Local access to mental healthcare and crime

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Abstract

We estimate the effect of local access to office-based mental healthcare on crime. We leverage variation in the number of mental healthcare offices within a county over the period 1999 to 2014 in a two-way fixed-effects model. We find that increases in the number of mental healthcare offices modestly reduces crime. In particular, 10 additional offices in a county reduces crime by 1.7 crimes per 10,000 residents, with an implied crime-treatment elasticity of -0.06. These findings suggest an unintended benefit for expanding the office-based mental healthcare workforce: reductions in crime.
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1 Introduction

During the 1960s and 1970s as part of the de-institutionalization movement, state psychiatric hospitals closed and, among remaining hospitals, censuses declined markedly (Lamb & Bachrach, 2001). The change in the number of Americans receiving treatment in psychiatric hospitals was staggering: in 1955 there were roughly 339 per 100,000 individuals institutionalized in a psychiatric hospital and this number declined to 22 per 100,000 by 2000 (Lamb & Weinberger, 2005). While the factors that lead to de-institutionalization are complex, healthcare scholars suggest that public support for more humane treatment of the mentally ill,[1] advancements in pharmacology (in particular, Thorazine; earlier treatments for mental illness often relied on lobotomy and electroshock therapy), and the high cost to government of institutionalizing large numbers of Americans alongside changes in federal social welfare programs (e.g., Medicaid) were important drivers (Lamb & Bachrach, 2001; Yohanna, 2013). These closures and reduced censuses lead to many mentally ill patients who would have otherwise been hospitalized for extended periods of time being displaced into the community and receiving treatment in federally funded community mental health centers, and relying on social support for basic services and living arrangements.

As a result of de-institutionalization, concerns that individuals with mental illness presented a danger to the community grew (Frank & McGuire, 2010). While advocates contended that those with mental illness posed no greater crime threat than the general population, the limited data available at the time did not allow empirical testing of this relationship and public concerns remained. These concerns diminished as crime rates declined substantially in the 1990s (Levitt, 2004). Several recent violent attacks, particularly mass shootings, have been linked, at least anecdotally, to mental illness.[2] These events have revitalized public interest and discussion in the potential importance of mitigating mental illness through increased receipt of effective treatment as a tool to reduce crime and violence.

While a large literature documents that many modalities of mental healthcare treatment improve individual patient outcomes (American Psychiatric Association, 2006; Markowitz & Cuellar, 2007; Marcotte & Markowitz, 2011), there may be broader social benefits from such treatment, including crime control. If mental healthcare treatment has additional benefits, these benefits may suggest that enhanced investment by government is potentially warranted.

[1] Several institutions were exposed publicly as providing very poor, arguably abusive, treatment to mentally ill patients. An example was the novel 'One Flew Over the Cuckoo's Nest' which captured treatment of institutionalized mentally ill patients from the perspective of a nurse's aid.

[2] Please see, for example, the following news story: https://www.vox.com/2019/8/5/20754770/trump-el-paso-dayton-speech-white-house-mental-illness-video-games-guns (last accessed October 1st, 2019).

Indeed, clinical studies document a robust correlation between mental illness and crime (Swanson et al., 2001; Frank & McGuire, 2010). For example, 50% of those incarcerated in jails and prisons have a mental illness (James & Glaze, 2006). Further, and strikingly, the number of mentally ill individuals housed in certain large U.S. incarceration facilities, such as the Los Angeles County Jail, Cook County Jail, and Rikers Island, exceeds the number of individuals in any psychiatric institution in the country (Frank & McGuire, 2010). Similarly, two thirds of juvenile inmates are diagnosed with mental illness. These correlations offer *prima facie* evidence that mental illness contributes to crime rates.

To date, improving access to mental healthcare has generally been overlooked as a potential policy lever to reduce crime. Criminal justice mental healthcare policies in the U.S. are mainly restricted to involuntary mental healthcare treatment, thereby largely ignoring the efficacy of voluntary treatment. For instance, the criminal justice system has made use of involuntary outpatient commitment laws, which afford ample discretion to judges to mandate that convicted offenders receive mental healthcare treatment. The available evidence, while somewhat mixed, suggests that such treatment may improve crime outcomes modestly in some settings (Ridgely, Borum, & Petrila, 2001; Swanson et al., 2001; Swartz et al., 2001; Hiday, Swartz, Swanson, Borum, & Wagner, 2002; Swartz & Swanson, 2004; Kisely, Campbell, & O'Reilly, 2017; Swartz, Bhattacharya, Robertson, & Swanson, 2017). However, this evidence plausibly provides a lower bound on treatment benefits as it is based on seriously mentally ill patients in specific treatment modalities, and the affected patients do not wish to receive treatment and are instead required by the criminal justice system or face incarceration in jail or prison. On the other hand, there is substantial evidence among non-incarcerated populations suggesting that many major mental illnesses can be effectively treated (Bronfenbenner, 1979; Henggeler & Borduin, 1990; Aos, Barnoski, & Lieb, 1998; Brestan & Eyberg, 1998; Kazdin, 2002; Aos, Lieb, Mayfield, Miller, & Pennucci, 2004; National Institute for Clinical Excellence, 2006; Hill, Roberts, Grogger, Guryan, & Sixkiller, 2011; Layard & Clark, 2014; Nathan & Gorman, 2015). Thus, mental healthcare treatment may be underutilized as a crime reduction policy in the U.S.

While mental healthcare is on average valuable, there are well-established shortages of providers who deliver this care in the U.S. Estimates suggest that 77% of the U.S. counties have a mental healthcare provider shortage (Thomas, Ellis, Konrad, Holzer, & Morrissey, 2009) and, in a given year, more than 50% of individuals meeting diagnostic criteria for mental illness do not receive any treatment (Center for Behavioral Health Statistics and

Quality, 2018).[3] A commonly cited barrier among individuals who sought, but did not receive, mental healthcare treatment is inability to locate a provider (Center for Behavioral Health Statistics and Quality, 2018). Given this backdrop, expanding access to mental healthcare treatment, by increasing the number of providers offering these services, may allow patients to receive care, manage their illness and associated symptoms, and, in turn, reduce propensity to commit crime and/or the likelihood of victimization.

This study is the first to explore the effect of access to office-based mental healthcare treatment on crime. We estimate two-way fixed-effects models using county-level crime rates provided by the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program to construct measures of crime and the U.S. Census Bureau's County Business Patterns (CBP) over the period 1999 to 2014. Thus, we identify spillover effects of mental healthcare treatment to crime by exploiting variation in the county-level number of office-based mental healthcare providers.

Our results suggest that increased local access to office-based mental healthcare providers, proxied by the number of providers per county, leads to a modest reduction in county-level crime rates. In particular, an additional 10 mental healthcare provider offices in a county, which corresponds to an 8.2% increase in offices, reduces the overall crime rate by 1.7 per 10,000 residents (0.5%), and the violent crime rate by 0.9 per 10,000 residents (2.0%) and the nonviolent crime rate by 0.08 per 10,000 residents (0.2%). Thus, our estimates imply crime-treatment elasticities of -0.06 (total), -0.24 (violent), and -0.03 (nonviolent). We also show that serious mental illness, proxied by the suicide rate, declines as the number of office-based mental healthcare providers increases. While our estimated effects are arguably not large in magnitude, given the high costs of crime to society – for instance, the cost of a murder is $10,904,900 (McCollister et al., 2017)[4] – they suggest important spillovers that have not been previously documented.

This paper proceeds as follows. Section 2 reviews the related literature and provides a conceptual framework. Section 3 outlines our data and methods. Results are reported in section 4. Sensitivity analyses are reported in Section 5. Section 6 concludes.

[3]As recently as 2018, just 26% of mental healthcare needs were met in the U.S. (Kaiser Family Foundation, 2018). The Kaiser Family Foundation uses the following definition for meeting mental healthcare needs: '...dividing the number of psychiatrists available to serve the population of the area, group, or facility by the number of psychiatrists that would be necessary to eliminate the mental health HPSA (based on a ratio of 30,000 to 1 (20,000 to 1 where high needs are indicated)).' HPSA=Health Professional Shortage Area.

[4]The estimate was inflated to 2019 dollars by the authors using the Consumer Price Index.

2 Related literature and conceptual framework

2.1 Related literature

This section describes two bodies of literature that are related to our research question: what is the causal effect of access to office-based mental healthcare treatment on crime? (i) The literature that explores the extent to which mental illness symptoms pre-dispose individuals to crime-related outcomes. (ii) Economic studies that estimate the causal effect of mental healthcare treatment on crime.

2.1.1 Mental illness symptoms that may pre-dispose individuals to crime

The epidemiology of many mental illnesses provides plausible mechanisms through which these conditions can affect crime-related outcomes. While discussing the specifics of each illness is beyond the scope of our study, we next review several illnesses and their symptoms to offer intuition on the mental illness-crime link.

Symptoms of psychotic disorders – a class of serious mental illnesses (e.g., schizophrenia) in which the affected individual has sensory experiences of things that do not exist and/or beliefs with no basis in reality – are hallucinations, delusions (of paranoia or grandeur), and disordered thinking. Bipolar disorder causes extreme oscillations in mood from emotional highs (mania) to lows (depression). Common symptoms experienced during a mania phase include aggression and agitation, an exaggerated sense of self-confidence, racing thoughts, poor decision-making, and risk-taking behaviors (e.g., substance misuse and unsafe sexual activity). Antisocial personality disorder is characterized by aggressive and violent behavior, a disregard for the safety and well-being of others, and a lack of remorse. Individuals with narcissistic personality disorder believe that they are special and more important than others, are arrogant, frequently take advantage of others, and are unable to recognize the needs and feelings of others. Those with paranoid personality disorder have relentless suspicion of others without reason and feel that other people are 'out to get them.' These feelings lead to anger and retaliation in response to perceived threats, hostility, hypersensitivity to criticism, and continued beliefs that those closest to them are unfaithful (e.g., a cheating spouse/partner). These symptoms could contribute to crime through distorted assessments of costs and benefits.

Even mental illnesses that are, anecdotaly, less likely to be linked with crime can have criminogenic symptoms. For example, the most common mental illnesses in the U.S., the focus of our study, are anxiety and depression (Center for Behavioral Health Statistics and

Quality, 2018). These illnesses are associated with symptoms such as aggression, incessant concern for personal safety and well-being without any evidence to support such unease, delusions, disruptive behaviors, feeling helpless and worthless, and irritation, all of which could potentially increase the propensity to commit crime.

Similarly, many mental illness imposes side effects such as decision difficulties, headaches, fatigue, insomnia, memory loss, nausea and gastrointestinal difficulties, physical pain, and a sense of hopelessness and worthlessness that could reduce employment opportunities (Chatterji, Alegria, Lu, & Takeuchi, 2007; Chatterji, Alegria, & Takeuchi, 2011; Frijters, Johnston, & Shields, 2014; Banerjee, Chatterji, & Lahiri, 2017). The incentive to commit financially-motivated crimes may increase when legal employment options decline.

Mental illness may also raise the likelihood of developing a substance use disorder (SUD). Many individuals self-medicate their symptoms with alcohol and illicit drugs (Drake & Wallach, 1989; Levy & Deykin, 1989; Kilpatrick et al., 2003; Elliott, Huizinga, & Menard, 2012), which could lead to crime through distorted decision-making, interacting with criminals through the purchasing of drugs, or a need to secure funds to procure substances (Wen, Hockenberry, & Cummings, 2017; Bondurant, Lindo, & Swensen, 2018).

In addition to increasing the propensity to commit crime, mental illness could also affect the probability of being victimized as the mentally ill may be viewed as 'easy' targets. Mentally ill individuals experience substantially higher rates of victimization than the general population (Teplin, McClelland, Abram, & Weiner, 2005; Maniglio, 2009), plausibly because their symptoms make them vulnerable. For instance, disordered and aggressive behavior may draw the attention of offenders (e.g., offenders may view aggressive behavior by a person with a mental illness as a threat which requires retaliation). Those with mental illness are substantially more likely to be homeless than the general population (Fazel, Khosla, Doll, & Geddes, 2008), which plausibly increases opportunities for victimization.

Finally, the 'criminalization of mental illness' may influence the likelihood of incarceration rather than crime *per se* (Lamb, Weinberger, & DeCuir Jr, 2002). For instance, an individual with mental illness may be more likely to be arrested than a person without mental illness even if they committed the same crime, or no crime at all, if police officers react to disturbed behavior. Criminal behavior can confused with mental illness symptoms, leading to inappropriate incarceration for those with mental illness. Some symptoms of mental illness such as a mis-perceived threat, aggression and violence could either be attributed to mental illness or could be directly considered criminal behavior.[5] Further, given elevated

[5]Potentially due to de-institutionalization, police are viewed by some healthcare scholars as first line

rates of homelessness (Fazel et al., 2008), those with mental illness are potentially more likely to attract police attention for crimes such as loitering.

