How Do Acquisitions Affect the Mental Health of Employees?

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Abstract

Using employer-employee level data linked to individual health records, we document that the incidence of stress, anxiety, depression, psychiatric medication usage, and even suicide increase following acquisitions. These effects are prevalent among employees from both targets and acquirers, in weak as well as in growing, profitable firms. Employees who experience negative career developments within the merging firms, 'blue-collar' workers, and employees with lower cognitive and non-cognitive skills are most affected. A variety of tests address endogeneity concerns, including an analysis exploiting failed mergers. Our findings point to mental illness as a significant non-pecuniary cost of acquisitions.

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1 Introduction

To survive and prosper in a continually changing economic environment, businesses often reorganize and restructure through consolidation.[1] While a large body of academic research in economics and finance has analyzed the causes and consequences of mergers and acquisitions (M&As), most of this work has focused on their financial determinants and outcomes. This is perhaps unsurprising, since the fiduciary duty of the management team is to maximize shareholder value it is natural to ask what is the financial return to merge.

This almost exclusive focus on firm value does not allow us to understand whether mergers are good for society. After all, maximization of firm value may come at the expenses of other stakeholders. This is because the impact of a merger can reach far beyond the wallets of shareholders and debtholders. IO economists and antitrust regulators have long understood that customers may be harmed by anticompetitive mergers. However, the impact of mergers on workers has been largely neglected both by academics and policy makers.Despite the importance of human capital for firms, few studies have examined the consequences of acquisitions for employees.

Through mergers, firm boundaries are redrawn and internal work processes are reorganized. These changes may transform the scope and nature of jobs and may lead to dismissals. The uncertainty, anxiety, and stress caused by such events may have a profound impact on the workers in the affected firms. In this paper, we investigate how acquisitions affect the mental health of employees.

Mental health is a key determinant of individual well-being and economies incur significant costs due to mental illness in the population.[2] While these costs are difficult to

[1] According to data from The Institute for Mergers, Acquisitions and Alliances (IMAA), the value of global mergers and acquisitions deals amounted to almost $4tn in 2019. In comparison, according to Dealogic, $0.7tn was raised globally through the equity capital markets in the same year.

[2] Several studies document a strong and robustly negative correlation between life satisfaction and the presence of mental illness (e.g., Rissanen et al. (2013), Strine et al. (2009), Fergusson et al. (2015), Rissanen et al. (2011), Bray and Gunnell (2006), Touburg and Veenhoven (2015), Layard et al. (2013), Lombardo et al. (2018)). According to the World Health Organization (WHO), the directing and coordinating authority for health within the United Nations System, "depression is one of the leading causes of disability. Suicide is the second leading cause of death among 15 to 29 year-olds. People with severe mental health conditions die prematurely—as much as two decades early."

quantify precisely, the direct (e.g., health expenditures related to impaired mental health) and indirect (e.g., lower productivity, reduced labor market participation, and premature mortality) costs of mental illness could amount to up to four percent of GDP (Smetanin et al. (2011)).

Existing evidence on the impact of acquisitions on mental health is scant. An important hurdle in such studies is data availability: detailed employer-employee matched data and more importantly, data on employees' individual health status is required. This sensitive information is not included in standard databases and may be generally difficult to compile in countries with fragmented healthcare systems, like the U.S. Selection effects are another challenge. Because both the firm's decision to restructure and the employee's choice to work for a particular firm may be related to innate and unobservable mental resilience, it is difficult to isolate the effect of a merger on mental health.

Our data cover all M&A transactions in the Swedish economy between 2007 and 2015. The employer-employee level data enable us to track the employment of workers before and after a merger. Furthermore, because Sweden has universal healthcare and centrally administers contact with healthcare providers, we are able to link all employees to their individual health outcomes, including episodes of stress and anxiety, depression diagnoses, hospital visits, psychiatric medication usage and dosages, and death and suicide records.

Whether employees of target and acquirer firms develop mental illness due to a merger is, a priori, theoretically ambiguous. On the one hand, mergers can significantly increase the levels of anxiety and stress of workers due to the potential impact of the firm's reorganization on their careers and work-life balance. On the other hand, working at a larger firm with more market power, a larger internal labor market, and better access to finance may provide a level of wage and employment stability that is beneficial for workers' mental health.

