The Early-Career Effects of Mental Illness*

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Abstract

Mental illness tends to arise when young workers are establishing their positions in the labor market. Using nationwide employer-employee records and medical claims data from Colombia, we use a matched difference-in-differences design to estimate the long-run effects of common mental health disorders on earnings. Among a cohort of young, formally-employed workers, newly diagnosed anxiety and depression reduce earnings for at least five years following diagnosis. In the short run, earnings losses mainly reflect fewer days worked; in the longer run, they reflect lower employment and wages. Part of the earnings effect is driven by reduced employment at firms with high wage premia, consistent with impaired mobility up the job ladder. To assess for bias arising from reverse causality, we implement a mass layoff design and find no evidence of worsening mental health following plausibly exogenous job loss.

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1 Introduction

Mental health disorders affect a substantial share of the global population and often emerge in early adulthood, when workers invest intensively in human capital and sort into higher-productivity firms (Murphy and Welch, 1990; Topel and Ward, 1992). In standard models of human capital accumulation and on-the-job search, temporary disruptions to skill investment and job mobility can generate persistent earnings effects through dynamic complementarities and state dependence (Ben-Porath, 1967; Burdett and Mortensen, 1998; Cunha and Heckman, 2007; Postel-Vinay and Robin, 2002). The symptoms associated with mental health disorders—including fatigue, impaired concentration, and reduced motivation—may lower human capital accumulation and search intensity, raising the question of whether shocks to mental health have persistent effects on career trajectories.

Existing evidence on the economic consequences of mental illness is largely drawn from cross-sectional associations between measures of mental health and labor market outcomes, which are prone to confounding from unobserved heterogeneity and reverse causality. Randomized evaluations of mental health treatments, while shielded from concerns about identification, rarely examine labor market outcomes; those that do tend to involve selected populations, have short follow-up horizons, or lack the granular data needed to elucidate the mechanisms that may give rise to earnings penalties. As a result, there is limited causal evidence on the long-run effects of mental illness on earnings and the mechanisms through which such effects arise.

In this paper, we use nationwide administrative data from Colombia to estimate the causal effects of common mental health disorders on labor market outcomes. Our research question is motivated by Figure 1, which shows age-earnings profiles and encounters for anxiety and depression across the population of formally employed workers. Consistent with seminal work by Murphy and Welch (1990), earnings growth is concentrated in the early career years, a period during which encounters for anxiety and depression are also most prevalent. This descriptive pattern raises the question of whether the incidence of mental health conditions during early adulthood may have persistent effects on labor market outcomes.

To investigate this question, we use a matched difference-in-differences approach that follows young, formally employed workers before and after the onset of anxiety and depression and compares their labor market trajectories to those of observationally similar workers. This approach allows us to trace the dynamic effects of mental health shocks on earnings over multiple years and to examine the mechanisms underlying these effects. The key identifying assumption underlying our approach is that, absent the onset of a new diagnosis of anxiety or depression, the earnings trajectories of individuals who received a diagnosis and those who did not would have evolved in parallel. This assumption could be violated if the timing of newly diagnosed mental illness is correlated with other time-varying shocks that also affect earnings, including physical health shocks or adverse labor market events. We assess the validity of our identification assumption after presenting baseline results.

To derive our estimates, we begin by linking employer-employee records for all formally employed individuals to the universe of health care utilization records between 2009 and 2019. The resulting individual-month panel allows us to observe both granular labor market outcomes, including formal employment, days worked, wage, and employer, as well as mental health disorders identified using physician-assigned diagnosis codes from outpatient and emergency department encounters. To isolate plausibly exogenous shocks to mental health, we follow Dobkin et al. (2018a) and restrict attention to encounters where individuals are diagnosed with anxiety or depression and were not diagnosed with any mental health disorder in the preceding three years. Under this definition, our estimates capture the labor market effects of *new* episodes of anxiety or depression—periods of active symptoms severe enough to prompt medical evaluation and a formal diagnosis. Such episodes are intended to reflect either a first lifetime episode or recurrence after at least three years of remission; in both cases, they plausibly correspond to a worsening of mental health that affects cognitive functioning, energy, or motivation.

Having identified shocks to mental health, we use propensity score matching to construct a comparison group of workers who are never diagnosed with a mental health condition during our sample period. Following Weber et al. (2026), we additionally require matched controls to work at the same baseline firm as treated workers. This restriction serves two purposes: it helps recover the labor market path that treated workers would likely have followed in the absence of diagnosis, and it mitigates concerns that our estimates are driven by firm-level shocks to workplace conditions that both affect earnings and precipitate mental illness. We then compare the earnings trajectories of treated workers to those of their matched controls to estimate the causal effect of mental illness on labor market outcomes.

We find that newly diagnosed anxiety and depression lead to earnings losses that persist over time: earnings are 4.6 percent lower six months after onset, 7.4 percent lower after two years, and 5.5 percent lower after five years. To shed light on the labor market mechanisms underlying these effects, we decompose the changes in earnings into changes in employment, days worked, and wages. In the first three months following diagnosis, the earnings decline is driven primarily by adjustment along the intensive margin, reflecting reductions in days worked among those who remain employed. By six months, extensive-margin adjustments and wage declines account for the majority of the earnings loss, a pattern that persists over the longer run. Taken together, these results indicate a dynamic adjustment process in which workers initially reduce labor supply within jobs and subsequently experience job loss or transition into lower-paying jobs.

Long-run wage declines may reflect a combination of within-firm wage adjustments and transitions to lower-paying firms. To distinguish between these channels, we follow Abowd et al. (1999) to estimate an Abowd-Kramarz-Margolis (AKM) decomposition of wages into worker and firm components using the largest connected set of formal-sector workers and firms and examine the dynamic effects of mental illness on the resulting firm components for individuals in our analysis

sample. We find that newly diagnosed anxiety and depression cause a long-run decline in the probability of working at firms with high wage premia, consistent with impaired upward mobility up the job ladder. Such impairment could stem from the effects of mental illness on search intensity, on-the-job performance, or other factors influencing early-career advancement.

Having established our baseline results, we address several possible concerns regarding identification. Our event study design relies on two core assumptions: first, that there are no anticipatory changes in earnings prior to the onset of mental illness, and second, that no time-varying shocks coincide with diagnosis and differentially affect earnings for treated workers. We test the no-anticipation assumption using pre-diagnosis trends. When we conduct event studies around any mental health diagnosis—pooling new and ongoing cases—we observe clear pre-trends in earnings. In contrast, when we restrict attention to new diagnoses, consistent with the approach used by Dobkin et al. (2018a), pre-trends in earnings are no longer evident. This supports our focus on new cases, which are plausibly less likely to reflect anticipatory labor market adjustments or the lingering effects of prior episodes of poor mental health.

We next address potential identification concerns in the absence of pre-trends. The primary concern in our setting is reverse causality, where worsening labor market conditions may precipitate mental illness. Importantly, such shocks need not generate gradual earnings declines prior to diagnosis; they may occur contemporaneously with diagnosis, thus evading detection in tests of parallel trends. Our use of a matched control group of observationally similar workers within the same firm suggests that firm-level shocks are unlikely to account for the observed earnings losses. To further assess for the possibility of reverse causality, we follow Sullivan and von Wachter (2009) and implement a mass layoff design to isolate plausibly exogenous job loss and examine subsequent health outcomes. We find no increase in diagnoses of depression or anxiety, further suggesting that labor market shocks alone are unlikely to explain our results.

Our findings relate to a growing literature on the effects of mental health on labor market outcomes. Economists have long conceptualized health as a form of human capital that affects labor supply and productivity (Becker, 1964; Grossman, 1972). Unlike most physical illnesses, mental health disorders disproportionately affect young adults and impair cognitive function, raising the question of whether and how they affect labor market outcomes. As summarized by Currie and Madrian (1999), early empirical work documented negative cross-sectional associations between measures of mental health and employment and earnings, though estimates varied widely and were difficult to interpret given concerns about unobserved heterogeneity and reverse causality.

More recent studies have attempted to address these concerns using randomized trials and quasi-experimental research designs that examine the effects of mental health treatment (Banerjee et al., 2017; Biasi et al., 2021; Blattman et al., 2023; de Oliveira et al., 2023; Haushofer et al., 2023; Mahomed, 2020; Oparina et al., 2024; Trautmann et al., 2016). This work generally finds that treating mental illness improves earnings in the short run but provides limited evidence on

longer-run effects or on the labor market mechanisms through which mental health may shape outcomes over the life cycle. Our study advances this literature by studying a well-defined shock to mental health, tracing labor market outcomes over a long horizon, and doing so in a nationwide sample of formally employed workers. We view our approach as closest in spirit to Biasi et al. (2021), who show that the introduction of lithium improved long-run labor market outcomes for individuals with bipolar disorder in Sweden, and to Dobkin et al. (2018a), who document persistent financial consequences of health shocks resulting in hospitalization among older adults in the United States. We complement these studies by showing that common mental health disorders alter early career trajectories in ways that generate lasting labor market consequences.

Our findings on labor market dynamics relate to a longstanding literature in labor economics emphasizing the central role of early career trajectories in shaping long-run earnings growth. Early descriptive work in this literature showed that wage growth is steepest at the beginning of workers' careers and that a substantial share of this growth arises from job-to-job transitions rather than within-firm wage increases (Murphy and Welch, 1990; Topel and Ward, 1992). These empirical patterns motivated search-based models of the labor market in which workers climb a job ladder over time and current job quality affects the distribution of future opportunities through state dependence (Burdett and Mortensen, 1998; Postel-Vinay and Robin, 2002). In these models, persistent wage differences can arise even from temporary shocks if such shocks alter early labor market trajectories.

We interpret mental health shocks as a source of time-varying worker heterogeneity that plausibly affects on-the-job productivity and search intensity, thereby shifting workers' positions on the job ladder. Although evidence from the medical literature indicates that episodes of depression and anxiety are typically short-lived, with mean durations of three to six months, we find persistent declines in firm-level wage components. This pattern is consistent with state dependence rather than permanent losses in productivity. Our results thus identify mental health as an underemphasized source of long-run wage variation, alongside other quantitatively important factors such as early-career firm placement (Card et al., 2013).

Finally, we relate to a longstanding literature on the association between income and health. Early work in this tradition established that health is strongly associated with socioeconomic position (Marmot, 2010). More recent studies using large survey and administrative datasets have continued to find strong income-health gradients across the lifecycle (Case and Deaton, 2015; Chetty et al., 2016; Currie and Schwandt, 2016; Danesh et al., 2024). A central empirical challenge in this literature is disentangling the direction of causality underlying these gradients. This challenge is particularly salient for mental illness, where declines in earnings or job quality may themselves precipitate psychological distress (Meisenzahl et al., 2024; Thomson et al., 2022). Despite this concern, we find no evidence of earnings pre-trends prior to the onset of depression or anxiety and no effect of mass layoffs on the incidence of these conditions. Together with our main results, these findings support a causal interpretation running from mental health to earnings and lend empirical

support to recent work conceptualizing mental illness as a "cognitive tax" that can generate poverty traps even in the absence of material shocks (Ridley et al., 2020).

The remainder of this paper is organized as follows. Section II provides background on our research setting, including the Colombian labor market, the landscape of mental health care in Colombia, and information on depressive and anxiety disorders. Section III describes the data and construction of our analysis sample. Section IV describes our empirical approach. Section V presents our results on the effects of mental health episodes on earnings, including decomposition of the effect into employment, days worked, and wages, heterogeneity analyses, and robustness. Section VI presents ongoing work on the effects of treatment and discusses our plans to further refine these results in the coming three months. Section VII concludes.

2 Background

2.1 The Colombian Labor Market

The Colombian labor market includes a range of employment arrangements and is characterized by considerable movement across jobs and employment states over the life cycle. A central distinction within this labor market is between formal and informal employment. Formal-sector jobs are generally defined by compliance with labor regulations and mandatory contributions to social insurance programs, including health insurance and pensions. These jobs are more likely to involve written contracts, regular payroll earnings, and employment relationships recorded within the social security system (OECD, 2015). Informal jobs operate outside these arrangements and include self-employment, unpaid family work, and wage employment without formal contracts or payroll-based contributions. Relative to formal employment, informal jobs are associated with lower and more unstable earnings, lower-productivity activities, and reduced access to social protection. Formal jobs, in contrast, are more likely to be found in larger firms, to provide stable employment relationships, and to offer higher average labor income and greater earnings stability (Bernal, 2009; Peña, 2013).

In 2019, approximately 54% of workers were employed in the formal sector and 46% in the informal sector (DANE, 2019). Informality is more common among younger and lower-income workers and among those employed by small firms or in self-employment. Sectoral attachment is highly persistent: approximately 94.7% of formal workers remain formal after one year, while only 3.1% of informal workers transition into formality (Mondragón-Vélez et al., 2009). These features are central to our empirical design. Because PILA captures only formal-sector employment and earnings, our analysis focuses on workers with strong attachment to the formal labor market, for whom earnings, job transitions, and firm affiliation are reliably observed in administrative data.

2.2 Access to Mental Health Care

Colombia has near-universal health insurance coverage, with recent estimates indicating that roughly 98% of the population is insured (Ministerio de Salud y Protección Social de Colombia, 2025). The health system is organized around two main insurance regimes. The contributive regime primarily covers individuals with formal employment relationships and is financed through payroll contributions from workers and employers, while the subsidized regime primarily covers individuals outside formal employment and is financed largely by the government. Because formal workers are generally required to affiliate with the contributive regime, they are much more likely to be insured through it. Within the contributive regime, access to mental health care may still differ across insurers and over time because of variation in provider networks, referral practices, and specialist availability.

Mental health care became a more explicit policy priority in Colombia with the passage of the National Mental Health Law in 2013. The law formally recognized mental health care as a fundamental right and sought to reduce stigma, promote early detection, and expand access to care (Chaskel et al., 2015; Congreso de la República de Colombia, 2013). It was motivated in part by recognition of the psychological consequences of Colombia's long history of armed conflict, violence, and displacement. Subsequent reforms were aimed at expanding the availability of mental health services and strengthening the integration of mental health care into primary care settings.

Individuals in Colombia may access mental health care through several entry points, including primary care clinics, outpatient specialist care, emergency departments, and inpatient services. Most care for common mental health disorders is initiated in primary care. Patients typically first present to a general practitioner, who evaluates symptoms, initiates treatment when appropriate, and may refer to a specialist. Depending on their insurer and local provider network, individuals may also bypass primary care and obtain care directly from psychologists or psychiatrists.

2.3 Depressive and Anxiety Disorders

Depression and anxiety are among the most common mental health disorders worldwide and are especially prevalent during adolescence and early adulthood (Centers for Disease Control and Prevention , CDC; Weinberger et al., 2018). The median age of onset is 30 for depression and 31 for generalized anxiety disorder (Solmi et al., 2022). Both can impair domains of functioning relevant to labor market performance, including concentration, motivation, sleep, energy, decision-making, and the ability to manage interpersonal stress.

The onset of depressive and anxiety episodes is often associated with identifiable stressors. Common precipitating factors for depressive episodes include conflict within close relationships, occupational or academic stress, and major changes in family or social circumstances (Meisenzahl et al., 2024). Work-related stressors are especially relevant to our research question, since some mental health episodes may be triggered or exacerbated by adverse labor market conditions, giving rise to

the possibility of reverse causality when studying the association between mental health and labor market outcomes.

Major depressive disorder is characterized by symptoms such as depressed mood, loss of interest, sleep disturbance, guilt, low energy, difficulty concentrating, appetite changes, psychomotor slowing, and suicidal thoughts. Diagnostic criteria require that an individual experience at least five symptoms over a two-week period, including either depressed mood or loss of interest, with symptoms causing clinically significant distress or impairment and not being better explained by another cause (MDCalc, 2025). Anxiety disorders are characterized by excessive fear, worry, and related behavioral disturbances, often accompanied by restlessness, fatigue, irritability, sleep disruption, difficulty concentrating, and physical symptoms such as palpitations or shortness of breath. As with depression, diagnosis depends on symptom severity, duration, and functional impairment.

When individuals present for care, clinicians determine whether they meet diagnostic criteria and record the relevant diagnosis in the medical record. Under the ICD coding system, panic disorder and generalized anxiety disorder are coded as F41.x. In the case of depression, clinicians should distinguish between depressive episodes and recurrent depressive disorder. First or single depressive episodes should be coded as F32.x, while recurrent depressive disorder should be coded as F33.x. Diagnoses may be recorded as primary or secondary depending on whether the mental health condition is determined to be the main reason for the encounter.

Treatment for depression and anxiety can be delivered by generalists and specialists, who differ in the services they are able to provide. Primary care physicians can evaluate symptoms, make diagnoses, and prescribe pharmacologic treatment. Psychologists may provide psychotherapy but cannot independently prescribe medications. Psychiatrists can provide both medication management and psychotherapy. A large body of randomized evidence indicates that both psychotherapy and pharmacologic treatment reduce symptoms severity in the short-term (e.g., 90 days), though longer-run effects are less certain (Cuijpers et al., 2013). There is growing evidence that treatment quality varies across providers and that specialists may achieve better outcomes than non-specialists (Costantini, 2024; Cuddy and Currie, 2024; Currie and Zhang, 2024; Currie and MacLeod, 2020; Wang and Patten, 2007).

Both depression and anxiety often have an episodic course, characterized by periods of symptom exacerbation followed by partial or full remission, either spontaneously or with treatment. For depression, the median duration of an episode among treated and untreated individuals is estimated to range from three to six months, with approximately 20% of cases persisting for more than a year (Koenen et al., 2017). Recurrence is common: conditional on one prior depressive episode, the probability of an additional episode is roughly 50%, rising to 90% among individuals with three or more prior episodes (Hardeveld et al., 2010). For anxiety disorders, the median duration is 7.5 months and the mean duration is 15.2 months; approximately 39% of cases persist for more than a year (Ten Have et al., 2021).

3 Data

Our analysis uses two primary sources of national administrative panel microdata: Planilla Integrada de Liquidación de Aportes (PILA), and Registro Individual de Prestaciones de Salud (RIPS).

PILA, maintained by Colombia's Ministry of Health and Social Protection, records monthly contributions to health insurance, pension funds, and workers' compensation for formally employed workers, including wage earners and formally self-employed individuals. Covering the period from July 2008 through December 2019, the data provide detailed labor market information at the individual-month level, including firm identifiers, earnings, formal employment, days worked, and wages for more than 20 million individuals and over 300,000 firms.

RIPS, also maintained by the Ministry of Health and Social Protection, contains administrative claims records for healthcare encounters in Colombia, including outpatient visits, emergency department visits, and hospitalizations. Spanning 2009 to 2019, the data include patient demographics, setting (outpatient, inpatient, emergency department), specialty (e.g., primary care, psychiatry), municipality of residence, and primary and secondary ICD-10 diagnosis codes.

We link PILA and RIPS using individual identifiers, allowing us to track workers longitudinally across both labor market and healthcare records. The linked data make it possible to identify diagnoses of depression and anxiety in claims data and relate the timing of these diagnoses to trajectories of labor market outcomes.

3.1 Sample Construction

To identify the effect of depression on earnings, we make several sample restrictions aimed at identifying individuals who experience their first episode of anxiety or depression and to reliably measure the effect on earnings. We start by using PILA to identify the population of wage earners in 2009, where wage earners are defined as individuals who worked in the formal labor market for at least nine out of twelve months. While this restriction has the limitation of narrowing the scope of our analysis, it has two important benefits. First, by restricting our analysis to individuals with higher levels of attachment to the labor market, who are more likely to have stable earnings patterns at baseline, we are more likely to capture impacts on earnings. Second, given that we do not observe earnings in the informal labor market, this restrictions helps ensure that we are capturing individuals who are most attached to the formal labor market at baseline.

We then merge this dataset of wage earners to their medical records in RIPS, restricting to outpatient or emergency department visits where individuals presented for anxiety or depression. This merge identifies individuals who were ever diagnosed with anxiety or depression during our observation period. If individuals were diagnosed with depression or anxiety during multiple encounters, we keep all such encounters. We identify depressive and anxiety episodes by selecting encounters where physicians assigned a primary ICD-10 code beginning with F32 (depressive episode)

or F41 (non-phobia anxiety disorders, including panic disorder and generalized anxiety disorder). We exclude F33 (depressive disorder) since these are defined as recurrent encounters for depressive symptoms, and we do not include F40 (phobic anxiety disorders) since these are often tied to specific triggers or settings and often begin in childhood, making it difficult for us to capture disease onset (Solmi et al., 2022). We also do not include cases in which F32 or F41 was coded as a secondary diagnosis code since these visits are presumably more related to an alternative disease. Table A.1 lists the specific codes used.

Having identified the population of wage earners diagnosed with depression or anxiety, we restrict to individuals who are between the ages of 18 and 45 years old at the time of diagnosis. The purpose of this restriction is to increase the likelihood that we identify individuals' first depressive or anxiety episode, rather than a repeat episode that is less likely to represent a shock to mental health. If we were to include older individuals in our sample, we would likely falsely classify many first *observed* depressive or anxiety episodes as first *overall* depressive episodes. For example, if we were to include individuals who were 46 in 2009, we would observe their mental health encounters between ages 46 and 57 given the 11-year window during which we observe RIPS. The median age of onset of depression is estimated to be 30 (Solmi et al., 2022), so many of the 46-year-olds whom we identify with depression or anxiety may have had their first mental health episodes before they could be observed in our data.

