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ABSTRACT

We estimate the effect of local access to office-based mental healthcare on juvenile arrest outcomes. We leverage variation in the number of mental healthcare offices within a county over the period 1999 to 2016 in a two-way fixed-effects model. Office-based treatment is the most common modality of mental healthcare received by juveniles. We find that ten additional office-based mental healthcare providers in a county leads a decrease of 2.3% to 2.6% in the per capita costs to society of juvenile arrest. Findings are similar for arrest rates although often less precise, which suggests that accounting for social costs is empirically important. Crime imposes substantial costs on society and individuals, and interventions during early life can have more pronounced effects than those received at later stages, therefore our results imply increased juvenile access to mental healthcare may have an unintended benefit for the current and future generations.

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1 Introduction

Criminal behaviors are common among juveniles with the prevalence of committing criminal activities peaking during adolescence. Among industrialized countries, the United States, the focus of our study, is the leader in terms of the juvenile incarceration rate (Annie E. Casey Foundation (2013)) with over 70,000 juveniles in custody on a given day and an average direct annual cost of $112,000 per juvenile (Office of Juvenile Justice and Delinquency Prevention (2011); Annie E. Casey Foundation (2011)).[1] In 2019, juveniles accounted for 7.1%, 9.8%, and 11.2% of total, violent, and property arrests in the U.S. (Federal Bureau of Investigation (2020)).[2] For the juvenile, being convicted of a crime and associated penalties impedes education, health, and social development, and later earnings. Further, society loses future productivity, tax revenues, and so forth from the incarceration of juveniles. Collectively, these costs are estimated to be $9.8B to $26.7B per year (Justice Policy Institute (2014)).[3]

Mental health disorders (MHDs) are the most costly health condition among juveniles, accounting for $16.7B in 2021 with nearly half of this treatment financed by Medicaid (a public insurance program for the poor in the U.S. (Soni (2018)).[4] The most common MHDs among juveniles are anxiety, depression, and conduct disorder (Ghandour et al. (2019)), with prevalence rates of 6.1%, 10.5%, and 7.5% among 12 to 17 year olds in 2018. Alarmingly, while these conditions can be mitigated with early diagnosis and treatment (Satcher (2001)), juvenile MHDs are increasing over time (e.g., the juvenile suicide rate has increased each year since 2010 (Ruch et al. (2019)). The age profile of criminal trajectories mirrors development of MHDs closely, as adolescence is a period

[1] These numbers are inflated by the authors from the original estimate ($88,000 in 2008 dollars) to 2021 dollars using the Consumer Price Index.

[2] Some persons can be arrested for multiple crimes, thus the shares of juvenile arrests for violent and property crimes exceed the share of total.

[3] These numbers are inflated by the authors from the original estimates ($7.9B and $21.47 in 2011 dollars) to 2021 dollars using the Consumer Price Index.

[4] This figure is inflated by the authors from the original estimate ($13.8B in 2011 dollars) to 2021 dollars using the Consumer Price Index.

in which many MHDs emerge − for example, the median age of onset for both anxiety and impulse-control disorders is 11 years old (Kessler et al. (2005)). Correspondingly, criminal activity increases sharply during childhood (through age 12), peaks during adolescence (12 to 18 years), and then declines in the early twenties and beyond (Lee and McCrary (2017)).

In addition to following similar age profiles, there are several mechanisms through which MHDs can affect crime. Indeed, previous research documents a strong association between such disorders and crime (Cascardi et al. (1996); Swanson et al. (2001); James and Glaze (2006); Frank et al. (2011)). First, common features of MHDs, such as disordered thinking, may lead to violent interactions if an individual with a MHD perceives a regular interaction as threatening. Common features of more 'criminogenic' MHDs include aggression towards other people and animals (e.g., bullies, threatens, or intimidates others; physical fighting; using a weapon; enjoys being cruel to others; forcing others into sexual activity; and lack of empathy or remorse); destruction of property (e.g., deliberately destroying or setting fire to the property of others); stealing and lying; and violation of rules (e.g., running away from home) (The American Academy of Child and Adolescent Psychiatry (2018)).[5] Even anecdotally less criminogenic MHDs, such as anxiety, cause compulsion, obsession, and tantrums (The American Academy of Child and Adolescent Psychiatry (2017)), which may ultimately lead to violence. Second, individuals with MHDs may be more prone to 'self-medicate' with substances, and substance use is associated with increased risk for crime commission (Khantzian (1987)). Third, people with MHDs may be more likely to be victimized if offenders perceive them as vulnerable, thus reducing the ability of an affected juvenile to accurately assess their safety, and so forth, and increasing victimization propensity (Hiday et al. (1999); Hiroeh

[5]As a specific example, the American Academy of Child and Adolescent Psychiatry defines conduct disorders as: '...a group of repetitive and persistent behavioral and emotional problems in youngsters. Children and adolescents with this disorder have great difficulty following rules, respecting the rights of others, showing empathy, and behaving in a socially acceptable way (The American Academy of Child and Adolescent Psychiatry (2018)).'

et al. (2001); White et al. (2006)). Finally, the 'criminalization' of MHDs may increase the probability of being arrested, holding actual criminal behaviors constant, as police may mis-interpret symptoms of a MHD as criminal behaviors.

Previous literature provides evidence that access to certain modalities of mental healthcare (e.g., prescription spending, providing a psychiatric hospitalization, or expanding access) decreases crime. In particular, Cuellar and Markowitz (2007) find that increasing Medicaid spending on prescriptions for antidepressants and stimulants during the 1990s and 2000s led to a reduction in violent crimes. Landersø and Fallesen (2021) show short-run declines in crime following a psychiatric hospitalization. Three recent studies show that expanding access to mental healthcare treatment, measured by the number of providers within the local healthcare market, reduces overall crime (Wen et al. (2017); Bondurant et al. (2018); Deza et al. (2021)).

Despite the fact that adolescence is a period that merits separate analysis due to the age profile of both development of MHDs and crime, the literature focusing on mental healthcare as a tool to reduce crime among juveniles is limited. Within the juvenile justice system, Cuellar et al. (2004) show that mental health treatment reduces detention rates among children in foster care and Cuellar et al. (2006) document that being assigned to a mental health diversion program (vs. proceeding through the criminal justice system) delays youth recidivism. Similarly, Dalsgaard et al. (2014) find that attention deficit hyperactivity disorder treatment, in particular medication-based treatment, reduces youth interactions with law enforcement. While not targeting MHDs *per se*, Heller et al. (2017) provide evidence from a randomized control trial that a program with components of cognitive behavioral therapy − used to treat anxiety, conduct disorder, depression, and other MHDs, targeted to at-risk juveniles reduces total arrests by 28% to 35% and violent crime arrests by 45% to 50%.

Given this background, we empirically explore how increasing local access to a common modality of mental healthcare, office-based treatment, affects juvenile crime. We proxy crime with arrest outcomes. A large clinical literature establishes the effectiveness of numerous treatment modalities for management of most major MHDs among juveniles (American Psychiatric Association (2006); Knickman et al. (2016)).[6] Office-based mental healthcare providers include psychologists, psychiatrists, and social workers who deliver care in private offices. Treatment can include the use of psychotropics (i.e., medications used to treat MHDs), counselling services (e.g., cognitive behavioral therapy and other forms of 'talk therapy'), and 'wrap-around' services (e.g., crisis management plan development).[7] This modality of care reflects the majority (59%)[8] of mental healthcare received by juveniles and its prevalence has increased over time in the U.S.[9]

Despite established effectiveness, treatment is arguably underused in the U.S. Further, juveniles with MHDs are less likely to receive treatment than any other age group (McGorry and Mei (2018)). For example, while nearly two-thirds of adults in 2018 with a past-year major depressive episode received treatment for depression, only half of juveniles with this condition received any related care (NSDUH (2018)). While there are myriad reasons for failure to receive treatment, shortages of mental healthcare providers are likely salient to many juveniles and their families. Offering credence to this hypothesis, one of the commonly cited barriers to accessing services is the lack of mental healthcare providers (NSDUH (2018)). In addition, estimates suggest that nearly 35% of U.S counties have no licensed psychologists and nearly 60% do not have a single psychiatrist (Lin et al. (2016); New American Economy (2017)). A potential strategy to improve treatment receipt is thus to increase the number of mental healthcare providers.

The objective of our study is to provide evidence on expanding access to office-based

[6] While many modalities are effective, optimal treatment varies across patients. As stated by the National Alliance on Mental Illness, there is no 'one size fits all' treatment for MHDs (National Alliance on Mental Ilness (2020)).

[7] Other settings of formal mental healthcare are psychiatric hospitals, community mental health centers, and specialized outpatient facilities and residential facilities. Increasingly, mental healthcare is being delivered in primary care settings as well.

[8] Authors' calculations based on the 2016 National Survey on Drug Use and Health.

[9] Such care comprised 24% of total mental healthcare expenditures in 1986 and that number increased to 44% in 2014 (SAMHSA (2016)). We calculate these numbers by taking the sum of payments for prescriptions filled at retail pharmacies and office-based professionals listed in Exhibit 14.

mental healthcare on juvenile arrest outcomes. We estimate two-way fixed-effects regression models using county-level juvenile arrest from the Federal Bureau of Investigation Uniform Crime Reporting over the period 1999 to 2016. We use two juvenile arrest measures: (i) per capita arrest costs and (ii) arrest rate per 1,000 juveniles. The former is our preferred measure as per capita costs account for differences across arrests in their costs to society (Chalfin and McCrary (2018)). To measure treatment access, we utilize variation in the county-level number of office-based mental healthcare providers obtained from the U.S. Census Bureau County Business Patterns.

Our results show that increasing local access to office-based mental healthcare providers leads to a reduction in county-level juvenile arrest outcomes. In particular, we find that ten additional office-based mental healthcare providers per county reduces the social costs of juvenile arrests by 2.3% to 2.6%. Declines are observed for violent and property arrest costs, although the latter finding is sensitive to specification. Findings are similar for arrest rates although often less precise, which suggests that accounting for social costs is empirically important. We provide evidence on the first stage: juvenile mental health (proxied by suicide rates) improves as access to office-based care increases. Given that interventions during early life can have more pronounced effects than interventions later in life, our modest declines in the number of juvenile arrests may understate the full benefits of such treatment across the lifecourse. Our findings imply that policies designed to increase investments in office-based mental healthcare treatment, especially for those at young age, may have unintended benefits in reducing crime.

2 Data and empirical strategy

2.1 Arrest data

We use Clearances by Arrests segments of the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR) monthly files for the period 1999 to 2016 to measure juvenile arrest outcomes.[10] All law enforcement agencies that operate under a U.S. jurisdiction, state, county, city, university/college, tribal, and federal law enforcement agencies, submit arrest data to the UCR, either through a state UCR program or directly to the FBI's UCR program. These files include arrests for the most commonly reported Part I violent crimes (homicide and manslaughter ['homicide'], rape, robbery, and aggravated assault) and Part I property crimes (burglary, larceny, and motor vehicle theft). We aggregate arrest counts by the age of the offender (18 years and under) to the primary county in which each police agency is located. In constructing the analysis sample, we restrict our data to agencies that report full 12 months in a given year and 18 years of data (Maltz and Targonski (2002)). The final dataset is a balanced panel of 878 counties in 46 states spanning 18 years. Although our analysis sample reflects a sub-set of all U.S. counties, we show in robustness checking that are our results are not sensitive to alternative sample selection rules.

Although arrests are potentially endogenous to the level of police enforcement and are self-reported by police agencies, our primary reason for using arrest data is that detailed age information is not available in the UCR offense known reports.[11] Therefore, we cannot use the offense known data for our study and instead must rely on arrests. Although arrests are not a perfect measure of juvenile criminal behavior and understate the true level of crime (Gould et al. (2002)), such data can serve as a reasonable representation of underlying criminal activity. Further, arrest data are commonly used within the economics literature as they offer the opportunity to isolate offender demographics and thus shed light on questions not otherwise possible to study (Kline (2012); Anderson (2014); Chu (2015); Deza and Litwok (2016); Dave et al. (2021)). Because we do not consider all crimes, instead only those crimes that lead to arrests, our results will likely underestimate the true gains (in terms of reduced crime) attributable to expanding local

[10] We use agency-monthly files prepared by the Inter-university Consortium for Political and Social Research (ICPSR). At the time of writing, ICPSR agency-monthly files are only available until 2016.

