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Mental Health and Firm Pay Premiums

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Non-technical summary

Mental health problems are now the heaviest health burden among working-age adults, and the costs are set to grow as today's affected young people age. Health is strongly correlated with income, but the direction of causation between the two remains debated; the pathways likely depend on life-cycle stage as well as context, including the nature of the income source.

The best existing evidence for the effect of income on mental health draws on lottery wins and cash transfers, whose mental-health effects are small and tend to fade. Yet the largest income source for most people is their pay, and evidence on earnings specifically is thin and mixed.

We provide new evidence using Dutch administrative data that link employers' earnings records to prescriptions for anti-anxiety and anti-depressant medication for millions of workers over 2006–2022. We focus on the pay premium a firm attaches to a job, tracking workers who change employer and comparing those who gain more with those who gain less.

Moving to a higher-paying firm causally reduces medication use compared to moving to a firm that raises pay less, and the benefit builds over three to four years rather than fades. An increase in pay of 10% reduces use of anti-depressant medications by 4% relative to average use. Effects are broadly similar for younger and older workers, and across genders.

The effects are strongest in industries with lower inherent levels of stress; in high-stress sectors, higher pay largely compensates for demanding conditions rather than improving health. Separately, workers taking large pay cuts show worsening mental health *before* they move. This reflects poor health driving wage reductions, rather than the reverse.

These findings highlight that competition and labour-market policies that affect firms' wage-setting power also carry consequences for worker well-being.

Mental Health and Firm Pay Premiums*

Ben Etheridge†, Hiromi Yumoto‡

Abstract

Determining the effect of income on mental health remains an important and open question. We provide new causal evidence using a theory-driven mover design: exploiting job-to-job moves in a continuous-treatment event study, we estimate the effect of changes in firm pay premiums on the use of anti-anxiety and anti-depressant medications. The analysis links Dutch employer–employee records to administrative prescription data over 2006–2022. For the bulk of workers, higher firm pay reduces medication use: five years after a move a 10% increase in firm pay premium reduces anti-depressant use by around 4% of mean usage. This effect holds across gender and age, and among within-sector movers. However, among the subset of workers experiencing large pay declines, medication use rises before the move and falls afterwards, consistent with mental-health shocks having negative career effects. Effect sizes are larger than those documented for unearned transfers, suggesting that the income–mental-health channel depends on the source of income.

JEL Classification: J31, I12, J32

Keywords: Firm wage premiums, mental health, compensating differentials, AKM decomposition, prescription drug use, event study

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1 Introduction

Mental health problems have increased globally over the last decade (WHO, 2025), and are already the most burdensome group of health conditions among adults of working age (Layard, 2017). Given that the recent deterioration in mental health has been most salient for young people, and given the persistence of mental health conditions over the life-course (Kessler et al., 2007), the large economic and social costs associated with mental illness are likely to grow substantially among the working age population in the near term. Identifying the determinants of mental health is therefore an increasingly important task in economics.

Among socio-economic factors, the strong correlation between income and broader health is well-established across societies and systems of healthcare (Case & Kraftman, 2024). However, the mechanisms behind this relationship are less well understood. Cutler et al. (2011) describe the effect of income on health as an open question, and emphasise that the causal channels are likely to vary both across the life-course and with the type of income received. For mental health specifically, the picture is similarly unsettled. In a systematic review, Thomson et al. (2022) conclude that income likely has a causal effect on mental health, but cautions that the underlying evidence is of low quality and points to effects that are at best modest in magnitude. The highest quality evidence comes from basic-income-type transfers (Miller et al., 2024) or lottery winnings (Cesarini et al., 2016; Lindqvist et al., 2020), whose effects on mental health are positive but small, and may differ from other income types.

The largest source of income for working-age adults is labour earnings. Among available evidence, a strong improvement in mental health has been found for increases in the minimum wage (Reeves et al., 2017). On the other hand, within-firm promotions, which are bundled with pay rises, have been found to cause psychological strain (Boyce & Oswald, 2012), even if they do lead to improved physical health (Anderson & Marmot, 2012). Overall, evidence on the effect of labour earnings on mental health is mixed and thin. Yet providing evidence on this front is important for understanding how income and mental-health inequalities co-evolve over working life, the focus of recent work (Jolivet & Postel-Vinay, 2025).

In this paper we provide new evidence on the effect of labour earnings on mental health by using Dutch administrative data which links employer-employee earnings records to prescription drug use. In particular we focus on the *firm*-specific component of earnings, and assess its effect on individuals' use of both anti-anxiety and anti-depressant medications observed over 2006-22. Firm pay premiums have been shown to account for a quantitatively important share of earnings variation, even among observationally identical workers (Card et al., 2013; Song et al., 2019; Bonhomme et al., 2023). In addition, this component of pay is attractive for study because it changes only when a worker switches employer, so that job moves generate sharp variation in pay. Under the logic of the Burdett & Mortensen (1998) job-ladder model, which underpins much of the empirical analysis of firm premiums since Abowd et al. (1999) (AKM), changes in firm pay premium are plausibly orthogonal to prior characteristics, conditional on making a move.[1] We accordingly exploit this in an event-study mover design with continuous treatment intensity following Finkelstein et al. (2016) (see also Finkelstein & Gentzkow, 2026; Miller, 2023). The same design has recently been applied to firms' effects on workers' broader health-care use (Ahammer et al., 2023; Bíró & Elek, 2025), though not in detail to mental health, which is our focus.

We measure mental health through the use of anti-anxiety and anti-depressant medications, which provide objective, longitudinal indicators that avoid the reporting biases of survey-based measures (Persson & Rossin-Slater, 2018; Bögl et al., 2024). To isolate firm pay premiums, we estimate an AKM decomposition, adapting the approach of Åslund et al. (2023) to group small firms into productivity-based clusters and mitigate limited mobility bias (Bonhomme et al., 2023). Our identifying variation comes from workers who change employers through a job-to-job transition exactly once during the sample period, allowing us to estimate event-study effects of the change in firm pay premium on medication use over a five-year window around the move. Intuitively we then identify the effect of earnings on mental health by comparing workers who receive, for example, a 20% increase in firm pay premium to those who receive only a 10% increase in firm premium, holding potential confounders constant.

In our main analysis we find that higher firm pay causally reduces medication use, with event-study effects accumulating over five years. Results are similar for both older and younger workers, and for those moving industry sectors as well as for industry stayers, indicating that we are isolating a mechanism at the firm level, rather than confounding industry switches. The results are particularly strong for anti-depressant medication, with a 10% increase in firm pay premium reducing medication use by around 4% of mean usage at the end of the horizon. Despite gender differences in the level of medication use, we find broadly similar results for both males and females, especially for anti-depressants. These effects are substantially stronger than those for unearned income, such as found by Miller et al. (2024) and Lindqvist et al. (2020), though smaller than those found for earned income by Reeves et al. (2017). This difference between estimates for earned versus unearned income suggests that the effect of income on mental health depends on its source.

Strictly speaking, the logic of the Burdett & Mortensen model applies only to moves to higher valued firms, i.e. *up* the wage ladder. For this reason the analysis described so far focuses on moves in the upper 90% of the distribution of firm pay changes. For this group our event-study analysis shows flat pre-event trends in medication use before the job move, across the distribution of firm pay changes. Our results are robust to studying the strict subset of workers who receive pure firm pay *increases*, with similarly flat pre-trends and post-event declines in drug use. As a diagnostic we then assess changes in medication use for those who suffer large pay *declines*. Here we see an entirely different pattern. For this group, medication use declines after their job move while use of anti-depressants increases steadily beforehand. This pattern suggests that this group of workers on average experiences a spike in mental health problems which triggers a

[1]See Di Addario et al. (2023) for an empirical assessment of the AKM model in terms of *changes* in firm pay.

move down the firm-pay distribution. This joint pattern of a positive effect of firm pay on mental health when workers move up the job ladder combined with mental health problems contributing to worsening career outcomes is consistent with the mechanisms in Jolivet & Postel-Vinay (2025).

These findings leave open the question, why does firm pay improve mental health? The idea that different jobs come with different psychosocial stress is common in the psychiatric literature (Fancourt & Steptoe, 2024). Within economics, the association of jobs with different bundles of amenities is the focus of current research (Sorkin, 2018; Lamadon et al., 2024). Although we do not observe amenity valuations in our data we are able to control for certain candidates. On this front, our results remain unchanged when we control for firm location, firm size, sector, or even co-worker quality. This evidence suggests that the effect of firm pay on mental health cannot be explained by obvious and standard correlates of firm amenities.

We explore this issue further by investigating the hypothesis that firm pay premiums improve mental health by correlating specifically with *reduced* job stress, as in models of rent sharing (Mortensen, 2003). While we do not observe job stress directly, we proceed empirically by splitting the sample by industry stress level, which we identify from a regression of medication use by sector fixed effects. We find that in low-stress sectors, the positive effect of firm pay premiums on worker mental health remains robust. In high-stress sectors, on the other hand, where job stress is a more important part of the amenity bundle, the effect of firm pay is attenuated and close to zero. Rather than reduced job stress, we interpret these results as consistent with compensating differentials for *increased* job stress (as in Rosen, 1986). This finding is also consistent with recent evidence that job stress in particular carries a compensating wage premium (Nagler et al., 2025), and more broadly with heterogeneous compensation across amenities (Humlum et al., 2025). Alongside the standard amenity correlates considered in the previous paragraph - firm location, size, sector, and co-worker quality - we therefore also rule out reduced job stress as the channel linking firm pay to mental health. What remains is most consistent with a direct income effect, clearest in sectors where job stress matters less.

This paper makes two contributions. First, we provide new causal evidence on the effect of earned income on mental health, exploiting firm pay variation within a job-ladder framework. As discussed, the bulk of credible evidence on income and mental health comes from unearned transfers: Miller et al. (2024) find that randomised cash transfers produce stress reductions that fade within a year, while lottery windfalls generate only small effects (Cesarini et al., 2016; Lindqvist et al., 2020). The clearest evidence on earned income comes from Reeves et al. (2017), who exploit the introduction of the UK minimum wage to document substantial reductions in self-reported mental health among low-wage workers. Our estimates lie between these earlier estimates and, interestingly, accumulate rather than fade over the horizon studied. This pattern suggests an effect of income on mental health that depends on its source: sustained higher earnings appear to operate differently from one-off or time-limited transfers. The joint pattern of job-ladder improvements in mental health alongside downward moves driven by mental health deterioration further provides direct empirical content for the co-evolution of income and mental health over working life emphasised by Jolivet & Postel-Vinay (2025). Closest to our setting, Bíró & Elek (2025) study Hungarian job movers with a related firm-premium design but find only weak, subgroup-specific effects of firm pay on antidepressant use. We show that these results mask a strongly non-linear relationship: by separating large pay declines, associated with preceding mental health deterioration, from the rest of the distribution we recover a clear effect for the majority of movers that a pooled, linear analysis obscures.

Second, we speak to the compensating differentials literature by showing how firm pay premiums relate to the mental-health component of the amenity bundle. The bulk of the literature, such as Sorkin (2018) and Lamadon et al. (2024), decompose firm effects into compensation and valuations of the total amenity bundle. Recently Humlum et al. (2025) use direct elicitation to ascertain how firms provide amenities of different types, finding that while the bulk of amenities, such as job flexibility, are positively correlated with firm pay, job stress also correlates positively with firm pay as a compensated *dis*amenity. Nagler et al. (2025) reach a similar conclusion using a stated-choice experiment, estimating a substantial willingness to pay to avoid workplace pressure. We complement this finding by providing evidence on outcomes from this latter type of amenity, and providing evidence consistent with job stress being compensated by firms, given that the pay-health gradient is attenuated in high-stress industries.

The paper proceeds as follows: in Section 2 we present a model of the relationship of pay and worker well-being. In Section 3 we discuss the data. In Section 4 we formalize the empirical specification, before discussing results in Section 5. Section 6 concludes.

2 Conceptual Framework: Firm Pay, Amenities and Mental Health

Here we provide a simple conceptual framework to interpret the relationship between firm pay premiums and mental health. The main purpose of this framework is to clarify how unobserved amenities mediate the estimated relationship between pay and mental health. Building on this, it aims to provide insight into how this relationship may vary across sectors, depending on the importance of job stress in the amenity bundle. To conceptualize what firm pay premiums capture, we organize our framework around that of Sorkin (2018), adapting it to mental health outcomes. The framework abstracts from reverse causality, where mental health affects sorting into firms; we address this concern empirically through our identification strategy in Sections 4 and 5 rather than through the model.

Mental health from working in firm j is determined by:

$$M_j = \gamma \Psi_j + \theta \ln a_j \tag{1}$$

where Ψ_j is the firm pay premium, $\ln a_j$ is a composite amenity index, γ is the direct effect of the firm pay premium on mental health, and the importance of the amenity bundle on mental health is captured by θ. The mental health of individual i is determined by individual-specific characteristics, including the worker-specific component of pay, together with this firm contribution. We interpret the composite amenity as bundling factors that directly affect mental health, such as job stress, alongside factors more distantly related, such as location or workplace perks. An important conceptual point to note is that $\ln a_j$ is priced in terms of utility equivalents, by which workers make their labour supply decisions. By allowing γ and θ to differ, equation (1) therefore permits the effect of amenities on mental health to differ from their role in compensating and attracting workers.

When amenities are unobserved, a regression of M_j on Ψ_j recovers γ plus a term reflecting the covariance of pay with firm-provided amenities. As has long been acknowledged, this covariance can take either sign. In a competitive labour market in which firms differ in their cost of providing amenities but must deliver the same total compensation as their competitors, the covariance is negative: better amenities are paid for through lower wages. This is the Rosen, or compensating-differentials, motive (Rosen, 1986). If instead firms vary in productivity or market power, then Ψ_j and $\ln a_j$ tend to covary positively, since, for example, more productive firms can afford to pay more on both margins. This is the Mortensen, or rent-sharing, motive (Mortensen, 2003). When the Rosen motive dominates the covariance is negative and a regression of M_j on Ψ_j understates the direct effect γ; when the Mortensen motive dominates the covariance is positive and the regression overstates it. Appendix A formalises this argument along the lines of Sorkin (2018) in terms of promised utility and a firm maximisation problem.

It is natural to expect that the correlation of firm pay with amenities depends on the nature of work and the composition of the amenity bundle. In the context of our results, a possible explanation for a positive association between firm pay and mental health is that firm-pay premiums are associated with reduced job stress. However, the available evidence points, if anything, the other way. Nagler et al. (2025) document that high-pressure work carries a sizeable wage premium, suggesting that job stress is a compensated disamenity, positively rather than negatively correlated with pay. Humlum et al. (2025), using survey-elicited measures of firms' amenity provision, corroborate this, while also showing that other (positively valued) amenities, such as perks, correlate *positively with pay*, consistent with a Mortensen motive in which more productive firms pay more on all margins. We therefore conjecture that this covariance is more negative in sectors where job stress looms larger within the bundle, so that the estimated gradient is more attenuated in high-stress sectors. Appendix A extends the basic framework to multiple amenities, providing a model in which all sectoral heterogeneity in the pay-mental health gradient operates through this differential covariance.

