The Effect of Depression on Income: Evidence from Colombia*

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Abstract

Depression affects an estimated 10% of individuals worldwide and may have negative impacts on economic activity. We use nationwide administrative data from Colombia to estimate the causal impact of depression on earnings using a difference-in-differences approach that leverages cross-individual variation in the timing of major depressive episodes. Among young, formally employed workers, depressive episodes reduce earnings by 8.2%, an effect that persists for up to 24 months and is primarily mediated by the extensive margin. To further support a causal relationship between depression and earnings, we use propensity score matching to compare labor market outcomes for individuals with depression who plausibly vary only in their access to specialty care. Relative to individuals who do not access such care, those who do experience an increase in earnings which compensates for most of the income loss due to depression.

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1 Introduction

Depression affects an estimated one in ten individuals during their lifetime. Most affected individuals are of working age, raising the question of whether depression affects labor market outcomes. Such an impact is plausible: depression is characterized by sadness, loss of interest, fatigue, difficulty concentrating, and other mental states that may causally affect job-seeking behavior or on-the-job productivity. However, most existing estimates of the economic burden of mental illness are derived from cross-sectional associations that may be confounded by unobserved heterogeneity and reverse causality, or from randomized trials whose results may not extrapolate to other contexts.

There are several challenges to accurately estimating the economic costs of depression. First, from a measurement perspective, there is no clear consensus on how depression should be measured. The current gold standard is a structured clinical interview with a physician, but data on the results of such interviews is difficult to obtain at a large scale. Furthermore, because depression is an episodic disorder characterized by periods of remission and relapse, associating measures of depression with earnings in the cross section—as is commonly done in existing literature—would not capture dynamic effects. Second, isolating the causal effect of depression on income requires a methodological approach that can address biases arising from unobserved heterogeneity and reverse causality. Such an approach would be possible with quasiexperimental variation in depression or its severity, but identifying sources of such variation is challenging since depression tends to co-occur with factors that are correlated with earnings. Finally, few data sources allow for simultaneous observation of measures of mental distress and labor market outcomes. This limits the extent to which researchers can understand the impact of depression on labor market outcomes and dynamics. For example, supposing there is a causal effect of depression on earnings, it is unclear if this effect is persistent or temporary, and to what extent it operates through the extensive versus the intensive margin. Distinguishing these possibilities could inform policies aimed at improving labor market outcomes for individuals with depression.

In this paper, we address these challenges using linked, nationwide administrative data from Colombia to estimate the causal impacts of depression on labor market outcomes. We do so by examining two sources of variation in depression. In the first part of the paper, we study the impact of depression by documenting income trajectories before and after major depressive episodes, leveraging the panel nature of our data to control for unobservables. In the second part of the paper, motivated by literature demonstrating that standard-of-care mental health treatments improve depressive symptoms, we compare labor market outcomes

for individuals who plausibly vary only in their access to such care. Under both approaches, our results are consistent with a negative impact of depression: income falls by 8.2% following a major depressive episode, an effect that remains persistent for up to two years after the onset of depression. Furthermore, timely access to mental health care increases income, compensating for most of the earnings loss induced by depression.

To derive our estimates, we begin by linking employer-employee data for all formally employed individuals in Colombia with the universe of health care utilization records between 2009 and 2019. The resulting panel dataset allows us to observe a rich set of labor market outcomes, including earnings and days worked, as well as diagnosed illnesses, which we identify using diagnosis codes assigned by physicians during each encounter. To identify depressive episodes, we restrict to individuals' first observed diagnosis of depression and, following Baicker et al. (2018), further restrict to individuals without a diagnosed mental health disorder in the three years preceding their diagnosis of depression. These restrictions are intended to identify an individual's first depressive episode—or "mental break"—which we argue constitutes a negative shock to mental health.

We then turn to examining income trajectories around depressive episodes. To do so, we use an event study approach that leverages variation in the timing of depression across individuals. We find that depression has sharp and persistent effects on earnings: immediately following a depressive episode, earnings fall by 8.2%, an effect that persists for up to 24 months. Two-thirds of this effect is explained by increased flows from employment to unemployment and nonparticipation. One-third is explained by a reduction in wages, though this effect is imprecisely estimated, and there is no significant effect on days worked.

Having established our main effect, we spend the next part of the paper addressing concerns that our estimated effect may not be causal. One potential concern is that our results may be driven by reverse causality. If this were the case, we would have expected to observe a fall in earnings or an increase in employment-to-unemployment transitions prior to the onset of depressive episodes. We do not observe either. Moreover, the probability of job loss is statistically indistinguishable from zero in the month depression begins, further reassuring us that depression precedes job loss.

Another potential concern is that health shocks coinciding with depression are driving our results. This is plausible given that depression tends to co-occur with other chronic medical problems that may themselves affect earnings. But it is less of a concern in our sample of young adults, for whom visits to the physician are most commonly for acute medical problems that self-resolve. Pregnancy is a potential threat to our design given that it tends to

co-occur with depression and is associated negatively associated with labor market outcomes. To address this concern, we remove individuals from our sample who have any records of pregnancy or parental leave in the three years prior to their depressive episode.

A third potential concern with our empirical approach is the presence of time-varying unobserved factors correlated with both depression and earnings. Surveys of individuals in the months leading up to depressive episodes indicate that such episodes are frequently preceded by one or multiple stressors, such as interpersonal conflicts and changes in close relationships (Meisenzahl et al., 2024). The individual fixed effects in our event study address confounding due to time-invariant characteristics, but we are unable to rule out the possibility that our effect may be driven by such time-variant factors. We thus interpret the results from the first part of our paper as the effect of depression and any stressors it comes "bundled" with in real-world settings.

In the second half of the paper, motivated by a large body of medical literature showing that evidence-based treatments for depression reduce depressive symptoms, we estimate the effect of treatment for depression on earnings. The main empirical challenge to estimating the effect of such treatment is that individuals who receive it systematically differ from those who do not. In our sample, individuals who receive mental health care are younger, more likely to be female, earn less, and have higher levels of health care utilization at baseline. To adjust for these differences, we use a rich set of individual, socioeconomic, and health-related characteristics to propensity-score-match individuals with and without access to treatment. Leading up to depressive episodes, our matched samples have similar earnings trajectories, suggesting that the groups are similar on both observable and unobservable characteristics. In the months after, individuals who receive treatment experience a gradual increase in income that, two years following the onset of depression, compensates them for most of the earnings loss induced by depression. We view this as evidence that timely access to mental health care improves earnings.

The primary threat to interpreting our estimated effect of treatment as causal is the presence of unobserved heterogeneity between treated and untreated individuals conditional on the set of variables used to construct our propensity scores. To address this concern, we use an instrumental variables strategy which leverages geographic variation in the probability of receiving treatment. Motivated by cross-hospital differences in the probability of referral to mental health specialists, which could arise due to the differences in practice style or the availability of specialists, we instrument an individual's own treatment status with the leave-out mean probability of treatment among the set of individuals who visited the same

hospital as the index patient. Our treatment effects from this approach are smaller but qualitatively similar to those from our propensity score matching exercise, lending further credibility to our results on the effects of treatment.

Our findings contribute to two main strands of literature. First, we contribute to a literature on the impacts of health on human capital (Banerjee et al., 2017; Currie and Madrian, 1999; de Oliveira et al., 2023; Dobkin et al., 2018; Doran and Kinchin, 2017; Grossman, 1972; Mahomed, 2020; Trautmann et al., 2016). Most studies in this literature examine the impacts of physical illness on earnings and find negative effects. Very few studies examine the impacts of mental health on earnings. Those that do tend to make simple comparisons between individuals with and without mental illness, use instrumental variable approaches which may violate the exclusion restriction, or are sensitive to how mental health is measured. Our study contributes to this literature by studying a clearly defined health shock and providing one of the first quasiexperimental estimates of depression on earnings.

We also contribute to a growing literature on the impacts of mental illness treatment on economic outcomes (Angelucci and Bennett, 2022; Baranov et al., 2020; Bhat et al., 2022; Biasi et al., 2021; Blattman et al., 2023; Luck-Sikorski et al., 2017; Lund et al., 2024; McKelway et al., 2023; Ridley et al., 2020; Shreekumar and Vautrey, 2022). Findings in this literature on the effects of depression on earnings are mixed, and most of the evidence comes from randomized trials, the results of which may not extrapolate to other contexts. Our study is one of the first to use national linked administrative data to study the relationship between mental health and earnings.

The remainder of this paper is organized as follows. Section II describes our setting. Section III describes the data. Section IV describes our empirical approach. Section V describes our results. Section VI the model. Section VII concludes.

2 Background

2.1 Mental Health Care in Colombia

Colombia has near-universal health insurance coverage, with recent estimates indicating that 98% of individuals are insured (Ministerio de Salud y Protección Social de Colombia, 2025a). There are two primary forms of insurance coverage. The contributive regime primarily covers individuals who are employed in the formal sector and is financed by both employees and employers. The subsidized regime primarily covers individuals in the informal sector and is largely financed by the government. Formally employed individuals, who are the focus of our

study, are significantly more likely to be covered by the contributive regime. The range of services in both regimes, however, has converged over the last decade. (Ministerio de Salud y Protección Social de Colombia, 2025b).

In recent years, the Colombian government has increasingly recognized mental health as a key aspect of overall health. The National Mental Health Law, signed in 2013, formally established mental health care as a fundamental right (Congreso de Colombia, 2013). The law acknowledged the psychosocial impacts of Colombia's history of armed conflict and the need to provide mental health care to all individuals. It aimed to reduce stigma, promote early detection, and increase access to care. It increased access by expanding insurance coverage to include mental health care and better integrating mental health care into primary care services (Gómez-Restrepo et al., 2016).

To seek mental health care in Colombia, individuals may present to the outpatient setting, emergency department, or inpatient setting. Most mental health care takes place in the outpatient setting, where individuals may present to a primary care physician or directly to a specialist such as a psychologist or psychiatrist. Primary care physicians may also refer patients to specialists for further diagnosis and treatment.

2.2 Diagnosis and Treatment of Depression

Depression is a common mental health condition characterized by depressed mood, loss of interest, poor sleep, guilt, low energy, difficulty concentrating, low or high appetite, psychomotor slowing, and thoughts of suicide. The diagnostic for depression criteria require that an individual has at least five of the aforementioned symptoms over a two-week period, at least one of which must be depressed mood or loss of interest, and for their symptoms not to be attributable to another cause. (MDCalc, 2025)

When individuals present for care, a physician determines whether they meet diagnostic criteria for depression. If the patient meets criteria, the physician determines whether the patient's depression should be classified as an episode (ICD F32.x) or disorder (ICD F33.x). An episode is defined as an individual's first onset of depression, while a disorder requires multiple prior depressive episodes with intervening period of remission. After making this determination, the physician further classifies the depressive episode or disorder as mild, moderate, severe, or unspecified. The final diagnosis is recorded in the patient's medical records as either a primary or secondary diagnosis—primary if the physician determines that depression is the main reason for the patient's visit, and secondary otherwise.

Depression is most commonly managed by primary care physicians or mental health

specialists. Treatment options differ by provider type. Primary care physicians prescribe pharmacologic treatments but are unable to themselves provide psychotherapy, while psychologists and social workers offer therapy but cannot prescribe pharmacologic treatments. Psychiatrists are able to provide both types of treatment. According to a large body of randomized trials studying the impacts of mental health treatments, both antidepressants and psychotherapy reduce depressive symptoms (Cuijpers et al., 2013). The effects of these treatments are considered to persist for at least six months; evidence on longer-term effects is more uncertain. While mental health treatments have been shown to be effective on average, there is growing evidence that effects vary by physician, and that specialists may achieve better mental health outcomes than non-specialists (Costantini, 2024; Cuddy and Currie, 2024; Currie and Zhang, 2024; Currie and MacLeod, 2020; Wang and Patten, 2007)

2.3 Natural History of Depression

Unlike most physical health conditions, depression and other mental health disorders tend to affect individuals at younger ages. In the United States, the prevalence of depression is highest among individuals who are age 18-24 (21.5%) and lowest in those over 65 (14.2%) (Centers for Disease Control and Prevention , CDC). Other countries generally exhibit similar time trends, with older individuals suffering from depression at lower rates than young- and middle-aged individuals (Weinberger et al., 2018). This high prevalence among young, working-age individuals makes it plausible that depression could affect economic activity. The median age of depression onset is 30; among individuals who eventually receive a diagnosis of depression, 37% are diagnosed by age 25 (Solmi et al., 2022).

Depression is often experienced as *episodes*, or a series of recurrent periods of low mood. Episodes are often preceded by specific stressors. The most common stressors include conflict with family and in other close relationships, occupational or academic stress, changes in family and close relationships, and psychopathology stemming from unclear causes (Meisenzahl et al., 2024). Roughly 38% of individuals report high qualitative or quantitative demands at work as a preceding stressor, raising the possibility for reverse causality (Sullivan and von Wachter, 2009).

The median duration of a depressive episode pooling across individuals who do and do not receive treatment ranges from three to six months. Approximately 20% of cases persist for over a year. (Koenen et al., 2017). Recurrence is common: conditional on one prior depressive episode, the probability of an additional episode is 50%; conditional on three or more prior episodes, the probability of an additional episode is 90% (Hardeveld et al.,

2010). The persistence of depressive episodes and the high likelihood of recurrence raise the possibility that depression could have long-term impacts on labor market outcomes.

3 Data

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

The PILA dataset, sourced from Colombia's Ministry of Health and Social Protection, contains information on monthly contributions to health care, pension funds, and workers' compensation for formally employed workers, including both wage-earners and formal self-employed individuals. This dataset spans from July 2008 to December 2019 and provides information on occupational choices, payroll, earnings, and days worked for over 10.5 million individuals and more than 300,000 firms.

The RIPS dataset, also provided by the Ministry of Health and Social Protection, records health care utilization for all Colombians. It aggregates data from outpatient visits, emergency department visits, and hospitalizations. The dataset spans from 2009 to 2019 and provides information on patient demographics, municipality of residence, and ICD-10 diagnosis codes.

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 major depressive episode and at reliably measuring the effect of depression on their earnings. We start by using PILA to identify the population of wage earners in 2009, where wage earners are defined as individuals who worked 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, because we do not observe earnings in the informal labor market, this restriction helps ensure that we are capturing individuals who are most likely to remain within the formal labor market, allowing us to observe their earnings.

Having identified the population of wage earners in 2009, we restrict to individuals who were between the ages of 18 and 34 years old. This restriction is made to increase the likelihood that we identify individuals' first major depressive 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 of their first observed depressive episodes as their first overall depressive episodes. For example, if we were to include individuals who were 45 in 2009, we would observe their major depressive episodes between age 45 and 56 given the 11-year window for which we observe RIPS. The median age of onset of depression is estimated to be 30 (Solmi et al., 2022), so the majority of 45-year-olds whom we identify with depression would have had their first episode before we could observe them in our data.

We then merge this dataset of wage earners between the ages of 18 and 34 to their medical records in RIPS, restricting to outpatient or emergency department visits where individuals presented for a depressive episode between the age of 18 and 34. This merge identifies individuals who were ever diagnosed with depression during our observation period. If individuals were diagnosed with depression during multiple encounters, we keep all such encounters. We identify depressive episodes by selecting encounters where physicians assigned an ICD-10 code beginning with F32 (depressive episode) or F33 (depressive disorder). We thus implicitly rely on physicians to accurately diagnose patients and code visits.

Next, we make two further restrictions aimed at increasing the probability that we are identifying shocks to mental health. First, we restrict to encounters with a primary, rather than secondary, diagnosis code for depression. The primary diagnosis code indicates the main reason for the patient's visit. Encounters with a primary diagnosis code of depression are therefore presumably more likely to be related to depression rather than another simultaneous medical problem. Second, following Dobkin et al. (2018), 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 visits for follow-up. We dropped cases where individuals had prior encounters for mental health problems other than depression 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 addresses the concern that depression is 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 a major 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 episode.

