The Labor Market Costs of Mental Health: Evidence from Depression, Anxiety, and Self-harm*

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Abstract

Depression and anxiety impose large economic costs worldwide, yet causal evidence on their labor market consequences remains scarce. We provide the first causal estimates of the effect of severe episodes of depression, anxiety, and self-harm on labor market outcomes. Using Swedish administrative data, we exploit quasi-random variation in the timing of first hospitalization among individuals who all eventually experience an acute mental health crisis. We find that these shocks generate immediate and persistent declines in employment and earnings. These losses reflect both labor market exit and lasting earnings reductions among those who remain employed, and show no meaningful recovery even ten years after hospitalization. Public assistance programs partially offset the earnings losses in the short run but fail to compensate for them over time, leaving total income persistently depressed. The labor market costs of severe mental health crises exceed those of physical health shocks, and because these conditions typically occur earlier in adulthood, they carry substantially larger lifetime earnings losses.

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1 Introduction

Mental health disorders represent a major global health challenge, accounting for 16% of the total global burden of disease (Arias et al., 2022). Depression and anxiety are among the most common mental health conditions, ranking as the second and fifth leading causes of disability worldwide, outranking both diabetes and cardiovascular disease (World Health Organization, 2025). In the United States, approximately one in five adults lives with a mental illness; in any given two-week period, 18% report symptoms of anxiety and 21% report symptoms of depression (NIMH, 2024; Terlizzi and Zablotsky, 2024). At their most severe, these conditions can be fatal: suicide is the second leading cause of death among individuals ages 10-34 (CDC, 2023).

Mental illness also carries a large economic burden through reduced productivity, absenteeism, and disability.[1] Depression and anxiety alone account for more than 12 billion days of lost productivity each year, with their combined economic cost estimated at over $1 trillion annually (Chisholm et al., 2016). Although these estimates underscore the magnitude of the economic burden, they are derived from observational associations and rely on strong modeling assumptions about counterfactual outcomes. Consequently, evidence on the causal impact of these highly prevalent mental health conditions on economic outcomes remains limited.

Our study provides the first causal estimates of the effect of depression, anxiety, and self-harm on labor market outcomes. We do so using Swedish administrative data, which—unlike data available in the United States—link population-wide health records to longitudinal labor market outcomes. We focus on severe manifestations of these disorders that culminate in a hospitalization, and we show that these acute events generate substantial disruption in both earnings and employment. While inpatient hospitalizations represent the more severe end of the clinical spectrum, they are not rare: among individuals ever diagnosed with depression, anxiety, or self-harm, we estimate that between 10 and 20 percent experience a hospitalization before midlife. The population we study, therefore, represents a meaningful share of the hundreds of millions worldwide affected by these conditions.

The main empirical challenge for studying the impact of mental health conditions on labor market outcomes is that mental illness is inherently difficult to measure in data. While pharmacy records and outpatient visits capture individuals seeking treatment, the timing of these events is endogenous, and patient severity is often unclear (e.g., someone filling a

[1] The global cost of mental health conditions was estimated at $2.5 trillion (USD) in 2010 and is projected to rise to $6 trillion by 2030 (Bloom et al., 2012).

prescription may be on a path to recovery rather than in acute distress). To overcome this challenge, we focus instead on inpatient hospitalizations, which provide a less ambiguous marker of severe mental health. Hospitalizations are also largely outside the individual's control, marking a discrete moment at which mental health has deteriorated to a crisis point severe enough to compel clinical intervention, with its timing driven by the underlying illness rather individual choice.

To estimate causal effects, we focus on individuals who eventually experience a hospitalization for depression, anxiety, or self-harm, and we exploit variation in the *timing* of their first hospitalization. By comparing individuals who all eventually experience a mental health crisis, this strategy accounts for systematic differences between individuals with mental illness and the broader population.[2] We compare individuals who are hospitalized with those hospitalized five years later and estimate difference-in-differences and event-study specifications to trace out the dynamic effects around this acute mental health event. The identifying assumption is that conditional on eventually being hospitalized, the exact timing of first hospitalization is as-good-as-random and uncorrelated with labor market outcomes. Consistent with this assumption, we find that the two groups display similar trends in employment and earnings in the years preceding hospitalization.

Our first main finding is that individuals who experience a severe mental health shock suffer an immediate and persistent decline in earnings and employment. One year after hospitalization, employment falls by 13 percentage points (16% of the control group mean) and earnings decline by roughly SEK 64,000 (27%). Even four years after hospitalization, annual employment and earnings remain 14% and 18% lower, respectively. Over this period, cumulative earnings losses amount to roughly SEK 216,000 (approximately \$20,000–\$25,000). These losses reflect both labor force exit and scarring among those who remain employed. While some individuals leave the labor force around the time of hospitalization, those who remain employed face persistently lower earnings, consistent with reductions in productivity or hours worked. Our results are robust to alternative bandwidth choices and sample restrictions.

These findings show that although hospitalized individuals were already on a lower labor market trajectory prior to the crisis than the general population, the hospitalization itself generates an additional, permanent downward shift in earnings capacity from which they do not recover. Whether driven by lasting neurobiological effects, deterioration of human capital during absence, or persistent stigma from employers, these results point to a

[2] Throughout the paper, we use the terms "mental illness" and "mental health conditions" to refer specifically to depression, anxiety, and self-harm, which are the conditions we study in this paper.

discrete and lasting loss of work capacity.

To assess the full impact on living standards, we examine how Sweden's robust public assistance programs cushion these labor market losses. In the year following hospitalization, the safety net provides meaningful support: individuals are substantially more likely to receive temporary sickness benefits, which offset roughly two-thirds of the concurrent earnings decline. Over time, however, this support largely fades and a smaller subset of individuals transition to permanent disability benefits. To capture overall economic well-being, we examine income for all sources and find that it remains persistently depressed in the years following the mental health episode. The annual income loss of SEK 25,000 is equivalent to nearly three months of basic living expenses for a single adult (Konsumentverket, 2024), representing a meaningful decline in material well-being.

To benchmark the magnitude of our estimated effects against those of other adverse health events, we replicate our empirical strategy and estimate the impact of hospitalizations for heart attacks and strokes (akin to Dobkin et al. 2018 and Fadlon and Nielsen 2021). We find that the labor market losses from mental illness substantially exceed those of heart attacks and strokes. Moreover, because mental health hospitalizations occur earlier in adulthood, the implied lifetime earnings losses are also substantially larger. We also consider hospitalizations for transport-related accidents, finding that the labor market effects of mental health crises are also considerably larger than those of accidents.

We examine heterogeneity in the effects to understand which individuals are most affected. First, we find that the magnitude of the response scales strongly with hospitalization length, a proxy for episode severity, consistent with more severe crises generating more durable scarring. Second, prior labor market attachment shapes the magnitude of the employment response: lower-wage individuals experience substantially larger employment declines while those in higher-wage occupations are better able to remain attached to the labor force, consistent with the role of accumulated human capital and employer incentives to retain and accommodate skilled workers. Notably, we find no meaningful heterogeneity by gender, which helps rule out interpretations in which the effects are driven by gender-specific contemporaneous shocks, such as domestic violence or pregnancy-related complications. This final finding is consistent with our broader result that the effects we observe reflect the mental health crisis itself, rather than other shocks like job loss, relationship dissolution, or family deaths.

To assess the longer-run trajectory of affected individuals, we complement the baseline analysis with a matched difference-in-differences design that extends the post-hospitalization horizon to ten years. Using observationally similar never-hospitalized individuals as the comparison group, we continue to find little evidence of recovery: annual employment, earnings,

and total income are persistently lower in the decade after hospitalization. In this sense, severe mental health crises are not just temporary disruptions, but inflict lasting damage on individuals' labor market trajectories.

In the last part of the paper, we examine how Sweden's universal healthcare and income-replacement systems shape both the magnitude and persistence of the labor market losses. A natural question is whether the large employment declines may reflect the generosity of Sweden's social insurance system, rather than the underlying health shock itself. We show that while more generous income replacement modestly amplifies post-hospitalization employment declines, tightening benefit generosity far from eliminates them. Even in stricter policy regimes, individuals experience substantial and persistent employment losses.

Our primary contribution is to document the large and persistent economic costs of depression, anxiety, and self-harm, which are among the most prevalent mental health conditions worldwide. A large body of research across medicine and the social sciences has documented associations between mental illness and adverse labor market outcomes, but causal evidence remains scarce (see, e.g., Lerner and Henke 2008; Hakulinen et al. 2019; Virgolino et al. 2022; Wang et al. 2023). Within economics, prior work shows that early-life mental health conditions, particularly attention-deficit/hyperactivity disorder (ADHD), are associated with long-run socioeconomic disadvantage (Currie et al., 2010; Fletcher, 2014). More recent work shows that adult mental health can also have economic consequences.[3] Most closely related to our study is Biasi et al. (2023), which leverages cross-cohort exposure to the 1976 approval of lithium treatment to show that this innovation improved earnings and employment among individuals with bipolar disorder.[4] We build on this literature in two ways. First, we study a group of highly prevalent mental health conditions, allowing us to contribute directly to quantifying the global economic burden of mental illness. Second, by focusing on hospitalizations as markers of severe mental health and comparing individuals who all eventually experience such a crisis, we are able to credibly isolate the causal

[3] A related literature considers the relationship between mental health and other outcomes. See, e.g., Currie and Stabile (2006), Fletcher (2010), and Busch et al. (2014) on education, and Jácome (2020), Arora and Bencsik (2023), and Emanuel et al. (2025) on criminal behavior. Recent work has also established that economic circumstances, including poverty, are themselves a cause of depression and anxiety (Ridley et al., 2020).

[4] A related literature studies the effects of mental health treatment on health outcomes across both developing and high-income settings, using experimental and quasi-experimental variation (e.g., Baranov et al., 2020; Currie and MacLeod, 2020; Cuddy and Currie, 2026). In this paper, we abstract away from treatment and focus on the economic consequences of severe mental health crises regardless of their treatment trajectory.

impact of these mental health shocks. Leveraging population-wide administrative records, we characterize the full dynamic trajectory of earnings and employment around this event, revealing that labor market losses are both immediate and persistent.

Our paper also contributes to a literature studying the economic consequences of health shocks. Prior work has documented large labor market losses following hospitalizations for physical health conditions such as heart attacks and strokes (García-Gómez et al., 2013; Dobkin et al., 2018; Fadlon and Nielsen, 2021). We extend this literature by showing that mental health hospitalizations generate losses that are at least as large, and because they occur earlier in adulthood, carry substantially greater lifetime costs. We also contribute to work examining how the social safety net cushions the economic consequences of health shocks (Stepner, 2019). While we find that Sweden's generous system provides meaningful short-run support, it does not fully offset the earnings losses, a finding that speaks to the limits of income replacement in the face of durable reductions in work capacity.

More broadly, our findings contribute to a growing literature documenting how adverse events in adulthood shape long-run labor market trajectories. Recent work has shown that criminal justice contact (Garin et al., 2025; Hjalmarsson et al., 2025), criminal victimization (Bindler and Ketel, 2022; Adams et al., 2024a,b), and occupational stress (i.e., burnout; Nekoei et al., 2025) all have lasting impacts on earnings and employment. We add to this literature by documenting that mental health crises generate labor market losses that exceed those of most other adverse events and are comparable in magnitude to those of rape and burnout. Taken together, the large and persistent nature of these effects, combined with the fact that these hospitalizations occur early in adulthood, imply substantial lifetime earnings losses for the population suffering from depression, anxiety, and self-harm.

Finally, it is worth emphasizing that because we study only the most severe manifestations of these conditions, our estimates should be interpreted as a lower bound on the true societal cost of depression, anxiety, and self-harm. The far larger population who experience these conditions without ever reaching a crisis severe enough to require inpatient care likely also face meaningful earnings losses, implying that the aggregate burden is substantially larger than what we estimate here.

2 Institutional Background

Sweden provides a useful setting to study the labor market consequences of mental health shocks. The prevalence of mental health disorders in Sweden is similar to other high-income countries: in 2021, the rate was 16 per 100 individuals in Sweden versus 18 per 100 in the U.S. (Institute for Health Metrics and Evaluation, 2022). At the same time, Sweden combines universal healthcare with generous income replacement and strong employment

protections, which shape both the incentives and constraints individuals face following a health shock. In this section, we briefly describe the health care system and social insurance institutions relevant for interpreting our results.

2.1 Mental Health in Sweden

Mental health care in Sweden is delivered through a combination of primary care and specialized psychiatric services. Individuals typically enter the system through a general practitioner, with most treatment, including psychotherapy, provided in outpatient settings at relatively low out-of-pocket cost (approximately SEK 100—300 per visit). Pharmacological treatments are also initiated in these settings and dispensed through retail pharmacies, with costs largely covered under the national reimbursement system. Both outpatient care and prescription drugs are subject to high-cost protection schemes, with annual out-of-pocket caps of roughly SEK 1,300–1,500 for medical visits and approximately SEK 2,850–3,000 for pharmaceuticals. Thus, unlike other high-income countries, cost is not a barrier for accessing mental health treatment. While financial cost is unlikely to be a primary barrier to accessing mental health treatment, non-price barriers such as wait times and capacity constraints remain.

Most acute mental health episodes are evaluated in emergency departments—often in dedicated psychiatric emergency units within general hospitals—where patients may be discharged, observed, or admitted to inpatient psychiatric care. Inpatient treatment is provided in either specialized psychiatric wards within general hospitals or dedicated psychiatric hospitals and facilities, depending on the severity of the case and regional organization of services. A physician can admit a patient to the hospital either voluntarily or involuntarily. In 2021, hospitalizations with a mental health diagnosis accounted for roughly 10% of all hospitalizations among working-age adults (National Board of Health and Welfare, 2025).