3 Data

This study uses multiple administrative datasets from Statistics Netherlands (CBS), linked through unique employer and personal identifiers.[2] We construct linked employer-employee data integrated with prescription records, spanning 2006 to 2022. We provide the main details here, leaving additional details to Appendix B.

3.1 Labour Market and Demographic Data

Information on all enterprises, including firm age, location, and sector classification, is provided in the Firm Register (ABR).[3] We integrate this with Production Statistics, which records production inputs and outputs, including value added, employment, and the wage bill, for all enterprises with 50 or more employees, with smaller firms partially represented through random sampling.[4]

We link firm data to employee information from the Tax Registers (SPOLISBUS), which are derived from employers' tax declarations and capture monthly wages and hours worked for approximately 10 million employees annually. These Tax Registers cover all employment relationships at formally registered firms.[5] The resulting dataset tracks the near-universe of active firms and their employees over time, enabling us to monitor employer transitions. We further merge worker records with the Municipal Register (BRP), which provides demographic information including age, gender, and country of birth. Our dataset captures employed workers only and therefore excludes the self-employed.

3.2 Mental Health Measurement

The worker-level dataset can be linked with health records. Our primary health outcomes are indicators for the prescription of psychotropic medications, drawn from Medicijntab, which records all medicines reimbursed under the mandatory basic health insurance scheme (*basisverzekering*) for individuals registered in the BRP. We focus on two categories within the Anatomical Therapeutic Chemical (ATC) classification: N05 (psycholeptics, commonly used to treat anxiety and sleep disorders) and N06 (psychoanaleptics, commonly used to treat depression). Common N05 medications include diazepam (Valium) and zolpidem (Ambien); common N06 medications

[2]The names of these datasets in Dutch are: *Productiestatistieken*, *SPOLISBUS*, *Algemeen Bedrijfsregister* (ABR), *Basisregistratie Personen* (BRP), and *Medicijntab*. We mainly use English names or Dutch abbreviations for the remainder of the paper.

[3]The unit of observation is the *bedrijf* (business unit), which for the vast majority of firms corresponds closely to the standard notion of an enterprise rather than establishment/plant. See Appendix B for further details.

[4]For small businesses (fewer than ten employees) data for the Production Statistics are taken from tax registrations as much as possible. In addition, businesses with fewer than 50 employees receive a questionnaire on a sample basis, while enterprises with 50 employees or more are included in every survey. Overall, more than 80,000 enterprises are invited to participate in the survey per year, about 10 percent of businesses in the Netherlands. Over the course of our sample this implies that the majority of businesses are sampled at some point.

[5]Employment relationships must involve at least 4 paid hours within the reporting month to be included. See Appendix B for further details on coverage.

include fluoxetine (Prozac) and sertraline (Zoloft). The composition of sub-categories of these drug types is documented in Appendix Table B.1.

We measure mental health as a binary indicator equal to one if an individual was prescribed any medication within the relevant ATC category in a given year. While much of the literature measures treatment intensity through Defined Daily Doses (e.g., Persson & Rossin-Slater, 2018; Bögl et al., 2024), this DDD information is available in our data only from 2020 onward. The binary indicator we use has the advantage of capturing the clinically meaningful extensive margin, i.e. the initiation or continuation of pharmacological treatment, rather than variation in dosing that may reflect physician prescribing habits as much as patient need.

Two features of the Dutch institutional setting strengthen our measurement. First, basic health insurance is mandatory for all legal residents and covers primary care, hospital care, and prescription drugs, implying substantially lower financial barriers to treatment than in settings such as the United States.[6] This broad coverage also implies that our data capture close to the universe of outpatient prescription drug use. Second, the Netherlands exhibits comparatively low antidepressant consumption by European standards: OECD figures put national consumption at approximately 37 to 50 DDDs per 1,000 inhabitants per day over our study period, roughly half the rate in the United Kingdom or Sweden.[7] This low usage rate suggests our analysis captures a relatively high-threshold margin of treatment, presumably indicative of more severe conditions.

As with all studies using medication data, our measure reflects physician-assessed treatment need rather than true prevalence. Individuals who experience symptoms but do not seek treatment, or who are treated through talk therapy rather than medication, will not appear in our data; the latter margin is likely modest, however, given the documented reluctance to use talk therapy (Cronin et al., 2025). This would introduce bias only if willingness to seek treatment varies systematically with firm pay levels. If anything, in the more likely case that higher-paying firms facilitate treatment access, this would bias our estimates *upward* (i.e. higher pay being associated with higher medication use) and thus strengthen our inference of a negative relationship between firm pay premiums and medication use. Indeed, Bíró & Elek (2025) document higher overall health-care spending among workers at higher-premium firms.

3.3 Sample Construction

Our estimation sample comprises employed individuals aged 18 to 67, observed between 2006 and 2022. We compute hourly wages by dividing annual earnings by paid hours.[8] We exclude

[6] Danesh et al. (2024) report that unmet healthcare needs among low-income households in the Netherlands are only 0.4%, and that prescription rates among those reporting symptoms are roughly constant across the income distribution, consistent with low financial barriers to treatment in the Netherlands.

[7] See OECD Health Statistics (`https://www.oecd.org/en/data/datasets/oecd-health-statistics.html`), accessed February 12, 2026.

[8] Paid hours are the hours for which wages are paid: contractual hours plus any paid additional hours, less hours of unpaid absence. Paid leave and sick leave therefore count as paid hours, since wages continue during these periods, whereas unpaid absence does not. For employees on flexible contracts remunerated on the basis of an agreed average, we use that fixed average.

Table 1: Summary Statistics: Full Sample

	All		Female		Male	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Male	65.08%		0.00%		100.00%	
Age	39.31	13.01	37.40	12.81	40.33	13.00
Hourly wages	20.01	10.79	17.54	9.47	21.33	11.22
Tenure	4.27	3.83	3.86	3.58	4.49	3.94
Total moves	1.67	1.71	1.64	1.67	1.68	1.77
Firm ages	20.28	15.04	19.65	14.51	20.61	15.31
Firm size	6,758.55	18,085.20	8,845.67	20,172.34	5,638.87	16,752.52
Drug-intake						
N05	2.70%		3.42%		2.46%	
N06	4.99%		6.58%		4.13%	
Individuals	7,473,977		2,947,390		4,527,739	
Observations	54,788,972		19,130,133		35,658,839	

Note: Wages are deflated at the 2015 level in Euros. Tenure is measured as the total number of years observed with the same employer. Total moves refers to the cumulative number of job changes recorded during the study period.

observations with implausible hours (negative values or exceeding 4,380 per year, equivalent to 12 hours daily for 365 days) and winsorize hourly wages at the 1st and 99th percentiles. Part-time workers are retained.

Since we employ the AKM model to obtain firm-specific pay premiums (see Section 4), we restrict the data to the largest connected set of firms and workers (e.g., Abowd et al., 1999; Card et al., 2013, 2016), constructed separately by gender. The largest connected sets cover more than 96 percent of all observations. The resulting full sample contains 7,473,977 individuals and 54,788,972 person-year observations.

From this broader sample, we construct an event study sample by applying further restrictions. We select individuals who change employers exactly once during the study period, ensuring that changes in prescription patterns can be attributed to a specific mobility event rather than confounded by multiple transitions. We define the event window as five years before and after the job change ($\tau \in [-5, 5]$, where $\tau = 0$ is the year of the job change), and require that the switch occurs between 2011 and 2017 so that the full event window falls within the 2006–2022 data period. We further require at least six total observations per individual, including at least two at both the origin and destination firms, and require observation at $\tau = -1$ (the year immediately before the switch) to precisely identify the timing of the transition. Our sample therefore captures job-to-job transitions and intentionally excludes moves through unemployment. These restrictions yield a sample of 186,983 individuals (62,129 female, 124,866 male) and 1,730,217 person-year observations.

3.4 Summary Statistics

Table 1 presents summary statistics for the full sample, disaggregated by gender. Males account for 65% of the sample. Men have higher average wages (€21.33 versus €17.54 per hour) and

Table 2: Summary Statistics: Single-Mover Event Study Sample

	All		Female		Male	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Male	67.62%		0.00%		100.00%	
Age	42.90	11.93	41.63	11.87	43.51	11.91
Hourly wages	21.84	10.67	18.82	9.28	23.29	10.98
Tenure	4.07	2.40	3.93	2.36	4.14	2.42
Total moves	1.00	0.00	1.00	0.00	1.00	0.00
Firm ages	18.30	15.08	17.98	14.64	18.45	15.28
Firm size	4,754.24	14,896.14	7,564.31	19,400.00	3,408.43	11,930.02
Drug-intake						
N05	2.14%		2.67%		1.88%	
N06	4.62%		6.29%		3.83%	
Individuals	186,983		62,129		124,866	
Observations	1,730,217		560,300		1,169,917	

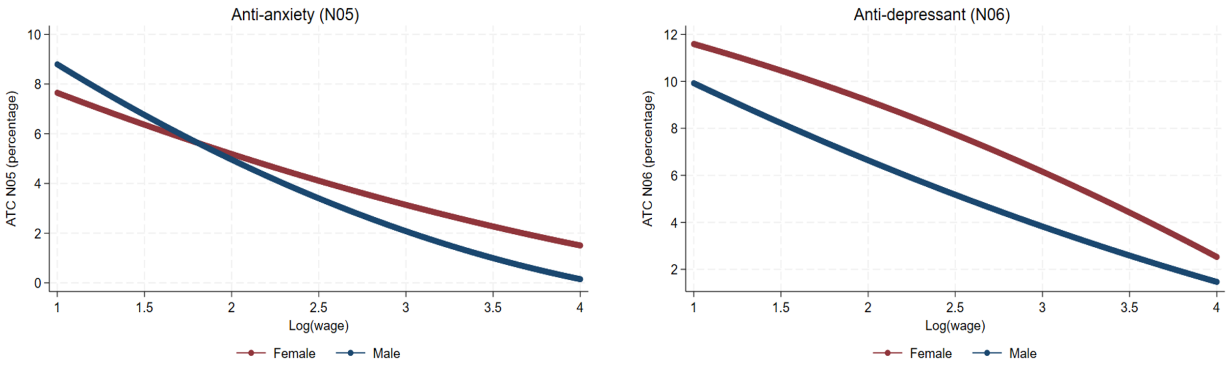
Note: The sample is restricted to individuals who change employers exactly once during the study period. We require at least six total observations per individual, including at least two observations at both the origin and destination firms. The event window spans five years before and after the job change (i.e., $t \in [-5, 5]$, where $t = 0$ is the year of the job change). Wages are deflated at the 2015 level in Euros. Tenure is measured as the total number of years observed with the same employer. Total moves refers to the cumulative number of job changes recorded during the study period.

longer tenure (4.49 versus 3.86 years). Women are more likely to be prescribed mental health medications: the gender gap is particularly pronounced for antidepressants (N06), at 6.58% versus 4.13%. These differences indicate important gender disparities in both labour market trajectories and health outcomes, which we account for in the subsequent analysis either by estimating outcomes separately by gender, or by including rich interactions of gender with age polynomials.

Table 2 reports summary statistics for the event study sample. It shows that this group is somewhat older, has higher wages, and shows lower medication rates than the full sample, particularly for N05 psycholeptics (2.14% versus 2.70%). Overall, though, this narrower sample is broadly similar to the full sample. For further detail on panel structure, Table C.1 reports the distribution of person-year observations per worker. The median number of observations is 13, while for 20% of the event study sample we observe the full 17-year panel.

Figure 1 plots the raw relationship between wages and medication use, controlling for age differentially by gender. We find a clear negative association with wages for both medication classes. For N05, the Figure shows a somewhat steeper wage gradient for males than females; for N06, the gradient is similar, with a gender gap of around two percentage points at the centre of the wage distribution when averaged across ages, and a smaller gap at both tails. This conditional gap is smaller than the raw 2.5-percentage-point difference in Table 1 because of wage composition effects. Again, these systematic gender differences motivate our practice of estimating the bulk of analyses separately for men and women.

Figure 1: Mental Health Medication Use across the Wage Distribution



(a) Anti-anxiety (N05) **(b)** Anti-depressant (N06)

Note: Estimated by $y_{it} = \alpha + gender_i + log(wage_{it}) + gender_i \times log(wage_{it}) + cont_{it} + u_{it}$, where y_{it} is a binary variable capturing whether the individual i is prescribed the mental health medication in year t, $cont$ (controls) includes the interaction between linear/quadratic ages and gender. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

4 Empirical Specification

4.1 AKM Estimation

Our first step is to estimate firm pay premiums, which we do using the standard wage specification:

$$w_{it} = \alpha_i + \psi_{j(i,t)} + X'_{it}\beta + \epsilon_{it} \tag{2}$$

where $X'_{it}\beta$ reflects components of wages captured by observable characteristics and α_i captures individual differences in productivity. The fixed firm premiums of interest are captured by $\psi_{j(i,t)}$, where $j(i,t)$ denotes the firm employing worker i at time t. Residual wages, which include match effects between firm and worker, are given by ϵ_{it}. As usual, key to identification is the assumption that unobserved productivity shocks are orthogonal to worker mobility decisions (Card et al., 2013). The controls are a cubic in age as well as year dummies.

The recent literature on AKM estimation emphasizes the relevance of bias in estimates of the firm premiums which arises from limited mobility of workers, mainly in and out of the smaller firms. Intuitively, as firm premiums are identified by average wage changes for workers that move between firms, limited mobility restricts identification of the premiums for these firms (Bonhomme et al., 2023). In line with recent literature (Bonhomme et al., 2019) we mitigate this issue by grouping similar firms. Following Åslund et al. (2023), we group firms with fewer than 50 employees into 10 clusters based on productivity, including even micro-enterprises. This covers around a third of workers in our sample. To test sensitivity to including the smallest firms, and following Sorkin (2018), we also present the main event study excluding firms with fewer than 15 workers, which drops around 15% of the population of workers in any given year. As we show in Section 5, this yields near identical results.

Table 3: Variance Decomposition of Log Wages

	Female	Male
Mean of log wages	2.749	2.941
Standard Deviation of log wages	0.470	0.486
Share of variance of log-wages due to:		
Person effects (α)	46.36%	60.66%
Firm effects (ψ)	5.64%	5.09%
Covariates ($X\beta$)	15.76%	8.63%
Residuals (ϵ)	21.88%	16.23%
Covariance of person and firm effects ($2cov(\alpha, \psi)$)	2.68%	3.12%
Covariance of person effects and covariates ($2cov(\alpha, X\beta)$)	8.00%	6.66%
Covariance of firm and covariates ($2cov(\psi, X\beta)$)	-0.32%	-0.38%

Note: The model is estimated separately for female (Column (1)) and male (Column (2)) within the connected set of each group as in the AKM specification in eqn 2.