Overall, the epidemiology of mental illness suggests a correlation between these health conditions and a range of crime-related outcomes. The extent to which these correlations reflect a causal relationship, however, is unclear. Indeed, both mental illness and crime are likely driven by factors such as parental abuse or neglect, trauma, poverty, and genetic predisposition. These factors are rarely found in data sets, preventing adequate adjustment for differences between those with and without mental illness in regression models.

2.1.2 Studies examining the effect of mental healthcare on crime

To the best of our knowledge, only three economic studies have examined the causal effect of mental healthcare treatment on crime outcomes. First, Cuellar and Markowitz (2007) documents that increased Medicaid spending by states and psychotropic prescriptions for stimulants and depression lead to a reduction in violent crimes during the 1990s and 2000s. Second, Marcotte and Markowitz (2011) show that the increased prescribing of psychotropics that occurred over the 1990s and 2000s reduced crime rates. Finally, in a recent study Landersø and Fallesen (2016) show short-run reductions in crime following a psychiatric hospitalization using administrative data from Denmark. Effects dissipate quickly and the authors hypothesize that they may capture mechanical incapacitation effects. Our study contributes to this small literature by examining how changes in access to office-based mental healthcare providers lead to changes in crime rates over a recent time period in the U.S.

2.2 Conceptual framework

We build a framework in which we can think through the relationship between access to office-based mental healthcare and crime. To do so, we leverage intuition offered by previous economic and sociological models of crime (Becker, 1968; Goldstein, 1985; Wen et al., 2017).

mental healthcare providers (Lamb et al., 2002). However, lack of integration between the criminal justice and healthcare systems may also lead to unnecessary incarceration of those with mental illness. For example, law enforcement agents in all U.S. states have the authority to refer an individual to a mental health evaluation if the individual is believed to be at risk of harming themselves or others. However, law enforcement agents face uncertainty when referring a mentally ill individual displaying potentially deviant behavior to the healthcare system (e.g., how much of the agent's time will be involved with admitting the patient, which reflects time that the agent cannot allocate to fulfilling crime-control duties) but such agents are more certain regarding the process of the criminal justice system (where the individuals can also receive a mental health evaluation), which may prompt law enforcement to prefer incarceration in many settings (Lamb et al., 2002).

The relationship between crime and mental illness can be specified as follows. A crime that occurs in county j and in time t ($Crime_{i,k,j,t}$) that is committed by potential offender i against victim k is a function of mental illness symptoms experienced by the offender i ($Mental\ illness_{i,j,t}$) and the mental illness symptoms experienced by the potential victim k in county j in time t ($Mental\ illness_{k,j,t}$). Crime rates are also plausibly influenced by other observed factors that affect offending and victimization propensities, $X_{1i,j,t}$ and $X_{1k,j,t}$ respectively. Finally, there are observed and unobserved factors that enhance or limit opportunities for crime that affect both offenders and victims such as police presence and citizen actions (e.g., installation of home security systems) in county j and time t: $P_{j,t}$. We can combine these factors in a crime rate production function as outlined in Equation 1:

$$Crime_{i,k,j,t} = f(Mental\ illness_{i,j,t}, Mental\ illness_{k,j,t}, X_{1i,j,t}, X_{1k,j,t}, P_{j,t}) \tag{1}$$

We estimate the reduced form relationship between access to mental healthcare treatment proxied by the offices of physician and non-physicians specializing in mental healthcare treatment in a county ('MH') and crime. To derive the reduced-form equation, we rewrite mental illness symptoms as a function of office-based mental healthcare treatment providers in county j and in time t ($MH_{j,t}$), which affect mental illness symptoms of the potential offender i and victim k. Finally, $X_{2i,j,t}$ and $X_{2k,j,t}$ are the covariates that affect mental illness and associated symptoms of the potential offender and victim.

We can write the reduced form potential offending and victimization equations as follows:

$$Mental\ illness_{i,j,t} = g(MH_{j,t}, X_{2i,j,t}) \tag{2}$$

$$Mental\ illness_{k,j,t} = h(MH_{j,t}, X_{2k,j,t}) \tag{3}$$

After substituting Equations 2 and 3 into Equation 1, we have the reduced form equation for crime rates in county j and time t which is outlined in Equation 4:

$$Crime_{i,k,j,t} = k(MH_{j,t}, X_{1i,j,t}, X_{1k,j,t}, X_{2i,j,t}, X_{2k,j,t}, P_{j,t}) \tag{4}$$

For simplicity, we assume that $X_{1i,j,t} = X_{1k,j,t} = X_{j,t}$ and that $X_{2i,j,t} = X_{2k,j,t} = X_{j,t}$. This simplification is reasonable since local socioeconomic variables that may influence crime or mental illness such as exposure to social changes would affect the propensity to commit crime, become victimized and would also affect the mental illness of both the offender and victim. This simplification leads us to the following crime production function for crimes that occur in county j and time t:

$$Crime_{j,t} = l(MH_{j,t}, X_{j,t},, P_{j,t}) \quad (5)$$

Based on the established effectiveness of mental healthcare in treating mental illness, we hypothesize that, *ceteris paribus*, more office-based mental healthcare providers in a county will increase a prospective patient's access to treatment which will increase treatment uptake. Correspondingly, mental illness and associated symptoms will improve through the increased treatment receipt and, in turn, crime outcomes will fall. Thus, we expect to observe: $\partial Crime_{j,t}/\partial MH_{j,t} < 0$.

While there are other determinants of access to mental health treatment besides the number of offices in the local area (e.g., ability to pay, transportation issues, cognitive and social difficulties among those with mental illness, perceived stigma associated with mental illness and associated treatment), the ability to locate a provider within the local market is plausibly an important predictor of access. We acknowledge that there are other channels through which mental illness may influence crime. For example, individual-level variables that enter $X_{1i,j,t}$ and $X_{1k,j,t}$ such as the education or general health. As a particular example, if mental illness affects education quantity or quality (Solomon, 2018), this change can reduce employment opportunities through reduced marginal product and earned wage, leading to an increased propensity to commit crime.

Our empirical models, outlined in the next section, will capture the overall effect of mental healthcare providers on county crime rates. A limitation of our analysis is that we will not be able to isolate the relative importance of mental healthcare providers for offending and victimization. While these relationships are clearly important, we view our study as the first step in understanding the practical importance of local access to office-based mental healthcare providers as a tool to reduce crime rates. We encourage future work, using different data sets, to provide additional insight on this important question.

3 Data, methods, and identification

3.1 Uniform Crime Reports

We use the Uniform Crime Report between 1999 and 2014. These data are commonly used within the economics literature to study crime (Freedman & Owens, 2011; Swensen, 2015; Wen et al., 2017; Bondurant et al., 2018; Dave, Deza, & Horn, 2018). The UCR is compiled by the FBI to track criminal offenses known to police in the U.S. Hence, while we use the term 'crime' for brevity, we note that we capture crimes that are known to police officers which are a subset of all crimes that occur. In particular, we use the County-Level Detailed Arrest and Offense Data (UCRC) prepared by the Inter-University Consortium for Political and Social Research (ICPSR) staff.

Unlike the UCR which reports data at the police agency level, the UCRC imputes missing data and includes a 'coverage' indicator to diagnose the quality of county-level data. In particular, coverage in this context corresponds to the share of county data that is imputed. This variable ranges from zero when all county data is based on imputation to 100 when all agencies in the county reported data for 12 months in that year. We follow Freedman and Owens (2011) and retain only those county-year observations for which no more than 50% of the data is imputed. This exclusion leads us to drop 2,962 county/year pairs or 6% of the full sample. We opt to use the county-level data rather than the agency-level data so that we can apply this quality adjustment. We convert crime counts to the rate per 10,000 residents using the population covered by the UCRC. We note that this inclusion criterion, while standard within the literature, requires that we focus on more populous counties within the U.S. Our findings therefore apply to those counties only without additional assumptions.

We examine changes in Part 1 crimes only. We choose to examine Part 1 crimes only as other, less serious, crimes are more likely to go unreported to police, leading to systematic measurement error in our outcome variables which can cause difficult to sign bias *ex ante* (Bound, Brown, & Mathiowetz, 2001). The UCRC includes the following Part 1 crimes: murder, manslaughter, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft. We combine murder and manslaughter into one group due to the relatively small number of known offenses in these categories, and refer to the combined group as 'murder.' We construct aggregate measures of total, total violent (murder, rape, aggravated assault, and robbery), and total nonviolent (burglary, larceny, and motor vehicle theft) offenses.

3.2 Access to office-based mental healthcare providers

We use the number of establishments of physician and non-physician providers specializing in mental healthcare treatment in each county to proxy local access to office-based care drawn from the U.S. Census Bureau's Community Business Patterns (CBP) data. The U.S. Census Bureau defines an establishment as follows: 'A single physical location where business is conducted or where services or industrial operations are performed.'[6] In our context, an establishment is a unique office in which one or more physicians or non-physician providers deliver mental healthcare services. For simplicity, we refer to establishments as 'offices.'

CBP data are collected and maintained by the U.S. Census Bureau and include the near universe of establishments in the U.S. each year during the week of March 12. These data have been utilized to study the effect of behavioral health treatment access in recent economic studies (Swensen, 2015; Bondurant et al., 2018). We select establishments in the CBP with the following North American Industry Classification System (NAICS) codes to form a count of physician and non-physician offices in each county: 621112 (offices of physicians, mental health specialists) and 621330 (offices of mental health practitioners except physicians). Prior to 1998, the CBP did not include sufficiently fine information to allow identification of mental healthcare providers.[7] In our empirical models, we lag offices one year as is common in the literature (Swensen, 2015; Bondurant et al., 2018). This lag structure allows, for example, time for an office to open, patients to access care, and mental illness and associated symptoms (including crime) to improve. Hence, we begin the study period in 1999. We merge the UCRC into the CBP data on county and year. We have 45,577 county-year pairs.[8] We examine all office-based mental healthcare providers in our main analysis.[9]

A patient seeking mental healthcare treatment has a wide range of treatment options from which to chose. We focus on just one modality: office-based care. Other options include hospitals, community mental health centers, specialized outpatient facilities, and residential facilities. Office-based care for mental healthcare treatment captures evaluation of the patient and associated health conditions,[10] prescriptions for medications, and/or counseling services. We note that many mental illnesses, from conditions such as anxiety and depression to arguably more criminogenic illnesses such as anti-social personality disorder, bipolar disorder, paranoid personality disorder, and psychotic disorders can be – at specific stages of the illness

[6] See https://www.census.gov/programs-surveys/susb/about/glossary.html (last accessed October 1st, 2019).

[7] Prior to 1998 the CBP included four digit industry codes and the establishments we study were combined with general healthcare providers.

[8] We are unable to match a small number of counties due to changes over time in the CBP county-identifiers and/or U.S. Census county definitions. Please see https://www.census.gov/geo/reference/county-changes.html (last accessed October 1st, 2019).

[9] We note that some of the office-based providers may operate within the same healthcare delivery system, which we cannot observe in our data. For example, two offices may be affiliated with the same hospital and thus there may be interactions in referrals across the offices that the CBP does not measure.

[10] Patients with mental illness are at elevated risk for both SUDs and physical health conditions such as cardiovascular disease (Janssen, McGinty, Azrin, Juliano-Bult, & Daumit, 2015) which complicate treatment of the mental illness.

– treated in office-based settings.

There are several reasons why office-based care may be a particularly relevant, yet understudied, tool to fight crime through mental illness. First, office-based providers play an increasingly important role in delivering mental healthcare treatment. For instance, while in 1986 24% of total mental healthcare expenditures were for office-based care, by 2014 this share had increased to 44% (Substance Abuse and Mental Health Services Administration, 2016).[11] Second, many patients prefer to receive mental healthcare in general settings that we study rather than in specialized treatment facilities or psychiatric hospitals, thus office-based care may be more acceptable to patients which may improve treatment adherence.[12] Finally, by examining office-based providers we are less concerned with confounding from incarceration effects, that is, when a patient is 'incarcerated' through residential or hospitalization treatment, crime (through offending or victimization) is mechanically reduced.

The average treatment effect that we will estimate in our empirical models is local to the type of patient who (i) seeks treatment when an office-based mental healthcare provider opens in their county but not otherwise, and (2) is likely to benefit, in terms of improved mental illness and reduced crime, from this access and the ensuing treatment use. We hypothesize that the 'compliers' in our analysis are those patients with relatively well-managed mental illness, to the extent that their health outcomes can be effectively treated in office-based, relative to specialty facilities (e.g., residential treatment or hospitalizations). These are the patients from whom our estimates are likely generated. The hypothesized complier foreshadows the modest effect sizes that we estimate in our empirical models.

