0.03]
PPO	0.268***	[0.08]	0.164***	[0.04]	0.227***	[0.04]
CDHP	-0.025	[0.04]	-0.004	[0.03]	-0.025	[0.02]
Ln(AvgSpend$_{ct}$)	0.163	[0.13]	0.225***	[0.07]	0.255***	[0.06]
Unemp Rate$_{ct}$	-0.073***	[0.01]	-0.056***	[0.01]	-0.055***	[0.01]
Constant	3.525***	[1.08]	5.806***	[0.58]	6.485***	[0.49]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	2.61		8.13		8.63	
Adj R2	0.11		0.21		0.15	
Obs	55778		55778		55778	

Notes: This table replicates Table 2.7 in the sample of patients whose did not receive treatment on the same day as their diagnosis. The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Table B.11: *Relationship between county-level access to waivered providers and OUD treatment utilization, omitting county-year controls*

	(1) Any		(2) MAT		(3) Med-free Res		(4) Med-free OP	
BP MD Share$_{ct}$	0.029	[0.06]	0.169***	[0.05]	-0.035	[0.02]	-0.105***	[0.04]
BP MD Share$_{c,2008}$	0.156*	[0.09]	0.108	[0.09]	0.019	[0.03]	0.029	[0.06
Female	-0.077***	[0.00]	-0.057***	[0.00]	-0.004***	[0.00]	-0.016***	[0.00]
Age 25-34	-0.078***	[0.01]	0.071***	[0.01]	-0.032***	[0.00]	-0.117***	[0.01]
Age 35-44	-0.185***	[0.01]	-0.010*	[0.01]	-0.041***	[0.00]	-0.134***	[0.00]
Age 45-54	-0.303***	[0.01]	-0.092***	[0.01]	-0.046***	[0.00]	-0.165***	[0.00]
Age 55-64	-0.391***	[0.01]	-0.144***	[0.01]	-0.055***	[0.00]	-0.193***	[0.00]
Employee	0.020***	[0.00]	0.046***	[0.00]	-0.010***	[0.00]	-0.016**	[0.00]
HMO	0.002	[0.01]	0.001	[0.01]	-0.025***	[0.00]	0.026***	[0.01]
POS	0.011	[0.01]	-0.007	[0.01]	-0.005*	[0.00]	0.023***	[0.01]
PPO	0.020**	[0.01]	0.013	[0.01]	-0.023***	[0.00]	0.031***	[0.01]
CDHP	-0.010**	[0.00]	-0.022***	[0.00]	0.002	[0.00]	0.010**	[0.00]
Constant	0.707***	[0.01]	0.319***	[0.01]	0.089***	[0.00]	0.299***	[0.01]
State-Year FEs	Yes		Yes		Yes		Yes	
Depvar Mean	0.53		0.30		0.04		0.19	
Adj R2	0.13		0.08		0.03		0.05	
Obs	78222		78222		78222		78222	

Notes: This table replicates Table 2.5 where we have omitted the county-year variables (average county health care spending and the county unemployment rate). The unit of observation is a patient *i* who received a "new" diagnosis of OUD in year *t* while living in county *c* as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Table B.12: *Relationship between county-level access to waivered providers and OUD patient clinical outcomes, omitting county-year controls*

	(1)		(2)		(3)	
	Overdose		Drug-related ER		Other ER	
BP MD Share$_{ct}$	0.024	[0.02]	-0.103**	[0.05]	-0.006	[0.04]
BP MD Share$_{c,2008}$	-0.052	[0.03]	-0.009	[0.08]	0.074	[0.07]
Female	0.009***	[0.00]	0.031***	[0.00]	0.086***	[0.00]
Age 25-34	-0.029***	[0.00]	-0.066***	[0.01]	0.050***	[0.01]
Age 35-44	-0.033***	[0.00]	-0.080***	[0.01]	0.061***	[0.01]
Age 45-54	-0.028***	[0.00]	-0.068***	[0.01]	0.059***	[0.01]
Age 55-64	-0.028***	[0.00]	-0.076***	[0.01]	0.067***	[0.01]
Employee	-0.013***	[0.00]	-0.066***	[0.00]	-0.067***	[0.00]
HMO	-0.006*	[0.00]	-0.017**	[0.01]	-0.012*	[0.01]
POS	-0.004	[0.00]	-0.015**	[0.01]	-0.007	[0.01]
PPO	-0.004	[0.00]	-0.024***	[0.01]	0.004	[0.01]
CDHP	0.001	[0.00]	0.008*	[0.00]	0.006	[0.00]
Constant	0.074***	[0.00]	0.354***	[0.01]	0.179***	[0.01]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	0.05		0.26		0.23	
Adj R2	0.01		0.03		0.02	
Obs	78222		78222		78222	