Next, following Dobkin et al. (2018b), we restrict to individuals with no prior encounters for any mental health problem in the three years prior to the index encounter. This restriction is intended to identify larger, unexpected shocks to mental health, rather than follow-up visits for an existing diagnosis. We dropped cases where individuals had prior encounters for mental health problems other than depression or anxiety to account for possible diagnostic errors. For example, if a patient presented with bipolar disorder in the months prior to the index visit (for depression), it is plausible that the patient was misdiagnosed during the initial visit and had actually presented with depression. We would thus not want to classify the index visit as the patient's first visit for depression.

Our final sample restriction is intended to address the concern that depression and anxiety are temporally correlated with other health conditions which may themselves affect labor market outcomes. Given our focus on the younger adult population, the association between the perinatal period and depression, and the effect of pregnancy and childbirth on economic outcomes of women, pregnancy is an important possible confound in our setting. To address this concern, we remove individuals with any records of pregnancy or parental leave in the three years prior to their depressive or anxiety episode. We show results aimed at addressing possible confounding due to health conditions other than pregnancy in Section 5.

Appendix Table A.2 provides a step-by-step accounting of how the initial population of stable wage earners is filtered into our final sample, and reports our sample characteristics before and after each of the aforementioned sample restrictions.

3.2 Construction of matched comparison group

Having identified workers with an incident diagnosis of depression or anxiety, we construct a matched comparison group of workers who are never diagnosed with a mental health condition during the sample period. The goal of this exercise is to recover the counterfactual labor market path that treated workers would have followed in the absence of diagnosis. Relative to designs that use not-yet-treated workers as controls, this approach reduces concern that control workers are already experiencing worsening mental health prior to their first observed diagnosis, which would tend to attenuate estimated treatment effects.

Let $\mathcal{T}$ denote the set of treated workers, where treatment is defined as a first observed diagnosis of depression or anxiety during the sample period. For each treated worker $i \in \mathcal{T}$, let t_i^* denote the month of first diagnosis. Define the employment indicator $E_{it} \in \{0, 1\}$ to equal one if worker i is formally employed in month t, and let J_{it} denote worker i's employer in month t. If worker i is employed by multiple firms in month t, we define J_{it} as the employer paying the highest monthly earnings.

We define worker i's baseline firm as the modal employer over the thirty-six months preceding diagnosis:

$$J_i^0 \;\equiv\; \arg\max_j \left|\{t \in [t_i^* - 36,\, t_i^* - 1] : J_{it} = j\}\right|,$$

and the baseline month as the most recent month of formal employment at that firm:

$$b_i \;=\; \max\{t \in [t_i^* - 36,\, t_i^* - 1] : J_{it} = J_i^0\}.$$

Workers with $E_{it} = 0$ for all $t \in [t_i - 36, , t_i - 1]$ are classified as baseline non-employed. We similarly define I_{it} as the worker's industry and C_{it} as the worker's city in month t, both evaluated at the dominant employer in that month. Baseline industry and city are therefore $I_i^0 \equiv I_{ib_i}$ and $C_i^0 \equiv C_{ib_i}$.

For each treated worker $i \in \mathcal{T}$, we define a matched risk set consisting of never-treated workers who share the same baseline labor market environment in month b_i. Let $\mathcal{N}$ denote the set of never-treated workers. The candidate control set for worker i is

$$\mathcal{R}_i = \left\{k \in \mathcal{N} : E_{kb_i} = 1,\; J_{kb_i} = J_i^0,\; I_{kb_i} = I_i^0,\; C_{kb_i} = C_i^0\right\}.$$

Restricting to comparisons within baseline firm absorbs a broad class of employer-side shocks that could otherwise confound the estimates, including changes in labor demand, compensation policies, promotion opportunities, and workplace conditions. Industry and city ensure comparability for firms that operate across sectors or locations.

Within each risk set $\mathcal{R}_i$, we implement propensity score matching. Let D_i indicate treatment status, and let X_i denote a vector of pre-diagnosis characteristics measured at or before b_i, including

prior earnings, days worked, sex, birth year, and prior healthcare utilization. Matching on prior healthcare utilization is important because it makes treated workers comparable to controls with similar prior attachment to the healthcare system, reducing concern that diagnosis merely proxies for differences in underlying health, care-seeking behavior, or the probability that symptoms are recorded in administrative data. We estimate the propensity score using a logit model of treatment status on these pre-diagnosis characteristics:

$$\Pr(D_i = 1 \mid X_i) = \Lambda(X_i'\gamma),$$

where $\Lambda(\cdot)$ denotes the logistic cumulative distribution function. Let $\hat{p}_i = \Lambda(X_i'\hat{\gamma})$ denote the estimated propensity score. Each treated worker is then matched to the never-treated worker in the same baseline firm-industry-city cell with the closest value of $\hat{p}_i$, using one-to-one nearest-neighbor matching without replacement.

3.3 Sample Characteristics

Appendix Table A.2 shows how the characteristics of the analysis sample evolve as we impose the restrictions described in Section 3.1. Column (1) reports summary statistics for all individuals with wage earnings in the formal labor market for at least 270 days in 2009. Column (2) restricts the sample to workers ages 18–45 in 2009. Column (3) further restricts the sample to workers diagnosed with depression or anxiety between ages 18 and 45, reducing the sample size by 86.7 percent. Relative to the broader working population, this group is more likely to be female and has greater use of outpatient and emergency department care.

Column (4) further restricts the sample to individuals with a primary diagnosis of depression or anxiety, reducing the sample by an additional 34.5 percent. Relative to workers with either a primary or secondary diagnosis, those with a primary diagnosis have somewhat higher wages but otherwise appear similar on observable characteristics. Column (5) excludes workers with prior mental health history, reducing the sample by 76.2 percent. These newly diagnosed workers are younger, have lower wages, fewer outpatient visits, and more emergency department visits than workers who are not newly diagnosed. Finally, Column (6) excludes workers with a history of pregnancy or parental leave, reducing the sample by a further 13.3 percent. As in Section 3.2, we denote this final treatment sample by $\mathcal{T}$.

Table A.3 compares the individuals in Column (6) of Appendix Table A.2 to the workers from the final treatment sample that find a nearest-neighbor match in our propensity score procedure. Of the 58,813 workers in the original treated sample $\mathcal{T}$, 51,359 are retained after matching, implying a reduction of about 12.7 percent. This attrition arises because some baseline industry-city cells contain too few eligible coworkers to support one-to-one matching under our never-treated definition, and because potential controls are excluded if they have prior mental health encounters or missing

matching covariates.

Appendix Figure A.1 shows covariate balance between treated and never-treated workers before and after propensity score matching. Prior to matching, there are statistically significant differences between treated and never-treated workers; treated workers (i.e., those with diagnosed mental health conditions) are younger, have lower earnings, and are more likely to be female. After matching, there are no statistically significant differences between treated and control workers in these characteristics.

Table 1 reports descriptive statistics for the final matched samples used in our main event study analysis. Column (1) shows the characteristics of the 51,359 workers diagnosed with depression or anxiety. The average age is 31, consistent with our focus on younger workers, and 58 percent are female, in line with the higher prevalence of common mental health conditions among women. Ninety-two percent live in a large municipality. Average annual wage earnings are 8,802 USD, equal to 2.3 times the minimum wage. Individuals have on average 0.29 outpatient visits and 0.08 emergency department visits per year. Column (2) reports the characteristics of the 51,359 matched controls with no diagnosed mental health disorder. Relative to diagnosed individuals, controls are less likely to be female, have lower wages, take fewer non-labor sick leave days, and have fewer outpatient and emergency department visits.

4 Empirical Strategy

To estimate the effect of depression and anxiety on labor market outcomes, we use monthly panel data and a difference-in-differences design with matched controls. Treatment is defined as the first observed outpatient or emergency department encounter with a primary diagnosis of depression or anxiety among individuals with no mental-health-related visits in the preceding three years. We use this approach to follow labor market outcomes in the months before and after diagnosis, comparing treated workers to an observationally similar set of workers who were never diagnosed with depression or anxiety during our sample period.

4.1 Event study specification

Using treated individuals and their matched controls, we estimate the following event study specification:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=\underline{C}}^{\bar{C}} \beta^k D_{it}^k + u_{it}\,, \tag{1}$$

where d_i denotes the month of individual i's first observed depressive or anxiety episode, and $D_{it}^k = \mathbf{1}(t = d_i + k)$ is an indicator equal to one if individual i is k periods away from diagnosis at time t, with k allowed to be negative. The parameters α_i denote individual fixed effects and γ_t denote calendar-time fixed effects. The constants $\underline{C} < 0$ and $\bar{C} > 0$ define the event window. The coefficients β^k are the parameters of interest and trace the evolution of earnings around the

onset of a mental health episode. We normalize the coefficient for the period immediately preceding diagnosis ($k = -1$) to zero such that all estimates are interpreted relative to that baseline period. Standard errors are clustered at the individual level to account for serial correlation within workers over time.

Because treatment timing is staggered and treatment effects may vary across diagnosis cohorts and over event time, we rely on the estimator proposed by Callaway and Sant'Anna (2021). This approach identifies cohort- and time-specific average treatment effects on the treated and aggregates them into event-time effects.

Our identifying assumption is a conditional parallel trends condition: absent diagnosis, diagnosed workers and their matched controls would have experienced similar changes in outcomes over time. Exact matching on baseline firm and baseline month is designed to reduce concerns about differential shocks in the workplace environment at baseline, while propensity score matching improves comparability on observed worker characteristics. We assess the credibility of this assumption by examining pre-diagnosis dynamics (the estimated lead coefficients) and by checking balance in baseline covariates and baseline outcomes.

4.2 Decomposition of the earnings effect

Mental health episodes may reduce earnings through three distinct channels: by lowering the probability of employment (the extensive margin), reducing days worked conditional on employment (the intensive margin), or lowering wages conditional on working. To distinguish among these mechanisms, we decompose the earnings response into these three margins as follows.

Let β^τ denote the event study coefficient from the earnings regression at event time τ, with $\tau = -1$ as the omitted reference period. β^τ is the change in mean earnings at event time τ relative to the baseline period:

$$\beta^\tau = \Delta y_\tau.$$

To decompose the earnings effect, we begin from the accounting identity

$$y_\tau = e_\tau d_\tau w_\tau,$$

where y_τ, e_τ, d_τ, and w_τ denote mean monthly earnings, employment, days worked, and daily wage at event time τ. The total earnings effect at event time τ is

$$\Delta y_\tau \equiv y_\tau - y_{-1} = e_\tau d_\tau w_\tau - e_{-1} d_{-1} w_{-1}.$$

We decompose this change using a midpoint decomposition. The contribution of the extensive

margin is

$$\Delta y_\tau^e = (e_\tau - e_{-1}) \left(\frac{d_\tau + d_{-1}}{2} \right) \left(\frac{w_\tau + w_{-1}}{2} \right) = \beta_e^\tau \left(\frac{d_\tau + d_{-1}}{2} \right) \left(\frac{w_\tau + w_{-1}}{2} \right),$$

where β_e^τ is derived from an event study regression similar to Equation (1) but with employment on the left-hand side. Similarly, the contribution of the intensive margin is

$$\Delta y_\tau^d = (d_\tau - d_{-1}) \left(\frac{e_\tau + e_{-1}}{2} \right) \left(\frac{w_\tau + w_{-1}}{2} \right) = \beta_d^\tau \left(\frac{e_\tau + e_{-1}}{2} \right) \left(\frac{w_\tau + w_{-1}}{2} \right),$$

and the contribution of daily wages is

$$\Delta y_\tau^w = (w_\tau - w_{-1}) \left(\frac{e_\tau + e_{-1}}{2} \right) \left(\frac{d_\tau + d_{-1}}{2} \right) = \beta_w^\tau \left(\frac{e_\tau + e_{-1}}{2} \right) \left(\frac{d_\tau + d_{-1}}{2} \right),$$

where β_d^τ and β_w^τ are the corresponding event study coefficients from regressions with days worked and daily wage on the left-hand side, respectively. The total earnings effect is then approximated by the sum of these three components:

$$\beta^\tau = \Delta y_\tau \approx \Delta y_\tau^e + \Delta y_\tau^d + \Delta y_\tau^w.$$

Since monthly earnings are multiplicative in employment, days worked, and daily wages, the total change in earnings includes higher-order interaction terms across these margins. Our midpoint decomposition provides a transparent approximation that attributes the earnings effect to the three components, but the components need not sum exactly to the total estimated effect at each event time.

4.3 Decomposition of the wage effect

Changes in wages following a mental health episode may arise because workers move to firms that pay lower wage premia or because wages decline for reasons not captured by firm wage premia. To distinguish between these channels, we decompose the wage effect using a two-way fixed-effects approach following Abowd, Kramarz, and Margolis (1999). Specifically, for worker i employed at firm $j(i,t)$ in month t, we estimate

$$\log w_{it} = \alpha_i + \psi_{j(i,t)} + X_{it}'\Gamma + \varepsilon_{it}, \tag{2}$$

where α_i is a worker fixed effect, $\psi_{j(i,t)}$ is a firm fixed effect, X_{it} is a vector of time-varying controls, and ε_{it} is an error term. In this specification, α_i captures persistent worker-specific determinants of pay, while the $\psi_{j(i,t)}$ terms capture firm-specific wage premia.

To estimate the firm effects, we first construct the largest connected set of workers and firms in

the formal labor market and estimate the AKM model on that sample. We restrict attention to firms observed for at least three consecutive years, with at least 10 employees in every year they appear, and with at least one worker observed at multiple firms. We estimate the AKM model on this sample, so that firm effects are estimated for employers with sufficient size and worker mobility to yield reasonably precise estimates. We then merge the estimated firm effects back to the workers in our analysis sample. Not all workers in the analysis sample can be assigned a firm effect: some are employed at firms that are too small to meet our size threshold, while others work at firms that fall outside the connected set used to estimate the AKM model. We therefore exclude these workers from the wage decomposition analysis.

We then estimate event studies using the estimated firm component $\hat{\psi}_{j(i,t)}$ and the remaining wage component

$$r_{it} = \log w_{it} - \hat{\alpha}_i - \hat{\psi}_{j(i,t)} \tag{3}$$

as outcomes. Since the worker effect is time invariant, changes in wages around diagnosis that operate through mobility across firms appear in the firm component, while changes not explained by sorting across firms are captured by the remaining component.

Let β_w^τ denote the event-study coefficient for log daily wages at event time τ, and let β_ϕ^τ and β_r^τ denote the corresponding coefficients for the firm and remaining components. We interpret

$$\beta_w^\tau \approx \beta_\phi^\tau + \beta_r^\tau \tag{4}$$

as decomposing the wage effect into a component due to changes in firm wage premia and a component due to within-firm or within-match wage changes.

5 The Effect of Anxiety and Depression on Earnings

5.1 Effect on Earnings

Figure 2 shows the effect of newly diagnosed anxiety or depression on monthly earnings. In the two years leading up to the diagnosis, earnings are roughly flat, indicating that treated and control individuals are on similar earnings trajectories prior to diagnosis. In the months immediately following diagnosis, earnings begin to fall, with losses accumulating steadily over time. Three months after diagnosis, earnings are lower by approximately 30 USD, or about 4.6 percent relative to baseline earnings of 844 USD. One year after diagnosis, monthly earnings are about 6.5 percent below baseline; by two years after diagnosis, they are about 7.4 percent below baseline.

The absence of pre-trends in Figure 2 may seem surprising, since one might expect mental health to worsen before workers seek care and receive a diagnosis. Appendix Figure A.2 shows that such pre-trends do appear once we remove the restriction that workers have no prior mental health diagnosis in the previous three years. Thus, pre-trends dissipate only after we restrict our

attention to cases with no mental health diagnosis in the preceding three years. Appendix Figure A.3 reinforces the distinction between new and recurrent cases. There, we separately examine depressive *episodes*, which correspond to new diagnoses, and depressive *disorders*, which correspond to recurrent cases. The earnings effect is large and negative for depressive episodes but indistinguishable from zero for recurrent depressive disorders.

To maximize sample size, our primary analysis focuses on a two-year event window. This raises the question of how earnings evolve over a longer horizon, especially since depressive and anxiety episodes tend to be transient, with median durations of less than one year (Ten Have et al., 2021). Appendix Figure A.4 extends our analysis to five years after diagnosis. The earnings effect continues to grow through roughly three years after diagnosis, reaching an effect of about 37 USD, or approximately 4.7 percent in relative terms. This effect persists for up to at least five years after diagnosis. Because estimating longer-run effects requires restricting attention to workers observed over a longer post-period, the sample used in Appendix Figure A.4 is smaller than the main analysis sample. Appendix Table A.4 compares the characteristics of the main sample to those of the sample used in the five-year analysis and shows that they are qualitatively similar.

Decomposition of earnings effect

Our estimated effect on earnings may reflect lower employment, fewer days worked conditional on employment, or lower wages conditional on working. Figure 3 decomposes the main earnings effect into these three margins using the method described in Section 4.2. In the two months following diagnosis, reductions in days worked account for more than half of the overall decline in earnings. Over time, however, the extensive margin and wages become increasingly important. By two years after diagnosis, the extensive margin, intensive margin, and daily wages account for 74.9, 7.7, and 17.4 percent of the total effect, respectively. These findings are consistent with a dynamic adjustment process in which mental health episodes initially reduce labor supply and later translate into more persistent effects on employment and pay.

The estimated effect on employment could arise either from an increase in transitions out of employment or from a decline in transitions back into employment. To examine these margins, Figure 4a shows the effect of mental health episodes on transitions from employment to non-employment (EN) and from non-employment to employment (NE). Following diagnosis, the probability of an EN transition rises immediately and remains elevated throughout the two-year follow-up period. The effect peaks two months after diagnosis at a 43.4 percent relative increase. By contrast, the probability of an NE transition falls in the months immediately surrounding diagnosis, reaching a trough one month after diagnosis at a -10.9 percent relative effect, but then becomes modestly positive later in the event window. Taken together, these results suggest that mental health episodes increase labor market churn. Because the rise in EN transitions is larger and more persistent than the change in NE transitions, the net effect is a decline in employment. In Appendix Figure A.10,

we additionally examine the effect of mental health episodes on employment-to-employment (EE) transitions and find evidence of a modest short-term decline in the probability of EE transitions, though the results are difficult to interpret given positive coefficients in the pre-period.

Transitions out of employment may reflect either short interruptions in work or more persistent detachment from the formal labor market. Because we do not observe unemployment insurance claims, we cannot separately identify unemployment and exit from the formal labor force. To shed light on this distinction, Appendix Figure A.5 shows the effect of mental health episodes on indicators for remaining non-employed for different durations. The estimated effects are modest for short non-employment spells and substantially larger for longer spells. This suggests that mental health episodes more often lead to persistent rather than transitory detachment from formal employment.

We next examine daily wages among employed workers. Figure A.6 shows a modest downward drift in wages in the months preceding diagnosis, though the pre-period estimates are not statistically distinguishable from zero. In the first two months after diagnosis, daily wages rise slightly, but the estimates are imprecise. Thereafter, wages begin to decline. Starting about seven months after diagnosis, the point estimates turn negative, and by roughly 9 to 12 months they remain mostly below baseline through the end of the two-year event window. By the end of the period, daily wages are about one percent below baseline and the estimates are marginally significant.

The pattern in Figure A.6 should be interpreted together with the employment transitions shown in Figure 3. The small short-run increase in observed wages may reflect compositional change in the set of employed workers after diagnosis. For example, if lower-wage workers are more likely to exit employment, average wages among those who remain employed may rise temporarily even in the absence of wage gains within jobs. It may also reflect changes in the distribution of firms or jobs among employed workers. In the next subsection, we use an AKM decomposition to examine whether the wage effects operate through movement across firms with different wage premia.

Decomposition of wage effect

To assess the effect of mental health episodes on mobility up or down the job ladder, we first estimate AKM firm effects using the largest connected set of workers and firms, as described in Section 4.3. Some individuals in our main analysis sample work at firms that are not assigned a firm effect under this procedure. We therefore carry out the AKM analysis on the subset of workers who can be matched to an estimated firm effect.

Next, we define an indicator for whether individual i in month t is employed at a firm with an AKM effect in the top quartile of the firm-effect distribution in our primary sample. As shown in Figure 4b, mental health episodes lead to a gradual but persistent decline in the probability of working at a top-quartile firm. Two years after diagnosis, individuals are -6.8 percent less likely to be employed at such firms. This pattern is consistent with mental illness impairing mobility up the

job ladder.

5.2 Heterogeneity

Figure 5 examines heterogeneity in the earnings effect of newly diagnosed depression or anxiety across demographic and baseline labor market characteristics. The earnings effect at 24 months is negative for most subgroups. Point estimates are larger for women than men, and they become more negative with age, although the confidence intervals overlap across these groups. Effects are also broadly similar across municipalities of different size.

