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BEHAVIORAL HEALTH TREATMENT AND POLICE OFFICER SAFETY

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ABSTRACT

We study the effect of community access to behavioral health (mental health and substance use disorders) treatment on police officer safety, which we proxy with on-duty assaults on officers. Combining agency-level data on police officer on-duty assaults and county-level data on the number of treatment centers within the community that offer behavioral health treatment, we estimate two-way fixed-effects regressions and find that that an additional four centers per county (average increase) leads to a 1.3% reduction per police agency in on-duty assaults against police officers. Previously established benefits of access to treatment on behavioral health extend to the work environment of police officers.

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1 Introduction

Law enforcement is ranked as one of the most dangerous careers in the United States (Industrial Safety & Hygiene News, 2020) due to the inherently precarious nature of police work. In 2019, 12.8% of police officers were assaulted by civilians in the line of duty,[1] and 3.6% of assaulted officers sustained injuries from the assault (Federal Bureau of Investigation, 2020).[2] Moreover, on-duty assaults against police officers by civilians are on rising, which can be highly consequential for officers themselves and the communities they serve. For example, Figure 1 Panel A, based on Federal Bureau of Investigation (FBI) data, demonstrates an upward trend in on-duty assaults experienced by police officers beginning in 2015.

This study examines the effect of expanding access to behavioral health disorder (BHD) treatment within the community on police officer safety. All community members can use this treatment, that is we do not examine treatment exclusively targeting specific populations (e.g., incarcerated populations, those at risk for crime involvement, or police officers). In the U.S., police officers often serve as first responders to persons experiencing BHD crises.[3] BHDs include mental health disorders (MHDs) and substance use disorders (SUDs). Persons experiencing BHD crises require specialized handling that is quite different from standard police practices and training (Compton et al., 2014; Rohrer, 2021).[4] BHDs increase the risk for crime commission and victimization Hiday et al. (1999); Swanson et al. (2001); Douglas et al. (2009); Frank and McGuire (2010); Witt et al. (2013); Coid et al. (2013); Department of Health & Human Services (2022). These factors suggest that better management of BHDs within the population, through increased treatment access and utilization, can potentially have positive spillovers to police officer on-duty safety. We test for such spillovers.

Increased risk of on-duty violence against officers may exacerbate ongoing concerns and

[1]Firearms are often involved in such assaults: among a sample of 102 police officers experiencing on-duty assaults in 2017, 77 of the assaults involved a firearm (Federal Bureau of Investigation, 2018)

[2]These estimates are based on 9,457 law enforcement agencies that employed 475,848 officers (serving 67% of the nation's population) and provided data to the Federal Bureau of Investigation (FBI). Our study examines assaulted officers who sustained injuries from the assault.

[3]Between 21% to 38% of 911 calls are potentially related to BHD crises (Center for American Progress, 2020), and roughly 50% of those incarcerated have an MHD (James and Glaze, 2006). In response to the BHD crises, numerous communities across the U.S. are experimenting with either pairing police officers with BHD healthcare professionals or replacing police officers with such professionals for first responses (Fialk, 2022; Dee and Pyne, 2022; Leys and Zionts, 2022). At the federal level, the Bureau of Justice Assistance supports police-BHD healthcare professional collaboration (Bureau of Justice Assistance, ND).

[4]Standard police training does not emphasize skills to effectively interact with civilians experiencing BHD crises. For example, a 2015 Police Executive Research Forum survey found that new recruits received roughly 50 hours on weapons and defensive tactics, but just eight hours on crisis interventions (Police Executive Research Forum, 2015). Further emphasizing this mis-match, in 2021, the International Association of Chiefs of Police stated 'Police are often the only ones left to call to situations where a social worker or mental health professional would have been more appropriate and safer for all involved (Fialk, 2022).'

staffing challenges faced by numerous police departments across the U.S, which in turn can impede the effectiveness of police officers (e.g., increased response time, unmanned districts, and limited backup teams (Rebik and Ong, 2022)) and tactics in fighting crime.[5] While police departments have resorted to unconventional (and, in some cases, costly) recruitment techniques such as offering bonuses to new recruits, student loan repayment, hiring civilians, and relaxing candidate disqualifications (International Association of Chiefs of Police, 2019; Smith, 2022a), reducing on-the-job violence could mitigate the staffing crisis and associated complications for policing by improving recruiting and retention and deterring early retirement and resignation. The consequences of decreasing the risk of on-duty violence against officers likely extend beyond staffing issues, and have fundamental and damaging impacts on public safety. On-duty assault risk may raise the probability of police officers using excessive force (Stoughton, 2014a,b; Cho et al., 2021; Holz et al., 2020) as officers often list fear as the reason for deploying potentially excessive tactics (e.g., shooting or TASER use) (Sierra-Arevalo, 2019). Violent interactions between officers and civilians negatively impact the perception of, and trust in, police,[6] which in turn can discourage police-civilian cooperation (Bennett and Wiegand, 1994; Gottfredson and Gottfredson, 1988; Carr et al., 2007; Goudriaan et al., 2004; Tankebe, 2013; Tyler and Fagan, 2008), decrease incident reporting to police (Kochel, 2016; Kochel et al., 2013; Tankebe, 2009; Bennett and Wiegand, 1994; Khondaker et al., 2017; Kwak et al., 2019; Tankebe, 2013; Wolfe et al., 2016), increase the risk of future officer-civilian encounters escalating to violence (Gau and Brunson, 2010), decrease officer morale,[7] and overall reduce effective policing. For arrestees, assaults on officers may increase incarceration time and other penalties.

Given the central role that police play in promoting public safety,[8] reducing the risk of on-

[5]Police association surveys reveal a personnel crisis: 78% of police departments report difficulties in recruitment and 18% report an increase in resignations, and vacancy rates are 7% (International Association of Chiefs of Police, 2019; Police Executive Research Forum, 2021). Several large cities (e.g., Baltimore, Maryland; Chicago, Illinois; New Orleans, Louisiana; New York City, New York; and Philadelphia, Pennsylvania (Smith, 2022b; Rebik and Ong, 2022; MacDonald, 2023; Cotton, 2023b,a)) report severe staffing shortages. Summarizing the staffing challenges in Baltimore − which had 600 vacancies and a police force of 2,150 in 2022 − Chief District Judge James K. Bredar stated 'I can't overstate the seriousness of the staffing issue.'

[6]Mistrust promotes an environment conducive to violence against law enforcement officers (MacDonald, 2016). In 2020, U.S. law enforcement faced the lowest level of public support in nearly thirty years with only 48% of Americans stating they had confidence in the police, which is 16 percentage points below the peak confidence rate of 64% in 2004 (Brenan, 2020).

[7]A common reason for police officer burnout and leaving the profession is the stressful and dangerous nature of police work (Violanti and Aron, 1993; Vuorensyrjä and Mälkiä, 2011; McCarty and Skogan, 2013).

[8]A line of economic research provides compelling empirical evidence establishing the importance of police in preventing crime (Sherman and Weisburd, 1995; Levitt, 1997; McCrary, 2002; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Evans and Owens, 2007; Gould and Stecklov, 2009; Draca et al., 2011; Braga et al., 2014; MacDonald et al., 2016; Chalfin and McCrary, 2018; Mello, 2019; Weisburst, 2019; Weisburd, 2021). For example, a recent study using granular data from Dallas, Texas, shows that a 10% decrease in police presence leads to a 7% increase in crime (Weisburd, 2021).

duty violence faced by officers is a first-order concern. Surprisingly, there is scant economic literature regarding potential interventions to decrease violence against police officers. To date, studies have been limited to relatively controversial interventions such as police militarization (i.e., use of military-style weapons by police) with decidedly mixed results (Bove and Gavrilova, 2017; Harris et al., 2017; Mummolo, 2018; Masera, 2021), costly investments such as increasing police force size with results indicating that larger forces reduce the likelihood of an on-duty assault (Chalfin et al., 2022),[9] and determinants that are not easily malleable by public policy such as temperature with Annan-Phan and Ba (2020) demonstrating that higher temperatures increase the risk of on-duty officer assaults by civilians.

To study the effect of expanding access to BHD treatment on police officer safety, we combine administrative data on officer on-duty assaults and the number of treatment centers 1999 to 2020 with two-way fixed-effects regressions. We have several findings. First, we document that increased access to BHD treatment increases treatment uptake, improves BHD management, and reduces crime. Second, our findings imply that an additional four centers per county (the average annual county-level increase observed in our sample) leads to a 1.3% reduction per police agency in on-duty assaults against police officers. Third, improved access to BHD treatment reduces the probability that, conditional on crime occurring, interactions between police and civilians escalate to violence against officers, as measured by the number of officers' on-duty assaults and killings.[10] Fourth, we demonstrate heterogeneity across community characteristics and public policies that reduce the time and financial costs of accessing BHD treatment. Finally, we document that expanding access to BHD treatment may reduce civilian deaths by police officers. Our findings are robust to an extensive set of sensitivity analyses and robustness checks.

The paper is structured as follows. Section 2 provides background details on BHDs and associated care, and a discussion of how access to care may impact police safety. Data and methods are described in Section 3. We present our main results in Section 4, and offer a discussion and conclusion in Section 5.

[9]In particular, Chalfin et al. (2022) finds that each office hired leads to 0.14 to 0.23 fewer officer assaults, and this effect is partially driven by faster speed to assist fellow officers in distress as well as more officers patrolling in teams. See Table A14 in that paper.

[10]We refer to these outcomes as officer 'assaults' as there are very few officer deaths (see Section 3.4).

2 Background

2.1 Behavioral health disorders and treatment

As described in Section 1, we focus on treatment for BHDs, which include both MHDs and SUDs. MHDs are defined as '...conditions involving changes in thinking, emotion, or behavior' by the American Psychiatric Association (2018). This organization classifies SUDs as conditions that occur '...when the recurrent use of alcohol and/or drugs causes clinically and functionally significant impairment, such as health problems, disability, and failure to meet major responsibilities at work, school, or home' (American Psychiatric Association, 2013). These conditions are common in the U.S.: 19.1% and 7.4% of adults met the diagnostic criteria for a MHD and a SUD, or 47.6M and 20.3M Americans respectively in 2020 (Substance Abuse and Mental Health Services Administration, 2021). These conditions impact the affected individual and society: in 2022, MHDs and SUDs cost the U.S. over $1T in healthcare expenditures, disability payments, crime and violence, a less productive workforce, and so forth (Insel, 2008; Caulkins et al., 2014).[11]

An individual with an untreated BHD can receive care in a range of clinical settings: private clinician offices (e.g., psychiatrists, social workers, psychiatric nurse practitioners, or psychologists), specialized centers (outpatient or residential), crisis centers, or hospitals (specialty behavioral health units in community hospitals, psychiatric hospitals, and 'scatterbeds' in community hospitals[12]). Primary care providers can also deliver some modalities of treatment (e.g., medication). See Deza et al. (2022b) for more details.

Treatment is generally effective in improving BHDs and reducing associated behaviors that impose costs on society (e.g., crime) (Lu and McGuire, 2002; American Psychiatric Association, 2006; Hunot et al., 2007; Scott et al., 2007; Gaynes et al., 2009; Cuijpers et al., 2011; Faghri et al., 2010; Murphy and Polsky, 2016; Olfson, 2016; Kisely et al., 2017; Krebs et al., 2018; National Institute on Drug Abuse, 2018; Jetty et al., 2021), but there is substantial unmet need for care. For example, in 2020 less than half of Americans who could benefit from mental healthcare received any treatment, and just one-tenth of Americans with a SUD received any care (Substance Abuse and Mental Health Services Administration, 2021). While there are myriad reasons for needing, but not receiving, care – including ability to pay for care, locating a provider is a commonly stated barrier (Ali et al., 2017). Difficulty in locating treatment is not surprising given BHD workforce challenges: 77% of

[11]The original estimates are inflated by the authors to 2022 dollars using the Consumer Price Index.

[12]Scatterbed is a term used for patients treated in community hospitals that do not have a specialized unit for BHD patients, instead such patients are treated with the general patient population.

U.S. counties have a shortage of mental healthcare providers (Thomas et al., 2009).[13]

Appendix Table A1 provides respondent demographics of the 2020 National Survey on Drug Use and Health (NSDUH) stratified by past-year receipt of care in a BHD treatment center. Respondents who report receiving treatment in the past year are observably less advantaged. For example, those with past-year treatment are more likely to live below the poverty line (30% vs. 15%), receive government assistance (39% vs. 18%), and use illicit drugs (51% vs. 20%) than other respondents, and are less likely to report very good or excellent self-reported health (68% vs. 75%).

Our study focuses on treatment received in specialized outpatient and residential BHD treatment centers. These centers reflect 16% of total spending on MHD treatment ($38.1B) and 37% of total spending on SUD treatment ($15.5B) in 2020 (Substance Abuse and Mental Health Services Administration, 2014).[14] In 2021, 2.9M Americans received at least one episode of care in these settings reflecting 54% of formal care delivered (Center for Behavioral Health Statistics and Quality, 2022).[15] While these modalities do not capture all treatment modalities available to patients seeking care, they do capture modalities that are effective, important in terms of costs, and part of the continuum of care supported by behavioral health treatment experts (Mee-Lee et al., 2013). Further, they capture a setting in which patients with particularly severe disorders are likely to receive treatment as many primary care providers do not offer the services required for such patients (Ramanuj et al., 2019). Appendix Table A2 reports services offered by standalone MHD treatment centers in the 2020 National Mental Health Services Survey (N-MHSS).[16] A feature of BHD treatment, which is somewhat different from general healthcare, is the provision of 'wrap around' services (Pringle et al., 2002) such as housing services (22%), psycho-social rehabilitation (41%), vocational rehabilitation (15%), employment services (15%), and legal advocacy services (4%). Wrap-around services can assist patients in re-integrating into society as they receive treatment for their BHDs (Evans et al., 2020).

2.2 Behavioral health disorders and police

Police officers often serve as first responders to individuals experiencing BHD crises as a consequence of an under-resourced and fragmented BHD healthcare delivery system. Many

[13] Defined as less than 50% of county-level need met.

[14] In 2020, the U.S. spent $42B on SUD treatment and $238B on MHD treatment (Substance Abuse and Mental Health Services Administration, 2014).

[15] Formal care is defined as care not received in jail/prison, an emergency department, or a self-help group.

[16] These data are collected from specialized MHD treatment centers known to SAMHSA. We exclude centers that do not list MHD or SUD treatment provision as the primary focus (centers often provide both MHD and SUD treatment), and centers located in hospitals. We make these exclusions to focus on centers similar to those we evaluate in our study. We also exclude centers with missing variable information.

individuals with BHDs do not receive adequate, or any, care and police officers fill a critical treatment gap (Green, 1997; Lamb et al., 2002; Wood and Watson, 2017). Indeed, each year, people experiencing a mental health crisis are more likely to be jailed than to receive treatment for their condition (Butler and Sheriff, 2020). Healthcare scholars suggest 'de-institutionalization' that occurred in the 1950s and 1960s created the current reliance on the police for managing many persons with BHDs (Lamb et al., 2002). De-institutionalization led to a tremendous reduction in the number of BHD hospitals and the number of persons with serious BHDs institutionalized − in 1555 there were 39 per 100,000 Americans receiving care in an institution and just 22 per 100,000 in 2000, a 94% decline Lamb and Weinberger (2005). The movement was spurred by the development of new and effective medications to treat BHDs, the high cost to government of institutionalizing large numbers of Americans, concerns about inhumane treatment in some BHD institutions, and a perception that individuals should be integrated into the community rather than institutionalized. However, a key component of de-institutionalization was development of a robust outpatient-based community healthcare system where individuals could receive care outside of institutions. This system was not adequately supported by governments, leaving individuals without sufficient options for treatment, particularly individuals with severe BHDs.

