"A Danger to Self and Others" Involuntary Hospitalization's Health and Criminal Consequences*

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Abstract

The involuntary hospitalization of people experiencing a mental health crisis is a widespread practice, as common in the US as incarceration in state and federal prisons and 2.4 times as common as dying of cancer. The intent is to prevent individuals from harming themselves or others through incapacitation, stabilization and medical treatment over a short period of time. Does involuntary hospitalization achieve its goals? We leverage quasi-random assignment of the evaluating physician and administrative data from Allegheny County, Pennsylvania to estimate the causal effects of involuntary hospitalization on harm to self (proxied by death by suicide or overdose) and harm to others (proxied by violent crime charges). For individuals whom some physicians would hospitalize but others would not, we find that hospitalization nearly doubles the probability of being charged with a violent crime and more than doubles the probability of dying by suicide or overdose in the three months after evaluation. Involuntary hospitalization causes increases in homeless shelter usage and decreases employment and earnings, suggesting housing and economic disruption as potential mechanisms. Our results suggest that the system we study is hospitalizing too many individuals.

Keywords: Involuntary hospitalization, psychiatric detention, mental health treatment, violent crime

JEL Classifications: I18, I12, K14

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1 Introduction

In the US, 1.2 million individuals are hospitalized involuntarily every year (Lee and Cohen, 2021). Involuntary hospitalization occurs when an emergency department physician determines that an individual poses a clear and present danger to themselves or others due to mental illness. It is intended to prevent perceived dangers posed to oneself or others from being realized.

Involuntary hospitalizations are common and increasing. The present rates of 357 per 100,000 residents are nearly identical to the 350 individuals per 100,000 residents incarcerated in state and federal prisons every year (Carson, 2022), are 2.4 times greater than the rates of dying of (any type of) cancer (Institute, 2024), and are 57% greater than the rates of homelessness (de Sousa and Henry, 2024).[1] Rates of involuntary hospitalization have grown three times faster than the population since 2011 (Lee and Cohen, 2021) and emergency department visits for acute psychiatric distress have likewise risen (Larkin et al., 2005; Twenge et al., 2019; Goodwin et al., 2020; Gold, 2020; Theriault et al., 2020).

Ex ante, the impact of an involuntary hospitalization is unclear. During episodes of mental health crisis, involuntary hospitalization may create distance from dangerous settings and give a person access to care that can stabilize them. This care might facilitate connections to medications, outpatient treatment, or other continuing services. Incapacitation may also reduce risk to others, since individuals are held in inpatient care under supervision and the majority of violent offenses are impulsive (Brouwers et al., 2010), and likewise reduce risk to oneself, since most suicides are premeditated very briefly (Deisenhammer et al., 2009). In these ways, involuntary hospitalization may be salubrious.

On the other hand, involuntary hospitalization may disrupt beneficial social supports such as therapeutic relationships, housing, employment. It may introduce (new) treatments that are not sustainable or appealing to the patient, or last for a duration that is not sufficient to stabilize an individual's mental illness beyond the hospitalization. Resource constraints, such as under-resourced and porous follow-up services can make for less than ideal experiences. Moreover, if individual finds the involuntary hospitalization unwelcome, it may degrade patients' trust in the healthcare system.

[1] While we benchmark against individuals who are incarcerated or in shelter, those who are involuntarily hospitalized are generally a different population. In our sample, only 4.7% were incarcerated last year and 3% were in shelter. Indeed, those who are evaluated for involuntary hospitalization include many individuals who are functioning normally: 56% were employed in the year before with average earnings of $22,150.

In these cases, an involuntary hospitalization may be neutral or deleterious.

Understanding the impact of involuntary hospitalization is important not only because of its pervasiveness, but also because the population brought in for evaluation is particularly vulnerable, experiencing high levels of pain and suffering. In surveys that are used to compare different health conditions by assigning each condition a weight on a scale of 0 (perfect health) to 1 (death), mental health disorders receive among the highest weights of any ailments. The weight for acute schizophrenia is 0.778 and 0.658 for severe major depressive disorder (Global Burden of Disease Collaborative Network, 2020).[2] For context, severe heart failure's weight is 0.51 and asthma's weight is 0.019. Indeed, individuals with behavioral health conditions experience vastly shorter lifespans with individuals with schizophrenia-spectrum disorders, for example, estimated to live 15.4 years less than people without (Chan et al., 2023).

Moreover, individuals evaluated for involuntary hospitalization tend to both have high needs and high costs. Of those who have been evaluated for involuntary hospitalization in our setting, over 60% use an emergency room within one year after the evaluation. In our setting, individuals evaluated in the previous three years account for nearly $1 of every $4 of behavioral health Medicaid spending despite making up only 1.5% of Medicaid enrollees (Welle et al., 2023). Despite these expenditures, 20% die within five years after the evaluation—a rate that is higher than that for individuals exiting jail, enrolling in homeless shelters, or living with severe mental illness in general. Further, 24% are are charged with a criminal offense within a year of evaluation (Welle et al., 2023).

Several jurisdictions have begun to expand the legal ability to treat individuals with mental illness without their consent. For example, New York Governor Kathy Hochul proposed lowering the threshold for involuntary hospitalization in 2025 (Hochul, 2025). Washington DC, California, and Oregon have also expanded their respective similar policies. In light of the magnitude of the problem and such policy initiatives, understanding the potential impact of involuntary hospitalization on the individuals it is trying to serve is crucial.

In keeping with the statutory mandate, we investigate how involuntary hospitalizations change the probability that a person is a danger to themselves — measured using death by suicide or overdose — or a danger to others — measured using violent crime charges. For the latter, we use the Federal

[2] An alternative interpretation of schizophrenia's weight is that living 1.28 years with acute schizophrenia is equivalent to dying one (healthy) year earlier than expected (1.28=1/0.778). As a result, health experts estimate that the US loses the equivalent of 4,707,170 years of full health to schizophrenia, depressive and bipolar disorders each year (Institute for Health Metrics and Evaluation, 2024).

Bureau of Investigation (FBI) Uniform Crime Reporting Program's definition of violent crime: "those offenses that involve force or threat of force" (FBI, 2019).

Answering this question has been challenging for several reasons: first, a raw comparison of those who are hospitalized to those who are not would conflate the sorting that physicians do when selecting whom to hospitalize with the causal impact of hospitalization. Second, data privacy laws and disconnected data systems in the US have made it challenging to track an individual across databases for hospitalization, legal services, and autopsy. We tackle these challenges by leveraging data from Allegheny County, Pennsylvania, where the city of Pittsburgh is located and approximately 1.2 million people reside. In Allegheny County, there is quasi-random assignment to the evaluating physician, allowing us to use a physician's tendency to hospitalize patients as an instrument for actual hospitalization. Further, Allegheny County has pioneered integrated data systems, which facilitate observing a variety of patient outcomes.

We find that in Allegheny County, involuntary hospitalization does *not* appear to serve its statutory purpose for the patients who would be hospitalized by some physicians, but not by others. Specifically, involuntary hospitalization increases the probability of being charged with a violent crime and increases the probability of death by suicide or overdose in the three months following an evaluation. These results are based on an instrumental variable measurement strategy and are consequently producing a local average treatment effect of how hospitalization impacts individuals who would be hospitalized by some physicians but not by others.

We consider several hypotheses that might explain why involuntary hospitalization increases adverse outcomes. We find evidence that involuntary hospitalization causes a decrease in employment and earnings, as well as an increase in shelter usage among those who had not used a shelter in the prior year. We do not find evidence to suggest that a decrease in drug tolerance is causing deleterious effects. We also do not find evidence that hospitalization increases utilization of continuing care such as outpatient mental health services or increases adherence to prescribed medications.

This paper contributes to several strands of literature exploring mental healthcare's impacts on crime and death, as well as on employment and homelessness, two of the mechanisms we consider.

Much of the current literature has focused on voluntary, outpatient therapies and their impacts, given the US's shift away from longer-term institutionalization.[3] Consistent with the Penrose (1939)

[3]There is, however, a literature on longer-term institutional facilities. Raphael and Stoll (2013) find that closures of psychiatric institutions in the 1950s and 1960s did not cause movement of individuals with serious mental illness

hypothesis that untreated mental illness increases crime, Deza et al. (2023) find that having more centers offering (outpatient, voluntary) mental health and substance use programs decreases assaults of on-duty police officers, the individuals who are often first to respond to mental crises. Jácome (2020) and Deza et al. (2024) document that losing Medicaid access increases crime, particularly among those with mental illness. Nesbit (2023) notes that when a judge mandates (usually outpatient) mental health treatment as part of a defendant's probation, recidivism declines. This paper contributes to the literature by considering how involuntary, short-term, hospitalization affects the criminal trajectories of those experiencing a crisis.

The paper also contributes to a literature on how mental healthcare relates to mortality, again by considering involuntary, short-term, inpatient treatment. Many papers document that individuals with SMI have much higher mortality rates (e.g., Liu et al., 2017). Yet use of medications such as lithium, neuroleptics, and antidepressants reduces mortality significantly (Baldessarini et al., 1999; Simpson and Jamison, 1999; Angst et al., 2005). Costantini (2024) find that receiving (voluntary) healthcare decreases mortality, in part due to better handling of co-occurring chronic illnesses. Yet Jordan and McNiel (2020) find that patients who perceived a hospitalization as being coercive are more likely to subsequently attempt suicide. Studying those who are admitted to the emergency department for self harm in California, Goldman-Mellor et al. (2022) condition on observables and find "no clear evidence that hospitalization reduces suicide risk." Hallett et al. (2024) further document that there can be adverse consequences of mental health inpatient experiences. Numerous studies find that there is an increased risk of death by suicide just after release from the hospital (e.g., Simpson and Jamison, 1999; Qin and Nordentoft, 2005).

The mechanisms we explore relate to a literature on mental healthcare's impact on employment and homelessness. Those with mental illness tend to have lower rates of employment and earnings (Banerjee et al., 2017; Biasi et al., 2021). But Biasi et al. (2021) find that access to (voluntary) medical care in the form of lithium medication can reduce the gap in earnings by one third for people with bipolar disorder, a result consistent with other work finding that medication can enhance employment and earnings (e.g., Bütikofer et al., 2020). This paper contributes by considering the impact of involuntary, short-term treatment on employment and homelessness.

(SMI) into the prison system, but closures of the 1970s and 1980s account for four to seven percent of the growth in incarceration in the following two decades. Davis et al. (2012) suggest that those with SMI were made worse off by the closure of intensive, long-term psychiatric facilities, while those with less serious issues who could access community-based treatment became better off.

While there is a robust and active debate among policy-makers, advocates and academics about the ethics of involuntary hospitalization, we are largely silent on this point, except to note that the aim of any policy is to achieve its explicitly stated goals.

Section 2 provides background context on the involuntary hospitalization process. Section 3 notes our data sources and sample construction. Section 4 explains our empirical approach. Section 5 discusses local average treatment effects of involuntary hospitalization on the individuals whom some physicians would hospitalize and others would not. Section 6 explores possible mechanisms. Section 7 explores marginal treatment effects for those individuals who are not on the margin of hospitalization in our main analysis. Section **??** performs a costing exercise. Section 8 offers a discussion of our findings.

2 Involuntary Hospitalization Process

In every US state and in many countries across the world, involuntary hospitalization is a legal tool meant to be used in emergency situations, when other safeguards for preventing adverse outcomes arising from mental illness—family, friends, outpatient care, and the like—are insufficient. In Pennsylvania, Section 302 of the 1976 Mental Health Procedures Act gives physicians the power to involuntarily hospitalize an individual deemed a danger to themselves or to others for psychiatric reasons (MHPA, 1976).

Evaluation proceeds as follows. If a person such as a family member or neighbor sees behavior or hears an individual indicate an intention to harm themselves or others, they call a centralized, public hotline to request that the individual be evaluated for involuntary hospitalization. A government delegate listens to their account of the individual's behavior and language. If the reporter's information reaches a given threshold,[4] the delegate will certify that the individual may be brought to a hospital emergency department for evaluation by a physician.[5] This process is in place for the 48.8% of individuals in our sample who are referred by family and other individuals. Physicians or police officers may directly refer an individual to be evaluated without consulting with a government delegate. Of

[4] A given case may not be certified if, for example, the reporter did not themselves witness the behavior or hear the language or if the individual's language/behavior does not actually indicate that they pose a danger to themselves or others.

[5] Technically if a person's case has been certified by a delegate, they may be brought in for evaluation any time in the next 30 days. However, the vast majority are evaluated much more rapidly (Figure B.1).

the referrals in our sample, 17.5% originate with medical professionals and 22.5% with police officers. The remainder come from other assorted individuals such as case workers or administrative personnel in healthcare settings. If the referral is successful, the individual is transferred to a hospital emergency department by police, a family member, or other means.

In the emergency department, a physician is tasked with determining if the individual currently poses a danger to themselves or others due to mental illness. They do this by speaking with the individual and evaluating their behavior. If the physician determines that they pose such a danger, the individual may be involuntarily hospitalized and transferred from the emergency department to an inpatient psychiatric facility.

In Pennsylvania, an individual may be hospitalized for up to 5 days based only on the judgment of the emergency department physician.[6] If the physicians in the inpatient mental health unit believe a stay longer than 5 days is necessary before a person can safely be released, they may petition the Court of Common Pleas (CCP) for a longer stay. A CCP magistrate may choose to extend the hospitalization for 20, 90 or 180 days.

Figure A.1 describes the flow of decisions for individuals in our setting. Out of 16,630 evaluations, 78% result in a decision to hold an individual for up to 5 days; the others are released. Physicians petition for 51% of the emergency holds to be extended (39.4% of the initially evaluated cases). Of those extension petitions, 73% are upheld by the magistrate, keeping the patient hospitalized for up to 20 days total (28.6% of the initially evaluated cases). Put differently, 8,150 or 63.1% of the initial involuntary hospitalizations do not extend beyond 5 days. Of those initially involuntarily hospitalized, only 8.2% are petitioned to be held up to 90 days (6.3% of the initially evaluated cases) and 1.1% are petitioned to be held for up to 180 days (0.8% of the initially evaluated cases).

2.1 Features Allowing Empirical Analysis

Our identification strategy leverages variation in physicians' tendencies for hospitalizing individuals involuntarily. Several features of our setting facilitate our empirical approach. First, there are multiple physicians in any given emergency department at any given time. This means that who will serve as the evaluating physician is unclear and we can compare outcomes across physicians.

Second, there is quasi-random assignment to physician. Cases are added to the triage list, usually

[6] This duration varies across US states and countries, with 72 hours also being common.

with a priority level of 2 on a scale of 1 (very urgent) to 5 (not so urgent).[7] This triage list determines the order in which cases are handled, creating quasi-random assignment in which physician handles a particular case. There are exceptions to this rule, however. For example, if a patient is being involuntarily hospitalized regularly and is known the examiners, there may be an attempt to ensure that the same physician sees a patient they have seen before if that physician is on shift and available. If the individual is a child or is elderly, an attempt may be made to assign the patient to someone with pediatric or geriatric experience, and caregivers may have a greater say. As such, we limit our sample to those individuals who are being evaluated for involuntary hospitalization for the first time and who range in age between 18 and 65 years old.

It is possible that there may be endogeneity in when a patient is brought into the emergency department and which physicians who serve during those times. Consequently, we residualize our instrument, effectively comparing *within* a given hospital's shift so that we only compare outcomes among patients who could have been seen by another physician. This decision follows many prior papers that use similar instruments where decision-makers are conditionally randomly assigned (e.g., Dobbie et al., 2018; Frandsen et al., 2023; Norris et al., 2021; Gross and Baron, 2022).

Third, there is limited possibility for influencing which physician will conduct an involuntary hospitalization evaluation. Evaluations in Pennsylvania must occur within two hours of an individual's arrival at the emergency department, making physician availability at the time of arrival and placement on the triage list a key determinant of who performs the evaluation (Welle et al., 2023).[8] Since quasi-random assignment of cases to physicians is critical to our empirical strategy, we test the relationship between individual patients' observable traits and physician leniency or proclivity to hospitalize involuntarily in Section 4.2.

Finally, what happens after the evaluation in the emergency department is handled by a different set of physicians: the physicians in the inpatient mental health ward. After a patient is transferred out of the emergency department, decisions about what therapies to provide and what medications to try are made by the inpatient care team. Decisions to apply for an extension and grant a hold beyond the 5 days allowed by an emergency department's evaluation are determined by the inpatient care team and CCP magistrate, respectively. This helps with the key exclusion restriction, which we

[7] For example, an evaluation for involuntary hospitalization is likely to be handled before a broken rib, but after surgery for acute trauma.