3.3 Controls

As outlined in Section 2.2, county-level crime rates are plausibly determined by myriad factors independent of local access to office-based mental healthcare treatment. We attempt to control for such factors in our regressions. To this end, we link state- and county-level variables into the merged UCRC and CPB data set. In terms of state-level variables, we control for the political party of the Governor (University of Kentucky Center for Poverty Research, 2019)[13] and the state police force (police officers per 100,000). We adjust for

[11] See Exhibit 14. We define office-based care as the sum of office-based professionals and retail prescription medications, the latter are generally prescribed in office-based settings. For comparison, hospitals and specialty treatment programs typically rely on institutional pharmacies for medications.

[12] Experiencing acute episodes is not uncommon for patients with mental illness and, when such episodes occur, office-based care may not be sufficient. We are simply arguing that for some patients, at some stages of the illness, office-based care may be both appropriate to the patients' needs and preferred by the patient.

[13] We exclude DC as this locality does not have a Governor.

the following county-level variables: per capita income (converted to 2014 dollars using the Consumer Price Index), poverty rate, unemployment rate, and demographics (race, age, and sex) from the U.S. Census, Bureau of Labor Statistics, and Surveillance, Epidemiology, and End Results Program (SEER)[14] respectively. The time-varying county-level socioeconomic characteristics addresses plausible economic growth that certain counties experienced.

3.4 Empirical model

We estimate the relationship between local access to office-based mental healthcare providers and crime using the following two-way fixed-effects regression model:

$$Crime_{i,s,t} = \beta_0 + \beta_1 MH_{i,s,t-1} + X_{s,t}\beta_2 + H_{i,s,t}\beta_3 + \lambda_i + \gamma_{s,t} + \mu_{i,s,t} \tag{6}$$

$Crime_{i,s,t}$ is a crime rate for county i in state s in year t. $MH_{i,s,t-1}$ is the number of office-based mental healthcare providers in the county lagged one year. $X_{s,t}$ and $H_{i,s,t}$ are vectors of state- and county-level characteristics. λ_i is a vector of county fixed-effects and $\gamma_{s,t}$ is a vector of state-by-year fixed-effects.[15] $\mu_{i,s,t}$ is the error term. We estimate least squares and cluster the standard errors around the county and weight by the county population covered by the UCRC. We use variation in the number of mental healthcare provider offices to identify treatment effects. This variation is driven by the opening and closing of mental healthcare provider offices within a county.

We note that other studies examining the effects of access to behavioral healthcare treatment on crime, in particular treatment for SUDs, have relied on instrumental variable approaches that use state-level policies as instrumental variables for access to treatment (Wen et al., 2017). We are concerned that using such state-level policies (e.g., state-level laws that compel insurers to cover mental healthcare treatment at parity with physical healthcare and Medicaid expansions for mental healthcare treatment) not only expand coverage to the modalities of care that we study here but also access to modalities such as residential treatment, intensive outpatient, and hospitalization. Treatment received in alternative settings affected by such policies are plausibly related to crime. If this is the case, then using such a state policy as an instrument could potentially lead to a violation of the exclusion restriction (Angrist & Pischke, 2009). For this reason, we chose to estimate two-way fixed-effects models in the spirit of Swensen (2015) and Bondurant et al. (2018).

[14] We use SEER data made available through the National Bureau of Economic Research.

[15] $\gamma_{s,t}$ subsumes year fixed-effects.

3.5 Identification

The identifying assumption of Equation 6 is outlined in Equation 7:

$$Cov(MH_{i,s,t-1}, \mu_{i,s,t} | X_{s,t}, H_{i,s,t}, \lambda_i, \gamma_{s,t}) = 0 \tag{7}$$

In words, the number of office-based mental healthcare providers per county is uncorrelated with the error term in Equation 6 after conditioning on state- and county-level time-varying characteristics, county fixed-effects, and state-by-year fixed-effects. There are several threats to the validity of our empirical strategy which we next discuss and later investigate empirically.

An important threat to the validity to our research design is reverse causality. Put differently, changes in crime may lead to changes in the number of office-based mental healthcare providers through demand-side effects. We do not suspect that mental healthcare providers observe changes in crime and then elect to open/close their offices. However, crime could indirectly prompt individuals to seek care through the criminal justice system, the actions of family or friends, employers, and so forth, which, in turn, could increase demand for these services. We note that lagging our measure of mental healthcare access by one year partially addresses this concern. We formally explore reverse causality by estimating a dynamic model. That is, we include in Equation 6 a measure of mental healthcare provider offices measured two years prior to the period in which crime rates are measured, in the contemporaneous period, and one and two years in advance Swensen (2015) and Bondurant et al. (2018). If we find that the lead coefficient estimates are statistically indistinguishable from zero, this pattern of results would suggest that changes in mental healthcare offices lead to change in crime and are not simply an artifact of reverse causality.

A second threat to the validity of our study is that both changes in the number of office-based mental healthcare providers and changes in crime could both respond to a third unobserved (to the econometrician) factor. An example of such unobserved confounder would be public funds that could increase policing (which could affect crime) and the generosity of public insurance (which could increase demand for mental healthcare providers' services). To the extent that these factors predominantly vary at the state-level, the state-by-year fixed effects plausibly address this concern. Changes in the number of office-based mental healthcare providers and changes in crime could both respond to a third observable county-level confounder. An example of such observable confounder would be a positive shock in local economic conditions (which is not account for by our county-level controls), which could decrease incentives for financially motivated crimes while simultaneously increasing demand for mental healthcare providers. We explore the importance of observable confounders in two ways. (i) We regress the number of office-based mental healthcare providers on controls included in Equation 6 in order to assess balance across the 'treatment' and 'comparison' groups (Pei, Pischke, & Schwandt, 2018).[16] (ii) We report how office-based mental healthcare provider coefficient estimate changes as we progressively include covariates in Equation 6. If the coefficient estimate remains largely unchanged as we include additional covariates in the regression model, particularly any covariates that we observe predict office-based mental healthcare in (i), that would support the hypothesis that our results are not driven by confounders (Altonji, Elder, & Taber, 2005).

Finally, a third threat to the validity of our study is that openings and closing of office-based mental healthcare providers may induce some residents to move to/away from a county. This behavior is a form of program-induced migration that can lead to bias in regression coefficients (Moffitt, 1992). We address this concern and test for program-induced migration by using data on the propensity to move across county lines.

4 Results

4.1 Summary statistics and trends

Table 1 reports population-weighted summary statistics. The total crime rate per county is 360.0 per 10,000 residents; with 44.1 violent crimes and 315.9 nonviolent crimes per 10,000 residents. In terms of specific violent crimes, the rates per 10,000 residents are 0.52 murders, 2.95 rapes, 27.4 aggravated assaults, and 13.2 robberies. Similarly, the rates per 10,000 residents for nonviolent crimes are: 69.4 burglaries, 213.4 larcenies, and 33.1 motor vehicle thefts. On average there are 122.0 mental healthcare provider offices per county, with 67.4 physician offices and 54.7 non-physician offices.

Recall that we focus on more populous counties (see Section 3.1) and we weight the data by the population covered by the UCRC. Thus, our data potentially suggest better access to mental healthcare than previous reports (see for example Thomas et al. (2009) or Kaiser Family Foundation (2018)). This appearance is attributable to our sample exclusions and use of population weights (details available on request).

[16] All counties are to some extent treated, thus more accurately we are testing for balance across counties with greater (and lesser) intensity of treatment.

Figure 1 reports trends in crime rates over our study period in total, violent, and nonviolent crime rates. All three crime rates are declining modestly over our study period which is in line with previously established trends in these outcomes.

We next document trends in the number of office-based mental healthcare providers and provide some evidence on the amount of variation in our data. First, Figure 2 graphically depicts counties with no offices and one or more offices in 2014 (the last year of our study period, weights are not applied). While most counties have at least one office in this year, many counties in the Midwest and South do not. Second, we report trends in the average number of offices in each year of our study (Figure 3), the number of offices is trending upwards modestly over the study period. Third, we plot the number of counties across the country that experience a change in offices in each year of our study (Figure 4). This number ranges from a low of 728 in 1999 to a high of 1,101 in 2002. Overall, there appears to be substantial variation that we can use for identification.

Finally, we first remove year and county effects from total crime rates and scatter the resulting residuals and office-based mental healthcare providers lagged one year (Figure 5). There is a modest and negative correlation between the two variables: -0.019.

4.2 Regression analysis of aggregate crime rates

Table 2 reports selected regression results for the effect of county-level changes in the number of office-based mental healthcare providers on aggregate crime rates: total, violent, and nonviolent. Ten additional mental healthcare providers in the county reduces the crime rate by 1.7 crimes per 10,000 residents or 0.5% (comparing the point estimate with the sample mean, all relative effects are calculated in this manner henceforth). Both violent and nonviolent crime rates decline following the opening of an office-based provider: 10 additional offices in a county leads to 0.9 fewer violent crimes per 10,000 residents (2.0%) and 0.8 fewer nonviolent crimes per 10,000 residents (0.2%). The relative effect sizes are larger for violent crimes than nonviolent crimes as the baseline means are quite different: 44.1 violent crimes vs. 315.9 nonviolent crimes per 10,000 residents. Our estimates imply crime-treatment elasticities of -0.06 (total), -0.24 (violent), and -0.03 (nonviolent).

4.3 Testing the identification strategy

As outlined in Section 3.5, there are three key threats to our identification strategy: reverse causality, unobserved county-level heterogeneity, and program-induced migration. We next conduct several tests to assess the empirical importance of these threats and, therefore, the validity of our identification strategy. We focus on total crime rates in our testing.

4.3.1 Reverse causality

We first explore the possibility that our findings are driven by reverse causality. We estimate an augmented version of Equation 6 that includes two one year leads in the number of mental healthcare offices, the contemporaneous number of offices, and two lags in the number of offices. Note that our sample size is smaller as we lose observations in the creation of the two year lag variable (we can only identify the offices of mental healthcare providers we study beginning in 1998). Results are reported in Table 3 and indicate that crime is unaffected by future levels of office-based mental healthcare and only the one year lag is statistically in office-based providers is statistically distinguishable from zero.[17]

4.3.2 Unobserved county-level heterogeneity

We next turn the analyses to the second validity threat to our design and explore the extent to which our findings may be vulnerable to potential confounders. First, we assess the extent to which certain variables predict the number of office-based mental healthcare providers. Results are reported in Table 4. While office-based mental healthcare openings and closings are not predicted by our set of state-level controls, they are affected by certain county-level covariates such as the county-age distribution and gender composition.

Second, we explore the sensitivity of our estimates to different sets of control variables. We initially estimate a model with no time-varying controls, and then sequentially add in each state- and county-level control, re-estimate Equation 6, and report the relevant coefficient estimate (Table 5). Results are very stable as we add controls to the regression model, with 95% confidence intervals overlapping.

[17] In unreported analyses, we also estimate a specification that is more comparable to a standard event-study with a binary treatment (Autor, 2003). In particular, we locate a sub-sample of counties that have no offices in 1998 (this is one year prior to the beginning of our study period) and that we observe in each year of our study period. We exclude states that revert to no offices over our study period. We then identify the first year in which an office opens in each county and we refer to that period as the year of the event, and construct one-year leads and lags around this event. Most counties (57%) do not experience such an event and are thus coded as zero for all leads and lags. We construct an event window that extends six years before and after the event (Lovenheim, 2009). We treat six years in advance of the event as the reference period. We observe no evidence of differential pre-trends in this specification. This specification is not our preferred test for reverse causality because we cannot be confident that we are capturing the correct 'event' as we cannot measure openings and closing prior to 1998. Further, these counties may reflect a unique sub-set of all U.S. counties and findings from this set of counties may not generalize.

While we would prefer to observe full balance in terms of our covariates across counties with different levels of treatment intensity (Table 4), it is reassuring that the effect of office-based mental healthcare on crime remains largely unchanged if we include, or do not include, covariates for which there may be imbalance.

4.3.3 Program-induced migration

A final empirical concern that we investigate is program induced migration (Moffitt, 1992). We test for this behavior using data on past-year cross-county migration available in the Annual Social and Economic Supplement to the CPS. Specifically, we construct cross-county migration rates in each county and regress that outcome on the lagged number of office-based mental healthcare providers using Equation 6. Results are reported in Table 6 and reveal no evidence that changes in the number of office-based mental healthcare providers influences such migration. The sample size is smaller in this analysis as the majority of counties are suppressed in the CPS data for confidentiality reasons.

