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#2024/01

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The Mental Health and Labor Market Effects of Anticipating Job Loss

Imprint

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ISSN 2199-8744 (online)

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Abstract

Exploiting future exposure to job termination in the UK, this paper finds that sharply increased job loss expectations before job termination significantly increase mental distress. This anticipation effect is largest in tight labor markets but does not spill over within couples. In contrast, anticipating job termination allows workers to switch positions without suffering unemployment. Leveraging variation in the industry-specific labor market tightness before the job termination, this paper shows that switching from a terminated position before its closure offsets over 70 percent of the negative labor market effects of the job termination, and mitigates its entire mental burden.

Keywords*: Job loss, anticipation, mental health, unemployment.*

JEL classification*: D84, I18, J28, J63.*

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* *I am extremely grateful to my advisor Kristina Strohmaier for her invaluable guidance and support. I thank Daniel Kuehnle, Christoph Kronenberg, Roger Prudon, Hendrik Schmitz, and Sonja Spitzer for their helpful feedback and Amanda Agan, Breno Sampaio, and Max Steinhardt for constructive suggestions and insightful conversations at various stages of this project. I am also thankful to the audience at the 2023 Essen Health Conference, the 2023 Economics of Mental Health Workshop, the 2023 EuHEA PhD Conference, the 2023 CINCH Academy, the 2024 ifo Dresden Workshop, and the 2024 ESPE.*

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1 Introduction

Job terminations pose an adverse life event for the affected workers. The economic consequences include reduced labor force participation (e.g., Bertheau et al. 2023; Schmieder et al. 2023), withering firm- and industry-specific skills (e.g., Lachowska et al. 2020; Huckfeldt 2022), and absent human capital accumulation (e.g., Burdett et al. 2020), all resulting in severe losses in lifetime earnings (e.g., Davis and von Wachter 2011; Jacobson et al. 1993). Moreover, job termination affects individuals' health, especially their mental health.[1] For governments, job terminations pose a leading cause of expenditure due to unemployment benefit uptake, lower tax income, and increased health care costs. In the UK, for example, over 8 percent of the annual healthcare budget is allocated to mental health services, approximately £13 billion per year (NHS 2023). Kuhn et al. (2009) show the health effects of job losses contribute to these expenditures.

A largely overlooked fact about job termination is that they are possible to anticipate. Most OECD countries (i.a., the US, Germany, and the UK) legally mandate firms to inform workers of their lay-off multiple weeks or months in advance (OECD 2019).[2] Beyond this certain source of information, workers receive noisy signals in their workplaces about their impending job termination, e.g. rumors or insolvency filings, that may spark anticipation up to a year in advance (Wunder and Zeydanli 2021). This anticipation induces workers with fear of job loss, which in different settings has been shown to harm mental health.[3] At the same time, workers anticipating their job loss can switch from terminated positions (Cederlöf et al. 2023) and thus circumvent the negative economic and health effects of unemployment.

In this paper, I quantify and contrast these two opposing effects using large panel survey

[1]Exploiting firm closures and downsizing events as a quasi-experimental variation in the employment status, Ahammer and Packham (2023), Black et al. (2015), Browning and Heinesen (2012), Eliason and Storrie (2009a), Eliason and Storrie (2009b), Eliason and Storrie (2010), Kuhn et al. (2009) and Marcus (2013) find negative mental health effects of job loss. The results of Been et al. (2024), Michaud et al. (2016), Salm (2009) and Schiele and Schmitz (2016) are mixed.

[2]Plans of large-scale job termination are also commonly announced publicly. Recent examples are Meta laying off 21,000 workers, Amazon 27,000 workers, and Accenture 19,000 workers (CNN 2023).

[3]Ahammer et al. (2023), Cottini and Ghinetti (2018), Le Clainche and Lengagne (2023), and Reichert and Tauchmann (2017) study workers surviving downsizing events. Blasco et al. (2022) investigate fear of job loss induced by automatization.

data from the UK. Using survey data is advantageous in the setting at hand as it combines information on self-assessed job loss expectations with validated screening tools that pick up on small variation in mental health without delay and without relying on thresholds in a latent variable, like it is for example the case for diagnoses in health insurance records. I link the survey with vacancy data from the Office for National Statistics (ONS) to proxy the labor demand respondents are exposed to.

To causally identify the mental burden of anticipating job loss, I leverage exposure to imminent job terminations due to firm closures, section closures, and firm reshaping. In the UK, these types of job termination are bundled under the legal term *redundancy*. Redundancies must be announced between two and twelve weeks in advance but might be anticipated much earlier through signals observed by the workers. Relying on redundancies thereby addresses the problem of reversed causality, i.e. workers being dismissed due to their mental health.

Descriptively, job loss expectations remain constant over time but increase drastically in the year before workers experience job termination. Based on this finding, I employ a difference-in-differences (DiD) approach to identify the mental health effects of anticipating job loss using continuously employed workers as the control group. Applying entropy weights (Hainmueller 2012) balances observable baseline characteristics, which further improves the fit of the control group. I perform the estimation in stacked design to address bias due to heterogeneous effects (Cengiz et al. 2019; Baker et al. 2022). The estimates are robust against spatial or industry-specific shocks and replicate when using workers experiencing redundancy multiple years in the future as the control group.

I find that anticipating job termination increases the prevalence of severe mental distress by 11 percentage points (pp). For workers not anticipating their job termination, a lead effect is absent. The intent-to-treat effect of facing job termination within a year regardless of its anticipation amounts to a significant 3 pp increase of severe mental distress. A causal forest heterogeneity analysis shows that the anticipation effect is especially pronounced for middle-aged adults, high-income groups, and workers in tight labor markets. Additional analyses reveal that the anticipation effect does not spill over to the mental health of cohabiting partners, yet there is suggestive evidence of an anticipatory added-worker effect.

Next, I investigate the mental health and labor market effects of workers using information on their impending layoff to switch from terminated positions in time. For that, I distinguish between individuals that either do or do not suffer an unemployment spell due to their job termination. As a benchmark, I first compare individuals that became unemployed due to their job termination to a continuously employed control group. This approach captures the total effects of job termination compared to a counterfactual, in which job termination does not occur. Then, I estimate the effects of remaining in a position until its closure compared to a control group that experiences job termination at the same time but switches positions without suffering an unemployment spell. I overcome endogeneity in the ability to switch from closing positions by leveraging variation in the region- and industry-specific labor market tightness before the job termination. Benchmarking these effects then provides a measure of what share of the effects of job terminations can be offset by anticipating the lay-off and switching positions in time.

Job terminations have a scarring labor market effect regardless of a consecutive unemployment spell. Still, I find that avoiding unemployment by switching positions in time increases subsequent labor supply by 22 hours per week and monthly labor income by $\pounds 1,400$. As such, switching positions offsets over 70 percent of the negative labor market effects of job termination. Also, the average uptake of social benefits decreases by more than $\pounds 6,200$ within the first year after the job switch, alone. From a health perspective, switching in time reduces the likelihood of suffering from severe mental distress by 7 pp. This benefit offsets over 100 percent of the mental burden of job termination and closely resembles the negative anticipation effect in size. I provide evidence that re-employment in subjectively higher-quality matches may drive the effect.

This paper links the two disconnected strands of the literature on employee-side lead effects and health externalities of layoff events. Previous studies show that individuals anticipate and adjust to an upcoming job loss (Hendren 2017; Lachowska et al. 2020 Schwerdt 2011;Wunder and Zeydanli 2021) and that job loss expectation induces a negative mental health effect (Ahammer et al. 2023; Blasco et al. 2022; Cottini and Ghinetti 2018; Reichert and Tauchmann 2017). This paper creates a bridge by showing that anticipation causes a negative mental health effect and thus contributes to both strands.[4]

When studying workers before their layoff, one must consider that those (and only those) who anticipate their job termination face deteriorating mental health. There is a trade-off between using anticipation for advantageous decision-making and suffering a mental burden. Hence, providing workers with information on an upcoming shock is not necessarily a Pareto-improvement over facing the shock unexpectedly. The anticipatory mental health effect, but also the long-run mental health benefits of switching from termination positions, should thus be considered when designing mandatory early notification laws (see Cederlöf et al. 2023 for a discussion of the costs and benefits of early notification). Importantly, however, the mental burden of anticipation does not paralyze workers in their job search (Gerards and Welters (2022)).

For the health economic literature, the presence of an anticipatory mental health effect suggests that the health externalities of job terminations are larger than previously considered. For spillover effect to spouses, this is however not the case (e.g., Marcus 2013; Zhao 2023). Moreover, the anticipation effect emphasizes the threat of underestimating the effect of job loss on reactive health outcomes in DiD designs (e.g., Everding and Marcus 2020; Marcus 2013; Preuss and Hennecke 2018) due to reference point contamination. When using unreactive outcomes, such as diagnoses or hospitalization, the anticipation effect might shift into the post-job loss period and be confused with the effect of the job loss itself (e.g., Ahammer and Packham 2023; Browning and Heinesen 2012).

This paper also speaks to the broad literature on the effects of job termination. Regardless of whether previous studies investigate the labor market (e.g., Bertheau et al. 2023;

[4]A previous paper potentially linking the strands is Carlson (2015), who finds a higher prevalence of low birth weight in regions with more announcements of firm closures.

Cederlöf 2021; Schmieder et al. 2023) or health effects of job loss (e.g., Ahammer and Packham 2023; Been et al. 2024; Schiele and Schmitz 2016), the empirical approaches rely on comparing workers experiencing a layoff to others who do not, and thus impose a counterfactual in which the layoffs never occurred. Their results capture the benefits of preventing job loss. (Mass) layoffs are, however, equilibrium objects, and preventing them may lead to a large deadweight loss (Fujita and Ramey 2012). This study provides an alternative measure. By comparing workers who do and do not leave their terminated positions before experiencing unemployment, layoffs occur both in the observed and in the counterfactual state of the world. The estimated effects thus capture how much of the effects of job terminations may be mitigated without preventing job termination in the first place. The results of this paper thereby highlight that the detrimental effects of job terminations found by previous studies are not caused by the job terminations per se but by workers not being able to switch positions in time.

The remainder of this paper is structured as follows. Section 2 provides an overview of the institutional setting and the data. In Section 3, I elaborate the the identification and estimation of the anticipation mental health effect. In Section 4, I contrast the total effect of job termination with the benefits of anticipating the lay-off and switching in time. Section 5 concludes.

2 Background and Data

2.1 Institutional Setting

This study is situated in the UK between 2010 and 2019. At that time the UK labor market is characterized by high employment, steady growth, and rather weak employment protection. In 2010, 70.5 percent of the working-age population was employed. Over the following nine years, this number increased to 76.2 percent. By comparison, the UK employment rate exceeds the one of the US by about 5 pp, the OECD average by 8 pp, and the EU-27 average by 10 pp (OECD 2023a). Moreover, the UK labor market underlies less regulation compared to the OECD average, yet more than, for example, in the US or Canada (OECD 2023b).

Specific job terminations in the UK fall under the legal term *redundancy*. In particular, UK law defines redundancies as job losses due to the closure of the whole firm, the closure of a section in the firm, or the firm reshaping its labor demand so that it has no more need for a specific type of work.[5] Job terminations are not classified as redundancy if employers can provide the laid-off workers with a similar job within the firm. (UK Law 1996 c.18 Section 139). If not all workers are laid off simultaneously, employers must establish an objective selection system that is independent of workers' characteristics, such as workers' health.[6]

Before terminating the positions, firms are legally required to inform the workers of their impending lay-off. This period of early notification period increases with the duration of the employment spell to a maximum of 12 weeks (UK Government Website 2022b).[7] Similar legal notification periods exist in most OECD countries, in which previous research treats job termination as a pseudo-exogenous shock. Table A.1 provides an overview of the legal notification period of countries, in which the health effects of job terminations have been studied. OECD (2019) summarizes early notification laws and other means of employment protection in all OECD countries.

2.2 Data

For the empirical analysis, I rely on panel survey data from waves 1 to 11 of the UK Household Longitudinal Study (UKHLS) (Buck and McFall 2011). In the UKHLS, new survey waves start annually and contain interviews conducted over a two-year period. Individuals' follow-up interviews are scheduled to lie one calendar year apart. The data is well-suited to study the research question at hand for multiple reasons. First, the UKHLS follows a large number of individuals over time. With its approximately $40,000$ respondents per wave, the

[5]I use 'firms' as a synonym for employers, as redundancies also occur in the public sector.

[6]Workers on sick leave can be made redundant, however not due to their sick leave. Typically, workers who enter the firm last are made redundant first (UK Government Website 2022a).

[7]Firms can make redundancies effective immediately without early notification, but they still have to pay workers their income during the notification period. In addition, firms must pay redundant workers a compensation payment of up to $£17,130$ (UK Law 1996 c.18 Section 89).

UKHLS surpasses other prominent household surveys in sample size, thus ensuring precise estimates even when narrowing down onto specific sub-populations. Its panel dimension allows for analyses being based on within-individual variation.