[11] The UCR known offense reports capture all offenses known to law enforcement, including offenses that lead to arrests (which we measure) and those that do not (which we do not measure).

access to office-based mental healthcare.

The arrests we study have heterogeneous costs to society. For example, in terms of criminal justice system and victim costs, a homicide is over 3,000 times more costly than a burglary (Chalfin and McCrary (2018)). From the perspective of the social planner, arrests that impose higher social costs are more important for improving welfare. Indeed, Chalfin and McCrary (2018) argue that the costs associated with reducing property crimes likely exceed the expected benefits to society while lowering violent crimes is socially optimal (i.e., expected benefits exceed expected costs). This backdrop − not all arrests are equally important in terms of social costs but resources must be allocated to reduce any arrests − impacts how we measure arrests in our study. Our preferred measure is one, described below, that explicitly accounts for differences in social costs across Part I crimes. We also, for comparison with much of the crime literature, report arrest rates which implicitly treat all arrests homogeneously in terms of their social costs.

To construct our preferred outcome variable, we adjust arrests according to their expected social costs using a procedure proposed by Chalfin and McCrary (2018). In particular, we use the following weights (see Table 1 in Chalfin and McCrary (2018)): homicide ($7,000,000), rape ($142,020), robbery ($12,624), aggravated assault ($38,924), burglary ($2,104), larceny ($473), and motor vehicle theft ($5,786). These weights capture the annual expected costs per capita for each arrest we study. We convert cost-adjusted arrests to per capita terms using population data from the National Cancer Institute's Surveillance Epidemiology and End Results program (SEER). We use the total population, rather than the juvenile population, as arrests impose costs on society as a whole. Costs are adjusted to 2016 dollars using the Consumer Price Index (CPI).

2.2 Office-based mental healthcare providers

Administrative data on the annual number of office-based mental healthcare provider offices in a county are drawn from the U.S. Census Bureau's County Business Patterns

(CBP). These data include the universe of establishments in the U.S. and are based on Internal Revenue Service (IRS) annual tax forms. An establishment is defined by the U.S. Census as a 'single physical location at which business is conducted or services or industrial operations are performed.' The CBP provides a 'snap shot' of the number of establishments in the week of March 12th in each year. Given the high penalties of inaccurately reporting information to the IRS − this action is a felony crime punishable by both fines and incarceration − and internal editing conducted by this Service when applying the U.S. tax code, we suspect that reporting errors are infrequent in the CBP.

The CBP uses the North American Industry Classification System (NAICS) six-digit codes beginning in 1998. Prior to 1998, Standard Industrial Classification (SIC) four-digit codes are used to classify establishments in the CBP. The four-digit SIC codes do not allow us to isolate office-based mental healthcare provider establishments from general healthcare establishments. Thus, we begin the study period in 1999 as we lag offices by one year as described in Section 2.3. Starting in 2017, counties with fewer than three establishments are suppressed for privacy concerns. This change leads us to close the panel in 2016. However, as we show in robustness checking, our results are not sensitive to using post-2016 years and imputing values for suppressed counties.

Further adding to our confidence that we have accurately recorded industry codes by using the CBP, while the IRS does not release specific details on how this Service determines which specific businesses will be audited in any given year, the IRS uses 'principle business codes,' reported by the filer, to screen tax returns for outlier businesses. Outliers are at elevated risk for an IRS audit. Therefore, businesses have an additional incentive (i.e., in addition to minimizing risks of fines and incarceration) to accurately report the principle business code: reducing the risk of an IRS audit. Tax experts encourage businesses to accurately report the principle business code for this reason.[12] These principle

[12] See for example: 'Although the formula for flagging taxpayers for audits remains a secret, a portion of the determination comes from statistical comparisons of financial ratios derived from tax returns filed. The IRS has a large comparison base and uses anomalies as one of many triggers. Choosing the correct principle business code will help you file numbers comparable to your peers

business codes are recorded in the CBP as the six-digit NAICS industry codes which we use to identify office-based mental healthcare providers. We note that establishments may change their principle business code (and thus their NAICS code in the CBP will also change) over time. Based on the above-noted reasons for accurate reporting on IRS forms and IRS internal editing of the data, this new coding would measure a true change in business structure that we want to incorporate into our analysis.

Following Deza et al. (2021), we use the NAICS codes 621112[13] to identify the number of establishments of office-based mental health physician providers (e.g., psychiatrists), and 621330[14] for office-based mental health non-physician providers (e.g., psychologists, psychoanalysts, and social workers) to proxy local access to these modalities. We refer to these establishments as 'offices.'

As noted by Deza et al. (2021), one or more physicians or non-physician providers delivering mental healthcare services can work in one unique establishment. Unfortunately, the employment information included in the CBP (and other comparable datasets such as the Quarterly Census of Wages and Employment [QCEW]) is altered for privacy reasons by the U.S. Census prior to publication. In particular, roughly 70% of the employment data has noise infused by Census. Further, there is no information on jobs and thus, at the extreme, we cannot distinguish between a psychiatrist and a maintenance worker. For these reasons, and similar to other studies using the CBP, we are not able to leverage variation in establishment size in our analysis.[15]

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and may possibly reduce your chances of being audited.' (`https://smallbusiness.chron.com/principal-business-code-filing-taxes-1554.html`; last accessed May 10, 2021).

[13]The NAICS definition is as follows: 'This U.S. industry comprises establishments of health practitioners having the degree of M.D. (Doctor of Medicine) or D.O. (Doctor of Osteopathy) primarily engaged in the independent practice of psychiatry or psychoanalysis. These practitioners operate private or group practices in their own offices (e.g., centers, clinics) or in the facilities of others, such as hospitals or HMO medical centers.'

[14]The NAICS definition is as follows: 'This industry comprises establishments of independent mental health practitioners (except physicians) primarily engaged in (1) the diagnosis and treatment of mental, emotional, and behavioral disorders and/or (2) the diagnosis and treatment of individual or group social dysfunction brought about by such causes as mental health disorder, alcohol and substance abuse, physical and emotional trauma, or stress. These practitioners operate private or group practices in their own offices (e.g., centers, clinics) or in the facilities of others, such as hospitals or HMO medical centers.'

[15]Other sources of healthcare data, for example the Area Health Resource File, do not include suffi-

The majority of mental healthcare received by juveniles is received in office-based settings based on available data which leads us to focus on this modality. More specifically, we analyze the 2016 National Survey on Drug Use and Health (NSDUH) and calculate that 59% of MHD healthcare received by juveniles occurs in office-based settings. From an empirical perspective, examination of office-based care reduces concerns that the coefficient estimates are capturing a mechanical effect of 'incarceration.' Put differently, when examining residential or hospitalization care (and to some extent intensive outpatient care which can last many hours most days of the week) there is less opportunity to commit crimes as the patient is 'incapacitated' by the receipt of treatment.

We emphasize mental health treatment provision in the offices we study, indeed the six-digit NAICS code labels explicitly include physicians and non-physicians specializing in mental healthcare. However, the providers we study can offer mental health and substance use disorder (SUD) treatment. For example, a psychologist can treat patients for both depression and alcohol use. Given the high degree of comorbidity between MHDs and SUDs ('co-occurring disorder') − for example, estimates suggest that 60% to 75% of juveniles with SUD also have a MHD (Youth.gov (ND)), fully isolating treatment for MHDs (separate from SUD care) is not likely feasible or even appropriate. Thus, one may view our treatment variable as influencing both MHDs and SUDs, for brevity we label this care mental healthcare in our study. The defining feature of the modalities we study is that the treatment is received in an office-based setting, one can view this type of treatment as 'light touch' care (vs. receiving treatment in a specialized facility).[16]

ciently detailed information that would allow us to isolate the office-based providers that we study over our time period. Broadly, there is very limited county-level data for the providers we examine (i.e., many missing years) and we are unable to isolate those providers that operate in office-based settings, this isolation is essential for our study. Full details available on request.

[16]While we note that we cannot fully separate MHD and SUD treatment, several factors suggest to us that, for the juvenile population we study, the majority of care received in these settings is likely related to MHDs rather than SUDs. First, MHDs are more common among juveniles than are SUDs. For example, in the 2016 NSDUH, 12.8% of those 12-17 years had a major depressive episode (MDE) while just 4.3% of this age group had an SUD, thus the MDE rate is three times the SUD rate within this age group. The NSDUH does not report a summary MHD measure for juveniles (e.g., 'any mental illness') and thus our focus on only MDE likely substantially under-counts the true prevalence of MHD among this age group we do not account for other common MHDs such as anxiety and conduct disorder

In our main analysis, we examine the overall count of office-based mental healthcare providers, but we explore differences across physicians and non-physicians in supplementary analyses given that, while both provider types deliver mental healthcare, the specific treatments they are likely to offer differ to some extent (discussed in Section 4.2). In our empirical models, we lag the number of offices one year. Hence, we merge the arrest data (1999 to 2016) into the CBP data (1998 to 2015) on county and year.

Figure 1 reports the number of office-based mental healthcare providers in the first (1999) and last (2016) years of our study in each county that we include in our analysis.[17] Two trends are evident. First, the number of offices are increasing over time. Second, there is some geographic clustering of offices, they are more common in the West and Northeast reasons than in the Midwest or South regions, although the previously noted growth over time is occurring in all regions. We note that, based on exclusions we make to construct our analysis sample following previous studies examining arrest data (Maltz and Targonski (2002)), our analysis sample includes more urban counties.

2.3 Empirical model

This study aims to evaluate the relationship between local access to office-based mental healthcare providers and juvenile arrest outcomes using two-way fixed-effects (TWFE) regression model. We use variation in the number of county-level offices to identify treatment effects. Our variation is driven by office openings and closings within U.S. counties. We estimate the regression model outlined in Equation 1:

$$Arrest_{c,s,t} = \beta_0 + \beta_1 Office_{c,s,t-1} + X_{c,s,t}\beta_2 + \alpha_c + \alpha_{s,t} + \epsilon_{c,s,t} \tag{1}$$

where $Arrest_{c,s,t}$ represents a juvenile arrest outcome. $Office_{c,s,t-1}$ is the number

(Ghandour et al. (2019)). Further, many healthcare providers prioritize diagnosis and treatment for MHDs over SUDs due to myriad factors such as preferential reimbursement for MHD, stigma around SUDs (particularly diagnosing SUDs among juveniles), and so forth (Sterling et al. (2010)).

[17] Values are lagged on year to match our empirical models.

of office-based mental healthcare providers in the county lagged one year to allow these offices to open (close), access to increase (decrease), mental health to improve (decline), and arrest outcomes to change. $X_{c,s,t}$ is a vector of county-level characteristics, α_c is a vector of county fixed-effects, and $\alpha_{s,t}$ is a vector of state-by-year fixed-effects. $\epsilon_{c,s,t}$ is the error term. All models are estimated with weighted least squares where county juvenile population (i.e., the number of county residents aged 18 or younger) is used as the weight,[18] and standard errors are clustered at the county level.

To minimize potential omitted variables bias, in $X_{c,s,t}$ we control for year-to-year changes in county-level demographic composition from SEER and educational attainment from American Community Survey. Additionally, we control for county-level economic conditions: unemployment rates from the Bureau of Labor Statistics, personal income (in thousands) from the Bureau of Economic Analysis (inflated to 2016 dollars using the CPI), and poverty rates from the U.S. Census Bureau. We further control for the number of county-level of police officers per 1,000 people from the UCR Program Data: Law Enforcement Officers Killed and Assaulted Program.[19]

Equation 1 implicitly defines the office-based mental healthcare market at the level of the county. To the best of our knowledge, there is no standard definition for this market. However, clinical evidence on juvenile mental healthcare receipt suggests that the county is reasonable. For example, Upadhyay et al. (2019) document that 70% of juveniles receive depression treatment within 15 miles of their home. Further, previous economic studies investigating the effect of access to mental healthcare use the county to define the healthcare market (Swensen (2015); Wen et al. (2017); Bondurant et al. (2018); Deza et al. (2021)).[20]

[18] We take the average population for each county in our analysis sample over the study period.

[19] We linearly impute values for a small number of observations with missing county-level covariates.

[20] We note that some cited studies emphasize SUD treatment, but at least some of the providers in all of these studies offer some mental healthcare treatment. Details available on request.