Heterogeneity is more pronounced across baseline labor market characteristics. Earnings losses are largest among workers in the middle and top terciles of baseline earnings, while estimates for the bottom terciles are close to zero. A similar pattern appears for baseline attachment to the formal labor market, where effects are small and positive for workers with the weakest attachment. Heterogeneity by index firm size and prior outpatient utilization is comparatively modest.

These heterogeneity patterns provide suggestive evidence on mechanisms. That the largest earnings losses occur among individuals with higher baseline earnings and pre-diagnosis attachment to the formal labor market suggests that mental health shocks are especially costly when they disrupt workers with more stable formal employment relationships. This pattern is consistent with mental health disorders impairing progression within the formal sector. Importantly, they do not support our earnings effect being driven by transitions to informality, as individuals with higher levels of attachment to the labor market are likely less predisposed to moving between informal and formal.

Figure A.11 presents estimates for more granular diagnostic categories. Effects are negative across all groups, with somewhat larger point estimates for anxiety disorders. Although there is some variation across diagnoses, the estimates are imprecise, limiting our ability to draw firm conclusions.

5.3 Robustness

In this section, we present the results of several robustness exercises intended to address possible threats to identification.

Presence of heterogeneous treatment effects. Two-way fixed effects models can be confounded by heterogeneous treatment effects, potentially generating negative weights that pose challenges for causal inference (see, e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). These concerns are particularly salient when treatment effects are estimated using only "ever-treated" individuals (Goodman-Bacon, 2021). In our setting, the presence of heterogeneous treatment effects is plausible. For example, individuals working in certain sectors may be more or less protected from the economic costs of depression, and those with pre-existing health conditions may experience more severe impacts due to interactions between depression and underlying health.

For these reasons, our main specifications use the Callaway and Sant'Anna estimator. In Figure A.7, we present results from our main event study excluding "never-treated" individuals, thereby identifying effects solely from variation in treatment timing among treated individuals. We find qualitatively and quantitatively similar results, which reassures us that both types of comparisons—earlier- versus later-treated and treated versus untreated—are informative. For completeness, Figure A.9 reports estimates of the effect of depression on earnings using a standard two-way fixed effects model, which are also qualitatively similar.

Underdiagnosis of depression. We identify depressive episodes using physician-assigned diagnoses from outpatient and emergency department encounters. A large clinical literature shows that many individuals with depressive symptoms do not seek care. To assess the extent of underdiagnosis in our sample, Appendix Figure A.16 plots the probability of seeking care for depression as a function of underlying symptom severity using nationally representative survey data from Colombia in 2015. The horizontal axis measures depressive symptom severity using the Quick Inventory of Depressive Symptomatology (QIDS), with the vertical dotted line indicating the threshold for a diagnosis of major depressive disorder. The probability of seeking care increases with symptom severity. Conditional on meeting diagnostic criteria, the probability of seeking care is 52.4 percent, implying that roughly half of individuals who meet diagnostic criteria do not receive care and thus remain undiagnosed.

If individuals with undiagnosed depression are included in the control group, this would likely attenuate our estimates. Comparing event study results from the full analysis sample (Figure 2; 7.5 percent relative effect) with those from the sample restricted to ever-diagnosed individuals (Figure A.7; 8.6 percent relative effect) shows that the estimated effect is 13 percent bigger when restricting the sample to ever-diagnosed individuals, where underdiagnosis is less likely to be present. This difference suggests that our main estimates may understate the true effect of depression on earnings due to bias arising from underdiagnosis.

Health shocks other than depression. Depression and anxiety commonly co-occur with other medical conditions, such as cancer, heart disease, diabetes, and chronic pain. Our observed effects may therefore partly reflect the influence of these comorbid conditions. This concern is mitigated in our setting, as our sample is restricted to individuals under age 45, for whom the prevalence of chronic disease is relatively low. Pregnancy is a potential concern given the prevalence of postpartum depression; however, as discussed in Section 3.1, we exclude individuals with any record of pregnancy or parental leave in the three years prior to their depressive episode.

To further assess whether our results are driven by coincident medical conditions, Table A.6 examines the robustness of our baseline event study estimates to excluding individuals with various comorbidities from the analysis sample. The estimated treatment effects are similar before and after these exclusions, consistent with our findings reflecting the effects of depression and anxiety rather than those of comorbid conditions.

Substitution into the informal labor market. PILA captures earnings only for individuals employed in the formal labor market, which accounts for roughly 80 percent of Colombia's GDP. A limitation of these data is that we do not observe earnings for individuals working in the informal sector. As a result, we cannot distinguish between individuals with positive informal earnings and those with zero formal earnings; both are recorded as having zero earnings. This limitation could lead us to overstate the impact of depression on earnings.

To assess whether individuals may substitute into informal employment, we stratify the sample by baseline attachment to the formal labor market, measured as the number of months employed in the formal sector during the three years prior to diagnosis. We then estimate our main event study separately for each group. The results, presented in Figure 5, show that individuals with the strongest pre-diagnosis attachment to the formal sector experience the largest declines in earnings. This pattern is less consistent with our findings arising from workers with weaker attachment to formality transitioning into informal employment.

Reverse Causality. A remaining identification concern is reverse causality, whereby adverse labor market shocks both reduce earnings and precipitate mental health episodes. Our baseline design partly addresses this concern by matching newly diagnosed workers to control workers employed at the same firm. This comparison nets out shocks common to the employer; identification comes from differential earnings changes between diagnosed and undiagnosed workers facing the same firm-level conditions.

While our baseline estimates are thus robust to labor-demand shocks operating at the firm level, firm-level matching does not rule out all forms of confounding. Labor market shocks may still vary across workers within the same firm, and idiosyncratic shocks to a worker's employment trajectory may both lower earnings and precipitate diagnosis. Importantly, such shocks need not generate earnings declines prior to diagnosis. If they arrive discretely, or roughly contemporaneously with diagnosis, they may not be detectable in pre-trends.

To assess whether coincident labor-demand shocks can account for our findings, we implement a displaced-worker event-study design following Jacobson et al. (1993) and closely related to Khanna et al. (2021). Using the same linked employer-employee and health-care utilization records as in the main analysis, we identify mass-layoff episodes between 2012 and 2017 at firms whose maximum monthly employment in 2009 is at least 20 and whose employment falls by 30 to 90 percent within the subsequent six months relative to the firm's maximum employment over the previous 12 months. To avoid capturing firms undergoing sharp pre-layoff expansion, we require this pre-layoff maximum to be no more than 130 percent of the firm's maximum employment in months 13 to 24 before the event. We then restrict attention to displaced formal-sector workers ages 18–45 with at least 270 days of formal employment in 2009 who were on the firm's roster 12 months before separation and employed there for at least 30 of the previous 36 months. Treatment is assigned at the worker level as the month of actual separation from the layoff firm, and we require the separation to persist for

at least two months to avoid classifying temporary absences or leaves as layoffs.

We then estimate the dynamic effect of displacement on monthly labor earnings and on the probability of a first diagnosis for anxiety or depression. Figure 6 shows a large and persistent decline in earnings following displacement. By contrast, estimated effects on the probability of a first mental health diagnosis remain close to zero throughout the post period, with some evidence of a decline in the months immediately after displacement. In Appendix Figure A.8, we show that effects on *any* mental health diagnosis similarly remain close to zero.

One possible concern is that job loss may temporarily disrupt access to care, leading to short-run reductions in observed diagnoses even if underlying mental health worsens. But this explanation is less plausible over longer horizons. In Colombia, workers who exit formal employment should typically regain coverage either through the contributive system or through the subsidized regime, consistent with the country's near-universal insurance system. The absence of any medium- or long-run increase in diagnosed anxiety or depression therefore suggests that displaced workers do not experience a delayed rise in mental health diagnoses following separation.

Overall, these findings make it less likely that contemporaneous labor-demand shocks can explain the earnings losses we observe following newly diagnosed depression and anxiety.

6 The Role of Specialist Care in Mitigating Earnings Losses

In Section V, we document a persistent decline in earnings following a new diagnosis of depression or anxiety. The estimates there combine workers who do and do not subsequently receive treatment. This raises two related questions: whether earnings losses differ by treatment status, and, if so, whether any such difference reflects the causal effect of treatment. In this section, we present preliminary evidence on these questions.

Evidence from randomized controlled trials suggests that standard treatments for anxiety and depression—including medication, psychotherapy, or their combination—reduce symptoms in the short run (Cuijpers et al., 2008, 2013; Mitte, 2005). Recent work in economics finds that treatment can affect a broader set of outcomes, including quality of life, crime, earnings, and mortality (Angelucci and Bennett, 2022; Baranov et al., 2020; Bhat et al., 2022; Biasi et al., 2021; Blattman et al., 2023; Heller et al., 2017; Lund et al., 2024; McKelway et al., 2023; Ridley et al., 2020; Shreekumar and Vautrey, 2022). Studies of the effects on earnings generally point to positive impacts, though the evidence remains mixed.

While the question of whether treatment improves earnings is of independent interest, it is also useful for identification. In the previous section, we try to rule out coincident shocks that could generate both depression and lower earnings, including comorbid health conditions and demand-side labor market shocks. But some potentially important confounding shocks remain difficult to address directly, such as relationship difficulties or the death of a parent. One view is that depression and anxiety are often bundled with such shocks, so that the policy-relevant object is the combined

effect of the mental health episode and its surrounding circumstances. Another view is that these shocks should be regarded as confounders. Estimates of the effect of treatment are useful from the perspective of this latter view because treatment receipt is plausibly less likely than diagnosis itself to coincide with broader life shocks. For this reason, evidence on treatment effects provides a useful complement to the diagnosis-based results in the first part of the paper, asking in effect the reverse question: not what happens to earnings when mental health worsens, but whether earnings recover when mental health improves.

6.1 Descriptive evidence

We begin with a descriptive comparison of earnings trajectories for diagnosed workers who do and do not receive treatment, where treatment is defined as an indicator for whether the worker is seen by a psychologist, psychiatrist, or social worker within 90 days of the index diagnosis. Appendix Figure A.17 plots event studies for the two groups. Earnings are relatively stable prior to diagnosis in both groups, although workers who are later treated may exhibit a modest downward pre-trend. After diagnosis, earnings decline in both groups, but the decline is smaller among workers who receive treatment.

The absence of pronounced pre-diagnosis divergence in earnings makes this comparison more informative than a simple cross-sectional difference. Nevertheless, the post-diagnosis differences in earnings should be interpreted cautiously. Treatment status is not randomly assigned, and workers who obtain specialist care may differ systematically from those who do not. Appendix Table A.7 compares characteristics of treated and untreated workers. Roughly a quarter of workers obtain specialist care within 90 days of diagnosis. Treated workers are slightly younger, more likely to be female, have lower baseline earnings, and have higher levels of attachment to the health care system as measured by the number of outpatient and emergency department visits. These differences show that selection into treatment is nontrivial. The earnings gap in Appendix Figure A.17, though suggestive, should thus not be interpreted as causal.

6.2 Instrumental variables approach

To address selection into treatment, we implement an instrumental variables strategy that exploits insurer-level variation in access to specialist mental health care. The instrument is motivated by the idea that insurers differ in referral practices, provider networks, and administrative frictions, thereby generating differential access to specialist care conditional on diagnosis. Appendix Figure A.18 illustrates this variation by showing, for each year and for each of the 10 largest insurers, the share of individuals diagnosed with any mental health disorder in the outpatient or emergency department setting who are subsequently referred for specialist care.

Let $Treat_i$ be an indicator equal to one if individual i has at least one visit with a psychiatrist, psychologist, or social worker within 90 days of the index diagnosis. We instrument $Treat_i$ using a

leave-out mean based on insurer treatment patterns. Specifically, consider the sample of individuals in RIPS who present in the outpatient or emergency department setting and receive a primary diagnosis of any mental health disorder. Let $j(i)$ denote individual i's insurer-by-diagnosis-year cell, and let $J_{j(i)}$ denote the set of other individuals diagnosed in the same year and enrolled in the same insurer. We define the instrument as

$$Z_i = \frac{1}{|J_{j(i)}| - 1} \sum_{i' \in J_{j(i)}} \sum_{i' \neq i} S_{i'}, \tag{5}$$

where $S_{i'}$ equals one if individual i' receives specialist care within 90 days of diagnosis. This leave-out mean captures insurer-specific treatment propensity, excluding individual i's own treatment status.

We then estimate the effect of treatment on post-diagnosis earnings using two-stage least squares. For each post-diagnosis horizon $p \in \{3, 6, 9, 12, 15, 18, 21, 24\}$ months, we estimate

$$\begin{aligned} Treat_i &= \pi Z_i + W_i' \rho + v_i, \\ \Delta y_{i,p} &= \alpha_p + \beta_p Treat_i + W_i' \gamma_p + u_{i,p}, \end{aligned} \tag{6}$$

where $\Delta y_{i,p} \equiv y_{i,p} - y_{i,-1}$ denotes the change in individual i's monthly-equivalent earnings between horizon p and the quarter prior to diagnosis. The control vector W_i includes calendar controls (year-of-event and day-of-week indicators) and municipality fixed effects. We estimate the system in a stacked specification across horizons and cluster standard errors at the individual level.

Appendix Figure A.19 assesses the balance of the leave-out mean (LOM) instrument with respect to predetermined worker characteristics. Without additional controls, the baseline covariates are jointly significant with an F-statistic of 3.33. Once we add baseline controls, the F-statistic falls to 0.91, while individual t-statistics remain small. This pattern motivates our preferred specification, which controls for year-of-event, day-of-week, and municipality fixed effects.

Appendix Figure A.20 presents the first-stage relationship between the instrument and specialist mental health care. We find a strong and precisely estimated positive relationship, with a first-stage coefficient of 0.902 and a t-statistic of 13.9. This indicates that insurer-level variation in access to specialist care translates into substantial differences in treatment. Appendix Figure A.21 then reports the reduced-form relationship between the instrument and earnings. The estimated relationship is positive but imprecise, with a t-statistic of 1.75.

Table A.8 reports OLS and IV estimates of the effect of specialist mental health care on earnings across horizons. OLS estimates are generally positive, while IV estimates are consistently larger and tend to rise over time. This difference between OLS and IV estimates is consistent with negative selection into treatment, whereby individuals who receive specialist care have lower counterfactual earnings growth. By 18–24 months after diagnosis, the IV estimates imply monthly earnings gains of roughly 150–220 USD and are statistically significant. Results are qualitatively similar with and without baseline controls, though estimates are somewhat larger and less precisely estimated when

controls are included. Taken together, the results suggest that improved access to specialist mental health care raises earnings, with effects that grow over time.

6.3 Objectives over the next three months

Over the next three months, we plan to focus on refining our IV approach. Because our current 2SLS estimates are statistically imprecise and large relative to the benchmark implied by our estimated effects of depression and anxiety, our first priority is to increase the sample size and characterize the complier population in order to better understand who is on the margin of treatment. To better contextualize the magnitude of the IV estimates, we will also re-estimate our main event study results after reweighting the sample to match complier characteristics. We also plan to explore alternative, more granular definitions of our instrument—for example, by incorporating regional variation in referral propensities—which may improve power. Finally, we plan to study mechanisms by which treatment may improve earnings by merging in medication data available for a subsample of the analysis sample. This would build upon Currie and MacLeod (2020), which shows that mental health specialists may deliver better care in part through more effective selection of antidepressants.

We aim to complete these refinements by July 2026.

7 Conclusion

Mental health conditions are a leading cause of disability worldwide and often emerge in early adulthood, when workers are investing in human capital and establishing their positions in the labor market. In standard models of human capital accumulation and on-the-job search, even temporary disruptions to skill investment or job mobility can have persistent consequences through dynamic complementarities and state dependence. We study the long-run labor market consequences of common mental health episodes by linking nationwide employer-employee administrative records to the universe of health care utilization data in Colombia. This allows us to follow formally employed workers before and after the onset of new depressive and anxiety episodes and to quantify their effects on earnings.

We find that new diagnoses of anxiety and depression lead to large and persistent earnings losses that grow over time. Earnings are about 7.4 percent lower two years after diagnosis and 5.5 percent lower five years later, with no evidence of recovery at longer horizons. In the short run, losses are driven primarily by reductions in days worked among those who remain employed. Over time, earnings losses increasingly reflect lower employment and wages. Evidence from an AKM decomposition indicates that part of the long-run wage decline operates through transitions to firms with lower firm wage premia, consistent with impaired mobility up the job ladder.

Taken together, our findings suggest that common mental illnesses can durably alter early-career trajectories. More broadly, they point to a mechanism through which health shocks may generate

persistent inequality: by disrupting job mobility and match quality at precisely the stage of the life cycle when workers are accumulating experience and sorting across firms. An important direction for future research is to better understand how mental health shapes job mobility, firm sorting, and long-run inequality, and to quantify the welfare and fiscal consequences of policies that expand access to effective treatment.

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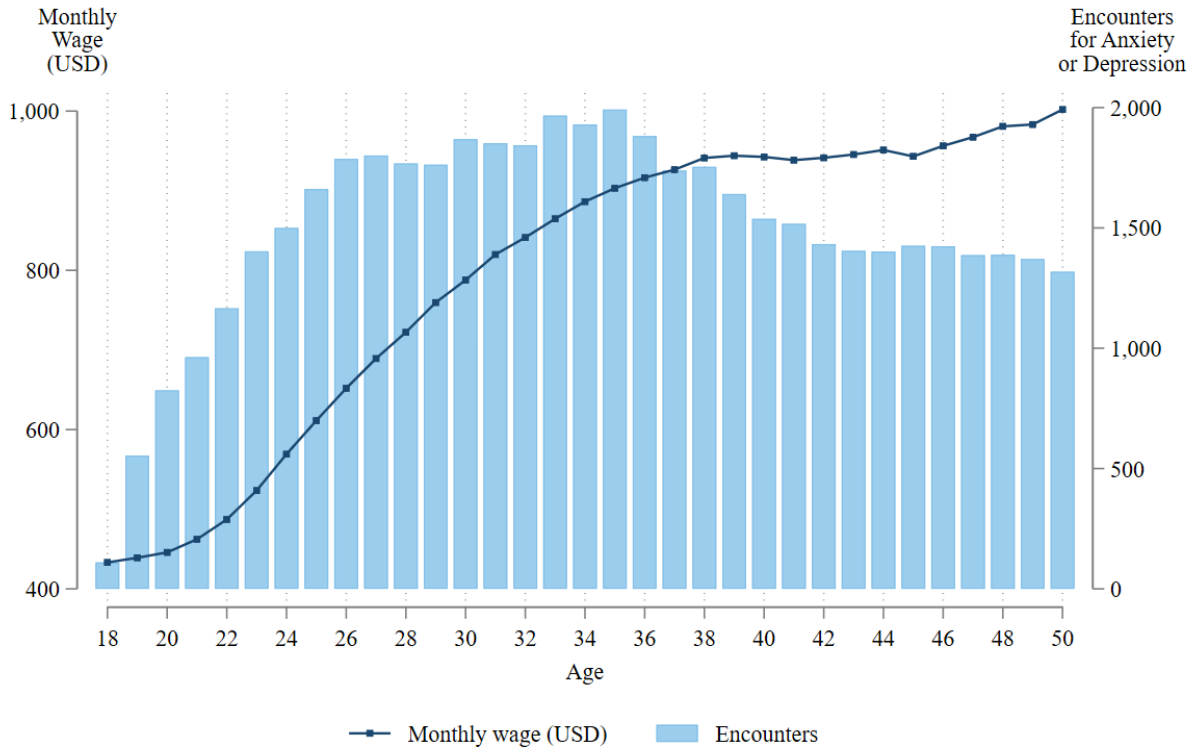
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Tables and Figures

Figure 1: Age-Earnings Profile and Encounters for Anxiety or Depression



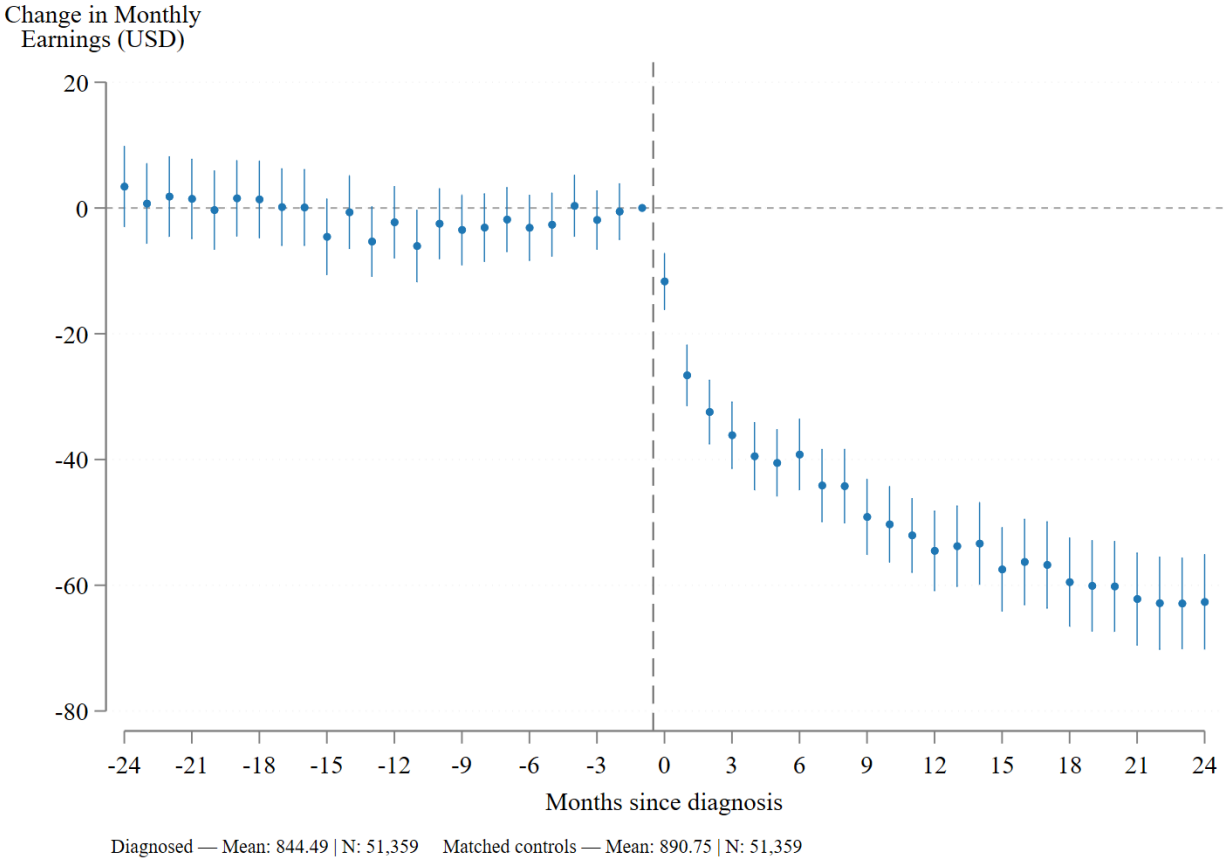
Note: This figure presents the age-earnings profile and incidence of encounters for anxiety or depression among formally employed individuals from 2009 to 2019. The sample includes individuals between the ages of 18 and 60 (inclusive). The left axis displays average monthly wages conditional on employment in USD, while the right axis shows the total number of outpatient or emergency department encounters with a principal diagnosis of anxiety or depression.

