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DISCUSSION PAPER SERIES

IZA DP No. 15993

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ABSTRACT

Prison, Mental Health, and Family Spillovers*

Does prison cause mental health problems among inmates and their family members? Correlational studies tend to find much higher prevalence of mental health problems among inmates than in the general population, but remain silent on the issue of causality. We combine detailed Norwegian data on visits to health-care professionals with quasi-experimental designs to measure the impacts of incarceration on mental health-related visits by defendants and their family members. Our causal evidence consistently shows that the positive correlation is misleading: incarceration in fact lowers the prevalence of mental health-related visits among defendants. Family members, especially spouses, also experience positive mental health spillovers, while there are fewer episodes of child protection services involvement. We demonstrate that these effects last long after release and are unlikely driven by shifts in health-care demand holding health status constant. We interpret these findings in light of the rehabilitative role of correctional services in the Norwegian context.

JEL Classification: K42, I10, I18

Keywords: mental health, incarceration, family spillovers

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1 Introduction

Mental health is a serious public health concern. In a report by the World Health Organization (WHO, 2021), depression is listed among the leading causes of disability worldwide, especially among young adults.[1] Mental health is particularly a problem for prison and jail inmates, of whom a majority are young male adults. According to a study by the US Department of Justice, 50% of inmates in US prisons faced a mental health problem (Bronson and Berzofsky, 2017).[2] Given the high prevalence of mental health problems among inmates, it is important to understand whether, and in what situations, time spent in prison can improve or exacerbate mental health problems.

The impacts of prison on the mental health of inmates and on their family members are theoretically ambiguous, making this an interesting empirical question. On the one hand, prisons can help improve mental health if prisoners are able to obtain better access to health care than they could otherwise afford, given their average lower incomes and less formal employment. Prisons can also help inmates stay drug-free, which can improve their mental health. On the other hand, the lack of freedom, poor incarceration conditions (e.g. overcrowding, poor hygiene and nutrition) and increased (threat of) violence in prison can adversely affect inmate mental health. Further, beyond the inmates themselves, the potential effects of prison on inmate mental health can spill over to the health of close family members through increased trauma, stigma, and financial hardship when the partner, parent or child serves prison time. That said, prisons can also remove a potential negative influence from the lives of their family members. In addition, potential improvement (worsening) in inmate mental health could have direct spillovers on the well-being of family members.

The main contribution of this paper is to provide causal evidence concerning the effects of prison on the mental health of inmates and their family members. Much of the existing research has found it challenging to identify the causal effects of prison on health. Data availability is a major concern in this respect as the ideal data set requires information on repeat measures of criminal behavior and health over time, data which in most settings are regarded as highly sensitive and difficult to access and link. Measuring mental health can also be challenging as the utilization of health-care services can be both suggestive of mental health problems, and preventive if it hinders more long-term serious mental health issues. Another major challenge relates to correlated unobservables. Inmates both enter and exit prison with mental health problems that could be driven by unobserved factors other than time served in prison. Finally, estimating spillovers can be even

[1] For instance, suicide is the fourth-leading cause of death among 15–29-year-olds (WHO, 2021).

[2] These statistics are based on a US survey that classified prisoners as having a mental health problem either if they were told by a mental health professional that they had a mental disorder or if they met the threshold for serious psychological distress (SPD), defined as having a score above 7 on the Kessler 6 SPD scale in the 30 days prior to survey. The corresponding share of US jail inmates with a mental health problem was 44%. While we lack comparable statistics for Norway, a survey of Norwegian inmates reportedly found that only 8% had no signs of mental disorders, while 73% had a personality disorder, 42% had an anxiety disorder, and 23% had a mood disorder (Cramer, 2014).

more difficult as it requires the linking of inmates to family networks of spouses and children and the appropriate addressing of common environmental and demographic factors.

This paper draws on multiple strengths of the Norwegian setting to overcome these challenges as we link several administrative data sources and construct a panel with complete records of criminal behavior, prison time, and health status for every Norwegian from 2006 to 2014. Using this panel data set, we can follow inmates for up to 5 years before and after the prison sentence, which allows us to assess their mental health outcomes over a prolonged period after release. Our measure of health is the total number of visits made by an individual to health-care professionals or the probability of any health-care visit over a given period. We further decompose these outcomes into mental or physical health-related visits using the diagnosis codes associated with each visit, which follow international classifications. We use two different research designs to identify the causal effects of prison. First, we use an event study design that takes advantage of the variation in the criminal case decision date under the identifying assumption that the timing of the case decision is conditionally random. Second, we exploit a random judge design as in Bhuller et al. (2020), where we instrument prison sentencing decisions using variation in randomly assigned judges that differ systematically in their stringency. These strategies complement each other by providing two separate sets of causal impacts of prison on inmate mental health visits for different subpopulations and under different identifying assumptions. The event study design provides the average treatment effect on the treated (ATT), whereas the random judge instrumental variable design provides a local average treatment effect (LATE). However, given precision concerns with the IV design, we only use the event study design for the results on heterogeneity and the family spillovers.

We offer four key findings. First, both the event study and the IV estimates reveal a large decrease in mental health-related visits for inmates that persist after their release. By contrast, the ordinary least squares (OLS) estimates show a positive association between imprisonment and subsequent mental health-related visits. For instance, the event study shows that imprisonment causes a 19.5% decline in the probability of a mental health visit during months 13-60 after the prison sentencing decision in court, while the OLS suggests a 12.6% increase.[3] Overall, the event study estimates show a 10% decline in the probability of any health-care visit at month 60 after sentencing. However, we find no meaningful impacts on non-mental health visits. We also show the robustness of our main findings to a battery of specification checks, including alternative definitions of event timing and a placebo using matched controls from the general population.

Second, in interpreting the evidence, we find that incapacitation only explains a small share of the decline in mental health-related visits as the impacts persist and indeed become stronger in the periods when inmates have typically left prison. Furthermore, we observe a decline in

[3]Most incarcerated defendants have completed their prison sentence by month 12 after prison sentence. The event study provides an estimated 4.7 percentage point decrease in the monthly probability of a mental health-related visit at months 13-60 post prison sentence, which can be compared to a monthly baseline rate of 24.1%.

both addiction- and depression-related diagnoses, suggesting that the impacts extend beyond de-addiction. We also obtain evidence consistent with our estimates reflecting an improvement in mental health and not merely a decrease in health-care demand. Several pieces of evidence support this argument. First, many inmates have a very high level of health-care utilization at the baseline, and we find similar reductions across inmates with and without past mental health history. The relatively humane prison conditions in Norway make it unlikely that prison negatively affects trust. Moreover, we observe a decline in emergency health-care visits for mental health reasons, a decline in the severity of mood disorder-related visits, and we also do not see any upticks in mental health diagnoses long after the prison sentence. A short-term decline in health-care use despite latent mental health conditions could have resulted in longer term increases in mental health problems. The absence of a decline in physical health visits also suggests that health-care demand is unaffected.

Third, we show evidence suggesting that humane prison conditions and widespread access to rehabilitation programs are important features of our setting that may lead to improved mental health among former inmates. Three pieces of evidence speak in favor of this mechanism. First, we find stronger reductions in the number of mental health-related visits among defendants who are initially assigned to so-called open prisons that tend to have milder conditions for confinement.[4] Second, we find stronger reductions in mental health-related visits among those assigned to prisons with educational and work-related programs, although admittedly we do not have precision to statistically reject effect homogeneity. Third, we provide event study estimates for defendants assigned to alternative non-custodial punishments–probation and community service–that also offer access to rehabilitation programs similar to in-prison programs, and consistently find reductions in mental health-related visits also for defendants assigned to probation and community service. By contrast, we find no changes among those receiving a monetary penalty, without access to rehabilitation programs. Our evidence should thus be interpreted within the context of a rehabilitation focused Norwegian correctional system, which is aimed at ensuring good prison conditions, but where prison expenditures per inmate are also substantially higher than in many other countries.[5]

Finally, we find significant spillovers on spouses, for whom we also observe a large decrease in mental health-related visits. Five years after the sentencing of the inmate, only about one-third of previously partnered inmates and their spouses are still together, and the spillovers to spouses are driven by those that split up, suggesting that the "removal of a bad influence" channel could be at play. Incarcerated defendants' children also experience a decline in mental health-related visits. Using data on incident reports from the Child Protection Services (CPS), including home

[4] In Norway, most prisoners are transferred to open prisons for resocialization prior to release as part of a rehabilitation plan. However, prisoners who served time in open prisons to begin with likely faced milder conditions for longer periods.

[5] As we describe in Section 2.4, Norway spent about $118 thousand per inmate in 2013, almost double that was spent on average in Western European countries ($66 thousand), and four times that in the US ($31 thousand).

removal/foster care as well as assistance in the home, we also find a reduction in the incidents of CPS involvement in these families. Taken together, spillovers to spouses and fewer child protection-related incidents in these families suggest that prison can have important positive spillovers on family health and well-being. The potential benefits of rehabilitation through improvements in mental health are therefore large and go beyond the direct effects on the inmates themselves.

This paper provides an important contribution to the existing literature on prison and health. The majority of studies on this topic are correlational (Binswanger et al., 2007; Weidner and Schultz, 2019; Haglund et al., 2014; Sailas et al., 2006; Turney et al., 2012), often relying on comparisons of incarcerated and nonincarcerated individuals matched on some observable characteristic such as age or gender. Typically, this literature finds that incarceration is associated with higher levels of morbidity, mortality, and mental health disorders.[6] However, this positive association does not inform us about whether incarceration causes poorer health outcomes, as incarcerated and nonincarcerated individuals are likely to differ significantly along unobservable dimensions. The causal literature focusing on mortality is limited to a few studies. Norris et al. (2023) use a difference-in-differences (DiD) strategy around the removal of the treatment (release from prison) using US data and show a negative effect of prison on mortality during incapacitation. The decrease found is strongest for homicides and overdoses, but also marked for suicides and mortality from natural causes, and while Norris et al. (2023) are unable to estimate the effect precisely, they do not find any post-release positive impact of incarceration on mortality. For Sweden, Hjalmarsson and Lindquist (2022) use policy-induced variation at the intensive margin (prison length) and find a decrease in mortality risk, especially when they focus on specific subgroups or causes of death (e.g., suicide, violent death), which they argue is driven by in-prison health treatment and services. We extend this literature by using alternative research designs, focusing on the extensive margin of prison and on less extreme health outcomes.[7] Beyond the extreme outcome of death, we are therefore able to capture improvements or deteriorations in health that have dramatic consequences for the daily lives of inmates. We further present new evidence on the family health spillovers of prison.

Our work is also related to a large literature concerning the effects of prison on other outcomes, particularly recidivism and employment. Descriptive studies (Gottfredson, 1999; Western et al., 2001) report a positive correlation between imprisonment and recidivism and nonemployment. A smaller set of studies use a judge fixed-effects instrument with mixed findings. For example, using US data, Green and Winik (2010) and Loeffler (2013) fail to detect any effect of incarceration on recidivism, whereas Kling et al. (1999) provides suggestive evidence of a positive though imprecise impact on post-release labor market outcomes. However, Aizer and Doyle Jr (2015) and Mueller-Smith (2015) both find a negative effect of incarceration on future outcomes: Aizer and

[6] See also a recent overview by Western (2021).

[7] The extensive margin makes particular sense in the Norwegian setting where long prison sentences are rare.

Doyle Jr (2015) measure lower high school completion rates and higher future incarceration rates on a population of juveniles, while Mueller-Smith (2015) reports higher recidivism rates and poorer labor market outcomes on adults. In Ohio, Norris et al. (2021) find that incarceration reduces the number of crimes committed by inmates over the 3 years following judge assignment, an observation consistent with incapacitation effects, but do not find any significant post-incarceration effects. Their paper closely relates to ours in that they also examine spillovers, and reveal that the incarceration of a parent or sibling has a negative effect on the likelihood of being charged for children, with no detectable effect on education. Using an alternative methodology relying on discontinuities in North Carolina's sentencing guidelines, Rose and Shem-Tov (2021) find that incarceration has a reoffending-reducing effect that diminishes with sentence length. In the Norwegian context, Bhuller et al. (2020) show that incarceration reduces recidivism and improves future employment. An assessment of the impact of incarceration on other dimensions, including health, and on the whole family, is necessary for a comprehensive understanding of the effects of prison. Our paper therefore contributes to this comprehensive assessment to aid the better design of sanctions.

Finally, our study relates to the broader literature on the causes and consequences of mental health. This often focuses on an adolescent population and relies on the use of sibling fixed effects, and individual and neighborhood controls. It has been shown, for instance, that mental health problems are associated with poorer education and labor market outcomes, lower future marriage stability, and higher criminal activity later in life (Goodman et al., 2011; Lundborg et al., 2014; Currie and Stabile, 2006; Anderson et al., 2015; Fletcher and Wolfe, 2009).[8] Studies taking advantage of changes in health policy or the local availability of treatment also suggest a causal link between mental health and employment, human capital, and criminal outcomes in the adult population (Bütikofer et al., 2020; Deza et al., 2020; Bondurant et al., 2018). For instance, exploiting family fixed effects and a change in the treatment of bipolar disorders, Biasi et al. (2021) demonstrate that the large earnings penalties entailed by mental health disorders are partly offset by access to treatment. Relying on a DiD estimation, Jácome (2020) reports the positive effect of a loss in Medicaid eligibility on future criminal behavior, especially among those with mental health histories. The multidimensional impacts of mental health disorders highlight the importance of understanding the potential impact of prisons on mental health.

2 Institutional Setting and Data Sources

We describe below the key features of our institutional setting, which is similar to the one in Bhuller et al. (2020) used to estimate the causal effects of incarceration on defendant's recidivism and

[8]The literature on the consequences of mental health issues among adolescents is relevant to the extent that our sample is quite young, with a median age of just 31 years.

future employment. We begin by briefly describing the court system in Norway and how cases are assigned to judges. We then describe the prison system, and how health care is provided in prisons. We finish the section by describing our data sources and sample restrictions.

2.1 The Norwegian Court System

We study defendants facing trial in the Norwegian criminal justice system. If the police suspect an individual of a crime, they file a formal report. A public prosecutor then decides whether the individual should be charged with a crime as well as whether the case should proceed to a court trial. About half of all police reports lead to a formal criminal charge. Of these charged cases, the public prosecutor advances about 40% to trial. The other charged cases are either dismissed, directly assigned a fine, or sent to mediation by the public prosecutor. Of the cases that proceed to trial, some 60% are nonconfession cases, while the remaining are cases where the defendant has confessed to the charges filed by the public prosecutor.[9] We focus on nonconfession cases in this paper. Appendix Figure A1 plots the typical timeline of events with the average and median time between each step from the date of the crime to prison release for cases processed in the period 2011–2014.

Once a case proceeds to trial, it is assigned to a judge. If the judge finds the accused guilty, they can assign a combination of punishments that are not necessarily mutually exclusive. Slightly over half of all cases result in incarceration, with probation, community service, and fines combined accounting for 44% of outcomes. From 2009 onwards, electronic home monitoring became an alternative to prison time, and is currently used in 18% of cases in which defendants face an incarceration decision.[10] In a small number of cases (5%), the defendant is found not guilty.[11] If multiple individuals are charged in the same case, they take part in the same trial, but can have different charges and varying sentences depending on their role in the crime.

The law in Norway dictates that cases are assigned to judges according to the principle of randomization (NOU, 2002; Bohn, 2000). There are a few exceptions, such as for especially severe crimes or cases involving juveniles, which we exclude from our sample. To obtain a sample of randomly assigned cases for the same pool of judges, we limit our sample to regular judges handling nonconfession cases. Regular judges are permanent civil servants (versus deputy judges who

[9] A defendant chooses whether to confess prior to knowing who their assigned judge will be. The absence of plea bargaining makes the interpretation of our estimates easier (see Dobbie et al. 2018).

[10] This includes defendants that partly served prison time in combination with electronic monitoring. This policy was introduced in some regions from September 2008 and implemented nationally from May 2014 onwards.

[11] The justice system in Norway further allows for forced psychiatric care/confinement ("tvungen psykisk helsevern") as part of the sentencing guidelines. However, these sentences are rare (only 44 forced confinements were made in 2020) and often relate to extreme cases such as murder or severe violence, which are some of the exceptional cases nonrandomly assigned to judges and thus excluded from our sample.

generally serve for a limited 3-year term).[12] As in Bhuller et al. (2020), we measure the strictness of a judge based on their incarceration rate for all other cases they handled between 2005 and 2014. There are 596 judges, each of whom presided over an average of 241 randomly assigned court cases. To construct our judge stringency measure for the random judge design, we calculate the leave-out mean judge incarceration rate conditional on the fully interacted court and year fixed effects to account for the fact that randomization occurs only within the pool of available judges.

2.2 The Norwegian Correctional System

To assist interpretation of our findings, we describe next the conditions inmates face in prisons in Norway. We also describe the services that defendants may receive outside prisons, either upon release after completion of a prison sentence or when sentenced to other non-custodial sentences. Generally, prisons in Norway emphasize rehabilitation and follow the "principle of normality" set forth by the Norwegian Correctional Services. This principle dictates that "life inside will resemble life outside as much as possible" and that "offenders shall be placed in the lowest possible security regime." This means that the main punishment is the restriction of liberty, and that no other rights should be taken away from inmates serving time in Norwegian prisons.

There are a total of 61 prisons in Norway. The largest prison (in Oslo) has 392 cells, while the smallest has just 13 cells. Norway has a strict policy of one prisoner per cell and attempts to place prisoners close to home so that inmates can maintain links with their families. Further, there are two types of prisons based on the level of security. A high-security prison (also referred to as a closed prison) has a wall or high fence around the prison area. All doors are locked. When the inmates are not at work or at school or participating in leisure activities under the control of the prison guards, they are locked in their cells. Closed prisons make up nearly 70% of all prison beds in Norway. Prisons with lower security levels (also referred to as open prisons) have fewer physical security measures than do high-security prisons, but also usually have a fence around the prison area and do not permit inmates to leave the facility.[13] Inmates in open prisons have more freedoms and responsibilities compared with closed prisons. Whether a convicted defendant is initially sent to an open or closed prison depends on the severity of the crime, as well as geographical proximity and the available space at open versus closed prisons. The two types of prisons create a separation between minor and more hardened criminals, at least until the hardened criminals have demonstrated good behavior. While more serious offenders serve most of their sentence in closed

[12] We further restrict the data set to judges that handle at least 50 randomly assigned cases and to courts with at least two regular judges each year.

[13] The prison buildings are locked at night, but the inmates are not locked in their cells. Inmates can share rooms, and a great emphasis is placed on the possibility of contact with society through various types of outings, visiting arrangements and more lenient control measures. There are several options for using the phone, but calls can be intercepted.

prisons, they are usually transferred to open prisons for resocialization and further rehabilitation prior to release.