Table C.2 in Appendix C shows properties of this firm sample. The median number of movers from any firm or cluster of firms for the grouped-firm dataset is 213 for females and 95 for males, over the panel we observe. It is also worth noting that our approach maintains a size dominance of the largest connected set, with 96% of firms covered by these largest sets. As in Bíró & Elek (2025), our approach of grouping smaller firms and using firm-specific estimates for those with more than 50 employees also helps to reduce noise in estimates of firm premiums.

Table 3 shows that we obtain estimates of firm premiums with desirable properties. It shows the percentage shares of a standard variance-covariance decomposition of the wage variance. In line with Bonhomme et al. (2023), firm premiums play a small but important role in wage inequality, contributing through a raw variation in differences (a little over 5%) and a positive covariance with individual fixed effects (a little under 3%). The alignment of these variance decomposition results with the recent literature suggests that our grouping approach adequately addresses potential bias concerns without requiring additional bias correction methods.

4.2 The Relationship Between Firm Pay and Mental Health

4.2.1 Fixed Effects Analysis

We begin with fixed effects regressions using our full sample to establish the baseline association between firm wage premia and mental health outcomes. This analysis also permits a sample that is broadly representative of the working population as a whole.

Recycling some notation, our primary specification is:

$$m_{it} = \lambda_i + \varphi_t + \mu_c + \psi_j \delta + X_{it}\beta + \varepsilon_{it} \tag{3}$$

where m_{it} is an indicator for mental health medication use by worker i in year t, λ_i and μ_c are individual and municipality fixed effects, and φ_t are calendar year fixed effects. Then ψ_j is the wage premium of worker i's firm j in year t, and X_{it} includes time-varying controls, such as age, and which in various specifications include match quality, firm size and average co-worker quality. Location and firm assignment are both determined at the individual-year level and could be written as $c(i,t)$ and $j(i,t)$ for consistency with the AKM specification above, although this extra notation is suppressed here for brevity.

We estimate this model separately by gender and cluster standard errors two-ways at the individual and firm levels to account for serial correlation at the individual level, as well as within-firm correlation in outcomes.

As discussed in Section 2, we are also interested in how the relationship between firm premiums and mental health varies by sectors. To implement this, we estimate broad industry-specific effects:

$$m_{it} = \lambda_i + \varphi_t + \mu_c + \psi_j\delta_0 + \psi_j \times \mathbb{1}[\text{HighStress}_s]\delta_1 + X_{it}\beta + \varepsilon_{it} \tag{4}$$

where $\mathbb{1}[\text{HighStress}_s]$ indicates industries (denoted by s) with above-median stress levels, identified using industry fixed effects from a first-stage regression on mental health outcomes (see details in Appendix B.5). The coefficient δ_1 tests whether workers in high-stress industries show differential mental health responses to firm wage premia.

In more detail we identify industries as high-stress by running an additional two-way fixed effects model of medication use with worker and industry effects. Industries with elevated prescription rates include public administration and retail trade, while mining, agriculture, and construction exhibit comparatively low rates. These patterns are not specific to the Dutch institutional setting: as we show in Appendix Figure C.1, industry-level mental health measures from our Dutch data correlate positively (r=0.49) with lifetime depression prevalence rates across US industries reported by Sussell et al. (2025), based on telephone interviews (through the Behavioral Risk Factor Surveillance System). This cross-country and cross-measure alignment suggests our classification captures genuine differences in sector-level mental health burden rather than idiosyncratic features of the Dutch system, or systematic biases in measurement from the use of medication data.

While the fixed effects specifications control for time-invariant individual characteristics, they do not address dynamic selection into firms based on health trajectories. We therefore implement an event-study mover design.

4.2.2 Event-Study Mover Design

We implement a mover design with continuous treatment intensity following Finkelstein et al. (2016), which allows us to establish the causal relationship of the effect of firm wage premiums on mental health. The same design has recently been applied to firm effects on broader health-care use (Ahammer et al., 2023) and, most closely to our setting, to antidepressant prescriptions

(Bíró & Elek, 2025). This approach exploits variation from worker mobility across firms with different wage premia to separate selection from treatment effects.

For worker i moving from origin firm $o(i)$ to destination firm $d(i)$ in year t_i^*, we estimate:

$$m_{it} = \lambda_i + \varphi_t + \pi_{\tau(i,t)}\Delta\psi_i + \rho_{\tau(i,t)} + X_{it}\beta + \varepsilon_{it} \tag{5}$$

where $\Delta\psi_i \equiv \psi_{d(i)} - \psi_{o(i)}$ denotes the change in firm wage premium and $\tau(i,t) \equiv t - t_i^*$ is event time.[9] The coefficients $\pi_{\tau(i,t)}$ capture the effect of the premium change at each event-time, while $\rho_{\tau(i,t)}$ are relative-year fixed effects. The latter absorb any systematic changes in mental health that accompany job transitions regardless of the wage premium change involved, such as short-run disruption from the move itself. By controlling flexibly for these transition-related patterns, the π_τ terms isolate variation in outcomes attributable to the *magnitude* of $\Delta\psi_i$. As before, we include in X_{it} controls for firm size, firm age and contemporaneous match effect, as well as a polynomial in age. Again we cluster standard errors multi-way by individual and firm.[10]

We normalize $\pi_{-1} = 0$. The coefficients π_τ then trace out the dynamic response to changes in firm wage premia: under a causal interpretation, π_τ should be flat for $\tau < 0$, i.e. with no pre-trends. We estimate Equation 5 on the top 90% of the $\Delta\psi$ distribution, excluding the bottom decile, since large downward moves in firm premium plausibly reflect different forces, such as a reverse causal effect of deterioration in mental health, rather than moves up the job ladder. We examine this excluded decile separately in Section 5, where we develop this motivation.

The design exploits the variation in firm pay premium changes that mobility generates. Figure 2 shows that, among movers, $\Delta\psi$ exhibits substantial two-sided variation, approximately normally distributed and centred slightly above zero, so that identification draws on sizeable premium changes in both directions.

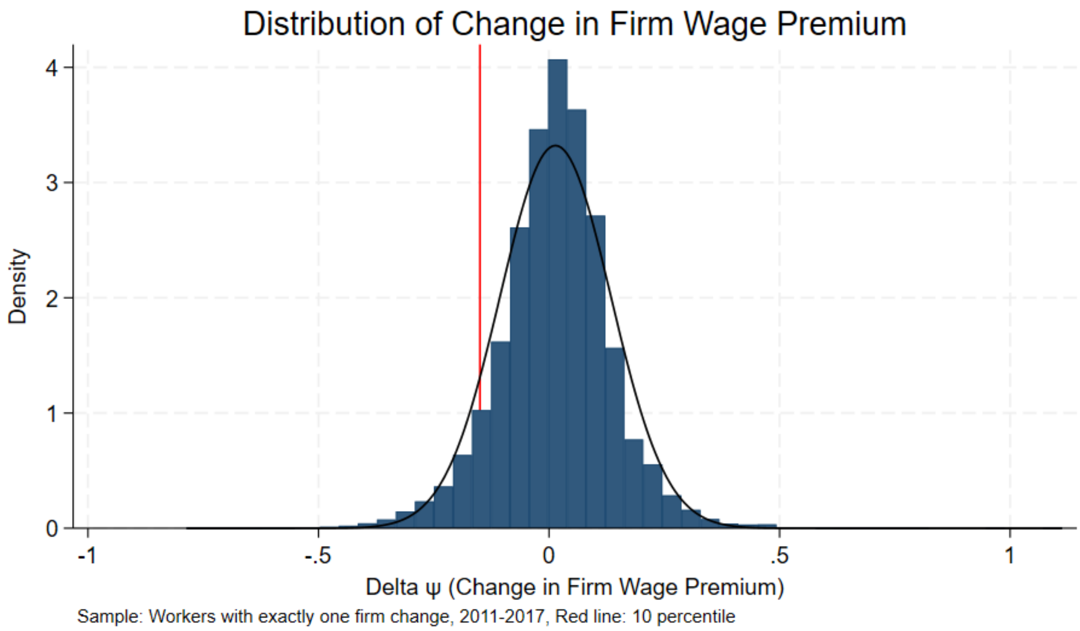
4.2.3 Identification Strategy

The key identifying assumption is that, conditional on making a job transition, the magnitude of the wage premium change is orthogonal to any unobserved determinants of mental health. In this light our argument for a causal interpretation of the π_τ coefficients draws on several arguments. We begin with the theoretical logic. Our design draws on the job ladder mechanism of Burdett & Mortensen (1998): employed workers receive outside offers according to a random process, and the wage improvement each offer represents depends on which firm happens to make the offer. Conditional on a worker choosing to transition, and conditional on other characteristics at event time $\tau = -1$, the magnitude of the pay premium change $\Delta\psi_i$ reflects variation in the identity of poaching firms. The Burdett & Mortensen model is also the theoretical basis

[9] In a single-move sample, $\Delta\psi_i$ is fixed at the individual level. Equation 5 could therefore be related to Equation 3 by being recast in terms of the *level* of ψ_{it}. See Finkelstein et al. (2016) for discussion of the reformulation.

[10] We do this both for the origin firm prior to the job change ($\tau < 0$) and the destination firm following the job change ($\tau \geq 0$). This accounts for within-firm correlations in mental health outcomes on both sides of the transition.

Figure 2: Distribution of $\Delta\psi$ among Single Movers



Note: The figure shows the distribution of changes in firm premiums ($\Delta\psi$) among single movers in the period from 2011 to 2017. The red line shows the 10th percentile of $\Delta\psi$.

for the AKM decomposition which we employ, which gains extensive empirical support in the literature, including as a wage process (Di Addario et al., 2023). In our analysis, job characteristics at the origin firm, such as tenure and wage match effect, are controlled for with our individual fixed effects. The continuous-treatment design strengthens identification by comparing workers who move in the same time window but experience different magnitudes of $\Delta\psi_i$: any confounder would need to scale proportionally with the size of the premium change, not merely correlate with the decision to switch jobs.

As standard, the event-study design provides a direct test of the identifying assumption through the pre-transition coefficients π_τ for $\tau < -1$. If unobserved confounders were driving both mental health changes and the magnitude of $\Delta\psi_i$, we would expect to see differential medication trajectories in the years preceding the job change. As we show in Section 5, the pre-transition coefficients are flat and individually insignificant in our main event-study analysis, indicating that workers who go on to experience larger firm premium changes were not on different mental health trajectories beforehand. We discuss an important qualification to this argument below, when we consider workers with large wage declines.

With a continuous treatment variable there is no direct way to compare treatment and control groups. However, in the spirit of a balancing test, Table C.3 shows a correlation of the change in firm premium with baseline characteristics, measured at event time $\tau = -1$. We show this for the full sample, the top 90% of changes in firm premium, and for strictly positive changes in firm premium, conforming to the strictest interpretation of the Burdett & Mortensen model. It shows that the change in firm premium is indeed correlated with various characteristics, in ways that would be expected. An increase in pay is associated with a higher individual fixed effect, reflecting sorting of workers to firms over time, as well as match effect, as those with a higher wage residual at the origin firm have a higher reservation wage for firm moves. The associations with job tenure, firm age and firm size are all significant, but much smaller in magnitude.[11] It should be stressed that all differences in characteristics are absorbed in our analysis by the individual fixed effects.

Most importantly Table C.3 shows a correlation of the independent variable, $\Delta\psi$, with baseline mental health. For both the full sample and the sample with only positive wage changes, the correlation is insignificantly different from zero. Equally we see an insignificant result for N06 anti-depressants for the sample in the middle column. Only for N05 anti-anxiety drugs do we find a significant association, but the effect is economically small: as Figure C.2 shows in standardized units for this top-90% sample, a one-standard-deviation change in baseline use of N05 predicts a change in $\Delta\psi$ of less than 0.015 standard deviations. Overall we take this as reassurance of our design, and a complement of the pre-trends analysis, which is the main assessment of our approach.

In addition to the individual fixed effects and a polynomial in age, we include all relevant time varying factors, including (change in) match effect, job tenure and characteristics of the firm, such as firm age and firm size. Controlling for match effect is particularly important here: Although in the full sample match quality is orthogonal to firm premium by construction, it is plausibly correlated with the premium in this more restricted sample. We also explore differential trajectories by additionally splitting the sample by characteristics in various heterogeneity analyses.

We gain further validation from isolating a subgroup which *a priori* plausibly falls outside our theoretical motivation and for which evidence of reverse causality is in fact picked-up by pre-trends. In our context, as we discuss in detail in the following Section 5, those with large wage *declines* seem to exhibit a different mental health trajectory from the remainder of the sample. For this reason for the bulk of the analysis we remove this bottom 10% of changes in firm premium. This split does not of course pick up sharply delineated groups, but we provide empirical support that this captures an appropriate division of the data to pick up these heterogeneous processes.

We finish by briefly considering a natural alternative identification approach: mass layoffs as exogenous separations from origin firms, following the displacement literature (e.g. Jacobson et al., 1993). This approach we consider unsuitable for studying the mental health effects of firm pay premiums. While mass layoffs may be exogenous to individual worker characteristics, the destination firm, and hence the post-displacement pay premium, is not. Workers experiencing

[11] The t-ratios on these coefficients are smaller than for the wage components and should not be interpreted too strongly. However, the fact that workers from older and larger origin firms make smaller jumps in firm premium suggests that these firms offer better amenities.

mental health consequences of job loss may search less effectively, accept lower-quality matches, or adjust their reservation wages. The firm premium obtained after displacement would thus be endogenous to individual-specific mental health effects of the displacement event itself.

5 Results

5.1 Fixed Effects Analysis

We begin by examining the fixed effects specification shown in Equation 3, estimated on the full sample. While this approach is limited in terms of causal interpretation, it has the advantage of being more representative of the population as a whole and offers greater precision due to the larger sample size.

Table 4 presents results for the use of anti-anxiety medication (N05) separately for males and females. As a reminder, the main coefficient of interest, presented in the top rows, captures the effect of the estimated firm pay premium (ψ_j) taken from the AKM wage regressions. All columns include individual fixed effects, which absorb all time-invariant individual characteristics, including productivity differences as well as any underlying predisposition to medication use. All columns also include controls for age profiles and calendar year effects.

