The Role of Employment and Employers in Mental Health

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Abstract

How does employment affect workers' mental health, and how do employers mediate this relationship? We investigate these questions on employer-employee matched data for Denmark from 2008 onwards, linked to administrative records on mental-health prescriptions. We leverage job starts and job separations in a dynamic difference-in-differences design, comparing workers with "soon-to-be-treated" peers, matched on pre-treatment mental-health histories and demographics. Results indicate that job losses produce an increase in the incidence of mental health medication of about 20-25%, while job finding generates a symmetric decline in medication of about 15-20%. The beneficial effects of employment are especially strong in firms with low prevalence of mental health issues, and muted in firms with high prevalence. Using the same empirical set-up for firm-to-firm transitions without intervening unemployment, we find that employee mental health experiences changes of the same sign as the differential in the firm-level mental health.

Keywords: Firms, mental health, matched employer-employee data
JEL classifications: I14, J31, J32

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1 Introduction

Work and workplaces play a central role in the economic fortunes of individuals, and decades of research on employer-employee matched data have provided rich evidence on career patterns within and across firms. While it is plausible that work experiences contribute to the personal well-being and mental health of workers, much less is known on their impacts beyond the economic sphere. Indeed, work can be a protective factor for mental health. Employment provides income, structure, social connections, and purpose — all of which promote mental well-being (WHO, 2022). However, not all workplaces are equally conducive to good mental health. Demands on one's time that encroach on personal life, competition in the workplace, job insecurity, and even bullying or other forms of workplace violence are all likely to negatively affect mental well-being.

In this paper, we study the impacts of employment and workplaces on individuals' mental health using matched employer–employee data for the universe of Danish residents, which we link to medical prescription data, including medications used for the treatment of depression and anxiety. We define mental health treatment as receiving any medication associated with a mental health condition. Importantly, the data record mental health treatment, rather than the underlying mental health status. In the spirit of Grossman (1972), we interpret treatment as a restorative investment in one's human capital that is triggered when mental health falls below a certain threshold. Hence, receiving mental health treatment is indicative of poor mental health, likely attenuating positive state dependence in mental health status.

In order to identify the effects of interest, we begin by focusing on the mental health impacts of transitions between employment and unemployment. One major identification challenge arises from the joint determination of mental health treatment and employment status. In particular, individuals who are more likely to become unemployed (our treatment group) may differ systematically in unobserved determinants of mental health from those who remain employed (our control group). To address this concern, we restrict the control group to individuals who undergo the same transition as the treatment group at a later date, in the spirit of Fadlon & Nielsen (2019, 2021). Intuitively, future movers are more comparable to current movers in terms of underlying employability (and related unobservables) than individuals who never transition. The choice of treatment lag involves a trade-off between the need to ensure comparability of treatment and control groups and maintaining a sufficiently long horizon over which to observe mental health impacts.

The second challenge concerns the dynamic structure of employment and mental health processes – each of which is likely to exhibit (positive) state dependence and to be affected by lagged values of the other. Identification of the causal effects of interest therefore requires

accounting for this interdependence and for dynamic cross-effects. To illustrate this point, let's consider that deteriorating mental health may lead to job loss; and, because poor mental health in a given period predicts poor mental health in subsequent periods, one might incorrectly attribute further deterioration in mental health to the job separation. The reverse pattern may arise for unemployed individuals whose mental health improves and who subsequently transition into employment as a result. This identification problem is analogous to that encountered in the literature on the effects of training on employment (Ashenfelter, 1978). To address this challenge, we compare treatment and control groups with similar mental health histories. Building on Sullivan & Card (1988)'s approach to identifying the impact of training programs on employment transitions, we propose a matching estimator of the effect of employment on mental health based on mental health histories, gender, and age. The validity of our approach, combining staggered treatment and matching techniques, rests on the assumption that, conditional on a sufficiently long observed mental health history, the timing of employment transitions is as good as random.

We additionally investigate differential impacts of joining or leaving workplaces of heterogeneous quality, defined as the baseline level of mental health of their workforce. In doing this, we posit the existence of a firm-level component in the determination of workers' mental health, and we estimate changes in an individuals' mental health as they join firms at different quantiles in the mental health distribution, in a similar spirit to Card et al. (2018)'s analysis of wage changes. Symmetrically, we estimate heterogeneous mental health impacts of separating from firms at different quantiles of the mental health distribution. We complete this analysis by looking at direct job-to-job transitions across the health distribution of firms.

To pre-empt our results, we find a positive, sizeable, and statistically significant effect of employment on mental health. In particular, transitioning into unemployment produces a gradual deterioration of mental health, proxied by an increase in the incidence in (any) mental health medication around 20-25% within a year of job loss, over a baseline of 3.6% for men and 5.9% for women. Symmetrically, those who find employment experience a gradual improvement in their mental health, with a 15-20% decline in the incidence of medication within a year of job finding, over a baseline of 5.4% for men and 8.5% for women

When we investigate heterogeneous effects across the firm-level mental health distribution, we find that job losses are particularly detrimental when they involve separation from especially "healthy" firms, i.e. those at the bottom of the distribution of mental health incidence. Conversely, the beneficial effects of job finding are larger when individuals are hired by healthier firms

Our findings contribute to various strands of literatures. Although it is well established

in the psychology and psychiatry literature that depression and anxiety – the two most common mental health disorders in the population – stem from a combination of genetic, biological, psychological, and environmental factors (WHO, 2022), credible evidence on the causal effect of employment, let alone firm characteristics, on mental health remains scarce. Of course, there is ample evidence of a positive association between employment and earnings on the one hand and well-being and the incidence of mental health symptoms on the other, dating back to Clark & Oswald (1994). Yet, these correlations are often likely to be spurious, as contextual factors – such as poverty (Ridley et al., 2020) or negative life events (Jensen & Zhang, 2023) – may affect both wellbeing and employment dimensions. Moreover, there is evidence that mental health influences employment, leading to reverse causality. In particular, a small but influential body of research in economics has found credible evidence that certain treatments can improve mental health (Angelucci & Bennett, 2023; Bhalotra et al., 2025; Biasi et al., 2025), although evidence of their subsequent impact on labor market or other outcomes is mixed. While more recent work has attempted to identify the causal impact of job loss on mental health by leveraging variation from plant closures (Kuhn et al., 2009; Eliason & Storrie, 2009), there is no evidence in this literature about the mental health benefits of finding employment, or about the heterogeneous effects of employment transitions according to the health of work environments. Our main contribution to this literature is to provide an integrated framework for the causal identification of potentially heterogeneous effects of work on mental health, embracing the dynamic structure of the processes involved and correlated unobservables.

Our research also extends the growing body of work on the analysis of non-wage job amenities of work (see (Mas, 2025)), by establishing that mental health represents a quantitatively important dimension of employment and job quality. Our results inform the broader debate about the welfare effects of unemployment insurance and active labor market policies, and provide a foundation for understanding workplace determinants of employee wellbeing.

The rest of the paper is organized as follows. Section 2 describes the institutional contenxt and the data used, resulting from the combination of several administrative registers covering the universe of the Danish population. Section 3 presents descriptive statistics on the incidence of mental health medication by employment status and observable employee characteristics. Section 4 lays our econometric model and identification strategy. Section 5 discusses the empirical implementation while Section 6 presents our results. Section 7 finally concludes.

2 Institutional background and data

2.1 Mental healthcare in Denmark

Denmark provides universal, tax-financed healthcare that is – to a large extent – free at the point of use. General practitioners (GPs) act as gatekeepers, as most specialist and hospital care requires a GP referral (Olejaz et al., 2012). We describe below the institutional features of the Danish mental health care system that are most relevant for our analysis, and explain why prescription medication is our preferred measure of adverse mental health outcomes.

Clinical guidelines. Depression and anxiety disorders are the two most prevalent mental health conditions in the Danish adult population (OECD & European Observatory on Health Systems and Policies, 2023). Danish clinical practice for the management of these conditions is guided by national reference programmes, clinical guidelines, and pharmacological treatment recommendations.

For unipolar depression, the Danish Health Authority's reference programme (see page 47 Sundhedsstyrelsen, 2007b) and its national clinical guideline on non-pharmacological treatment (Sundhedsstyrelsen, 2019) outline a severity-graded treatment approach. For mild depression watchful waiting is advised, whereas moderate-to-severe depression warrants pharmacological treatment – typically with an antidepressant – either alone or in combination with psychotherapy. Danish treatment guidelines (Sundhedsstyrelsen, 2007b; RADS, 2015) recommends selective serotonin reuptake inhibitors (SSRIs) as first-line agents for moderate depression, with serotonin–noradrenaline reuptake inhibitors (SNRIs) and other antidepressants as second- or third-line alternatives, or first line treatment for severe depression. This severity-graded approach is broadly consistent with international guidelines, including those from the UK (NICE, 2022).

For anxiety disorders – including generalised anxiety disorder and panic disorder – the Danish national reference programme (Sundhedsstyrelsen, 2007a) typically recommends cognitive behavioural therapy or antidepressant medication (primarily SSRIs) as first-line pharmacological treatment when medication is indicated. SSRI medication thus serves as a first-line pharmacological treatment to both major classes of common mental disorder.

Access to publicly subsidised psychotherapy. Under the public psychologist scheme, GPs can refer patients to an authorised, clinical psychologist for psychotherapy treatment with a partial public subsidy – currently covering approximately 60 percent of the session fee.[1]

[1]The residual co-payment is approximately DKK 400 per session.

A referral entitles the patient to up to 12 subsidised psychotherapy sessions.[2] The referral must generally be made within 6 months of the triggering event. A key feature of the scheme is that substantial waiting times are common. The number of psychologists holding a public agreement (*ydernummer*) is capped, and each agreement holder faces an annual quota on publicly subsidised patients. The quotas for depression and anxiety referrals tend to be exhausted quickly, resulting in particularly long waiting lists for these conditions. In 2021, average waiting times for publicly subsidised psychologist treatment averaged 15-17 weeks depending on the reason for referral (Jacobsen et al., 2023).

Treatment through private health insurance and observability. Beyond the public psychologist scheme, patients can access therapy through private health insurance or direct out-of-pocket payment. Private supplementary health insurance, typically employer-provided, has expanded rapidly since a tax exemption for employer-paid premiums was introduced in 2002. By the mid-2010s, approximately 30 percent of the Danish population held supplementary insurance, with the vast majority of policies obtained through employers in the private sector (Vrangbaek, 2020; Olejaz et al., 2012).[3] These supplementary policies typically cover consultations with psychologists and psychiatrists, often with short waiting times and low or no co-payment.

Crucially, however, our administrative health registers only record publicly funded or publicly subsidised health services.[4] Therapy sessions funded entirely through private insurance or paid out of pocket are not captured in these data. Since employer-provided insurance is the dominant route to privately funded therapy, the employed population has systematically better access to *unobserved* psychotherapy. This creates two problems for using therapy as an outcome variable. First, a *measurement* problem: we would undercount therapy use among the employed relative to the unemployed, because the privately funded sessions of the employed are not in our data. Second, a *selection* problem: the availability of employer-provided insurance is itself a function of employment status, so any therapy-based measure would confound the mental health effect of employment with differential access to

[2]Patients referred for mild to moderate depression or anxiety can receive up to 24 sessions. Referral requires that the patient falls within one of eleven qualifying categories, which include: victims of robbery, violence, rape, or incest; traffic or other accident victims; close relatives of persons with severe mental illness, debilitating disease, or who have deceased; persons who had a late abortion; persons who have attempted suicide; persons suffering from mild to moderate depression; and persons with mild to moderate anxiety, including OCD. See the referral guidelines at https://www.sundhed.dk/borger/patientrettigheder/tilskud-stoettemuligheder/gratis-behandling-og-tilskud/psykologhjaelp/.

[3]Data from the Danish insurance industry indicate that psychologists, psychiatrists, and related services accounted for approximately 10 percent of all supplementary insurance reimbursements in 2009, behind surgery (67 percent) and physiotherapy and related services (17 percent) (Seiersen et al., 2011).

[4]With the exception of hospital treatment, where private hospital treatment is also reported in the National Patient Registry (LPR).

treatment.

Access to prescription medication. In Denmark, all prescription drugs – whether prescribed by a GP, a private psychiatrist, or a hospital specialist – must be dispensed at community pharmacies. The National Prescription Registry records every prescription redeemed at any Danish community pharmacy, using data from the pharmacies' electronic dispensing systems (Pottegård et al., 2017; Wallach Kildemoes et al., 2011). This registration is universal: it does not depend on the patient's insurance status, the prescriber's public or private affiliation, or whether the visit that generated the prescription was publicly funded.[5] The drug reimbursement scheme is likewise universal: all residents are automatically covered regardless of employment or insurance status, with the public subsidy share increasing in the patient's cumulative annual expenditure and out-of-pocket costs capped at DKK 4,850 per year in 2026-levels.[6]

Prescription medication therefore provides an outcome measure that is (i) recorded with near-complete population coverage regardless of insurance status, (ii) subject to low access barriers in terms of cost and waiting time relative to therapy, and (iii) available at a high frequency (every single prescription), providing a continuous measure of mental health status rather than a count of discrete therapy sessions. By contrast, a therapy-based measure would be selectively observed – with privately funded sessions invisible in our data precisely for those individuals whose employment status provides insurance-based access to therapy – making it unsuitable for our research design.