3.2 Sample Characteristics

Table 1 shows sample characteristics after each of our sample restrictions, starting with the population of wage earners in 2009 and ending with our analysis sample. The average age of individuals in our analysis sample is 25.23. Sixty-three percent are female, reflecting the higher incidence of depression among females compared to males. Ninety-two percent are from large municipalities, reflecting the high concentration of wage earners in urban areas. The average daily wage is 19.61, substantially higher than the daily minimum wage of 10.80, reflecting our focus on individuals who are highly attached to the labor market. Finally, individuals average 0.33 outpatient visits per year, reflecting the relatively high levels of attachment to the health care system among young Colombians.

4 Empirical Analysis

In this section, we describe two complementary empirical approaches we use to study the effect of depressive episodes on earnings.

In the first approach, we use an event study approach which allows us to observe the dynamics of income before and after depressive episodes. Let d_i denote the date of individual i's first observed depressive episode. Let $D_{it}^k = 1\left(t = d_i + k\right)$ be an indicator variable that equals one if the individual was diagnosed with depression k periods ago, where k may be negative. Our event-study model compares the outcome y around event time across individuals using the following specification:

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

where α_i are individual fixed effects, γ_t are calendar time fixed effects, $\bar{C} > 0$ and $\underline{C} < 0$ are constants, and u_{it} is the error term. The β^k coefficients are our main parameters of interest and provide estimates of earnings around the time of depressive episodes accounting for individual- and time-specific effects. We normalize the first lead to be zero to interpret the effects relative to event time -1. Standard errors are clustered at the individual level to account for within-person correlation over time.

To identify causal effects using this approach, we rely upon several key assumptions. First, we assume that, absent a depressive episode, the income trajectories of individuals would have evolved along parallel paths. We evaluate this parallel trends assumption by testing for the absence of pre-trends in earnings; the absence of such trends suggests that any post-diagnosis changes in income are likely attributable to the onset of depression rather than to other confounding factors. We also assume that depressive episodes are not driven by anticipatory behavior. Recognizing that standard two-way fixed effects estimators may yield biased estimates in the presence of heterogeneous treatment effects, we rely on the Callaway and Sant'Anna (2021) framework. This approach constructs appropriate comparison groups for each treatment cohort and aggregates the cohort-specific treatment effects, thereby addressing potential biases arising from staggered timing of treatment.

In our second approach, we assess the effect of specialty mental health care on labor market outcomes by propensity score matching individuals with and without access to such care. Following Abadie and Imbens (2016), let T_i be a binary indicator for whether an individual saw a mental health specialist, and let X_i be a vector of covariates of dimension k. We begin by computing the propensity score $p(X) = P(T = 1|X)$ using logistic regression. We then use nearest-neighbor matching to compare individuals with a depression diagnosis who received mental health care to a matched group of untreated individuals. Denoting $Y(1)$ and $Y(0)$ as potential outcomes with and without treatment, our PSM approach will identify the causal effect of treatment if $Y(1), Y(0) \perp T|X$; that is, if conditional on X, treatment is orthogonal to the potential outcomes.

We test this assumption in two ways. First, we demonstrate that our matched samples are balanced on a rich set of observable characteristics, including ones that were "held out" of X. To further address concerns regarding unobserved confounders, we run the following regression:

$$Y_{it} = \alpha + \sum_{k=-24}^{-1} \gamma_k Dep_{it} + \sum_{k=0}^{24} \gamma_k Dep_{it} + \sum_{k=-24}^{-1} \phi_k T_i + \sum_{t=0}^{24} \phi_k T_i + \lambda_i + \delta_{td} + \varepsilon_{tme}$$

where λ_i are individual fixed effects and δ_{td} are month-by-treatment fixed effects. Here, γ_k represent the earnings trajectory of individuals who did not receive treatment and ϕ_k capture the dynamic effects of treatment. Because the ϕ_k are taking a first difference between two sets of difference-in-difference estimates, they resemble a triple differences estimator (Gruber, 1994). We use this setup to evaluate for parallel trends between individuals who did and did not receive treatment and, following the logic of a triple-differences design (Olden and

Møen, 2022), view this as a way to test that income trends between treated and untreated individuals would have evolved along parallel paths in the absence of the treatment.

5 The Effect of Depression on Earnings

5.1 Results

We begin by examining the effect of depression on earnings. Figure 1 shows our main result. In the two years leading up to first depressive episodes, earnings are nearly flat, indicating that individuals treated with depression earlier versus later in our observation window are on similar earnings trends prior to their depressive episodes. In the months immediately following a depressive episode, earnings begin to decline. They reach a plateau roughly nine months later and remain around this level for up to two years, at which time they are 8.3% lower than they were the month prior to the start of the depressive episode.

The observed decline in earnings could arise from any combination of extensive and intensive margin effects. To evaluate which margin drives our earnings effect, we repeat this exercise with various labor market outcomes on the left-hand side of our event study regression. Results are shown in Figure 2. In Panel A, we decompose the decline in earnings into three mutually exclusive margins: extensive margin, days worked, and daily wage. The extensive margin accounts for the majority of our observed effect of depression on earnings, with daily wage accounting for a smaller portion and days worked accounting for the smallest portion. These results are quantified in Table A3, which shows that these margins account for 61.7%, 6.6% and 31.7% of the effect, respectively.

Our extensive margin effect can be further decomposed into employment-to-unemployment (EU) and unemployment-to-employment (UE) transitions. The effect could be explained by an increase EU transitions, which could occur if depression makes individuals more likely to lose their jobs or seek employment elsewhere. It could also be explained by a decrease in UE transitions, which could occur if depression makes unemployed individuals less motivated to search for jobs. Results of our decomposition are shown in Figure 2, Panel B. Consistent with the former hypothesis, depression induces a large and immediate increase in the probability of EU transitions. In the month after a depressive episode, there is a 51.4% relative increase in the probability of job loss, an effect which attenuates over time but remains positive for up to two years after the diagnosis. Comparatively, the effect on UE transitions increases more gradually such that, two years after the onset of depression, the effect on UE transitions roughly offsets the effect on EU transitions. These results are quantified in Table A4.

Taken together, these results are consistent with depression causing a long-term reduction in earnings. In the months immediately following the start of the depressive episode, this effect appears to be mediated by job loss. In the longer run, it is mediated by an increase in job churn, and—assuming wages are correlated with job quality—by depressed individuals selecting into worse jobs.

5.2 Robustness

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

Inaccurate measurement of depressive episodes. We identify depressive episodes when individuals visit a clinic or emergency department and are diagnosed by a physician as having depression. A large medical literature demonstrates that many individuals with depression do not seek treatment. Individuals with depression who do not seek care would confound our estimates if they were included in the control group of our regressions; by falsely being coded as not depressed, they would bias our effect downward assuming a negative effect of depression on earnings. However, in our setup, such individuals do not pose an identification concern because they are not in our control group; our effect is identified only using individuals who were ever diagnosed with depression. Though we are unable to estimate effects for this population, they do not pose an identification concern.

Relatedly, individuals with depression may present for care long after the onset of symptoms. If this were the case, and if depression were severe enough to impact earnings, then we would expect to observe a downward pre-trend in our event study results, which we do not. We interpret this as evidence that the cases of depression we study have relatively sudden onset, or that young adults in Colombia have a relatively low threshold for presenting to the clinic or emergency department for depressive symptoms. This latter hypothesis is consistent with the near-universal health insurance in Colombia and the high baseline levels of attachment to outpatient care we observe among our study population. Consistent with the hypothesis that higher levels of attachment lower the threshold for individuals to seek care for depression, Figure A2 shows that individuals who were more attached to the health care system in the pre-period have smaller effects on earnings, consistent with them having milder cases of depression.

The sharp on-impact effect of depression on income we observe suggests that we are indeed identifying shocks to depression. To further test for we are identifying a shock to mental health, we relax the sample restriction which keeps only those encounters with no

prior encounters for mental health issues in the three years prior to the index visit. The result is shown in Figure A1. Relative to our findings in Figure 1, we observe a decline in earnings prior to the onset of depression, as well as a smaller on-impact effect. These findings help reassure us that restricting to individuals with no recent mental health history is helping to isolate new shocks to mental health.

Presence of heterogeneous treatment effects. Several recent studies in the econometrics literature have shown that two-way fixed effects models can be confounded by the presence of heterogeneous treatment effects, leading to the generation of negative weights, which may pose significant problems for the estimation of causal effects (see, e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 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 individuals with pre-existing health conditions may experience more severe impacts of depression due to interaction effects between depression and their underlying health. These sorts of concerns are heightened in cases where treatment effects are estimated using only "ever-treated" individuals (Goodman-Bacon, 2021). For this reason, all of the main specifications we present use the Callaway and Sant'Anna estimator. Results for our estimate of depression on earnings using a simple two-way fixed effects approach are shown in Table A2.

Substitution into the informal labor market. PILA accounts for earnings only among individuals who are employed in the formal labor market, which accounts for roughly 80% of Colombia's GDP. A limitation of our data is that we do not observe earnings for individuals who are informally employed. We are therefore unable to distinguish between individuals with positive earnings in the informal labor market and individuals with zero earnings in the formal labor market; both groups are considered to have zero earnings. This limitation may lead us to overestimate the impacts of depression on earnings.

To test whether individuals may be substituting to the informal labor market, we partition individuals in our sample into those with below- and above-median levels of attachment to the labor market at baseline, as measured by the number of days during which they have positive earnings in 2009. We then run our main event study estimates separately for these two groups, with the hypothesis that individuals with lower attachment to the labor market would have lower earnings after the onset of depression, in part because they would be more likely to substitute into the informal labor market. Results are shown in Figure A3. There is no statistically significant difference between the effect sizes for the two groups, which reassures us that transitions to informality, though possibly occurring, are less likely to be

driving our results.

Reverse Causality and Other Supply-Side Explanations. In Figure 1, we observe a fall in earnings which begins immediately after an encounter for depression and which is primarily explained by job loss. A concern in our setup and in the literature more broadly is that job loss may have induced depression. If this were the case, we would have expected to observe a fall in earnings or an increase in EU transitions prior to event time zero. Neither of these are observed in Table A3: monthly earnings and job loss are statistically indistinguishable from zero prior to $k = 0$. Job loss is also statistically indistinguishable from zero *at* $k = 0$, which further reassures against the possibility of reverse causality.

A remaining possibility is that firms announced their intention to fire individuals, causing individuals to become depressed before they are fired. In this case, we would observe the onset of depression prior to job loss. Though this is consistent with our current results, we view it as unlikely to be driving our effects.

Health shocks other than depression. Depression commonly co-occurs with other medical conditions - especially chronic conditions such as cancer, heart disease, diabetes, and chronic pain. It is therefore possible that our observed effect of depression is driven by coincident medical conditions. This is less of a concern in our setting since we are studying individuals age 18-34, who are unlikely to have chronic conditions at baseline. Pregnancy is a plausible concern given the prevalence of post-partum depression, but as discussed in Section 3.1, we remove individuals with any records of pregnancy or parental leave in the three years prior to their depressive episode.

Time-varying factors. Our event study regression specification includes individual fixed effects and thereby addresses confounding due to time-invariant characteristics such as stable chronic health conditions. Our specification also includes month fixed effects and thereby accounts for time-specific shocks that are common across all individuals, such as secular trends in earnings.

Time-varying unobserved factors correlated with both depression and earnings remain a concern. The presence of such factors is likely given that depression is often preceded by one or multiple stressors, including interpersonal conflicts and changes in close relationships (Meisenzahl et al., 2024). There are (at least) two possible stances one could take regarding this concern. One stance is to argue that disentangling depression from its preceding stressors would make our estimand *more* difficult to interpret, since depression seldom comes without stressors in the real world. In this case, time-varying unobserved factors which come "bundled" with depression would not be seen as a threat to identification.

Another stance is to attempt to isolate the impact of changes of depressive symptomatology holding all else, including any stressors, fixed. Our current quasiexperimental design is unlikely to isolate this impact; this would require an instrumental variable which affects earnings only through its impact on depressive symptoms. In the next section, we study the impact of access to treatment on earnings and argue that this may more closely approximate the experimental ideal of altering depressive symptoms while holding all else constant.

6 The Effect of Treatment on Earnings

6.1 Results

In the previous section, we identified the effect of depression on earnings by examining earnings trajectories before and after the onset of depressive episodes, which we argue constitute a negative shock to mental health. An alternative strategy is to study the impact of a positive shock to mental health on earnings. In this section, we attempt to do this by studying the impact of depression treatment on earnings.

To identify impacts, we compare outcomes for individuals in our primary analysis sample who were treated by a specialist within 90 days of their diagnosis. We define a specialist as a psychiatrist, psychologist, or social worker. Our analysis is guided by an extensive medical literature demonstrating that specialty care for depression, including antidepressants and therapy, reduce depressive symptoms, with effects persisting for at least six months (Gartlehner et al., 2023).

Motivated by our event study approach in Section 5, we begin with a naive comparison of income trajectories between individuals who did and did not access specialty care. Results are shown in Figure A5. Panel A shows that prior to the onset of depressive episodes, both groups exhibit largely flat earnings trajectories. Upon impact, both groups experience a reduction in earnings, but individuals who do not access treatment experience a larger decline than individuals who do access treatment. In Panel B, we examine the difference between these difference-in-differences coefficients. The earnings of individuals with and without access to treatment exhibit parallel trends in the months leading up to depressive episodes. Following depressive episodes, the difference in earnings between individuals who do and do not access treatment gradually grows until two years after the onset of depression, when individuals who accessed treatment earn roughly 47.3 USD more than individuals who do not.

This difference could be interpreted as a causal effect if treatment were randomly assigned. However, it is likely that individuals who receive treatment systematically differ from indi-

viduals who do not. If individuals who receive treatment are negatively selected—because, for example, individuals with more severe depression have a higher likelihood of receiving treatment—then the ordinary least squares comparison in Figure A5 is likely to underestimate the effect of treatment on earnings. On the other hand, if individuals with more severe depression are less likely to follow up or adhere to treatment, then results in Figure A5 are likely to overestimate the treatment effect.

Table A5 compares the characteristics of individuals who do and do not access specialty care within 90 days of their diagnosis. Individuals who access specialty care are slightly younger, more likely to be female, have lower wages in the months prior to the onset of depression, have more interactions with the health care system in the months prior to the onset of depression, and are more likely to have mild depressive episodes. Based on these results, it is not immediately apparent whether individuals who receive treatment are positively or negatively selected—though they have lower wages, they appear to have milder cases of depression on average. Nonetheless, the imbalances in this table pose issues for identification since the covariates that are unbalanced with respect to treatment status are likely correlated with earnings.

To correct for these imbalances, we propensity score match individuals who do and do not receive treatment using a large set of demographic, labor market and health characteristics shown in Table A5. Figure A6 shows the resulting propensity scores. Prior to propensity score matching, individuals who receive treatment have systematically higher propensity scores, as shown in Panel (a). After propensity score matching, the distributions of propensity scores for treated and untreated individuals are almost entirely overlapping. Table A6 compares the characteristics of individuals with and without treatment after propensity score matching. The results confirm that our matching procedure results in balance between treated and untreated individuals with respect to observables.

Having propensity score matched individuals with and without treatment, we then repeat the exercise from Figure A5, running event study regressions separately for treated and treated individuals, now in the propensity score matched sample. Results are shown in Figure 3. Prior to the onset of depression, treated and untreated individuals in the matched samples exhibit parallel trends in income. Following the onset of depression, earnings fall faster for individuals without treatment than individuals with treatment such that two years after the shock, earnings are 96 USD higher for individuals who were treated. Interestingly, the point estimate for the effect of treatment is higher in the matched sample in the unmatched sample, consistent with treated individuals being negatively selected, though the difference between

the two point estimates is not statistically significant.