To promote and facilitate rehabilitation, prisons commonly offer education, mental health, and training programs. The mental health programs are targeted at social or emotional skills such as anger management and interpersonal relationships and programs aimed to combat recidivism or drug addiction. Between 2009 and 2014, around 28% and 36% of inmates in open and closed prisons, respectively, participated in some type of mental health program. The most common programs are for high school and work-related training, but inmates can also take other miscellaneous courses. Closed prisons are also more likely to have formal employment and education programs (82.5% vs. 79% and 74.3% vs. 58.8%, respectively). All inmates are involved in some type of regular daily activity, unless they have a serious mental or physical disability. If they are not enrolled in an educational or training program, they must work within prison.[14] All inmates have the right to daily physical exercise and access to a library and newspapers. After release, there is an emphasis on helping offenders reintegrate into society, with access to programs set up to help ex-prisoners find a job and access social services like housing support.

The large majority of unincarcerated defendants are also convicted and receive instead a non-custodial sentence, rather than acquittal. Such sentences can be probation, community service, or fine. While these sentences vary by their degree of punitiveness (e.g., amount of fine, length of probation, days of community service), there are also differences in the extent of rehabilitative services that are offered to convicted defendants facing such sentences. Especially, defendants that are sentenced to probation may often also receive access to de-addiction programs and young defendants (majority of our sample) may partake in a youth assistance program and receive guidance on job search, similar to many in-prison programs. Defendants that are assigned to community service also face active involvement by the correctional services, through supervision and regular meetings with assigned caseworkers and access to rehabilitative programs. They also perform mandatory unpaid work for a public employer or a publicly sponsored non-profit entity as part of their sentence, which may provide useful work experience. By contrast, defendants that are fined would tend to have little to no interaction with the correctional services and face only a monetary penalty.

2.3 Health-Care in Prison

Norway has a publicly-funded system of health-care that is built on the principle that all inhabitants have equal access regardless of social status, income, and geography (The Commonwealth Fund, 2010). By law, prisoners also have the same rights to health-care services as the population-at-large. Norwegian prisons follow the "import model", which means that all public care and health

[14] All prisoners, whether working or participating in training or education programs, receive a small stipend while in prison (around $8 per day in 2015).

services should be provided in the same conditions inside as outside prison (Moe, 2018). The health services are typically delivered from the community to prisons by local and municipal providers. By guaranteeing access to services in prison that are "as good as outside", Norwegian prisons seek to avoid disruption in the quality of health-care provision.

The Norwegian Directorate of Health is responsible for managing health programs for inmates. Following the import model, prison health workers are financially and administratively independent from the correctional facility and the Department of Justice, and funded through the Department of Health and Social Welfare. The medical staff is often specifically trained in addiction and mental health disorders.[15] Kjelsberg et al. (2006) provide a description of mental health consultations offered in six medium-to-large Norwegian prisons representing one third of the Norwegian prison population in 2005. As discussed there, within the first few weeks of incarceration, all new prisoners are screened for health problems by a primary health worker. They first conduct a personal interview and then provide treatment and refer to specialist services if needed. Each inmate is assigned a prison officer as their primary contact, who will oversee a consultation with a primary health care worker if needed. Then, if deemed necessary, the primary health worker arranges a psychiatric consultation for the inmate. In the six studied prisons, there was about one psychotherapist for every 100 inmates, on top of the administrative staff and primary health workers.[16]

This implies that prison will typically offer a new opportunity to access health-care. This could be important for both those with previous mental health problems and those without or undetected mental health issues. Prison could provide new treatment, health-care consultations or programs for those that already had been diagnosed or it could be the first point of contact for those that did not use the system before entering prison. In our analysis we will split by previous mental health history to see if prison is more important for continuing or initiating care.

Upon release, former inmates have the same rights as any other resident to publicly provided health-care services. However, as in-prison health services are often provided by local and municipal providers as part of the "import model", former inmates have already established contact with these providers in prisons. This may allow for better continuity of health-care services initiated in prisons upon release. As health-care is an essential part of public services provided to residents, many inmates may already be familiar with health-care providers practising in their municipality or region, making it easier for them to consult both inside and outside prisons. Limited access to services, both outside and inside prison, is thus unlikely to be an important margin in our setting.

[15] According to the Norwegian Directorate of Health, around 60% of nurses have received specific training to handle these issues, and prisons have access to psychologists or psychotherapists.

[16] None of the surveyed psychotherapists had a waiting list at the time of the study, suggesting that there was sufficient capacity to meet demand.

2.4 Comparison to Other Countries

Along many dimensions, the Norwegian criminal justice system looks like most Western European countries and to a lesser extent, the US. In Norway, the incarceration rate was 72 per 100,000 in 2015, close to the rate in Western European countries of about 100 per 100,000 (World Prison Brief, Institute for Crime & Justice Policy Research). The US is an outlier in that respect, as its incarceration rate was 672 per 100,000 in 2015, with only 11 countries worldwide with incarceration rates exceeding 400 per 100,000. While Norway shows many similarities with other Western European countries and with the US in terms of inmate population characteristics (Aebi et al. (2015); Carson (2015); Kristoffersen (2014); Raphael and Stoll (2013)), it differs regarding prison expenditures and conditions. Similar to Sweden, Denmark, and the Netherlands, Norway spends about $118 thousand per inmate per year, almost double that spent on average in Western European countries ($66 thousand per inmate per year), and four times that in the US ($31 thousand).[17]

Consequently, Norway can ensure better prison conditions, with an emphasis on rehabilitation and the principle of normality rather than punishment and the removal of privileges (Bhuller et al., 2018).[18] Some of these conditions may play a crucial role in mediating the impact of prison on health. Norwegian cells are individual, and prisons are not overcrowded.[19] This may particularly matter as identifying mental health issues entails allocating specific resources to the mentally ill inmates, including space resources, which are not necessarily available where there is overcrowding. Detecting signs of a worsening mental health condition also requires close monitoring by prison staff, which is made easier with the lower inmate-to-staff ratio and reduced physical barriers in Norwegian prisons.[20] Overcrowding and a lack of resources and staff have been highlighted as potential factors explaining that mentally ill inmates are often undiagnosed in US prisons (Haney, 2017).[21] Prisons in the US also offer job training, education and drug treatment programs. However, those offered in the US are often not accessible in practice because of a lack of funding, and long waiting lists (Davis et al. 2014; GAO, 2012). Finally, the Norwegian system provides inten-

[17] However, there is substantial heterogeneity within the US, with per inmate expenditures ranging from $60 thousand in New York state to $17 thousand in Alabama. Cost estimates are calculated by dividing total prison budgets by the number of prisoners. The numbers for Western Europe are for 2013 and are purchasing power parity-adjusted (Aebi et al., 2015). The data for 40 US states are for 2010 (Henrichson and Ruth Delaney, 2012).

[18] This approach not only determines prison conditions, but also potentially creates a different culture with more interpersonal trust and closeness between inmates and correctional staff, which could help in the identification of symptoms and the treatment of mental disorders.

[19] The occupancy rate is 76% in Norway, which ranks at the lower end of the highest occupancy rate distribution (42^{nd} out of the 57 European countries in the /www.prisonstudies.org). The US rate is 99.8%.

[20] In 2016, the number of personnel in adult prisons per 100,000 inhabitants was 96.5 in Norway, ranking 6^{th} of the 33 European countries for which we have data (Eurostat).

[21] The overcrowding that came with the era of mass incarceration in the US has also triggered the rise of a punitive mind-set with the use of harsher discipline, e.g., segregated placement and solitary confinement (Haney, 2017). These negative forms of institutional control may place the mental health of inmates in jeopardy by increasing the level of stress, anger, and psychological pain, and by reducing social contact.

sive post-release support, e.g., active labor market programs specifically designed for ex-prisoners, housing support, social assistance, and disability insurance.[22]

2.5 Data

Our analysis employs several data sources that we can link using unique individual identifiers. Information on the court cases is from the Norwegian Courts Administration. The main data set contains information on all court cases over the period 2005–2014. We observe the start and end dates of every trial, various case characteristics, the verdict, and unique identifiers for judges, defendants, and district courts. We link this information with administrative data containing a complete record of all criminal charges, including the type of crime, when it took place, and suspected offenders. These data can additionally be linked to the prison register with information on the actual time spent in prison.

We merge the court data with administrative registers provided by Statistics Norway using a rich longitudinal database that covers every resident from 1967 to 2019. For each year, it contains individual demographic information (including sex, age, and the number of children), socioeconomic data (such as years of education, earnings, and employment), as well as geographical and firm identifiers. Finally, we link these data to a database of all reimbursement claims (KUHR) for the period 2006-2019 that are filed by publicly-funded health-care providers to the national health insurance system.[23] Using this information, we categorize a health-care visit as a separate entry identified by the date of visit. Further, an entry in this database has information on the type of health-care provider (e.g., type of practice, speciality, municipality of practice), the patient (e.g., identification number, month of birth, municipality of residence, sex, age), the associated health diagnosis, and the reimbursement rate and deductible paid by the patient. Depending on the type of health-care provider, the diagnosis codes assigned to each entry follow either the International Classification of Diseases (ICD-10) or the International Classification of Primary Care (ICPC-2).[24]

As in Bhuller et al. (2020), to ensure the validity of the random judge design, we restrict the sample to randomly allocated nonconfession cases decided by a regular judge. Our main IV sample uses cases decided from 2011 to 2014 so that each defendant's health outcomes can be followed for up to 5 years before and after the decision, while the judge stringency instrument is based on the entire period from 2005 to 2014. The main IV sample includes 22,456 cases. For the event study analysis, we will focus on the subset of incarcerated defendants including 12,314 cases.

[22] By contrast, offenders in the US are not eligible for unemployment insurance benefits upon release, have little access to public housing (Council of Economic Advisers, 2016), and are often denied access to food stamps, leading to higher rates of recidivism (Tuttle, 2019).

[23] For each contact a patient has with a publicly-funded health-care provider, a bill is sent to the Norwegian Health Economics Administration (HELFO).

[24] More information can be found here and here.

The main outcome variables we examine are the total number of health-care visits or the probability of any health-care visit over a given period. We further decompose these outcomes into mental or physical health-related visits based on the ICD-10 or ICPC-2 diagnosis codes associated to each visit. These codes give the reason for a visit, even when it is not a first-time diagnosis. This means that, after a patient has been diagnosed with a certain disease for the first time, any follow-up visits are assigned the same diagnosis code. Each visit can be associated with single or multiple codes, and all of them are used in our definition. This implies that a visit associated with multiple codes related to a physical and mental health issue will be defined as both a physical and a mental health-related visit.

Mental health visits are defined as visits associated with a code in the psychological category of the international classifications (i.e., categories F00-F99 of the ICD-10), or with a code whose label includes some specific words such as "depression", "suicide", and "addiction".[25] Given the distribution of mental health diagnoses (see Appendix Table A1), we further decompose mental health visits into addiction- and non-addiction-related subcategories. The "addiction" category includes all substance abuse-related visits, whereas the "other" category includes all other mental health diagnoses. Substance abuse visits account for about 48.5% of mental health visits,[26] and correspond to drug abuse, alcohol, medication, and stimulant abuse (Appendix Table A2). The "other" category primarily consists of depression, anxiety and stress-related diagnoses (see Appendix Table A3).[27] For simplicity, we hereafter refer to this category as mood disorder diagnoses, although the category is broader. Physical health visits are defined as visits associated with a code not belonging to the mental health category. Appendix Table A4 indicates that the most common diagnoses in this category are either general or related to a musculoskeletal disorder.

2.6 Descriptive Statistics

Appendix Table A6 provides descriptive statistics for our main sample. Overall, the defendants in our sample are young, very frequently male, low educated and have a high rate of unemployment. About half of the cases in our sample are sentenced to prison, and more than one quarter involve a violent crime. Economic, property, and drug crimes make up slightly more than 10% each of all crimes. Prison sentences are usually short, with a median of 6 months (the full distribution of sentence length is available in Appendix Figure A2). Interestingly, health-care utilization is high among defendants, with 90% having at least one health-care visit the year preceding the crime, and 50% having more than eight visits over the same period. Mental health visits are also highly

[25] In practice, 99.9% of mental health visits defined this way are associated with a code entering the psychological category of the international classification.

[26] As measured in the data set of all health-care visits for our sample of defendants in 2010.

[27] This categorization is also based on descriptive studies run in Norwegian prisons finding that substance use, depression, anxiety, and personality disorders are the most prevalent mental disorders (Kjelsberg et al., 2006).

prevalent, with 54% of the sample having at least one mental health visit the year preceding the crime. [28]

Appendix Table A5 compares the prevalence of health-care visits in the sample of defendants and in a sample drawn from the general population and matched to our sample for 2010. Column (1) shows the prevalence of health-care visits for this "matched control" population.[29] Since our sample of defendants is very negatively selected on many dimensions, including health, this matched control population provides a more relevant comparison group than the whole general population. Column (5) systematically displays significant differences between incarcerated defendants and the matched control population. In 2010, the sample of defendants had, on average, a 1.1 standard deviation higher number of mental health visits (i.e., five) than the general population. The difference is lower for physical health visits, down to 0.2 standard deviations.

Table A8 provides detailed descriptive statistics on the distribution of different types of health-care visits in the sample, comprising the average monthly probability and number of visits computed over the 30–36 months before the crime. This confirms the high prevalence of mental health visits, with an average probability of 20% having at least one mental health visit each month. Among mental health visits, the most common reasons are for substance abuse (includes any type of substance, such as alcohol, drugs, medication, etc.), severe mood disorders (i.e., depression), and light mood disorders (i.e., anxiety, stress, or sleep disturbance). Differences between defendants sentenced and not sentenced to prison after the case decision are typically small but indicate that incarcerated defendants have a slightly higher prevalence of mental health problems.

Overall, the descriptive statistics support the idea that the population of defendants is negatively selected in terms of health, calling for the use of econometric methods dealing with this selection. They also point to a high level of health-care utilization among the sample of defendants, seemingly despite their less favorable socioeconomic status.[30]

3 Methodology

To evaluate the causal impact of incarceration on the health of the defendants and their families we use two different methodologies. We start by presenting the two-way fixed-effects (TWFE) method-

[28] Appendix Table A7 provides the same statistics for the event study sample restricted to cases sentenced to prison. This sample is quite similar to our main sample, with a slightly higher proportion of men, and a higher likelihood of having been charged in the 5 years before the crime.

[29] This population includes individuals from the general population who were never involved in any criminal activity as measured by police arrests, that are matched to the sample of incarcerated defendants based on the following characteristics: time-invariant variables (female, foreign-born, month of birth) and time-varying variables matched on years 1, 2, 3, 4, and 5 before case decision (years of education, marital status, number of children, employment status, number of hours worked, if parents had a charge or prison spell).

[30] We return to the distinction between health and health-care utilization in Section 4.2.

ology that accounts for unobserved permanent heterogeneity by including case-by-defendant fixed-effects and common time effects, relying effectively on variation in the timing of court decisions. We then describe how we exploit the random assignment of cases to judges in an IV strategy.

3.1 Event Study Design

The event study compares the evolution of outcomes for defendants incarcerated at different points in time. For this analysis, we restrict the sample to cases of incarcerated defendants decided between 2011 and 2014 to be able to observe outcomes both 5 years before and after the case decision. We define the event as the month of court case decision[31] to incarcerate the defendant and estimate the following equation:

$$Y_{i,t} = \alpha_{ic} + \sum_{\substack{j=-60 \\ j\neq -1}}^{60} \beta_j D_j + \gamma_t + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the outcome variable (e.g. number of health-care visits) in month t for individual i, D_j are dummies measuring the distance to the month of the court's incarceration decision (i.e., the event), α_{ic} are case-by-defendant FEs, and γ_t are common calendar time (month $\times$ year) effects. By including case-by-defendant FEs, we control for all factors that are time-invariant at the individual and case levels. Calendar month $\times$ year FEs account for the common influence of time trends on the defendant's outcome.

Identification of distance-to-event effects $\hat{\beta}_j$ based on the event study above relies on the assumption that the timing of the court decision is random, conditional on α_{ic} and γ_t. Under this assumption, an OLS estimation of Equation (1) would provide estimates of $\hat{\beta}_j$ that can be interpreted as the average treatment effect at month j since the court decision event for defendants who were incarcerated. However, as we only include incarcerated individuals in the event study estimation (no never-treated units),[32] we can run into the issues related to (i) under-identification, (ii) negative weights, and (iii) identification of long-term causal effects, as highlighted by Borusyak et al. (2021). We therefore implement the methodology they propose in all our TWFE estimations.[33]

[31] The month of court case incarceration decision is chosen as it complements the timing used for the IV strategy presented below. Alternatives to the incarceration decision include the time of crime occurrence or the time of prison entry. In Section 4.3 we provide robustness checks from event studies centered around these alternative events.

[32] One could have attempted to included unincarcerated defendants as "never-treated" units, but the selection into incarceration makes it unsuitable as a control group. The unincarcerated are on very different (lower) levels when it comes to mental health visits and do not follow the same trends over time in the pre period. Indeed, even if we condition on committing a crime, being incarcerated is very likely correlated with the severity of the crime, and this could, for instance, influence health outcomes on top of the effect of incarceration. An additional challenge in our context is that most unincarcerated defendants typically receive other types of punishments, such as probation, community service or fine, which further complicates the suitability of this group as a pure control. We return to this issue in Section 4.2.

[33] Underidentification refers to the fact that in the absence of never-treated units and when unit and time FEs are included, it is impossible to point identify the distance to the event dummies D_j in the fully dynamic specification. In addition, in the 'static' specification where all pre- and post-event distance dummies are aggregated into a binary

A potential threat to identification could come from concurrent influences of events that precede the event of incarceration. In our context, the event of incarceration is always preceded by the events of crime and the onset of trial. If defendant health outcomes are also affected by these events, then we could expect changes in defendant outcomes already prior to incarceration. A visual inspection of pre-trends from the event studies can be informative about the presence of such influences. We return to this in Section 4.3, where we also provide robustness checks by alternating the reference points used in the event studies, as well as sensitivity analysis from a battery of additional tests.

3.2 Random Judge Design

We complement the event study design with an IV strategy that takes advantage of the random assignment of cases to judges, as in Bhuller et al. (2020). We are interested in estimating the following relationship:

$$Y_{i,t} = \beta_t I_{i,0} + X_i^{'} \theta_t + \upsilon_{i,t} \tag{2}$$

where β_t is the coefficient of interest, $I_{i,0}$ is an indicator variable equal to one if individual i has been sentenced to prison at time zero (normalized to be the time of the court decision), and $Y_{i,t}$ is the outcome variable measured in time t after individual i's court decision. As the randomization of cases to judges occurs within the pool of available judges within a court-by-year cell, we always include fully interacted court-by-year FEs among the vector of controls $X_i^{'}$.