3 Results

3.1 Summary statistics

Before preceding to our analysis on the impact of access to office-based mental healthcare on juvenile arrest outcomes, we examine the demographics of juveniles receiving such care. To this end, we draw data from the 2016 (the last year of our study period) NSDUH, these data are used by the federal government to generate official mental health and healthcare statistics for the U.S. As noted earlier, our calculations using these data show that office-based care comprises 59% of mental healthcare received by juveniles, suggesting that our focus on this modality is reasonable.

Table 1 reports statistics for juveniles who report such care in the past year and those who report no care or some other modality of care. Juveniles who receive office-based mental healthcare are more likely to be female, non-minority, experience poor health, and report substance use (tobacco products, alcohol, and illicit drugs) than other juveniles. The two groups appear broadly similar in terms of school enrollment, family structure, government assistance, family income, and insurance coverage.

Table 2 reports demographics across alternative treatment modalities among juveniles who received mental healthcare in the past year in the 2016 NSDUH. There is overlap in the samples as patients can receive treatment in multiple modalities within a given year. Overall, juveniles who receive office-based care appear to be somewhat more advantaged than juveniles receiving care in hospital, residential, and other settings. Juveniles who receive treatment from family doctors are more similar to those who receive care in the office-based settings we study. In terms of thinking through what these findings can tell us about the marginal complier in our analysis, we arguably examine juveniles who are more advantaged in terms of socio-demographics than the modal juvenile who receives mental healthcare treatment.

Table 3 reports summary statistics for the period 1999 to 2016. We analyze violent and property arrests separately because any detectable effects on total arrests would be likely driven by changes in property arrests given the low prevalence of violent arrests relative to property arrests. Despite their low prevalence, violent arrests account for 96% of total arrest costs over our study period. The average juvenile arrest rate is 5.44, with 0.98 arrests for violent crimes, and 4.46 arrests for non-violent crimes per 1,000 juveniles. This translates into an average cost of arrest per capita of $35.52, with violent arrests contributing higher costs than property arrests: $34.19 vs. $1.33. That is our motivation for focusing on costs of total crime and later analyze both costs and rates of violent and property arrests separately. Figure A1 displays trends in arrest costs and rates among juveniles over our study period, these outcomes are decreasing. Table 3 also indicates that on average, there are 153 offices of mental healthcare physicians and non-physicians in each county, with fewer physician offices than non-physician offices: 67 vs. 86 and Figure A2 displays trends in offices 1999 to 2016.

Given that our empirical model relies upon variation offered by the opening and closing of mental health physician and non-physicians offices, we next investigate county-level correlates of these offices. To this end, we regress the number of offices on county-level covariates, county fixed-effects, and state-by-year fixed-effects (Table 4). We observe that counties with higher numbers of office-based mental health physicians and non-physicians are characterized by lower shares of men, racial minorities, younger individuals, and individuals with higher levels of education, and larger populations.

We next discuss the financing of office-based care within the U.S. healthcare delivery system as this financing has implications for how we specify our TWFE regression model. Historically, within this delivery system, mental healthcare has been more reliant on government payments (insurance or grants) than general healthcare. For example, in 2014 public payers financed 59% of mental healthcare treatment and 49% of general healthcare (SAMHSA (2016)). However, there are important differences in financing across modalities. For example, and unlike community mental healthcare clinics or standalone residential and outpatient mental health facilities, the office-based mental healthcare that we study is generally delivered by private providers who rely on insurance (public and private) and self-paying patients. For example, among patients[21] who receive office-based mental healthcare in the 2016 NSDUH, payment sources include: 50% private insurance, 46% self-pay, and 21% public insurance or some other payment form (payment sources can sum to more than 100% as patients can use more than one source). Contrawise, there is limited provision of 'charity' care or payment through government programs for patients who cannot pay (e.g., Substance Abuse and Mental Health Services Administration block grants): 3.6% of patients report receiving free office-based mental healthcare and 2.2% report another public source of payment (which could include other public insurance programs not specifically queried) in the 2016 NSDUH. The providers we study generally do not receive government grants and contracts to support treatment as the are private businesses and therefore not eligible. Most insurance policies within the U.S. are set at the federal- (e.g., Medicare is a public insurance system for elderly Americans) or state-level (e.g., changes in Medicaid coverage for mental health services). Therefore, our inclusion of state-by-year fixed-effects in Equation 1 accounts for such changes.

3.2 Regression analysis of juvenile arrest outcomes

Table 5 presents estimates for β_1 in Equation 1. Each cell illustrates results from a separate regression where the dependent variable is arrest costs per 1,000 population or rates per 1,000 juveniles for total, violent, and property crimes. We 'build up' our regression model by progressively adding covariates, moving from left to right across Table 5. We begin estimation with models controlling for differences across county and over time, in the form of county and year fixed-effects (column 1). We next add county-level demographic and economic variables (column 2). Third, we control for factors that

[21] This information is not collected for juveniles. Hence, we use adults as proxy. We note that Medicaid and the Children's Health Insurance Program, programs that generally cover the services we examine, may imply that insurance is somewhat more common as a source of payment for juveniles.

predominantly vary at the state-level in the form of state-by-year fixed-effects (column 3). Finally, we replace state-by-year fixed-effects with time-varying state-level controls and year fixed-effects (column 4).[22]

Reporting results in this manner allows us to conduct a test of balance. Finding that our results are stable across specifications that include different sets of covariates offers suggestive evidence that our findings are not driven by an unobserved confounder. We view the specifications reported in columns 3 and 4 as our preferred models. While including state-by-year fixed-effects allows us to control for all time-varying state-level factors, due to our sample selection rules we have some states that include a small number of counties and for this reason we view the specification that controls for state-level time-varying observables as informative. Overall, our findings are very similar across specifications, suggesting that our results are not driven by a confounding factor.

We follow Deza et al. (2021) and scale the coefficient estimates, thus reporting the effect of ten additional office-based mental healthcare providers in the county. The average county-level year-to-year increase in the number of offices over our time period is 9.9, leading us to follow Deza et al. (2021) and apply the ten office scaling factor.

As we mentioned earlier, our preferred arrest outcome is costs per capita given that total arrest rates are mostly driven by more prevalent but less socially costly property crimes. In columns 3 and 4 of the top panel, our results imply that ten additional mental health physician and non-physician offices in a county reduces total arrest costs per capita by \$0.8 to \$0.9. Comparing these coefficient estimates to the sample means implies a 2.3% to 2.6% reduction. We observe that cost reductions are attributable to

[22]We control for Governor's political party, effective minimum wage, mental health parity laws (Solomon (2018)), mandatory prescription drug monitoring program (Horwitz et al. (2021)), medical marijuana law (Sabia and Nguyen (2018)), recreational marijuana law (Chan et al. (2019)), beer tax per gallon (NIAAA APIS (ND)), minimum school drop out age (Anderson (2014)), and the higher of the state's Medicaid or Children's Health Insurance Program income eligibility level (Hamersma and Maclean (2021)). Data on the Governor's political affiliation and minimum wage come from the University of Kentucky Center for Poverty Research (2021). The minimum wage and beer tax are inflated to 2016 dollars using the CPI. Mental health parity laws require private insurance to cover mental healthcare 'at parity' with general healthcare in terms of cost-sharing, service limitations, and so forth.

arrests for violent and property crimes. For example, using coefficient estimates from specification 4, an additional ten offices per county leads to an 2.4% reduction in violent crime arrest costs and an 2.2% reduction in property crime arrest costs. We note that the property crime arrest cost coefficient estimate is not precise in column 3. Our findings are similar when we examine arrest rates, although the coefficient estimates are smaller in size and less likely to be statistically different from zero.

For brevity, we report all results that follow for our preferred arrest outcome (costs per capita) but arrest rate results are (i) similar (although, as in Table 5, smaller and often less precise) and (ii) available on request. In addition, we report results from a specification of Equation 1 that includes state-by-year fixed-effects. However, findings are very similar, and available on request, if we instead replace state-by-year fixed-effects with time-varying state-level controls described in Section 3.2 and year fixed-effects.

3.3 Evidence on the first stage

The hypothesized chain of events we assume is that offices open and access to mental healthcare increases within the county. In turn, patients (including juveniles) take up treatment and mental health improves. Finally, we expect reductions in juvenile arrest outcomes that are attributable to improved mental health.

We next offer evidence on the first stage of our assumed causal chain. To this end, we use restricted-use death certificate data obtained from the National Center for Health Statistics (Multiple Cause of Death data) to measure the impact of access to office-based mental healthcare on juvenile deaths by suicide. Specifically, we select death certificates for which the decedent is 19 years or under and the cause of death is listed as suicide using International Classification of Diseases (ICD) codes. We use ICD-10 codes Y10-Y34 and Y870. The age records only allow us to identify individuals aged 19 or under, hence we are not able to exactly match the age range in our arrest data.

While deaths by suicide are arguably a 'blunt' measure of mental health, they do have the value of being objective, self-reported measures of mental health are more open to accuracy concerns. More specifically, we take the count of the number of deaths by suicide among those 19 years and under in counties appearing in our arrest sample. We estimate Equation 1 in these data.

Table 6 reports results from this analysis. Deaths by suicide are relatively rare within our age group, as evidenced by the low baseline mean: 0.02 deaths per 1,000 juveniles. We observe that as the number of office-based mental healthcare providers increases, the number of juvenile deaths by suicide declines. In particular, ten additional offices leads to 0.0002 fewer deaths per 1,000 juveniles or 1.0% relative to the sample mean (coefficient estimates are the same in specifications that include state-by-year fixed-effects and those that replace these fixed-effects with time-varying state-level covariates and year fixed-effects). Given the severity of the outcome we study (death by suicide), we expect that the effect of office-based care on other, less severe, measures of mental health is likely larger, suggesting that we capture a lower-bound on the overall mental health benefits.

3.4 Assessing reverse causality

An important threat to the validity of our empirical strategy is the possibility that changes in the number of offices in an area might be driven by trends in our arrest outcomes. While *ex ante* juvenile arrests seem unlikely to be an important factor in determining the propensity to open or close a mental healthcare physician or non-physician office − salient factors instead appear to include demand for services, operating costs, and, in particular, provider 'burnout' (Fothergill et al. (2004); Morse et al. (2012)), we wish to test for this possibility as juvenile arrests may correlate with other factors.

To investigate this issue, we follow Cengiz et al. (2019) and estimate a 'local' event study for continuous treatment variables. We define local events as occurring in counties that experience an increase in the number of offices, and that (i) do not experience any change in the number of offices three years prior to the event and (ii) are followed by no changes or an increase in the number of offices two years after the event. We then select 'comparison' counties that do not experience any change in the number of offices for this 'event window.' Results are presented in Figure 2 and show no evidence of differential pre-trends for total crime and violent crime arrest costs.[23] Post-event, total and violent crime arrest costs decline. Findings, pre- and post-event, are less clear for property crime arrest costs, which leads us to interpret all findings for this measure with some caution.

We also estimate a local event study in which we define a local event as a reduction in the number of offices. The comparison group and event window are constructed as described here, and the regression model is identical. The treatment group is composed of counties that experience no change in the number of offices prior to the event and then experience a reduction in offices in the year of the event, followed by no change in office in the post-period. Results (Figure 3) are very similar: we observe no differences in pre-trends for counties that will and will not experience a reduction in offices.

4 The importance of other healthcare providers, heterogeneity, and sensitivity analysis

We next explore the importance of other healthcare providers whose access has been shown to reduce crime outcomes and test for heterogeneity in treatment effects across office-based mental healthcare providers, juveniles themselves, and communities. We then discuss series of robustness checks to assess the stability of our findings. Our results are robust to these checks. One exception to this pattern is property crime arrests, which are somewhat sensitive to specification.

[23] We include local event specific unit and time fixed-effects.

4.1 The importance of other healthcare providers

We focus on office-based mental healthcare provider, but changes in access to other mental healthcare providers, in particular standalone residential and outpatient care, have been documented to reduce overall crime. Bondurant et al. (2018) find that expanding the number of stand-alone residential and outpatient facilities reduces overall crime rates (they do not focus on juveniles).[24]

We include the count of stand-alone residential and outpatient facilities as an additional control in Equation 1. Results are listed in Table 7. We find that our main coefficient estimates on office-based mental healthcare providers retain their sign, magnitude, and statistical significance. Further, we find no statistically significant evidence that changes in the number of stand-alone residential and outpatient facilities impact juvenile crime outcomes. Coefficient estimates are small in magnitude and often carry a positive sign. Our findings therefore stand in contrast to recent studies on the general population that find expanding the number of stand-alone facilities reduces crime (Wen et al. (2017); Bondurant et al. (2018)).