Comprehensiveness of the medication measure. An important question is whether prescription medication captures a sufficiently broad share of adverse mental health episodes. Clinical guidelines recommend antidepressant medication as a first-line treatment for moderate to severe depression, and antidepressants are also indicated for anxiety disorders, OCD, and post-traumatic stress disorder. Using survey data linked to medication records across 19 European countries, Chen et al. (2022) estimate that, among individuals aged 50 and over who screen positive for depressive symptoms, the share receiving antidepressant or anxiolytic medication averaged 38.7 percent across Europe in 2015–2018, with substantial cross-country

[5]The three exceptions where drugs are not captured by the pharmacy-based prescription registry are: (i) drugs administered during hospital inpatient stays; (ii) drugs dispensed directly by hospitals or treatment centres (e.g. certain antipsychotics provided free of charge to outpatients with schizophrenia); and (iii) drugs for certain institutionalised individuals. See Pottegård et al. (2017) and Jensen et al. (2021) for details on the registry's coverage.

[6]The first DKK 1,135 of annual drug expenditure is paid by the patient, after which the reimbursement rate increases in steps from 50% to 75% to 85%, reaching 100% above the cap. See the Danish Medicines Agency: https://laegemiddelstyrelsen.dk/en/reimbursement/reimbursement-thresholds/.

variation. For Denmark, the corresponding figures ranged from approximately 28 to 66 percent depending on age group and survey wave, with a significant increase over the study period.[7] These figu res suggest that prescription medication captures a substantial – but not complete – share of depressive episodes, with the remainder treated through psychotherapy alone, through GP counselling, or left untreated. Our measure is broader than antidepressants alone, as it additionally includes anxiolytics and psychostimulants – together covering the main pharmacological responses to depression, anxiety, and related conditions.

2.2 Data

We combine data from several Danish administrative registers covering the universe of the resident population from 2008 to 2022. All registers are maintained by Statistics Denmark and can be linked at the individual level through the personal identification number (CPR number) assigned to every resident. The main advantage of these data is that we observe both worker-firm linkages and healthcare participation for all individuals, as well as the usual set of demographics.

We aim to detect changes in mental health associated with job and employment changes. We observe employment outcomes at monthly frequency, including information on earnings, hours worked, occupation, and a firm identifier (the CVR number). Whenever an individual works at multiple firms within a month, we focus on the primary employer, i.e. the firm at which they work the most hours.[8] We obtain firm-level measures of mental health by computing leave-out means of employee mental-health medication.[9]

We define employment as having positive working hours in a given month (equivalently, positive wage income, as the two coincide in the Danish data) and unemployment as having zero working hours. Months in which an individual receives parental leave benefits, sickness benefits, or student benefits – identified through the OF register, which records monthly public transfer receipts from 2007 onward – are excluded from both states, so that each remaining individual–month observation is classified as either employed or unemployed.

Our main outcome variable – mental-health related medication – is constructed from the National Prescription Registry (LMDB), which records every prescription purchase at any

[7] The SHARE survey instrument asks respondents whether they use "drugs for anxiety or depression" at least once a week. The denominator consists of individuals scoring above the clinical cut-off on the EURO-D depression screening scale. See Chen et al. (2022) for details.

[8] While information on employer-employee linkages at the annual frequency dates back to the early 1980s (Kleven et al., 2019; Morin, 2023), we focus on monthly-level data for a shorter sample period to reduce the incidence of measurement error that may arise from annual snapshots.

[9] The specific registers used are BEF for demographics, UDDA for educational qualifications, BFL for employment outcomes, OF for benefit receipt, and IDAP for unemployment incidence within each calendar year.

Danish community pharmacy (Pottegård et al., 2017; Wallach Kildemoes et al., 2011). For each prescription, we observe the date of purchase, the Anatomical Therapeutic Chemical (ATC) classification code, and the dispensed quantity. We define mental health medication as any prescription within the ATC groups N05 (psycholeptics – covering antipsychotics, anxiolytics, and hypnotics/sedatives) and N06 (psychoanaleptics – covering antidepressants and psychostimulants). Table A.1 in the Appendix provides a detailed breakdown by 5-digit ATC code. The main subcategories observed in our data are SSRIs (N06AB), other antidepressants (N06AX, N06AA), anxiolytics (N05BA, N05BB), hypnotics and sedatives (N05CF, N05CH, N05CD), antipsychotics (N05AH, N05AF, N05AX, N05AD, N05AA, N05AB), psychostimulants (N06BA), and lithium (N05AN).

To construct a continuous measure of medication use at the individual–month level, we convert each prescription into a treatment window. For each purchase, the window begins on the date of purchase and extends for a number of days equal to the dispensed quantity divided by the WHO Defined Daily Dose (DDD) for the relevant ATC code.[10] For example, a prescription of 1.8 grams of fluoxetine (an SSRI with a DDD of 20 milligrams) translates into a treatment window of 90 days. When consecutive prescriptions for the same individual and ATC group overlap or follow each other without interruption, we link them into continuous medication spells.

Our binary outcome variable M_{it} equals one if individual i has an active treatment window for any mental health medication in calendar month t, and zero otherwise. This approach avoids misclassifying months in which a previously purchased medication is still being consumed as treatment-free.

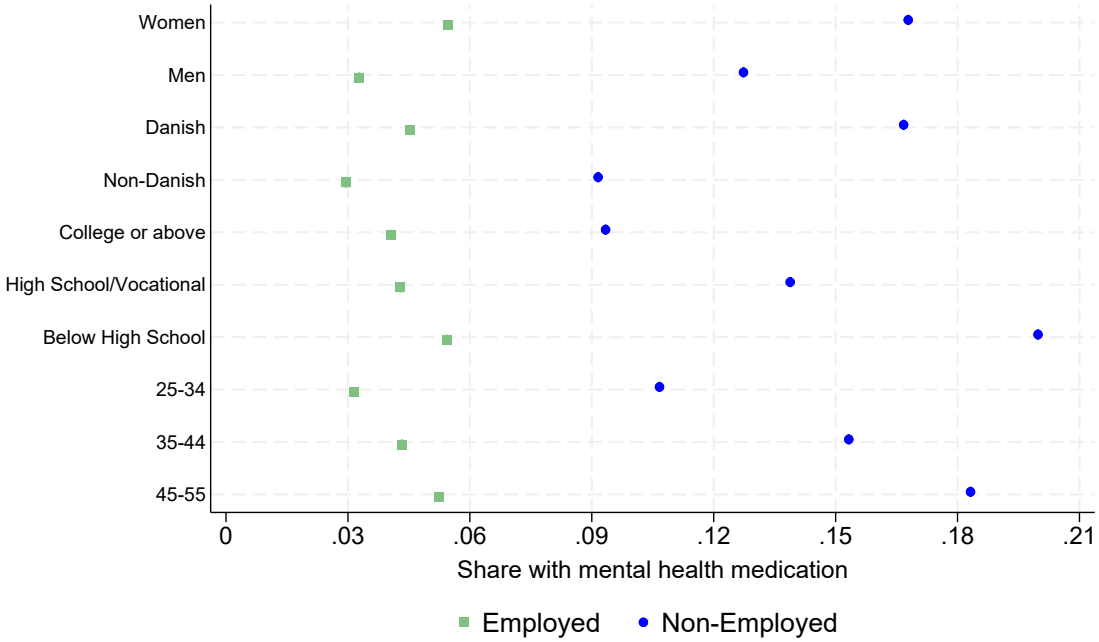
Other medical data. We additionally observe all visits to privately practising health professionals reimbursed by the Danish health authorities, including GPs and specialists such as psychologists and psychiatrists. In Denmark, specialist physicians usually treat patients who do not need inpatient care but still need more specialist care than that provided by GPs. We also observe hospital contacts – both outpatient and inpatient – with the ability to distinguish somatic from psychiatric wards. These data allow us to observe publicly subsidised psychotherapy and psychiatric treatment. While we do not use these data as primary outcome variables, they inform our descriptive analysis of the healthcare system and are utilised in robustness exercises.

[10]The DDD is the assumed average maintenance dose per day for a drug used for its main indication in adults, as defined by the WHO Collaborating Centre for Drug Statistics Methodology. See https://atcddd.fhi.no/.

3 Descriptive evidence

Figure 1 shows evidence on the incidence of medication by demographics and employment status. Employment includes any spell with positive wage income, while unemployment includes spells without wage income, parental leave benefits, sickness benefits, or student benefits. For all categories of employed individuals, the incidence of medication ranges between 3% and 6% and tends to be higher among women, Danish-born, low educated, and elderly individuals. Among the unemployed, incidence of medication is much higher for all groups, between 9% and 21%. Despite variation across demographics, the dimension that mostly stands out in the association with mental health is unemployment. Figure **??** additionally shows that the differential in medication between the employed and the unemployed is visible for each detailed cathegory of medication.

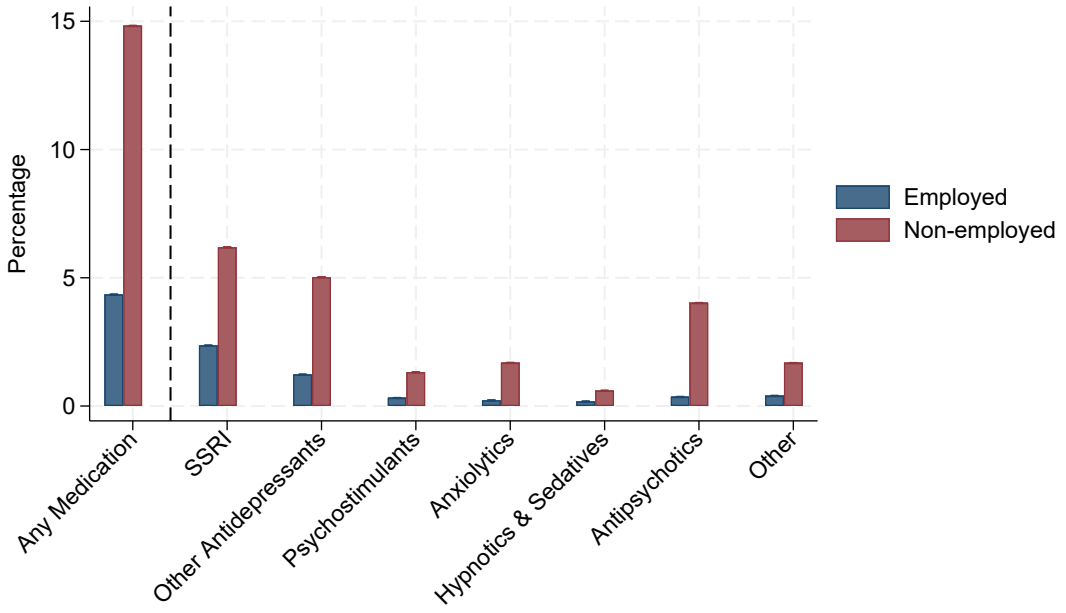
Figure 1: Mental health medication by demographics and employment status



Notes: The Figure plots the share of individuals on mental health medication in a given month, by demographic group and employment status. Employment includes any spell with positive wage income, while unemployment includes spells without wage income, parental leave benefits, sickness benefits, or student benefits. Sample: men and women aged 25-55; monthly observations over 2008–2022.

Across employed individuals, Figure 3 documents variation in mental health medication across firm and job characteristics, disaggregated by gender. A consistent pattern across all employment partitions is that the incidence of medication is substantially higher among women than men. Firm gender composition is also correlated with mental health outcomes

Figure 2: Detailed mental health medication incidence – By employment status



Notes: Bars represent the percentage of employed and unemployed individuals on each type of medication.

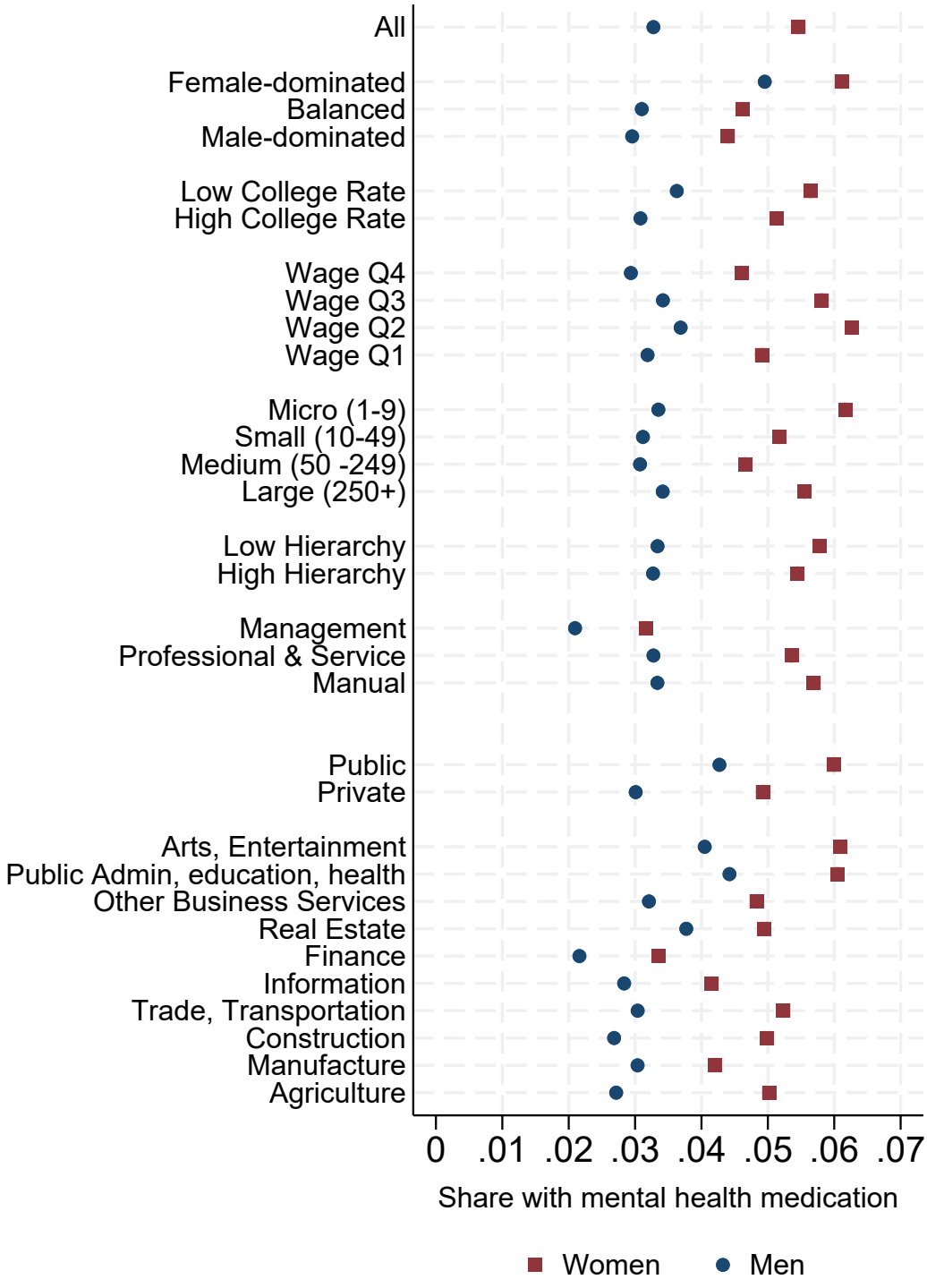
beyond purely mechanical effects: both male and female employees exhibit better mental health in male-dominated firms.

