ABSTRACT

Title of Dissertation:	THE EFFECT OF COMMUNITY HEALTH CENTERS ON HEALTH CARE ACCESS, CRIME, AND INTERACTIONS WITH THE MEDICAID PROGRAM
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Health centers are community-based clinics that provide services to medically underserved populations. They serve nearly 30 million adults nationwide and more than 90% of patients come from households earning below 200% of the federal poverty level. To date, we know very little about the impact of health centers on measures of social wellbeing.

This dissertation estimates the causal impact of the health centers using the staggered expansion of health centers between 2006 and 2020 and advancements in causal inference methods that allow for unbiased identification of treatment effects in the presence of variation in treatment timing and treatment effect heterogeneity. I use the Centers for Medicare and Medicaid Services Provider of Services file to identify the introduction of health centers over time. Measures of primary care access come from the Dartmouth Atlas and the FBI's UCR Offenses Known and Clearances by Arrest (2005-2016) files are used to measure agency and county level crime rates. Area-by-year covariates are compiled from several sources.

The empirical approach uses staggered difference-in-differences where treatment is defined as the year the first health center receives certification in a county-year. Major findings suggest health centers increase annual visits with a primary care clinician by 4.5% within 7 years after certification among Medicare fee-for-service beneficiaries. I find health centers

reduce the total crime rate by 7% over the period. Results are robust to several alternative specifications. While results on Medicaid interactions are inconclusive, they suggest declines in crime are largest in counties that experienced a health center opening and Medicaid expansion.

My dissertation adds to the literature on the impacts of the Health Center Program's main objective—increasing access to care. In addition, my findings broaden the literature related to health access programs and crime. The Health Center Program has grown considerably in size and scope since inception, and it is a centerpiece of many policy approaches to reform the US health care system. Findings from my dissertation have important policy implications for health, criminal justice, and social justice reforms.

THE EFFECT OF COMMUNITY HEALTH CENTERS ON HEALTH CARE
ACCESS, CRIME, AND INTERACTIONS WITH THE MEDICAID PROGRAM

by

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Chapter 1. Introduction

Health centers are community-based providers that serve populations experiencing a shortage of health services (42 USC §254b: Health Centers, 2021). Health centers serve 30 million individuals nationwide (HRSA, 2019a). Nearly 70% of health center patients are uninsured or covered by Medicaid or CHIP and over 91% come from households below 200% of the federal poverty level (HRSA, 2019a). Health centers are a significant and growing source of primary medical and behavioral health services, providing 81 million medical and 13 million behavior health visits per year (Chang et al., 2019; HRSA, 2019a). In fiscal year 2019, the federal government allocated $1.6 billion for Section 330 grant appropriations and $4 billion in mandatory spending through the newly created Community Health Center Fund (Rosenbaum et al., 2019).

Health centers are required to provide primary care, basic lab services, and emergency medical services on a sliding fee scale or at a reduced cost for low-income patients. However, Rural Health Clinic (RHC) and Federally Qualified Health Center (FQHC) programs operate under distinct regulatory guidelines and health centers with designation under these programs must follow similar, but separate rules. For example, FQHCs (but not RHCs) must also provide pharmacy, preventive services, enabling services (case management; outreach; transportation) and preventive dental. Also unlike RHCs, FQHCs must have a 51% patient governing board (HRSA, 2006). All health centers must operate in areas with scarce health care resources suggesting their introduction may address barriers to access among the populations they serve.

A large body of literature suggests health centers are associated with increased access to primary care services and reductions in hospitalizations for preventive conditions (Bailey & Goodman-Bacon, 2015; C. Evans et al., 2015; Falik et al., 2001; McMorrow & Zuckerman,

2014; Rothkopf et al., 2011). However, few studies have attempted to isolate the causal impact of health centers access, health, or social outcomes

Bailey and Goodman-Bacon (2015) demonstrated that the roll-out of the program in 1960's reduced elderly mortality (Bailey & Goodman-Bacon, 2015). More recent work has established a causal link between health centers and reductions in teen births (Farid, 2020). However, no other studies, to my knowledge, have used a credible identification strategy to isolate the causal impacts of health centers and none of focused on measures of social well-being.

A growing literature, primarily focused on Medicaid, suggests that health access programs not only have positive effects on financial protection, health care access and health, but also promote broader dimensions of social and economic well-being (Boudreaux & Lipton, 2019; Callison & Sicilian, 2018; Cohodes et al., 2016; Goodman-Bacon, 2016; Lee, 2019; Levine & Schanzenbach, 2009; Miller & Wherry, 2019). One important dimension that has received increasing attention is crime (Arenberg et al., 2020; Aslim et al., 2020; Fry et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017).

Crime is a disruptive force. In 2019, the estimated losses for victims of property crime, alone, reached nearly $16 billion nationwide (U.S. Department of Justice, 2020). It also consumes a large share of government spending. Federal, state, and local levels of government spent more than $280 billion on criminal justice programs in fiscal year 2012 (GAO, 2017).

Historically, the primary policy tool used to control crime has been to increase the presence of police. Empirical evidence generally suggests that increased policing does indeed reduce crime (W. Evans & Owens, 2007; Levitt, 2002, 2004). However, increased policing results in large costs to local communities in the form of excessive use of force (Edwards et al.,

2019). The burdens of crime as well as punitive efforts by police agencies disproportionately impacts Black, Hispanic, and low-income populations (Travis et al., 2014).

The policy focus on policing focuses only on increasing the costs of crime to potential criminals and ignores a larger set of determinants, including health, and especially behavioral health, that give rise to crime. A small, but emerging body of work provides evidence that health policy interventions are effective in reducing crime (Arenberg et al., 2020; Aslim et al., 2020; Fry et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017). This work, primarily focused on Medicaid, suggests that programs designed to increase access to care significantly reduce crime and incarceration. A related set of papers find that increasing the supply of mental health and substance use providers is also associated with reductions in crime (Bondurant et al., 2016; Deza et al., 2020, 2021).

This project extends that literature by examining the impact of the health centers on crime. I also examine whether the association of the health centers and crime varies by Medicaid expansion. Health centers and Medicaid expansion may act as substitutes if they both reduce crime independent of the other or they may act as complements if program effects are larger where both treatments are observed.

The objectives of this dissertation are three-fold:

(1) Estimate the effect of community health centers on annual visits with a primary care clinician between 2009 and 2016

(2) Estimate the effect of health center availability on county level crime between 2006 and 2016

(3) Evaluate the interactive effects of the ACA Medicaid expansion and health center availability on county level crime between 2006 and 2016

To accomplish these objectives, I use the CMS Provider of Services (POS) files to identify the introduction of new rural health clinic and federally qualified health center (FQHC) locations over time, at the county level. RHC/FQHC "openings" are measured as the first date of certification with CMS. The public-use POS data is then merged with county-by-year measures of annual visits with a primary care clinician from the Dartmouth Atlas, publicly available county-level crime rates from the FBI's Uniform Crime Reporting Program (Kaplan, 2021), and area-by-year covariate data compiled from a variety of sources. The empirical approach uses a staggered difference-in-differences strategy that compares changes in outcomes, before and after a county obtains a center, to changes in a set of comparison counties that lack a center. I use a variety of estimators to implement the difference-in-differences comparisons that account for bias from heterogenous treatment effects (Cengiz et al., 2019; Deshpande & Li, 2019; Fadlon & Nielsen, 2015; Farid, 2020; Sun & Abraham, 2020).

This dissertation is organized into five main chapters. In Chapter 2, I provide an overview of health center program requirements and services. Next, I review the literature on health centers and access, and crime. I close Chapter 2 with a description of the mechanisms that may influence the relationship between health centers and crime as well as the conceptual model that motivates hypotheses and drives my analytical approach. In Chapter 3, I examine the effect of health centers on annual visits with a primary care clinician among Medicare fee-for-service beneficiaries using data from the Dartmouth Atlas. My results demonstrate that health centers increase primary care visits by 4.5% within seven years after health center certification and effects emerge three years following certification. These are some of the first quasi-experimental estimates of the causal effect of health centers on use of primary care services and they support my conceptual model that links health centers and crime, partially through health centers' effect

on access. In Chapter 4, I examine the effect of health centers on county level total, violent and property crime. Results from this chapter suggest health centers reduce total crime by 7% and the results are robust to several sensitivity tests. The analysis of the interaction effects between health centers and Medicaid expansion on county level crime outcomes is provided in Chapter 5. Overall, I find suggestive but inconclusive evidence that health centers and Medicaid expansion work as complements. In Chapter 6, I provide a summary of my results from Chapters 3-5 and offer concluding remarks on implications and future directions.

Chapter 2. Background and Conceptual Model

Overview

In this chapter I begin with a review of health center program requirements and services. Next, I review the literature on health centers, health care access, and crime. I provide evidence from the literature to support the mechanisms that I hypothesize influence the relationship between health centers and crime. Last, I present a conceptual framework that motivates the project and guides my empirical approach.

Health Centers

History

Health centers were first established as a small anti-poverty initiative in the 1960s, alongside several programs in President Lyndon Johnson's "War on Poverty" (Bailey & Danziger, 2013). Unlike other health access programs such as Medicare and Medicaid, the Health Center Program was designed to fund delivery sites directly, targeting low-income communities. Contemporary health centers are primarily financed by a combination of federal grant dollars and enhanced payments from Medicaid (Rosenbaum et al., 2019). Health Center Program awardees and look-alikes (those who meet all health center requirements but do not receive grant funding under Section 330 of the Public Health Service Act) can be Community Health Centers, Migrant Health Centers, Health Care for the Homeless Health Centers, and Public Housing Primary Care Centers. All of these are community-based clinics or outpatient health programs that provide a core set of required services to patients in medically underserved areas, regardless of ability to pay. Patients are responsible for up to 20% coinsurance which is adjusted based on ability to pay using a sliding fee scale (HRSA, 2006). Section 330 grant recipients, those deemed eligible for a grant (look-alikes), or outpatient health programs operated

by a tribe or tribal organization are considered eligible to receive enhanced, cost-based, reimbursement from the Medicare and Medicaid programs (FQHC certification) as well as discounts for pharmaceutical products through the 340B Drug Pricing Program (HRSA, 2016).

Unlike HRSA-designated health centers, Rural Health Clinics (RHCs) do not receive a Section 330 grant. Although they are similarly eligible to receive enhanced reimbursement, they apply directly for RHC status with Medicare. Those deemed eligible for Medicare are automatically eligible to accept Medicaid. Eligibility requirements for RHCs and FQHCs are provided in Table 2.1, below.

Services

Table 2.1. Health center eligibility requirements		
Criteria	**Rural Health Clinic**	**Federally Qualified Health Center (FQHC)**
Location	Non-urbanized Area	N/A
Shortage Area	MUA, HPSA or Governor Designated Shortage Area	MUA or MUP
Corporate Structure	Unincorporated, public, nonprofit or for profit	Tax-exempt nonprofit or public
Board of Directors	N/A	Required, Majority Patient
Clinical Staffing	MLP required at least 50% of the time the clinic is open	No specific requirements
Source: HRSA. (2006). *Comparison of the Rural Health Clinic and Federally Qualified Health Center Programs.* https://www.hrsa.gov/sites/default/files/ruralhealth/policy/confcall/comparisonguide.pdf **Notes:** MUA/P= Medically underserved area or population, HPSA= Health professional shortage area, MLP= Mid-level provider (Physician Assistant or advanced practice nurse).		

RHCs and FQHCs are both required to provide primary care, basic lab services, and emergency medical services (HRSA, 2006). The FQHC program further requires that health centers provide referrals for specialty care and other services like behavioral health care; case management; and enabling services (42 USC §254b: Health Centers, 2021).

Health centers have always provided social support in the form of enabling services, which are designed to address the social determinants of health (Geiger, 2005). These include services like patient transportation, language translation, eligibility determination for housing and other benefits, and patient education. Yue and colleagues, (2019) used a nationally representative sample of health center patients to examine the association between enabling services and outcomes. They found that more than 80% of patients reported receiving at least 1 enabling service. Enabling services were positively associated with utilization including visits to a health center (1.92 additional visits) and the probability of having a routine checkup (11.78 percentage-point increase) compared with matched controls (Yue et al., 2019).

Patient Governance

A core and unique feature of the Health Center program is the requirement that 51% of the governing board must include patients and the board, as a whole, must be representative of the patient population (42 CFR § 51c.304). The board is granted some regulatory authority over scope and availability of services and therefore this requirement could enable health centers to efficiently address community needs using insights from patient board members. However, recent evidence suggests that less than 25% of board members reflect the socio-demographic composition the patient population served by health centers with considerable regional variation (Wright, 2013). And despite regulatory authority, most decision-making on scope and service availability happens at the clinician and director levels rather than originating with health center governing boards (Wright & Martin, 2014). While patient board members may, in practice, have limited authority over clinic operations they may have a unique role in building local connections with community leaders, enabling health centers to outreach through trusted community members.

Patient population

Today there are over 13,000 individual FQHC sites serving nearly 30 million patients nationwide. Between 2000 and 2019 the number of patients seen by health centers grew by 211% (HRSA, 2019b). To receive designation, RHCs and FQHCs must provide services to medically underserved areas or populations (MUA/Ps), defined as an area or population group with a shortage of primary care health services (42 USC §254b: Health Centers, 2021; HRSA, 2006). MUA geographies can vary and may include a whole county, a group of neighboring counties, or smaller geographies (HRSA, 2021). MUAs are often characterized as being low-income and with larger shares of the population being of racial/ethnic minority groups and this is reflected in the demographic characteristics of patients seen by health centers. In 2019, 63% of health center patients were from racial and/or ethnic minority groups, more than 24% were best served in a language other than English, and over 91% came from households earning less than 200% of the federal poverty level (HRSA, 2019a).

Growth of health centers

The Health Center Program has experienced considerable recent growth due to public investments to address gaps in the health care safety-net. First, FQHCs (a subset of CHCs who meet certification requirements) were added as a required benefit under Medicare and Medicaid in 1991 which enabled cost-based payment by CMS (CMS, 2017). The Health Center Growth initiative (2001) aimed to expand the program by adding 1,200 new or expanded health center sites in areas of greatest need. By fiscal year 2007, federal funding for the Health Center Program was nearly double ($2 billion) 2002 funding levels and the number of patients seen increased by almost 6 million between 2001 and 2007 (HRSA, 2008). The 2009 American Recovery and Reinvestment Act (ARRA) added $2 billion in federal funding for clinic and staff

and service expansions at the height of the economic recession. The ACA provided an additional $11 billion to support infrastructure improvements and expansion of service delivery sites and services. Between 2007 and 2014 there was an 82.7% increase in health center sites with considerable growth occurring in 2014 compared with previous years (Chang et al., 2019).

In addition to increased funding levels, the ACAs Medicaid expansion significantly reduced the number of uninsured patients at health center sites (Health Resources & Services Administration (HRSA), 2011; Pourat et al., 2018; Shin et al., 2015) while the overall number of patients seen between 2009 and 2019 grew by more than 10 million (HRSA, 2020). This would suggest that clinics from communities with historically low levels of insurance coverage may have benefited the most. This aligns with results from Behr et al., (2022) who found that areas newly served by a health center following the ACA were more likely to have higher rates of uninsurance and poverty (Behr et al., 2022).

As evidenced by its longstanding and significant growth (in terms of federal funds and scope), the health center model has enjoyed broad popularity among policymakers. There was a growing recognition that primary care is associated with reductions in mortality (Bailey & Goodman-Bacon, 2015; Shi et al., 1999), and improved population health outcomes (Shi et al., 1999, 2005; Shi & Starfield, 2000; Starfield et al., 2005) which fueled program expansions in the late 1990's and early 2000's. The program has been viewed by health policymakers as a tool to address disparities in outcomes based on socioeconomic status and race/ethnicity since its creation during the War on Poverty (Bailey & Danziger, 2013). Health centers are frequently a centerpiece of national health reforms including those prescribed by the ACA like the Patient Centered Medical Home (PCMH) model, which emphasized the role of the primary care physician.

Theoretical perspectives on the RHC and FQHC programs

Theoretical frameworks from economics and sociology lend support for RHC and FQHC programs as tools to promote public health. At their core, health centers aim to increase the accessibility of primary health services by reducing costs and increasing availability. The Grossman human capital model of the demand for health describes health as both a consumption and investment good. Under this model individuals are born with an initial stock of health that depreciates over time and can be increased through investments. Those investments are limited by an individual's budget constraint (Grossman, 1972). However, price reductions would, according to Grossman, drive individuals to maximize their utility by consuming more health services on a fixed budget constraint. A large body of evidence suggests that health care consumption is indeed sensitive to price. I return to that literature in the pages that follow. In addition to reducing the price of health care services themselves, health centers may reduce the indirect costs of obtaining services if they are located geographically closer to the communities they serve, when they provide enabling services, and if they provide a more culturally appropriate setting that is easier to navigate.

At the population level, Link and Phelan's theory of fundamental cause suggests that even with scientific and policy advancements in the health care system, disparities between those with and without access to flexible resources such as money, information, and power would persist (Link & Phelan, 1995). Therefore, policy prescriptions to address health disparities would require more a more equitable distribution across these "social conditions" (Phelan et al., 2010). By directly funding delivery sites in medically underserved areas health centers are designed to redistribute resources from taxpayers into health investments in underserved areas. Moreover,

patient eligibility for the FQHC sliding-fee discount applies only to those earning less than 200% of the federal poverty level. The targeted approach of the cost-reduction mechanism and the MUA service requirements may have helped to shrink the persistent disparities in access and outcomes among communities who have been clinic recipients.

The Aday-Andersen model of health services use provides a broader framework for organizing the insights of both Grossman and Link and Phelan. There is broad agreement in the literature that health centers increase access to in primary care (Bailey & Goodman-Bacon, 2015; Farid, 2020; McMorrow & Zuckerman, 2014) and reduce hospitalizations (Epstein, 2001; C. Evans et al., 2015; Falik et al., 2001; Rothkopf et al., 2011). The Aday-Andersen model of health services use provides a robust framework for understanding health care access (Aday & Andersen, 1974). Under this framework predisposing factors are those that describe individual likelihood of health care use (or use of health centers in this context). Health center data show that patients are more likely to be non-white, below 200% of the federal poverty level, on Medicaid or uninsured, and (inherently by the program's requirements) lack sufficient availability to other health resources (HRSA, 2019a). Enabling factors describe resources which support the use of services. These include area-level poverty, unemployment, and public insurance rates.