Most individuals with mental health conditions are treated outside the hospital, so inpatient admissions account for a relatively small share of overall treatment. However, from a lifetime perspective, hospitalization is not rare: in our data, between 10 and 20 percent of individuals diagnosed with depression, anxiety, or self-harm experience a hospitalization by midlife. We focus on these hospitalizations as our measure of severe mental health shocks.

2.2 Institutional Responses to Severe Mental Illness

When a worker experiences a severe mental health episode, both employers and the state play a role in supporting their recovery and facilitating their return to work.

Under Swedish law, employers bear a "rehabilitation duty" toward employees who

cannot perform their work duties due to illness or injury, regardless of its cause.[5] Importantly, illness or injury does not constitute grounds for termination under the Employment Protection Act (1982). On the contrary, affected employees experience heightened employment protection ("förstärkt anställningsskydd"). Dismissal can only occur if an employee's work capacity is permanently diminished and no suitable role is available, which typically coincides with the employee being granted permanent sickness compensation. As part of their rehabilitation duty, employers must take active steps to help the employee regain their working capacity (Swedish Association of Local Authorities and Regions, 2023). This may include adapting tasks and workplace conditions, or facilitating a gradual return to work at reduced hours (for instance, initially working 25 or 50 percent of normal hours).

The state provides financial support through a tiered system of income replacement. Workers unable to perform their job due to illness or injury are eligible for sickness benefits, which replace approximately 80% of prior wages up to an income ceiling and scale with the degree of work capacity loss. To qualify for sickness benefits, individuals must have a doctor-certified reduction in work capacity of at least 25% and a qualifying work history.[6] If the condition is severe enough to prevent any return to work, individuals are transitioned to permanent disability compensation, which replaces around two-thirds of prior income. For those without sufficient work history to qualify for sickness insurance, or whose benefits expire without a transition to disability compensation, means-tested social assistance (*ekonomiskt bistånd*) provides a modest final safety net, administered by municipalities.

Taken together, these institutions provide substantial income support following a mental health shock while also limiting employer separations. As a result, individuals face reduced financial pressure to work and strong employment protections to remain at their employer. This institutional backdrop shapes both the level and the persistence of labor market adjustment, and motivates our examination of social assistance receipt and total income as outcomes alongside earnings and employment.

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[5] The Work Environment Code (1977) is the cornerstone of Sweden's labor laws on health and safety. The law states that working conditions must be adapted to people's differing physical and mental capabilities.

[6] Specifically, individuals must earn at least SEK 14,100 per year (in 2025) and have worked for at least six consecutive months (or have periods of work that recur each year).

3 Data & Summary Statistics

3.1 Data Sources

The goal of this paper is to examine the impact of mental illness on labor market outcomes. Because mental health conditions are difficult to observe or measure in data, we focus on severe mental health shocks, defined as those culminating in a hospitalization. Specifically, we use the use the primary diagnosis code and external cause of harm codes on inpatient hospitalization records from the National Board of Health and Welfare to identify mental health hospitalizations.

After identifying individuals with a mental health hospitalization, we create a panel at the person-year level. Using unique personal identification numbers, we merge in additional health records as well as records from the Longitudinal Integration Database for Sickness Insurance and Labor Market Studies (LISA). Each annual LISA dataset contains records on individuals aged 16 or older who are registered as residents of Sweden on December 31st of that year. Correspondingly, we begin the person-year panel at age 16. We also merge in demographic information from Statistics Sweden, including a person's birth year, gender, county and municipality of residence, and marital status. For more details on sample and variable construction, we refer the reader to Appendix B.

Using LISA, we construct our two primary outcomes: earnings and employment. To measure earnings, we use total earned income from employment and business activities. To measure employment, we construct a binary indicator equal to one if earnings in a given year exceed 10,000 SEK (approximately $1,000).[7] We select this threshold so that we do not classify individuals with positive but very low earnings as gainfully employed in that year.

We also construct several secondary outcomes to provide a fuller picture of individuals' economic circumstances. Social assistance income is the sum of income from welfare, temporary sickness compensation (covering illness, work injury, and rehabilitation), and permanent sickness compensation (early retirement and disability insurance). We also use a measure of *total* income, which is the sum of income from all sources. For readability, all monetary outcomes are expressed in thousands of SEK.

To construct our baseline analysis sample, we impose three sample restrictions. First,

[7] Because our measure is based on earnings rather than administrative employment status, a value of zero does not necessarily indicate unemployment. In Sweden, individuals may remain formally employed by a firm while earning zero or very low earnings (for example, during periods of parental leave). To avoid misclassification, we define employment as having earnings more than 10,000 SEK in a given calendar year.

we restrict the sample to individuals born in 1975 or later. Since the labor market data begin in 1990 and pertain to individuals ages 16 and older, this restriction ensures that the first hospitalization we observe in our dataset corresponds to an individual's *first* hospitalization as an adult, rather than a continuation of earlier hospitalizations that occurred before our observation window. Second, given the paper's focus on labor market outcomes, we restrict the sample to first hospitalizations occurring at ages 25 or older. This restriction allows us to focus on hospitalizations that occur once individuals are likely to have entered the labor market. Finally, we restrict the set of first hospitalizations to those for depression, anxiety, and self-harm. These hospitalizations are relatively common, comprising around 40% of the set of first mental health hospitalizations.[8] In robustness exercises in Section 5.4, we consider the sensitivity of estimates to these sample restrictions.

While a mental health hospitalization is a relatively rare event in any given year, cumulative exposure over the life cycle is substantial. Among individuals with a diagnosis of depression, anxiety, or self-harm (based on inpatient, outpatient, or pharmacy records), 9% are hospitalized between ages 17 and 27, 6% between ages 27 and 37, and 4.4% between ages 37 and 47. Under conservative assumptions, these figures imply that between 10 and 20 percent of this population experience a severe episode requiring inpatient care before age 47.[9] The population we study is therefore not a narrow clinical outlier but a meaningful share of the broader population affected by these common mental health conditions.

3.2 Summary Statistics

Table 1 displays summary statistics for our baseline sample alongside non-hospitalized individuals from the same birth cohorts. Hospitalized individuals differ markedly from the broader population: they have lower educational attainment, are more likely to have only compulsory schooling as their highest level of education, and are less likely to have completed high school by age 20. Appendix Figure A1 shows that these differences extend to labor market outcomes: hospitalized individuals have lower employment rates and earnings even before their hospitalization occurs. Such differences could reflect the effects of mental illness itself or other factors correlated with mental illness, such as family background. The differences between hospitalized and non-hospitalized individuals motivate our baseline em-

[8] The largest group of omitted hospitalizations are those for alcohol or drug use (roughly 30% of first mental health hospitalizations).

[9] We combine age-specific hospitalization shares from adjacent birth cohorts to construct bounds on cumulative exposure. The lower bound assumes full overlap across age bins (that is, those hospitalized at later ages were also hospitalized earlier), while the upper bound assumes no overlap. Both calculations assume stable age-specific hospitalization risk across cohorts.

pirical strategy, which compares individuals *within* the hospitalized sample rather than to the population at large.

Table 2 summarizes characteristics of the hospitalizations in our sample. The vast majority are unplanned, and most individuals arrive from home and are discharged back home, consistent with these being acute, sudden episodes rather than planned admissions. Lengths of stay are short—the median hospitalization lasts three nights—suggesting that these hospitalizations do not involve extended inpatient care.

Finally, Figure 1 previews our main findings by plotting raw means of employment and earnings around the age of first hospitalization. Both outcomes fall sharply in the year of hospitalization. Employment remains persistently depressed in subsequent years and does not recover to pre-hospitalization levels, while earnings continue to grow with age but along a distinctly lower trajectory. To quantify the causal impact of hospitalization on these outcomes, we now turn to our empirical strategy.

4 Empirical Strategy

Our empirical strategy focuses on individuals hospitalized for a mental health condition and exploits variation in the timing of hospitalization to estimate causal effects. As discussed in Section 3, hospitalized individuals differ systematically from the non-hospitalized population. We therefore use individuals hospitalized at a later date as the counterfactual, exploiting the assumption that hospitalization timing is as good as random conditional on eventual hospitalization.

We follow the approach of Fadlon and Nielsen (2019) and compare individuals hospitalized in year y (the treatment group) to those hospitalized in year $y + \delta$ (the control group), setting $\delta = 5$ in our baseline specification. For each treatment cohort, we construct a panel centered on year y, covering five years before and five years after y, and we restrict to individuals in both groups who are observed in all ten years. Importantly, the control group consists of individuals hospitalized in year $y + \delta$, but we observe their outcomes over the same calendar window around year y. For example, individuals hospitalized in 2000 are compared to those hospitalized in 2005, and we observe the outcomes for both treatment and control units around the year 2000. We repeat this for all treatment cohorts from 2000 through 2017, stacking the cohort-specific panels into a single estimation sample. An individual hospitalized in year y can therefore appear both as a treated unit in their own cohort panel and as a control unit for the year $y - \delta$. In robustness exercises, we consider the sensitivity of results to the choice of δ.

With this sample in hand, we estimate a difference-in-differences regression of the following form:

$$Y_{it} = \alpha_0 + \alpha_1 \text{Treat}_i + \alpha_2 \text{Post}_t + \beta_{\text{Post}}(\text{Treat}_i \times \text{Post}_t) + \phi_i + \eta_t + \delta_a + \epsilon_{it} \quad (1)$$

where Y_{it} denotes outcome Y for person i in calendar year t. Treat_i and Post_t are indicator variables equal to one for the treated group and post-hospitalization period, respectively. The term ϕ_i corresponds to individual fixed effects.[10] We also include η_t and δ_a fixed effects to flexibly control for differences in outcomes by calendar time and age, respectively. The error term is ϵ_{it} and standard errors are clustered at the individual level.

We also estimate a fully dynamic version of this same specification:

$$Y_{it} = \sum_{\tau \neq -2} \alpha_\tau \gamma_i^\tau + \sum_{\tau \neq -2} \beta_\tau (\text{Treat}_i \times \gamma_i^\tau) + \phi_i + \eta_t + \delta_a + \epsilon_{it} \quad (2)$$

Here, we denote $\tau = 0$ as the year of the hospitalization and we include event-time dummies γ_i^τ to measure outcomes over the five years before and after the hospitalization. We omit the event-time indicator for two years prior to hospitalization as the reference period. We choose this category because the effects of a mental health shock may begin to manifest in the year immediately preceding hospitalization (particularly for individuals hospitalized early in the calendar year), and we therefore want to allow for the possibility that $\gamma_i^\tau = -1$ is partially treated.

The coefficients of interest are β_{Post} and β_τ. The former captures the average difference between treated and control individuals in the post-hospitalization period, relative to the pre-period. The latter capture the differences between the treated and control individuals relative to their difference at $\tau = -2$. Throughout the paper, we report percent changes by scaling the estimated coefficients by the post-period mean of the control group. This provides a measure of the magnitude of the effect relative to the counterfactual outcome.

The key identifying assumption is that, absent the acute mental health shock, treated individuals would have continued to trend similarly to control individuals. That is, within the hospitalized sample, the *timing* of the mental health shock is as good as random and uncorrelated with labor market outcomes. This assumption motivates our focus on the first hospitalization, whose timing is plausibly more exogenous than that of subsequent episodes, which may be shaped by prior labor market outcomes or earlier hospitalizations. We assess the plausibility of this parallel trends assumption by plotting the raw data as well as the β_τ

[10] Because an individual can appear in the sample as both a treated and control unit, the individual fixed effects are not perfectly collinear with Treat_i.

coefficients from equation (2) in the years leading up to hospitalization.

A limitation of this empirical design is that the control group is itself hospitalized at year $y + \delta$, which has two implications for our analysis. First, because control units are approaching their own hospitalization date by $\tau = 4$, their outcomes may already reflect the onset of their mental health shock. We therefore restrict the post-period to $\tau \in \{0, 1, 2, 3\}$ to avoid confounding recovery among the treated with deterioration among the control group. Second, this feature of the design prevents us from studying longer-run effects within the baseline framework. To study outcomes beyond a four-year horizon, we complement the baseline analysis with a matched comparison group of never-hospitalized individuals with similar observable characteristics, which we describe in Section 7.

Importantly, the identifying assumption does not require that mental health crises are random events. Indeed, they may reflect a sudden deterioration within an already poor mental state, or they may be precipitated by life events such as relationship dissolution, bereavement, or financial stress. The identifying assumption requires only that the *timing* of the first hospitalization, conditional on eventually being hospitalized, is as good as random with respect to labor market outcomes. The intuition is that individuals with similar underlying mental health trajectories may nonetheless be hospitalized at different points in time due to idiosyncratic factors that are orthogonal to their economic circumstances. Consistent with this interpretation, in Section 5.3 we find no evidence that hospitalization is systematically preceded by job loss or job switching: these outcomes are stable in the years leading up to hospitalization, suggesting that deteriorating labor market outcomes are not what drives individuals into acute care.

Finally, a concern in this setting is whether our estimates capture the effect of severe mental illness, or instead reflect the impact of the precipitating events that may have preceded the hospitalization. We address this directly in Section 5.3, where we examine the prevalence of potential confounding events we can observe in the data. Among individuals in our sample, these events are relatively rare and the difference in their prevalence between treated and control units in the years prior to hospitalization is small, suggesting that the deterioration in outcomes is not driven by these other factors. More fundamentally, many individuals experience such life events without ever requiring inpatient mental health care. What the individuals in our sample have in common is not a particular precipitating event but the fact that their mental health deteriorated to the point of acute hospitalization, and it is the impact of that crisis that we evaluate in our study.