Certain job characteristics are associated with improved mental health. In particular, individuals in managerial occupations and those employed in the private sector display lower rates of medication relative to their counterparts. There is also some heterogeneity across industries, with segments of the service sector exhibiting comparatively worse mental health outcomes than other parts of the economy. In contrast, most other dimensions of firm-level variation – including firm-wage quartiles – are relatively modest in magnitude and generally smaller than the observed gender differences. Given that many of these firm characteristics are correlated, we turn later to multivariate regression analysis of firm-level mental health.

Turning to the firm-level distribution of mental-health incidence, Figure 4 shows that, in 80% of firm, at most 10% of employees are on mental health medication (and roughly half of them have zero incidence). The share of firms declines very rapidly for higher levels of medication, but the distribution includes a long right-tail of (very few) firms with very high incidence of medication.

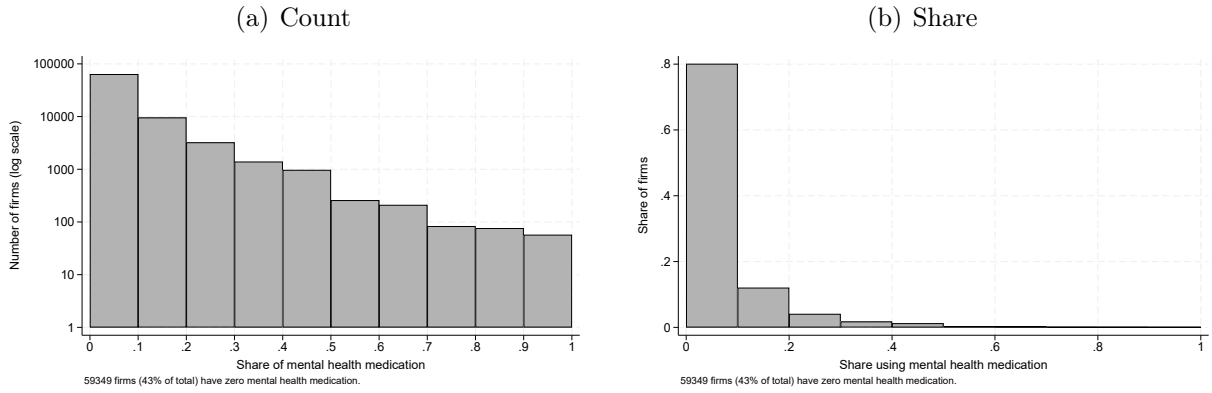
To probe the persistence of mental health conditions at the individual level, Figure 5 plots the survivor function on medication for those starting a spell after at least 12 months without medication – thus focusing on a major change in underlying conditions, rather than short-term movements in and out of medication. Following initiation, usage declines by

Figure 3: Mental health medication and firm characteristics



Notes: The Figure plots the share of employed individuals on mental health medication in a given month, by firm and job characteristics. Sample: employed men and women aged 25-55; monthly observations over 2008–2022.

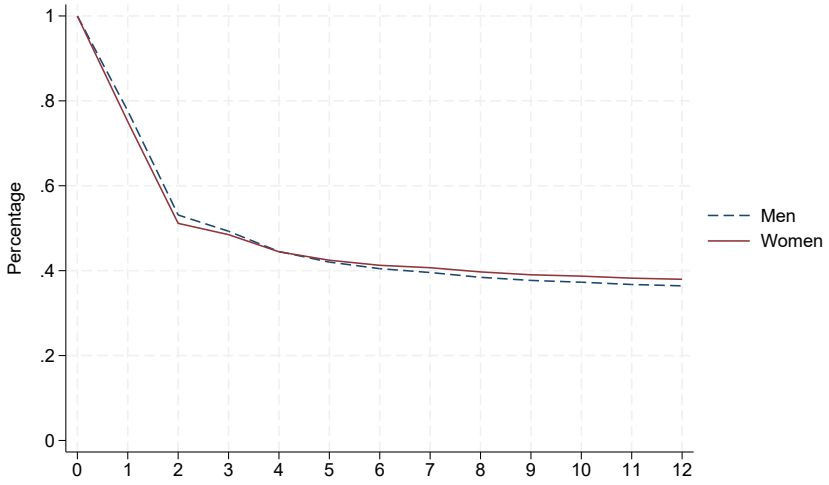
Figure 4: Distribution of firm-level incidence of mental health medication



about one half during the first two months, with very high persistence rates after that. After 12 months, slightly less than 40% of individuals are still on medication, with very similar patterns across genders. See also Figure A.1 in the Appendix for additional detail on the duration distribution of medications spells.

The pattern shown in the Figure reflects changes in the composition of survivors by underlying mental health conditions. For anxiety-related medication, usage drops sharply to about 15% within the first two months. By contrast, between 50%-60% of patients on antidepressants stay on medication for at least 12 months (see Figure A.2 in the Appendix for detail).

Figure 5: The survivor function of mental health medication



Notes: The Figure plots the survivor function of medication for those starting a new medication spell after at least 12 months without medication.

We next focus on changes in medication around the time of transitions in and out of

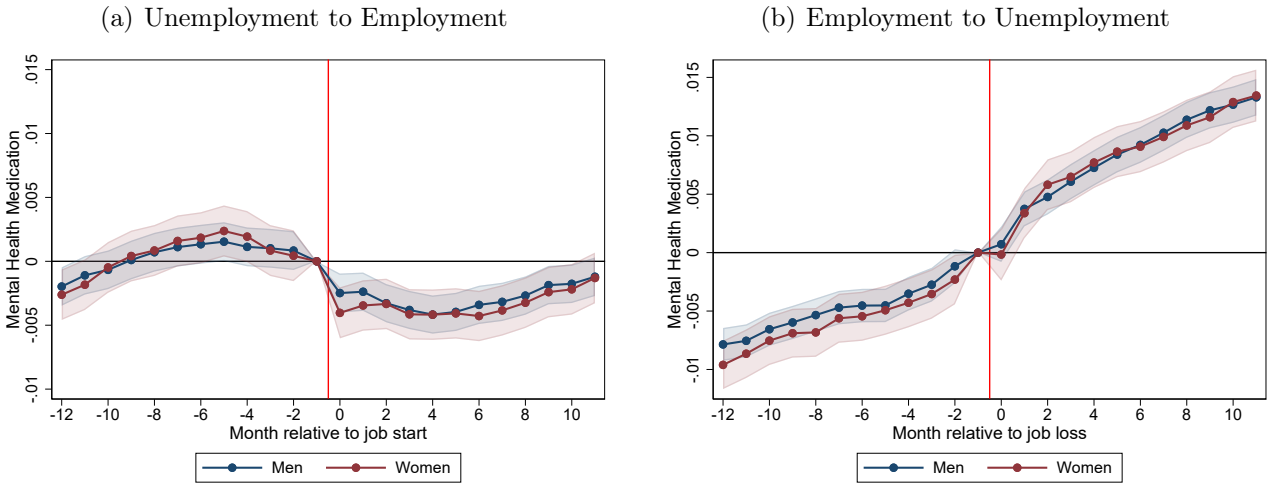
unemployment. Figure 3(a) in the Appendix shows average medication rates around a transition from unemployment to employment (U-to-E), tracking individuals from 12 months before to 12 months after a job start (on a fully balanced panel centered around the time of a transition). We restrict the sample to those continuously out of work for at least 6 month up to month 0, and continuously employed for at least 6 months afterwards. Both men and women exhibit fairly stable medication rates while unemployed, just about 10% among women and around 7% among men. Medication rates declines by about half of a percentage point upon job finding, remaining fairly stable afterwards. Figure 3(b) shows trends in medication around the reverse E-to-U transition. Medication rates increase gradually in the year leading to job loss, and continue rising on a steeper trend after job loss.

To better visualize these patterns, we estimate unconditional differences in medication rates relative to lead -1:

$$M_{it} = \beta + \sum_{j=\underline{j},\neq -1}^{\bar{j}} \beta_j \cdot \mathbb{1}(t - t_{i0} = j) + u_{it} \tag{1}$$

where $M = 0/1$ denotes (any) mental health medication, i and t denote individuals and calendar months, respectively, and t_{0i} denotes the month in which individual i transitions in or out of employment.

Figure 6: Mental Health Treatment Around Employment Transitions



Notes: This figure presents estimates of unconditional differences in mental health treatment rates around employment transitions. Panel (a) shows the transition from unemployment to employment, with month 0 marking job start. Panel (b) shows the transition from employment to unemployment, with month 0 marking job end. The sample excludes individuals on parental leave, sickness leave, and students. Coefficients represent the estimated difference in treatment rates relative to the reference period (month -1). 95% confidence intervals indicated.

The estimates shown in Figure 6. Panel 6(a) focus on transitions of out unemployment and confirms the pattern observed in the raw means: job finding is associated with an immediate decline in medication rates, around 0.2-0.3 percentage points for both men and women, with the effect persisting for nearly one year. Ahead of transition, medication rates are slightly hump-shaped, but mot of the estimates are not significantly different from zero. Panel 6(b) focuses on transitions into unemployment and reveals a deterioration in individuals' mental health in the year preceding job loss, though the incidence of medication clearly accelerates upon job loss and remains on a steeper trend during the unemployment window.

4 Econometric Model

This Section formalizes our identification strategy. The goal is to estimate the causal impact of employment on mental health. We proxy mental health conditions by the use of medication, M_{it}, a binary variable denoting whether individual i receives any mental-health related medication at time t. Employment status, E_{it}, is also binary, denoting whether individual i is working a positive number of hours at time t. In the empirical analysis we will additionally consider heterogeneous employment outcomes.

A simple comparison of medication rates between employed and unemployed individuals is unlikely to provide causal estimates. First, employment is not randomly assigned: employed individuals may differ from those who remain unemployed in ways that are difficult to observe, including for example their motivation, social support, resilience, severity of symptoms, local networks, etc. If these unobserved factors affect both employment and mental health, naive comparisons will confound the causal effect of employment with selection.

Second, mental health treatment is persistent and can influence future employment. For example, individuals who received medication in the past may have different underlying health trajectories, and this can affect their job-finding prospects. This creates a mechanical link between current employment and medication history. Even if contemporaneous unobservables were irrelevant, employed and unemployed individuals would still differ in their respective mental health histories, which could affect their current mental health.

Our empirical strategy is built around two ideas that address these two sources of bias. First, we match treatment and control groups on mental health histories, which caters for their compositional differences due to medication history (in the spirit of (Sullivan & Card, 1988)). Second, we implement a "future movers" design, which makes the treatment and control groups more comparable to one another in terms of hard-to-observe employability factors (in the spirit of (Fadlon & Nielsen, 2021)).

We use a simple latent-variable model to formalize these ideas and to illustrate why the combined estimator is expected to have smaller bias than alternative matching estimators. Appendix A describes our approach in detail. The remainder of this Section summarizes the main steps.

4.1 Model primitives

We observe two binary outcomes for individuals over time: mental health $M_{it} \in \{0, 1\}$, where $M_{it} = 1$ denotes use of medication, i.e. "poor" mental health, and employment, $E_{it} \in \{0, 1\}$. Both outcomes are assumed to arise from underlying latent propensities that cross thresholds, allowing for a transparent discussion of selection and unobservables:

$$M_{it} = \mathbb{1}\{M_{it}^* > t_M\}, \qquad E_{it} = \mathbb{1}\{E_{it}^* > t_E\},$$

where t_M and t_E are the respective thresholds.

The latent mental health propensity evolves according to the following structural equations:

$$M_{it}^* = \psi + \kappa M_{it-1} + \omega E_{it} + u_{it}, \tag{2}$$

where κM_{it-1} captures persistence in mental health: those who took medication in the previous period are more likely to be take medication in the current period ($\kappa > 0$), all else equal; ωE_{it} denotes the effect of interest, namely the causal impact of employment on mental health; u_{it} is an unobserved shock that captures changes in mental health needs, access, preferences, and any other unobserved determinants of medication at time t.

The latent employment propensity is described by:

$$E_{it}^* = \phi + \gamma M_{it-1} + \delta E_{it-1} + v_{it}, \tag{3}$$

where δE_{it-1} captures persistence in employment status, while γM_{it-1} allows past mental health to affect current employability (whereby γ is expected to be negative), and v_{it} denotes an unobserved employability shock at time t.