These perspectives suggest injecting communities with shortages of primary care services with health center delivery sites and offering high quality care at a reduced price should increase the use of health services among low-income groups.

Evidence on health centers and access

Health center impacts are difficult to assess for several reasons. First, communities that receive health center sites may be different from those who do not receive them based on

insurance coverage rates, income, racial/ethnic make-up, health care resources, and other hard measure features of community level infrastructure that enable health care utilization. Next, the health center program does not operate in isolation. US health reforms have largely targeted low-income and uninsured groups. Therefore, communities often receive multiple treatments such as insurance expansions, payment reforms, and other system-level interventions in conjunction with new health center sites.

At the individual level, health center patients tend to be lower income, more likely to be from racial/ethnic minority groups, and more likely to be uninsured or covered by Medicaid/CHIP making confounding in observational studies a significant concern (Austin, 2011). This is particularly challenging if researchers are interested in clinical outcomes (via administrative claims) where demographic information on patients is relatively limited (compared with federal surveys), and of mixed quality. Next, there are few nationally representative survey samples that can identify health center patients (and control groups). Finally, from a causal inference perspective, there is heterogeneity in treatment timing (resulting from variation in the creation of new health centers from one geography to the next) which creates a significant challenge in isolating health center effects and identifying adequate control groups when the treatment is not limited to a single unit of observation or a single post treatment period (Athey & Imbens, 2021; Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2018b; Sun & Abraham, 2020).

Research examining health centers and health care use among the Medicaid or uninsured populations has generally suggested health centers are associated with reductions in hospitalizations (Epstein, 2001; C. Evans et al., 2015; Falik et al., 2001; Laiteerapong et al., 2014; Rothkopf et al., 2011) and emergency department use (Falik et al., 2001; Laiteerapong et

al., 2014) and positively correlated with access to primary care services (McMorrow &

Zuckerman, 2014; Shi & Stevens, 2007). However, these studies are largely limited to using

state-based cross-sectional data.

Laiteerapong et al., (2014) used the Medical Expenditure Panel Survey (2004-2008) to

examine the relationship between health centers and health care use. They matched health center

provider addresses with HRSA-provided health center locations, using the panel design of the

MEPS to explore changes in health care use. They defined health center patients as those with

greater than or equal to 50% of outpatient visits at a health center, using a propensity score

method to balance treatment and control groups. Among all patients, receiving primary care at a

health center was associated with fewer office visits and hospitalizations, and higher odds of

receiving breast cancer screening. Results were similar in a subgroup of uninsured patients,

however, among this group they found no statistically meaningful differences in hospitalizations

which is inconsistent with results from similar studies (Epstein, 2001; C. Evans et al., 2015;

Falik et al., 2001; Rothkopf et al., 2011). This study relied on using outpatient care as a proxy for

primary care in the MEPS and due to the lack of a primary care measure they were unable to

determine whether their control group received primary care services. This limitation may bias

their results towards a larger effect size without an ability to measure the extent of the bias.

Additionally, propensity score methods rely on the assumption that all factors associated with

treatment and outcomes are observed and included in the propensity score estimation model

(Imbens & Wooldridge, 2009). This is difficult to achieve in practice, making causal

interpretation limited.

LoSasso and Byck (2010) used data from HRSA's Uniform Data System to examine the

associations between federal, state, and private grant funding support and a range of outcomes

including scope of services provided (e.g., number of sites per grantee; 24-hour operation, emergency medical and urgent care), provision of behavioral health services, staffing, and provision of uncompensated care. Among other findings, their results suggested that federal funding increases were associated with increases in the provision of 24-hour operation, behavioral health services (including mental health treatment and counseling; 24-hour crisis intervention; and substance use disorder treatment and counseling) and reductions in uncompensated care. However, they did not have the ability to come to conclusions about how increased provision they observed impacted the health outcomes of health center patients (Lo Sasso & Byck, 2010).

McMorrow and Zuckerman (2014) combined data from HRSA's Uniform Data System with individual level outcome data from the National Health Interview Survey (2001-2008) to examine the impact of per person health center funding at the hospital referral region level on several measures of access among low-income adults (below 200% of the federal poverty level) aged 19-64. Using linear probability models with and without market-level fixed effects they found that increases in health center funding were positively associated with the probability of having an office visit or a general doctor visit, and a usual source of care. However, the NHIS did not permit them to identify actual health center users, rather they examined the effects of funding among low-income groups who are most likely to benefit from health center expansions (McMorrow & Zuckerman, 2014). This study provides strong evidence that health center funding can improve access to primary care.

Saloner et al., (2019) conducted a two-period, 10-state "audit" style study to examine appointment availability between community health centers (CHCs) and private clinics before and after implementation of the ACA. Although the study only included two time periods—

researchers used a difference-in-differences style framework to examine the effects of the ACA on availability. The sampling frame of primary care offices came from a list of practicing office-based physicians including CHCs. Trained callers who were randomly assigned to insurance status and various clinical health scenarios. Callers followed a standard protocol seeking to schedule an appointment at the earlies available appointment window. Relative to non-CHCs appointment rates increased and wait times decreased at CHCs among callers assigned to employer sponsored insurance and remained stable for those assigned to Medicaid or the uninsured group (Saloner et al., 2019). Their data did not permit them to inspect pre-period trends limiting the interpretation of their results.

These studies provide important evidence that health centers are associated with improved access to primary care services, but they do not examine health effects or other downstream outcomes of patients served by the program. In addition, there may be unobservable differences between counties with and without health centers that would limit their comparability (apart from McMorrow & Zuckerman, 2014). And due to the staggered growth of the health center program across time and geography there may be county treatment cohort effects, beyond having a health center, that are not addressed by these previous studies. For example, policy learning and or program reforms may influence the rollout of health in counties who adopted in more recent years (Bailey and Goodman-Bacon, (2015) present suggestive evidence to this effect). The event-study framework and recent extensions to the difference-in-differences setup, discussed further in the methods section, can overcome these challenges.

Bailey and Goodman-Bacon, (2015) used the variation in the rollout of the first community health centers to examine short and long-term effects on age-adjusted mortality. They combined data on health center adoption from administrative Public Health Service

Reports and 1965-1974 National Archives Community Action Program (NACAP) files, linking estimated mortality-rates from 1959-1988 Vital Statistics files by county. Their event-study approach was conducted in relative time, capturing outcomes before and after treatment and using a binary treatment indicator to identify counties that received a health center to counties that did not. The introduction of a health center was associated with a 2 percent reduction in age-adjusted mortality within 10 years, largely driven by a 2.2 percent reduction among adults 50 years or older. Analyzing the effects by cause of death revealed the greatest reductions occurred for chronic conditions, supporting the notion that health centers improve access to early detection and disease management. Further analyses revealed that health center openings were associated with improvements in the likelihood of reporting a usual source of care and reductions in the likelihood of paying out of pocket for prescription drugs among low-income older adults (Bailey & Goodman-Bacon, 2015).

Farid (2020) examined the effect of FQHCs on teen birth rates taking advantage of the staggered expansion of the health center program between 2007 and 2018. Unlike Bailey and Goodman-Bacon who compared counties with a health center opening to those without, Farid (2020) compared those with an FQHC opening between 2007 and 2012 to counties that received treatment at a later period outside of the study window (and conducted a robustness check using never-treated counties). A stacked regression approach was used by constructing treatment and control datasets for each year (2007-2012) cohort and appending them to have a long panel dataset. FQHC openings were identified using the Centers for Medicare and Medicaid (CMS) provider of services (POS) files. Data on births came from Vital Statistics records and graduation rates were estimated using the American Community Survey (ACS). Results suggested the introduction of a FQHC was associated with a 10 percent decline in teen birth rates. Effects on

birth rates were largest in counties that experienced 3 or more openings. The authors also found that among counties with more than 1 FQHC opening there was a decline (between 17 and 28%) in the proportion of women who did not graduate, suggesting heterogeneous treatment effects based on the number of new clinics (Farid, 2020).

Analogous Evidence from other Health Care Expansions and Experiments

Health centers are unlike other access programs, such as Medicare and Medicaid, because they provide direct delivery sites to the target population. However, consistent with insurance programs, health centers also reduce the cost of health care. By offering services on a sliding-fee scale based on the patient's ability to pay there may be fewer financial barriers and/or consequences to seeking treatment for medical and behavioral health care. Causal evidence on health center impacts is small relative to the body of work exploring the effects of out-of-pocket cost reductions and demand for medical services. The following review of that literature provides insights about the CHC program.

Some of the most influential applied work on health care price elasticity comes from the Rand Health Insurance Experiment (HIE). Initiated in 1974 by what is now the US Department of Health and Human Services, the randomized study enrolled families at six sites into one of 15 treatment arms with varying levels of cost sharing. The main goals of the study were to understand the relationship between cost sharing and health care utilization, health care quality, and health among the nonelderly, nondisabled, and noninstitutionalized US population (Manning et al., 1987; Newhouse et al., 1981). Manning et al., (1987) found that demand for outpatient services (consistent with those provided by health centers) was sensitive to out-of-pocket amounts defined by the fee-for-service plan treatment assignment. Compared with those on the 95% coinsurance plan, outpatient spending on the free plan was 67% higher and the largest

difference in outpatient spending occurred between the free plan and the 25% coinsurance plan (Manning et al., 1987).

The Oregon Health Insurance Experiment (2008-2010) was the first *randomized* study focused on expansion of the Medicaid program. Enrollment reductions in years prior to 2008 enabled the state, with approval from CMS under waiver authority, to enroll additional low-income adults into their Oregon Health Plan (OHP). The state selected new enrollees through a lottery system, essentially randomizing assignment into the program. Through this variation, researchers have been able to examine the effects of Oregon's Medicaid expansion on health care utilization, health, and related social outcomes like medical debt and employment (Baicker et al., 2013, 2014; A. Finkelstein et al., 2012; A. N. Finkelstein et al., 2016; Taubman et al., 2014).

Evidence from the experiment has shown that Medicaid coverage significantly increased physician office visits and emergency department use (Baicker et al., 2013; A. N. Finkelstein et al., 2016; Taubman et al., 2014). Finkelstein et al., (2016) show that increases in both office visits and emergency departments were persistent two years after implementation of the program suggesting the increased utilization was not due to pent up demand (A. N. Finkelstein et al., 2016). Baicker et al., (2013) examined the effect of winning the lottery on health services use, self-reported (compared with the previous year) and clinically measured (diagnosis of hypertension, high cholesterol, diabetes, and depression) health, and financial strain from medical costs (self-reported). Due to less than perfect take-up and post-selection eligibility exclusions, compliance to randomized assignment was not perfect. Therefore, they deployed an instrumental variable approach using lottery selection as the instrument. Their results were consistent with previously reported findings that Medicaid coverage increased utilization, self-reported health, and reduced financial strain (A. Finkelstein et al., 2012). They estimated that

coverage was associated with an additional $1,172 in annual medical spending and a more than 80% relative reduction in catastrophic out-of-pocket expenditures. Results also suggested that coverage led to a 9.15 percentage point decrease in the rate of depression and improvements in self-reported mental health but no meaningful effects on clinical measures of hypertension or high cholesterol (Baicker et al., 2013). Finally, Baicker et al., (2018) examined the effect of Medicaid coverage on individuals with and without a history of depression. They found that coverage reduced the percent of respondents reporting unmet mental health care needs by 40% and reduced untreated depression by 60% (Baicker et al., 2018).

Other quasi-experimental studies of the Medicaid program have shown results consistent with findings from the Oregon Health Study. Primarily that Medicaid coverage expansions are associated with increased access and use of medical services (Wherry and Miller, 2016; Sommers, Gunja, Finegold et al., 2015; Nikpay et al., 2017) and reductions in out-of-pocket spending on medical and mental health care (Golberstein and Gonzales, 2015; Sommers and Oellerich, 2013) in the short term.

While OHE failed to find changes in mortality, the ACA Medicaid expansion literature has. For example, Miller et al. (2021) found that Medicaid expansion states experienced a 9.4% reduction in mortality compared with non-expansion states (Miller et al., 2021). Mortality effects from the ACA expansion are consistent in other settings. Goodman-Bacon found reductions in infant mortality due to the introduction of Medicaid (Goodman-Bacon, 2018a). Goldin, Lurie, and McCubbin evaluated a randomized experiment in which the IRS sent informational letters (which varied by timing and personalization) to individuals who had paid a tax penalty for noncompliance with the ACA federal insurance mandate. The letters reduced mortality among

middle aged adults (Goldin et al., 2019). This large body of evidence suggests that the failure of the OHE to find mortality effects likely stems from lack of power and a short follow-up period.

Another body of literature has examined the effects of the Medicaid program on additional dimensions of social well-being. In general, these studies have found that Medicaid coverage may lead to improvements in educational attainment (Cohodes et al., 2016; Levine & Schanzenbach, 2009; Miller & Wherry, 2019), labor market outcomes (Boudreaux & Lipton, 2019; Callison & Sicilian, 2018; Goodman-Bacon, 2016; Lee, 2019), and reductions in criminal activity (Arenberg et al., 2020; Aslim et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017). This literature provides strong support for the hypothesis that health centers may also influence a broad array of social measures, including crime.

Crime

Having laid out a case, based on theory and existing empirical evidence, that the health center program increases access to health care and promotes public health, I now turn to crime. In this section I discuss theories that explain the determinants of crime, national trends in violent and property crime, and review the literature that connects health access with crime outcomes.

Theoretical Perspective on the Determinants of Crime

Theoretical criminology frameworks offer explanations for why crimes are committed at the individual and societal levels as well as the role of social structures at enabling or preventing crime from occurring. Rational choice theory argues that criminal offenders are rational actors who seek to maximize utility. Offenders choose to commit crimes after weighing the costs and benefits, including the potential risk of punishment (Becker, 1968; Cornish & Clarke, 1987). This theory provides support for trends in increased police hires and harsher punishment as it

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suggests that increased force may influence the perceived risk of punishment through deterrence mechanisms (Abrams, 2012; Levitt, 2002; Nagin, 1998).

Routine activity theory suggests a subset of crime (direct-contact predatory violations) is a routine activity in society, influenced by the presence of rational and capable offenders, a person or object to target (i.e. "opportunity"), and the absence of police or other factors protective from criminal offending. Therefore, changes in societal activities which enable or prevent the convergence of these factors can impact crime rates. One example of this might be a societal shift in employment behaviors where more individuals from suburban areas begin using public transportation to get to work in an urban setting whereas prior to this shift these individuals were working at settings within their own communities. Another example would be a change from accepting credit card-only at a point of sale to accepting and keeping cash on hand. This would keep other factors constant while increasing the number of suitable targets (Cohen & Felson, 1979).

Social control theory (Hirschi, 1969) draws on other existing principles of social control to explain the extent to which people may not break the law, even when it is in their individual interest to do so. Social ties to family, school, and community serve as protective factors against engaging in criminal behavior and breaking or weakening these ties may leave individuals with a greater likelihood of criminally offending (Hirschi, 2002). As such, community institutions, such as health centers, might play an important role in building community level social environments that protect against crime.

Sharkey et al., (2017) examined the relationship between growth in local organizations and crime using an instrumental variable approach. Their results suggest 10 additional community organizations per 100,000 residents reduced violent crime by 10% and property crime by 7%.

Interestingly, organizations in their sample included those focused on broader social cohesion and not simply those formed to reduce crime or address substance use disorders (Sharkey et al., 2017).

Taken together, these theoretical perspectives suggest that crime arises from a complex set of factors that include individuals that rationally respond to incentives and the social environment that shapes both the nature of those incentives and the individual's calculation of costs and benefits. As I will show to be important later, Health Centers might influence two key features of the social system that shapes the dynamics of crime: They modify access to health which itself influences the risk of criminal behavior and they are they are sources of social capital that Hirschi suggests will reduce the likelihood of crime.

Trends

The U.S. Department of Justice deploys two primary data collection activities to produce estimates of crime in the United States, the Uniform Crime Report (UCR) Program and the National Crime and Victimization Survey. The Uniform Crime Report (UCR) Program, which has been in operation since 1930, exists to provide reliable benchmarks. The primary purpose of these data is to support law enforcement agencies in carrying out their intended goals. The UCR Program data are a collection of monthly agency-level reported crimes aggregated by the FBI. The primary "crime rate" measures come from the UCR Offenses Known and Clearances by Arrest file which contains agency-level counts of index crimes (also known as Part 1 crimes) including homicide, rape, robbery, aggravated assault, burglary, theft, motor vehicle theft, and simple assault. These index crimes are commonly reported as violent (murder; rape; robbery; aggravated assault) and property crime (burglary; theft; motor vehicle theft) indices (Kaplan, 2021; U.S. Department of Justice, 2020).

The United States has experienced dramatic shifts in crime from the 1960s to today. The first period of significant growth in violent crime began in 1962 and lasted until the mid 1970s. During this period the violent crime rate increased from 158 per 100,000 in 1961 to nearly 488 per 100,000 inhabitants in 1975. Scholars suggest the pre-1980 increases in crime were due to a combination of factors including concentrated social disadvantage resulting from the Second Great Migration of Black Americans away from the south to northern and midwestern city centers (Travis et al., 2014), incentives by local police agencies to skew data in favor of receiving increased federal support (Thompson, 2010), and growing social unrest related to anti-war and Civil Rights movements (Lafree & Drass, 1997; Travis et al., 2014).

During the early 1980s violent and property crimes began to decline, falling to ~540 and 4,500 per 100,000, respectively (Figure 2.1). However, that trend quickly reversed with the onset of the crack epidemic and strict anti-drug enforcement which disproportionately impacted large urban centers and young black men. Scholars find that both, increased policing during the War on Drugs and increases in black youth homicides were significant factors in the growth of crime during this period (Blumstein & Rosenfeld, 1998; Grossi, 2020; Saadatmand et al., 2012).