[8] If an evaluation does not occur within two hours, the person is supposed to be released. We cannot tell from our data how often this happens, but from our conversations with psychiatrists, this is rare.

test below as well.

3 Data Sources

We study the impact of involuntary hospitalization in Allegheny County, Pennsylvania, an area that encompasses the city of Pittsburgh and its suburban and rural surroundings. It is home to about 1.2 million residents.

The County's administrative data available to us include information on individuals who interact with government services, of which involuntary hospitalization is one (Allegheny County Department of Human Services, 2024). Across the many services offered and many service-specific databases, the county connects records and generates a single record for each person. While these data are available to Allegheny County Department of Human Services personnel, they are not seen by service-specific decision-makers. The County's data include demographic information such as age, legal sex, and educational level for some individuals.

Involuntary Hospitalization Data. Data on involuntary hospitalizations are aggregated by the Allegheny County's Office of Behavioral Health. For each call to a government delegate, the date, the identifying information of the individual in question, the relationship of the reporter to the individual, and the identity of the delegate who decides whether or not to certify the case are all documented, as well as which of the four legal criteria — unable to care for self, danger to others, attempted suicide, or mutilated self — is being reported in the petition.

Data on involuntary hospitalization evaluations are likewise aggregated from hospitals where the evaluation occurred. For each evaluation, the reporter, criteria noted by the reporter, the physician deciding the case, and the conclusion of their evaluation is recorded. All of the information from initial call through the evaluation is recorded in the county's custom software created to manage and record data on involuntary hospitalizations.

Since any further holds beyond the initial five day hold must go through the Court of Common Pleas, this extension data is not included in county system. Rather, the court records are matched back through Allegheny County's systems to complete the picture of the involuntary hospitalization.

Violent Charge Data. Data on criminal charges come from local Magisterial District Courts, and show the date on which an individual was charged and with what offenses they were charged. We focus on violent offenses, as defined by the FBI's Uniform Crime Reporting Program: "those offenses that involve force or threat of force" (FBI, 2019). This includes murder, negligent and non-negligent manslaughter, rape, simple and aggravated assault, robbery, and intimidation.

We focus on being *charged* with a violent offense as we believe that it strikes a reasonable balance of competing approaches to measuring whether someone is a danger to others. On the one hand, it is a more stringent measure of being a danger to others than and arrest, as being charged with a crime generally requires two decision-makers—the arresting officer and the charging prosecutor—to believe there is sufficient evidence to regard this person as having endangered others. On the other hand, we cannot rely on conviction for a violent offense because, for instance, mental illness can be grounds for finding a defendant unfit to stand trial. So an outcome measure like conviction, while perhaps more precisely identifying guilt, could get muddied by the legal process, including by the treatment of having been recently involuntarily hospitalized.

Suicide and Overdose Death Data. We determine whether a death is due to either suicide or drug overdose based on autopsy reports from the Allegheny County Medical Examiner's Office. Most deaths are the result of disease and are not sent to the Medical Examiner for autopsy. However, deaths that are suspected to be due to suicide, drug overdose, homicide, or vehicular accidents are referred to the Medical Examiner, who provides an official determination of the cause of death.[9]

The examiner determines the immediate cause of death, which is the underlying disease or injury that explains why someone died (e.g., asphyxiation, sharp force/incised, blunt force trauma, firearm) as well as the manner of the death, which can be categorized as natural cause, (drug and non-drug) accident, homicide, suicide, and undetermined.[10] If the cause of death is related to drugs and the manner of death is an accident or suicide, or if the manner of death is a suicide (and the cause of death is anything), we consider the death to fall into our category of focus.

Other Data Sources. We merge in additional data to obtain auxillary outcomes and controls.

[9] We focus on the determinations done by the Medical Examiner rather than data from death certificates because a prior literature has found that there are often errors in cause and manner of death on death certificates completed by non-Medical Examiners, even when the certificate is filled out by a physician (McGivern et al., 2017).

[10] Many immediate causes of death could fall into various possible categorizations. For instance, some asphyxiation deaths are categorized as homicides, some as non-drug accidents, and some as suicides.

The primary additional source of data are Medicaid claims. For any individual on Medicaid, we observe every individual billing episode for services, the diagnosis the service was billed under, the provider who billed the service. Services included could be things like inpatient or outpatient treatment, Assertive Community Treatment, service coordination, or prescription filling.

We have information on shelter usage through the county's Homeless Management Information System.

Data on wages, employment, and income come from state unemployment insurance records. Data includes wages by employer at the quarterly level.

Lastly, physicians are matched based on name through to the National Plan Provider Enumeration System to retrieve a National Provider Identifier (NPI) record. Once matched, we can extract information the doctor such as how long they have been in practice and their specialty.

4 Empirical Approach

We seek to understand the impact of the *involuntary hospitalization* of individual i on their outcomes Y_i such as death by suicide or overdose and subsequent violent crime charges. This goal be modeled as:

$$Y_{ijr} = \gamma_0 + \gamma_1 Hospitalization_{ijr} + \gamma_2 X_{ij} + \epsilon_{ijr} \tag{1}$$

where X_{ijr} is a vector of individual- and case-specific characteristics and ϵ_{ijr} is an error term.

A key problem in an ordinary least squares (OLS) regression of this relationship is that there may be a correlation between involuntary hospitalization and the unobserved traits of the individual that are also correlated with our outcomes of interest: that is, the OLS regression is likely to suffer from omitted variable bias. Indeed, given that those who are hospitalized look broadly similar to those who are not (Table 1), there may be reason to believe that physicians' discernment in deciding who is hospitalized is based on traits unobservable to the researcher.

Assuming physicians have some skill at differentiating which patients would benefit from hospitalization beyond flipping a coin, the individuals who are hospitalized are meaningfully different from those who are not hospitalized. Thus, any OLS-driven comparison would capture both the treatment effect of hospitalization and the inherent differences in the two groups that affect our outcomes of

interest. For example, the evaluating physician may notice that one patient has more suicidal intent than another who has suicidal ideation (but no intent) and choose to hospitalize the former and not the latter. Such unobserved features would produce biased OLS estimates.

Moreover, OLS assumes constant treatment effects for all groups, which may obfuscate heterogeneous treatment effects. If some (unobservably different) individuals respond positively to hospitalization and others negatively, we might find no effect. Or, if many respond positively, but some respond negatively, we would likely miss the latter in an OLS approach.

To overcome the challenges posed by OLS, we use an instrumental variables approach that leverages quasi-random assignment of each evaluation to an emergency department physician. Specifically, we use a physician's tendency to hospitalize an individual as an instrument for actual hospitalization. This strategy, often called "judge IV" has been used widely.[11]

In our approach, we estimate the following two equations:

$$Hospitalized_{ijr} = \alpha_1 PhysicianTendency_{ij} + \alpha_2 X_{ijr} + \mu_{ijr} \tag{2}$$

$$Y_i = \beta_0 + \beta_1 \widehat{Hospitalized}_i + \beta_2 X_i + v_i \tag{3}$$

with equation 2 as the first stage and equation 3 as the second stage in a two stage least squares regression. Our estimate for β_1 captures the effect of involuntary hospitalization on our outcomes of interest.[12]

This approach estimates the effect of involuntary hospitalization on the individuals who would be hospitalized by some physicians but not by other physicians. This is called the local average treatment effect (LATE) (Imbens and Angrist, 1994) and gives us a measure of the effect of involuntary hospitalization on those individuals whose hospitalizations are "judgment calls" to the extend that physicians differ on whether or not they would hospitalized this individual.

The fact that our estimates pertain only to "judgment call" marginal cases suggests considerable caution in generalizing our results to all involuntary hospitalizations. For example, patients' likelihood

[11] For example, judge IV designs have been used to study the impacts of incarceration (Kling, 2006; Aizer and Doyle, 2015; Norris et al., 2021), foster care (Doyle, 2007; Gross and Baron, 2022), pretrial detention (Dobbie et al., 2018), and disability insurance (Dahl et al., 2014), among many others.

[12] A recent literature (Cellini et al., 2010; Gelber et al., 2016) discusses that treatment assignment might affect outcomes both directly and indirectly by influencing future hospitalizations. β captures both effects, which is precisely the policy-relevant element that we would want to understand. We discuss this in Section 5.3 to help interpret the findings.

of violent behavior may be quite different for those who would be hospitalized by *all* physicians in our sample, and who are therefore not marginal evaluations. However, in many ways, our estimates are the policy-relevant measures. The individuals whose effects we estimate are those who would be affected if slightly more or slightly fewer individuals were hospitalized through the mechanism of physician discretion.

Instrument Construction. We use physician tendencies to hospitalize an individual as our instrument. Specifically, we use a residualized, leave-one-out mean construction of our instrument.

This residualized measure is preferable in settings with quasi-random assignment for two main reasons, as noted in the literature (e.g., Dahl et al., 2014; Dobbie et al., 2018). First, our case assignment is only random conditional on the rotation in which a patient was brought to the emergency department, not truly random, where rotation is given by time-of-day and month in each hospital. As such, selection could bias our results if we did not residualize. For example, if more stringent physicians tend to work on days of general carousing, and individuals with substance use disorders are more likely to be brought in on these days, we may see some selection effects. Residualizing helps address any such selection issues.

Second, this approach controls for differences across hospitals. Since patients may be more likely to go to a hospital closer to their home, or a relative may bring a person to a hospital where they have previously received care, patient characteristics may well differ across hospitals. Moreover, hospitals may feature different norms about hospitalizing individuals.

To account for any systematic differences, we include fixed effects for the hospital-rotation in which an individual is seen. We create a residualized hospitalization measure by regressing hospitalization on hospital-rotation fixed effects and subtracting the covariates multiplied by their estimated coefficients from the $Hospitalized_{ijr}$ indicator. This approach means our analysis compares patients who encountered the same set of physicians, leveraging within-rotation variation only, and is consistent with prior work (e.g., Dobbie et al., 2018; Leslie and Pope, 2017; Frandsen et al., 2023).

We construct the instrument for each individual by computing the leave-one-out mean of the residualized measure of the physician's other cases — both past and future. We use the leave-one-out mean, which excludes the physician's decision in the present case from the instrument, so that we do not introduce estimation errors on both sides of the equation and bias into our estimates.[13]

[13]The residualized leave-one-out mean is equivalent to the reduced form of the jackknife instrument that is recom-

There are 1834 unique rotations featuring first-time evaluations. The typical rotation has 8.26 physicians performing evaluations (including those who are evaluating non-first-time cases).

Variation in Physician Tendency. Figure 1 shows the distribution of our residualized instrument for physician hospitalization in the grey histogram. Our sample contains 424 physicians at 14 different hospitals. On average, each physician performs an average of 39.22 first-time involuntary hospitalization evaluations in our sample. Our residualized measure of physician tendency to hospitalize ranges from -0.427 to 0.271, with a standard deviation of 0.098. The lowest rate of hospitalization that one physician has is 11%, while 46 physicians hospitalize all of the cases they evaluate.

4.1 Sample

Our sample consists of involuntary hospitalization evaluations that occurred between the June 2014 through to the end of 2023 in Allegheny County. We limit our sample in several ways. Table A.1 shows the cases, patients, physicians, and hospitals eliminated due to each restriction.

First, we focus on the initial time a specific patient is evaluated. Some hospitals attempt to have the same emergency department physician evaluate a patient who has been through the involuntary hospitalization process before if the physician happens to be on duty. For these patients, physician assignment is unlikely to be quasi-randomly assigned.

Second, we limit to cases where the patient is between the ages of 18 and 65. For those under 18, parents and physicians tend to be co-deciders and so the monotonicity of a leave-one-out measure of physician tendency to hospitalize may not hold. Likewise, for those over age 65, there are often caregivers who are involved, giving rise to similar concerns. Further, in these cases, hospitals often have protocols to involve physicians with pediatric or geriatric experience. As such, we focus only on patients who are likely to see whichever physician is on staff.

Third, we limit to cases handled by physicians who have encountered at least 15 involuntary hospitalization evaluations, in order to minimize noise in our measure of hospitalization tendency. This restriction ensures that we accurately measure physicians' tendencies to hospitalize as physicians' tendencies to hospitalize an individual stabilize after evaluating about 15 patients (Figure A.2).

Finally, we limit to evaluations that happen during rotations where more than one physician is

mended when the number of instruments (physician fixed effects) is likely to increase with the sample size Stock et al. (2002); Kolesár et al. (2015).

available and where more than one evaluation takes place, a necessary restriction when we include rotation fixed effects.

Table 1 describes the analysis sample. Our sample consists of 16,630 evaluations of which 12,909 result in an involuntary hospitalization.

To whom do our estimates apply? Our instrumental variable analysis generates estimates about the impact of an involuntary hospitalization *only* for those individuals who would be hospitalized by one physician, but not by another. This group of people whose hospitalization is a judgment call are traditionally called "compliers" (Imbens and Angrist, 1994). An individual cannot be identified as a complier. However, by comparing physicians who hospitalize a greater or smaller share of the individuals they evaluate, we can both calculate the share of individuals who are compliers and describe their observable characteristics (Frandsen et al., 2023). Formally, if we order the physicians according to the share of people they hospitalize, $j = 1, 2, ...J$, the proportion of compliers is given by $E[Hospitalized_{iJ}] - E[Hospitalized_{i0}]$, namely the difference in hospitalization rates between the physicians most and least inclined to hospitalize. With linearity of the first stage, this can be approximated by $\alpha(PhysicianTendency_J - PhysicianTendency_0)$. Compliers represent 43% of our sample, indicating that our estimates are relevant for a substantial portion of the population in question.[14]

These estimates cannot speak to the effects of hospitalization on those individuals whom all doctors would elect to hospitalize. To better understand to whom our estimates apply, we characterize this group of people, using the methods developed by Frandsen et al. (2023). We describe them in the fourth column of Table 1.

Panel A shows demographic information. Men are a bit more likely than women to be evaluated for involuntary hospitalization. About half of the patients in our sample are between the ages of 18 and 35. Most of the cases involve a White patient. However, relative to the racial breakdown in Allegheny County, Black patients are over-represented, making up 28% of the evaluations, whereas only 13.5% of the county population is Black. About half have less education than a high school degree. For a quarter of evaluations, we do not know educational attainment. About 24% of the evaluated population is enrolled in Medicaid. Compliers are more likely to be male, more likely to be

[14] Under the assumption of average monotonicity, $\alpha(PhysicianTendency_J - PhysicianTendency_0)$ represents a lower bound.

white and more likely to be enrolled in Medicaid than the sample of evaluated individuals.

Panel B shows past diagnoses for the individuals enrolled in Medicaid or with a county-paid medical claim. Within this population, about half have been diagnosed with what the County characterizes as a severe mental illness. The County's definition includes schizophrenia, schizotypal disorder, schizoaffective disorder, unspecified psychosis, persistent mood disorders, bipolar, and borderline personality disorder. Over 65.8% have some sort of mental health diagnosis. And 21.5% have a substance use diagnosis. An individual may have multiple diagnoses and may be double-counted in these rows. Compliers are less likely to have mental illnesses, severe or otherwise, or substance use disorder than the general sample of evaluated individuals.

Panel C shows prior encounters in our study period with local government. About 22.3% have a prior criminal charge and about 9.6% have a prior noncriminal charge. About 30.2% have had a visit to the emergency department. Compliers are more likely to have prior charges and prior emergency department visits.

Panel D shows the traits associated with the involuntary hospitalization evaluation. Just under half of the cases are referred by a relative. Another 22.5% are referred by a police officer. Compliers are more likely to be brought in by a family member. The concerns brought to the evaluating physician's attention during the process include having attempted suicide in half the cases, being unable to care for one's self in 44.1% of cases, and posing a danger to others in 47.2% of cases. These categories are not mutually exclusive. Compliers look like the general population of evaluated individuals in terms of the concerns noted.

4.2 Internal Validity

Several assumptions are necessary for our instrumental variable analysis to be valid, which we seek to test. These are: (1) *relevance:* physicians' tendencies to hospitalize other patients are associated with hospitalization of a given individual, (2) *random assignment:* physicians are (conditionally) randomly assigned to involuntary hospitalization cases, (3) *monotonicity:* patients who would be hospitalized by physicians with a low tendency to hospitalize would also be hospitalized by physicians with a high tendency to hospitalize and vice versa, (4) *exclusion restriction:* physician assignment impacts outcomes only through hospitalization.