This modeling structure implies two distinct sources of bias in naive mental-health comparison between the employed and the unemployed. First, if u_{it} and v_{it} are correlated, those more likely to become employed also tend to have systematically different unobserved determinants of mental health (*unobservables channel*). Second, even if u_{it} and v_{it} are uncorrelated, employment depends on past mental health (via γ) and mental health status is persistent (via κ). Therefore the employed and unemployed differ in their distribution of M_{it-1}, which mechanically affects M_{it} (*history/composition channel*).

To highlight the model's properties, we impose the following structure on the distribution of unobservables. We assume that unobserved shocks to employment and mental health are jointly normally distributed and we model their contemporaneous correlation through a simple factor structure

$$u_{it} = \rho v_{it} + \varepsilon_{it}, \qquad \varepsilon_{it} \sim N(0, \sigma_\varepsilon^2),$$

where the parameter ρ captures the sign and strength of their correlation. We additionally assume that shocks are intertemporally uncorrelated, i.e. $\varepsilon_{it} \perp v_{is}\ \forall s \neq t$. Finally, we allow employment shocks to be persistent over time:

$$v_{it} = \lambda v_{it-1} + \eta_{it}, \qquad \eta_{it} \sim N(0, \sigma_\eta^2), \qquad \lambda \in (0, 1).$$

where the assumed AR(1) structure captures the idea that employability (and the unobserved factors behind it) does not change arbitrarily from one period to the next. This is a parsimonious way to capture selection on unobservables while keeping the logic interpretable.

4.2 Estimators and intuition

A naive approach would compare medication rates at time t for those who find employment between $t-1$ and t (the treatment group) and those who remain unemployed (the control group).[11] In Appendix A we formalize this estimator as the difference in probit indices:

$$b_{SD} \equiv \Phi^{-1}\big(P(M_{it} = 1 \mid E_{it} = 1, E_{it-1} = 0\big) - \Phi^{-1}\big(P(M_{it} = 1 \mid E_{it} = 0, E_{it-1} = 0\big).$$

This estimator generally does not identify β for two reasons. The first concerns correlation in unobservables: if $\rho \neq 0$, those employed are selected on the employability shock v_{it}, which shifts the distribution of the mental health shock u_{it}. The second concerns composition effects: even if $\rho = 0$, employment depends on M_{it-1} via γ, and M_{it} depends on M_{it-1} via κ. As a result, the employed and unemployed groups differ in their mental health history, which mechanically affects current mental health.

To address the composition channel, we compare employed and unemployed individuals *within cells of past mental health status*. Specifically, we form groups defined by $M_{it-1} = j$, for $j \in \{0, 1\}$. Within each group, we compare those who find employment ($E_{it-1} = 0, E_{it} = 1$)

[11] Equivalently, one may defined as treated those who remain employed between $t-1$ and t and control those who leave employment.

to those who remain unemployed ($E_{it-1} = 0, E_{it} = 0$). This produces the matched estimator

$$b_M \equiv \sum_{j=0}^{1} \left[\Phi^{-1}(P^j_{10,t}) - \Phi^{-1}(P^j_{00,t})\right] fr_j,$$

where $P^j_{10,t}$ and $P^j_{00,t}$ are treatment probabilities within each history cell, and fr_j are the corresponding population shares.

This matching step removes bias that comes purely from differences in mental health history, though it does not resolve selection on unobservables: even conditioning on M_{it-1}, treatment and control groups at t may differ in their respective v_{it}. If $\rho \neq 0$, this implies remaining selection in u_{it}.

To address the unobservables channel, we refine the definition of the control group. Instead of comparing current movers ($E_{it-1} = 0, E_{it} = 1$) to those who remain unemployed at t, we compare them to individuals who are still unemployed at t, but will become employed at a later date $t + h$.

Intuitively, future movers are more similar to current movers in their underlying employability (and related unobservables) than individuals who stay unemployed for a long time. In the AR(1) environment, future employment is informative about current employability. In other words, $E_{it+h} = 1$ is more likely when v_{it} is high. The shorter the horizon Δ, the stronger this signal. Therefore, selecting "soon-to-be-treated" controls makes the control group closer to the treated group in unobservables such as v_{it} and, through ρ, in the treatment shock u_{it}.

Formally, for each history cell $M_{it-1} = j$ we define a future-mover control group:

$$S_{it,h}(j) \equiv \{E_{it-1} = 0,\ E_{it} = 0,\ E_{it+h} = 1,\ M_{it-1} = j\}.$$

Having denoted by $P^j_{00,h,t}$ the probability of treatment at time t for these future movers, the resulting estimator is

$$b^h \equiv \sum_{j=0}^{1} \left[\Phi^{-1}(P^j_{10,t}) - \Phi^{-1}(P^j_{00,h,t})\right] fr_j.$$

The appendix derives approximate expressions for the bias of b^h under mild regularity conditions. The key insight is that the future-mover restriction acts like a noisy signal of the current employability shock v_{it}. Because $v_{it+h} = \lambda^h v_{it} + \eta_{it,h}$, the signal becomes noisier as Δ grows (the loading factor λ^h shrinks). As a result, the future-mover control group becomes less and less similar to the treated group as the horizon increases. In the limit, very distant future movers those who stay unemployed, reproducing the standard matching estimator.

Under regularity conditions that guarantee interior probabilities and monotonicity of se-

lection with Δ, the appendix shows that the bias term is minimized at the shortest horizon, $\Delta = 1$. In other words, among the class of matching estimators that use future movers as controls, using immediate future movers yields the closest control group in terms of unobservables and therefore the smallest bias.

This result is not meant to claim that the estimator is perfectly unbiased in all settings; rather, it provides theoretical support for why the specific design used in the paper is particularly attractive, as it simultaneously addresses issues of history composition through exact matching on M_{it-1} and selection on unobservables through the future-movers comparison.

Our analysis will complement the model-based motivation with empirical transparency checks. A key implication of the identifying logic is that, after matching and with the future-movers design, treated and control groups should display similar patterns in outcomes prior to the employment transition. We therefore show pre-matching (and pre-treatment) trends in mental health in the periods before the employment change, providing an empirical diagnostic for the plausibility of the identifying assumptions. If treated and soon-to-be-treated individuals exhibit similar pre-matching trends in medication, this supports the claim that the design is balancing the relevant unobservables that drive treatment dynamics.

5 Empirical Implementation

Our approach involves a comparison between matched treatment and control groups who experience the same same employment transition within an interval Δ. The choice of Δ needs to balance two considerations. As discusses in the previous Section, a shorter lag enhances comparability between treatment and control groups, as their baseline characteristics and labor market environments are more similar. Conversely, a longer lag allows us to trace out longer-run dynamic effects, since our event study framework identifies treatment effects up to $\Delta - 1$ months following the initial transition. With these considerations in mind, we select as control units those individuals who experience the same transition as the treated units, between 12 and 36 months later. This choices allows us to observe post-treatment mental health effects up to a year post-treatment, while allowing for a relatively large window of 24 months for defining control-group transitions helps with sample size and precision.

We build a balanced panel of observations for each individual, covering multiple months before and after an employment transition. The pre-treatment period includes the months used for matching on observable characteristics and mental health medication, and an earlier, "unmatched" period, which allows us to assess differential pre-trends, if any. As we impose an exact matching on medication, gender and age in each of the matched months, a longer matching period becomes especially demanding in terms of sample sizes and has consequences

for statistical power. We settle on a matching period of 18 months, which provides an acceptable compromise between treatment/control comparability and statistical power. We additionally observe unmatched outcomes between 19 and 24 months before an employment transition. We will show post-treatment outcomes for the first 12 months following an employment transition.

We estimate the following event-study specification:

$$M_{it} = \sum_{j=\underline{j},j\neq -1}^{\overline{j}} \alpha_j \mathbb{1}(t - t_{i0} = j) + \sum_{j=\underline{j},j\neq -1}^{\overline{j}} \beta_j \mathbb{1}(t - t_{i0} = j)\mathrm{T}_i + \lambda_t + \mu_{it} + \mathrm{T}_i + \varepsilon_{it}, \quad (4)$$

where T_i indicates whether an individual belongs to the earlier ($T_i = 1$) or later-treated ($T_i = 0$) group, t_{i0} denotes the time of transition for the earlier-treated group and λ_t denotes calendar time effects. μ_{it} denotes age-in-years FEs. The month before the transition ($j = -1$) serves as the omitted reference category. The coefficients of interest are β_j, capturing the effect of experiencing an employment transition at each event time j relative to the counterfactual trajectory represented by the soon-to-be-treated control group. Under our identification assumptions, these coefficients estimate the dynamic treatment effects of the employment transition.[12] To complement the event-study analysis and obtain summary measures of the overall impact of employment transitions, we also estimate standard difference-in-differences models, based on the same definition of treatment and control groups.

5.1 Employment transitions

Our sample consists of individuals aged 25–55 who are strongly attached to the labour market, defined as experiencing their first observed unemployment spell during our sample period, which begins in 2008. Leveraging annual information on employment status available from 1980 onward, we further exclude individuals who spent more than 50% of any year between 1980 and 2008 in unemployment.

Our analysis focuses on "major" transitions between employment (E), defined as any spell with positive wage income—and unemployment (U), defined as spells without wage income, parental leave benefits, sickness benefits, or student benefits. For E-to-U transitions, we select individuals who start an unemployment spell lasting at least six months, following a period of at least six months of continuous employment.[13] This restriction ensures that

[12] General event-study specifications typically include individual fixed effects. As the coefficients of interest β_j, denoting within-individual changes in outcomes between time j and time -1, are identified by individuals observed j periods from the event, individual fixed-effects cater for changes in the sample composition throughout the observation window around an event. Because individuals included in our sample are observed for exactly the same set of observations around an event, individuals fixed-effects are not required.

[13] This selection criterion excludes individuals with spells of parental leave, sickness benefits, or student

the transition captures a meaningful disruption in labor market attachment, rather than short-lived or intermittent employment interruptions. Similarly, for U-to-E transitions, we select individuals who exit an unemployment spell of at least six months into an employment spell lasting at least six months. This ensures that job finding corresponds to the end of a substantive period of non-employment, rather than a brief gap between jobs.

5.2 Matching

Building on the empirical framework described in Section 4, we impose an exact match between treatment and control based on gender, 5-year birth-cohort blocks, and mental health medication history for the 18 months preceding the treated group's pivotal employment transition.

To equalize treatment and control overall weight within matching blocks, we reweigh treated units in each block according to the relative sizes of treatment and control. Specifically, for each matching block b defined by a combination of 5-year birth cohort $\times$ gender $\times$ pre-treatment medication:

$$\begin{cases} w_i^C = 1 & \text{for control units in } b \\ w_i^T = \dfrac{N_b^C}{N_b^T} & \text{for treated units in } b \end{cases} \tag{5}$$

with T and C denoting treatment and control, respectively. This ensures so that $\sum_{T_i \in b} w_i^T = \sum_{C_i \in b} 1 = N_b^C$. In all regressions that follow, we use the obtained w_i as sampling weights.

6 Results

6.1 Transitions between employment and unemployment

Figure 7 presents event-study estimates of the impact of employment transitions on mental health medication, based on specification (4), separately for men and women. Panel (a) focuses on E-to-U transitions. Estimates are (near) zero by construction in the 18-month period used for matching.[14] Before the matching period, the (unconstrained) estimates are clearly consistent with the parallel trend assumption. Estimates for the treated period show a gradual and significant increase in mental health issues, as proxied by the incidence of medication. Men who lose their jobs experience an increase in the likelihood of medication

benefits in the six months preceding job loss.

[14]The reason why these are not always exactly zero (and we obtain nondegenerate confidence intervals) lies in minor approximations in the reweighting procedure.

of nearly one percentage point within a year of job loss, over a baseline of about 3.6%. Women experience a higher absolute increase of 1.3 percentage points, over a higher baseline of 5.9 percentage points%.

In Panel (b), patterns of medication are remarkably symmetric and only marginally larger in magnitudes for those who find jobs. A year after finding employment, men and women experience a reduction of mental health issues of 1.1 and 1.4 points, respectively. As the unemployed have higher incidence of medication at baseline than the employed, the proportional benefit of employment in terms of mental health is slightly smaller than the proportional cost of unemployment.

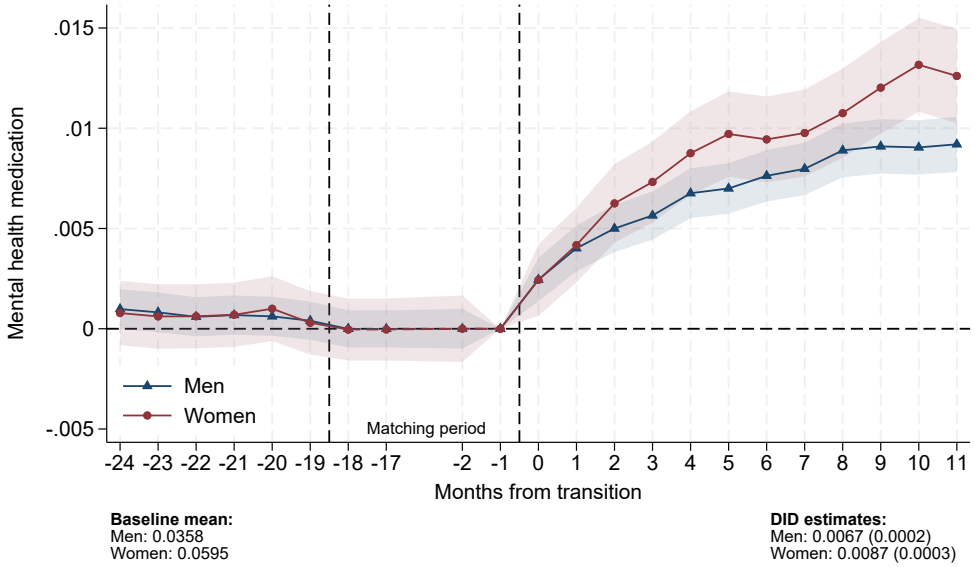
Proportional impacts are plotted in Figure 8, zooming in on the post-treatment period. Estimate show that employment transitions produce sizeable changes in mental health, which accumulate (at least) over the first year into the new employment state. Job loss produces an increase in the incidence of medication between 20%-25%, while job finding implies a reduction between 16%-20%. Proportional effects are slightly larger for men than for women, although gender differences are very imprecise.