The OLS estimation of Equation (2) could raise concerns of a selection bias, as incarcerated defendants are unlikely to be comparable to the unincarcerated. Indeed, Appendix Table A8 confirms differences in their background characteristics. The random judge design addresses this concern by exploiting the fact that cases are conditionally randomly assigned to judges and that some judges are systematically more stringent that others. Taken together, this leads to as-good-as random variation in the probability a defendant will be incarcerated depending on the judge the case is assigned. We utilize this exogenous variation in $I_{i,0}$ to draw inferences about the causal effects of incarceration on defendant health. Our main analysis is based on the two-stage least squares (2SLS) estimation of β_t with Equation (2) as the second-stage equation and a first-stage equation specified as:

$$I_{i,0} = \gamma Z_{j(i)} + X_i^{'} \delta + \eta_{i,0} \tag{3}$$

where $Z_{j(i)}$ is the leave-out mean incarceration stringency of judge j assigned to handle the case

post variable, the long-term effects are associated with negative weights because it is implicitly assumed that the effect of each period is constant. Finally, this also leads to the spurious identification of the long-term effects given that no nontreated unit can serve as a reference group in the final period. We therefore implement the methodology and associated Stata package *did_imputation* developed by Borusyak (2021). See Borusyak et al. (2021) for further details.

of individual i.[34] Under the assumptions of instrument exogeneity and monotonicity, the 2SLS estimand can be interpreted as the positive weighted average of the causal effect of incarceration among defendants that are more likely to receive an incarceration decision if assigned to a stricter judge, and vice versa. This means that, unlike the event study, the IV approach yields an estimate of the effect of incarceration on the population of compliers. To improve precision, we include, in addition to the court-by-year FEs, a rich set of background characteristics capturing defendants demography, type of crime, past work, and criminal history in the vector of control variables X_i'.

Importantly, the validity of our IV design requires the instrument to be relevant, i.e., that judge stringency has a significant impact on the incarceration probability of defendants. Appendix Table A12 reports first-stage estimates with and without the set of control variables. As shown, the first-stage estimates are stable across specifications and by year following the court decision with an F-statistic of around 50. For a 10-percentage point increase in judge stringency, the probability of being incarcerated increases by about 3.6 percentage points.

For our instrument to be valid, the stringency of a judge must also be uncorrelated with pre-existing defendant and case characteristics that could affect a defendant's future outcomes (even conditional on fully interacted court-by-year FEs). Appendix Table A13 tests the assumption of the random assignment of cases to judges. While demographics, type of crime, past work, and criminal history variables are highly predictive of the incarceration decision, these factors are not associated with the stringency of the assigned judge. This provides evidence that court cases are randomly assigned in our sample, conditional on court-by-year FEs. Appendix Figure A3 further supports this randomization, showing no systematic correlation between judge stringency and the predicted number of health-care visits (predicted using the same set of covariates as those in Table A13).

The conditional random assignment of cases to judges is sufficient for a causal interpretation of the reduced form impact of being assigned to a stricter judge on defendant outcomes. However, interpreting the IV estimates as measuring the causal effect of incarceration further requires an exclusion restriction: the incarceration rate of the judge should affect the defendant's outcomes only through the incarceration sentencing channel and not directly in any other way. Under heterogeneous effects, monotonicity must also be assumed for the IV estimates to be interpreted as LATEs (Angrist and Imbens, 1994), which requires that defendants who are incarcerated by a lenient judge would also need to be incarcerated by a stricter judge, and vice versa for nonincarceration.[35]

[34] As described in Section 2.1, we calculate judge stringency as the leave-out mean judge incarceration rate for all randomly-assigned cases each judge has handled over the 2005–2014 period, including both past and future confession and nonconfession cases.

[35] Recent literature raises concerns about the monotonicity assumption in the random judge IV designs (see, e.g., Frandsen et al. (2023)). As our study uses the same random judge IV design as in Bhuller et al. (2020), we refer to Section IV.B in that paper for further discussion of the exclusion and monotonicity assumptions in our context.

4 The Impacts of Incarceration on Defendant Health

We now provide evidence on the impact of incarceration on defendant mental health using the event study design and the random judge design discussed in Section 3.

4.1 Main Results

We start by estimating Equation (1) using the monthly panel of incarcerated defendants with cases decided between 2011 and 2014, following each defendant across the 60 months before and after court decision. Figure 1 graphically illustrates the results from this event study where we plot the coefficient estimates of the time-to-event dummies $\hat{\beta}_t$ along with the corresponding 95% confidence intervals.[36] These coefficient estimates should be interpreted as showing the effects of being incarcerated at time zero, relative to the pre-event period.

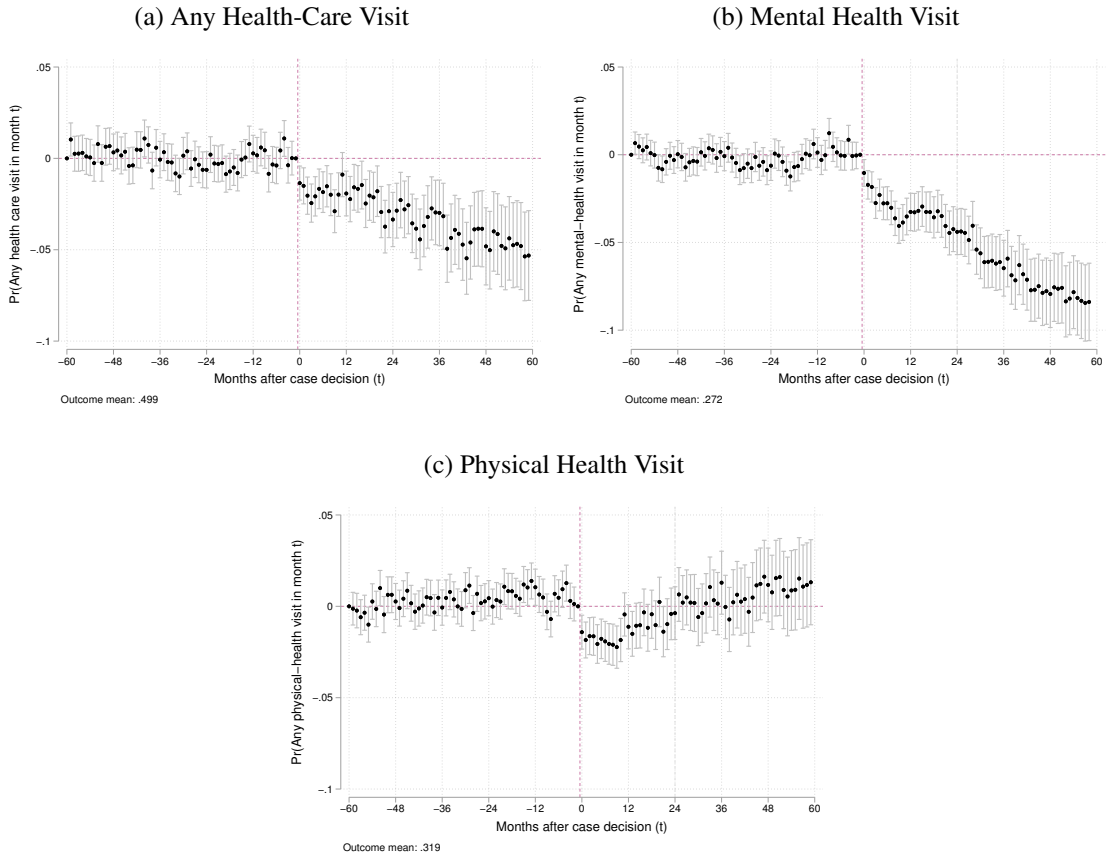
We first consider whether a defendant had any visits to a health-care provider in a given month, irrespective of the nature of the visit or the type of health diagnosis. Figure 1a indicates that there are no changes in the probability of a health-care visit prior to the incarceration event, and large, persistent and statistically significant reductions post-event. At 60 months after the incarceration event, incarcerated defendants have a five percentage point lower probability of a health-care visit, which suggests a 10 percent reduction compared to the pre-event mean of 50 percent.

Next, we decompose health-care visits depending on whether the visit is related to a mental or a non-mental health problem using diagnoses codes that are assigned to each visit. Focusing on mental health-related visits in Figure 1b, we again find no changes in the probability of a mental health visit prior to the event, and large, persistent and statistically significant reductions post-event. In relative terms, we find that incarcerated defendants experience a 30 percent reduction in the probability of a mental health visit at 60 months after the event, when we compare the estimated 8 percentage point reduction to the pre-event mean of 27 percent. By contrast, we do not find any meaningful impacts on the probability of a physical (non-mental) health visit beyond a reduction in the first 12 months. We return to an interpretation of this temporary reduction in Section 4.2.

We now tune in on mental health-related visits. Table 1 provides results from the TWFE and the random judge IV estimations, along with standard OLS estimates. Panel A shows estimates for the probability of having a mental health visit in a given month, while Panel B shows estimates for the number of monthly mental health visits. In each panel, we further distinguish between health visits that take place during months 1–12 and months 13–60 after the case decision, respectively. Comparing Columns (1)-(3), we find striking differences between the positive OLS estimates reported

[36] As we include both calendar time FEs and case-by-defendant (unit) FEs in the event study specification, we exclude two time-to-event dummies to identify calendar time effects separately from the time-to-event effects for the graphical representation (Borusyak and Jaravel, 2017). We exclude both the first time-to-event dummy (i.e., month 60 before the court decision) and the last one before the event (i.e., month 1 before the court decision).

Figure 1: The Effects of Incarceration on Health-Care Visits.



Notes: The sample consists of nonconfession criminal cases sentenced to prison and processed in 2011–2014. The graphs plot the coefficients from the distance dummies $\hat{\beta}_t$ from an OLS estimation of equation (1). The estimations includes controls for case-by-defendant and month × year FEs. Standard errors are clustered at the case level and the vertical bars plot 95% confidence intervals.

in Column (1) and the negative TWFE and IV estimates in Columns (2)-(3). Consistent with the descriptive evidence presented in Section 2.6, the OLS estimates suggest that mental health visits are more common among incarcerated defendants. By comparison, TWFE estimates show significant reductions in both the probability of having at least one mental health visit per month and the average number of monthly mental health visits. Using the TWFE coefficient estimate over the 5 year period post-event at an annual frequency, we find an estimate of about 0.8 mental health-related visits per year per inmate that did not occur because of incarceration. This can be compared to the average number of mental health related visits per inhabitant per year in the general population of 0.96 in 2010.[37] As seen in Column (3), the IV estimates, while more imprecise, also indicate strong reductions in mental health visits.

[37] We provide estimates from alternative frequencies of mental health visits in Section 4.3. Another way to benchmark our estimates is to compare these with the mental health impacts of other interventions. For instance, Baicker et al. (2013) take advantage of an Oregon experiment where Medicaid coverage was randomly allocated to people on a waiting list with a lottery. Using the lottery as an instrument for actual Medicaid enrolment, they found a 30% decrease in the likelihood of a positive depression screening. Although the context and measure of mental health (using an eight-question version of the Patient Health Questionnaire) are different, this suggests that our effect is sizable.

Table 1: The Effects of Incarceration on Mental Health Visits.

A. Probability of Mental Health Visit			
	OLS	**TWFE**	**IV**
	(1)	**(2)**	**(3)**
Months 1–12	0.013***	-0.028***	-0.253*
	(0.004)	(0.003)	(0.139)
Dependent Mean	.256	.239	.257
Months 13–60	0.033***	-0.047***	-0.165
	(0.004)	(0.006)	(0.119)
Dependent Mean	.262	.241	0.263
B. Number of Mental Health Visits			
	OLS	**TWFE**	**IV**
	(1)	**(2)**	**(3)**
Months 1–12	0.015	-0.056***	-1.130**
	(0.016)	(0.012)	(0.569)
Dependent Mean	.633	.583	0.634
Months 13–60	0.098***	-0.074***	-0.886*
	(0.015)	(0.023)	(0.489)
Dependent Mean	.680	.596	0.681
Number of Observations	22,498	12,314	22,456
Controls:			
Demographics			✓
Type of Crime			✓
Past Work & Crime History			✓
Case x Individual FEs		✓	
Period FEs		✓	
Court x Case Entry Year FEs			✓

Notes: The esample of nonconfession criminal cases processed in 2011--2014. Standard errors clustered at the case level in the OLS and TWFE estimations and two-way clustered at the judge and defendant level in the IV estimation. 95\% confidence intervals.The table reports the estimates of the effect of being incarcerated on the probability (Panel A) and number (Panel B) of mental health visits. Column (1) reports the OLS estimates without controls or FEs, while column (2) reports the TWFE estimates which includes case-by-defendant and period (month $\times$ year) FEs. Column (3) reports the estimates from the IV, where the prison indicator is instrumented with the stringency score of the judge to whom the case has been assigned, and where we control for demographics (age, sex, foreign-born status, number of children, marital status, level of education), type of crime, past work, and crime history (indicator for being employed in year t-1 to t-5 before the year of the crime, indicator for being ever charged in year t-1 to t-5 before the year of the crime, indicator for being ever incarcerated in year t-1 to t-5 before the year of the case decision) and court-by-case entry year FEs. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

The sharp contrasts between the OLS and TWFE/IV in Table 1 are informative about the importance of selection bias in observational comparisons of incarcerated and unincarcerated defendants. The positive OLS estimates reported in Column (1) are likely due to selection bias–incarcerated defendants have worse health outcomes than do unincarcerated defendants, not because the former faced incarceration and the latter did not, but because the two groups also differ in background characteristics, either observed or unobserved, that correlate with their health. Once permanent individual characteristics are accounted for–as in the TWFE estimates in Column (2)–the incarceration effect estimates change signs and become negative. The latter finding indicates that incarceration may instead reduce the mental health adversities facing incarcerated defendants. When we rely on cross-sectional comparisons between incarcerated and unincarcerated defendants that otherwise

are identical along their observed or unobserved background characteristics–as in the IV estimates in Column (3)–we again reach the conclusion that incarceration lowers mental health visits.

It is further noteworthy that the IV estimates in Table 1, Column (3), are substantially higher than the corresponding TWFE estimates in Column (2), although the IV estimates also have much larger standard errors. A likely explanation is that under heterogeneous treatment effects and monotonicity in judicial decision-making, the IV estimates provide the LATE for compliers who were incarcerated solely because their case was assigned to a strict judge and who otherwise would have remained unincarcerated (Angrist and Imbens, 1994).[38] By contrast, the TWFE estimates provide the ATT, the average effect for all incarcerated defendants. Thus, if compliers have larger mental health responses of being incarcerated than do always-takers, then such treatment effect heterogeneity could be reflected in differences between the IV and TWFE estimates.[39]

4.2 Interpreting the Evidence

Incapacitation. One explanation for the observed drop in mental health visits immediately after the incarceration event is that when inmates are incapacitated, they might also be restrained from accessing out-of-prison health services.[40] If reductions in mental health visits are driven solely by such incapacitation effects, then we would expect to find only temporary declines over the duration of a prison spell, and no differences beyond this. By contrast, the negative effects on monthly mental health visits reported in Figure 1b extend up to 5 years after the incarceration decision, with gradually stronger effects as we move further away from the incarceration event. This evidence thus lends strong support against the declines in mental health visits being driven mainly by prisoner

[38] We discuss results from a reverse sample IV in Section 4.3 that provides a test of an implication of monotonicity.

[39] Alternative explanations are unlikely. For instance, the IV estimates capture differences in the potential outcomes of incarcerated and unincarcerated compliers, and in our context, unincarcerated compliers also receive other non-custodial punishments (e.g., community service, probation or fine). The IV estimates therefore are by construction of a relative nature comparing different treatments. In Section 4.2, we provide evidence on treatment effects for alternative punishments that are assigned to unincarcerated defendants using the TWFE approach showing that some of these alternative treatments also improves mental health. All else equal, this implies that the IV estimates should be smaller than the TWFE estimates. Moreover, the TWFE estimates may also be influenced by contemporaneous events that arise at the same time as the incarceration event (e.g., job displacement, family disruption, victimization). Such concurrent events may also affect defendants' health outcomes and thus confound the TWFE estimates of incarceration. On the contrary, to the extent that such events are not caused by incarceration, the IV estimates purge their influence on health outcomes. Note, however, that one may expect negative shocks to adversely impact incarcerated defendants' mental health, which might negatively bias the TWFE estimates. The presence of concurrent negative shocks is thus unlikely to explain the differences between our TWFE and IV estimates, as we find the latter to be larger than the former.

[40] Inmates in Norwegian prisons have equal rights to public health services as the population at large (Moe, 2018). However, the health-care database (KUHR) we have access to covers primarily out-of-prison public health services, along with health services procured by the prison authorities from out-of-prison practitioners for inmates that require such special services. Standard in-prison health services or checkups are not usually recorded in this database. To the extent that in-prison health services substitute for out-of-prison health services, we would thus expect a decline in health-care utilization measures based on the KUHR database during the period inmates are incarcerated.

incapacitation.[41] Indeed, prison sentences are typically short in Norway, with a median length of 6 months, and with most inmates having a sentence of 1 year or less (see Appendix Figure A2). As shown in Table 1, we reach the same conclusion regarding persistent declines in mental health visits when we split the window of observation to be the first year and then the ensuing 4 years post-incarceration. By contrast, the temporary decline in physical health visits and no significant differences beyond the first year that we found in Figure 1c could be attributed to incapacitation effects. In the remainder of this section, we will thus focus exclusively on the 13–60 month window after the case decision as the large majority of our sample then only access out-of-prison services.

De-addiction. Drug use is highly prevalent among prison populations, with survey evidence for Norwegian inmates suggesting that six of every 10 inmates report having consumed illegal drugs in the month prior to the prison spell served (Friestad and Kjelsberg, 2009). To help inmates suffering from substance abuse, the Norwegian correctional services provide extensive de-addiction prison programs, besides maintaining provisions for open prisons and offering prison work, education, and other rehabilitation services (see Section 2.2). Thus, one explanation for the observed drop in mental health visits could be that spending time in prison helps former inmates recover from drug-related problems, implying that they need fewer addiction-related treatments post-release. Substance abuse is also widely recognized as being strongly associated with mental health problems (National Institute on Drug Abuse, 2020). To the extent drug-related problems impair mental health (e.g., by causing depression, mood disorders), we may also expect fewer visits related to such mental health problems among former inmates. Similarly, if prison directly improves inmate mental health, then this may collaterally reduce their propensity for substance abuse.

To investigate these channels, we decompose our measures of mental health visits into those that strictly relate to addiction or drug use and those that relate to regular mental health diagnoses such as depression, mood disorders, and suicidal tendencies. Table 2 shows large, persistent, and statistically significant reductions in both addiction-related visits and regular mental health visits related to depression and mood disorder to health-care providers. The extensive margin effects are very similar for both addiction-related and mood-disorder-related visits, while the number of visits only significantly drops for mood-disorder-related visits. This evidence suggests that the drop in mental health visits extends beyond de-addiction, either because incarceration directly improves former inmates' mental health (which possibly also lowers their inclination for substance abuse) or because de-addiction improves general mental health.

[41] Another argument could be that the persistent reductions in mental health visits result from the incapacitation effects of *future* incarceration. Underlying this is that prison begets future crime, rather than deterring it, and thus also leads to a higher risk of future incarceration. However, Bhuller et al. (2020) find that incarceration reduces future crime in our context, with no meaningful impacts on future incarcerations.