Table 1: Sample Characteristics

	(1) Diagnosed (matched)	(2) Matched controls
Panel A. Individual characteristics		
Age	31.04	31.21
Female	0.58	0.54
Large municipality	0.92	0.92
Intermediate municipality	0.05	0.06
Small municipality	0.02	0.03
Panel B. Job market		
Wage	8,802.30	8,584.51
Formality	1.00	1.00
Daily wage	25.18	24.54
Worked days	346.56	346.99
Vacation days	0.71	0.70
Temporal suspension days	0.07	0.06
Non-labor sick leave days	0.25	0.20
Labor sick leave days	0.06	0.06
Parental leave days	0.04	0.05
Employment-to-unemployment	0.01	0.01
Unemployment-to-employment	0.01	0.01
Job-to-job	0.02	0.02
Panel C. Health sector		
Outpatient visits	0.29	0.24
ED visits	0.08	0.01
Observations	51,359	51,359

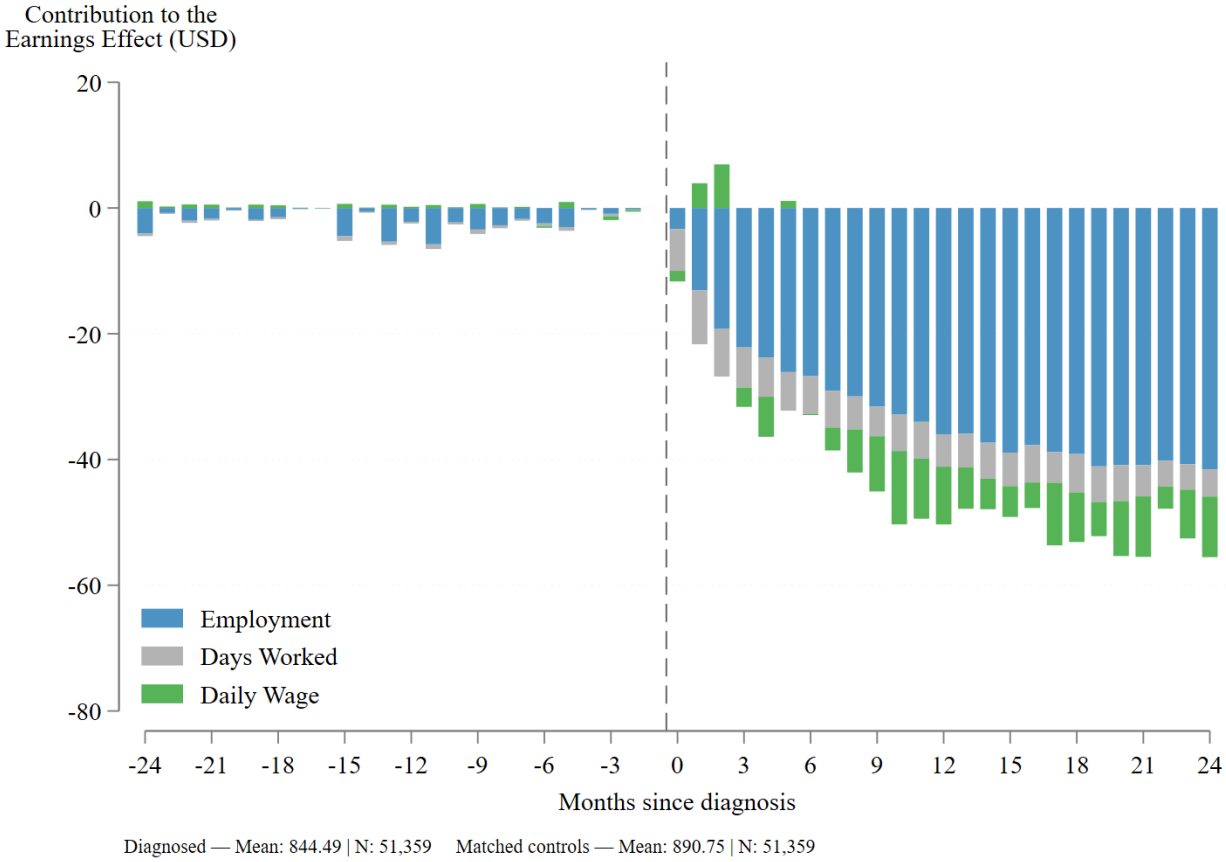
Note: This table reports descriptive statistics for the primary analysis sample, which consists of individuals diagnosed with depression or anxiety and matched controls selected using propensity score matching. Column (1) reports characteristics of individuals newly diagnosed with depression or anxiety, and Column (2) reports the characteristics of matched controls. Details on sample construction and the propensity score matching procedure are provided in Section 3. Municipalities are classified using the 2018 national census: small municipalities have populations of less than or equal to 20,000, intermediate municipalities have populations between 20,001 and 100,000, and large municipalities have populations above 100,000. Wage denotes total formal earnings from social security records expressed in 2008 dollars. Daily wage denotes average daily earnings based on the contribution base. Worked days denotes the total number of paid days. Employment-to-unemployment, unemployment-to-employment, and job-to-job transitions are defined between consecutive months.

Figure 2: Effect of Newly Diagnosed Anxiety and Depression on Earnings



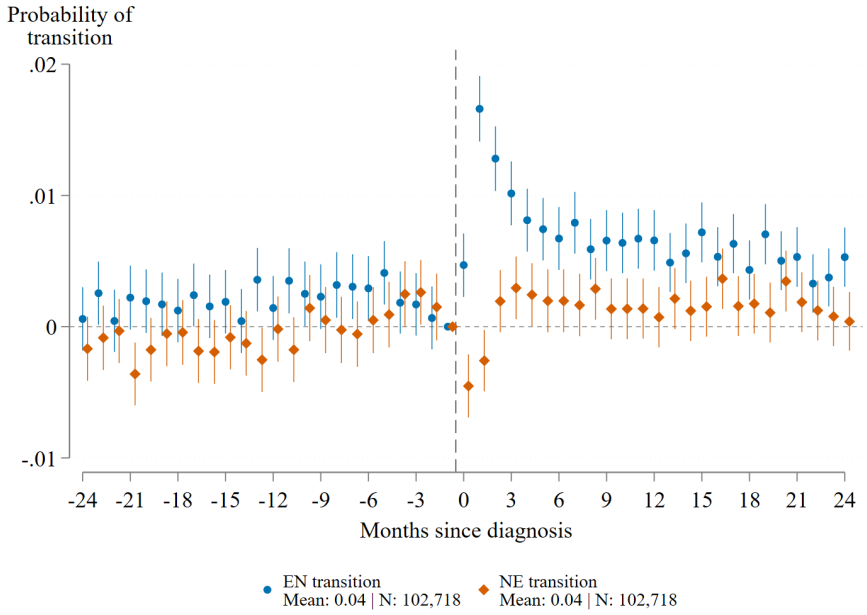
Note: This figure shows the effect of anxiety and depressive episodes on earnings. The horizontal axis represents months since diagnosis, and the vertical axis represents the difference in US dollars relative to earnings one month prior to diagnosis. The sample includes 51,359 individuals diagnosed with depression or anxiety and 51,359 matched controls. Estimates are obtained using the Callaway and Sant'Anna (2021) estimator; further details are provided in Section 4.

Figure 3: Decomposition of Earnings Effect into Employment, Days Worked, and Wages

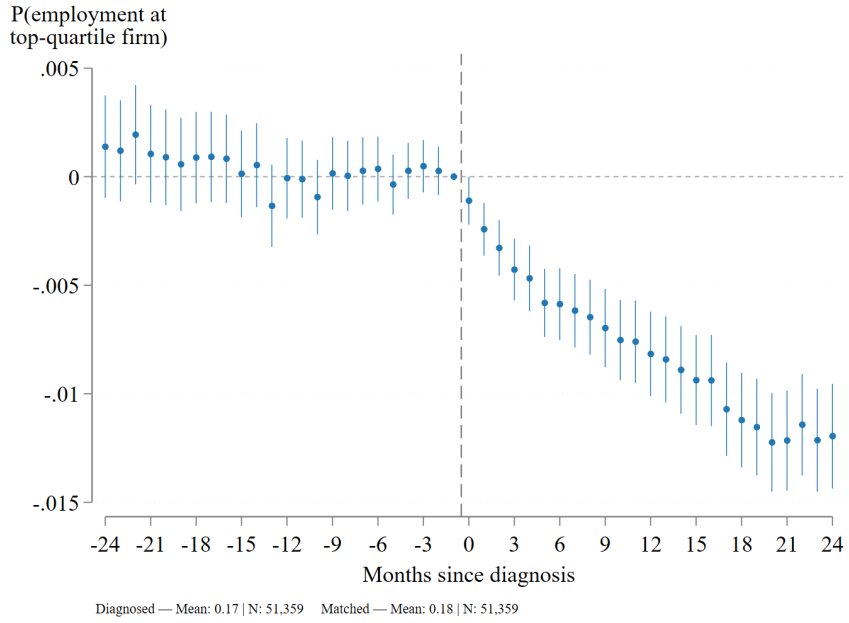


Note: This figure decomposes the change in earnings following a new diagnosis of depression or anxiety into three components: employment, days worked, and daily wages. The horizontal axis shows months relative to diagnosis, and the vertical axis shows each component's contribution, in U.S. dollars, to the change in earnings relative to month -1. The contribution of each component in event month t is computed as the change in that component between month -1 and month t, with the remaining two components evaluated at the average of their values in months -1 and t. Additional details are provided in Section 4.

Figure 4: Effect of Newly Diagnosed Anxiety or Depression on Employment Transitions and Firm Wage Premia



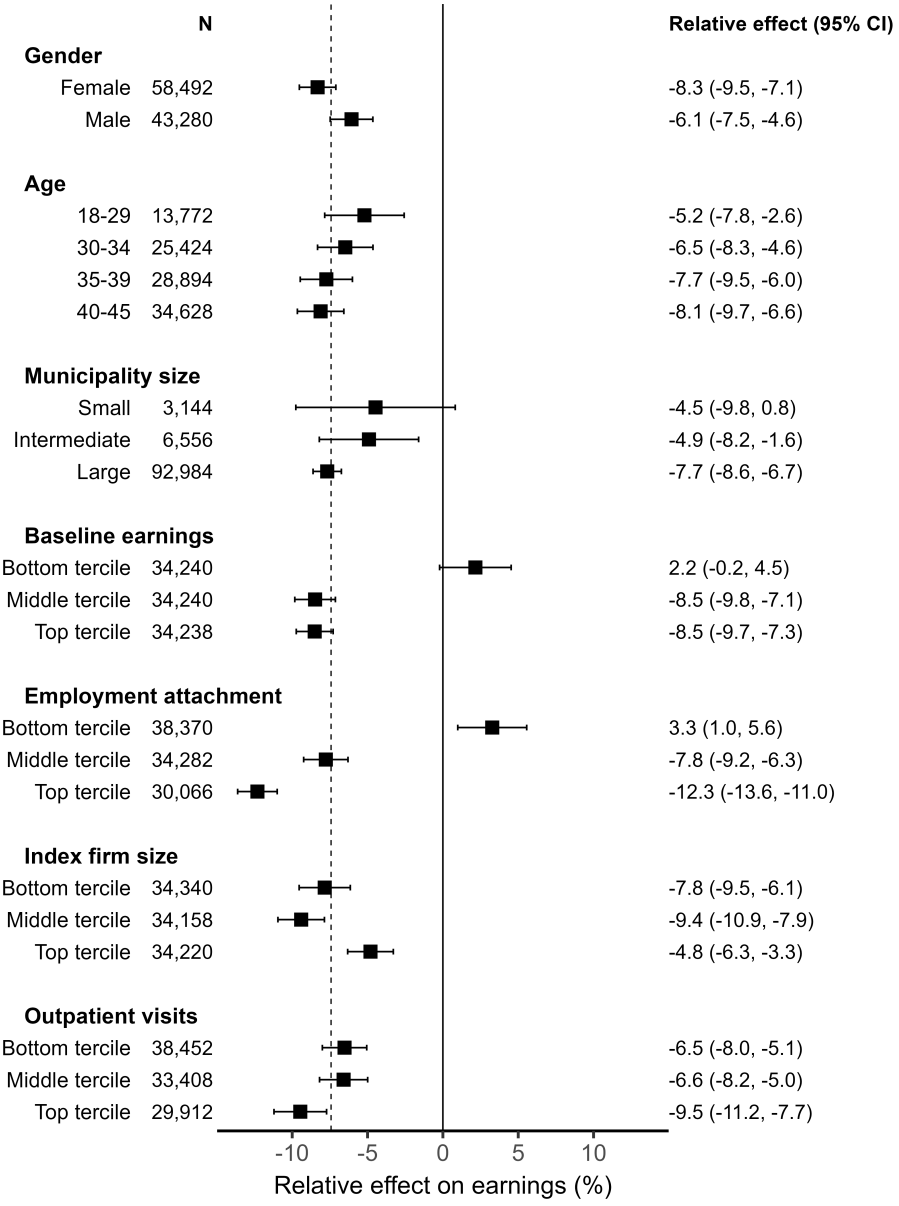
(a) Effect on Employment Transitions



(b) Effect on Probability of Employment at Top-Quartile Firm

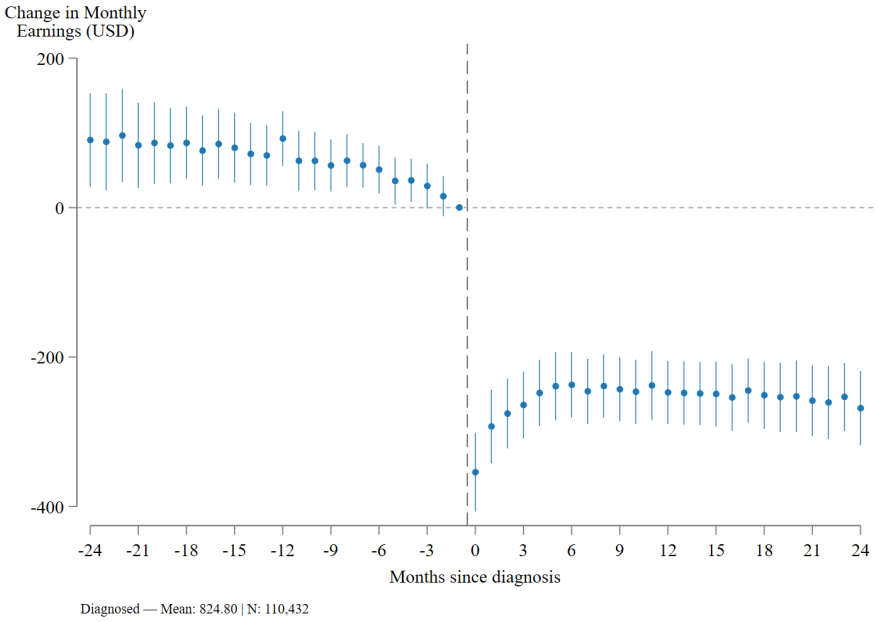
Note: This figure shows event study estimates of the effect of newly diagnosed anxiety or depression on employment transitions and sorting across firms. Panel (a) reports effects on employment-to-nonemployment (EN) and nonemployment-to-employment (NE) transitions. Panel (b) reports effects on the probability of working at a firm in the top quartile of the firm wage-premium distribution. Time is measured in months relative to diagnosis, and coefficients are normalized to zero in month 1. Estimates are obtained relative to propensity score–matched controls. Vertical bars represent 95 percent confidence intervals. Further details are provided in Section 3.

Figure 5: Heterogeneity in the Effect of Newly Diagnosed Anxiety or Depression

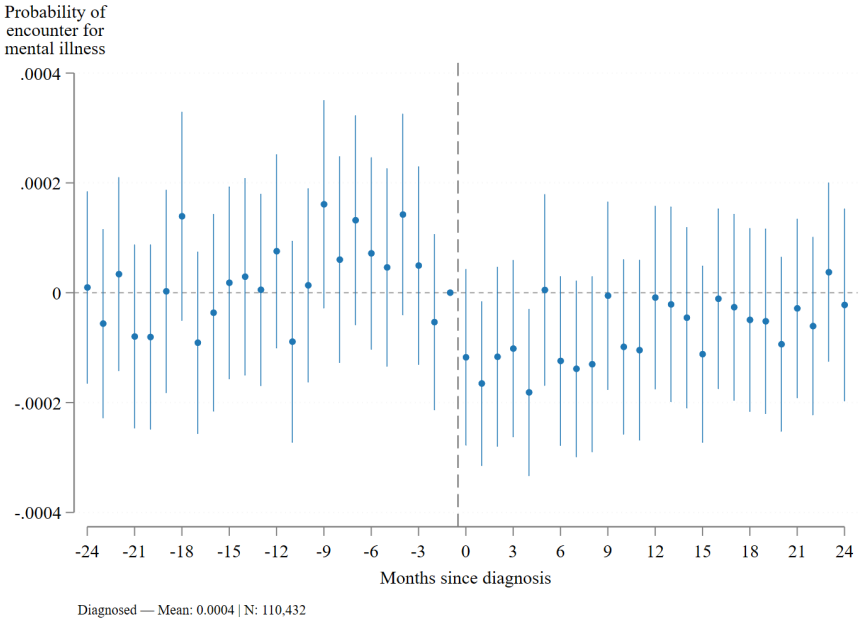


Note: This figure shows heterogeneity in the effect of a newly diagnosed anxiety or depression episode on formal earnings 24 months after diagnosis across subgroups defined by gender, age, municipality size, baseline earnings, employment attachment, index firm size, and outpatient visits. Point estimates are expressed as a percentage of each subgroup's mean pre-diagnosis earnings, and horizontal bars denote 95 percent confidence intervals. The dashed vertical line shows the overall sample estimate. Estimates are obtained separately by subgroup using the Callaway and Sant'Anna (2021) event-study estimator. Municipality size is defined using the 2018 National Population Census. Baseline earnings, employment attachment (defined as the number of months with positive earnings), and outpatient visits are defined in the two years preceding diagnosis and grouped into terciles. Index firm refers to the modal employer during the three years preceding diagnosis.

Figure 6: Effect of Mass Layoffs on Earnings and the Probability of New Anxiety or Depression



(a) Effect on monthly labor earnings



(b) Effect on probability of encounters for mental illness

Note: This figure shows the effect of displacement from a mass layoff event on earnings (panel a) and on the probability of encounters for mental illness (panel b). The horizontal axis denotes months since the layoff event, and the vertical axis reports coefficients relative to the month prior to displacement. Following Jacobson et al. (1993), a mass layoff is defined as a 30-90 percent reduction in employment at a firm with at least 20 baseline workers. Treatment is assigned at the worker level as the month of separation. Estimates are obtained using a two-way fixed effects estimator. The sample is restricted to displaced formal-sector workers ages 18-45 who worked at least 270 days in 2009. Vertical bars indicate 95 percent confidence intervals.