The first column, which additionally controls for industry dummies, shows that the firm premium has a significantly negative effect on medication use, which we interpret as a positive impact on mental health. For presentation purposes, we have scaled medication use by 100, so the coefficient for females implies that a 0.1 log unit increase in the firm premium (roughly a 10 percent increase in pay) corresponds to a 0.07 percentage point reduction in medication use, or approximately 2 percent of the mean. To put this in context, we compare to the raw regression slope of medication use on wages shown earlier in Figure 1. There, the average slope was approximately 1 for females and 1.5 for males, implying that controlling for individual-specific differences beyond gender reduces the wage-medication gradient by roughly one-third to one-half.

The subsequent columns introduce additional and alternative controls. Column 2 replaces industry dummies with log firm size, which is itself correlated with firm pay premiums. Although firm size is positively associated with medication use for men, the coefficient on firm pay is barely affected. Similarly, replacing industry with municipality fixed effects in column 3 does not alter the estimates, suggesting that the results are not driven by systematic differences in both firm composition and mental health across locations.

We next explore the possibility that the effect of firm premiums operates through characteristics of co-workers. Consistent with the recent literature on AKM estimation, we find positive assortative matching of workers across firms, raising the possibility that co-worker quality plays a role. The results for males in column 4 show that higher-quality co-workers (as measured by average co-worker α) do modestly reduce medication use, but this inclusion again barely affects the main estimates.

Table 4: Firm Pay Premiums and Anti-Anxiety Medication (N05)

	(1)	(2)	(3)	(4)	(5)
Panel A: Female					
Firm premium (ψ)	-0.663***	-0.658***	-0.699***	-0.700***	-0.791***
	(0.226)	(0.241)	(0.242)	(0.242)	(0.123)
No. of workers (log)		0.00387			
		(0.00726)			
Average coworker α				0.0258	
				(0.0747)	
High stress industry					0.154***
					(0.0265)
High stress industry*ψ					0.410
					(0.371)
Mean	3.420	3.420	3.420	3.420	3.420
Observations	19,130,133	19,130,133	19,130,131	19,065,449	19,130,133
Panel B: Male					
Firm premium (ψ)	-0.731***	-0.684***	-0.674***	-0.671***	-1.024***
	(0.0892)	(0.0972)	(0.0885)	(0.0889)	(0.0999)
No. of workers (log)		0.0109**			
		(0.00450)			
Average coworker α				-0.109**	
				(0.0444)	
High stress industry					0.145***
					(0.0155)
High stress industry * ψ					0.994***
					(0.137)
Mean	2.456	2.456	2.456	2.456	2.456
Observations	35,658,839	35,658,839	35,658,839	35,641,746	35,658,839
Age controls	✓	✓	✓	✓	✓
Individual effects	✓	✓	✓	✓	✓
Year effects	✓	✓	✓	✓	✓
Industry effects	✓				
Municipality effects			✓	✓	

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors are two-way clustered at the individual and firm levels. The sample is restricted to the largest connected set by gender. Average co-worker α represents the average worker fixed effects among co-workers at the individual's firm. High stress industry is an indicator variable equal to one if the industry's mental health prescription rate exceeds the sample median. The industry-level prescription rates are calculated as industry fixed effects from a two-way fixed effects model controlling for worker and industry effects. Industries are defined using the 2-digit Standard Business Classification 2008. ATC code N05 (psycholeptics, commonly used to treat anxiety).

The final column addresses the most natural alternative to a pay channel: that higher-paying firms are simply less stressful, so that what we attribute to pay is in fact the benefit of escaping a stressful workplace. If higher pay chiefly signals lower stress, the gradient should steepen in high-stress sectors, where workers have more stress to escape. However, as discussed in the Introduction and in Section 2, prior evidence indicates that job stress is instead a compensated disamenity. In that case higher pay in these sectors in part compensates for stress, so the mental health benefits of higher wages should be attenuated rather than amplified there.

The results in column 5 show that the coefficient on the interaction between high-stress industry and firm pay is positive for women, although not statistically significant. For men, however, the interaction coefficient is large and highly significant, fully offsetting the baseline effect and implying that the net effect of firm pay on medication use in high-stress industries is essentially zero. The near-complete offset implies that the apparent return to pay in these sectors is almost entirely compensation for the disamenity. As discussed in Section 2 and Appendix A, we cannot pin down precisely what this implies for the prevalence of compensating differentials, but the pattern rules out stress reduction as the mechanism behind the firm-pay effect and corroborates the compensated-disamenity reading.

Finally, it is worth noting that estimates on firm premiums are noticeably more precise for men than for women throughout. The final column suggests this reduced precision for women is concentrated in high-stress industries, indicating particularly high variability in medication use among women in these sectors.

We next examine the relationship between firm pay premiums and the use of anti-depressant medication (N06). These are presented in Table 5. Overall, the results are broadly similar to those for anti-anxiety drugs: there is a strong negative relationship between pay premiums and medication use, indicating a positive effect on mental health. As with anti-anxiety medication, there is little change in the estimated effects when we control separately for industry, firm size, municipality, or co-worker quality. Coefficients on these controls do differ somewhat from Table 4 and in particular, firm size has a stronger positive association with medication use. These are not the focus of our analysis, however, so we do not interpret them in detail. The more notable difference concerns the gender pattern: the estimated effects of firm premiums for males are now noticeably smaller than for females. Comparing to the raw regression slopes in Figure 1, where the gradient for anti-depressant medication is approximately -3 for both genders, individual fixed effects absorb over three-quarters of the raw wage-medication gradient, compared to one-third to one-half for anti-anxiety drugs.

Turning to the role of high-stress industries, the final column shows somewhat different results compared to Table 4. The point estimates on the interaction term are again positive, indicating that the gradient on firm pay premiums is attenuated in high-medication-use sectors. However, these estimates are not statistically significant, and for males in particular are quite small in magnitude. This pattern suggests that stress and anxiety, rather than depression, are the more salient disamenities that firms compensate workers for through higher pay.

5.2 Results from Event Study

We now turn to our event study design to pursue direct causal evidence on the effect of firm pay premiums on mental health. This design restricts attention to workers who change employers exactly once during our sample period, with identification resting on differential changes in firm

Table 5: Firm Pay Premiums and Anti-Depressant Medication (N06)

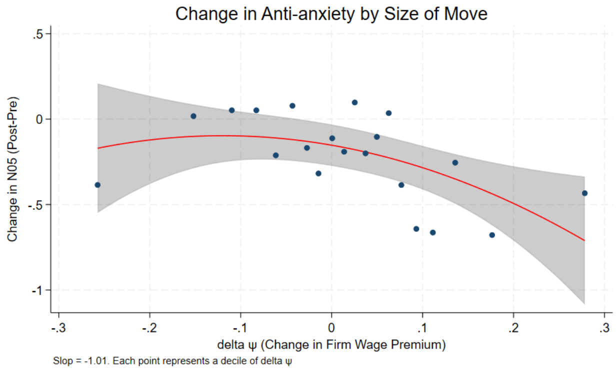
	(1)	(2)	(3)	(4)	(5)
Panel A: Female					
Firm premium (ψ)	-0.709***	-0.833***	-0.724***	-0.706***	-0.801***
	(0.103)	(0.104)	(0.104)	(0.103)	(0.120)
No. of workers (log)		0.0259***			
		(0.00609)			
Average coworker α				-0.274***	
				(0.0990)	
High stress ind.					0.0886***
					(0.0241)
High stress ind.*ψ					0.192
					(0.174)
Mean	6.579	6.579	6.579	6.579	6.579
Observations	19,130,133	19,130,133	19,130,131	19,065,449	19,130,133
Panel B: Male					
Firm premium (ψ)	-0.330***	-0.517***	-0.420***	-0.412***	-0.364***
	(0.0811)	(0.0829)	(0.0719)	(0.0716)	(0.101)
No. of workers (log)		0.0347**			
		(0.00540)			
Average coworker α				-0.265***	
				(0.0464)	
High stress ind.					0.137***
					(0.0204)
High stress ind. * ψ					0.0643
					(0.163)
Mean	4.133	4.133	4.133	4.133	4.133
Observations	35,658,839	35,658,839	35,658,839	35,641,746	35,658,839
Age controls	✓	✓	✓	✓	✓
Individual effects	✓	✓	✓	✓	✓
Year effects	✓	✓	✓	✓	✓
Industry effects	✓				
Municipality effects			✓	✓	

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors are two-way clustered at the individual and firm levels. The sample is restricted to the largest connected set by gender. Average co-worker α represents the average worker fixed effects among co-workers at the individual's firm. High stress industry is an indicator variable equal to one if the industry's mental health prescription rate exceeds the sample median. The industry-level prescription rates are calculated as industry fixed effects from a two-way fixed effects model controlling for worker and industry effects. Industries are defined using the 2-digit Standard Business Classification 2008. ATC code N06 (psychoanaleptics, commonly used to treat depression).

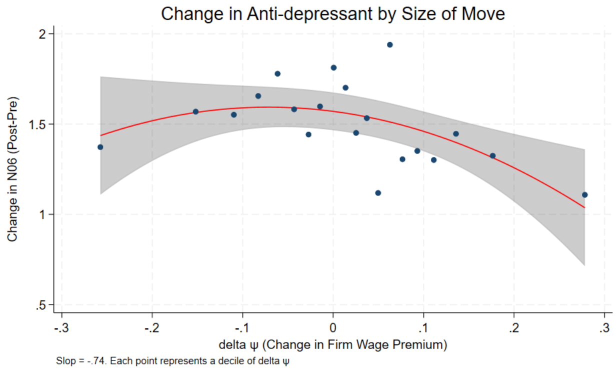
pay premiums across these moves. This strategy therefore allows for arbitrary differences between those moving with different frequency, including those who never move. By focusing exclusively on one-time movers, however, we work here with a somewhat narrower population than in our fixed effects analysis.

Figure 2, introduced in Section 4, shows that changes in firm pay premiums among movers have an interquartile range of roughly 15 log points (approximately 15 percent). For purposes

Figure 3: Changes in Medication Use by Size of Firm Premium



(a) Anti-anxiety (N05)



(b) Anti-depressant (N06)

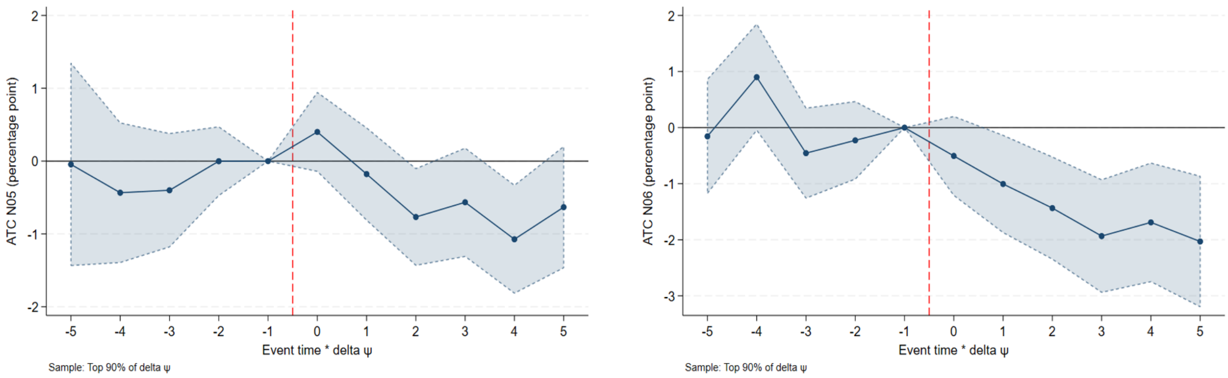
Note: The figure shows binned scatter plots of changes in mental health medication against changes in firm effects ($\Delta\psi$) upon job change, with a quadratic fit. Each point represents a ventile (5-percentile bin) of $\Delta\psi$. The sample is restricted to single movers. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

of the analytical choices that follow, we also indicate the bottom decile threshold, which corresponds to a decline in firm pay premium of approximately 13 log points. A simple interpretation of our identification strategy treats the change in firm pay premiums as exogenous conditional on making a move. The more theoretically-grounded interpretation, discussed in Section 4, recognises that positive wage changes (the right-hand side of this distribution) are likely driven by a job ladder mechanism in the spirit of Burdett & Mortensen (1998). Moves *down* the job ladder, by contrast, may reflect different underlying forces, such as workers escaping poor working conditions. We therefore additionally examine whether effects differ for a strict interpretation of the Burdett & Mortensen model with purely positive changes in firm premium.

Before proceeding to regression results, we explore this potential non-linearity non-parametrically. Figure 3 presents binned scatter plots relating changes in mental health medication use to changes in firm pay premiums, with each point representing a ventile of $\Delta\psi$. For both anti-anxiety (N05) and anti-depressant (N06) medications, the relationship is clearly negative over most of the distribution: workers who move to higher-paying firms experience reductions in medication use. However, the pattern diverges markedly in the left tail. Workers experiencing the largest pay premium declines (roughly those in the bottom decile) also show a decline in drug use relative to those in the centre of the $\Delta\psi$ distribution. This asymmetry motivates our decision to analyse the top 90% and bottom 10% of movers separately in the event study that follows.

With this in mind, Figure 4 presents event study estimates for both medication types around the job move, for our trimmed sample consisting of those in the top 90% of the $\Delta\psi$ distribution. Recall that estimates show the differential effect of an improvement in firm pay premium, relative to medication use in the year immediately preceding the move. The left-hand panel therefore shows that, four years after a move, workers transitioning to a firm with a 10 log point higher pay premium exhibit 0.1 percentage point lower use of anti-anxiety medication, relative to those

Figure 4: Event Study: Effect of Firm Premium Changes on Medication Use



(a) Anti-anxiety (N05)

(b) Anti-depressant (N06)

Note: The figures show event study estimation as specified in Equation 5 for the effects of changes in firm premiums upon a job change (i.e., $\pi_{i,t}$). ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

experiencing no change in premium. The effects on anti-depressant medication, shown in the right hand panel, are stronger still, with a nearly 0.2 percentage point reduction five years out, around 4% of mean usage.

A key advantage of the event study design is the ability to test pre-trends. Here we find no systematic differences: workers moving to firms with higher pay premiums do not exhibit differential trends in medication use prior to their move, at least within this sub-group. This is presented more systematically in Appendix Tables C.4 and C.5 which show the event study results in tabular form, separately for men and women, mirroring Tables 4 and 5. These results group estimates for event-time periods $[-5,-3]$ (pre-event), $[0,1]$ (short-term) and $[2,5]$ (longer-term), all relative to $[-2,-1]$ (eve-of-event). These tables show robustly that there are no systematic differences in the pre-event period. They also show robustness to a wide array of potential confounders, including, in addition to those presented in the earlier fixed-effect tables, firm age and origin- and destination-firm match effects.[12] In further unreported extensions we additionally interact event time with the origin and destination match effects, allowing medication trajectories to differ by match premium and thereby absorbing any residual dynamics based on these components; our estimates are unaffected. These tables also show some small differences in outcomes by gender, which we come back to later.