Second, the UKHLS provides crucial information on the exposure to job termination, job loss expectations, and two validated mental health scales. In particular, all respondents reporting a disruption in their employment are asked "*Can you tell me why you stopped doing that job?*". Based on the pre-made answer "*made redundant*", I identify which individuals experience job termination at which point in time. Based on the retrospective number of unemployment spells, I can further distinguish whether the job termination led to unemployment. Regarding job loss expectations, respondents in every other survey wave are asked "*How likely do you think it is that you will lose your job during the next 12 months*". I classify that individuals answering "*very likely*" or "*likely*" expect a job loss and consider individuals expecting job loss in the interview prior to reporting redundancy to anticipate their job termination.[8] To quantify mental health, I rely on the SF-12 Mental Component Summary (MCS), a six-item questionnaire that was designed for screening mental health in large populations (Ware et al. 1994). Its items are weighted and aggregated to a scale ranging from 0 (worst) to 100 (best), with a mean of 50 and a standard deviation of 10. The weights are provided by the operators of the survey. Appendix A contains the questionnaire and the pre-made answers. The MCS is widely used in related research (i.a., Reichert and Tauchmann (2017); Schiele and Schmitz (2016)) making the results particularly comparable across studies. Yet unlike the survey data used by previous studies, the UKHLS contains the MCS in each wave. For the analysis, I use the MCS both as a continuous measure and as a dichotomized indicator proxying the absence of mental distress and severe mental distress.[9] The MCS is missing in approximately 15 percent of interviews. For robustness, I replicate

[8]Overall, individuals expecting job loss are three times as likely to experience in gap in their paid employment until their next interview.

[9]The underlying thresholds are 45 and 36, as recommended by Gill et al. (2007). With the threshold of 45, the authors find the dichotomized MCS predicts active or recent depression with a sensitivity of 87 percent and a specificity of 83 percent. Figure A.2 shows the distribution of the MCS.

the main findings of this paper using the General Health Questionnaire (GHQ).

Last, the UKHLS allows linking granular labor market statistics provided by the ONS. Adding information on the monthly industry-specific vacancy rates per 100 employed workers (1-digit ISIC) and quarter-by-district unemployment rates provides me with a measure for the labor market tightness individuals are exposed to in the year before their redundancy. To do so, I average the vacancy rate and the unemployment rate during the quarter a redundancy was reported and the three quarters beforehand and relate both measures.

Between 2009 and 2019, 3021 unique respondents experience redundancy. I restrict this sample to individuals aged 20 to 60 that do not retire before 2019 and do not experience more than one redundancy. Moreover, I require individuals to have an interview two years before reporting their redundancy. In the final sample, $1,479$ individuals experience redundancy.[10]

Naturally, the data has some limitations. Relying on survey data opens room for measurement error. Exploiting the panel dimension of the data accounts for any measurement error in the outcome as long as it is time-invariant. Miss-reported job terminations would likely downwards bias the anticipation effect if dismissals are anticipable to a lesser degree. Moreover, attrition may bias the results. Correlations between the key variables and the propensity to drop from the survey are, however, low.[11] To counteract group-specific attrition, I restrict the empirical approach to comparisons of individuals remaining in the survey for the same number of interviews.

3 The Mental Burden of Anticipation

3.1 Identification

Figure 1 visualizes the average mental health and job loss expectations over time (years) for individuals experiencing job termination due to redundancy. Redundancy thereby occurs between periods -1 and 0 and is first reported in period 0. During the periods -6

[10]The number of redundancies in the UKHLS highly correlates with the regions' population (see Figure A.2)

[11]Individuals missing their next interview are 1 pp more likely to expect job loss and have 0.005 MCS units worse mental health, which amounts to 0.05 percent of a standard deviation.

to -2, both the MCS and job loss expectations follow a flat trend.[12] In the year before reporting redundancy, the share of individuals expecting job loss abruptly peaks from 20 to 43 percent, i.e. more than doubling its baseline levels. The figure thus emphasizes that individuals' awareness of their impending job termination arises within the year before the event. Coincidentally to the increased job loss expectations, the average MCS plummets by approximately 1.5 scale points, suggesting workers' awareness of their job termination evokes a psychological burden.

The presence of an underlying time trend in the MCS would yield this conclusion as a fallacy. To adjust for an unobserved time trend between the reference period, -2, and one single other period, the so-called target period, I rely on a control group of individuals that are continuously employed at the time the treatment group units experience job termination.[13] The resulting two-by-two DiD approach estimates the target period-specific effect of job termination on the laid-off workers. To estimate the anticipation effect, -1 serves as the target period, for the estimation of post-redundancy effects, the target period is 0, 1, 2, 3 or 4, and for placebo tests, the target period is -3, -4, -5 or -6. To avoid drawing forbidden comparisons in a setting with a staggered treatment (Sun and Abraham 2021), I implement this identification in a stacked DiD design (Cengiz et al. 2019). For robustness, I repeat the estimation using a control group exclusively consisting of individuals experiencing redundancy in three or more years.

Table 1 shows reference-period means of covariates for the treatment and control group. At baseline, treatment group units are less likely to be female, married or tertiary educated, and have lower income. To adjust these imbalances, I employ entropy weights to the control group (Hainmueller 2012). These weights are produced by minimizing a distance function between the entropy weights and a vector of even weights, under the constraints that the weighted control group moments for a set of covariates must match the moments of the treat-

[12]The baseline share of individuals fearing job loss thereby closely resembles the level found by Reichert and Tauchmann (2017) for German private sector employees.

[13]Figure A.3 replicates Figure 1 for the control group. Spikes both in job loss expectations and in mental health are absent.

ment group.[14] The balance includes all covariates in Table 1 as well as industry fixed effects, *government office regions* fixed effects and individuals' number of survey responses. After applying the entropy weights, the differences shown in Table 1 become indistinguishable from zero.

3.2 Estimation

To prepare the estimation, I stack the data using the following algorithm: First, define a target period, $t \neq -2$. Second, keep all observations of individuals either reporting redundancy or a continued employment spell in survey wave, s. Third, keep observations from the reference period, -2, and period t. Fourth, compute the difference in outcome between the two periods for each individual, i, as $\Delta y_{is}^{t} = y_{is}^{t} - y_{is}^{-2}$. Fifth, repeat steps two to four for all s and stack the data sets. Sixth, compute the entropy weights for the target period-specific control group. At this point, the data allows for estimating the effect of redundancy for a single target period. Thus at last, repeat steps one to six for each t.[15] Using these target period-specific data sets, and running separate regressions for all t, the most basic regression model takes the following form:

$$\Delta y_{is}^{t} = \alpha + \varsigma_s + \gamma^t Treat_i + \epsilon_{is}^{t} \tag{1}$$

Using the long difference between the target and the reference period as outcome eliminates any time-invariant heterogeneity, including a time-invariant measurement error in self-reported outcomes. The constant, α, captures a group-invariant time trend. For more flexibility, the set of stack fixed effects, ς_s, allows this common trend to vary over time. The stack fixed effects also restrict the estimation to within-stack variation, thereby preventing forbidden comparison and shielding the estimation against contamination arising from effect

[14]For binary variables, matching the first moment suffices, whereas for continuous variables, I balance the first two moments. The loss function assures that the sample is changed as little as possible, to not overweight a small set of control group observations. The Lorenz-curves of the target period-specific control group weights are shown in Figure A.3.

[15]The number of treatment group units in each target period-specific data set is provided in Table A.2.

heterogeneity (Goodman-Bacon 2021; Sun and Abraham 2021). The coefficient, γ^t, of the treatment group indicator, $Treat_i$, poses the parameter of interest. It captures by how much the change in outcome in the treatment group deviates from the one in the control group. The corresponding ordinary least squares (OLS) estimator, $\hat{\gamma}^t$, thus estimates the target period-specific average treatment effect on the treated (ATT) in period t.

The consistency of $\hat{\gamma}^t$ relies on two identifying assumptions. First, the anticipation effect must not arise in the reference period already. With anticipation of job termination occurring in period -1 only, this assumption should be of no concern. Second, any time-varying heterogeneity must not predict both the group assignment and the change in outcome. This assumption would be violated if the trend in outcome was a function of baseline covariates that are not identically distributed across the groups. To address this concern, I employ the entropy weights function, $W^e()$, to both sides of the regression equation.[16] In doing so, I approximate the trends in untreated outcomes of the treatment group using individuals with the same observable demographic, socioeconomic, and labor market characteristics. The preferred specification of this study hence takes the following form:

$$W^e(\Delta y_{is}^t) = W^e(\alpha + \varsigma_s + \gamma^t Treat_i + \epsilon_{is}^t) \tag{2}$$

In all specifications, I estimate the parameters using OLS and cluster the standard errors at the individual level (Abadie et al. 2022).

3.3 Results

Panel A of Figure 2 shows the results of separately estimating $\hat{\gamma}^t$ from eq. (2) for treatment group units that do or do not anticipate their redundancy, hence for individuals that do or do not expect a job loss in period -1. The MCS serves as the outcome. Table B.1 contains es-

[16]To obtain the desired weighted OLS estimator $\hat{\beta} = (X'WX)^{-1}X'WY$, let the function $W^e(A) := W^{0.5}A$, with $W^{0.5}$ being the matrix root of the vector containing the entropy weights, and A be an arbitrary n by 1 column vector.

timates of different model specifications and using the dichotomized MCS as the outcome.[17] Across specifications, all estimated placebo coefficients are statistically insignificant. In line with the descriptive evidence, the lead effect provided by $\hat{\gamma}^{-1}$ is negative and highly significant. Individuals anticipating their job termination face a large negative mental health effect of 3.3 MCS units, amounting to a third of a standard deviation. This anticipation effect increases the prevalence of severe mental distress by 11 pp among treatment group units anticipating their job loss. In contrast, individuals not expecting their job termination face no lead effect at all, which underlines that the anticipation is driving the lead mental health effect.

Splitting the treatment group by job loss expectations may induce endogeneity as factors contributing to anticipating the lay-off may also predict changes in mental health. Therefore and because job loss expectations are only available for half the sample, Panel B of Figure 2 contains the estimates using all available treatment group units irrespective of their job loss expectations. Thus, the estimates pose intent-to-treat (ITT) effects of experiencing job termination within a year. Table B.2 contains estimates of different model specifications. Again, the placebo estimates do not hint at any violations of the parallel trends assumption. Across all treatment group units, the anticipation effect shrinks to 1.2 MCS units but remains highly significant in statistical and economic terms. The possibility to anticipate redundancy thereby increases the prevalence of severe mental distress by 3 pp.

The results are robust against controlling for regional or industry-specific time-varying shocks (see Tables B.1 and B.2) that may confound the change in mental health and the propensity of job termination (Gathmann et al. 2020). The results also replicate in a plain TWFE framework regardless of using heterogeneity-robust estimators (see Figure B.2), when using the GHQ as the outcome (see Figure E.2), and when using future candidates for redundancy as the control group (see Figure B.3).

Qualitatively, the sign and significance of $\hat{\gamma}^{-1}$ in Figure 2 panel B matches previous find-

[17]See Figure B.1 for the post-redundancy effects.

ings of mental health effects of fearing job loss in survey data. To contextualize the effect quantitatively, the settings of Reichert and Tauchmann (2017) and Cottini and Ghinetti (2018) yield suitable benchmarks. The size of $\hat{\gamma}^{-1}$ thereby exceeds the reduced form effect of surviving a downsizing event in the German private sector factor of three yet mimics the effect of fearing job loss due to firms' workforce reductions in Denmark.

The question arises of why an anticipation effect has not been documented so far. Østhus (2012) is to my knowledge the only study estimating a lead mental health effect of pseudo-exogenous job losses using survey data.[18] In Finland, they find no effects of upcoming dismissal on self-reported psychological distress. The null results may, however, stem from power issues due to their sample containing just 93 treated units or from a specification that does not control for unobserved regional heterogeneity. Studies conducted on administrative data also do not find anticipation effects, which is likely an artifact of using medical records as the health outcome. Due to waiting times and administrative burden, there is by construction a delay between a mental health shock and an increase in the uptake of medical services that is picked up in the data. Empirically, Ahammer et al. (2023) find a 6 (18) months delay between surviving a downsizing event and an increase in psychotherapy (mental health diagnoses). Thus, dynamic studies on administrative data might unknowingly mix up the health effects of the job loss with the effects of anticipating the job loss.

Given the large body of research finding that job terminations affect spousal mental health (e.g., Marcus 2013; Zhao 2023) and labor supply (e.g., Halla et al. 2020; Kohara 2010), I investigate whether the same holds true for the anticipation effect. Applying eq. (2) to a sample of individuals whose partners either experience job termination or a continued employment spell does, however, not hint at within-couple mental health spillovers due to anticipating job loss. The null findings are robust across specifications and precise given the sample size at hand (see Appendix C for further information and the result tables).

[18] Most other surveys used by previous research only contain mental health data in every other wave, making it challenging to conduct dynamic analyses. The leading example is the German Socioeconomic Panel, which is used by Preuss and Hennecke (2018), Reichert and Tauchmann (2017), Schiele and Schmitz (2016) and Schmitz (2011).