As we do not directly observe individuals' mental health or wellbeing, but only their use of mental health–related medication, a potential concern is that our estimates may capture variation in health-seeking behavior rather than underlying health. For instance, employed individuals may have better access to relevant information and healthcare coverage. In addition, income effects may make them more willing to pay the (relatively small) co-payments required to obtain medication. If employment encourages greater health-seeking behavior for a given underlying condition, this mechanism would bias against finding negative effects of employment on medication use. Conversely, unemployed individuals may be more attentive to their overall health, including mental health, and/or may simply have more time to seek medical advice and treatment. In this case, the observed increase in medication use may reflect changes in health-seeking or self-care behavior, over and above any genuine changes in mental health status.

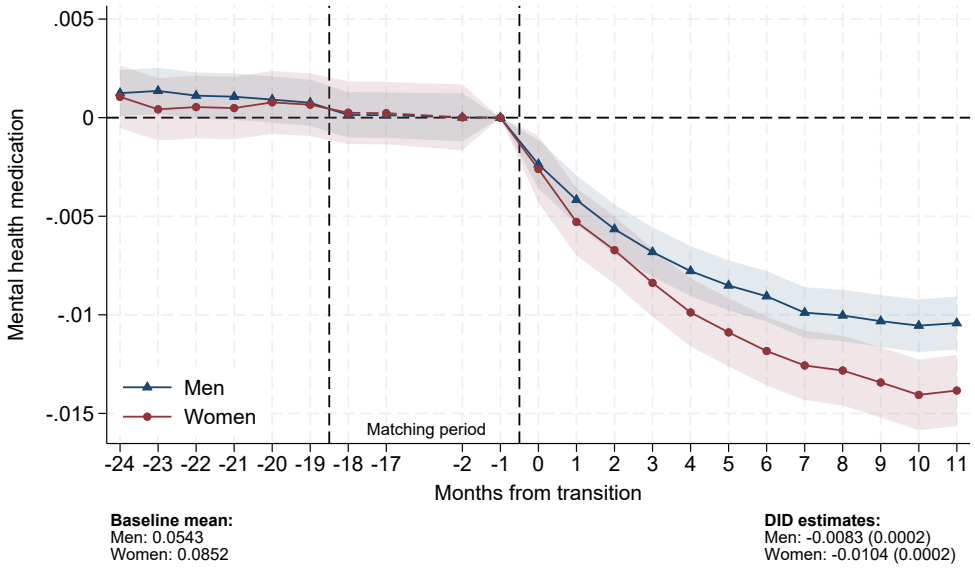
Based on the premise that changes in self-care behavior may concern several health conditions, as a falsification test we look into changes in medication for chronic conditions that are in principle unrelated to employment transitions, such as diabetes and hypertension. Panel (a) in Figure 9 shows that the take-up of medication related to diabetes is entirely unaffected by job loss. This speaks against an interpretation of the estimated effects on mental health medication based on changes in healthcare behavior rather than changes in underlying health conditions. Interestingly, Panel (b) considers medication for hypertension and all post-treatment estimates are not significantly different from zero, but display a mild upward trend. One possible interpretation is that long-term unemployment may indeed be

Figure 7: Employment transitions and mental health

(a) Employment-to-unemployment transition



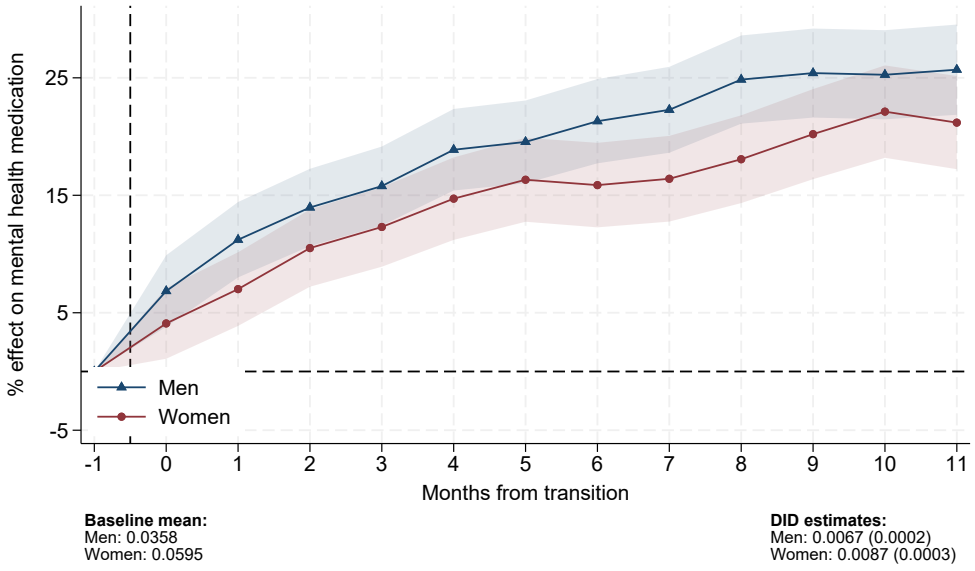
(b) Unemployment-to-employment transition



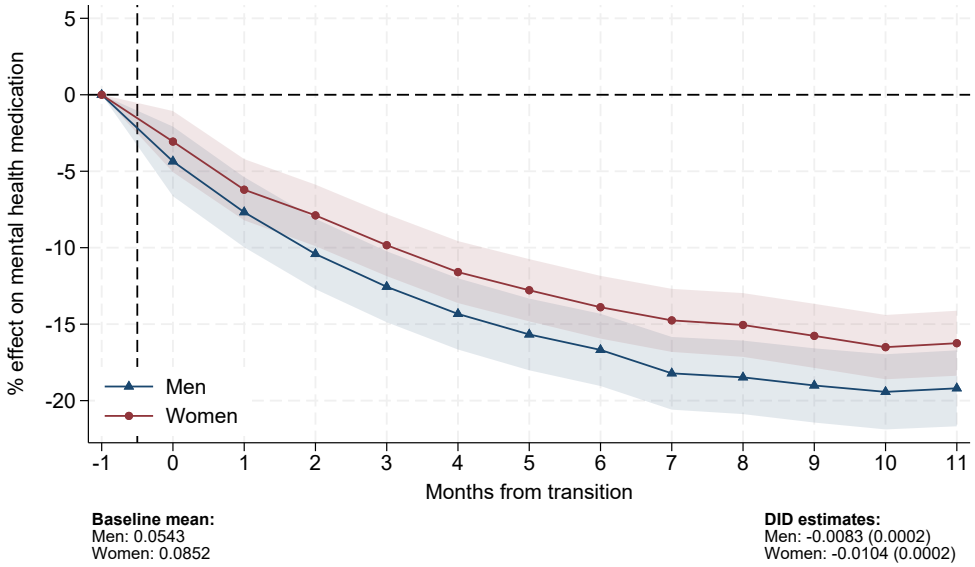
Notes: The Figure shows estimates of Equation (4) for the effect of employment transitions on mental health medication. The treatment group consists of individuals experiencing a transition at month 0, while the control group consists of individuals experiencing the same transition 12-36 months later, matched on gender, 5-year birth cohorts, and mental health medication history over an 18-month period prior to month 0. Regressions also include age and calendar month fixed effects. The shaded areas represent 95% confidence intervals. DiD estimates in the bottom-right represent the average treatment effect over the post-transition period. Baseline means in the bottom-left corner represent the incidence of medication at at $t = -1$.

Figure 8: Mental health and employment transitions (% Effect)

(a) Employment-to-unemployment transition



(b) Unemployment-to-employment transition

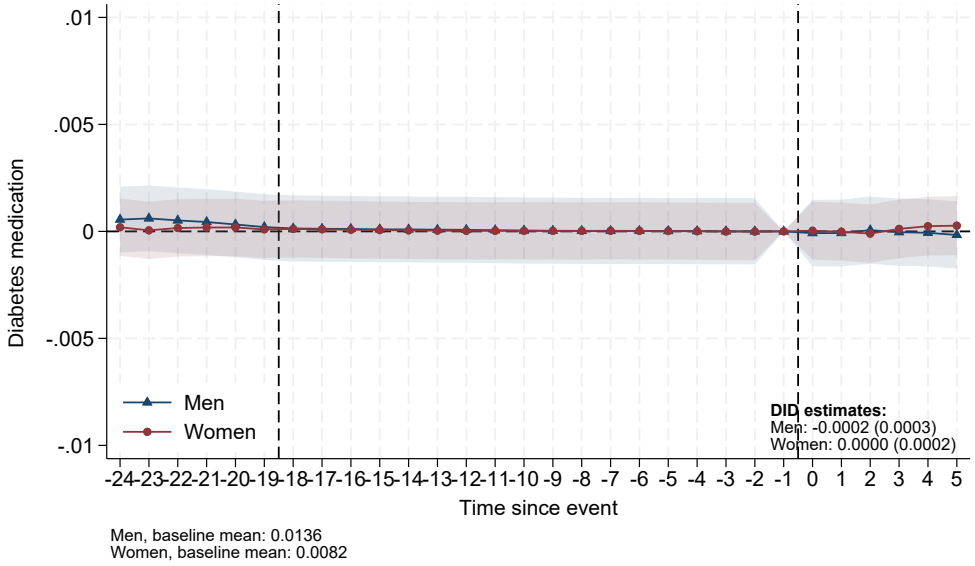


Notes: The Figure shows estimates of Equation (4) for the effect of employment transitions on mental health medication, expressed as a proportion of the incidence of medication at baseline. See notes to Table 7 for additional details.

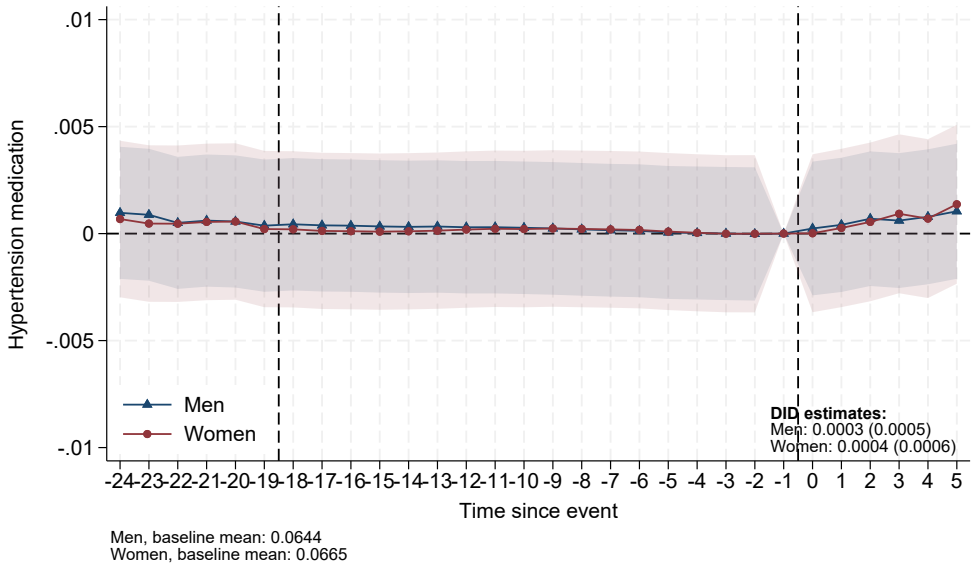
associated to higher incidence of hypertension.

Figure 9: Job loss, diabetes, and hypertension

(a) Diabetes-related medication



(b) Hypertension-related medication



Notes: The Figure shows estimates of Equation (4) for the effect of E-to-U transitions on medication related to diabetes (Panel (a)) or hypertension (Panel (b)). The treatment group consists of individuals experiencing a transition at month 0, while the control group consists of individuals experiencing the same transition 12-36 months later, matched on gender, 5-year birth cohorts, and mental health medication history over an 18-month period prior to month 0. Regressions also include age and calendar month fixed effects. The shaded areas represent 95% confidence intervals. DiD estimates in the bottom-right represent the average treatment effect over the post-transition period. Baseline means in the bottom-left corner represent the incidence of medication at at $t = -1$.

6.2 Heterogeneous effects

While the previous subsection has shown that employment has beneficial impacts on mental health, it is natural to posit that different working conditions and environments have heterogeneous impacts on the wellbeing of those employed.

To study this directly, we build a firm-level indicator of mental health as the average of mental health medication across its employees at a certain point in time and we group firms into quartiles of the workplace mental–health distribution. Given our measurement of mental health based on medication, the bottom quartile represents the healthiest firms, while the top quartile represents the least healthy.

We next distinguish, among E-to-U transitions, four cases of job separations from firms at each quartile of such distribution. In doing so, for each job leaver we obtain the leave-out mean mental health in their workplace in the month preceding a job separation, and assign their workplace to one of the quartiles of the firm-level distribution in the same month. Similarly, for U-to-E transitions, we obtain the leave-out mean mental health in the workplace joined by new hires, as of the first month after joining.