5 Results

5.1 Decline in Labor Market Outcomes

We begin by examining the labor market trajectories of individuals who experience a severe mental health shock. Panels (a) and (b) of Figure 2 plot raw means of employment and earnings for treated and control individuals in the years before and after the treated group's mental health shock. In the pre-hospitalization period, both groups display rising employment rates and earnings levels as individuals enter the labor market. In addition to showing similar employment and earnings levels, this figure also highlights that treated and control individuals had comparable trends in labor market outcomes prior to the shock, supporting the parallel trends assumption.

Panels (c) and (d) of Figure 2 present the dynamic event-study estimates. These results show a modest decline in both employment and earnings in the year preceding hospitalization, suggesting that for a small subset of individuals, worsening mental health begins to affect labor market outcomes even before their hospitalization.[11]

In the post-period, we observe large and persistent declines in employment. While the drop begins the year of the hospitalization, employment reaches its lowest point in the year after hospitalization and remains depressed in subsequent years. Table 3 quantifies the average post-period effect, showing that individuals who experienced a severe mental health shock were, on average, 11 percentage points less likely to be employed than the control group, or a 14% decline relative to the control mean. The persistent decline in employment is notable given employers' "rehabilitation duty" to actively support return-to-work, potentially through reassignment or reduced responsibilities. This pattern suggests that the constraints imposed by severe mental health crises may exceed what workplace accommodations alone can address.

Earnings follow a similar trajectory, with a sharp decline in the year of hospitalization that persists in subsequent years. The estimates in Table 3 reveal that a severe mental health shock reduced annual labor income by roughly 22% relative to the mean. Summing over a four-year horizon, this decline amounts to a cumulative loss of SEK 216,000 (approximately $23,000 at current 2026 exchange rates).

To benchmark these results, we consider how the effect sizes compare to those of physical health hospitalizations. Following Dobkin et al. (2018) and Fadlon and Nielsen (2021),

[11] In line with this interpretation, Appendix Figure A2 finds evidence that earnings declines are largest at $t = -1$ for individuals admitted to the hospital early in year $t = 0$, consistent with the shock affecting outcomes a few months prior to hospitalization.

we estimate the impact of hospitalizations for heart attacks and strokes among a comparable hospitalized sample in our data, finding a 6% decline in employment and an 18% decline in earnings (Appendix Figure A3). While the earnings effects are broadly comparable, the employment decline is substantially larger for mental health hospitalizations. Notably, individuals who experience heart attacks or strokes are significantly older than those in our baseline sample, so their longer tenure and labor market attachment may facilitate employment recovery. We therefore also benchmark our baseline results against transportation-related accidents, which affect individuals at ages more comparable to our baseline sample. The employment and earnings effects of accidents are substantially more muted (3% and 9%, respectively), suggesting that the labor market consequences of severe mental health crises may exceed those of physical health shocks in early adulthood.

Finally, we benchmark the effects against other adverse events studied in the literature. The earnings losses we document substantially exceed those associated with criminal justice contact (Garin et al., 2025; Hjalmarsson et al., 2025) and criminal victimization (Bindler and Ketel, 2022). The effects are more comparable in magnitude to those documented for gender-based violence (Adams et al., 2024b,a) and occupational stress (Nekoei et al., 2025). Crucially, unlike many of the adverse events studied in the literature, the labor market effects of severe mental health crises show no meaningful recovery over time.

5.2 Unpacking the Employment and Earnings Declines

The results so far establish that a severe mental health shock significantly reduces annual earnings. To assess whether this decline reflects reduced employment, lower earnings among those who remain employed, or both, we re-estimate equation (1) on the subsample of person-year observations in which the individual is employed. Panel (a) of Appendix Figure A4 replicates the baseline earnings result, while panel (b) reports estimates conditional on employment. The two series track closely in the year of hospitalization but diverge in subsequent years, as the conditional earnings gap narrows. These patterns indicate that among those who remain employed, earnings partially recover over time but do not return to pre-shock levels.

To quantify the relative contributions of the extensive and intensive margins, we perform an accounting decomposition of the earnings effect in each post-hospitalization year. Table A2 reports the results. In the year of hospitalization, the extensive margin accounts for roughly a quarter of the total earnings decline, with the remainder driven by lower earnings among those who remain employed.[12] The role of the extensive margin grows substantially

[12] The relatively small extensive margin contribution in year $\tau = 0$ is partly mechanical. Because

over time, so that by years two and three, it accounts for slightly over half of the total effect. This pattern reflects the fact that the employment gap remains persistently wide while the earnings gap among the employed narrows as some workers recover. Taken together, the decomposition confirms that both margins contribute meaningfully to the total earnings loss: a substantial share of individuals exit the labor force following hospitalization, while those who remain employed face persistently lower earnings, consistent with lasting reductions in productivity or hours worked.

To better understand the nature of the labor market dynamics, the first panel of Appendix Figure A5 plots cumulative non-employment in the post-hospitalization period. The bulk of employment loss occurs in years $\tau = 0$ and $\tau = 1$, with only modest additional exits in subsequent years. This pattern suggests that hospitalization causes a sharp initial drop in employment, with little additional exit in subsequent periods. Consistent with this interpretation, we observe a spike in employment-to-nonemployment transitions around the time of hospitalization, but transition rates converge back toward control group levels in subsequent years (panel b).[13] Similarly, we do not find evidence of increased job-to-job switching among those who remain employed (panel d), suggesting that the mental health shock does not generate ongoing labor market instability for those who stay attached to the workforce.

These patterns are difficult to reconcile with a purely treatment-based explanation, in which ongoing care needs (such as intensive outpatient therapy, frequent clinical visits, or participation in day-treatment programs) generate repeated employment disruptions or induce workers to transition into more flexible jobs. While treatment-related demands may plausibly contribute to the sharp initial exit from employment, the subsequent dynamics are less consistent with this mechanism. Instead, our results indicate that severe mental health crises generate a discrete and persistent break in labor market attachment, consistent with a lasting reduction in work capacity or job match quality that limits both continued employment and subsequent re-entry.

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employment is defined as earning more than 10,000 SEK in a given calendar year, individuals hospitalized partway through the year may still meet this threshold despite significantly reduced work activity. As a result, the intensive margin estimate in year $\tau = 0$ likely captures both lower hourly earnings and reduced time in work within the year.

[13] The results in this panel also serve as a robustness check for the baseline employment findings, as these transitions are measured using employer matches in administrative tax records, rather than using an earnings-based definition of employment.

5.3 Ruling out contemporaneous shocks

We interpret the observed changes in earnings and employment as responses to severe mental health shocks that culminate in hospitalization. A potential concern is that individuals in the treatment group may experience other shocks around the time of hospitalization, leading us to misattribute the labor market impacts to deteriorating mental health when they may instead reflect an alternative event. We take this concern seriously and examine several potential confounders.

One possibility is that the observed economic effects are driven by job loss, which has been shown to negatively affect health (see, e.g., Sullivan and Von Wachter, 2009; Black et al., 2015). Two pieces of evidence point against this interpretation. First, we do not see an Ashenfelter dip in employment: the decline at $\tau{=}{-}1$ is modest, suggesting that hospitalizations are not typically preceded by job separations. Second, Appendix Figure A6 shows that unemployment benefit receipt remains unchanged around the time of hospitalization, further indicating that job loss is unlikely to be driving the labor market effects we document.

We also assess whether the observed effects could reflect other personal or family-level shocks that may precipitate a mental health crisis. Table A3 compares the prevalence of several such events for treated and control individuals at event times $\tau{=}{-}1$ and $\tau{=}{-}2$. A few patterns are worth noting. The treated-control differences in marriage and cohabitation rates are small in the years leading up to hospitalization, suggesting that relationship breakdown is not the primary driver of the treatment group's mental health shock. Family deaths are similarly rare and balanced across groups. Next, we proxy for criminal victimization by examining hospitalizations for assault. Although we do find that assault is more prevalent among treated individuals than controls, it affects fewer than 0.2% of the sample even in the year before hospitalization, making it unlikely to be a meaningful confounder. Finally, burnout-related hospitalizations are rare in the pre-period, though this is partly mechanical given our data construction.[14] More informatively, only 5% of the mental health hospitalizations in the baseline sample include burnout as a secondary diagnosis code, confirming that these episodes are largely distinct from the work-related stress episodes studied in Nekoei et al. (2025).

We also consider gender-specific shocks that are known to affect mental health, including pregnancy and gender-based violence (see, e.g., Adams et al., 2024a,b). Treated

[14] By construction, any individual whose first hospitalization was for burnout would have been excluded from our sample given our restriction on hospitalizations for depression, anxiety, and self-harm. Burnout-related events can therefore only appear in the pre-period as a secondary diagnosis for a hospitalization.

individuals are somewhat less likely to be pregnant than controls in the pre-period, which goes against postpartum mental health complications as a driver of the results. More broadly, if domestic or sexual violence were a primary driver of the labor market effects, we would expect both a predominantly female sample and attenuated effects among men. Instead, our sample is only slightly majority female (53 percent), and Table A7 shows that the employment and earnings effects are comparable across genders, which is inconsistent with this interpretation.

Taken together, these findings suggest that the labor market effects we document are not driven by any single contemporaneous shock that we can observe in the data. Of course, it is not possible to rule out every potential precipitating event, and mental health crises may arise in the context of a wide range of adverse circumstances. Our empirical design does not require that such events be absent; rather, it requires that their timing is not systematically correlated with labor market trajectories within our comparison group. In this sense, our estimates capture the average economic impact of reaching a mental health crisis severe enough to require inpatient care, regardless of its underlying cause.

5.4 Robustness of Labor Market Effects

We assess the robustness of the main findings to alternative sample and specification choices, with results reported in Table A4. Our baseline specification compares individuals hospitalized in year y with those hospitalized five years later ($\Delta = 5$). The choice of bandwidth involves a trade-off: smaller bandwidths yield control groups that are more comparable to the treatment group but shorten the post-period window, while larger bandwidths allow a longer horizon but require comparing individuals hospitalized further apart in time. To assess sensitivity to this choice, we re-estimate the models using alternative bandwidths of three, four, seven, and nine years. Appendix Figure A7 plots the event-study estimates across these specifications, showing that the results are remarkably stable.

We also consider the sensitivity of estimates to our sample restrictions. Relaxing the restrictions to include all birth cohorts, or to include all mental health hospitalizations rather than only those for depression, anxiety, and self-harm, produces estimates that are highly similar to the baseline. We also modify the construction of the control group. Instead of comparing all individuals hospitalized in y with all those hospitalized in $y+5$, we match each treated individual to a control unit of the same sex and birth year.[15] The resulting estimates are nearly identical to the baseline, indicating that differences in sex and birth year between

[15] For each treated individual, there are typically multiple eligible controls of the same sex and birth year. We select one at random.

treatment and control groups are not driving the observed effects.

6 Social Insurance and Total Income

Sweden's robust safety net provides substantial income support following a mental health hospitalization; however, it does not fully offset the labor market shock. Panels (a) and (b) of Figure 3 show a sharp rise in social assistance income in the years immediately following hospitalization (Table 4 summarizes the post-period results). In years τ=0 and τ=1, this increase averages roughly 40,000 SEK, offsetting about two-thirds of the concurrent earnings decline. However, social assistance income fades in years τ=2 and τ=3, providing diminishing income replacement over time.

Beyond the overall level of support, Panel (b) reveals an important shift in the composition of support over the post-period. Temporary sickness compensation dominates in the first two years after the shock, but permanent sickness compensation rises gradually thereafter and becomes an important source of assistance by the end of the post-period. This transition suggests that a share of hospitalized individuals never recover sufficiently to return to work and instead move onto permanent disability benefits.

To better understand how these changes translate into individuals' economic states, Table A5 examines joint outcomes of employment and social assistance receipt one year after hospitalization (τ=1) and at the end of the post-period (τ=3). The results reveal a clear pattern of sorting in the wake of hospitalization. In the short run, the dominant effect is a sharp decline in the probability of being employed without any assistance (23 p.p. decline), as individuals flow into two states: employment while receiving temporary sickness compensation (10 p.p. increase) and non-employment while receiving public assistance (14 p.p. increase). Notably, the probability of being nonemployed without any assistance barely changes, suggesting that the safety net system is largely successful in catching individuals who leave employment.

Four years after hospitalization, a different pattern emerges. The sharp decline in unassisted employment narrows, and the share of nonemployed individuals receiving assistance also falls, suggesting that a meaningful share of individuals return to work and decrease their reliance on public assistance. The largest remaining treated-control difference is the elevated probability of being nonemployed while receiving assistance, reinforcing that a share of hospitalized individuals do not reattach to the labor market. Nevertheless, two-thirds of treated individuals are working at the end of the post-period, so nonemployment with assistance, while the largest margin of change, remains a minority outcome.

Taken together, these patterns raise a natural question: to what extent does the safety net offset the overall earnings losses? We address this by examining total income from

all sources. Despite increased public assistance in the aftermath of hospitalization, total income remains persistently depressed (final panel of Figure 3). On average, individuals who experience a severe mental health shock face an 8% reduction in total income each year, implying a meaningful and sustained decline in living standards over the medium run. To put this into perspective, the annual SEK 25,000 loss is equivalent to nearly three months of basic living expenses (e.g., food, clothing, and utilities) for a single adult (Konsumentverket, 2024). This decline in total income reflects the fact that public assistance falls over time and does not fully replace earnings losses, whether from job separation or from reduced earnings among those who remain employed. By comparison, individuals hospitalized for a transport accident experience only a 3% decline in total income, underscoring that the financial consequences of severe mental health crises are substantially larger than those of other physical health shocks in early adulthood.