Online Appendix

A Appendix

Table A.1: ICD-10 Codes for Depression and Anxiety Disorders

Code	Definition
Depressive Episodes (F32)	
F32.0	Mild depressive episode
F32.1	Moderate depressive episode
F32.2	Severe depressive episode without psychotic symptoms
F32.3	Severe depressive episode with psychotic symptoms
F32.8	Other depressive episodes
F32.9	Depressive episode, unspecified
Other Anxiety Disorders (F41)	
F41.0	Panic disorder (episodic paroxysmal anxiety)
F41.1	Generalized anxiety disorder
F41.2	Mixed anxiety and depressive disorder
F41.3	Other mixed anxiety disorders
F41.8	Other specified anxiety disorders
F41.9	Anxiety disorder, unspecified

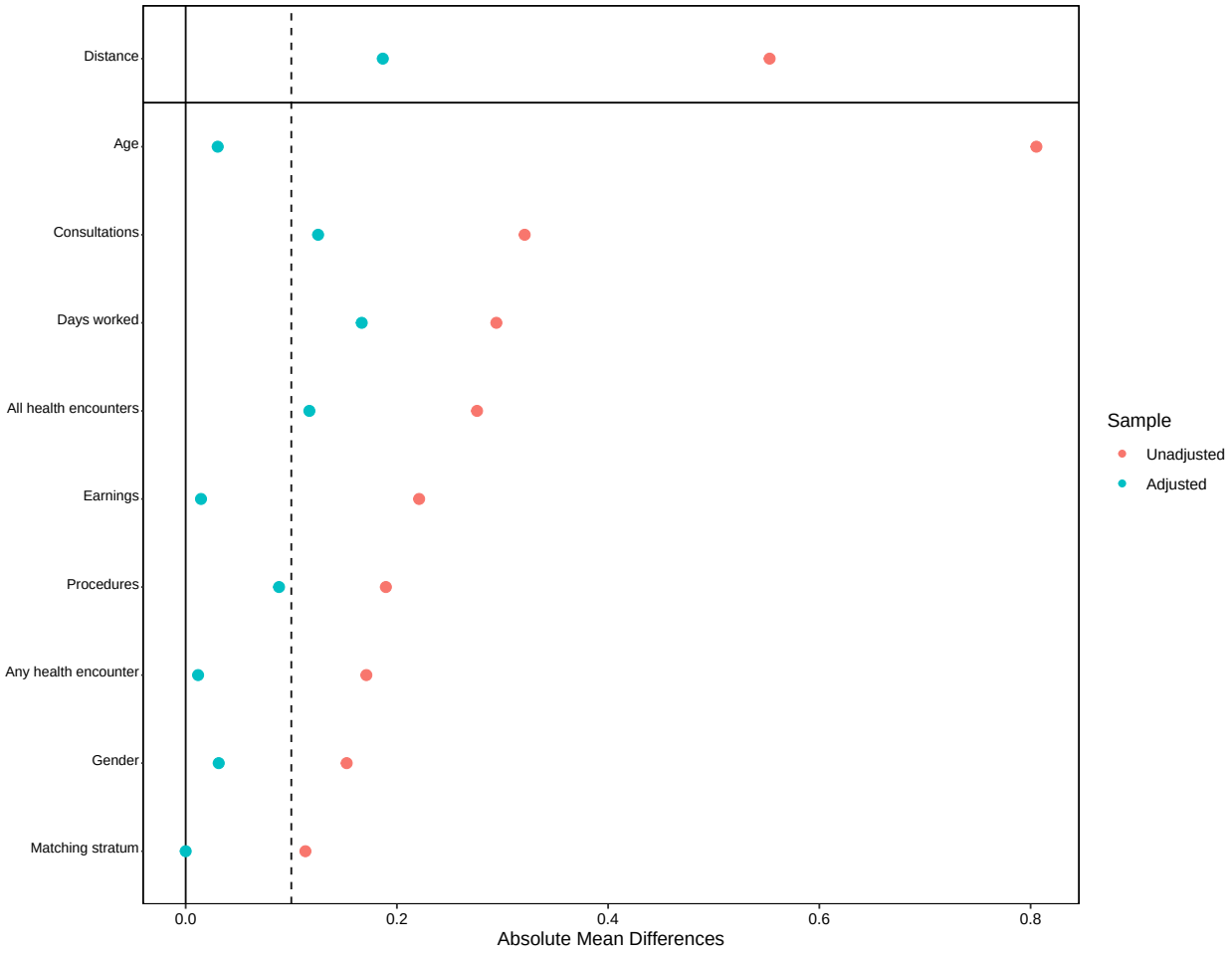
Note: The table shows ICD-10 codes used to identify mental health diagnoses. F32 codes represent single depressive episodes, and F41 codes represent anxiety disorders.
Source: WHO ICD-10 Classification.

Table A.2: Sample Characteristics Before and After Applying Sample Restrictions

	(1) Wage Earners in 2009	(2) 18-45 y.o. in 2009	(3) 18-45 y.o. at depression diagnosis	(4) Depression as primary diagnosis	(5) No history of mental health (3 years) 2012-2017	(6) No history of pregnancy or parental leave (3 years)
Panel A. Individual characteristics						
Age	37.04	32.75	32.12	32.02	30.73	31.08
Female	0.42	0.44	0.66	0.65	0.63	0.59
Large municipality	0.89	0.89	0.93	0.93	0.92	0.92
Intermediate municipality	0.08	0.07	0.05	0.05	0.06	0.06
Small municipality	0.03	0.03	0.02	0.02	0.02	0.02
Panel B. Job market						
Wage	9,292.97	8,661.47	8,573.06	9,206.37	8,609.87	8,599.02
Formality	1.00	1.00	1.00	1.00	1.00	1.00
Daily wage	26.60	24.87	24.53	26.30	24.66	24.62
Worked days	345.87	344.93	346.81	347.20	346.10	346.19
Vacation days	0.65	0.62	0.71	0.74	0.68	0.68
Temporal suspension days	0.09	0.09	0.08	0.08	0.07	0.07
Non-labor sick leave days	0.22	0.22	0.32	0.33	0.25	0.24
Labor sick leave days	0.08	0.08	0.06	0.06	0.06	0.06
Parental leave days	0.07	0.09	0.04	0.04	0.05	0.04
Employment-to-unemployment	0.01	0.01	0.01	0.01	0.01	0.01
Unemployment-to-employment	0.02	0.02	0.01	0.01	0.02	0.02
Job-to-job	0.02	0.02	0.02	0.02	0.02	0.02
Panel C. Health sector						
Outpatient visits	0.19	0.19	0.43	0.42	0.30	0.29
Medical procedures	0.16	0.15	0.11	0.00	0.00	0.00
ED visits	0.01	0.01	0.04	0.05	0.08	0.08
Observations	4,270,403	3,288,215	436,667	285,810	67,913	58,813

Note: The table presents descriptive statistics of 2009 wage earners, applying a series of progressive filters. Column (1) includes all individuals who were wage earners for 270 or more days in 2009. Column (2) additionally restricts to those who were between 18 and 45 years old in 2009. Column (3) further restricts to those who were diagnosed with anxiety or depression between 18 and 45 years of age. Column (4) adds the condition that anxiety or depression is the primary diagnosis. Column (5) excludes individuals with a mental health history in the three years before the depression diagnosis and whose diagnosis took place between 2012 and 2017. Finally, Column (6) also excludes those who had any pregnancy-related contact with the health system or any parental leave up to 3 years before the depression diagnosis. Age is measured in 2009. Female equals 1 for women. Municipality size classifications use the 2018 National Population Census: small municipalities have populations $\leq$20,000; intermediate municipalities have populations between 20,001 and 100,000; and large municipalities have populations $>$100,000. Wage represents total formal earnings from social security records, expressed in 2008 dollars. Daily wage is the average daily income based on the contribution base. Worked days is the total number of days paid. Vacation days, temporal suspension days, non-labor sick leave days, labor sick leave days, and parental leave days reflect different types of leave taken. Employment-to-unemployment indicates cessation of employment between consecutive months. Unemployment-to-employment indicates transitions from unemployment to employment between consecutive months. Job-to-job indicates direct employer changes without unemployment spells. Outpatient visits counts total consultations per month. Medical procedures counts total procedures performed per month. ED visits counts emergency room visits per month.

Figure A.1: Covariate Balance: Diagnosed vs. Never-Diagnosed Workers



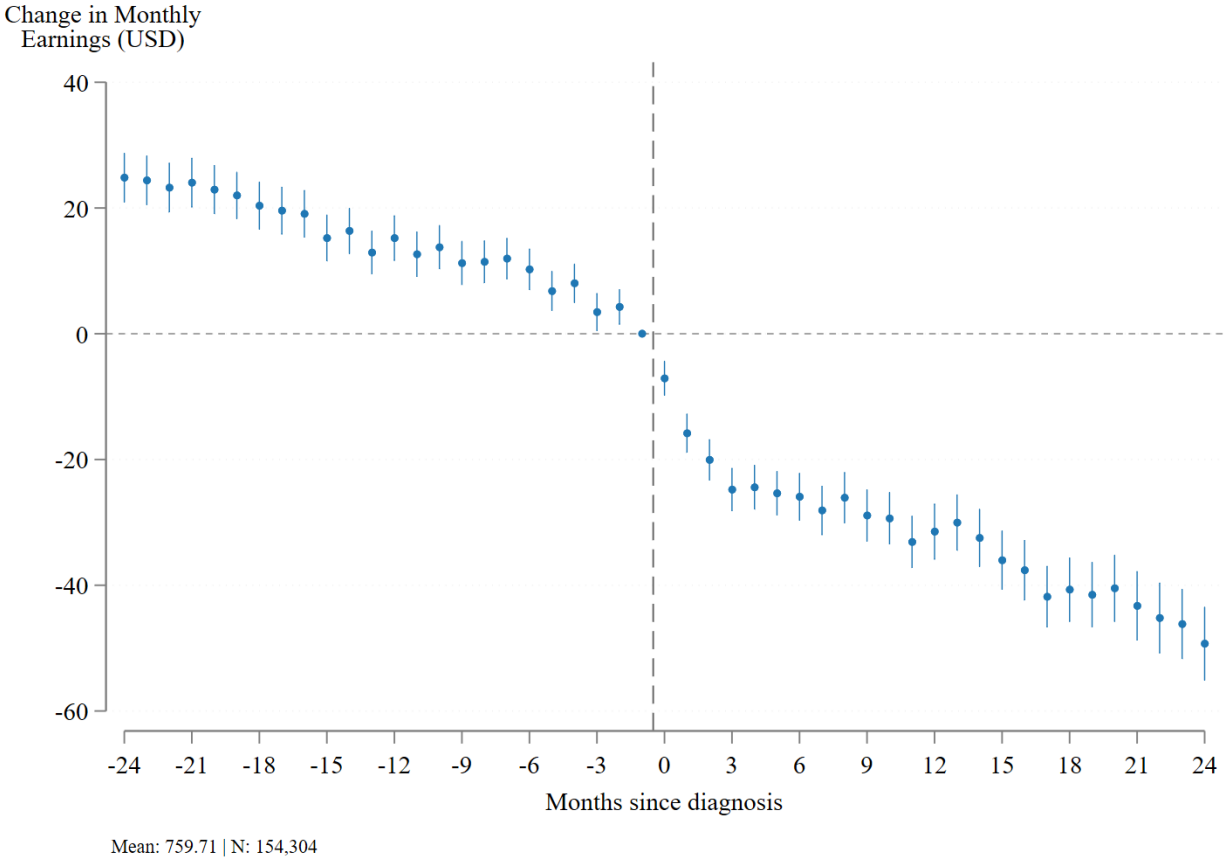
Note: The figure displays absolute mean differences between diagnosed (treated) and never-diagnosed (control) workers before (Unadjusted) and after (Adjusted) 1:1 nearest-neighbor propensity score matching. Matching is performed in two stages. First, exact matching is imposed within a firm × month stratum, defined by the treated worker's modal employer over the 36 months preceding the diagnosis date. Second, within each stratum, workers are matched on a propensity score estimated from age, gender, pre-diagnosis earnings (mean contribution base), pre-diagnosis days worked, and 2009 health system utilization (total encounters, outpatient consultations, medical procedures, and any encounter). Controls are restricted to workers who never had any mental health encounter over the sample period. The dashed vertical line marks an absolute mean difference of 0.10, the conventional threshold for acceptable covariate balance. Distance refers to the estimated propensity score.

Table A.3: Sample Characteristics with matched control

	(1) Diagnosed sample	(2) Diagnosed (matched)
Panel A. Individual characteristics		
Age	31.08	31.04
Female	0.59	0.58
Large municipality	0.92	0.92
Intermediate municipality	0.06	0.05
Small municipality	0.02	0.02
Panel B. Job market		
Wage	8,599.02	8,802.30
Formality	1.00	1.00
Daily wage	24.62	25.18
Worked days	346.19	346.56
Vacation days	0.68	0.71
Temporal suspension days	0.07	0.07
Non-labor sick leave days	0.24	0.25
Labor sick leave days	0.06	0.06
Parental leave days	0.04	0.04
Employment-to-unemployment	0.01	0.01
Unemployment-to-employment	0.02	0.01
Job-to-job	0.02	0.02
Panel C. Health sector		
Outpatient visits	0.29	0.29
Medical procedures	0.00	0.00
ED visits	0.08	0.08
Observations	58,813	51,359

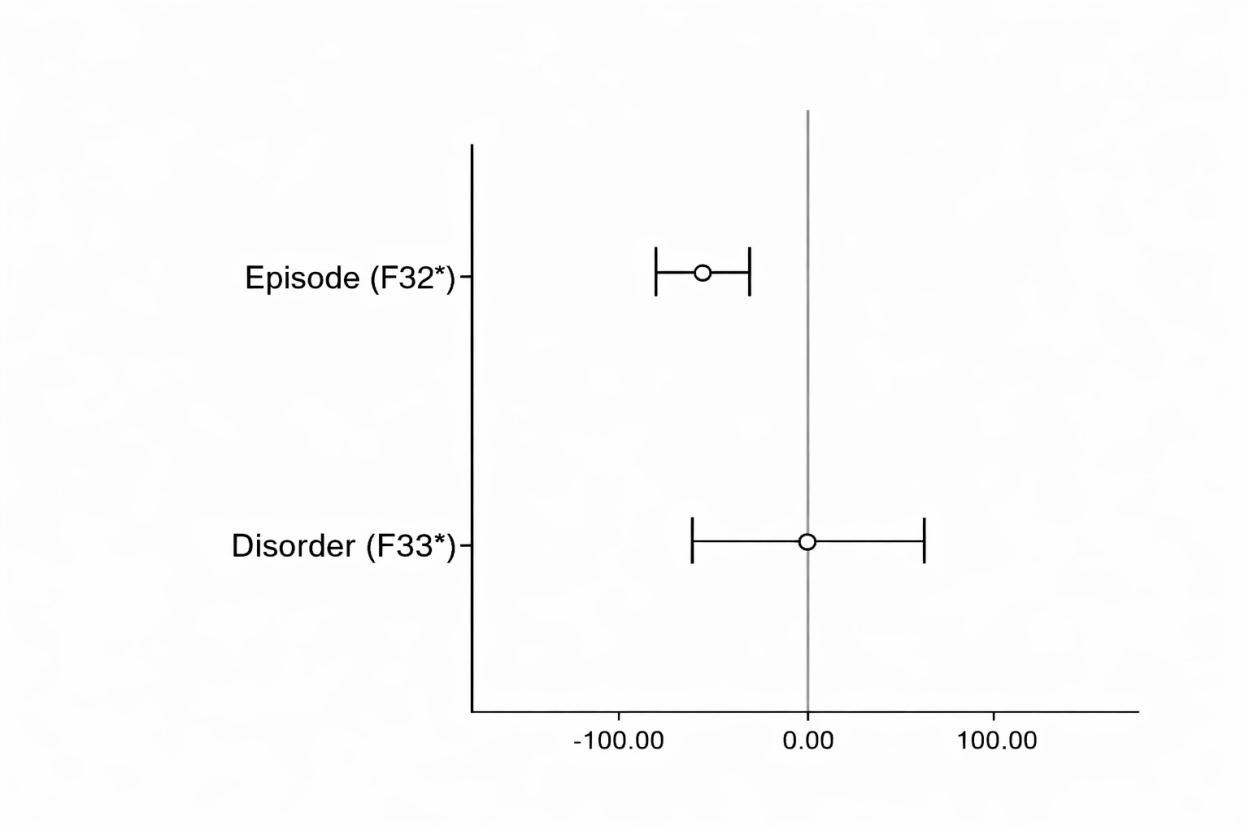
Note: TThe table compares pre-diagnosis annual characteristics of individuals diagnosed with anxiety or depression. Column (1) includes all diagnosed individuals in the sample. Column (2) restricts to the subset of diagnosed individuals who were successfully matched to a control. All variables in Panels B and C are measured as annual averages in the pre-diagnosis period. Age is measured in 2009. Female equals 1 for women. Municipality size classifications use the 2018 National Population Census: small municipalities have populations $\leq$20,000; intermediate municipalities have populations between 20,001 and 100,000; and large municipalities have populations >100,000. Wage represents total formal earnings from social security records, expressed in 2008 dollars. Daily wage is the average daily income based on the contribution base. Worked days is the total number of days paid. Vacation days, temporal suspension days, non-labor sick leave days, labor sick leave days, and parental leave days reflect different types of leave taken. Employment-to-unemployment indicates cessation of employment between consecutive months. Unemployment-to-employment indicates transitions from unemployment to employment between consecutive months. Job-to-job indicates direct employer changes without unemployment spells. Outpatient visits counts total consultations per month. Medical procedures counts total procedures performed per month. ED visits counts emergency room visits per month.

Figure A.2: Effect of Mental Health Episodes on Earnings Without Restricting to New Episodes



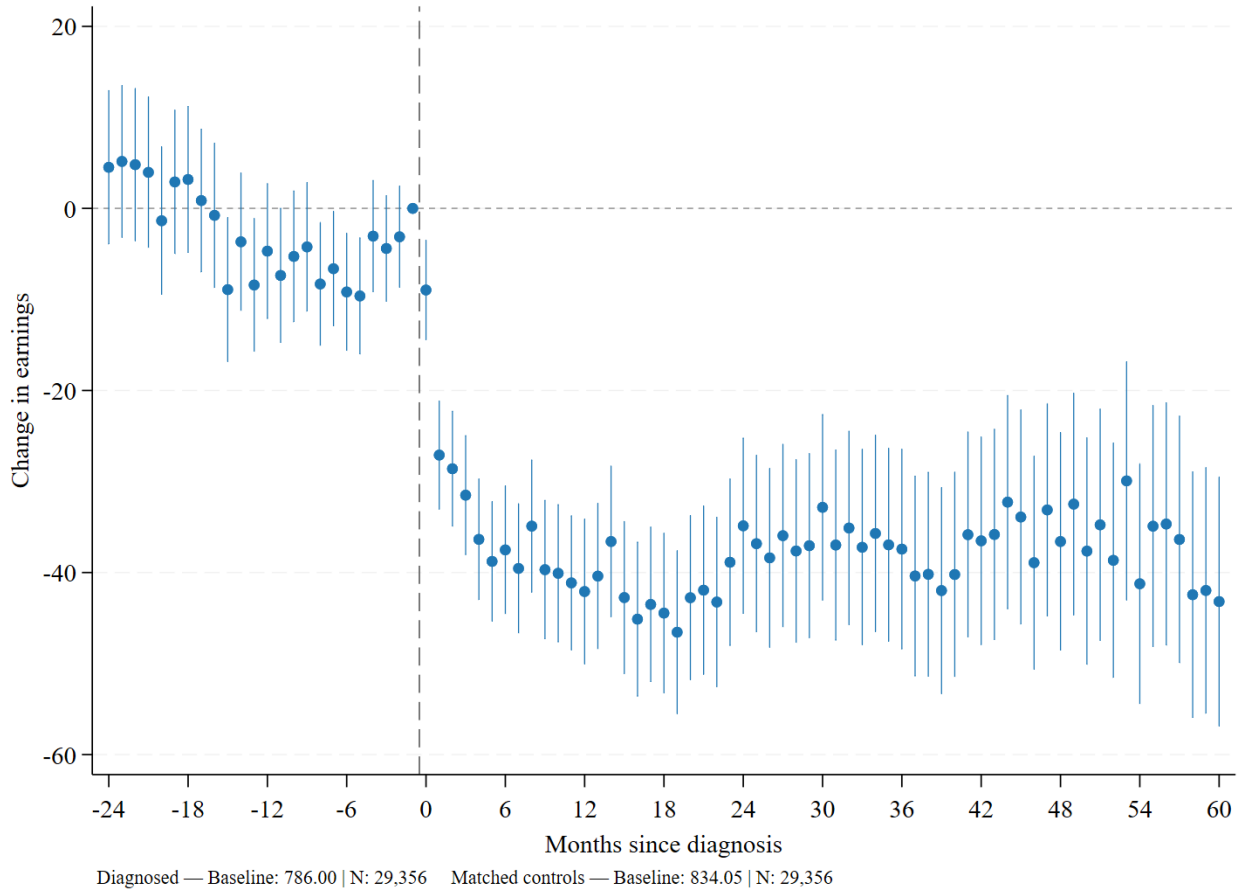
Note: The figure shows the effect of depression or anxiety episodes on earnings without restricting to new episodes, using the Callaway and Sant'Anna (2021) event-study estimator. Estimates are based on the matched sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis changes in monthly earnings in US dollars.