Using a wide array of measures, we find that acquisitions negatively impact workers' mental health. Relative to the pre-merger period, the likelihood of seeking outpatient (ambulatory) care increases by 2% after an acquisition, while the likelihood of becoming a patient in a hospital increases by 5%. The likelihood of being diagnosed with a mental illness

(including depression and anxiety) increases by 3%; the likelihood of being diagnosed with depression specifically increases by 8%. The probability of taking antidepressants increases by 2% after the takeover, as does the probability of taking psychiatric medication. Most strikingly, acquisitions have a large effect on the probability of death and death by suicide, which increase by, respectively, 30% and 44% (the annual risk of dying before the M&A is approximately 0.14%). Many of these effects remain elevated for three or more years after the takeover.

To put these economic magnitudes into perspective, we compare the effects of business combinations on mental health with several other marked events that the workers in our sample experience. The negative effect of mergers on mental health is quantitatively similar to the effect of marriage, but with the opposite sign. While business combinations may induce mental health problems that last for five years, marriages alleviate them for a similar time period. The effect of a merger on the mental health of employees is however smaller in absolute terms than that of divorce, unemployment, or the birth of the first-born child.

Mergers are not random events. Therefore, a worsening of mental health following a merger may not reflect a causal effect of acquisitions on worker well-being. To address endogeneity concerns, we conduct a variety of additional tests that, taken as a whole, increase the confidence that the negative effect of acquisitions on mental health is indeed causal. First, using the procedure proposed by Chetty, Looney and Kroft (2009), we address the concern that the effects might be driven by some random unobservable variation rather than the acquisition event itself. We randomly assign 'placebo-merger' events to employees and re-estimate our baseline regression 1,300 times. Based on this exercise, we find that it is exceedingly unlikely that the results are driven by random confounding variation instead of the actual merger events. We also conduct an additional placebo test in which we study the evolution of genetic physical disorders around merger events and find no effect.

Another concern is that economic and/or financial distress of the target may trigger an acquisition, and that the underlying factors causing the corporate distress may be the root cause of the decline in the well-being of workers. Because we document that employees at the

acquirer firms also suffer from a deterioration of their mental health, our findings are unlikely to be solely driven by the economic or financial distress of the target. Notwithstanding, to address this omitted variable concern more directly, we re-estimate our baseline regression using subsamples of target firms that are profitable and growing prior to the acquisition (i.e., firms unlikely to be experiencing economic distress), as well as low-leverage targets (i.e., firms unlikely to be financially distressed). Consistent with the analysis based on the full sample, we find that the workers in non-distressed target firms experience a worsening of their mental health following an acquisition.

Our main empirical strategy to address endogenous selection and other threats to identification is to compare successful mergers to mergers that were registered but failed. We first identify the employees of companies that are involved in *failed* mergers. In this subsample, we re-estimate our baseline regression comparing the mental health of workers pre- and post-attempted merger. We detect no change in the mental health status of workers of firms that attempt but fail to merge. These findings further support the interpretation of our main tests: acquisitions, and not other confounding factors that trigger a merger, are responsible for the adverse mental health outcomes experienced by workers. Next, we employ a difference-in-differences specification where we compare the evolution of the mental health of workers from successfully merged firms—the 'treatment' group—with the mental health of workers of firms that initiated but did not consummate a merger—the 'control' group—around the date of the attempted merger. In these difference-in-differences regressions, we observe broadly comparable results to those reported in the main specifications. We also test for differences in 'pre-trends' across the two groups. After the merger, workers in the treatment group experience a deterioration in their mental health relative to workers in the control group, while the mental health of the two groups evolves in parallel prior to the merger. Taken as a whole, the evidence supports the conclusion that acquisitions cause a worsening of the mental health of workers.