4.3.4 Assessment of the design

Thus, we view these analyses as suggesting that reverse causality, unobserved heterogeneity, and program-induced migration do not drive our findings and they support of our interpretation of the coefficients estimated in the two-way fixed-effect model as the causal effect of office-based mental healthcare on crime. For the remainder of the manuscript, we estimate two-way fixed-effects models.

4.4 Cost-adjusted crime rates

Our results implicitly treat the crimes equivalently in the sense that one additional homicide and one additional burglary are considered identically as one additional crime. This assumption is not likely true as different crimes are more and less costly in terms of criminal justice system and victim costs. Indeed, Chalfin and McCrary (2018) point out that the benefits from reductions in property crime are not sufficient to justify the expense of additional police officers. On the other hand, even relatively small reductions in violent crimes are sufficient to justify additional investments. We next estimate a cost-adjusted version of our main regression model following Chalfin and McCrary (2018).

We weight the specific types of crimes based on their estimated social costs: murder ($7M), rape ($142,020), robbery ($12,624), aggravated assault ($38,924), burglary ($2,104), larceny ($473), and motor vehicle theft ($5,786). This measure allows us to account for the fact that some crimes (e.g., murder) are more costly to society than other crimes (e.g., larceny). Accounting for differences in social costs across crime types does not appreciably alter our main findings (Table 7). An additional 10 office-based mental healthcare providers per county leads to a 2.1% reduction in total crime rates, a 2.2% reduction in violent crime rates, and a 0.9% reduction in nonviolent crime rates. The implied crime-treatment elasticities are somewhat larger than on our main findings for total and nonviolent crimes: -0.26 (total), -0.27 (violent), and -0.11 (nonviolent).

4.5 Mental illness

The mechanism through which we expect that changes in local access to office-based mental healthcare treatment lead to changes in crime is mental illness: additional office-based mental healthcare offices in a county allow more patients to receive care which, in turn, should reduce mental illness and associated crime. We next test this hypothesis using data on suicides from the Centers for Disease Control and Prevention's Public Use National Vital Statistics System (NVSS) Underlying Cause of Death public use files. These data record the universe of deaths in U.S. and classify deaths by cause, and are commonly used by economists (Klick & Markowitz, 2006; Lang, 2013; Solomon, 2018; Maclean, Tello-Trillo, & Webber, 2019).

We select deaths for which the ICD-10 code implies that the cause of death is suicide: U03, X60-X84, and Y87.0. We impute a value of five for suppressed cells.[18] We convert the deaths to annual rate per 10,000 adults and estimate Equation 6. Results are listed in Table 8. Increases in the number of office-based mental healthcare providers in a county, leads to a reduction in the suicide rate, suggesting that mental illness improves. In particular, 10 additional offices in a county leads to a 0.4% reduction in the suicide rate, with an implied suicide rate-treatment elasticity of -0.05.

We acknowledge that we focus on a single, and very severe, measure of mental illness: suicides. While mental illness is a much broader construct, focusing such an extreme degree of mental illness potentially allow us to recover lower bound estimates for the benefits of expanding office-based care. That is, effects on mental illness more broadly defined are plausibly larger than we are able to capture in our data. Moreover, suicide is not subject to measurement error or reporting issues, while other proxies contained in survey data (e.g.,

[18]In the public use files cells with less than 10 deaths are suppressed. We choose to use a value of five for imputation as this value is roughly the mid-value of the possible range of suppressed values; zero through nine. In unreported analysis available on request we have imputed a value of (i) zero and (ii) nine, and results are not different.

self-assessed health) are potentially vulnerable to these concerns.

4.6 Heterogeneity

Thus far in the analysis, we have examined all crimes, providers, and counties collectively. However, there are reasons to suspect that there maybe heterogeneous effects across specific crime types, physicians and non-physicians, and counties with different characteristics. We next explore several forms of heterogeneity.

First, we examine whether there is heterogeneity in the effect of local access to office-based mental healthcare providers by specific types of crimes. Tables 9 and 10 report crime-specific effects for violent and nonviolent crimes respectively. In terms of violent crimes, increases in office-based mental healthcare providers by 10 lead to a statistically significant reduction in murder (2.5%), aggravated assault (2.2%), and robbery (2.0%); the implied crime-treatment elasticities are -0.30, -0.27, and -0.24. We note that the coefficient estimate in the rape regression carries a negative sign but is statistically indistinguishable from zero. Turning to nonviolent crimes, a corresponding of increasing the number of office-based mental healthcare providers by 10 reduces burglary rates by 0.3% (-0.04) and motor vehicle theft by 1.8% (-0.22) – implied crime-treatment elasticities are reported in parentheses. The coefficient estimate in the larceny regression is negative but imprecise.

Second, we replace overall mental healthcare provider offices with two variables that separately consider offices of physicians (e.g., psychiatrists) and offices of non-physicians (e.g., psychologists, psychoanalysts, and social workers). The two providers types deliver somewhat distinct types of treatment which may have differential effects on crime. For instance, physicians are better able to prescribe medications (i.e., generally only those providers holding an MD or DO can prescribe medications) than non-physicians. Medications have known side effects (e.g., aggression, lethargy) which may *increase* the propensity to commit crime or of victimization. Non-physicians rely more on non-medication forms of treatment (e.g., cognitive behavioral therapy, social support, and family therapy). The profile of the marginal patient seeking care with a physician and non-physician may also differ, leading to differential effects on crime. Finally, although the CBP has very limited information on the number of employees working in an establishment,[19] offices of non-physicians are much larger than offices of physicians (i.e., roughly 1.7 times larger) suggesting that these offices have dif-

[19]For instance, over 70% of the employment data is imputed and there is no information on job-type for any employee, thus at the extreme we cannot distinguish between a janitor and a physician. Full details available on request.

ferential scope to treat patients. Our main findings appear to be driven by non-physicians (Table 11). Coefficient estimates on the number of physician offices, while not precise, carry *positive* signs. On the other hand, the coefficient estimates on the non-physicians are similar to our main findings. In particular, an additional 10 non-physician offices reduces total crime rates by 0.6%, violent crime rates by 2.4%, and nonviolent crime rates by 0.4%.[20] An additional 10 non-physician offices implies a 14.8% increase in supply. Thus, our implied crime-treatment elasticties are -0.04 (total), -0.16 (violent), and -0.03 (nonviolent). In the bottom panel of Table 11, we include an interaction term between the two provider types in the regression model. While the main effects are not appreciably different (the main effects for non-physicians are large but 95% confidence intervals overlap with the confidence interval associated with the comparable estimate in the non-interacted model), the interaction term is *positive* which suggests that there may be declining marginal utility for non-physicians as the number of physicians increases. We note that the interaction term coefficient estimate is not precise in the violent crime specification.

Third, we examine heterogeneity by county-level characteristics (all measured in 1998, thus prior to our study period): (i) population density proxied by the population per square mile of land area,[21] (ii) total crime rates, (iii) personal income, and (iv) number of offices of mental healthcare providers. We note that the total sample size of the sub-samples is less than our overall sample size, we lose sample because we do not rely on a balanced panel of counties. The effect of office-based mental healthcare provider offices appears to be larger in counties with lower population density; indeed coefficient estimates are more precisely estimated and larger in less dense population areas (Table 12). Table 13 reports coefficient estimates for counties with above and at/below the mean total crime rate. While the point estimates differ in absolute size, the relative effect sizes are similar. Turning to heterogeneity by average personal income (Table 14), coefficient estimates, both in absolute and relative terms, are larger among lower income counties. For instance, an additional 10 office-based mental healthcare providers reduces total crime rates by 0.29% in counties with incomes above the sample mean and 8.61% in counties with incomes at or below the sample mean. Finally, our findings appear to be larger in counties with relatively high baseline numbers of offices (Table 15). We note that while point estimates differ across some sub-samples, many

[20]We have 45,577 county/year pairs in our analysis sample. 7,375 county/year pairs have no physician offices but non-zero non-physician offices. On the other hand, 2,908 county/year pairs have zero non-physician offices but zero physician offices.

[21]Population density is measured in 2000. We could not locate this information for 1998. Details available on request.

of the 95% confidence intervals overlap, thus we are reluctant to place too much emphasize heterogeneity by county-level characteristics.

4.7 Interactions between healthcare providers

We next explore whether there are interactions between office-based mental healthcare providers and other providers operating in the same healthcare market. That is, in this section we investigate whether office-based mental healthcare acts as a complement or substitute to care delivered by other providers. This analysis is motivated, in part, by calls from both providers and payers to integrate behavioral and general healthcare to improve overall patient well-being (American Psychiatric Association, 2016; Centers for Medicare and Medicaid Services, 2019). Individuals with mental illness are at elevated risk for a range of chronic conditions (e.g., high blood pressure and tobacco product use) and are less likely to receive guideline concordant care for general healthcare needs (Newcomer & Hennekens, 2007; Chesney, Goodwin, & Fazel, 2014; Lê Cook et al., 2014; Janssen et al., 2015; McGinty, Baller, Azrin, Juliano-Bult, & Daumit, 2015). Randomized control trials suggest that integrated care models are effective in improving health (Druss et al., 2009, 2016). Further, if providers are complements in the production of mental health and, in term, crime, then there may be efficiency gains from policies that encourage agglomeration of providers.

We estimate augmented versions of Equation 6 that include interactions between the lagged number of office-based mental healthcare providers and the lagged number of (i) specialty behavioral healthcare (i.e., mental illness and SUD) facilities,[22] (ii) general physicians, and (iii) community hospitals. We construct the number of alternative healthcare providers in each county using the CPB data. Results are reported in Table 16.

Our main point estimates retain their sign and magnitude, although we lose some precision in the nonviolent crime specification. The interaction term between the number of specialty behavioral health facilities and office-based mental health providers is *positive*. This pattern of results suggests that crime control benefits that occur when the number of office-based providers increases is offset when there is a corresponding increase in specialty providers. We are unsure of the specific mechanism that drives this result, but we hypothesize that there maybe suboptimal interactions between provider types. We do not find any additional evidence of interactions across providers: the main effects for office-based mental

[22]Specialty behavioral healthcare facilities include psychiatric hospitals, residential treatment facilities, and outpatient facilities. The industry coding scheme provided in the CBP does not allow the researcher to separate mental illness and SUD treatment facilities in these settings.

health providers are stable and the interaction terms are small in magnitude and not precise.

5 Sensitivity analysis

We next report several sensitivity analysis to investigate the stability of our main findings.

First, we estimate different specifications and samples. We remove population weights, exclude state-by-year fixed-effects,[23] examine crime counts,[24] take the logarithm of crime rates, and apply an inverse hyperbolic sine to the crime rates (Appendix Table A1). We exclude counties with no office-based mental healthcare providers, exclude years in which the Affordable Care Act (ACA) was effective (2010-2014) – this Act reflects a fundamental change in the healthcare delivery system and may have altered access to mental healthcare treatment in ways we do not adequately model, and use the Quarterly Census of Employment and Wages (QCEW) to construct our establishment count (Appendix Table A2).[25]

Second, we aggregate the data to the state-year and core-based statistical area (CBSA)-year level, which implies that we are treating the state or the CBSA as the relevant healthcare market rather than the county as in our main analysis. While the county arguably provides a better proxy for 'local' access to mental healthcare treatment, we are concerned that the county may reflect a healthcare market that is too narrow to capture the market that actual patients may consider when opting to seek, or not seek, care. In particular, there are well-established mental healthcare provider shortages in the U.S. (Thomas et al., 2009). Further, if patients seek care outside their county this behavior would violate the the no interference between units assumption of two-way fixed-effects models. We replace county-level fixed-effects and state-by-year fixed-effects with state- and year-fixed-effects in state-level specification. In the CBSA-level analysis, we replace all county-level fixed-effects with CBSA equivalents. We also replace county-level demographics with comparable state-/CBSA-level variables. We also cluster standard errors around the state or CBSA as this is the level of treatment. Results are reported in Appendix Table A3. Results are robust, although less precise in the state-level analysis which we interpret as suggestive evidence

[23]We include year fixed-effects in this specification to account for secular changes over time.

[24]We control for the UCR population in this specification.

[25]We prefer CBP for our study for the following reasons. (i) We can use data beginning in 1999 in CPB vs. 2002 in QCEW (full details available on request). (ii) CBP is more commonly used in the related literature and we wish to facilitate comparison across studies. (iii) the QCEW does not include proprietors and therefore likely under-counts many of the providers that we seek to study (https://www.bls.gov/cew/overview.htm; last accessed October 1st, 2019). (iv) CBP excludes most establishments with government employees but this restriction does not pose a problem when studying office-based mental health providers as these providers are generally not employed by governments.

that the state is potentially too large a market for office-based mental healthcare.[26]

Third, we cluster standard errors in our county-level analysis at the state-level to account for within-state correlations in access to care (e.g., Medicaid coverage or state regulation of mental healthcare coverage in private insurance markets). Estimates of standard errors are not appreciably different (Appendix Table A4).