Figure A.3: Effect of Depressive Disorders on Earnings



Note: The figure shows the estimated effect of a depressive disorder diagnosis on earnings, distinguishing between depressive episodes (F32*) and recurrent depressive disorders (F33*). Depressive episodes correspond to new diagnoses among individuals with no prior mental health diagnosis in the preceding three years—the definition used throughout the paper. Recurrent depressive disorders, which carry a distinct ICD-10 code, identify individuals who have previously been diagnosed with a depressive episode. Point estimates are shown with 95% confidence intervals. Estimates are obtained from the Callaway and Sant'Anna (2021) event-study estimator applied to each diagnostic cell separately.

Figure A.4: Effect of Mental Health Episodes on Earnings at Five Years



Note: The figure shows the effect of depression or anxiety episodes on earnings up to 5 years after diagnosis, using the Callaway and Sant'Anna (2021) event-study estimator. Estimates are based on the matched sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis measures changes in monthly earnings in US dollars.

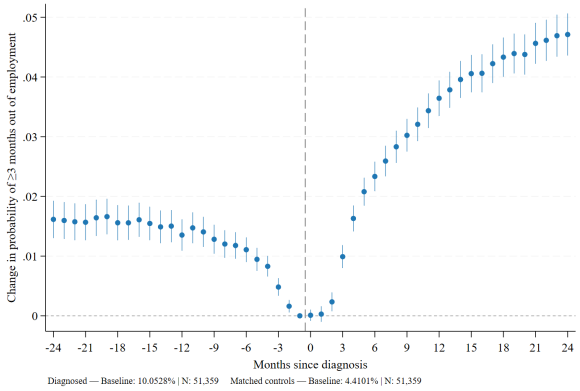
Table A.4: Diagnostic: 5-Year Subsample vs. Main-Sample Complement

	(1) 5y window (2012–2014)	(2) Complement (2015–2017)
Panel A. Individual characteristics		
Age	31.80	30.10
Female	0.57	0.58
Large municipality	0.93	0.93
Intermediate municipality	0.05	0.05
Small municipality	0.02	0.02
Panel B. Job market		
Wage	8,801.00	8,806.00
Formality	1.00	1.00
Daily wage	25.17	25.21
Worked days	346.60	346.50
Vacation days	0.71	0.71
Temporal suspension days	0.07	0.08
Non-labor sick leave days	0.24	0.26
Labor sick leave days	0.06	0.06
Parental leave days	0.03	0.05
Employment-to-unemployment	0.01	0.01
Unemployment-to-employment	0.02	0.02
Job-to-job	0.02	0.02
Panel C. Health sector		
Outpatient visits	0.29	0.30
Medical procedures	0.23	0.24
ED visits	0.02	0.02
Observations	29,356	22,003

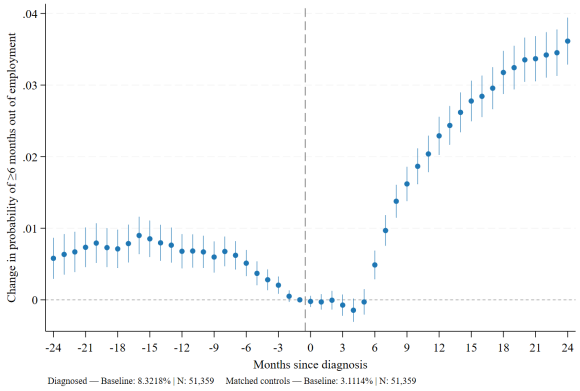
Note: Column (1) covers diagnosed workers with index events in 2012–2014, the subsample used for the 5-year effect estimation. Column (2) covers diagnosed workers with events in 2015–2017, excluded from the 5-year estimation because the panel does not extend far enough post-diagnosis. Panels A–C report person-level means from the 2009 pre-period panel, conditional on appearing in that panel. Large municipalities have populations >100,000; intermediate municipalities have populations between 20,001 and 100,000; and small municipalities have populations ≤20,000. Monthly earnings and daily wage are expressed in 2008 real USD. Days worked is the total number of days contributed to social security. Job loss, job gain, and job-to-job transition are monthly indicator variables. Any outpatient visit, procedure, and emergency visit are annual indicators from RIPS.

Figure A.5: Effect of Newly Diagnosed Depression or Anxiety on Nonemployment for Various Durations

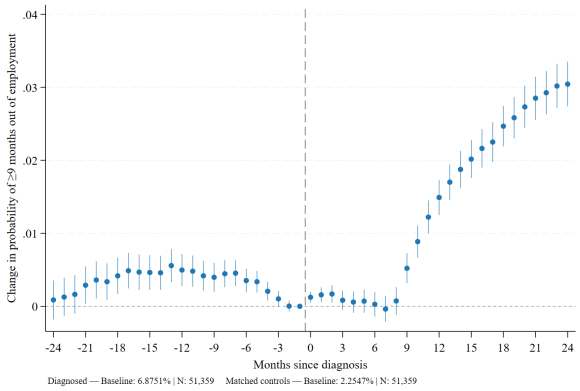
(a) ≥3 months out of formal employment



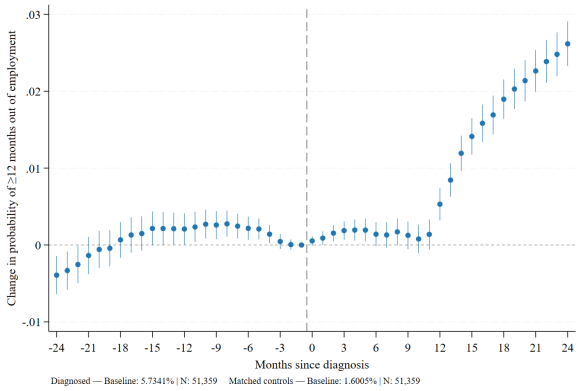
(b) ≥6 months out of formal employment



(c) ≥9 months out of formal employment

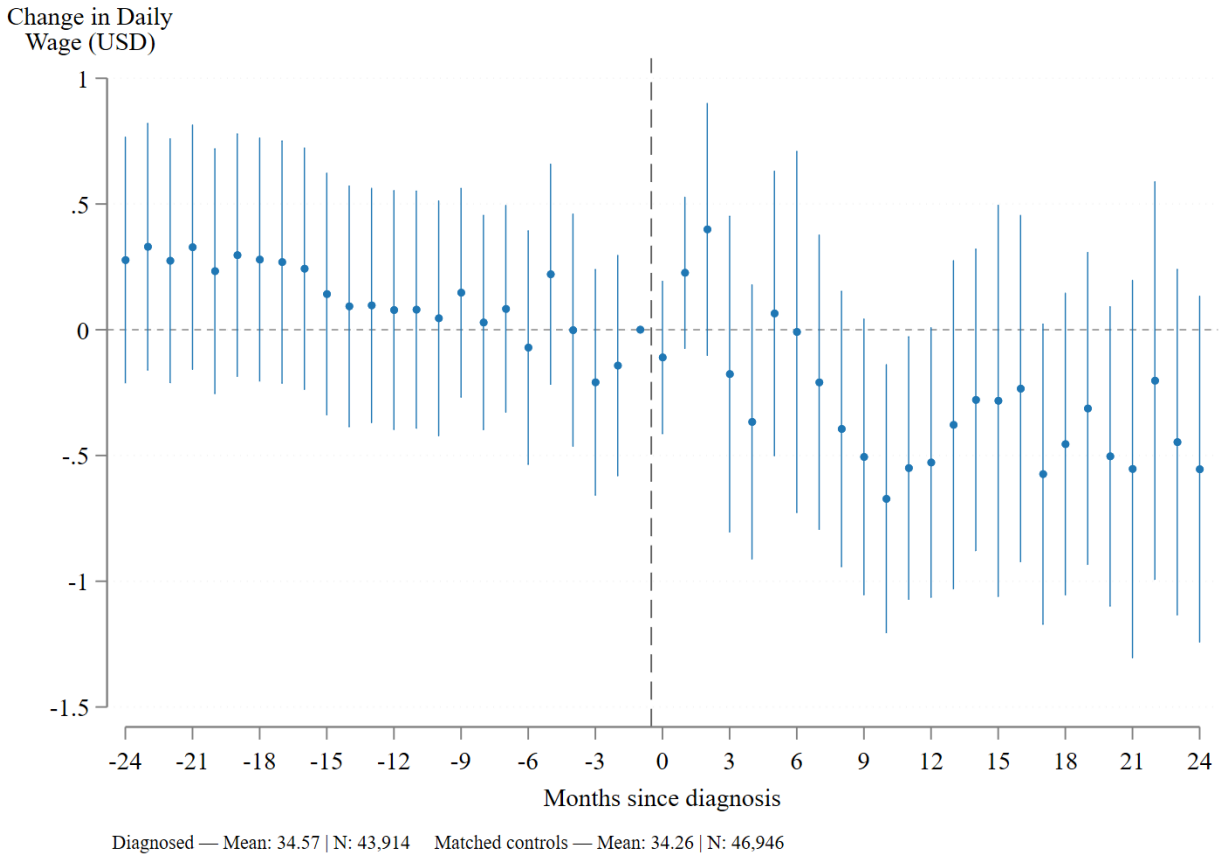


(d) ≥12 months out of formal employment



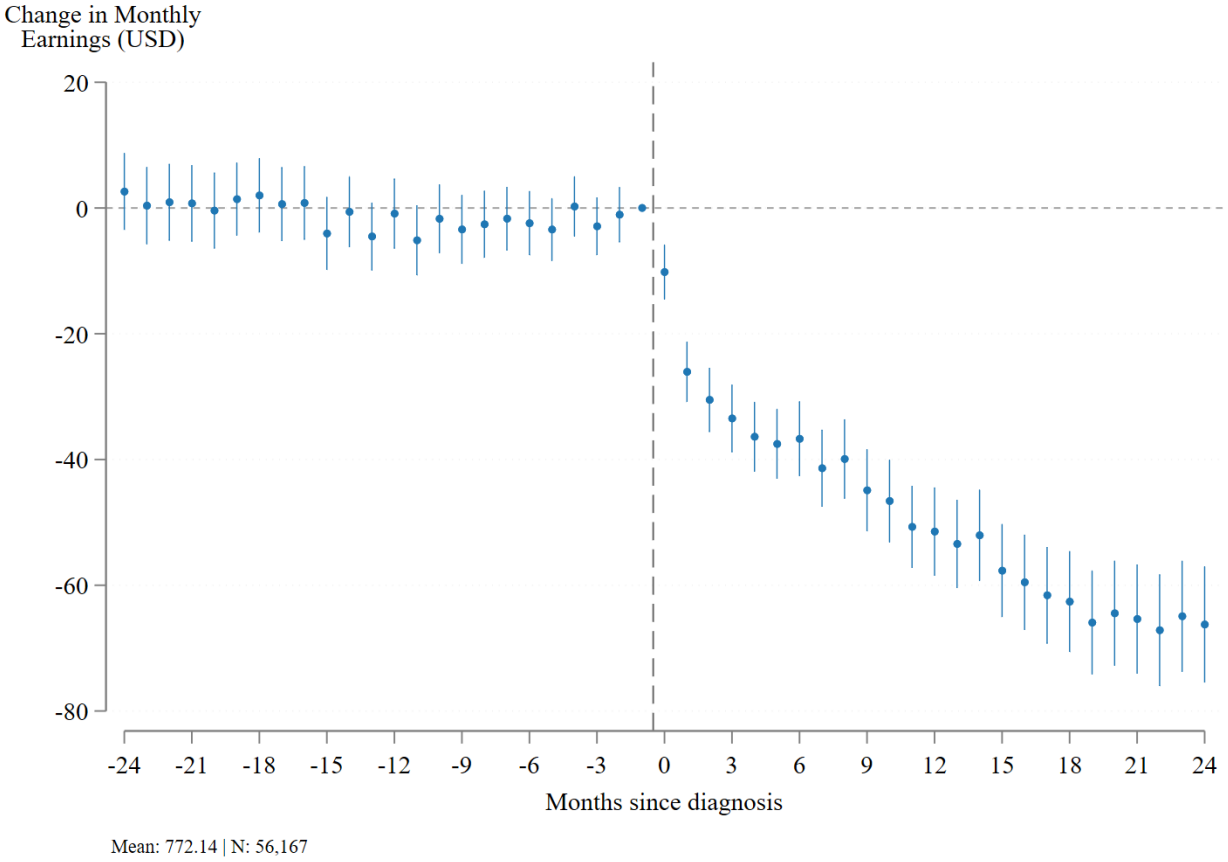
Note: Each panel plots the effect of a diagnosis for anxiety or depression on the probability of accumulating at least k months out of formal employment within the observation window, for $k \in \{3, 6, 9, 12\}$. The horizontal axis denotes months since diagnosis, and the vertical axis reports coefficients relative to the month prior to diagnosis. Estimates are obtained using the Callaway and Sant'Anna (2021) estimator. Vertical bars indicate 95 percent confidence intervals.

Figure A.6: Effect of Newly Diagnosed Depression or Anxiety on Wages



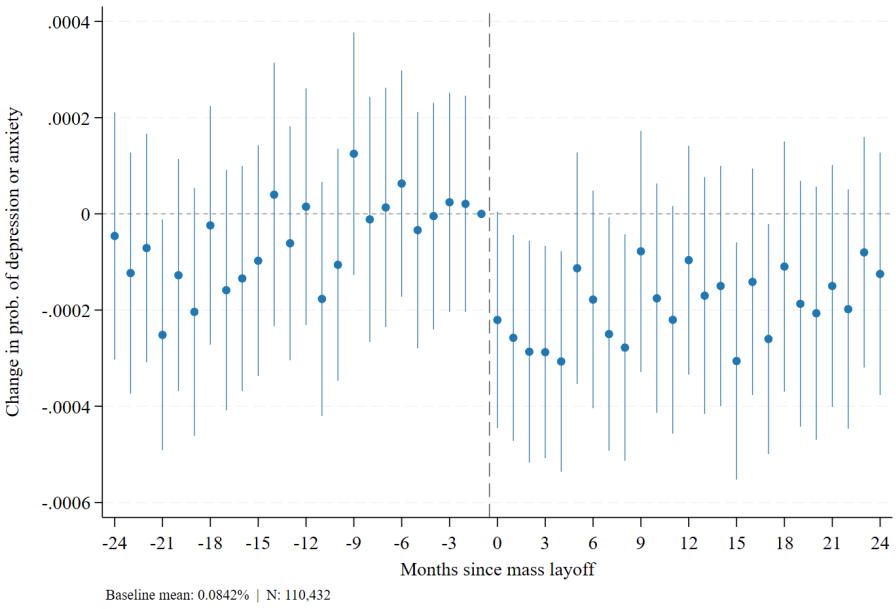
Note: This figure shows the effect of anxiety and depressive episodes on wages. The horizontal axis represents months since diagnosis, and the vertical axis represents the difference in US dollars. The sample includes 51,359 individuals diagnosed with depression or anxiety and 51,359 matched controls. Estimates are obtained using the Callaway and Sant'Anna (2021) estimator.

Figure A.7: Effect of Mental Health Episodes on Earnings Excluding Never Treated Individuals



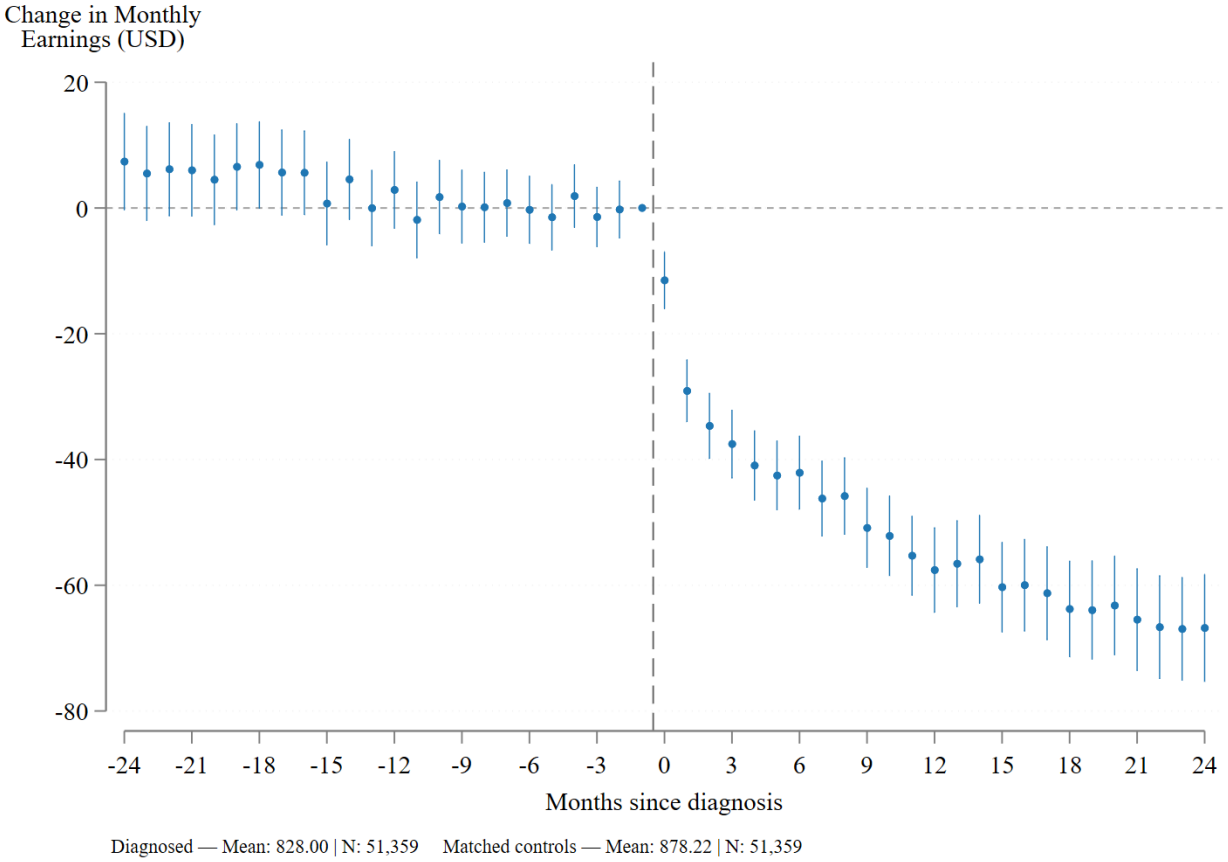
Note: Note: The figure shows the effect of depression or anxiety episodes on earnings, using the Callaway and Sant'Anna (2021) event-study estimator. Estimates are based on the eventually treated sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis measures changes in monthly earnings in US dollars.

Figure A.8: Effect of Mass Layoffs on the Probability of Any Depression or Anxiety Diagnosis



Note: This figure shows the effect of displacement from a mass layoff event on the probability of any observed depression or anxiety diagnosis. The horizontal axis denotes months since the layoff event, and the vertical axis reports coefficients relative to the month prior to displacement. Following Jacobson et al. (1993), a mass layoff is defined as a 30–90 percent reduction in employment at a firm with at least 20 baseline workers. Treatment is assigned at the worker level as the month of separation. Estimates are obtained using a two-way fixed effects estimator. The sample is restricted to displaced formal-sector workers ages 18–45 who worked at least 270 days in 2009. Vertical bars indicate 95 percent confidence intervals. This outcome captures any observed diagnosis in a given month, rather than only first diagnoses.