7 Long-Run Effects of Mental Health Shocks

In this section, we examine the longer-run trajectories of hospitalized individuals to better understand the persistence of the labor market losses. To do so, we complement the baseline analysis with a matched difference-in-differences strategy that uses observably similar, never-hospitalized individuals as the control group. Within cells defined by birth year and gender, we perform nearest-neighbor matching on immigrant status, marital status, cohabitation status, education, one-digit occupation, and municipality of residence measured at $\tau{=}{-2}$ as well as earnings deciles measured at event-time $\tau \in (-2, -3, -4, -5)$.[16] To assess the sensitivity of the results to the choice of matching variables, we also estimate a more limited specification that excludes the earnings deciles.

Using the matched sample, we estimate event-study specifications analogous to equation (2) but extend the post-period to a ten-year horizon. Because the matched controls are never hospitalized, we are not constrained by the bandwidth limitation of the staggered timing design. We view the staggered timing approach as our preferred design since it compares individuals who all eventually experience a severe mental health shock, more directly accounting for unobserved characteristics associated with mental illness. Nevertheless, the matched design provides a credible complement, allowing us to measure labor market outcomes up to ten years after the hospitalization.

[16] For each treated individual, we compute the Euclidean distance between the treated unit and all eligible controls using standardized (z-scored) covariates. We retain the single nearest control for each treated individual. As in the baseline empirical strategy, we restrict to individuals in both groups who are observed in all years before and after the hospitalization.

As shown in Table 5, the short-run estimates using the never-hospitalized broadly replicate the baseline short-run effects, albeit with larger magnitudes. In the baseline specification, hospitalization reduces employment and earnings by 14% and 22%, respectively. The declines when using the never-hospitalized are 16% and 32%, respectively. While the extensive-margin employment losses are similar across designs, the earnings losses are substantially larger relative to never-hospitalized individuals.

This pattern is consistent with selection on unobservables. In the staggered timing framework, we compare hospitalized individuals to other individuals who are also eventually hospitalized and therefore share underlying risk factors and weaker labor market trajectories. In contrast, the matched design compares hospitalized individuals to those who are never hospitalized and exhibit higher baseline earnings and employment levels. Even conditioning on rich observables, this group likely has a stronger latent counterfactual labor market trajectory, thereby generating larger earnings effects.

We next turn to the long-run estimates, which provide little evidence of recovery or convergence even ten years after hospitalization (Appendix Figure A8). In the ten-year window, employment and earnings remain 15% and 31% lower, and total income is 17% lower in the post period (Appendix Table A6). Together, these estimates indicate that the initial labor market disruption does not attenuate meaningfully over time. These losses are robust to the choice of matching characteristics: the alternative specification yields nearly identical long-run effects (panel b of Tables 5 and A6).

In economic terms, these patterns point to substantial labor market scarring following severe mental health hospitalization. The hospitalization shock generates not only an immediate decline in employment and earnings but also a persistent shift in the earnings trajectory relative to observationally similar peers. We find no evidence that affected individuals gradually transition back toward their pre-hospitalization path or converge to the earnings profile of the matched control group. Instead, the initial disruption appears to translate into long-lasting reductions in employment and earnings that persist for at least a decade.

8 Heterogeneity in Labor Market Losses

8.1 Demographic and Hospitalization Characteristics

Table A7 examines heterogeneity in the employment and earnings effects across several dimensions. A first finding is the absence of meaningful heterogeneity by gender: the employment and earnings effects are highly similar for men and women. As discussed in Section 5.3, this is inconsistent with an interpretation in which the labor market effects are primarily driven by domestic violence or sexual assault, which would predict larger effects

among women Adams et al. (2024a,b)

The effects vary more substantially across episode severity and diagnosis type. Individuals with longer hospital stays, a proxy for the severity of the condition, experience substantially larger employment and earnings effects, consistent with more severe episodes generating more durable scarring. This pattern also helps explain the heterogeneity across diagnosis type. Self-harm hospitalizations are considerably shorter than hospitalizations with an anxiety or depression diagnosis, and individuals hospitalized for self-harm exhibit more muted effects than those hospitalized for depression or anxiety. Indeed, self-harm is not always indicative of a chronic psychiatric disorder (Franklin et al., 2017; Simon et al., 2018), and the shorter hospitalizations we observe among this group are consistent with a less severe underlying condition.

Finally, individuals living alone experience larger effects than those living with others in the year prior to the hospitalization. One might have expected the opposite, since individuals without household financial support might face stronger incentives to return to work quickly. The pattern we observe may instead reflect the protective role of co-residence: living with others may facilitate recovery and provide informal support that eases reattachment to the labor market. We interpret this finding with caution, however, as individuals living alone and those living with others likely differ along multiple dimensions (e.g., age, relationship status, and baseline labor market attachment) that may independently explain the differential response.

8.2 Occupation and Firm Size Prior to Hospitalization

Table A8 examines heterogeneity by pre-hospitalization labor market characteristics. Individuals with the weakest labor market attachment (i.e., those who were not employed in the year prior to hospitalization) experience the largest employment effects. At the other end, high-wage workers experience a more muted employment decline, consistent with longer job tenure and accumulated firm-specific human capital increasing both the worker's incentive to remain attached to their employer and the employer's incentive to retain them. Notably, however, the relative earnings losses across occupational groups are more similar, suggesting that while high-wage workers stay more attached to the labor force, they do not fully return to their pre-shock earnings capacity. This pattern could reflect reduced hours or productivity on the part of the worker, but it may also reflect employer-side adjustments (e.g., reassignment to lower-paying or less demanding tasks, either due to stigma or as part of the employer's rehabilitation duty).

We also find a gradient by firm size, with workers at smaller firms experiencing larger employment and earnings effects than those at larger firms. This gradient is partly composi-

tional, as large firms are more likely to employ high-wage workers. Restricting the sample to low-wage workers, the earnings gradient largely disappears, but a meaningful employment gradient persists: low-wage workers at small firms experience a 16% employment decline compared to 12% at large firms. This residual gradient may reflect differences in firms' capacity to accommodate returning workers. For instance, large firms might be more able to easily adjust workloads, offer part-time arrangements, or absorb reduced productivity while smaller firms may face greater constraints in retaining a worker whose work capacity has been diminished by the mental health shock.

9 Role of Social Program Design

[Note: This is a temporary section pending receipt of more granular data from Sweden's Social Insurance Agency.]

The large and persistent labor market effects documented above arise in the context of Sweden's generous income-replacement and employment-protection system. An important question is therefore how much of the observed decline in earnings and employment reflects the underlying health shock itself versus the policy environment in which that shock occurs.

To assess how Sweden's income-replacement system shapes the labor market consequences of severe mental health shocks, we exploit two reforms in the sickness and disability insurance system: (i) the July 2008 Rehabilitation Chain, which imposed statutory work-capacity assessments at 90, 180, and 365 days and tightened benefit duration and eligibility and (ii) a January 2009 reform that altered marginal incentives for permanent disability recipients to return to work.[17] Because statutory replacement rates remained broadly stable, these reforms primarily changed the effective generosity of income replacement by altering how long workers could remain on benefits.

Appendix Table A9 shows average employment effects and average benefit receipt for the pre-reform (2005–2007) and post-reform (2009–2011) hospitalization cohorts.[18] We measure benefit generosity using average post-hospitalization benefit receipt among treated individuals ($\bar{B}_y$), in thousands of SEK.[19] Column 2 reports estimates for temporary sickness

[17] The July 2008 reform entailed a cap on days at 80% replacement and stricter access to permanent disability insurance.

[18] We limit attention to those ages 25–30 within each cohort in order to rule out any age composition differences across cohorts. This yields a sample of approximately 10.5K individuals per year.

[19] An alternative measure of benefit generosity is the hospitalization-induced benefit response ($\hat{\beta}_y^B$), estimated via difference-in-differences to net out common macroeconomic trends. This measure has the advantage of isolating the causal effect of hospitalization on benefit receipt, but it has an important limitation in the present context: because the 2008–2009 reforms tightened eligibility

benefits whereas Column 3 reports analogous estimates for total benefit receipt, including temporary sickness, permanent disability, and welfare benefits.

A clear pattern emerges: employment declines are larger among the cohorts facing more generous benefit environments and fall after the reform. Strikingly, however, even among the post-reform cohorts facing a much stricter policy environment, employment declines remain on the order of 12.5 percentage points.

To quantify the sensitivity of employment effects to benefit generosity, we compute arc elasticities relating the percentage change in employment effects to the percentage change in average benefit receipt around the reform. For temporary sickness benefits, average receipt fell 22.1% around the reform, closely tracking the 22.4% decline in the magnitude of the employment effect, implying an arc elasticity close to one. Turning to total benefit receipt, we find average receipt fell 19.9% against the same 22.4% employment effect decline, again implying an arc elasticity close to one. The consistency of this estimate across both columns is reassuring: regardless of how broadly benefits are measured, a one percent decline in benefit generosity is associated with approximately a one percent decline in the magnitude of the employment effect.

We note that these estimates are illustrative rather than structural. The average benefit level may reflect cohort composition and macroeconomic conditions in addition to policy generosity. For instance, differences in pre-hospitalization earnings or health severity across cohorts would mechanically affect benefit levels through the statutory 80% replacement rate, as would the global financial crisis, which coincided with the post-reform period. To the extent these factors contributed to the observed decline in benefit levels, our elasticity estimates should be interpreted with caution.

Notwithstanding these caveats, while the generous policy environment in Sweden appears to amplify the post-hospitalization labor market detachment, it is far from a complete explanation. Employment declines remain substantial even under the strictest policy regime, and the heterogeneity by hospitalization length documented in Section 8 further underscores that it is the severity of the mental health crisis, rather than the generosity of available benefits, that primarily determines long-run labor market outcomes. Additional evidence

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for *all* workers, not just those hospitalized for mental health, the DiD estimator differences out precisely the system-wide generosity change we wish to capture. Indeed, the DiD response indicates a modest post-reform *increase* of 9.8% that contrasts with the 22.1% decline in average benefit levels. This is consistent with the reforms having tightened the system for treated and control workers, and if anything, better targeting benefits towards those hospitalized with mental health conditions. For this reason, we prefer the level of average benefit receipt as our primary measure of generosity.

against a pure benefit-response interpretation comes from the accidents sample: individuals facing the same generous benefit environment experience substantially smaller employment declines, suggesting that the larger effects among mental health patients reflect the severity of the underlying health shock rather than differential take-up of available benefits.

10 Conclusion

This paper provides the first causal estimates of how severe mental health crises affect labor market outcomes. Using Swedish administrative data that links inpatient hospitalization records to earnings and employment, we exploit quasi-random variation in the timing of first hospitalization among individuals who all eventually experience an acute mental health shock. We focus on hospitalizations for depression, anxiety, and self-harm, which are common conditions that affect a substantial share of the working-age population, and find that these acute events generate immediate and lasting damage to labor market outcomes.

In the years following hospitalization, employment falls by 14 percent and annual earnings decline by roughly 22 percent relative to the counterfactual. Importantly, the loss in earnings reflects both labor market exit as well as significant scarring among individuals who remain employed. These losses do not recover over time: using a complementary matched difference-in-differences design, we find that employment and earnings remain persistently lower even ten years after hospitalization. Although public assistance programs compensate for some of the earnings losses in the immediate aftermath of the hospitalization, they fail to offset the overall earnings losses. Indeed, even after accounting for increases in social assistance, total income remains persistently depressed, indicating that these individuals are economically worse off after this mental health shock. We also show that while Sweden's generous income-replacement system modestly amplifies post-hospitalization employment declines, the effects remain large and persistent even under stricter benefit regimes, suggesting that the labor market detachment we document reflects the underlying health shock rather than benefit-induced reductions in work incentives.

To situate these findings within the broader literature, we benchmark the effects against other adverse events studied in prior work. The employment decline we document exceeds that of heart attacks and strokes, as well as the losses associated with incarceration, criminal victimization, and job loss. Because mental health hospitalizations in our sample occur early in adulthood, the implied lifetime earnings losses are substantially larger than those of physical health shocks that typically strike later in the life cycle. Our findings thus suggest that common mental health conditions generate labor market scarring that rivals, and, in cumulative terms, likely exceeds the costs of severe physical health shocks or other adverse events.

These findings carry a direct policy implication. While individuals who experience hospitalization were already on a weaker labor market trajectory, the crisis itself causes a persistent decline in earnings capacity and economic well-being. The absence of recovery suggests that these episodes mark a turning point after which labor market prospects are permanently diminished. Whether this reflects lasting changes in health, erosion of human capital, or persistent employer responses, the evidence points to a sustained reduction in work capacity. Because this loss appears difficult to reverse once realized, prevention of severe episodes, rather than treatment after the fact, is especially valuable: interventions that avert acute crises may prevent not only the immediate disruption but the permanent scarring we document.

More broadly, our estimates likely understate the full economic burden of mental illness. Because we focus on cases that culminate in hospitalization, they capture only the most severe end of the clinical spectrum. The larger population experiencing depression, anxiety, and self-harm outside of inpatient settings may also face meaningful, if less visible, labor market consequences. As a result, the aggregate societal costs, and the potential gains from effective prevention and treatment, are likely substantially greater than what we document in this study.