This procedure results into 8 types of transitions – from working in one of the four firm-level quartiles into unemployment, and from unemployment into one of the four firm-level quartiles. For each type of transition, the control group is represented by the set of (matched) individuals who make the same transition with a lag between 12-36 months.

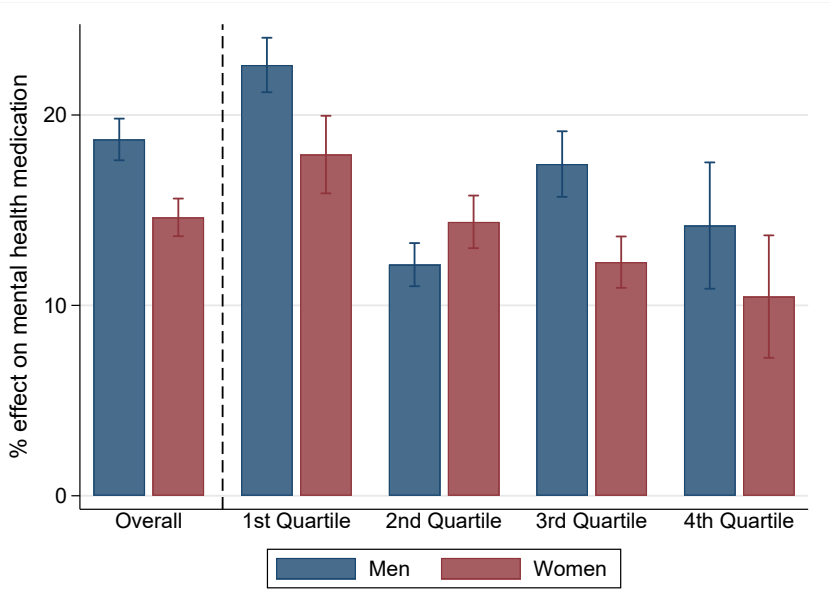
Figure 10 summarizes heterogeneous effects by reporting estimates of the *proportional* effects of job loss and job finding on mental health, overall and by quartile of the separating and hiring firm, respectively. The estimates reported are obtained from a DiD specification, representing the mean of the monthly estimates that would be obtained for the post-transition period in the corresponding event-study specification. The detailed event-study estimates are shown in Figures **??** and **??** in the Appendix.

Panel (a) plots heterogeneous impacts of job loss on mental health. The deterioration in mental health is evident at each quartile of the distribution of mental health in the separating firm. For both genders, the mental health cost of job loss decreases monotonically with the quality of the separating firm (with one exception for men between the 1st and 2nd quartile). In particular, the rise in medication among men following a separation from a firm in the bottom quartile is about 60% larger compared to a separation from a firm in the top quartile. For women, the difference is about 75%.

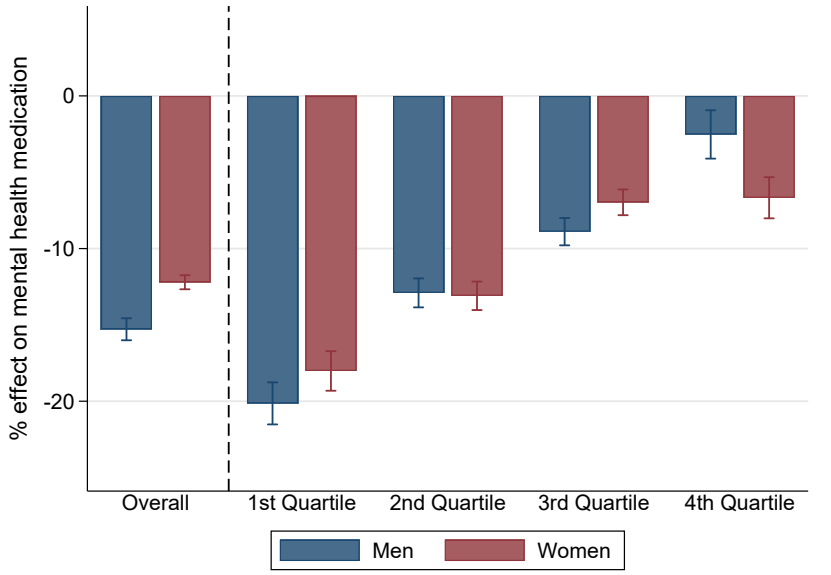
Panel (b) focuses on job finding. For both men and women, the mental health benefits of finding work increase monotonically with the average mental health of the hiring firm – although even firms in the least healthy quartile provide some significant improvement in mental health. For men, joining a firm in the bottom quartile implies a reduction in

Figure 10: Overview of Mental Health Treatment Effects by Firm Quartile

(a) Employment-to-Unemployment Transitions



(b) Unemployment-to-Employment Transitions



Notes: The Figure presents heterogeneous mental health effects of employment transitions across quartiles of the firm-level mental health distribution. Firm-level indicators of mental health are obtained as leave-out means. Bars represent DiD estimates over the post-transition period. The treatment group consists of individuals experiencing a transition at month 0, while the control group consists of individuals experiencing the same transition 12-36 months later, matched on gender, 5-year birth cohorts, and mental health medication history over an 18-month period prior to month 0. Regressions also include age and calendar month fixed effects. The vertical lines represent 95% confidence intervals.

medication that is about 8 times larger than joining a firm in the top quartile. For women, the corresponding reduction in medication is 4.5 time larger.

6.3 Firm-to-firm transitions

We finally look into changes in mental health associated with direct firm-to-firm transitions, without intervening unemployment, based on the usual matching procedure during the 18 months preceding a transition and the future movers design. The results are shown in Figure 11.

Panel (a) focuses on transitions from the lowest to the top quartile of mental-health medication. Men's mental health displays some deterioration when moving from the healthiest to the least healthy quartile of the firm distribution, though with relatively broad confidence intervals. For women, by contrast, we do not detect any significant change following such transition.

Panel (b) shows instead that upward mobility in the health distribution of firms produce fairly large and significant improvements in the mental health of individuals, of very similar magnitudes of those produced by transition from unemployment into any firm (see Panel (b) in Figure 7).

7 Conclusion

This paper provides causal evidence on the relationship between employment and mental health by exploiting variation in the timing of employment and firm-to-firm transitions in Danish administrative data. Our identification strategy, which combines soon-to-be-treated controls with matching on pre-transition mental health trajectories, reveals that (un)employment has substantial (negative) positive effects on mental health. We find that job loss triggers a gradual increase in the incidence of mental health medication, up to 20-25% within a year from job loss, while job finding produces a symmetric decline of similar magnitude. In comparison, Jensen & Zhang (2023) find an 11-12% increase in the uptake of mental health prescriptions following the severe family shock of the death of a parent. This highlights the large relative importance of employment for mental health.

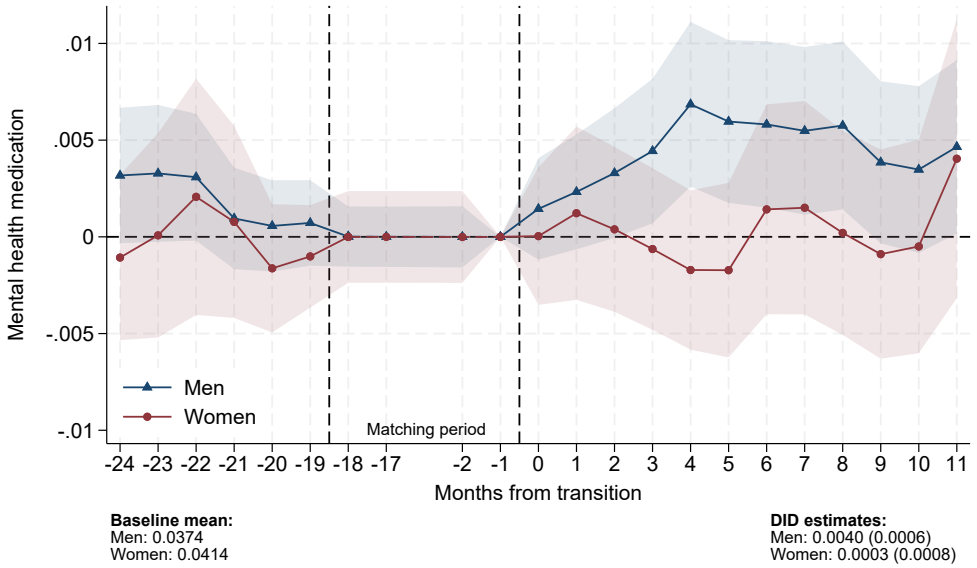
Our heterogeneity analysis uncovers an important dimension of firm effects that has been largely overlooked in the matched employer-employee literature. While previous work has extensively documented firm-specific wage premia, we show that firms also differ substantially in their effects on worker mental health. Notably, the mental health benefits of job finding improve monotonically with the quality of the hiring firm, as proxied by firm-level mental

health incidence. Symmetrically, the costs of job loss are substantially lower when separating from a work environment conducive to poor mental health.

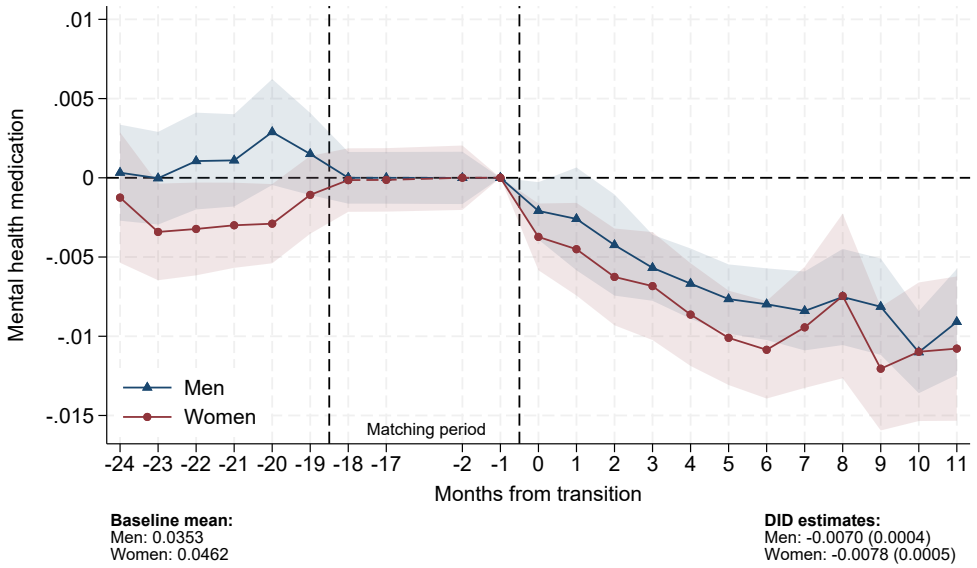
Future work will investigate the specific firm characteristics and management practices that drive these mental health differentials. Understanding whether factors such as work intensity, job flexibility, or workplace culture explain firm-level variation in mental health outcomes could inform both firm-level interventions and regulatory approaches to improving worker wellbeing.

Figure 11: Firm-to-firm moves and mental health

(a) Q1 to Q4



(b) Q4 to Q1



Notes: The Figure shows estimates of Equation (4) for mental health effects of firm-to-firm transitions. Panel (a) shows transitions from Q1 to Q4 firms (healthiest to least healthy), and Panel (b) shows Q4 to Q1 transitions (least healthy to healthies). The treatment group consists of individuals experiencing a transition at month 0, while the control group consists of individuals experiencing the same transition 12-36 months later, matched on gender, 5-year birth cohorts, and mental health medication history over an 18-month period prior to month 0. Regressions also include age and calendar month fixed effects. The shaded areas represent 95% confidence intervals. DiD estimates in the bottom-right represent the average treatment effect over the post-transition period. Baseline means in the bottom-left corner represent the incidence of medication at $t = -1$.

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A Appendix: Econometric model

A.1 Introduction and Overview

We provide below a theoretical foundation for our identification strategy. Our objective is to estimate the causal effect of employment on mental health treatment. A simple comparison in mental health treatment among those in employment and not in employment is unlikely to provide causal estimates of impact.

A first challenge is that people who find jobs may differ systematically from those who do not, and the determinants of the probability of job finding are likely correlated with the probability of undergoing mental health treatment. The sign of the correlation is likely ambiguous. On the one hand, those with poor mental health and hence likely to undergo mental health treatment are less likely to be in employment. This is for example if poor mental health is associated with behaviors that negatively affect effective job search, such as substance or alcohol abuse. On the other hand, mental health treatment might be more likely among high socioeconomic status and highly educated individuals, who have better access and perhaps display lower stigma towards medications, and these individuals are more likely to be in employment. A second source of bias comes from mental health treatment affecting job finding rates. Again, this effect, and hence the ensuing bias, in simple comparisons of means, is ambiguous, as better mental health and hence lower propensity to access medications likely causes better employment. Insofar as mental health treatment is restorative, however, the reverse might be true.

In order to tackle these two sources of potential bias, in the paper we combine two strategies. First we rely on a matching procedure that conditions on mental health treatment history in comparisons of treated and control observations. The approach is similar to the matching procedure in Sullivan & Card (1988). As matching estimates still potentially suffer from unobserved heterogeneity, we further integrate this approach with an estimator that compares the treatment group, namely those who transition from unemployment to employment, to a control group made of individuals who make the same transition in a very close future window. The approach is similar to Fadlon & Nielsen (2021).

A.2 Model Setup and Definitions

A.2.1 Primitives

We observe individual i at time t. There are two binary outcomes.

Mental health treatment. $M_{it} \in \{0, 1\}$ indicates whether individual i receives mental health medications at time t.

Employment status. $E_{it} \in \{0, 1\}$ indicates whether individual i is employed at time t.