One of our main contributions is to investigate the mechanisms that drive the negative impact of mergers on the mental health of workers. We do so by studying the extent to

which the effect varies across workers and firms. Because acquisitions tend to be associated with job displacement, our results could, in principle, be entirely driven by job loss.[3] At the same time, acquisitions typically involve a variety of restructuring measures which are not confined to employment cuts and that could also impair workers' mental health. For example, expected promotions may be delayed or may not materialize at all, workers may be reassigned within the firm to less desirable positions, or their workload may increase considerably. To study whether non-departing workers of the merging firms also experience negative mental health effects, we focus on a sub-sample of 'stayers', that is, workers who remain with the merging firms during the year of the merger and the year thereafter. We continue to observe a statistically significant increase in the incidence of mental health problems among this group of workers. To shed further light on the effect of merger-related restructuring on the mental health of workers, we compare 'stayers' who experience wage cuts following the mergers to other 'stayers'. We find that relative to workers whose wages increase or remain unchanged after the merger, the mental health among workers who experience wage cuts significantly deteriorates. Taken together, these results suggest that takeover-related restructuring, not only job loss, is associated with the worsening of workers' mental health.

Next, we study the mental health outcomes of workers in firms that are more likely to be restructured in the aftermath of a merger. It is plausible that a takeover target may be subject to more intense restructuring and reorganization activities than the acquiring firm, which may lead to more pronounced effects experienced by the target firm's workers. Consistent with this conjecture, we find that workers at the target firm are more likely to develop mental health problems than those in the acquiring firm—the negative effect on mental health among target firm workers is about twice as large as that among acquirer workers. Further, horizontal mergers, in which firms that are active in the same industry are combined, may plausibly create larger opportunities for employee cost savings than vertical or conglomerate mergers. This may lead to more detrimental mental health effects

[3]The link between job losses and mental health problems has been documented in studies such as Björklund (1985), Cygan-Rehm, Kuehnle and Oberfichtner (2017), and Farre, Fasani and Mueller (2018).

for employees of firms involved in a horizontal merger. Using industry codes of different granularity, we find evidence that is consistent with this hypothesis. Relative to the average effect for all workers, employees involved in a horizontal merger are significantly more likely to develop mental health problems than workers in other merging firms.

Besides the characteristics of the merging firms, differences in worker attributes may affect how workers respond to corporate restructuring. First, we examine whether there is heterogeneity in the response to acquisitions based on workers' skills, innate abilities, and organizational status. Highly skilled workers may more easily adapt to and even thrive in a changing environment, while workers with a more modest skill endowment may be more exposed to the risk of becoming redundant or seeing their status within the firm diminish. Using data on military enlistment test scores for male workers, we observe consistently worse mental health effects of M&As for workers with innate ability that ranks below the sample median, both when it comes to cognitive skills (such as IQ) and noncognitive skills (such as empathy, perseverance, and intrinsic motivation). We also find that 'blue-collar' workers, that is, clerks, service workers, plant and machinery operators, and workers in other elementary occupations, experience worse mental health effects after a merger than other workers. For example, the incidence of depression, anxiety, suicide, and antidepressant and psychiatric medication use increase significantly more for blue-collar workers after an acquisition compared to other employees. Consistent with these observations, workers with lower educational attainment (high school or less) experience more detrimental mental health effects than more educated employees.

Finally, we study several personal characteristics of workers that may plausibly lead to heterogeneity in the response to the merger. We are unaware of evidence of innate gender differences in mental health per se. However, women are more likely to acknowledge health problems and to seek healthcare than men, and they do so earlier when problems arise (e.g., Leong and Zachar (1999), Mackenzie, Gekoski and Knox (2006), Nam et al. (2010), and WHO (2002)). These gender differences are useful to understand the differences in mental health effects of acquisitions between male and female workers in our sample. We

find that relative to their male co-workers, women are more likely to experience mental health problems following the merger. We also investigate whether foreign-born workers experience different mental health outcomes after the merger than other workers—perhaps because such workers are more likely to become unemployed.[4] However, we obtain estimates that are small and only marginally statistically significant, suggesting that native-born and foreign-born workers experience broadly similar mental health effects following mergers. Similarly, we do not find any significant differences in the effect of mergers on mental health across the age spectrum of workers.

Taken as a whole, our results paint a rich picture of the evolution of employees' mental health around corporate restructuring through M&A. We document negative effects of acquisitions on the well-being of workers, comparable in magnitude to other important personal events in workers' lives such as marriage. We find that these effects are not confined to workers who experience job loss: employees who remain employed at the merging firms also experience negative mental health effects, in particular if their jobs are more affected by the reorganization within the firm.