Figure 2.1: *Trends in property and violent crime rates in the United States, 1981-2018*

Panel A. Trends in Violent Crime in the United States
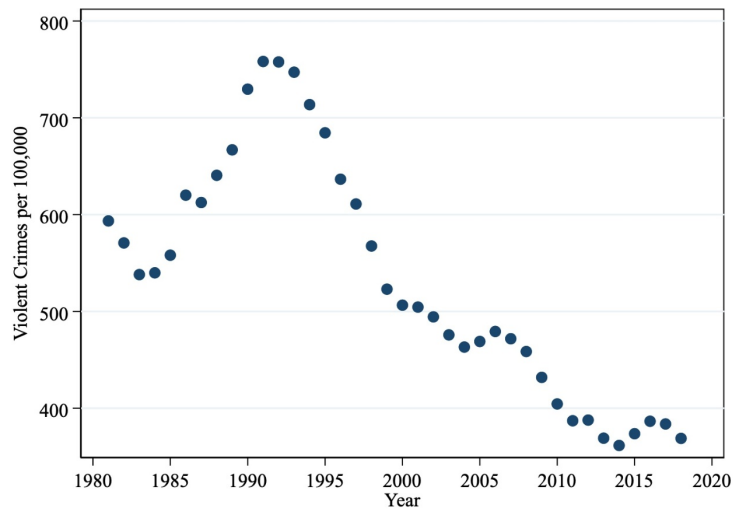
Panel B. Trends in Property Crime in the United States
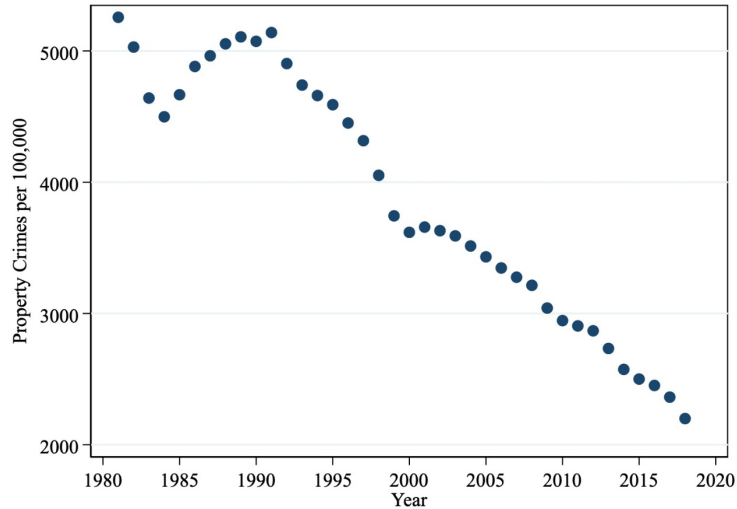
Notes: Violent crimes are offenses of murder, forcible rape, robbery, and aggravated assault. Property crimes are offenses of burglary, larceny-theft, and motor vehicle theft. **Source:** FBI Crime in the United States annual reports (2000, 2019).

Both violent and property crime indices peaked in 1991 (758 and 5,140 per 100,000, respectively) at before falling sharply into steady declines. With few interruptions (a short-term spike in violent crimes from 2004-2006), both crime indices have continued to decline until another short-term increase in violent crime from 2014 to 2016. Explaining the consistent declines in crime beginning in the 1990s has been the focus of a large literature, but no strong consensus has emerged as to its cause and any monocausal explanation is likely insufficient. It is partially explained by growth in the number of police (Chalfin et al., 2020; W. Evans & Owens, 2007; Levitt, 2002; McCrary, 2002) supporting deterrent effect principles. Levitt (2004) argues that in addition to more police, declining crime rates during this period were driven by the dramatic growth of incarceration which started in the 1970s, the decline in the market for crack cocaine, and the U.S. Supreme Court's decision on Roe vs Wade to legalize abortion (Levitt, 2004). Others have argued that reductions in exposure to lead, which causes heighted aggression when exposure occurs early in the life course, might explain the dramatic reductions in crime (Reyes, 2007).

Empirical evidence supporting Hirschi's theory of social control suggests the growth of community-based organizations, including those with a broader strategy aimed at social cohesion, were responsible for considerable reductions in violent and non-violent crime (Sharkey et al., 2017). These organizations, like health centers, are aimed at addressing social determinants and improving the social environment in areas they serve. This evidence connects the non-clinical aspects of a health centers primary mission with crime. However, a growing literature has also linked health and health access with reductions in crime and incarceration as discussed in greater detail below.

Health, Health Care, and Crime

The connection between crime, health, and health care access is complex and bi-directional. The primary motivating connection that motivates this project is that health, as I will show, likely acts as a determinant of crime. While not the focus of this project, crime also likely shapes health outcomes. Below I first discuss health as a determinant of crime and then move on to discuss crime as a determinant of health.

Two strands of evidence suggest that health, particularly mental health, causally influences crime. The first strand suggests that mental health conditions are highly prevalent in the criminal justice involved population. The second suggests that health care interventions causally reduce crime. I review both strands of evidence in turn.

While the vast majority of people with a mental health condition do not commit crimes and pose no public safety risks, among the criminal justice-involved population there is a substantial morbidity of mental health concerns. For example, nearly 67% of jail inmates and 50% of prisoners report serious psychological distress or history of a mental health problem (Bronson & Berzofsky, 2017). Compared with the general U.S. population (5%), prisoners experience nearly three times greater prevalence of serious psychological distress (Bronson & Berzofsky, 2017). This indicates that community policing of mental health is a common practice. Our nation's limited supply of community-based mental health care resources is one factor resulting in the criminalization unmet mental health needs. During the late 1970s and early 1980s the closing of state and county psychiatric facilities ("deinstitutionalization") resulted in large numbers of individuals with unmet mental health needs in the community-- and ultimately growth of persons with mental health needs in jails or prisons (Bloom, 2010). As many as 10% of U.S. police encounters involve persons with mental illness (Franz & Borum, 2011; Livingston, 2016).

The second body of evidence linking health as a causal determinant of crime comes from health-based interventions that reduce crime. These interventions include 1) Environmental regulations that reduce toxic exposures (primarily lead); 2) Distinct health care interventions such as cognitive behavioral therapy; 3) Increases in the supply of behavioral health services; and 4) Expansion of health care access via Medicaid.

The dramatic decrease in environmental lead exposure has been linked to reductions in crime. Lead is a metal that, if absorbed, even at very low levels, causes extreme disruptions in the cognitive development of children (Bellinger, 2008). The effects of lead on the neurological development of children can emerge as social-emotional disorders in adolescents and persist through adulthood (Reuben et al., 2019). Reyes (2007) exploited variation from the state-level phase out of lead in gasoline. They provide strong evidence that reductions in lead exposure may explain more than half of the reduction in violent crime between 1992 and 2002 (Reyes, 2007). While lead has been the most well studied environmental exposure, there is also evidence that other common environmental exposures, including ambient air pollution, increases crime rates (Bondy et al., 2018). This link is a critical component to the hypothesis that health access programs like health centers may influence crime outcomes because it connects improvements in health to declines in crime.

The second set of health-based interventions that have been shown to influence crime are distinct health care service programs. Heller and colleagues, (2013) present results from a randomized controlled trial which examined the impact of delivering cognitive behavioral therapy on crime and educational outcomes among school-aged males (7-10th grade) from high-crime neighborhoods of Chicago. The intervention, "Becoming a Man" took place in 18 Chicago-area public schools and randomly assigned 2,740 male students into intervention and

control groups. The intervention took place over one academic year and included exposure to mentoring by adult role models, in-school and after-school activities, and cognitive behavioral therapy to enhance students' ability to think before they act. The intervention reduced short-term arrests for violent crimes by 44% and weapons and trespassing related crimes by 36% relative to controls (Heller et al., 2013). Evans Cuellar et al., (2006) used exogenous variation from the Texas-based Special Needs Diversionary Program (SNDP) to examine the effect of mental health diversion on youth criminal recidivism. The program provided funds to 19 counties for mental health services among juvenile offenders with mental health needs. Services were contracted by local providers and included therapy, medication monitoring, and crisis mitigation. To evaluate the SNDP program, authors linked administrative data from juvenile corrections with mental health diagnoses and compared program participants with those who were eligible but had not yet participated in the program. They used a propensity score matching method to address selection into treatment. SNDP was associated with a reduction of 63 arrests per 100 participants over a one-year period. (Evans Cuellar et al., 2006). These studies provide evidence that programs which offer targeted mental health services can reduce crime, in the short term, among those who are most likely to criminally offend.

The third set of interventions that have been studied, which is most analogous to the current project, are studies of plausibly exogenous increases in the supply of health care services. Bondurant, Lindo, and Swenson, (2016) studied the effect of substance use treatment facilities on county-level crime. Using variation from annual changes in the number of treatment facilities within a county they found that growth in the number of facilities was associated with reductions in both violent and financially motivated crimes and drug-related mortality. Their results were robust to alternative specifications and data sources (Bondurant et al., 2016). Among other

potential mechanisms they posit that the effects on crime reduction are likely driven by a combination of reducing the use of drugs causing a reduction in violent offenses and reducing the number of financially-motivated offenses by those seeking to purchase drugs (Bondurant et al., 2016).

Consistent with growth in the number of SUD treatment facilities, health centers may expand access to behavioral health services like substance-use disorder treatment—although these services (mental health and SUD combined) are only used by ~10% of health center patients (HRSA, 2019a). While the broad use of illicit drugs is similar across race/ethnicity (Substance Abuse and Mental Health Services Administration (SAMHSA), 2009), and Black and Hispanic populations share the same or lower lifetime risk of mental disorders compared with Whites (Alvarez et al., 2019), there are large differences in mental health services use by race/ethnicity. Among adults with any mental illness, 29.8% of non-Hispanic Blacks and 27.3% of Hispanics report any mental health services use compared with 46.3% among non-Hispanic Whites (Substance Abuse and Mental Health Services Administration (SAMHSA), 2015). While the entirety of these differences cannot be explained by access to a treatment facility, Cummings and colleagues, (2014) show that counties with larger shares of Black residents are less likely to have outpatient substance use disorder facilities suggesting that accessibility may be partially driving the disparities in utilization (Cummings et al., 2014). Nearly half of those with a substance use disorder have a concurrent mental health condition (National Institute on Drug Abuse, 2020). This would suggest that despite the presence of a SUD treatment facility there may still be unmet need for mental health services, particularly in counties with high concentrations of Black and Hispanic residents who have historically received less behavioral health care.

Deza et al., (2020) examined the effect of office-based mental health providers on county level crimes using a two-way fixed effects approach. They identify variation from changes in the number of providers in a county year. Overall, results suggest that 10 additional providers reduce total crime by 0.5% and violent crime by 2%. Similarly, Deza et al., (2021) examined the effect of office-based mental health providers on juvenile arrests and the per capita costs of juvenile arrest to society. They found 10 additional providers was associated with a 2.6% reduction in the societal cost of juvenile arrest (Deza et al., 2021). Although health centers, compared with these providers, are more likely to serve a patient population that is lower-income and covered by Medicaid, this is strong evidence that health centers may also influence reductions in crime.

Finally, interventions that increase health care access via health insurance expansion, have been shown to influence crime. These papers show that Medicaid is associated with reductions in all types of criminal offenses primarily through access to mental health and substance use disorder treatment. Jácome, (2020) leveraged the fact that South Carolina does not offer insurance coverage to childless adults to study the effects of losing coverage at age 19 on later likelihood of incarceration. This study is unique because it links individual administrative records between Medicaid encounters and law enforcement agencies providing significant information on individual offender health characteristics. Using a matched difference-in-differences design, this study shows that men with histories of mental illness who lost coverage at age 19 were significantly more likely to be incarcerated in the following two years compared with those who were estimated to be eligible for Medicaid but not enrolled. Loss of access to mental health services was associated with increases in violent, drug, and property crimes (Jácome, 2020). These results, and others (Wen et al., (2017); Vogler, (2017); Aslim et al., (2020); Arenberg et al., (2020); Fry et al, (2020)), suggest Medicaid coverage reduces criminal

activity through improved access to behavioral health. However, studies examining coverage expansions have shown that expanding Medicaid has had no effect on (Golberstein & Gonzales, 2015) or only marginally increases use of mental health services (Breslau et al., 2020). This topic requires further investigation. Despite finding significant declines in criminal recidivism following Medicaid expansion in Midwest and Southwest counties, Fry et al found an overall increase in the probability and number of arrests in East Baton Rouge (Fry et al., 2020). They posit that the lack of coordination between behavioral health and criminal justice systems may be driving this result.

A second mechanism connecting access expansions, including Medicaid expansion and the CHC program, with potential crime reduction effects is a possible income transfer effect of the program for those with low incomes. Reducing financial stress may reduce financially motivated and violent criminal activity. Previous work has shown that neighborhood disadvantage and individual level financial stress are both associated with increased intimate partner violence (Benson et al., 2003). Additionally, area economic conditions (measured as consumer sentiment) is negatively associated with robbery, burglary, larceny, and car theft (Blumstein & Rosenfeld, 1998).

Unfortunately, the effect of CHC's on financial distress has been understudied. Baily and Goodman-Bacon, (2015) found that the introduction of CHCs was associated with reductions in out-of-pocket costs for prescription medications among adults 50 and older. However, they did not examine outcomes among younger groups who are at greater risk of criminal offenses. The majority of persons arrested for violent and property crimes are between the ages of 25 to 39 years (FBI, Criminal Justice Information Services Division, 2016). Several studies have shown that gaining access to Medicaid can improve financial circumstances (Baicker et al., 2013; Hu et

al., 2018; Sommers & Oellerich, 2013). And studies examining effects of Medicaid on crime find reductions in financially motivated criminal offenses (Arenberg et al., 2020; Vogler, 2017). It could be possible that reductions in medical debt or out of pocket payments for necessary medical care reduce financial stress and associated criminal offenses.

The review above provides strong evidence that health and health care interventions are a determent of crime. While not the focus of this project, crime also likely affects health. There is increasing recognition that crime, incarceration, and victimization are major public health concerns that disproportionately impact Black, Hispanic, and low-income communities. The Department of Health and Human Services' Healthy People 2030 objectives recognize crime and community violence as social determinants of health under the neighborhood and built environment domain (U.S. Department of Health and Human Services, 2022).

Nationally, there were 3.3 million victims (12 years or older) of violent crime in 2018 (Morgan & Oudekerk, 2019). The same year, the age-adjusted death rate from homicide was 5.9 per 100,000 in the United States (NCHS, 2019). Beyond the immediate morbidity and mortality associated with being a victim of violent criminal offenses, neighborhood violence can have negative consequences even for those who have not been directly impacted. For example, people who have neighborhood safety concerns may be less likely to engage in physical activity and may experience worse mental health outcomes compared with those without community safety concerns (Meyer et al., 2014; Won et al., 2016). A review by Buka et al., 2010 shows that children who witness violence report higher rates of post-traumatic stress disorder (PTSD), depression, and other behavioral consequences (Buka et al., 2010). Findings from a 1994-1996 longitudinal study of adolescent exposures to violence and psychosocial outcomes show that

behavioral consequences from violence exposure can persist beyond immediate exposure (Schwab-Stone et al., 1999).

Researchers conducted a school-based survey to 2,600 adolescents from grades 6, 8 and 10 in an urban public-school setting to explore the moderating effects of age, gender, and race/ethnicity on the relationship between youth violence exposure and behavioral outcomes. Longitudinal analysis revealed that among those in 6th grade at the first wave of the survey, the effects of violence exposure were persistent in externalizing behavioral outcomes like anti-social behaviors and internalizing behaviors (e.g. depressive symptoms) two years after exposure (Schwab-Stone et al., 1999). These findings suggest violence exposure can have differential and lasting effects on adolescents.

In addition to the proximal impacts of crime on health outcomes, there is significant evidence that incarceration, used as a tactic to reduce criminal offending and expanded during the late 1970s, only marginally reduces crime rates (Stemen, 2017), but has lasting negative consequences for impacted communities (Clear, 2008). In the United States, the per capita incarceration rate is 550 per 100,000 residents, higher than any other nation (Kang-Brown et al., 2021). Black and Hispanic populations are overrepresented in the incarcerated population. Raphael and Stoll, (2014) suggest that nationally, nearly 8% of Black men are incarcerated on a given day compared with just 1.1% of non-Hispanic White men (Raphael & Stoll, 2014). High rates of crime and incarceration among Black and Hispanic communities have significant economic and developmental consequences, particularly for children and adolescents. For example, a review by Travis et al., (2014) suggests a parent's incarceration is associated with increased aggressive behaviors, and increased delinquency and risk of arrest among male children. There is also strong evidence of negative effects on educational attainment (Travis et

al., 2014). Educational attainment is a protective factor against criminal justice involvement (Lochner & Moretti, 2004) and a proxy for income, suggesting the incarceration of a parent may have intergenerational poverty effects and a revolving door to the criminal justice system. In 2007, nearly 7% of Black children and 2.4% of Hispanic children under the age of 18 had an incarcerated parent (Glaze & Maruschak, 2010).

Conceptual model

The theoretical perspectives and empirical evidence reviewed above motivates my conceptual model, summarized in Figure 2.2. I suggest that health center availability reduces crime through three primary pathways: 1) Health centers increase access to care; 2) Health centers reduce the financial burden of medical services; and 3) Health centers build social capital as community-based institutions. All three pathways, in turn, have a causal effect on crime.

First, I hypothesize that health centers reduce crime by increasing access to effective health care services, in a manner similar to other access programs, including office-based mental health, Medicaid, and SUD treatment facilities, which have been found to reduce crime (Arenberg et al., 2020; Aslim et al., 2020; Bondurant et al., 2016; Deza et al., 2020, 2021; Jácome, 2020; Vogler, 2017; Wen et al., 2017) primarily through improved access to behavioral health care. This insight is supported by existing evidence and is driven first by Grossmans model of health which predicts that health care utilization is negatively associated with the price services. Use of effective health care services will in turn improves health. Second, the large body of empirical evidence reviewed above suggests that improvements in health will reduce crime.