We conclude this subsection by studying the extent to which the effect of treatment on earnings is mediated by the extensive versus intensive margin. Results are shown in Figure A7. Panel (a) shows that prior to the onset of depression, there is no statistically significant difference in the probability of work between individuals who do and do not access treatment. Following the onset of depression, individuals who access treatment become gradually more likely to work relative to those who do not. Two years after the onset of depression, treated individuals are 8% more likely to work than untreated individuals.

Panels (b) and (c) show similar results for days worked and daily wage. For both outcomes, there is no statistically significant difference between treated and untreated individuals in the months leading up to the onset of depression. In the months immediately following the onset of depression, treated individuals work more days than untreated individuals, though the effect is not statistically significant. There is no clear statistically significant long-term effect on days worked. There are no differences between treated and untreated individuals in daily wage in the months following depressive episodes.

In summary, the results in this section are consistent with specialty treatment inducing an increase in earnings that is primarily mediated by the extensive margin. Viewing this result in context of our results from Section 5 on the effects of depression shows that treatment compensates for approximately 74% of the effect of depression on earnings.

6.2 Robustness

The main threat to a causal interpretation of our results in Section 6.1 is the presence of unobserved heterogeneity between treated and untreated individuals. While we attempt to address this with propensity score matching on a rich set of observables, it is possible that our resulting estimate is confounded by residual unobserved heterogeneity. For example, individuals who access treatment may be more resilient than individuals who do not. If resilience or similar threats are time-variant, orthogonal to the set of variables used to propensity score match, and correlated with both treatment status and earnings, then they pose a concern for identification.

To address this concern, we use an instrumental variables strategy which leverages geographic variation in the probability of treatment. The motivation for this strategy is that an individual's likelihood of receiving a referral for specialty treatment likely depends on the availability of specialists at the hospital in which the patient seeks care. If such availability varies across hospital, then we can use such variation as the basis for an instrumental variable.

Following this logic, we construct the leave-out mean, which is the probability that individuals other than the index patient who visited the same hospital received specialty care. A histogram of the leave-out mean is shown in Figure A8. There are two main observations. First, the probability of treatment varies widely, ranging from 0% to 100%. This variation lays the foundation for our empirical approach. Second, there is a large mass at zero corresponding to the 27.5% of individuals who visit hospitals who do not refer patients to specialty care. The presence of this large mass leads us to construct a binary version of instrument, Z_i, an indicator for whether an individual's leave-out mean is above the 25th percentile of Z_i.

In order to establish validity of Z_i, we test for balance with the following model:

$$Z_i = \beta X_i + \lambda_t + EPS_i + \epsilon_i$$

where X_i is a vector of patient characteristics and β reflects the association between those characteristics and the instrument conditional on year and insurance-type fixed effects. Figure A9 provides a visual summary of the estimates from this regression. The instrument is uncorrelated with most observables conditional on year and insurance status fixed effects, though living in urban areas and having higher incomes remain predictive of the instrument.

We next check for relevance by testing whether the instrument is predictive of treatment. To do so, we first residualize T_i and Z_i on year and insurance status fixed effects. We then visualize the association between these residuals in a binned scatter plot. The result is shown in Figure A10, which shows that the leave-out mean is highly predictive of treatment.

Next, we examine the reduced form effect of visiting a high treatment intensity hospital. The result is shown in Figure A11. To construct this plot, we partition individuals into those with $Z_i = 0$ and $Z_i = 1$. We then run event studies separately for these groups and plot the difference between the resulting coefficients. The results are qualitatively similar to our propensity score matching results, further validating our results on the effects of treatment.

6.3 Possible Mechanisms

We conclude this section with a brief discussion of the mechanisms by which specialty treatment may lead to improved earnings.

In our design, treated individuals are those who receive care from a mental health specialist within 90 days of diagnosis, where a specialist is defined as a psychologist, psychiatrist, or social worker. Control individuals, on the other hand, receive care from non-specialists, such as primary care physicians. Our treatment effect is thus likely mediated by treatments where

specialists have a comparative advantage relative to generalists.

Broadly, there are two margins in which specialists may have a comparative advantage in the treatment of depression. First, psychologists, psychiatrists, and social workers are all able to provide therapy, which is outside of the scope of practice for primary care physicians and other non-specialists. In our sample, 80% of treated individuals see psychologist, who are able to provide therapy but is unable to prescribe medications. We therefore view therapy as a likely mediator of our impacts.

A second margin by which specialists may have a comparative advantage is through antidepressant prescription. Though both generalists and psychiatrists are able to prescribe antidepressants, generalists may have less comfort prescribing these medications. They may also be less skilled at matching patients to the appropriate medications (Currie and MacLeod, 2020). Though we are unable to directly evaluate these hypotheses, they may also be mechanisms by which access to treatment improves earnings.

7 Conclusions

Mental health conditions are a major cause of disability worldwide. There is longstanding concern that these conditions have negative economic consequences, but reliably estimating their causal impacts on labor market outcomes remains challenging due to unobserved heterogeneity and reverse causality. We address these challenges by linking administrative records from Colombia that allow us to simultaneously measure depression and earnings. Using two supplementary empirical strategies, we find evidence that depression reduces earnings. Our results strengthen recent calls to further invest in prevention, diagnosis, and treatment of mental health disorders, particularly in settings similar to Colombia. Directions for future work include investigating the ways in which depression alters job-seeking behavior, assessing the extent to which depression and other mental health disorders may create poverty traps, and estimating the impacts of population-wide screening or treatment programs on macroeconomic indicators.

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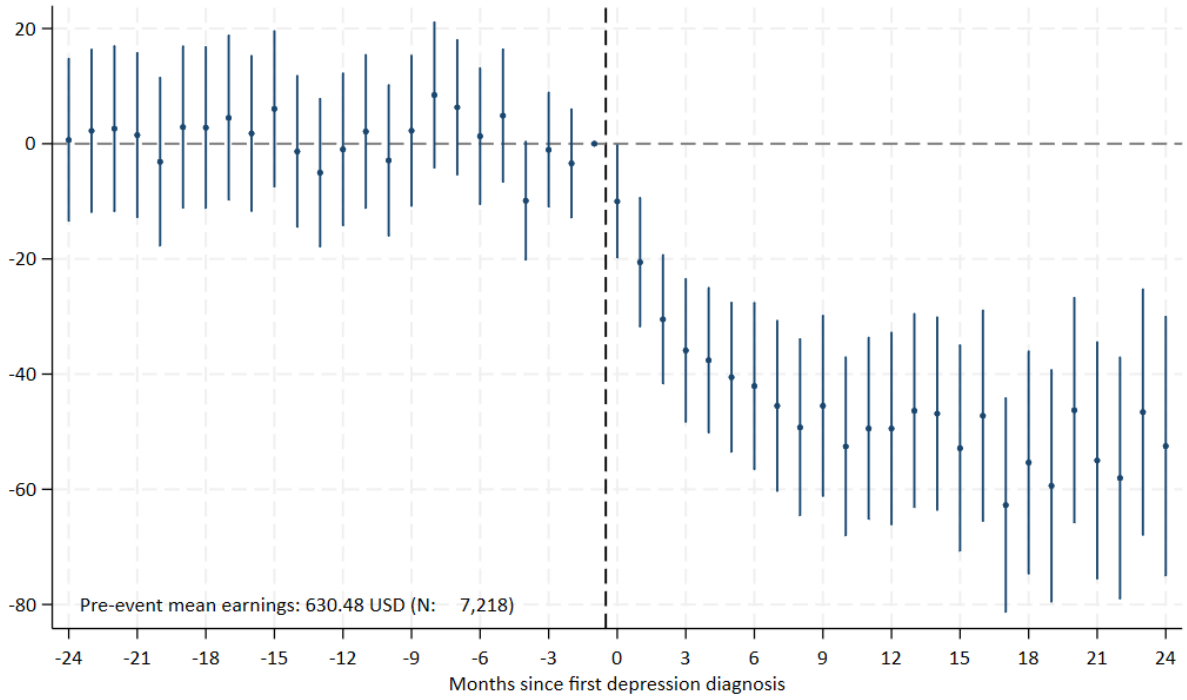
Tables and Figures

Table 1: Sample Characteristics

	Wage Earners in 2009	18-34 y.o. in 2009	18-34 y.o. at depression diagnosis	(&) Depression as primary diagnosis	(&) No Mental health history (3 years) (&) 2012-2017	(&) No history of pregnancy or parental leave (3 years)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Individual characteristics						
Age	37.04	27.75	26.29	26.27	25.24	25.23
Female	0.42	0.45	0.72	0.71	0.70	0.63
Large municipality	0.89	0.90	0.93	0.93	0.93	0.92
Intermediate size municipality	0.08	0.07	0.05	0.05	0.05	0.05
Small municipality	0.03	0.03	0.02	0.02	0.02	0.02
Panel B. Job market						
Total Formal Earnings	9,292.97	7,644.22	6,839.57	7,012.38	6,772.44	6,742.05
Wage Earner	1.00	1.00	1.00	1.00	1.00	1.00
Daily wage	26.60	22.08	19.85	20.33	19.69	19.61
Worked days	345.87	343.03	342.25	342.60	341.78	341.51
Vacation days	0.65	0.56	0.59	0.61	0.57	0.55
Unpaid leave days	0.09	0.09	0.11	0.11	0.11	0.11
Non-labor sick leave days	0.22	0.23	0.51	0.51	0.46	0.42
Labor sick leave days	0.08	0.07	0.10	0.09	0.08	0.08
Parental leave days	0.07	0.13	0.21	0.21	0.21	0.20
Employment-to-unemployment	0.01	0.01	0.01	0.01	0.01	0.01
Unemployment-to-employment	0.02	0.02	0.02	0.02	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	0.21	0.45	0.45	0.38	0.33
Medical procedures	0.16	0.16	0.00	0.00	0.00	0.00
ED visits	0.01	0.01	0.05	0.05	0.06	0.07
Individuals	4,270,403	1,929,948	29,280	23,388	13,711	7,189

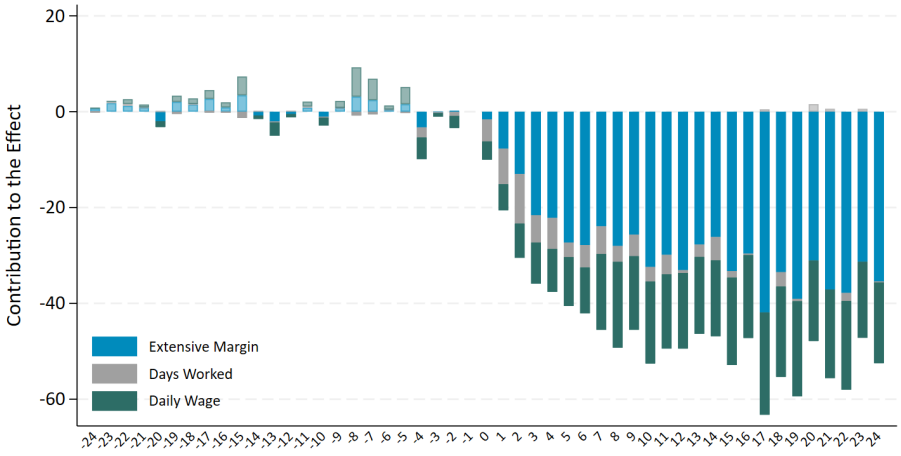
Note: This table presents descriptive statistics of 2009 wage earners, applying a series of progressive filters. Column (1) includes all individuals who were wage earners for 270 or more days in 2009. Column (2) additionally restricts to those who were between 18 and 34 years old that in 2009. Column (3) departs from the population in Column (1) and further restrcits to those who were diagnosed with depression between 18 and 34 years of age. Column (4) adds the condition that depression is the primary diagnosis, and column (5) excludes individuals with a mental health history in the three years before the depression diagnosis and whose diagnosis took place 2021 and 2017. Finally, column (6) also excludes those who had any pregnancy contact with the health system or any parental leave up to 3 years before the depression diagnosis. Regarding the list of characteristics shown, the age variable is defined as the age of the individual in 2009, female equals 1 if the individual is a woman. For municipality's size we use the 2018 National Population Census. Small municipalities are defined as those with a population lower or equal to 20,000 inhabitants, intermediate is equal to 1 if they have a population between 20,001 and 100,000 inhabitants, and large municipalities are equal to 1 if they have a population greater than 100,000 inhabitants. The Total Formal Earnings variable represents the total formal earnings as per social security records, expressed in 2008 dollars. Wage Earner is equal to 1 for persons who have records in the (IBC) and who do not have any missing value on working days. The Daily wage refers to the average daily income that the worker receives according to his contribution base. Worked days is the total number of days paid by the employee. Vacation days are the number of days that the worker has taken vacation. Unpaid leave days indicates the days on which the worker was temporarily suspended. Non-labor sick leave days reflect the days the worker was incapacitated by a common (non-work related) illness. Labor sick leave days are days of incapacity due to an accident at work or occupational disease. Parental leave days, reflect the days of maternity or paternity leave. Employment-to-unemployment is a dummy that indicates whether a person has ceased employment in the current month after having been employed in the previous month. Unemployment-to-employment is a dummy that identifies the times when a person has gone from being unemployed (no employment record) in a previous month to being employed in the current month. Job-to-job is a dummy that identifies a job change when a person moves from being employed in one job in one month to having a different job in the following month, without ever having ceased employment. Outpatient visits count the total number of queries made by a person in a given month. Medical procedures is the total number of medical procedures performed on a person in a specific month. Finally, the variable ED visits is the total number of emergency room visits a person made in a given month.
Source: Authors' calculation using data from PILA and RIPS.

Figure 1: The Effect of Depression on Earnings

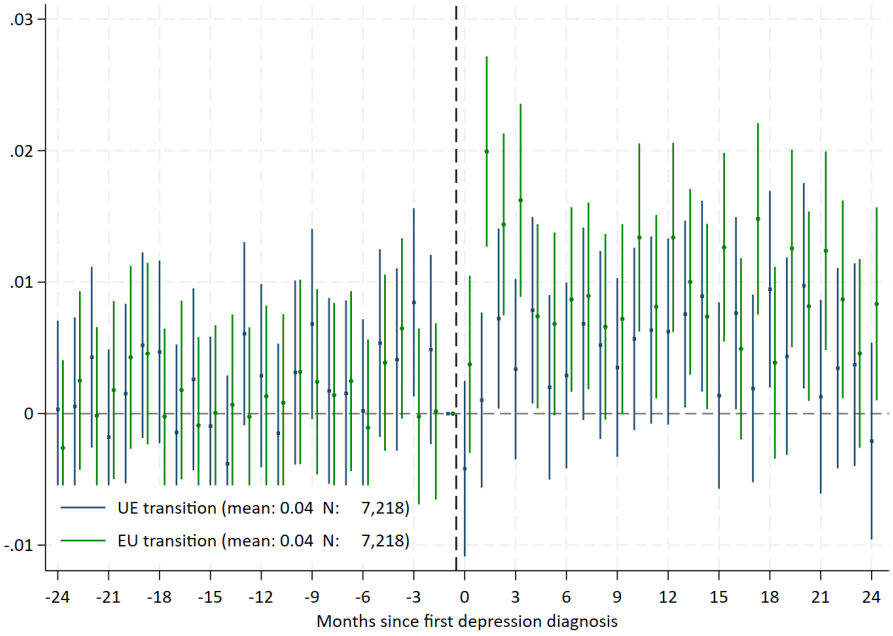


Note: This figure shows the effect of depression on earnings using the Callaway and Sant'Anna (2021) estimator. The estimated coefficients are presented with 95% confidence intervals. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars.
Source: Authors' calculation using data from the PILA and RIPs.