Table 2: The Effects of Incarceration on Addiction- and Mood-Disorder-Related Visits.

A. Probability of Mental Health Visit		
	Addiction-Related Visit	**Mood Disorder-Related Visit**
	(1)	**(2)**
Months 13–60	-0.026***	-0.028***
	(0.005)	(0.005)
Dependent Mean	.128	.138
B. Number of Mental Health Visits		
	Addiction-Related Visit	**Mood Disorder-Related Visit**
	(1)	**(2)**
Months 13–60	-0.008	-0.068***
	(0.019)	(0.013)
Dependent Mean	.331	.268
Number of Observations	12,314	12,314

Notes: The sample sample of nonconfession criminal cases processed in 2011--2014. The estimation includes controls for case-by-defendant and period (month × year) FEs. Standard errors clustered at the case level. Emergency visits are health-care visits to an emergency room (ER). Addiction- and mood disorder-related visits are health-care visits to any health service provider for an addiction- and a mood disorder-related reason, respectively. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Health-care utilization. Another interpretation of the observed drop in mental health visits is that this reflects a reduction in former inmates' demand for health-care services rather than a mental health improvement per se. For instance, one can argue that inmates demand fewer health-care services after release as incarceration weakens their institutional trust and leads to animosity against public authorities. In the context of Norwegian prisons that offer relatively humane prison conditions and maintain equal rights to public health services for inmates as the population at large, we expect such effects to be less pronounced. This is supported by the descriptive evidence suggesting that our prison population maintains high levels of health-care utilization before and after prison (Appendix Table A6). In addition, we perform three additional analyses that support an actual mental health improvement. First, we consider the events of mental health emergencies (e.g., suicide attempts, acute stress disorders) and any health emergencies (i.e., any mental or physical health emergency). We envision that emergency visits more often capture changes in health conditions or behavior and not health-care demand effects that could exist holding constant individuals' health conditions. Second, we consider the severity of mood disorders to assess whether prison affects the intensity of mental health conditions, especially among individuals who tend to visit health-care practitioners more regularly. Thirdly, we split the sample by past mental health history to look at another dimension of heterogeneity in regularity of visits to health-care practitioners.

Table 3 provides TWFE estimates of the effects of incarceration on the probabilities (Panel A) and numbers (Panel B) of mental and any health emergency, respectively. In both cases, we find reductions in emergencies in the 13-60 months after the incarceration event.

Table 3: The Effects of Incarceration on Health Emergency Visits.

A. Probability of Health Visit		
	Any Health Emergency Visit	**Mental Health Emergency Visit**
	(1)	**(2)**
Months 13–60	-0.045***	-0.024***
	(0.003)	(0.002)
Dependent Mean	.078	.028
B. Number of Health Visits		
	Any Health Emergency Visit	**Mental Health Emergency Visit**
	(1)	**(2)**
Months 13–60	-0.062***	-0.022***
	(0.013)	(0.005)
Dependent Mean	.120	.044
Number of Observations	12,314	12,314

Notes: The sample sample of nonconfession criminal cases processed in 2011--2014. The estimation includes controls for case-by-defendant and period (month × year) FEs. Standard errors clustered at the case level. Emergency visits are health-care visits to an emergency room (ER). Mental health emergency visits are health-care visits to an ER for a mental health reason. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Next, we test whether the severity of mental health disorders is affected by facing prison. First, we construct a mental health severity index as the outcome, which is equal to 1 if the defendant has a health-care visit related to light mood disorders (e.g., anxiety, stress), equal to 2 for severe mood disorders (e.g., depression), and 0 otherwise (i.e., no or other mental health-related visits).[42] Second, we consider a 'conditional' mood severity index that is equal to 1 for light mood disorders (e.g., anxiety, stress), equal to 2 for severe mood disorders (e.g., depression), and missing otherwise (i.e., no or other mental-health visits). A possible concern with the latter analysis is related to sample selectivity, as this requires conditioning our sample based on an outcome that is also affected. To avoid that compositional changes severely affect our inference, we use an annual measure of mood-disorder visits and perform these analyses at an annual level, as having a mood-disorder visit in a given year is much more frequent than having a mood-disorder visit in a given month.[43]

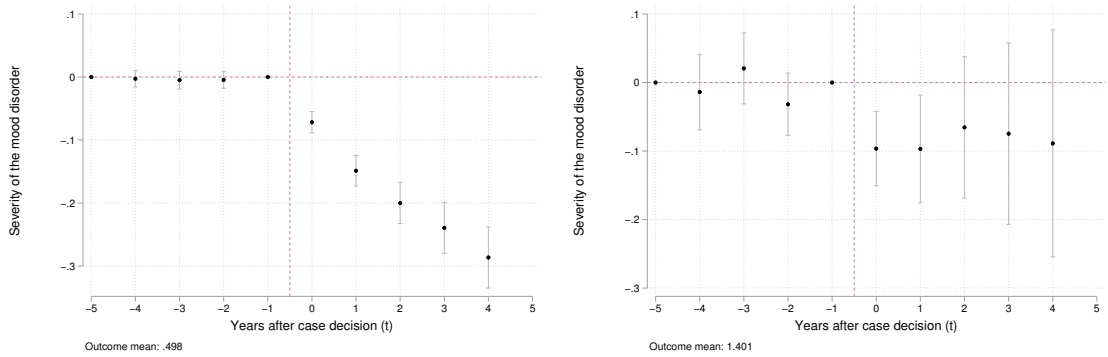
The TWFE estimates on our mood severity outcomes are shown in Figure 2. Panel (a) shows estimates on the 'unconditional' mood severity index as the outcome, suggesting a reduction in the severity index associated with mental health visits for mood disorders. These estimates, however, capture the combined effect of having a mood disorder-related visit and a change in the severity of mood disorder conditional on having a mood disorder-related visit. Next, we consider the 'conditional' mood severity index for defendants who continue experiencing mood disorder-related visits. Figure 2, panel (b), shows a reduction in the conditional severity index post incarceration.

[42]We also attempted using the Charlson comorbidity index (Charlson et al., 1987) as an alternative measure of health severity. However, this measure is not particularly suited for mental health disorders, as only one of the included comorbidities is related to mental health.

[43]The 'conditional' severity analysis is limited to incarcerated defendants who had at least one mood disorder-related visit in the five-year period before decision, and at least one mood disorder-related visit each year after decision.

Figure 2: The Effects of Incarceration on Severity of Mood Disorder-Related Visits.

(a) Unconditional Mood Disorder Severity Index (b) Conditional Mood Disorder Severity Index



Notes: The sample consists of nonconfession criminal cases sentenced to prison and processed in 2011–2014. Panel (a) uses an unconditional severity index that is equal to one for light mood disorders (e.g. anxiety, stress), two for severe mood disorders (e.g. depression) and zero for none or other mental-health disorders. Panel (b) uses a conditional severity index that equals one for light mood disorders (e.g. anxiety, stress), two for severe mood disorders (e.g. depression) and is missing for none or other mental-health disorders. Panel (b) also uses a sample restricted to individuals with cases decided between 2011 and 2014, who had at least one mood disorder visit in the five years before decision and at least one mood disorder visit per year after decision. The graphs plot the coefficients from the distance dummies $\hat{\beta}_t$ from an OLS estimation of equation (1). The estimations includes controls for case and month × year FEs. Standard errors are clustered at the case level and the vertical bars plot 95% confidence intervals.

Further, we consider inmates' mental health history and investigate heterogeneity in the impacts across inmates who are predicted to have, respectively, below median and above median mental health visits pre-event based on their background characteristics. If prison reduces the likelihood of mental health disorders among the former, then this could mean that prison safeguards against the onset of new mental health disorders among those with fewer pre-existing mental health conditions. Alternatively, if prison only affects the latter group, then this may reflect that prison rehabilitates or reduces the intensity of mental health disorders among inmates with pre-existing mental health disorders. To characterize inmates by their mental health history, we leverage our rich socio-demographic data to predict the probability of having any mental health visit within 5 years prior to the crime.[44] As shown in Table 4, Columns (1)-(2), there are no significant differences neither on the extensive (Panel A) or intensive (Panel B) margin between those predicted to be below and above median mental health visits pre-event. Keeping in mind the caveats of splitting the sample by pre-event outcome in a TWFE estimation, we also assessed this type of heterogeneity using inmates' actual mental health history. We define as defendants with no mental health history those with zero mental health visits in 2006 (56% of the sample), and as defendants with a mental health history those with a positive number of mental health visits in 2006.[45] We find that defendants with no mental health history experience a slightly larger decline in the probability and

[44]The prediction is based on the following covariates: gender, age, foreign-born, marital status, number of children, education level, employment status, benefit recipient, wage, municipality, a variable indicating if a child died in the last 5 years, and the age of the child at death. Details on this prediction are provided in Online Appendix B.

[45]We chose 2006 because this is the first year we have data on health-care visits. By holding the year of conditioning fixed, the health-care visits for treatment and control group in the future will be compared for the same year. However the distance from event will differ. An alternative is to condition on mental health visit five years before the event. This will hold distance from treatment constant but the outcome will then be measured in different years for treatment and controls. We chose to present results here for the former, however, results for the latter go in the same direction.

number of mental health visits but the differences are not statistically different. This set of results suggests that our findings are not mainly driven by inmates being discouraged to utilize services they accessed pre-prison. Prison may also safeguard against onset of new mental health disorders.

Table 4: Heterogeneous Effects by Mental Health History.

	Predicted Mental Health History		Actual Mental Health History	
	< Median Past Mental Health Visits (1)	> Median Past Mental Health Visits (2)	No Past Mental Health Visits (3)	Had Past Mental Health Visits (4)
A. Probability of Mental Health Visit				
Months 13–60	-0.052***	-0.040***	-0.053***	-0.039***
	(0.009)	(0.008)	(0.007)	(0.009)
Dependent Mean	.260	.227	.120	.391
B. Number of Mental Health Visits				
Months 13–60	-0.076**	-0.090***	-0.094***	-0.049
	(0.036)	(0.034)	(0.020)	(0.044)
Dependent Mean	.629	.571	.251	1.015
Number of Observations	5,248	5,526	6,849	5,465

Notes: The sample sample of nonconfession criminal cases processed in 2011--2014. The estimation has been run separately for each subgroups, and always includes controls for case-by-defendant and period (month × year) FEs. Standard errors clustered at the case level. Mental health history is predicted based on individuals' background characteristics using a sample of the Norwegian population aged 10 years or more in 2009 and alive in 2010. The estimation sample is split in two based on the median predicted probability of having had a mental health visit in the 3 years preceding the crime. Actual mental health history is measured as the total number of mental-health visits in 2006. 56% of the sample did not have any mental health visits in 2006 (i.e., first year of data). The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Rehabilitation and prison conditions. To assess additional mechanisms, we leverage the richness of our data to explore heterogeneity in the effects of incarceration on mental health along dimensions capturing differences in rehabilitation programs across prisons.[46] We consider four different splits of the data, capturing different dimensions of rehabilitation. First we look at inmates assigned to prisons with milder conditions, i.e., open prisons, compared to prisons with harsher conditions, i.e., closed prisons. Then we split the samples in whether inmates are assigned to prisons that have employment programs or not, and whether prisons have educational programs or not. Finally, we consider whether inmates are assigned to prisons offering mental-health/de-addiction programs or not. These results are only suggestive as there could also be compositional differences across inmates assigned to different prisons that could explain some of the differences we find.

Focusing on Table 5, Panel A, Columns (1)–(2), we find that inmates facing both mild (open prisons) and harsh (closed) prisons conditions have lower probabilities of mental health visits with similar sized impacts. However, when we consider the number of mental health visits in Panel B, we find much stronger reductions for inmates facing milder prison conditions. Notably, these

[46]For this analysis, only 85% of the sample of sentenced defendants is included, as the information on prison identifiers is missing for the other inmates, which is required to classify the type of prison facility inmates served in.

Table 5: Heterogeneity Effects by Prison Conditions and Rehabilitation Program Availability.

	Type of Prison Facility		Had Employment Program		Had Education or Training Program		Had Mental Health or De-addiction Program	
	Open	Closed	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Probability of Mental Health Visit								
Months 13–60	-0.045***	-0.040***	-0.040***	-0.022	-0.045***	-0.025*	-0.033***	-0.045***
	(0.009)	(0.010)	(0.007)	(0.042)	(0.007)	(0.014)	(0.011)	(0.008)
Dependent Mean	.198	.310	.254	.247	.264	.224	.275	.241
B. Number of Mental Health Visits								
Months 13–60	-0.098***	-0.011	-0.050*	-0.026	-0.058*	-0.018	-0.003	-0.072*
	(0.035)	(0.040)	(0.028)	(0.099)	(0.031)	(0.034)	(0.038)	(0.037)
Dependent Mean	.464	.788	0.624	0.630	0.652	0.549	0.679	0.594
Number of Observations	5,158	5,094	9,184	1,068	7,669	2,583	3,750	6,500

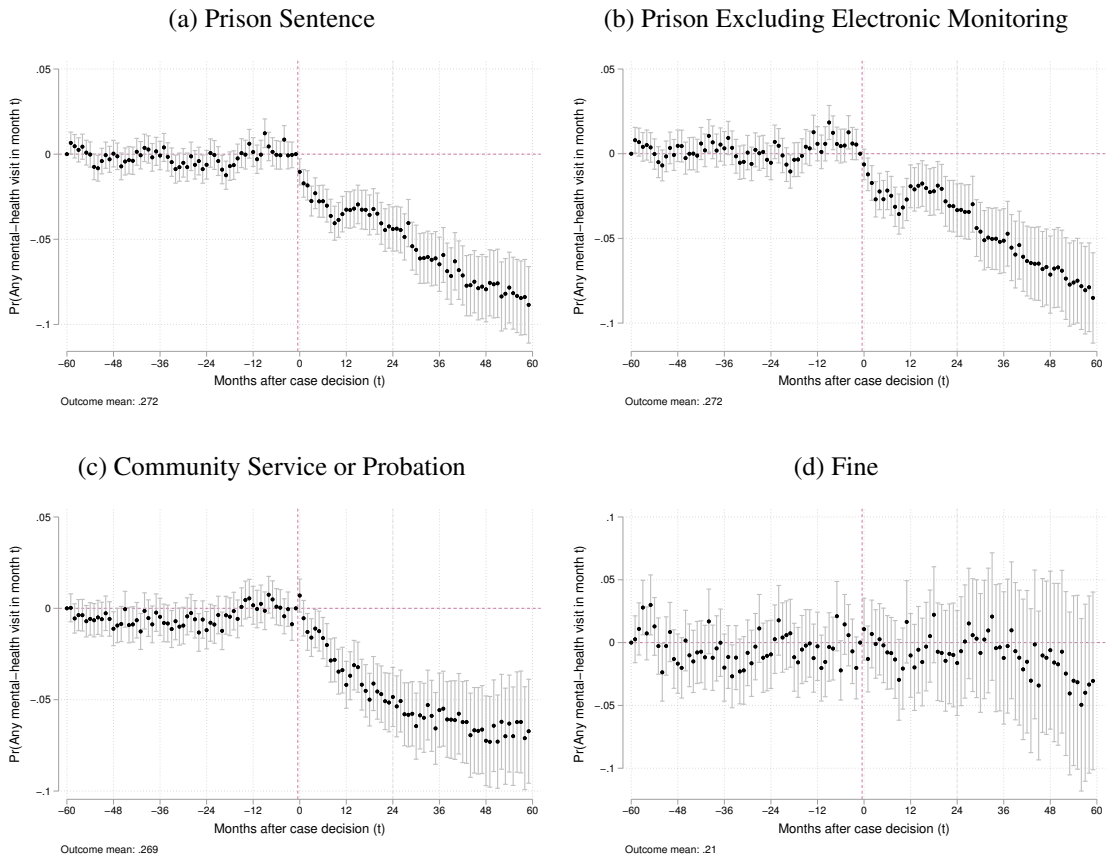
Notes: The sample sample of nonconfession criminal cases processed in 2011--2014. The estimation has been run separately for each subgroups, and always includes controls for case-by-defendant and period (month × year) FEs. Standard errors clustered at the case level. Prison conditions are defined based on the first prison that an incarcerated defendant is assigned to after court decision and the information on prison IDs is available for 85% of incarcerated defendants. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

inmates experience larger reductions despite having a lower dependent mean on average. These findings suggest that exposure to milder prison conditions seems to matter more at the intensive than the extensive margin of mental health problems. Next, we examined more closely the impacts of being assigned to prisons with specific rehabilitation programs—work-related (Columns (3)–(4)), educational (Columns (5)–(6)), and mental health and de-addiction-related (Columns (7)–(8))—on mental health visits. While we fail to reject that the effect estimates are statistically significantly different across these program types, neither at the intensive nor extensive margins, we do observe some differences in point estimates. In particular, the effect estimates show larger reductions for those assigned to prisons with educational and work-related programs, while the effects are bigger for those assigned to prisons with no mental health and addiction-related programs.[47]

Rehabilitation and non-custodial punishments. The aforementioned analysis suggests that exposure to milder prison conditions (i.e., open prisons or prisons with rehabilitation programs) may contribute to some of the impacts we find on incarcerated defendants' mental health visits. In order to delve deeper into the role that rehabilitation programs play in tandem with prison sentences, we now assess treatment effect heterogeneity across defendants assigned to various custodial and non-custodial punishments, such as prison, electronic monitoring, probation or community service, and fine. Notably, as described in Section 2.2, these punishments vary not only in the degree of punitiveness but also the extent to which rehabilitation programs enter as an essential feature.

[47] In order to learn more about other mechanisms for why prison reduces mental health visits in our context, we also performed a series of additional heterogeneity analyses. First, we considered inmates' employment history, depending on whether or not they were working in at least one of the five years preceding case decision. The recidivism-reducing and employment-enhancing effects of incarceration found in Bhuller et al. (2020) are concentrated among previously non-employed inmates. Thus, evaluating the heterogeneous effect of prison on mental health along the employment dimension is particularly relevant for understanding whether the employment channel is also important for mental health outcomes. Appendix Table A9, Columns (1)–(2), report the effects separately for the previously employed and nonemployed. We find no significant differences in extensive or intensive margin responses by previous employment status. This suggests that improvements in employment outcomes do not channel the whole effect on mental health. We then consider heterogeneity in whether defendants faced a prison sentence for a violent or a nonviolent crime. Comparing defendants who committed violent and nonviolent crimes, we find little evidence for heterogeneity in the effects of incarceration on the probability of mental health visits (Appendix Table A9, Columns (3) – (4)). However, we find that the point estimate on the number of mental health visits is larger for inmates who committed a violent crime. Finally, we consider heterogeneity by prison spell length in Appendix Table A9, Columns (5) – (6), by dividing the sample across those who received a sentence below the median length at 6 months and those who received a sentence above 6 months. We find that effects are stronger for those receiving a sentence above 6 months, however, on the extensive margin there is a meaningful reduction also for those with shorter sentences. These two latter set of results give some support to effects being stronger for defendants committing violent crimes and serving longer sentences, however, these groups do not explain the whole reductions in mental health.