One potential solution to improve BHD management within the population and reduce reliance on police officers as first responders to crises, and reduce both crime and victimization levels, is to expand access to healthcare for these conditions. Previous research shows that expanding access increases BHD treatment uptake and improves management of BHDs (Swensen, 2015; Wen et al., 2017; Bondurant et al., 2018; Messel et al., 2023; Bradford and Maclean, 2022; Corredor-Waldron and Currie, 2022).[17]

Improved management of BHDs within the population can reduce the probability of on-duty officers assaults through several channels: fewer BHD crises to which police respond, less crime as BHDs increase the risk of crime commission and victimization (Swanson et al., 2001; Douglas et al., 2009; Frank and McGuire, 2010; Witt et al., 2013; Coid et al., 2013; Department of Health & Human Services, 2022),[18] and lower the likelihood that a police-

[17]Increases in treatment could be driven by several factors: there will be more treatment 'slots' available which can allow patients to take up treatment, reduced prices through increased competition, and reduced travel times (which can be non-trivial for patients (Harris-Taylor, 2022; Drake et al., 2020)). Expanding access may allow incumbent patients (i.e., those receiving treatment) to locate a provider that is a better match. While patient-provider match quality is a complex phenomenon, match quality has been identified as important for effective BHD care (Meredith et al., 2001; Kantrowitz, 2016). Reduced wait times for care that may occur when there are more providers can also improve outcomes, as delays in receiving behavioral health treatment worsen the underlying condition (Penttilä et al., 2014; Reichert and Jacobs, 2018).

[18]50% of those incarcerated in U.S. jails and prisons have a MHD (James and Glaze, 2006) and the number of individuals with a MHD housed in large U.S. incarceration facilities (Los Angeles County Jail, Cook County Jail, and Riker's Island) exceeds the number of individuals in any BHD institution in the

civilian interaction escalates to an altercation, potentially due to BHD symptoms being perceived as threatening (Morabito and Socia, 2015) combined with limited training of police on how to handle persons experiencing a BHD crisis.[19] [20]

3 Data and methods

3.1 Data on officer assaults

We use data from the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) series for the period 1999 to 2020,[21] but due to data limitations (described later in this section) our main analysis sample includes the years 1999 to 2017.[22] We access these data using the concatenated LEOKA files (Kaplan, 2020a) available from Inter-university Consortium for Political and Social Research. The LEOKA series is a subprogram of the FBI's Uniform Crime Reporting (UCR) Program that provides counts of officers' deaths and non-fatal assaults by civilians that occurred in the line of duty. An on-duty assault occurs when the officer reacts to a situation that would ordinarily fall within the scope of official duties, in an official capacity, as a member of a law enforcement agency, regardless of whether the officer is on- or off-duty at the time of the incident. LEOKA exclude officers' deaths resulting from natural causes, suicide, or on-duty deaths that are attributed to a personal situation such as domestic violence (FBI, ND). LEOKA also include information on the employment of police officers and civilians for all contributing law enforcement agencies. The purpose of LEOKA is to allow exploration of the factors that lead to officer fatalities and assaults, as opposed to merely describing circumstances of the incident, with the ultimate goal to allow law enforcement agencies to build programs and systems for incorporation into officer

country (Frank and McGuire, 2010). Those with BHDs are substantially more likely to be crime victims. For example, individuals with severe MHDs are more than three to 140 times more likely to be victims of violent crime than the general population (Hiday et al., 1999). Finally, the 'criminalization' of BHDs in which, regardless of actual behavior, those with such disorders are more likely to be arrested and incarcerated than individuals without such a disorder (Dvoskin et al., 2020).

[19] There may be additional benefits to reduced substance use. Some patients respond violently to medication used to reverse drug overdoses (e.g., Naloxone). Clinical evidence suggests that such responses can lead to persons assisting the overdosing individual to be assaulted (Gaddis and Watson, 1992).

[20] For mechanisms described here, the improvement in BHD management could be driven by changes for civilians, police officers, or other persons involved in the incident (e.g., persons reporting the incident). We will assess overall changes in population-level BHDs as treatment access varies in Section 4, but we are not able to separately isolate which group(s) benefit from better access to treatment in our analysis.

[21] We do not use data beyond 2020 in our analysis due to a change in the LEOKA data collection. 2020 is the last year LEOKA are available under the UCR reporting system. Beginning in 2021, LEOKA are collected under the FBI's National Incident-Based Reporting System (NIBRS). Fewer agencies report to NIBRS than report to UCR, thus we cannot combine the two datasets.

[22] We attempt to balance using the most recent and most accurate data. We elect to report results 1999-2020 (i.e., the most recent data) but focus on results 1999-2017 (i.e., the most accurate data).

safety awareness training and improve officer safety (FBI, ND). Being a segment of the UCR Program, LEOKA include information from more than 18,000 city, university and college, county, state, tribal, and federal law enforcement agencies, which participate voluntarily either through a state UCR program or directly to the FBI's UCR Program (FBI, ND).

We combine fatal and non-fatal injurious incidents in the main analyses, as there is some degree of chance between incidents being fatal, especially when shooting is involved (Zimring, 1972; Braga and Cook, 2018; Cook et al., 2019). Thus, focusing on fatal assaults solely represents an incomplete picture of police exposure to violence (Bierie and Detar, 2016; Bierie, 2017). Fatal assaults are quite rare, accounting for 0.2% of total assaults (Table 1). Following Chalfin et al. (2022), we measure officer deaths as deaths that occur as a result of a civilian felony and officer assaults as assaults by civilians that resulted in officer injuries.[23] Please see Appendix B for details on more details on construction of our analysis dataset.

3.2 Behavioral health disorder treatment center data

Our key variable of interest is the number of BHD treatment centers in each county. We follow the literature and use data on establishments ('single physical location at which business is conducted or services or industrial operations are performed') sourced from the U.S. Census Bureau (2022d)'s County Business Patterns or 'CBP' (Swensen, 2015; Bondurant et al., 2018; Bradford and Maclean, 2022; Deza et al., 2022a,b; Messel et al., 2023). These data reflect the universe of known establishments in the U.S. in March of each year. The Census Bureau receives data from the Internal Revenue Service (IRS) constructed from business tax returns to create the CBP. We expect the quality of the CBP data to be high as there are substantial costs to businesses for falsely reporting information on tax returns to the IRS (i.e., fines and incarceration). Businesses are incentivized to provide accurate data as means to avoid a costly IRS audit. Further, as part of applying the U.S. tax code and ensuring that businesses pay the correct amount of taxes to the federal government, the IRS cleans the submitted tax return data. See Deza et al. (2022b) for details on the CBP.

The data provided to the Census by IRS are at the six-digit NAICS code-county-year level,[24] which allows us to construct a count of the number of BHD treatment centers for our analysis. We consider two NAICS codes in our main analysis: outpatient and residential BHD treatment centers (Swensen, 2015; Bondurant et al., 2018; Bradford and Maclean, 2022). The

[23] LEOKA also provide the activity that officers responded to, the time of the assault, and the weapon used for all assault types, that is assaults that do and do not lead to injuries. We are not able to separate assaults that result in officer injuries from assaults that do not when looking at these specific dimensions.

[24] In the IRS data, businesses report a single principal business code, which is a six-digit NAICS code. This mapping permits us to accurately isolate centers of interest for our analysis.

specific six-digit NAICS codes are: 621420[25] (outpatient) and 623220[26] (residential). These centers respectively reflect 2.0% and 0.2% of healthcare and all establishments in the CBP over our study period (1999-2017).

In our main analyses, we use the total number of BHD centers in each county (i.e., we sum the number of establishments with NAICS codes 621420 and 623220) to proxy access to treatment.[27] We separately consider these two codes, and examine other BHD provider types, in heterogeneity analysis reported in Section 4.5. As we describe in Section 3.3, we follow the crime literature, beginning with Levitt (1997), as well as the economic literature on access to BHD treatment (Swensen, 2015; Bondurant et al., 2018; Messel et al., 2023; Deza et al., 2022b; Bradford and Maclean, 2022) and lag centers by one year to minimize endogeneity concerns. We use CBP data $1998-2016$ in our preferred sample which measures police officer on-duty assaults $1999-2017$.[28]

Beginning in 2017, the U.S. Census suppresses cells (county-NAICS code-year pairs in our study) with fewer than three establishments for privacy reasons. That is, these cells are not reported in the data.[29] We impute suppressed cells with their last non-imputed value. For example, if the 2017 value is suppressed, we impute the 2016 value. In robustness checking, we show that our results are not sensitive to imputing the full range of possible values (zero, one, and two) and excluding imputed observations. Suppression generally appears in more rural counties, which are disproportionately excluded from our analysis sample based on exclusions we apply to the LEOKA data (see Appendix B). There are 19,530 imputed county-NAICS-year pairs in 2,749 counties in the CBP 2017 to 2020.

[25]The NAICS definition is: 'Outpatient Mental Health and Substance Abuse Centers. This industry comprises establishments with medical staff primarily engaged in providing outpatient services related to the diagnosis and treatment of mental health disorders and alcohol and other substance abuse. These establishments generally treat patients who do not require inpatient treatment. They may provide a counseling staff and information regarding a wide range of mental health and substance abuse issues and/or refer patients to more extensive treatment programs, if necessary' (https://www.naics.com/naics-code-description/?code=621420, last accessed 3/22/2023.

[26]The NAICS definition is: 'Residential Mental Health and Substance Abuse Facilities. This industry comprises establishments primarily engaged in providing residential care and treatment for patients with mental health and substance abuse illnesses. These establishments provide room, board, supervision, and counseling services. Although medical services may be available at these establishments, they are incidental to the counseling, mental rehabilitation, and support services offered. These establishments generally provide a wide range of social services in addition to counseling' (https://www.naics.com/naics-code-description/?code=623220, last accessed 3/22/2023.

[27]While we refer to our measures as 'access,' we recognize that what we measure is just one dimension of access. Access includes an ability to pay, trust in the system, the ability to locate culturally appropriate care, stigma, and so forth. We are not able to incorporate such factors in our measures, but we believe that these are interesting avenues for future work.

[28]Prior to 1998, the CBP did not include six-digit NAICS codes (rather four-digit SIC codes were included) and we cannot isolate the centers of interest from other healthcare providers.

[29]For example, in the county-level files, DeKalb County in Tennessee does not have an entry for NAICS code 621420 in 2017.

The CBP data do not allow us to separate BHD centers that offer MHD and SUD treatment, that is there is not a separate NAICS code for these two types of centers. We do not believe that this data feature poses a substantial concern in our analysis given comorbidity across these conditions: half of all individuals diagnosed with a MHD will experience a SUD in their lifetime and vice-versa (Ross and Peselow, 2012; Kelly and Daley, 2013). Further, in our analysis of the 2020 National Survey of Substance Abuse Treatment Services (N-SSATS), 58.2% of centers provide comprehensive MHD assessment and diagnosis.

We follow the previous literature and focus on the effect of levels of BHD treatment centers, as opposed to a per-capita measure, for two reasons. First, we seek to estimate the marginal returns to officer safety associated with an additional BHD treatment center, which cannot be directly addressed with a per capita model or other functional forms. From a policy perspective, the decision to invest in a new behavioral health treatment center (and the centers we study are disproportionately likely to be supported by federal, state, or local governments) is likely made based on expectations regarding the value of the center to the community. Second, the economic literature on access to BHD treatment uses center counts and we wish to compare our findings with the broader literature. For completeness, we report results using per-capita centers in Section 4.2 and our findings are qualitatively similar.

3.3 Methods

We estimate the two-way fixed-effects regression in Equation 1 to test the impact of changes in local access to BHD treatment centers on police officer on-duty assaults:

$$Assault_{i,c,s,t} = \beta_0 + \beta_1 Center_{c,s,t-1} + X_{i,c,s,t}\beta_2 + \alpha_i + \delta_{s,t} + \epsilon_{i,c,s,t} \tag{1}$$

$Assault_{i,c,s,t}$ is the number of officer assaults per 100 officers in each agency i in year t, and $Center_{c,s,t-1}$ is a count of the number of BHD treatment centers (lagged one year) in each county c. We also control for $X_{i,c,s,t}$ which is a vector of time-varying agency (the log of agency-covered population) and county (county-level demographics (Surveillance, Epidemiology, and End Results, 2022), educational attainment (U.S. Census Bureau, 2022a), unemployment rates (Bureau of Labor Statistics, 2022b), and poverty rates (U.S. Census Bureau, 2022e)). α_i is a vector of agency fixed-effects and $\delta_{s,t}$ is a vector of state-by-year fixed-effects. Finally, $\epsilon_{i,c,s,t}$ is the error term.

Our source of variation is the opening and closing of BHD treatment centers within U.S. counties over time. We select our control variables to allow us to account for factors that potentially explain this variation and predict office on-duty assaults. For example, time-varying county-level factors and agency fixed-effects (which subsume county fixed-effects) account for supply- and demand-side factors that determine the opening and closing of centers such as patient need for treatment, costs to businesses, and so forth.[30] State-by-year fixed-effects account for all federal and state policies, demographics, and other shocks that vary over time nationally or by state. For example, most insurance regulations and policies (e.g., private insurance mandates for BHD treatment or telemedicine coverage) vary at the federal and state levels. We do not include crime as a covariate in our preferred specification as crimes are influenced by BHD treatment centers (see Bondurant et al. (2018)). However, we show that our results are robust to controlling for crime (see Section 4.2).

Data are weighted by the agency-level number of officers. We estimate least squares regressions and our standard errors are clustered around the county.[31] All coefficient estimates are converted to relative effects by comparison with the sample mean. In tables, beta coefficients reflect a one-center increase, but in the text when discussing effect sizes, we scale our coefficient estimates by four centers as that is the weighted average county year-over-year increase that we observe in our data.

By measuring access to BHD treatment at the county-level, we implicitly assume that the county is the correct market for such care. This assumption is in line with recent economic studies that explore the implications of access to BHD care (Swensen, 2015; Bondurant et al., 2018; Bradford and Maclean, 2022; Deza et al., 2022b). Further, clinical studies that assess the distance patients travel to receive such care suggest that this assumption is reasonable: more than 60% of outpatient opioid use disorder treatment is received within ten miles of patients' homes (Rosenblum et al., 2011).

3.4 Summary statistics and trends

The summary statistics presented in Table 1 Column (1) at the agency-year level describe the key outcomes, treatment variables, and control variables, weighted by the agency-level police employee population. There are 2.86 on-duty police assaults per 100 officers in any given agency-year, and they are mostly composed of non-fatal injuries (2.85 or $>$ 99% of all events) relative to officer deaths by felony (0.01). Table 1 Column (2) reports (available) variables at the county-level, weighted by the county-level police employee population.