Our estimates are also robust to the treatment of the smallest firms in the AKM step, discussed in Section 4. Figure C.3 re-estimates the main event study on a firm sample that excludes firms with fewer than 15 workers, and yields near-identical results for both medication types.

The effect sizes shown in Figure 4 are noteworthy in a couple of related respects. First, they

[12]We do not control for match effects in the fixed effect analyses presented earlier, because these are, by construction, uncorrelated with firm premiums. We include them in the single-mover analysis because their correlation with firm premiums is nonzero in this restricted sample.

are somewhat larger than the corresponding fixed effects estimates discussed above, though given wide confidence intervals on our estimates and the different sample we do not pursue a direct comparison here. Second, and as a more important comparison, they are only around a third to a half smaller than the raw medication-wage gradients documented in Figure 1. Given the known importance of individual selection and reverse causation, we consider this a sizeable estimate. The closest study in design, Bíró & Elek (2025), finds the same negative relationship between firm pay and antidepressant use in Hungarian administrative data, though weaker in size, possibly because they do not isolate the non-linear relationship shown in Figure 3.

To set these magnitudes against the wider income–health literature, it is helpful to express our estimates in standard deviations of the outcome. We find that a 10 percent increase in firm pay premium lowers anti-depressant use by roughly 0.01 standard deviations.[13] For comparison, the meta-analysis of Thomson et al. (2022) places the effect of a 10 percent income increase, drawn largely from unearned or mixed sources, at just 0.003 standard deviations, around a third of our estimate. The most credible evidence on unearned income points to weaker and less durable effects. Most recently, in a randomised experiment paying $1,000 per month (over 50 percent of recipients' average personal income), Miller et al. (2024) find that self-reported stress improves by around 0.1 standard deviations in the first year but returns to baseline thereafter: when pooled over three years, their mental-health index rules out improvements larger than 0.028 standard deviations. The lottery studies of Cesarini et al. (2016) and Lindqvist et al. (2020) likewise find small long-run effects of wealth on mental health. Cesarini et al. (2016), whose outcome is prescription-drug consumption in administrative data much like ours, find a reduction of roughly 0.03 standard deviations per $140,000 of lottery winnings (roughly 70% of 10-year median personal income).[14] However, for earned income, and as mentioned previously, Reeves et al. (2017) find that the introduction of the UK national minimum wage, which gave a pay rise averaging 30% for their treated group, improved recipients' mental health by approximately 0.37 standard deviations.

In contrast to most of these studies, therefore, we find seemingly large and accumulating benefits of working at a higher-paying firm on mental health. Our takeaway is that these results suggest that employment conditions operate through different channels than non-wage income changes. A natural question is how these firm premiums relate to the most relevant non-wage amenity: job stress. As with the fixed effect analysis, we return to a more nuanced discussion of this now by examining differences across sectors.

[13] We express the event-study estimate in standard deviations by dividing the percentage-point effect by the standard deviation of the binary medication indicator, $\sqrt{p(1-p)}$, evaluated at the event-study sample mean ($p \approx 0.046$ for anti-depressants). As the standardised effects in this literature combine continuous and dichotomous outcome scales, this discussion should be read as a rough rather than precise comparison.

[14] Mental-health medication is the only health outcome for which Cesarini et al. (2016) detect a significant effect, and even then only when it is tested singly; once it is assessed jointly with their other health outcomes, correcting for multiple hypotheses, the result is no longer statistically distinguishable from zero.

Sector-Level vs Firm-Level Channels, and Sectoral Differences

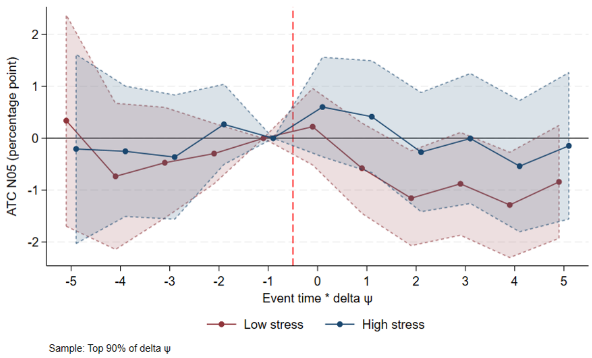
The main event study results show that moving to a higher-paying firm reduces mental health medication use. Before examining differences in effect sizes *across* sectors, we first consider the role of correlated sectoral changes as a possible *confounder*. Although the main specification controls for industry, workers who change sectors experience a simultaneous change in both firm pay premium and sector-level working environment. It is therefore possible that the mental health benefits of larger increases in firm pay premium reflect benefits of progressing *within* the sector of origin (and receiving a larger pay increase) compared to having to make a sector switch.

To assess whether such correlated sector-level changes are contributing to the estimated effect, Figure C.4 presents event study estimates separately for workers who remain within the same 1-digit industry when changing employers and those who move across industries, pooled across genders. Within-sector movers change firm premium while holding their sector-level working environment approximately constant. The results show broadly similar patterns for both groups. For anti-depressant medication, effects are significant in both subgroups; for anti-anxiety medication, point estimates are similar in magnitude but lack precision to reach significance individually. The similarity across the two groups indicates that sector-level amenity differences are not driving the main findings.

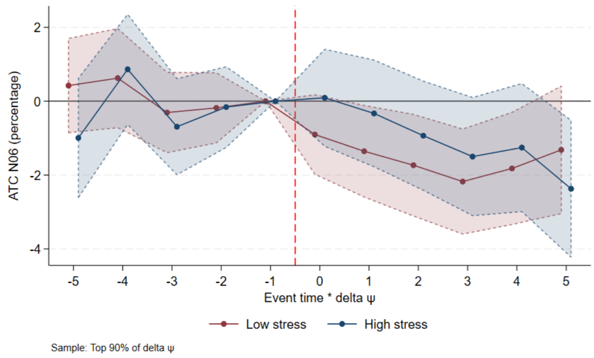
Having established that the effect operates primarily through firm-level rather than sector-level channels, we return to the question raised by our conceptual framework and the fixed effects results above: whether the pay–mental health gradient differs across sectors. As there, the two accounts make opposite predictions. If higher pay in high-stress sectors chiefly bought escape from stress, the gradient should be *steepest* there; if instead stress is a compensated disamenity, as prior evidence indicates, the gradient should *attenuate*, because more of the premium represents compensation than a pure income gain. Figure 5 presents event study estimates separately for workers in high-stress and low-stress industries, pooling across genders. For anti-anxiety medication, workers in low-stress industries exhibit a clear reduction in medication use following moves to higher-paying firms, while those in high-stress industries show no discernible effect throughout the post-move period. The pattern for anti-depressant medication is similar, though the contrast is less stark: low-stress sectors show a clear negative trajectory, while high-stress sectors exhibit noisier estimates closer to zero. In both cases the gradient attenuates in high-stress sectors rather than steepening, the direction predicted by compensation, not by stress reduction.

To gain further precision, Appendix Table C.6 groups the event study coefficients into pre- and post-move periods. The point estimate on the interaction with the high-stress indicator, shown in the second row, is generally positive, consistent with attenuation. The result is statistically significant at the 5% level for anti-depressant medication among males, consistent with the pattern of improving mental health being clearer for depression than for anxiety in our event study analysis. Given the number of subgroups examined, however, we interpret this result cau-

Figure 5: Event Study by Industry Stress Level



(a) Anti-anxiety (N05)



(b) Anti-depressant (N06)

Note: The figures show event study estimation as specified in Equation 5, estimated separately for workers in high-stress and low-stress industries. Industry stress level is defined by whether the industry's fixed effect, from a regression of mental health prescription on age and year effects, lies above or below the median, at the 2-digit SBI 2008 level. Sample is restricted to top 90% of the $\Delta\psi$ distribution, pooled across genders. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

tiously. Overall, therefore, the event study evidence is consistent with the compensating differentials pattern found in the fixed effects analysis, but does not establish it conclusively on its own. As discussed in Section 2, this relationship between firm pay and job stress is consistent with direct evidence that high-pressure work commands a compensating wage premium (Nagler et al., 2025; Humlum et al., 2025).

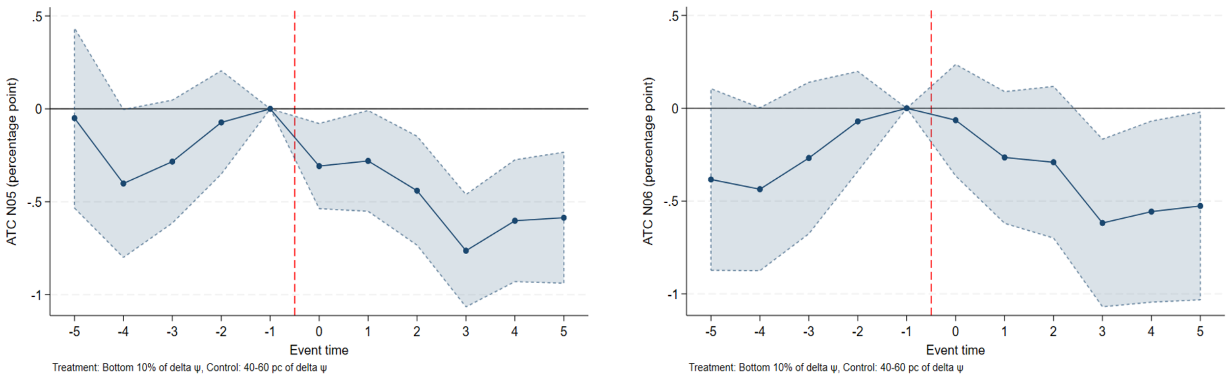
Heterogeneity by Characteristics

We augment these results by examining detailed outcomes for different subgroups. Overall, we continue to see clearer results for anti-depressant use than for anti-anxiety medications.

We start by splitting by gender and by age; results appear in Appendix Figure C.5. Both women and men show significant and similar effects for anti-depressant medication. However, consistent with the grouped event study estimates shown in Table C.4 and the fixed effects results in Table 4, estimates for anti-anxiety use among women are comparatively noisy and statistically insignificant. It is worth noting that our anti-depressant effects being, if anything, somewhat stronger for women stands in contrast to Bíró & Elek (2025), whose comparable Hungarian estimates are significant only among men and older workers. Turning to age, workers who switch jobs before age 45 and those who are older both show similar and significant reductions in anti-depressant use. Results for younger workers are marginally significant for anti-anxiety medications, while estimates for the older group are less precise.

Figure C.6 presents results for workers with only positive changes in firm pay premium. As emphasized in Section 4 this is the subset where the Burdett-Mortensen job ladder argument for quasi-random assignment of $\Delta\psi$ is most direct. The evidence for N06 is clear and consistent with the main sample, with stable pre-trends and a reduction in use post-move. We see the same

Figure 6: Event Study: Workers with Large Downward Moves (Bottom 10% of $\Delta\psi$)



(a) Anti-anxiety (N05) **(b)** Anti-depressant (N06)

Note: The figures plot average mental health medication use, by event time around the job move, for workers in the bottom decile of the $\Delta\psi$ distribution (those experiencing the largest declines in firm premium) relative to workers in the middle decile of the distribution. Unlike the main event study, this is a binary comparison between groups rather than the continuous-treatment specification in Equation 5. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

reduction post-move for N05, though the pre-event pattern is less stable than for the main sample. The bottom panel splits the sample by origin firm premium level. Effects are concentrated among workers starting at low-premium firms, with no significant result for those already at high-premium employers. Pre-trends remain stable for N06 across both groups, although for N05 the pre-event estimates are again noisier. The results in the bottom panel provide loose confirmation that we see strongest support for our argument by workers with pure pay *increases*, who are inevitably found more in below-median paying origin firms.

Large Declines in Firm Premiums

The preceding analysis deliberately excluded workers in the bottom decile of the $\Delta\psi$ distribution: those experiencing the largest pay premium declines. As conjectured in Section 4, these workers are plausibly affected by forces outside the Burdett & Mortensen job ladder, and the descriptive evidence in Figure 3 already indicated a distinct pattern of medication use around their job changes. With this in mind, we therefore perform an event study for this group separately, with results shown in Figure 6. Now rather than a continuous treatment outcome, this Figure plots average outcomes for this decile compared to the average for the middle decile of the $\Delta\psi$ distribution.

The pattern shown in Figure 6 differs markedly from that in the main sample. Rather than flat pre-trends followed by a post-move decline, medication use seems to spike in the years *preceding* the job transition. There is also a gradual decline after the move, despite the reduction in pay. Although the smaller sample here reduces precision, augmenting the main specification by adding a time trend in the pre-event period yields a significant pre-trend for anti-depressants (p-value =

0.012).[15] However, as is apparent from the estimate for $\tau = -5$ in the left-hand panel, the trend is insignificant for anti-anxiety medication.

This pattern is consistent with a selection mechanism by which deteriorating mental health precipitates job separation. Workers in stressful or otherwise unsuitable positions may experience worsening mental health, eventually leading them to leave for lower-paying but less demanding employers. The subsequent improvement in mental health outcomes suggests these workers benefit from escaping adverse working conditions, even at the cost of lower pay. This interpretation is consistent with the mechanism in Jolivet & Postel-Vinay (2025), whereby workers who experience adverse health shocks face negative career consequences.

Taken together, the event study results reveal a two-way but clear relationship between firm pay premiums and mental health. For the majority of job movers, changes in firm pay premiums lead to a positively correlated improvement in mental health outcomes, consistent with a causal effect of pay on wellbeing. For a subset of workers making large downward moves, however, the direction of causality is reversed: declining mental health (specifically depression) precedes and contributes to job transitions.

6 Conclusions

This paper investigates how firm-specific wage premiums affect worker mental health using Dutch administrative data linking employer-employee records to prescription drug use over 2006-2022. We decompose wages using the AKM framework, adapting estimation to address limited mobility bias by grouping smaller firms, following recent literature. Using individual fixed-effects estimation and an event study mover design following Finkelstein et al. (2016), we examine both the cross-sectional relationship between firm pay premiums and medication use and the dynamic patterns around job transitions.

Our results reveal a two-way relationship between firm pay and mental health. Broadly, conditional on individual fixed effects, higher firm pay premiums are associated with substantially lower use of both anti-anxiety and anti-depressant medications: individual fixed effects absorb between one-third and three-quarters of the raw wage-health gradient, leaving estimates we regard as sizeable. In high-stress industries, however, the beneficial effect of firm pay on mental health is attenuated to zero for males, consistent with compensating differentials operating where work directly threatens psychological well-being. Event study analysis confirms these effects are causal for the bulk of job transitions: changes in medication use follow rather than precede moves to higher-paying firms. However, workers experiencing large wage cuts show elevated use of anti-depressants *before* their move, falling *after* it: for this group declining mental health appears to precede and precipitate a move to a less demanding, lower-paying position, reversing the direction of causality.