Figure 3: Custodial and Non-Custodial Punishments.



Notes: The sample consists of nonconfession criminal cases processed in 2011–2014 where the defendant was sentenced to either community service or probation in panel (c) covering 35.1% of sentences (N=8,548) or fine in panel (d) covering 4.4% of sentences (N=1,072). The graphs plot the coefficients from the distance dummies $\hat{\beta}_t$ from an OLS estimation of equation (1). The estimations includes controls for case and month × year FEs. Standard errors are clustered at the case level and the vertical bars plot 95% confidence intervals.

In Figure 3, Panel (a), we start by showing our baseline estimates for defendants assigned to a prison sentence. From September 2008 and onwards, Norway gradually introduced electronic monitoring as an alternative way of prisoners serving short prison sentences in some regions, until a national implementation in May 2014. In our data, we find that around one out of five defendants assigned to a prison sentence actually served this sentence either partly or fully under electronic monitoring (EM). In Figure 3, Panel (b), we thus focus on the remaining defendants who served their sentence solely in a custodial facility. This illustration shows that our main findings hold if we restrict our analysis to defendants who served their sentence in prison and would generally have a somewhat different access to in-prison programs than those serving a sentence on EM.

In our context, about 47% of the defendants who go through a court trial are not given a prison sentence. Three out of four non-incarcerated defendants are convicted and assigned to a community service or probation sentence, rather than receiving an acquittal, while one in ten receives only a fine punishment. In Figure 3, Panels (c)-(d), we provide TWFE estimates for defendants that were assigned to a community service/probation sentence or a fine sentence, respectively. In the latter case, the defendant only faces a monetary penalty, while in the former case, there is a more active

involvement of correctional services, through supervision and provision of rehabilitative services (e.g., guidance on job search and de-addiction programs). In this sense, some of the rehabilitative services that are provided to incarcerated defendants are also served to those sentenced to the former group receiving non-custodial punishments. Indeed, in Figure 3, we find evidence that while a fine treatment has no detectable impact on mental health visits (Panel (d)), there is a reduction in mental health visits for those given community service or probation (Panel (c)). Interestingly, as the latter group of defendants have the same rights to public health services (outside of prisons) as normal residents, this decline in mental health visits is not driven by a short-term incapacitation, which might be the case for incarcerated defendants (Panels (a)-(b)). We interpret this evidence as providing additional support for the rehabilitative role of correctional services in our context.

Relation to the literature. Contrary to correlational studies often highlighting that inmates are at a higher risk of death or are more likely to have health issues, our results point to the positive causal impacts of prison on mental health. The two causal studies that are closest to ours, however, consistently find that incarceration decreases mortality risk. Using a modified DiD framework, Norris et al. (2023) find that incarcerated defendants face lower mortality rates while in prison than do nonincarcerated defendants, and they do not find a positive effect on mortality risk post-release. This protective effect of prison is stronger for deaths caused by homicides, overdoses, or suicide. Focusing on the intensive margin of prison sentence, Hjalmarsson and Lindquist (2022) exploit reforms in Sweden that provided a quasi-experimental variation in the time spent in prison. They show that more time spent in prison causes a decrease in mortality rates when focusing on specific populations or causes of deaths. Interestingly, they find that the decrease in mortality risk is partly driven by a decrease in the chance of suicide of about 80% in the 3 years post-release. This reduction is particularly strong for violent offenders and for offenders with a history of mental health disorders. Although both studies use mortality as an outcome and cannot be quantitatively compared with ours, their findings of positive incarceration effects on defendant health coincide with ours. In particular, the causes of deaths for which they assign the strongest decrease are related to mental health (suicide and overdose).

4.3 Robustness

In this section we assess the robustness of our main findings on how prison affects inmates' mental health to alternative specifications.

Alternative frequencies of health visits. Our data allows us to observe mental-health visits at a daily frequency, which we further aggregate to the monthly level in our analysis. Observing high frequency changes in mental-health visits allows us to build a strong case for causal inference as we

can closely assess changes around the incarceration event. However, a concern could be that such a high frequency is not appropriate to measure changes in health-care visits as these visits rarely happen every month. In our context, however, the population of interest is very negatively selected in terms of (mental) health. Incarcerated defendants in our sample have on average a 27% chance to having a mental-health related visit in a given month across the 12 months before case decision. Thus, the monthly frequency seems to be an appropriate unit of measurement for mental health visits in our setting. We still provide additional evidence on mental health visits at a quarterly and a yearly frequency in Appendix Figure A4. Based on these alternative frequencies, we observe 32% to 36% decline in the probability of having a mental health visit five years after case decision, comparable to our baseline estimates.

Preceding events and mean reversion. Our baseline event study is centered around the month of court case decision. As discussed in Section 2.1, a court decision is always preceded by the events of crime occurrence and criminal investigation. If the events of crime and investigation also causally affect incarcerated defendants' mental health, then we may expect pre-event changes in defendants' mental health outcomes. A related concern could be that defendants facing a court decision had just prior to the crime experienced a deterioration of their mental health, which possibly led them to a path of illicit behavior and crime. Following a court decision, we may thus see a reduction in mental health visits not because incarceration improves defendants' mental health condition, but simply because these individuals 'revert back' to a less extreme mental health condition. Similar to the concerns related to alternative preceding events, this concern of mean reversion may also lead to temporary spikes in pre-event mental health visits prior to the court decision.

While our graphical evidence in Figure 1b for mental health visits shows little indication of such pre-event effects, we take several steps in the following to address the above-mentioned concerns. First, in an attempt to encompass such pre-event effects, we alternate the reference points used in the event study. Our baseline uses the first time-to-event dummy (i.e., the 60^{th} month before the court decision) and the last time-to-event dummy before the event (i.e., the month before the court decision) as reference points. We now instead re-estimate the event study specification by setting the second reference point as the time-to-event dummy for the 20^{th} month before the court decision rather than 1 month before, as more than three of four defendants in our sample had committed the crime they were facing trial for during the 20-month period prior to the court decision. The event study estimates using the alternative reference point at -60 and -20 months are provided in the Appendix Figure A5b. These estimates confirm our baseline estimates (repeated in Figure A5a) that prison significantly lowers inmates' likelihood of having mental health-related visits, with reductions persisting beyond the first 12 months after the court decision. If anything, incarcerated defendants have slightly higher rates of mental health-related visits in the 20 months prior to the

court decision, which could be related to the onset of crime or the trial process. However, we do not see any spikes in pre-event mental health visits before the 20 months prior to the court decision, as would be expected if a deterioration of defendants' mental health led them to commit a crime.

Second, rather than changing the reference points, we re-estimate our event study using crime occurrence as the event. The results based on this analysis are presented in Figure A5c, where we assess changes in the probability of having a mental health visit during the four years before and the six years after the crime.[48] The patterns found are quite interesting. Apart from the first couple of months immediately after crime occurrence, where we see a spike of 5 percentage points in the probability of mental health visits, there is a gradual decline in the likelihood of mental health visits over the six year period after crime occurrence. Importantly, we see no evidence of changes in mental health visits *prior* to crime occurrence. The absence of pre-event effects prior to crime occurrence suggests that mental health episodes are not causing or preceded by crime. This finding provides a contrast to mean reversion, where we would expect a rise in visits prior to the crime and a fall afterwards. Instead, we see a spike in mental health visits *after* crime. A plausible explanation of this finding could be that the crime investigation initiated by the police authorities after a crime (in some instances) involves a mental health check-up of the offender, which gets recorded as a mental health visit in our data. In this sense, this immediate and short-lived spike is somewhat "mechanical". The short-lived spike in mental health visits after crime occurrence and before case decision may also provide an explanation for why we find some indications of increases of mental health visits in Figure A5b between crime occurrence and case decision. If instead an abrupt deterioration of the mental health condition causes crime, then we would expect to see an increase prior to the month of crime occurrence, which we do not find evidence for.

Consistent with our baseline event study evidence in Figure A5a, we also see that the decline in mental health visits in Figure A5c takes place mostly in the periods after case decision and release from prison. In Figure A5d, we further re-estimate the event study specification using the month of prison release as the event.[49] We again observe a decline in mental health visits after release.

Repeat offenders. Another potential issue with our baseline event study estimates relates to the presence of repeat offenders. If some defendants offend repeatedly and are reincarcerated within the observed period, then their future self will be used as a control for their first offence. To the

[48] Unlike our baseline that follows defendants over a five year period before and after the case decision event, we have a shorter pre-period and a longer post-period in Figure A5c that is centered around crime occurrence. This shift encompasses the median delay between crime occurrence and case decision of 328 days; see Appendix Table A7. In Figure A5c, we also mark as vertical dotted lines the 75th percentiles in the distributions of the time to case decision, the time to prison entry, and the time to prison release, which are all events that can only take place after crime occurrence. Similarly, we mark the 25th percentile in the distribution of time to case decision. The majority of incarcerated defendants are released from prison by the 2.5 years after crime occurrence.

[49] In this analysis, we drop all months between crime occurrence and prison release, and also drop 12% of the sample who we do not have a prison ID for (for instance, either because they emigrated, or served their sentence under EM).

extent that the treatment effects of incarceration are not constant over time, or when the effects of repeated incarcerations are not additive, the presence of repeat offenders in our sample can make it more difficult to interpret the event study estimates. To address these concerns, we re-estimate the event study specification by restricting the sample to the first observed offence within our sample period. The results from this exercise are presented in the Appendix Figure A5e. The results remain virtually unchanged. In fact, the decrease in the probability of mental health visits is somewhat more pronounced and precisely estimated than in the baseline.

Placebo. Another concern–also related to the issue of mean reversion raised above–can be that our estimates are driven by lifecycle patterns of mental health around particular age brackets. In particular, ages where most defendants commit crime or face trial overlap to a great extent with ages where we observe a rise in mental health disorders (typically in the twenties).[50] To safeguard against such concerns, we show in Appendix Figure A5f event study estimates from a placebo test performed for a sample drawn from the general population that is matched to our baseline sample of incarcerated defendants. We use a one-to-one matching procedure without replacement on a wide range of individual characteristics.[51] Using the decision month of the matched treated defendant as the event time, we perform the same event study estimation on the probability of having any mental health visit as in our baseline. Figure A5f shows no sign of any change in the probability of having any mental health visit after the assigned case decision month for the matched controls. If instead, we had found significant changes around the event in this exercise, then our estimates could be driven by lifecycle patterns of changes in mental health visits.

Sensitivity of the IV estimates. In Table 1, Column (3), we provided IV estimates of the effects of incarceration on defendants' mental health using a random judge design and the assigned judge's stringency as an instrument. While the IV estimates were much more imprecise than the TWFE estimates, both approaches suggested reductions in mental health outcomes. Following Bhuller et al. (2020), we also assess the sensitivity of our IV estimates. While the baseline IV estimates were constructed for cases assigned to judges who had handled at least 50 cases (to ensure sufficient number of cases to measure precisely judge stringency), we also estimated IV models for cases assigned to judges handling at least 75 or 100 cases, respectively. We further assessed the sensitivity of estimates to calculating judge stringency using nonconfession cases only, while in our baseline,

[50] In our data, 39% of defendants are aged between 20-30 at the time of their case decision.

[51] We use the same sample of matched controls from the general population as in Table A5. This sample includes individuals from the general population who were never involved in any criminal activity as measured by police arrests, that are matched to the sample of incarcerated defendants based on the following characteristics: time-invariant variables (female, foreign-born status, month of birth) and a wide range of time-varying variables matched on years 1, 2, 3, 4, and 5, respectively, before case decision (years of education, marital status, number of children, employment status, number of hours worked, if parents had a charge or prison spell).

we used all randomly assigned cases (i.e., including confession cases). Furthermore, we estimated a reverse sample IV, where we randomly split our sample in half and used one half to calculate the average incarceration rate of each judge, and then used these measures of judge stringency as an IV for incarceration in the other half of the sample. The results from these various sensitivity analyses are provided in the Appendix Table A11. While our estimates based on these approaches do not qualitatively change, their magnitude and precision vary across specifications. For the number of mental health visits, the coefficients vary from -0.5 to -0.9, and we lose precision in Columns (3)-(4). However, the point estimates based on the IV approach still confirm our main findings.

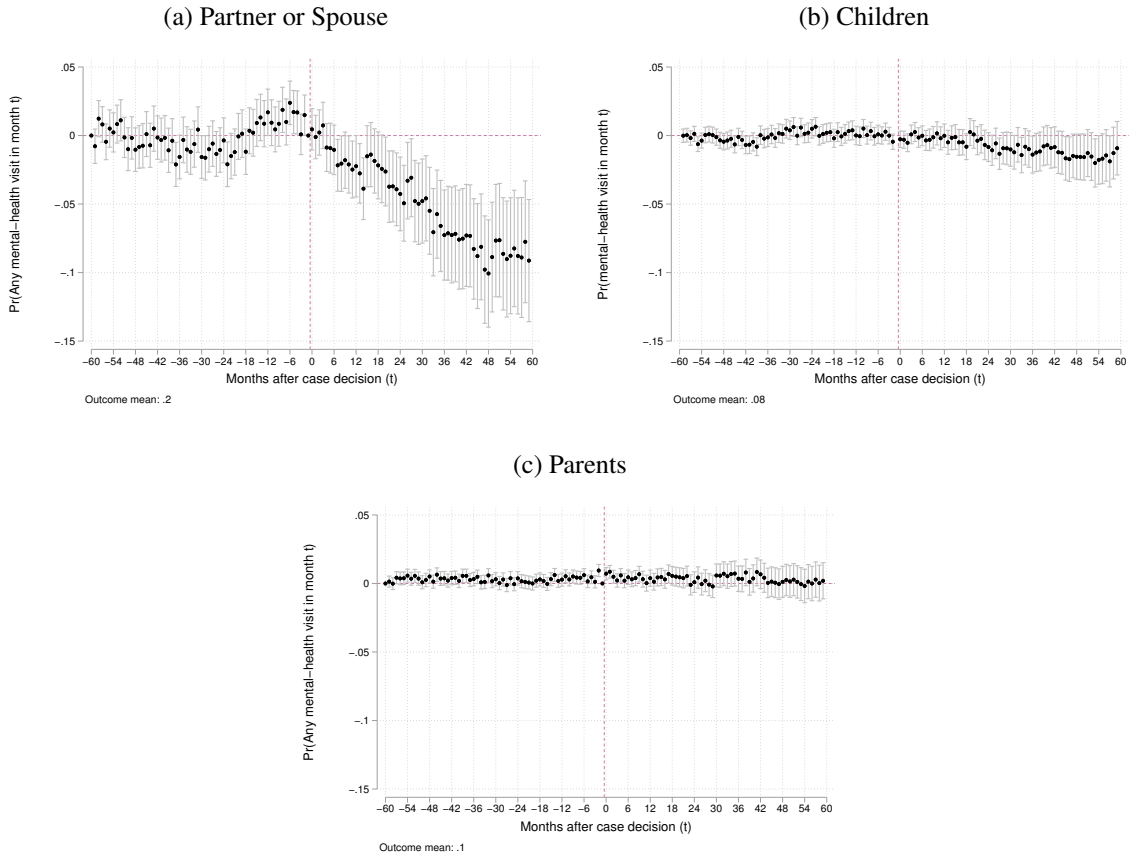
5 Spillover Effects on Family Health and Well-being

We now turn to spillover effects of defendant incarceration on their preexisting family members' mental health. These spillovers could come from the effect of removing the defendant from the family or as a spillover from the changes in the defendants' mental health because of prison. We start by estimating the impacts on the mental health outcomes of partners (including both the marital spouse or the previously cohabiting partner), children, and parents using the event study design.[52] The resulting estimates are shown in Figure 4. Consistent with the large and persistent negative effects in Section 4 found on defendants' mental health visits, we also find similar impacts on defendants' partner's mental health visits in Figure 4a. Notably, the impact profile is relatively flat before the defendant's incarceration and declines sharply after the event. Focusing next on the defendants' children in Figure 4b, we again notice the tendency of negative point estimates on their mental health visits post-incarceration, although the confidence intervals are comparatively wide. For the defendants' parents in Figure 4c, there is no evidence that they are affected.

Table 6 reports the corresponding estimates from the TWFE specification, which summarizes the event study estimates for the 1–12 months and the 13–60 months post-court decision. Column (1) confirms the event study results, indicating that incarcerated defendants' partners have a lower likelihood of a mental health visit (Panel A) and fewer mental health visits (Panel B), and the improvements in their mental health persists after the 12-month period post-court decision, when the defendant is also likely to be out of prison. Unlike defendants, their family members by construction do not experience incapacitation and thus, the reductions in their mental health visits are likely to reflect fewer mental health problems already from the initial period when the defendants faced the court decision. The similarity across the impact profiles for defendants and their partners, however, could reflect that both experience gradual improvements in their mental health. Next,

[52] Regarding children, we only include those aged 8 years or older at the time of decision. We end up with samples for the spillovers analysis consisting of 8,191 children spells, 2,346 partner spells, and 17,459 parent spells that are linked to the 12,314 incarcerated defendants in our main sample who had their case decided between 2011 and 2014.

Figure 4: The Effects of Incarceration on Family Peers' Mental Health Visits.



Notes: The sample of partners, children and parents of defendants sentenced to prison with nonconfession criminal cases processed in 2011–2014. The estimation includes controls for case and month × year FEs. Standard errors clustered at the case level. 95% confidence intervals. The graph plots the coefficients from the distance dummies.

we consider the incarcerated defendants' children in Column (2) and their parents in Column (3), respectively. As in the event study, we find reduction in the probability that children have a mental health visit, both during the 12-month post period and the 4-year period that follows. Similarly, we find negative coefficients on their number of visits although these estimates are imprecise. By contrast, for incarcerated defendants' parents, we do not find any evidence of an impact.

We now investigate some of the channels through which defendants' incarceration may affect their partners' mental health visits. We first consider the role of defendants' future partnership status. Notably, the sample of partners we considered above is defined as the incarcerated defendants' marital spouse or cohabiting partner in the year before the case decision, and is not limited to partners that continue staying together with the defendant after prison. While around 20% of the incarcerated defendants had a partner prior to case decision, only about one-third of these remained with the same partner 5 years after case decision.[53] We indeed find a reduction in the probability of

[53] By comparison, as shown in Appendix Figure A6a, around half of the previously partnered unincarcerated defendants remained with the same partner 5 years after case decision. And almost 80% of the incarcerated defendants had moved their residential address from the year prior to decision to 5 years after decision; see Appendix Figure A6b.

having a partner or spouse at month 60 after case decision for both defendants as well as their previous partners, as shown in Appendix Table A10, Panel A. This findings may indicate that changes in partnership status and household structure could contribute to our findings.

Table 6: The Effects of Incarceration on Family Peers' Mental Health Visits.