Figure A.9: Effect of Mental Health Episodes on Earnings: Two-Way Fixed Effects Estimates



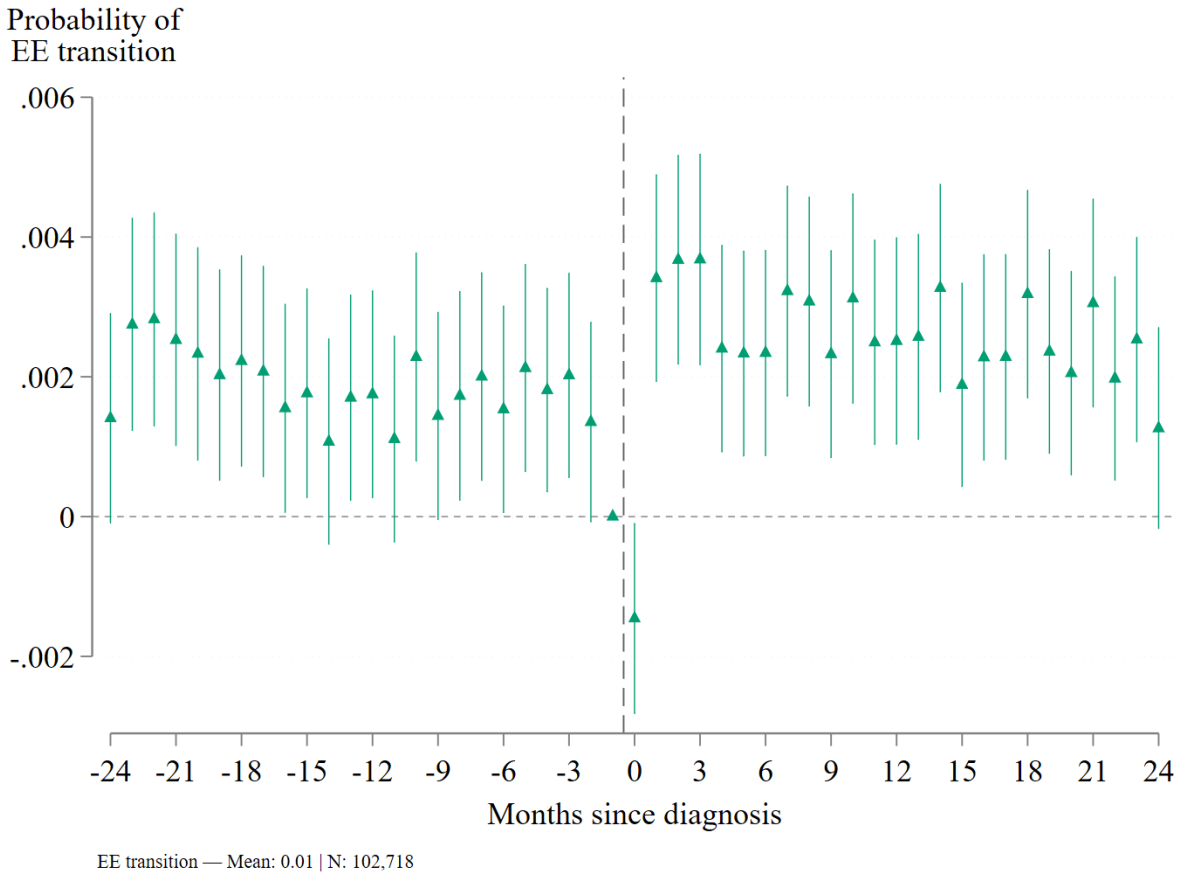
Note: Note: The figure shows the effect of depression or anxiety episodes on earnings, using the Two-Way Fixed Effects event-study estimator. Estimates are based on the matched sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis measures changes in monthly earnings in US dollars.

Table A.5: Treatment Effects on Days Worked Components at Diagnosis

	Non-Labor Sick Leave (1)	Labor Sick Leave (2)	Vacation Days (3)	Temp. Suspension (4)	Parental Leave (5)	Total Not Worked Days (6)	Worked Days (7)	Billed Days (8)
Coefficient	-0.0096**	0.2004***	-0.0169	0.0010	0.0059***	0.1988***	-0.3858***	-0.1938***
SE	(0.0043)	(0.0116)	(0.0112)	(0.0020)	(0.0008)	(0.0184)	(0.0341)	(0.0318)
Relative effect	-24.27%	73.65%	-2.34%	6.92%	7.15%	14.22%	-1.55%	-0.74%
Control mean	0.0396	0.2722	0.7221	0.0141	0.0831	1.3976	24.8421	26.1515

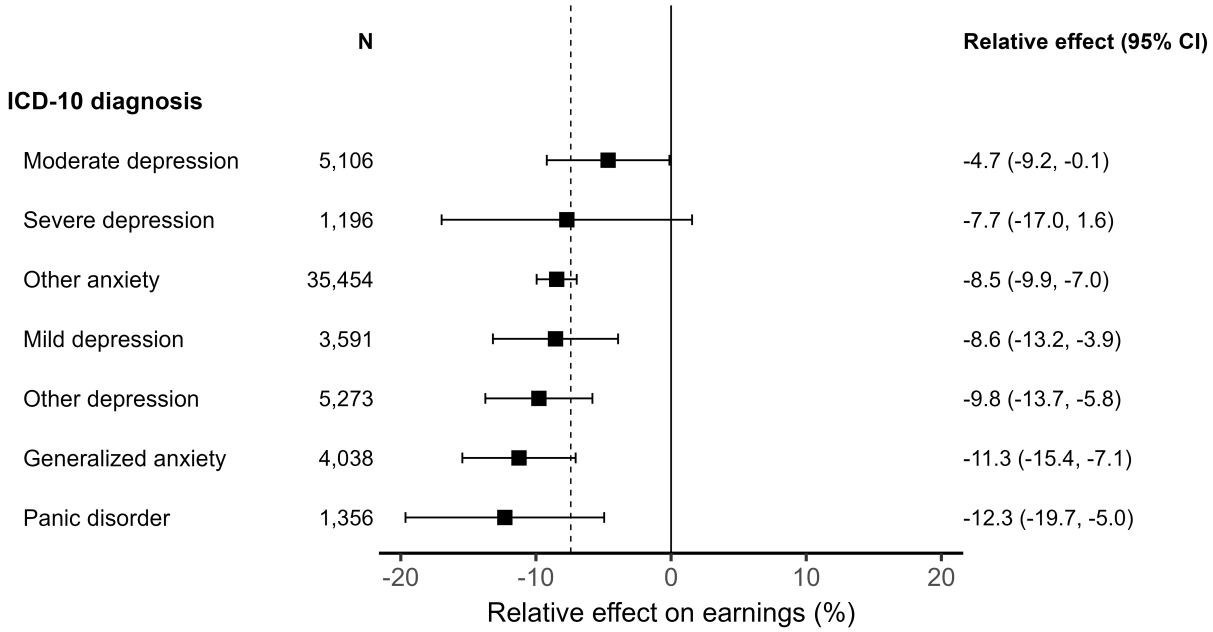
Note: This table reports Callaway and Sant'Anna (2021) estimates of the treatment effect during the first 3 months after diagnosis for each component of billed days. Columns (1)–(5) report effects on the individual components: non-labor sick leave, labor sick leave, temporary suspension, vacation, and parental leave. Column (6) reports the effect on total not-worked days, Column (7) on worked days, and Column (8) on total billed days. Standard errors are reported in parentheses. The control mean corresponds to matched controls at $t = -1$. The relative effect is the coefficient expressed as a percentage of the control mean. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.10: Effect of Mental Health Episodes on Employment-Employment Transitions



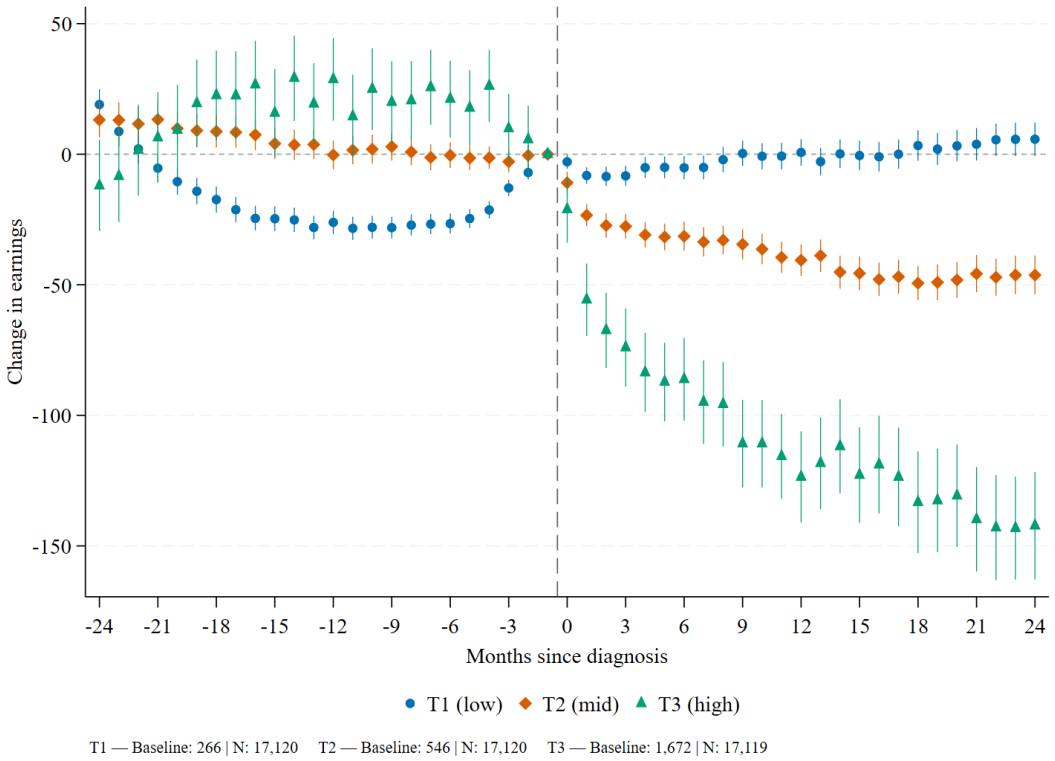
Note: Note: The figure shows the effect of depression or anxiety episodes on employment transitions, using the Callaway and Sant'Anna (2021) event-study estimator. Estimates are based on the matched sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis measures changes in the probability of an employment transition. EU represents employment to unemployment transition. UE represents unemployment to employment transition. EE represents employment to employment transition.

Figure A.11: Effects by Diagnostic Group



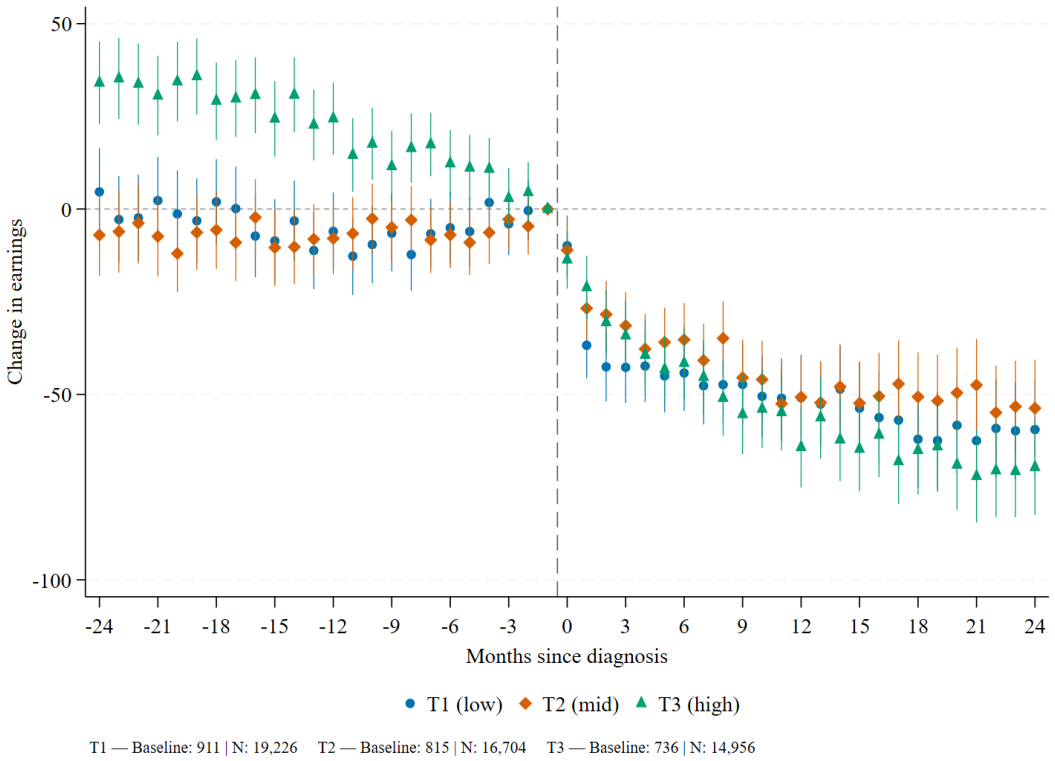
Note: This figure displays heterogeneous effects at 24 months after diagnosis by ICD-10 diagnostic category for just the eventually diagnosed units. Each point represents the estimated effect as a percentage of the subgroup's pre-diagnosis mean, with 95 percent confidence intervals. Estimates are obtained from the Callaway and Sant'Anna (2021) event-study estimator applied to each diagnostic cell separately.

Figure A.12: Monthly Labor Earnings by Baseline Earnings Tercile



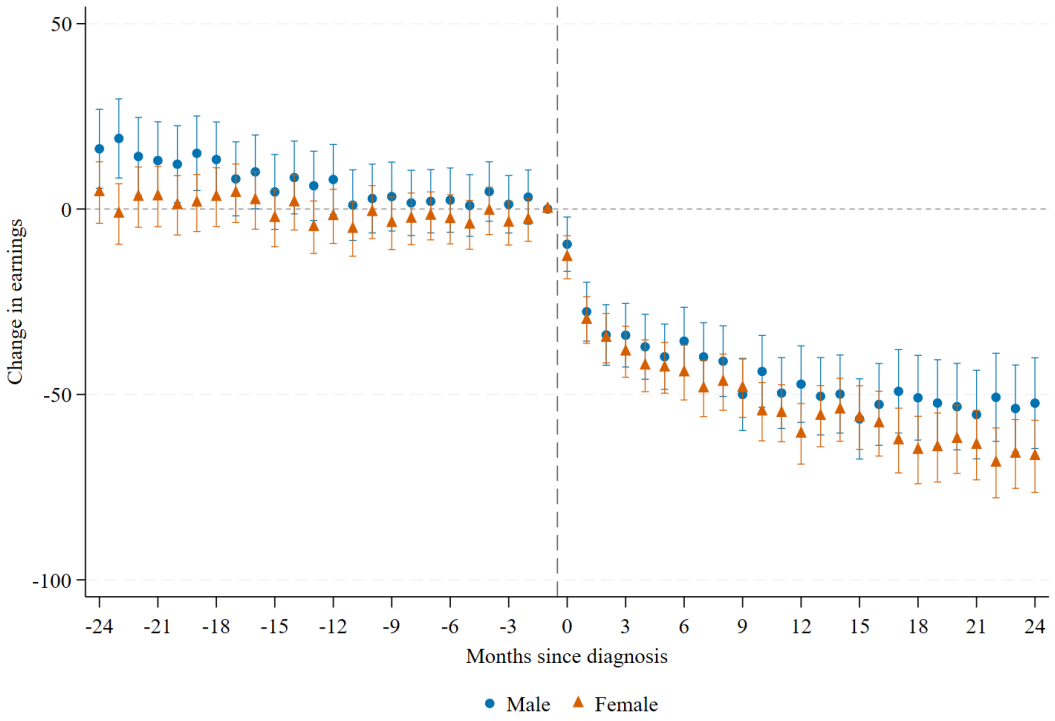
Note: The figure presents event-study estimates of the effect of a mental health diagnosis on monthly labor earnings, stratified by pre-diagnosis earnings tercile. Workers are assigned to terciles (T1 = low, T2 = middle, T3 = high) based on their average monthly earnings in the 24 months prior to diagnosis. Estimates for each tercile are obtained from the Callaway and Sant'Anna (2021) estimator applied separately within the matched sample. The horizontal axis denotes months relative to the initial diagnosis and the vertical axis measures changes in monthly earnings in Colombian pesos, with 95 percent confidence intervals. The caption reports the pre-diagnosis baseline mean and sample size for each tercile.

Figure A.13: Monthly Labor Earnings by Outpatient Utilization Tercile



Note: The figure presents event-study estimates of the effect of a mental health diagnosis on monthly labor earnings, stratified by pre-diagnosis outpatient utilization tercile. Workers are assigned to terciles (T1 = low, T2 = middle, T3 = high) based on the total number of outpatient visits recorded in the 24 months prior to diagnosis. Estimates for each tercile are obtained from the Callaway and Sant'Anna (2021) estimator applied separately within the matched sample. The horizontal axis denotes months relative to the initial diagnosis and the vertical axis measures changes in monthly earnings in Colombian pesos, with 95 percent confidence intervals. The caption reports the pre-diagnosis baseline mean and sample size for each tercile.

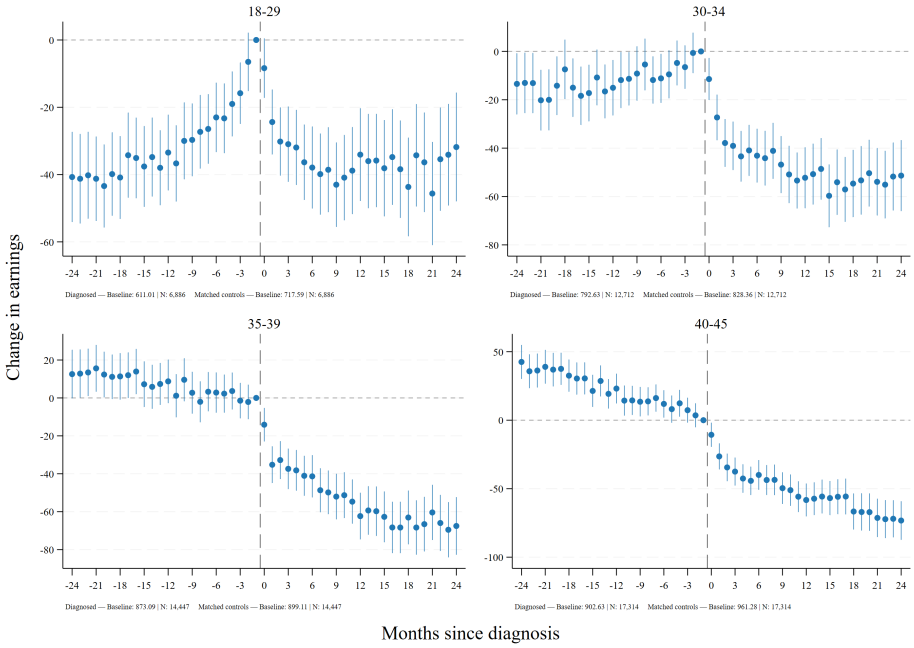
Figure A.14: Monthly Labor Earnings by Gender



Male -- Baseline: 863.49 | N: 21,640 Female -- Baseline: 801.98 | N: 29,246

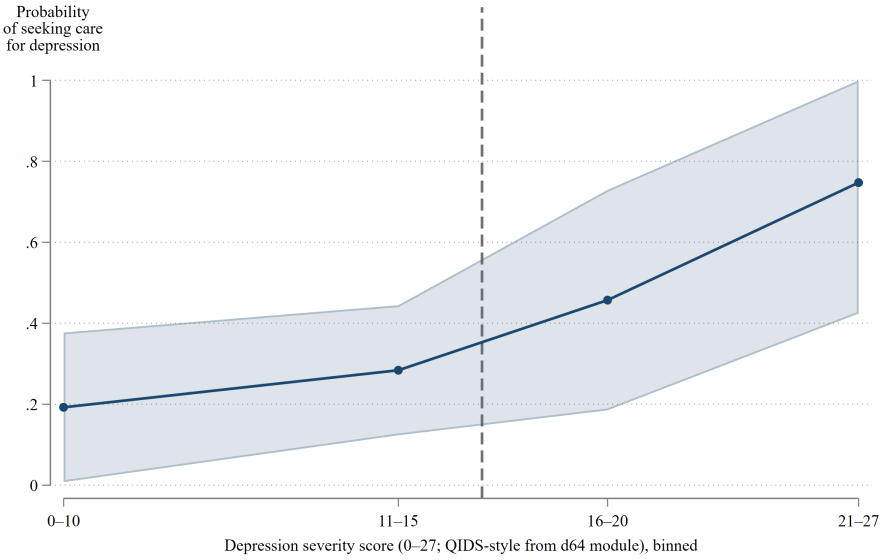
Note: The figure presents event-study estimates of the effect of a mental health diagnosis on monthly labor earnings, separately for male and female workers. Estimates are obtained from the Callaway and Sant'Anna (2021) estimator applied to the matched sample. The horizontal axis denotes months relative to the initial diagnosis and the vertical axis measures changes in monthly earnings in Colombian pesos, with 95 percent confidence intervals. The caption reports the pre-diagnosis baseline mean and sample size for each group.

Figure A.15: Monthly Labor Earnings by Age Group

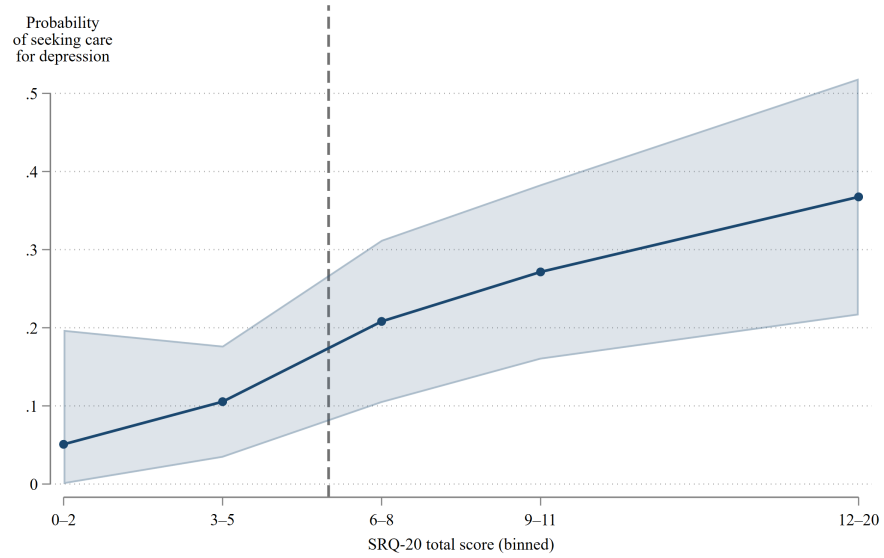


Note: The figure presents event-study estimates of the effect of a mental health diagnosis on monthly labor earnings for five age groups defined at the time of first diagnosis: 18–29, 30–34, 35–39, and 40–45 years. Estimates for each age group are obtained from the Callaway and Sant'Anna (2021) estimator applied separately within the matched sample. The horizontal axis denotes months relative to the initial diagnosis and the vertical axis measures changes in monthly earnings, with 95 percent confidence intervals. The caption in each panel reports the pre-diagnosis baseline mean and sample size for diagnosed and control groups.