[15] A cruder comparison of medication use between $\tau = -1$ and $\tau = -4$ yields a p-value of 0.06 for both anti-anxiety and anti-depressant medications.

These findings carry implications for industrial and competition policy. Our results suggest that policies affecting market structure will have consequences for worker well-being that extend beyond standard wage effects. That said, the relationship depends on the source of market power: To the extent that firms possess product market power and share resulting rents with workers through higher pay, policies reducing monopoly power may diminish the mental health benefits workers currently enjoy from elevated firm premiums. Conversely, our finding that higher firm pay causally improves mental health implies that monopsony power in labour markets imposes costs on worker well-being through the suppression of wages. The two cases then differ in how they align the interests of workers and shareholders. In the case of monopsony power in particular, and if greater shareholder wealth leads to improved mental health, our results imply that market power accentuates not only resource differences but mental health differences across income types.

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A Conceptual Framework: Details and Extensions

This appendix formalises the conceptual framework introduced in Section 2. Section A.1 presents the single-amenity baseline, following Sorkin (2018), and shows how the covariance of firm pay with amenities can take either sign. Section A.2 sketches the extension to a composite amenity comprising mental-health and other components, with sector-specific cost structure. Section A.3 derives the central interpretive expression for the within-sector pay–mental-health gradient.

A.1 Single-Amenity Baseline

Consider a firm j with revenue R_j, a single composite amenity a_j, and amenity-provision cost c_j. The firm chooses pay premium Ψ_j and amenity level a_j to maximise profits subject to a hiring constraint:

$$\max_{\Psi_j, a_j} R_j - c_j a_j - \exp(\Psi_j) \qquad \text{s.t.} \quad \zeta(\Psi_j + \ln a_j) \geq V_j, \tag{6}$$

where V_j is promised utility and $\zeta > 0$ scales utility into wage units. Substituting the binding constraint into the objective and taking first-order conditions yields:

$$\Psi_j = \tfrac{1}{2}(V_j/\zeta + \ln c_j), \qquad \ln a_j = \tfrac{1}{2}(V_j/\zeta - \ln c_j). \tag{7}$$

Pay is increasing in promised utility and in the cost of providing amenities: the former captures rents passed through to workers, the latter the compensating-differential margin. Defining $v_j \equiv V_j/\zeta$ and $\ell_j \equiv \ln c_j$, the within-population covariance of pay and amenities is

$$\text{Cov}(\Psi_j, \ln a_j) = \tfrac{1}{4}(\sigma_v^2 - \sigma_\ell^2),$$

which can take either sign. When variation in V_j dominates, more productive or less constrained firms pay more on both margins and the covariance is positive (a Mortensen channel). When variation in c_j dominates, firms with cheaper amenities deliver them in lieu of higher pay and the covariance is negative (a Rosen channel). The sign of the covariance is, in general, an empirical question.

A.2 Two Amenities and Sector-Specific Costs

To connect the framework to mental health, decompose the composite amenity into a mental-health component a_j^M (capturing job stress, autonomy, and work–life balance) and other amenities a_j^O (capturing location, perks, and office quality). We adopt a Cobb–Douglas aggregator with constant returns to scale:

$$a_j = (a_j^M)^\lambda (a_j^O)^{1-\lambda}, \qquad \lambda \in (0, 1), \tag{8}$$

where the preference weight λ is common across sectors. Amenity costs combine a firm-specific efficiency component with sector-specific relative prices: $c_j^M = c_j \cdot c_s^M$ and $c_j^O = c_j \cdot c_s^O$, where c_s^M captures the relative cost of providing mental-health amenities in sector s. In sectors where the work itself is inherently demanding, providing good mental-health amenities is more costly, so c_s^M is high.

Standard cost minimisation over (a_j^M, a_j^O) for given a_j, followed by application of the firm's joint choice over (Ψ_j, a_j), reduces the problem to one structurally identical to (6) with an effective firm cost that combines c_j with a sector aggregator ϕ_s of relative amenity prices. The resulting solutions are:

$$\Psi_j = \tfrac{1}{2}\big(v_j + \ell_j + \ln\phi_s\big), \tag{9}$$

$$\ln a_j = \tfrac{1}{2}\big(v_j - \ell_j - \ln\phi_s\big). \tag{10}$$

Mental health from working at firm j then takes the form

$$M_j = \gamma\,\Psi_j + \theta\,\ln a_j + \kappa_s, \tag{11}$$

where the sector-specific constant κ_s is decreasing in c_s^M: workers in sectors where mental-health amenities are costly to provide have worse baseline mental health, reflecting equilibrium substitution toward other amenities. Both $\ln\phi_s$ and κ_s enter as constants within sector and are absorbed by industry fixed effects.[16]

A.3 The Pay–Mental-Health Gradient Across Sectors

Substituting (9)–(10) into (11), mental health within sector s is

$$M_j = \tfrac{1}{2}(\gamma+\theta)\,v_j + \tfrac{1}{2}(\gamma-\theta)\,\ell_j + \text{const}_s. \tag{12}$$

Setting the within-sector covariance $\sigma_{v\ell,s} = 0$ for transparency,[17] and defining the compensating-differentials share of pay-premium variance in sector s,

$$\rho_s \equiv \frac{\sigma^2_{\ell,s}}{\sigma^2_{v,s} + \sigma^2_{\ell,s}}, \tag{13}$$

the within-sector regression coefficient of M_j on Ψ_j simplifies to

$$\boxed{\beta_s = \gamma + \theta\,(1 - 2\rho_s).} \tag{14}$$

This is the central interpretive expression. The gradient equals $\gamma + \theta$ when $\rho_s = 0$ (pure rents), γ at $\rho_s = 1/2$, and $\gamma - \theta$ at $\rho_s = 1$ (pure compensating differentials). It is decreasing in ρ_s throughout. Sectors in which a larger share of firm-pay-premium variance reflects compensating differentials therefore exhibit an attenuated pay–mental-health gradient. The empirical finding that

[16] Full expressions for the relevant constants are: $\kappa_s \equiv \theta\,(1-\lambda)\,\ln\!\left(\frac{\lambda\cdot c_s^O}{(1-\lambda)\,c_s^M}\right)$ and $\phi_s \equiv \left(\frac{c_s^M}{\lambda}\right)^{\lambda}\left(\frac{c_s^O}{1-\lambda}\right)^{1-\lambda}$.

[17] Allowing $\sigma_{v\ell,s} \neq 0$ yields an amendment to expression 14 that follows: $\beta_s = \gamma + \theta(\sigma^2_{v,s} - \sigma^2_{\ell,s})/(\sigma^2_{v,s} + 2\sigma_{v\ell,s} + \sigma^2_{\ell,s})$. The deviation of β_s from γ retains the sign of $\sigma^2_{v,s} - \sigma^2_{\ell,s}$; a positive covariance shrinks its magnitude.

the gradient is attenuated in high-stress sectors is consistent with the conjecture, advanced in Section 2, that compensating differentials are larger in those sectors.

It is worth remarking that the framework rationalises the conjecture but does not generate it: ρ_s is determined by the joint distribution of (V_j, c_j) within each sector, which is not pinned down by the model's primitives. There are nevertheless good reasons to expect ρ_s to be larger in high-stress sectors. Within any sector, ρ_s will be determined by the shape of provision of amenities that are most important there. In high-stress sectors, job stress forms a more important component of the bundle of (dis-)amenities. We therefore conjecture that ρ_s will be larger in high-stress sectors, given the evidence in Nagler et al. (2025) and Humlum et al. (2025) that job stress is a compensated disamenity, while other amenities, such as job location or flexibility, covary positively with firm pay.

Two features of the framework are worth noting. First, under constant returns to scale in amenity provision, the structural amenity coefficient θ does not vary by sector: all sectoral heterogeneity in β_s operates through ρ_s. Sector variation in θ would require relaxing CRS. Second, ρ_s is in principle measurable using sector-by-sector variance decompositions in the spirit of Sorkin (2018), although the data requirements are demanding and we leave such an exercise to future work.

Table B.1: Composition of Medication Use by Subcategory

Medication	Example	Percent
N05, Psycholeptics (Anti-anxiety)		
N05A, *Antipsychotics*	Olanzapine	32.3%
N05B, *Anxiolytics*	Diazepam (Valium)	43.8%
N05C, *Hypnotics and sedatives*	Temazepam	23.9%
N05 Total		100.0%
N06, Psychoanaleptics (Anti-depressant)		
N06A, *Antidepressants*	Sertraline	76.4%
N06B, *Psychostimulants / ADHD agents*	Methylphenidate (Ritalin)	23.3%
N06D, *Anti-dementia drugs*	Donepezil	0.4%
N06 Total		100.0%

Note: The table shows the share of each subcategory within ATC code N05 (psycholeptics) and N06 (psychoanaleptics), expressed as a percentage of total prescriptions within each category, pooled across the full sample period 2006–2022. Each example is a representative generic agent in the subcategory; for a few widely recognised drugs the brand name is given in brackets. Fixed-dose combinations of psycholeptics and psychoanaleptics (N06C) are not recommended in the Dutch primary-care depression guideline (Nederlands Huisartsen Genootschap, 2019) and are virtually absent from national prescription data (Zorginstituut Nederland, 2026); we therefore do not report them.

B Further Details on Data and Methodology

Sections B.1–B.3 provide additional detail complementing the data description in Section 3. Section B.4 describes the grouping of small firms underlying the AKM estimation in Section 4, and Section B.5 the classification of high- versus low-stress sectors used in the empirical analysis in Section 5. Section B.6 describes how the key regression variables are constructed.

B.1 Workers' Employment

As stated in Section 3, we identify employed individuals using SPOLISBUS, an administrative employer-employee dataset compiled by CBS from wage tax declarations (*Polisadministratie*). It covers all employment relationships (*inkomstenverhoudingen*) between Dutch employers and workers that fall under payroll taxation (*loonheffing*) and involve at least 4 paid hours within the reporting month. The dataset excludes the self-employed and very short-tenure jobs below this hours threshold. These exclusions are unlikely to affect our results, as self-employment is concentrated in micro-firms outside our analytical sample.

B.2 Mental Health Measurement: Further Details

ATC Classification and Subcategories

Our N05 (psycholeptics) measure comprises antipsychotics (N05A), anxiolytics (N05B), and hypnotics/sedatives (N05C). Our N06 (psychoanaleptics) measure comprises antidepressants (N06A), psychostimulants (N06B), and anti-dementia drugs (N06D). Table B.1 reports the composition of prescriptions across these subcategories.

Coding of Prescription Outcomes

If an individual appears in the BRP in a given year but has no record of an N05 or N06 prescription, we code the outcome as zero. Gaps in prescription sequences are rare: among person-years for individuals ever prescribed N05, 0.64% exhibit a one-year gap followed by resumption and 0.28% a two-year gap. For N06, these figures are 0.77% and 0.38%, respectively. Because such gaps are infrequent, the coding choice has little bearing on our results. Where gaps do occur, we treat them as genuine zeros rather than imputing continued treatment: temporary interruptions are a normal feature of pharmacological treatment courses (Liu et al., 2021), so a gap year reflects a true pause in prescribing rather than missing data.

ATC Reclassifications

The ATC classification system has undergone periodic revisions over our study period. Most reclassifications for psychotropic medications occur within the same three-digit category (e.g., N05AX09 → N05AH06 in 2010; N05CM17 → N05CH01 in 2008), maintaining their classification as either N05 or N06. A small number of drugs were reclassified into (e.g., N07BA02 → N06AX12 in 2009; N01AX01 → N05AD08 in 2013) or out of (e.g., N05AK01 → N07XX06 in 2008) the N05/N06 categories. Calendar-year fixed effects, included in all specifications, absorb any resulting level shifts in measured prescription rates, consistent with the approach of Bögl et al. (2024).

A distinct source of temporal variation is the 2009 change in Dutch reimbursement criteria for anxiolytics (N05B) and hypnotics/sedatives (N05C), which tightened conditions for prescription coverage. This policy produced a level shift in N05 prescription rates but does not affect antidepressant (N06) prescriptions. For our event study analysis, the policy change falls strictly within the pre-move period for all cohorts of job changers (2011–2017) and is absorbed by calendar-year fixed effects.

Exclusions from Medicijntab

Medicijntab includes prescriptions dispensed by outpatient hospital pharmacies, but excludes medications provided directly within hospitals and under the AWBZ/WLZ long-term care systems. These exclusions are largely irrelevant for our population of labour-market-attached individuals.

B.3 Sample Construction

Full Sample

When an individual works for multiple firms in a given calendar year, we retain only the primary employer, defined as the firm where the individual worked the most hours. This ensures each worker-year observation corresponds to a single employer and captures the work environment to which the worker was most exposed, consistent with the approach used by Abraham et al. (2010) and Jolivet & Postel-Vinay (2025).

We exclude observations with annual hours exceeding 4,380 or total annual earnings exceeding €1,000,000. Log real hourly wages are winsorized at the 1st and 99th percentiles of the pooled sample distribution, following Åslund et al. (2023). The Tax Register contains fewer than 1% missing values in hours worked and earnings; these observations are excluded, leaving no missing values in the analytical sample.

To contribute to the sample, firms are required to have at least 2 years of observed data, at least 2 workers in each year, and non-missing value added. Employer data are merged through the *beid* identifier, which corresponds to the CBS statistical unit *bedrijf* (business unit).[18]

Following standard practice (e.g., Card et al., 2013, 2016; Åslund et al., 2023), we restrict AKM estimation to workers and firms in the largest connected set. We estimate the AKM model separately by gender, reflecting substantial differences in both wage levels and the wage–prescription relationship (as shown in Figure 1).

Event Study Sample

As in Section 3, for the event study we focus on workers who experienced a single job change between 2011 and 2017. The restriction to a single transition ensures that changes in prescription patterns can be attributed to that specific mobility event. The 2011–2017 window for job changes ensures the full five-year event period ($\tau \in [-5, 5]$) falls within our data period of 2006–2022.

We require at least six observations during the event window, including at least two periods at both the origin and destination firms, and a non-missing observation at $\tau = -1$. These restrictions ensure we can distinguish pre-move from post-move dynamics and estimate individual-level trends. The final event study sample comprises 62,129 female and 124,866 male individuals.

B.4 Firm Grouping for AKM Estimation

To minimise limited mobility bias in the AKM estimation, we group firms with fewer than 50 workers following Bonhomme et al. (2019) and Åslund et al. (2023). Firm size is measured as mean employment across all observed years, and thus is time invariant.