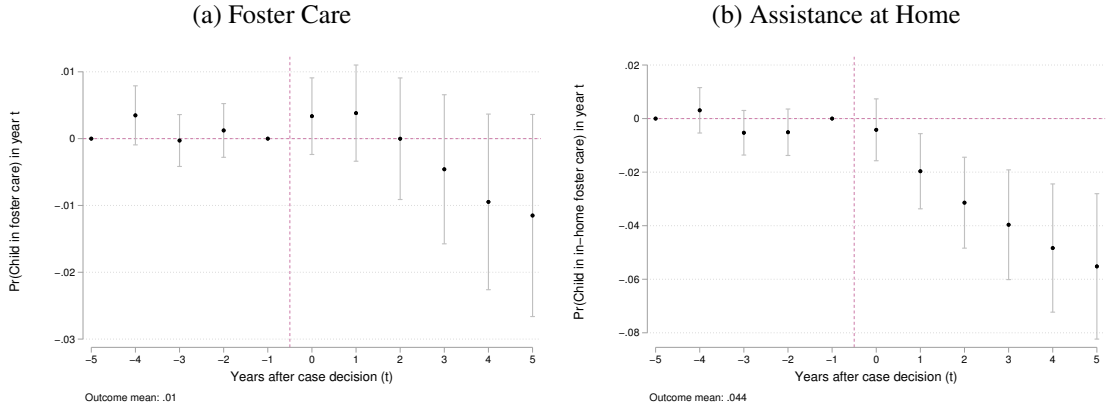
A. Probability of Mental Health Visit			
	Partner or Spouse	**Children**	**Parents**
	(1)	**(2)**	**(3)**
Months 1–12	-0.021***	-0.006**	0.001
	(0.006)	(0.002)	(0.002)
Dependent Mean	.170	.073	.100
Months 13–60	-0.056***	-0.009**	-0.001
	(0.011)	(0.004)	(0.003)
Dependent Mean	.171	.074	0.100
B. Number of Mental Health Visits			
	Partner or Spouse	**Children**	**Parents**
	(1)	**(2)**	**(3)**
Months 1–12	-0.046**	-0.009	0.006
	(0.019)	(0.007)	(0.004)
Dependent Mean	.363	.146	.180
Months 13–60	-0.138***	-0.018	0.006
	(0.034)	(0.012)	(0.007)
Dependent Mean	.361	.150	0.179
Number of Observations	2,346	8,191	17,459

Notes: The sample of family members of defendants with nonconfession criminal cases processed in 2011--2014. Column (1) reports the TWFE estimates on the sample of partners, and includes controls for case-by-defendant and period (month × year) FEs. Column (2) reports the TWFE estimates on the sample of children aged 8 years or older at the time of the case decision, and includes controls for case-by-child and period (month × year) FEs.. Column (3) reports the TWFE estimates on the sample of parents, and includes controls for case-by-parent and period (month × year) FEs. Standard errors clustered at he case level. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Next, we consider changes in mental health visits for incarcerated defendants who prior to case decision were single and partnered, respectively. Comparing incarcerated defendants by their past partnership status in Appendix Table A10, Panel B, Columns (2)-(3), we find that the previously partnered exhibit much stronger declines in the probability of mental health-related visits. This finding could be related to the presence of strong mental health spillovers within families. For instance, an improvement in the offender's mental health may spill over to the mental health of family members, which in turn may trigger further improvements in the focal family member's (i.e., offender's) mental health.[54] Similarly, in Appendix Table A10, Panel C, Columns (2)-(3), we also find such heterogeneity in the impacts on number of mental health visits.

[54]Interestingly, we found similar differences in the impacts on mental health-related visits when we assessed such heterogeneity across defendant age and education cells by past partnership status (results are available upon request). This led us to conclude that the differences in impacts for single and partnered defendants are not driven by other observed differences in their characteristics.

Figure 5: The Effects of Incarceration on Incidents of Child Protection Services Intervention.



Notes: The sample of incarcerated defendants in nonconfession criminal cases processed 2011–2014. In Panel (a), the outcome is a yearly indicator for whether any child of the incarcerated defendants aged 8 or above at the time of case decision was provided foster care by the Child Protection Services (CPS), and in Panel (b), the outcome is a yearly indicator for any in-home assistance by the CPS. The estimation includes controls for case and year FEs. Standard errors clustered at the case level. 95% confidence intervals. The graph plots the coefficients from the distance dummies.

Another channel that could explain the stronger impacts on the previously partnered defendants and their previous partners is the "removal of a bad influence". Focusing on the previously partnered defendants in Appendix Table A10, Panel B-C, Columns (4)-(5), we find stronger reductions in both the probability and the number of mental health visits for defendants that split up from their previous partners by five years post decision. Similarly, in Columns (7)-(8), we consider heterogeneity by future partnership status for the defendant's previous partner, again finding much stronger reductions among partners who split up from the defendant. Interestingly, in Panel B-C, Columns (10)-(11), we also find similar evidence of stronger impacts among children of previously partnered defendants who later split up from their partner. While noting that we cannot interpret these results causally, as the decision to stay on with the incarcerated defendant post prison is potentially endogenous, we view these findings as suggestive of the "removal" mechanism.

To shed further light on the broader impacts on family and child well-being, we now utilize additional data on yearly child protection services (CPS) incident reports. Specifically, we measure two types of CPS-related events at the annual level for families with incarcerated defendants. First, we observe all events of foster care provided to a child at high risk of neglect or abuse. We consider whether at least one child in the defendant's family was taken out of the family residence by the CPS and provided with alternative shelter following a transfer of custodial rights. Second, we observe events of in-home assistance provided by CPS caseworkers to families that experience social problems and where a child faces some risk of neglect or abuse. As earlier, we exploit the timing of defendants' incarceration in an event study to study how the prevalence of foster care and in-home CPS assistance change following the incarceration event. The results are illustrated in Figure 5. Consistent with our previous findings on improvements in family mental health, we reveal significant reductions in the likelihood of in-home assistance post-event in Figure 5b. Similarly, we

also note a decline in the likelihood of foster care, although the effect estimates are imprecise.[55]

Taken together, the evidence that incarcerated defendants' family members experience fewer mental health problems and that there are far fewer child protection-related incidents in these families suggests that incarceration can have important spillovers on family health and well-being.

6 Conclusion

In this paper, we examined the impacts of incarceration on inmates' visits to health-care professionals for mental health reasons. Our causal evidence suggested that the positive association often found between incarceration and the prevalence of mental health disorders can be misleading. Using an event study design that exploited the variation in the timing of incarceration decisions while controlling for time and case fixed-effects, we showed that incarcerated defendants experience meaningful reductions in mental health visits post event. We showed that these findings are robust to a battery of specification checks and also supported by evidence from a random judge IV design.

In interpreting our findings it is important to keep in mind the rehabilitative focus of the Norwegian correctional system. A prison sentence in Norway seldom entails solitary confinement. Instead this system enshrines the principle that most prisoners shall serve some of their time in open prisons with relatively mild confinement conditions and have access to rehabilitation programs that are aimed at building their educational skills, increasing chances of employability in regular jobs post release, and improving mental health and de-addiction. While we provide some suggestive evidence on how access to particular prison conditions or programs may explain the impacts we found on inmates' mental health, ultimately our evidence should be understood as capturing the compound impacts of prison within the context of a rehabilitation focused correctional system.

Our study also contributes to the existing literature by providing the first causal evidence on mental health-related outcomes of inmates' family members. We found evidence of positive spillovers on the mental health of spouses and children. The decrease in child protection-related incidents for inmates' children further supports the argument that the well-being of the family is positively impacted. Overall, our findings suggest that time spent in prison with a focus on rehabilitation can have large positive effects that go beyond any direct effects on the inmates.

[55] Given the heterogeneous impacts by partnership status described above, we also considered whether the collateral effects on children through CPS involvement varied by their incarcerated parent's future partnership status. In Appendix Figure A7, we split our sample depending on whether the incarcerated parent stayed together with the same partner or not by year 5 post event. This evidence shows that the decline in the prevalence of CPS-related incidents is slightly bigger for children whose incarcerated parent had split up with the partner, although the confidence intervals are wide. Simply put, CPS involvement is reduced for children living both in stable and unstable families post prison.

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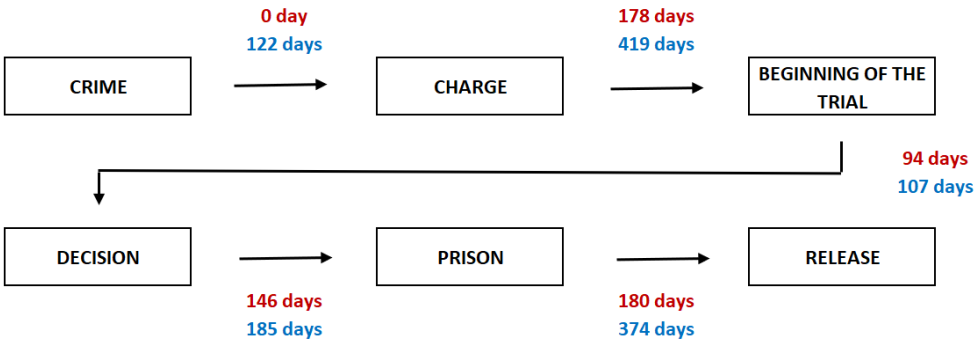
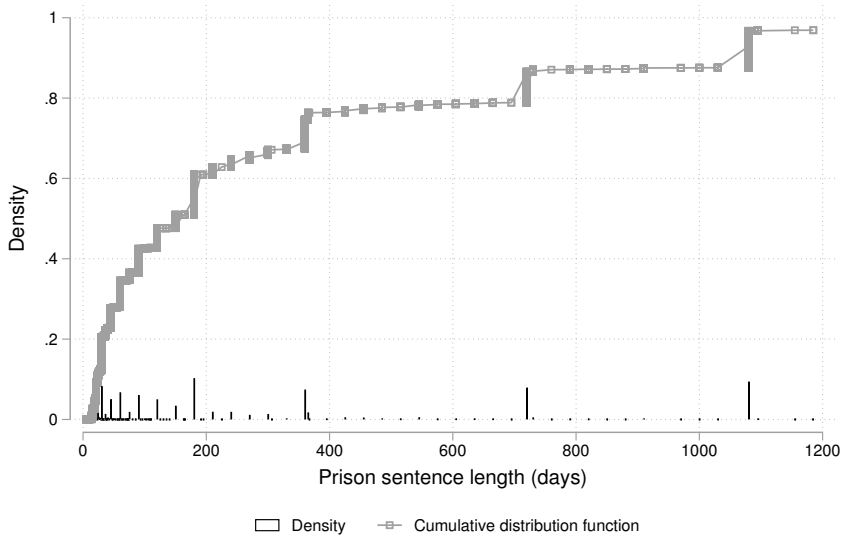
Appendix A. Additional Figures and Tables

Figure A1: Timeline from Crime to Prison Release.



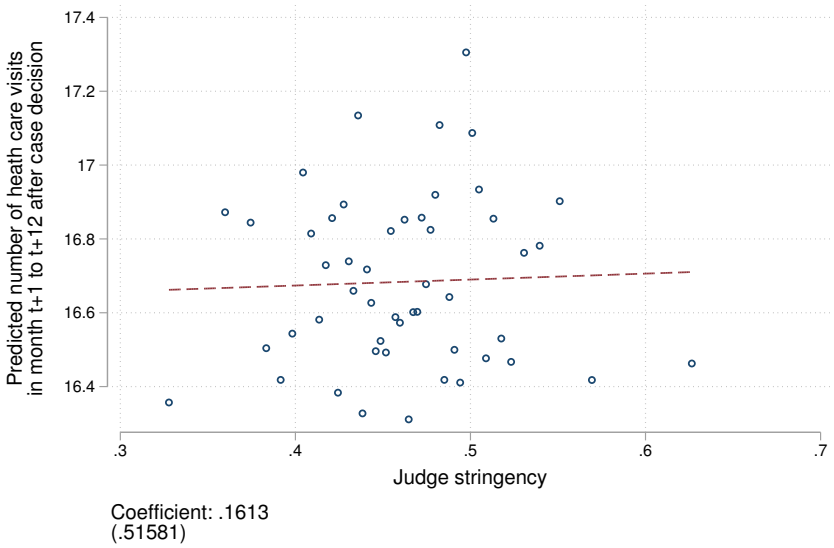
Notes: This figure plots the median (top figure in red) and average (bottom figure in blue) time between each step of the timeline for the sample of nonconfession criminal cases sentenced to prison and processed in 2011–2014.

Figure A2: Cumulative Distribution and Density of Prison Spell Length.



Notes: The sample of nonconfession criminal cases sentenced to prison and processed in 2005–2014. The graph plots the density and cumulative distribution function of prison sentence length.

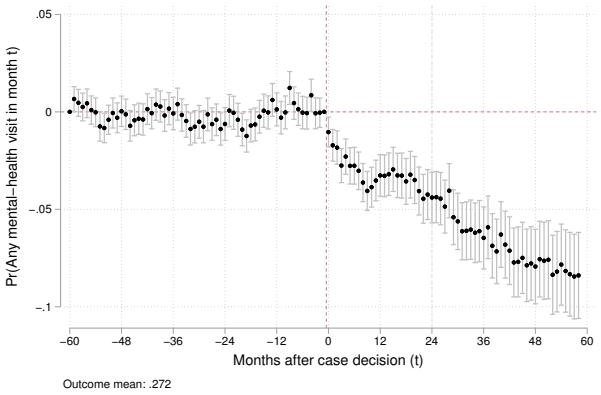
Figure A3: Correlation between Predicted Health-Care Visits and Judge Stringency.



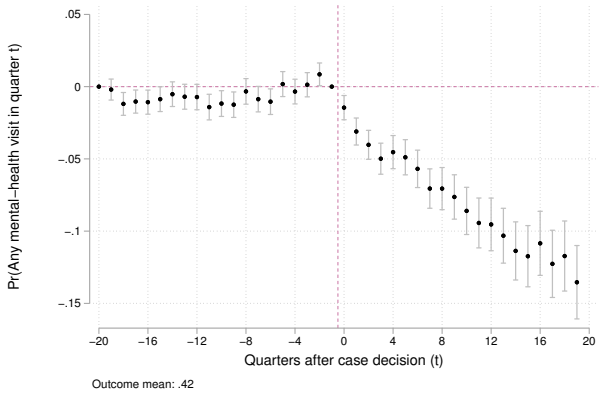
Notes: Sample of all nonconfession criminal cases decided in 2006–2014. The number of health-care visits has been predicted using the same set of sociodemographic and past and current crime variables as those in Table A13.

Figure A4: Robustness: Alternative Frequencies for Measurement of Mental Health Visits.

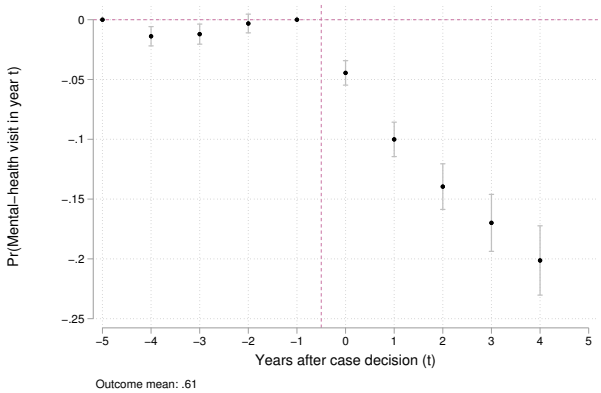
(a) Monthly Frequency



(b) Quarterly Frequency



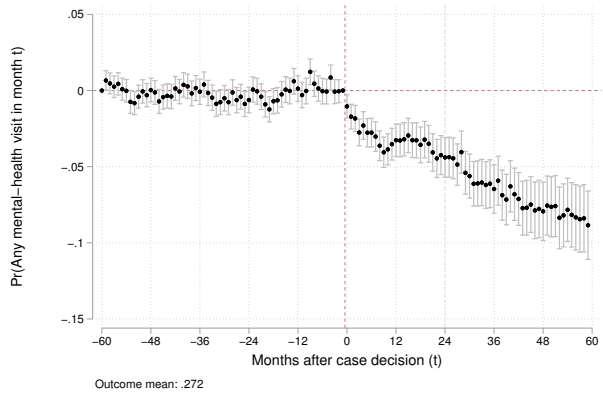
(c) Yearly Frequency



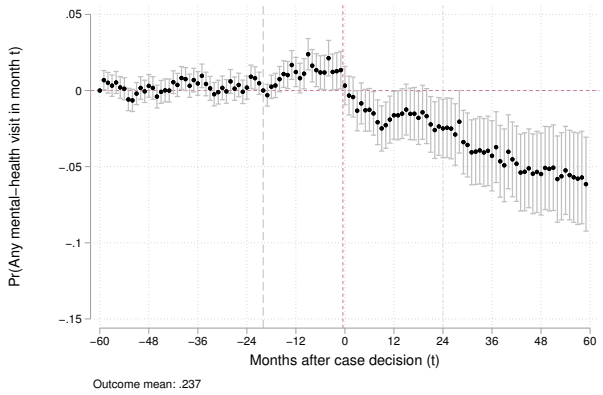
Notes: The Figures (a) and (b) are built on a sample of nonconfession criminal cases sentenced to prison and processed in 2011–2014. Figure (c) is built on a sample of nonconfession criminal cases sentenced to prison and processed in 2011–2014 to observe five years before and after decision. The estimation includes controls for case and month × year FEs for Figure (a), case and quarter × year FEs for Figure (b), and case and year FEs for Figure (c). Standard errors clustered at the case level. 95% confidence intervals. The graph plots the coefficients from the distance dummies.

Figure A5: Robustness: Alternative Event and Sample Definitions.