One interpretation of this evidence is that stand-alone facilities, for myriad reasons, are effective in terms of reducing adult crime, but not juvenile crime. However, we note that this conclusion is somewhat speculative and other factors may reconcile differing findings. For example, the share of juveniles with any past-year mental health treatment receiving care in stand-alone facilities is arguably small: based on our calculation of the 2016 NSDUH 6.1% and 9.8% of juveniles (conditional on receiving any mental healthcare) received mental health treatment in residential and outpatient settings. The relatively limited use of these treatment modalities among juveniles is potentially an artifact of government regulations. For example, U.S. federal regulations prohibit juveniles from

[24] We follow Swensen (2015) and Bondurant et al. (2018) to identify such facilities using NAICS codes 621420 and 623220. These studies refer to facilities with these codes as providing SUD treatment, but industry codes (similar to those we examine) indicate provision of both SUD treatment and mental healthcare.

receiving care in facilities that offer certified opioid treatment programs, which are often located in stand-alone facilities. Juveniles are viewed as special populations by treatment experts, generally requiring specific programming, and many providers to not offer these services. For example, in the 2016 National Mental Health Services Survey, a survey of standalone residential and outpatient mental healthcare facilities, only 28.4% offered programs dedicated to juveniles. See Hamersma and Maclean (2021) for a discussion of this issue.[25] In sum, we interpret this collective body of research to further motivate separate study of different sub-groups of the population as extrapolation can, in some cases, potentially lead to inaccurate conclusions.

4.2 Heterogeneity

4.2.1 Office-based mental healthcare provider types

In this section, we explore if there is a differential effect on juvenile arrest outcomes by office-based mental healthcare provider type. The premise for this exercise is the difference in the type of care that physicians and non-physicians offer. For example, physicians are more likely to be able to prescribe psychotropics than non-physicians.[26] Non-physicians are more likely to rely on counselling and non-psychotropic treatments, and wrap-around services.

Finding the right psychotropic medication to treat MHDs often involves trial-and-error and side effects of some psychotropic medications can plausibly be linked to crime commission and therefore arrests. For example, Prozac (indicated for treatment of depression) can cause agitation while Depakene (indicated for treatment of pediatric bipolar affective disorder) can cause general mood problems. In terms of juvenile arrest

[25] We note that there is a difference in the way crime is measured across the studies: we examine arrest outcomes while Wen et al. (2017) and Bondurant et al. (2018) examine offenses known to law enforcement officers. Such difference could possibly contribute to discordant findings across studies.

[26] We note that several states adopted policies allowing some non-physicians to prescribe medications, including psychotropics, over our study period. As we report in Section 4.4, our results are not sensitive to excluding those states from the analysis sample.

outcomes, these side effects could offset some of the benefits of such treatment. Further, there may be differences in patients who seek care in these settings.[27] Of particular relevance, conduct disorder is predominantly treated with talk therapy (e.g., cognitive behavioral therapy, family therapy, peer group therapy) and not medications.[28] These treatment modalities may be more commonly provided by non-physicians.

To explore this issue, we estimate separate regressions for the number of (i) physicians (e.g., psychiatrists) and (ii) non-physicians (e.g., psychologists, psychoanalysts, and social workers) specializing in mental health offices. Results are presented in Table 8, Panel A reports results for physicians and Panel B reports those for non-physicians. We document that the effect of county-level changes in mental healthcare providers on juvenile arrest outcomes is mainly driven by changes in the number of non-physician offices (Panel B). The coefficient estimates on the non-physician office variables are comparable to our main findings, while the findings for physicians are imprecise.

4.2.2 Juvenile demographics and county characteristics

We next explore heterogeneity in the effect of access to office-based mental healthcare across (i) juveniles themselves and (ii) counties. Understanding differential effects of access to office-based mental healthcare across both juveniles and communities (which we proxy with the county in our analysis) allows us to speak to equity effects, which are important to consider alongside the overall impact of an intervention. Previous research opens the door to the possibility that expanding access to care will have differential effects across these different individuals and communities. For example, in the U.S. Black juveniles are less likely to receive any mental healthcare treatment than Whites juveniles (Cook et al. (2013)).

To study heterogeneity across individuals, we estimate regressions in which we stratify

[27]Unfortunately, we cannot study differences in characteristics across patients in these two settings with available data. The NSDUH asks one question that captures all care received by juveniles from physicians or non-physicians.

[28]We note that medications may be prescribed to treat other co-morbid MHDs.

the sample based on sex (boys and girls), race (White, Black, and other race), and age (14 years and under, and 15 to 18 years). Results are reported in Figure 4. Broadly results appear to be driven by boys, Whites, and older juveniles. We note that there is less heterogeneity for property arrest costs than for other arrest outcomes. We find that differences across groups for total and violent arrests are statistically distinguishable from zero at the 5% level or better using a non-parametric bootstrap with 500 repetitions.

We complement our analysis of heterogeneity by juvenile demographics to examine differences across counties based on their characteristics: sex and racial distribution of juveniles, poverty rate, and unemployment rate. In particular, we stratify the sample based on median values for these variables measured in 1998.[29] Results are reported in Figure 5. We observe heterogeneity in the effect of office-based mental healthcare across these county-level factors. Effects are larger in counties with higher (vs. lower) shares of male juveniles, non-White juveniles, poverty rates, and unemployment rates.[30] Bootstrapping (using a non-parametric bootstrap with 500 repetitions) the difference in the effect sizes suggests that counties with higher shares of poverty, and unemployment rates experienced larger reduction in total and violent crime arrest costs.

4.3 Asymmetry in openings and closings of offices

We next investigate whether the effects of office openings and closings on arrest outcomes are symmetric. In particular, we follow Mocan and Bali (2010) and Carpenter et al. (2017), and explicitly allow increases and decreases in the number of office-based mental healthcare providers per county to have heterogeneous effects on juvenile arrest outcomes. We estimate the following regression model outlined in Equation 2:

[29] The specific data sources are as follows: sex and racial distribution are drawn from SEER, poverty rates are drawn from the Small Area Income and Poverty Estimates Program, and unemployment rates are drawn from the Bureau of Labor Statistics Local Area Unemployment Statistics.

[30] Ideally, we would like to measure communities at a more granular level, for example at the level of the ZIP code. However, we are not aware of national arrest data at such granular levels of geography.

$$Arrest_{c,s,t} = \pi_0 + \pi_1 Increase_{c,s,t-1} + \pi_2 Decrease_{c,s,t-1} + X_{c,s,t}\pi_3 + \alpha_c + \alpha_{s,t} + \mu_{c,s,t} \quad (2)$$

$Increase_{c,s,t-1}$ is defined as the number of offices when the number of offices is *higher* than the year prior and $Decrease_{c,s,t-1}$ is defined as the number of offices when the number of offices is *lower* than the year prior (zero otherwise). We continue to lag the number of offices by one year. All other variables are as defined in Equation 1.

Table 9 reports our main findings for comparison in the top panel and results from our analysis of symmetry is reported in the bottom panel. Coefficient estimates on both the $Increase_{c,s,t-1}$ and $Decrease_{c,s,t-1}$ variables are negative and broadly similar (although generally smaller in size) to our main findings (Table 5). We report p-values from a test of equality of the $Increase_{c,s,t-1}$ and $Decrease_{c,s,t-1}$ coefficient estimates, we reject equality in half of the regressions. We interpret these findings to suggest that increases and decreases in the number of offices have symmetric effects on juvenile arrest outcomes.

4.4 Sensitivity analysis

First, we report results based on different specifications in Figure 6. In particular, we (i) estimate unweighted regressions; (ii) cluster standard errors at the level of the state (vs. county) to account for shared policy factors (e.g., public and private insurance policies are generally set at the level of the state, not county, in the U.S.); (iii) remove counties located in states where non-physicians can prescribe psychotropics (i.e., exclude Illinois, Indiana, Iowa, Louisiana, and New Mexico); (iv) remove counties with no office-based mental healthcare providers; and (v) control for county-level expenditures (which we proxy with payroll data) on health, police, and education to better account for unobservables (Kaplan (2021)). Payroll data are based on a sample of counties, thus we lose sample size in this analysis. Finally, we (i) include all agencies in cities with more than

10,000 residents in our analysis and (ii) extend the sample period to include 2017-2019, imputing values of offices in suppressed counties as zero.[31]

We note that our specification check that excludes counties with no providers allows us to provide some suggestive evidence as to whether our main effects are driven by changes in the extensive (any offices) or intensive (additional offices) margin. In particular, we can compare our coefficient estimates in the full sample (which includes both changes along the extensive and intensive margins) and the sample that excludes counties without any offices (which includes changes along the intensive margin only). The coefficient estimates are very similar across the two samples. For example, in the full sample an additional ten offices leads to a 2.3% to 2.6% reduction in total cost-adjusted arrests per capita while in the sample that excludes counties with no offices the associated arrest reduction is 2.4% reduction in such costs. While this analysis has numerous caveats (for example, most counties experience changes along the intensive margin in our sample), we interpret the findings to suggest that our main findings may capture predominantly intensive margin effects.

Second, we conduct a falsification exercise where we randomly re-shuffle the number of offices across counties and time, keeping constant the number of counties in each state. MacKinnon and Webb (2020) show that t-statistics have better analytic properties than estimated coefficients in permutation tests. Thus, we re-shuffle 1,000 times and obtain 1,000 placebo t-statistics for each of our three juvenile per capita arrest cost outcomes. We plot the placebo t-statistics in Figure 7. 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 for total and violent crime arrest costs, however results for property crime arrests costs are less conclusive.

[31] At the time of writing, ICPSR agency-monthly files are only available through 2016. Thus, we report results based on agencies in cities with more than 10,000 residents for later years. Starting in 2017, the CBP suppressed a cell containing three or less establishments.

Finally, we explore the robustness of our findings to treatment effect heterogeneity across county and time. To date, how best to test this robustness with a continuous treatment variable (such as the number of office-based mental healthcare providers per county) has not been established.[32] We dichotomomize our treatment variable into an indicator variable coded one if there are any offices in a county and zero otherwise. Thus, we are changing our research question slightly. Now, we ask what is the effect of having one office per county? There may be important non-linearities in the number of offices. For example, there may be agglomeration effects that come into play when we consider the continuous treatment measure. With these caveats in mind, we apply an estimator proposed by De Chaisemartin and d'Haultfoeuille (2020) that is robust to treatment effects that are heterogeneous across county and time, and that can accommodate reversible treatments. This estimator also allows for placebo testing and examination of treatment effect heterogeneity. We report results graphically (we use the same number of leads and lags as we do in our local event study reported earlier) in Figure 8. This analysis supports our main findings. Following the opening of an mental healthcare provider office within a county, juvenile arrest costs decline. Further, we observe no evidence of pre-trends and effects appear to increase over time. We note that, as we observed in other robustness checks, the results are less stable for property crime costs.

5 Discussion

Reducing crime is an important social objective given the high associated costs of crime for both individuals and society. A series of economic studies suggests that better access to treatment – measured by more providers or insurance coverage which includes mental health services – leads to reductions in crime outcomes (Wen et al. (2017); Bondurant et al. (2018); Deza et al. (2021); Fone et al. (2020); Vogler (2020)). We add

[32] We note that the method proposed by De Chaisemartin and d'Haultfoeuille (2020), which we use, can account for multi-valued treatments, but this method does not preform well with a truly continuous variable.

to this literature by examining the impact of expanding access to office-based mental healthcare, that is offices of physicians and and non-physicians such as psychologists and social workers specializing in the treatment of MHDs, on juvenile arrest costs per capita and rates. This modality reflects 59% of mental healthcare received by juveniles (authors' calculations of the 2016 NSDUH).