Grouping requires a single, time-invariant index of each small firm's productivity. We obtain it from the firm-specific intercept in a regression of log value-added per worker on year fixed effects, estimated on firms with mean employment below 50:

[18] The *bedrijf* is defined as the coordinated statistical unit in the General Business Register (ABR), characterised by autonomy and external orientation. It corresponds most closely to the enterprise concept, though for large diversified groups a single *beid* may consolidate multiple legal entities. In practice, for the vast majority of firms the business unit aligns with the standard notion of an enterprise. Each business unit may comprise multiple establishments (*vestigingen*) across different municipalities.

$$\omega_{jt} = \xi_j + \text{year}_t + \epsilon_{jt}$$

where ω_{jt} is productivity, measured by log value-added per worker of firm j in year t, year_t are year fixed effects, and ϵ_{jt} is the error term. The firm effect ξ_j is simply the firm's average log value-added per worker net of common year effects. We then group firms into deciles of $\hat{\xi}_j$. Firms with 50 or more employees remain ungrouped and are treated as individual units in the AKM estimation.

B.5 Identification of High-stress Sectors

To identify high-stress sectors, we estimate industry-specific mental health prescription rates using industry fixed effects. Specifically, we estimate separate regressions by gender of mental health prescription (an indicator for any N05 or N06 prescription) on age (linear and quadratic terms), year fixed effects, and industry fixed effects:

$$m_{it} = \lambda_i + \eta_{s(i,t)} + X_{it}\beta + v_{it}$$

where m_{it} is a binary indicator equal to one if individual i was prescribed either N05 or N06 in year t, λ_i is an individual fixed effect, $\eta_{s(i,t)}$ denotes the sector in which the individual works in year t, X comprises the age controls and year effects, and v_{it} is the error term. The estimated industry fixed effects $\hat{\eta}_s$ capture industry-specific prescription rates, adjusted for age composition and secular trends.

We classify an industry as high-stress if its estimated fixed effect exceeds the gender-specific sample median:

$$\text{HighStress}_s \begin{cases} = 1 \text{ if } \hat{\eta}_s > \text{med}_g(\hat{\eta}) \\ = 0 \text{ if } \hat{\eta}_s \leq \text{med}_g(\hat{\eta}) \end{cases} \tag{15}$$

where $\text{med}_g(\hat{\eta})$ is the median of the estimated industry effects computed separately for each gender g, consistent with the gender-specific first-stage regressions above.

Industry classification is based on the 2-digit SBI, as recorded at the time of firm registration in the ABR.

B.6 Variable Construction

Table B.2 describes how we construct the key variables used in our regression specifications.

Table B.2: Construction of Key Variables

Variable name	Construction
Firm age	Years since the firm's registration date recorded in the Firm Register (ABR).
Firm size	Employee headcount recorded in the Tax Register (SPOLIS).
Tenure	Number of years the individual is observed at the firm.
Firm premium	Firm-specific wage component estimated from the AKM model.
Match quality	Residual component from the AKM model, averaged over the worker's tenure.
High-stress industry	An indicator variable equal to one if the industry's mental health prescription rate exceeds the sample median. The industry prescription rate is extracted from industry fixed effects on mental health prescription (either N05 or N06).
Municipality effects	342 municipalities (LAU 2 level) capturing the firm's headquarters location.

Table C.1: Distribution of Panel Length in the Event Study Sample

Obs. per individual	Frequency	Percent	Cumulative
6	10,264	5.49	5.49
7	10,877	5.82	11.31
8	13,531	7.24	18.54
9	13,998	7.49	26.03
10	13,590	7.27	33.30
11	13,379	7.16	40.45
12	13,873	7.42	47.87
13	16,972	9.08	56.95
14	13,319	7.12	64.07
15	14,458	7.73	71.80
16	15,234	8.15	79.95
17	37,488	20.05	100.00
Total	186,983	100.00	

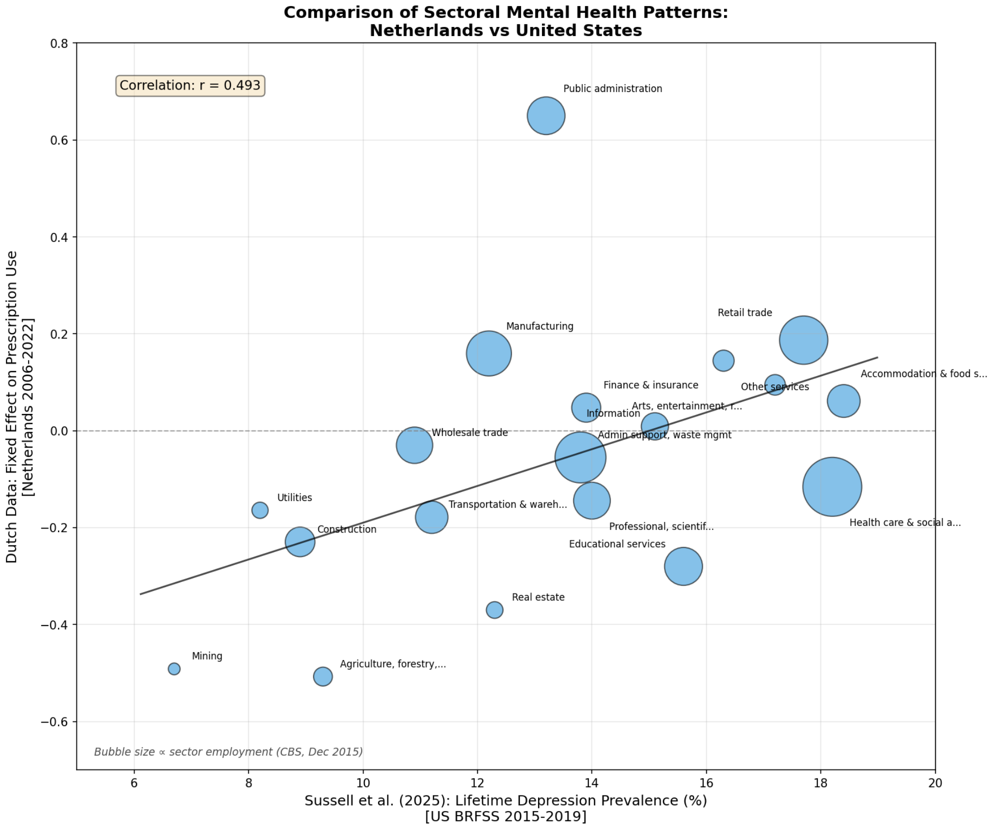
C Additional Results

Table C.2: Sample Construction: Full, Connected, and Grouped-Firm Samples

	Female			Male		
	Full set	Connected set	Grouped-firm	Full set	Connected set	Grouped-firm
Sample counts						
Unique firms	90,601	88,971	19,780	95,478	94,976	19,862
Unique workers	3,742,213	2,947,390	2,947,390	5,397,149	4,527,739	4,527,739
Distribution of movers (worker-weighted)						
10^{th} percentile	3	3	8	1	1	5
50^{th} percentile	102	101	213	36	35	95
90^{th} percentile	4,169	4,070	9,566	2,088	1,937	8,902
Log wage						
Variance	0.229	0.221	0.221	0.243	0.236	0.236

Notes: This table presents sample characteristics across three analytic samples. Full set includes all workers and firms in the raw data with valid wage observations. Connected set restricts to the largest connected component, the subset of workers and firms linked through worker mobility, ensuring all firm effects are identified relative to a common reference point. Grouped-firm further aggregates smaller firms into groups based on productivity, maintaining connectedness; firms within the same group are treated as a single entity. The distribution of movers shows the number of workers who have moved to or from each firm (worker-weighted percentiles), reflecting the size distribution of firms by mobility connections.

Figure C.1: Cross-Country Validation of Sectoral Mental Health Rankings



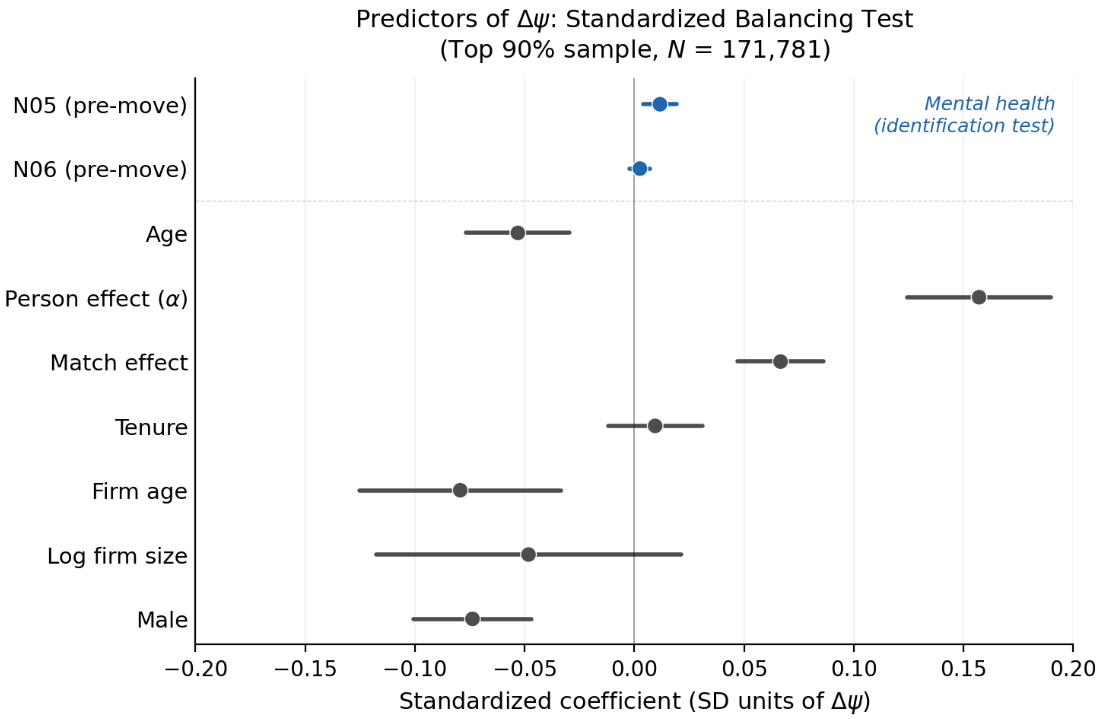
Note: Each bubble represents a sector mapped from Dutch SBI 2008 codes to US NAICS classifications. The horizontal axis shows lifetime depression prevalence from Sussell et al. (2025, JAMA Network Open) using US BRFSS data (2015–2019). The vertical axis shows Dutch industry fixed effects on mental health prescription use (2006–2022), estimated separately by gender and aggregated to the industry level as a worker-weighted average. Bubble size is proportional to sector-wide employment in the Netherlands (CBS, December 2015). The solid line shows the OLS regression fit ($r = 0.49$).

Table C.3: Balancing Table: Pre-Move Characteristics and $\Delta\psi$

	(1) Full	(2) Top 90%	(3) $\Delta\psi > 0$
N05 (pre-move)	0.00562	0.00894***	0.000596
	(0.00357)	(0.00294)	(0.00232)
N06 (pre-move)	−0.000555	0.00133	−0.000225
	(0.00154)	(0.00129)	(0.00119)
Age	−0.000131	−0.000494***	−0.000901***
	(0.000255)	(0.000112)	(8.02e-05)
Person effect (α)	0.0513***	0.0484***	0.0460***
	(0.00643)	(0.00517)	(0.00455)
Match effect	0.0382***	0.0364***	0.0259***
	(0.0136)	(0.00546)	(0.00538)
Tenure	0.00201**	0.000438	−0.000930**
	(0.000834)	(0.000506)	(0.000441)
Firm age	−0.00115***	−0.000584***	−0.000274**
	(0.000335)	(0.000172)	(0.000130)
Log firm size	−0.00710**	−0.00347	0.00153
	(0.00307)	(0.00255)	(0.00122)
Male	−0.0192***	−0.0175***	−0.0227***
	(0.00369)	(0.00324)	(0.00333)
Constant	−0.0613**	−0.0445**	0.00590
	(0.0268)	(0.0219)	(0.0163)
Observations	186,983	171,781	105,187
R^2	0.071	0.058	0.066

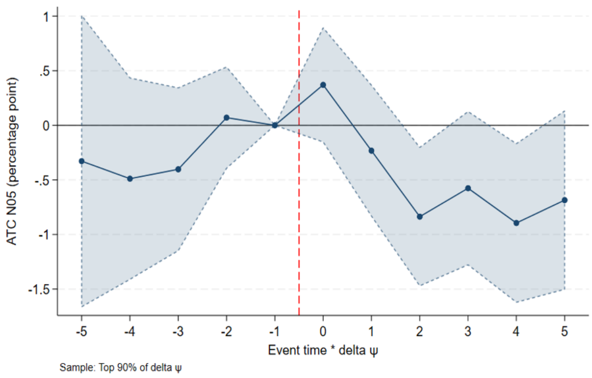
Notes: Dependent variable is $\Delta\psi$. Standard errors clustered at the pre-move firm level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All specifications include year effects. Column (1) for full sample, Column (2) for top 90% of movers by $\Delta\psi$, and Column (3) for those who moved to higher ψ.

Figure C.2: Balance Test

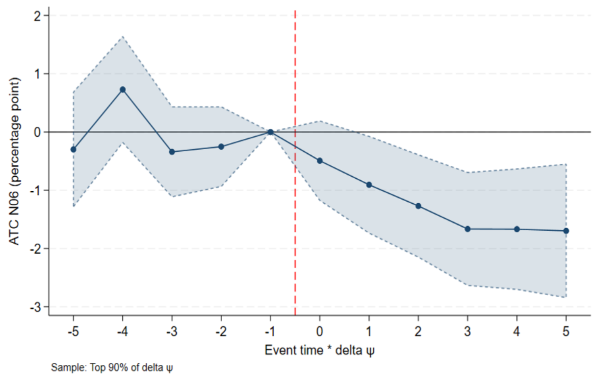


Note: This figure presents standardized coefficients from balancing tests regressing the change in firm effects ($\Delta\psi$) on pre-move worker and firm characteristics. The dependent variable measures the difference in firm pay premiums between the destination and origin firms. All independent variables are measured in the year prior to the move (t = -1). Coefficients are standardized by multiplying the raw coefficient by the standard deviation of the regressor and dividing by the standard deviation of $\Delta\psi$ (SD = 0.111), allowing for comparison of effect sizes across variables measured in different units. Sample restricted to the top 90% of movers by $\Delta\psi$ (N = 171,781). Standard errors clustered at the pre-move firm level. 95% confidence intervals shown.