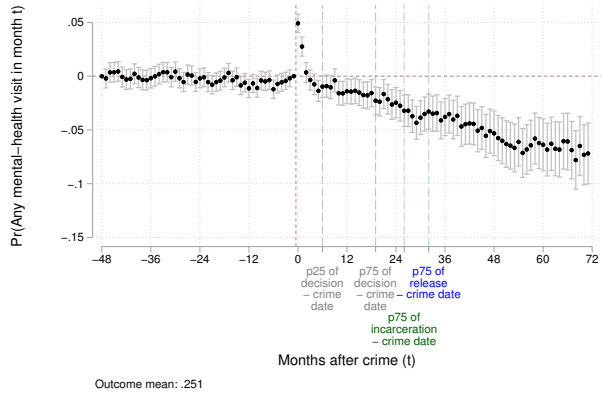
(a) Baseline



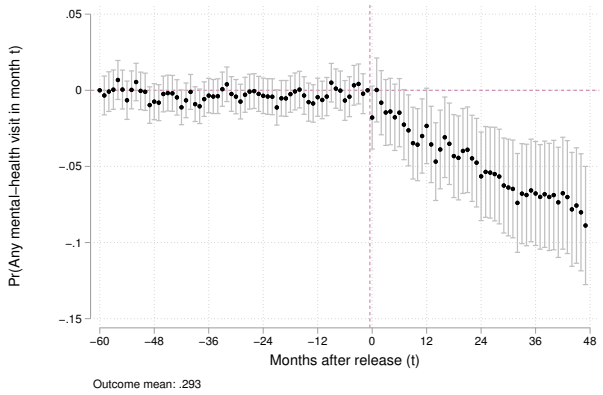
(b) Alternative Reference Point



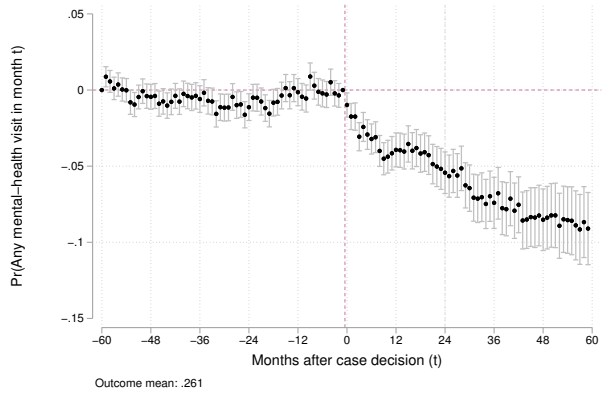
(c) Crime Occurrence as the Event



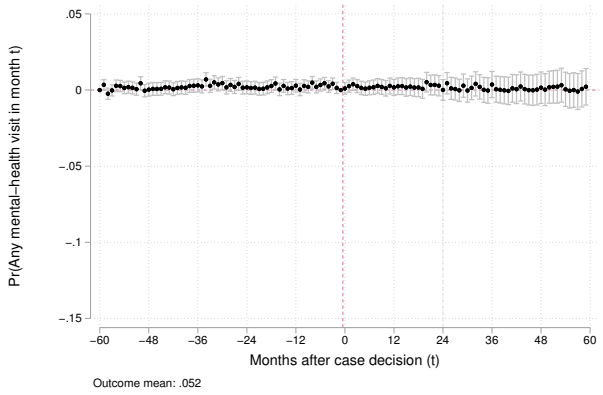
(d) Prison Release as the Event



(e) Restrict Sample to First Observed Offences



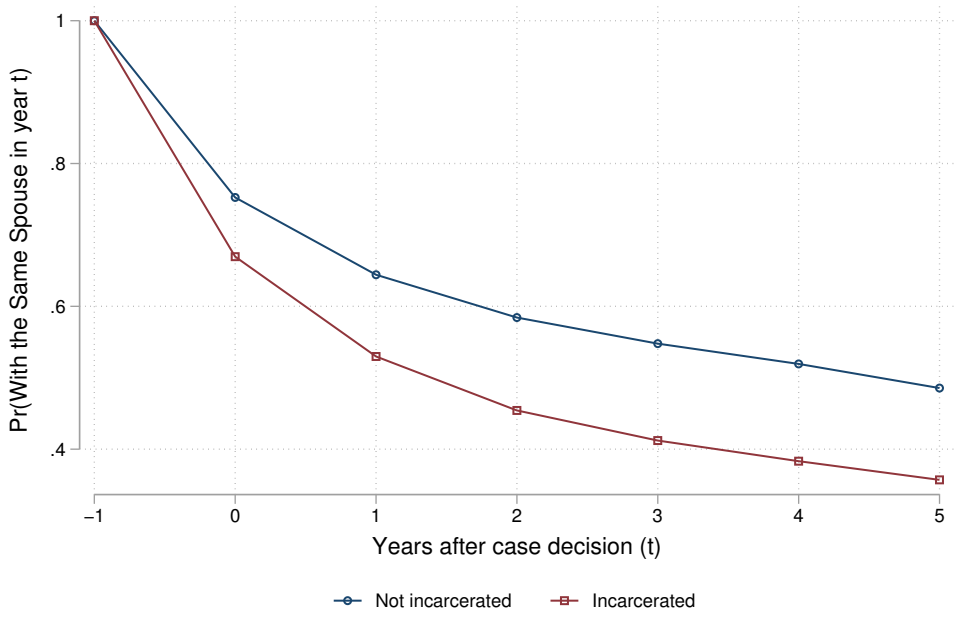
(f) Placebo Event: Matched General Population Controls



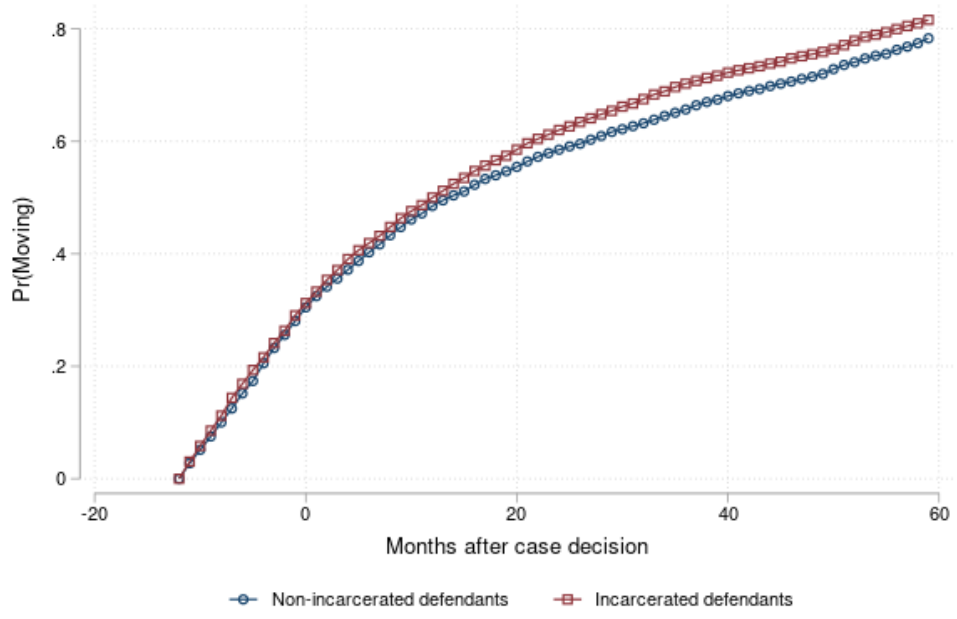
Notes: Panels (a)-(e) are based on the sample of nonconfession criminal cases sentenced to prison and processed in 2011–2014. Panel (a) shows our baseline event study around the month of case decision. In Panel (b), we alternate the references points to months 60 and 20 before case decision and mark the 20 month pre-event period with vertical dotted line. In Panel (c), we center the event study around the month of crime occurrence and mark as vertical dotted lines the 75th percentiles in the distributions of the time to case decision, the time to prison entry (incarceration), and the time to prison release. Similarly, we mark the 25th percentile in the distribution of time to case decision. In Panel (d), we center the event study around the month of prison exit (release), and drop the months between the crime and the release. In Panel (e), we restrict the sample to first observed offences within the 2011-2014 period. In Panel (f), we use a sample of matched controls from the general population that were matched one-to-one (without replacement) to incarcerated defendants with cases decided between 2011 and 2014. The sample of controls are individuals from the general population who were never involved in any criminal activity as measured by police arrests. The matching is based on the following variables: time-invariant variables (female, foreign-born status, month of birth) and a wide range of time-varying variables matched on years 1, 2, 3, 4, and 5, respectively, before case decision (years of education, marital status, number of children, employment status, number of hours worked, if parents had a charge or prison spell). The estimation includes controls for case and month × year FEs. Standard errors clustered at the case level. 95% confidence intervals. The graphs plot the coefficients from the distance dummies.

Figure A6: Marital/Partnership Dissolution and Residential Mobility By Incarceration Status.

(a) Fraction Staying With the Same Partner

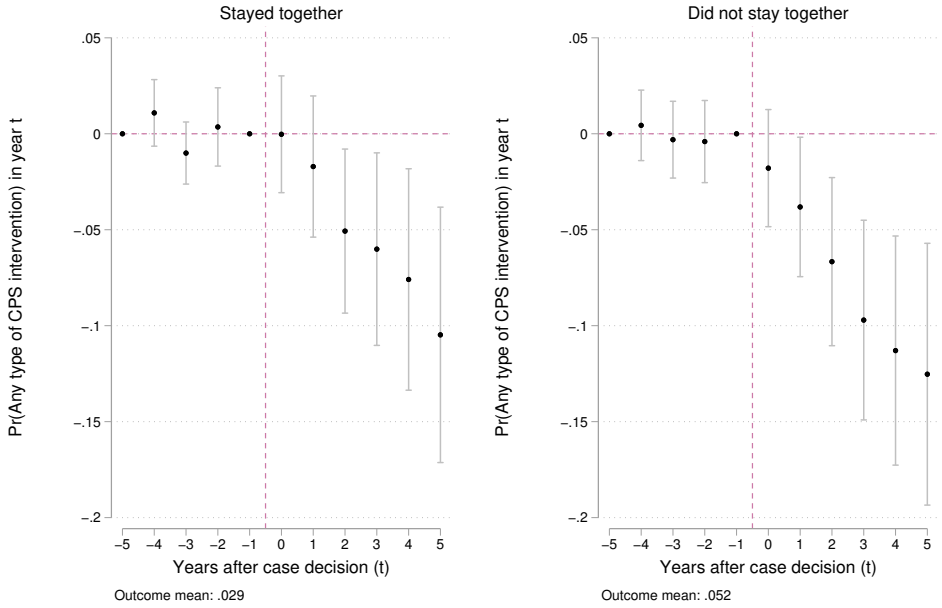


(b) Fraction Moving Residential Address



Notes: The sample of nonconfession criminal cases sentenced to prison and processed in 2011–2014 with a partner the year before case decision. Figure (a) plots the fraction of defendants that keep staying with the same partner as in the year before case decision by incarceration status. Figure (b) plots the fraction of defendants that moved from the home address that they had in month 12 before case decision by incarceration status.

Figure A7: Heterogeneous Effects on Incidents of CPS Intervention by Future Partnership Status.



Notes: The sample of incarcerated defendants in nonconfession criminal cases processed in 2011–2014 who have a partner year 1 before case decision. The estimation includes controls for case and month × year FEs. Standard errors clustered at the case level. 95% confidence intervals. The graph separately plots the coefficients from the distance dummies for children whose incarcerated parent continued staying with the same partner at 5 years after the case decision (LHS) and for children whose incarcerated parent did not stay with the same partner at 5 years after the case decision (RHS). The outcome is an annual indicator for any type of child protection services (CPS) intervention, involving either foster care or assistance at home, provided to any child aged 8 or above at the time of case decision.

Table A1: Top 15 Most Common Mental Health Diagnoses.

	Number	Frequency	Cumulative Frequency
1. Drug abuse	46212	22.25	22.25
2. Depressive disorder	15429	7.43	29.67
3. Anxiety disorder	10667	5.13	34.81
4. Hyperkinetic disorder	8496	4.09	38.9
5. Mental and behavioral disorders due to use of opioids : dependence syndrome	13428	6.46	45.36
6. Medication abuse	5992	2.88	48.25
7. Chronic alcohol abuse	5846	2.81	51.06
8. Acute stress reaction	5577	2.68	53.74
9. Sleep disturbance	5568	2.68	56.42
10. Mental and behavioral disorders due to use of opioids : unspecified mental and behavioral disorder	4165	2	58.43
11. Feeling anxious/nervous/tense	4056	1.95	60.38
12. Psychological disorders, other	4038	1.94	62.33
13. Psychological symptom/complt other	3849	1.85	64.18
14. Disturbance of activity and attention	3472	1.67	65.85
15. Mental and behavioral disorders due to use of alcohol : dependence syndrome	3157	1.52	67.37
Number of Observations		207,739	

Notes: This table reports the most common diagnoses defined as mental health-related for 2010 in the sample of nonconfession criminal cases processed in 2005–2014 with at least one mental health diagnosis in 2010.

Table A2: Top 15 Most Common Addiction Diagnoses.

	Number	Frequency	Cumulative Frequency
1. Drug abuse	46212	45.37	45.37
2. Mental and behavioural disorders due to use of opioids : dependence syndrome	13428	13.18	58.55
3. Medication abuse	5992	5.88	64.43
4. Chronic alcohol abuse	5846	5.74	70.17
5. Mental and behavioral disorders due to multiple drug use and use of other psychoactive substances : dependence syndrome	4786	4.7	74.87
6. Mental and behavioral disorders due to use of alcohol : dependence syndrome	3157	3.1	77.97
7. Mental and behavioural disorders due to use of cannabinoids : dependence syndrome	3019	2.96	80.93
8. Mental and behavioral disorders due to multiple drug use and use of other psychoactive substances : harmful use	2790	2.74	83.67
9. Mental and behavioral disorders due to use of alcohol : harmful use	2007	1.97	85.64
10. Mental and behavioral disorders due to use of other stimulants, including caffeine : dependence syndrome	1716	1.68	87.32
11. Acute alcohol abuse	1071	1.05	88.37
12. Mental and behavioral disorders due to use of cannabinoids : harmful use	874	0.86	89.23
13. Mental and behavioral disorders due to use of other stimulants, including caffeine : harmful use	628	0.62	89.85
14. Mental and behavioral disorders due to multiple drug use and use of other psychoactive substances : unspecified mental and behavioral disorder	589	0.58	90.43
15. Mental and behavioral disorders due to use of alcohol : dependence syndrome	567	0.56	90.98
Number of Observations		101,867	

Notes: This table reports the most common diagnoses defined as addiction related for 2010 in the sample of nonconfession criminal cases processed in 2005–2014 with at least one addiction diagnosis in 2010.

Table A3: Top 15 Most Common Depression or Mood Disorder-Related Diagnoses.

	Number	Frequency	Cumulative Frequency
1. Depressive disorder	15429	14.57	14.57
2. Anxiety disorder/anxiety state	10667	10.08	24.65
3. Hyperkinetic disorder	8496	8.02	32.67
4. Acute stress reaction	5577	5.27	37.94
5. Sleep disturbance	5568	5.26	43.2
6. Feeling anxious/nervous/tense	4056	3.83	47.03
7. Psychological disorders, other	4038	3.81	50.85
8. Psychological symptom/complt other	3849	3.64	54.48
9. Disturbance of activity and attention	3472	3.28	57.76
10. Affective psychosis	2823	2.67	60.43
11. Schizophrenia	2237	2.11	62.54
12. Feeling depressed	2121	2	64.54
13. Personality disorder	2022	1.91	66.45
14. Mental disorder, not otherwise specified	1921	1.81	68.27
15. Phobia/compulsive disorder	1869	1.77	70.03
Number of Observations		105,872	

Notes: This table reports the most common diagnoses defined as depression or mood disorder-related for 2010 in the sample of nonconfession criminal cases processed in 2005–2014 with at least one depression or mood disorder diagnosis in 2010.

Table A4: Top 15 Most Common Physical Health Diagnoses.

	Number	Frequency	Cumulative Frequency
1. General disease NOS	27392	9.36	9.36
2. Back symptom/complaint	7349	2.51	11.87
3. Limited function/disability NOS	6661	2.28	14.15
4. Low back symptom/complaint	5461	1.87	16.02
5. Back syndrome without radiating pain	5302	1.81	17.83
6. Back syndrome with radiating pain	5060	1.73	19.56
7. Neck symptom/complain	4037	1.38	20.94
8. Shoulder symptom/complaint	3422	1.17	22.11
9. General symptom/complaint other	3213	1.1	23.21
10. Shoulder syndrome	3164	1.08	24.29
11. Upper respiratory infection acute	3142	1.07	25.36
12. Knee symptom/complaint	3074	1.05	26.41
13. Abdominal pain/cramps general	2715	0.93	27.34
14. Muscle pain	2557	0.87	28.21
15. Neck syndrome	2470	0.84	29.05
Number of Observations		292,659	

Notes: This table reports the most common diagnoses defined as physical health-related for 2010 in the sample of nonconfession criminal cases processed in 2005–2014 with at least one physical health diagnosis in 2010.

Table A5: Comparisons of the General Population and the Sample of Defendants.

	(1)	(2)	(3)	(4)	(5)
		Raw Numbers		Standardized Differences	
	Matched General Population Controls	Incarcerated Defendants	Non-incarcerated Defendants	(2)–(3)	(2)–(1)
Pr(Any Health-Care Visit)	0.761	0.879	0.882	-0.012	0.283***
				(0.007)	(0.008)
Pr(Any Physical-Health Visit)	0.723	0.823	0.830	-0.004	0.240***
				(0.009)	(0.009)
Pr(Any Mental-Health Visit)	0.137	0.484	0.459	-0.115***	1.021***
				(0.015)	(0.012)
No. of Health-Care Visits	7.280	14.741	14.907	-0.012	0.283***
				(0.007)	(0.008)
No. of Physical-Health Visits	5.542	7.541	7.944	0.007	0.223***
				(0.012)	(0.010)
No. of Mental-Health Visits	1.054	5.790	5.596	-0.105***	1.061***
				(0.029)	(0.020)
Number of Observations	23,511	16,957	19,060	36,017	40,468

Notes: This table reports summary statistics in 2010 for the matched general population and the sample of nonconfession criminal cases processed in 2005-2014. Controls from the general population are matched 1-to-1 (without replacement) to incarcerated defendants. The sample of controls are individuals from the general population who were never involved in any criminal activity as measured by police arrests. The matching is based on the following variables: time-invariant variables (female, foreignborn, month of birth) and time-varying variables matched on years 1, 2, 3, 4, and 5 before case decision (years of education, marital status, number of children, employment status, number of hours worked, if parents had a charge or prison spell).

Table A6: Summary Statistics on the Sample of Defendants.

	Mean	SD	p10	p50	p90
Age at the time of case decision	34.11	11.89	20.36	32.01	50.60
Female	0.119	0.32			
Foreign-born	0.15	0.36			
Married in year t-1	0.10	0.31			
Number of children in year t-1	0.81	1.26			3.00
High school by year t-1	0.18	0.38			
Some college education in year t-1	0.05	0.22			
Type of crime: violent crime	0.28	0.45			
Type of crime: property crime	0.11	0.32			
Type of crime: economic crime	0.09	0.30			
Type of crime: drug-related crime	0.15	0.36			
Type of crime: drunk driving	0.08	0.27			
Type of crime: traffic violation	0.05	0.23			
Missing data on demographics	0.03	0.17			
Court decision: incarceration	0.56	0.50			
Days between crime and case decision	710.90	1047.58	133	357	1643
Days between case decision and prison entry	244.92	280.27	0	165	596
Days of prison sentence if incarcerated	374.18	462.75	28	180	1080
Employed in year t-1	0.32	0.47			
Ever employed in years t-2 to t-5	0.46	0.50			
Ever charged in year t-1	0.47	0.50			
Ever charged in years t-2 to t-5	0.65	0.48			
Ever incarcerated in year t-1	0.13	0.34			
Ever incarcerated in years t-2 to t-5	0.29	0.46			
Ever health-care visit in months t-1 to t-12	0.91	0.29	1	1	1
No. of health-care visits in months t-1 to t-12	16.81	22.69	1	9	42
Ever physical health visit in months t-1 to t-12	0.85	0.36	0	1	1
No. of physical health visits in months t-1 to t-12	7.91	13.21	0	4	19
Ever mental health visit in months t-1 to t-12	0.55	0.50	0	1	1
No. of mental health visits in months t-1 to t-12	7.26	14.38	0	1	22
Number of Observations		21,928			

Notes: The sample of nonconfession criminal cases processed in 2011–2014 with nonmissing demographics, type of crime, past work, crime, and health history variables.