Notes: This table replicates Table 2.6 where we have omitted the county-year variables (average county health care spending and the county unemployment rate). The unit of observation is a patient i who received a "new" diagnosis of OUD in year t while living in county c as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Table B.13: *Relationship between county-level access to waivered providers and OUD patient spending outcomes, omitting county-year controls*

	(1) Ln(Treatment Spend)		(2) Ln(Non-TX Spend)		(3) Ln(Total Spend)	
BP MD Share$_{ct}$	0.041	[0.46]	-0.631*	[0.35]	-0.222	[0.22]
BP MD Share$_{c,2008}$	1.423*	[0.76]	-0.442	[0.55]	-0.115	[0.33]
Female	-0.606***	[0.03]	0.846***	[0.02]	0.357***	[0.01]
Age 25-34	-0.829***	[0.05]	0.214***	[0.04]	0.071***	[0.02]
Age 35-44	-1.709***	[0.05]	0.944***	[0.04]	0.290***	[0.02]
Age 45-54	-2.634***	[0.05]	1.520***	[0.04]	0.467***	[0.02]
Age 55-64	-3.369***	[0.05]	2.013***	[0.04]	0.660***	[0.02]
Employee	0.097***	[0.03]	-0.539***	[0.02]	-0.375***	[0.01]
HMO	-0.029	[0.07]	-0.056	[0.05]	-0.060**	[0.03]
POS	0.221***	[0.05]	0.135***	[0.04]	0.158***	[0.02]
PPO	0.267***	[0.07]	0.129***	[0.05]	0.160***	[0.03]
CDHP	-0.077*	[0.04]	-0.002	[0.03]	-0.026	[0.02]
Constant	5.646***	[0.07]	6.399***	[0.05]	8.222***	[0.03]
State-Year FEs	Yes		Yes		Yes	
Depvar Mean	4.08		7.39		8.56	
Adj R2	0.12		0.19		0.14	
Obs	78222		78222		78222	

Notes: This table replicates Table 2.7, where we have omitted the county-year variables (average county health care spending and the county unemployment rate). The dependent variables use the log(x+1) transformation for six-month post-diagnosis spending $x. The unit of observation is a patient *i* who received a "new" diagnosis of OUD in year *t* while living in county *c* as described in the text. Except where noted, control variables vary at the patient level. Omitted categories are age 18-24 and EPO plan type. Standard errors are clustered at the county-year level and reported in brackets.

Appendix C

Appendix to Chapter 3

C.1 Data Appendix

C.1.1 State COVID-19 Data

We obtain publicly released facility-level data on COVID-19 infections from 18 states. We began collecting this data in mid-April and continued to do so approximately every week until the week of July 10, 2020. Since some states do not report facilities with "closed outbreaks"'–i.e. facilities with no current cases–we use the historical data to build a cumulative measure of whether a facility was ever infected as if they appeared on any list. These data should largely reflect all nursing homes that have ever reported a COVID-19 infection, though data is usually self-reported by facilities and may contain errors, and states also differ in the exact data that they report. Notably, Maryland only reported facilities that had cases after April 15, New York only reports deaths that occurred at the nursing home (rather than all deaths among nursing home residents), and four states only report nursing homes with 2+ cases. We matched the names on these lists to the administrative data on the universe of nursing homes. This allows us to calculate the number of deaths per bed at each nursing home, and to compare characteristics of nursing homes with low and high numbers of deaths. In terms of numbers of deaths, the 18 states in our sample represent over 80% of the total deaths from COVID at the time of data collection and contain all of the top 10 states. The states with many deaths for whom we do not have facility-level data include Texas, Ohio, Indiana, Arizona, Texas, and Virginia.

Eight of our sample states also released easily accessible data on confirmed or probable COVID-19 cases at a finer geography than county. These states and the lowest level of geography at which they supply data on cases were: Connecticut (town), Florida (town), Illinois (zip), Louisiana (tract), Massachusetts (town), Maryland (zip), Rhode Island (zip), and South Carolina (zip). For all states except Louisiana, we use the case rates as of the date the nursing home infection data was pulled. For Louisiana, we use data from May 31, because we have not been able to obtain data from the previous week.

C.1.2 Nursing Home Staff Geography Measures

To calculate our nursing home staff neighborhood characteristics, we first use the 2017 LODES Workplace Area Characteristics data to identify the nursing home's census block. This data provides the number of workers who work on a given census block in twenty industry groupings (roughly equivalent to NAICS two-digit codes). I use national estimates of total nursing home workers (1.6 million using the American Community Survey) to total nursing home residents (1.3 million from Nursing Home Compare data), and facility-level estimates of residents from Nursing Home Compare to calculate a predicted employment count for each facility by multiplying each facility's resident count by the national ratio of nursing home workers to residents (1.6/1.3=1.23). I start by assuming that the census block returned by entering the facility's address in Nursing Home Compare into the Census geocoder is correct if health care and social assistance employment is at least half of the predicted nursing home employment, which is true in 83% of cases. However, if employment in the health care and social assistance sector on this block is less than half the predicted employment, I instead use the closest block in the same block group whose health care employment satisfies this condition. I am able to find a match this way for 11% of facilities (out of 17%). This leaves about 511 facilities unmatched, which we exclude from our sample. These facilities could potentially represent facilities who use a different address in the

Nursing Home Compare data than in the administrative employment data, or errors in the geocoding process.