Fourth, we explore the robustness of our results to different lag structures. In Equation 6 we use a one year lag. However, it is possible that more or less time is required for changes in treatment access to lead to changes in crime outcomes. We estimate auxiliary models in which we use the contemporaneous number of offices, and two and three year lags in this variable. Results are not appreciably different from our main findings (Appendix Table A5), although coefficient estimates in the nonviolent crime specifications with longer lag structures are not statistically distinguishable from zero. We note that sample sizes decline as we incorporate more distal lags as our CBP data only allows us to isolate office-based mental healthcare providers beginning in 1998 (see Section 3.2).

Fifth, we test for asymmetry between increases (i.e., office openings) and decreases (i.e., office closings) in the number of office-based mental healthcare providers. We construct an indicator for an increase in the number of office-based mental healthcare providers in a county between *t-2* and *t-1*, and interact this variable with the number of providers in *t-1*. We include the interaction and main effects in Equation 6. Results, reported in Appendix Table A6, provide no evidence of asymmetry in effects for total and nonviolent crime rates (i.e., the interaction term is not statistically different from zero). However, in the violent crime specification, we observe evidence of asymmetry: the coefficient estimate on the interaction term is positive and precise.

Sixth, we allow for a quadratic in the number of offices in a county (Appendix Table A7). The main effect coefficient estimates retain their sign and statistical significance but the magnitude increases. In terms of the quadratic term, the point estimate is small and imprecise in the total and violent crime specifications. However, this term is positive and statistically distinguishable from zero in the nonviolent crime specification which suggests potential diminishing marginal returns at higher levels of offices for this type of crime.

Seventh, we sequentially exclude each state from the analysis, re-estimate Equation 6, and compare the coefficient estimates. The purpose of this exercise is to assess heterogeneity across states and to ensure that our findings are not being driven by the unique experiences of

[26]We note that not all areas in the U.S. are within a CBSA, with more rural localities less likely to be in a CBSA. We use a county-to-CBSA crosswalk prepared by the National Bureau of Economic Research. Full details available on request.

a small number of states. Results are reported in Appendix Figures A1 (total), A2 (violent), and A3 (nonviolent). The results appear to be relatively homogenous across U.S. states – 95% confidence intervals overlap, although we note that point estimates excluding California are generally larger in magnitude.

Finally, we conduct placebo testing. In particular, we conduct a falsification exercise to ensure that we are not erroneously attributing office-based mental healthcare provider effects to some other variable that follows the same evolution across U.S. counties (we are not aware of any such policy or factor). To do so, we randomly re-assign the office variable across counties and re-estimate our Equation 6 100 times, generating 'placebo estimates.' If we are indeed capturing a 'true' effect of access to office-based mental healthcare, and not some other unobserved factor, we would expect our main estimate to be an outlier relative to all placebo estimates. We report our placebo testing in Appendix Figures A4 (total), A5 (violent), and A6 (nonviolent). In all specifications our estimate is an outlier from the placebo estimates. We conclude from our falsification exercise that we are not capturing the effect of some other factor or policy in our main estimates.[27]

6 Discussion

In this study we provide the first evidence on the effect of office-based mental healthcare treatment local access on crime. We combine two-way fixed-effects models with government data on crime counts and the universe of office-based mental healthcare treatment providers to study this question. Our findings suggest that increases in office-based mental healthcare providers reduce crime rates. In particular, an additional 10 mental healthcare provider offices in a county leads to a 0.5% reduction in the overall county crime rate, with an implied crime-treatment access elasticity of -0.06. We provide evidence that suicide decreases, a proxy for serious mental illness, as the number of office-based mental healthcare treatment increases within a county. Our results are robust to numerous sensitivity checks.

These findings suggest that increasing access to office-based mental healthcare treatment providers have positive, albeit modest, spillover effects to crime. The magnitude is reasonable given our hypothesized complier: a patient with a relatively well-managed mental illness, to the extent that they can be effectively treated in office-based settings. However, the

[27]We also examine the effects of establishments that we would not expect to affect crime rates through the same mechanisms as office-based mental healthcare providers on our crime outcomes as a second form of placebo testing. More specifically, we examine the effects of retails sales and financial establishments on crime. We are not able to replicate our findings.

costs of crime to society are extremely high, thus even modest declines in the number of crimes may be valuable. For instance, the value of one averted murder, rape, aggregated assault, and robbery are estimated to be as high as \$10,920,120; \$292,781; \$130,135; and \$51,448 (McCollister et al., 2017).[28] Thus, policymakers seeking to reduce crime may wish to consider enhanced investments in this modality of mental healthcare, for instance, subsidies or tax credits to mental healthcare providers who open in areas with high crime rates. Because expansions of incarceration and policing services are extremely expensive policies, for instance the annual salary (not including non-wage benefits or any on-the-job training) of a police officer in 2018 was \$65,400,[29] expanding access to mental healthcare treatment may be a cost-effective policy to reduce crime – in 2018 the median salary for a mental health counselor was \$44,630.[30] This benefit of office-based mental healthcare is in addition to better mental and physical health, improved labor market outcomes, reduced general healthcare service use, and improved interpersonal relationships for patients.

Our study has limitations. We consider only one dimension of mental healthcare treatment access: the number of office-based providers in a county. Also, our estimates have an intent-to-treat (ITT) interpretation. While the ITT is arguably informative from a policy perspective, information on the treatment-on-the-treated (TOT) estimate is also of interest.

Our study adds to a small but growing literature which suggests that supportive, rather than punitive, policies are effective in reducing crime related to behavioral health conditions. Wen et al. (2017) exploit changes in state Medicaid Health Insurance Flexibility and Accountability (HIFA) waivers to non-traditional populations (low-income, childless adults) that cover SUD treatment to study the effect of SUDs on crime. The authors document that improved access to SUD treatment through waivers reduces crime, in particular robbery, aggravated assault, and larceny. Using a similar identification to the one we employ, Bondurant et al. (2018) evaluate the extent to which expanding access to specialty SUD treatment facilities, measured by opening and closings of such facilities, affect crime. The authors show that the increased numbers of SUD treatment facilities in a county reduce violent and financially motivated crimes with the county, in particular murder. Overall, these

[28] Inflated by the authors to 2019 dollars using the Consumer Price Index. We note that estimates vary across studies, for instance, Chalfin and McCrary (2018) use lower estimates for these crimes in their cost-adjustment approach that we apply in robustness checking.

[29] Estimate derived from the Bureau of Labor Statistics: https://www.bls.gov/oes/CURRENT/oes333051.htm (last accessed October 1st, 2019).

[30] Estimate derived from the Bureau of Labor Statistics: https://www.bls.gov/ooh/community-and-social-service/substance-abuse-behavioral-disorder-and-mental-health-counselors.htm (last accessed October 1st, 2019).

studies suggest that expanding the behavioral healthcare workforce can have both direct health benefits and indirect crime-control benefits to society.

Table 1: Summary statistics 1999-2014

Variable:	Mean/proportion
County crime rates per 10,000 residents	
Total crime	360.0
Violent crime	44.1
nonviolent	315.9
Murder rate	0.52
Rape rate	2.95
Aggravated assault rate	27.4
Robbery rate	13.2
Burglary rate	69.4
Larceny rate	213.4
Motor vehicle theft rate	33.1
Mental healthcare provider offices	
Physicians & non-physicians	122.0
Physicians	67.4
Non-physicians	54.7
State controls	
Governor Democrat	0.46
Officers in force (per 100,000)	246.2
County controls	
Per capita income	43673.0
Poverty rate	13.9
Unemployment rate	6.45
White	0.87
African American	0.13
Population 0-17 years	0.25
Population 18-64 years	0.63
Population 65+ years	0.12
Male	0.51
Female	0.49
Observations	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. Observations are weighted by the population covered by the UCR data.

Table 2: Effect of office-based mental healthcare providers on aggregate crime rates: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.1675***	-0.0887***	-0.0788**
	(0.0421)	(0.0077)	(0.0400)
Observations	45577	45577	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 3: Effect of office-based mental healthcare providers on aggregate crime rates: 1999-2014

Outcome:	Total crime rate
Mean	357.0
Two year lag in offices of mental healthcare providers	-0.0826
	(0.0734)
One year lag in offices of mental healthcare providers	-0.1058**
	(0.0414)
Contemporaneous offices of mental healthcare providers	-0.0327
	(0.0556)
One year lead in offices of mental healthcare providers	-0.0443
	(0.0450)
Two year lead in offices of mental healthcare providers	0.1058
	(0.0941)
Observations	43180

Notes: The data set is the combined UCRC and CBP 1999-2014. The sample size is smaller than the main sample as we lose one year of data by including leads and lags (CPB data are only available as of 1998 at the 6-digit occupation coding level). The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 4: Effect of controls on number of mental healthcare provider offices: 1999-2014

Outcome:	Offices of mental healthcare providers
Mean	122.0
State-level controls	
Governor Democrat	-6.7894
	(5.7995)
Police officers (per 100,000)	-0.0009
	(0.0086)
County-level controls	
Per capita personal income	0.0012
	(0.0009)
Poverty rate	-1.9473
	(1.2745)
Unemployment rate	1.6076
	(2.7264)
African American	-155.009
	(135.3318)
Population 65+ years	-521.4826***
	(151.4003)
Female	871.4276***
	(210.6285)
F-test of joint significance	4.31
(p-value)	(<0.0000)
Observations	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 5: Effect of office-based mental healthcare providers on aggregate crime rates using different sets of control variables: 1999-2014

Outcome	Total crime rate
Mean	0.0430
Model includes fixed-effects only	-0.2011***
	(0.0538)
Include state-level controls	
Added control variable: Governor Democrat	-0.2011***
	(0.0538)
Added control variable: Police officers (per 100,000)	-0.2011***
	(0.0538)
Include county-level controls	
Added control variable: Per capita personal income	-0.2116***
	(0.0528)
Added control variable: Poverty rate	-0.2116***
	(0.0528)
Added control variable: Unemployment rate	-0.2232***
	(0.0504)
Added control variable: African American	-0.2243***
	(0.0496)
Added control variable: Age controls	-0.1972***
	(0.0441)
Added control variable: Share female	-0.1675***
	(0.0421)
Observations	45577

Notes: Each row reflects the coefficient estimated in a regression that includes the listed variable and all variables reported in the preceding rows. The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for state characteristics, county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the county population. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 6: Effect of office-based mental healthcare providers on migration rates: 1999-2014

Outcome	Past year cross-county migration rate
Mean	0.0430
Offices of mental healthcare providers	0.0000
	(0.0000)
Observations	3953

Notes: The data set is the combined ASEC-CPS and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. The sample size is smaller than the main sample due to suppression of counties in the ASEC-CPS. All models estimated with OLS and control for state characteristics, county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the county population. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 7: Effect of office-based mental healthcare providers on aggregate cost-adjusted crime rates: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	573.4	529.52	43.9
Offices of mental healthcare providers	-1.2091***	-1.1707***	-0.0385***
	(0.1124)	(0.1127)	(0.0106)
Observations	45577	45577	45577

Notes: Crime rates are cost-adjusted following Chalfin and McCrary (2018). The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. See Section 4 for full details. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 8: Effect of office-based mental healthcare providers on suicide rates: 1999-2014

Outcome	Suicide rate per 10,000
Mean	1.69
Offices of mental healthcare providers	-0.0004***
	(0.0001)
Observations	45577

Notes: The data set is the combined NVSS and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for state characteristics, county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the county population. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 9: Effect of office-based mental healthcare providers on specific violent crime rates: 1999-2014

Outcome:	Murder	Rape	Agg. assault	Robbery
Mean	0.52	2.95	27.4	13.2
Offices of mental healthcare providers	-0.0013***	-0.0006	-0.0599***	-0.0269***
	(0.0001)	(0.0005)	(0.0057)	(0.0030)
Observations	45577	45577	45577	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 10: Effect of office-based mental healthcare providers on specific nonviolent crime rates: 1999-2014

Outcome	Burglary	Larceny	Motor vehicle theft
Mean	69.4	213.4	33.1
Offices of mental healthcare providers	-0.0193*	-0.0000	-0.0595***
	(0.0102)	(0.0264)	(0.0166)
Observations	45577	45577	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 11: Effect of office-based mental healthcare providers on aggregate crime rates, heterogeneity by mental illness provider type: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Do not interact provider types			
Offices of physicians	0.3422	0.0664	0.2758
	(0.2957)	(0.0630)	(0.2477)
Offices of non-physicians	-0.2310***	-0.1080***	-0.1230**
	(0.0560)	(0.0085)	(0.0512)
Observations	45577	45577	45577
Interact provider types			
Offices of physicians	0.2866	0.0674	0.2192
	(0.3091)	(0.0636)	(0.2608)
Offices of non-physicians	-0.5320***	-0.1021***	-0.4298***
	(0.1718)	(0.0319)	(0.1463)
Interaction	0.0006**	-0.0000	0.0006**
	(0.0003)	(0.0001)	(0.0003)
Observations	45577	45577	45577