This paper increases our understanding of the economic effects of acquisitions. Most of this research has focused on the financial causes and consequences of such events (e.g., Ahern and Harford (2014); Betton, Eckbo and Thorburn (2008)). A recent strand of the literature studies the impact of takeovers on employees, but focuses on worker compensation, employment (e.g., Davis et al. (2014), Li (2013)), and workplace safety (Cohn, Nestoriak and Wardlaw (forthcoming)).[5] Our study also relates to two recent studies on the labor market consequences of mental illness. Biasi, Dahl and Moser (2021) document significant career effects of mental illness and Garcia-Gomez, Maug and Obernberger (2020) show that pre-deal health characteristics of target firm workers predict employment outcomes in the aftermath of private equity buyouts. In contrast, we study how corporate policy choices affect the mental health of workers.

[4] According to OECD data, immigrant workers are affected to a greater extent by unemployment than native-born workers in European countries.

[5] In less directly related work, Cohn and Wardlaw (2016) document that financial constraints affect workplace safety. Further, Keloharju, Knüpfer and Tåg (2020) study the mental health attributes of CEOs.

Our paper also relates to work in health economics which has investigated the impact of adverse macroeconomic conditions on mental health outcomes (for a recent survey of this literature, see Lombardo et al. (2018)). Such estimates cannot disentangle the many channels through which the macroeconomic environment affects peoples' lives. Instead, we focus on specific corporate restructuring events which are not purely driven by business cycles and are likely to be mostly idiosyncratic in nature. Moreover, because our effects are not limited to instances of job loss, we can document the degree to which corporate restructuring can affect mental health outcomes even for workers who remain employed during the business combinations.

Closer to our paper are studies in health economics which investigate the impact of specific corporate events on the mental health of affected workers (as measured by the prescription of antidepressants). Dahl (2011) studies the impact of organizational changes on Danish workers, while Magnusson Hanson et al. (2016) investigate the consequences of mass dismissals in Swedish firms. Our work differs from theirs in several ways. First, unlike these studies, we focus on a concrete form of organizational change, mergers and acquisitions, rather than restructuring more broadly, which enables us to better address endogeneity concerns. Second, we do not only consider the mental health of employees at companies which are likely to be most affected by dismissals, i.e., those at target firms, but also workers at acquiring firms that are less likely to lose their job. Indeed, we document that workers who are not dismissed also experience an increase in mental health problems. Third, we examine a much more exhaustive array of mental health outcomes, which is not restricted to the usage of antidepressants but also includes actual mental illness diagnoses made by physicians, as well as suicides.

Finally, our paper also contributes to studies in the field of organizational psychology and occupational medicine that examine the effects corporate restructuring on the well-being and mental health of workers. These studies typically analyze one restructuring event and its effect on workers in a case-study type setting. de Jong et al. (2016) review recent work in this field. In contrast to these studies, our paper provides a systematic analysis of the

economic effects of acquisitions on different workers using a large sample of M&A events and a wide range of mental health indicators.

2 Empirical Setting, Data, and Summary Statistics

In this section, we describe our data sources as well as our main variables of interest, and we present summary statistics.

2.1 Data sources

The main dataset used in our analysis is obtained by matching longitudinal data on socio-economic outcomes of Swedish individuals, the Longitudinal Database on Education, Income and Occupation (LISA) from Statistics Sweden (SCB), with medical data from the National Board of Health and Welfare, firm-level data from the Serrano database, and data from military enlistment records from the National Archives and The Swedish Defense Recruitment Agency.[6] LISA contains detailed employee-employer matched information for the whole Swedish population aged 16 years or older. A large set of socio-economic variables, such as age, gender, employment, uncensored wages, and social security benefits, are contained in LISA. Our sample period is 2006 to 2015.

The data from the National Board of Health and Welfare includes (i) information on all diagnoses and surgical treatments of both in- and out-patients from the National Patient Register, (ii) all causes of death from the National Death Register, and (iii) information on all prescribed drugs dispensed at pharmacies from the Swedish Prescribed Drug Register. These data include, among other information, the drug name, a classification of the active ingredient of the drugs according to the Anatomical Therapeutic Chemical (ATC) code, the prescribed quantity/number of packages, strength, date of prescription, and date of purchase.