The findings in this paper provide support for increasing local access to mental health treatment as a means to reduce juvenile arrest costs and rates. Specifically, we use a two-way fixed-effect model to estimate the effect of changes in the number of office-based mental health care providers per county on county-level juvenile arrest outcomes. We combine data on arrest outcomes for Part I crimes from the Federal Bureau of Investigation's Uniform Crime Reports and the number of providers from the U.S. Census County Business Patterns data between 1999 and 2016. To account for substantial differences in the costs to society across specific arrests (e.g., a homicide is over 3,000 times more costly than a burglary), we apply a cost-adjustment algorithm to the arrests for different crimes proposed by Chalfin and McCrary (2018). Specifically, we upweight more costly arrests and downweight less costly arrests to more accurately reflect the costs to society associated with specific Part I crimes.

Overall, we find that ten additional office-based mental healthcare providers in a county reduces the costs of juvenile arrests by $0.8 to $0.9 per capita or 2.3% to 2.6% relative to the sample mean. We estimate that the total per capita annual costs of crime over our study period are $106,773 (=$35.52 x 3,006 U.S. counties). A 2.3% reduction would imply a saving of $2,456 in crime costs per capita per year for the U.S. Further, we provide suggestive evidence that non-physicians appear to be more effective in reducing juvenile crime than physicians, this finding mirrors a recent study on the general population (Deza et al. (2021)). We show that our findings for total and violent arrest outcomes are not driven by confounding factors or differential trends across counties that do and do not observe increases in the number of office-based physicians and non-physicians, and are robust to numerous sensitivity checks and placebo testing. However, our findings for property crime arrest outcomes are somewhat sensitive to specification and thus we interpret findings for this outcome with some caution.

We provide evidence to support our hypothesis that expanded access to office-based care is attributable to treatment uptake and improved mental health: ten additional offices within a county reduces juvenile deaths by suicide by 1%. The average U.S. county over our sample period experienced 0.63[33] juvenile deaths by suicide per year. Using $9.88M as the value of a statistical life,[34] this estimate suggests that each year expanded access to office-based mental healthcare by ten offices per county would save 18.9 juvenile lives (=0.63 deaths by suicide per county x 1% x 3,006 counties) and generates $187M in terms of saved lives for the U.S.

By focusing on arrests, which reflect a fraction of all offenses, we arguably provide a lower bound for the true effects on crime. Ideally, we would have a measure of the share of juvenile offenders and non-offenders, and we would use this information to provide a corrected estimate. Data limitations prevent us from implementing such a correction (e.g, there are no data on the total number of offenses committed, and there is no break out by age in the offenses known data). With these caveats in mind, we can consider available evidence on 'clearance' rates, that is the rate at which reported offenses lead arrests. An important limitation of clearance rates, separate from the above-noted concerns about offenses known data, is that the arrest of one person may 'clear' multiple crimes and the arrest of many individuals may clear just a single crime. Therefore, the correspondence between clearance rates and arrest outcomes is unclear. The UCR reports that 45.6% of violent crimes and only 17.6% of property crimes were cleared by arrest or exceptional

[33]This number is unweighted, the population-weighted value is 5.34. We use all counties in the U.S. to calculate these numbers as our data use agreement does not allow us to report tabulations with values less than ten for any sub-national sample.

[34]We use a value proposed by the U.S. Environmental Protection Agency: $7.4M in 2006 dollars (https://www.epa.gov/environmental-economics/mortality-risk-valuation#whatisvsl; last accessed September 12, 2021). We then inflate this value to 2021 dollars using the Consumer Price Index.

means[35] in 2017 (Federal Bureau of Investigation (2018)). While there are important data limitations, clearance rates suggest that we may substantially under-estimate the true effects of expanded access to office-based care on juvenile crime outcomes.

We examine an arguably non-traditional 'policy' to reduce arrest outcomes, thus comparing our effect sizes with more 'traditional' crime-reduction policies may be informative. For example, Chalfin and McCrary (2018) and Evans and Owens (2007) estimate cost-adjusted crime elasticities of -0.21 to -0.47 of police force size and crime. Our cost-adjusted arrest estimate is similar in magnitude. We find a 2.6% (using the upper bound of our estimates) reduction in the per-capita costs of juvenile arrests following an increase of ten offices of mental healthcare providers. Ten additional offices reflects a 6.5% increase in the supply of offices (=10/153.21). Therefore, our implied office-cost of juvenile arrests elasticity is -0.40, which is similar to the implied elasticities of Chalfin and McCrary (2018) and Evans and Owens (2007). We note that our comparisons are not direct as we examine arrest with Chalfin and McCrary (2018) and Evans and Owens (2007) consider known offenses. With this important caveat in mind, the similarity in effect sizes suggests non-trivial scope for reducing arrest outcomes among juveniles through improved access to healthcare.

Our focus on juveniles is an important contribution to the literature since many MHDs emerge during childhood or adolescence (Kessler et al. (2005)), and crime trajectories are established during this stage of development (Greenwood (2008)). More generally, the economics literature has shown, for a wide-range of different outcomes, that interventions during early life can have more pronounced effects than interventions later in life (Carneiro and Heckman (2003)). Our findings are in line with the small literature that has examined the causal effect of mental healthcare on juvenile crime outcomes (Cuellar and Markowitz (2007), Cuellar et al. (2004), Cuellar et al. (2006),

[35] Exceptional means occurs when the agency is prevented from arresting or formally charging the offender for reasons beyond law enforcement control such as the death of the offender or the victim's refusal to cooperate with the prosecution.

Dalsgaard et al. (2014), Heller et al. (2017)). Collectively, this body of research suggests that leveraging mental healthcare can improve both health and social outcomes.

In summary, we show that expanding local access to office-based mental healthcare providers – psychologists, psychiatrist, social workers, and counsellors – reduces juvenile arrest costs and rates. The 'interventions' that we examine are not designed as a crime-reduction policy, rather these interventions are based on the decisions of independent businesses. Curtailed crime is an unintended positive spillover from private agents to the public. Our findings could imply that policies (e.g., tax incentives) designed to increase investments in mental healthcare treatment, especially for those at young age, may have unintended and persistent benefits in reducing crime and associated costs.

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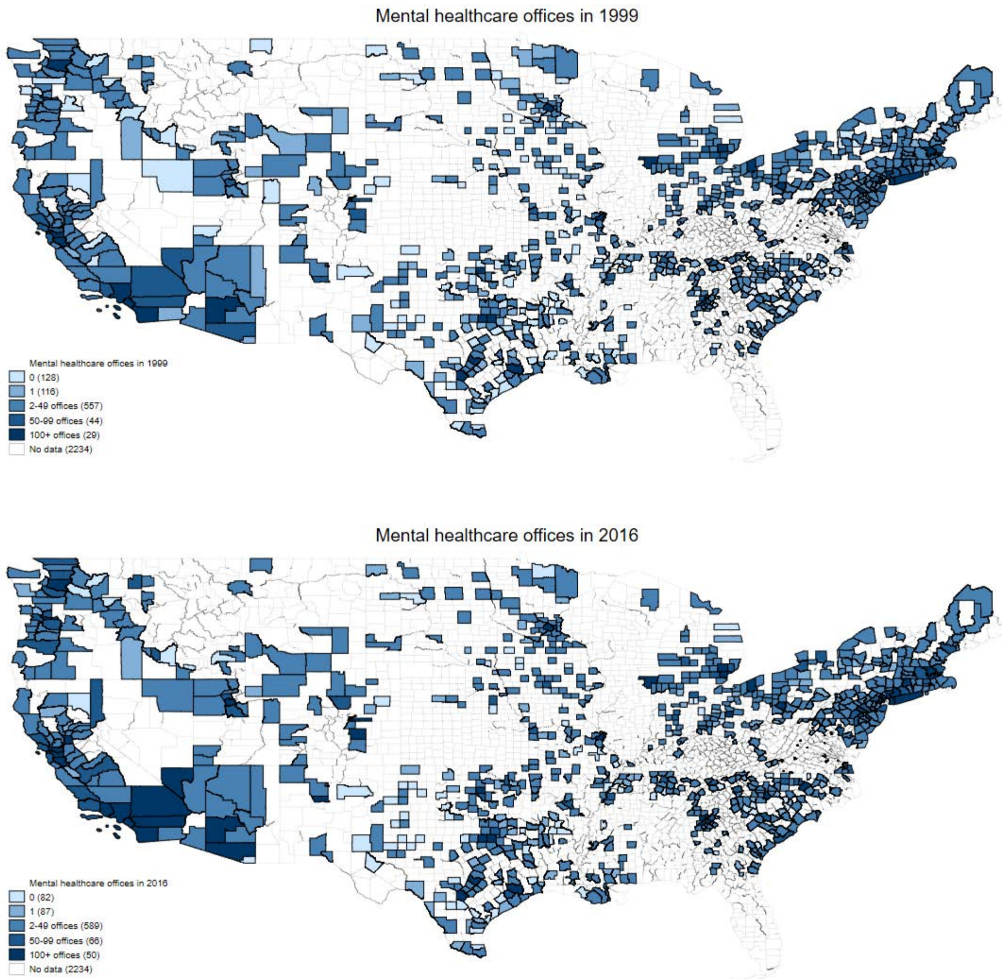
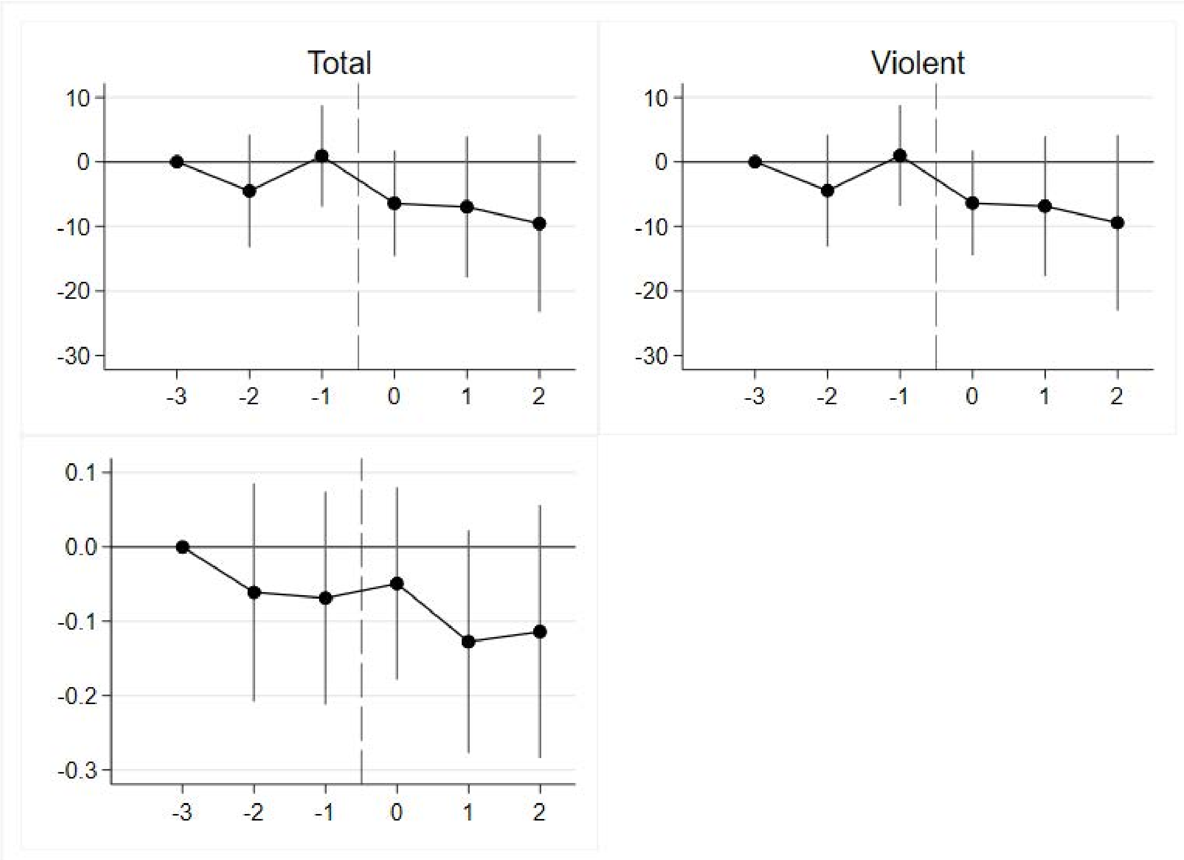
Figure 1: Number of offices between 1999 to 2016