3.4 Causal Forests

The ITT anticipation effect can hide important effect heterogeneity across types of treatment group units. To estimate the conditional average treatment effect (CATE), hence the effect of anticipating redundancy as a function of observable covariates, I apply the causal forest estimator by Wager and Athey (2018). The approach allows the identification of multidimensional dependencies of covariates on the treatment effect at hand while reducing researcher bias by replacing subjectively chosen sample splits with data-driven ones.

The method requires single-period data consisting of a treatment and a control group. A data-driven algorithm then computes the difference in outcome between the two groups after splitting the sample throughout on a vector of covariates. Furthermore, the algorithm computes the importance of each covariate for predicting the effect heterogeneity. Applying the 'honest approach' cross-validates the sample splits against test data to avoid overfitting. Estimating the CATE consistently requires the unconfoundedness of the treatment group indicator and the outcome (Wager and Athey 2018). Under the parallel trends assumptions, this is given for Δy_{is}^{-1} once partialling out the intercept and the stack fixed effects.[19]

Figure 3 provides the CATE across ociles of continuous baseline covariates. The grey line denotes the variables' kernel densities in the treatment group. Figure B.2 contains the results for binary covariates. Personal income, hence the severity of the expected economic loss, is the main driver of effect heterogeneity. Coherently, the anticipation effect is largest for tertiary educated individuals, for age groups at the peak of the income-age gradient, and for individuals with a mortgage on their house. Furthermore, the anticipation effect is worse for job terminations occurring in tight labor markets. This could be driven by labor market tightness both predicting the average duration of unemployment spells and the likelihood of finding work at a given period, hence the expected level and the variance of the socioeconomic decline.[20]

[19] I do this by estimating $\Delta y_{is}^{-1} = \alpha + \varsigma_s + \epsilon_{is}$ and using the fitted values for ϵ_{is} as outcome.

[20] To test whether the effect heterogeneity is driven by certain types of individuals performing better at predicting their job loss, I run a logistic lasso regression to identify which baseline-level covariates predict the ability of treatment group units to anticipate their job termination. Only age and some industry dummies survive the selection process, implying that neither better-educated nor high-income individuals anticipate

4 The Effects of Switching from Terminated Position

4.1 Empirical Setting

The anticipability of job termination is a necessary condition for workers switching positions before their closure. Without anticipation being possible, individuals would inevitably experience an unemployment spell after their jobs' termination. Cederlöf et al. (2023) shows that extending the period of anticipation increases the share of workers transitioning from a terminated position into employment without an intermediate period of unemployment. In the UKHLS, anticipating job termination is highly predictive of averting a consecutive unemployment spell: individuals expecting their job termination in period -1 are 10 pp (23 percent) more likely to not become unemployed afterward. Thus, one potential benefit of anticipation is that it provides individuals the opportunity to circumvent the negative effects of unemployment due to job termination.

The effects of switching from terminated positions in time have not been researched so far. The previous literature on job terminations instead compares the observed outcome trajectories of individuals experiencing a job termination with a counterfactual state, in which the job termination never occurred.[21] The difference between individuals' observed and counterfactual trajectories in outcome thereby captures the total effect of job termination.[22]

The measure of interest is rather the effect of staying in a terminated position compared to a counterfactual, in which job termination occurred without leading to unemployment. Benchmarking this measure against the total effect of job termination provides insight into how much of the scarring effects of job termination can be mitigated by workers anticipating their layoffs and switching positions in time. Thus, whereas previous research provides an intuition of the benefits of preventing job termination by intervention, this measure captures

their job termination more accurately.

[21]This counterfactual is typically approximated using a control group of either continuously employed workers (e.g., Couch and Placzek 2010), workers with the same propensity of experiencing a layoff (e.g., Schmieder et al. 2023), or workers who are laid off in the future (e.g., Ahammer et al. 2023).

[22]Schwerdt (2011) documents that this total effect of job loss is smaller for workers leaving closing firms early.

the gain of preventing unemployment while allowing job termination to take place. Given that preventing job terminations poses a larger distortion to the labor market than providing workers the chance to anticipate their layoff, this measure is particularly relevant for policymakers.

In the UKHLS, approximately half of the individuals experiencing job termination switch positions in time, whereas the remainders stay and suffer an unemployment spell. In particular, I define *switchers* ($N_{Switch} = 574$) as individuals reporting a change in their employment due to redundancy without an unemployment spell in the last 12 months and *stayers* ($N_{Stay} = 607$) as those with at least one unemployment spell.[23] By construction, only stayers experience a drop in their employment rate after the job termination. Figure D.1 shows the employment rates of stayers and switchers by event time. Table D.1 shows covariate means at baseline.

4.2 Estimation

To quantify the total effect of job termination as the benchmark, I compare stayers to a control group of continuously employed workers using eq. (2). The parameters $\hat{\gamma}^t$ thereby again pose the target period-specific ATTs, with the treatment being job termination with a consecutive unemployment spell for stayers. To study whether job terminations have scarring effects despite switching positions in time, I draw the same comparison for switchers. To obtain the measure of interest, the total effect on stayers must be put in relation to the effect of staying in a terminated position on stayers. Identifying this effect dictates a direct comparison of stayers to switchers that experience job termination at the same time. The continuously employed workers are thus omitted from the sample. In this new setting, remaining in a position until its termination serves as treatment. The baseline model takes

[23] Due to data limitations, the group of stayers also includes individuals that switched in time but became unemployed from their new job before their next interview. These individuals were, however, unsuccessful in switching to a suitable position. The unemployment spell occurring before the redundancy is unlikely due to the typically three- to six-month probation period. During this period, employers may dismiss workers without cause and without labeling the layoff as redundancy.

the following form:

$$\Delta y_{is}^{t} = \alpha + \varsigma_s + \delta^t Stay_i + \epsilon_{is}^{t} \tag{3}$$

Compared to previous models, α and ς_s not only absorb a general trend in outcome but also the group-invariant effects of job termination. The group indicator, $Stay_i$, equals one for individuals being stayers. Its coefficient, δ^t, poses the target period-specific effects of unemployment after job terminations on stayers compared to switching into different employment. Thus, by switching positions stayers would have avoided the effect δ^t, whereas stayers would not have experienced the effect γ^t had the job termination never happened in the first place. With that, the expression $\frac{\delta^t}{\gamma^t}$ denotes what share of the total effect of job termination that would have been offset by stayers switching positions.

Consistent estimation of this fraction requires consistent estimation of both γ^t and δ^t. The identifying assumption for consistent $\hat{\gamma^t}$ is the same as already stated and discussed in Section 3.3.. Based on eq. (3), the identifying assumption for a consistent $\hat{\delta^t}$ is the absence of factors confounding the group assignment and the change in outcome. Taking differences in outcome again eliminates any time-invariant confounders, like inherent ability or motivation, yet the absence of time-varying factors predicting the likelihood of leaving a terminated position and the change in untreated outcome remains a strong assumption.

To overcome this issue, I leverage variation in the labor market tightness in the year before the job termination in combination with rich background information provided in the UKHLS to employ a doubly robust machine learning (DRML) approach. The identification exploits that workers in terminated positions are less likely to switch in time if labor demand in their industry is at a low. I thereby estimate the δ^t semi-parametrically using inverse probability weighted regression adjustment according to the textbook formula (e.g., Robins et al. 1994; Sant'Anna and Zhao 2020):

$$\delta^t = E[\frac{\Delta y^t - \mu_0(x) \cdot Stay}{Pr(Stay = 1)} - \frac{(\Delta y^t - \mu_0(x) \cdot (1 - Stay) \cdot p(x)}{(1 - p(x)) \cdot Pr(Stay = 1)}] \tag{4}$$

The estimation of eq. (4) requires estimation of the plug-in parameters $p(x)$, $\mu_1(x)$ and $\mu_0(x)$, which denote the propensity score of being a stayer and the expected outcome of stayers and switchers, respectively, conditional on a vector of covariates, x. I estimate the propensity score using logistic regression and the conditional mean outcomes using OLS.

Due to the doubly robust property of the estimator, the identifying assumption is the absence of large systematic estimation errors either in the propensity score or in the conditional expected outcomes (or both). In the main specification, x contains the industry-specific labor market tightness, stacked fixed effects, and all available baseline covariates surviving a post-lasso selection.[24] For robustness, I estimate the plug-in parameters using only the industry-specific labor market tightness and stack fixed effects. In this setting, $\hat{\delta}^t$ would be biased only if unobserved time-varying heterogeneity confounds the industry-specific labor market tightness and individuals' propensity to switch positions and if there was large prediction error in the conditional mean outcome, simultaneously.

4.3 Benchmark Effects

The black markers of Figure 4 provide estimates of the total effect of job termination on stayers using different labor market and mental health outcomes. In the initial year after the job termination, stayers face a 29 hours reduction in their weekly labor supply and just over a £2000 decrease in their monthly labor income. For both outcomes, the immediate effect results in an almost 100 percent decrease relative to their baseline levels, which is expected given stayers' near-zero employment rate in period 0. Over the following three years, labor supply and income recover but remain significantly below their baseline trajectories.

[24] The lasso-selection ensures maintaining the predictive power for the plug-in parameters while minimizing the number of dependent variables, and hence potential sources of bias (Huber 2023).

Regarding mental health, stayers face a 1.7 unit decrease in their MCS following their job termination, leading to a 7 pp increase in severe mental distress. This post-redundancy effect thereby exceeds the anticipation effect in size, especially when using the dichotomized MCS as the outcome (see Table D.2). The mental burden of job loss vanishes one year after the job termination.

The grey markers provide analogous estimates for switchers. Despite being employed at consistently high rates, switchers also experience a scarring labor market effect from job termination, resulting in an immediate decrease of 7 weekly hours and £526 in monthly labor income. Compared to stayers, these effects are substantially smaller in size. Moreover, switchers almost fully recover from their labor market shock. Regarding mental health, switchers only suffer a negative effect before the job termination. This anticipation effect matches that of stayers in both size and significance, suggesting that the mental burden of anticipation has no paralyzing effect on job search. After the switch, the MCS of switchers exceeds its baseline trajectory, resulting in a positive long-run mental health effect from switching positions before their termination. I discuss potential mechanisms in Section 5.5..

4.4 The Benefits of Switching

To emphasize that stayers and switchers are comparable in their labor market and mental health trajectories, the left-side graphs of Figure 5 show the raw means of different outcomes over time. The right graphs provide the corresponding OLS and DRML estimates of eq. (3) and eq. (4). Under the assumptions discussed in the previous section, these estimates pose the losses stayers suffered due to not leaving their terminated positions, or vice versa, the benefit stayers would have gained had they switched positions in time. All results are robust to restricting the pool of covariates used in the DRML approach (see Table D.3).

Both in their labor supply (Panel A) and in their labor income (Panel B), stayers would have highly profited switching. Remaining in the terminated position led to a decrease in 22 working hours per week and a loss of £1,428 in monthly gross labor income. Compared to the total effect of job termination on stayers, switching positions would offset 75 percent of the immediate labor supply shock and 70 percent of the labor income shock of the job termination. In the years after reporting job termination, the gap in labor supply and labor income narrows, yet stayers still face a loss of £400 in monthly labor income three years after their job termination.

In addition to the economic effects, leaving terminated positions also spares individuals the mental burden of unemployment. Panel C of Figure 5 shows the mental health benefit stayers would have experienced had they circumvented their unemployment spell. In line with the anticipation effect being the same for stayers and switchers, this benefit exclusively occurs after the job termination. It amounts to an immediate increase of 2.2 MCS units, which is equivalent to a 8 pp reduction in the likelihood of suffering from severe mental distress. As such, the mental health benefit of anticipating job termination and switching in time surpasses the total mental health effect of job termination. The benefit of switching also exceeds the anticipation effect on stayers. Furthermore, the positive effect of switching lingers for an additional period, hence also exceeding the anticipation mental health effect in its duration.

4.5 Mechanisms

To understand the mechanism behind the mental health benefit of switching positions, Figure 6 shows the total mental health effect of job termination on each item of the MCS questionnaire individually, as well as for additional measures of well-being. The positive mental health effect of switchers thereby arises in the dimension of role emotional, hence the impact of emotional problems on their ability to perform their roles in daily life (Ware et al. 1994). Due to their persistently high employment rate, the daily life of switchers composes of work and their leisure time, alike. The null effects on the satisfaction with the amount of leisure time thereby suggest that work-related changes drive the positive mental health benefits of switchers.

To further investigate, Figure 7 shows estimates of eq. (2) using an indicator of whether individuals are mostly or completely satisfied with their job serves as the outcome. The sample is restricted to individuals being employed both in the reference and the target period. The figure highlights that individuals employed after job termination are more satisfied with their new match at strikingly high rates. These findings confirm previous evidence that job termination alleviates miss-matches on the labor market (Chadi and Hetschko 2018), and add that replacing bad matches is associated with improved mental health.[25] Moreover, the findings show that the the quality of new matches improves regardless of individuals having experienced an unemployment spell. The persistence of the estimates speaks against concerns that the effects might be driven by workers' relief of either having left an insecure position or unemployment.