Second, health center sites may influence reductions in crime through improved financial well-being consistent with existing literature on the Medicaid program. Area and individual level financial well-being is associated with violent (Benson et al., 2003), and property crime (Blumstein & Rosenfeld, 1998). Indeed, evidence from Medicaid suggests health access programs improve financial well-being (Baicker et al., 2013; Hu et al., 2018; Sommers & Oellerich, 2013) and reduce financially motivated crimes (Arenberg et al., 2020; Vogler, 2017).

Finally, health centers might reduce crime in their role as a community-based institutions. Beyond offering access to primary care services, health centers also provide social support to communities (HRSA, 2006). Additionally, FQHCs are required to have a majority patient governing body which is given regulatory authority over scope and availability of services. While there is evidence that contemporary governing boards may have limited decision-making power over services (Wright & Martin, 2014), they still might play an important role in building local relationships with communities. Additionally, the role of patient governance may vary based on the size of health center grantees. Nonetheless, these mechanisms might enable health centers to address broader community development which may influence the decision to criminally offend. This insight is motivated by Hirschi's theory of crime and social control (Hirschi, 2002) and is supported by Sharkey's empirical evidence (Sharkey et al., 2017).

My conceptual model also suggests that ACA Medicaid expansion might moderate the effect of crime. The conceptual expectations are ambiguous. Health centers and Medicaid expansion may act as complements if expansion increases service availability due to coverage under the State Plan. Gaining coverage may also induce increased use of health centers simply due to receiving eligibility (Baicker et al., 2013; A. Finkelstein et al., 2012). And health centers may increase service scope and availability due to increased demand from those newly eligible

for Medicaid (Pourat et al., 2018; Wallace et al., 2016). Health centers and Medicaid expansion

may act as substitutes if they both, independently, increase access to care.

Figure 2.2 summarizes these conceptual expectations, causal pathways and mechanisms

discussed above. The figure also maps how my objectives (aims) relate to the conceptual model I

study.

Figure 2.2: The relationship between health centers, access to primary care, and crime.
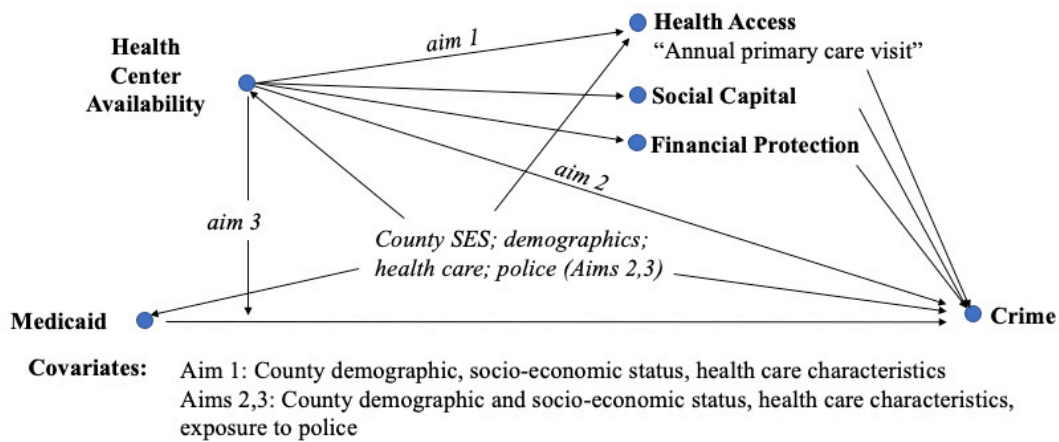
Figure Notes: The perpendicular line (aim 3) indicates a modifying effect of health centers.

Chapter 3. The Effect of Health Centers on Access

In this chapter I examine the effect of health center certification on access to primary care. I leverage the staggered growth of health centers from 2009 to 2020 using difference-in-differences and event study designs.

This is the first study to directly measure the effect of plausibly exogenous variation in health center availability on use of primary care services in any population. As highlighted in Chapter's 1 and 2, a considerable body of literature has examined the relationship between health centers and health care access. For example, McMorrow & Zuckerman, (2014) found that increases in health center funding was associated with increases in the probability of having an office visit or a general doctor visit and a usual source of care in the previous year among low-income adults. However, they examine changes in federal funding for health centers rather than directly assessing health center effects. Bailey and Goodman-Bacon, (2015) examined the effect of health centers on primary care utilization among older adults as a potential mechanism driving reductions in age-adjusted mortality rates. They found health centers increase the probability or reporting a usual source of care among low-income older adults by nearly 23 percent. Further, they examined evidence from the rollout of the first health centers from the 1960s. Health center services have grown considerably in scope over the past fifty years making this an important health policy question in the context of the contemporary program. This chapter contributes to our understanding of the impact of health centers on primary care access.

The analysis presented here estimates the impact of health centers on primary care utilization in the Medicare program. Conceptually, the relationship between health centers and access may be a critical mechanism establishing the link between health centers and crime. However, the Medicare population (adults 65 years and older) are beyond the prime crime age

and 10% of health center patients are covered by Medicare (HRSA, 2019a). Because Medicare patients are insured by definition, not necessarily low-income, and comprise such a small share of total health center users the expected effects of health centers on access among this group are likely a conservative compared with the uninsured or Medicaid populations. Additionally, the literature on health and crime has primarily suggested the link between health access programs and crime is through behavioral health services (see Chapter 2). Due to data limitations, I am unable to examine access to behavioral health or impacts on Medicaid and uninsured populations. This remains and important area for future work.

Although Medicare is not a perfect population to establish this first-stage effect understanding the impact of health centers on access among this group will provide important insights. First, this research addresses a critical gap in our understanding about health centers more broadly. Second, as noted above, results from this analysis will provide a low-end estimate of what might be expected among low-income uninsured or Medicaid populations. This analysis will support the causal interpretation of the relationship between health centers and crime (Chapters 4 and 5).

Data

Data on primary care access came from the Dartmouth Atlas Selected Primary Care Access and Quality Measures (2008-2016). Specifically, I used the percent of Medicare FFS enrollees with an annual ambulatory visit to a primary care clinician to assess the effect of health centers on access to primary care. These county-level rates are derived from Medicare fee-for-service enrollment and claims data among beneficiaries aged 65 years or older. The Dartmouth Atlas Project makes these data publicly available. Estimated rates from small populations or those indicating numbers too small for statistical precision are suppressed from the files. For analysis,

estimated crude and adjusted (adjusted for age, race, and sex) rates were log transformed adding

1 to the outcome prior to transformation. Suppressed county-year observations (n=37) and those

without a full 9-year panel from 2008-2016 (n=147) were removed from analysis leaving 28,107

county-year observations representing 3,123 counties and county-equivalents from each state and

the District of Columbia. These data were then merged with health center openings.

The Centers for Medicare & Medicaid Services (CMS), Provider of Services (POS) File

(2006-2020) was used to identify Rural Health Clinic and Federally Qualified Health Center

openings. The POS file includes data on each Medicare-approved provider including the date the

individual clinic site was first certified by CMS as well as the site-specific address. This

information was used to identify the first clinic opening (RHC or FQHC) at the county level.

Importantly, the dataset does not identify the date a clinic first opened. Rather, what it captures is

the date the clinic was first certified to accept payment from Medicare and Medicaid. Treated

counties are defined as those which received a health center between 2009 and 2016. Counties

that eventually received a health center (between 2017-2020) outside the analytical window act

as controls. Counties that were always treated in the sample (received a clinic prior to 2009) or

those that never received a health center were excluded from the main analysis.

The final analytic sample includes 3,213 county-year observations from 269 treated and

88 control counties. I provide a robustness test which includes 226 never treated counties as

controls in Appendix Section 1.

State and county-by-year covariates were obtained from several sources. These covariates

were chosen based on the theoretical perspectives and empirical evidence laid out in Chapter 2.

State-level ACA Medicaid expansion dates came from the Kaiser Family Foundation (KFF,

2021). A state was considered to have expanded Medicaid if expansion was effective prior to

July of that year. County-by-year median household income, percent of the population living at or below 100 percent of the federal poverty level, and population shares by age (0-19; 20-29; 30-39; 40-49; 50-51), sex, and race/ethnicity (Hispanic; NH-Black; NH-White) came from the U.S. Census Bureau (US Census Bureau, 2022). County-by-year unemployment rates came from the U.S. Bureau of Labor Statistics (US Bureau of Labor Statistics, 2022). Last, I used rural-urban continuum codes from the U.S. Department of Agriculture to identify the metropolitan status of each county as of 2013 (USDA Economic Research Service, 2020).

Design and Estimation

I used difference-in-differences (DD) and event-study designs to examine the effect of health center openings on the share of Medicare fee-for-service enrollees aged 65 years or older with an annual visit to a primary care clinician. My preferred difference-in-differences specification is outlined in Equation 1.

Equation 1: Generalized Difference-in-Differences Specification

$$y_{it} = \beta^{DD} D_{it} + \alpha_i + \alpha_t + x_{it} + e_{it}$$

This approach leverages the uneven county adoption of new health centers. Traditionally, this variation has been used in two-way fixed effects regressions, consistent with Equation 1, and the coefficients of interest, estimators of the average-treatment effect on the treated (ATET), have been interpreted exactly like they would be interpreted in a 2 period DD design in which treatment timing is the same for all treated units. However, an influential literature has emerged which suggests that the TWFE estimator suffers from serious limitations. In this section I

describe recent extensions in methods used for causal inference which enable applied researchers to overcome problems arising from pre-trending outcomes, and/or heterogeneity in treatment adoption and effects across treatment cohorts.

The basic two-group, two-period difference-in-differences (DD) approach has become ubiquitous in health services research as a tool to understand causal effects of public programs and health interventions (Currie et al., 2020). The basic DD setup is calculated as the difference in the treatment group between two time-periods (pre and post treatment implementation) less the difference in an untreated group between the same two time periods. If assumptions hold, the DD estimates the ATET, a causal parameter. These assumptions are generally expressed using the potential outcomes framework (The Rubin Causal Model) in which the causal parameters are defined as differences in the outcome, for the same unit, when they are treated versus when they are not. Because an individual is either treated or not, one outcome is counterfactual, and the outcomes are referred to as potential outcomes because they are not necessarily realized.

The first assumption is SUTVA, the stable unit treatment value assumption. This assumption implies that unit-specific potential outcomes are invariant to the treatment assignment of other units (no spillover). Next, the estimator assumes no treatment anticipation. This can take place, for example, when a large policy reform is publicized and those effected change their behavior in anticipation of the change. The last major assumption is the parallel (or "common") trends assumption which assumes that in the absence of treatment the mean outcome trends in both treated and untreated units would be parallel (Angrist & Pischke, 2008). In other words, this assumption requires that treated and untreated units are not differentially selected on their potential outcome trends (selection bias).

The parallel trends assumption is useful because it offers a less restrictive assumption than cross-sectional comparisons of treated and untreated units -- the parallel trends assumption does not require any specific distribution of potential outcome levels or covariates; it simply requires that the change in the untreated group represents the change that would have occurred in the treated group had treatment not occurred. However, many find the parallel trends assumption implausible if variables (or changes in variables) are unbalanced across treatment status and thus many implement the DD estimator in a regression model that includes covariates (Abadie, 2005). Another way of thinking about the use of covariates is that it replaces the parallel trends assumptions with a conditional parallel trends assumption. The regression of interest is usually a two-way fixed effect regression that includes a fixed effect for the treated group, for the time-period, the interaction of the treatment and post (the DD parameter), and a vector of covariates. Many also include a group specific linear trend when multiple pre and post time points are available. However, this complicates interpretation as the coefficient of interest now represents departures from the common trend rather than changes in levels.

In most policy settings multiple units receive treatment and the timing of the treatment can vary (e.g., ACA Medicaid expansion). In such settings researchers often rely on the same sort of TWFE model, replacing the treatment/comparison fixed effects with group level (e.g., state) fixed effects and the pre/post indicator with time fixed effects in a set up often called generalized difference-in-differences. The model has traditionally been interpreted exactly as the 2x2 canonical case.

However, recent advances in the econometric literature show that in the presence of staggered treatment adoption the TWFE model should not be interpreted in the same way. Goodman-Bacon (2018) shows that the DD estimate from a TWFE design with the presence of

staggered timing is a weighted average of all possible group-time difference-in-differences estimates. For example, in a setting with 3 treatment timing groups (never treated, treated early, treated late), there are 4 2x2 DD's (late vs never, early vs never, early versus late (prior to late's adoption date), and late vs early (after early's adoption date). The TWFE regression aggregates these 2x2 comparisons using weights that are driven by group size *and* treatment variance across group-time cohorts (Goodman-Bacon, 2018b). Variance weighting means that units treated in the middle of the panel receive larger weights, even when they are not a larger share of the population. Indeed, depending on the treatment variance, negative weights can arise that potentially flip the sign of the underlying comparison of interest (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2018b; Sun & Abraham, 2020). Furthermore, heterogeneous treatment effects across time, which is plausible in many health policy contexts due to implementation and public awareness effects, would violate the parallel trends assumption in the 4th 2x2 comparison (late vs early, after early's adoption).

Goodman-Bacon (2018) and de Chaisemartin and D'HaultfEuille (2019) provide tools to decompose these 2x2 comparisons and uncover the TWFE weights. These decompositions provide evidence on whether the TWFE estimator is biased.

When TWFE regressions are not appropriate, several approaches have been developed to implement the DD design. These approaches, differ based on the characteristics of the treatment and identifying assumptions, but share a common underlying approach to the problem—transparently choose comparison groups, rather than allowing TWFE to choose them. Sun and Abraham (2020) developed one approach to overcome these issues in a dynamic setting using an event study framework. They use the last treated cohort as controls, removing outcome data for the dates after cohort receives treatment to estimate cohort specific average treatment effect on

the treated (CATT). First, they estimate a linear fixed-effects model with interactions for each relative time and treatment cohort indicator excluding the last-treated (or never treated) units which serve as controls. Next, they estimate the weights as a share of each cohort per relative time. These weights, unlike those under a standard TWFE design, are non-negative and sum to one. Last, they take a weighted average of the cohort ATTs (Sun & Abraham, 2020). Similarly, the Callaway and Sant'Anna (2020) approach creates cohort specific propensity scores and estimates all individual 2x2 group comparisons to get the same cohort or group time ATTs. However, they differ on their choice of comparison groups by using the not yet treated unlike the last treated used by Sun and Abraham (2020) and they relax the parallel trends assumption by making it conditional on covariates based on their propensity score approach (Callaway & Sant'Anna, 2020). De Chaisemartin and D'Haultfoeuille, (2019) offer another similar approach for instances when treatment can turn on and off and can be non-binary. Their approach requires there to be units with stable treatment levels between each time period comparison and, similar to Sun and Abraham (2020) require a strict parallel trends assumption between groups who change treatment and the stable control cohort (de Chaisemartin & D'Haultfœuille, 2020). Callaway, Goodman-Bacon, and Sant'Anna have developed another approach to overcome the issues with TWFE regression with staggered timing that allows for continuous treatments (expanding Callaway and Sant'Anna, 2020) (Callaway et al., 2021). Others have put forward "stacked" difference-in-differences approaches which take each cohort and construct a control group, appending individual treatment group datasets together to obtain multiple 2x2 comparisons. Thus, this approach overcomes the issues with multiple groups and periods in TWFE regression (Cengiz et al., 2019; Deshpande & Li, 2019). Gardner (2020) retains a regression-based approach (the "two-stage difference-in-differences or 2SDiD") to the staggered

timing problem using a two-stage regression. In the first stage, group and timing or period effects are estimated on untreated observations. Those estimated effects are subtracted from observed outcomes in the second stage and the second stage regression simplifies to the transformed mean outcome on treatment status recovering the overall ATT (Gardner, 2021).

These solutions address the issue that arises from negative weights when there is staggered timing but still require a parallel trends assumption based on treated and not yet or never treated groups. If this assumption cannot be achieved there are alternative identification approaches that relax the parallel trends assumption (beyond covariate balancing) while allowing for dynamic treatment effects and multiple treated units. For example, the synthetic control method (Abadie et al., 2010; Abadie & Gardeazabal, 2003), which overcomes parallel trends by matching treated units with a weighted average of control units to balance outcomes leads and predictors, has been extended for staggered timing with multiple treated units (Ben-Michael et al., 2021).

Empirical Approach

My preferred difference-in-differences specification outlined in Equation 1 represents a generalized approach that leverages the uneven county adoption of new health centers where y_{it} represents log(PCP visit rate +1) in county i and year t. D_{it} is the binary treatment variable indicating the health center opening in county i and year t. β^{DD} is the variance-weighted average of cross-cohort treatment effects. α_i and α_t are county and year fixed effects and x_{it} is a vector of county-by-year covariates (Goodman-Bacon, 2021). Additionally, I present results from an event-study specification that allows us to examine the dynamic path of treatment and visually

inspect for pre-trending. The event-study specification includes relative year and treatment interactions covering six years prior and seven years after the county receives a health center where the tail ends for each relative time are combined ($T \leq -6$; $T \geq 6$) to overcome smaller shares of the sample at each tail. The preferred event-study specification includes the same set of county-year covariates and fixed effects. Both specifications cluster robust standard errors on the county-level.

As a test for pre-trending, I provide results from an F-test which examines whether all pre-period treatment effects are jointly different from zero. As an additional robustness check I supply results from the event-study specification which controls for county-year trends. I also examine whether outcomes differ among key sub-groups of interest including counties with a larger proportion of older adults (50 years or older), those with more concentrated poverty, and counties that are non-metro or metro adjacent. I provide further support for my results by replicating the event-study approach using newer bias-corrected estimators developed by Gardner (2020), Sun and Abraham (2020), and Callaway & Sant'Anna (2020). Last, I provide results which include counties which never received a health center as controls.