Relevance. A natural test of relevance is a strong and significant first stage, as measured in α_1 in equation 2. In both Table 2 and Figure 1, we find just such a robust first stage; the residualized measure of a physician's tendency to hospitalize in an evaluation is highly predictive of whether a patient is hospitalized.

The top panel of Figure 1 gives a graphical intuition of the first stage. The grey histogram shows the residualized physician tendency to hospitalize a patient. The upward sloping curve flexibly represents the first stage, showing that those assigned to physicians with a higher residualized tendency are more likely to be hospitalized. The rate of hospitalization increases monotonically and nearly linearly. At the 5th percentile, physicians hospitalize 64% of the patients they evaluate; at the 95th, 93%. A 10 percentage point increase in the physician tendency is associated with approximately an 8.3 percentage point increase in the probability of being hospitalized in our preferred specification.

Table 2 presents the results of estimating equation 2. Each column adds additional controls. Column 2 includes hospital fixed effects. While there is a slight decrease in the first stage when we control for hospital, this is not surprising as different populations may go to different hospitals. Column 3 shows rotation fixed effects, which capture the set of physicians who are staffing the specific emergency department at the time of the involuntary hospitalization evaluation. Column 4 includes demographic controls such as sex, age, racial category, education, and medicaid enrollment as shown in Table 1. When we add rotation- and individual-level controls, the magnitude of the first stage does not meaningfully change, consistent with quasi-random assignment of physicians. Column 5 includes controls for whether the individual has a prior criminal or noncriminal arrest or emergency department visit in our sample period, which is our preferred specification. Column 6 includes the reporter's concern. Finally, column 7 includes medical controls for the subsample of those enrolled in Medicaid, including whether the individual has a severe mental illness, a mental illness, or a substance use disorder diagnosis. The penultimate specification is our preferred specification used throughout the rest of the paper.

Additionally, the effective F-statistic is 104.8, suggesting we do not have a weak instruments problem (Olea and Pflueger, 2013). Nevertheless we show results for Anderson-Rubin confidence intervals, which are impervious to weak instruments.

Our instrument is not perfectly predictive of hospitalization, possibly because physicians may respond to traits we do not observe, creating measurement error, or because their tendencies may

vary over the sample period. Nevertheless, we find a very strong first-stage relationship.

Random Assignment. A key assumption is that our instrument is uncorrelated with the error term. This would hold if there were quasi-random assignment. To test that physicians are quasi-randomly assigned to involuntary hospitalization evaluations, we confirm that while demographic traits may be related to hospitalization, they are unrelated to the physician assigned to the case. Table 3 shows these tests. In column 1, we relate a set of observable traits to an individual's likelihood of being hospitalized. We find a significant F-statistic of joint significance of 10.9(p-value = 0.0227).

Column 2 shows that these traits are unrelated to the assigned physician's tendency to hospitalize. Our F-statistic of joint significance is 1.192, which is not statistically significant (p-value = 0.25).

Monotonicity. One condition needed to interpret our two stage least squares estimate as the LATE is that those who are hospitalized by a physician who hospitalizes a small share of patients would also be hospitalized by a physician who hospitalizes a large share of patients; and conversely that those who are released by a physician who releases a small share of patients would also be released by a physician who releases a large share of patients.

One test of monotonicity is that first-stage estimates should be non-negative in all subsamples. Table 4 displays the first-stage estimates separately by demographic group and Table 5 does so by the concern that precipitated the involuntary hospitalization evaluation. In all subsamples, the estimates are non-negative, stable, and significant, consistent with monotonicity.[15]

A closely related test is proposed by Frandsen et al. (2023), who show that one can recover a weighted average of individual treatment effects with a relaxed average monotonicity assumption. This assumption implies that the covariance between physician's hospitalization rate and the rate of the outcomes observed is weakly positive. This is akin to showing that the first stage should be positive in all subsampes of the data. We pass their explicitly proposed test.

We explore how physicians treat patients who are observably different (Appendix Figure A.3). We calculate a residualized hospitalization measure for each physician separately for either side of each trait (e.g., a tendency among female patients and another among male patients). We plot the measures and note the correlation between the two measures. All of these correlations are positive,

[15]These analyses are also helpful in understanding who the compliers are because heuristically, compliers are more heavily concentrated in subgroups where the instrument has a stronger relationship with hospitalization.

suggesting that physicians' tendencies toward hospitalization are similar across observably different patients. This is consistent with our monotonicity assumption.

Further, we believe there are no defiers in our sample. There are two possible types of defiers: individuals whom a physician determines should be hospitalized but are not, and those whom a physician releases but who choose to be hospitalized. The former is nearly impossible given that individuals may be held by force. The latter is possible: individuals may choose to voluntarily commit themselves to hospitalization. In the parlance of Pennsylvania's statute, this is called a 201. Some of these voluntary hospitalizations may occur after a physician has opted not to hospitalize the individual involuntarily, and these individuals would be defiers. All 201 commitments, of which there are 1066, are dropped from our sample accordingly.

Exclusion Restriction. The exclusion restriction requires that physician assignment only impacts outcomes via their decision about whether to hospitalize a patient or not. This is impossible to test empirically. However, several institutional features help give confidence that the exclusion restriction holds in our context. First, the emergency department physician who performs the involuntary hospitalization evaluation is not the physician who provides care on the mental health inpatient floor. Rather, inpatient physicians tend to provide care while the patient is in the hospital. This means that the exact care provided is divorced from the decision of whether to hospitalize or not. This mitigates concerns about omitted treatment bias (Mueller-Smith, 2015).[16]

Second, the emergency department physician does not decide the duration of hospitalization; inpatient physicians decide whether or not to file for an extension of involuntary hospitalization with a magistrate in the CCP and it is that magistrate who decides whether a patient should remain hospitalized for longer than 5 days.

Consistent with the exclusion restriction, we find that conditional upon hospitalization, the physician's tendency to hospitalize is unrelated to further downstream outcomes (Table 6). Specifically, it is unrelated to whether an individual has a hearing with the CCP or whether the magistrate extends their hospitalization. Likewise for those enrolled in Medicaid, it is unrelated to whether there is an inpatient visit. The F-statistic and p-value from a test of joint significance is shown at the bottom.

[16]In our data, 6% of hospitalizations do not show an inpatient stay. This may occur if the individual is held for a very short period of time before being released and the transfer does not occur. It can occur if there is no available spot in the mental health inpatient unit and the patient must be held in the emergency department. For this reason, we test the robustness of our findings to excluding these individuals in Section 5.3.

If our exclusion restriction is violated, then the reduced form estimates can still be interpreted as the causal effect of being assigned to a physician more or less inclined to hospitalize. Our reduced form estimates are reported in our tables and tend to be fairly similar to the estimates arrived at via two stage least squares. This makes sense given that we have a very strong first stage. The similarity of the reduced form and two stage least squares estimates provides confidence that we may be capturing the causal effect of hospitalization.

4.3 Contextualization

To better understand the magnitude of our estimates, we provide several pieces of context. First, we provide the control mean, the average outcome among those who are not hospitalized. Among those who are not hospitalized, 0.83% are charged with a violent crime within the first 3 months after hospitalization. Further 0.28% die by suicide or drug overdose in that time period.

In the context of calculating the local average treatment effect, we also report the complier control mean, which estimates the rate of outcomes would have been among the complier population had they not been hospitalized. We follow Frandsen et al. (2023) in calculating these, finding that 3.27% of compliers are charged with a violent crime; 0.92% die by suicide or drug overdose.

Finally, we are able to add one more number for contextualization: what the outcome would be if *everyone* had been treated. Note that this number should not be interpreted as the causal effect of hospitalizing everyone, as we do not know the counterfactual of what would have happened if all of these individuals had *not* been hospitalized. We recover this measure by leveraging the fact that 46 physicians hospitalize all the individuals they evaluate. Together with quasi-random assignment of cases to physicians, this means that the random subsample of 702 individuals who are assigned to these physicians are *always* hospitalized. Among the individuals quasi-randomly assigned to these physicians, we find that 0.58% percent are charged with a violent crime within three months of their evaluation. Additionally, 0.3 % die by suicide or overdose in that time.

5 Local Average Treatment Effects of Involuntary Hospitalization

Naive ordinary least squares regressions find no statistically significant impact of hospitalization on either death or violent crime charges. As noted above, these regressions conflate the causal impacts of involuntary hospitalization with the sorting that physicians may be doing. A null finding could hypothetically belie either physicians selecting less risky individuals and then hospitalization increasing their risk or physicians hospitalizing more risky individuals and then hospitalization decreasing their risk. A null finding can also indicate that there are heterogeneous treatment effects that go in opposite directions for different groups. Under this assumption, the sign of the null effects suggests that possibly the allocation of hospitalization has a positive effects on those outside the complier group.

5.1 A Danger to Others

Our instrumented estimates find that involuntary hospitalization increases the probability of being charged with a violent crime and the probability of death by suicide or overdose. Table 7 shows our first stage and both reduced form and instrumental variables estimates of the impacts.

For being charged with a violent crime, our reduced form estimate finds that a 10 percentage point increase in a physician's tendency to hospitalize is associated with a 2.6 percentage point increase in being charged with a violent crime within the following 3 months. Column 3 divides the reduced form estimate by the first stage, finding a slightly higher effect. Figure 2(A) shows the time trends in these effects. The grey dots show the control complier mean—which can be interpreted as the cumulative share of the compliers who would have experienced the given outcome in the absence of hospitalization. The blue triangles show the level effects that would be implied by our two stage least squares estimates. They are calculated by adding the IV coefficient β_1 from equation 3 to the control complier mean.

This statistically significant increase in being charged with a violent crime is most interesting because it goes in the opposite direction from a possible mechanical effect. There are behaviors that are potentially considered both illegal and related to psychiatric decompensation, for instance threatening to kill a person. If a physician finds this person to be mentally unsound, they may

not be subject to the same strictures from the criminal justice system. If they are not in need of hospitalization in the physician's estimation, it is possible that they would have been charged with a crime. As such, if this dynamic were occurring, it would push *against* finding the results that we do. We hypothesize that this dynamic is indeed occurring because we find no effect on violent charges among those referred for evaluation by police (Figure A.6). As such we consider the of our result by excluding cases brought in by police in Section 5.3.

5.2 A Danger to Oneself

Our results find that hospitalization increases death by suicide or overdose in the three months after an evaluation. A 10 percentage point increase in a physician's tendency to hospitalize yields an increase in the probability of death in the next three months of 0.98 percentage points. This represents a doubling of the control complier mean. This is consistent with prior literature on the time following discharge from a hospitalization being especially high-risk for death by suicide (e.g., Qin and Nordentoft, 2005).

5.3 Robustness.

Our results are robust to several different design elements in our main specification. Table A.2 shows the first stage, reduced form estimates, and instrumented LATE estimates for both violent charges and deaths by suicide or overdose. Row 1 shows the same estimates with no controls. Our main estimates are measured 3 months after the individual has been evaluated for hospitalization. Rows 2 and 3 show our outcomes as they are measured at 6- and 12- months after the evaluation.

We next shift which physicians are included. Our main specification requires that a physician has evaluated at least 15 cases. This is based on Figure A.2, which shows that physicians' hospitalization tendency seems to stabilize after they have evaluated approximately 15 cases. Nevertheless, we consider samples in which we include physicians who have evaluated more than 5 cases (row 4), and restrict to those who have evaluated more than 30 cases (row 5).

Our results are also robust to considering different ways of clustering our standard errors. Our main estimates cluster at the physician level in case those evaluated by the same physician are somehow correlated in ways not accounted for by the construction of the instrument. Another approach would be to cluster at the rotation level since that is the level at which treatment is assigned. Row 6 shows our results persist if we cluster at the rotation level. Yet another approach would be to cluster at the

physician-rotation level, which also does not affect the significance of our effects. We have limited to only first-time evaluations to ensure that assignment to physician is random. As such, it does not make sense to cluster at the patient- or patient-physician or patient-rotation levels.

We next turn to conceptual robustness. One might worry that individuals referred by police have a different counterfactual to hospitalization than do other individuals. Specifically, some behaviors may be grounds for arrest if they were committed in sound mind. For example, verbally threatening a person may be considered either evidence of mental illness or grounds for arrest. If an individual who has exhibited such behavior is not hospitalized, a police officer may arrest them. If that were the case, the control group may have elevated levels of being charged with a violent crime and the true results may be attenuated. As such, we exclude cases referred by the police in row 8 of Table A.2, finding that while our point estimates are a bit higher than our main estimates, they are not so large as to conclude that our main estimates are artifacts of mechanical incarcerations in the control group. Another concern is that those referred by physicians may be different themselves or may be treated differently. As such, row 9 shows results excluding cases referred by physicians.

Likewise, we had noted that not everyone has an inpatient visit in their chart. We limit accordingly in row 10, finding similar results. In our main specification, we measure deaths by suicide and overdose among those who are not yet charged with a crime. This is because we worry that those individuals who have been charged with a crime would have been held in jail, which might incapacitate attempts at suicide or overdose. This approach is also consistent with avoiding double-counting these individuals in both sets of outcomes. However, it is also reasonable to wonder about the overall effects, regardless of whether an individual has spent time in jail. To this end, Table A.2 row 11 shows the effects when allowing for possible incapacitation, finding slightly noisier effects that point in the same direction.

We allow our main sample to include people even if we do not observe them for the entire post-period after the evaluation (e.g., their evaluation occurred only 2 months before the end of our data). Row 12 shows the estimates when limiting to a balanced panel for which we can see the whole post-period.

We also consider alternative instrument constructions. Our main estimate uses a residualized leave-one-out mean for physician tendency used in much of the recent judge IV literature. We also use a split sample instrument in which we estimate physician tendency separately for half of the rotations and then apply these tendencies to the other half the sample and vice versa. We find similar results

in row 13. Row 14 presents results using a full set of physician fixed effects as instruments. We also construct instruments that are allowed to differ based on whether a person has been indicated to have attempted suicide or not (row 15) or be a danger to others or not (row 16). These two instruments relax the monotonicity assumption. The similarity of these results suggests that potential monotonicity violations are likely to introduce only minimal bias.

We consider Anderson-Rubin confidence intervals, which are valid regardless of whether an instrument is weak since their procedure uses test inversion to obtain confidence intervals (Anderson and Rubin, 1949; Andrews et al., 2019). Figure A.7 shows that the Anderson-Rubin confidence intervals are nearly identical to the conventional LATE ones. This is consistent with the fact that our effective F-tests shown in Table 2, which measure weak instruments per Olea and Pflueger (2013), are sufficiently large that we are not concerned about weak instruments.

Interpretation. These estimates speak directly to the policy experiment we have in mind is dialing up or down what share of people coming in for evaluation are hospitalized for two reasons. First, the measured effects are those of involuntary hospitalization on compliers. As discussed above, the compliers are those who might be hospitalized by some physicians but not by other physicians.

Second, our estimates measure the effects both of direct and indirect effects. That is, if hospitalization changes the likelihood of future involuntary hospitalizations, as discussed in a recent literature (**?**), we're capturing both effects. To more deeply understand our context, note that while we measure no causal effect of hospitalization on subsequent evaluations (Table A.3), 10.23% of treated individuals have another evaluation for involuntary hospitalization in the three-month primary outcome study period. The null effect of hospitalization on subsequent evaluations and hospitalizations is consistent with the fact that physicians' mandates are to evaluate if a person poses at danger at the *present* moment rather than future moments. It is also consistent with the fact that medical records available to the evaluating physician do not always include prior evaluations unless the individual is being evaluated within the same hospital system.

Note that finding limited indirect effects via subsequent involuntary hospitalization is neither good nor bad for our strategy. Capturing both direct and indirect effects in β is precisely the policy-change of interest.

6 Mechanisms

Several hypotheses could explain the increase in risks resulting from hospitalization among our compliers: disrupted housing, disrupted employment, trauma from the evaluation process itself, destabilized relationships, decreased drug tolerance. These effects are not mutually exclusive. We endeavor to consider the weight of the evidence for each hypothesis.

6.1 Disrupted Employment

Involuntary hospitalization may disrupt employment. For many jobs, an unexplained absence—and especially when it is sustained over several days—is interpreted as quitting or is grounds for termination. Individuals are rarely given an opportunity to call relevant employers before hospitalization. If the employment were a source of structure or daily rhythm, this might contribute to negative impacts.

To evaluate this, we use data from Pennsylvania's Department of Labor and Industry, which includes employers and earnings in each quarter from June 2016.[17]

Among our sample 8% reported an employer at some point during the year before the evaluation. Those who were employed had the average income of $5,804 in the quarter before the evaluation.