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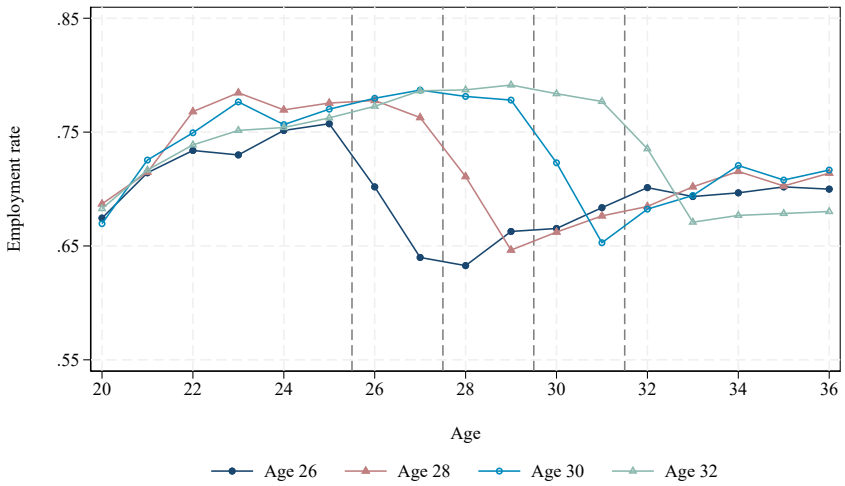
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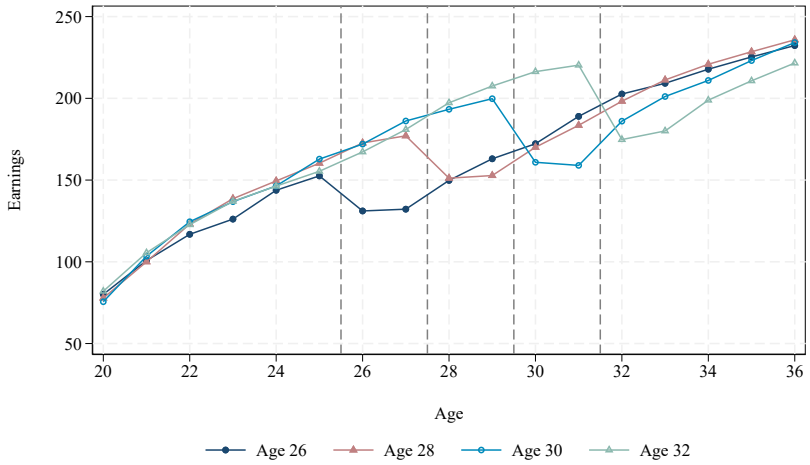
Figures and Tables

Figure 1: Labor Market Outcomes Around First Mental Health Hospitalization, by Age of Hospitalization

(a) Employment



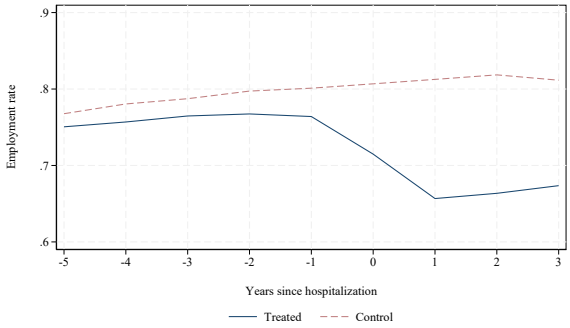
(b) Earnings



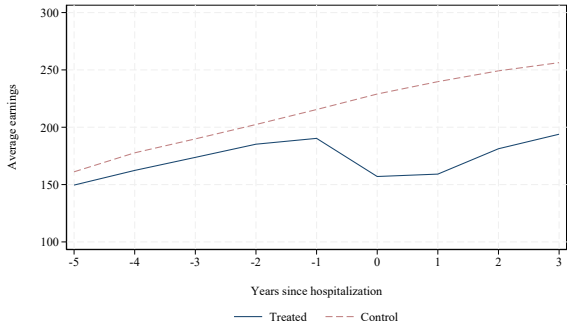
NOTE: This figure plots the employment rate and average earnings of hospitalized individuals who were hospitalized at different ages. Earnings refers to income from labor or business activities. Employment refers to whether the individual had any earnings that year. Individuals in this figure had available data for all years between ages 20 and 36. All income outcomes are measured in thousands of SEK.

Figure 2: Labor Market Outcomes Around Mental Health Shock
(Raw Means & Difference-in-Differences Estimates)

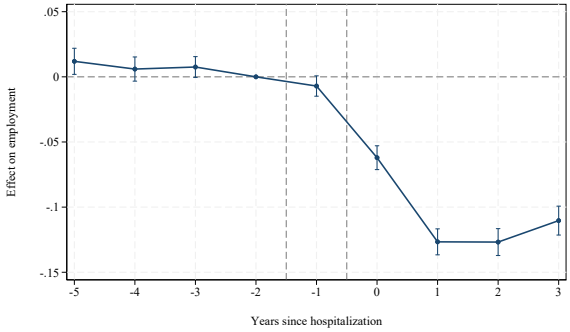
(a) Raw means: employment



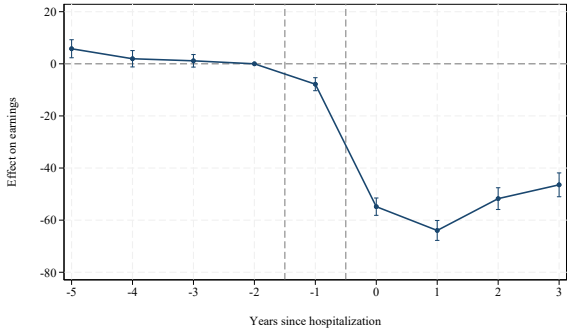
(b) Raw means: earnings



(c) Diff-in-diff: employment



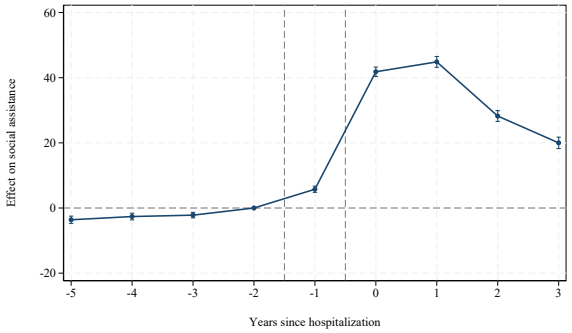
(d) Diff-in-diff: earnings



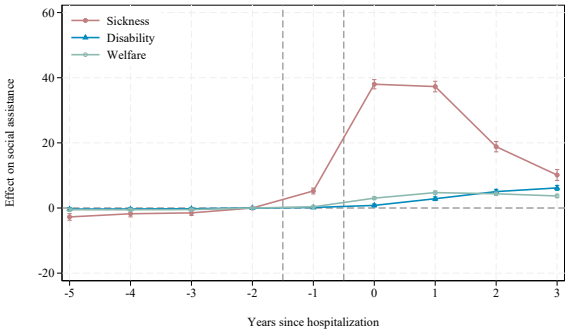
NOTE: These figures plot employment and earnings outcomes around the year of a person's first mental health hospitalization. Panels (a) and (b) plot raw means for the treatment and control groups described in Section 4. Panels (c) and (d) plot difference-in-differences estimates from equation (2). All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure 3: Economic Outcomes Around Mental Health Shock

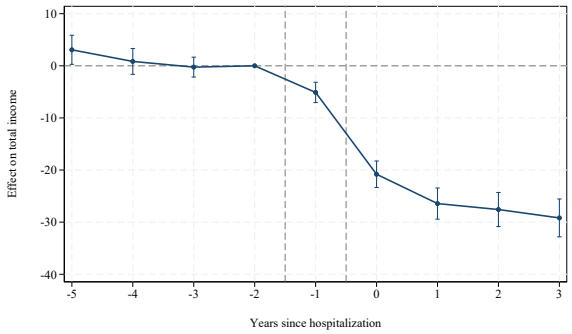
(a) Social assistance



(b) Social assistance components



(c) Total income



NOTE: This figure displays dynamic difference-in-differences estimates using the baseline sample described in Section 4 and equation (1). Panel (b) splits social assistance into its three components. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table 1: Summary Statistics of Hospitalized and non-Hospitalized Individuals

	Hospitalized Sample (1)	Non-hospitalized Sample (2)
A. Demographics		
Birth year	1982	1984
Female	0.53	0.48
Foreign-born	0.11	0.11
Ever married	0.36	0.36
B. Economic outcomes (age 20)		
Compulsory education	0.31	0.13
High school	0.65	0.78
Higher education	0.05	0.09
Enrolled in school	0.49	0.48
Employment	0.68	0.78
Earnings	83.79	114.53
Social assistance	12.14	5.15
Individuals	30,000	2,395,643

NOTE: This table displays summary statistics for our baseline sample. The hospitalized sample, described in Section 3, is displayed in column (1), and individuals in the same birth cohorts but who are not hospitalized are displayed in column (2). Panel (b) measures economic outcomes at age 20. The three levels of education refer to the highest educational attainment of individuals by that age. All income outcomes are measured in thousands of SEK.

Table 2: Summary Statistics of First Mental Health Hospitalization

Age	30.66
Unplanned	0.93
From home	0.94
Back home	0.90
Length of stay	3.00
Depression	0.46
Anxiety	0.28
Self-harm	0.28
Individuals	30,000

NOTE: This table displays summary statistics for the hospitalizations in our baseline sample (i.e., first mental health hospitalization occurring at age 25 or later). Each row displays the average across the sample, except for "length of stay," which refers to the median number of nights for the hospitalization. The rows for depression, anxiety, and self-harm do not sum to 1 because a hospitalization with a diagnosis for depression or anxiety can also have an external cause of harm indicating self-harm.

Table 3: Labor Market Outcomes Around Mental Health Shock

	β_{Post}	(S.E.)	$\bar{Y}$	N
Employment	-0.110***	(0.004)	0.81	302,499
Earnings	-54.426***	(1.521)	243.61	302,499
Earnings if employed	-39.225***	(1.653)	299.92	228,019
E-to-N transition	0.017***	(0.002)	0.06	302,499
N-to-E transition	0.000	(0.002)	0.08	302,499
Job switching	-0.011***	(0.003)	0.14	302,499

NOTE: This table displays difference-in-differences estimates using equation (1) and the baseline sample described in Section 4. All income outcomes are measured in thousands of SEK. "$\bar{Y}$" refers to the average outcome of control units in the post-period. Standard errors are clustered at the individual level. Sample sizes differ across rows because certain variables are not available in every year.

Table 4: Other Economic Outcomes Around Mental Health Shock

	β_{Post}	(S.E.)	$\bar{Y}$	N
Any social assistance	0.202***	(0.004)	0.34	302,499
Social assistance	34.252***	(0.613)	27.18	302,499
Temporary sickness	26.207***	(0.584)	17.70	302,499
Permanent sickness	3.857***	(0.255)	5.69	302,499
Welfare	4.188***	(0.195)	3.80	302,499
Total income	-25.698***	(1.201)	308.92	302,499

NOTE: This table displays difference-in-differences estimates using equation (1) and the baseline sample described in Section 3 All income outcomes are measured in thousands of SEK. "$\bar{Y}$" refers to the average outcome of control units in the post-period. Standard errors are clustered at the individual level.

Table 5: Long-Run Economic Outcomes Around Mental Health Shock

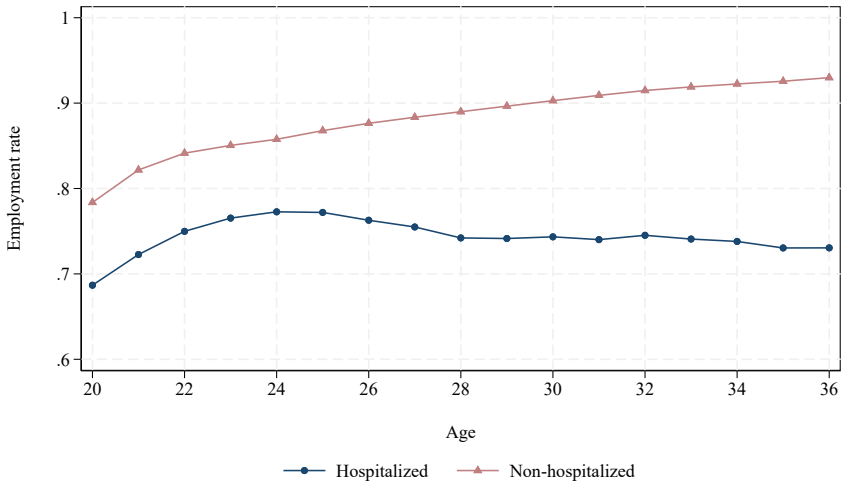
	Employment			Earnings			
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	N
Baseline	-0.110***	(0.004)	0.81	-54.426***	(1.521)	243.61	302,499
A. Preferred matching							
5-year effect	-0.132***	(0.004)	0.84	-86.652***	(1.954)	272.34	293,940
10-year effect	-0.130***	(0.005)	0.87	-92.707***	(2.656)	296.88	173,945
B. Alternative matching							
5-year effect	-0.122***	(0.004)	0.87	-88.098***	(1.935)	303.31	294,203
10-year effect	-0.122***	(0.005)	0.90	-85.438***	(2.766)	326.83	174,236

NOTE: This table displays difference-in-differences estimates using the matched sample described in Section 7. "Baseline" refers to the estimates that use the baseline sample, reproduced from Table 3. "Preferred matching" refers to nearest-neighbor matching on birth year, gender, immigrant status, marital status, cohabitation status, education, one-digit occupation, municipality of residence, and earnings deciles prior to hospitalization. "Alternative matching" omits earnings deciles in the matching process. "Short-run effect" and "long-run effect" refer to five- and ten-year post-hospitalization windows. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