Figure 1 reports trends in the rate of on-duty officer assaults (Panel A) over our study period and BHD treatment centers lagged one year to match Equation 3.3 (Panel B). Rates of on-duty assaults follow a U-shaped pattern: rates decline fairly steadily between 1999 and 2011, increase slightly between 2011 and 2012, are relatively flat from 2012 to 2015, and

[30] Agency fixed-effects account for time-invariant agency factors and allow us to minimize bias from a changing composition of agencies over time.

[31] We implement the estimation using the *reghdfe* command developed by Correia (2019).

then sharply increase from 2016 to 2020. The number of centers is (nearly monotonically) increasing over the study period (overall and for outpatient and residential centers), although in the post-Affordable Care Act (ACA) period the growth of outpatient centers has out-paced that of residential centers. Thus, there is not an obvious unadjusted trend in the two time series, we will explore the relationship more rigorously later in the manuscript.

Figure 2 displays the geographic distribution of BHD centers (lagged one year) in 1999, 2017, and 2020 across U.S. counties. Gray indicates the counties not in our sample and increasingly deeper shades of red indicate higher counts of centers. While there is geographic clustering in the intensity of treatment centers, we have reasonably good coverage across the U.S. Appendix Figure A1 reports a histogram of the distribution of BHD centers. 17.9% of county-year pairs have no centers, but our results are robust to excluding such counties.

4 Results

4.1 Evidence on the first stage

Central to our proposed causal chain from changes in BHD treatment access to changes in police officer safety is improved management of BHDs. While we lack the ability to separately examine BHDs across potential offenders, victims, officers, and other persons involved in a civilian-officer interaction, we can explore changes in overall population BHD metrics which will capture the net effect of all such changes. To this end, we follow the literature on treatment access (Swensen, 2015; Bondurant et al., 2018; Deza et al., 2022b) and use restricted-use death certificate data from the National Center for Health Statistics. We examine changes in rates (per 100,000 county residents) in deaths by (i) suicide and (ii) alcohol poisonings and drug overdoses. We estimate a county-level version of Equation 1: our unit of observation is a county in a state in a year, and the data are weighted by the county population, and we replace agency fixed-effects with county fixed-effects. We include the same set of county characteristics that are used in Equation 1. For all first-stage analyses, we use (non-imputed) data over our preferred CBP period (i.e., 1999-2017).

Death rates are arguably blunt metrics for BHD management, but they do have the benefit of being less prone to subjective perceptions by respondents or reporting errors due to stigma concerns than are other metrics (e.g., self-assessed mental health or substance use based on survey data).[32] Further, if we observe changes in blunt metrics of BHD management, then we may also expect to observe changes in less severe measures.

[32]We realize that there could be reporting errors by coroners in death certificates (Hollingsworth et al., 2017). However, given our focus on broad categories of death (i.e., not specific drug types), we suspect that such error does not likely drive our findings.

Results are reported in Table 2. In line with earlier studies, we find that increased access to BHD treatment centers within the county reduces death rates related to BHDs. We note that those persons who die due to BHDs may have been more prone to both experience a BHD crisis to which police respond and to have violent interactions with police officers, conditional on a crime, which suggests that our coefficient estimates for police officer safety are potentially lower bounds. In particular, four additional treatment centers per county lead to 0.04 and 0.09 fewer deaths by suicide and due to drug overdoses and alcohol poisonings per 100,000 county residents, respectively. Comparing these coefficient estimates to the sample means suggests that a four-center increase per county reduces deaths by suicide by 0.3% and deaths due to drug overdoses and alcohol poisonings by 0.7% respectively.

We next examine the effect of increased access to BHD treatment centers on admissions to such centers. We lack admissions information in the CBP, but we can use data from the N-SSATS. These survey data only include annual admissions to public and private treatment centers through 2012 and thus our analysis uses data on admissions over the period 1999 to 2012.[33] Further, while N-SSATS includes a large share of SUD treatment centers in the U.S., N-SSATS is a survey and therefore misses a non-trivial share of centers. With these caveats in hand, we estimate the effect of expanding access to BHD treatment on annual admissions using Equation 1, although we replace agency fixed-effects with county fixed-effects. We estimate a Poisson model to account for skewness in the number of admissions (county population serves as the exposure variable).[34] We find that admissions increase as access to treatment rises: four additional centers lead to 127 more annual admissions (or 1.2% relative to the sample mean of 10,962 admissions). Because we use a survey of SUD treatment providers (i.e., some SUD treatment providers are not included in this survey for various reasons, see Maclean et al. (2021) for a discussion), we likely under-count the number of BHD treatment admissions (in particular, we miss centers that provide exclusively MHD treatment and thus admissions to these centers, or choose not to be included in the SAMHSA treatment locator directory). As a result, we expect that our estimated increase in admissions to BHD treatment as access rises is smaller than the actual increase.

Finally, we estimate the effect of expanding access to BHD treatment on crime (per 100,000 agency-covered population) using the UCR 1999 to 2017 (Kaplan, 2020b). We focus on agencies in our analysis sample that reported 12 months of crimes known to law enforcement, thus this UCR analysis sample is slightly smaller than our main sample. Bondurant

[33] After 2012, admissions are reported in broad categories which are not suitable for our analysis. Further, we do not have data at the level of the county-level, only the state-level. For these reasons, we do not use data beyond 2012. We exclude centers located in hospitals and retain only those that list BHD treatment and their primary focus to match the centers we study in the CBP.

[34] We implement the Poisson regression using the *ppmlhdfe* command by Correia et al. (2020).

et al. (2018) show that four additional BHD treatment centers per county leads to a 1% reduction in homicides, aggravated assaults, and financially-motivated crimes. Results (Table 2, Panel D, in that paper) suggest that increased access to BHD treatment centers reduces total and violent crime. Four additional centers per country reduce total crime rates by 7.8 and violent crime rates by 5.1 per 100,000 agency-covered population or 0.2% and 0.9%. The coefficient estimate in the property crimes regression is negative but is imprecise.[35]

Table 2 provides evidence of the first stage. As local access to treatment increases, BHD management improves, more patients receive treatment, and crime (in particular violent crime which is potentially most salient to police officer on-duty assaults) declines. With this evidence in hand, we turn to our analysis of police officer safety.

4.2 The effect of access to BHD treatment on officer safety

Table 3 reports our main findings. We show results based on regressions with different covariate sets and time periods. Moving from column (1) to (4) displays coefficient estimates based on regressions with increasingly longer sets of control variables: agency and state-by-year fixed-effects (column [1]), county-level demographics (column [2]), county-level socio-economic (column [3]), and agency-level (column [4]) variables. 'Building up' the specification documents the extent to which adding controls impacts our findings. Panel A reports results based on the (preferred) non-imputed sample (1999 to 2017) and Panel B reports results based on the full sample (1999 to 2020), where suppressed observations over the period 2018 to 2020 are imputed (see Section 3.1).

Reassuringly, our results are stable across included controls and time periods. For example, the coefficient estimate in Panel A (non-imputed data) ranges from -0.011 in the most parsimonious specification (column [1]) to -0.009 in the full specification (column [4]). Comparing coefficient estimates using the period 1999 to 2017 and 1999 to 2020 in the full specification reveals coefficient estimates of -0.009 and -0.007, respectively. Our preferred specification includes agency and state-by-year fixed-effects, county demographics, county economic variables, and agency-level control, and coefficient estimates from this specification suggest that an additional four centers per county lead to 0.04 fewer assaults per 100 officers or a 1.3% decline over the 1999 to 2017 period, and to 0.03 fewer assaults per 100 officers or 1.0% decline over the 1999 to 2020 time period. We have bootstrapped the difference between these two coefficient estimates using a non-parametric bootstrap procedure (500 repetitions) and the difference is not statistically different from zero (p-value = 0.136).

[35] While we find no statistically significant effects on property crimes, in unreported results we find that increases in BHD treatment centers reduce motor vehicle theft as documented by Bondurant et al. (2018).

In Table 4, we report results using the sample period 1999 to 2020 across different imputation approaches for suppressed counties in the 2017 to 2019 CBP (recall that we lag our treatment access metric by one year; prior to 2017 there is no suppression). In our main sample, we 'back fill' observations (Section 3.1). Here, we assign these observations values of zero (the smallest possible value for suppressed counties), one, and two (the largest possible value for suppressed counties. Finally, we drop all suppressed observations. The coefficient estimates are essentially identical (out to three decimal places) to our preferred coefficient estimate in Table 3 Panel B column (3): -0.007.

In the last column of Table 4, we report results using a sample that excludes agencies with imputed police officer employment data from the ASG survey. Beginning in 2018, the number of agencies reporting zero officers increased (see Appendix B). Thus, the period between 2018 and 2020 accounts for 95% of observations using ASG values, and observations using ASG values account for less than 0.1% of the total observations. We are not certain why the data display this pattern, but reassuringly our results are not sensitive to removing agencies with zero officers: the coefficient estimate is -0.007, which is identical to our preferred coefficient estimate in Table 3 Panel B, column (4). Findings are very stable (out to three decimal places) across samples and imputation approaches.

Given the similarity in results across our two samples (1999-2017 and 1999-2020), for brevity, we report the remaining results using the 1999-2017 period (our preferred sample which includes only the non-imputed CBP data). Results based on the 1999-2020 period are similar and are available on request.

In our main analyses, we sum officer assaults and killings for reasons described in Section 3.1. In Table 5, we examine the impact of access to BHD treatment centers separately on police officer non-fatal and fatal assaults. Our overall findings are driven by reductions in non-fatal assaults. The coefficient estimate (-0.009) in the non-fatal assaults regression is nearly identical to our main coefficient estimate in Table 3. The coefficient estimate in the officer fatal assaults regression is essentially zero (0.000) and imprecise.

We choose to use the count of centers in our main specification for reasons discussed in Section 3.2. We next report results where we replace the lagged number of centers with the lagged number of (i) centers per 100,000 county residents in a county[36] (we use the log of the count of police officer on-duty injuries to account for skewness in the outcome, we add one to all observations prior to logging so that the variable is defined for all observations) and (ii) imputed employees following a linear programming method that takes advantage of 'adding up' rules in the CBP (e.g., in any year the national count of establishments within an industry must equal the sum of the state counts within this industry code) following Eckert

[36]We do not include logged agency-level population as a control variable in this regression.

et al. (2020). Results are robust (Table 6). More specifically, one more center per capita leads to a 1.3% reduction in officer assaults and 153 additional employees (the average county-level year-over-year increase observed during our study period) leads to a 1.6% reduction. Our results are highly robust to a wide range of alternative specifications, samples, and approaches to inference. See Appendix Section C for details.

Changing access to BHD treatment can impact police officer safety through at least two channels. First, there may be a 'mechanical' effect from reductions in BHD crises to which police officers respond and less crime. That is, there are fewer opportunities for police officers to be assaulted on duty (i.e., fewer police-civilian interactions). This channel is not assured as the first responses and crimes that are deterred through increased access to BHD treatment need not be the types of police-civilian interactions that are associated with assaults on officers. Second, the interactions between civilians and police officers during first responses to BHD crises and crime may become less likely to lead to assaults on officers. Our main findings (Table 3) reflect both of these channels. We next augment Equation 1 with total Part I crime incidents known to the police from the FBI's UCR database.[37] We acknowledge that total crime is likely a function of treatment access (see Table 2), thus we view results from this exploratory analysis as suggestive. We focus on agencies that report crimes for all 12 months in a given year, thus this sample is slightly smaller than our main sample and we replicate Equation 1 to ensure that our main findings hold. Including total crime as a right-hand side variable in Equation 1 reduces the coefficient estimates on treatment centers modestly from -0.009 to -0.007 (Table 6 Panel C). Proportionately, these changes reflect an 22% decline in the coefficient estimate. While results from this analysis should be interpreted with caution, they suggest that much of the net improvement in officer safety is potentially attributable to interactions between civilians and police officers being less likely to lead to an officer assault rather than a 'mechanical' effect of less BHD crises that require police response and less crime/victimization.

4.3 Internal validity

4.3.1 Local event study

A key assumption of our TWFE regression is parallel trends, that is agencies located in counties that do and do not experience changes in the number of BHD treatment centers would have followed the same trends had agencies in 'treatment' counties not experienced a change in the number of centers. This assumption is inherently untestable, but we can follow

[37] A limitation of this analysis is that we lack data on incidents where police serve as first responders to persons experiencing a BHD crisis that do not also include a crime report.

the literature and estimate a local event study (Cengiz et al., 2019) to provide suggestive evidence on the ability of our data to satisfy this assumption. To this end, we define treatment agencies as those that are located in counties that experience no change in the number of centers for three years followed by an increase (i.e., an event) in the number of centers in year four, and then are observed two years after the event, independent of whether there are fluctuations after the event as long as the number of centers after the event remains at least as large as the number of centers in the local event year. Comparison agencies are those located in counties that experience no change in the number of centers over the local six-year event window. We use contemporaneous values in the number of centers to identify the local event year and the timing of the event year to define 'cohorts' of treatment and comparison agencies. We end the period in 2016 as this is the last year of non-imputed CBP data and include the 1998 LEOKA data to increase the number of cohorts.[38] Over the period 1998 to 2016, there are 14 cohorts, with the first cohort defined as 1998-2000 as the pre-event period, 2001 as the local event, and 2002-2003 as the post-event period. We stack the 14 cohorts and estimate a regression that includes event leads and lags, time-varying covariates, agency-by-cohort fixed-effects, and state-by-year-by-cohort fixed-effects.[39] We omit the period prior to the event from the regression. We note that our local event study does not exploit the continuous variation of our two-way fixed-effects regression and instead examines a binary increase in access.

The findings are reported in Figure 3 and reveal no evidence of differential pre-trends between agencies located in counties that will and will not experience an increase in the number of BHD treatment centers. Further, following the increase in the number of centers in a county, Figure 3 shows that officer on-duty assaults decline sharply.[40]

We estimate a variant of the local event study in which we define a local event as a decrease in the number of centers. Results are reported in Figure 4. Examination of the leads, similar to Figure 3, suggest that treatment and comparison counties follow similar pre-trends. However, and distinct from Figure 3, we observe limited evidence of changes in

[38]Results, available on request, are robust to excluding the 1998-2003 cohort. We have also estimated the local event study using data from 2017-2020, that is the time period when some CBP data are suppressed (i.e., NAICS-county cells with less than three establishments). To do so, we i) use all unsuppressed data and ii) impute the number of centers for counties that have the number unchanged during the entire 1998-2016. Results, available on request, are not appreciably different than those reported in this manuscript.

[39]Including time-varying covariates in this manner assumes that their effect is constant across cohorts. We have estimated the local event-study without these covariates and the results are not appreciably different.

[40]Effect sizes between the local event study and TWFE regressions we estimate in this paper are not directly comparable. TWFE regressions recover, under certain assumptions, an estimate of the average causal response (ACR) or ACR on the treated (ACRT). The local event study, on the other hand, estimates the effect of an average treatment effect on the treated (ATT). Further, the samples differ due to the restrictions we must place on the local event study sample. Thus, we focus on similarities in sign and statistical significance across the two estimators.

on-duty police officer assaults post-event. These patterns suggest asymmetry in the effects of expanded access to BHD treatment centers: following an increase in BHD treatment centers assaults decline, but a decrease is not associated with an observable change in assaults.