In the quarter following an evaluation, there is a statistically significant decline of $1,040(Table 8A). The decease persists throughout the quarters following the evaluation. Given the baseline earnings in the quarter before hospitalization, this represents a decrease of 18%. This aligns with a decrease in employment as well (Table 8B), although this is only significant in later quarters.

There are two mechanical effects worth discussing. First, earnings may decrease while hospitalized. This is slightly less of a concern because we measure income at a quarterly level and the vast majority of individuals are held for 20-days or less (Figure A.1), or approximately 14 business days. Considering the quarters removes this mechanical effect for most individuals.

Second, those who die or are incarcerated are, mechanically, going to show up as earning less income. As such, we perform the same analysis focusing on those who do not show up in jail and are not dead (for any reason). We find that in the quarter after hospitalization, the decrease in earnings is also a statistically significant decrease of $1,210.

[17] These data are requested from the state by the county for reasons unrelated to this research. Not every person's data is requested by the county from the state. We are successfully able to match around 46.2% of our sample. None of the individuals evaluated in the first two years of the data are matched.

6.2 Disrupted Housing

We next consider that involuntary hospitalization could disrupt housing. People who have been living with others may find that their relatives or housemates are unwilling to live with someone who has been unwell enough to warrant involuntary hospitalization and has been officially deemed a "danger" to others or themselves. If individuals have lost their job, they may be unable to pay rent. If a person is living in a shelter, absence for even one night is usually sufficient to lose one's spot in the shelter.

Within our sample, shelter usage is relatively rare. In the year before evaluation, only 3% had used the shelter system. In the month before, that figure is just 0.6254%. So that we do not conflate inflows to shelters of people who previously had non-shelter housing and outflows from shelters of individuals who had previously been living in shelters, we evaluate the impact of hospitalization on shelter usage among those who had not stayed in a shelter in the prior year, and separately for those who had been staying in a shelter. We use data from the Allegheny County's shelter system, which documents on a daily basis who is using services.

Involuntary hospitalization increases shelter usage. In the month following a hospitalization, shelter usage increases by 1.522 percentage points among those who had never used shelters before (Table 8C). Limiting to those who neither died nor were incarcerated, the results are virtually unchanged. The elevated shelter usage is statistically significant in the second month as well, and remains elevated but not statistically significant in the following months. There's no statistically significant change among prior shelter users, likely due to power issues arising from small sample size.

One of the mechanisms proffered by psychiatrists we have spoken to is that an irregular circadian rhythm is not conducive to well-being. Hospitalization disrupts supports that impose regularity such as employment and a place to sleep. Moreover, when a person is hospitalized, their daily routines and structures are, *perforce*, set by hospital staff. When to eat, when to sleep is generally dictated. When this rhythm is removed, it may be difficult to return to one's own older, disrupted routines. While we do not have detailed data on circadian rhythms, we consider both shelter and employment as two possible sources of daily routines. We cannot prove that the impact of either operates through the circadian rhythm, but we found the psychiatrists' hypothesis a useful starting point for understanding the possible mechanisms.

6.3 Disrupted Trust or Evaluation Process

We explore whether the evaluation process itself may impact subsequent behavior. The hypothesis is that being brought to the hospital emergency department—possibly by police—might already destabilize a person. A related hypothesis is that the evaluation may cause a rift between the individual and people who would otherwise provide social support such as family, friends, and school members.

To examine this possibility, we leverage the fact that the government delegate who chooses whether to certify that a person ought to be brought in for an evaluation is randomly assigned (see Section 2 for a description of the process). Among the 27 delegates who have heard more than 100 such calls, there is substantial heterogeneity in their tendency to certify a person for evaluation, with some certifying 73% of calls, and others certifying 95% (Figure B.2). As such, we use a nearly identical empirical approach as described in Section 4 to estimate the causal effects of experiencing an evaluation, consistent with the literature on multiphase system (Baron et al., 2024). The assumptions of relevance, independence, monotonicity, and exclusion, which are necessary for the validity of this approach, are shown in Appendix B. This instrument passes the same tests applied to our main approach in Section 4.2.[18]

We find no significant impact of being evaluated on subsequently being charged with a violent offense or dying from suicide or overdose. While there is a strong, positive first stage, we find no statistically significant effects on our main outcomes (Table A.4). This suggests that being evaluated—while it may be unpleasant or even traumatic—is likely not responsible for the adverse outcomes we measure.

Notably, these estimates apply only to people referred by family, friends, or school personnel. This is because physicians and police may self-certify, deciding to bring an individual in for evaluation without discussing first with a delegate. However, heterogeneity analyses find similarly adverse effects among referrers who are family, police, physicians or other individuals (Figure A.6). We do not have strong evidence that the effects occur only for those referred by family or friends, which would be an implication of this particular mechanism.

Second, the compliers in this setting—those whose evaluation might be certified by some delegates but not by others—are likely a different group of people from the compliers in our main estimates—

[18]Importantly, we find no association between the delegate's tendency to certify and the physician's tendency to hospitalize (Table B.5). Likewise, there is no association between a delegate's tendency to certify and the likelihood of treatment, conditional on being evaluated.

who may be hospitalized by some physicians but not others. It would be reasonable to assume that most of the people for whom hospitalization is a physician's judgment call are more likely to be certified for evaluation by delegates. If evaluations are meaningfully more traumatic for those who would always be certified, this analysis may miss some of the effect.

Nevertheless, we consider these results to suggest that evaluation itself is unlikely to be the main mechanism inducing the adverse impacts of involuntary hospitalization. We note this also because one concern is that when a physician decides *not* to involuntarily hospitalize an individual, it is possible that the physician gives referrals to other services or supports that may stabilize them, thereby treating with a different, less invasive treatment the "untreated" group. The null effect of evaluation suggests that this story is not the case. We further note that if this were the case, we might expect to see a significant negative coefficient on outpatient care in our main estimates.

6.4 Decreased Drug Tolerance

Another possible mechanism is that hospitalization decreases patients' tolerance for the illicit substances that they may be using. Then when they are discharged from the hospital, they may use their old dosage with deleterious effects.

While this hypothesis is a plausible explanation for the increase in deaths by overdose, decreased tolerance to a substance may not increase a person's proclivity toward violence. Even if cravings induce violence to obtain the drug, the connection between decreased drug tolerance and being charged with a violent crime is more tenuous.

Our main estimates in Figure 2 show that the LATE rises over the course of the first few months after an evaluation. If one thinks that immediately upon discharge, people are returning to illicit drugs with an inaccurate sense of their own tolerance, then the profile for the treatment effect would start high upon release and then perhaps taper off. Thus the shape that we find suggests either that diminished tolerance is not the explanation or that individuals who are hospitalized maintain sobriety for longer than just the time they are hospitalized before using drugs again.

Nevertheless, we consider whether we see more intense effects among those individuals with a diagnosis of substance use disorder than for those without such a diagnosis (Figure A.8). We find that the increase in violent charges is present for both those with and without a diagnosis of substance use disorder, consistent with substance tolerance being generally unrelated to violent charges. We

find a significant increase in suicide/overdose deaths for those *without* diagnoses and a positive point estimate that is not statistically significant for those with such a diagnosis.

We do a similar exercise, thinking about those who are treated with medication assisted treatment (MAT) for substance use at their evaluation (e.g., given methodone for opioid use). Though only 398 individuals receive MAT, we still find statistically significant effects for the remainder of the population in the six months following. The very noisy null is at best suggestive, but the fact that the effects persist even with this group removed suggests that those who are not receiving MAT drive our results.

Another approach is to consider heterogeneity based on how long a person was hospitalized. Unfortunately, our data do not document the date of release. Instead, we consider whether a person who experienced a hospitalization that was extended from 5 days to 20 or more days by the court sees different outcomes than an individual who stayed at most 5 days in the hospital. We find similar effects among both populations.[19]

Together, these analyses do not provide compelling evidence that a decrease in drug tolerance due to abstinence while hospitalized drives our results.

6.5 Continuing Care

Another possible mechanism for the adverse effects we measure is that hospitalization is not paired with continuing care among those who are hospitalized. Possibly, hospitals are not differentially connecting those who are involuntarily hospitalized to continuing care upon discharge or hospitalized patients are unwilling to accept these services, perhaps because of a negative experience with hospitalization.

To test this, we explore whether involuntary hospitalization increases utilization of three types of continuing care. Outpatient care is available for anyone with mental health needs, though observable in our data only for those on Medicaid. Case management, in which a care coordinator helps navigate the medical system and ensure the patient gets connected with the care they need is available for those with a severe mental illness diagnosis and is, likewise, observable only among those who are on Medicaid. Additionally, the community treatment team (CTT) is the local implementation of the ACT model that provides outreach and community-based mental health services to individuals with

[19] The similarity of our results based on duration of hospitalization is consistent with the fact that even when a stay is *allowed* to be longer than 5 days by the CCP team, it does not automatically mean that it will be the full 20 days. It is also consistent with 5-days potentially being a sufficient amount of time to lower a person's drug tolerance, depending on their prior usage.

serious and persistent mental illnesses. CTT provides services such as service coordinators, substance use treatment, and psychiatric and social work counseling three- to five times per week per patient. They serve about 500 people in the county and there are severe capacity constraints, with only about 25 people being enrolled in 2023. For CTT, we limit our observation to those with severe mental illnesses, who would be eligible for these programs.

We find no increase in any of the forms of continuing care (Table A.5) for those who are hospitalized. Notably, our data only show *utilization* of continuing care, meaning we are unable to distinguish between services that have been recommended but not used and services that have not been recommended. As such, we cannot pinpoint precisely where (in)action occurs.

For the subset of the population that has been prescribed antipsychotic medications and is on Medicaid (9.7% of our total sample), we can see whether involuntary hospitalization increases adherence to the prescribed medications. We compare the share of the days for which a person has filled the prescription for their medication to the total days for which they had a prescription. This is an imperfect measure of adherence since we cannot measure actual ingestion of the medications. Nevertheless, adherence is low, with only 25.9% the prescribed days having pills been filled in the month before evaluation. We find no increase in prescriptions for antipsychotics, nor in adhering to said prescription (Table A.6)

7 Effects For Non-Marginal Cases

Our core identification strategy identifies the impact of involuntary hospitalization for individuals who would be hospitalized by some physicians but not by other physicians. This local average treatment effect may or may not reflect the average treatment effect of involuntary hospitalization for all evaluated individuals. To more thoroughly understand the broader set of effects of involuntary hospitalization, we consider both the ordinary least squares (OLS) estimates and the marginal treatment effects (Heckman and Vytlacil, 2005).

OLS estimates can be driven by omitted factors that may be correlated with both involuntary hospitalization and unobservable determinants of subsequent violent charges or death by suicide or overdose. Thus they are useful for giving context, but not for identifying causal effects. The OLS estimates point in a similar direction to the local average treatment effect, but are not statistically

significant (Figure A.9).

To attempt to understand the heterogeneous treatment effects across unobservable characteristics that might bias OLS results, we consider marginal treatment effects (Heckman and Vytlacil, 2005).

The MTE estimates help highlight how involuntary hospitalization impacts different people, as we consider those individuals who would be on the cusp of hospitalization as we move from physicians more likely to hospitalize to physicians who are less likely to hospitalize. This helps shed light on whether our local average treatment effects are likely to apply to those whom more or fewer physicians would tend to hospitalize.

MTE estimates may only be calculated where there are physicians who both hospitalize and do not hospitalize individuals, what is referred to as the area of common support. In our setting, this ranges from hospitalizing 67% to 86% of those evaluated, when trimming 5% off of each side of the distribution (Figure A.10A).

To calculate the MTE function, we first predict the probability of hospitalization with physician tendency to hospitalize as the only explanatory variable, in line with the literature (e.g., Doyle, 2007; Arnold et al., 2022). Next, we predict the relationship between our two main outcomes and the predicted probability of involuntary hospitalization using a local quadratic estimator with a bandwidth of 0.08 (Heckman et al., 2006).[20] We calculate the numerical first derivative of the local quadratic estimator. Standard errors are computed from the standard deviation of 250 bootstrapped iterations of the MTE procedure, each of which cluster resamples at the rotation level.[21]

Figure A.10B shows the treatment effects for people just on the margin of hospitalization as we move from individuals who are unobservably more likely to be hospitalized (left side of the graph) to those who are unobservably more likely to be hospitalized (right side of the graph). The figure shows an MTE function for being charged with a violent crime that is decreasing as the physician's predicted probability of hospitalization increases (and therefore the unobservable likelihood of being hospitalized in the first place decreases) and an MTE function for death by suicide or overdose that is goes in the opposite direction.

This suggests that for individuals who would otherwise be least likely to be hospitalized (that

[20] A larger bandwidth will yield flatter MTE estimates. We opt for the minimum bandwidth, when minimizing the sum of squared errors between the local quadratic estimator and a fourth-degree polynomial model for each outcome. Higher and lower polynomials and both larger and smaller bandwidths yield similar results.

[21] MTEs requires strong assumptions of pairwise monotonicity and separability between observed and unobserved heterogeneity in treatment effects in order to be identified over the common support of the propensity score. These assumptions have been the subject of scrutiny (**???**). As such, MTE estimates should be interpreted with caution.

is, the ones who are on the margin of hospitalization for the physicians that hospitalize the most individuals), involuntary hospitalization has the most extreme increase in likelihood of death by suicide or overdose. But these same individuals have the largest decrease in likelihood of being charged with a violent offense. That suggests that in some sense, those who are the "least deserving" of involuntary hospitalization see the biggest increase in death by suicide or overdose and also a decrease in being charged with a violent crime.

The fact the two MTE curves go in the opposite directions imply that a prescription to simply hospitalize more or fewer people may have opposite effects on our two outcomes of interest.

8 Conclusion

Involuntary hospitalization is a last-resort approach for handling individuals experiencing a mental crisis. Its use is rather common and present policy initiatives from local leaders suggest it is likely to expand. Given this policy context, it is important to understand the causal impact of involuntary hospitalization on its statutory mandate: to reduce an individual's likelihood of being a danger to themselves or others.

Our paper finds that for individuals on the margin, for whom the physician evaluating them influences whether or not they are hospitalized, involuntary hospitalization is not reducing danger. On the contrary, we find that it increases the risk of being charged with a violent crime in the months following evaluation and increases the risk of dying by suicide or overdose. We find evidence that hospitalization disrupts housing and increases homeless shelter usage, decreases employment and earnings, and see no evidence that hospitalization increases an individual's likelihood of being connected to continuing care three months after the evaluation. Taken together, while involuntary hospitalization may stabilize an individual in the moment, it comes with significant disruption costs that play out over time and does not generate improved connections to care upon release.

Our paper leaves several avenues open for additional research. First, we have estimated the LATE — the impact of involuntary hospitalization on individuals for whom physician assignment impacts whether or not they are hospitalized. Two other measures might be useful to estimate. The impact of hospitalization on those whom physicians agree should be hospitalized — the average treatment on the treated — would provide insight about the effects of the program on those who are

currently hospitalized, giving an evaluation of the present implementation in our sample. The impact of hospitalization on those who are evaluated but physicians agree should not be hospitalized — the average treatment on the untreated — would provide insight about massive expansions of involuntary hospitalizations. These two measures are often recovered by estimating marginal treatment effects at different levels of treatment (Heckman and Vytlacil, 2005; Heckman et al., 2006). However, given our sample size, we are largely under-powered to do this analysis without strong assumptions.

Second, we do not tackle the general equilibrium effects of whether more or less stringent policies on involuntary hospitalization would impact trust in the medical system among those who would be hospitalized and those who would refer them. We also leave the patient- and family-rights debate to ethicists and policymakers.

Despite the areas not fully tackled in this paper, the results we present have important policy implications. As policymakers consider expanding involuntary hospitalization, our local average treatment effects provide insight as to what would happen if those making the decision to hospitalize were slightly more expansive in who is hospitalized. The results in this paper suggest that expanding involuntary hospitalization in our setting is unlikely to reduce danger to others or to the patients themselves.