Figure 2: The Effect of Depression on Labor Market Outcomes



(a) Intensive Versus Extensive Margin

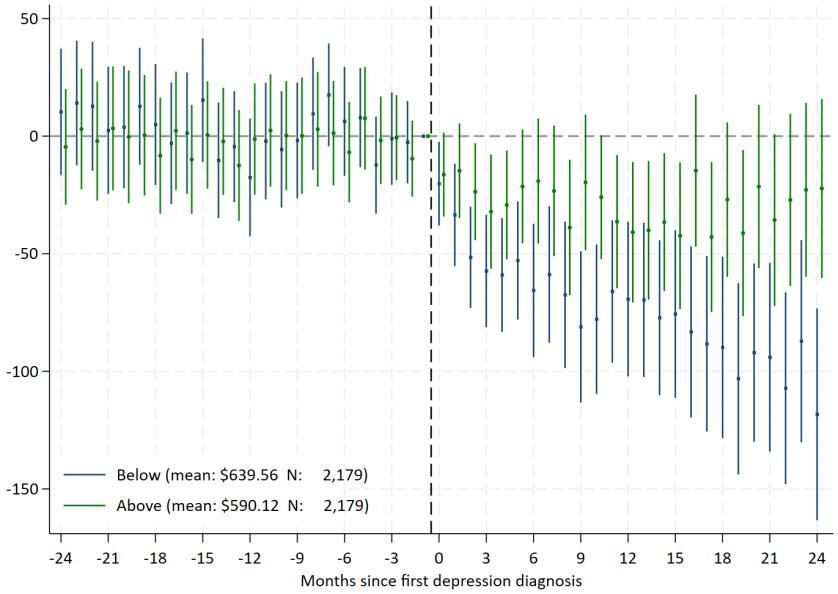


(b) EU and UE transitions

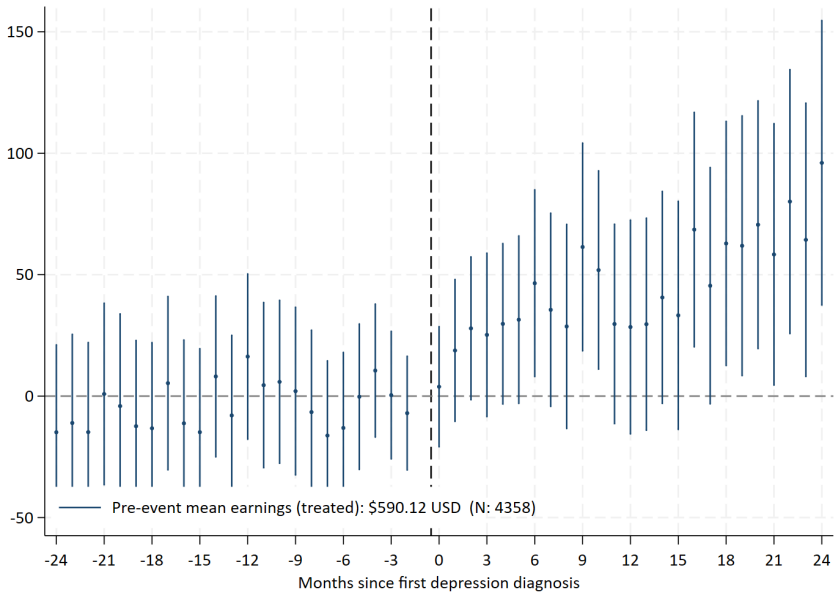
Note: The panel (a) shows the breakdown of the total impact into three components: extensive margin, working days, and daily wage. The stacked bars reflect the contribution of each component. The broad margin represents changes in the number of formal workers, days worked capture variations in duration of employment, and daily wages measure changes in per-day pay. The panel (b) shows the effect of depression on earnings using the Callaway and Sant'Anna (2021) estimator. The estimated coefficients are presented with 95% confidence intervals. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in the likelihood of losing employment.
Source: Authors' calculation using data from the PILA and RIPs.

Figure 3: The Effect of Specialty Care on Earnings

(a) Income Trajectories for Treated and Untreated Individuals



(b) Difference in Earnings Between Treated and Untreated Individuals



Note: The panel (a) and (b) shows the event-study coefficients together with the corresponding 95 percent confidence intervals derived from the Callaway and Sant'Anna (2021) estimator with the matched sample. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars. Panel (a) shows the trajectories of income before and after the first diagnosis of depression by highlighting the different trajectories of individuals who accessed treatment vs. those who did not, illustrating the trajectories in earnings from difference averages. While panel (b) shows the difference in averages of coefficients between treated and controls.
Source: Authors' calculation using data from the PILA and RIPs.

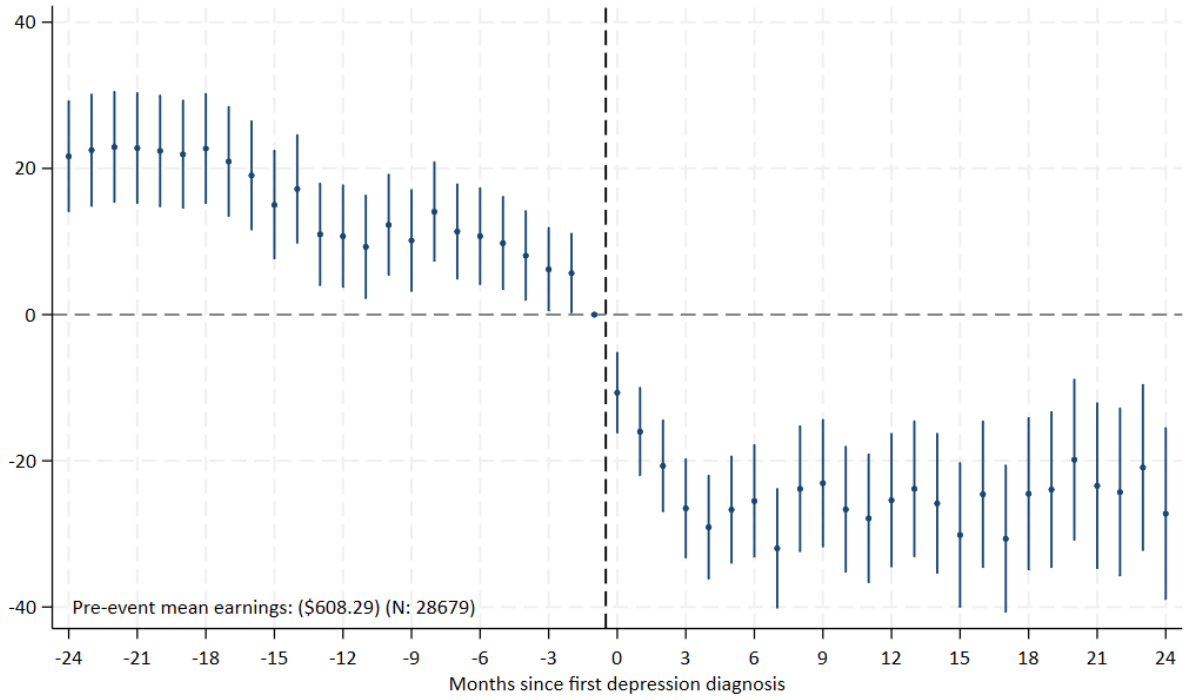
Online Appendix

A Appendix

Table A.1: Sample restrictions

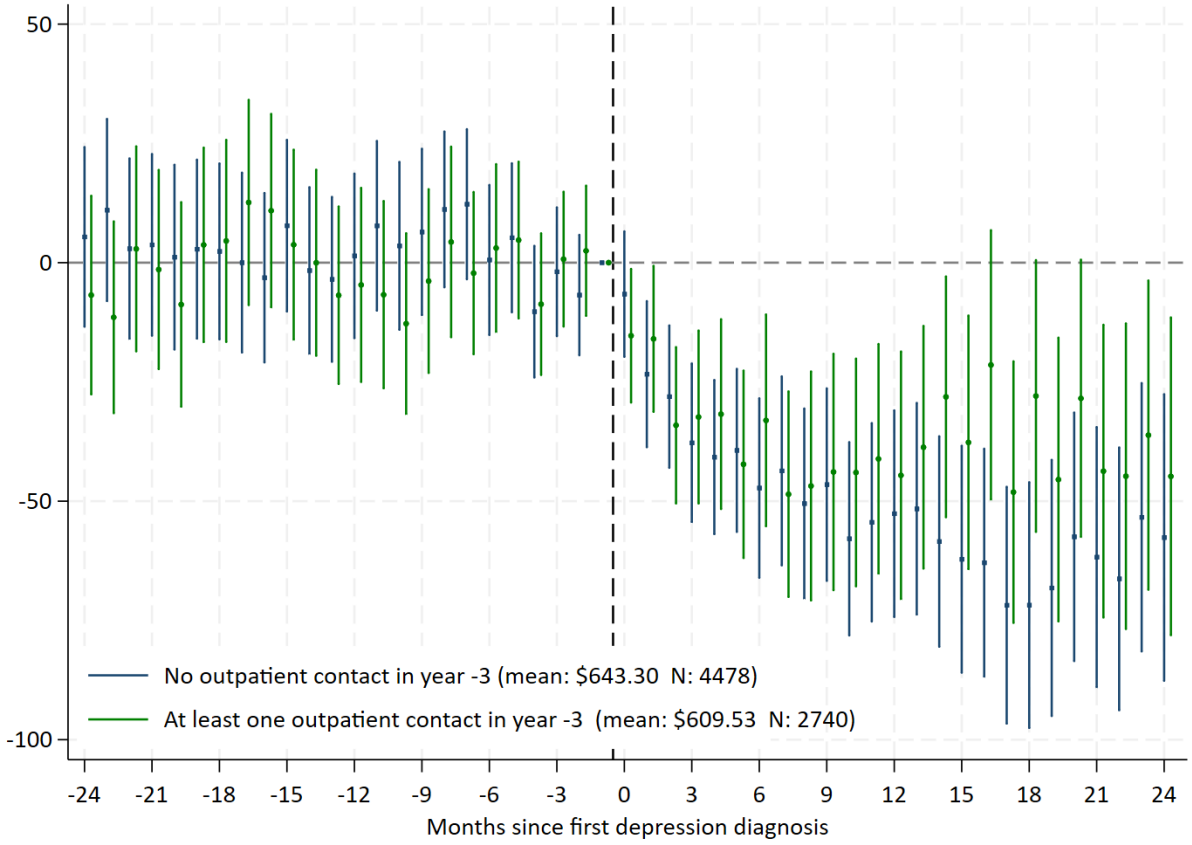
Description	**Number**
Total unique individuals in 2009 with over 270 days as wage earners	4,270,403
Individuals aged 18-34 as of 2009	1,929,948
Depression cases Among Individuals Aged 18-34 at the time of the diagnosis	55,233
Individuals with depression between 18 and 34 years	29,280
Depression cases as primary diagnosis	45,225
Individuals with depression as primary diagnosis	23,388
Depression cases without any diagnosis of mental health 3 years before and cases between 2012-2017	13,850
Individuals with depression without any mental health diagnosis 3 years before and cases between 2012-2017	13,711
Depression cases without pregnancy or maternity leave 3 years before	7,219
Individuals with depression without pregnancy or maternity leave 3 years before	7,189

Figure A.1: The Effect of Depression Not Restricting to Individuals With New Diagnosis of Depression



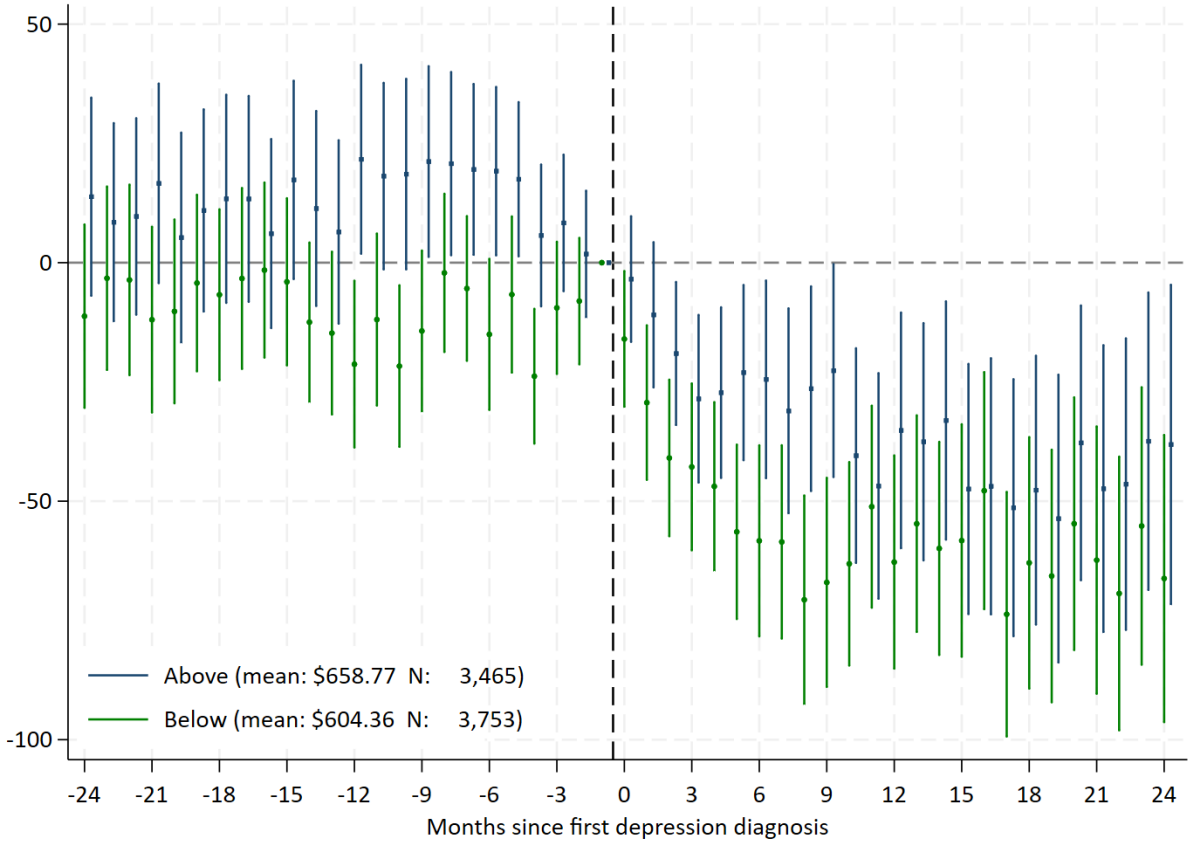
Note: This figure shows the effect of depression on earnings using the Callaway and Sant'Anna (2021) estimator. The estimated coefficients are presented with 95% confidence intervals. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars.
Source: Authors' calculation using data from the PILA and RIPs.

Figure A.2: The Effect of Depression By Prior Attachment to Outpatient Care



Note: This figure shows the effect of depression on earnings using the Callaway and Sant'Anna (2021) estimator. The estimated coefficients are presented with 95% confidence intervals. The X-axis represents the months before and after depression diagnosis, while the Y-axis shows the variation in monthly income in dollars. The blue line represents those who had no outpatient contact in the year -3 before diagnosis, while the green line represents those who had at least one outpatient contact during the same period.
Source: Authors' calculation using data from the PILA and RIPs.