4.6 Public Finance Effects

In addition to the affected workers, policymakers could also profit from workers switching from terminated positions. First, the labor income maintained by switching from termination positions is taxed. Computing the exact income tax revenues of an averted unemployment spell is difficult due to non-linearities in the UK payroll tax system. Based on the average stayer, that resides in England and earns £2,200 pre-tax income per month before the job termination, the loss in tax earnings amounts to £4,147 within the first year after the job termination.[26] Second, avoiding unemployment reduces the uptake of social benefits. Panel A of Figure 8 shows the total effect of job termination on the amount of social benefits received, which amounts to an immediate increase of £120 per week or approximately £6200 per year. In the following years, stayers face a persistent increase in their benefit take-up of approximately £2800 per year. Panel B of Figure 8 then shows the additional benefit uptake due to stayers not switching positions in time. Here, close to 90 percent of the

[25] The group-invariant effects on job satisfaction paired with the lower employment rate of stayers thereby match the pattern that only switchers exceed their baseline mental health trajectories.

[26] This figure was computed based on the 2024 tax code 1257L using the government-provided tax calculator. It includes income tax and compulsory national insurance payments.

government social benefit expenditures due to the job termination were offset had stayers switched positions. This figure likely poses a lower bound quantity since individuals tend to under-report their benefits uptake in surveys (Meyer et al. 2009). Averted sick days and health-care spending potentially exacerbate the fiscal benefits of switching from terminated positions.

5 Conclusion

This paper contrasts the mental health externalities of anticipating job terminations with the economic and mental health benefits of anticipating job termination and switching positions in time. The results show a negative mental health effect in the year before job loss, that can be attributed exclusively to the anticipation of the upcoming layoff. Given a yearly average of $131,000$ redundancies in the UK between 2009 and 2019, my findings suggest the anticipation effect alone causes approximately $5,100$ workers to develop symptoms of psychological distress each year. This finding is of high external validity since early notification laws, and hence anticipable job terminations, exist in most OECD countries.

The ephemerality of the anticipation effect suggests a limited efficiency of mental health intervention targeting still-employed workers in insecure positions such as in firms announcing downsizing or filing for insolvency. Moreover, the mental burden of anticipating the layoff has no paralyzing effect on workers' success in switching positions. For policymakers still aiming to counteract the anticipation effect, educating individuals on better job satisfaction once re-entering employment after the layoff might pose a suitable intervention. Allocating resources over-proportionally towards industries in which the labor market is tight may amplify the effectiveness of such interventions.

In direct contrast to the anticipation effect stand the benefits of workers utilizing the information on their impending lay-off and switching from their terminated positions in time. Avoiding an unemployment spell thereby offsets a large share of the negative labor market effects of job termination. Still, job terminations scar workers' labor market trajectories despite switching in time. From a public health perspective, leaving terminated positions early spares individuals the entire mental burden of unemployment. The results of this paper hint at large benefits of embracing workers to switch from insecure positions in time. To switch in time, it is necessary that workers take notice of their future job losses. Interventions assisting workers anticipating their job termination and in their job search may thus yield large benefits. The costs of these interventions may be partially reimbursed by increased tax revenue and decreased social benefit expenditure.

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6 Tables

	(1) Control		(2) Treatment		(3) Difference	
Outcomes						
MCS	49.68	(8.94)	48.27	(9.84)	-1.41**	(0.24)
MCS<45	0.75	(0.43)	0.70	(0.46)	-0.05**	(0.01)
MCS<36	0.91	(0.28)	0.88	(0.32)	-0.03**	(0.01)
Monthly labor income	2,273	(1,636)	2,259	(1,724)	-14.41	(42.85)
Weekly hours	28.33	(15.49)	30.45	(14.42)	2.12**	(0.41)
Covariates						
Female	0.54	(0.50)	0.50	(0.50)	-0.04**	(0.01)
Age	40.79	(9.92)	40.19	(10.29)	-0.59*	(0.26)
UK ethnicity	0.80	(0.40)	0.79	(0.41)	-0.01	(0.01)
England	0.78	(0.41)	0.81	(0.39)	0.03**	(0.01)
Wales	0.06	(0.25)	0.06	(0.23)	-0.01	(0.01)
Scotland	0.09	(0.28)	0.08	(0.27)	-0.01	(0.01)
Northern Ireland	0.06	(0.24)	0.05	(0.22)	-0.01	(0.01)
Urban	0.77	(0.42)	0.80	(0.40)	0.03*	(0.01)
Married	0.56	(0.50)	0.49	(0.50)	-0.07**	(0.01)
Number of children	0.78	(1.01)	0.73	(0.98)	-0.05*	(0.03)
PCS	53.37	(7.73)	52.88	(8.08)	-0.48*	(0.21)
University degree or equiv.	0.49	(0.50)	0.43	(0.50)	-0.06**	(0.01)
House, fully paid	0.16	(0.37)	0.15	(0.36)	-0.00	(0.01)
House, mortgage	0.60	(0.49)	0.55	(0.50)	-0.05**	(0.01)
N unique	110,985		1,479			

Table 1: Balance Table

Note: This table contains reference period means and standard deviations (in parentheses) of covariates for all unique individuals in the treatment and the unweighted control group. Column 3 displays the differences in means, the differences' standard deviation, and asterisks indicating the p-value of a two-sided t-test. * p< 0.05, ** p< 0.01.

7 Figures

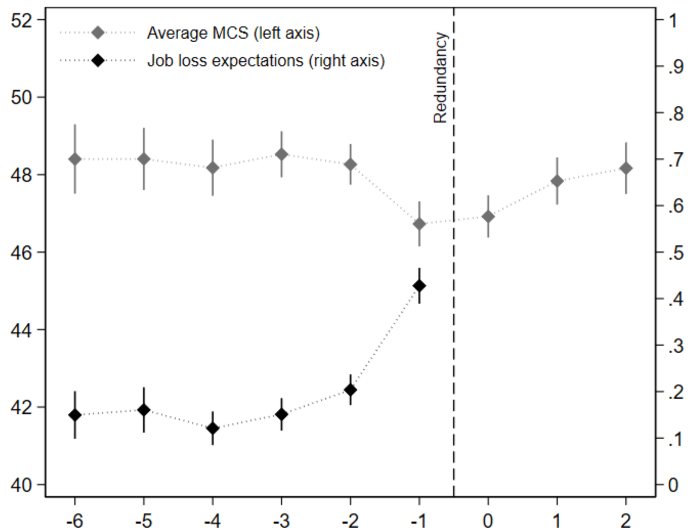


Figure 1: Mental Health and Job Loss Expectations

Note: The figure shows the average MCS scores and the shares of employed individuals reporting finding it likely or most likely to lose their job within 12 months by event time (years). The sample consists of individuals experiencing redundancy between periods -1 and 0 and it in period 0. Information on job loss expectations is only contained in every other survey wave. The standard errors are clustered at the individual level. The whiskers depict 95%-level confidence intervals.

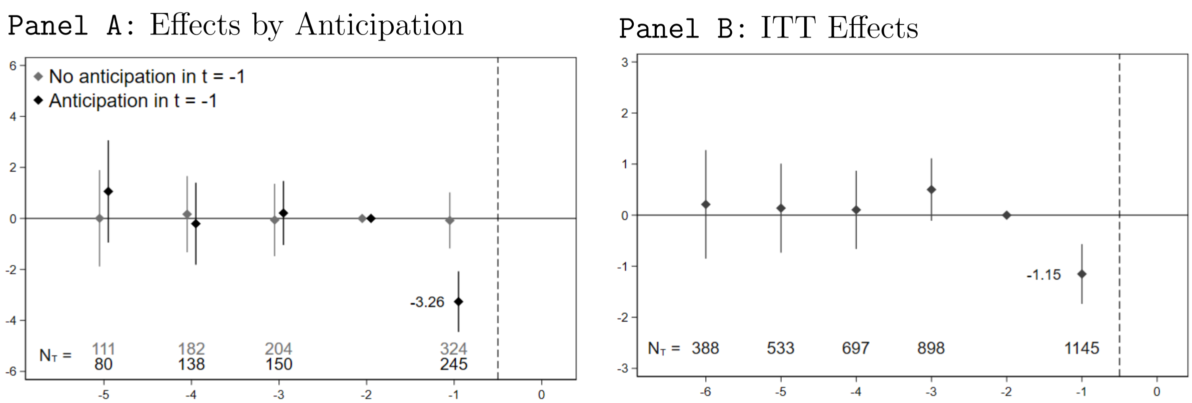


Figure 2: Mental Health Effect of Anticipating Job Termination

Note: The figure contains stacked event study estimates of the anticipation mental health effect of job termination. The MCS serves as the outcome, continuously employed workers as the control group, and period −2 serves as the reference period. Each point estimate stems from a separate entropy-weighted OLS estimation of eq. (2). In Panel A, the estimation is done separately for treatment group units that either do or do not expect job loss in period −1. Panel B contains the effects using the full sample. The numbers at the bottom of both panels denote the size of the treatment group. Standard errors are clustered at the individual level. The whiskers depict 95%-level confidence intervals.

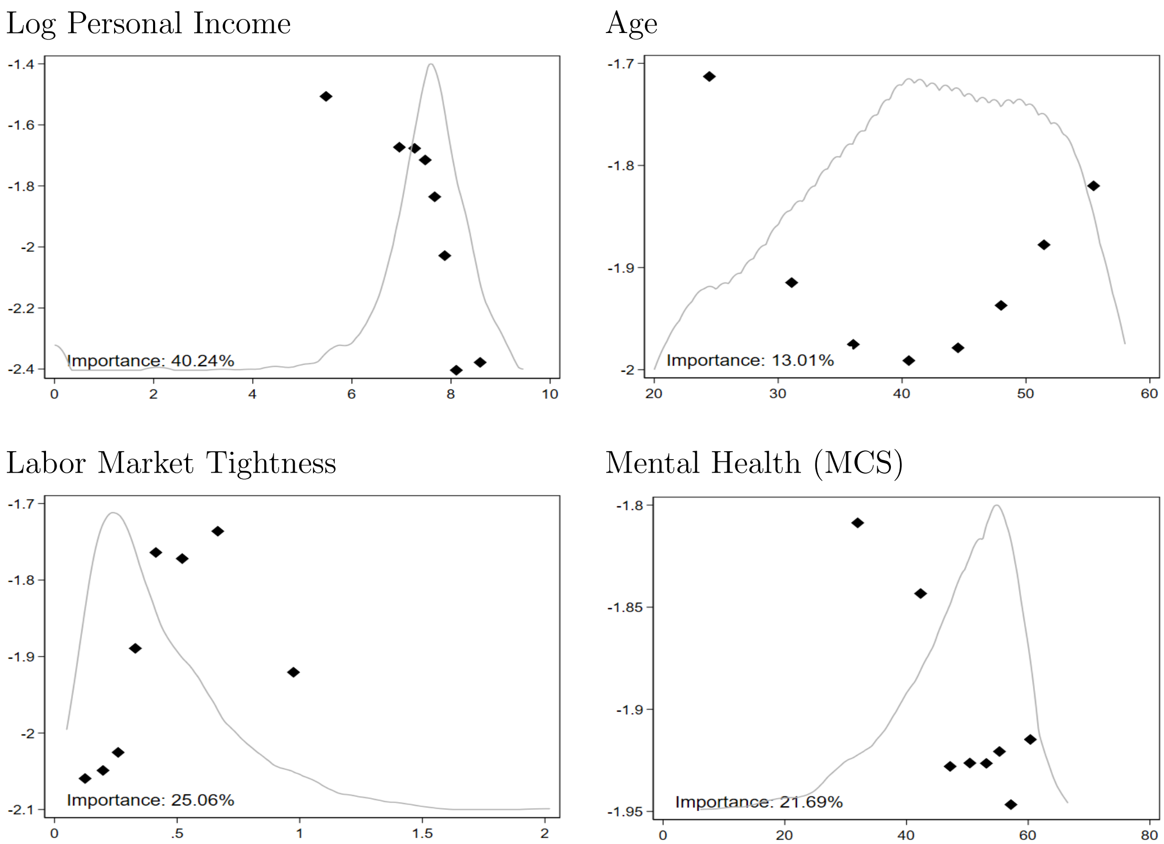


Figure 3: Causal Forest Heterogeneity Analysis

Note: The figure contains the CATE of the anticipation mental health effect across octiles of four continuous covariates at baseline level using the causal forest algorithm proposed by Wager and Athey (2018). The title of each sub-figure denotes the covariate. The covariates are measured during the reference period, -2. Each sub-figure also shows the importance of the covariate in predicting the effect heterogeneity. The change in the MCS with a partialled-out common time trend serves as the outcome. For computation, the R package *grf* was used. The grey lines show the reference-period density of the covariates for the treatment group.