Figure C.3: Event Study: Robustness to Restricting the Firm Sample



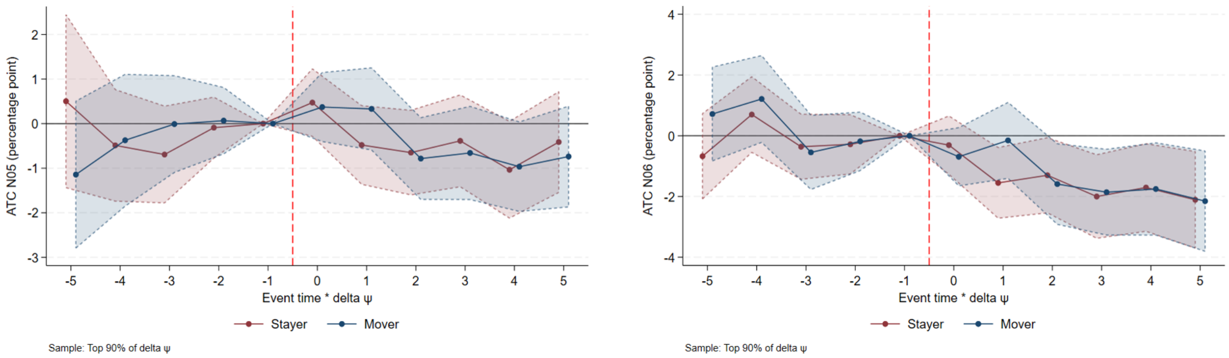
(a) Anti-anxiety (N05)



(b) Anti-depressant (N06)

Note: The figures show event study estimation as specified in Equation 5 for the effects of changes in firm premiums upon a job change (i.e., $\pi_{i,t}$). ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression). The sample is restricted to firms with at least 15 employees.

Figure C.4: Event Study: Within-Sector vs Cross-Sector Movers

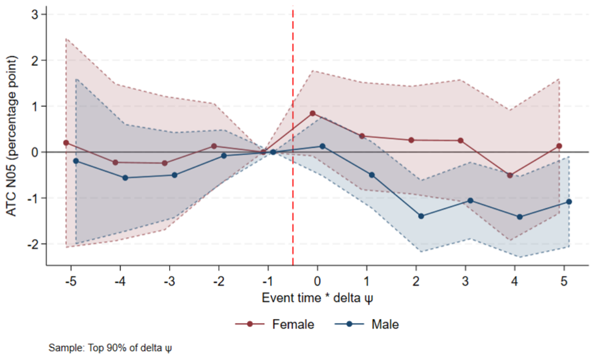


(a) Anti-anxiety (N05) by Mover/Stayer

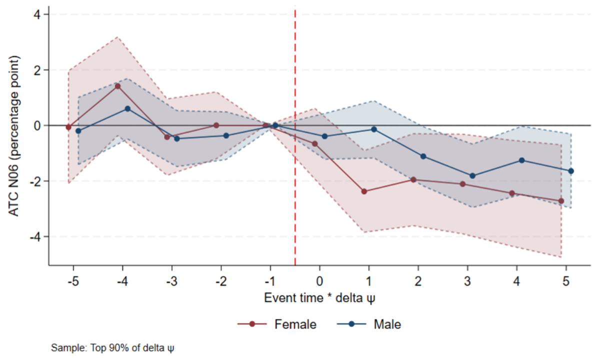
(b) Anti-depressant (N06) by Mover/Stayer

Note: Event study estimates as specified in Equation 5, estimated separately for workers who change employer within the same 1-digit industry (SBI 2008) and those who move across industries. Panel (a) shows anti-anxiety medication (N05) and panel (b) anti-depressant medication (N06). The sample is restricted to single movers whose change in ψ is within the top 90% of the distribution, pooled across genders. Shaded areas represent 95% confidence intervals with standard errors two-way clustered at the individual and firm levels. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

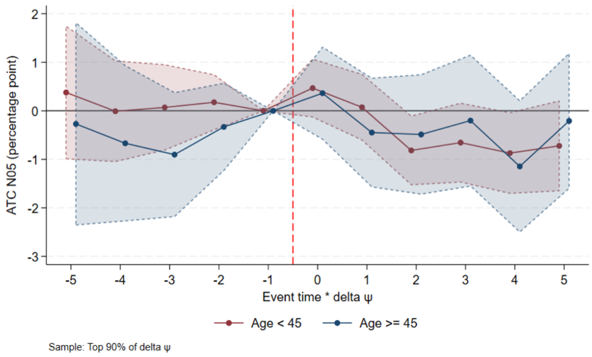
Figure C.5: Event Study by Gender and Age



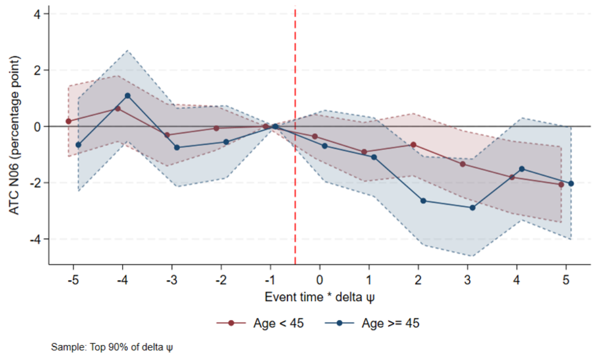
(a) Anti-anxiety (N05) by Gender



(b) Anti-depressant (N06) by Gender



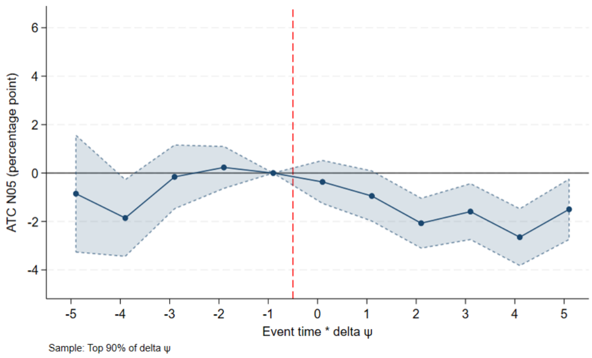
(c) Anti-anxiety (N05) by Age



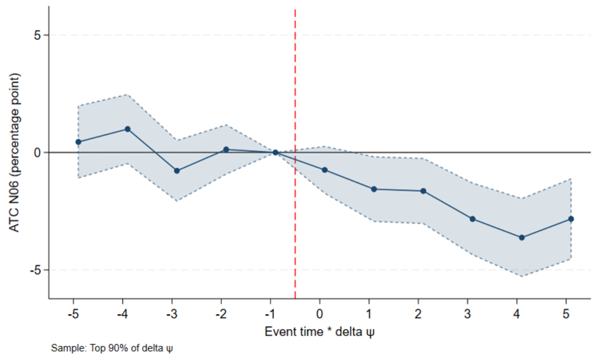
(d) Anti-depressant (N06) by Age

Note: Event study estimates as specified in Equation 5. Panels (a) and (b) split the sample by gender; panels (c) and (d) by age group (below/above 45). The left column shows anti-anxiety medication (N05) and the right column anti-depressant medication (N06). The sample is restricted to single movers whose change in ψ is within the top 90% of the distribution. Shaded areas represent 95% confidence intervals with standard errors two-way clustered at the individual and firm levels. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

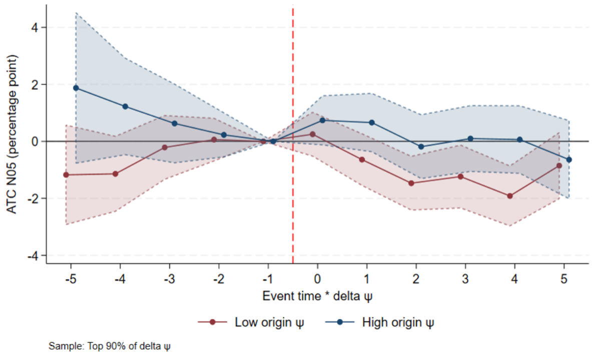
Figure C.6: Event Study: Upward Movers and Origin Firm Premium



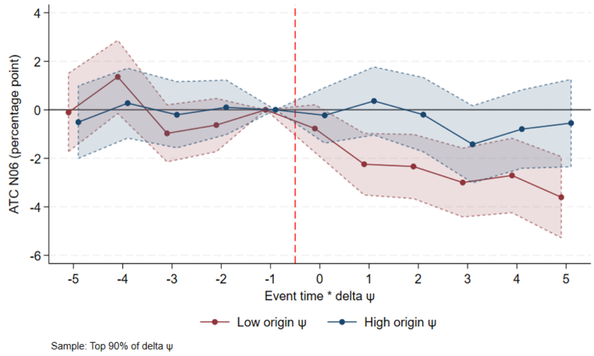
(a) Anti-anxiety (N05) for those with Positive $\Delta\psi$



(b) Anti-depressant (N06) for those with Positive $\Delta\psi$



(c) Anti-anxiety (N05) by Origin Firm Effect



(d) Anti-depressant (N06) by Origin Firm Effect

Note: Event study estimates as specified in Equation 5. Panels (a) and (b) restrict to workers with positive $\Delta\psi$ (upward moves on the job ladder); panels (c) and (d) split the sample by origin-firm premium (above or below the median pre-move ψ). The left column shows anti-anxiety medication (N05) and the right column anti-depressant medication (N06). The sample is restricted to single movers whose change in ψ is within the top 90% of the distribution, pooled across genders. Shaded areas represent 95% confidence intervals with standard errors two-way clustered at the individual and firm levels. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).

Table C.4: Grouped Event Study Results: Top 90% Sample (N05)

	(1)	(2)	(3)	(4)
Panel A: Female				
Pre-event$\times\Delta\psi$	-0.156	-0.185	-0.145	-0.119
	(0.662)	(0.663)	(0.657)	(0.657)
Post-event-short$\times\Delta\psi$	0.547	0.337	0.114	0.117
	(0.431)	(0.434)	(0.470)	(0.470)
Post-event-long$\times\Delta\psi$	-0.00342	-0.216	-0.545	-0.568
	(0.501)	(0.503)	(0.513)	(0.511)
Observations	513,120	513,120	513,110	512,304
Mean	2.6935	2.6935	2.6935	2.6935
Panel B: Male				
Pre-event$\times\Delta\psi$	-0.390	-0.398	-0.370	-0.375
	(0.438)	(0.437)	(0.437)	(0.438)
Post-event-short$\times\Delta\psi$	-0.142	-0.0723	-0.0974	-0.109
	(0.278)	(0.274)	(0.289)	(0.288)
Post-event-long$\times\Delta\psi$	-1.206^{***}	-1.132^{***}	-1.140^{***}	-1.147^{***}
	(0.352)	(0.345)	(0.355)	(0.355)
Observations	1,083,919	1,083,919	1,083,914	1,083,833
Mean	1.8766	1.8766	1.8766	1.8766
Event period FE	✓	✓	✓	✓
Match effects	✓	✓	✓	✓
Log(workers)		✓		
Firmage		✓		
Coworker α			✓	
Ind FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
SBI FE	✓			
Municipality FE			✓	✓

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ Standard errors are two-way clustered at the individual and firm levels. The sample is restricted to the largest connected set by gender. Average co-worker α represents the average worker fixed effects among co-workers at the individual's firm. Industries are defined using the 2-digit Standard Business Classification 2008. ATC code N05 (psycholeptics, commonly used to treat anxiety).

Table C.5: Grouped Event Study Results: Top 90% Sample (N06)

	(1)	(2)	(3)	(4)
Panel A: Female				
Pre-event$\times\Delta\psi$	0.338	0.289	0.256	0.267
	(0.637)	(0.638)	(0.616)	(0.615)
Post-event-short$\times\Delta\psi$	-1.410^{**}	-1.606^{***}	-1.837^{***}	-1.816^{***}
	(0.524)	(0.527)	(0.609)	(0.610)
Post-event-long$\times\Delta\psi$	-2.185^{***}	-2.366^{***}	-2.680^{***}	-2.652^{***}
	(0.709)	(0.711)	(0.748)	(0.750)
Observations	513,120	513,120	513,110	512,304
Mean	6.317	6.317	6.317	6.317
Panel B: Male				
Pre-event$\times\Delta\psi$	0.152	0.130	0.145	0.141
	(0.408)	(0.406)	(0.410)	(0.410)
Post-event-short$\times\Delta\psi$	-0.0576	-0.0114	-0.0160	-0.0288
	(0.380)	(0.374)	(0.394)	(0.395)
Post-event-long$\times\Delta\psi$	-1.230^{***}	-1.182^{**}	-1.154^{**}	-1.160^{**}
	(0.476)	(0.469)	(0.485)	(0.485)
Observations	1,083,919	1,083,919	1,083,914	1,083,833
Mean	3.8150	3.8150	3.8150	3.8150
Event period FE	✓	✓	✓	✓
Match effects	✓	✓	✓	✓
Log(workers)		✓		
Firmage		✓		
Coworker α			✓	
Ind FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
SBI FE	✓			
Municipality FE			✓	✓

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ Standard errors are two-way clustered at the individual and firm levels. The sample is restricted to the largest connected set by gender. Average co-worker α represents the average worker fixed effects among co-workers at the individual's firm. Industries are defined using the 2-digit Standard Business Classification 2008. ATC code N06 (psychoanaleptics, commonly used to treat depression).

Table C.6: Event Study: Heterogeneous Effects by Industry Stress Level

	N05 (Anti-anxiety)			N06 (Anti-depressant)		
	Pooled	Female	Male	Pooled	Female	Male
Post × $\Delta\psi$	−0.447	−0.093	−0.608	−1.617***	−1.129	−1.931***
	(0.363)	(0.631)	(0.433)	(0.487)	(0.804)	(0.595)
Post × High stress × $\Delta\psi$	0.303	1.151	−0.279	0.579	−1.232	1.792**
	(0.578)	(0.985)	(0.672)	(0.736)	(1.265)	(0.897)
Post × High stress	0.194***	0.403***	0.105	0.257***	0.668***	0.052
	(0.069)	(0.133)	(0.073)	(0.087)	(0.166)	(0.095)
Net effect (high-stress sectors)						
Post × $\Delta\psi$ + Post × High stress × $\Delta\psi$	−0.144	1.059	−0.887*	−1.038*	−2.361**	−0.139
SE	(0.470)	(0.802)	(0.537)	(0.584)	(1.039)	(0.683)
Observations	1,221,176	400,765	820,410	1,221,176	400,765	820,410
R^2	0.450	0.433	0.462	0.633	0.638	0.627
Mean of dep. var.	2.139	2.811	1.976	4.619	6.438	3.895
Individual FE	yes	yes	yes	yes	yes	yes
Year × gender	yes	yes	yes	yes	yes	yes
Age × gender	yes	yes	yes	yes	yes	yes
Event-time FE	yes	yes	yes	yes	yes	yes
SBI FE	yes	yes	yes	yes	yes	yes

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ Standard errors are two-way clustered at the individual and firm levels. The sample is restricted to the largest connected set by gender. Industries are defined using the 2-digit Standard Business Classification 2008. ATC code N05 (psycholeptics, commonly used to treat anxiety) and N06 (psychoanaleptics, commonly used to treat depression).