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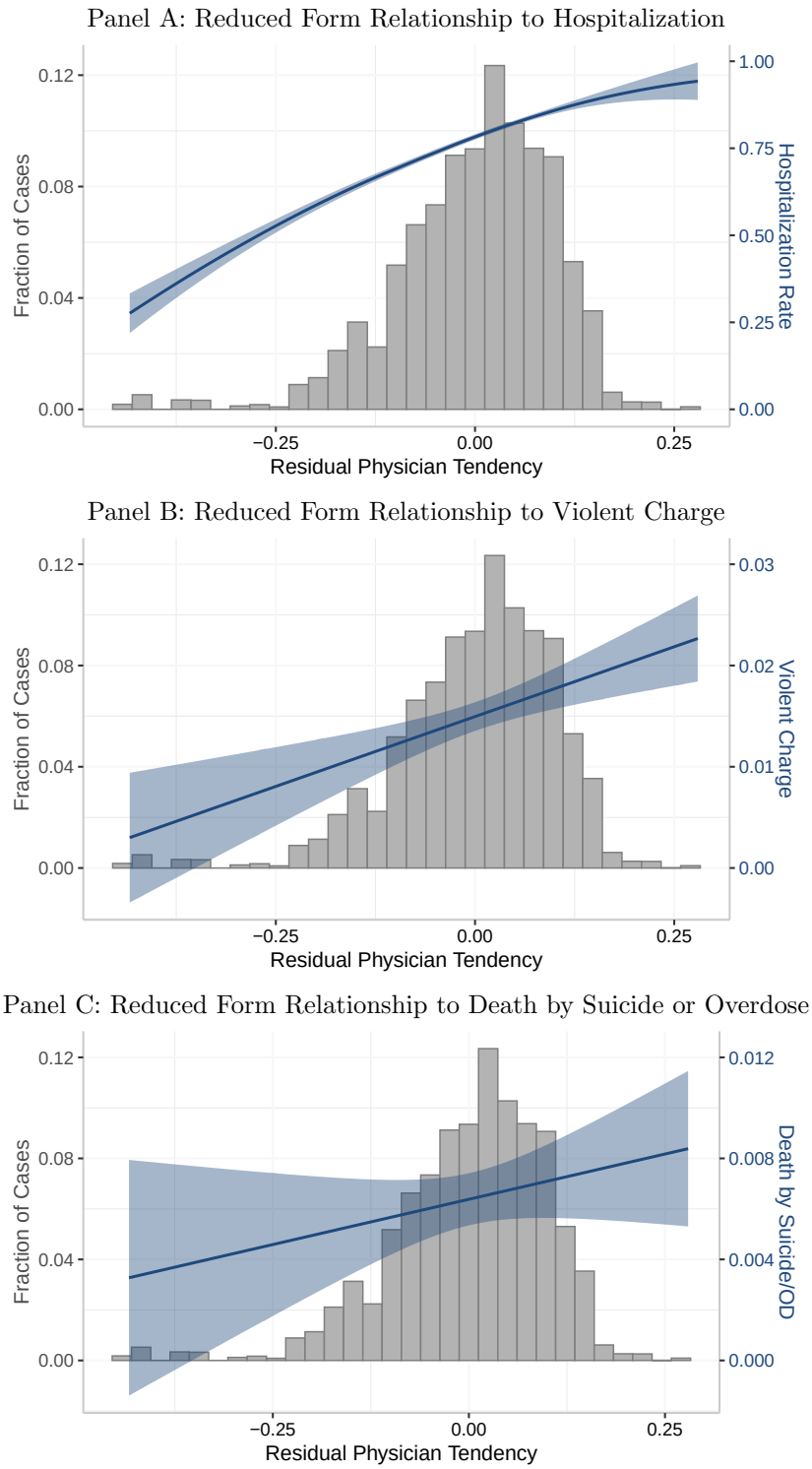
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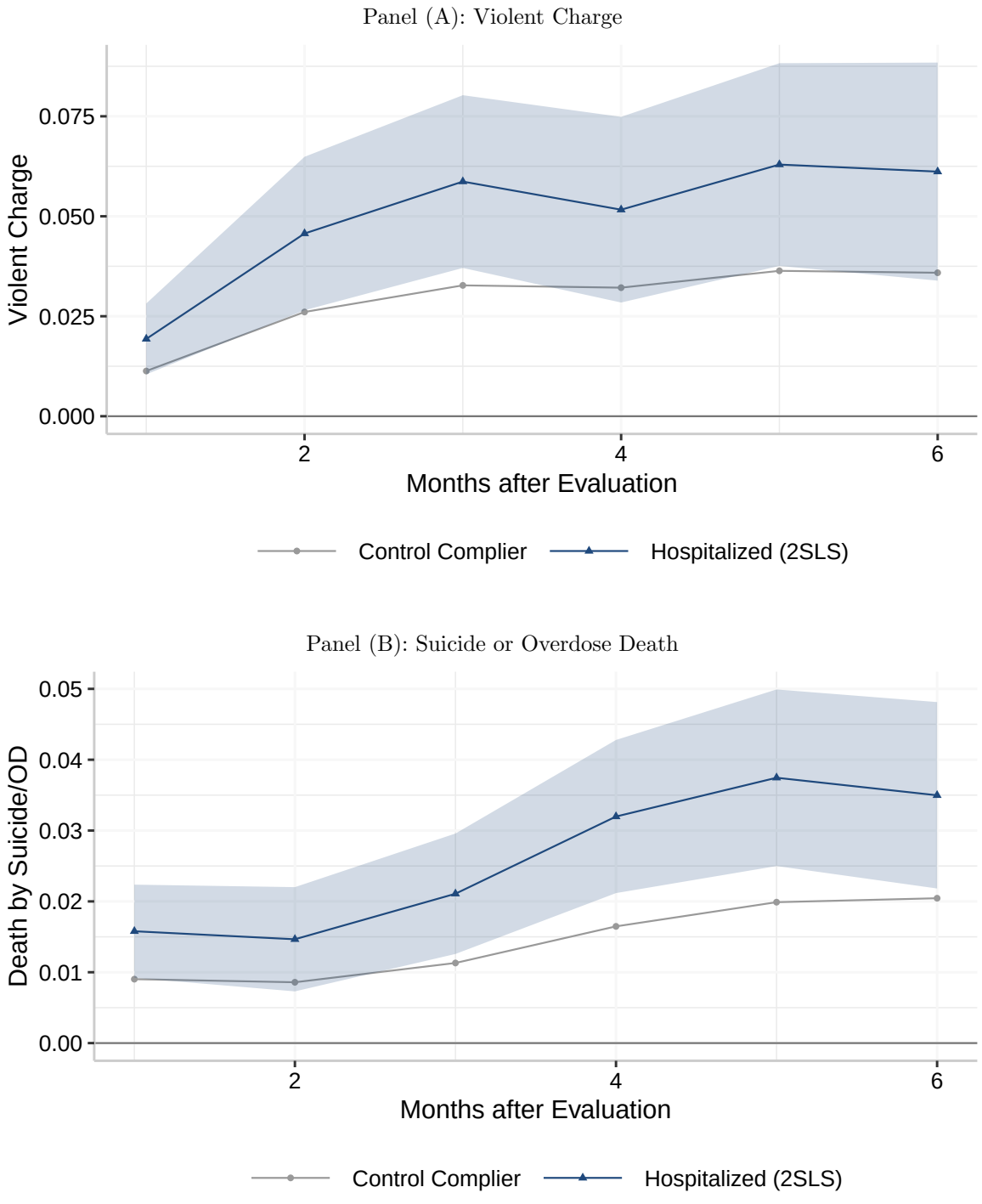
9 Figures and Tables

Figure 1: Distribution of Physician Tendency Instrument



Note: This figure shows the distribution of the physician tendency to hospitalize in involuntary hospitalization evaluations instrument. The instrument construction is described in Section 4. The blue line shows the relationship between the instrument and hospitalization in the first stage (top), a criminal charge in the reduced form (middle) and suicide or overdose death in the reduced form (bottom).

Figure 2: Local Average Treatment Effects



Note: This figure shows the local average treatment effect of hospitalization on violent charges in Panel (A) and suicide or overdose deaths in Panel (B) for those individuals for whom the physician who evaluates them may change whether or not they are hospitalized. The ribbons indicate 95-percent confidence intervals based on standard errors clustered at the physician level. Table 7 shows the numerical version of these estimates.

Table 1: Summary Statistics

	All	Hospitalized	Not Hospitalized	Compliers
(A) Demographics				
Female	0.447	0.442	0.463	0.342*
Age 18-35	0.508	0.497	0.543	0.468
White	0.660	0.664	0.644	0.733*
Black	0.273	0.267	0.294	0.23
Some High School	0.532	0.527	0.551	0.545
Enrolled in Medicaid	0.241	0.237	0.256	0.348**
(B) Past Diagnoses (Medicaid & County-Paid Population)				
Serious Mental Illness	0.519	0.561	0.382	0.347**
Mental Health	0.658	0.656	0.664	0.447**
Substance Use Disorder	0.215	0.207	0.240	0.168**
(C) Prior Encounters				
Prior Criminal Charge	0.223	0.215	0.252	0.338**
Prior Noncriminal Charge	0.096	0.092	0.109	0.146**
Prior ED Visit	0.302	0.292	0.337	0.506***
(D) 302 Evaluation Traits				
Reporter				
Relative	0.488	0.504	0.435	0.633**
Police Officer	0.225	0.196	0.324	0.259
Other Reporter	0.287	0.300	0.241	0.108*
Concern				
Attempted Suicide	0.510	0.484	0.598	0.586
Unable to Care for Self	0.441	0.465	0.356	0.38
Mutilated Self	0.063	0.059	0.075	0.066
Danger to Others	0.472	0.475	0.459	0.532
Observations	16,630	12,909	3,721	[0.43]

Note: This table describes our study sample of individuals evaluated for involuntary hospitalization in Allegheny County for 2014-2023. Information on past diagnoses is only available for the individuals who are enrolled in Medicaid or have some county-paid claims. The first column contains information on the whole sample. The middle two columns show the population based on whether they are hospitalized or not. The final column shows the observable traits for those who are compliers—those individuals for whom the evaluating physician changes whether or not they would be hospitalized. The final row shows the number of evaluations in the whole sample, evaluations among those hospitalized and not hospitalized, and in square brackets the share of compliers calculated. See the Section 4.1 for additional details on the sample and variable construction. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 2: LATE First Stage: Physician tendency and Hospitalization

	Hospitalized						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Physician Propensity	0.92***	0.88***	0.83***	0.83***	0.83***	0.83***	0.79***
	(0.03)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.12)
Hospital FEs		✓					
Rotation FEs			✓	✓	✓	✓	✓
Demographic Controls				✓	✓	✓	✓
History Controls					✓	✓	✓
Concern Controls						✓	✓
Medical Controls							✓
Mean Hospitalization	0.776	0.776	0.776	0.776	0.776	0.776	0.776
Effective F	158.11	120.23	109.19	107.08	104.83	106.17	45.29
Observations	16,630	16,630	16,630	16,630	16,630	16,630	4,009

Note: This table shows first stage results, documenting the relationship between our residualized physician tendency to hospitalize a patient instrument and their likelihood of hospitalization. The regressions are estimated in the our main sample, using an instrument constructed as described in Section 4. Each column adds successive control variables. Demographic controls include sex, age, racial category, education and medicaid enrollment. History controls include prior jail stay, prior use of a shelter, prior violent and criminal charges, prior emergency department use, prior use of community treatment teams, prior use of case management. Concern controls include indicators for whether the referring individual indicated that the individual had attempted suicide, posed a danger to others, had mutilated themselves or was unable to care for themselves. Medical controls, which are only available for those enrolled in Medicaid or who have had bills paid by the county include whether the individual has a severe mental illness, a mental illness, or a substance use diagnosis. Because the final set of controls is available only for a subset of cases, the penultimate specification is our preferred specification used throughout the rest of the paper. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 3: Randomization Test

	Hospitalized	Physician Propensity
F-Statistic	10.901	1.1916
p-value	0.0000	0.25
Observations	16,630	16,630

Note: This table relates observable characteristics to an individuals' likelihood of being hospitalized (Column 1) and to the assigned physician's tendency to hospitalize on a involuntary hospitalization evaluation (Column 2). The F-statistic jointly tests the significance of the control variables and the p-value of the joint test is reported below. Standard errors are clustered at the physician level. For the sake of space, the coefficients for Column 2 are shown in Figure A.4, Panel A.

Table 4: First Stage by Demographic Characteristic

	Hospitalized							
	Female	White	Some HS	< 35 Years	Medicaid	SMI	MH	DA
Physician Propensity	0.81***	0.83***	0.85***	0.81***	0.95***	0.80***	0.96***	1.09***
	(0.10)	(0.08)	(0.10)	(0.09)	(0.12)	(0.10)	(0.10)	(0.16)
Rotation Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	7,426	10,972	8,855	8,049	4,914	3,843	5,438	1,624

Note: This table presents the first stage relating a physician's tendency to hospitalize a patient on their hospitalization. The final columns, subsetting to individuals who have a diagnosis of severe mental illness (SMI), of a mental illness (MH) or of substance use disorder (DA), are available only for individuals who are enrolled in medicaid or have had a County-paid medical claim. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 5: First Stage by Involuntary Hospitalization Evaluation Concern

	Hospitalized			
	Unable to Care	Danger to Others	Suicide Att	Mutilated Self
Physician Propensity	0.68***	0.79***	0.89***	0.94***
	(0.06)	(0.07)	(0.12)	(0.27)
Rotation Fixed Effects	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Observations	7,331	7,843	8,473	1,042

Note: This table presents the first stage relating a physician's tendency to hospitalize a patient on their hospitalization. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 6: Physician tendency and Downstream Events

	Physician Propensity	
CCP Hearing	0.003	−0.001
	(0.004)	(0.009)
CCP Extension	−0.004	0.008
	(0.003)	(0.010)
Inpatient Visit		0.001
		(0.008)
F-stat from joint test	1.0834	0.6002
p-val from joint test	0.3385	0.615
Observations	12,875	1,989

Note: This table relates considers the relationship between a physician's tendency to hospitalize individuals and the downstream events among those who are hospitalized. Conditional upon hosptialization, the physician's tendency is unrelated to whether an individual has a hearing with the Court of Common Pleas (CCP) or whether the magistrate extends their stay. Likewise for those enrolled in Medicaid, it is unrelated to whether there is a subsequent inpatient visit. The F-statistic and p-value from a test of joint significance is shown at the bottom. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 7: Impacts of Involuntary Hospitalization on Violent Charges and Death by Suicide or Overdose

	First Stage	Violent Charge		Suicide/OD Death	
		RF	IV	RF	IV
Hospitalized	0.826***	0.021**		0.008**	
	(0.080)	(0.008)		(0.004)	
Physician Propensity			0.026**		0.010**
			(0.011)		(0.004)
Dependant Mean	0.7762	0.0095	0.0327	0.0038	0.0092

Note: This table presents the first stage, reduced form (RF), and instrumental variables (IV) estimates for the impact of involuntary hospitalization on 3-month outcomes that proxy for being a danger to others or to oneself. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table 8: Impacts of Involuntary Hospitalization on Earnings, Employment and Shelter Usage

A: Earnings

	0Q	1Q	2Q	3Q	4Q
Hospitalized	−344.00	−1,040.00**	−807.10*	−821.80*	−468.80
	(312.90)	(467.30)	(467.70)	(456.10)	(520.20)
Control Complier Mean	1,662	1,163	1,177	1,389	1,372
Observations	7,683	7,222	6,941	6,707	6,436

B: Employment

	0Q	1Q	2Q	3Q	4Q
Hospitalized	0.041	−0.062*	−0.072*	−0.018	0.009
	(0.034)	(0.036)	(0.039)	(0.049)	(0.041)
Control Complier Mean	0.07	0.016	0.046	0.027	0.044
Observations	7,683	7,222	6,941	6,707	6,436

C: Shelter Usage Among Non-Users

	1 Mo	2 Mo	3 Mo	4 Mo	5 Mo	6 Mo
Hospitalized	0.015*	0.016*	0.015	0.014	0.014	0.009
	(0.008)	(0.009)	(0.011)	(0.014)	(0.014)	(0.014)
Control Complier Mean	0.0124	0.0211	0.0245	0.0262	0.0286	0.0253
Observations	16,234	16,116	16,006	15,891	15,769	15,695

D: Shelter Usage Among Prior Users

	1 Mo	2 Mo	3 Mo	4 Mo	5 Mo	6 Mo
Hospitalized	0.318	0.063	0.087	−0.248	−0.001	−0.166
	(0.473)	(0.666)	(0.693)	(0.714)	(0.654)	(0.652)
Control Complier Mean	0.8944	0.8192	0.3129	0.1345	0.2999	0.3681
Observations	307	306	305	303	303	301

Note: This tables shows the local average treatment effect of involuntary hospitalization on earnings and employment among and shelter usage, from estimating equation 3. Panels A and B are limited to those individuals for whom Allegheny County has data from the Pennsylvania Department of Labor and Industry. Regressions include the usual controls as well as earnings or employers over the quarters prior to evaluation, respectively. Panels C and D contain the whole sample, and divide it among people who have not used a shelter in the year prior to the evaluation and those who have. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Appendix to

"A Danger to Self and Others" Involuntary Hospitalization's Health and Criminal Consequences

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A Appendix: Additional Tables and Figures

Table A.1: Sample Restrictions

Restriction	Evaluations	People	Physicians	Hospitals
All Cases	45,085	27,689	2,168	18
Limit to first case	27,689	27,689	1,810	18
Exclude voluntary hospitalizations	26,623	26,623	1,783	18
Limit to MDs with ≥ 15 cases	22,978	22,978	427	18
Limit to patients with known sex	22,978	22,978	427	18
Limit to patients over age 18	20,985	20,985	427	18
Limit to age < 65	17,226	17,226	426	18
Limit to hospitals that see > 40 people	17,151	17,151	424	14
Limit to rotations with variation in cases/physicians	16,630	16,630	424	14

Note: This table shows the number of evaluations, individuals, physicians and hospitals in our sample at each stage of the sample construction process.