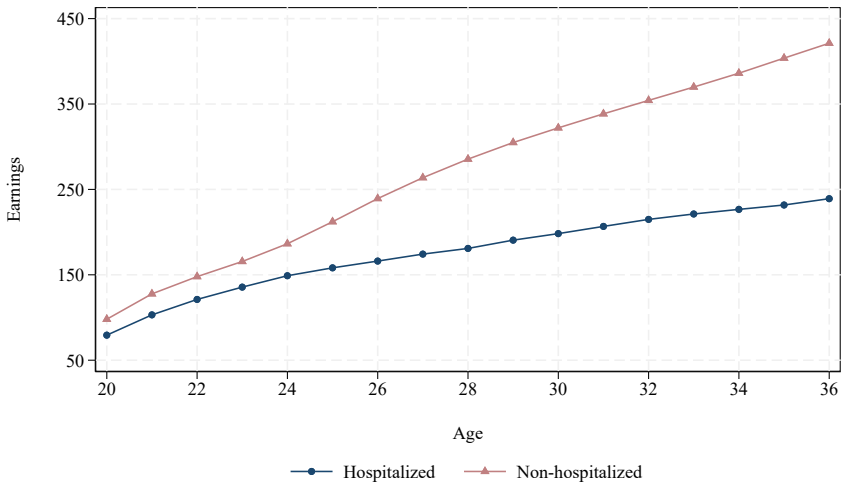
A Appendix Figures and Tables

Figure A1: Labor Market Outcomes of Hospitalized and Non-hospitalized Samples

(a) Employment



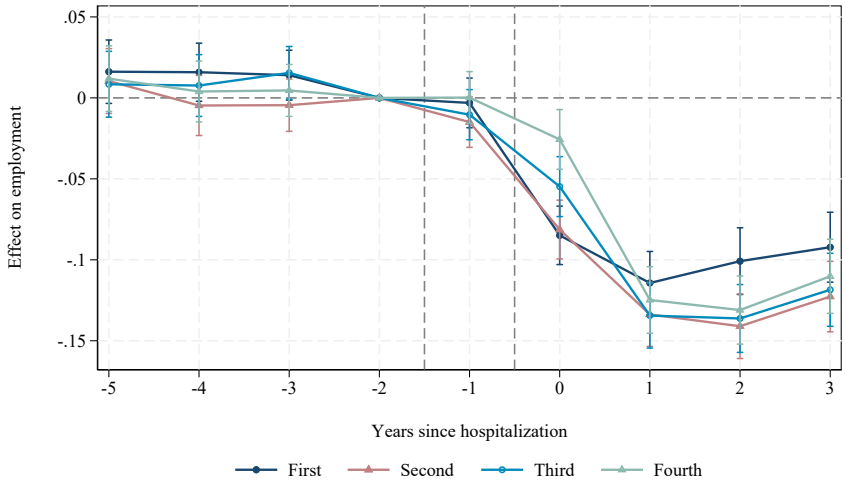
(b) Earnings



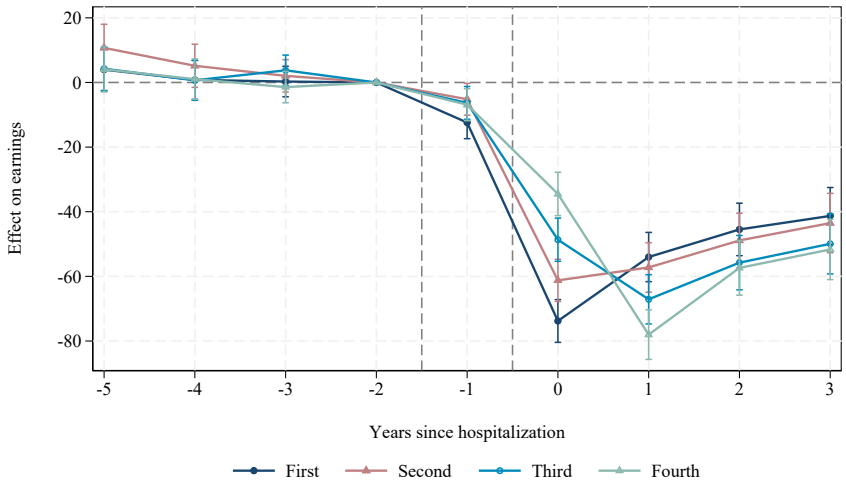
NOTE: This figure plots the employment rate and average earnings of the hospitalized and non-hospitalized sample. The baseline hospitalized sample is described in Section 3, and the non-hospitalized sample consists of individuals in the same birth cohorts who are not hospitalized. Earnings are measured in thousands of SEK.

Figure A2: Labor Market Outcomes of Hospitalized, by Quarter of Hospitalization

(a) Employment



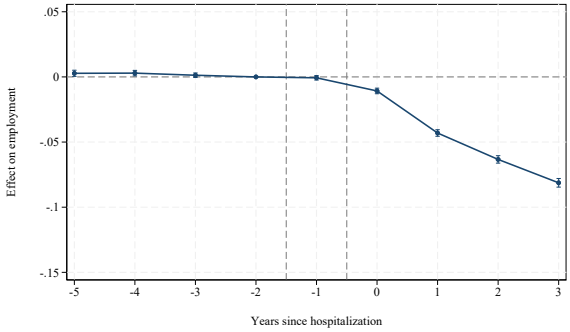
(b) Earnings



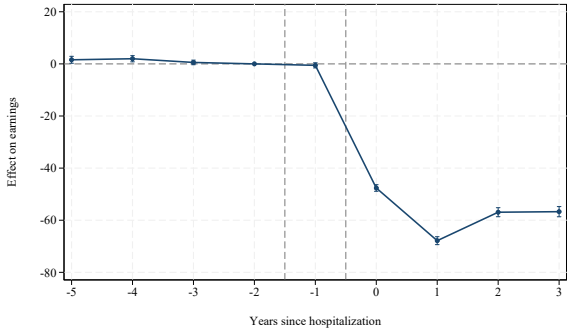
NOTE: This figure displays dynamic difference-in-differences estimates using the baseline sample described in Section 4 and equation (1). Both panels split the treatment group based on their quarter of hospitalization in year τ=0, and use the full group of later-hospitalized individuals as the comparison group. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure A3: Economic Outcomes Around Physical Health Shock

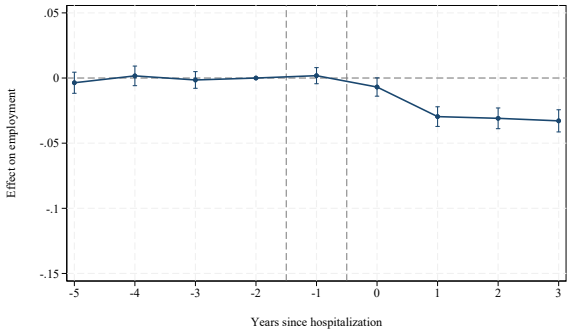
(a) Employment, Heart Attack/Stroke



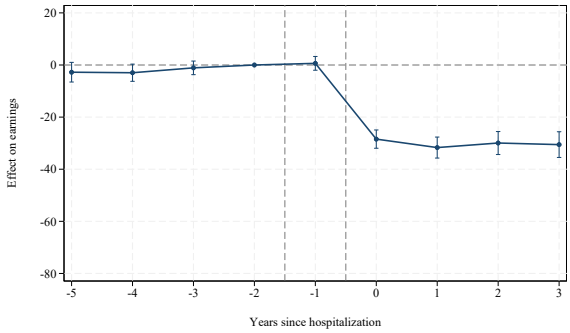
(b) Earnings, Heart Attack/Stroke



(c) Employment, Accident



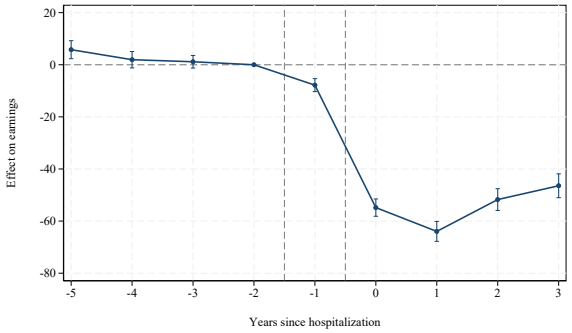
(d) Earnings, Accident



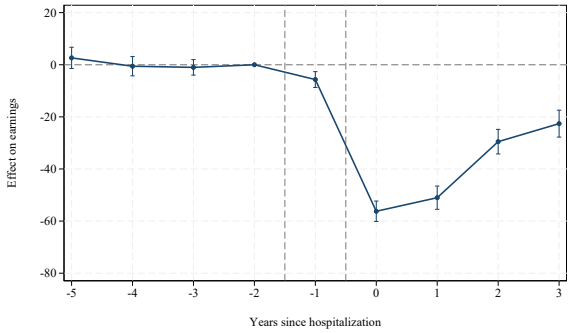
NOTE: This figure displays dynamic difference-in-differences estimates using equation (1) around the year of a person's first physical health hospitalization for a heart attack or stroke (panels a and b) or transport-related accident (panels c and d). All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure A4: Earnings Trajectories Around Mental Health Shock

(a) Earnings



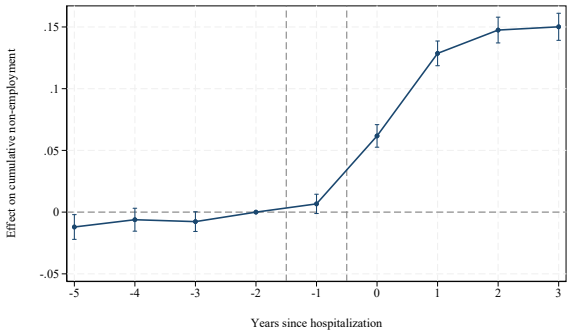
(b) Earnings Conditional on Employment



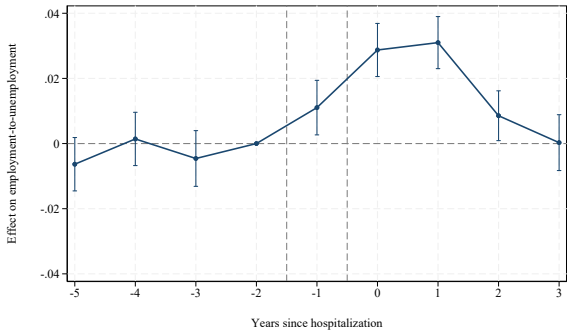
NOTE: This figure displays dynamic difference-in-differences estimates using the baseline sample described in Section 4 and equation (1). Panel (a) replicates the main finding on earnings from Figure 2. Panel (b) uses earnings conditional on employment as the outcome. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure A5: Labor Market Transitions Around Mental Health Hospitalization

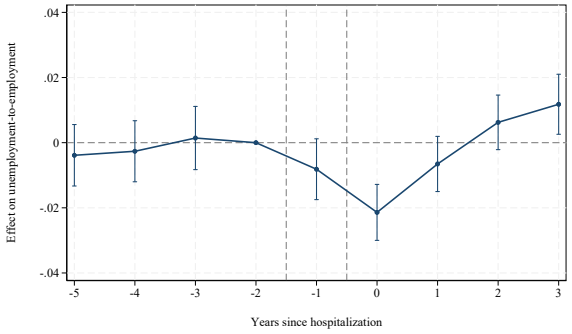
(a) Cumulative non-employment



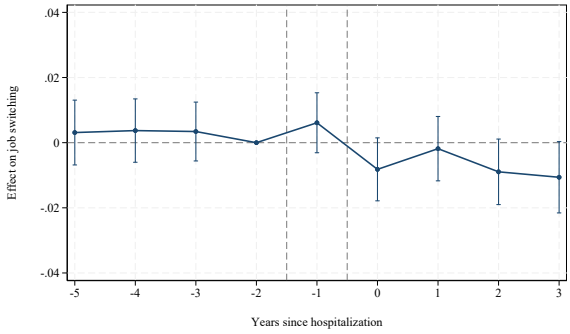
(b) Employment to non-employment



(c) Non-employment to employment

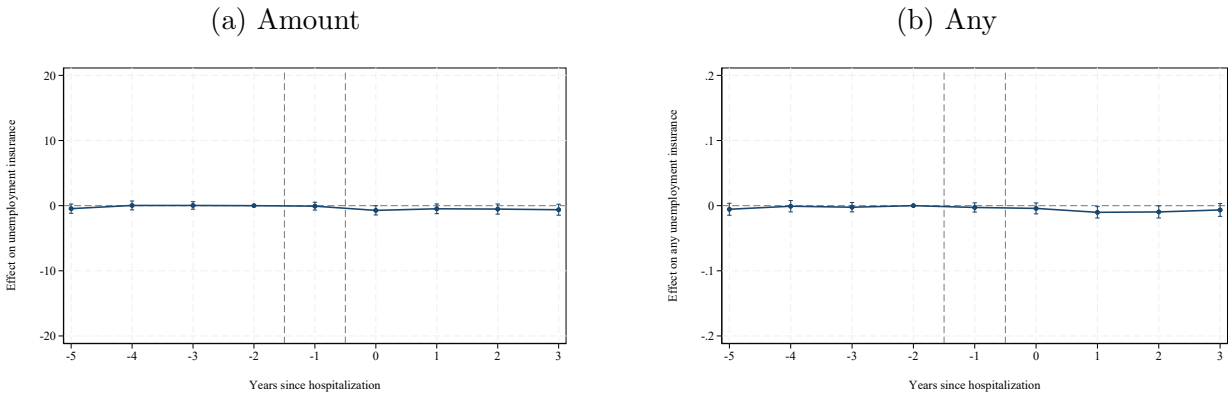


(d) Job switch



NOTE: This figure displays dynamic difference-in-differences estimates using the baseline sample described in Section 4 and equation (1). In panel (a), the outcome is an indicator for cumulative non-employment, defined as equal to one if the individual is not employed in that period or in any prior post-hospitalization period, and zero otherwise. Panels (b)–(d) use the *JobbByte* variable from Statistics Sweden's LISA database, which records year-to-year labor market transitions based on employer matches in administrative tax records. The variable distinguishes between transitions from employment to non-employment (panel b), transitions from non-employment to employment (panel c), and employer changes among the continuously employed (panel d). Standard errors are clustered at the individual level.