A.2.2 Latent Variable Structure

We assume that both binary outcomes are generated by underlying continuous latent variables crossing thresholds.

Latent mental health treatment propensity. M_{it}^* is a continuous latent variable representing individual i's propensity to receive mental health treatment at time t. The observed outcome is determined by:

$$M_{it} = \mathbb{1}\{M_{it}^* > t_M\},$$

where t_M is a threshold parameter and $\mathbb{1}\{\cdot\}$ is the indicator function.

Latent employment propensity. E_{it}^* is a continuous latent variable representing individual i's propensity to be employed at time t. The observed outcome is determined by:

$$E_{it} = \mathbb{1}\{E_{it}^* > t_E\},$$

where t_E is a threshold parameter.

A.2.3 Structural Equations

Mental health equation.

$$M_{it}^* = \psi + \kappa M_{it-1} + \omega E_{it} + u_{it}. \tag{6}$$

Employment equation.

$$E_{it}^* = \phi + \gamma M_{it-1} + \delta E_{it-1} + v_{it}. \tag{7}$$

A.2.4 Distributional Assumptions

Assumption 1 (Normality)**.** *The shocks u_{it} and v_{it} are jointly normally distributed.*

Assumption 2 (Employment shock distribution)**.** *The employment shock v_{it} follows a stationary AR(1) process:*

$$v_{it} = \lambda v_{it-1} + \eta_{it},$$

where:

- $\lambda \in (0, 1)$ *is the persistence parameter,*
- $\eta_{it} \sim N(0, \sigma_\eta^2)$ *is an i.i.d. innovation,*
- *the process is stationary:* $v_{it} \sim N(0, \sigma_v^2)$ *where* $\sigma_v^2 = \sigma_\eta^2/(1 - \lambda^2)$.

Assumption 3 (Correlated unobservables)**.** *The mental health shock admits the factor structure:*

$$u_{it} = \rho v_{it} + \varepsilon_{it},$$

where:

- $\rho \neq 0$ *is the factor loading,*
- $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$ *is an idiosyncratic shock,*
- $\varepsilon_{it} \perp v_{is}$ *for all* s, t,
- $\sigma_u^2 = \rho^2 \sigma_v^2 + \sigma_\varepsilon^2$.

Lemma 1 (AR(1) decomposition)**.** *For any horizon* $\Delta \geq 1$*:*

$$v_{it+h} = \lambda^h v_{it} + \eta_{it,h},$$

where:

- $\eta_{it,h} \equiv \sum_{k=1}^{h} \lambda^{h-k} \eta_{it+k}$,
- $\eta_{it,h} \sim N(0, \sigma_{\eta,h}^2)$ *where* $\sigma_{\eta,h}^2 = \sigma_\eta^2 \sum_{k=0}^{h-1} \lambda^{2k}$,
- $\eta_{it,h} \perp v_{it}$.

A.2.5 Derived Quantities

Mental health treatment threshold. Given $M_{it-1} = j$, $j \in \{0, 1\}$:

$$A(j) \equiv -t_M + \psi + \alpha j.$$

With this definition, the treatment condition becomes:

$$\begin{aligned} M_{it} = 1 &\iff M_{it}^* > t_M \\ &\iff \psi + \kappa M_{it-1} + \omega E_{it} + u_{it} > t_M \\ &\iff u_{it} > t_M - \psi - \kappa M_{it-1} - \omega E_{it} \\ &\iff u_{it} > -A(M_{it-1}) - \omega E_{it}. \end{aligned}$$

Employment threshold. Given $M_{it-1} = j$ and $E_{it-1} = f$:

$$C(j, f) \equiv t_E - \phi - \gamma j - \delta f.$$

In the rest we restrict to the case $E_{it-1} = 0$ and follow individuals who transition from unemployment to employment. Similar derivations can be obtained for those making the opposite transition, namely $E_{it-1} = 1$. For $E_{it-1} = 0$:

$$C(j) \equiv C(j, 0) = t_E - \phi - \gamma j.$$

With this definition:

$$\begin{aligned} E_{it} = 1 &\iff E_{it}^* > t_E \\ &\iff v_{it} > C(M_{it-1}, E_{it-1}) = C(M_{it-1}). \end{aligned}$$

A.3 Naive Comparisons

A naive estimator compares the probability of mental health treatment among those employed to that among those not employed.

Definition 1 (Naive estimator)**.**

$$b_{SD} \equiv \Phi^{-1}(P(M_{it} = 1 \mid E_{it} = 1, E_{it-1} = 0)) - \Phi^{-1}(P(M_{it} = 1 \mid E_{it} = 0, E_{it-1} = 0)).$$

Proposition 1. *The naive estimator does not recover the parameter of interest.*

See below for proof.

A.4 Exact Matching on Past Mental Health Treatment

Let us condition on $M_{it-1} = j$ for some $j \in \{0, 1\}$ and define

$$S_{it}(j) \equiv \{E_{it} = 1,\ E_{it-1} = 0,\ M_{it-1} = j\}.$$

Define the conditional treatment probability for the treatment group:

$$P^{j}_{10,t} \equiv P(M_{it} = 1 \mid S_{it}(j)).$$

Similarly, for the control group define

$$S^{-}_{it}(j) \equiv \{E_{it} = 0,\ E_{it-1} = 0,\ M_{it-1} = j\},$$

and

$$P^{j}_{00,t} \equiv P(M_{it} = 1 \mid S^{-}_{it}(j)).$$

Definition 2 (Matched difference estimator)**.**

$$b_M \equiv \sum_{j=0}^{1} \left[\Phi^{-1}(P^{j}_{10,t}) - \Phi^{-1}(P^{j}_{00,t})\right] fr_j,$$

where $fr_j(M_{it-1} = j)$ and $\sum_{j=0}^{1} fr_j = 1$.

Proposition 2. *The matching estimator does not recover the parameter of interest when* $\rho \neq 0$.

Proof. **Proof of Propositions 1 and 2.**

Throughout, when conditional variances are small relative to σ^2_ε, we use a first-order (delta-method) approximation:

$$\mathbb{E}[\Phi(a + bv) \mid K] \approx \Phi(a + b\,\mathbb{E}[v \mid K])\,,$$

which becomes exact as $\mathrm{Var}(v \mid K) \to 0$.[15]

Step 1: matched estimator. Let

$$\mu_j \equiv \mathbb{E}[v_{it} \mid S_{it}(j)], \qquad \mu^{-}_j \equiv \mathbb{E}[v_{it} \mid S^{-}_{it}(j)].$$

Given $u_{it} = \rho v_{it} + \varepsilon_{it}$ with $\varepsilon_{it} \perp v_{it}$,

$$P^{j}_{10,t} = \mathbb{E}\left[\Phi\left(\frac{A(j) + \beta + \rho v_{it}}{\sigma_\varepsilon}\right) \;\middle|\; S_{it}(j)\right] \approx \Phi\left(\frac{A(j) + \beta + \rho\mu_j}{\sigma_\varepsilon}\right), \tag{8}$$

and

$$P^{j}_{00,t} = \mathbb{E}\left[\Phi\left(\frac{A(j) + \rho v_{it}}{\sigma_\varepsilon}\right) \;\middle|\; S^{-}_{it}(j)\right] \approx \Phi\left(\frac{A(j) + \rho\mu^{-}_j}{\sigma_\varepsilon}\right). \tag{9}$$

[15] A second-order expansion gives $\mathbb{E}[\Phi(a + bv) \mid K] \approx \Phi(a + b\mathbb{E}[v \mid K]) + \frac{1}{2}b^2\Phi''(a + b\mathbb{E}[v \mid K])\,\mathrm{Var}(v \mid K)$.

Therefore,

$$b_M = \sum_{j=0}^{1} \left[\Phi^{-1}(P^j_{10,t}) - \Phi^{-1}(P^j_{00,t})\right] fr_j \approx \frac{\beta}{\sigma_\varepsilon} + \frac{\rho}{\sigma_\varepsilon} \sum_{j=0}^{1} (\mu_j - \mu_j^-)\, fr_j,$$

which differs from β/σ_ε whenever $\rho \neq 0$ and selection on v_{it} differs between $S_{it}(j)$ and $S^-_{it}(j)$. If $\rho = 0$, then $u_{it} = \varepsilon_{it}$ and the selection term vanishes, so $b_M \approx \beta/\sigma_\varepsilon$.

Step 2: Naive estimator and composition. Define the composition weights

$$\pi_{dj} \equiv P(M_{it-1} = j \mid E_{it} = d,\ E_{it-1} = 0), \qquad d \in \{0, 1\}.$$

By the law of total probability,

$$P(M_{it} = 1 \mid E_{it} = 1,\ E_{it-1} = 0) = \sum_{j=0}^{1} P^j_{10,t}\, \pi_{1j}, \qquad P(M_{it} = 1 \mid E_{it} = 0,\ E_{it-1} = 0) = \sum_{j=0}^{1} P^j_{00,t}\, \pi_{0j}.$$

Hence the naive estimator can be written as

$$b_{SD} = \Phi^{-1}\left(\sum_{j=0}^{1} P^j_{10,t}\, \pi_{1j}\right) - \Phi^{-1}\left(\sum_{j=0}^{1} P^j_{00,t}\, \pi_{0j}\right).$$

Substituting the delta-method approximations for $P^j_{10,t}$ and $P^j_{00,t}$ yields

$$b_{SD} \approx \Phi^{-1}\left(\sum_{j=0}^{1} \Phi\left(\frac{A(j) + \beta + \rho\mu_j}{\sigma_\varepsilon}\right) \pi_{1j}\right) - \Phi^{-1}\left(\sum_{j=0}^{1} \Phi\left(\frac{A(j) + \rho\mu_j^-}{\sigma_\varepsilon}\right) \pi_{0j}\right).$$

This expression differs from β/σ_ε in general. In particular, even if $\rho = 0$ the estimator fails to identify β unless the composition weights coincide, $\pi_{1j} = \pi_{0j}$ for $j \in \{0, 1\}$ (or unless treatment persistence is absent so that $A(0) = A(1)$). □

A.5 The Future Movers Matching Estimator

This estimator combines exact matching on M_{it-1} with a control group made of immediate future movers.

For horizon $\Delta \geq 1$, define the future-mover control event

$$S_{it,h}(j) \equiv \{E_{it-1} = 0,\ E_{it} = 0,\ E_{it+h} = 1,\ M_{it-1} = j\},$$

and

$$P^j_{00,h,t} \equiv P(M_{it} = 1 \mid S_{it,h}(j)).$$

This is the treatment probability at time t for individuals who are employed at time $t+h$ but not at time t.

Define the horizon-Δ future-movers matched estimator as

$$b^h \equiv \sum_{j=0}^{1} \left[\Phi^{-1}(P^j_{10,t}) - \Phi^{-1}(P^j_{00,h,t})\right] fr_j,$$

where $P^j_{10,t}$ is defined in Section A.4.

Proposition 3. *Under mild regularity conditions, using immediate movers as a control group minimizes the bias in the future movers matching estimator.*

Proof. **Proof of Proposition 3**.

Fix $j \in \{0, 1\}$. Let

$$\mu_{h,j} \equiv \mathbb{E}[v_{it} \mid S_{it,h}(j)], \qquad s^2_{h,j} \equiv \mathrm{Var}(u_{it} \mid S_{it,h}(j)).$$

Since $u_{it} = \rho v_{it} + \varepsilon_{it}$ with $\varepsilon_{it} \perp v_{it}$,

$$s^2_{h,j} = \rho^2 \,\mathrm{Var}(v_{it} \mid S_{it,h}(j)) + \sigma^2_\varepsilon \geq \sigma^2_\varepsilon, \quad \text{so} \quad s_{h,j} \geq \sigma_\varepsilon.$$

For the control probability at time t (note that $E_{it} = 0$ on $S_{it,h}(j)$, so there is no β term),

$$P^j_{00,h,t} = \mathbb{E}\left[\Phi\left(\frac{A(j) + \rho v_{it}}{\sigma_\varepsilon}\right) \;\middle|\; S_{it,h}(j)\right] \approx \Phi\left(\frac{A(j) + \rho\mu_{h,j}}{s_{h,j}}\right).$$

For the treated group (Section A.4),

$$P^j_{10,t} = \mathbb{E}\left[\Phi\left(\frac{A(j) + \beta + \rho v_{it}}{\sigma_\varepsilon}\right) \;\middle|\; S_{it}(j)\right] \approx \Phi\left(\frac{A(j) + \beta + \rho\mu_j}{\sigma_\varepsilon}\right),$$

where $\mu_j \equiv \mathbb{E}[v_{it} \mid S_{it}(j)]$.

Therefore,

$$b^h \approx \sum_{j=0}^{1} \left[\frac{A(j) + \beta + \rho\mu_j}{\sigma_\varepsilon} - \frac{A(j) + \rho\mu_{h,j}}{s_{h,j}}\right] fr_j = \frac{\beta}{\sigma_\varepsilon} + \sum_{j=0}^{1} \left[\frac{A(j) + \rho\mu_j}{\sigma_\varepsilon} - \frac{A(j) + \rho\mu_{h,j}}{s_{h,j}}\right] fr_j.$$

Now consider how $\mu_{h,j}$ varies with Δ. By Lemma 1,

$$v_{it+h} = \lambda^h v_{it} + \eta_{it,h}, \quad \text{with} \quad \eta_{it,h} \perp v_{it}.$$

The event $E_{it+h} = 1$ imposes a truncation based on v_{it+h}, which is a noisy signal of v_{it} with loading λ^h. As Δ increases, the signal-to-noise ratio weakly decreases because λ^h decreases. Under standard monotonicity properties of truncated multivariate normals, this implies that $\mu_{h,j}$ is weakly decreasing in Δ and converges to the corresponding mean without the $E_{it+h} = 1$ restriction as $\Delta \to \infty$. Moreover, the conditional variance $\mathrm{Var}(v_{it} \mid S_{it,h}(j))$ is weakly increasing in Δ, hence $s_{h,j}$ is weakly increasing in Δ.