While we lack data to empirically explore the potential asymmetry, we can offer some hypotheses. First, we may lack power to detect effects for counties that experience a loss of centers as there are far decrease events than increase events over our study period (807 increases and 470 decreases).[41] Our post-event period is not long (i.e., two years) due to the fact that estimation of the local event study is 'data hungry' (e.g., when we add a year to the pre- or post-event period, we can include fewer cohorts). Thus, effects following a decline may emerge over a longer time period. Patients may experience improvements in BHDs that extend over the post-event window. Further, police officers may become accustomed to taking individuals experiencing behavioral health crises to BHD treatment centers or hospitals, where police often take those experiencing BHD crises (Wood et al., 2021), may be better able to refer patients to BHD treatment centers for longer-term care. This police or hospital 'knowledge' may be retained after a center closes. Previous evidence offers support for such asymmetries from sustained losses of BHD treatment options within the local community: Muchow and Laurito (2022) find no increases in crime after a 50% reduction in the number of MHD treatment centers in the City of Chicago that occurred in 2012 and Sachs (2019) documents that neighboring hospitals are able to absorb some (not all) of patient demand following the closure of psychiatric hospitals in California.

In Appendix Table A3, we report characteristics of counties that experience no change in the number of centers (i.e., our comparison counties), an increase in the number of centers (i.e., counties in the 'increase' event study treatment group), and a decrease in the number of centers (i.e., counties in the 'decrease' event study treatment group). There is some overlap in the counties appearing in the final two groups as counties can appear in both treatment groups. While these groups are not identical, they are broadly similar in terms of the characteristics we include in our regressions. This pattern of results suggests that, while counties do experience different changes in the number of centers, these counties may be similar in terms of other determinants of on-duty police officer assaults.

4.3.2 Balance

We conduct balance testing in which we regress each time-varying agency and county characteristic that we include in Equation 1 on the number of centers and fixed-effects. For

[41] The average size of the increase and decrease in the two samples differs. In counties experiencing an increase (decrease) in the number of centers, the average increase (decrease) is 1.9 (1.5). Thus, increase doses are 27% larger than decrease doses on average.

county-level variables, we aggregate the data to the county-year level and include county and year fixed-effects. We use the 1999-2017 sample in this analysis and data are weighted by the police employee population. The logic of this test is to assess whether our covariates are influenced by changes in the number of treatment centers.

Results are reported in Appendix Table A4. While we do see some evidence that centers predict some county (but not agency) level characteristics, we are reassured that differences are not large and our main results (see Table 3) are not appreciably different when we do and do not control for these variables.

4.3.3 Migration

A third threat to validity is program-induced migration (Moffitt, 1992). We test for this behavior using data on past-year cross-county migration information available in the Current Population Survey (CPS) 1999-2017. Specifically, we construct past-year cross-county migration rates at the county-year level and regress that outcome on the lagged number of BHD treatment centers.

We estimate a county-level version of Equation 1: our unit of observation is a county in a state in a year, the data are weighted by the county population, and we replace agency fixed-effects with county fixed-effects. We include the same set of county characteristics that are described in Equation 1. We note that sample sizes are smaller than our county-level analysis due to privacy-related suppression in the CPS.

Results are reported in Appendix Table A5 and reveal no evidence that changes in the number of treatment centers influence such migration. These null findings suggest that the opening and closing of BHD treatment centers do not induce migration and are in line with recent work by Horn et al. (2021) documenting that such openings and closings do not impact residential property values.

4.4 Staggered treatment adoption

Recent econometric advancements document that TWFE regression with a staggered policy roll-out can produce biased coefficient estimates due to heterogeneity across treated units and time dynamics. Much of the work to date has focused on binary treatment variables, but Callaway et al. (2021) addresses the continuous context. In the continuous setting, a target parameter of potential interest to researchers is the average causal response (ACR) as there is more than one possible treatment dose. As in the case of a binary treatment, the researcher assumes homogeneous and static treatment effects. Callaway et al. (2021) state that additional assumptions are required for the continuous setting. In particular, a 'stronger' version of parallel trends the researcher must assume: the path of outcomes with different doses of treatment (centers in our context) would have been the same had each received the same dose, thus the researcher must make assumptions about paths of treated potential outcomes as well as untreated potential outcomes. With these assumptions in hand, TWFE regression can recover an estimate of the ACR. However, to recover an estimate of the ACR on the treated or ACRT (comparable to the average treatment effect on the treated, or ATT, in the binary case), an additional assumption is required: no selection into treatment dose.

To the best of our knowledge, there is no established approach to accounting for concerns related to treatment effect heterogeneity and dynamics with a continuous treatment variable. However, to speak to these concerns, we apply a recently developed estimator proposed by Gardner (2021). This approach is a two-step approach to difference-in-differences (TSDID) in our setting, with a continuous treatment variable. In the first stage of this procedure, the relationships between the time-varying covariates and fixed-effects (i.e., the agency and time fixed-effects) with the outcome variable are estimated using only untreated observations. The estimated parameters in the first stage are used to residualize the outcomes for both treated and untreated observations. These parameter estimates are not vulnerable to concerns regarding bias from treatment effect heterogeneity as they are based on only untreated observations. In the second stage of this procedure, and using all observations in the sample (treated and untreated), residualized outcomes are regressed on the treatment variable. Standard errors are estimated with GMM following Hansen (1982) and account for within-county clustering. The estimator compares untreated units to units treated at specific doses, and then constructs an overall average of these comparisons. Thus, this estimator allows us to estimate an ATT parameter, though where doses differ in size.

To locate untreated observations, we restrict our sample to agencies with at least three years of the number of centers remaining constant since the beginning of the analysis sample to better estimate the agency fixed-effects. We next identify the year of the event (treated period) as the year that an agency experiences the first change in the number of centers. On average, we observe each agency seven times in the first stage of the TSDID sample. Since the untreated observations can have a positive number of treatment centers in the first stage, we transform our lagged center variable to a 'dose' variable that takes on the values of the difference between the number of lagged centers in the current period and the number of lagged centers in the first period. We use this dose variable as the treatment variable in the second stage. We drop agencies that ever have a number of centers below the number of centers in the first period because it leads to a negative value in the dose variable.[42] Results are reported in Appendix Table A5. Findings are similar, but larger in

[42] We implement the TSDID estimation using the *did2s* command by Butts and Gardner (2021).

size (as we would expect as we are not making 'forbidden' comparisons that compare newly treated observations to earlier treated observations), and less precise (we must exclude a non-trivial share of our sample).[43]

4.5 Heterogeneity

In Figure 5 Panel A, we consider the importance of other providers who can deliver BHD treatment: private practice offices of physicians (e.g., psychiatrists) and non-physicians (e.g., psychologists), general physicians, and crisis centers. In particular, we separately replace our measure of lagged centers with lagged values of each of these provider types. Our findings show that police officer assaults decline as access to other forms of BHD treatment increases (private offices of BHD treatment physicians and non-physicians and crisis centers) and to a much smaller extent as access to general physicians increases.[44] While we observe declines in officer assaults as access to the various modalities of care increases, the reductions are largest for the outpatient and residential centers that are the main focus of our study.

In Panel B, we separately consider residential and outpatient centers. While there are similarities across these modalities, there are important differences in terms of patients (e.g., Medicaid enrollees − due to federal regulations such as the Institutions of Mental Disease exclusions − may not be able to easily utilize residential care in all states and court-ordered treatment is generally received in outpatient settings) and myriad other factors (e.g., residential treatment is generally more expensive than outpatient treatment (French et al., 2008)). Effect sizes are larger for residential treatment centers, but 90% confidence intervals overlap, preventing us from drawing strong conclusions about heterogeneity across modalities.

A concern with our findings is that we are capturing mechanical 'incarceration' effects associated with treatment. That is, while patients are in treatment, they are less able to interact with and therefore assault police officers. We do not believe that our findings are driven by such effects. First, as we show in Figure 5 Panel B, we see very similar effects when we separate residential treatment and outpatient treatment centers. Incarceration effects are more salient for residential treatment (where the patient is in treatment 24 hours per day) than for outpatient treatment (where the patient does not stay at the center for long periods of time). In 2020, 13%, 9%, 14%, and 65% of standalone SUD treatment was short-term residential (up to 30 days), long-term residential (30 to 90 days), intensive outpatient (several hours per day), and non-intensive outpatient (several hours per week).[45] The fact that effects

[43]Further, as noted earlier in this section, TSDID recovers and estimate of the ATT while our TWFE regressions recover and estimate of the ACR or, assuming no selection into treatment dose, an ACRT.

[44]General physicians deliver a non-trivial amount of BHD care in the U.S. in terms of diagnosing, prescribing medication, and providing referrals to specialists. See Maclean et al. (2023) for a discussion.

[45]Estimates in this paragraph are based on the authors' analysis of the 2020 Treatment Episode Dataset.

are similar for residential and outpatient care offers suggestive evidence of improved BHD management as the main contributor to our overall finding. Further, in 2020 the average duration of SUD residential treatment (combined short- and long-term) was 73 days, thus our use of a one-year lag in treatment centers in Equation 1 exceeds the length of treatment for most patients (just 1% of all residential patients reported a duration longer than one year) and our results are robust to including a two-year or three-year lag (see Figure C1).

Equation 1 imposes the assumption that increases and decreases in the number of centers within a county have symmetric effects, which may not be the case. We next follow Mocan and Bali (2010) and Carpenter et al. (2017) and allow for asymmetric effects of center increases and decreases. To this end, we separate our lagged center variable into two variables: (i) the number of lagged centers in periods when center counts are rising and (ii) the number of lagged centers when center counts are declining. Following Mocan and Bali (2010), observations in the first year of the sample period (i.e., 1999 for most agencies) are coded as missing in this analysis. Results (reported in Figure 5 Panel C) are similar across periods of increasing and decreasing center counts. These findings suggest that effects are symmetric, which is in line with earlier studies on BHD treatment access (Deza et al., 2022a).[46]

In Panels D through F of Figure 5 we explore heterogeneity by county-level characteristics. In particular, we estimate separate regressions for counties above and below the sample median unemployment rate, level of local expenditures on social programs,[47] and crime rates, and Panel G reports results from an analysis that separately examines agencies that cover smaller (10,000 to 50,000) and larger (over 50,000) populations. While confidence intervals overall overlap, which prevents us from drawing strong conclusions, our findings are more pronounced in higher (vs. lower) unemployment rate counties, counties that have above (vs. below) median social expenditures, lower (vs. higher) crime rates, and larger (vs. smaller) counties. Note that the coefficient estimates in Panel F show that BHD treatment

[46]We note that these findings could be interpreted as discordant with our local event study analysis. The local event study captures a different aspect of changes in access to treatment. For example, in the local event study, by the way we construct the sample (in particular, requiring no change in center for a minimum of three years), we focus on markets that are relatively stable and these markets may be better able to absorb follows of patients following the closing of a center. Because many of the centers we study are potential 'vulnerable' providers in that they often operate on tight budgets and are at the mercy of government funding (Buck, 2011), closures are not uncommon. Hence, the local event study samples include counties with, plausibly, more stable providers than the full sample of providers. Using the N-SSATS data described in Section 4.1, we isolate BHD treatment centers observed in the year 2000, by the year 2011 64% appear to have closed operations for at least one year (full details available on request). Further, as described earlier in the manuscript, TWFE regressions estimate an ACR or ACRT parameter while the local event study estimates an ATT-type parameter.

[47]We include the sum of police, streets and highways, health, and education expenditures. Data are sourced from (Kaplan, 2021) and U.S. Census Bureau (2022b). We do not use these, or other, expenditures as instrumental variables for centers. Thees variables could impact other determinants of on-duty officer assaults, leading to a violation of the exclusion restriction.

leads to statistically significant decreases in assaults in both high and low-crime areas. These estimates support our earlier finding that a reduction in crime is not the sole mechanism driving our main results.

Finally, we examine the interaction between access to BHD treatment and state-level policies that reduce the financial costs of receiving treatment in Panels H and I. First, we consider state-level paid sick leave (PSL) policies that require employers to provide employees with approximately seven days of PSL that can be used for the employee's healthcare needs (including BHD treatment) and the needs of the employee's dependents. Taking time off work can be prohibitively expensive for some individuals: in 2020 the median wage for a non-elderly adult employee in the U.S. was \$200 (National Equity Atlas, ND).[48] PSL mandates increase access to and use of PSL (Maclean et al., 2020). We obtain data on state-level PSL policies from the National Partnership for Women & Families (2022). We estimate Equation 1 in the sample of agencies in states that do and do not mandate PSL by the end of 2020, resulting in 11 states with PSL policies. Results are similar across these two groups of states. Second, we stratify the sample by ACA Medicaid expansion status (Kaiser Family Foundation, 2022). Medicaid expansion plans generously cover BHD treatment services for lower-income Americans and previous studies show that Medicaid expansion increases use of these services (Maclean et al., 2018; Maclean and Saloner, 2019; Ortega, 2023). By the end of 2020, 32 states have expanded Medicaid coverage. Again, similar to our PSL policy stratification, results are broadly similar for expansion and non-expansion states.

4.6 Civilian deaths

This paper aims to examine the impact of expanding access to BHD treatment on fatal and non-fatal injurious assaults on police officers. While a full analysis of how changes in BHD treatment centers impacts civilian deaths by officers is beyond the scope of this paper, improved BHD access among civilians may prevent police interactions from escalating and hence could confer safety benefits to civilians. We next provide an exploratory analysis into the effect of BHD treatment on police killings of civilians.

We draw data on the number of civilians killed by police using the Fatal Encounters (FE) database.[49] This data source documents and verifies officer-involved deaths of civilians using public records, news reports, and other media outlets starting in 2000. Appendix Table A6 reports estimates of the effect of BHD treatment on the number of police killings of civilians in a county (per 100,000). The estimated coefficient in column (1) suggests that expanding

[48] We use the reported hourly wage (\$20) and multiply by eight hours per day. If we use the median hourly wages for employees without a college degree (\$19), then our estimate is slightly smaller (\$152).

[49] Please see `https://fatalencounters.org/`, last accessed 1/2/2023.

local access to BHD care has little effect on such deaths. However, when we dis-aggregate by race of civilians, in columns (2) through (4), we see that improved access to BHD treatment leads to a statistically significant decrease in white civilian deaths at the hands of police. An additional four BHD treatment centers in a county results in a 0.8% reduction in white civilian casualties. We also find a decrease in Black civilian deaths; however, the coefficient estimate is imprecise. Given that Black patients are less likely to receive BHD treatment and the treatment they receive is less likely to be adequate (Saloner and Cook, 2013; Saloner et al., 2014; Grooms and Ortega, 2022), our finding that local access to BHD treatment affects white residents more than Black residents is perhaps not suprising.[50]

Our analysis of civilian deaths at the hands of the police has important caveats. Civilian deaths at the hand of police are likely under-reported even in the FE database, which is superior to government-collected data on deaths at the hand of police (Collaborators et al., 2021).[51] There is no national database on police non-lethal use of force, and therefore, we cannot compare these results to our main outcome of fatal and non-fatal assaults of officers.