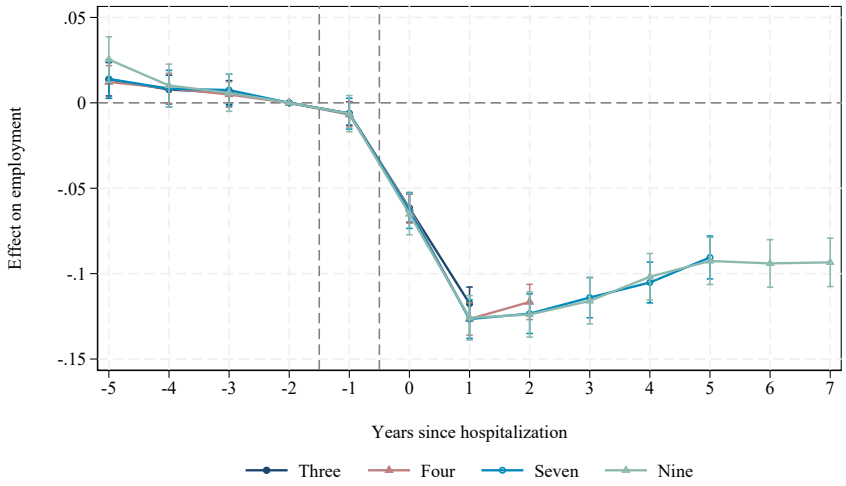
Figure A6: Income From Unemployment Insurance Around Mental Health Shock



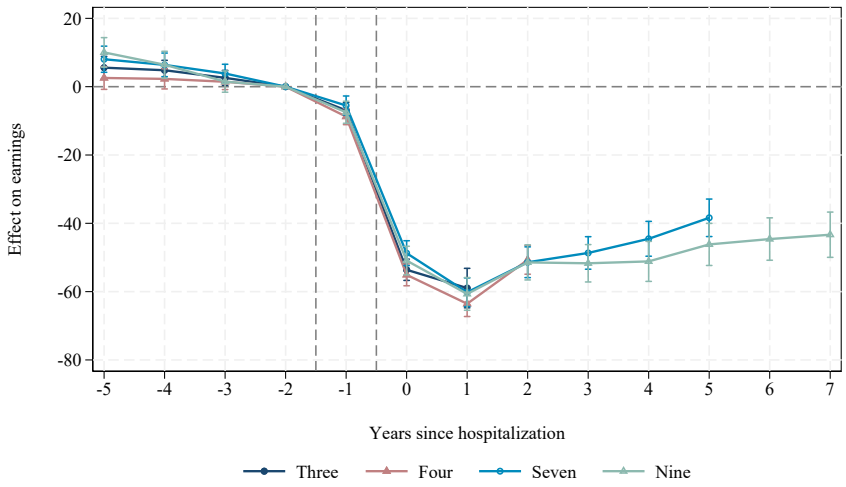
NOTE: This figure displays dynamic difference-in-differences estimates using the baseline sample described in Section 4 and equation (1). Panel (a) uses an outcome denoting income from unemployment insurance programs. Panel (b) uses a binary indicator equal to one if the person received any unemployment insurance income that year. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure A7: Labor Market Outcomes of Hospitalized, by Number of Years between Treatment and Control Hospitalization

(a) Employment



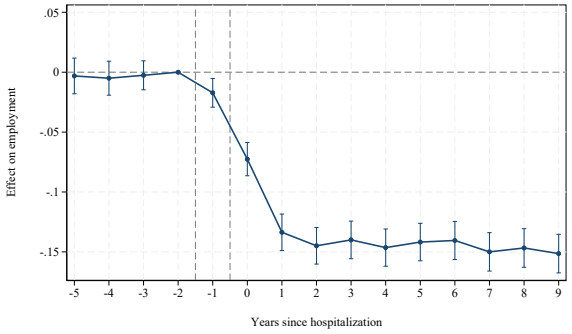
(b) Earnings



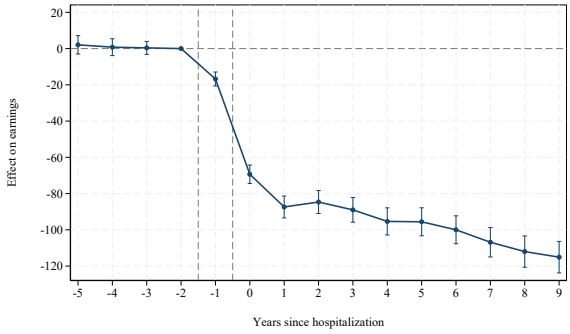
NOTE: This figure varies the sample construction described in Section 4, with each series changing the number of years between the treatment and control's hospitalization. The figure plots the dynamic difference-in-differences estimates using equation (2). All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Figure A8: Long-Run Economic Outcomes Around Mental Health Shock

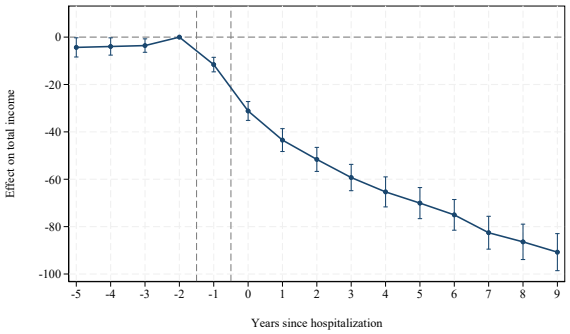
(a) Employment



(b) Earnings



(c) Total income



NOTE: This table displays difference-in-differences estimates using the matched sample described in Section 7. Control individuals are selected using nearest-neighbor matching on birth year, gender, immigrant status, marital status, cohabitation status, education, one-digit occupation, municipality of residence, and earnings deciles prior to hospitalization. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A1: Economic Outcomes Around Mental and Physical Health Shock

	β_{Post}	(S.E.)	$\bar{Y}$	N
A. Mental Health				
Employment	-0.110***	(0.004)	0.81	302,499
Earnings	-54.426***	(1.521)	243.61	302,499
Social assistance	34.252***	(0.613)	27.18	302,499
Total income	-25.698***	(1.201)	308.92	302,499
B. Heart Attack & Stroke				
Employment	-0.048***	(0.001)	0.79	2,742,126
Earnings	-57.665***	(0.633)	313.59	2,742,126
Social assistance	44.393***	(0.275)	45.37	2,713,208
Total income	-12.867***	(0.492)	380.48	2,713,208
C. Accidents				
Employment	-0.025***	(0.003)	0.88	281,836
Earnings	-28.918***	(1.658)	329.33	281,836
Social assistance	14.290***	(0.492)	18.92	281,836
Total income	-12.898***	(1.380)	376.38	281,836

NOTE: This table displays difference-in-differences estimates using equation (1). Panel (a) reproduces the baseline findings. Panels (b) and (c) replicate the baseline empirical strategy, but considering individuals hospitalized for heart attacks and strokes and transport-related accidents. All panels focus on first hospitalizations between the ages of 16 and 65. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A2: Decomposing Earnings Decline into Extensive and Intensive Margins

Horizon	Δ Earnings	Δ Employed	Δ Earnings if Employed	% Extensive Margin
Year 0	-54.82	-0.06	-56.23	24.76
Year 1	-63.95	-0.13	-51.02	45.98
Year 2	-51.74	-0.13	-29.52	56.83
Year 3	-46.44	-0.11	-22.60	53.58

NOTE: This table reports the results of a decomposition of the post-hospitalization change in earnings into extensive and intensive margin components. Each row corresponds to a year relative to the hospitalization. "Δ Earnings" and "Δ Employment" refer to the difference-in-differences estimate of the change in earnings and employment, respectively, using equation (2). "Δ Earnings if Employed" refers to the estimate that uses earnings conditional on employment as an outcome. "% Extensive Margin" refers to the share of the total change in earnings attributable to changes in employment. For details on the calculations, see Appendix C.

Table A3: Prevalence of Potential Confounding Events, Pre-Hospitalization

	Event time $\tau=-2$		Event time $\tau=-1$	
	Treated	Control	Treated	Control
A. Family-related shocks				
Married	15.294	17.053	16.048	18.928
Cohabitation	25.880	30.409	24.900	33.139
Family death	1.208	1.197	1.401	1.339
B. Other shocks				
Assault	0.084	0.021	0.157	0.048
Pregnancy	4.488	5.613	3.897	5.781
Burnout	0.053	0.021	0.047	0.014
Individuals	19,202	14,783	19,202	14,783

NOTE: This table displays the prevalence of potential confounding events for treated and control individuals at event times $\tau=-1$ and $\tau=-2$ using the baseline sample. All variables are expressed as percentages. Married and cohabitation refer to the share of individuals who are married or cohabiting, respectively, in that year. Family death refers to the share of individuals who experienced the death of a close family member (i.e., parents, siblings, children, or partner). Assault refers to the share of individuals with an inpatient hospitalization record with an external cause of harm code indicating assault or sexual assault. Pregnancy refers to the share of individuals with an inpatient hospitalization record with a recorded pregnancy. Burnout refers to the share of individuals with an inpatient hospitalization record with a primary or secondary diagnosis code indicating burnout (ICD-10 code F43).

Table A4: Robustness of Results on Labor Market Economic Outcomes

	Employment			Earnings			
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	N
Baseline	-0.110***	(0.004)	0.81	-54.426***	(1.521)	243.61	302,499
A. Choice of Δ							
$\Delta = 3$	-0.094***	(0.003)	0.80	-57.454***	(1.744)	235.75	299,249
$\Delta = 4$	-0.105***	(0.003)	0.81	-55.940***	(1.424)	239.33	303,910
$\Delta = 7$	-0.108***	(0.004)	0.82	-51.205***	(1.743)	250.10	285,730
$\Delta = 9$	-0.109***	(0.004)	0.83	-52.121***	(2.047)	257.97	253,877
B. Sample Construction							
All birth cohorts	-0.106***	(0.003)	0.81	-56.879***	(1.183)	249.90	509,374
All mental health hospitalizations	-0.098***	(0.002)	0.77	-47.959***	(1.016)	236.19	790,634
Matching: sex & birthyear	-0.109***	(0.004)	0.82	-54.303***	(1.793)	244.72	341,677

NOTE: This table displays difference-in-differences estimates using equation (1). The first row reproduces the baseline finding from Table 3. Panel (a) uses the same methodology described in Section 4 but varies the number of years between the treated and control groups' hospitalizations. Panel (b) changes the sample utilized; the first row considers all birth cohorts and the second row considers the baseline birth cohorts and all mental health hospitalizations. The third row in this panel matches each treated individual to a control individual of the same sex and birthyear. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A5: Joint Labor Market and Social Assistance Outcomes Around Mental Health Shock

	Event time $\tau = 1$			Event time $\tau = 3$			
Outcome	β	(S.E.)	$\bar{Y}$	β	(S.E.)	$\bar{Y}$	N
Employed, no assistance	-0.232***	(0.006)	0.59	-0.090***	(0.007)	0.56	302,499
Employed, with assistance	0.105***	(0.006)	0.22	-0.020***	(0.007)	0.25	302,499
Employed & temporary sickness	0.103***	(0.006)	0.19	-0.032***	(0.006)	0.22	302,499
Employed & permanent sickness	0.001	(0.001)	0.01	0.004***	(0.001)	0.01	302,499
Employed & welfare	0.017***	(0.003)	0.04	0.011***	(0.003)	0.03	302,499
Not employed, no assistance	-0.017***	(0.004)	0.08	0.003	(0.004)	0.07	302,499
Not employed, with assistance	0.143***	(0.004)	0.11	0.107***	(0.005)	0.12	302,499
Not employed & temporary sickness	0.094***	(0.003)	0.04	0.052***	(0.004)	0.05	302,499
Not employed & permanent sickness	0.023***	(0.002)	0.04	0.038***	(0.002)	0.04	302,499
Not employed & welfare	0.064***	(0.003)	0.05	0.040***	(0.004)	0.05	302,499

NOTE: This table displays difference-in-differences estimates using equation (1). Outcomes are defined as the product of employment status and receipt of social assistance. "$\bar{Y}$" refers to the average outcome of control units in the post-period. Standard errors are clustered at the individual level.