Results

Baseline characteristics of treatment (received a health center between 2009 and 2016) and control (received a health center on or after 2017) counties, averaged over the relative pre-period are provided in Table 3.1. I include national estimates from 2010 to gauge whether there are large differences between counties included in our analytic sample. Populations from treatment and control counties appear to be very similar based on average unemployment (7.63%; 6.33%), poverty (14.85%; 14.26%), median household income ($47,375; $47,947), respectively. Medicaid expansion is defined as any expansion as of 2020. Relative to treated

counties, a larger share of those in the control group come from eventual ACA Medicaid

expansion states (72.18% vs 67.49%). Socioeconomic characteristics between sample counties

and the national average appear very similar, overall. In general, a smaller share of US counties

come from eventual ACA Medicaid expansion states compared with sample counties. The

racial/ethnic composition of sample counties is consistent. However, sample counties include

populations with larger shares of non-Hispanic White residents compared with the US average.

Additionally, a considerably larger share of counties from the treatment group (46.26%) are

defined as metropolitan or metropolitan-adjacent compared with controls (35.44%).

Table 3.1: Baseline characteristics of sample counties and the US average

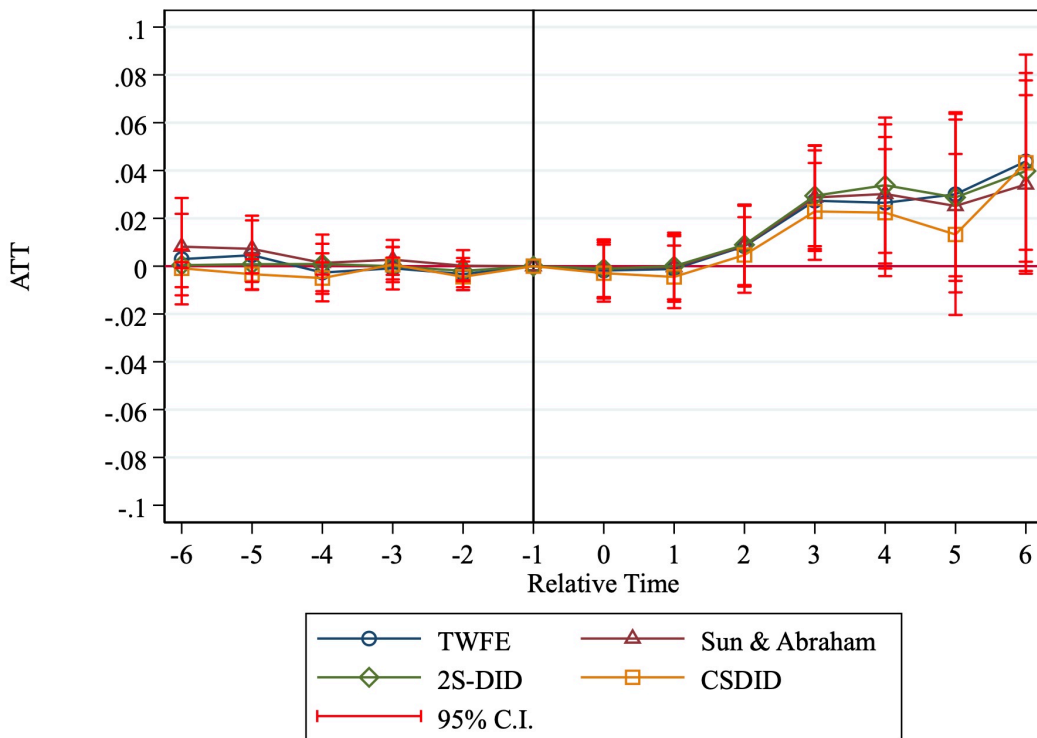|	Treat	Control	US (2010)
Socioeconomic Status			
Unemployment Rate	7.63	6.33	9.34
% Poverty	14.85	14.26	15.30
Median Household Income ($)	47,375	47,947	50,046
% Medicaid Expansion	67.49	72.18	63.84
Demographic Characteristics			
Age			
% 0-19 Years	26.53	25.39	26.10
% 20-29 Years	12.70	11.96	11.92
% 30-39 Years	11.80	11.69	11.58
% 40-49 Years	13.70	13.02	13.59
% 50+	35.26	37.92	36.81
Sex			
% Male	49.79	49.88	49.98
Race/Ethnicity			
% NH-Black	7.35	5.43	9.04
% Hispanic	6.47	5.76	8.33
% NH-White	81.94	85.75	78.38
% Metro Counties	46.26	35.44	37.11

Notes: Counties that received their first health center prior to 2009
or never received a health center were excluded. Estimates are
averaged over the relative pre-period. National Estimates come
from 2010. Analytic sample includes 269 treated counties and 88
controls.

Event study estimates from the preferred specification as well as those coming from newer estimators including Gardner (2020), Callaway & Sant'Anna (2020), and Sun and Abraham (2020) are provided in Figure 3.1. Each specification includes county and year fixed-effects and the full set of state and county-year covariates described above. Counties that received a health center between 2009 and 2016 serve as treated units while control counties include those that received a health center on or after 2017. The analytic window is limited to 2016, the period before control counties received a health center. The red vertical lines denote 95% confidence intervals for the estimates.

The results across all four specifications are similarly near zero in magnitude in the pre-period and suggest no evidence of pre-trending in primary care visit rates prior to the introduction of a health center site. Indeed, the F-test confirms that we cannot reject that the coefficients on all pre-treatment event-time indicators were jointly zero (F=0.94, p=0.46). This suggests that the parallel trends assumption was not violated.

After the introduction of health centers there is a gradual increase in primary care visits. Visits appear to increase 3 years following the certification of health centers across all specifications and grow to roughly a 4.5 percent increase in visits by year 7 (p=0.02). I arrived at this estimate by exponentiating the coefficient (0.044), subtracting 1 from that value, and multiplying by 100.

Figure 3.1. Event-study estimates of the effect of health centers on primary care visits among Medicare fee-for-service enrollees (2008-2016)

Notes: The outcome is the natural log (y+1) of the percent of Medicare fee-for-service enrollees with an annual visit to a primary care clinician. Estimates are the relative period-specific average treatment effect on the treated. Each specification includes county and year fixed effects, and standard error are clustered on the county. The vertical black line denotes the year prior to health center certification. An F-test was used to test whether all pre-period outcomes were jointly zero. F-statistic=0.94, p-value=0.46. **Source**: Dartmouth Atlas county-level primary care measures (2008-2016), Centers for Medicare and Medicaid Services Provider of Services (POS) File (2006-2020).

The static DD coefficients from Equation 1 are provided in Table 3.2. I provide estimates of the effect of health centers on crude and adjusted (adjusted for age, race, and sex) rates of primary care visits among Medicare enrollees. For each outcome, I provide results coming from unadjusted specifications and those fully adjusted for covariates. The coefficients of interest are coming from a log transformed outcome. I exponentiate the coefficients, subtract 1 from that value, and multiply by 100 to transform the DD estimates back into percent change.

The fully specified models suggest health center certification increased primary care visits among Medicare fee-for-service beneficiaries by between 0.12% (p=0.81) and 0.15% (p=0.85) compared with controls over the period. However, neither result is statistically significant. As detailed in the design and estimation section, the TWFE coefficient represents an average of all possible group time DDs and counties that received a health center in the middle of the panel receive larger weights. This bias drives the DD estimate closer to zero (Goodman-Bacon, 2018). As shown by Bailey and Goodman-Bacon, (2015), policy learning and program reforms may be driving heterogenous treatment effects over time. We are confident that our dynamic specification overcomes these challenges and therefore these results do not change our overall interpretation that health centers increase primary care visits among Medicare beneficiaries.

Table 3.2. Static DD estimates of the effect of health centers on primary care visits among Medicare fee-for-service enrollees (2008-2016)

|	Log Crude Rate		Log Adjusted Rate	
Covariates	No	Yes	No	Yes
DD Estimate	0.0005	0.0012	0.0008	0.0015
Standard error	0.0061	0.0061	0.0061	0.0061

Notes: The outcome is the natural log (y+1) of the percent of Medicare fee-for-service enrollees with an annual visit to a primary care clinician. Each specification includes county and year fixed effects, and robust standard errors are clustered on the county. **Source**: Dartmouth Atlas county-level primary care measures (2008-2016), Centers for Medicare and Medicaid Services Provider of Services (POS) File (2006-2020).

Subgroup Results

I provide estimates from the subgroup analyses on non-metro or metro-adjacent counties, counties with higher concentrated poverty, and counties with larger populations of adults greater than 50 in Figure 3.2. For poverty and age subgroups we assigned quartiles based on the

population demographics in 2016 (the last year in our analytic window). The analysis was limited to counties in the highest quartile of poverty and share of adults greater than 50, respectively. For comparison, I graph our preferred TWFE specification (labeled "main") from the main analysis among the full sample of counties. Although the magnitude of the coefficients in the pre-period are larger than the main specification, I come to similar conclusions that the effects in the pre-period are indistinguishable from zero. Consistent with the main results, primary care visits among the high poverty counties increase by roughly 3 percent 4 years following health center opening. However, there is a steeper trend break among counties with more older adults and those designated as non-metro. Among these counties there is a nearly 6 percent increase in visits at year 4, growing to 8.4 percent and 8.4 percent (twice the size of the treatment effect in the full sample) respectively, among older (p=0.02) and non-metro counties (p=0.03) 7 years after the health center opening.

Figure 3.2. Event-study estimates of the effect of health centers primary care visits among Medicare fee-for-service enrollees (2008-2016), by subgroup
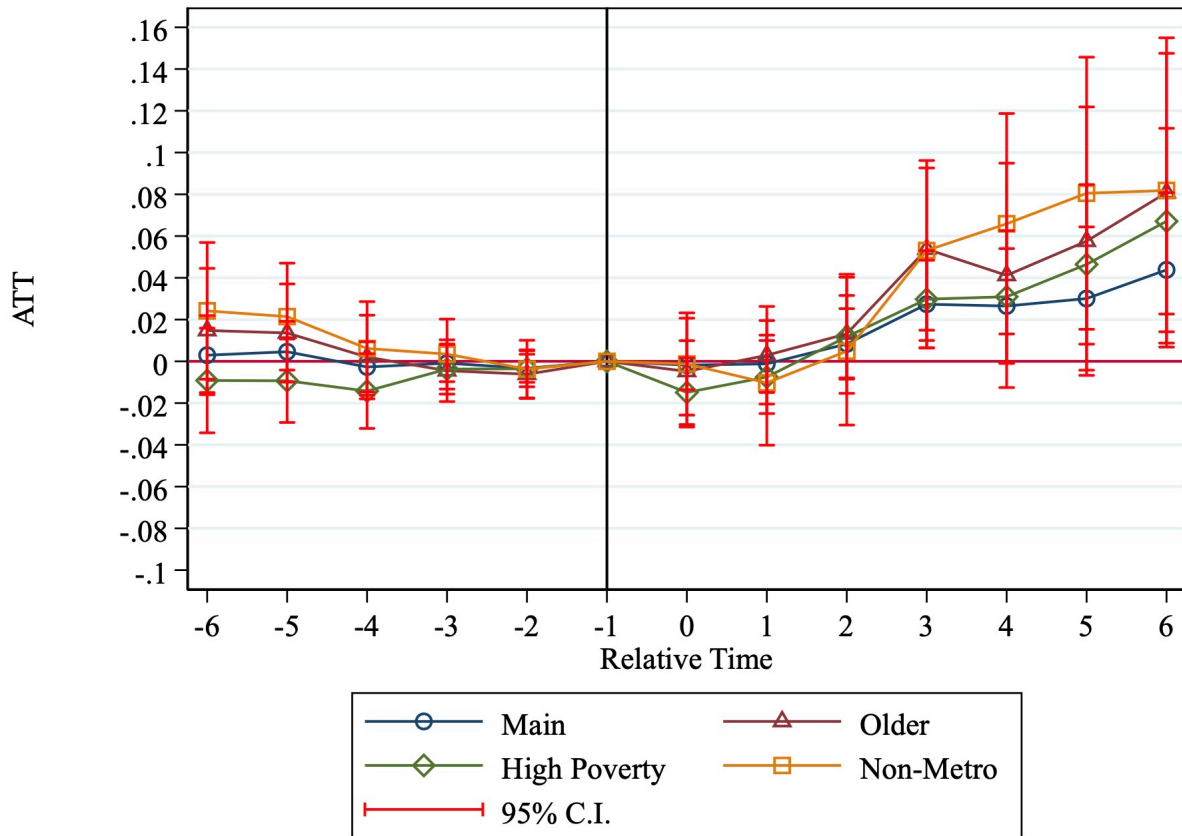
Notes: The outcome is the natural log (y+1) of the percent of Medicare fee-for-service enrollees with an annual visit to a primary care clinician. Estimates are the relative period-specific average treatment effect on the treated. Each specification includes county and year fixed effects, and standard error are clustered on the county. The vertical black line denotes the year prior to health center certification. **Source**: Dartmouth Atlas county-level primary care measures (2008-2016), Centers for Medicare and Medicaid Services Provider of Services (POS) File (2006-2020).

Robustness

I conducted a series of robustness tests. First, using the Sun and Abraham specification, I controlled for county linear trends and come to similar conclusions (Figure A.1). Last, I used counties that never received a health center certification as controls and compared results with

our main analysis. Those results are nearly identical to my preferred specification (Figure A.2). Results from these robustness tests are provided in Appendix Section 1.

Conclusions

In this chapter I show that the introduction of a CMS certified health center was associated with substantial increases in primary care visits among the Medicare population. Results were robust to several alternative specifications and robustness tests. Access to care is one of the primary mechanisms which I hypothesized link health centers to crime in my broader conceptual framework. Indeed, results suggest that primary care visits increase by roughly three percent, growing to 4.5 percent seven years following health center opening.

This study has a few limitations. First, using data which covers Medicare fee-for-service is perhaps less than ideal for my broader dissertation. However, that health centers improve access among a nearly universally insured population has important policy implications on the effectiveness of the program. Older adults are beyond the age where the propensity to commit crime is high and comprise only 10% of the health center patient population. I believe these estimates should be viewed as a lower bound of the expected effects on access among Medicaid and uninsured populations. Next, due to data limitations I am unable to examine other outcomes (like behavioral health) which might further solidify the link between access and crime. Last, due to data availability, most US counties were excluded from analysis because they were treated prior to the start of the panel. Although I show sample counties are largely similar to national averages these results are not generalizable to counties that received a health center before 2009.

My results are consistent with previous work by McMorrow and Zuckerman, (2014) and Shi and Stevens, (2007) who found health centers were associated with increased primary care

visits. Bailey and Goodman-Bacon, (2015) is perhaps more comparable because they examined program effects on a subgroup of low-income older adults. They found health centers increased the probability of reporting a usual source of care by more than 20% and although the point estimate for the total number of visits suggested a 38% increase it was not statistically significant (Bailey & Goodman-Bacon, 2015). Additionally, they were evaluating the earliest cohort of health centers which, due to policy intervention, have changed considerably over time.

This is the first study to directly measure the effect of health center availability on use of primary care services using plausibly exogenous variation from the growth of the program. These results suggest health centers increase primary care visits among even the most highly insured group. These results have important implications for health reforms which seek to improve access to care and suggest continued investments in the health center program may be an effective strategy to address access.

Chapter 4. The Effect of Health Centers on Crime

In this chapter I examine the effect of health center certification on crime. As described in Chapter 1 Section 3, a growing literature has demonstrated that health care access (primarily access to behavioral health) and health have considerable effects on crime (Arenberg et al., 2020; Aslim et al., 2020; Bondurant et al., 2016; Deza et al., 2020, 2021; Fry et al., 2020; Jácome, 2020; Reyes, 2007; Vogler, 2017; Wen et al., 2017). For example, Deza et al., (2020) found that for every 10 additional mental health providers in a county the total crime rate reduced by 0.5% and violent crimes by 2%. Jácome (2020) examined the effect of losing Medicaid eligibility on the probability of future incarceration. Among men with a history of mental health needs, they found that loss of Medicaid increased the probability of incarceration by 14% compared with controls. We know very little about the impact of health centers on social outcomes. However, this body of literature lends credibility to the hypothesis that health centers reduce crime.

Health centers, like mental health providers and Medicaid, serve as points of access to underserved areas. They offer a vast array of services including behavioral health care on a sliding-fee scale or at a reduced cost. In these ways, health centers function in a similar manner to other health access programs. CHC's also might uniquely effect crime a community building and social capital enhancing institutions. This chapter contributes to our understanding of the social impacts of health centers and add to the body of evidence related to health policy and crime.

Data

Crime Rates

Crime data comes from the publicly available FBI Uniform Crime Reports (UCR) Offenses Known and Clearances by Arrest (or "Offenses Known") dataset (2005-2016), a compilation of crime statistics reported to the FBI by law-enforcement agencies (Kaplan, 2021). It is widely used throughout the social sciences to study trends in crime (Blumstein & Rosenfeld, 1998; Chalfin et al., 2020; Rosenfeld & Fornango, 2008).

The Offenses Known file contains quarterly agency-level counts for seven reported crime categories including violent (murder; rape; robbery; aggravated assault) and property crimes (burglary; theft; motor vehicle theft). All fifty states and the District of Columbia are represented in the files which contain information reported from over 18,000 agencies in 2018. Crimes in the file are reported using a hierarchy rule, only the most serious crime for a particular offense is reported. For each agency represented in the file, the dataset contains the primary county of the police agency as well as up to three counties which are covered by the agency. For example, an agency can be responsible for populations residing in up to three counties. For each (up to 3) counties covered by the agency the share of the total population covered by the agency is provided.

Before allocating crimes to the county level, I impute missing data, following FBI suggested procedures. For agencies that reported all 12 months, no imputation was done. For agencies that reported 3-11 months I multiplied the total annual crime by 12/months reported. Last, for agencies that reported fewer than 3 months, I replaced their counts with the annual average of full reporting agencies from the same state and population group (provided in the file). I followed the FBI procedure which uses the last month reported to identify the number of months missing. For example, if an agency reports in December they are classified as reporting

12 months. This approach has obvious limitations, but it is how the FBI and National Archive of Criminal Justice Data (NACJD) construct their publicly-available county files so I used this approach for consistency (National Academies of Sciences, Engineering, and Medicine, 2016).