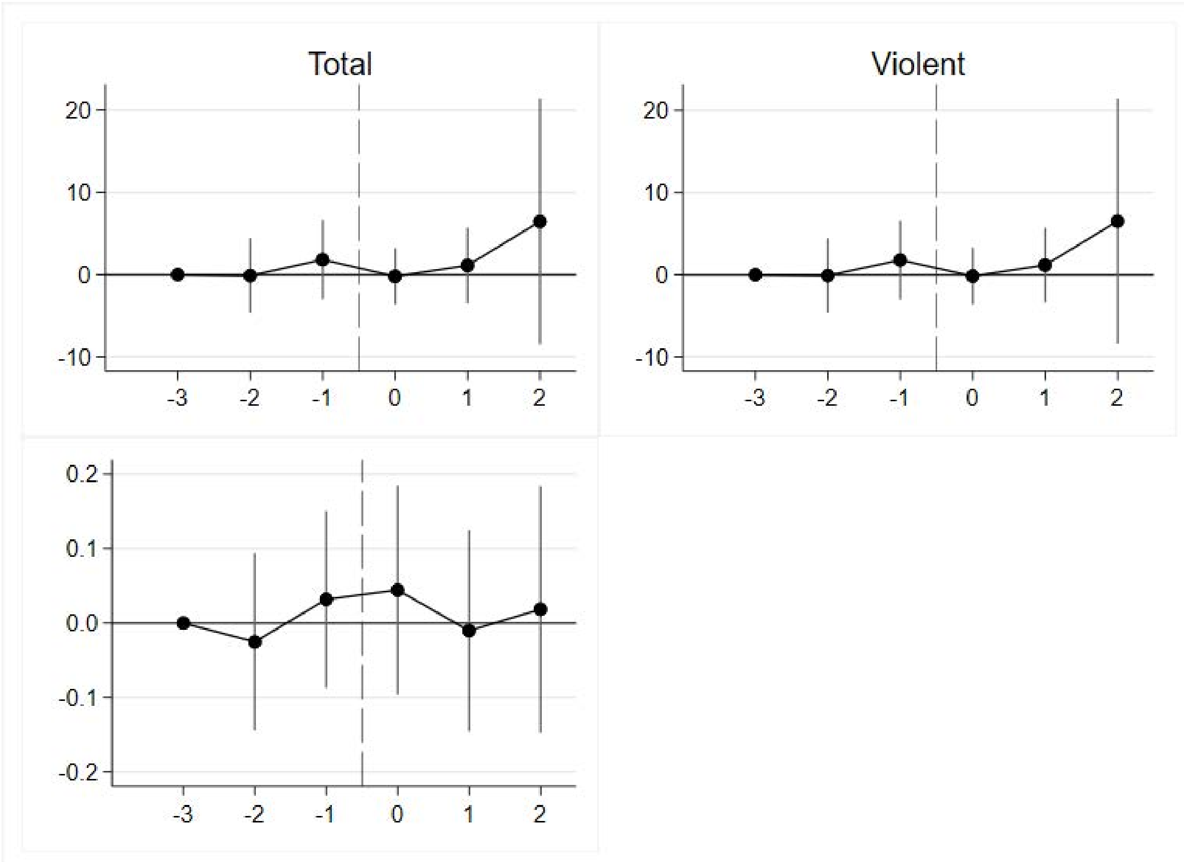
The dataset is the combined UCR and CBP 1999-2016. Providers are lagged one year and constructed as the sum of MH physican (NAICS 621112) and non-physician (NAICS 621330) providers in office-based settings. For ease of viewing, we exclude counties in Alaska (3) and Hawaii (1).

Figure 2: Local event study: Increase in offices



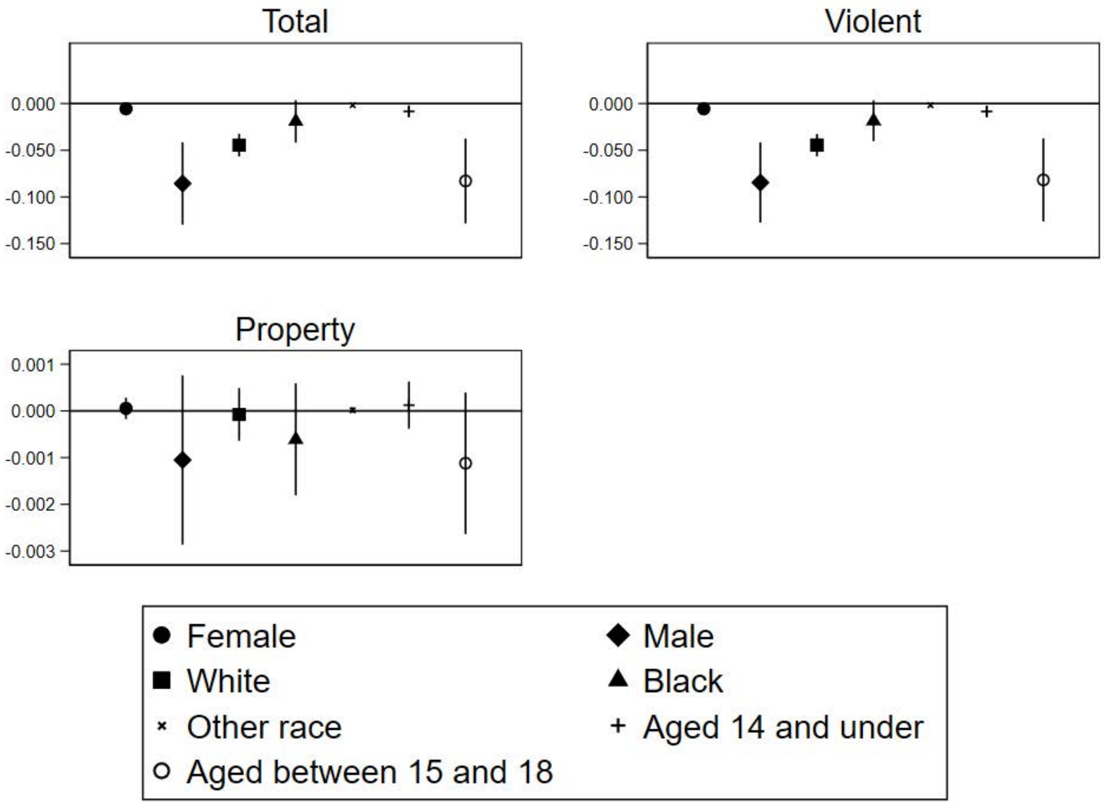
Between 1999 and 2016, there are 13 event-specific groups and we stack all of the event-specific data into one long data set. Our local event dataset is comprised of 304 treated counties and 997 comparison counties. All models estimated with OLS and control for county characteristics and dataset-specific county and state-by-year fixed-effects. Standard errors are clustered at county level. The sample means during pre-treatment period for the treated counties are $21.3, $20.1, and $1.2 for total arrests, violent crimes and property crimes respectively.

Figure 3: Local event study: Decrease in offices



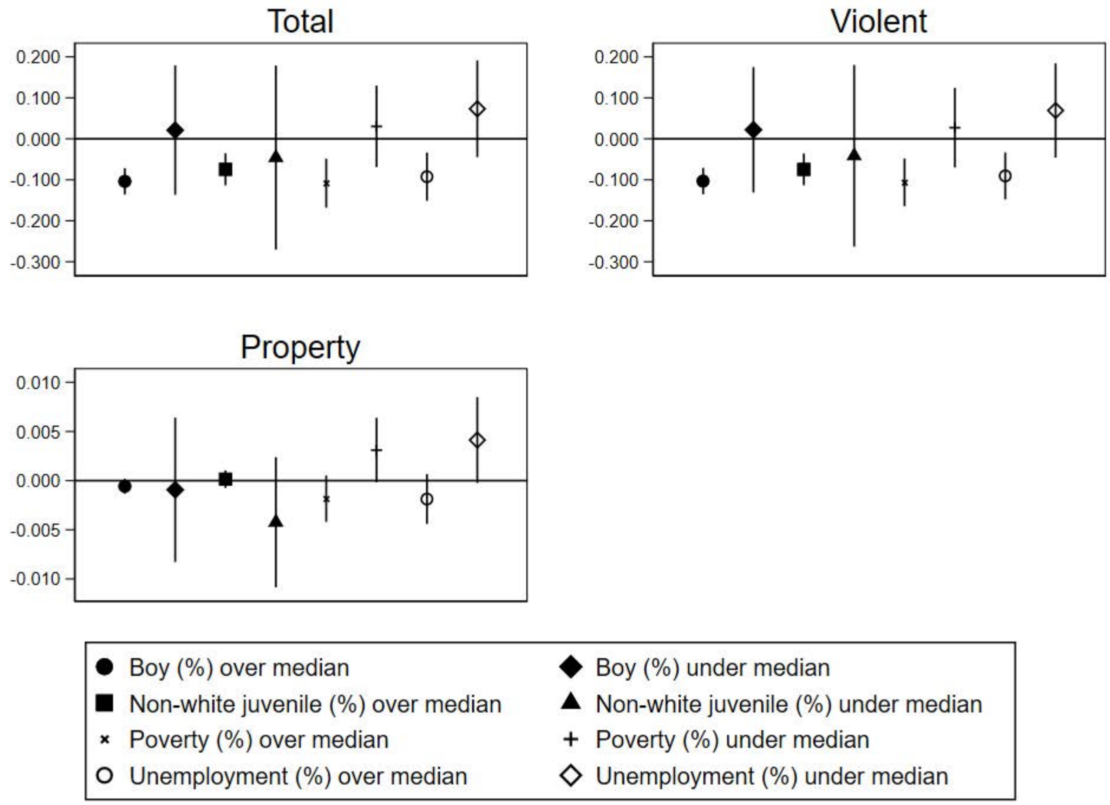
Between 1999 and 2016, there are 13 event-specific groups and we stack all of the event-specific data into one long data set. Our local event dataset is comprised of 157 treated counties and 997 comparison counties. All models estimated with OLS and control for county characteristics and dataset-specific county and state-by-year fixed-effects. Standard errors are clustered at county level. The sample means during pre-treatment period for the treated counties are \$9.6, \$8.6, and \$1.0 for total arrests, violent crimes and property crimes respectively.

Figure 4: Effect heterogeneity across demographics of arrestees: Arrest costs



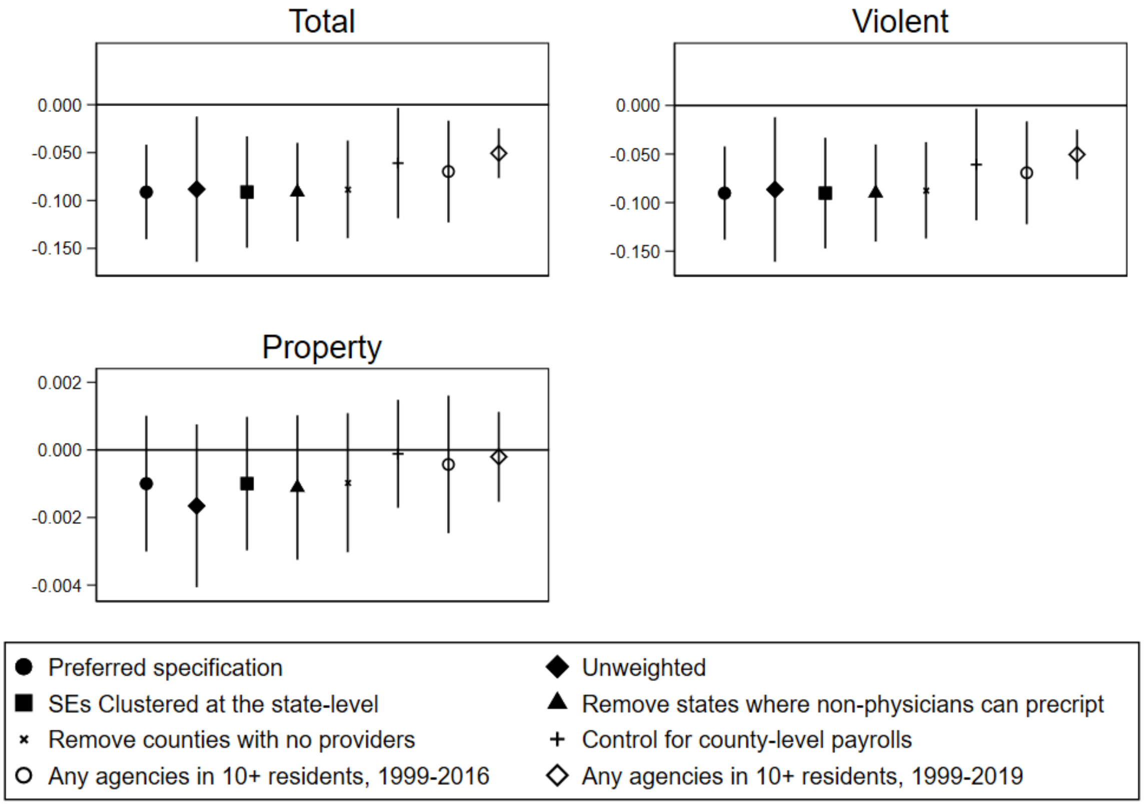
The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county population aged 18 and under. Standard errors are clustered at the county level.