Military enlistment records include information on the enlistment tests which contain

[6] Matching of these data sources is done by SCB, and this project falls within the oversight of the Swedish Ethical Review Authority.

assessments of cognitive ability and noncognitive ability. The military data cover the period 1968 to 2011 and are obtained from the National Archives ("Riksarkivet") and the Swedish Defence Recruitment Agency ("Rekryteringsmyndigheten").[7] The combined dataset allows us to track individuals and their characteristics over time and to study the evolution of health outcomes around merger events.

The information on the identity of firms involved in a business combination (as targets or acquirers), as well as the deal date, is obtained from Serrano. In Sweden, any merger between two limited liability companies (including unlisted firms) must be reported to the Swedish Companies Agency (Bolagsverket), which then makes information on the merging entities and the merger completion date available to Serrano.

2.2 Mental health outcomes

A distinguishing strength of our data is the availability of extensive information on individual-level health outcomes for the entire Swedish population. In particular, for each individual we can observe any diagnosis and (surgical) procedure registered within the secondary health-care system in Sweden. Diagnoses and procedures given within the primary care system are not maintained by the National Board of Health and Welfare. However, we do observe all drugs prescribed to each individual in both the primary and secondary healthcare system.[8]

To classify diagnoses of mental illnesses, we follow the International Classification of Diseases (ICD, version 10) that are registered within the Swedish Patient registry. ICD is a diagnostic tool used globally in epidemiology, health management, and clinical purposes. The ICD is maintained by the WHO. We define a diagnosis of a mental illness as having a

[7] Between 1968 and 2009, all Swedish males aged 18 or over were required to participate in enlistment tests for one to two days. Since 2010, both participation in the tests and military service itself have no longer been compulsory. The cognitive ability test consisted of four parts: synonyms, inductions, spatial reasoning, and technical comprehension; the combined score from the four parts was converted to a cognitive ability score from one to nine on the Stanine scale. Noncognitive ability was assessed through a structured interview with a psychologist, who graded test-takers on psychological abilities (the score was also mapped into the Stanine scale). Individuals who have the following character traits obtain high noncognitive test scores: willingness to assume responsibility, independence, outgoing character, persistence, emotional stability, initiative, ability to work in groups.

[8] Vaccines, drugs administered in hospital settings, and over-the-counter medicines are not included in the register. However, prescriptions given in a hospital and taken out in a pharmacy are included in our dataset.

code in one's medical record that is included in the chapter V(F) of ICD-10 titled 'Mental and Behavioural Disorders.' For the subset of ICD codes that signify a depression diagnosis we use the list of ICD-10 diagnostic codes proposed by Fiest et al. (2014). For details, please refer to Appendix Table A1 which contains the list of ICD codes that we use, including their labels. Similarly, a diagnosis of anxiety includes ICD codes that start with F40 or F41.

The data within the Swedish Prescribed Drug Register is structured according to ATC codes, which represent a unique code assigned to a medicine according to the organ or system the drug affects and other properties of the drug. The classification system is maintained by the WHO. In our analysis, psychiatric drugs encompass ATC codes N05A (antipsychotic drugs), N05B (anxiolytic medication), N05C (hypnotics and sedatives), N06A (antidepressants), N06B (psychoanaleptics), and N03AE (benzodiazepine derivatives). For antidepressants we use all ATC codes within N06A.

We combine information from these two datasets (diagnoses and prescriptions) to define the indicator variable *Composite mental illness*. This variable takes the value of one if an individual has either been diagnosed with a mental illness or has been prescribed drugs to treat a mental illness. Because it is our most encompassing measure, it will be our main outcome variable. However, we also analyze its individual components separately. The variable *Mental illness diagnosis* is an indicator that takes the value of one if an individual is diagnosed with a mental illness. The variables *Depression diagnosis* and *Anxiety diagnosis* are indicators that take the value of one if an individual is diagnosed with depression or anxiety disorders, respectively. To study the intake of mental health medication, we use the variable *Mental illness medication*, a dummy that takes the value of one if there is an intake of any mental health medication. We also study antidepressants specifically with the variables *Antidepressants medication* and *Antidepressants dosage*, which capture, respectively, whether there is an intake of antidepressants and the daily dosage of antidepressants.

We construct two indicators for the existence of contacts an individual has with the medical system based on administrative data within the Patient Registry. The first indicator (*Inpatient*) is equal to one if the individual is an in-patient, and the second (*Outpatient*)

is equal to one if the individual is an out-patient. The in-patient registry includes visits to medical facilities like hospitals or other specialized institutions that require an overnight stay, while out-patient visits do not require an overnight stay. For example, visits to the emergency room are registered as out-patient visits. Finally, to capture the most severe manifestations of mental illness, we study the evolution of deaths and deaths by suicide around merger events with the variables *Death* and *Suicide*.