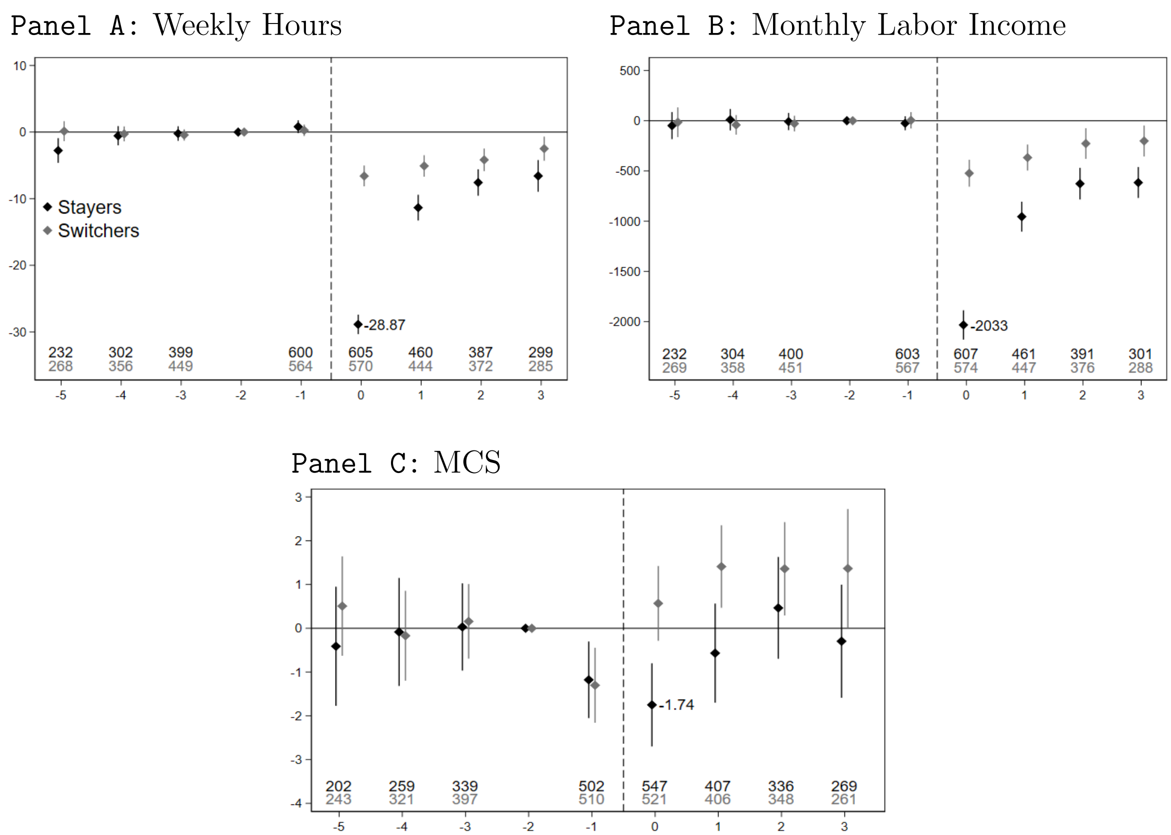


Figure 4: Benchmark Effects of Job Termination

Note: The figure contains stacked event study estimates that separately compare stayers and switchers to a control group of continuously employed workers. Both stayers and switchers experienced job termination between periods -1 and 0 yet only stayers had an unemployment spell afterward. The outcome is denoted by the title of each sub-figure. Period -2 serves as the reference period. Each point estimate stems from a separate entropy-weighted OLS estimation of eq. (2). The numbers at the bottom of each plot denote the size of the treatment group contributing to the respective point estimate. The standard errors are clustered at the individual level. The whiskers depict 95%-level confidence intervals.

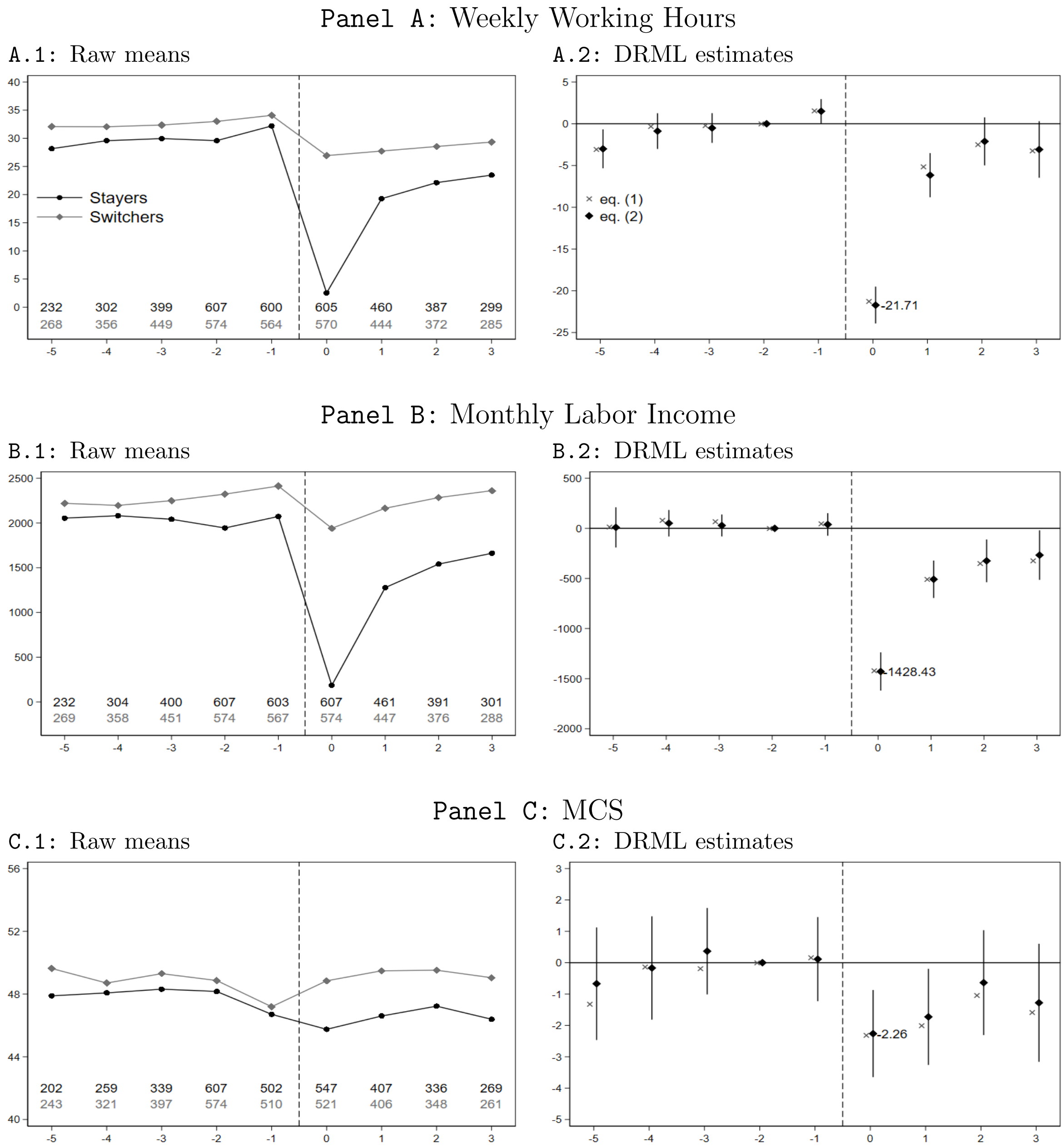


Figure 5: Effects of Leaving Terminated Positions

Note: The left-side figures contain raw group-specific means of the variables denoted in the row titles by event time. Both stayers and switchers experienced job termination between periods -1 and 0 yet only stayers had an unemployment spell afterward. The bottom numbers denote group-specific sample sizes. The right-side figures show analogous estimates of the effect of staying in terminated positions. The crosses and the diamonds denote the estimates from eq. (3) and eq. (4), respectively. The covariate vector for the DRML estimation contains the labor market tightness in the year before job termination, stack fixed effects, and baseline-level variables surviving post-lasso selection. The standard errors are clustered at the individual level. The whiskers depict 95%-level confidence intervals.

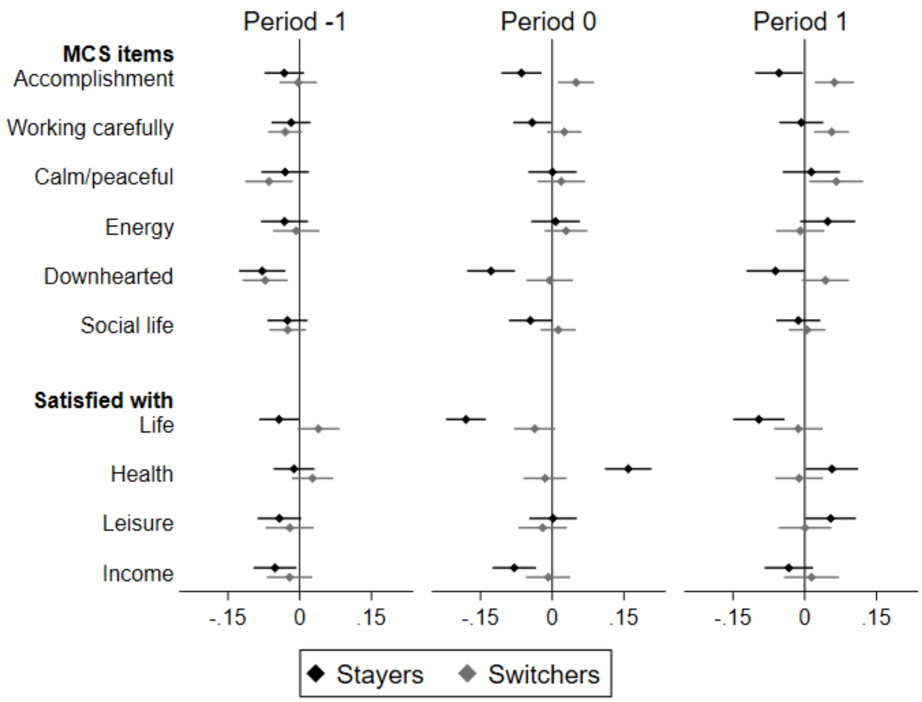


Figure 6: Factor Analysis

Note: The figure contains stacked event study estimates comparing stayers and switchers to a continuously employed control group. As outcomes serve the dichotomized questions of the MCS (1 if the respondent chose one of the two most positive answers) and additional indicators on whether an individual is "mostly" or "completely satisfied" with the respective dimension of well-being on a five-point scale. Each point estimate stems from a separate entropy-weighted OLS estimation of eq. (2). Period -2 serves as the reference period. The standard errors are clustered at the individual level. The whiskers depict 95% confidence intervals.

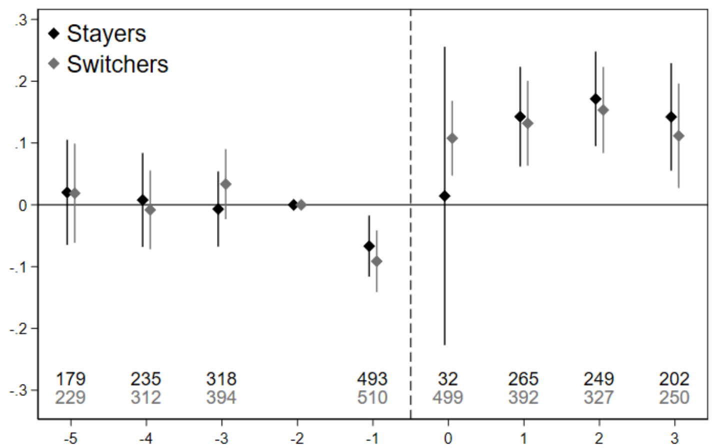


Figure 7: Effects on Job Satisfaction

Note: The figure contains stacked event study estimates comparing stayers and switchers to a continuously employed control group. The outcome is an indicator of being "mostly" or "completely satisfied" with their job. Each point estimate stems from a separate entropy-weighted OLS estimation of eq. (2). The sample contains individuals employed both in the reference period, -2, and in the target period. The standard errors are clustered at the individual level. The whiskers depict 95%-level confidence intervals.