Figure 5: Effect heterogeneity across county-level baseline characteristics: Arrest costs



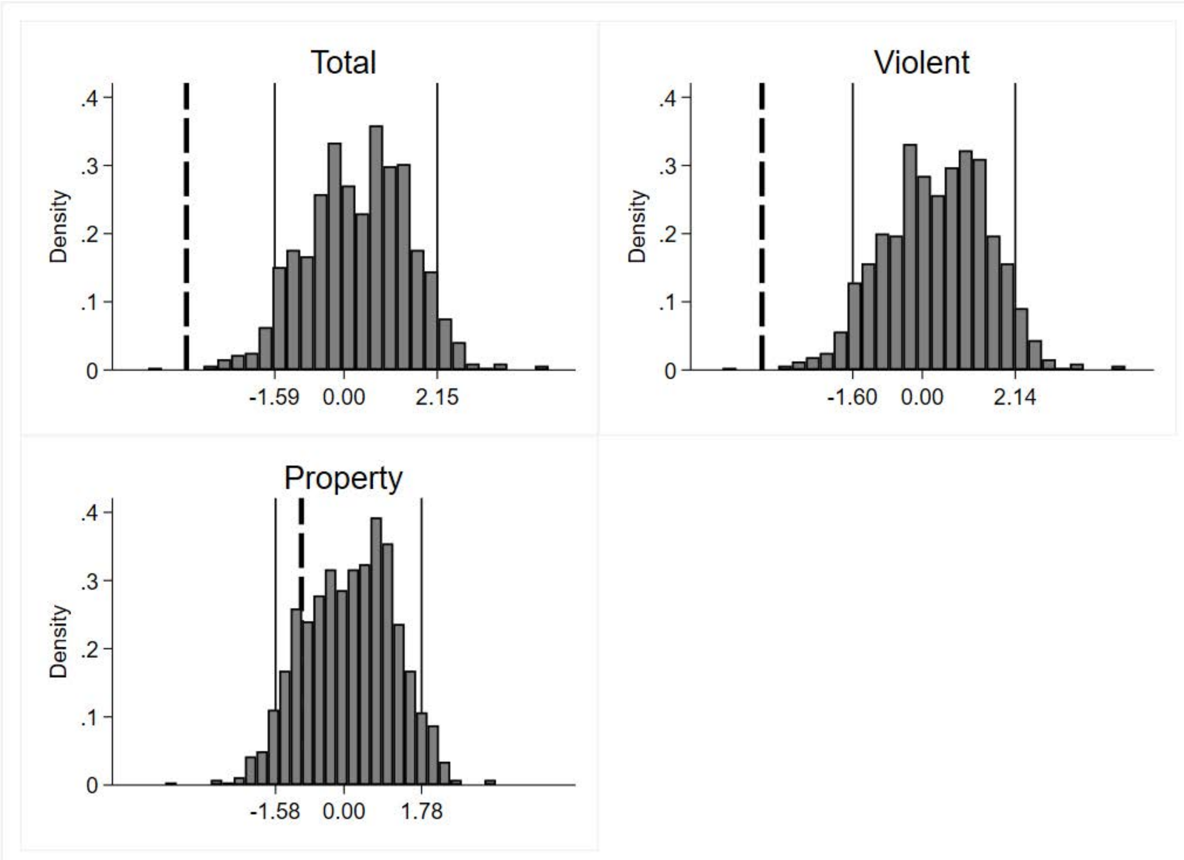
The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county population aged 18 and under. Standard errors are clustered at the county level.

Figure 6: Sensitivity analyses: Arrest costs



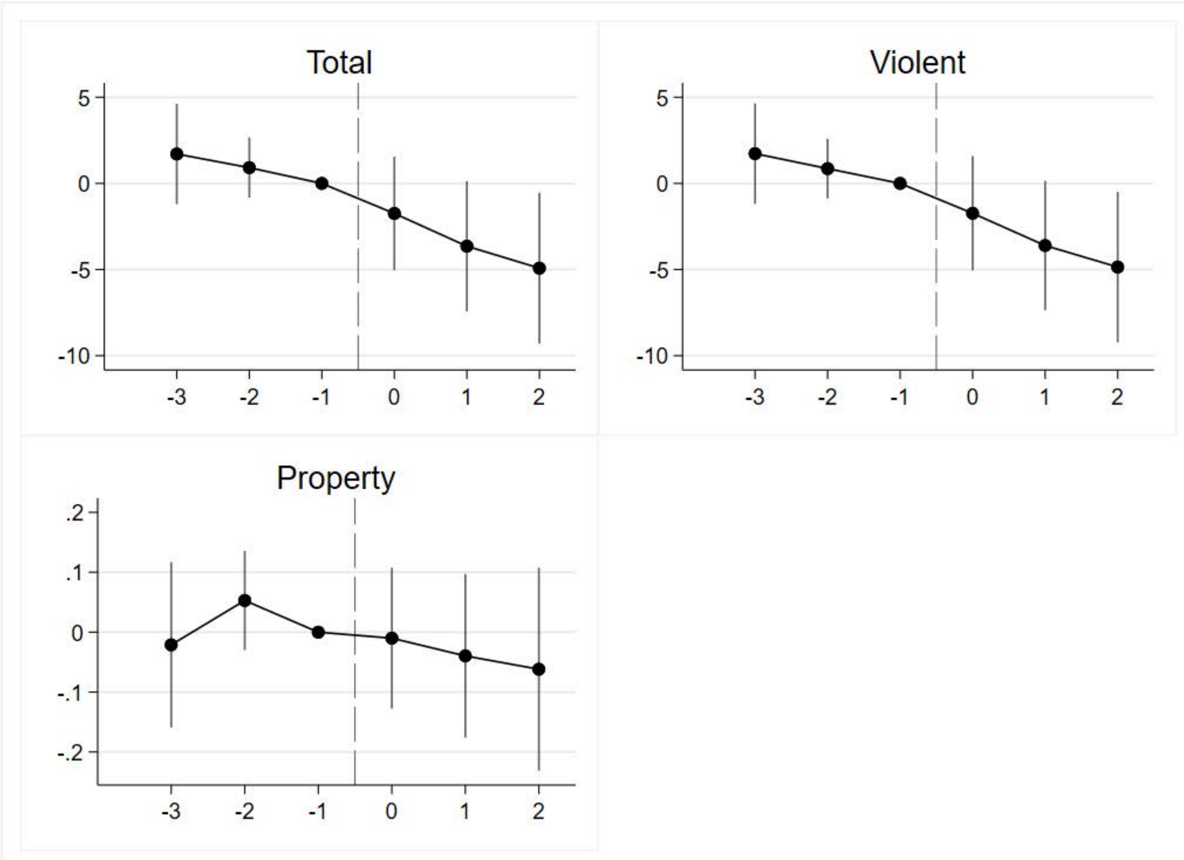
The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Healthcare providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and and control for county characteristics, state-by-year fixed-effects, and county fixed-effects unless otherwise noted. Observations are weighted by county juvenile population unless otherwise noted. Standard errors are clustered at the county level unless otherwise noted and are reported in parentheses.

Figure 7: Permutation tests of t-statistics: Arrest costs



We randomly re-shuffle the number of offices across counties and time, keeping constant the number of counties in each state. We repeat this procedure 1,000 times and obtain the null distribution of the placebo t-statistics for each of our three crime outcomes. The solid lines denote the 5th and 95th percentiles of the distribution. The dashed line is the estimated t-statistics from the actual regression. The vertical non-dashed lines represent the 5th and 95th percentiles of the distribution.

Figure 8: Effect heterogeneity across county and time: Arrest costs



We dichotomomize our treatment variable into an indicator variable coded one if there are any offices in a county and zero otherwise. We next apply an estimator proposed by De Chaisemartin and d'Haultfoeuille (2020) that is robust to treatment effects that are heterogeneous across county and time, and that can handle reversible treatments.

Table 1: Demographics of juveniles by past-year office-based mental healthcare treatment receipt

Sample:	No treatment in past year	Treatment in past year
Age in years	14.5	14.7
Male	0.53	0.36
Female	0.47	0.64
White race	0.52	0.64
Black race	0.14	0.084
Other race	0.098	0.076
Hispanic	0.24	0.19
Enrolled in school	0.90	0.91
Mother in HH	0.92	0.90
Father in HH	0.74	0.71
Below FPL	0.22	0.19
Assistance program e	0.25	0.24
Any health insurance	0.96	0.97
Private insurance	0.58	0.61
Medicaid or CHIP insurance	0.37	0.36
Medicare insurance	0.0047	0.0057
Military insurance	0.027	0.043
Very good or excellent health	0.76	0.68
PY tobacco product use	0.094	0.16
PY alcohol use	0.20	0.33
PY illicit drug use	0.14	0.26
Observations	12,220	1,819

The dataset is the 2016 National Survey on Drug Use and Health (NSDUH). The unit of observation is a juvenile (12-17 years). Office-based mental healthcare is defined as any treatment received from a 'private therapist, psychologist, psychiatrist, social worker, or counselor. Observations are weighted by NDSUH-provided sample weights. NSDUH reports separate mental healthcare questions for respondents 12-17, hence there are not data for the survey items we analyze for 18-year olds. HH = household, FPL = federal poverty level, CHIP = Children's Health Insurance Program, and PY = past year.

Table 2: Demographics of juveniles who received mental healthcare treatment receipt

Modality:	Office-based	Hospital	Residential	Outpatient	Family doctor	Other
Age in years	14.7	14.8	15.1	14.8	15.0	14.5
Male	0.36	0.38	0.34	0.35	0.35	0.41
Female	0.64	0.62	0.66	0.65	0.65	0.59
White race	0.64	0.51	0.59	0.63	0.69	0.53
Black race	0.084	0.19	0.13	0.082	0.083	0.14
Other race	0.076	0.073	0.081	0.066	0.064	0.096
Hispanic	0.19	0.23	0.20	0.22	0.17	0.23
Enrolled in school	0.91	0.79	0.80	0.89	0.91	0.89
Mother in HH	0.90	0.89	0.84	0.87	0.92	0.89
Father in HHd	0.71	0.63	0.64	0.67	0.71	0.70
Below FPL	0.19	0.34	0.32	0.26	0.20	0.25
Assistance program	0.24	0.33	0.37	0.31	0.24	0.30
Any health insurance	0.97	0.95	0.95	0.97	0.99	0.96
Private insurance	0.61	0.44	0.46	0.55	0.62	0.54
Medicaid or CHIP insurance	0.36	0.54	0.52	0.44	0.40	0.43
Medicare insurance	0.0057	0.018	0.0056	0	0.0045	0.0078
Military insurance	0.043	0.0100	0.020	0.026	0.024	0.025
Very good or excellent health	0.68	0.57	0.62	0.61	0.67	0.67
PY tobacco product use	0.16	0.25	0.37	0.21	0.20	0.16
PY alcohol use	0.33	0.35	0.49	0.34	0.36	0.27
PY illicit drug use	0.26	0.37	0.47	0.32	0.28	0.23
Observations	1,819	3,88	187	308	429	2,310

The dataset is the 2016 National Survey on Drug Use and Health (NSDUH). The unit of observation is a juvenile (12-17 years). Office-based mental healthcare is defined as any treatment received from a ‘private therapist, psychologist, psychiatrist, social worker, or counselor. Observations are weighted by NDSUH-provided sample weights. NSDUH reports separate mental healthcare questions for respondents 12-17, hence there are not data for the survey items we analyze for 18-year olds. HH = household, FPL = federal poverty level, CHIP = Children’s Health Insurance Program, and PY = past year.