Table A6: Long-Run Economic Outcomes Around Mental Health Shock

	Social Assistance			Total Income			
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	N
Baseline	34.252***	(0.613)	27.18	-25.698***	(1.201)	308.92	302,499
A. Preferred matching							
5-year effect	45.979***	(0.720)	13.97	-44.716***	(1.644)	321.97	292,850
10-year effect	34.267***	(0.864)	15.30	-60.390***	(2.283)	345.05	172,520
B. Alternative matching							
5-year effect	46.858***	(0.703)	10.77	-48.226***	(1.628)	350.21	293,116
10-year effect	34.192***	(0.841)	11.52	-56.516***	(2.335)	371.25	172,804

NOTE: This table displays difference-in-differences estimates using the matched sample described in Section 7. "Baseline" refers to the estimates that use the baseline sample, reproduced from Table 3. "Preferred matching" refers to nearest-neighbor matching on birth year, gender, immigrant status, marital status, cohabitation status, education, one-digit occupation, municipality of residence, and earnings deciles prior to hospitalization. "Alternative matching" omits earnings deciles in the matching process. "Short-run effect" and "long-run effect" refer to five- and ten-year post-hospitalization windows. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A7: Economic Outcomes Around Mental Health Shock, by Individual Characteristics

	Employment			Earnings			
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	N
(1) Baseline	-0.110***	(0.004)	0.81	-54.426***	(1.521)	243.61	302,499
(2) Male	-0.111***	(0.005)	0.82	-59.841***	(2.381)	283.66	138,564
(3) Female	-0.109***	(0.005)	0.80	-49.790***	(1.948)	208.06	163,935
(4) Depression	-0.123***	(0.005)	0.84	-65.724***	(2.297)	256.58	136,617
(5) Anxiety	-0.123***	(0.007)	0.80	-55.764***	(2.987)	240.81	79,711
(6) Self-harm	-0.081***	(0.007)	0.77	-35.312***	(2.733)	223.12	86,171
(7) Shorter stay	-0.082***	(0.005)	0.80	-39.129***	(2.161)	233.95	147,778
(8) Longer stay	-0.141***	(0.005)	0.83	-70.473***	(2.135)	251.50	154,721
(9) Living alone	-0.123***	(0.005)	0.81	-62.840***	(2.402)	253.47	132,581
(10) Not living alone	-0.098***	(0.005)	0.81	-50.465***	(1.977)	237.86	169,228

NOTE: This table displays difference-in-differences estimates using equation (1). The first row reproduces the baseline finding from Table 3. Rows 2–3 split the sample by gender. Rows 4–6 split the sample by the condition underlying the hospitalization. Rows 7–8 split the sample by the length of stay in the hospital with "longer" meaning 3 or more nights. Rows 9–10 split the sample by whether someone lived alone or not at τ=−1. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A8: Economic Outcomes Around Mental Health Shock, by Labor Market Characteristics

	Employment			Earnings			
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	N
(1) Baseline	-0.110***	(0.004)	0.81	-54.426***	(1.521)	243.61	302,499
(2) Not employed, τ=−1	-0.134***	(0.012)	0.36	-28.653***	(2.908)	64.84	41,437
(3) Irregular employment	-0.129***	(0.010)	0.84	-51.511***	(4.215)	235.52	41,248
(4) Low wage	-0.117***	(0.005)	0.87	-57.211***	(1.943)	250.43	162,626
(5) High wage	-0.077***	(0.007)	0.93	-69.760***	(4.521)	371.58	49,936
(6) Small firm	-0.123***	(0.006)	0.67	-42.212***	(2.077)	171.80	126,754
(7) Medium firm	-0.110***	(0.006)	0.90	-65.840***	(3.059)	286.13	73,873
(8) Large firm	-0.099***	(0.005)	0.92	-61.630***	(2.795)	299.62	100,607
(9) Low wage, small firm	-0.127***	(0.008)	0.80	-48.160***	(3.098)	206.50	58,382
(10) Low wage, medium firm	-0.115***	(0.008)	0.91	-62.955***	(3.575)	272.95	45,628
(11) Low wage, large firm	-0.108***	(0.007)	0.92	-61.720***	(3.272)	275.54	58,616

NOTE: This table displays difference-in-differences estimates using equation (1). The first row reproduces the baseline finding from Table 3. Rows 2–5 split the sample by an individual's occupational category in τ=−1, and rows 6–8 split by firm size in τ=−1. "Irregular employment" refers to individuals who were employed but did not have an occupational category recorded, consistent with irregular or intermittent employment. "High wage" refers to managers and occupations requiring higher education. "Low wage" refers to armed forces, administrative, service, agricultural, manufacturing, and elementary occupations. Workers are classified as working at small, medium, or large firms if their corresponding firm size in τ=−1 is 1–9, 10–49, and 50+ workers, respectively. Rows 9–11 consider the impact of varying firm sizes only among low wage workers. "$\bar{Y}$" refers to the average outcome of control units in the post-period. All income outcomes are measured in thousands of SEK. Standard errors are clustered at the individual level.

Table A9: Employment Effects and Benefit Generosity Around the 2008–2009 Reforms

	Employment Effect ($\hat{\beta}_c^{Emp}$)	Temporary Sickness Benefits ($\bar{B}_y$)	Total Social Assistance ($\bar{B}_y$)
Pre-Reform	−0.161	38.96	64.95
Post-Reform	−0.125	30.34	52.05
% Change	−22.4%	−22.1%	−19.9%

NOTE: This table reports average employment effects and average post-hospitalization benefit receipt for the pre-reform (2005–2007) and post-reform (2009–2011) hospitalization cohorts. The employment effect ($\hat{\beta}_y^L$) is estimated using a dynamic difference-in-differences specification (equation 1). Average benefit receipt ($\bar{B}_y$) is measured in thousands of SEK among treated individuals in the post-hospitalization period. Column (2) reports estimates for temporary sickness benefits; column (3) reports analogous estimates for total benefits, encompassing temporary sickness, permanent disability, and welfare. The percent change row reports the percentage change between the pre- and post-reform periods.

B Data Appendix

B.1 Sample Construction

To select the sample of individuals who have ever been hospitalized for mental illness, we use inpatient records from the National Board of Health and Welfare. Specifically, we identify individuals who have either (i) a primary diagnosis in the category of mental, behavioral, and neurodevelopmental disorders (ICD-9 codes 290–319 or ICD-10 codes starting with F), or (ii) an external cause of harm code indicating poisoning or self-harm (ICD-9 codes starting with E85, E86, or E95, and ICD-10 codes starting with X40–X49 or X60–X84). Once this sample is selected, we pull information on these individuals from the various administrative datasets.

Using these individuals and their records, we construct a dataset at the person-year level from 1990 through 2022 (the years for which records from the Longitudinal Integration Database for Sickness Insurance and Labor Market Studies [LISA] are available). Because LISA data are only available for individuals ages 15 or 16 and older (depending on the year), we drop observations for an individual if they would have been younger than 15 in that year. We also drop observations for an individual if the death register indicates that they died in that year or in any previous year.

We then merge in annual records for each individual from LISA, the inpatient hospitalization register, pharmacy prescription data, and the population registers. We restrict the sample to individuals whose first hospitalization occurred in 1990 or later and whose age at that hospitalization was between 16 and 65.

For our baseline sample, we further restrict to individuals born in 1975 or later and whose first hospitalization was for depression, anxiety, or self-harm. Depression refers to records with a primary ICD-10 diagnosis coded as F32 or F33. Anxiety refers to records with a primary ICD-10 diagnosis coded as F40 or F41. Self-harm refers to records with an ICD-10 external cause of harm code X60–X84. Finally, in the baseline sample, we restrict attention to individuals whose first hospitalization occurred at age 25 or older.

To benchmark our main findings, we also estimate the labor market effects of two types of physical health hospitalizations. First, we identify hospitalizations for heart attacks and strokes using primary diagnosis codes, where heart attacks correspond to ICD-10 codes I21–I22 and strokes correspond to ICD-10 codes I60–I64. Second, we identify hospitalizations due to transportation-related accidents using external cause of harm codes. In ICD-10, we flag codes V00–V89, which cover accidents involving pedestrians, cyclists, motorcyclists, and occupants of cars, trucks, buses, and other road vehicles. In ICD-9, we flag codes E800–E829, covering railway accidents, motor vehicle accidents, and other road vehicle accidents. For both benchmarking samples, we apply the same empirical strategy as in the baseline analysis, restricting to first hospitalizations occurring between ages 16 and 65 to focus on individuals with stronger labor market attachment.

B.2 Other Variable Definitions

Individuals are assigned to occupational groups based on their one-digit SSYK 2012 occupation code. Occupation data are available from 2001 onward, so we restrict occupation-based analysis to hospitalization cohorts from 2002 onward, ensuring that the pre-hospitalization occupation is observed. We classify individuals into four groups: (i) high-wage workers (SSYK major groups 1–3: managers and occupations requiring higher education), (ii) low-wage workers (SSYK

major groups 0 and 4–9: armed forces, administrative, service, agricultural, manufacturing, and elementary occupations), (iii) individuals who were employed but lack an occupation code (reflecting exclusion from the wage statistics population, e.g., contract-based, fee-based, project-based, or marginal self-employment), and (iv) individuals who were not employed.

We assign individuals to establishment size groups based on the number of employed persons at the individual's workplace according to RAMS (*AntalSys* variable), classifying workers into small (1–9 employees), medium (10–49 employees), and large (50 or more employees) establishments. Individuals who cannot be linked to a specific establishment are excluded from the firm size analysis.

We construct three labor market transition variables using the *JobbByte* variable from LISA, which records year-to-year labor market transitions based on employer matches in administrative tax records. The variable distinguishes between four states: employer change among the continuously employed (code 1), transition from employment to nonemployment (code 2), transition from nonemployment to employment (code 3), and no employer change (code 4). Employer identity is tracked using a firm-level identifier that follows firms through ownership changes and reorganizations, ensuring that transitions reflect genuine job changes rather than administrative reclassifications. We construct three binary indicators corresponding to codes 1, 2, and 3.

We construct two household composition variables from LISA. First, we define a cohabitation indicator equal to one if the individual is living with a partner, based on the *FamStF* family status variable. Second, we construct a measure of household size counting the total number of family members. We start with a base of one (the individual themselves), add one if the individual is part of a couple household, and then add the number of children across all age groups. This latter variable allows us to classify individuals as living alone or not alone prior to the hospitalization.

We also construct several variables capturing other shocks using hospitalization records and other administrative registers. Burnout is defined as a primary diagnosis of ICD-10 code F43, following Nekoei et al. (2025). Assault is identified using external cause of harm codes in the hospitalization records: general assault corresponds to ICD-10 codes X85–Y09, and sexual assault corresponds to code Y05, primary diagnosis code T74.2, or Z04.4, following Adams et al. (2024b). In the analysis, assault refers to either general or sexual assault. Pregnancy is defined as a primary diagnosis beginning with ICD-10 chapter O (pregnancy, childbirth, and the puerperium).

Family deaths are identified by linking hospitalized individuals to their biological relatives and partners using family registers. For biological relatives (parents, children, and siblings), we use linkage files to obtain death dates and construct an indicator equal to one in any year in which a biological relative died. For partners, we use end-of-year records to identify partners or spouses and merge in their death dates, constructing an indicator equal to one if a person's partner/spouse in year t died in $t + 1$. The combined family death variable is equal to one if either a biological relative or partner died in the relevant year.

C Decomposition of Earnings Decline in Intensive and Extensive Margin

Let Y_t denote annual earnings in period t, and let $E_t \in \{0, 1\}$ be an indicator for employment status. Average earnings can be written using the law of iterated expectations as

$$\mathbb{E}[Y_t] = \mathbb{E}[Y_t \mid E_t = 1] \Pr(E_t = 1) + \mathbb{E}[Y_t \mid E_t = 0] \Pr(E_t = 0).$$

Given that by design $\mathbb{E}[Y_t \mid E_t = 0] = 0$, this expression simplifies to:

$$\mathbb{E}[Y_t] = \mathbb{E}[Y_t \mid E_t = 1] \Pr(E_t = 1).$$

The object of interest is the change in average earnings between a pre-hospitalization period, denoted by pre, and a post-hospitalization period t. Formally:

$$\Delta\mathbb{E}[Y_t] = \mathbb{E}[Y_t] - \mathbb{E}[Y_{\text{pre}}].$$

Substituting the expression for average earnings into this last equation yields:

$$\Delta\mathbb{E}[Y_t] = \mathbb{E}[Y_t \mid E_t = 1] \Pr(E_t = 1) - \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1] \Pr(E_{\text{pre}} = 1). \tag{3}$$

We are interested in decomposing the post-hospitalization change in average earnings into a component due to changes in earnings conditional on employment (the intensive margin) and a component due to changes in employment itself (the extensive margin). To separate these two margins, we add and subtract the term $\Pr(E_t = 1)\,\mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]$ in equation (3), which yields:

$$\begin{aligned}\Delta\mathbb{E}[Y_t] = \Pr(E_t = 1)\Big(&\mathbb{E}[Y_t \mid E_t = 1] - \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Big) \\ &+ \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Big(\Pr(E_t = 1) - \Pr(E_{\text{pre}} = 1)\Big).\end{aligned}$$

To simplify notation, define the change in earnings conditional on employment and the change in employment probability as:

$$\begin{gathered}\Delta\mathbb{E}[Y_t \mid E = 1] = \mathbb{E}[Y_t \mid E_t = 1] - \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1], \\ \text{and} \\ \Delta \Pr(E_t) = \Pr(E_t = 1) - \Pr(E_{\text{pre}} = 1).\end{gathered}$$

Using this notation, the change in average earnings can be re-written as

$$\Delta\mathbb{E}[Y_t] = \Pr(E_t = 1)\Delta\mathbb{E}[Y_t \mid E = 1] + \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Delta \Pr(E_t). \tag{4}$$

To express this decomposition in terms of pre-hospitalization quantities, note that

$$\Pr(E_t = 1) = \Pr(E_{\text{pre}} = 1) + \Delta \Pr(E_t).$$

Substituting this expression into previous equation (4) yields:

$$\Delta\mathbb{E}[Y_t] = \Pr(E_{\text{pre}} = 1)\Delta\mathbb{E}[Y_t \mid E = 1] + \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Delta \Pr(E_t) + \Delta \Pr(E_t)\, \Delta\mathbb{E}[Y_t \mid E = 1]$$

This expression separates the total change in earnings into 3 components: a term reflecting changes in earnings conditional on employment, a term reflecting changes in employment, and an interaction term capturing the joint contribution of both margins.

In the empirical implementation in Appendix Table A2, we use a first-order approximation evaluated at pre-hospitalization levels, abstracting from the interaction term:

$$\Delta\mathbb{E}[Y_t] \approx \Pr(E_{\text{pre}} = 1)\Delta\mathbb{E}[Y_t \mid E = 1] + \mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Delta \Pr(E_t). \tag{5}$$

The first term represents the contribution of the intensive margin, while the second term captures the contribution of the extensive margin. We compute these terms using pre-hospitalization average outcomes as well as difference-in-differences estimates. The share of the total change attributable to the extensive margin is then given by:

$$\text{Extensive Margin Share}_t = 100 \times \frac{\mathbb{E}[Y_{\text{pre}} \mid E_{\text{pre}} = 1]\Delta \Pr(E_t)}{\Delta\mathbb{E}[Y_t]}.$$