Figure A.16: Probability of Seeking Care for Depression by Severity



(a) QIDS-style Score



(b) SRQ-20 Score

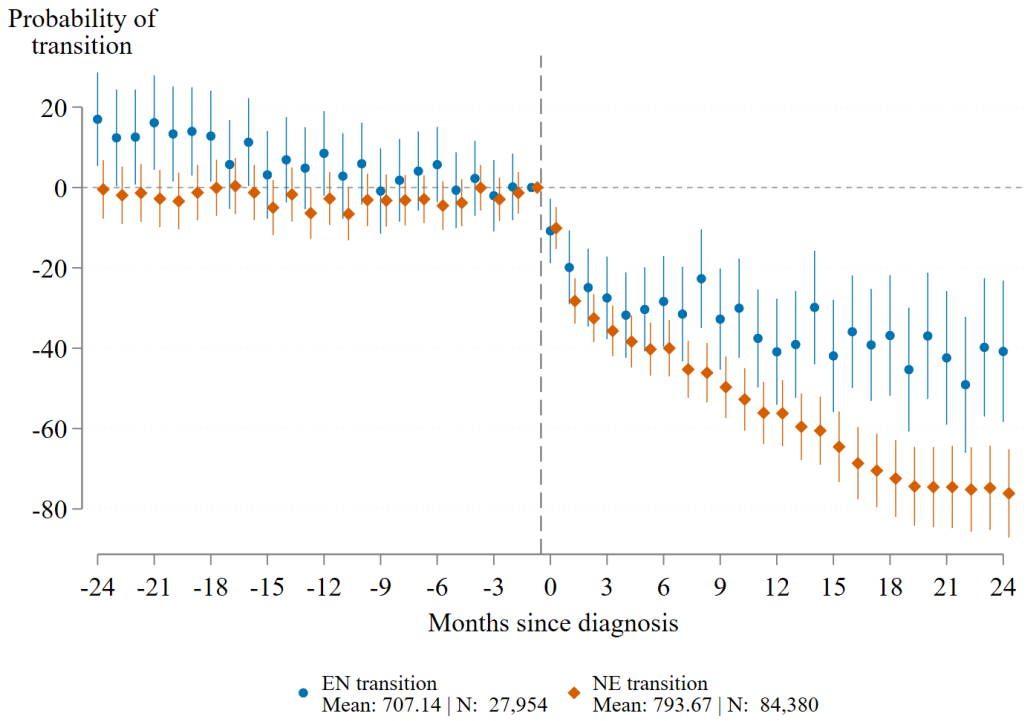
Note: Data are from the *Encuesta Nacional de Salud Mental 2015* (ENSM) Ministerio de Salud y Protección Social (2015). Both panels plot the weighted probability of ever seeking professional care for depression as a function of symptom severity among insured adults aged 18–45. Panel (a) uses a 0–27 depression severity score constructed from graded symptom responses in the ENSM depression module using a nine-domain 0–3 scaling, grouped into bins (0–10, 11–15, 16–20, 21–27). Panel (b) uses the SRQ-20 scale, grouped into bins (0–2, 3–5, 6–8, 9–11, 12–20). Shaded areas denote 95% confidence intervals based on effective sample size within bins. Vertical dashed lines indicate thresholds corresponding to moderate-to-severe symptomatology.

Table A.6: Comorbidity Robustness: Effect of Mental Health Diagnosis on Monthly Earnings

	Sample			Baseline mean		Post-diagnosis ATT		
Exclusion	N (ET)	N (NT)	Excl.	ET	NT	$t = 0$	$t = 12$	$t = 24$
None (baseline)	48,227	48,227	0	882.5	916.3	-13.1***	-65.9***	-74.8***
Excl. hypertension	47,889	47,889	676	882.2	915.7	-13.1***	-66.0***	-75.4***
Excl. HIV / diabetes markers	47,378	47,378	1,698	885.1	917.8	-12.9***	-65.7***	-74.0***
Excl. pain / musculoskeletal	46,514	46,514	3,426	882.6	916.3	-13.6***	-65.8***	-75.2***
Excl. dental / obesity / gastric	47,008	47,008	2,438	884.8	918.1	-13.3***	-66.6***	-74.6***
Excl. any comorbidity	44,361	44,361	7,732	887.3	919.0	-13.5***	-66.3***	-75.2***

Note: The table presents event-study estimates of the effect of a mental health diagnosis on monthly labor earnings under alternative exclusions of contemporaneous physical comorbidities. Estimates are obtained from the Callaway and Sant'Anna (2021) estimator applied to a matched sample of diagnosed workers and controls. Each row re-estimates the specification after excluding diagnosed workers and their matched controls when the diagnosed worker received the indicated comorbidity diagnosis as a de novo encounter within 30 days before the mental health event. The columns report sample sizes and baseline means for diagnosed and control groups, together with the ATT at event times $t = 0$, $t = 12$, and $t = 24$ months relative to the initial mental health diagnosis. Comorbidity groups are: (1) hypertension (I10); (2) HIV or diabetes markers (B24, Z124, E039, E119); (3) pain or musculoskeletal conditions (R51, M545, and related ICD4 codes); and (4) dental, obesity, or gastric conditions (K021, E669, E660, K051, K297). Baseline means are measured at $t = -1$, and Excl. reports the reduction in diagnosed-control observations relative to the baseline specification. Sample sizes and effect estimates in this table do not coincide with those in the main analysis because this exercise was produced using an outdated matched sample. Stars indicate statistical significance of the event-time coefficients: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.17: Monthly Labor Earnings: Treated vs. Not Treated



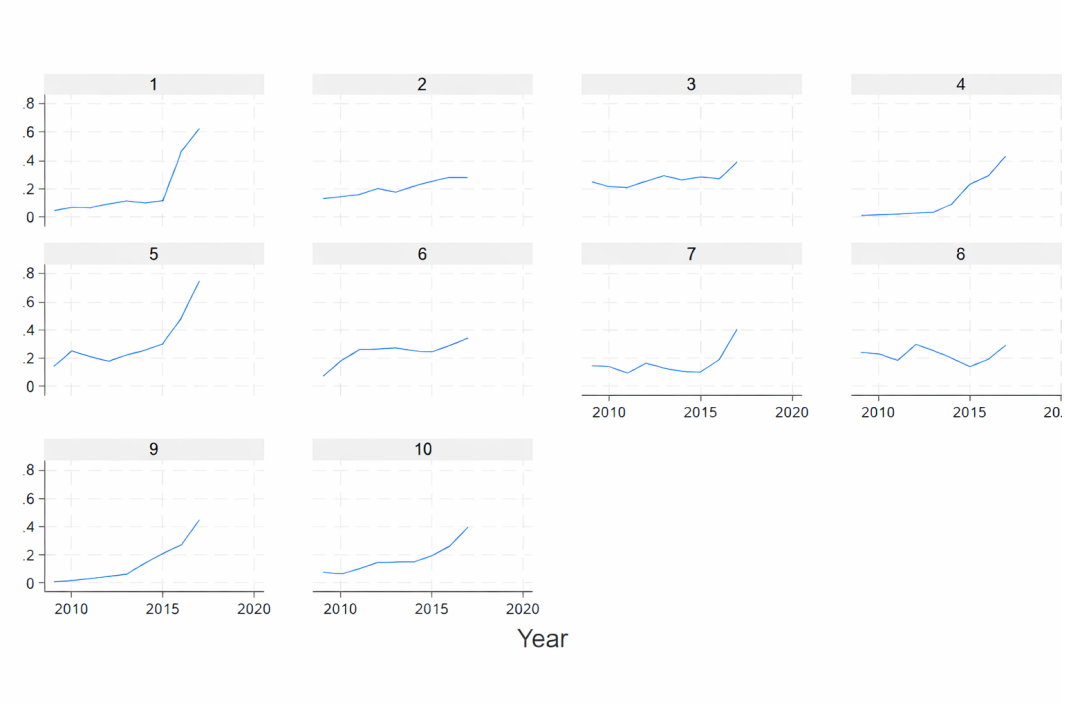
Note: The figure shows the effect of depression or anxiety episodes on monthly earnings, using the Callaway and Sant'Anna (2021) event-study estimator. Estimates are based on the matched sample. The estimated coefficients are presented with 95 percent confidence intervals. The horizontal axis denotes months relative to the initial diagnosis. The vertical axis measures changes in monthly earnings in US dollars. Blue estimates correspond to workers who subsequently receive formal treatment (Treated); red estimates correspond to workers who receive a diagnosis but no formal treatment record (Not Treated).

Table A.7: Characteristics: Treated vs. Not Treated Workers

	(1) Treated	(2) Not treated
Panel A. Individual characteristics		
Age	30.7	31.2
Female	0.594	0.580
Large municipality	0.926	0.918
Intermediate municipality	0.051	0.060
Small municipality	0.023	0.023
Panel B. Job market		
Wage	8,106	8,748
Formality	1.000	1.000
Daily wage	23.24	25.05
Worked days	346.1	346.2
Vacation days	0.675	0.675
Temporal suspension days	0.075	0.067
Non-labor sick leave days	0.254	0.236
Labor sick leave days	0.062	0.056
Parental leave days	0.047	0.040
Employment-to-unemployment	0.011	0.011
Unemployment-to-employment	0.015	0.015
Job-to-job	0.018	0.017
Panel C. Health sector		
Outpatient visits	0.306	0.288
Medical procedures	0.249	0.239
ED visits	0.019	0.017
Observations	13,977	42,189

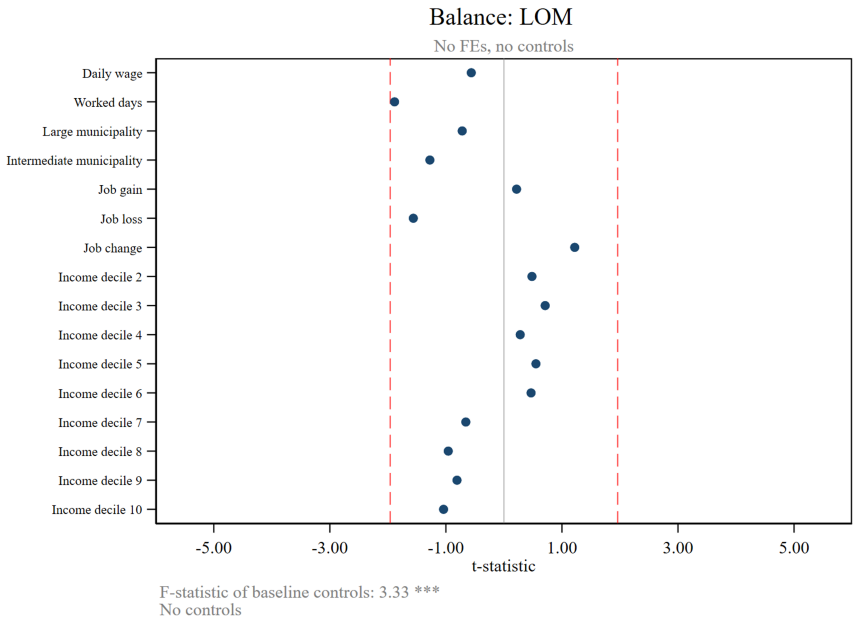
Note: Column (1) reports means for workers who received formal mental health treatment. Column (2) reports means for workers who did not receive formal mental health treatment. Panels A–C report event-level means of 2009 pre-period characteristics, merged at the person level. Large municipalities have populations >100,000; intermediate municipalities have populations between 20,001 and 100,000; and small municipalities have populations ≤20,000. Wage and daily wage are expressed in 2008 real USD. Worked days is the total number of days contributed to social security. Employment-to-unemployment, unemployment-to-employment, and job-to-job are monthly indicator variables. Outpatient visits, medical procedures, and ED visits are annual indicators from RIPS.

Figure A.18: Probability of Treatment for Mental Health Episodes by Insurer

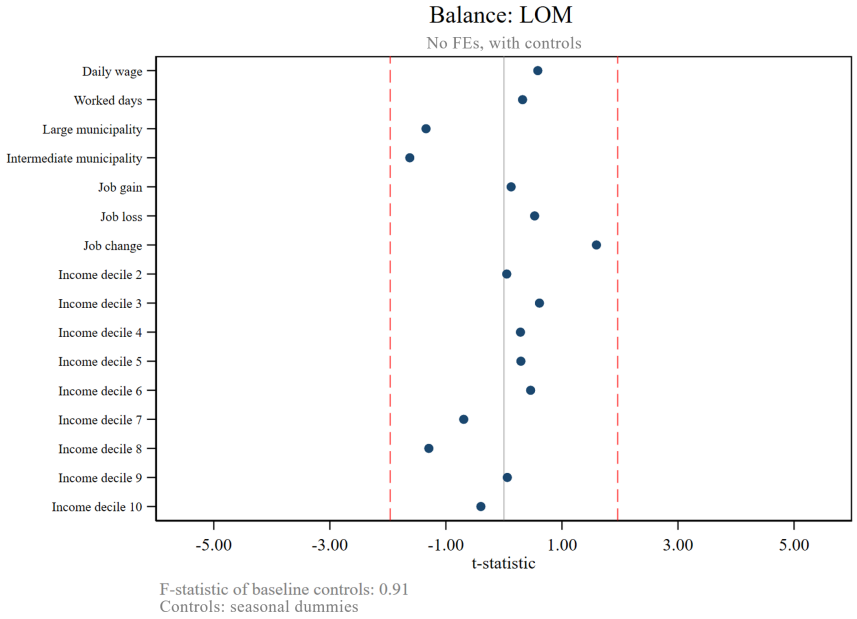


Note: The figure reports the probability of formal treatment for mental health episodes by insurer, based on the authors' main calculations using RIPS. The series are shown separately for the nine largest health insurers in Colombia; category 10 groups all remaining insurers. The horizontal axis denotes calendar years. The vertical axis measures the probability of receiving formal treatment.

Figure A.19: Balance of the Instrument



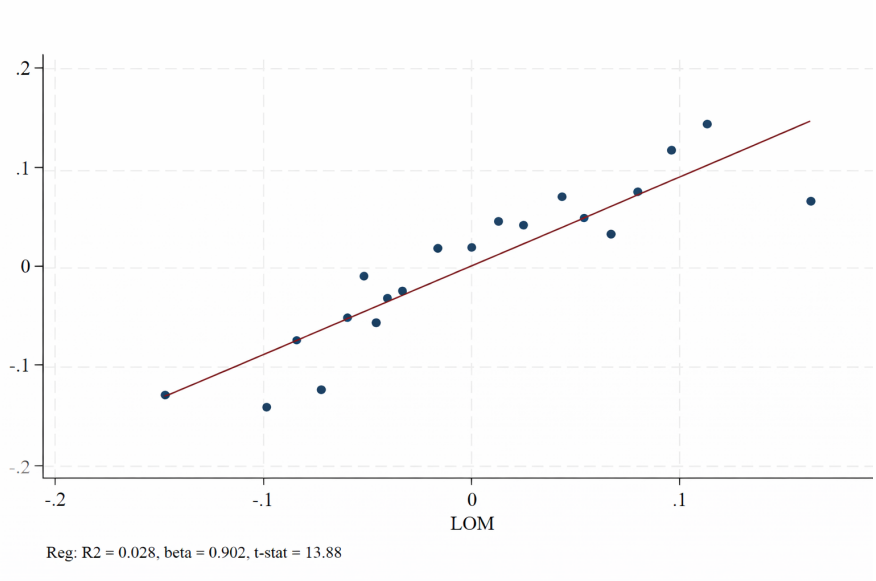
(a) No additional controls



(b) With additional controls

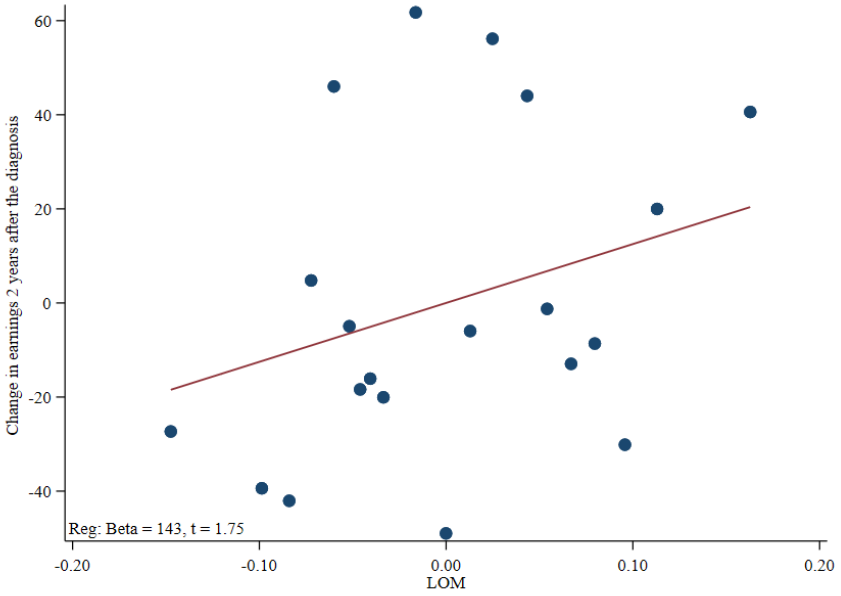
Note: We assess balance by estimating $Z_i = \alpha + X_i'\theta + W_i'\gamma + \varepsilon_i$, where X_i is a vector of pre-determined baseline characteristics and W_i is an auxiliary set of controls. Panel (a) presents results with $W_i = \emptyset$. Panel (b) includes in W_i indicators for year-of-event, indicators for day-of-week, and municipality fixed effects.

Figure A.20: First Stage Relationship Between Instrument and Treatment by a Mental Health Specialist



Note: The figure reports the relationship between the leave-out mean (LOM) instrument and specialist care. We estimate $Treat_i = \alpha + \pi\, Z_i + W_i'\gamma + \nu_i$, where $Treat_i$ indicates specialist care and Z_i is the LOM instrument. W_i includes indicators for year-of-event, day-of-week indicators, and municipality fixed effects.

Figure A.21: Reduced Form Relationship Between Instrument and Change in Earnings



Note: The figure reports the relationship between earnings and the leave-out mean (LOM) instrument. We estimate $y_i = \alpha + \rho\, Z_i + W_i'\gamma + \varepsilon_i$, where y_i is the change in earnings 2 years after the diagnosis and Z_i is the LOM instrument. W_i includes year-of-event indicators, day-of-week indicators, and municipality fixed effects.

Table A.8: OLS and IV Estimates of the Effect of Specialist Care on Earnings

	3	6	9	12	15	18	21	24
Panel A: No controls								
OLS	6.13	25.29**	27.95**	18.54	18.32	37.17**	32.64**	35.55**
	(8.79)	(10.70)	(11.89)	(12.46)	(13.63)	(14.73)	(14.86)	(15.32)
IV (2SLS)	-35.81	31.50	40.97	54.09	73.35	153.37***	186.67***	119.46**
	(32.00)	(37.01)	(39.85)	(43.25)	(45.63)	(56.47)	(60.19)	(58.81)
RF	-39.16	34.44	44.81	59.15	80.22	167.72***	204.14***	130.64**
	(34.92)	(40.48)	(43.60)	(47.31)	(49.86)	(61.48)	(65.34)	(64.21)
N	57744	57744	57744	57744	57744	57744	57744	57744
Panel B: With controls								
OLS	6.94	26.11**	28.77**	19.36	19.14	37.99**	33.46**	36.37**
	(9.58)	(11.31)	(12.44)	(13.07)	(14.14)	(15.33)	(15.40)	(15.91)
IV (2SLS)	-5.61	61.69	71.17	84.29	103.55	183.56**	216.86**	149.65*
	(65.28)	(68.85)	(71.16)	(73.92)	(74.63)	(82.95)	(86.74)	(85.94)
RF	-26.80	46.80	57.16	71.51	92.58	180.08**	216.49***	142.99*
	(59.69)	(63.09)	(65.29)	(68.23)	(68.76)	(77.91)	(82.07)	(81.75)
N	57744	57744	57744	57744	57744	57744	57744	57744

Note: OLS = Ordinary Least Squares; IV (2SLS) = Two-Stage Least Squares using instrumental variables; RF = Reduced Form (direct effect of the instrument on the outcome); N = number of observations. Standard errors in parentheses. For the quarterly construction, the outcome uses a $k = 3$ month rolling sum. Monthly-equivalent effects divide both coefficients and standard errors by $k = 3$ (fixed across horizons). Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.