Notes: The specification includes both offices of physicians and offices of non-physicians. In the top panel, the two provider variables are not interacted. In the bottom panel, the two provider type variables are interacted. The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 12: Effect of office-based mental healthcare providers on aggregate crime rates, interact with baseline population density: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
$>$ mean baseline population density			
Mean	396.3	52.1	344.2
Offices of mental healthcare providers	-0.0667	-0.0774***	0.0107
	(0.0611)	(0.0105)	(0.0552)
Observations	5029	5029	5029
$\leq$ mean baseline population density			
Mean	314.6	33.2	281.3
Offices of mental healthcare providers	-1.0133**	-0.0501	-0.9632**
	(0.4354)	(0.0471)	(0.4032)
Observations	31441	31441	31441

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. Baseline population densities are measured in 1998. The sample size is smaller than the main sample as the county must appear in the data in 1998. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 13: Heterogeneity in the effect of office-based mental healthcare providers on aggregate crime rates by baseline total crime rates: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
> mean baseline total crime rate			
Mean	466.0	58.8	407.2
Offices of mental healthcare providers	-0.0971	-0.0943***	-0.0028
	(0.1360)	(0.0283)	(0.1187)
Observations	12252	12252	12252
≤ mean baseline total crime rate			
Mean	62.5	15.3	55.1
Offices of mental healthcare providers	-0.1163***	-0.0340***	-0.0823**
	(0.0393)	(0.0055)	(0.0387)
Observations	24218	24218	24218

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. Baseline total crime rates are measured in 1998. The sample size is smaller than the main sample as the county must appear in the data in 1998. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 14: Heterogeneity in the effect of office-based mental healthcare providers on aggregate crime rates by baseline personal income: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
$>$ mean baseline personal income			
Mean	372.8	46.5	326.3
Offices of mental healthcare providers	-0.1069**	-0.0812***	-0.0258
	(0.0424)	(0.0088)	(0.0390)
Observations	22392	22392	22392
$\leq$ mean baseline personal income			
Mean	324.1	36.9	287.1
Offices of mental healthcare providers	-2.7889***	-0.2803**	-2.5086***
	(0.8598)	(0.1152)	(0.8172)
Observations	14078	14078	14078

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. Baseline personal income is measured in 1998. The sample size is smaller than the main sample as the county must appear in the data in 1998. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 15: Heterogeneity in the effect of office-based mental healthcare providers on aggregate crime rates by baseline number of providers: 1999-2014

> mean baseline providers			
Mean	394.2	50.9	343.4
Offices of mental healthcare providers	-0.0732	-0.0743***	0.0011
	(0.0480)	(0.0096)	(0.0430)
Observations	6940	6940	6940
≤ mean baseline MH providers			
Mean	284.8	28.7	256.1
Offices of mental healthcare providers	-0.4417	-0.1874	-0.2543
	(0.7401)	(0.1353)	(0.6736)
Observations	29530	29530	29530

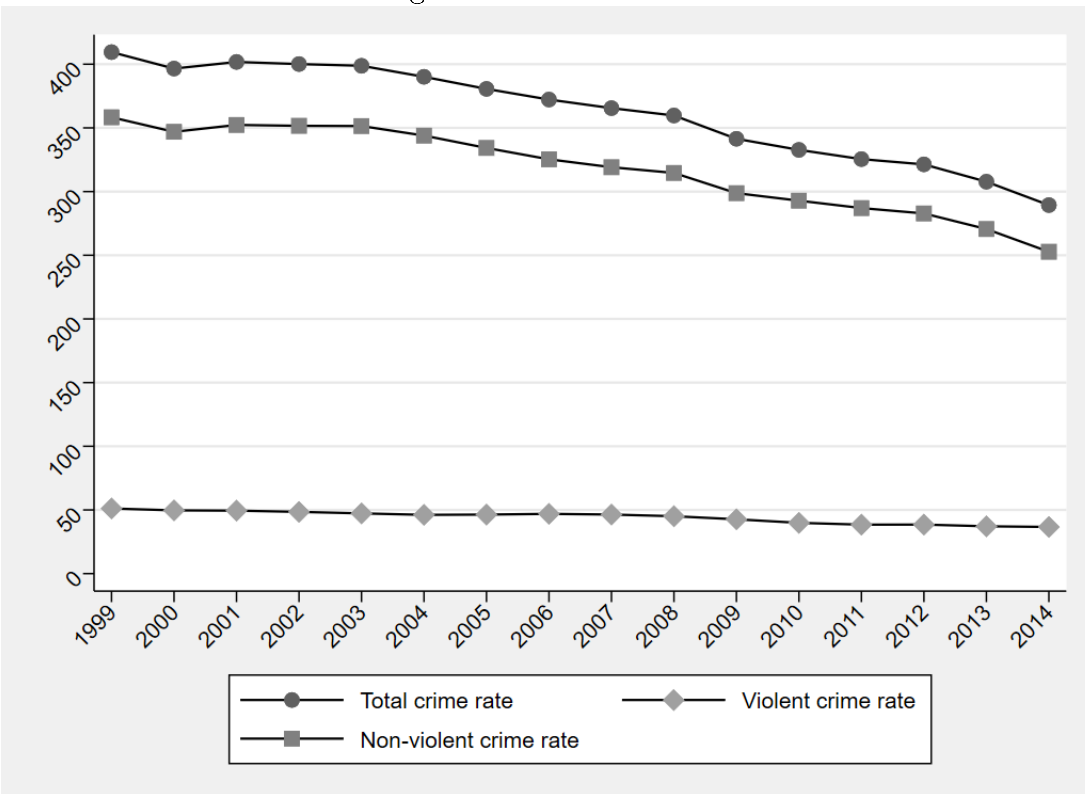
Notes: Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. Baseline mental healthcare providers are measured in 1998. The sample size is smaller than the main sample as the county must appear in the data in 1998. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCR data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 16: Effect of office-based mental healthcare providers on aggregate crime rates, interactions with other healthcare providers: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Interact with specialty providers			
Offices of mental healthcare providers	-0.1750	-0.0439**	-0.1311
	(0.1132)	(0.0215)	(0.0984)
*specialty providers	0.0003**	-0.0000	0.0003**
	(0.0002)	(0.0000)	(0.0001)
Interact with general physicians			
Offices of mental healthcare providers	-0.2134*	-0.0441*	-0.1693*
	(0.1177)	(0.0256)	(0.1012)
*general physicians	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Interact with community hospitals			
Offices of mental healthcare providers	-0.1174	-0.0876***	-0.0298
	(0.0931)	(0.0189)	(0.0821)
*community hospitals	0.0001	0.0002	-0.0001
	(0.0010)	(0.0002)	(0.0009)
Observations	45577	45577	45577

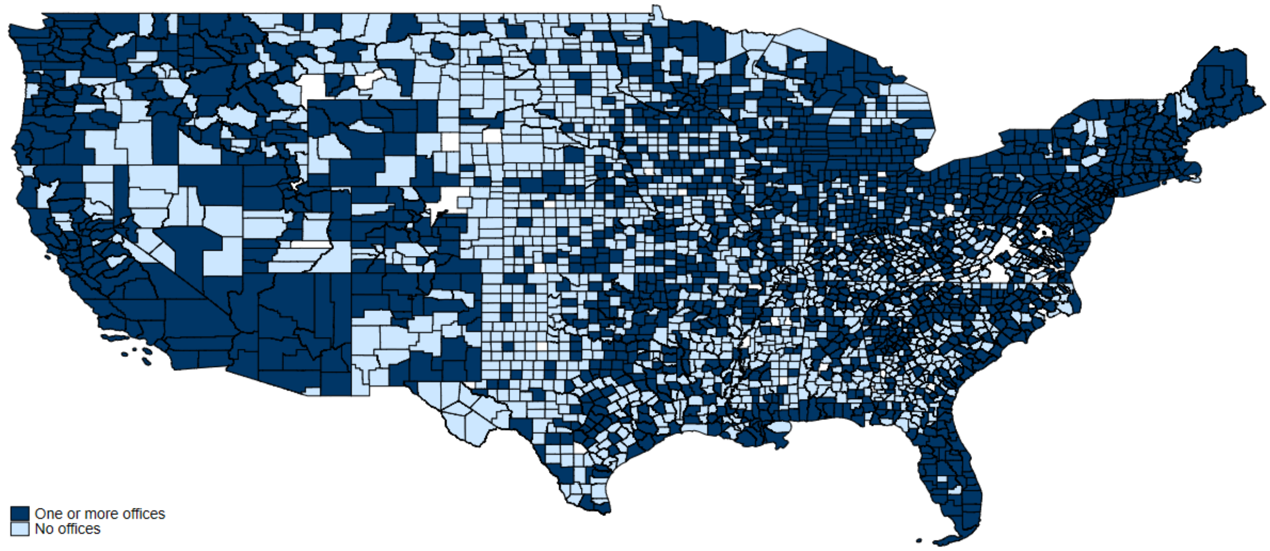
Notes: The data set is the combined UCRC and CBP 1999-2014. Each specification controls for the main effect of the alternative healthcare provider (i.e., specialty providers, general physicians, and hospitals). Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Figure 1: Trends in total crime rate



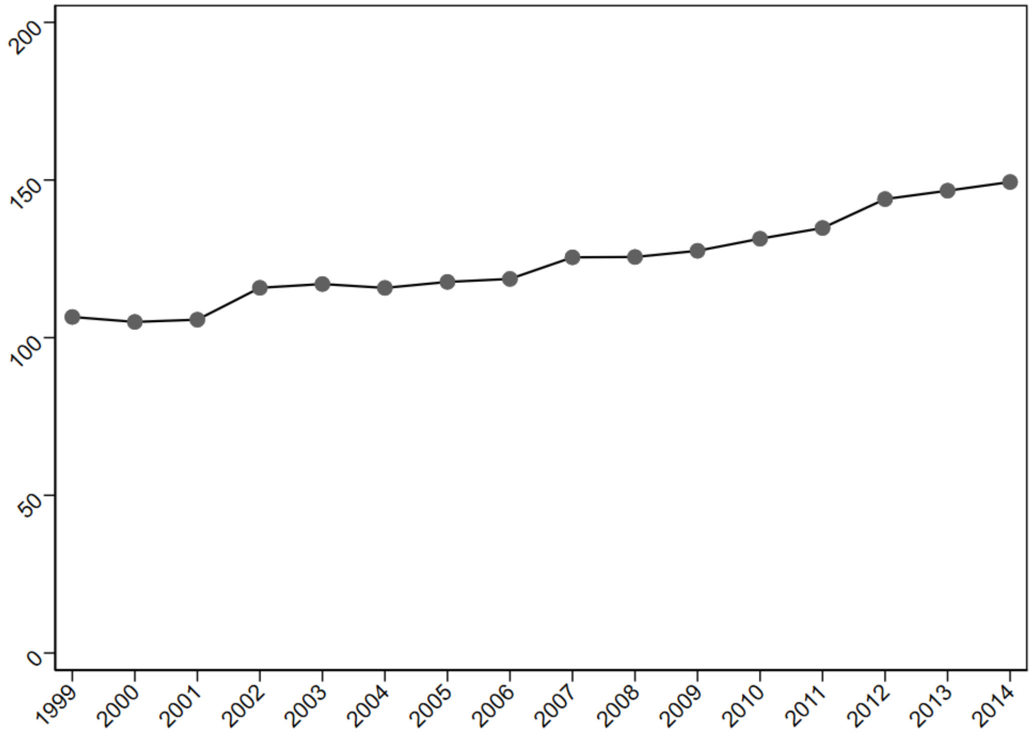
Notes: The data set is the UCRC 1999-2014. Data are aggregated to the year level using population weights. The sample mean value is 360.0 total crimes per 10,000 residents.