Using the census block chosen above, we then use the LODES Origin-Destination (OD) data to identify the home census blocks of workers who work on the same census block as the nursing home and belong to the "all other services'" industry group. One concern is that there may be many more service sector employers that are on the same census block as the nursing home, and these employers have staff who live in completely different neighborhoods than the nursing home's staff. We find on the median block, 92% (IQR: [.62, 1]) of the service employment on these census blocks is in the education and health care sector. This gives us reassurance that we are not mostly picking up an entirely different type of employer on these blocks and that our geocoding is likely to be fairly accurate.

However, it is still possible that there are other education or health-care employers on the same block as the nursing home. One particular case of this is nursing homes located near hospitals. Indeed, we do find some blocks with unreasonably large numbers of health care workers for a nursing home (5000+). However, in general, we find that the total employment numbers are reasonable. Using the calculation for predicted nursing home employment above, the median block in our sample has an actual service sector employment to predicted employment ratio of 1.5 (IQR [1.1, 2.7]). Thus, we believe it is likely that the measured neighborhood characteristics will be largely representative of the types of neighborhoods where a facility's employees are likely to live.

To calculate the "share frontline" measure, we define a frontline worker as a worker in an essential industry (as defined in Tomer and Kane, 2020) in an occupation that cannot work from home (as defined in Dingel and Neiman, 2020).

C.2 Supplementary Tables and Figures

Table C.1: *Summary statistics for analysis sample.*

Variable	**Mean**	**Std. Dev.**
Number of beds	119	67
For-profit	0.73	
Non-profit	0.24	
Public	0.03	
Chain	0.58	
Star rating	3.1	1.4
- Inspection rating	2.7	1.2
- Staffing rating	2.9	1.1
- Quality measure rating	3.8	1.2
RN wage	34.3	6.4
CNA wage	15	2.7
Occupancy rate	0.84	0.13
Medicaid share	0.6	0.23
Resident share non-white	0.23	0.23
Staff tract pop density (pp/sq mi)	4906	7386
Staff tract pub trans use	0.05	0.08
Staff tract share nonwhite	0.28	0.17
Staff tract pov rate	0.19	0.07
Staff tract share frontline	0.31	0.03
NH tract pop density (pp/sq mi)	4543	9484
NH tract pub trans use	0.05	0.1
NH tract share nonwhite	0.25	0.22
NH tract pov rate	0.18	0.17
NH tract share frontline	0.3	0.06

Table C.2: *Characteristics of nursing home workers from American Community Survey and BLS data*

Variable	Share
Top Occupations (OES)	
Certified Nursing Assistant	0.38
Licensed Practical Nurses	0.13
Registered Nurses	0.1
Food preparation and serving	0.1
Building cleaners	0.1
Office and administrative support	0.05
Other healthcare practitioners (therapists, etc.)	0.03
Laundry workers	0.02
Other	0.09
Demographics (ACS)	
Female	0.84
White non-hispanic	0.55
Black	0.27
Hispanic or Latino	0.06
High school or less	0.38
Some college	0.27
Two year degree	0.15
Four year degree or more	0.19
Commute to work by car	0.92
Commute to work on public transportation	0.04
Annual wage/salary income $<$ \$30,000	0.59
Annual wage/salary income $<$ \$50,000	0.82

Table C.3: *Relationship of facility deaths per bed with proxies for staff characteristics and staff neighborhood characteristics*

	(1)	(2)
Staff tract pubtrans use	1.155^{***}	
	[0.266]	
Staff tract PT share (Ed/Health)	0.048	
	[0.139]	
Staff tract share nonwhite		1.041^{**}
		[0.316]
Staff on block share nonwhite		-0.210
		[0.208]
For-profit	0.549^{**}	0.569^{**}
	[0.193]	[0.194]
Chain	0.427^{**}	0.415^{*}
	[0.162]	[0.162]
Star rating	0.034	0.033
	[0.091]	[0.091]
No prior infection viol.	0.230	0.228
	[0.195]	[0.196]
Medicaid share	0.030	0.030
	[0.088]	[0.088]
Resident share nonwhite	-0.202	-0.281^{*}
	[0.120]	[0.130]
Avg severity	-0.052	-0.044
	[0.081]	[0.082]
Occupancy Rate	0.661^{***}	0.669^{***}
	[0.087]	[0.087]
Num beds	0.298^{***}	0.290^{***}
	[0.084]	[0.085]
Constant	2.928^{***}	2.912^{***}
	[0.180]	[0.181]
Fixed Effects	County	County
Depvar mean	3.735	3.735
Adj R^2	0.29	0.29
N	6146	6146