Figure A.3: The Effect of Depression By Attachment to Labor Market



Note: This figure shows the effect of depression on earnings using the Callaway and Sant'Anna (2021) estimator. The estimated coefficients are presented with 95% confidence intervals. The blue line represents those who worked a number of days above the median in 2009, while the green line represents those who worked below the median in that same year. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars.
Source: Authors' calculation using data from the PILA and RIPs.

Table A.2: The Effect of Depression on Labor Market Outcomes Using Two-Way Fixed Effects Model

Outcome	$k \in [-24,-19]$ (1)	$k \in [-18,-13]$ (2)	$k \in [-12,-7]$ (3)	$k \in [-6,-4]$ (4)	$k \in [-3,-2]$ (5)	$k = 0$ (6)	$k \in [1,3]$ (7)	$k \in [4,6]$ (8)	$k \in [7,12]$ (9)	$k \in [13,18]$ (10)	$k \in [19,24]$ (11)	Mean t-1 (12)	Obs. (13)
Monthly labor earnings (zeros)	-17.18**	-11.73*	-4.91	-4.33	-5.08	-9.26**	-28.23***	-34.57***	-40.36***	-38.07***	-38.74***	656.94	7,219
	(7.97)	(6.87)	(5.56)	(4.86)	(4.12)	(4.71)	(4.66)	(5.53)	(6.42)	(7.86)	(9.15)		
Monthly labor earnings (miss)	24.20***	20.93***	17.38***	8.04*	-1.21	-10.32**	-14.15***	-14.00***	-16.17***	-9.45	-5.55	815.55	5,360
	(7.46)	(6.52)	(5.40)	(4.69)	(4.33)	(5.23)	(4.63)	(5.20)	(5.84)	(7.01)	(8.09)		
Wage earner	-0.0567***	-0.0451***	-0.0327***	-0.0226***	-0.0074***	-0.0041	-0.0280***	-0.0376***	-0.0508***	-0.0603***	-0.0651***	0.8055	7,219
	(0.0057)	(0.0049)	(0.0041)	(0.0035)	(0.0028)	(0.0028)	(0.0033)	(0.0041)	(0.0046)	(0.0053)	(0.0061)		
Worked days (miss)	0.2888**	0.2500**	0.1792	-0.0111	-0.0530	-0.5981***	-0.5996***	-0.3452***	-0.2960***	-0.2099*	0.0293	26.26	5,360
	(0.1219)	(0.1155)	(0.1100)	(0.1089)	(0.1020)	(0.1135)	(0.1068)	(0.1143)	(0.1116)	(0.1200)	(0.1240)		
Daily wage	-0.5984	-0.7548	-0.5236	-0.7805	-0.5508	-0.1302	-0.0048	-1.25*	-1.03	-1.10	-1.52**	33.58	5,156
	(0.6595)	(0.6337)	(0.6527)	(0.6255)	(0.7430)	(0.7305)	(0.6943)	(0.67)	(0.67)	(0.72)	(0.73)		
Job loss	0.0054**	0.0038*	0.0048**	0.0045*	0.0023	0.0027	0.0168***	0.0097***	0.0114***	0.0094***	0.0097***	0.0290	7,219
	(0.0022)	(0.0022)	(0.0022)	(0.0023)	(0.0025)	(0.0029)	(0.0024)	(0.0023)	(0.0021)	(0.0021)	(0.0021)		
Job gain	0.0045**	0.0045**	0.0049**	0.0043*	0.0081***	-0.0039	0.0039*	0.0048**	0.0059***	0.0060***	0.0054**	0.0324	7,219
	(0.0023)	(0.0023)	(0.0023)	(0.0024)	(0.0027)	(0.0029)	(0.0024)	(0.0024)	(0.0022)	(0.0022)	(0.0022)		
Job-to-job	0.0003	0.0001	0.0002	-0.0011	0.0001	-0.0027	0.0022	0.0011	0.0013	0.0005	0.0000	0.0206	7,219
	(0.0018)	(0.0018)	(0.0018)	(0.0019)	(0.0020)	(0.0022)	(0.0019)	(0.0019)	(0.0018)	(0.0019)	(0.0019)		
Job loss (last 6 months)	0.0128**	0.0096	0.0088	0.0095*	0.0059*	-0.0000	0.0221***	0.0491***	0.0433***	0.0411***	0.0372***	0.1981	7,219
	(0.0063)	(0.0061)	(0.0059)	(0.0052)	(0.0030)	(0.0027)	(0.0044)	(0.0061)	(0.0057)	(0.0060)	(0.0061)		
Job gain (last 6 months)	-0.0023	-0.0017	-0.0027	-0.0012	0.0024	-0.0037	-0.0111**	-0.0083	0.0003	0.0037	0.0032	0.2233	7,219
	(0.0064)	(0.0062)	(0.0062)	(0.0053)	(0.0031)	(0.0026)	(0.0043)	(0.0061)	(0.0058)	(0.0059)	(0.0062)		
Job-to-job (last 6 months)	0.0040	0.0020	0.0011	0.0012	0.0003	-0.0027	-0.0009	0.0050	0.0068	0.0038	0.0004	0.1044	7,219
	(0.0049)	(0.0046)	(0.0044)	(0.0037)	(0.0023)	(0.0019)	(0.0031)	(0.0044)	(0.0044)	(0.0047)	(0.0049)		
Number of jobs	0.0027	0.0019	0.0017	0.0011	0.0015	-0.0014	0.0025	0.0038*	0.0058**	0.0050**	0.0054**	1.02	5,360
	(0.0024)	(0.0022)	(0.0020)	(0.0020)	(0.0019)	(0.0021)	(0.0021)	(0.0022)	(0.0023)	(0.0025)	(0.0026)		
Full-time	-0.0613***	-0.0479***	-0.0360***	-0.0277***	-0.0125***	-0.0088**	-0.0391***	-0.0491***	-0.0623***	-0.0676***	-0.0682***	0.7360	7,219
	(0.0062)	(0.0054)	(0.0048)	(0.0043)	(0.0036)	(0.0039)	(0.0041)	(0.0049)	(0.0052)	(0.0059)	(0.0066)		
Health insurer change	-0.0011	-0.0018	-0.0020	-0.0019	-0.0017	-0.0065***	-0.0057***	-0.0045***	-0.0043***	-0.0037***	-0.0031**	0.0109	7,219
	(0.0013)	(0.0013)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0013)	(0.0014)	(0.0013)	(0.0013)	(0.0013)		

Note: This table presents the event-study coefficients (first differences) from specification (1) but collapses the event-time dummies into bins. From column (1) to column (5) describes the collapsed temporality before the event presented in column (6), while from column (7) to column (11) describes the collapsed temporality after the event. Column (12) and (13) show the mean in t-1 and the observations respectively. Monthly labor earnings (zeros) contain monthly basic earnings by contribution (IBC) values in 2008 constant price terms, missing values are allocated as zeroes. Monthly labor earnings (miss) contains monthly basic earnings by contribution (IBC) values in 2008 constant price terms, without imputing zeroes in missing values. Wage earners equal to 1 for persons who have records in the (IBC) and who do not have any missing value on working days. Worked days represents the days worked without imputing missing values. The daily wage refers to the average daily income that the worker receives according to his contribution base. The variable Job loss is a dummy that indicates whether a person has ceased employment in the current month after having been employed in the previous month. Job gain is a dummy that identifies the times when a person has gone from being unemployed (no employment record) in a previous month to being employed in the current month. The Job-to-job is a dummy that identifies a job change when a person moves from being employed in one job in one month to having a different job in the following month, without ever having ceased employment. Job loss (last 6 months) is a dummy that identifies if an individual has experienced a job loss in the past six months. Job gain (last 6 months) is a dummy that identifies whether an individual has obtained employment in the past six months. The variable Job change (last 6 months) is a dummy that captures whether an individual has experienced a change in employment within the past six months. The variable Number of jobs counts the number of different jobs that an individual has had. Full-time is a dummy if the sum of days worked, days of temporary sickness absence, days of leave, days of general disability, and days of maternity leave is equal to or greater than 30. The variable Health insurer change is a dummy that identifies changes in an individual's health insurer between two consecutive months.

$*p < 0.1; **p < 0.05; ***p < 0.01$.

Source: Authors' calculation using data from PILA and RIPS.

Table A.3: The Effect of Depression on Additional Labor Market Outcomes

Outcome	$k \in [-24,-19]$ (1)	$k \in [-18,-13]$ (2)	$k \in [-12,-7]$ (3)	$k \in [-6,-4]$ (4)	$k \in [-3,-2]$ (5)	$k = 0$ (6)	$k \in [1,3]$ (7)	$k \in [4,6]$ (8)	$k \in [7,12]$ (9)	$k \in [13,18]$ (10)	$k \in [19,24]$ (11)	Mean t-1 (12)	Obs. (13)
Monthly labor earnings (zeros)	-6.21	-2.83	0.70337	-2.12	-4.44	-9.72**	-29.25***	-39.04***	-50.43***	-54.34***	-60.64***	656.94	7219
	(6.24)	(5.89)	(5.31)	(4.84)	(4.21)	(4.85)	(4.90)	(5.89)	(6.86)	(8.18)	(9.67)		
Monthly labor earnings (miss)	20.29***	16.23***	16.48***	5.70	-0.6868	-7.83	-9.72**	-7.93	-14.13**	-1.53	8.31	815.55	5815
	(6.22)	(5.86)	(5.25)	(4.59)	(4.32)	(5.34)	(4.75)	(5.43)	(6.08)	(7.20)	(8.67)		
Wage earner	-0.0326***	-0.0255***	-0.0203***	-0.0155***	-0.0043	-0.0062**	-0.0345***	-0.0493***	-0.0689***	-0.0886***	-0.1025***	0.8055	7219
	(0.0049)	(0.0046)	(0.0040)	(0.0035)	(0.0029)	(0.0029)	(0.0035)	(0.0045)	(0.0051)	(0.0061)	(0.0072)		
Worked days (miss)	0.25423**	0.18864	0.15088	-0.0047	-0.0640	-0.5949***	-0.5525***	-0.2534**	-0.1757	-0.0428	0.28634*	26.26	5815
	(0.1194)	(0.1160)	(0.1131)	(0.1128)	(0.1035)	(0.1152)	(0.1122)	(0.1243)	(0.1247)	(0.1422)	(0.1553)		
Daily wage	-0.5260	-0.6865	-0.5562	-1.04	-0.3516	-0.4686	0.35671	-1.92***	-1.20	-1.46*	-1.48*	33.58	5548
	(0.6656)	(0.6508)	(0.6671)	(0.6463)	(0.7245)	(0.6787)	(0.7213)	(0.7085)	(0.7476)	(0.8113)	(0.8586)		
Job loss	0.00429*	0.00275	0.00375*	0.00374	0.00179	0.00307	0.01773***	0.01017***	0.01293***	0.01183***	0.01374***	0.0290	7219
	(0.0022)	(0.0022)	(0.0022)	(0.0024)	(0.0026)	(0.0030)	(0.0025)	(0.0025)	(0.0024)	(0.0025)	(0.0026)		
Job gain	0.00437*	0.00444*	0.00468**	0.00433*	0.00818***	-0.0033	0.00386	0.00458*	0.00603**	0.00594**	0.00618**	0.0324	7219
	(0.0023)	(0.0023)	(0.0023)	(0.0025)	(0.0028)	(0.0030)	(0.0025)	(0.0026)	(0.0025)	(0.0026)	(0.0028)		
Job-to-job	0.00011	-0.0003	0.00039	-0.0012	0.00001	-0.0025	0.00212	0.00075	0.00127	0.00147	0.00166	0.0206	7219
	(0.0019)	(0.0019)	(0.0019)	(0.0020)	(0.0021)	(0.0023)	(0.0020)	(0.0021)	(0.0021)	(0.0021)	(0.0023)		
Job loss (last 6 months)	0.00955	0.00662	0.00633	0.00713	0.00456	0.00094	0.02457***	0.05030***	0.04247***	0.04247***	0.03980***	0.1981	7219
	(0.0062)	(0.0061)	(0.0061)	(0.0053)	(0.0032)	(0.0028)	(0.0049)	(0.0069)	(0.0067)	(0.0073)	(0.0078)		
Job gain (last 6 months)	-0.0029	-0.0018	-0.0039	-0.0014	0.00227	-0.0030	-0.0104**	-0.0096	-0.0044	-0.0020	-0.0022	0.2233	7219
	(0.0062)	(0.0062)	(0.0063)	(0.0055)	(0.0033)	(0.0028)	(0.0048)	(0.0069)	(0.0069)	(0.0074)	(0.0080)		
Job-to-job (last 6 months)	0.00365	0.00064	0.00181	0.00178	-0.0004	-0.0026	0.00022	0.00650	0.00792	0.00682	0.00512	0.1044	7219
	(0.0047)	(0.0047)	(0.0045)	(0.0038)	(0.0024)	(0.0020)	(0.0034)	(0.0050)	(0.0052)	(0.0056)	(0.0061)		
Number of jobs	0.00093	-0.0000	0.00013	0.00028	0.00095	-0.0016	0.00340	0.00426*	0.00719***	0.00940***	0.01101***	1.02	5815
	(0.0023)	(0.0022)	(0.0020)	(0.0020)	(0.0020)	(0.0022)	(0.0022)	(0.0026)	(0.0027)	(0.0029)	(0.0032)		
Full-time	-0.0358***	-0.0272***	-0.0234***	-0.0213***	-0.0098***	-0.0114***	-0.0451***	-0.0596***	-0.0796***	-0.0957***	-0.1073***	0.7360	7219
	(0.0055)	(0.0052)	(0.0047)	(0.0044)	(0.0038)	(0.0041)	(0.0044)	(0.0053)	(0.0058)	(0.0068)	(0.0078)		
Health insurer change	-0.0013	-0.0020	-0.0022*	-0.0018	-0.0017	-0.0066***	-0.0057***	-0.0043***	-0.0037***	-0.0028*	-0.0022	0.0109	7219
	(0.0014)	(0.0013)	(0.0013)	(0.0014)	(0.0015)	(0.0015)	(0.0014)	(0.0014)	(0.0014)	(0.0015)	(0.0015)		

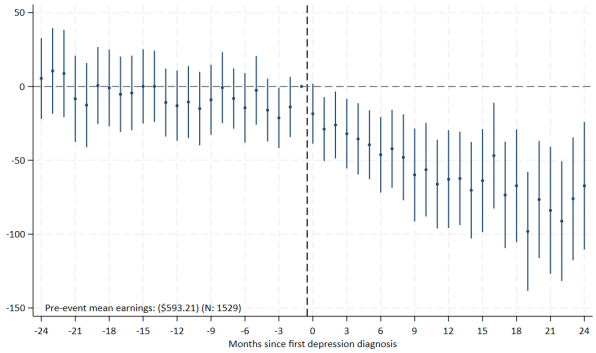
Note: This table presents robust coefficients following the Callaway and Sant'Anna (2021) methodology with pre-matching sample, also collapses the event-time dummies into bins. From column (1) to column (5) describes the collapsed temporality before the event, presented in column (6), while from column (7) to column (11) describes the collapsed temporality after the event. Column (12) and (13) show the mean in t-1 and the observations respectively. Monthly labor earnings (zeros) contain monthly basic earnings by contribution (IBC) values in 2008 constant price terms, missing values are allocated as zeroes. Monthly labor earnings (miss) contains monthly basic earnings by contribution (IBC) values in 2008 constant price terms, without imputing zeroes in missing values. Wage earners equal to 1 for persons who have records in the (IBC) and who do not have any missing value on working days. Worked days represents the days worked without imputing missing values. The daily wage refers to the average daily income that the worker receives according to his contribution base. The variable Job loss is a dummy that indicates whether a person has ceased employment in the current month after having been employed in the previous month. Job gain is a dummy that identifies the times when a person has gone from being unemployed (no employment record) in a previous month to being employed in the current month. The Job-to-job is a dummy that identifies a job change when a person moves from being employed in one job in one month to having a different job in the following month, without ever having ceased employment. Job loss (last 6 months) is a dummy that identifies if an individual has experienced a job loss in the past six months. Job gain (last 6 months) is a dummy that identifies whether an individual has obtained employment in the past six months. The variable Job change (last 6 months) is a dummy that captures whether an individual has experienced a change in employment within the past six months. The variable Number of jobs counts the number of different jobs that an individual has had. Full-time is a dummy if the sum of days worked, days of temporary sickness absence, days of leave, days of general disability, and days of maternity leave is equal to or greater than 30. The variable Health insurer change is a dummy that identifies changes in an individual's health insurer between two consecutive months.
Source: Authors' calculation using data from PILA and RIPS.