Panel A of Table 1 reports definitions of the dependent variables, while Panel B of Table 1 contains the definitions of other variables used in the analysis reported in the following sections. We multiply all binary outcome variables by 100 to be able to interpret coefficients as percentage points.

2.3 Sample selection

For a firm to be included in our sample, it must meet three criteria. First, it must complete a merger transaction (either as a target or an acquirer) between 2007 and 2015. Second, we only include firms that are incorporated and thus are limited liability firms. Third, we require a firm to have strictly more than four employees one year before the merger (i.e., at event-time $t = -1$). Furthermore, we impose two restrictions on the sample of employees. To be included in our sample, an individual must work at a firm involved in an M&A one year prior to that firm's merger (the individual may work elsewhere before and after that year, and remains in the sample throughout).[9] An exception to this is our analysis of deaths and suicides, where we require individuals to work at the firm three years before the merger to allow for deaths in the pre-merger period. After these restrictions, the sample underlying our analysis contains 562,741 unique individuals that we follow over a period of up to nine years; these individuals work at 150,941 firms of which 5,687 experience one or more mergers and jointly account for 3,480 unique mergers during our sample period.[10]

[9] In unreported robustness tests, we perform our analysis requiring employees to work two or three years prior to the merger at the firm. The results from those tests are consistent with the analysis reported in this draft.

[10] In the rare instances when an employee experiences more than one merger during our sample period, we only keep the first merger event that we observe in our panel for that employee.

Figure 1 reports the prevalence of mergers in the economy. About 1 percent of limited liability companies are involved in an M&A transaction every year, affecting around three to four percent of all employees. Figure 2 compares distributional features of limited liability companies in the Swedish economy to those in our sample. Panels (a) and (b) suggest that our sample covers around two-thirds of the M&A transactions in the economy during the sample period. Panels (c) and (d) show that the merging firms in our sample tend to be smaller than the population of limited liability firms at large: for example, in our sample, more than 40% of the employees work for firms with 20 workers or less, while the corresponding fraction in the economy is around 20%. Finally, panels (e) and (f) report the distribution of employees in target and acquirer firms in our sample. They show that target firms tend to be considerably smaller than acquirer firms: for example, more than half of the target firm employees in our sample work for firms with 20 workers or less, while less than 30% of the acquirer employees work in firms of that size.

2.4 Summary statistics

Table 2 reports the mean and standard deviation for background characteristics as well as outcome variables for the employees included in our sample. We present the summary statistics separately for the pre-merger period (event-time *t=[-5,-1]*) and the post merger period (event-time t = *[0,8]*).

A 'pre-to-post' comparison of the averages of the outcome variables already hints at a deterioration of the mental health status of workers following a merger. For example, our main measure of worker mental health, *Composite mental illness*, increases from 10% in the pre-merger period to 13% in the post-merger period. Furthermore, the share of employees who take psychiatric medication increases from 9% pre-merger to 12% in the post-merger period. We also observe that the incidence of depression increases, and that employees who take antidepressants increase their dosage from 17 to 26 daily doses. These patterns may of course be driven by a variety of factors other than M&As, such as secular time trends or worker age effects. The next section introduces our empirical strategy aimed at identifying

the effect of mergers on the mental health of workers.

3 Main Results

3.1 The impact of a merger on employees' mental health

An M&A event can fundamentally alter the relationship between a firm and its employees. The amalgamation of human and physical capital from acquirer and target firms that occurs during mergers often leads to staff reductions and to a reallocation of retained employees to different locations and tasks. Expectations about career progression within the merged firm are also likely affected by the merger-driven reorganization. The career pressures and uncertainty caused by such a fundamental transformation of one's employer may result in the build-up of anxiety and work-related stress. In more severe cases, depression and other mental illness may ensue, which may ultimately result in the intake of (potentially addictive) psychiatric medication, hospitalization, and even death.