Table A.2: LATE Estimate Robustness

			Violent Charge		**Suicide/OD Death**	
		First St.	Red. Form	LATE	Red. Form	LATE
1	No Controls	0.829***	0.022**	0.027**	0.009**	0.011**
		(0.079)	(0.009)	(0.011)	(0.004)	(0.005)
2	6 months	0.826***	0.021**	0.025*	0.012**	0.015**
		(0.080)	(0.010)	(0.014)	(0.005)	(0.007)
3	12 months	0.826***	0.028**	0.033**	0.020**	0.024**
		(0.080)	(0.012)	(0.015)	(0.008)	(0.010)
4	>5 Cases	0.732***	0.015**	0.020**	0.011**	0.015**
		(0.081)	(0.007)	(0.010)	(0.005)	(0.007)
5	>30 Cases	0.916***	0.021**	0.023**	0.011***	0.012**
		(0.071)	(0.010)	(0.011)	(0.004)	(0.005)
6	Clustered by Rotation	0.826***	0.021***	0.026***	0.008**	0.010**
		(0.040)	(0.007)	(0.009)	(0.004)	(0.005)
7	Clustered by Rotation-Physician	0.826***	0.021***	0.026**	0.008**	0.010**
		(0.078)	(0.008)	(0.010)	(0.003)	(0.004)
8	No Police	0.729***	0.033***	0.046***	0.008*	0.011*
		(0.073)	(0.010)	(0.016)	(0.004)	(0.006)
9	No Physician	0.954***	0.022**	0.024**	0.009*	0.009*
		(0.101)	(0.010)	(0.011)	(0.004)	(0.005)
10	Inpatient	0.819***	0.019**	0.023**	0.008**	0.010*
		(0.077)	(0.009)	(0.012)	(0.004)	(0.005)
11	Allow Incapacitation	0.826***	0.021**	0.026**	0.006	0.007
		(0.080)	(0.008)	(0.011)	(0.004)	(0.004)
12	Balanced Panel	0.828***	0.022***	0.026**	0.008**	0.010**
		(0.082)	(0.008)	(0.011)	(0.004)	(0.004)
13	Split Sample Instrument	0.587***	0.012*	0.020*	0.009**	0.015**
		(0.081)	(0.007)	(0.011)	(0.004)	(0.006)
14	Physician FE Instrument	0.826***	0.021**	0.017**	0.008**	0.005
		(0.080)	(0.008)	(0.007)	(0.004)	(0.004)
15	Split Danger to Others Instrument	0.591***	0.020***	0.034**	0.009**	0.016**
		(0.093)	(0.007)	(0.014)	(0.004)	(0.007)
16	Split Suicide Att Instrument	0.620***	0.017**	0.028**	0.010***	0.015**
		(0.088)	(0.007)	(0.012)	(0.004)	(0.006)
	Control Mean	0.776	0.01	[0.033]	0.004	[0.009]

Note: This table presents robustness of the estimates shown in Table 7. Each row shows the estimates where one of our design decisions is changed slightly. The first shows our specification with no controls. Rows 2 and 3 show the estimates when measuring the outcome at 6- and 12-months after the evaluation. Rows 4 and 5 show the estimates when including physicians who have seen more than five evaluations and limiting to physicians who have seen more than 30 evaluations. Rows 6 and 7 consider calculate standard errors clustered by rotation and two-way clustered standard errors by rotation and physician, respectively. Rows 8 and 9 show when evaluations referred by police and physicians are excluded, respectively. Row 10 limits based on inpatient visits. Row 11 considers when we include individuals who may have been incapacitated in our sample. Row 12 considers a balanced panel. Row 13 considers a split-sample instrument in which physician leniency is calculated based on half the rotations and applied in the other half of the rotations. Row 14 uses physician fixed effects as the instrument. Rows 15 and 16 show instruments that are allowed to vary based on whether the concern of being a danger to others or having attempted suicide are raised, thereby relaxing monotonicity concerns. The bottom row shows the average hospitalization rate, the outcomes in the control population under the reduced form columns and the control complier means in square brackets. Where not otherwise specified, standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table A.3: Effects of Hospitalization on Subsequent Evaluations

	Subsequent Interaction	
	Hearing	Hospitalization
	(1)	(2)
Hospitalized	−0.013	−0.0003
	(0.031)	(0.031)
Control Complier Mean	0.1072	0.1029
Observations	16,617	16,617

Note: This table shows the local average treatment effect of involuntary hospitalization on subsequent evaluations for involuntary hospitalization. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table A.4: Impacts of Evaluation

	First Stage	Violent Charge		Suicide/OD Death	
		RF	IV	RF	IV
Delegate Propensity	0.969***	0.003		0.005	
	(0.085)	(0.029)		(0.014)	
Case Certified			0.003		0.005
			(0.030)		(0.014)
Control Complier Mean	0.841	0.014		0.004	

Note: This table presents the first stage, reduced form (RF), and instrumental variables (IV) estimates for the impact of an *evaluation* for involuntary hospitalization on 3-month outcomes that proxy for being a danger to others or to oneself. The estimate uses random assignment of delegates' proclivity to certify a case as described in Section 2 and analyzed in Section 6. Standard errors are clustered at the delegate level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table A.5: Local Average Treatment Effects of Hospitalization on Continuing Care

	Outpatient	Case Management	Community Treatment Team
	(1)	(2)	(3)
Hospitalized	−0.033	0.007	0.007
	(0.086)	(0.143)	(0.014)
Control Complier Mean	0.5485	0.2583	0.0108
Observations	4,880	2,771	3,773

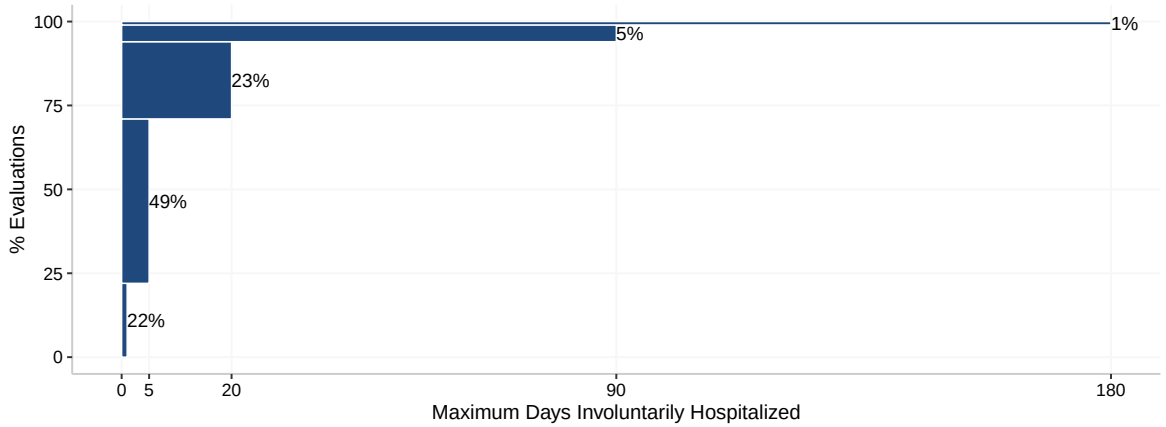
Note: This tables shows the local average treatment effect of involuntary hospitalization on outpatient treatment, case management and participation in community treatment teams. The samples are limited to people on Medicaid, those with severe mental illnesses in the second two columns. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table A.6: Local Average Treatment Effects of Hospitalization on Adherence to Medication

	Share of Days Covered						
	Prescribed	1 Mo	2 Mo	3 Mo	4 Mo	5 Mo	6 Mo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hospitalized	0.026	0.009	−0.150	−0.348	−0.285	−0.329	−0.342
	(0.028)	(0.342)	(0.327)	(0.324)	(0.325)	(0.316)	(0.322)
Control Complier Mean	0.0811	0.352	0.2089	0.0238	0.0811	0.0523	0.0738
Observations	16,630	1,621	1,621	1,621	1,621	1,621	1,621

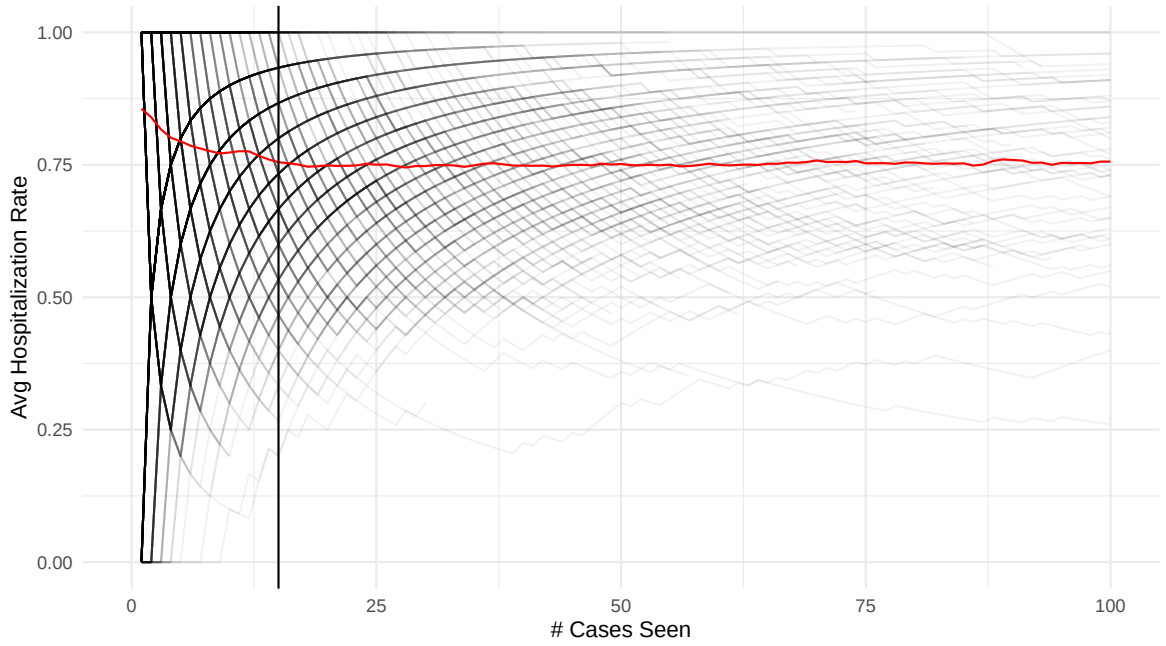
Note: This tables shows the local average treatment effect of involuntary hospitalization on having a prescription for an antipsychotic medication, and the share of the days of prescription in which a person has filled their prescription for the antipsychotic medications. The sample for columns 2-7 is limited to those who had a prescription for an antipsychotic. Standard errors are clustered at the physician level. *p< 0.1; **p< 0.05; ***p< 0.01.

Figure A.1: Share of Evaluations Hospitalized and For How Long



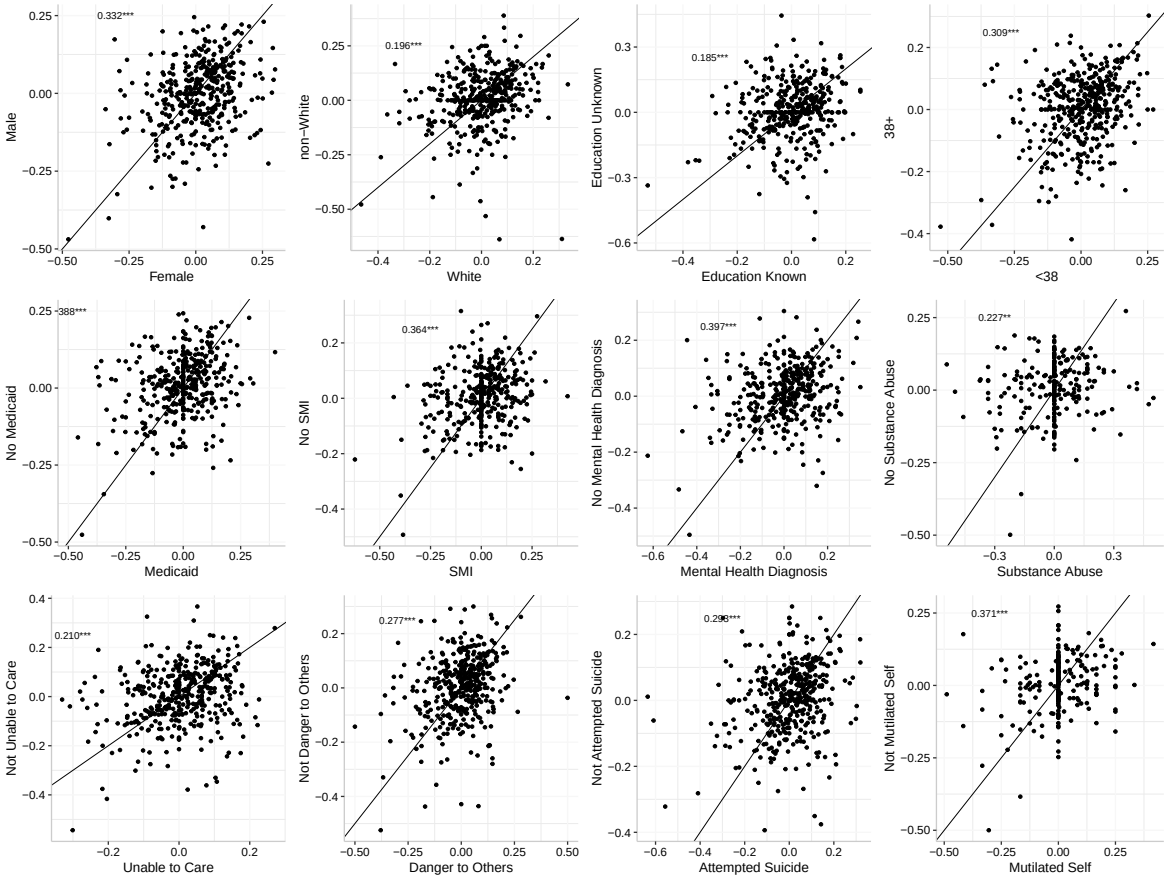
Note: This figure shows the share of evaluations and the maximum number of days that they are allowed to be involuntarily hospitalized.

Figure A.2: Hospitalization Stabilization



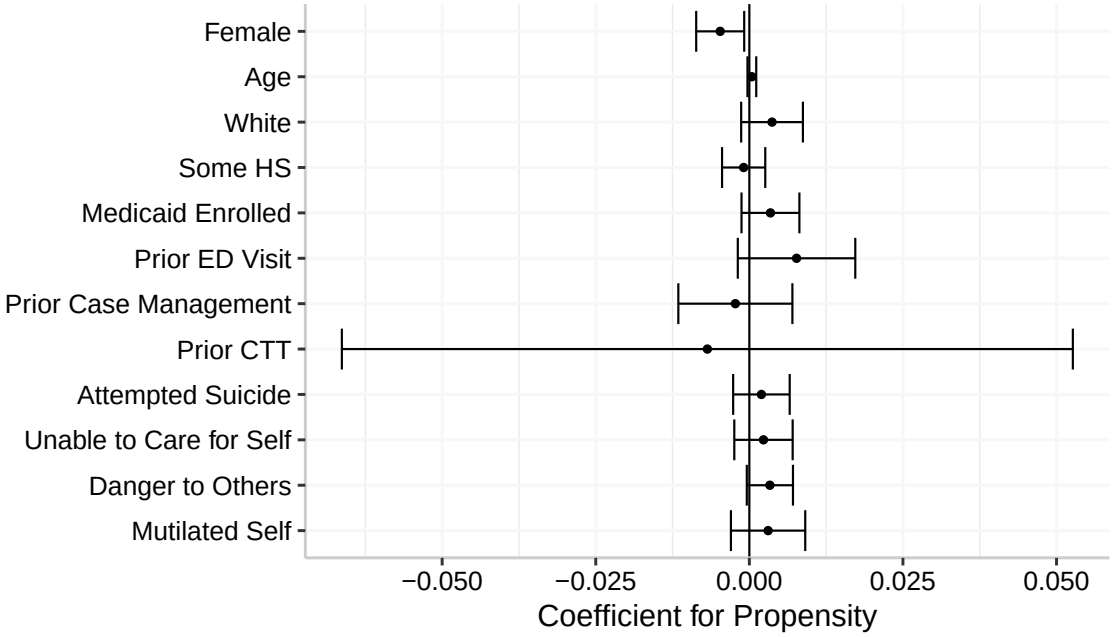
Note: This figure plots physicians' average hospitalization rate as a function of how many evaluations for involuntary hospitalization they have seen. The red line shows the average hospitalization rate. The vertical black line shows 15 cases, which is the number required in our main sample. Table A.2 shows our main estimates in samples that include physicians who have seen only 5 or more cases, and physicians who have seen 30 or more cases.