Following existing literature (Deza et al., 2020), I collapsed imputed agency counts to the county location of the reporting agency to determine the number of crimes (total; violent; property) per 100,000 population covered by the reporting agency. Agencies from special jurisdictions (e.g. university police, port authorities, federal agencies) are excluded. For analysis, estimated crime rates were log transformed adding 1 to the outcome prior to transformation.

Limitations of the UCR

Many have critiqued the UCR. The data represent only crimes reported to police agencies and undercounts actual crime in communities. This issue may be more serious depending on the propensity to report crime from community to community. Its measurement of crime is limited in scope, restricted to a limited set of "index" crimes (described in previous paragraphs) adopted in the early years of the UCR and continued for purposes of a stability in the measure (like poverty) (Kaplan, 2021). Additionally, some agencies may not report consistently from year to year or month to month. The imputation process for non-reporting agencies is flawed because the data does not capture the number of month/years reported but rather the last month the agency reported (National Academies of Sciences, Engineering, and Medicine, 2016). Furthermore, it defines a crime as a known offense and thus reflects racist patterns of policing that put some communities at higher risk of police interaction than others. However, it is the only consistent source of information on crime from police agencies and is collected using the same measures from all 18,000 reporting agencies nationwide (Kaplan, 2021).

To help ascertain how important some of the imperfections in UCR are to my analysis I conduct a robustness test by excluding agencies without 12 full months of reporting then I repeat the analysis at the agency-level. This test helps determine if the imputation of missing reporting months affects my results and allows me to avoid problems that might arise from aggregating to the county.

Health Center Availability

Consistent with the analysis from Chapter 3, I used the Centers for Medicare & Medicaid Services (CMS), Provider of Services (POS) File (2006-2020) to identify Rural Health Clinic and Federally Qualified Health Center openings. The POS file includes data on each Medicare-approved provider including the date the individual clinic site was first certified by CMS as well as the site-specific address. This information was used to identify the first clinic opening (RHC or FQHC) at the county level. Importantly, the dataset does not identify the date a clinic first opened. Rather, what it captures is the date the clinic was first certified to accept payment from Medicare and Medicaid. Treated counties are defined as those which received a health center between 2006 and 2016. Counties that eventually received a health center (between 2017-2020) outside the analytical window act as controls. Counties that were always treated in the sample (received a clinic prior to 2006) or those that never received a health center were excluded from the main analysis. The final analytic sample includes 6,000 county-year observations from 322 treated and 78 control counties.

Covariate Data

State and county-by-year covariates were obtained from several sources. State-level ACA Medicaid expansion dates came from the Kaiser Family Foundation (KFF, 2021). A state was considered to have expanded Medicaid if expansion was effective prior to July of that year.

County-by-year median household income, percent of the population living at or below 100 percent of the federal poverty level, and population shares by age (0-19; 20-29; 30-39; 40-49; 50-51), sex, and race/ethnicity (Hispanic; NH-Black; NH-White) came from the U.S. Census Bureau (US Census Bureau, 2022). County-by-year unemployment rates came from the U.S. Bureau of Labor Statistics (US Bureau of Labor Statistics, 2022). I used rural-urban continuum codes from the U.S. Department of Agriculture to identify the metropolitan status of each county as of 2013 (USDA Economic Research Service, 2020). Last, I used the UCR Law Enforcement Officers Killed and Assaulted (LEOKA) files to count the number of police employed in each county-year (Kaplan, 2021). Officers include any non-civilian employee at the level of the police agency (e.g., state trooper, city cop, county deputy). From these counts I created a measure of the number of police per 100,000 population.

Empirical Approach

The identification strategy is consistent with methods previously described in Chapter 3. My preferred difference-in-differences specification outlined in Equation 1 represents a generalized approach that leverages the uneven county adoption of new health centers where y_{it} represents log(PCP crime rate +1) in county i and year t. D_{it} is the binary treatment variable indicating the health center certification in county i and year t. β^{DD} is the variance-weighted average of cross-cohort treatment effects. α_i and α_t are county and year fixed effects and x_{it} is a vector of county-by-year covariates (Goodman-Bacon, 2021). Additionally, I present results from an event-study specification that allows us to examine the dynamic path of treatment and visually inspect for pre-trending. The event-study specification includes relative year and

treatment interactions covering six years prior and seven years after the county receives a health center where the tail ends for each relative time are combined (T<=-6; T>=6) to overcome smaller shares of the sample at each tail. The preferred event-study specification includes the same set of county-year covariates and fixed effects. Both specifications cluster standard errors on the county-level.

Consistent with Chapter 3, the preferred event-study approach is compared with newer estimators developed by Gardner (2020), Sun and Abraham (2020), and Deshpande and Li, (2019) that is avoids bias from heterogenous treatment effects. I also examine the effect of health center openings on crime rates among counties with a larger proportion of teens (19 years or younger), those with more concentrated poverty, counties that are non-metro or metro adjacent, and among counties with larger proportions of Non-Hispanic White populations. Young adults are more likely to criminally offend (Cornelius et al., 2017).

As a test for pre-trending, I provide results from an F-test which examines whether all pre-period treatment effects are jointly statistically different from zero. I also repeat the main analysis using an augmented synthetic control method which reduces the amount of bias that results from averaging period-specific treatment effects from multiple synthetic controls in cases with staggered treatment adoption (Ben-Michael et al., 2021). The advantage of the SCM estimates is that because the control group is matched on pre-treatment outcome levels and trends there is no threat from differential pre-treatment trends (although co-occurring shocks would still confound the estimates of interest). I provide more detail about the SCM in the results section where I also outline the SCM estimates.

Baseline characteristics of treated (received a health center between 2006 and 2016) and control (received a health center on or after 2017) counties are provided in Table 4.1, averaged over the relative pre-period. This table is different from Chapter 3 Table 3.1 because these are a slightly different set of sample counties. National estimates from 2010 are included to examine the populations of sample counties to the national average based on socioeconomic and demographic characteristics. Overall, there are few differences between treatment and control populations at baseline. They appear to be a close match on unemployment, poverty, median household income, age distributions, and racial/ethnic composition. However, a larger share of control counties come from states that eventually expanded Medicaid under the Affordable Care Act (72.18% vs 67.49%). Additionally, control counties have, on average, fewer police per 100,000 population (178 vs 202). However, compared with the national average, sample counties have lower average unemployment, include populations from counties with much smaller shares of non-Hispanic Black and Hispanic residents, and have fewer police officers per 100,000 residents. Some of this difference is likely due to including rural health centers in the health center definition.

Table 4.1: Baseline characteristics of sample counties and the US average

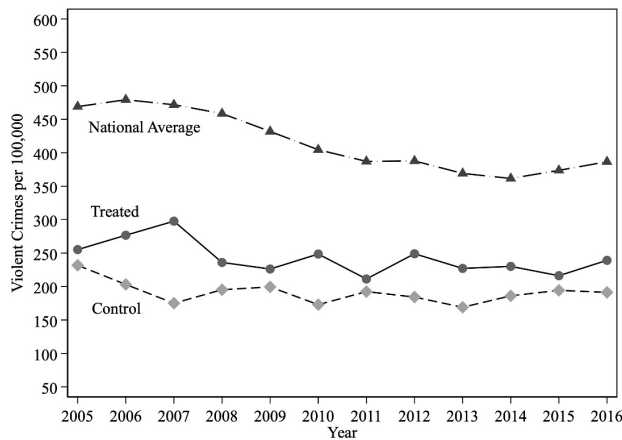|	Treatment	Control	US
Socioeconomic status			
Unemployment Rate	6.22	5.81	9.34
% Poverty	13.88	13.90	15.30
Median Household Income ($)	46,716	46,886	50,046
% Medicaid Expansion	67.49	72.18	63.84
Demographic characteristics			
Age			
% 0-19 Years	26.72	25.71	26.10
% 20-29 Years	12.69	12.06	11.92
% 30-39 Years	12.08	11.80	11.58
% 40-49 Years	14.32	13.45	13.59
Sex			
% Male	49.64	49.70	49.20
Race/Ethnicity			
% NH-Black	7.12	5.54	14.60
% Hispanic	6.45	5.78	16.30
% NH-White	82.88	85.61	76.20
Law enforcement			
Police Officers (per 100,000)	202	178	242

Notes: Counties that received their first health center prior to 2006 or never received a health center were excluded. Estimates are averaged over the relative pre-period. National estimates come from 2010. Analytic sample includes 322 treated and 78 control counties.

I provide trends in violent crime and property crime by treatment status between 2005 and 2016 in Figure 4.1. National trends in crime are provided for comparison. At the national level, violent crime (Panel A) is largely decreasing from 469 violent crimes per 100,000 in 2005 to 362 per 100,000 by 2014. However, there is a small increasing trend beginning in 2015 and violent crimes grow to 387 per 100,000 by 2016. Sample counties experience far less violent crime over this period with 239 and 191 violent crimes per 100,000 in treated and control counties in 2016, respectively. Additionally, between 2005 and 2007 violent crimes in treated

counties increase from 255 to 298 per capita while violent crimes decline from 232 to 175 per capita in controls. Trends in violent crimes flatten between 2008 to 2016 among both treated and control counties. Trends in property crime are provided in Figure 1 Panel B. Nationally, between 2005 and 2016 the United States experienced consistent declines in property crime from more than 3,400 crimes per 100,000 in 2005 to roughly 2,450 by 2016. Unlike the violent crime index, property crimes have not broken the downward trajectory. While there is more noise in the estimated property crimes for sample counties, both treated and control counties experienced a similar decline during this period. Consistent with violent crime, on average, sample counties experienced far fewer property crimes than the national average.

Figure 4.1. Trends in annual violent and property crime by treatment status, 2005-2016

**Panel A:** Violent Crime   **Panel B:** Property Crime
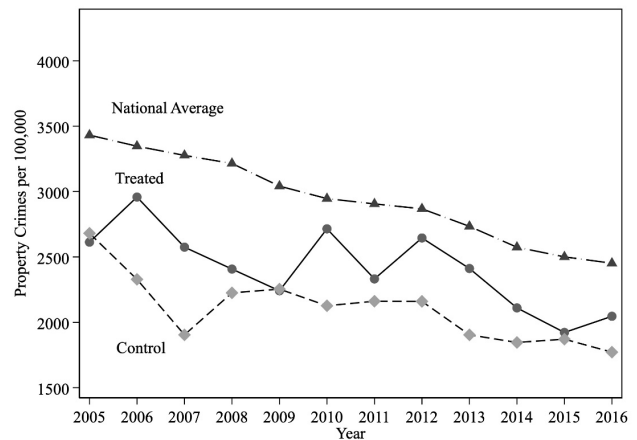
Source: National estimates come from the US Department of Justice (U.S. Department of Justice, 2020). Data for treated and control trends come from the UCR Offenses Known and Clearances by Arrest (2005-2016) files after merging with CMS Provider of Services health center certification dates.

Event study estimates from the preferred TWFE specification for total crime are provided in Figure 4.2. The vertical line at T-1 represents the year prior to health center opening. The estimated treatment effects of health center opening on treated counties in the pre-period are very

small in magnitude and confidence intervals are narrow suggesting no evidence of differential effects on log total crime in the pre-period. Furthermore, results from the F-test suggest that we cannot reject the null hypothesis that all pre-period effects on total crime are equal to zero (F-stat=0.45, p=0.82). This lends support to the parallel trends assumption.

There is a clear trend break with crimes decreasing by roughly 10.3% (p =0.02)[1] within two years after health center certification. Additionally, the effect grows to a 18.8% reduction in total crime (p=0.01) within seven years compared with controls.

Figure 4.2. Event Study: Effect of health center certification on log total crime
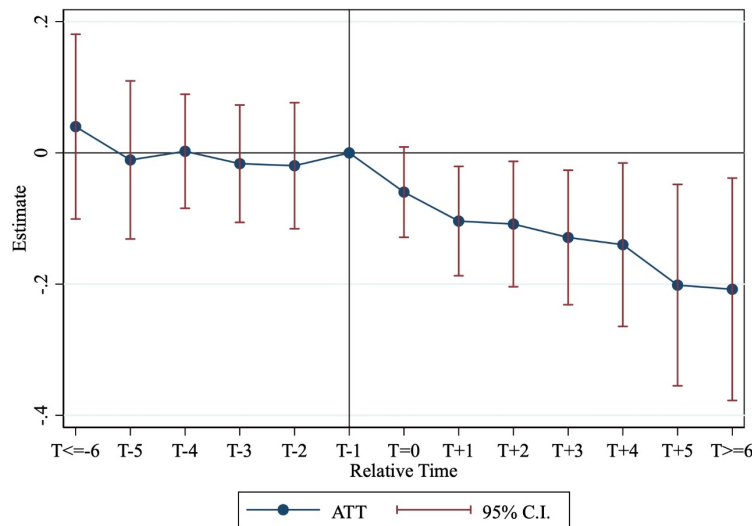
Figure Notes: Data come from the UCR Offenses Known and Clearances by Arrest (2005-2016). The event study specification is fully adjusted for state and county-year covariates and robust standard errors are clustered on the county (n=400). The vertical line at T-1 reflects the year prior to health center opening.

As discussed in Chapter 3, a large literature has described potential bias in TWFE coefficients. In this analysis, this would result in estimates biased toward zero, underestimating the true effect of health centers on crime. This would occur if the effects of health centers are

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[1] Percent change calculations are done by exponentiating the DD coefficient, subtracting 1, and multiplying that value by 100.

increasing in relative time such that the OLS estimator incorrectly subtracted out treatment effect in the late vs early 2x2 comparison.

In Figure 4.3 I provide additional results from several estimators which overcome bias from treatment effect heterogeneity (Deshpande & Li, 2019; Gardner, 2021; Sun & Abraham, 2020). Superimposing results from the main event-study specification I find little evidence that estimates are imprecise. Results from all three additional specifications are nearly identical and provide support for the main results. Further, the results coming from the stacked regression specification are limited to counties that received health centers between 2008-2015. I limited the estimation range to 3 periods before and 3 periods after treatment to balance by event-time. Therefore, we can also conclude that treatment effects do not appear to be driven by county compositional changes between treatment year cohorts.

Figure 4.3. Comparison with newer DiD estimators

Figure Notes: Data come from the UCR Offenses Known and Clearances by Arrest (2005-2016). The event study specification is fully adjusted for state and county-year covariates and robust standard errors are clustered on the county (n=400). The vertical line reflects the year prior to health center opening.
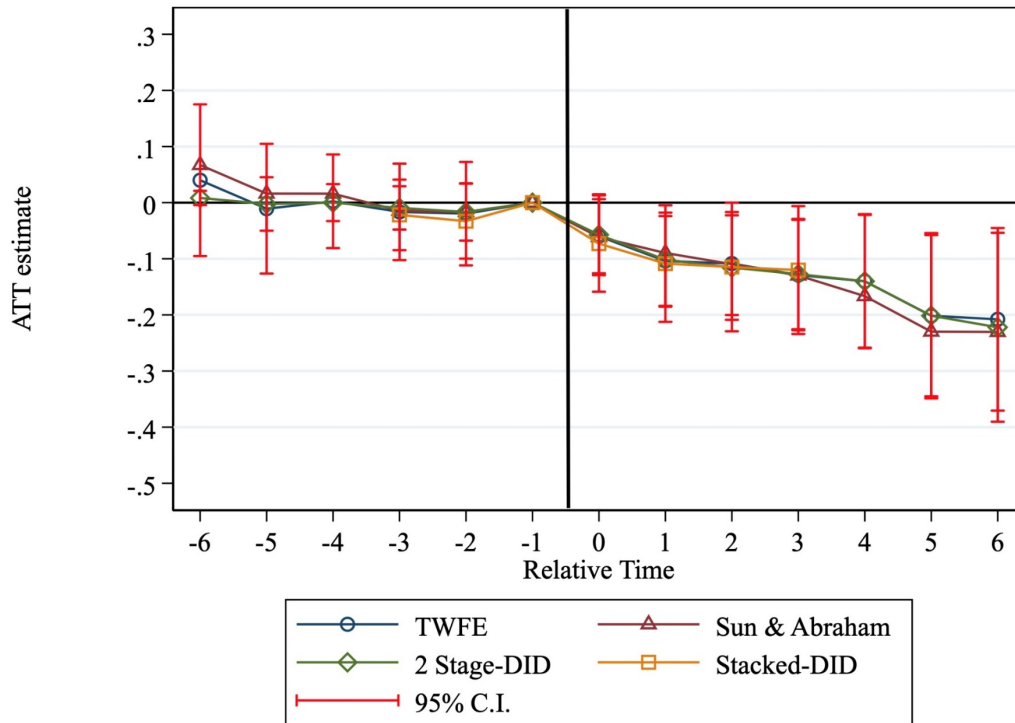
DD coefficients (Equation 1) of the overall effect of health centers on log total, violent, and property crime are provided in Table 4.2. For each of the three outcomes coefficients are provided from models with and without adjusting for covariates. All specifications include county and year fixed effects and standard errors clustered on the county level. Overall, coefficients for each of the three outcomes are similar in magnitude. Health centers have a statistically significant reduction on total crime (p=0.02) and property crime (p=0.02), suggesting a 7 percent reduction in total crime and a 7.3 percent reduction in property crime over the period relative to controls. Although the DD coefficients for violent crime are similar in magnitude, they are less precise (p=0.09).

Table 4.2. Effect of health centers on crime outcomes, overall TWFE

	Total Crime		Violent Crime		Property Crime	
Covariates	No	Yes	No	Yes	No	Yes
DD Estimate	-0.076**	-0.073**	-0.078*	-0.069*	-0.080**	-0.076**
Standard error	0.04	0.03	0.04	0.04	0.04	0.05

Source: FBI Uniform Crime Reports 2005-2016. **Notes:** *, **, and ***
indicate significance at p<0.10; p<0.05; p<0.01 levels. All specifications
include county and year fixed-effects. Clusters=400, Obs=4,800.