5 Discussion and conclusion

Despite the importance of officer safety for effective policing, retention, morale, and overall safety of both officers and civilians, there is very limited research on determinants of violence against police officers. Police officers often serve as first responders to civilians experiencing a BHD crisis, which can place officers at risk for on-duty assaults as police officers are generally not provided with the specialized training required for such interactions. Further, civilians experiencing BHDs are at increased risk for encounters with police officers as they are at elevated risk for committing crime, for being a crime victim, and for their behavior to be perceived as threatening by others. These factors suggest that inadequately treated BHDs within the population could increase the risk of a police-civilian interaction, and also increase the probability that any given interaction escalates to the point where the officer is assaulted. Motivated by these patterns and the established improvement in management of BHD symptoms and reductions in crime that arise as access to BHD treatment expands, in this study we evaluate whether such expansions have positive spillover effects on police officer safety.

Using a two-way fixed-effect regression that exploits variation in the number of BHD treatment centers within counties and over time, we find that four additional centers lead to

[50] We do not have officer race, which affects use of force (Cox et al., 2022; Hoekstra and Sloan, 2022).

[51] Data on police killings from government sources capture approximately 50% of the police-involved deaths in other non-governmental data (Barber et al., 2016; Feldman et al., 2017).

a 1.3% reduction in on-duty officer assaults. Four additional centers reflect a 13.3% increase in supply for the average county in our sample, which leads to an implied elasticity of -0.1. We provide suggestive evidence that our findings reflect at least two channels from changes in access to BHD treatment and officer assaults: (i) reduced BHD crises to which police serve as first responders and reduced crime that 'mechanically' reduces the opportunities for civilian assaults on officers, and (ii) conditional on crime occurring, the crimes that occur are less likely to escalate to assaults on officers (i.e., crime is 'safer' from the perspective of the officer). There are other channels that we lack data to study: for example, as access to treatment increases and individuals are better able to manage their conditions, leading to less likely to be perceived as engaging in criminal activity by police.

Our primary finding is that increasing access to BHD treatment reduces on-duty assaults against police officers. In particular, four additional centers per county reduce on-duty assaults on officers by 1.3%. Given that our findings estimate the intent-to-treat effect, our arguably modest-sized effects are not surprising.[52]

The objective of opening a BHD treatment center is to increase access to treatment, therefore our estimated effects on officer safety are arguably purely a positive spillover effect.[53] Therefore, quantifying the size of this spillover in a back-of-the-envelope cost-savings estimate is more appropriate than a cost-benefit analysis.[54] In 2021, there were 665,380 police and sheriff's patrol officers in the U.S. (Bureau of Labor Statistics, 2022a). 3.6% of those officers were assaulted and injured due to the assault (Federal Bureau of Investigation, 2020). These estimates imply 23,954 assaults leading to injuries in 2021. Our main findings imply that four additional centers per U.S. county would have reduced the number of office assaults leading to an injury in 2021 by 311 (=23,954 $*$ -1.3%). Chalfin and McCrary (2018) estimate that the cost of an assault to society as $54,220. [55] Thus, the annual social saving in 2021 to the U.S. associated with four additional BHD treatment centers per county would have been $16,883,991 (= $54,220 $*$ 311).[56]

[52]The effect for the treated (i.e., the treatment-on-the treated) is likely large. While we do not have a precise number of patients in a BHD treatment center, we use a survey of SUD treatment centers, which misses a non-trivial share of treatment, in order to estimate the following back-of-the-envelope calculation. First, our estimates suggest that four additional centers per county increase the number of admissions to SUD treatment by 1.2% per year, If we assume that we capture 50% of treatment, then a 'corrected' estimate of the first-stage would be a 2.4% increase in admissions to BHD (MHD and SUD) treatment. We can scale our intent-to-treat estimate by the first stage estimate to approximate the treatment-on-the-treated effect: 54%. This back-of-the-envelope calculation should be interpreted with caution as estimates of the TOT are sensitive to even small changes in the strength of the first stage.

[53]To the best of our knowledge, no jurisdiction in the U.S has proposed investments in BHD treatment centers as a means to reduce the number of on-duty police officer assaults or address police staffing issues.

[54]We note that industry estimates suggest the cost of opening a BHD treatment center is in the range of $100,000 to $2M (The Drug Rehab Agency, 2022).

[55]We inflate the original estimate ($38,924 in 2010) to 2022 dollars using the Consumer Price Index.

[56]Our estimates are based on a sub-set of counties, thus we are extrapolating to all counties which implies

We can compare our findings with findings from evaluations of traditional policy tools to improve police officer safety. For example, increasing the police force size. Chalfin et al. (2022) show that each additional police officer hired reduces on-duty assaults by 0.14-0.23 incidents or 0.04-0.1% in cities with more than 50,000 residents. This finding suggests if each of the 804 cities with more than 50,000 residents in 2020 hires one additional police officer (U.S. Census Bureau, 2022c), there will be 113-185 fewer on-duty office assaults nationally.[57]

Our findings contribute to the growing line of literature suggesting that, in addition to improving individuals' ability to manage chronic conditions, expanding access to BHD treatment has positive spillovers to socially valuable outcomes. Future research could examine possible spillovers in other settings such as human capital acquisition and employment, and the importance of other factors (e.g., government policies that reduce barriers to treatment) in improving the lives of both individuals who experience BHDs and society.

homogeneous effects across counties included and not included in the analysis. If this assumption is incorrect, then there is some bias in our calculation of cost-savings.

[57]The number is extrapolated from cities with more than 50,000 residents to the nation. While the samples differ across the two studies, we note that our findings based on larger agencies, those serving more than 50,000 residents, are very similar to our main findings. Thus, this extrapolation is potentially reasonable.

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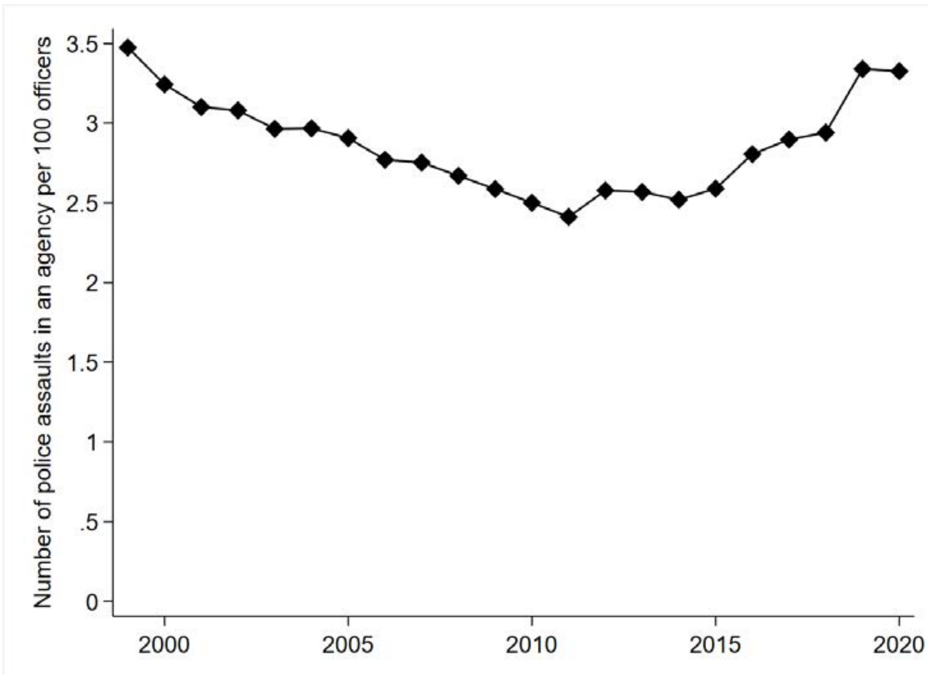
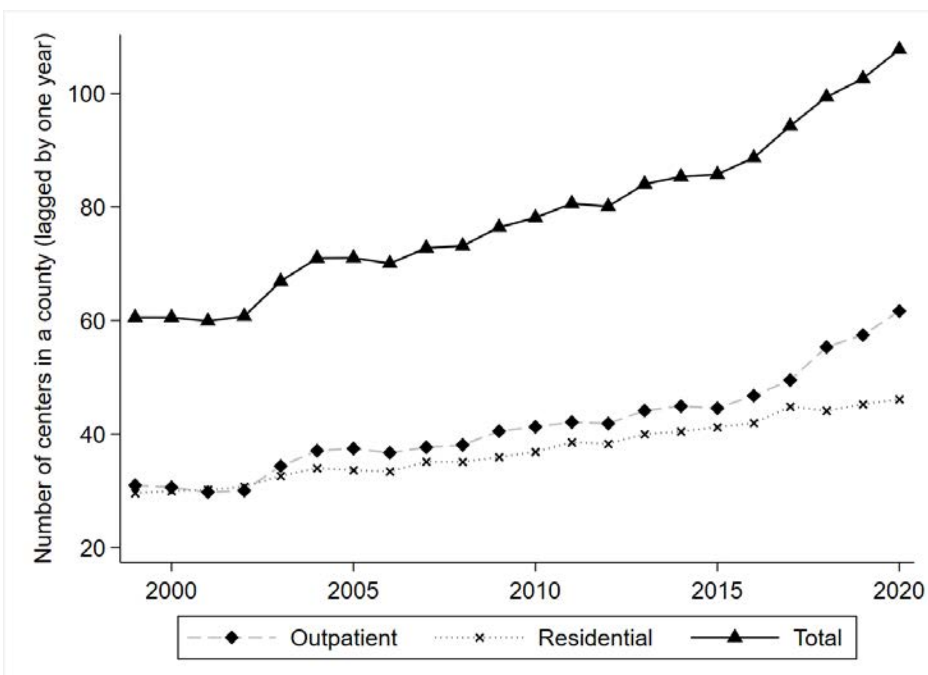
Figure 1: Number of BHD treatment centers and police officer assaults in each year between 1999 and 2020

Panel A: On-duty officer assaults

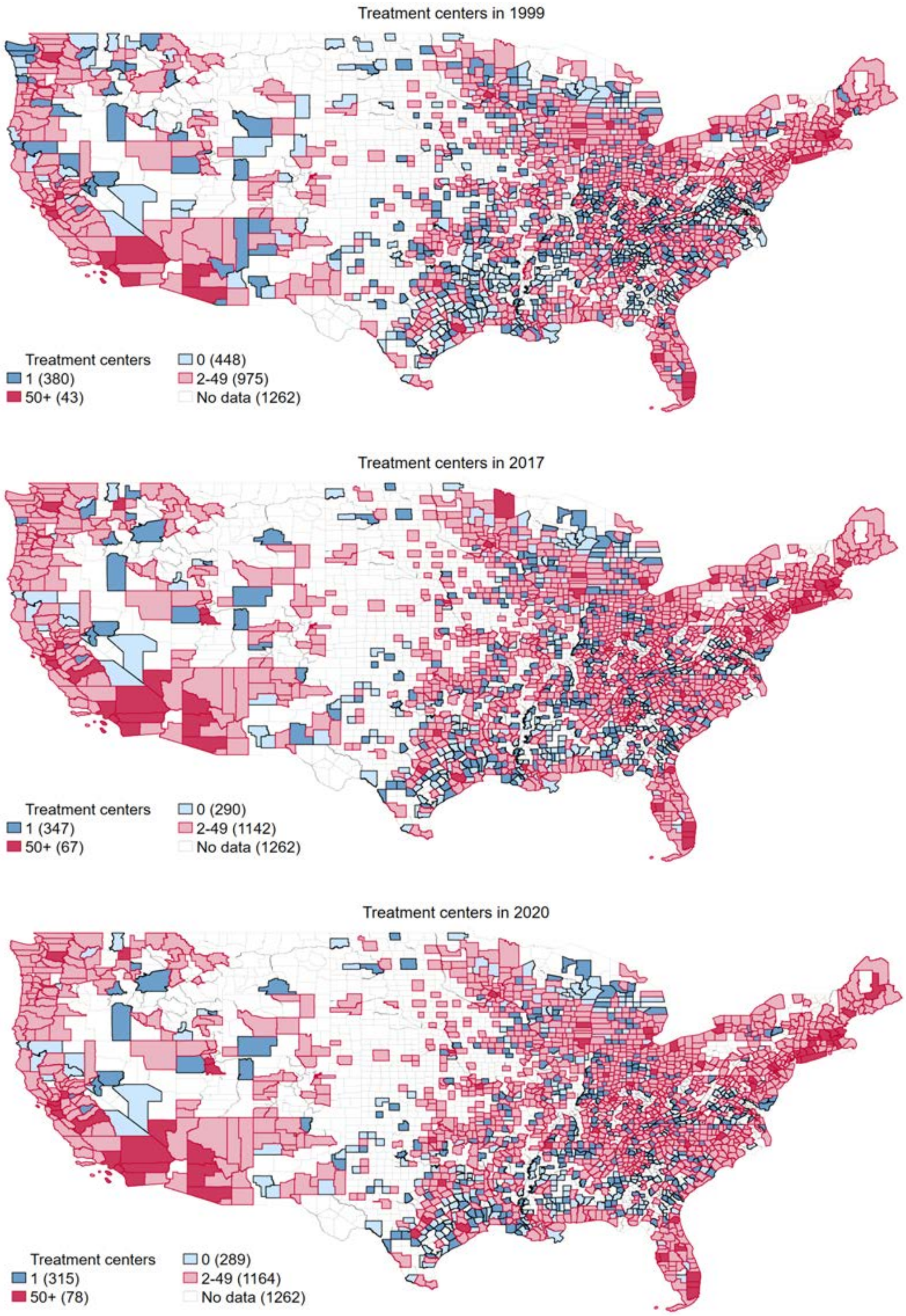


Panel B: Treatment centers



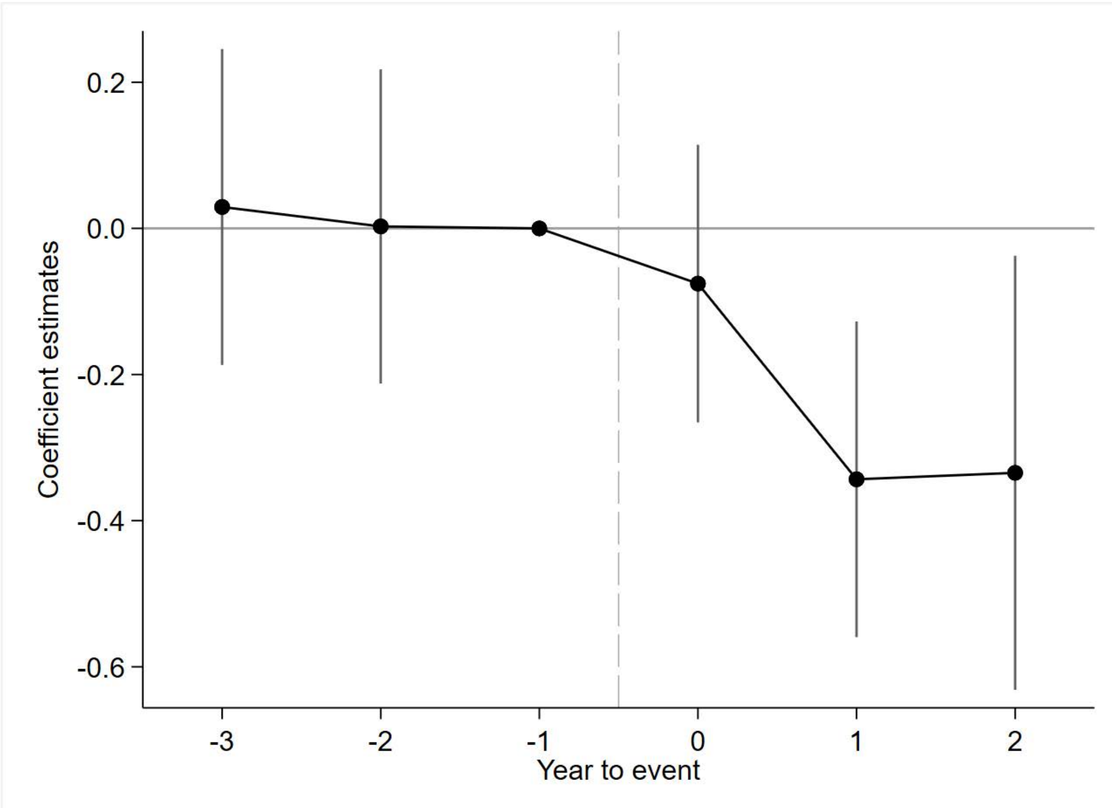
The data are the LEOKA in Panel A and the CBP in Panel B. Officer assaults are defined as the total of officer deaths and injurious officer assaults. We use the six-digit NAICS codes: 621420 (outpatient treatment) and 623220 (residential treatment). The number of treatment centers is lagged by one year. Observations are weighted by the average number of police officers employed.