Table A.4: Heterogeneity in the Effect of Depression

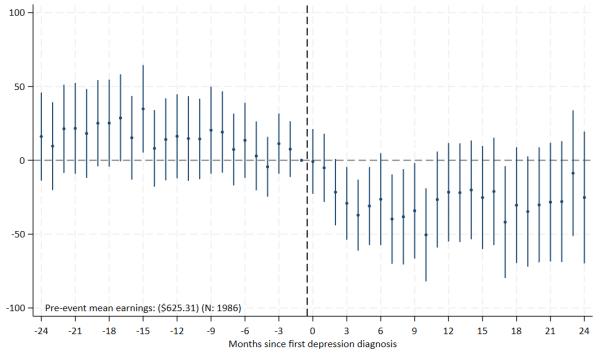
Outcome	$k \in [-24,-19]$ (1)	$k \in [-18,-13]$ (2)	$k \in [-12,-7]$ (3)	$k \in [-6,-4]$ (4)	$k \in [-3,-2]$ (5)	$k = 0$ (6)	$k \in [1,3]$ (7)	$k \in [4,6]$ (8)	$k \in [7,12]$ (9)	$k \in [13,18]$ (10)	$k \in [19,24]$ (11)	Mean t-1 (12)	Obs. (13)
All	-6.21	-2.83	0.70337	-2.12	-4.44	-9.72**	-29.25***	-39.04***	-50.43***	-54.34***	-60.64***	656.94	7219
	(6.24)	(5.89)	(5.31)	(4.84)	(4.21)	(4.85)	(4.90)	(5.89)	(6.86)	(8.18)	(9.67)		
Female	-11.84	-5.81	-5.31	-9.54*	-6.10	-11.71**	-33.28***	-43.76***	-53.75***	-49.32***	-53.35***	650.08	4572
	(7.51)	(6.86)	(6.00)	(5.74)	(5.08)	(5.89)	(5.98)	(6.99)	(8.16)	(9.76)	(11.78)		
Male	4.08	2.40	10.79	10.42	-1.33	-6.00	-21.74**	-30.68***	-44.23***	-62.85***	-71.72***	668.78	2647
	(11.08)	(10.83)	(10.17)	(8.71)	(7.41)	(8.46)	(8.55)	(10.59)	(12.24)	(14.54)	(16.73)		
Age 18-26	-7.13	-11.72	-8.13	-15.93	-0-.7343	-21.72*	-44.99***	-54.75***	-70.81***	-66.49***	-72.02**	495.77	752
	(14.57)	(13.30)	(12.53)	(10.56)	(10.11)	(11.13)	(11.01)	(14.63)	(18.25)	(22.74)	(32.17)		
Age 27-34	-5.28	-0-.8997	2.41	0.13032	-4.67	-8.35	-27.80***	-37.95***	-49.84***	-55.97***	-62.85***	675.68	6467
	(6.73)	(6.38)	(5.73)	(5.27)	(4.56)	(5.26)	(5.34)	(6.41)	(7.48)	(8.97)	(10.56)		
Large municipality	-5.87	-3.25	1.97	-1.53	-4.17	-8.64*	-28.51***	-39.60***	-51.32***	-56.42***	-62.27***	655.59	6849
	(6.54)	(6.19)	(5.55)	(5.03)	(4.39)	(5.03)	(5.05)	(6.05)	(7.14)	(8.53)	(10.12)		
Intermediate municipality	-2.94	16.04	-12.16	-2.40	2.17	-19.36	5.98	31.49*	40.46**	7.25	-8.56	631.58	288
	(23.18)	(19.23)	(17.33)	(16.10)	(10.73)	(13.12)	(13.41)	(18.13)	(18.45)	(22.67)	(28.42)		
Small municipality	-53.02	-56.80	1.69	-23.31	-26.86	25.98	-24.54	-15.52	-58.47	-25.00	-65.79	858.38	82
	(53.92)	(44.01)	(38.34)	(38.15)	(38.23)	(53.82)	(30.98)	(36.48)	(46.11)	(55.80)	(65.91)		
With treatment	-9.51	-10.16	-2.11	-3.45	-6.54	-19.10**	-24.76***	-20.29*	-33.49***	-36.14***	-32.23**	617.43	2179
	(11.24)	(10.09)	(9.87)	(8.96)	(7.19)	(8.44)	(8.76)	(10.80)	(11.88)	(13.36)	(16.14)		
Without treatment	-2.37	2.16	3.12	-1.28	-3.27	-5.64	-31.46***	-46.80***	-58.71***	-64.68***	-76.11***	674.02	5040
	(7.57)	(7.26)	(6.28)	(5.76)	(5.18)	(5.91)	(5.92)	(7.08)	(8.48)	(10.32)	(12.29)		

Note: This table presents the event-study coefficients from specification Callaway and Sant'Anna (2021) collapses the event-time dummies into bins. From column (1) to column (5) describes the collapsed temporality before the event presented in column (6), while from column (7) to column (11) describes the collapsed temporality after the event. Column (12) and (13) show the mean in t-1 and the observations respectively. All corresponds to the specific sample of individuals aged 18-34 with formal jobs. The variable Female is a dummy that takes the value of 1 if the individual is female, while Male is a dummy that takes the value of 1 if the individual is male. Age 18-26 indicates the results for the age group of 18 to 26, and Age 27-34 corresponds to the results for the age group of 27 to 34. As for the size classification of municipalities, Large municipality is a dummy equal to 1 if the population of the municipality exceeds 100,000 inhabitants; Intermediate municipality takes the value of 1 if the population of the municipality is between 20,001 and 100,000 inhabitants; and Small municipality equals 1 if the population of the municipality is less than 20,000 inhabitants. Finally, the variable With treatment takes the value of 1 if at least one consultation related to psychology, social work or psychiatry was registered, while treatment is equal to 0 if no consultations of this type were identified.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using data from PILA and RIPS.

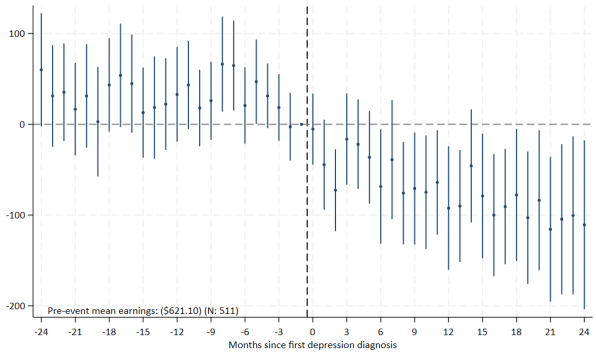
Figure A.4: The Effect of Depression by Depression Severity



(a) Mild



(b) Moderate

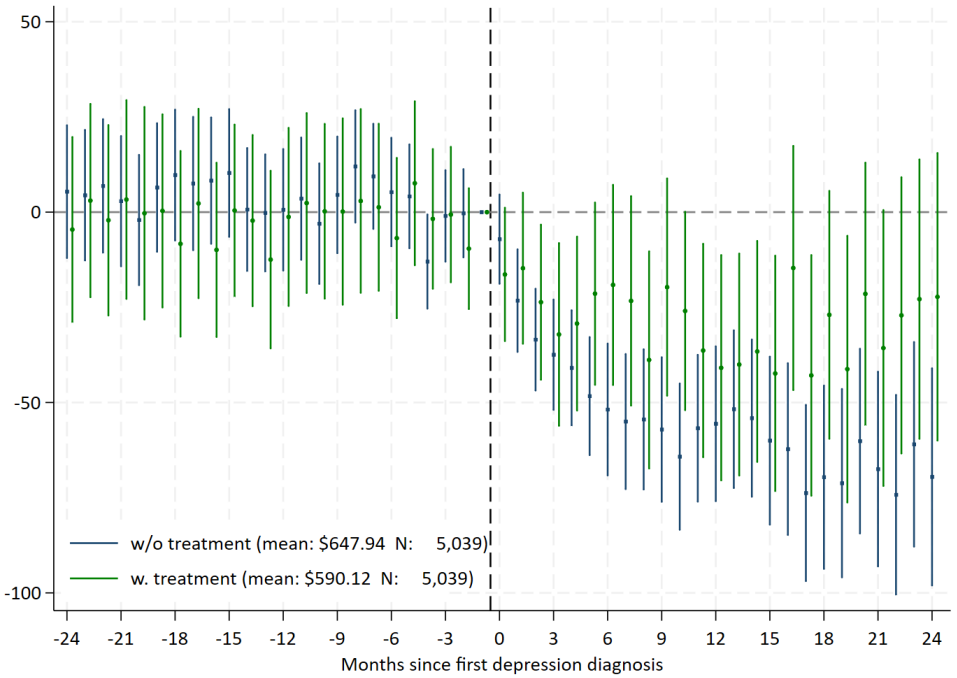


(c) Severe

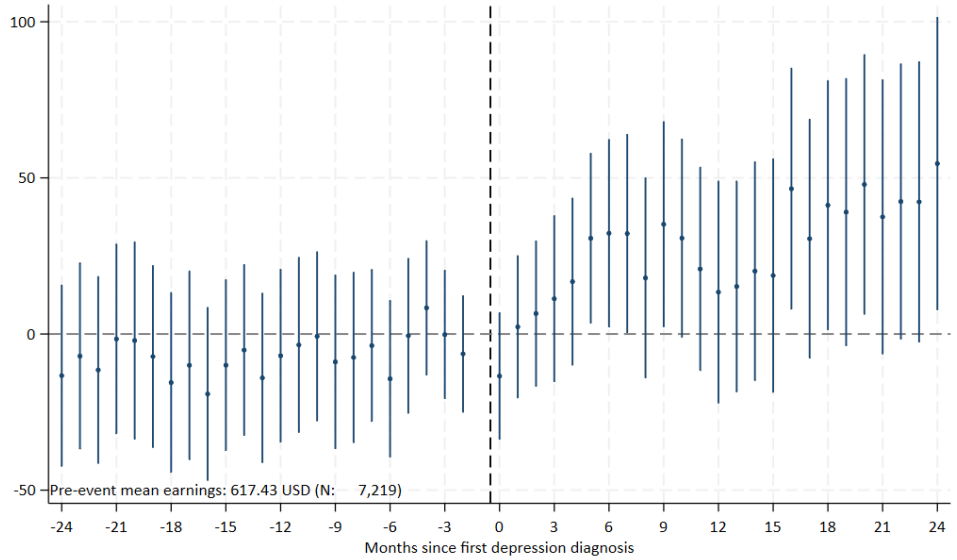
Note: Figures shows the event-study coefficients with the corresponding 95 percent confidence intervals derived from the Callaway and Sant'Anna (2021) estimator. Each panel shows the effect of depression on admissions for severity of depression: mild (a), moderate (b) and severe (c). The estimated coefficients are presented with 95% confidence intervals. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars.
Source: Authors' calculation using data from the PILA and RIPs.

Figure A.5: Income Trajectories Conditional on Treatment Status

(a) Treated vs. non-treated trajectories



(b) Triple difference



Note: Figures shows the event-study coefficients with the corresponding 95 percent confidence intervals derived from the Callaway and Sant'Anna (2021) estimator. Panel (a) shows the trajectories of income before and after the first diagnosis of depression by highlighting the different trajectories of individuals who accessed treatment vs. those who did not illustrates the trajectories in earnings from difference averages. While panel (b) shows the difference in averages between the coefficients of treated and controls. The X-axis represents the periods before and after initial diagnosis. The Y-axis measures the variation in monthly income in dollars.
Source: Authors' calculation using data from the PILA and RIPs.

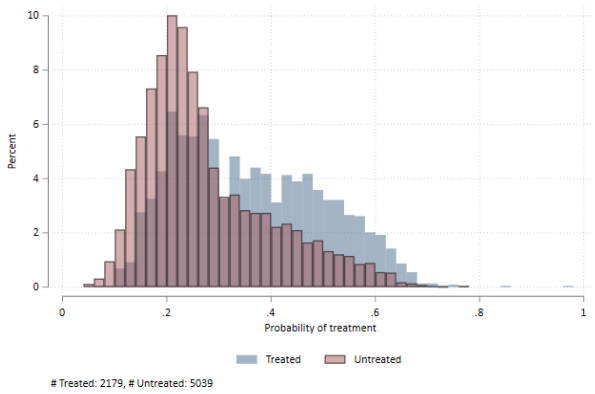
Table A.5: Comparison of Characteristics of Individuals With and Without Access to Specialty Care

	(1) Treated	(2) Untreated	(3) Mean diff.	(4) P-value
Panel A. Individual characteristics				
Age	24.77	25.42	0.6590	0.0000
Female	0.6631	0.6204	-0.0428	0.0005
Large municipality	0.9294	0.9201	-0.0093	0.1496
Intermediate municipality	0.0508	0.0561	0.0053	0.3395
Small municipality	0.0198	0.0238	0.0040	0.2533
Panel B. Job market				
Wage	6,310.49	6,926.80	616.32	0.0000
Formality	1.00	1.00	0.0000	.
Daily wage	18.36	20.14	1.79	0.0000
Worked days	341.75	341.45	-0.3028	0.6536
Vacation days	0.5316	0.5553	0.0237	0.2993
Temporal suspension days	0.1087	0.1051	-0.0036	0.8184
Non-labor sick leave days	0.4312	0.4299	-0.0014	0.9737
Labor sick leave days	0.0805	0.0781	-0.0023	0.9147
Parental leave days	0.2629	0.1719	-0.0910	0.0016
Employment-to-unemployment	0.0130	0.0139	0.0008	0.3259
Unemployment-to-employment	0.0186	0.0191	0.0005	0.6303
Job-to-job	0.0214	0.0207	-0.0007	0.5538
Pr. Worked days	1.00	1.00	0.0000	.
Pr. Vacation days	0.3346	0.3515	0.0169	0.1660
Pr. Temporal suspension days	0.0991	0.0953	-0.0039	0.6091
Pr. Non-labor sick leave days	0.3593	0.3445	-0.0148	0.2251
Pr. Labor sick leave days	0.0620	0.0534	-0.0086	0.1459
Pr. Parental leave days	0.0514	0.0353	-0.0161	0.0014
Panel C. Health sector				
Outpatient visits	0.3506	0.3183	-0.0323	0.0024
Medical procedures	0.2949	0.2706	-0.0243	0.0899
ED visits	0.0284	0.0243	-0.0041	0.0514
Pr. Outpatient visits	0.7045	0.6952	-0.0093	0.4309
Pr. Medical procedures	0.4695	0.4949	0.0255	0.0470
Pr. ED visits	0.1556	0.1419	-0.0137	0.1308
Mild epressive episode	0.2267	0.2054	-0.0213	0.0419
Moderate-severe depressive episode without psychotic symptoms	0.3153	0.3324	0.0171	0.1546
Severe depressive episode with psychotic symptoms	0.0165	0.0191	0.0025	0.4615
Other depressive episodes	0.3226	0.3126	-0.0101	0.3985
Recurrent depressive disorder, current episode mild	0.0202	0.0202	0.0000	0.9891
Recurrent depressive disorder, current episode moderate-severe without psychotic	0.0624	0.0546	-0.0078	0.1871
Recurrent depressive disorder, current episode severe with psychotic symptoms	0.0014	0.0042	0.0028	0.0587
Other recurrent depressive disorders	0.0454	0.0566	0.0111	0.0532
Panel D. Industry				
Agriculture	0.1184	0.1069	-0.0115	0.1281
Mining	0.0028	0.0032	0.0004	0.7885
Manufacture	0.0905	0.0989	0.0084	0.2455
Public sevices	0.0076	0.0105	0.0029	0.2259
Construction	0.0066	0.0065	-0.0001	0.9539
Commerce and transportation	0.1544	0.1356	-0.0187	0.0275
Communications	0.0245	0.0281	0.0036	0.3606
Finances	0.0862	0.0900	0.0038	0.5813
Real estate	0.0005	0.0000	-0.0004	0.1441
Professional activities	0.3294	0.3267	-0.0027	0.8126
Public admin. and education	0.0553	0.0696	0.0143	0.0181
Entertainment	0.0779	0.0809	0.0030	0.6461
Health services	0.0150	0.0112	-0.0039	0.1433
Observations	2,179	5,039		