Figure A.3: Physician tendency Instrument by Case Characteristics



Note: This figure shows the correlation between a residualized measure of physician tendency to hospitalize for different groups of patients. Each point shows the average of the residualized tendency within each subgroup for each physician. The lines show the OLS relationship between physicians' tendencies to hospitalize within one group relative to the other group. The coefficient is noted in the top left of each graph. *p< 0.1; **p< 0.05; ***p< 0.01.

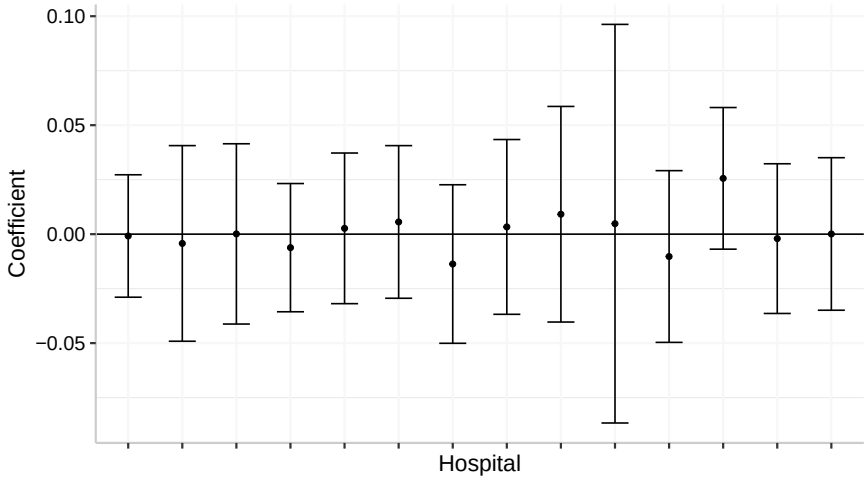
Figure A.4: Randomization Test: Observables' Relation to Physician tendency



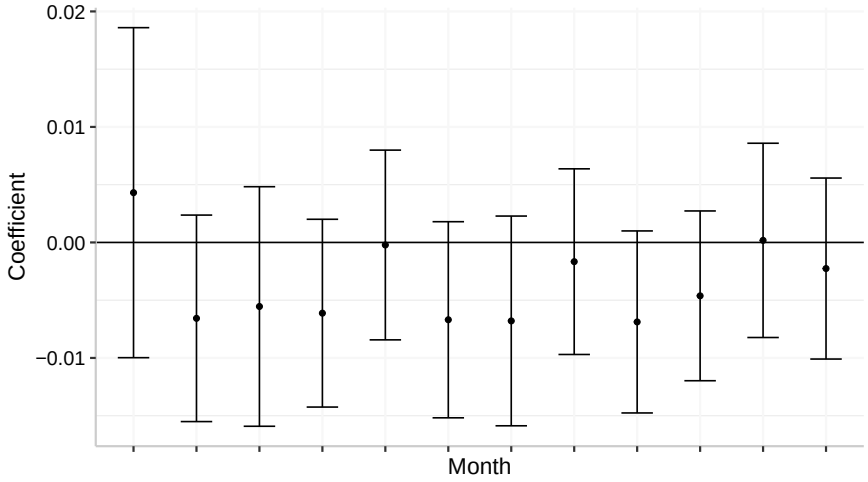
Note: This figure shows coefficients from Table 3, column 2, relating observable traits to our instrument for a physician's tendency to hospitalize a patient. The fact that nearly every predictor is not significant lends confidence that assignment to evaluating physician is indeed random. The coefficient on prior violent charge is mildly significant, which is not beyond the pale given the number of coefficients we include. All coefficients are from a single regression and standard errors are clustered at the physician level.

Figure A.5: Randomization Test: Hospital, Season and Shift

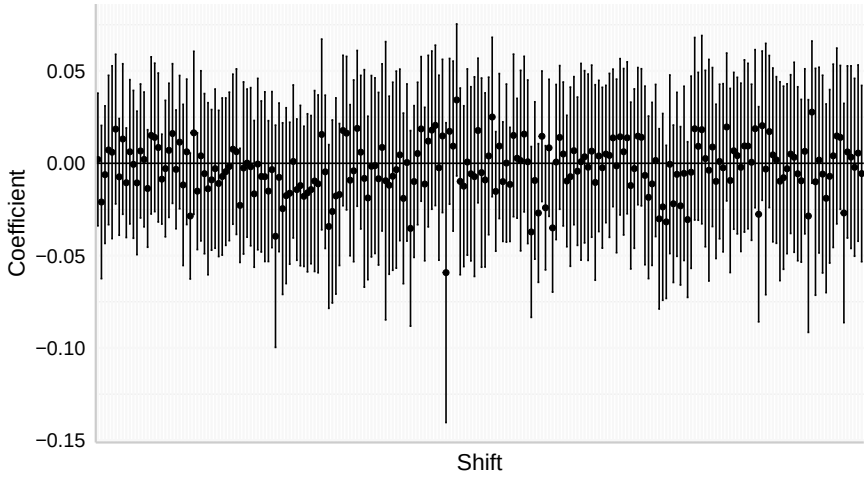
Panel (A): Hospitals and Physician tendency



Panel (B): Month and Physician tendency

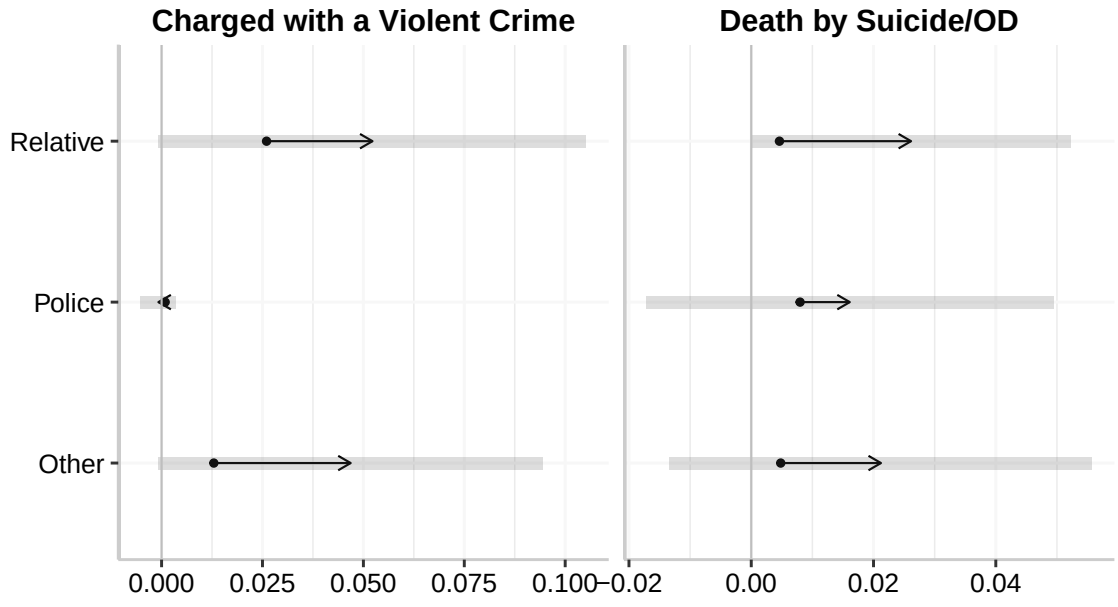


Panel (C): Rotation and Physician tendency



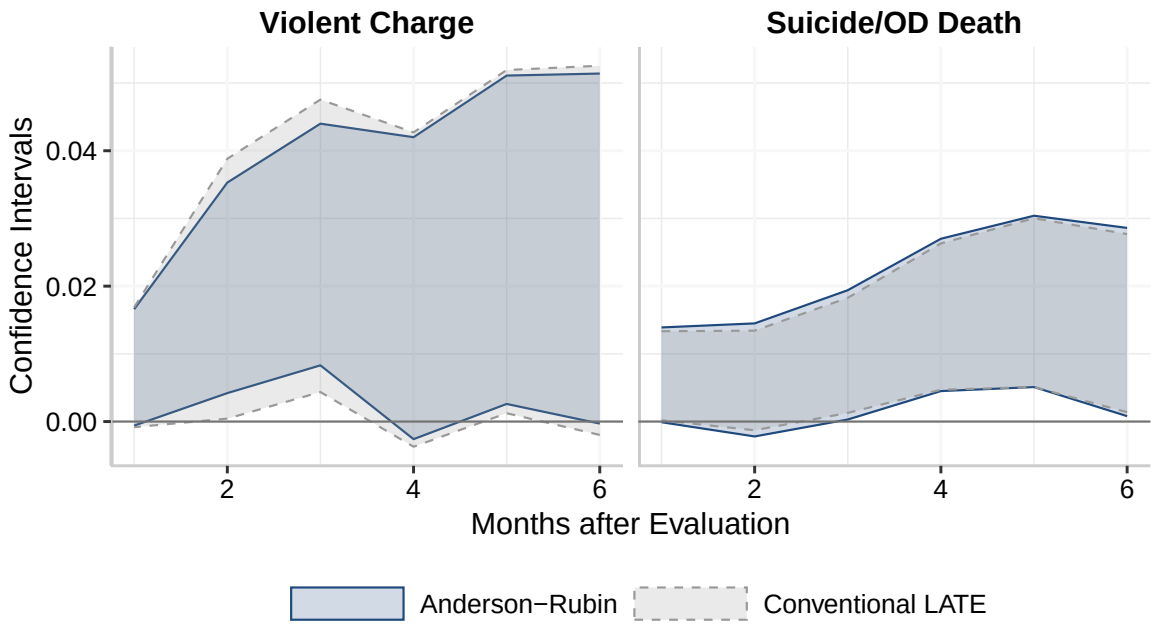
Note: This figure shows that neither the hospital, nor the month, nor the rotation is systematically related to the physician tendency. All coefficients in each panel are from a single regression and standard errors are clustered at the physician level.

Figure A.6: Heterogeneity by Referrer



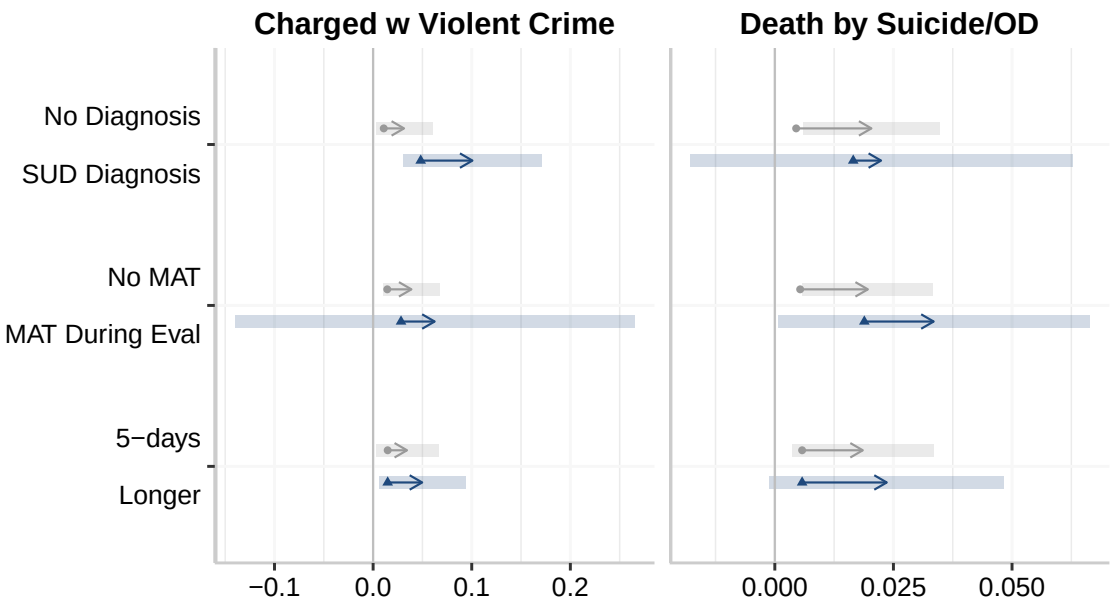
Note: This figure shows the point estimates for the effects of involuntary hospitalization based on whether the referral originated with a family member, police office or another party. Outcomes are measured 6 months after the evaluation. The dots show the control mean; the arrows the point estimate; the shaded region the confidence intervals. Each panel shows estimates from three different regressions.

Figure A.7: Anderson-Rubin Confidence Intervals



Note: This figure shows Anderson-Rubin confidence intervals in the months following an evaluation for both violent charges and for suicide or overdose deaths, in blue outlined with a solid line. These intervals are impervious to weak instruments. For comparison, we include conventional LATE confidence intervals as well, outlined in a grey dashed line. See Section 5.3 for more details.

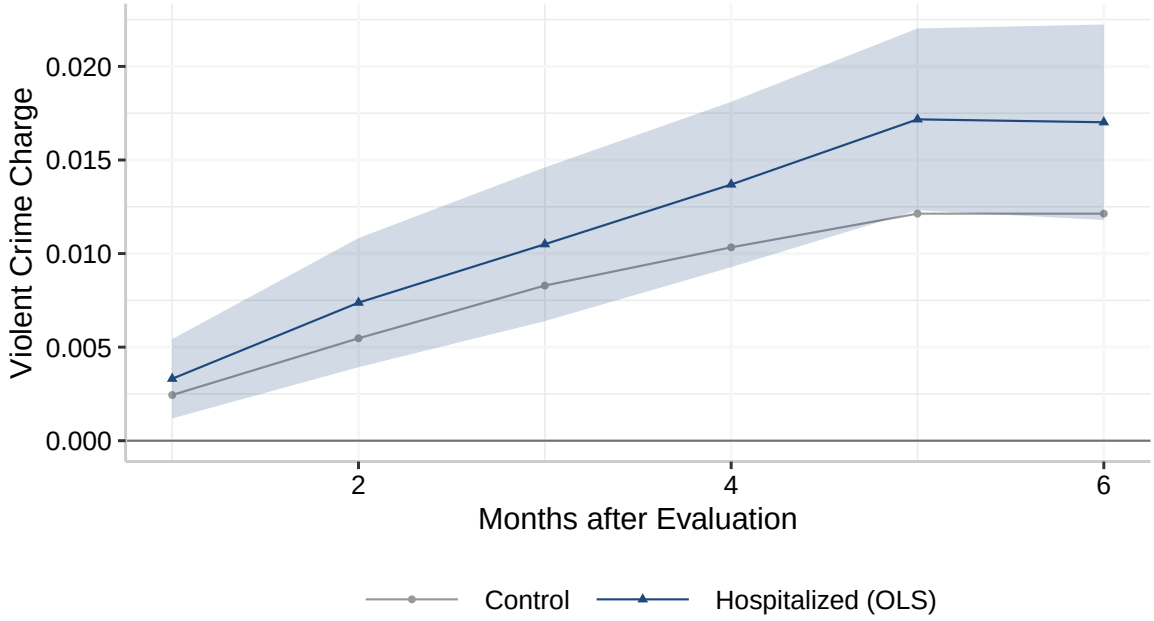
Figure A.8: Heterogeneity by Substance Use Tolerance