Figure 2: Number of BHD treatment centers 1999-2020



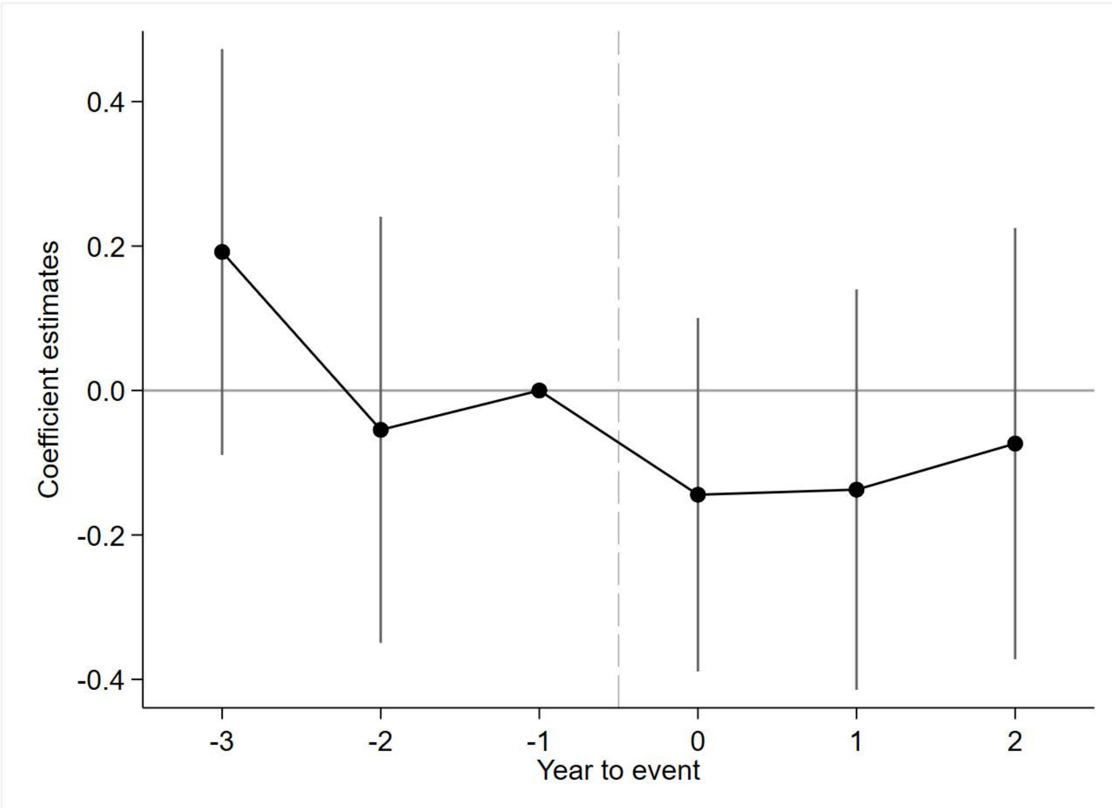
The data are the CBP. We use the six-digit NAICS codes: 621420 (outpatient treatment) and 623220 (residential treatment) to create the total number of treatment centers. The number of BHD treatment centers is lagged by one year.

Figure 3: Effect of BHD treatment centers on police officer assaults: Local event study (event defined as an <u>increase</u> in the number of treatment centers), 1998-2016



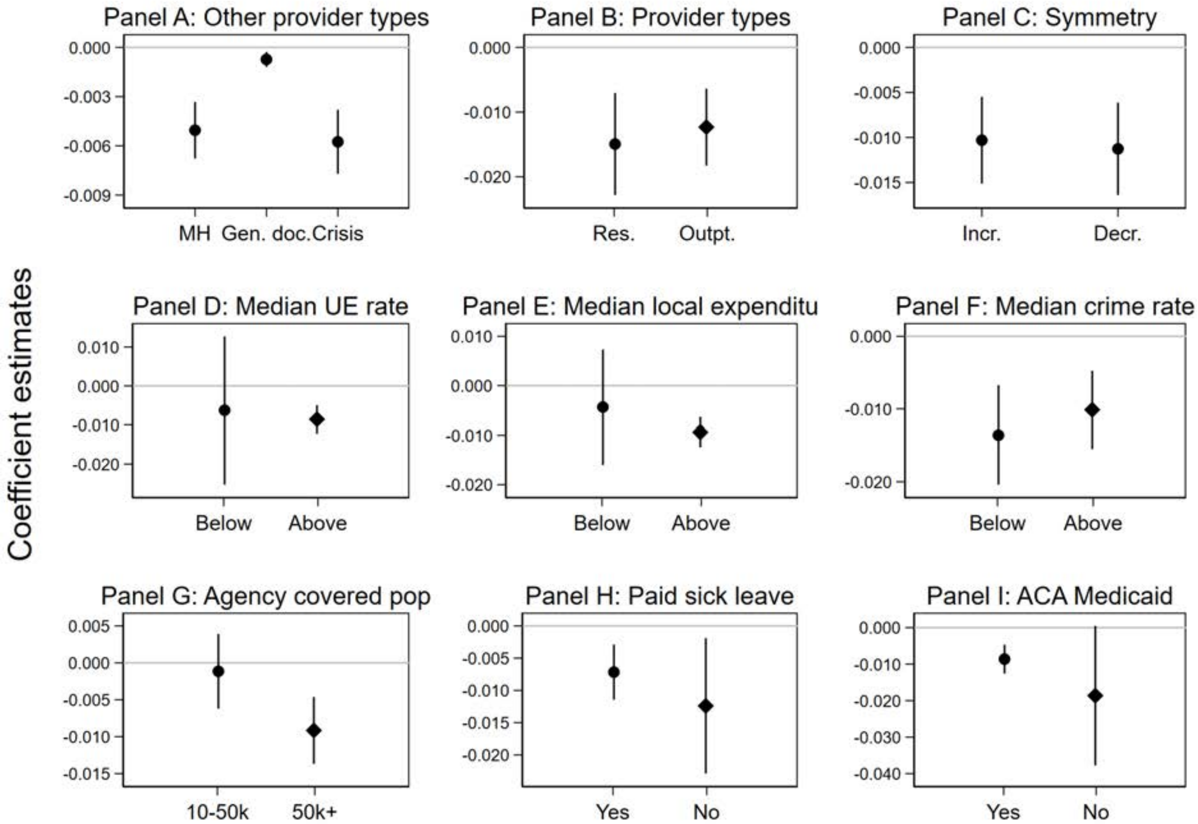
Between 1998 and 2016, there are 14 event-specific groups and we stack all of the event-specific data into one long data. Our final data is comprised of 1,192 event-specific treated agencies in 807 treated counties and 4,183 event-specific comparison agencies in 3,692 comparison counties. We use contemporaneous values to define the event year. The regression is estimated with least squares and control for agency and county characteristics, event-specific state-by-year fixed-effects, and event-specific agency fixed-effects. Observations are weighted by the number of police officers. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering. On average, there are 2.56 officer assaults per 100 officers in a treated agency. The average dose, weighted by the number of officers, is an increase of 1.9 centers.

Figure 4: Effect of BHD treatment centers on police officer assaults: Local event study (event defined as a decrease in the number of treatment centers), 1998-2016



Between 1998 and 2016, there are 14 event-specific groups and we stack all of the event-specific data into one long data. Our final data is comprised of 657 event-specific treated agencies in 470 treated counties and 4,183 event-specific comparison agencies in 3,692 comparison counties. We use contemporaneous values to define the event year. The regression is estimated with least squares and control for agency and county characteristics, event-specific state-by-year fixed-effects, and event-specific agency fixed-effects. Observations are weighted by the number of police officers. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering. On average, there are 2.59 officer assaults per 100 officers in a treated agency. The average dose, weighted by the number of officers, is a decrease of 1.5 centers.

Figure 5: Effect of BHD treatment centers on police officer assaults: Heterogeneity and the importance of other healthcare providers, 1999-2017



The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering. In Panels A, B, and D through I, each coefficient estimate is calculated in a separate regression. In Panel C, the coefficient estimates are calculated in a single regression that includes variables for periods of increases and decreases in the number of centers.

Table 1: Summary statistics: 1999-2020

Dependent variables (per 100 officers)	Agency-level (1)	County-level (2)
Total officer deaths and assaults	2.86	-
Felonious killings	0.01	-
Injurious assaults	2.85	-
Treatment variables		
Total centers last year	79.00	30.33
Outpatient centers	41.76	15.02
Residential centers	37.24	15.31
Agency-level variable		
Logged population	12.24	-
County-level variables		
% Male	48.87	49.51
% Black	15.31	10.79
% Hispanic	18.32	13.66
% Other	3.49	2.45
% Aged 18 and under	24.69	24.53
% Aged 65 and above	13.40	14.87
% Less than high school	15.88	16.95
% Some college	27.20	28.33
% College and more	30.95	24.71
Unemployment rate	6.08	6.59
Poverty rate	14.28	14.05
Observations	84,884	35,112

The data are the combined LEOKA and CBP. The unit of observation in Column (1) is an agency in a county in a state in a year and the unit of observation in Column (2) is a county in a state in a year. Observations are weighted by the number of police officers at the unit of observation level.

Table 2: Effect of BHD treatment centers on mortality, BHD treatment admissions, and crime: Evidence on the first stage

Panel A: Deaths rates by suicide (MCOD 1999-2017)	
Mean (per 100k)	12.1
Centers, t-1	-0.010***
	(0.002)
N	58,957
Panel B: Deaths rates by substance use (MCOD 1999-2017)	
Mean (per 100k)	12.6
Centers, t-1	-0.022**
	(0.007)
N	58,957
Panel C: Treatment admissions (N-SSATS 1999-2012)	
Mean	10,959
Centers, t-1	31.781***
	(5.515)
N	25,610
Panel D: Total crime rates (UCR-Known 1999-2017)	
Mean (per 100k)	3,834.9
Centers, t-1	-1.959**
	(0.900)
N	70,897
Panel E: Violent crime rates (UCR-Known 1999-2017)	
Mean (per 100k)	562.9
Centers, t-1	-1.280***
	(0.219)
N	70,897
Panel F: Property crime rates (UCR-Known 1999-2017)	
Mean (per 100k)	3272.1
Centers, t-1	-0.679
	(0.841)
N	70,897

Treatment centers are lagged one year and are measured at the county-level. The unit of observation in Panels A to B is a county in a state in a year and the sample of analysis is balanced at the county-level. The unit of observation in Panel C is a county in a state in a year and the sample of analysis is balanced at the county-level. The unit of observation in Panels D to F is an agency in a county in a state in a year and the sample of analysis is unbalanced at both agency and county-levels. All regressions control for county characteristics, state-by-year fixed-effects, and unit of observation geographical-level fixed-effects. We do not include the logged population in these analyses. In all Panels except in Panel C, we estimate least squares regressions. In Panel C, we estimate a Poisson regression using the county population as the exposure variable, we scale the number of centers by four prior to estimating the regression. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 3: Effect of BHD treatment centers on police officer assaults

	(1)	(2)	(3)	(4)
Panel A: 1999-2017 (Mean = 2.8 per 100 officers)				
Centers, t-1	-0.011***	-0.010***	-0.009***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.002)
N	73,755	73,755	73,755	73,755
Panel B: 1999-2020 (Mean = 2.9 per 100 officers)				
Centers, t-1	-0.009***	-0.008***	-0.007***	-0.007***
	(0.003)	(0.003)	(0.003)	(0.003)
N	84,884	84,884	84,884	84,884
Agency and state-by-year FEs	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	Yes
Socio-economic characteristics	No	No	Yes	Yes
Agency population (logged)	No	No	No	Yes

The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency fixed-effects and state-by-year fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 4: Effect of BHD treatment centers on police officer assaults: Alternative imputing values, 1999-2020

Sample:	Impute as 0	Impute as 1	Impute as 2	Drop imp. CBP	Drop imp. LEOKA
Mean (per 100k)	2.9	2.9	2.9	2.9	2.9
Centers, t-1	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	84,884	84,884	84,884	78,075	84,150

The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. Starting in 2017, the US Census Bureau suppressed county-NAICS-year cells with less than three establishments. We impute cells with missing data with a value of zero, one, and two. In the last two columns, we exclude counties with suppressed cells and include agencies with zero police employment, respectively. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 5: Effect of BHD treatment centers on police officer assault types: 1999-2017

Outcome:	Injurious assaults	Felonious killings
Mean (per 100 officers)	2.798	0.006
Centers, t-1	-0.009***	0.000
	(0.002)	0.000
N	73,755	73,755

The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency fixed-effects and state-by-year fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 6: Effect of BHD treatment centers on police officer assaults: Alternative measures and controlling for total crime, 1999-2017

	Outcomes
Panel A: Number of centers per 100,000 residents	Ln(count+1)
Mean	1.8
Centers, t-1	-0.0128***
	(0.0047)
N	73,755
Panel B: Number of total employment	rate
Mean (per 100 officers)	2.8
Employment,t-1	-0.0003***
	(0.0001)
N	73,755
Panel C: Controlling for crimes	rate
Mean (per 100 officers)	2.8
Centers, t-1	-0.007***
	(0.002)
N	73,755