Figure 2: U.S. counties with at least one office-based mental healthcare provider in 2014



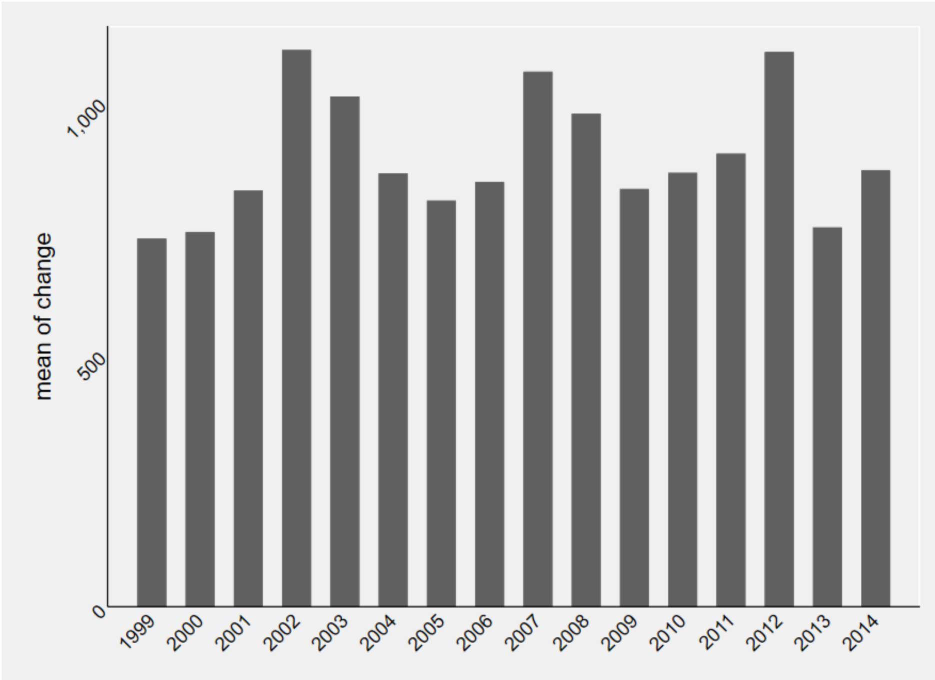
Notes: The data set is the CBP 2014. Counties in white are not included in our analysis sample. See 3.1.

Figure 3: Trends in total providers



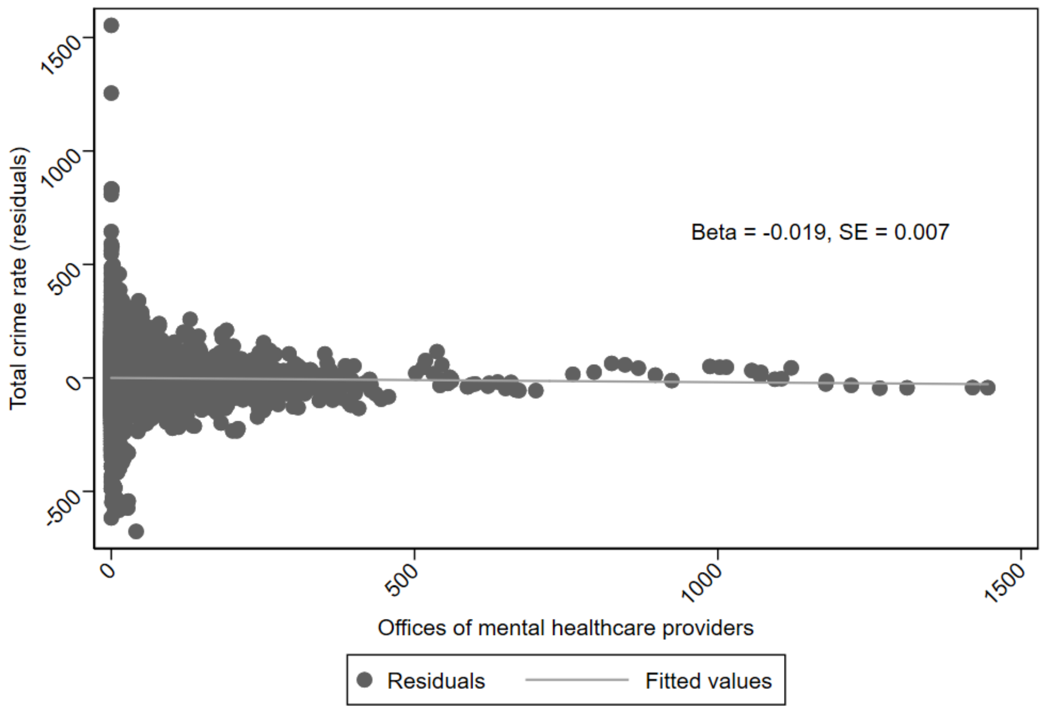
Notes: The data set is the CBP 1999-2014. Data are aggregated to the year level using population weights. The sample mean value is 122.0 offices.

Figure 4: Year-to-year changes in office-based mental healthcare providers



Notes: The data set is the combined UCRC and CBP 1999-2014. The vertical bars indicate the (unweighted) number of counties that experienced a change in the number of office-based mental healthcare providers between year *t* and *t-1*.

Figure 5: Scatter plot of residulized total crime rates and office-based mental healthcare providers



Notes: The data set is the combined UCRC and CBP 1999-2014. Year and county effects are removed from the total crime rates. The sample mean value is 360.0 total crimes per 10,000 residents.

Table A1: Effect of office-based mental healthcare providers on aggregate crime rates, using different specifications: 1999-2014

Outcome	Total	Total violent	Total nonviolent
Unweighted			
Mean	244.9	26.1	218.7
Offices of mental healthcare providers	-0.6687***	-0.1195***	-0.5492***
	(0.1785)	(0.0180)	(0.1704)
Observations	45577	45577	45577
Drop state-by-year fixed-effects			
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.0659	-0.0678***	0.0019
	(0.1159)	(0.0132)	(0.1056)
Observations	45577	45577	45577
Crime counts, control for population			
Mean	43037.4	6673.2	36364.2
Offices of mental healthcare providers	-232.5547***	-88.0993***	-144.4554***
	(38.9561)	(13.5962)	(25.8654)
Observations	45577	45577	45577
Logarithm of crime rate			
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.0012**	-0.0019***	-0.0010*
	(0.0005)	(0.0004)	(0.0005)
Observations	45577	45577	45577
Inverse hyperbolic sine of crime rate			
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.0008***	-0.0017***	-0.0006*
	(0.0003)	(0.0003)	(0.0003)
Observations	45577	45577	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects unless otherwise noted. Observations are weighted by the population covered by the UCRC data unless otherwise noted. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A2: Effect of office-based mental healthcare providers on aggregate crime rates, using different samples: 1999-2014

Outcome	Total	Total violent	Total nonviolent
Exclude counties with no offices			
Mean	241.0	26.8	214.2
Offices of mental healthcare providers	-0.1406***	-0.0844***	-0.0562
	(0.0417)	(0.0081)	(0.0390)
Observations	23919	23919	23919
Exclude ACA years (2010-2014)			
Mean	382.4	47.1	335.3
Offices of mental healthcare providers	-0.2807***	-0.1297***	-0.1510***
	(0.0637)	(0.0197)	(0.0577)
Observations	30523	30523	30523
Use QCEW 2002-2014			
Mean	351.3	42.9	308.4
Offices of mental healthcare providers	-0.1228	-0.0559***	-0.0669
	(0.1317)	(0.0210)	(0.1171)
Observations	37771	37771	37771

Notes: The data set is the combined UCRC and CBP 1999-2014 unless otherwise noted. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects unless otherwise noted. Observations are weighted by the population covered by the UCRC data unless otherwise noted. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A3: Effect of office-based mental healthcare providers on aggregate crime rates, aggregate to the state and CBSA: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Aggregate to the state			
Mean	271.1	30.7	240.5
Offices of mental healthcare providers	-0.0432	-0.0160***	-0.0272
	(0.0278)	(0.0032)	(0.0251)
Observations	800	800	800
Aggregate to the CBSA			
Mean	335.4	37.4	298.0
Offices of mental healthcare providers	-0.8960**	-0.0978***	-0.7982**
	(0.3540)	(0.0248)	(0.3454)
Observations	14111	14111	14111

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a state or CBSA in a year. State-level models estimated with OLS and control for state characteristics, year fixed-effects, and state fixed-effects. CBSA-level models estimated with OLS and control for CBSA characteristics, state-by-year fixed-effects, and CBSA fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the state/CBSA level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A4: Effect of office-based mental healthcare providers on aggregate crime rates clustering at the state-level: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.1675***	-0.0887***	-0.0788**
	(0.0355)	(0.0050)	(0.0348)
Observations	45577	45577	45577

Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the state level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A5: Effect of office-based mental healthcare providers on aggregate crime rates, using alternative lag structures: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Contemporaneous			
Offices of mental healthcare providers	-0.1570***	-0.0838***	-0.0732**
	(0.0392)	(0.0070)	(0.0370)
Observations	45577	45577	45577
Two year lag			
Mean	357.0	43.7	313.3
Offices of mental healthcare providers	-0.1516***	-0.0881***	-0.0635
	(0.0451)	(0.0095)	(0.0428)
Observations	43180	43180	43180
Three year lag			
Mean	354.2	43.3	311.0
Offices of mental healthcare providers	-0.1569***	-0.0947***	-0.0622
	(0.0529)	(0.0136)	(0.0506)
Observations	40505	40505	40505

Notes: The data set is the combined UCRC and CBP 1999-2014. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A6: Effect of office-based mental healthcare providers on aggregate crime rates, interact with increases in mental healthcare provider offices: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.1551***	-0.0901***	-0.0650
	(0.0503)	(0.0112)	(0.0464)
Offices of mental healthcare providers	0.0061	0.0030**	0.0031
*increase	(0.0058)	(0.0014)	(0.0049)
Observations	43180	43180	43180

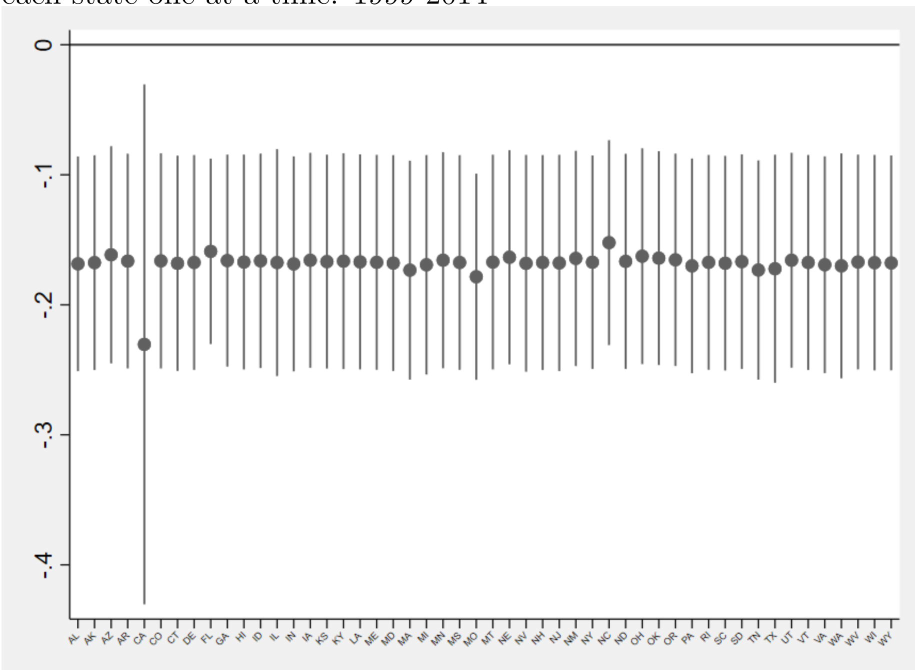
Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The sample is smaller as we lose a year od data when construct the variable indicating whether there was an increase in providers. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Standard errors are clustered at the county level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A7: Effect of office-based mental healthcare providers on aggregate crime rates allowing for a quadratic in offices: 1999-2014

Outcome:	Total	Total violent	Total nonviolent
Mean	360.0	44.1	315.9
Offices of mental healthcare providers	-0.3407**	-0.0710**	-0.2697**
	(0.1613)	(0.0318)	(0.1372)
Providers squared	0.0001	-0.0000	0.0001*
	(0.0001)	(0.0000)	(0.0001)
Observations	45577	45577	45577