While I failed to reject that the coefficients on all pre-treatment event-time indicators were jointly zero, testing of pre-treatment trends can suffer from lack of statistical power and it is possible that my results are driven by patterns in crime rates that pre-date the introduction of health centers. In Figure 4.4, I provide results from an additional analysis using the augmented synthetic control method to examine the effect of health centers on the log total crime rate. Consistent with the traditional cohort-style SCM, this augmented synthetic control constructs a synthetic control unit for each treated unit based on pre-period crime rates and covariates. Therefore, there is no pre-trending because the synthetic control is weighted to reflect the treated unit's pre-period outcome levels and trends. The augmented SCM uses a combined method for constructing weights which overcomes bias resulting from taking the average of each treatment effect for multiple treated units using the traditional SCM (Ben-Michael et al., 2021). Results from the SCM are broadly consistent with the main event-study specification. One important difference is that the estimated treatment effects are not grouped at the tail ends of the distribution as was done with our main TWFE specification. Therefore, effects in periods beyond 6 years after the introduction of a health center comprise of a much smaller share of counties which may explain much larger effect sizes (approaching a reduction of 36% in total crime) 8 or more years after treatment.

Figure 4.4. The effect of health centers on log total crime, SCM results
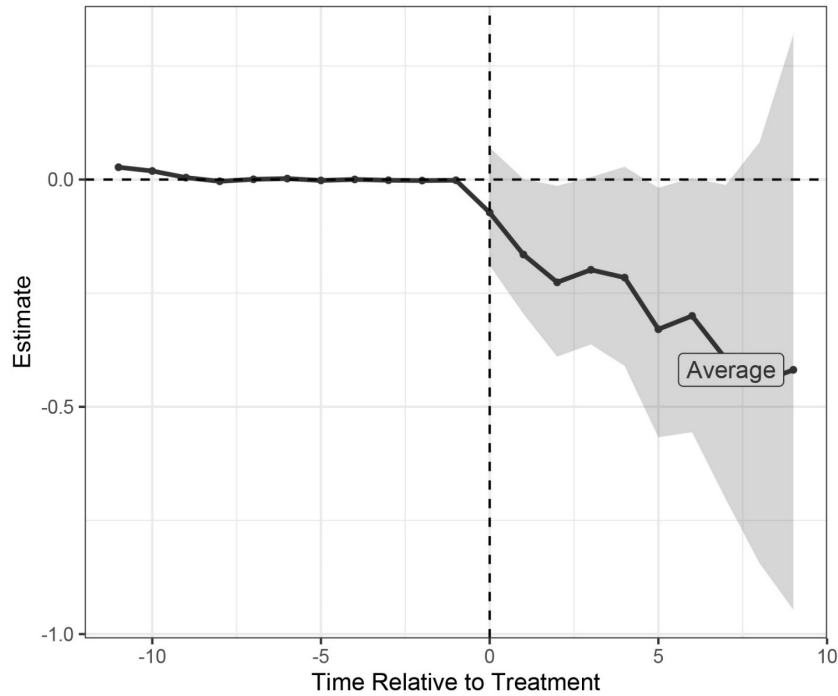
Figure Notes: Data come from the UCR Offenses Known and Clearances by Arrest (2005-2016). The SCM specification is fully adjusted for state and county-year covariates. The vertical line (t=0) reflects the first year a health center opening occurred.

Sub-Group Results

In a subgroup analysis, Table 4.3, I provide evidence that effects on total crime are larger among non-metro counties (estimate: -0.12; p=0.07), counties with higher concentrations of poverty (estimate: -0.08; p=0.08), those with a larger proportion of teens (estimate: -0.10; p=0.04), and counties with predominantly non-Hispanic White populations (estimate: -0.13; p=0.02).

Table 4.3: Effect of health centers on log total crime, by subgroup

	Full Sample	**Non-Metro**	**Poverty**	**Teens**	**White**
DD Estimate	-0.073**	-0.115*	-0.080*	-0.101**	-0.127**
Standard error	0.03	0.06	0.04	0.05	0.05

Source: FBI Uniform Crime Reports 2005-2016. **Notes:** *, **, and *** indicate significance at p<0.10; p<0.05; p<0.01 levels. All specifications include county and year fixed-effects and adjust for county-year covariates.

Sensitivity of Results to Alternative Approaches to the UCR

Finally, like many administrative records, the UCR data are imperfect and suffer from missing data. Methods to aggregate crime counts from agency to the county level can induce bias in the results (Kaplan, 2021). For example, allocating crimes from agency to county based on the share of the agency population covered in each county may underestimate or overestimate crimes. This would occur if an agency's crimes primarily took place in a single county. Next, the imputation procedure may not correctly identify the amount of missing data and therefore create estimates over-reflecting or under-reflecting true crimes at the agency-level. Due to these known limitations, I completed an additional analysis restricting the agency-level crime files to only those agencies which reported all 12 months of the year. I merged this agency level file to county-level health center data creating an agency-by-year file with treatment variation at the county. For estimation, standard errors were clustered at the agency level. In Appendix 2, Figure A.3, I provide the event-study specification from this approach. Results from this analysis lead to similar conclusions although coefficients are slightly smaller in magnitude.

Conclusions

In this chapter I provided the first evidence that health centers are associated with reductions in crime. The results demonstrate that health centers are associated with reductions of 7% and 7.3% for total and property crimes, respectively, over the period relative to controls. These findings were robust to several alternative specifications, and analyses at agency level.

This study has a few limitations. First, there are known limitations to the quality of the Uniform Crime Reporting data (Maltz & Targonski, 2002). I followed best practices by supplementing my main analysis with additional results coming from agencies with complete reporting and conducted at the agency level. Those results provide evidence that our main analysis is not driven by limitations from imputation and allocation procedures. Second, while this chapter provides new information about the effects of health centers on crime, I do not examine effects using a continuous treatment design. There may be important heterogeneity based on the number of health centers in a county year and those results would be an important addition to the work that I have presented. Additionally, this approach would limit allow for inclusion of previously treated counties because it would examine county-year changes in the number of clinics and as such would be more generalizable to a national setting.

Treatment effects from this analysis were larger, in general, than those observed in the Medicaid papers (Arenberg et al., 2020; Aslim et al., 2020; Fry et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017). For example, Vogler et al., (2017) examined the effect of Medicaid expansion on crime and found expansion was associated with a 3% reduction in total crime. Similarly, Deza et al., (2020) found 10 additional office-based mental health providers reduced total crime by 0.5% and violent crime by 2%. The static TWFE results from our agency-level analysis do suggest more modestly sized reductions (with less precision) of 2.4% for total

crime (p=0.16), 1.9% for violent crime (p=0.58), and 3.3% for property crime (p=0.08) over the period between 2005 and 2016.

These results have important policy implications. First, this study adds to the growing literature on access to care and suggests increased access via health centers can translate into improvements in social outcomes beyond health. This is important information for policy approaches seeking to address the social determinants of health and for broader reforms aimed at health reforms. Next, these findings, coupled with those from other similar programs, demonstrate that health interventions can meaningfully influence crime. Policymakers seeking to address crime should consider health interventions alongside other reforms to the criminal justice system.

Chapter 5. The Interaction Effects of Health Centers and Medicaid Expansion on Crime

Overview

Chapter 3 examined the effect of health center openings on access to primary care. Results suggested a modest increase in primary care visits among older adults 3-7 years after health center opening. In Chapter 4 I assessed the effect of health centers on crime and found suggestive evidence that health centers reduce crime beginning the year after health center opening. The approaches for both analyses included ACA Medicaid expansion as a covariate. In this Chapter I examine the interaction effects of health center openings and ACA Medicaid expansion on crime, extending analyses from Chapter 4.

Several studies have reported strong evidence that Medicaid reduces crime, primarily through access to behavioral health services (Arenberg et al., 2020; Aslim et al., 2020; Fry et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017). Conceptually, it seems natural to imagine that effects on crime may be stronger in areas that experience both treatments. Medicaid expansion creates an entirely new eligibility group made up of adults who may still be near peak age when the propensity to commit crime is high (Cornelius et al., 2017) and health centers enhance access for people on Medicaid. Last, Medicaid coverage may enhance access to additional services among health center patients. This chapter will contribute to our understanding of the intersection between public programs that seek to improve access to care. Additionally, it may shed more light on the relationship between Medicaid and crime.

Data

Crime data comes from the publicly available FBI Uniform Crime Reports (UCR) Offenses Known and Clearances by Arrest (or "Offenses Known") dataset (2005-2016), a

compilation of crime statistics reported to the FBI by law-enforcement agencies (Kaplan, 2021). It is widely used throughout the social sciences to study trends in crime (Blumstein & Rosenfeld, 1998; Chalfin et al., 2020; Rosenfeld & Fornango, 2008).

The Offenses Known file contains quarterly agency-level counts for seven reported crime categories including violent (murder; rape; robbery; aggravated assault) and property crimes (burglary; theft; motor vehicle theft). All fifty states and the District of Columbia are represented in the files which contain information reported from over 18,000 agencies in 2018. Crimes in the file are reported using a hierarchy rule, only the most serious crime for a particular offense is reported. For each agency represented in the file, the dataset contains the primary county of the police agency as well as up to three counties which are covered by the agency. For example, an agency can be responsible for populations residing in up to three agencies. For each (up to 3) county covered by the agency the share of the total population covered by the agency is provided.

Before allocating crimes to the county level, I followed a procedure to "impute" missing data. For agencies that reported all 12 months, no imputation was done. For agencies that reported 3-11 months I multiplied the total annual crime by 12/months reported. Last, for agencies that reported fewer than 3 months, I replaced their counts with the annual average of full reporting agencies from the same state and population group (provided in the file). I followed the FBI procedure which uses the last month reported to identify the number of months missing. For example, if an agency reports in December they are classified as reporting 12 months. This approach has obvious limitations, but it is how the FBI and National Archive of Criminal Justice Data (NACJD) construct their publicly-available county files so I used this approach for consistency (National Academies of Sciences, Engineering, and Medicine, 2016).

Following existing literature (Deza et al., 2020), I collapsed imputed agency counts to the county location of the reporting agency to determine the number of crimes (total; violent; property) per 100,000 population covered by the reporting agency. Agencies from special jurisdictions (e.g. university police, port authorities, federal agencies) are excluded. For analysis, estimated crime rates were log transformed adding 1 to the outcome prior to transformation.

The UCR has a few important limitations due to the imputation procedure as well as allocating counts from agency to county levels. I direct the reader to Chapter 4 for a robust explanation of these considerations.

Consistent with the analysis from Chapters 3 and 4, I used the Centers for Medicare & Medicaid Services (CMS), Provider of Services (POS) File (2006-2020) to identify Rural Health Clinic and Federally Qualified Health Center openings. The POS file includes data on each Medicare-approved provider including the date the individual clinic site was first certified by CMS as well as the site-specific address. This information was used to identify the first clinic opening (RHC or FQHC) at the county level. Importantly, the dataset does not identify the date a clinic first opened. Rather, what it captures is the date the clinic was first certified to accept payment from Medicare and Medicaid. Treated counties are defined as those which received a health center between 2006 and 2016. Counties that eventually received a health center (between 2017-2020) outside the analytical window act as controls. Counties that were always treated in the sample (received a clinic prior to 2006) or those that never received a health center were excluded from the main analysis. The final analytic sample includes 6,000 county-year observations from 322 treated and 78 control counties.

State and county-by-year covariates were obtained from several sources. State-level ACA Medicaid expansion dates came from the Kaiser Family Foundation (KFF, 2021). A state was

considered to have expanded Medicaid if expansion was effective prior to July of that year. Several states (including the District of Columbia) took advantage of policy options to expand Medicaid coverage for adults prior to the formal ACA expansion in 2014 (KFF, 2012). These early expanders included California (2011), Connecticut (2010), Colorado (2012), the District of Columbia (2011), Minnesota (2010), Missouri (2012), New Jersey (2011), and Washington (2011). I used the Medicaid expansion dates to create a binary variable set to 1 if the year of the panel was greater than or equal to the Medicaid expansion date and 0 otherwise. Importantly, never expansion adopters were not excluded from analysis because our framework relies on the identification of health centers. Counties from states that never expanded Medicaid during the analytic window are always coded as never adopters.

County-by-year median household income, percent of the population living at or below 100 percent of the federal poverty level, and population shares by age (0-19; 20-29; 30-39; 40-49; 50-51), sex, and race/ethnicity (Hispanic; NH-Black; NH-White) came from the U.S. Census Bureau (US Census Bureau, 2022). County-by-year unemployment rates came from the U.S. Bureau of Labor Statistics (US Bureau of Labor Statistics, 2022). I used rural-urban continuum codes from the U.S. Department of Agriculture to identify the metropolitan status of each county as of 2013 (USDA Economic Research Service, 2020). Last, I used the UCR Law Enforcement Officers Killed and Assaulted (LEOKA) files to count the number of police employed in each county-year (Kaplan, 2021). From these counts I estimated the number of police per 100,000 population.

Empirical Approach

Using the same sample counties from Chapter 4, I estimated the effect of experiencing both

Medicaid expansion and a health center opening using a similar generalized difference-in-

differences specification. However, I included time-varying treatment indicators equal to 1 in the

year the county experienced either health center certification, Medicaid expansion, or both

(indicated by the interaction term β_1 in equation 2).

Equation 2:
$$y_{it} = \beta_1 Opening_{it} \times Expansion_{it} + \beta_2 Opening_{it} + \beta_3 Expansion_{it} + \alpha_i + \alpha_t + x_{it} + e_{it}$$

An important caveat to this approach is that the Medicaid expansion effect is constrained

to occur in 2014-2016 due to the available policy variation. Therefore, Medicaid expansion is

limited to three post period years while the effect on health center availability, even among

counties that received both treatments, could occur between 2006 and 2013. If treatment effects

are delayed or grow over time this may underestimate the true effect of Medicaid expansion on

crime. Additionally, treatment effects for health centers on crime may be considerable in years

overlapping Medicaid expansion so even if Medicaid, itself, has no effect, it may appear as if the

programs act as complements.

I also examine the effect of Medicaid expansion and health center opening on log total

crime among counties that are non-metro or metro adjacent, those with more concentrated

poverty, counties where larger shares of the population are at the peak age for the propensity to

commit crime (less than or equal to 19 years), and among counties with larger proportions of

Non-Hispanic White populations.

Results

For brevity, I do not repeat a descriptive statistics table because the sample is the same as

that from Chapter 4 (I refer the reader to Table 4.1 of Chapter 4). Estimates from equation 2 are

provided in Table 5.1. Consistent with previous analyses, robust standard errors are clustered on the county and each specification includes county and year fixed effects. I provide estimates coming from a simple specification and a specification fully adjusted for state and county-year covariates[2]. Crime outcomes are the natural log of the crime rate + 1.

The results from the fully specified model suggest that health centers reduce total crime by 5.4% (p=0.06) over the study period in the absence of Medicaid. The coefficient on Medicaid, which measures the Medicaid expansion effect in counties without a health center, are positive and imprecisely estimated.

The coefficient on the interaction term is negative, of appreciable magnitude (suggesting a reduction in total crime of 10.1%), but imprecise (p=0.09) and therefore I cannot draw firm conclusions of a modifying effect of both treatments on total crime.

I find suggestive evidence that receiving a health center and Medicaid expansion is associated with a 15.4% reduction in violent crimes (p=0.04) relative to counties that received neither treatment.

Last, health centers reduce property crime by 5.7% (p=0.06) in the absence of Medicaid expansion. The coefficient for the interaction term (-.11) is nearly double that of health centers, alone, however not statistically significant after adjusting for covariates (p=0.07). Finding no effect of Medicaid after removing the treatment effects from health center openings, in addition to a larger beta estimate among dual treatments does lend support that health centers act as a modifier on the Medicaid crime pathway.

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[2] Percent change calculations are done by exponentiating the DD coefficient, subtracting 1, and multiplying that value by 100.

Table 5.1. Difference-in-differences estimates of the effect of health centers and Medicaid expansion on crime

	Total Crime		Violent Crime		Property Crime	
Covariates	No	Yes	No	Yes	No	Yes
Health Center	-0.059**(0.03)	-0.056*(0.03)	-0.053(0.04)	-0.044(0.04)	-0.062**(0.03)	-0.059*(0.03)
Medicaid	0.019(0.05)	0.021(0.05)	0.078(0.08)	0.042(0.07)	0.029(0.04)	0.033(0.06)
Medicaid*Health Center	-0.111*(0.06)	-0.107*(0.06)	-0.167**(0.08)	-0.166**(0.08)	-0.120**(0.06)	-0.114*(0.06)

Source: FBI Uniform Crime Reports 2005-2016. **Notes:** *, **, and *** indicate significance at p<0.10; p<0.05; p<0.01 levels. All specifications include county and year fixed-effects. Clusters=400, Obs=4,800. (Standard errors in parentheses).

Results from subgroup analyses are provided in Table 5.2. For brevity, I report only the coefficients that come from the interaction term from Equation 2. Each specification is fully adjusted for state and county year covariates and includes county and year fixed effects. Robust standard errors are clustered on the county. I provide estimates from the full sample, among non-metro or metro-adjacent counties, counties with higher concentrations of poverty, counties with larger populations of persons 19 years or younger, and those with larger shares of non-Hispanic White residents.

Among non-metro counties, receiving both, a health center and Medicaid expansion is associated with a 24.5% reduction in total crime (p=0.01). This is estimate is considerably larger (more than double) what was estimated as the effect of health centers on crime from Chapter 4 Table 4.3. However, we cannot rule out heterogenous treatment effects of health centers on crime. The larger effect observed here may also reflect scarce health care resources or higher intensity of Medicaid enrollment, following expansion, in rural areas (Foutz & Garfield, 2017). Results among counties with larger shares of children and teens receiving both treatments suggest a 13.6% decline in total crime (p=0.11). Although children and young adults did not directly benefit from coverage under Medicaid expansion there may have been spillover effects due to parents gaining coverage (Venkataramani et al., 2017). Young adults are at a prime age to

commit crime (Cornelius et al., 2017) so it seems reasonable that improvements in access among higher concentrations of teens would result in larger effects. Finally, among counties with larger population shares of non-Hispanic whites, receiving both treatments is associated with a 13.5% reduction in total crime compared with controls (p=0.09).