The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county levels. In Panel A, the treatment variable is the lagged number of behavioral health treatment centers per 100,000 county residents and the outcome variable is the natural log of the number of assaults (adding one to avoid dropping zero). In Panel B, the treatment variable is the lagged number of total employment at behavioral health treatment centers and the outcome variable is the number of assaults per 100 officers. On average, there are 1,796 employees in a county in a year during 1999-2017. In Panel C, the treatment variable is the lagged number of treatment centers and the outcome variable is the number of assaults per 100 officers. The treatment variable is measured at the county level. In Panel A, regressions control for county characteristics, state-by-year fixed-effects, and agency fixed-effects. In Panel B, regressions additionally control for agency characteristics as described in Equation 1. In Panel C, we additionally include the agency-level number of total crimes known to the police to the covariate list described in Equation 1. All regressions are estimated with least squares. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

For Online Publication

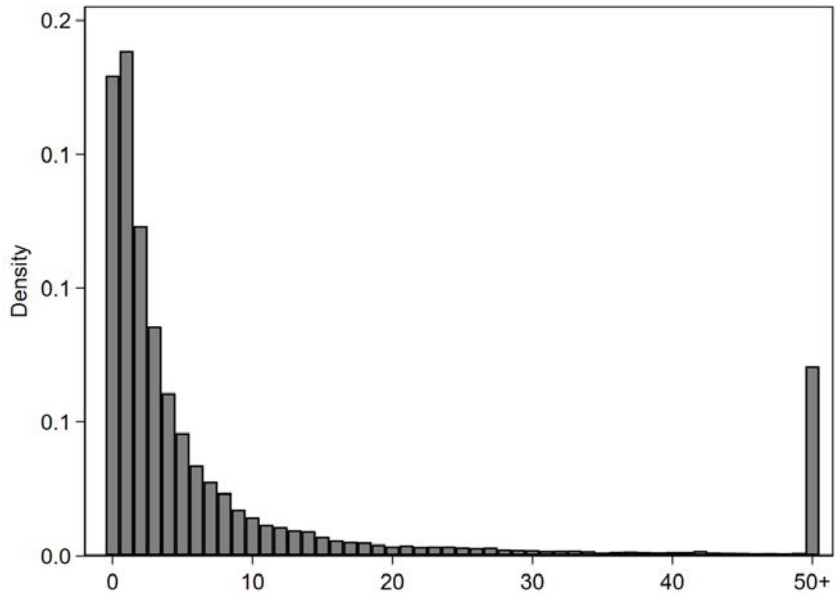
A) Additional figures and tables.

B) Data and procedures.

C) Robustness checks.

A Additional figures and tables

Figure A1: Histogram of the distribution of centers between 1999 and 2020



The number of treatment centers is lagged by one year.

Figure A2: Number of observations in each year between 1999 and 2020

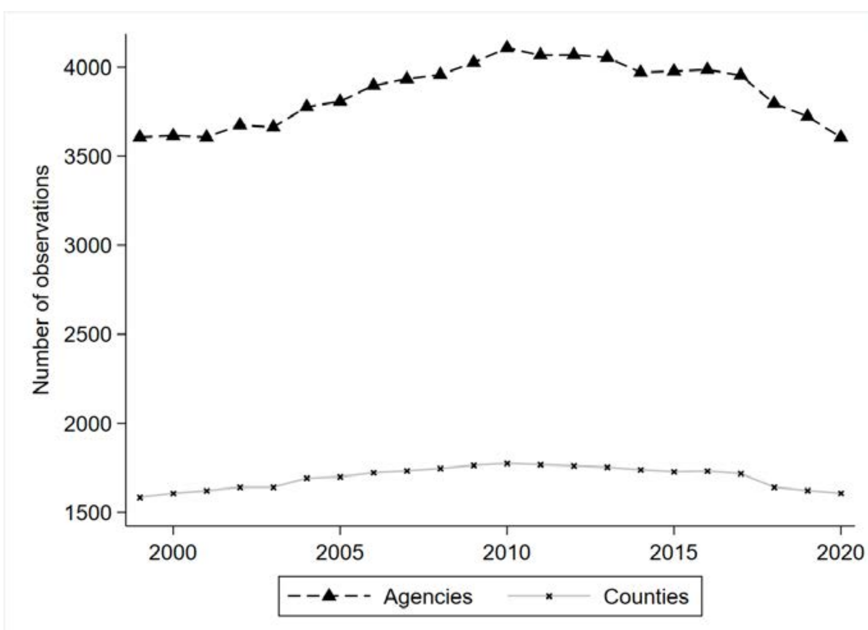


Table A1: Demographics of respondents without and with past year BHD treatment in specialized outpatient or residential centers: 2020

Sample:	No treatment	Treatment
12 to 18 years	0.11	0.024
19 to 34 years	0.25	0.32
25 to 64 years	0.44	0.57
65+ years	0.20	0.089
Male	0.49	0.41
Female	0.51	0.59
White race	0.62	0.69
Black race	0.12	0.13
Other race	0.088	0.066
Hispanic	0.17	0.11
Below the federal poverty level	0.15	0.30
Assistance program acceptance	0.18	0.39
Any health insurance	0.90	0.93
Private insurance	0.64	0.40
Medicaid or CHIP insurance	0.17	0.38
Medicare insurance	0.23	0.24
Military insurance	0.047	0.12
Very good or excellent health	0.75	0.68
Tobacco product use in the past year	0.22	0.47
Alcohol use in the past year	0.63	0.69
Illicit drug use in the past year	0.20	0.51
Observations	31,548	834

The data are the 2020 National Survey on Drug Use and Health (NSDUH). The unit of observation is a respondent. Observations are weighted NSDUH-provided survey weights.

Table A2: Treatment settings & services provided specialized outpatient and residential BHD treatment centers: 2020

Treatment settings	
Residential	0.23
Partial hospitalization	0.14
Outpatient	0.78
Services provided	
Individual, group, or family therapy	0.95
Psychotropics	0.54
Behavioral modification	0.70
Assertive community treatment	0.11
Case management	0.63
Housing services	0.22
Education services	0.29
Psycho-social rehabilitation	0.41
Vocational rehabilitation	0.15
Employment services	0.15
Legal advocacy	0.044
Peer support services	0.29
Court-ordered outpatient treatment	0.50
Observations	6,988

The data are the 2020 National Mental Health Services Survey. The unit of observation is a center in a county in a state in a year. Observations are unweighted.

Table A3: Summary statistics: Local event study samples, 1998-2016

Sample:	Comparison	'Increase' pre-treatment	'Decrease' pre-treatment
Total officer deaths and assaults	1.73	2.56	2.59
Treatment variables			
Total centers, current period	0.99	6.23	6.69
Agency-level variable			
Logged population	10.14	10.83	10.62
County-level variables			
% Male	49.65	49.15	49.48
% Black	13.34	12.32	11.47
% Hispanic	7.56	10.68	9.45
% Other	1.21	1.93	1.52
% Aged 18 and under	25.69	26.36	25.97
% Aged 65 and above	14.50	12.80	13.25
% Less than high school	19.88	17.16	18.28
% Some college	27.34	28.19	27.90
% College and more	17.38	23.43	20.96
Unemployment rate	6.88	6.15	6.07
Poverty rate	15.49	13.28	14.03
N	22,152	2,421	1,410

The data are the combined LEOKA and CBP. Observations are weighted by the number of police officers.

Table A4: Test of balance, 1999-2017

Variables	1999-2017
Agency-level covariates	
Log population	-0.000
	(0.000)
N	73,755
County-level covariates	
Male	-0.001
	(0.001)
Black	-0.004
	(0.004)
Hispanics	-0.011
	(0.007)
Other racial groups	0.003
	(0.002)
Aged under 18	-0.004
	(0.003)
Aged over 65	-0.007**
	(0.003)
Less than HS	-0.007*
	(0.004)
Some college	-0.011*
	(0.006)
College or more	0.012**
	(0.005)
Unemployment rates	-0.003
	(0.002)
Poverty rates	-0.009**
	(0.004)
N	32,404

Each row reports results from a separate regression. The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Behavioral health disorder treatment centers are lagged one year and are measured at the county-level. Regressions are estimated with least squares and control for state-by-year fixed-effects, and covariate-level fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each row is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table A5: Migration testing and TSDID, 1999-2017

Panel A: Migration test (CPS)	
Mean	0.05
Centers, t-1	0.000
	(0.000)
N	4,735
Panel B: TSDID (LEOKA)	
Mean (per 100 officers)	1.99
Centers, t-1	-0.148*
	(0.089)
N	12,293

The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level and are reported in parentheses. Each cell is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table A6: Effect of BHD treatment centers on police killing of civilians: Fatal Encounters, 2000-2017

	Total	White	Black	Other Race
Mean (per 100k)	0.04	0.27	0.91	0.38
Centers, t-1	-0.00005	-0.00057***	-0.00033	0.00003
	(0.00003)	(0.00021)	(0.00097)	(0.00037)
N	55,854	55,854	55,854	55,854

The data are the combined Fatal Encounters and CBP and the unit of observation is a county in a state-year. Treatment centers are lagged one year. All regressions are estimated with least squares and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by population for each respective race or group. Standard errors are clustered at the county-level and are reported in parentheses. Each column is a separate regression.
***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively

B Data and procedures

Dataset construction

In constructing the analysis sample, we make several exclusions. Table B1 reports the number of observations retained in the sample in each step. Among the 476,520 observations from LEOKA 1999-2020, we have 108,893 observations after we restrict the sample to law enforcement agencies that cover at least 10,000 residents. Since agencies are advised to report an indicator variable for whether there are zero officers killed or assaulted in the UCR Part I and not to submit to LEOKA when there are no officers killed or assaulted,[1] we cross-check LEOKA with the UCR data to replace the missing value with a true zero when applicable and also to note which observations have discrepancies in such cross-check. Discrepancies in the cross-check arise when LEOKA indicates zero officers killed or assaulted but the UCR data indicator reports a non-zero number.

The number of agencies reporting zero police employment has increased non-trivially since 2018. The FBI does not discuss this increase in LEOKA data documentation and thus we are not certain of the cause(s). However, we do not include these years in our main analysis sample (1999-2017). As we show in Section 4.2, our results are robust to excluding these years suggesting that whatever phenomenon drives this reporting pattern does not impact our results.[2] We then proceed to impute zero police employment in the LEOKA data with a non-zero number obtained from the Annual Survey of Government Employment (ASG) in the years where ASG is available.[3] Finally, we drop agencies with more than 15 imputed ASG values.[4] and exclude agencies with zero officer employment and extreme outliers and record errors for (1) police officer employment and (2) total officer assaults using a regression-based approach (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019; Chalfin et al., 2022). We describe this approach in detail in the Appendix. Out of a total of 108,893 observations from agencies with at least 10,000 residents between 1999-2020, we base the study on 89,884 observations, as we lose 17% of the observations to data cleaning. Figure A2 shows the number of agencies and counties each year in our analysis sample. We note

[1]Please see `https://ucr.fbi.gov/additional-ucr-publications/ucr_handbook.pdf`, last accessed 9/17/2022.

[2]The police employment number is particularly relevant since we use this number as a denominator for the dependent variable. We also observe that there is a drop in the number of agencies reporting zero employment in 2011 and between 2014 and 2017, and instead reporting one in these years in California. We thus replace agencies with one officer in these years as zero.

[3]We thank Arron Chalfin for sharing the ASG employment data prior to 2018. We obtain the 2019-2020 ASG employment data from the Census Bureau (U.S. Census Bureau, 2022b) The ASG data include information on all state and local government employees in years ending in '2' and '7,' and a sample of state and local governments in the intervening years. Please see `https://www.census.gov/programs-surveys/apes/about.html`, last accessed 10/10/2022.

[4]The majority of these agencies appear in Pennsylvania.

that there is a drop in the number of observations beginning in 2018 due to excluding the agencies with zero police officer employment. The final 1999-2020 analysis dataset includes an unbalanced panel of 84,884 agency-year observations spanning 1,848 of 3,143 counties and 50 states in the U.S.[5] Table B2 shows the number of unique agencies by observation frequency and we observe the majority of agencies 20 times or more.

Identifying Outliers

LEOKA datasets are voluntarily reported by police departments and are known for having issues with reporting and measurement (Chalfin and McCrary (2018)). First, we follow Chalfin and McCrary (2018) and Chalfin et al. (2022) to impute the UCR police employment measure for 2003. Next, we follow prior papers that clean these outcomes for outliers ((Evans and Owens, 2007; Mello, 2019; Weisburst, 2019; Chalfin et al., 2022)). We use the sample period 1990 to 2020 to better capture trends and predict outliers. Specifically, we separately regress the number of police assaults and police officer employment on a polynomial cubic time trend for each agency and calculate the percent deviation of the actual value from the values predicted by this regression (the outcomes used for this exercise are the raw values plus one). We then summarize separately the absolute value of these percent deviations within (i) agency and (ii) county population groups (of $\leq$ 10k, 10k-25k, 25k-50k, 50k-100k;100k-250k; and $\geq$ 250k residents in 1990). Outliers are identified if the value is greater than the 97.5^{th} percentile of this distribution or 50%, whichever is larger.

[5] We exclude D.C. as that locality has a single police agency which is perfectly collinear with the state-by-year fixed-effects included in our regression (see Section 3.3).

Table B1: Sample construction: LEOKA

Exclusions to the sample:	Observations
LEOKA 1999-2020	476,520
Drop if	
Missing or population with less than 10k	108,893
Agencies covered by another or agencies with missing officer employment, deaths, or assaults	107,788
Missing county identifiers or county boundary changed	106,581
'Real' missing data in LEOKA	99,990
Agencies use ASG officer employment more than 15 times	98,764
The number of observations being less than 4 for outlier identification	98,746
Identified as outliers or zero officer employment	84,914
Singletons and D.C.	84,884

Table B2: Number of unique agencies by frequency of observations, 1999-2020

Frequency of observations	Number of unique agencies	Frequency of observations	Number of unique agencies
2	3	13	61
3	8	14	64
4	16	15	111
5	13	16	99
6	14	17	142
7	26	18	190
8	31	19	295
9	45	20	573
10	45	21	1,063
11	66	22	1,435
12	75		

C Robustness checks

We report a series of robustness checks. Reassuringly, our findings are stable.

C.1 Alternative samples

We first hold the specification constant and estimate Equation 1 in different samples (Figure C1). In particular, we retain only agencies with only 15 or more observations over the study period;[6] and observations below the 99th percentile of the on-duty officer assault distribution to ensure that outliers do not drive our findings. We then aggregate the data to the county-year level using agencies reporting every year (without dropping outliers) during our study period following Deza et al. (2022a). We next remove county-year observations with no behavioral health treatment centers and remove agencies with no on-duty officer assault over our study period.