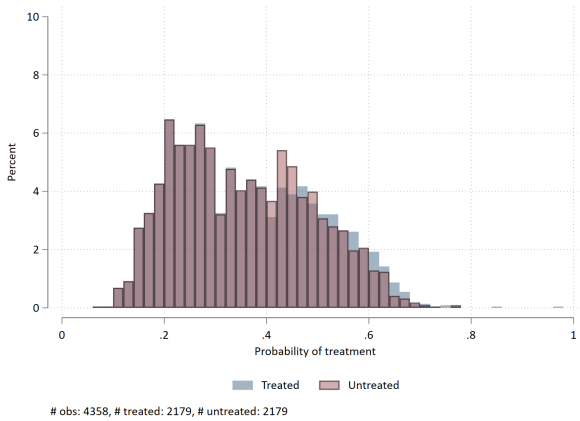
Note: This table presents a descriptive analysis of the characteristics of individuals who accessed and did not access treatment, differentiating between several relevant dimensions: individual characteristics, labour market, health sector and industry. Column (1) shows the averages of the variables for treated individuals, while column (2) shows the averages for untreated. Column (3) calculates the average difference between both groups, and column (4) reports p-values to contrast the statistical significance of these differences. Panel A includes demographic variables such as age, gender and residence in different types of municipalities. Panel B covers labour indicators, including wages, days worked, leave days and work transitions. Panel C examines the use of health services, such as outpatient consultations, medical procedures and emergency room visits. Finally, Panel D classifies individuals according to the industrial sector in which they work.
Source: Authors' calculation using data from PILA and RIPS.

Figure A.6: Propensity Scores for Individuals With and Without Access to Specialty Care Before and After Propensity Score Matching

(a) Propensity score before matching



(b) Propensity score after matching



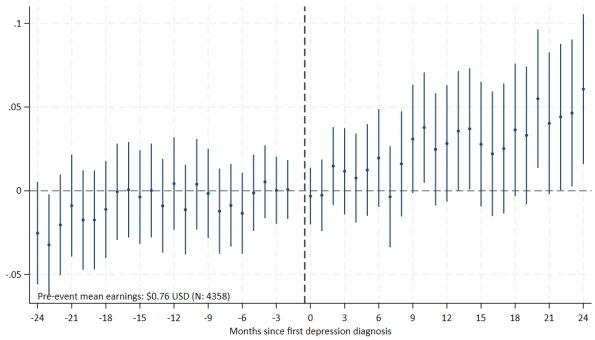
Note: This figure illustrates the distribution of propensity scores for treated and untreated individuals before and after the matching process. Panel (a) shows the propensity scores before pairing, where there is a noticeable difference in distributions between the two groups. On the other hand, panel (b) presents propensity scores after matching, highlighting a greater overlap between the treaties and the non-treaties.
Source: Authors' calculation using data from PILA and RIPS.

Table A.6: Covariate Balance Before and After Propensity Score Matching

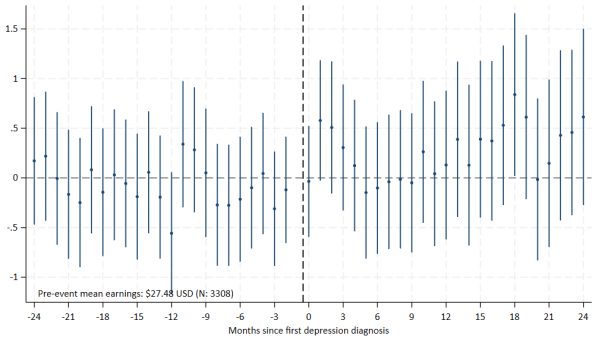
	(1) Treated	(2) Untreated	(3) Mean diff.	(4) P-value
Panel A. Individual characteristics				
Age	24.77	24.84	0.0698	0.4127
Female	0.6631	0.6604	-0.0028	0.8477
Large municipality	0.9294	0.9309	0.0015	0.8357
Intermediate municipality	0.0508	0.0480	-0.0028	0.6467
Small municipality	0.0198	0.0211	0.0013	0.7316
Panel B. Job market				
Wage	6,310.49	6,193.52	-116.9615	0.3232
Formality	1.00	1.00	0.0000	.
Daily wage	18.36	18.03	-0.3255	0.3255
Worked days	341.75	341.30	-0.4548	0.5646
Vacation days	0.5316	0.5220	-0.0096	0.7177
Temporal suspension days	0.1087	0.0927	-0.0160	0.3198
Non-labor sick leave days	0.4312	0.4199	-0.0114	0.8063
Labor sick leave days	0.0805	0.0826	0.0022	0.9253
Parental leave days	0.2629	0.2318	-0.0311	0.4138
Employment-to-unemployment	0.0130	0.0133	0.0003	0.7807
Unemployment-to-employment	0.0186	0.0189	0.0002	0.8373
Job-to-job	0.0214	0.0220	0.0006	0.6519
Pr. Worked days	1.00	1.00	0.0000	.
Pr. Vacation days	0.3346	0.3346	0.0000	1.0000
Pr. Temporal suspension days	0.0991	0.0991	0.0000	1.0000
Pr. Non-labor sick leave days	0.3593	0.3547	-0.0046	0.7519
Pr. Labor sick leave days	0.0620	0.0642	0.0023	0.7555
Pr. Parental leave days	0.0514	0.0459	-0.0055	0.3982
Panel C. Health sector				
Outpatient visits	0.3506	0.3351	-0.0155	0.2512
Medical procedures	0.2949	0.2783	-0.0166	0.3731
ED visits	0.0284	0.0301	0.0017	0.5254
Pr. Outpatient visits	0.7045	0.6985	-0.0060	0.6670
Pr. Medical procedures	0.4695	0.4511	-0.0184	0.2242
Pr. ED visits	0.1556	0.1652	0.0096	0.3861
Mild epressive episode	0.2267	0.2217	-0.0050	0.6896
Moderate-severe depressive episode without psychotic symptoms	0.3153	0.3084	-0.0069	0.6239
Severe depressive episode with psychotic symptoms	0.0165	0.0170	0.0005	0.9061
Other depressive episodes	0.3226	0.3318	0.0092	0.5186
Recurrent depressive disorder, current episode mild	0.0202	0.0220	0.0018	0.6735
Recurrent depressive disorder, current episode moderate-severe without psychotic	0.0624	0.0597	-0.0028	0.7043
Recurrent depressive disorder, current episode severe with psychotic symptoms	0.0014	0.0000	-0.0014	0.0832
Other recurrent depressive disorders	0.0454	0.0477	0.0023	0.7194
Panel D. Industry				
Agriculture	0.1184	0.1155	-0.0030	0.7449
Mining	0.0028	0.0030	0.0002	0.9206
Manufacture	0.0905	0.0925	0.0020	0.8157
Public sevices	0.0076	0.0078	0.0002	0.9509
Construction	0.0066	0.0062	-0.0003	0.8794
Commerce and transportation	0.1544	0.1519	-0.0024	0.8137
Communications	0.0245	0.0296	0.0051	0.2682
Finances	0.0862	0.0874	0.0012	0.8792
Real estate	0.0005	0.0000	-0.0004	0.3610
Professional activities	0.3294	0.3296	0.0002	0.9861
Public admin. and education	0.0553	0.0582	0.0029	0.6614
Entertainment	0.0779	0.0770	-0.0008	0.9124
Health services	0.0150	0.0096	-0.0054	0.0864
Observations	2,179	2,179		

Note: This table presents the comparison of characteristics between treated and untreated individuals after applying the propensity score matching. The structure of the table is organized into four columns: (1) shows the average values of the variables for the treated group, (2) for the untreated group, (3) the average difference between both groups and (4) the p-value associated with the statistical significance of these differences. The panels are divided into: Panel A includes individual characteristics such as age, gender and type of municipality of residence; Panel B focuses on labour market indicators such as wages, working days, leave and work transitions; Panel C addresses aspects related to the use of health services, such as outpatient consultations, medical procedures and emergency room visits; finally, Panel D classifies individuals according to industrial sector of occupation.

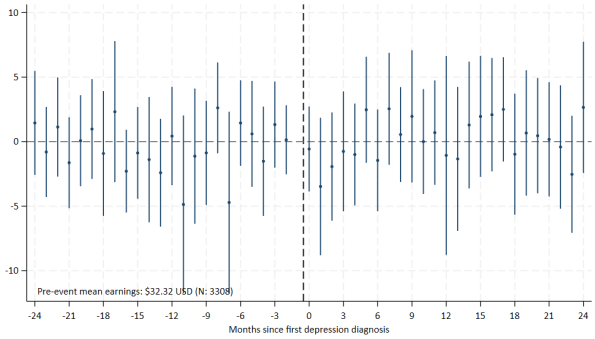
Figure A.7: The Effect of Specialty Care on Additional Labor Market Outcomes



(a) Extensive margin



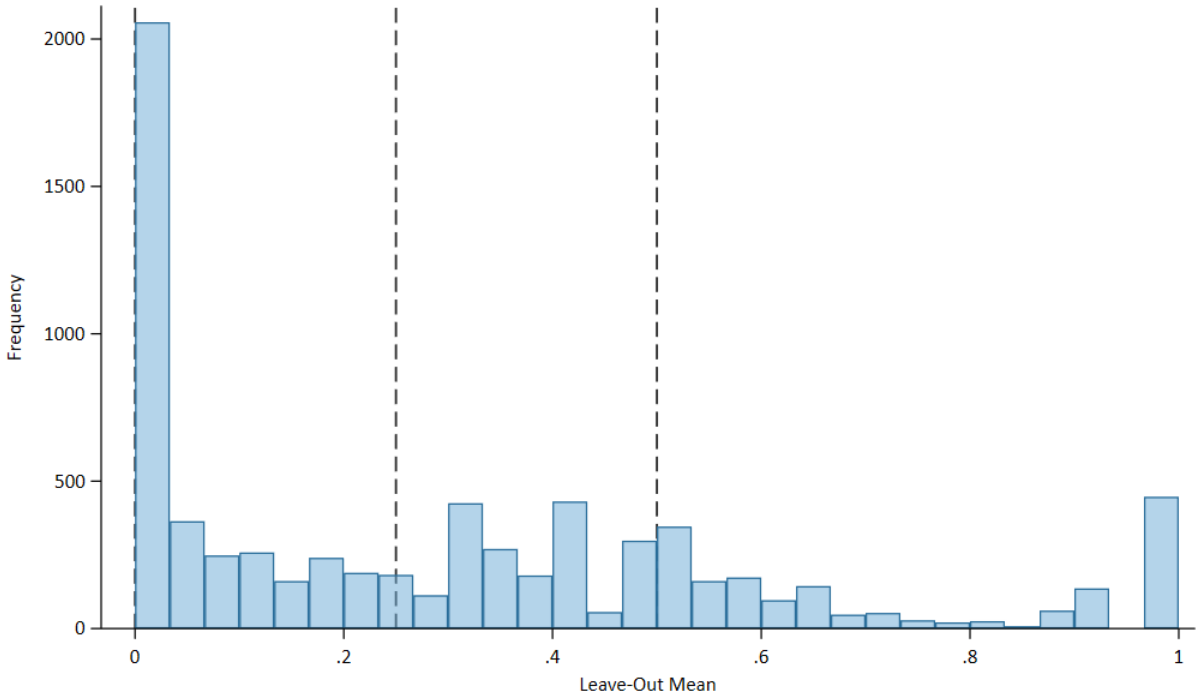
(b) Days worked



(c) Daily wage

Note: This figure illustrates the effect of specialty care on labor market outcomes using the Callaway and Sant'Anna (2021) estimator in a matched population. Panel (a) shows the impact on the probability of being a formal worker, Panel (b) presents the effect on days worked, and Panel (c) reports the impact on daily wages. Estimates reflect differences in means between treated and control groups, with 95% confidence intervals. The X-axis represents months before and after the first depression diagnosis, while the Y-axis measures the respective labor market outcome.
Source: Authors' calculation using data from the PILA and RIPs.

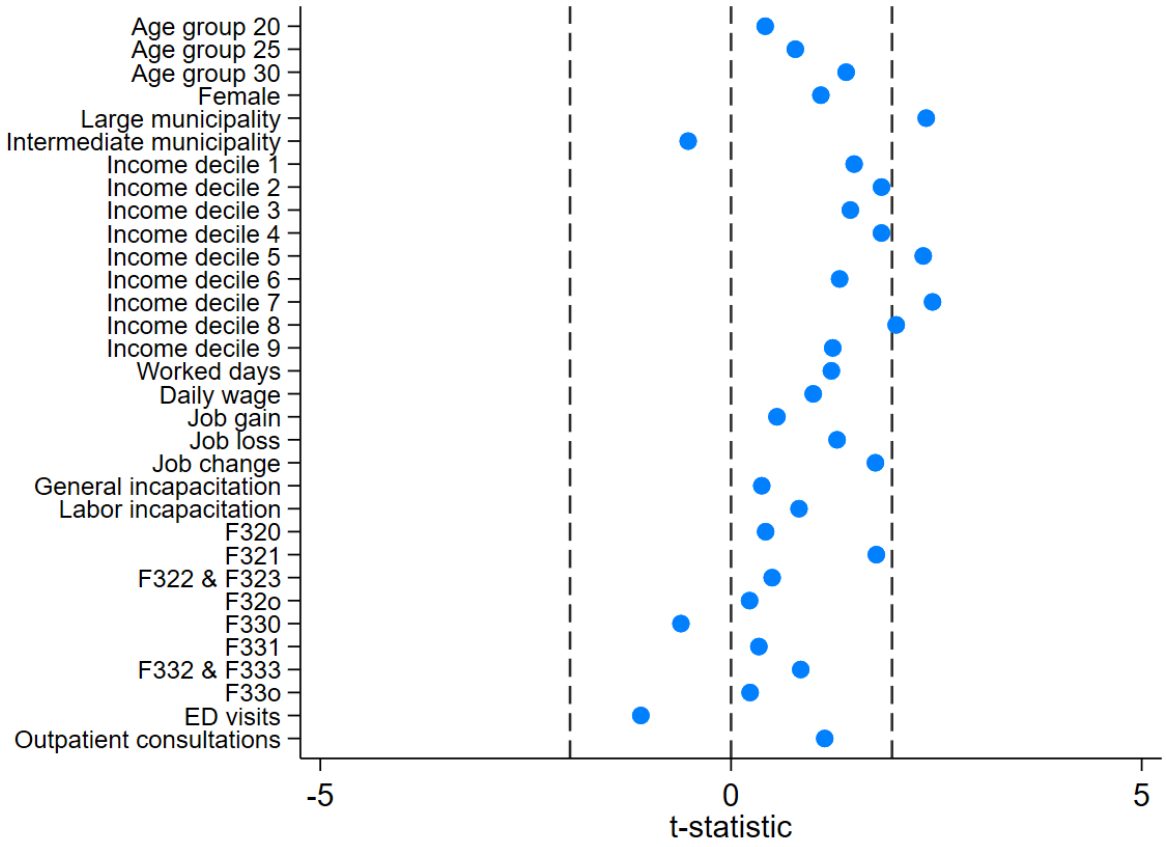
Figure A.8: Histogram of Leave-Out Mean of Probability of Treatment



Note: The leave-out mean is the probability that individuals other than the index patient who visited the same hospital received specialty care. As shown, the probability of treatment varies widely, ranging from 0% to 100%. There is a large mass at zero corresponding to the 27.5% of individuals who visit hospitals who do not refer patients to specialty care.
Source: Authors' calculation using data from the PILA and RIPs.