Table 5.2. Subgroup analysis examining the effect of health centers and Medicaid expansion on total crime

	Full Sample	Non-Metro	Poverty	Teens	White
Medicaid*Health Center	-0.107*	-0.282***	-0.059	-0.146*	-0.145*
Standard error	0.06	0.11	0.08	0.09	0.09

Source: FBI Uniform Crime Reports 2005-2016. **Notes:** *, **, and *** indicate significance at p<0.10; p<0.05; p<0.01 levels. All specifications include county and year fixed-effects and adjust for county-year covariates.

Conclusions

In this chapter I examined the effect of experiencing both Medicaid expansion and a health center opening on crime outcomes. After including the Medicaid and health center interactions in my analysis results suggested that in the absence of Medicaid, health centers reduce total crime by 5.4% and property crime by 5.7%. The interacted coefficients suggested a compounding treatment effect of Medicaid expansion and health centers. Although estimates were precise for violent crime, and we cannot come to firm conclusions about the causal interaction between these two programs because it's possible that heterogeneous treatment effects of health centers is driving the result.

Results from the subgroup analysis suggested that receiving Medicaid and a health center was associated with a 24.5% reduction in total crime among non-metro counties and a 13.6%

reduction among counties with larger populations of children and teens (less than 20 years old). However, our inability to disentangle the possibility of heterogenous treatment effects from health centers limits the interpretation of these results.

Nevertheless, results are aligned with my hypothesis that health centers and Medicaid act as complements. To my knowledge, this is the first study to explore this relationship so there is not a literature to compare it with. However, it has important implications for the existing literature on Medicaid and crime.

This study has a few important limitations. First, consistent with Chapter 4, there are known limitations to the quality of the Uniform Crime Reporting data (Maltz & Targonski, 2002). I supplemented results from Chapter 4 with agency-level estimates limiting to agencies with 12 months of reporting to overcome the challenges with imputation and allocation procedures. Second, my approach does not permit be to determine the degree to which the effects observed for Medicaid and health center interactions are driven by health center treatment effect heterogeneity. This is an important limitation that requires caution when interpreting these results.

Results from this chapter highlight the need to further examine the intersection of public programs aimed at improving access to care. Better understanding these relationships will enable more efficient approaches to address outcomes.

Chapter 6. Implications and Conclusions

In this chapter I summarize results from Chapters 3-5 and put results in context with my greater conceptual framework. Additionally, I discuss the policy implications and future directions for this work.

Summary of findings

In Chapter 3, I examined the effect of health center opening on annual visits to a primary care clinician among Medicare fee-for-service beneficiaries (aged 65 years or older) between 2009 and 2016. Access to care is one of the primary mechanisms which I hypothesized link health centers to crime. Indeed, results suggest that primary care visits increase by roughly three percent, growing to a 4.5 percent increase between three- and seven-years following health center opening. These results are aligned with previous studies which have found health centers and health center funding is associated with increased primary care utilization (McMorrow & Zuckerman, 2014; Shi & Stevens, 2007). Bailey and Goodman-Bacon, (2015) found the earliest version of the health center program was associated with a 23% increase in reporting a usual source of care among a similar population of older adults (Bailey & Goodman-Bacon, 2015). Importantly, results from Chapter 3 suggest effects on primary care visits are delayed to three years following health center opening. Delayed effects on primary care visits may be caused by a gap between the time a clinic is certified and when it begins to see patients, by the time required to scale up patient outreach efforts and for newly established providers to attract patients.

Chapter 4 examined the effect of health center openings on county-level total, violent, and property crime. Results suggest that health centers reduced total crime by 7% and property crime by 7.3% relative to controls over the period between 2005 and 2016. These findings were robust to several alternative specifications, and analyses at agency level. Treatment effects from

this analysis were larger, in general, than those observed in the Medicaid papers (Arenberg et al., 2020; Aslim et al., 2020; Fry et al., 2020; Jácome, 2020; Vogler, 2017; Wen et al., 2017) but consistent with treatment effects reported by Sharkey et al., (2017), which found that 10 additional community-based organizations reduced property crime by 7%.

Vogler et al., (2017) examined the effect of Medicaid expansion on crime and found expansion was associated with a 3% reduction in total crime. Similarly, Deza et al., (2020) found 10 additional office-based mental health providers reduced total crime by 0.5%. Static DD estimates from our agency-level analysis do suggest more modestly sized reductions (with less precision) of 2.4% for total crime ($p=0.16$), and 3.3% for property crime ($p=0.08$) over the period. However, those estimates are subject to heterogeneous treatment effect bias that can push the observed treatment effects on crime towards zero.

In Chapter 5, I examined the modifying relationship between Medicaid expansion and health centers on crime. In the absence of Medicaid, results suggest health centers reduced total crime by 5.4% and property crime by 5.7%. The interacted coefficients are suggestive of a compounding treatment effect of Medicaid expansion and health centers. However, interaction estimates among the full sample were imprecise apart from violent crime.

Estimates from the subgroup analysis in Chapter 5 suggest that receiving Medicaid and a health center was associated with a 24.5% reduction in total crime among non-metro counties and a 13.6% reduction among counties with larger populations of individuals less than 20 years old. However, as described in Chapter 5 I am unable to determine whether coefficients are being driven by a true modifying relationship or if the dose of a newly certified health center varies by county type. Overall, I cannot reject my hypothesis that health centers and Medicaid expansion act as complements to reduce crime.

Policy Implications

Improving access to care is the central goal of the health center program. Continued federal investments in the health center program may be needed to sustain or make further improvements on access to primary care services, particularly among geographies with shortages of healthcare providers and services. Additionally, our results suggest that moving the needle on access to care may take time. Areas that receive a health center are already experiencing shortages of primary care providers. Despite the immediate need, we our results suggest it may take several years before newly established providers are able to make a large impact. This period of delay between certification and showing effects on primary care visits may suggest that health centers could benefit from additional patient outreach and startup support if policymakers want to see a quicker return on federal investments in the program. Next, our results show that enhancing access to health care can translate to improvements in social outcomes beyond health.

Indeed, crime is a disruptive force that impacts victims, criminal offenders, and local communities. Increasing the presence of police and instituting harsher penalties for criminally offending has disproportionately impacted communities of color (Travis et al., 2014). Approaches to mitigate the impacts of crime on populations should consider investments in health interventions, alongside other criminal justice reforms. Health access programs, like health centers and Medicaid expansion, offer policymakers an array of policy options that prevent crime from occurring. My results suggest these preventive efforts, coupled with reactive criminal justice interventions, may offer a more equitable approach to addressing crime.

Future Directions

In Chapter 3 I examined the effect of health centers on primary care visits among the Medicare population. However, Medicare enrollees comprise only a small share of the total

health center population. In future work I plan to build on this research by examining outcomes among the uninsured and populations covered by Medicaid who comprise most of the health center patient population. Next, due to data limitations I only examined the effect of health centers on primary care visits. While primary care is a required service for both RHCs and FQHCs, health centers, as suggested by the literature, may have important impacts on other outcomes including behavioral health services, hospitalizations, and other measures of social well-being.

The results from Chapter 4 suggest health centers reduce crime. However, I was not able to determine the specific service offerings or other qualities of health centers that may have influenced that result. For example, health centers differ in the delivery of behavioral health services and may contract out much of the specialty care they provide. Additionally, while I show the introduction of the first health center reduces crime, my results mask possible treatment effect heterogeneity based on the number of health centers a county receives in a year. This is an important caveat to my results. To build on this analysis, I plan to use a continuous treatment design which will allow me to consider the additive effects of health centers over time.

Last, a large literature has attempted to explain the dramatic fall in violent and property crime from the early 1990s to today. This area of research remains critical as violent crimes in the US have been on an increasing trend. To date, authors have shown that growth in the police force (W. Evans & Owens, 2007; Kaplan & Chalfin, 2019; Levitt, 2002), community-based organizations (Sharkey et al., 2017), declines in the market for crack cocaine and legalization of abortion (Levitt, 2004), and reductions in childhood lead exposures (Reyes, 2007) were responsible for substantial national declines. Importantly, the FQHC program was established in the early 1990's which brought about considerable changes to the delivery of services at health

centers as well as enhanced payment for providers. To date, no studies have attempted to estimate the role of health centers in the greater crime decline.

Conclusion

This is the first study to examine the effect of health centers on crime, and one of the only studies to examine the effect of health centers on access using plausibly exogenous variation from the staggered expansion of the program. As suggested by my conceptual model, results demonstrate that health centers increase primary care utilization and reduce total and property crimes. While imprecise, my results also suggest health centers and Medicaid may act as complements to reduce crime.

Appendix

In this section I provide additional analyses to support results from Chapter 3.

In Figure A.1. I provide event-study estimates (period-specific treatment effects) from a robustness test which adjusts for linear trends in log visits to a primary care clinician. This is done to examine whether any existing differential pre-trending prior to health center certification causes bias in our results. Overall, these results lend support that we have not violated the parallel trends assumption.

The specification provided in the figure uses the Sun and Abraham (2020) interaction weighted estimator. In relative periods before treatment the treatment effects are closely grouped around zero suggesting no impact on primary care visits prior to health center certification. After the certification of a health center "health center opening" visits begin to increase at a rate consistent with that observed in our main analysis. Interestingly, at T=6 or seven years following health center opening the effects on primary care visits fall to zero and are much more imprecise. This is likely due to imbalance in the panel at the tail end of the distribution because a smaller number of counties are observed in that period.

Figure A.1. Event-study estimates of the effect of health centers on primary care visits, among Medicare fee-for-service enrollees (2008-2016), adjusted for linear trends
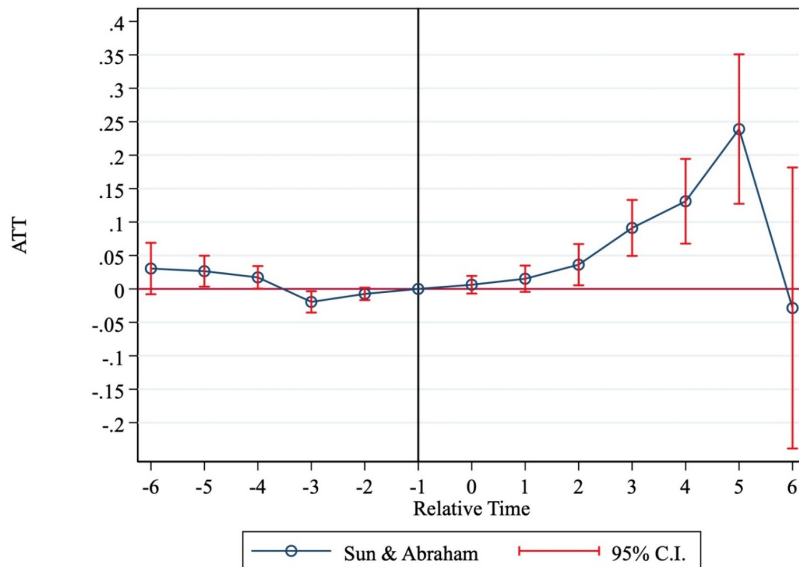
Notes: The outcome is the natural log (y+1) of the percent of Medicare fee-for-service enrollees with an annual visit to a primary care clinician. Estimates are the relative period-specific average treatment effect on the treated. The specification includes county and year fixed effects, and standard error are clustered on the county. The vertical black line denotes the year prior to health center certification. This specification is also adjusted for linear trends in log annual visits to a primary care clinician. **Source**: Dartmouth Atlas county-level primary care measures (2008-2016), Centers for Medicare and Medicaid Services Provider of Services (POS) File (2006-2020)

Part of the difficulty with this analysis is identifying an adequate counterfactual or control group.

For the main analysis, I use counties who received a health center after 2016 as controls because

I believe there are qualities that cannot be measured in my data that would make them more

closely approximate treated counties than those who never received a health center. However, as

a robustness check I include never treated counties as controls and repeat my TWFE

specification. Results from my main analysis and those including never treated as controls are

provided in Figure A.2. The period-specific treatment effects presented in this event study are

nearly identical across both approaches suggesting the inclusion or exclusion of never treated

counties does not impact on my results.

Figure A.2. Event-study estimates of the effect of health centers on primary care visits, among Medicare fee-for-service enrollees (2008-2016), alternative control group
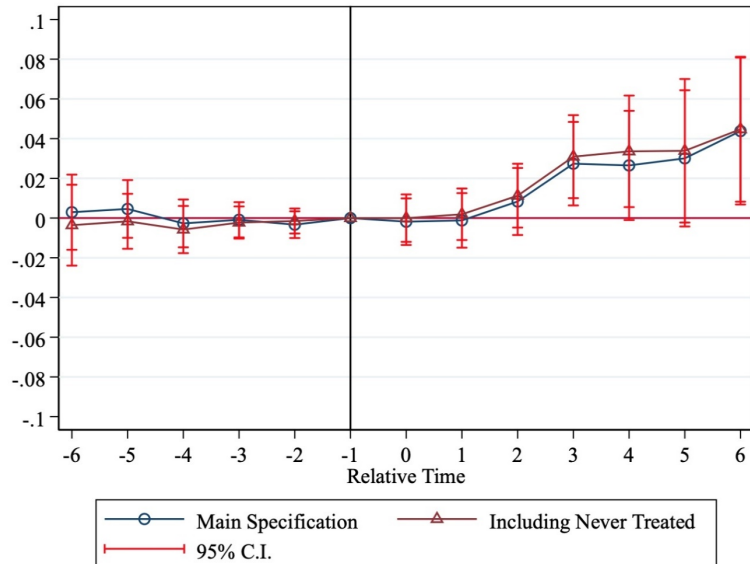
Notes: The outcome is the natural log (y+1) of the percent of Medicare fee-for-service enrollees with an annual visit to a primary care clinician. Estimates are the relative period-specific average treatment effect on the treated. Each specification includes county and year fixed effects, and standard error are clustered on the county. The vertical black line denotes the year prior to health center certification. **Source**: Dartmouth Atlas county-level primary care measures (2008-2016), Centers for Medicare and Medicaid Services Provider of Services (POS) File (2006-2020)

Section 2

In this section I provide additional robustness tests to support my main analysis from Chapter 4. The UCR data when used at the county level may have important limitations. First, each agency represented in the Offenses Known files may cover populations in more than 1 county. When allocating crimes from agency level to county level we are assuming that all crimes from that agency take place in the county where the agency is physically located. This has obvious limitations. Next, the UCR crime counts, like other administrative records, suffer from a missing data problem. Agencies do not always report all 12 months of the year and therefore crimes for underreporting agencies were imputed using the method described in Chapter 4. The imputation procedure is flawed and may undercount or overcount crimes based on the method used to identify the number of months an agency reports ((Kaplan, 2021). To overcome the limitations of the UCR data I provide results from an analysis in which I limited the agency files to only those which reported all 12 months of the year. The analysis is conducted at the agency level, so the files reflect multiple county observations per year (e.g. agency by year). In

Figure A.3. I provide event study estimates of the effect of health centers on log total crime. I provide results from my preferred TWFE specification as well as those coming from newly developed unbiased estimators (Deshpande & Li, 2019; Gardner, 2021; Sun & Abraham, 2020). For the stacked-DD specification, I used a group of control agencies that received treatment more than 4 years after each cohort of openings between 2008 and 2015. Therefore, event study years beyond 4 years after opening were dropped from the analysis and the results from this specification end at T=3.

The estimated treatment effects of health center opening on treated agencies in the pre-period are very small in magnitude and confidence intervals do not cross zero suggesting no evidence of differential effects on log total crime in the pre-period. After health center certification

The Sun and Abraham specification suggests agency-level total crimes decrease by 5.2% ($p < 0.05$) at four years following health center certification. Additionally, the effect grows to a 10.9% reduction in total crime ($p < 0.05$) within seven years compared with controls. While these estimates are more modestly sized and less precise than our county-level analysis they provide evidence that results from our county level analysis are not driven by the imputation and allocation procedures to construct the county-level files.

Figure A.3. Event-study estimates of the effect of health centers on log total crime, agency level analysis among full reporting agencies
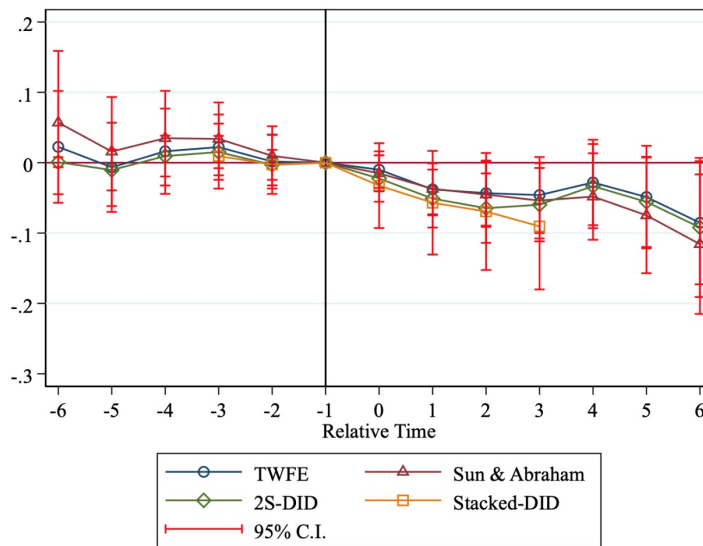
Figure Notes: Data come from the UCR Offenses Known and Clearances by Arrest (2005-2016). This agency-level analysis excludes any agency without 12 full months of reporting. The event study specification is fully adjusted for state and county-year covariates and robust standard errors are clustered on the agency. The vertical line reflects the year prior to health center opening.

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