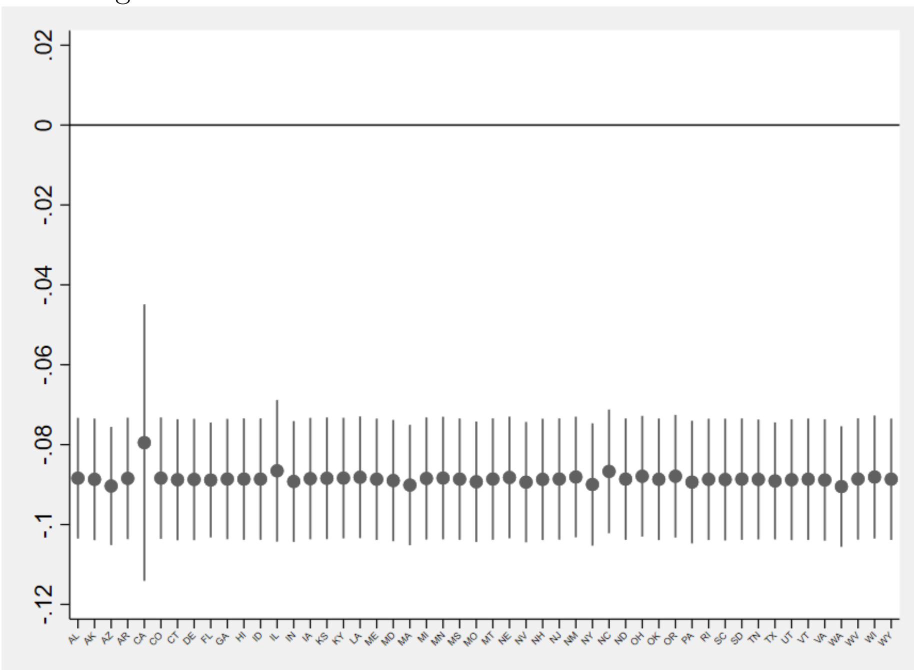
Notes: Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCR data. Standard errors are clustered at the county level. 95% confidence interval is reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Figure A1: Effect of office-based mental illness providers on aggregate total crime, excluding each state one at a time: 1999-2014



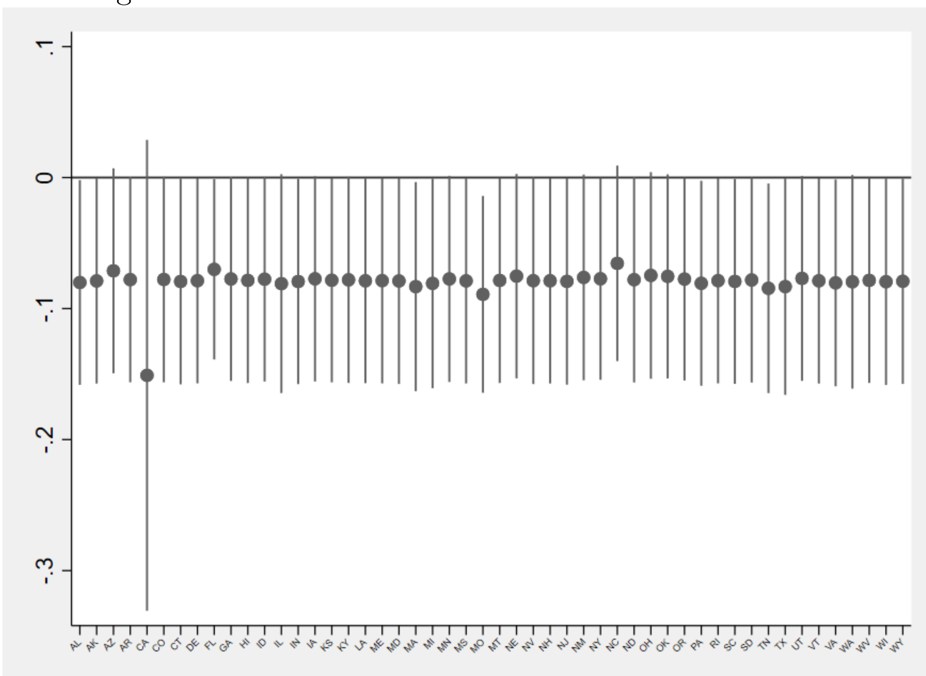
Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 360.0 total crimes per 10,000 residents.

Figure A2: Effect of office-based mental illness providers on aggregate total violent crime, excluding each state one at a time: 1999-2014



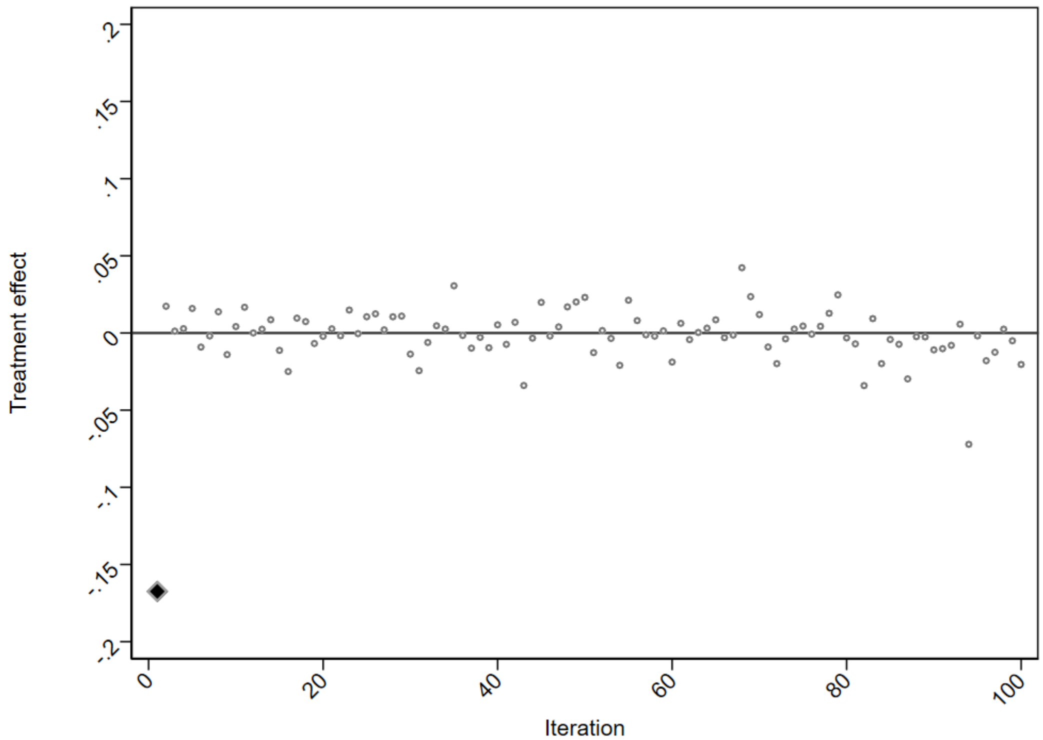
Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 44.1 violent crimes per 10,000 residents.

Figure A3: Effect of office-based mental illness providers on aggregate total nonviolent crime, excluding each state one at a time: 1999-2014



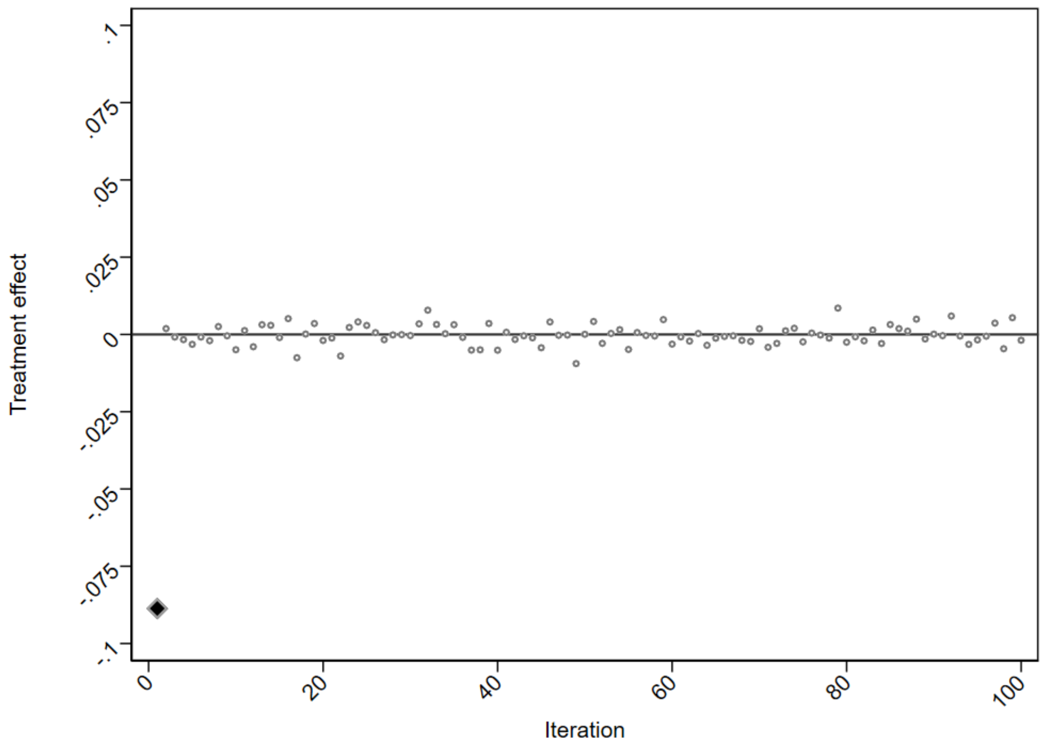
Notes: The data set is the combined UCRC and CBP 1999-2014. Office-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 315.9 nonviolent crimes per 10,000 residents.

Figure A4: Placebo testing results for total crime



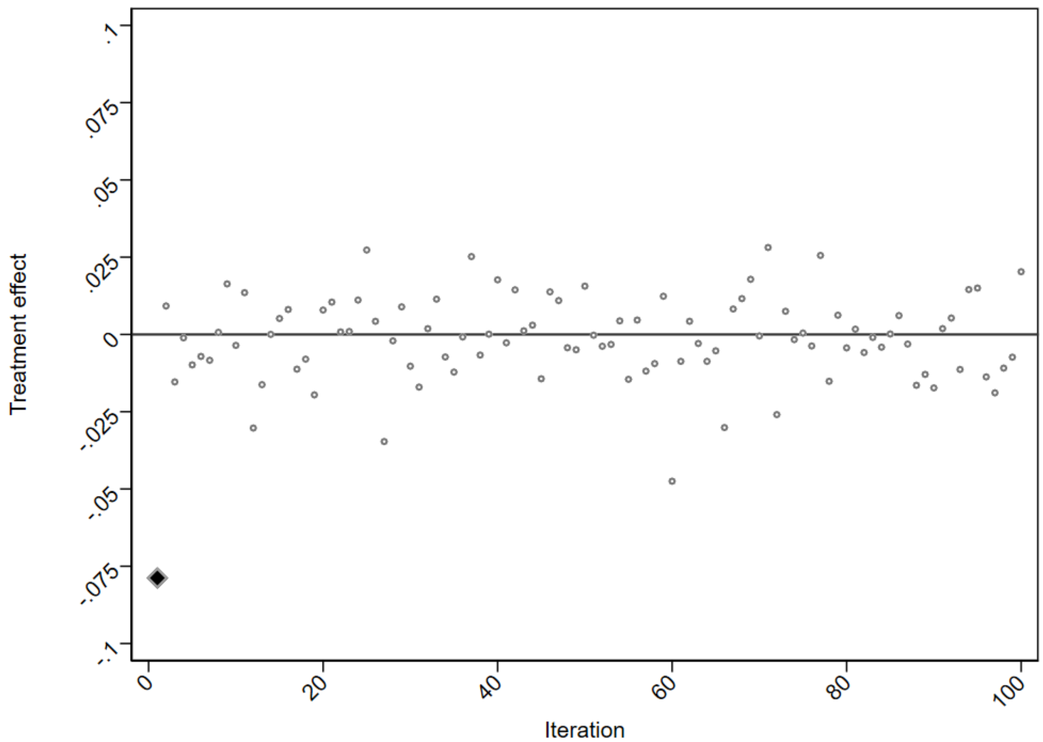
Notes: The data set is the combined UCRC and CBP 1999-2014. The large diamond is the coefficient estimate from the main two-way fixed-effects model. Circles represent placebo estimates in which we randomly re-assign state MMLs across states. ffice-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 360.0 total crimes per 10,000 residents.

Figure A5: Placebo testing results for total violent crime



Notes: The data set is the combined UCRC and CBP 1999-2014. The large diamond is the coefficient estimate from the main two-way fixed-effects model. Circles represent placebo estimates in which we randomly re-assign state MMLs across states. ffice-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 44.1 violent crimes per 10,000 residents.

Figure A6: Placebo testing results for total nonviolent crime



Notes: The data set is the combined UCRC and CBP 1999-2014. The large diamond is the coefficient estimate from the main two-way fixed-effects model. Circles represent placebo estimates in which we randomly re-assign state MMLs across states. ffice-based mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year. All models estimated with OLS and control for county characteristics, state characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by the population covered by the UCRC data. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals that account for within-county clustering are reported with vertical solid lines. The sample mean value is 315.9 nonviolent crimes per 10,000 residents.

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