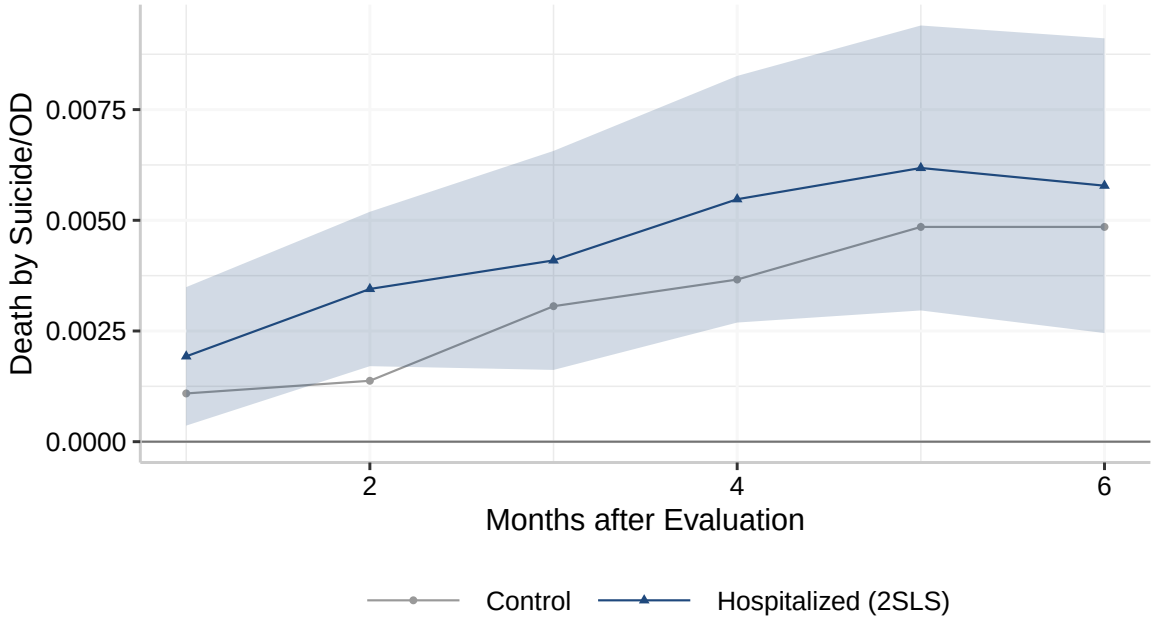
Note: This figure shows three heterogeneity analyses aimed at understanding if a decrease in substance use tolerance due to drug-abstinence while hospitalized drives our results. We plot the point estimates for the effects of involuntary hospitalization based on whether the patient has a substance use disorder (SUD) diagnosis or not, whether they received MAT treatment for opioid use during the evaluation and whether the hospitalization is at most 5-days or has been extended by the Court of Common Pleas (CCP). The dots show the control mean; the arrows the point estimate; the shaded region the confidence intervals. Each comparison is estimated from a single regression, meaning this figure shows 6 regressions. Outcomes are measured 6 months after the evaluation.

Figure A.9: Ordinary Least Squares

Panel A: OLS for Violent Charges



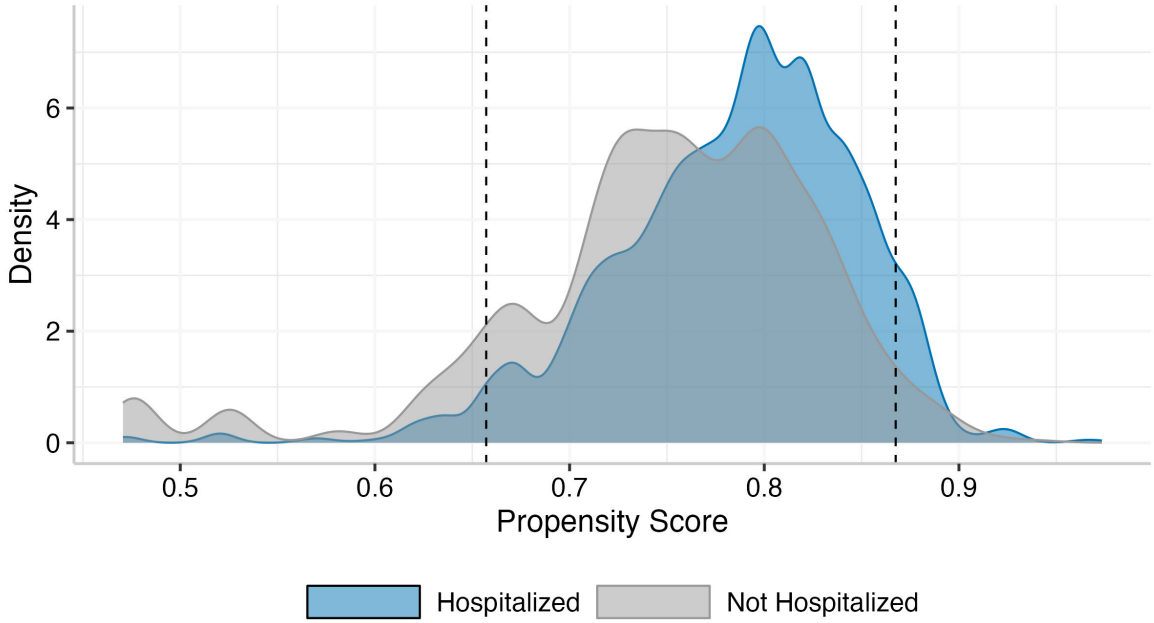
Panel B: OLS for Death by Suicide/Overdose



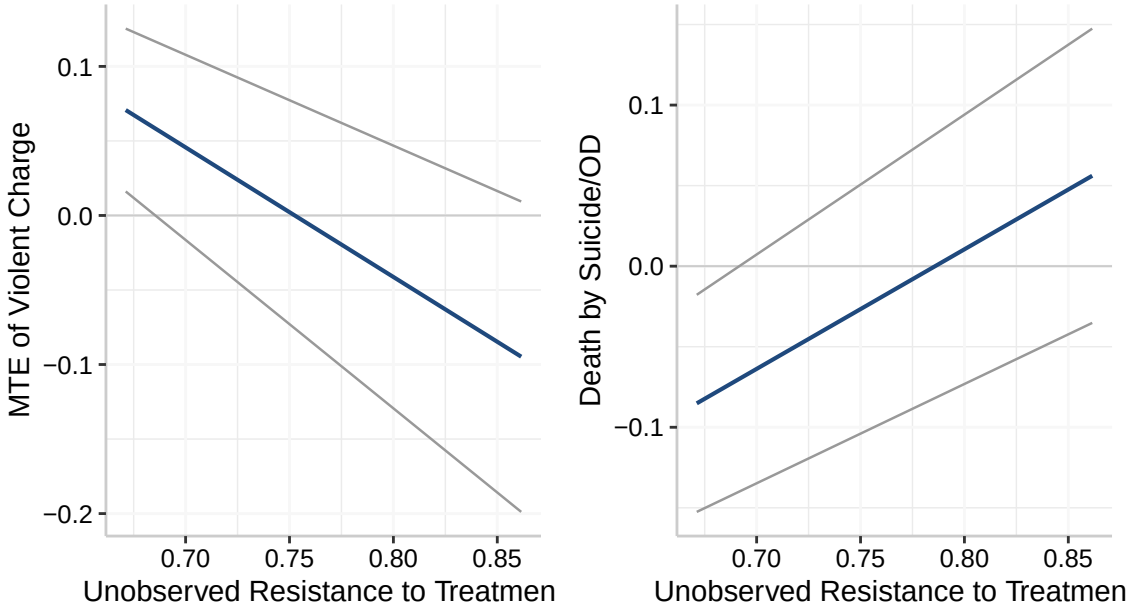
Note: This figure shows the ordinary least squares relationships between involuntary hospitalization and our two main outcomes. The grey line shows the control mean; the blue line shows the coefficient added to the control mean. The ribbon shows the 95% confidence interval. Point estimates show cumulative effects over time.

Figure A.10: Marginal Treatment Effects

Panel A: Area of Common Support



Panel B: Marginal Treatment Effects



Note: Panel A shows the area of common support between the treated and untreated individuals. Panel B shows marginal treatment effects for those who would be marginal among physicians who are most likely to hospitalize (on the left) toward those who are marginal among physicians who are least likely to hospitalize (on the right). Standard errors are based off of 250 bootstraps.

B Appendix: Causal Effects of Evaluation: Evidence from Delegate's tendency to Certify

This appendix presents additional information as well as tables and figures testing assumptions associated with the instrument validity of using call delegate to assess the causal impacts of being evaluated for involuntary hospitalization or not.

We note that unlike in our main analysis, it is possible to have defiers in this context. A case may be certified, but then the referrer may choose not to bring in the individual for an evaluation. We find 71cases in which the case was certified but no examination takes place, suggesting a very small share of certified cases are defiers. Note, that one may not bring a person for an evaluation without going through this process.

Sample: We construct the sample to include all calls answered by delegates who have handled 100 or more cases in the past, to reduce noise about delegate tendency to certify a case. As with our main analysis, we limit based on age, limiting to those over 18 and younger than age 65. While this is not necessary for the randomization of our cases (as in our main analysis), we do this to keep the sample under consideration here as similar to that in our main analysis as possible (Table B.1).

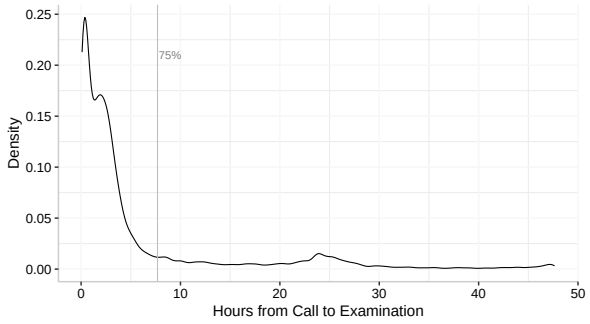
Table B.1: Delegate Sample Restrictions

Restriction	Evaluations	People	Delegates	Hospitals
All Calls	21,843	21,843	35	17
Limit to non-missing delegates	21,538	21,538	34	17
Limit to delegates with ≥ 100 cases	21,369	21,369	27	17
Limit to patients with known sex	21,369	21,369	27	17
Limit to patients over age 18	19,219	19,219	27	17
Limit to age < 65	15,200	15,200	27	17
Limit to rotations with variation in cases/delegates	14,112	14,112	27	17

Note: This table shows the number of evaluations, individuals, delegates and hospitals in our sample at each stage of the sample construction process.

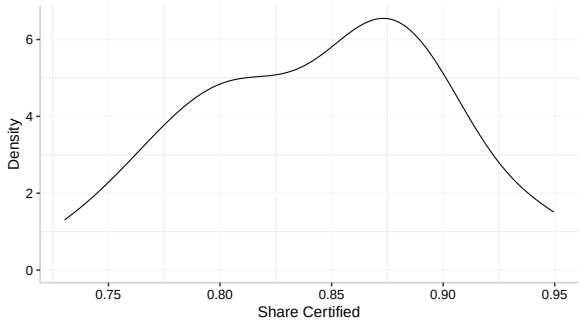
Table B.2 describes the sample as well as compliers. Compliers are less likely to be aged 18-35. They are more likely to be have a referral from a relative, less likely from a school or other reporter (e.g., friends). They are less likely to have been reported as having attempted suicide.

Figure B.1: Time between Call and Evaluation



Note: This figure shows the hours between a call to a government delegate and the evaluation in the hospital. While technically if a person's case has been certified, they may be brought in for evaluation any time in the next 30 days, the vast majority are evaluated much more rapidly.

Figure B.2: Distribution of Certification



Note: This figure shows the distribution in delegates' tendency to certify the cases that they hear.

Table B.2: Summary Statistics of Calls

	All	Evaluated	Not Evaluated	Compliers
(A) Demographics				
Female	0.441	0.440	0.448	0.404
Age 18-35	0.488	0.490	0.476	0.134***
White	0.664	0.669	0.640	0.759
Other Race	0.063	0.062	0.068	-0.01
Some High School	0.541	0.540	0.551	0.537
(D) 302 Evaluation Traits				
Reporter				
Relative	0.717	0.702	0.797	0.364***
School	0.075	0.076	0.066	0.323***
Other Reporter	0.276	0.290	0.202	0.682***
Concern				
Attempted Suicide	0.480	0.509	0.323	0.242**
Unable to Care for Self	0.472	0.462	0.526	0.608
Mutilated Self	0.056	0.061	0.028	0.027
Danger to Others	0.500	0.501	0.494	0.442
Observations	16,630	12,909	3,721	[0.146]

Note: This table describes our study sample of individuals for whom there has been a call in which a delegate can decide whether they should be evaluated for involuntary hospitalization in Allegheny County from 2014-2023. The first column contains information on the whole sample. The middle two columns show the population based on whether their case was certified or not. The final column shows the observable traits for those who are compliers—those individuals for whom the evaluating delegate changes whether or not their case is certified and they are evaluated for involuntary hospitalization. The final row shows the number of evaluations in the whole sample, evaluations among those whose cases are certified and not certified, and in square brackets the share of compliers calculated. *p< 0.1; **p< 0.05; ***p< 0.01.

Table B.3: First Stage: Delegate tendency and Case Certification

	Case Certified		
	(1)	(2)	(3)
Delegate Propensity	1.08***	0.98***	0.97***
	(0.07)	(0.08)	(0.09)
Date FEs		✓	✓
Demographic Controls			✓
History Controls			
Effective F	2.33	146.53	140.35
Observations	14,112	14,112	14,112

Note: This table shows first stage results for the delegate analysis, documenting the relationship between our residualized delegate tendency to certify a case instrument and the individual's likelihood of having their case certified for evaluation. The regressions are estimated in the our delegate sample, using an instrument constructed as described in Section 4, only applied to delegates. Each column adds successive control variables. Demographic controls include only those variables visible to delegates while assessing a case: the individual's sex, age, the reporter relationship to the individual in question. Standard errors are clustered at the delegate level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table B.4: Randomization Test

	Certified	Delegate Propensity
F-Statistic	47.1204	2.5038
p-value	0.0000	0.0573
Observations	14,112	14,112

Note: This table relates observable characteristics to an individuals' likelihood of having their case certified (Column 1) and to the assigned delegates's tendency to certify a case on a involuntary hospitalization evaluation (Column 2). The F-statistic jointly tests the significance of the control variables and the p-value of the joint test is reported below. Standard errors are clustered at the delegate level.

Table B.5: Delegate tendency and Downstream Events

	Delegate Propensity			
Physician Propensity to Hospitalize	0.007			
	(0.005)			
Hospitalized		−0.001		
		(0.001)		
CCP Hearing			0.0002	0.003
			(0.002)	(0.003)
CCP Extension			0.001	−0.004
			(0.002)	(0.003)
F-stat from joint test	0.666	2.5717	0.4069	1.2369
p-val from joint test	0.4145	0.1088	0.6657	0.2904
Observations	9,220	11,869	9,054	3,912

Note: This table relates considers the relationship between a delegate's tendency to certify a case and the downstream events among those who are evaluated. Conditional upon certification, the delegate's tendency is unrelated to the physician's tendency to hospitalize, whether the individual is hospitalized, whether the individual has a hearing with the Court of Common Pleas (CCP) or whether the magistrate extends their stay. The F-statistic and p-value from a test of joint significance is shown at the bottom. *p< 0.1; **p< 0.05; ***p< 0.01.

Table B.6: First Stage by Demographic Characteristic

	Case Certified			
	Female	White	Some HS	< 35 Years
Propensity	0.74***	1.09***	1.02***	0.91***
	(0.18)	(0.12)	(0.13)	(0.17)
Rotation Fixed Effects	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Observations	6,222	9,374	7,641	6,886

Note: This table presents the first stage relating a delegates's tendency to certify a case for evaluation. Standard errors are clustered at the delegate level. *p< 0.1; **p< 0.05; ***p< 0.01.

Table B.7: First Stage by Reported Concern

	Case Certified			
	Unable to Care	Danger to Others	Suicide Att	Mutilated Self
Delegate Propensity	0.97***	1.14***	0.75***	1.23
	(0.19)	(0.14)	(0.11)	(1.24)
Rotation Fixed Effects	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Observations	6,659	7,054	6,767	788

Note: This table presents the first stage relating a physician's tendency to hospitalize a patient on their hospitalization. The final columns, subsetting to individuals who have a diagnosis of severe mental illness (SMI), of a mental illness (MH) or of substance use disorder (DA), are available only for individuals who are enrolled in medicaid or have had a County-paid medical claim. Standard errors are clustered at the delegate level. *p< 0.1; **p< 0.05; ***p< 0.01.