Table A7: Summary Statistics for the Sample of Incarcerated Defendants.

	Mean	SD	p10	p50	p90
Age at the time of case decision	34.57	11.29	21.31	32.53	50.24
Female	0.08	0.27			
Foreign-born	0.15	0.36			
Married in year t-1	0.10	0.30			
Number of children in year t-1	0.80	1.25	0	0	3
High school by year t-1	0.17	0.38			
Some college education in year t-1	0.05	0.21			
Type of crime: violent crime	0.32	0.47			
Type of crime: property crime	0.12	0.32			
Type of crime: economic crime	0.08	0.27			
Type of crime: drug-related crime	0.14	0.34			
Type of crime: drunk driving	0.08	0.28			
Type of crime: traffic violation	0.05	0.21			
Missing data on demographics	0.04	0.19			
Days between crime and case decision	723.69	1132.85	123	328	1814
Days between case decision and prison entry	185.41	203.27	0	146	410
Days of prison sentence if incarcerated	374.18	462.75	28	180	1080
Employed in year t-1	0.31	0.46			
Ever employed in years t-2 to t-5	0.46	0.50			
Ever charged in year t-1	0.53	0.50			
Ever charged in years t-2 to t-5	0.71	0.46			
Ever incarcerated in year t-1	0.18	0.39			
Ever incarcerated in years t-2 to t-5	0.38	0.49			
Ever health-care visit in months t-1 to t-12	0.91	0.29	1	1	1
No. of health-care visits in months t-1 to t-12	17.00	22.94	0	9	43
Ever physical health visit in months t-1 to t-12	0.85	0.36	0	1	1
No. of physical health visits in months t-1 to t-12	7.81	13.45	0	4	18
Ever mental health visit in months t-1 to t-12	0.57	0.50	0	1	1
No. of mental health visits in months t-1 to t-12	7.44	14.33	0	1	22
Observations			12,007		

Notes: The sample of nonconfession criminal cases processed in 2011–2014 with nonmissing demographics, type of crime, past work, crime and health history variables that were sentenced to prison.

Table A8: Health Variable Distribution in the Sample of Defendants.

	Mean	SD	Percentile 10	Percentile 50	Percentile 90	Mean Difference: Non-incarcerated – Incarcerated Defendants
Ever health-care visit in month t	0.43	0.495				-0.010
						(0.002)
Number of health-care visits in month t	1.229	2.272	0	0	4	-0.057
						(0.010)
Ever physical-health visit in month t	0.286	0.452				0.007
						(0.002)
Number of physical-health visits in month t	0.582	1.391	0	0	2	0.012
						(0.006)
Ever mental health visit in month t	0.208	0.406				-0.020
						(0.002)
Number of mental health visits in month t	0.508	1.435	0	0	2	-0.050
						(0.006)
Ever visit for substance abuse in month t	0.091	0.287				-0.023
						(0.001)
Number of visits for substance abuse in month t	0.235	1.09	0	0	0	-0.054
						(0.005)
Ever visit for drug abuse in month t	0.075	0.264				-0.020
						(0.001)
Number of visits for drug abuse in month t	0.2	1.026	0	0	0	-0.049
						(0.004)
Ever visit for alcohol abuse in month t	0.013	0.115				-0.003
						(0.000)
Number of visits for alcohol abuse in month t	0.026	0.302	0	0	0	-0.002
						(0.001)
Ever visit for opioid abuse in month t	0.017	0.13				-0.004
						(0.001)
Number of visits for opioid abuse in month t	0.036	0.356	0	0	0	-0.007
						(0.001)
Ever visit for severe mood disorder in month t	0.036	0.186				0.001
						(0.001)
Number of visits for severe mood disorder in month t	0.067	0.462	0	0	0	-0.002
						(0.002)
Ever visit for light mood disorder in month t	0.051	0.22				0.001
						(0.001)
Number of visits for light mood disorder in month t	0.09	0.488	0	0	0	0.007
						(0.002)

Notes: This table reports summary statistics for the sample of non confession criminal cases processed in 2006–2014 measured in 36–30 months before the case decision.

Table A9: Heterogeneity Effects by Employment History, Type of Crime and Sentence Length.

	Employment History		Type of Crime		Sentence Length	
	Employed	**Non-Employed**	**Violent**	**Non-Violent**	**< Median**	**> Median**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
A. Probability of Mental Health Visit						
Months 13–60	-0.056***	-0.038***	-0.057***	-0.042***	-0.033***	-0.058***
	(0.008)	(0.008)	(0.009)	(0.007)	(0.008)	(0.008)
Dependent Mean	.204	.274	.179	.270	.224	.263
B. Number of Mental Health Visits						
Months 13–60	-0.067**	-0.079**	-0.127***	-0.048	-0.024	-0.114***
	(0.032)	(0.031)	(0.030)	(0.030)	(0.035)	(0.031)
Dependent Mean	.478	.695	.411	.677	0.541	0.657
Number of Observations	5,884	6,430	3,966	8,348	5,248	5,455

Notes: The sample of nonconfession criminal cases processed in 2011–2014. The estimation has been run separately for each subgroup, and always includes controls for case and period (month × year) FEs. Standard errors clustered at the case level. Subgroups according to employment history have been defined as in Bhuller et al. (2020). Defendants are classified as previously employed if they were working in at least one of the past five years; the other individuals are defined as previously non-employed. 48% of our sample is defined as previously employed. Sentence length is split at the median, which is 6 months. 28% of our sample has committed a violent crime. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Table A10: Heterogeneous Effects by Past and Future Partnership Status.

A. Future Partnership/Family Status at Month 60 After Decision for Defendants and their Past Family Members

Sample:	Incarcerated Defendants	Partner or Spouse	Children
Outcome:	*Had a Partner or Spouse*	*Had a Partner or Spouse*	*Lived in Stable Household*
	(1)	(2)	(3)
Point Estimate	-0.017***	-0.494***	-0.023**
	(0.005)	(0.015)	(0.011)
Dependent Mean	.191	.701	.380
No. of Observations	12,314	2,346	8,191

B. Probability of Mental Health Visit in Months 13–60 After Decision By Past/Future Partnership Status

Sample:	Incarcerated Defendants					Partner or Spouse			Children		
		By Past Partnership Status:		*By Future Partnership Status:*			*By Future Partnership Status:*			*By Future Family Status:*	
	All	Single	Partnered	Stayed	Split	All	Stayed	Split	All	Stayed	Split
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Point Estimate	-0.047***	-0.036***	-0.089***	-0.069***	-0.100***	-0.056***	-0.021	-0.093***	-0.009**	-0.012	-0.028***
	(0.006)	(0.006)	(0.012)	(0.012)	(0.015)	(0.011)	(0.015)	(0.015)	(0.004)	(0.008)	(0.010)
Dependent Mean	.241	.248	.208	.133	.247	.171	.117	.213	.074	.044	.070
No. of Observations	12,314	9,915	2,399	824	1,575	2,346	775	1,571	8,191	1,188	1,727

C. Number of Mental Health Visits in Months 13–60 After Decision By Past/Future Partnership Status

Sample:	Incarcerated Defendants					Partner or Spouse			Children		
		By Past Partnership Status:		*By Future Partnership Status:*			*By Future Partnership Status:*			*By Future Family Status:*	
	All	Single	Partnered	Stayed	Split	All	Stayed	Split	All	Stayed	Split
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Point Estimate	-0.074***	-0.047*	-0.194***	-0.085	-0.259***	-0.138***	-0.037	-0.237***	-0.018	-0.028	-0.055**
	(0.023)	(0.027)	(0.004)	(0.052)	(0.051)	(0.034)	(0.038)	(0.048)	(0.012)	(0.018)	(0.025)
Dependent Mean	.596	.619	.476	.269	.583	.361	.233	.451	.150	.084	.146
No. of Observations	12,314	9,915	2,399	824	1,575	2,346	775	1,571	8,191	1,188	1,727

Notes: In all three panels, the sample of incarcerated defendants is the sample of nonconfession criminal cases processed in 2011–2014. Partners and Spouses are defined as the cohabiting partners or spouses of our sample of incarcerated defendants as measured the year before case decision. Children are the children of our sample of incarcerated defendants who are aged 8 or more at the time of case decision. Panel A, column 1, looks at change in the probability that the defendant has any partner. Panel A, column 2, looks at the change in the probability that the partners of our sample of incarcerated defendants as measured the year before case decision have any partner. Panel A, column 3, looks at the change in the probability that the children of the defendants live in a stable household, i.e. the probability that the incarcerated parent has any partner. Panels B and C show the mental health outcomes when the sample is split by past and future partnership status. Past partnership refers to whether the incarcerated defendant has a cohabiting partner or spouse the year before case decision. 81% of our sample of incarcerated defendants is single the year before case decision. Future partnership refers to whether the incarcerated defendants and their partners as defined in the year before case decision are still together five years after case decision. Among partnered inmates, 34% are still with the same partner five years later. The estimation has been run separately for each subgroup, and always includes controls for case and period (month × year) FEs. Standard errors clustered at the case level. The dependent mean is the mean of the outcome in the sample included in the regression. *p<0.1, **p<0.05, ***p<0.01.

Table A11: Robustness: IV Estimates of the Effects of Incarceration on Mental Health Visits.

	Baseline IV	**Number of Cases Handled by Judge**		**Definition of Judge Stringency IV**	
Definition of Judge Stringency IV:	**Random**	**Random**	**Random**	**Non-Confession**	**Reverse Sample**
No. of Cases Handled By Judge:	**$\geq$ 50 cases**	**$\geq$ 75 cases**	**$\geq$ 100 cases**	**$\geq$ 50 cases**	**$\geq$ 50 cases**
	(1)	(2)	(3)	(4)	(5)
A. First-Stage Estimates					
Incarcerated	0.355***	0.367***	0.376***	0.275***	0.380***
	(0.051)	(0.052)	(0.053)	(0.041)	(0.042)
Dependent Mean	0.52	0.52	0.53	0.52	0.53
B. IV Estimates: Probability of Mental Health Visit					
Months 1–60	-0.115	-0.085	-0.047	-0.080	-0.120*
	(0.083)	(0.081)	(0.076)	(0.077)	(0.062)
Dependent Mean	0.25	0.25	0.26	0.25	0.25
C. IV Estimates: Number of Mental Health Visits					
Months 1–60	-0.726***	-0.577*	-0.379	-0.580*	-0.625**
	(0.341)	(0.327)	(0.304)	(0.324)	(0.259)
Dependent Mean	0.65	0.65	0.65	0.65	0.65
Number of Observations	35,082	34,202	32,746	35,082	35,082
Controls:					
Demographics	✓	✓	✓	✓	✓
Type of Crime	✓	✓	✓	✓	✓
Past Work & Crime History	✓	✓	✓	✓	✓
Court $\times$Year FE	✓	✓	✓	✓	✓

Notes: Sample of non-confession criminal cases processed 2009-2014. All estimations include court $\times$case entry year FEs and demographics (age, sex, foreign-born status, number of children, marital status, level of education), type of crime, past work and crime history (indicator for being employed in year t-1 to t-5 before the year of crime, indicator for being ever charged in year t-1 to t-5 before the year of crime, indicator for being ever incarcerated in year t-1 to t-5 before the year of case decision) controls. Standard errors are two-way clustered at the judge and defendant level in the IV estimation. 95% confidence intervals. The first column reproduces the baseline IV estimation presented in Table 1. Columns (2) and (3) vary the definition of the sample and exclude, respectively, judges who handled less than 75 and 100 randomly assigned confession or non-confession cases between the years 2005 and 2014. Column (4) excludes confession cases for the computation of the judge stringency score. *p<0.1, **p<0.05, ***p<0.01.

Table A12: First-Stage Estimates: The Effect of Judge Stringency on Incarceration Probability.

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation sample:	**Time of decision**	**Month 12 after decision**	**Month 24 after decision**	**Month 36 after decision**	**Month 48 after decision**	**Month 60 after decision**
Dependent variable:	**Pr(Incarcerated)**					
A. Court ×Year of Court Case Registration Interacted Fixed Effects						
Judge Stringency	0.3812***	0.3786***	0.3760***	0.3711***	0.3689***	0.3662***
	(0.0546)	(0.0550)	(0.0552)	(0.0554)	(0.0552)	(0.0558)
F-stat (Instrument)	48.65	47.35	46.39	44.82	44.58	43.04
B. Add Controls for Demographics and Type of Crime						
Judge Stringency	0.3683***	0.3649***	0.3623***	0.3591***	0.3577***	0.3566***
	(0.0536)	(0.0540)	(0.0542)	(0.0545)	(0.0542)	(0.0547)
F-stat (Instrument)	47.17	45.74	44.64	43.39	43.63	42.53
C. Add Controls for Demographics, Type of Crime, Past Work and Criminal History						
Judge Stringency	0.3606***	0.3583***	0.3589***	0.3569***	0.3569***	0.3548***
	(0.0493)	(0.0497)	(0.0499)	(0.0502)	(0.0502)	(0.0508)
F-stat (Instrument)	53.45	51.88	51.64	50.65	50.46	48.82
Dependent mean	0.5301	0.5292	0.5278	0.5261	0.5251	0.5239
Number of cases	59556	59059	58118	57193	56341	55459

Notes: The sample of non-confession criminal cases processed 2006-2014. The estimation includes controls for case × case decision year FEs. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for education is "Less than high school, year t-1" and the omitted category for type of crime is "Other crimes". Standard errors are two-way clustered at judge and defendant level. *p<0.1, **p<0.05, ***p<0.01.

Table A13: Tests of Randomization.

	Incarcerated		**Judge Stringency**	
	(1)		(2)	
Age at the time of case decision	0.0035***	(0.0003)	0.0000	(0.0000)
Female	-0.0589***	(0.0054)	-0.0014***	(0.0005)
Foreign-born	0.0054	(0.0044)	0.0003	(0.0004)
Married, year t-1	-0.0204**	(0.0089)	-0.0012	(0.0009)
Number of children, year t-1	-0.0016	(0.0023)	0.0004	(0.0002)
High school degree, year t-1	-0.0013	(0.0062)	0.0013**	(0.0007)
Some college, year t-1	-0.0440***	(0.0093)	-0.0007	(0.0012)
Violent crime	0.0945***	(0.0066)	-0.0005	(0.0008)
Property crime	-0.0431***	(0.0088)	-0.0003	(0.0009)
Economic crime	-0.0683***	(0.0091)	0.0007	(0.0010)
Drug-related crime	-0.0649***	(0.0079)	-0.0012	(0.0010)
Drunk driving	0.0713***	(0.0095)	-0.0011	(0.0009)
Other Traffic	-0.0574***	(0.0107)	-0.0012	(0.0011)
Missing X s	-0.2961***	(0.0995)	0.0053	(0.0114)
Employed, year t-1	0.0180***	(0.0062)	-0.0006	(0.0007)
Ever employed, years t-2 to t-5	0.0163***	(0.0062)	-0.0011*	(0.0006)
Ever Charged, year t-1	0.0529***	(0.0053)	-0.0004	(0.0006)
Ever Charged, years t-2 to t-5	0.0589***	(0.0061)	0.0001	(0.0007)
Ever incarcerated, year t-1	0.1472***	(0.0078)	-0.0001	(0.0009)
Ever incarcerated, years t-2 to t-5	0.1658***	(0.0069)	0.0009	(0.0007)
Number of health-care visits, month t-1	-0.0074**	(0.0033)	0.0001	(0.0004)
Number of mental-health diagnoses, month t-1	0.0057*	(0.0034)	-0.0002	(0.0004)
Number of physical-health diagnoses, month t-1	0.0043	(0.0036)	0.0001	(0.0004)
Missing health information	0.0947***	(0.0343)	0.0002	(0.0041)
Constant	0.2591***	(0.0113)	0.4619***	(0.0027)
F-statistic for joint test	152.980		1.274	
p-value	(0.000)		(0.173)	
Dependent variable mean	0.5301		0.4617	
Dependent variable sd	0.4991		0.0725	
Number of cases	59,556		59,556	

Notes: The sample of non-confession criminal cases processed 2006-2014. All estimations include controls for court × case decision year FEs. Reported F-statistic refers to a joint test of the null hypothesis for all variables. The omitted category for education is "Less than high school, year t-1" and the omitted category for type of crime is "Other crimes". Standard errors are two-way clustered at judge and defendant level. *p<0.1, **p<0.05, ***p<0.01.

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Appendix B. Details on the Prediction Model

Mental health score is computed by predicting the probability of at least one mental health-related visit in the 3–1 years before the crime. The prediction model is trained on the general population (excluding our sample) in the population register in 2009–2010. We then restrict the model to individuals aged 10 years or older in 2009 and alive by 2010. We retrieve their sociodemographic and health information from 2004 to 2010 and define a dummy variable equal to one if they had at least one mental health visit within the past 3 years. We then use a logit model – given the dependent variable is binary – that includes the following variables: female indicator, the year, deciles of age, indicator for foreign-born, the marital status the year before, the marital status 2 years before, number of children 1 and 2 years before (one indicator per value), employment status 1 year before, deciles of transfers received 1 year before, a set of indicators if a child has died within the last 5 years and was aged between 0–10 years, 11–20 years, etc., fixed effects for the municipality of residence 1 year before, and deciles of wage 1 and 2 years before. The total number of observations is 7,813,589 (3,950,508 individuals) and the pseudo-R^2 is 7.4%.

We use an alternative model where we predict the probability of at least one mental health visit 3 years before the crime (to be as far as possible from the event) using our full sample of nonconfession crimes processed between 2006 and 2014. This alternative model may better predict mental health as it is based on a sample of offenders but at the expense of using the same sample to train and test the model. We again use a logit model, where we include the following variables: a female indicator, dummies for each age value, dummies for each age-at-crime value, indicator for foreign-born, indicator for married the year before the crime, dummies for number of children the year before the crime, dummies for each year of education value 1 year before the crime, number of hours worked and monthly wage 36 months before the case decision, indicator equal to one if ever suspected or charged in the last 3 years before the case decision, number of suspected crimes and charges in the last 3 years before the case decision, indicator equal to one if ever suspected in years t-2 to t-5 before the year of the crime. In that case, the number of observations is equal to 35,363 and the pseudo-R^2 is 12.3%. Table correlates both measures with each other, with the actual probability of having at least one mental health visit in 3 years before the case decision. The correlation lies between 0.3 and 0.53. We also reproduce Figure **??** with the second measure (based on our sample directly) and the results are very similar.