Assume $\rho > 0$ and an interiority condition $A(j) + \rho\mu_{h,j} > 0$ for all relevant (h, j). Then the numerator $A(j) + \rho\mu_{h,j}$ is weakly decreasing in Δ while the denominator $s_{h,j}$ is weakly increasing in Δ, so the ratio

$$\frac{A(j) + \rho\mu_{h,j}}{s_{h,j}}$$

is weakly decreasing in Δ. Define

$$D_{h,j} \equiv \left[\frac{A(j) + \rho\mu_j}{\sigma_\varepsilon} - \frac{A(j) + \rho\mu_{h,j}}{s_{h,j}} \right].$$

Since $\frac{A(j)+\rho\mu_{h,j}}{s_{h,j}}$ is weakly decreasing in Δ, $D_{h,j}$ is weakly increasing in Δ.

Because $fr_j \geq 0$ and $\sum_{j=0}^{1} fr_j = 1$, the weighted sum $\sum_{j=0}^{1} D_{h,j} fr_j$ is weakly increasing in Δ. Note also that under the assumptions $\rho > 0$, $A(j)+\rho\mu_{h,j} > 0$ (interiority) for $j \in \{0, 1\}$, $s_{h,j} \geq \sigma_\varepsilon$, and $\mu_j \geq \mu_{h,j}$, we have

$$D_{h,j} \equiv \frac{A(j) + \rho\mu_j}{\sigma_\varepsilon} - \frac{A(j) + \rho\mu_{h,j}}{s_{h,j}} \geq 0 \quad \text{for each } j \in \{0, 1\},$$

and hence

$$\sum_{j=0}^{1} D_{h,j}\, fr_j \geq 0.$$

Using the approximation above,

$$\left| b^h - \frac{\beta}{\sigma_\varepsilon} \right| \approx \sum_{j=0}^{1} D_{h,j}\, fr_j,$$

and hence the smallest horizon minimizes the absolute bias:

$$\arg\min_{h \geq 1} \left| b^h - \frac{\beta}{\sigma_\varepsilon} \right| = 1.$$

□

To understand this result, recall that $A(j)$ is the baseline tendency to receive medication at time t, combined with a certain status at $t-1$ (capturing persistence and the average incidence of medication in the population). The term $\rho\mu_{h,j}$ is the predictable component of the time-t mental-health shock that comes from selecting people who will soon find work (future movers): they have higher expected employability v_{it}, and with $\rho > 0$ this translates into a higher expected mental-health-treatment shock. Thus $A(j) + \rho\mu_{h,j} > 0$ says that, within each past-treatment group, future movers are not a degenerate group with (almost) zero treatment propensity at time t; instead, they remain in a region where treatment probabilities respond smoothly to changes in selection strength as Δ varies.

Appendix B: Additional Tables and Figures

Table A.1: Mental health drugs (first line)

ATC5	Count	Interpretation
N06AB	244,546	Selective serotonin reuptake inhibitors (SSRIs)
N05CF	186,913	Benzodiazepine-related drugs (Z-drugs)
N05BA	138,371	Benzodiazepine derivatives (anxiolytics)
N05CH	112,925	Melatonin receptor agonists
N06AX	99,831	Other antidepressants
N06BA	68,598	Centrally acting sympathomimetics
N06AA	65,653	Non-selective monoamine reuptake inhibitors (tricyclic antidepressants)
N05AH	31,694	Diazepines, oxazepines, thiazepines, and oxepines (atypical antipsychotics)
N05CD	23,745	Benzodiazepine derivatives
N05AF	12,589	Thioxanthene derivatives (typical antipsychotics)
N05BB	12,523	Diphenylmethane derivatives (anxiolytics)
N05AX	11,262	Other antipsychotics
N05AD	9,483	Butyrophenone derivatives (typical antipsychotics)
N05AA	2,288	Phenothiazines with aliphatic side-chain (typical antipsychotics)
other	1,220	Other
N05AB	755	Phenothiazines with piperazine structure (typical antipsychotics)
N05AN	709	Lithium (mood stabilizers)

Table A.2: Balancing tests, E-to-U

	Women			**Men**		
	Treated	Control	Diff.	Treated	Control	Diff.
Age	37.729	39.306	-1.577***	37.863	39.194	-1.331***
College	0.387	0.395	-0.008***	0.257	0.274	-0.017***
HS & vocational	0.445	0.444	0.001	0.524	0.516	0.008***
Below high school	0.167	0.161	0.007***	0.219	0.211	0.009***
Danish	0.737	0.711	0.026***	0.772	0.712	0.060***
Married	0.474	0.454	0.020***	0.392	0.370	0.022***
Cohabiting	0.650	0.651	-0.001	0.560	0.554	0.006***
Live in Copenhagen	0.411	0.430	-0.019***	0.377	0.404	-0.028***
Number of children	1.250	1.131	0.119***	1.088	0.919	0.169***
N	98312	5478239		140791	7037367	

Notes: This table presents a comparison in characteristics between treatment and control groups one month prior to the job transition. The treatment group consists of individuals who transition from employment to unemployment at event time 0, while the control group includes individuals who experience the same transition 1 to 3 years later.

Table A.3: Balancing tests, U-to-E

	Women			**Men**		
	Treated	Control	Diff.	Treated	Control	Diff.
Age	36.771	37.122	-0.350***	37.312	37.559	-0.247***
College	0.414	0.397	0.017***	0.269	0.257	0.011***
HS & vocational	0.405	0.422	-0.016***	0.483	0.489	-0.007***
Below high school	0.181	0.181	-0.001	0.249	0.253	-0.005***
Danish	0.726	0.716	0.010***	0.780	0.758	0.022***
Married	0.467	0.440	0.027***	0.364	0.341	0.023***
Cohabiting	0.667	0.648	0.019***	0.563	0.526	0.037***
Live in Copenhagen	0.396	0.391	0.005***	0.366	0.366	-0.001
Number of children	1.319	1.267	0.052***	1.109	1.017	0.093***
N	170879	3872043		200774	4340377	

Notes: This table presents a comparison in characteristics between treatment and control groups one month prior to the job transition. The treatment group consists of individuals who transition from unemployment to employment at event time 0, while the control group includes individuals who experience the same transition 1 to 3 years later.

Figure A.1: Duration distribution of mental health medication spells

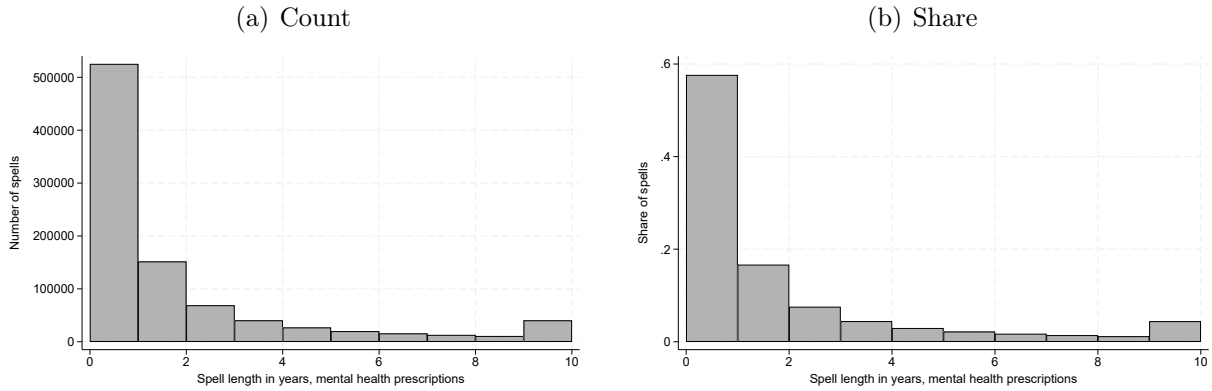
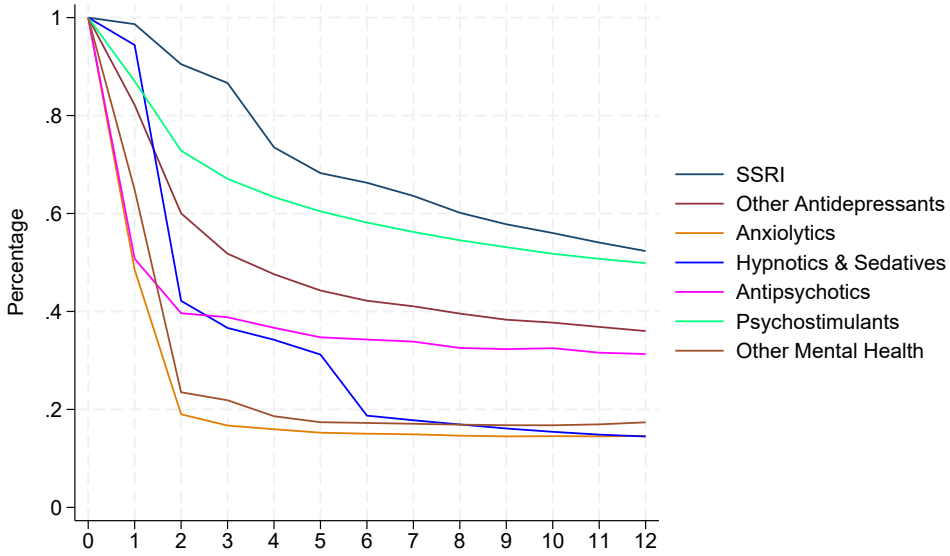


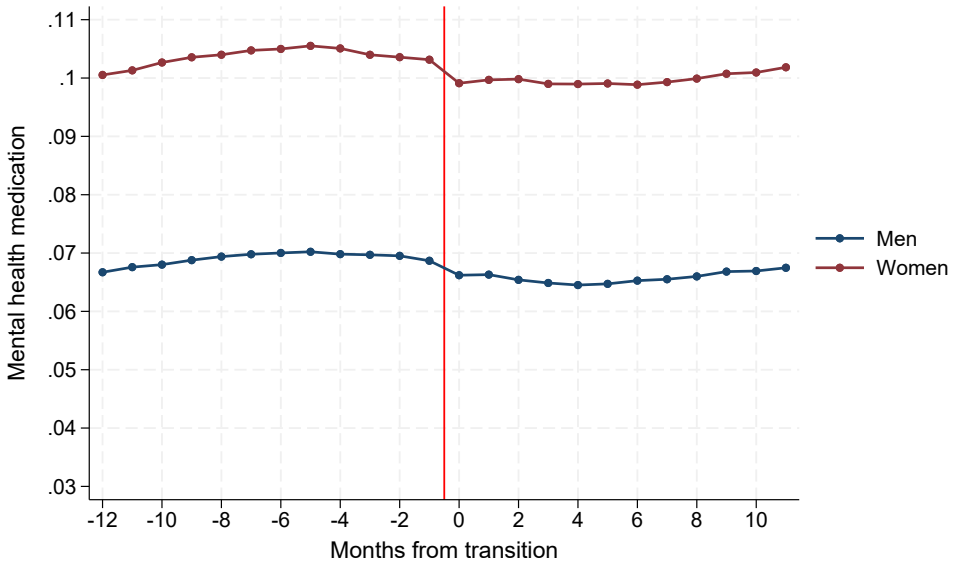
Figure A.2: The survivor function of mental health medication – By drug class



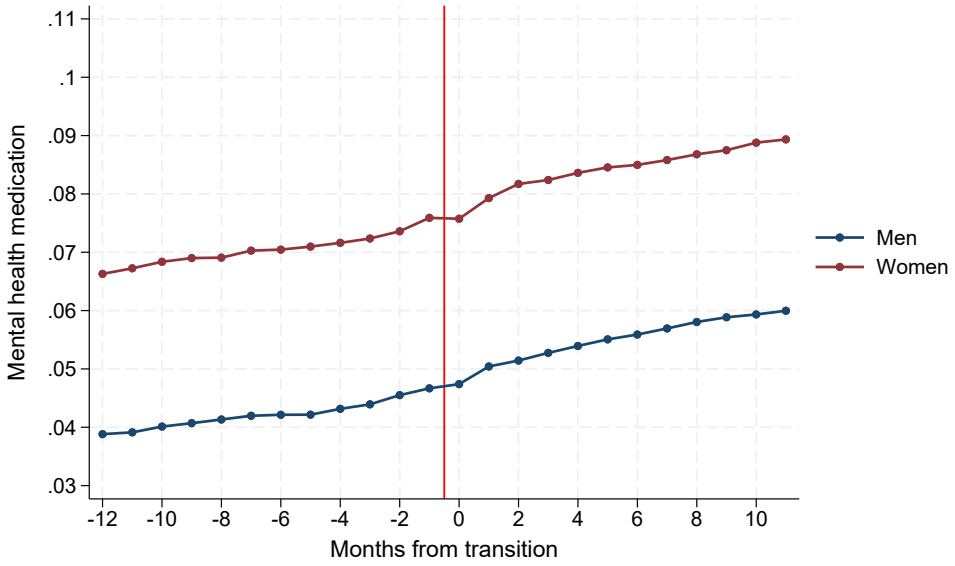
Notes: The Figure plots the survivor function of medication for those starting a new medication spell after at least 12 months without medication.

Figure A.3: Mental health medication around employment transitions

(a) Unemployment to Employment



(b) Employment to Unemployment



Notes: The Figure shows medication rates in the 24 months surrounding an employment transition.