C.2 Alternative specifications

Next, we hold our sample constant and vary the specification (Figure C1). We control for county-level expenditures (in $2020) on social programs (proxied by payroll).[7] The purpose of including these variables is to control for public investments in treatment centers using local resources.[8] We do not include these expenditure variables in the main regression for two reasons: (i) these data are based on a survey of counties and thus we linearly impute missing values (23% of the total sample) to avoid losing observations, and (ii) these variables could be outcomes of the centers and including them in the regression could lead to over-controlling bias. We also control for the number of civilian employees to proxy agency size, state-specific linear time trends, and state-specific quadratic time trends. We cluster standard errors at the level of the state (vs. county) as there may be correlated shocks experienced by counties within the same state (e.g., many private and public insurance regulations related to BHD treatment are set at the state level). We next use the total officer and civilian employees and the agency-covered population as the weights and also estimate an unweighted regression. We construct the dependent variable as the number of assaults per 10,000 agency-covered

[6] Political scientists note the importance of state government capacity for ensuring accurate reporting of administrative data, including crime data (Cook and Fortunato, 2023). By focusing on agencies that regularly report LEOKA data, we expect to minimize measurement error attributable to agencies that report poor quality data to the state.

[7] In particular, we include the sum of police, streets and highways, health, and education. We obtain the data from (Kaplan, 2021) and U.S. Census Bureau (2022b).

[8] Expenditures may take time to translate into treatment centers and improvements in BHD outcomes, which would not be captured by the contemporaneous expenditures we include in this regression. However, our results are robust to including one- or two-year lagged county-level expenditures.

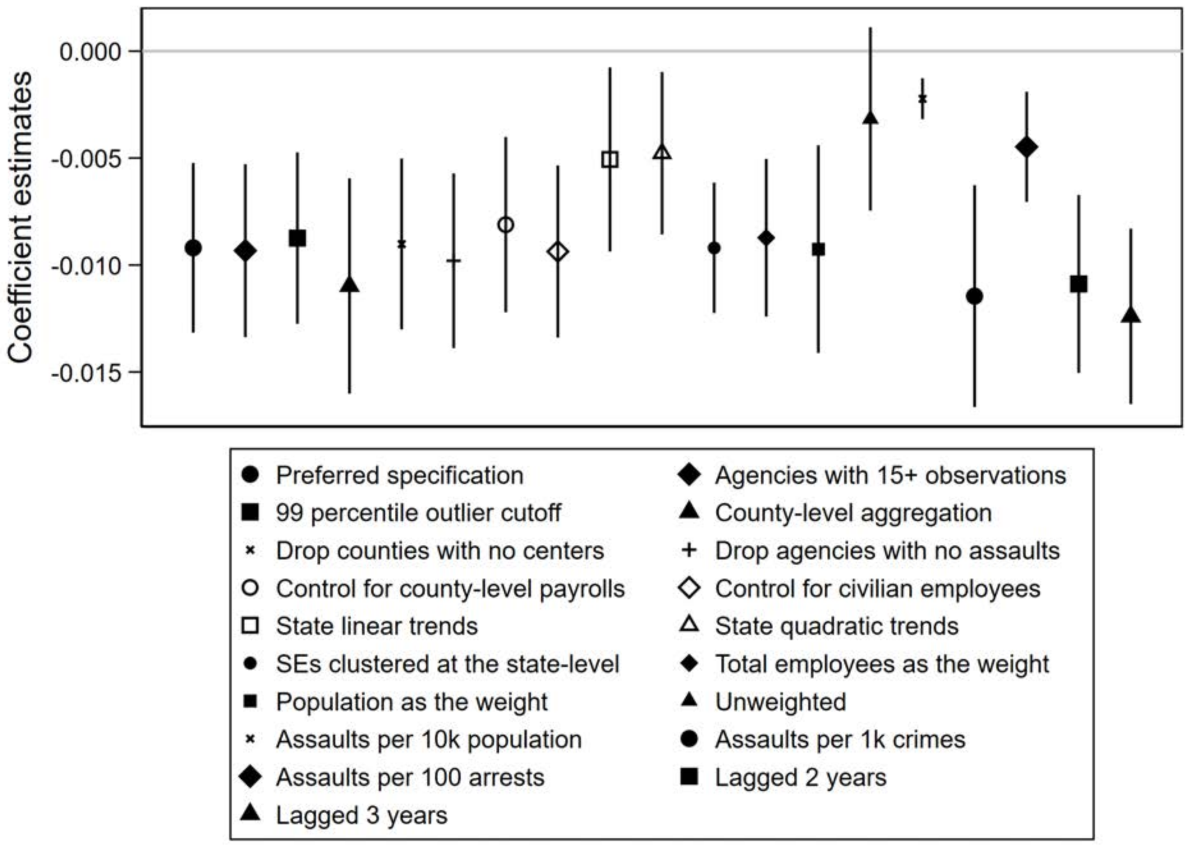
population; 1,000 crimes; or 100 arrests and use different center lag structures (i.e., two-year and three-year lags).

In Figure C2, we report results based on the logged rate of on-duty injuries per agency (we add a value of one to all observations before taking the log) and we estimate a Poisson regression (unweighted and weighted; the agency-level number of officers serves as the exposure variable).[9] In Figure C3, we sequentially drop agencies in each state ('leave-one-out' analysis) to ensure that the unique experiences of particular states do not drive our findings. The coefficient estimates from each of the leave-one-out samples are relatively homogeneous.

Finally, we take a different approach to statistical inference. In particular, we randomly re-shuffle the number of centers across counties and time, keeping constant the number of counties in each state. We re-shuffle 1,000 times and obtain 1,000 placebo t-statistics. We plot the placebo t-statistics in Figure C4. MacKinnon and Webb (2020) demonstrate that t-statistics have better analytic properties than estimated coefficients in this setting. The solid lines denote the 5th and 95th percentiles of the distribution. The dashed line is the estimated t-statistics value from the actual regression. We can see clearly that the estimated t-statistics are below the 5th percentile of the distribution.

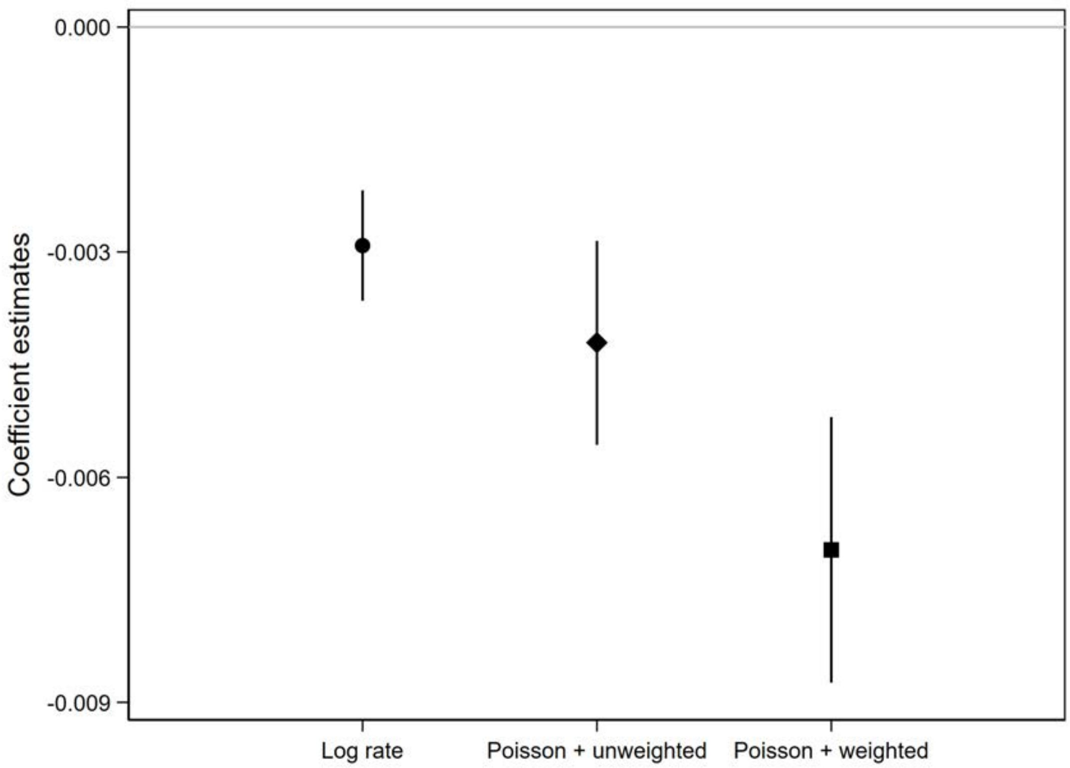
[9] We implement the Poisson regression using the *ppmlhdfe* command by Correia et al. (2020).

Figure C1: Effect of BHD treatment centers on police officer assaults: Robustness checks, 1999-2017



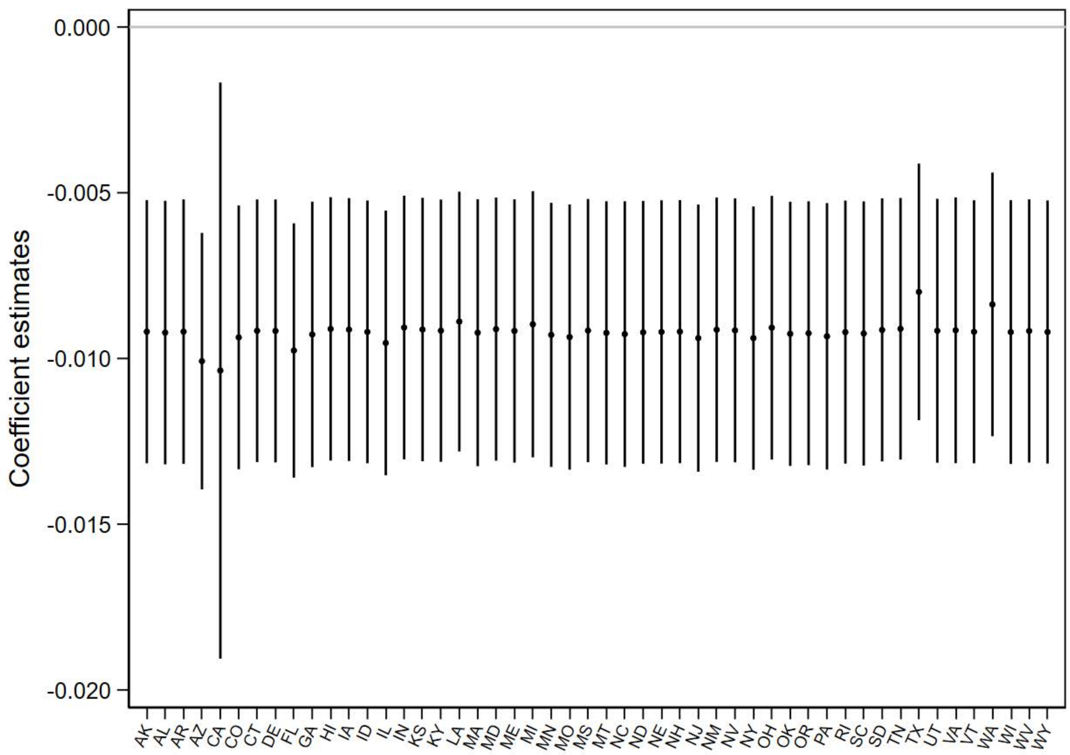
The data are the combined LEOKA and CBP. The unit of observation is an agency in a county in a state in a year, otherwise noted. The sample of analysis is unbalanced at both agency- and county-level, otherwise noted. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects, unless otherwise noted. Observations are weighted by the number of police officers, unless otherwise noted. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering, unless otherwise noted.

Figure C2: Effect of BHD treatment centers on police officer assaults: Alternative functional forms, 1999-2017



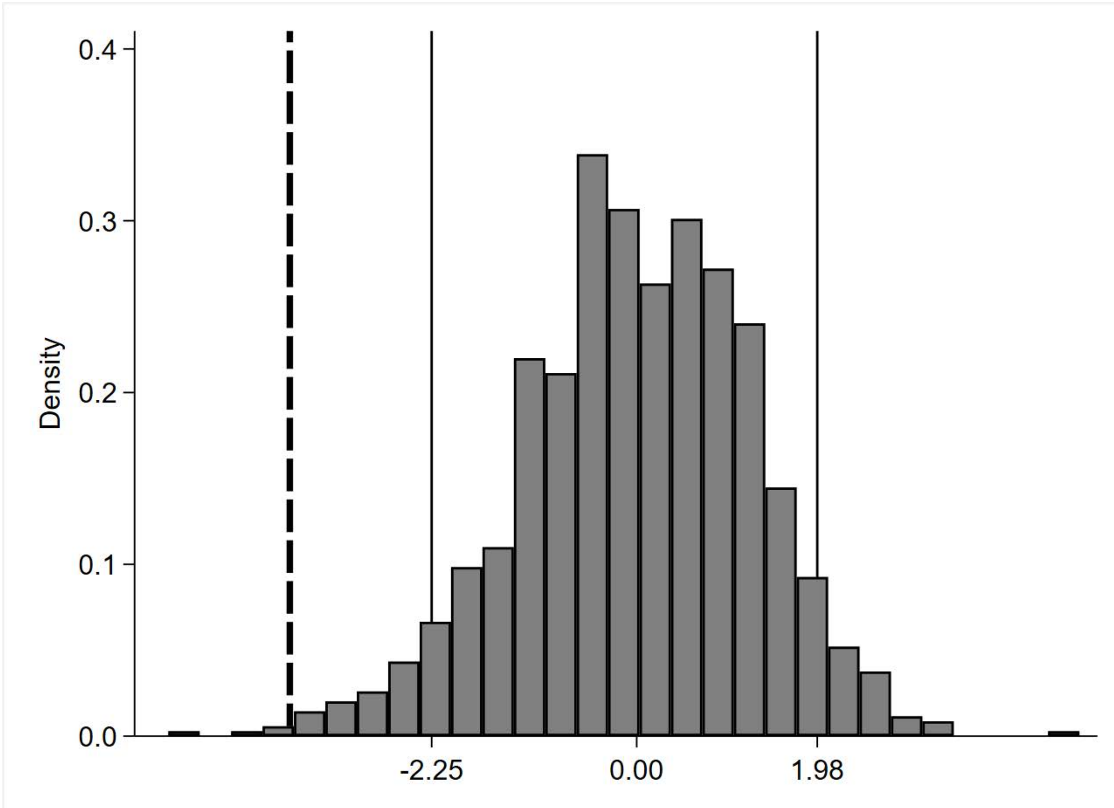
The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers, otherwise noted. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering level.

Figure C3: Effect of BHD treatment centers on police officer assaults: Leave one out analysis, 1999-2017



The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers. Circles represent point estimates and vertical lines depict 90% confidence intervals that account for within-county clustering level.

Figure C4: Effect of BHD treatment centers on police officer assaults: Placebo test, 1999-2017



The data are the combined LEOKA and CBP and the unit of observation is an agency in a county in a state in a year. The sample of analysis is unbalanced at both agency and county-levels. Treatment centers are lagged one year and are measured at the county-level. Treatment centers are randomly re-shuffled across county and time, keeping constant the number of agencies in each county. This procedure is repeated 1,000 times and obtain the null distribution of the placebo t-statistics for the main outcome. All regressions are estimated with least squares and control for agency and county characteristics, state-by-year fixed-effects, and agency fixed-effects. Observations are weighted by the number of police officers. Standard errors are clustered at the county-level. The x-axis reports the t-statistic value. The solid lines denote the 5th and 95th percentiles of the distribution. The dashed line is the estimated t-statistics from the actual regression.