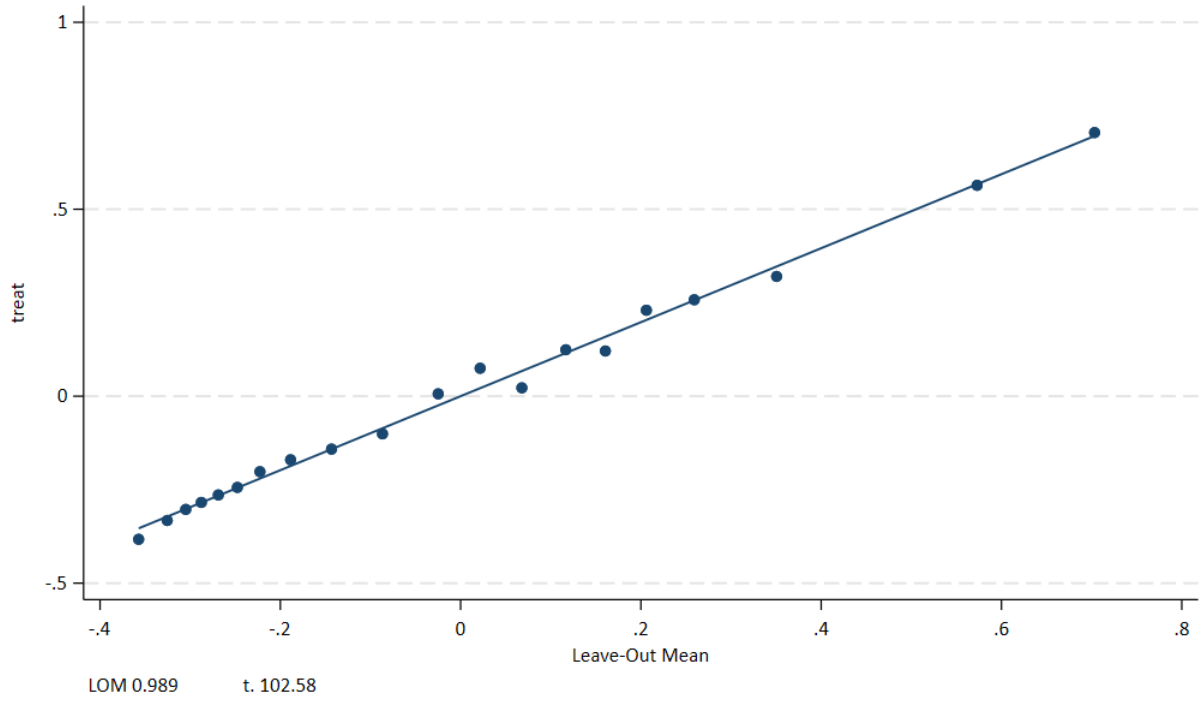
Figure A.9: Covariate Balance With Respect to Leave-Out Mean

Figure A.10: Above 25



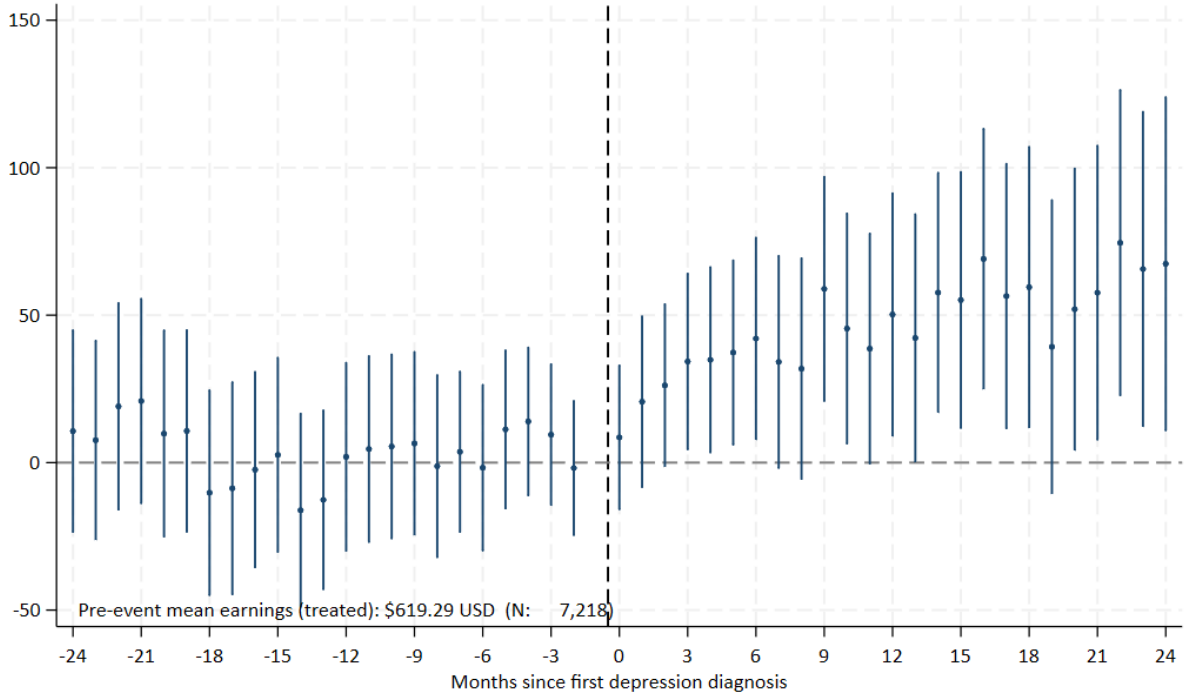
Note: This figure visually summarizes the balance estimates, depicting t-statistics from a regression of the binary instrument (being above the 25th percentile of the leave-out mean) on individual characteristics, conditional on insurer and year-fixed effects. The plot shows that the instrument is largely uncorrelated with most patient characteristics, indicated by t-statistics clustering around zero and generally within the critical thresholds of ± 1.96. Some characteristics such as residing in large municipalities and belonging to higher income deciles exhibit statistically significant correlations. *Source:* Authors' calculation using data from the PILA and RIPs.

Figure A.11: First-Stage Relationship Between Probability of Treatment and Leave-Out Mean



Note: The horizontal (Leave-Out Mean) and vertical (Probability of Treatment) axes depict residualized values, having partialed out the same set of covariates used in the Propensity Score Matching procedure. This residualization ensures that the plotted relationship is not confounded by other observed characteristics, isolating the association between the Leave-Out Mean and the Probability of Treatment. The linear fit indicates a strong first-stage relationship after controlling for covariates, highlighting the importance of leave-out information in predicting treatment status. *Source:* Authors' calculation using data from the PILA and RIPs. Please refer to Section 6 for details on how this plot was constructed.

Figure A.12: Reduced-Form Effect of Exposure to High Treatment Intensity Hospital



Source: Authors' calculation using data from the PILA and RIPS. Please refer to Section 6 for details on how this plot was constructed.

Table A.7: The Effect of Depression on Labor Market Outcomes in the Matched Sample

Outcome	$k \in [-24,-19]$ (1)	$k \in [-18,-13]$ (2)	$k \in [-12,-7]$ (3)	$k \in [-6,-4]$ (4)	$k \in [-3,-2]$ (5)	$k = 0$ (6)	$k \in [1,3]$ (7)	$k \in [4,6]$ (8)	$k \in [7,12]$ (9)	$k \in [13,18]$ (10)	$k \in [19,24]$ (11)	Mean t-1 (12)	Obs. (13)
Monthly labor earnings (zeros)	3.80	-2.55	0.26176	0.31235	-3.43	-18.57***	-35.49***	-41.89***	-51.11***	-58.11***	-64.74***	614.84	4358
	(8.38)	(7.65)	(7.21)	(6.60)	(5.66)	(6.32)	(6.75)	(8.12)	(9.33)	(10.74)	(12.65)		
Monthly labor earnings (miss)	10.28	7.57	9.26	-3.96	-6.46	-18.77***	-18.67***	-22.10***	-23.23***	-11.12	-4.99	809.27	3311
	(8.24)	(7.54)	(7.05)	(6.44)	(5.73)	(6.76)	(6.43)	(7.74)	(8.39)	(9.51)	(11.40)		
Wage earner	-0.0180***	-0.0191***	-0.0113**	-0.0089*	0.00034	-0.0083*	-0.0347***	-0.0476***	-0.0630***	-0.0814***	-0.0936***	0.7598	4358
	(0.0068)	(0.0063)	(0.0057)	(0.0050)	(0.0042)	(0.0043)	(0.0051)	(0.0063)	(0.0072)	(0.0086)	(0.0099)		
Worked days (miss)	-0.0125	0.02340	0.02832	-0.0384	-0.0830	-0.6462***	-0.5236***	-0.1060	-0.1091	-0.1074	0.06621	27.51	3311
	(0.1239)	(0.1209)	(0.1189)	(0.1290)	(0.1203)	(0.1415)	(0.1284)	(0.1371)	(0.1328)	(0.1554)	(0.1636)		
Daily wage	-0.1992	-0.1147	-0.1033	-0.8189	-0.2792	-0.2230	0.94665	-1.47*	-1.28	-1.34	-2.00**	32.87	3311
	(0.7533)	(0.7074)	(0.7976)	(0.7595)	(0.6691)	(0.8297)	(0.8922)	(0.8453)	(0.8527)	(0.9211)	(0.9356)		
Job loss	0.00038	-0.0014	4.55e-	0.00095	-0.0007	0.00334	0.01635***	0.00555	0.00920**	0.00888**	0.01106***	0.0383	4358
	(0.0033)	(0.0032)	(0.0033)	(0.0035)	(0.0037)	(0.0044)	(0.0037)	(0.0038)	(0.0036)	(0.0037)	(0.0039)		
Job gain	0.00129	0.00063	0.00146	0.00265	0.00602	-0.0038	0.00574	0.00483	0.00812**	0.00899**	0.00507	0.0395	4358
	(0.0033)	(0.0033)	(0.0033)	(0.0036)	(0.0039)	(0.0043)	(0.0036)	(0.0038)	(0.0035)	(0.0037)	(0.0041)		
Job-to-job	-0.0015	-0.0035	-0.0010	-0.0029	-0.0011	-0.0053*	-0.0014	-0.0023	-0.0028	2.06e-	-0.0034	0.0220	4358
	(0.0026)	(0.0025)	(0.0025)	(0.0027)	(0.0027)	(0.0030)	(0.0027)	(0.0029)	(0.0029)	(0.0030)	(0.0032)		
Job loss (last 6 months)	-0.0001	-0.0039	-0.0055	-0.0021	0.00033	0.00338	0.03060***	0.05101***	0.03977***	0.04290***	0.02575**	0.2267	4358
	(0.0083)	(0.0082)	(0.0081)	(0.0073)	(0.0044)	(0.0038)	(0.0067)	(0.0096)	(0.0095)	(0.0105)	(0.0113)		
Job gain (last 6 months)	-0.0110	-0.0153*	-0.0103	-0.0104	-0.0023	-0.0022	-0.0072	-0.0048	0.00078	0.00193	-0.0097	0.2492	4358
	(0.0084)	(0.0083)	(0.0083)	(0.0074)	(0.0045)	(0.0038)	(0.0067)	(0.0095)	(0.0096)	(0.0105)	(0.0115)		
Job-to-job (last 6 months)	0.00168	-0.0025	-0.0001	0.00257	-0.0007	-0.0014	-0.0001	0.00490	0.00362	0.01124	0.00098	0.1019	4358
	(0.0061)	(0.0061)	(0.0058)	(0.0050)	(0.0031)	(0.0024)	(0.0043)	(0.0065)	(0.0068)	(0.0076)	(0.0082)		
Number of jobs	0.00043	-0.0020	-0.0006	-0.0003	-0.0022	-0.0021	0.00257	0.00131	0.00487	0.00848**	0.00622	1.02	3477
	(0.0031)	(0.0029)	(0.0027)	(0.0028)	(0.0026)	(0.0030)	(0.0030)	(0.0036)	(0.0038)	(0.0041)	(0.0046)		
Full-time	-0.0107	-0.0085	-0.0095	-0.0035	-0.0039	-0.0485***	-0.0426***	-0.0387***	-0.0450***	-0.0604***	-0.0647***	0.5799	4358
	(0.0082)	(0.0079)	(0.0075)	(0.0075)	(0.0072)	(0.0078)	(0.0076)	(0.0084)	(0.0088)	(0.0101)	(0.0112)		
Health insurer change	-0.0016	-0.0019	-0.0027	-0.0031	-0.0011	-0.0074***	-0.0069***	-0.0039**	-0.0050**	-0.0043**	-0.0031	0.0119	4358
	(0.0018)	(0.0018)	(0.0018)	(0.0019)	(0.0021)	(0.0020)	(0.0019)	(0.0020)	(0.0020)	(0.0021)	(0.0022)		

Note: This table presents the event-study coefficients (first differences) using the Callaway and Sant'Anna (2021) estimator but collapses the event-time dummies into bins in the matched sample. From column (1) to column (5) describes the collapsed temporality before the event presented in column (6), while from column (7) to column (11) describes the collapsed temporality after the event. Column (12) and (13) show the mean in t-1 and the observations respectively. Monthly labor earnings (zeros) contain monthly basic earnings by contribution (IBC) values in 2008 constant price terms, missing values are allocated as zeroes. Monthly labor earnings (miss) contains monthly basic earnings by contribution (IBC) values in 2008 constant price terms, without imputing zeroes in missing values. Wage earners equal to 1 for persons who have records in the (IBC) and who do not have any missing value on working days. Worked days represents the days worked without imputing missing values. The daily wage refers to the average daily income that the worker receives according to his contribution base. The variable Job loss is a dummy that indicates whether a person has ceased employment in the current month after having been employed in the previous month. Job gain is a dummy that identifies the times when a person has gone from being unemployed (no employment record) in a previous month to being employed in the current month. The Job-to-job is a dummy that identifies a job change when a person moves from being employed in one job in one month to having a different job in the following month, without ever having ceased employment. Job loss (last 6 months) is a dummy that identifies if an individual has experienced a job loss in the past six months. Job gain (last 6 months) is a dummy that identifies whether an individual has obtained employment in the past six months. The variable Job change (last 6 months) is a dummy that captures whether an individual has experienced a change in employment within the past six months. The variable Number of jobs counts the number of different jobs that an individual has had. Full-time is a dummy if the sum of days worked, days of temporary sickness absence, days of leave, days of general disability, and days of maternity leave is equal to or greater than 30. The variable Health insurer change is a dummy that identifies changes in an individual's health insurer between two consecutive months.

$*p < 0.1; **p < 0.05; ***p < 0.01$.

Source: Authors' calculation using data from PILA and RIPS.