Table C.4: *Relationship of binary measure of facility infection (any death) with facility and neighborhood characteristics*

	(1)	(2)	(3)
Staff tract pubtrans use		0.094***	
		[0.025]	
NH tract pubtrans use		-0.029*	
		[0.013]	
Staff tract share nonwhite			0.063***
			[0.017]
NH tract share nonwhite			-0.014
			[0.009]
For-profit	0.046***	0.046**	0.045**
	[0.014]	[0.014]	[0.014]
Chain	0.024*	0.025*	0.025*
	[0.012]	[0.012]	[0.012]
Overall Rating	-0.016*	-0.015*	-0.015*
	[0.007]	[0.007]	[0.007]
No prior infection viol.	0.007	0.006	0.006
	[0.014]	[0.014]	[0.014]
Medicaid share	0.000	0.001	0.001
	[0.007]	[0.007]	[0.007]
Resident share nonwhite	0.032***	0.027**	0.021*
	[0.008]	[0.009]	[0.010]
Avg severity	-0.022***	-0.020***	-0.019**
	[0.006]	[0.006]	[0.006]
Occupancy Rate	0.021**	0.021**	0.021**
	[0.006]	[0.006]	[0.006]
Num beds	0.071***	0.071***	0.070***
	[0.006]	[0.006]	[0.006]
Constant	0.403***	0.404***	0.405***
	[0.013]	[0.013]	[0.013]
Fixed Effects	County	County	County
Depvar mean	0.455	0.455	0.455
Adj R^2	0.39	0.39	0.39
N	6132	6132	6132

Table C.5: *Relationship of facility deaths per bed with distance to central business district and staff and nursing home neighborhood characteristics*

	(1)	(2)
Distance to CBD	-0.985***	-1.057***
	[0.251]	[0.245]
Staff tract pubtrans use	0.823*	
	[0.336]	
NH tract pubtrans use	-0.060	
	[0.163]	
Staff tract share nonwhite		0.570*
		[0.244]
NH tract share nonwhite		-0.041
		[0.126]
For-profit	0.559**	0.559**
	[0.193]	[0.193]
Chain	0.425**	0.422**
	[0.162]	[0.162]
Star rating	0.036	0.036
	[0.091]	[0.091]
No prior infection viol.	0.238	0.241
	[0.195]	[0.195]
Medicaid share	0.074	0.068
	[0.088]	[0.088]
Resident share nonwhite	-0.276*	-0.327*
	[0.123]	[0.136]
Avg severity	-0.059	-0.053
	[0.081]	[0.082]
Occupancy Rate	0.653***	0.657***
	[0.088]	[0.088]
Num beds	0.280***	0.272**
	[0.084]	[0.084]
Constant	2.959***	2.956***
	[0.180]	[0.180]
Fixed Effects	County	County
Depvar mean	3.736	3.737
Adj R^2	0.29	0.29
N	6141	6142

Table C.6: *Subsample analysis of relationship of facility deaths per bed with staff neighborhood characteristics in sample excluding nursing homes whose blocks have significant additional service sector employment*

	(1)	(2)
Staff tract pubtrans use	1.032*	
	[0.424]	
NH tract pubtrans use	0.055	
	[0.214]	
Staff tract share nonwhite		0.975**
		[0.304]
NH tract share nonwhite		-0.152
		[0.163]
For-profit	0.327	0.328
	[0.274]	[0.274]
Chain	0.349	0.332
	[0.215]	[0.214]
Overall Rating	0.140	0.151
	[0.114]	[0.114]
No prior infection viol.	0.328	0.325
	[0.251]	[0.251]
Medicaid share	-0.124	-0.137
	[0.131]	[0.131]
Resident share nonwhite	-0.194	-0.265
	[0.157]	[0.180]
Occupancy Rate	0.661***	0.674***
	[0.128]	[0.127]
25-50 beds	0.000	0.000
	[.]	[.]
50-100 beds	0.873	0.869
	[0.554]	[0.554]
100-150 beds	1.481**	1.482**
	[0.552]	[0.552]
150-200 beds	2.157***	2.118***
	[0.582]	[0.582]
200+ beds	1.646**	1.616**
	[0.622]	[0.622]
Constant	2.030***	2.085***
	[0.584]	[0.584]
Fixed Effects	County	County
Depvar mean	3.967	3.967
Adj R^2	0.27	0.27
N	3991	3991