Table 3: Summary statistics

Dependent variables	
Cost-adjusted arrests per capita in $2016	
Total arrests	35.52
Violent crimes	34.19
Property crimes	1.33
Arrest rates per 1,000 juveniles	
Total arrests	5.44
Violent crimes	0.98
Property crimes	4.46
Mental healthcare provider offices	
Total offices	153.21
Physician offices	66.96
Non-physician offices	86.24
County-level variables	
% Male	49.18
% Female	50.82
% White	66.56
% Black	11.84
% Hispanic	18.29
% Other	3.31
% Aged under 15	20.78
% Aged 15 to 18	5.71
% Aged 19 to 64	61.55
% Aged 65 and above	11.96
% Less than high school	16.16
% High school	26.75
% Some college	28.78
% College and more	28.31
Unemployment rate	6.22
Poverty rate	16.16
Personal income (1,000s)	45.96
Number of police officers (per 1,000)	0.70
Observations	15,803

The dataset is the combined UCR and CBP 1999-2016. Mental healthcare providers are lagged one year. The unit of observation is a county in a state in a year in a year. Observations are weighted by county population aged 18 and under.

Table 4: Correlates of providers per county

Outcome	Number of providers
Mean	153.21
County-level controls	
Male	-0.00109***
	(0.00015)
Black	-0.00340***
	(0.00111)
Hispanic	-0.00256
	(0.00158)
Other	0.00015
	(0.00033)
Aged under 15	-0.00374***
	(0.00082)
Aged 15 to 18	-0.00065***
	(0.00022)
Aged 65 and above	-0.00239***
	(0.00033)
Less than high school	-0.00214***
	(0.00040)
Some college	-0.00282***
	(0.00039)
College and more	-0.00132
	(0.00145)
Unemployment rate	0.00113
	(0.00083)
Poverty rate	-0.00044
	(0.00090)
Personal income	0.00337
	(0.00445)
Police officers	0.00010
	(0.00011)
F-test of joint significance	3.27
(p-value)	0.0001
N	15,803

The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Mental healthcare providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county population aged 18 and under. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 5: Effect of office-based mental healthcare providers on juvenile arrests

	(1)	(2)	(3)	(4)
Panel A: Arrest costs (per capita in $2016)				
Total arrests (mean =35.52)				
Offices, t-1	-0.11704***	-0.08904***	-0.09119***	-0.08328***
	(0.01523)	(0.02809)	(0.02515)	(0.03055)
Violent crimes (mean =34.19)				
Offices, t-1	-0.11367***	-0.08580***	-0.09020***	-0.08035***
	(0.01441)	(0.02810)	(0.02445)	(0.03056)
Property crimes (mean =1.33)				
Offices, t-1	-0.00337**	-0.00325***	-0.00100	-0.00293***
	(0.00146)	(0.00117)	(0.00102)	(0.00102)
Panel B: Arrest rates (per 1,000 juveniles)				
Total arrests (mean =5.46)				
Offices, t-1	-0.00258***	-0.00331**	-0.00045	-0.00265*
	(0.00094)	(0.00132)	(0.00172)	(0.00138)
Violent crimes (mean =0.98)				
Offices, t-1	-0.00152***	-0.00140***	-0.00113**	-0.00133***
	(0.00019)	(0.00038)	(0.00045)	(0.00037)
Property crimes (mean =4.46)				
Offices, t-1	-0.00105	-0.00190*	0.00068	-0.00132
	(0.00090)	(0.00110)	(0.00135)	(0.00116)
County and year FEs	Yes	Yes	Yes	Yes
County-level characteristics	No	Yes	Yes	Yes
State-by-year FEs	No	No	Yes	No
State policies	No	No	No	Yes
N	15,803	15,803	15,803	15,803

The dataset is the combined UCR-Arrest and CBP 1999-2016 and the unit of observation is a county in a state in a year. Behavioral providers are lagged one year and constructed as the sum of providers in office-based settings. All models are estimated with OLS. Observations are weighted by county population aged 18 and under. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 6: Effect of office-based mental healthcare providers on juvenile deaths by suicide: MCOD 1999-2016

Outcome	Suicide rate
Mean	0.02
Offices, t-1	-0.00002***
	(0.000004)
N	15,803

The dataset is the combined MCOD and CBP 1999-2016 and the unit of observation is a county in a state in a year. Dependent variable is constructed as rate per 1,000 population aged 19 and under. Healthcare providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county population aged 19 and under. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 7: Effect of office-based mental healthcare providers on juvenile arrests: Controlling for other providers

Outcome	Total	Violent	Property
Panel A: Arrest costs (per capita in $2016)	35.52	34.19	1.33
Office-based providers	-0.08860***	-0.08847***	-0.00014
	(0.02645)	(0.02592)	(0.00100)
Residential and outpatient providers	-0.04234	-0.04125	-0.00109
	(0.08521)	(0.08377)	(0.00241)
Panel B: Arrest rates (per 1,000 juveniles)	5.46	0.98	4.46
Office-based providers	-0.00096	-0.00192***	0.00096
	(0.00198)	(0.00049)	(0.00165)
Residential and outpatient providers	0.00108	0.00102	0.00006
	(0.00487)	(0.00136)	(0.00386)
N	15,803	15,803	15,803

The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Healthcare providers are lagged one year and constructed as the sum of providers in office-based; outpatient, and residential; and general physician office settings. All models estimated with OLS and and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county juvenile population. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Table 8: Effect of office-based mental healthcare providers on juvenile arrests: Heterogeneity by provider types

Outcome	Total	Violent	Property
Panel A: Arrests costs (per capita in $2016)	35.52	34.19	1.33
Physician offices			
Offices, t-1	-0.03138	-0.03464	0.00326
	(0.18390)	(0.17911)	(0.00607)
Non-physician offices			
Offices, t-1	-0.09952***	-0.09831***	-0.00121
	(0.02404)	(0.02332)	(0.00103)
Panel B: Arrest rates (per 1,000 juveniles)	5.46	0.98	4.46
Physician offices			
Offices, t-1	0.01047	0.00179	0.00868
	(0.00983)	(0.00240)	(0.00779)
Non-physician offices			
Offices, t-1	-0.00085	-0.00131***	0.00046
	(0.00172)	(0.00045)	(0.00134)
N	15,803	15,803	15,803

The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Healthcare providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county juvenile population. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

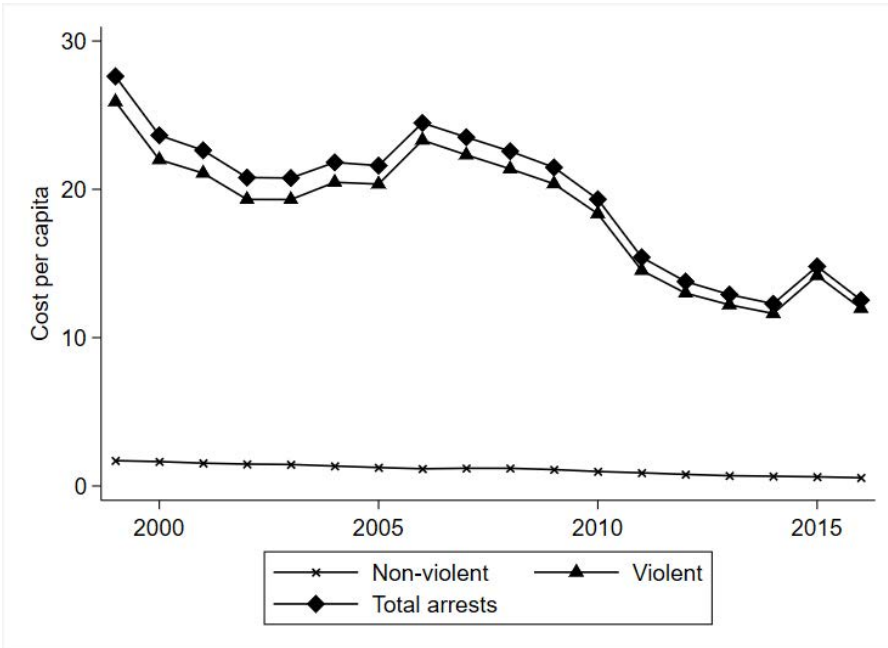
Table 9: Effect of office-based mental healthcare providers on juvenile arrests: Testing for asymmetry

Outcome	Total	Violent	Property
Panel A: Arrest costs (per capita in $2016)	35.52	34.19	1.33
Baseline model:			
Offices	-0.09119***	-0.09020***	-0.00100
	(0.02515)	(0.02445)	(0.00102)
Testing for asymmetry:			
Offices in periods when it is	-0.03563***	-0.03528***	-0.00036
higher than the prior period (i.e., rising)	(0.00780)	(0.00766)	(0.00030)
Offices in periods when it is	-0.02185***	-0.02165***	-0.00020
lower than the prior period (i.e., falling)	(0.00797)	(0.00788)	(0.00024)
p-value	0.0074	0.0073	0.1138
Panel B: Arrest rates (per 1,000 juveniles)	5.46	0.98	4.46
Baseline model:			
Offices	-0.00045	-0.00113**	0.00068
	(0.00172)	(0.00045)	(0.00135)
Testing for asymmetry:			
Offices in periods when it is	-0.00029	-0.00038***	0.00009
higher than the prior period (i.e., rising)	(0.00050)	(0.00013)	(0.00040)
Offices in periods when it is	-0.00016	-0.00021**	0.00005
lower than the prior period (i.e., falling)	(0.00041)	(0.00009)	(0.00035)
p-value	0.4380	0.0011	0.7847
N	15,803	15,803	15,803

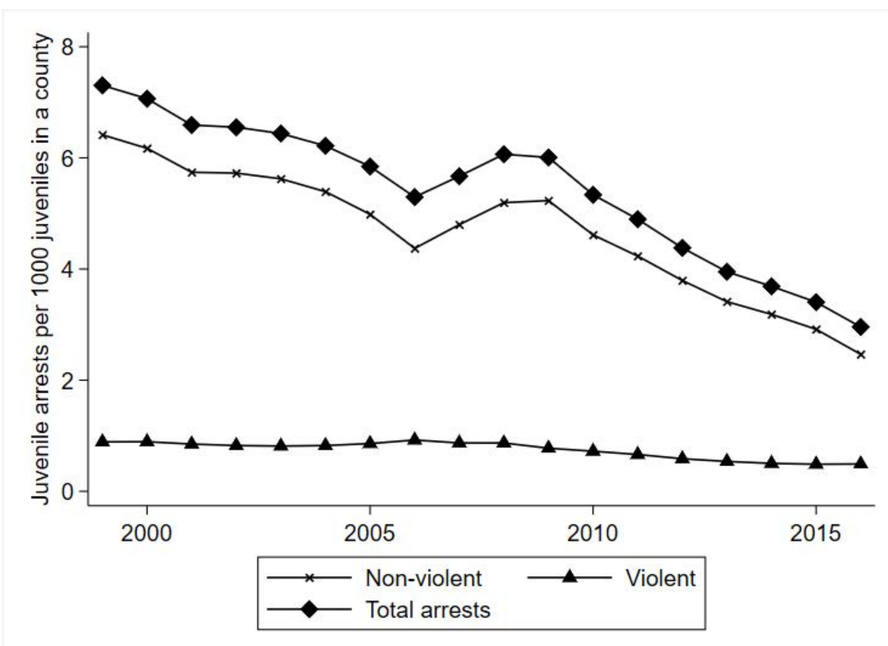
The dataset is the combined UCR and CBP 1999-2016 and the unit of observation is a county in a state in a year. Healthcare providers are lagged one year and constructed as the sum of providers in office-based settings. All models estimated with OLS and and control for county characteristics, state-by-year fixed-effects, and county fixed-effects. Observations are weighted by county juvenile population. Standard errors are clustered at the county level and are reported in parentheses. ***, **, and * represent statistical significance at the .01, .05, and .10 level, respectively.

Appendix

Figure A1: Juvenile arrests between 1999 to 2016



Arrest costs



Arrest rates

Figure A2: Number of offices between 1999 to 2016