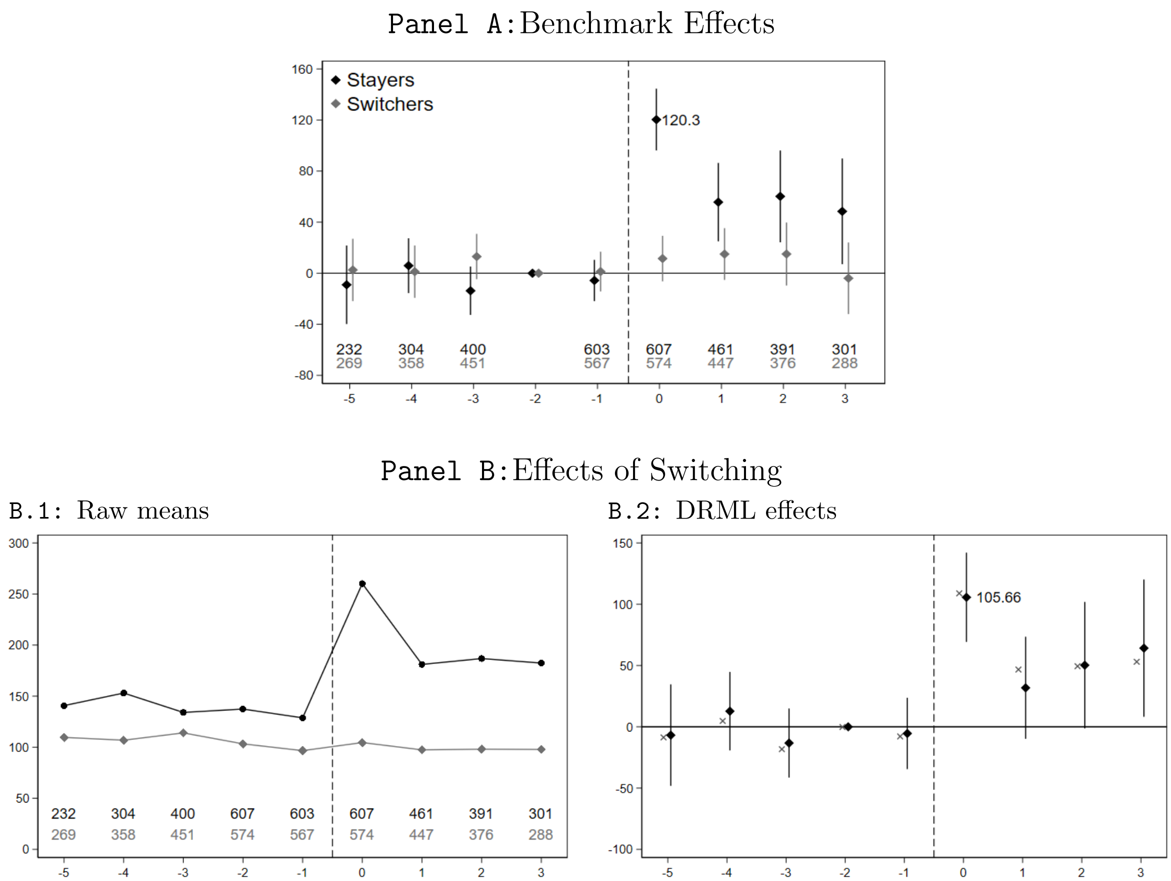


Figure 8: The Effects of Switching on Social Benefit Uptake

Note: Panel A (B) replicates Figure 4 (5) using the amount of weekly social benefit income (in £) as outcome. For additional information, see Figure 4 and 5 notes.

A Appendix A - Background Information and Sample Characteristics

		Early Notification Period	
Country	Paper	Min	Max
Austria	Kuhn et al. (2009)	1 day	5 months
Denmark	Browning et al. (2006); Browning and Heinesen (2012)	0 days	6 months
Germany	Everding and Marcus (2020); Kaiser et al. (2017); Kassenboehmer and Haisken-DeNew (2009); Kunze and Suppa (2017); Kunze and Suppa (2020); Marcus (2013); Marcus (2014); Preuss and Hennecke (2018); Schiele and Schmitz (2016); Schmitz (2011)	2 weeks	7 months
Norway	Michaud et al. (2016); Østhus (2012)	2 weeks	6 months
Netherlands	Been et al. (2024)	1 month	4 months
Sweden	Eliason and Storrie (2009a); Eliason and Storrie (2009b)	2 weeks	12 months
US	Salm (2009); Deb et al. (2011); Schaller and Stevens (2015)	0 days	60 days

Table A.1: Early Notification Laws and The Economic Literature on the Health Effects of Job Terminations

Note: This table lists peer-reviewed economic papers studying the health effects of job termination with the respective upper and lower bound of the countries' legal early notification periods in 2019 (OECD 2019). Depending on the country, the case-by-case notification periods differ by age, industry, tenure, and firm size.

MCS Questionnaire

- During the past 4 weeks, how much of the time have you had any of the following problems with your work or other regular daily activities as a result of any emotional problems (such as feeling depressed or anxious)?

  1) Accomplished less than you would like.
  2) Did work or other activities less carefully than usual.

- These questions are about how you feel and how things have been with you during the past 4 weeks. For each question, please give the one answer that comes closest to the way you have been feeling. How much of the time during the past 4 weeks...

  3) Have you felt calm and peaceful?
  4) Did you have a lot of energy?
  5) Have you felt downhearted and depressed?

6) During the past 4 weeks, how much of the time has your physical health or emotional problems interfered with your social activities (like visiting friends, relatives)?

A: All of the time / Most of the time / Some of the time / A little of the time / None of the time

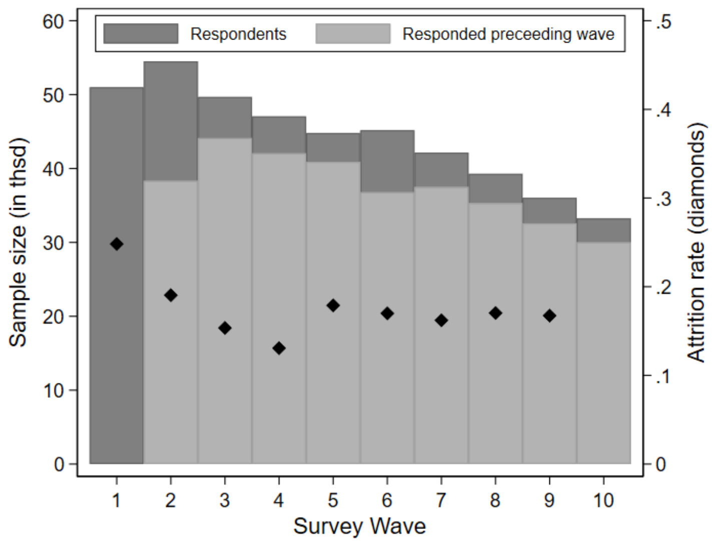


Figure A.1: Sample Size and Attrition

Note: The dark grey bars denote the number of new entrants and the light bars the number of individuals that did not drop out since the last wave. The markers denote the attrition rate after the respective survey wave.

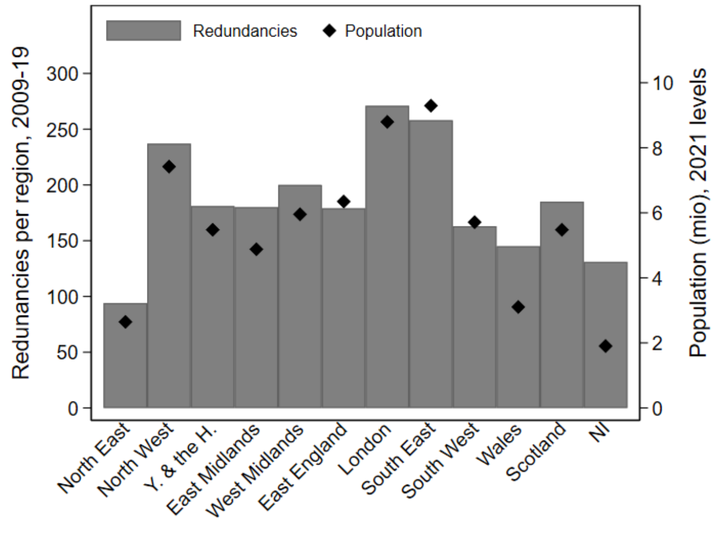


Figure A.2: Redundancies by Region

Note: The figure shows the number of reported redundancies in the UKHLS by region along with the region's population.

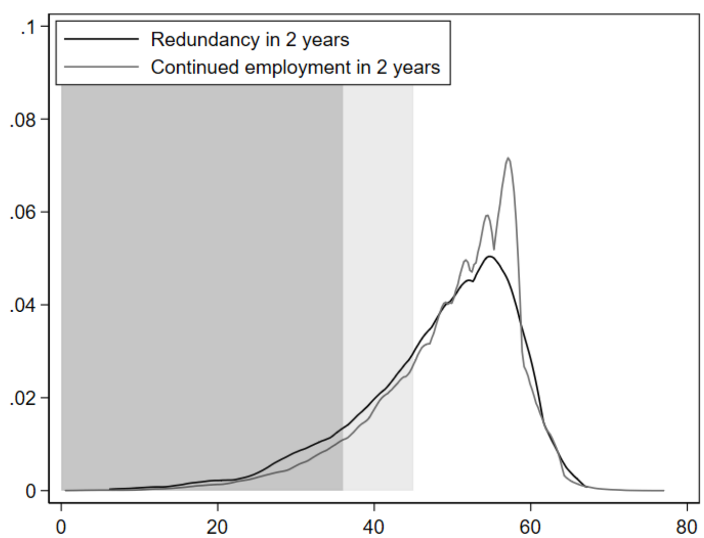


Figure A.3: MCS Kernel Density

Note: The figure shows the kernel density of the reference-period MCS for individuals in the treatment and the control group. The light-shaded area marks the presence of mental distress ($MCS < 45$) and the dark-shaded area the presence of severe mental distress ($MCS < 36$) using the thresholds proposed by Gill et al. (2007).

	Treatment Group	Stayers	Early Leavers	Control Group
-5	618	232	269	54314
-4	811	304	358	69413
-3	1053	400	451	86397
-2	1479	607	574	110985
-1	1347	603	567	105056
0	1457	607	574	110985
1	1126	461	447	84793
2	938	391	376	67537
3	733	301	288	52741
4	560	229	217	40118

Table A.2: Group Sizes

Note: The table shows the number of unique treatment and control group units by event time.

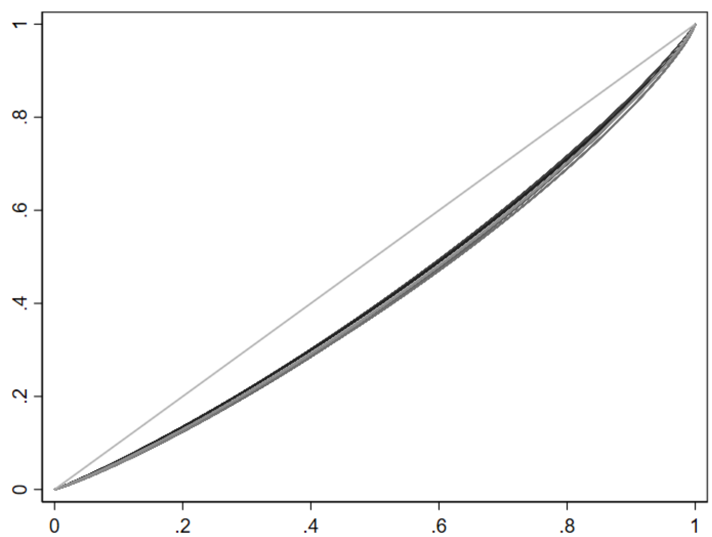


Figure A.4: Inequality Measures for Entropy Weights

Note: The Figure shows the Lorenz-Curves of the entropy weights for the control group units of each target period-specific control group. The line of perfect equality denotes the control group weights in the absence of entropy balancing.

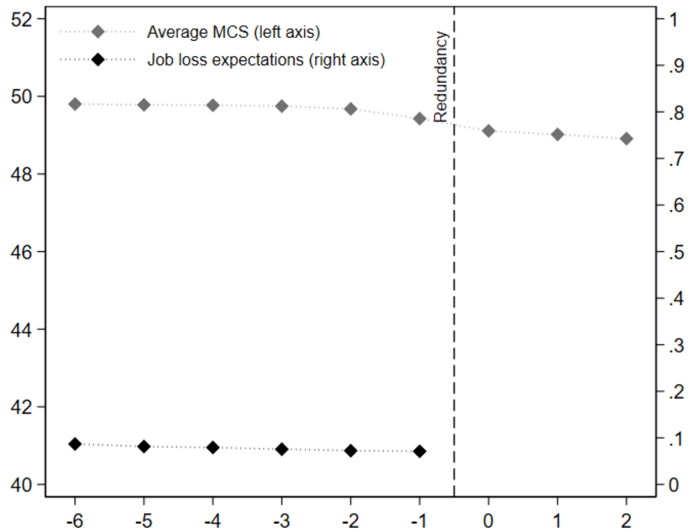


Figure A.5: Control Group Mental Health and Job Loss Expectations

Note: Analogous to Figure 1, the figure shows the average MCS scores and the shares of individuals expecting job loss in the control group by event time. The standard errors are clustered at the individual level. The whiskers depict 95% -level confidence intervals.

B Appendix B - Main Estimation

	Anticipation				No Anticipation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
t=-1	-3.66**	-3.26**	-3.11**	-0.11**	-0.19	-0.08	-0.01	0.01
	(0.63)	(0.61)	(0.48)	(0.03)	(0.56)	(0.56)	(0.47)	(0.02)
Mean	48.94	48.94	48.94	0.90	47.78	47.78	47.78	0.86
N Treat	245	245	245	245	324	324	324	324
p-val. leads								
t=-3	0.76	0.74	0.69	0.70	0.89	0.94	0.93	0.67
t=-4	0.46	0.77	0.79	0.50	0.94	0.83	0.59	0.76
t=-5	0.54	0.31	0.20	0.63	0.96	0.99	0.71	0.75
Outcome	MCS	MCS	MCS	MCS≥36	MCS	MCS	MCS	MCS≥36
Entropy		X	X	X		X	X	X
Year-Region FE			X				X	
Year-Industry FE			X				X	

Table B.1: Estimation Results by Anticipation

Note: The table contains stacked event study estimates of the anticipation effect using various model specifications and using subsets of the treatment group either that do or do not anticipate their job loss. The job terminations occur between periods -1 and 0. The underlying specifications and outcomes are shown at the bottom of the table. Continuously workers serve as the control group and period -2 as the reference period. All point estimates are produced in separate OLS estimations. The treatment group means are measured at baseline. The parentheses contain standard errors clustered at the individual level. For the placebo coefficients, only the p-values are reported. * $p< 0.05$, ** $p< 0.01$.

	(1)	(2)	(3)	(4)	(5)
t=-1	-1.20**	-1.15**	-1.24**	-0.04*	-0.03*
	(0.30)	(0.30)	(0.29)	(0.02)	(0.01)
Mean	48.25	48.25	48.25	0.69	0.88
N Treat	1145	1145	1145	1145	1145
p-val. leads					
t=-3	0.14	0.11	0.08	0.68	0.26
t=-4	0.83	0.79	0.52	1.00	0.87
t=-5	0.71	0.76	0.96	0.50	0.71
Outcome	MCS	MCS	MCS	MCS≥45	MCS≥36
Entropy Weights		X	X	X	X
Year-Region FE			X		
Year-Industry FE			X		

Table B.2: ITT Estimation Results

Note: The table contains stacked event study estimates of the ITT anticipation effect using various model specifications. The job terminations occur between periods -1 and 0. The underlying specifications and outcomes are shown at the bottom of the table. Continuously workers serve as the control group and period -2 as the reference period. All point estimates are produced in separate OLS estimations. The treatment group means are measured at baseline. The parentheses contain standard errors clustered at the individual level. For the placebo coefficients, only the p-values are reported. * $p< 0.05$, ** $p< 0.01$.

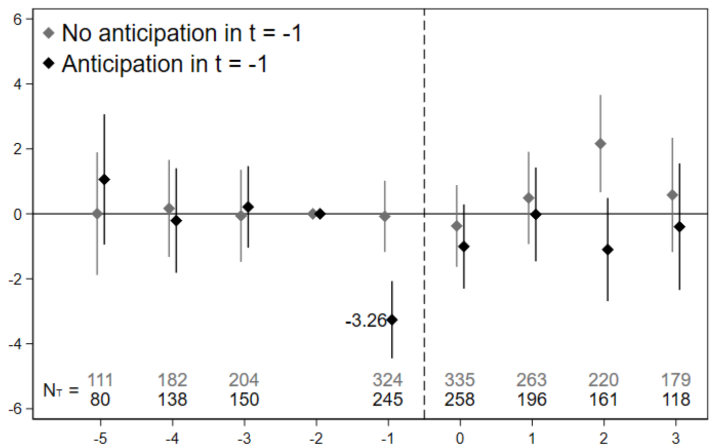


Figure B.1: Post-Treatment Estimates by Anticipation

Note: The figure extends Panel A of Figure 2 into the post-treatment period. See Figure 2 notes for further information.

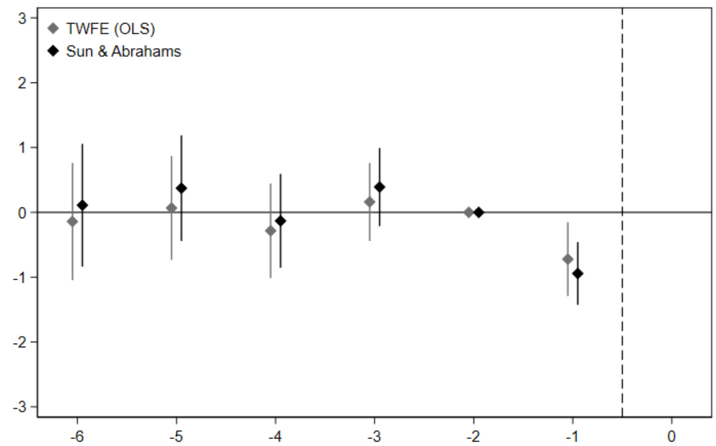


Figure B.2: TWFE Estimates

Note: The figure contains TWFE event study estimates using data containing individuals experiencing redundancy at one point in time and individuals in continued employment. The MCS serves as outcome. The underlying estimator is denoted in the legend. The standard errors are clustered at the individual level. The whiskers depict 95% -level confidence intervals.

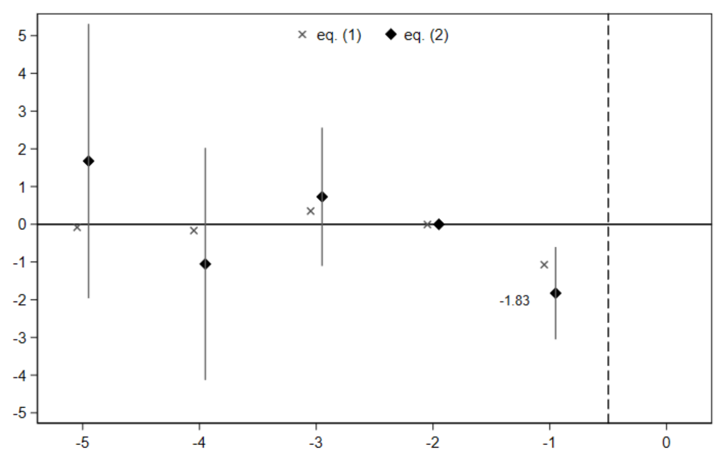


Figure B.3: ITT Estimates Not-Yet-Treated

Note: The figure contains stacked event study estimates of the anticipation mental health effect of job termination using individuals as the control group, who experience redundancy in three years or more in the future. The MCS serves as the outcome and period -2 serves as the reference period. Each point estimate stems from a separate OLS estimation of eq. (1) or eq. (2). The whiskers depict 95% confidence intervals. The standard errors are clustered at the individual level.

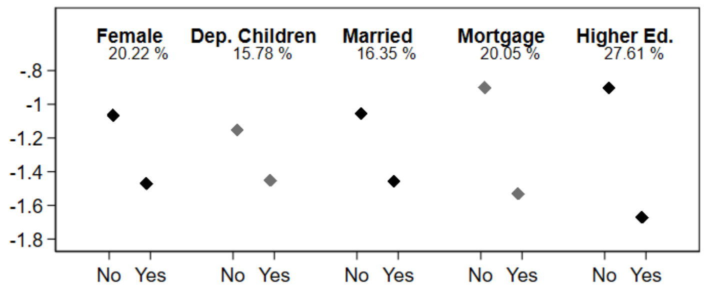


Figure B.4: CATE Across Binary Covariates.

Note: The figure contains the CATE of the anticipation mental health effect across binary covariates at baseline level using the causal forest algorithm proposed by Wager and Athey (2018). The covariates contain indicators on the respondent's sex, whether there are dependent children in the household and whether the respondent is married, has a mortgage on her house or has a university degree or equivalent qualification. The covariates are measured during the reference period, -2. The figure also shows the importance of each covariate in predicting the effect heterogeneity. The change in the MCS with a partialled-out common time-trend serves as the outcome. For computation, the R package *grf* was used.

C Appendix C - Within-Couple Spillovers

The previous literature discovered that experiencing job termination spills over to the partner's health and labor supply. To investigate health spillovers and added worker effects of anticipating job termination, I repeat the analysis on a sample of individuals whose spouse or cohabiting partner reported a redundancy or a continued employment spell in that respective wave. The UKHLS does not directly provide couple identifiers. I infer respondents as partners if either their household consists of only two adults and both report living with their partner, or if their household consists of more than two adults and exactly two of the household members report living with their partner. Moreover, I exclude individuals experiencing redundancy themselves. 713 individuals satisfy the inclusion conditions. Appendix Table C.1 contains baseline summary statistics for the group of treated and untreated partners. Using eq. (2), I estimate the period-specific household spillover effects while again fitting the distributions of observable characteristics of the control group to those of the treatment group using entropy weights. In the given setting, the identifying assumption required for consistent estimation of γ^t requires the absence of unobserved time-varying factors predicting the likelihood of spousal redundancy and changes in their outcome between periods -2 and t.

Columns 1 to 3 of Table C.2 contain the ITT results for the MCS as the outcome. Across specifications and outcomes, the placebo coefficients are statistically indistinguishable from zero. The results do not show evidence for the anticipation of job termination spilling over to the mental health of partners. Despite the small sample size, the anticipation effect is estimated rather precisely, ruling out any concerns of the null results masking effects due to power issues (Black et al. 2022). After the job termination, partners' mental health decreases slightly and to a much smaller extent compared to estimates from previous studies (e.g., Marcus 2013; Zhao 2023). The smaller effects are thereby intuitive, since in the setting of this study, not all job terminations result in unemployment.

In columns 3 to 6, I investigate anticipatory added worker effects using an employment indicator as outcome. In the baseline specification, this effect is a precise zero. Balancing the control groups' covariates using entropy weights, the anticipatory added worker effect turns positive and becomes significant at the five percent level. However, despite improving the fit of the control group, the p-values for the placebo coefficients drop vastly, raising concerns that the added-worker effect may be an artifact of a violated parallel trends assumption (Rambachan and Roth 2023). Thus overall, evidence of an anticipatory added worker effect is inconclusive.

	(1) Control		(2) Treatment		(3) Difference	
Mental Health						
MCS	49.76	(8.85)	49.12	(9.07)	-0.65	(0.34)
MCS≥45	0.76	(0.43)	0.72	(0.45)	-0.04*	(0.02)
MCS≥36	0.92	(0.27)	0.91	(0.28)	-0.00	(0.01)
Demographics						
Female	0.54	(0.50)	0.57	(0.49)	0.04*	(0.02)
Age	40.81	(8.65)	40.54	(8.99)	-0.28	(0.31)
UK ethnicity	0.81	(0.39)	0.81	(0.39)	0.01	(0.01)
England	0.79	(0.41)	0.83	(0.38)	0.04**	(0.01)
Wales	0.06	(0.24)	0.05	(0.21)	-0.01	(0.01)
Scotland	0.09	(0.28)	0.08	(0.28)	-0.00	(0.01)
Northern Ireland	0.06	(0.24)	0.04	(0.20)	-0.02*	(0.01)
Urban	0.76	(0.43)	0.79	(0.41)	0.03	(0.02)
SES						
Married	0.74	(0.44)	0.72	(0.45)	-0.02	(0.02)
Live w. Spouse	0.78	(0.42)	0.74	(0.44)	-0.04*	(0.02)
Live w. Partner	0.22	(0.42)	0.26	(0.44)	0.04*	(0.02)
Number of children	1.07	(1.07)	1.04	(1.04)	-0.03	(0.04)
University degree or equiv.	0.49	(0.50)	0.47	(0.50)	-0.02	(0.02)
House, fully paid	0.11	(0.32)	0.12	(0.33)	0.01	(0.01)
House, mortgage	0.69	(0.46)	0.64	(0.48)	-0.04*	(0.02)
Labour Market						
Monthly labour income	1,980	(1,647)	1,835	(1,723)	-145.41*	(59.70)
Weekly hours	25.98	(17.07)	24.14	(17.42)	-1.84**	(0.62)
Observations	57,867		773			

Table C.1: Household Balance Table

Note: This table contains reference period means and standard deviations (in parentheses) of covariates for all unique individuals, whose cohabiting partner experiences a continued employment spell or job termination due to redundancy. Column 3 displays the differences in means, the differences' standard deviation, and asterisks indicating the p-value of a two-sided t-test. * p< 0.05, ** p< 0.01.

	MCS			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
t=-1	-0.18	-0.14	-0.18	-0.00	0.03*	0.03*
	(0.36)	(0.36)	(0.35)	(0.01)	(0.01)	(0.01)
Mean	49.15	49.15	49.15	0.80	0.80	0.80
N Treat	649	649	649	716	716	716
t=0	-0.61	-0.54	-0.61	-0.00	0.03**	0.04**
	(0.37)	(0.36)	(0.34)	(0.01)	(0.01)	(0.01)
Mean	49.15	49.15	49.15	0.81	0.81	0.81
N Treat	692	692	692	766	766	766
p-val. leads						
t=-3	0.49	0.69	0.67	0.75	0.13	0.13
t=-4	0.59	0.64	0.85	0.66	0.36	0.29
t=-5	0.20	0.20	0.29	0.80	0.20	0.22
Entropy		X	X		X	X
Year-Region FE			X			X

Table C.2: Household Estimation Results

Note: The table contains stacked event study estimates of the anticipation and the post-redundancy effect on cohabiting spouses. Spousal job terminations occur between periods -1 and 0. Individuals whose spouse experiences continued employment serve as the control group, period -2 as the reference period, and either the MCS or an employment indicator as the outcome. All point estimates are produced in separate OLS estimations. The specification is stated at the bottom of the table. The treatment group means are measured at baseline. The parentheses contain standard errors clustered at the individual level. For the placebo coefficients, only the p-values are reported. * $p< 0.05$, ** $p< 0.01$.

D Appendix D - Stayers and Switchers

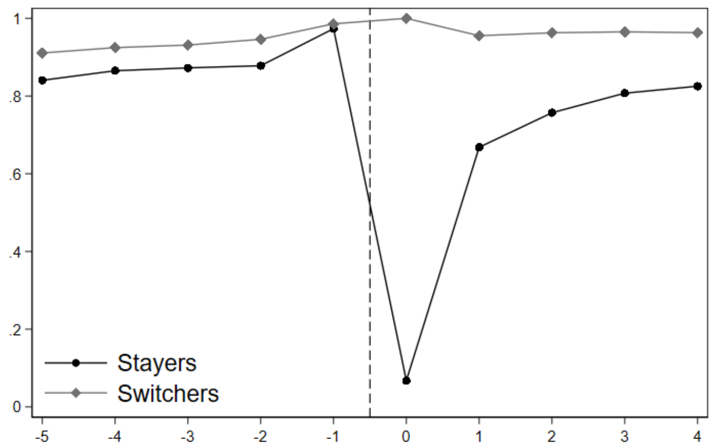


Figure D.1: Employment Rates

Note: The figure shows the employment rates by event time. Job termination due to redundancy occurs between periods -1 and 0. For stayers, job termination led to unemployment whereas switchers had an employment-to-employment transition.

	(1) Stayers		(2) Switchers		(3) Difference	
Outcome						
MCS	48.17	(9.66)	48.86	(9.66)	0.70	(0.58)
MCS≥45	0.70	(0.46)	0.72	(0.45)	0.02	(0.03)
MCS≥36	0.89	(0.32)	0.89	(0.31)	0.01	(0.02)
Monthly labor income	2,168	(1,672.80)	2,504	(1,767)	335.39**	(100.09)
Weekly hours	29.58	(14.84)	33.00	(12.58)	3.42**	(0.80)
Covariates						
Female	0.45	(0.50)	0.47	(0.50)	0.02	(0.03)
Age	40.72	(10.67)	40.62	(9.78)	-0.09	(0.60)
UK ethnicity	0.76	(0.43)	0.83	(0.38)	0.07**	(0.02)
England	0.82	(0.38)	0.82	(0.38)	-0.00	(0.02)
Wales	0.04	(0.20)	0.06	(0.24)	0.02	(0.01)
Scotland	0.08	(0.27)	0.08	(0.27)	-0.00	(0.02)
Northern Ireland	0.06	(0.23)	0.04	(0.20)	-0.02	(0.01)
Urban	0.83	(0.38)	0.77	(0.42)	-0.06**	(0.02)
Married	0.44	(0.50)	0.55	(0.50)	0.11**	(0.03)
Number of children	0.62	(0.92)	0.78	(1.01)	0.16**	(0.06)
Physical Health (PCS)	52.70	(7.96)	53.55	(7.65)	0.85	(0.47)
Degree or equiv.	0.41	(0.49)	0.48	(0.50)	0.07*	(0.03)
House, fully paid	0.17	(0.37)	0.14	(0.34)	-0.03	(0.02)
House, mortgage	0.50	(0.50)	0.62	(0.49)	0.12**	(0.03)
Labor Market Tightness	0.39	(0.26)	0.44	(0.28)	0.05**	(0.02)
Observations	607		574			

Table D.1: Balance Table for Stayers and Switchers

Note: The table contains reference period means and standard deviations (in parentheses) of covariates of stayers and switchers. The industry-specific labor market tightness presented in the last row is not measured in period -2 but poses the average over the quarter, in which the redundancy was reported, and the three quarters before. Column 3 displays the differences in means, the differences' standard deviation, and asterisks indicating the p-value of a two-sided t-test. For stayers, job termination led to unemployment whereas switchers had an employment-to-employment transition. * $p < 0.05$, ** $p < 0.01$.

	Stayers				Switchers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
t=-1	-1.25**	-1.18**	-1.39**	-0.02	-1.25**	-1.30**	-1.27**	-0.05**
	(0.46)	(0.45)	(0.39)	(0.02)	(0.44)	(0.44)	(0.38)	(0.02)
Mean	48.14	48.14	48.14	0.88	48.77	48.77	48.77	0.89
N Treat	502	502	502	502	510	510	510	510
t=0	-1.71**	-1.75**	-1.44**	-0.07**	0.61	0.57	0.40	0.01
	(0.48)	(0.48)	(0.42)	(0.02)	(0.44)(0.44)	(0.41)	(0.02)	
Mean	48.17	48.17	48.17	0.89	48.86	48.86	48.86	0.89
N Treat	547	547	547	547	521	521	521	521
t=1	-0.47	-0.57	-0.54	-0.05*	1.42**	1.41**	1.84**	0.04*
	(0.58)	(0.58)	(0.50)	(0.02)	(0.49)	(0.48)	(0.42)	(0.02)
Mean	47.76	47.76	47.76	0.88	49.02	49.02	49.02	0.89
N Treat	407	407	407	407	406	406	406	406
t=2	0.38	0.47	0.21	-0.02	1.25*	1.36*	1.92**	0.05**
	(0.60)	(0.59)	(0.50)	(0.03)	(0.55)	(0.54)	(0.51)	(0.02)
Mean	47.85	47.85	47.85	0.88	49.26	49.26	49.26	0.90
N Treat	336	336	336	336	348	348	348	348
Outcome	MCS	MCS	MCS	MCS≥36	MCS	MCS	MCS	MCS≥36
Entropy		X	X	X		X	X	X
Year-Region FE			X				X	
Year-Industry FE			X				X	

Table D.2: The Total Mental Health Effect of Job Termination

Note: The table contains stacked event study estimates of the total effect of job termination using various model specifications and using subsets of the treatment group. For stayers, job termination led to unemployment whereas switchers had an employment-to-employment transition. Job termination occurs between periods -1 and 0. The underlying specifications and outcomes are shown at the bottom of the table. Continuously workers serve as the control group and period -2 as the reference period. All point estimates are produced in separate OLS estimations. The treatment group means are measured at baseline. The parentheses contain standard errors clustered at the individual level. * $p< 0.05$, ** $p< 0.01$.

	(1)	(2)	(3)	(4)	(5)
Panel A: Working Hours					
γ^0	-21.2	-21.5	-21.5	-22.0	-21.8
	(1.10)	(1.13)	(1.21)	(1.17)	(1.18)
Panel B: Labor Income					
γ^0	-1408	-1413	-1434	-1466	-1444
	(97.1)	(100.4)	(97.1)	(96.7)	(95.9)
Panel C: MCS					
γ^0	-2.11	-2.19	-2.19	-2.11	-2.11
	(0.67	(0.6)7	(0.67)	(0.68)	(0.68)
Stack FE	X	X	X	X	X
LM tightness	X	X	X	X	X
Covariate pool for $p(x)$					
Region FE		X	X	X	X
Industry FE		X	X	X	X
Covariates			X		X
Covariate pool for $\mu(x)$					
Region FE		X	X	X	X
Industry FE		X	X	X	X
Covariates				X	X

Table D.3: DRML Covariate Selection

Note: The table contains DRML estimates of the period-specific effect of staying in terminated positions and suffering unemployment compared to switching positions in time using different pools of covariates for the post-lasso selection. The labor market tightness in the year before the job termination and stacked fixed effects are always used for predicting the propensity of being a stayer and the outcome model. Period -2 serves as the reference period. Column (5) corresponds to the main specification used in Figure 5. The outcome is denoted by the sub-headers. The standard errors (in parentheses) are clustered at the individual level. All estimates are significant at the 1% level.

E Appendix E - GHQ Replications

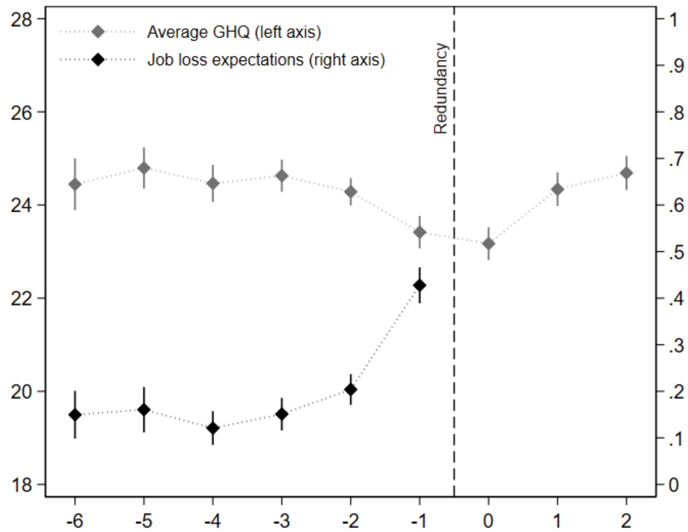


Figure E.1: GHQ and Job Loss Expectations

Note: The figure replicates Figure 1 using the GHQ as outcome. See Figure 1 notes for further information.

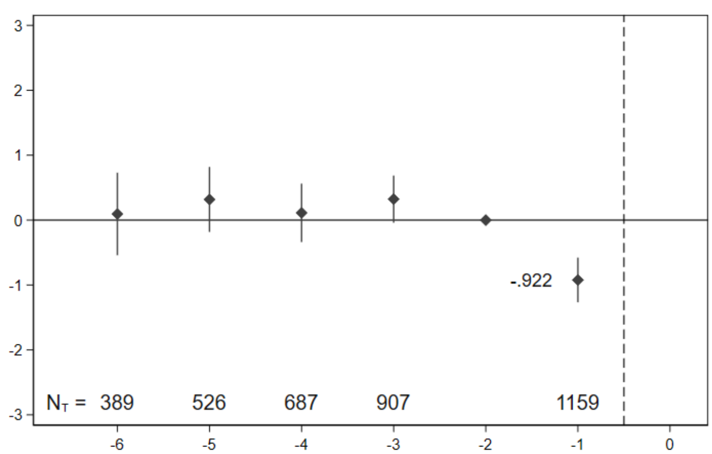
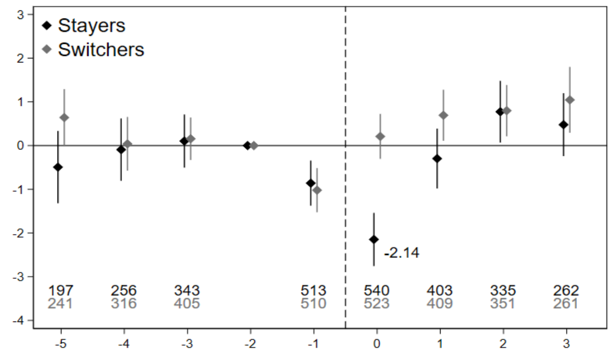


Figure E.2: GHQ Intent-to-Treat Anticipation Effect

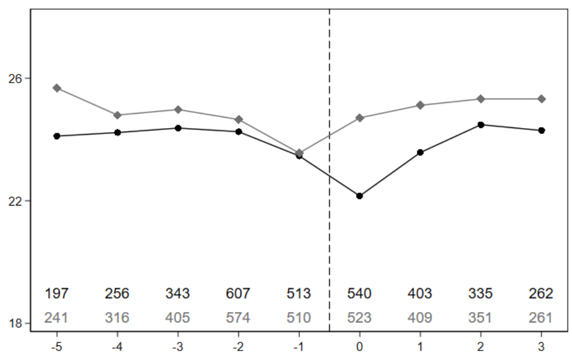
Note: The figure replicates Panel B of Figure 2 using the GHQ as outcome. See Figure 2 notes for further information.

`Panel A:`Benchmark Effects



`Panel B:`Effects of Switching

`B.1:` Raw means



`B.2:` DRML estimates

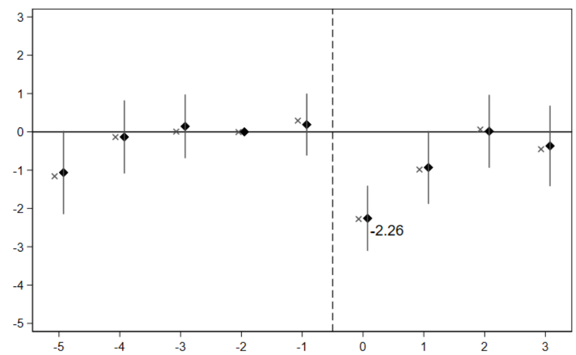


Figure E.3: Switching Effects on GHQ

Note: Panel A (B) replicates Figure 5 (6) using the GHQ as outcome. See Figure 5 and 6 notes for additional information.

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