To understand how employees' mental health is affected by an acquisition, we use a large set of mental health measures, ranging from diagnoses of stress and anxiety to the intake of psychiatric medication, hospitalizations, and death. Our main empirical approach is to make within-individual comparisons of mental health outcomes from before to after a merger for a set of workers who all experience an M&A event at some point during the sample period. Workers who have not yet been exposed to a merger event serve as the control group.[11] Equation (1) reports our baseline specification:

$$\textit{Mental health measure}_{i,t} = \beta \times Post_{i,t} + \omega_{FE} + \varepsilon_{i,t} \tag{1}$$

As the dependent variables we employ the mental health measures introduced in Section 2 and tabulated in Table 1. The unit of observation is a worker (i) in the years (t) around the merger event. $Post_{i,t}$ is an indicator variable taking the value of zero during the pre-merger

[11] Our findings are robust to alternative specifications, including using failed mergers as a control group. We discuss this and other robustness tests in Section 4.

period (event-time *t=[-5,-1]*) and a value of one during the post-merger period (event-time *t = [0,8]*). ω_{FE} contains a set of sector-by-year fixed effects, which non-parametrically account for factors such as the secular trend in mental health diagnosis and treatments[12] and industry-shocks (e.g., the possibility that the incidence of M&A transactions may be higher in industries that are in decline or that the redeployability of human capital after dismissals from takeovers may vary across industries). ω_{FE} also contains individual worker fixed effects, which account for the time-invariant propensity of workers to be diagnosed with and receive treatment for mental illness. Finally, ω_{FE} contains fixed effects for employee age, to account for the fact that mental health status tends to change over the life cycle (see Figure A1 in the Appendix). β, the main coefficient of interest, can be interpreted as the impact of takeovers on the mental health of employees. Because mental health outcomes may not be independent across workers of the same firm, standard errors are clustered at the level of the firm at which a worker is employed in the year of the announcement of the merger.

Table 3 presents the main results. In specification 1, which employs the summary measure of mental health *Composite mental illness*, we observe a statistically significant increase in the incidence of mental health problems after the merger. We also find statistically significant increases in the propensity to be diagnosed with a mental illness (column 2). While some mental health disorders, such as bipolar disorder or schizophrenia, are unlikely to be caused by corporate events, anxiety disorders, stress, and depression may plausibly increase following a merger. In columns (3) and (4), we focus on depression and anxiety, respectively, which may be particularly sensitive to corporate restructurings. Indeed, we observe a statistically significant 8% increase in depression diagnoses following the merger (the effect on anxiety diagnoses is small and imprecisely estimated). Finally, we observe statistically significant increases in the propensity to access ambulatory healthcare (column 5) or health services in a hospital (column 6). In terms of magnitudes, the likelihood of accessing outpatient (ambulatory) care increases by 2% after the takeover relative to the

[12]For trends in mental health diagnoses in Sweden see Figure A1 in the Appendix; for evidence elsewhere, see OECD (2018).

pre-period, while the likelihood of becoming a patient in a hospital increases by 5%.

Table 4 reports results on the use of psychiatric medication following a merger. We find that the probability of taking antidepressants increases by 2% after the takeover (there is no significant increase in the doses prescribed), and the probability of taking any type of psychiatric medication increases likewise by 2%. Most strikingly, as is evident in the results reported in Table 5, takeovers have a large effect on the probability of death and death by suicide, which increase by, respectively, 30% and 44% relative to the pre-merger period (the annual risk of dying before the M&A is around 0.14%).

A potential concern with these results is that they may be driven by differential trends, which originate prior to the merger date. While our sample contains only employees that will at some point be affected by a merger, and are therefore more likely to be comparable to each other, the timing of M&As is not exogenous. Thus, a worker's mental health conditions may evolve differently in the run-up to the takeover. In fact, a deterioration in mental health of a firm's workforce could be what causes an M&A to occur. To address this issue, in Figures 3 to 6, we analyze the dynamic evolution of the different mental health outcomes around merger events. We find no evidence of significant pre-trends in any of our mental health measures. While employee mental health appears to be generally stable and comparable across firms before a merger, it deteriorates after the event. From these figures it is also evident that most of the negative mental health effects of business combinations subside four to five years after the takeover and then revert to pre-takeover levels. An exception is the probability of seeking in-patient care, which remains elevated.

Taken as a whole, the results in this section imply that restructuring through mergers and acquisitions has a significantly detrimental impact on the mental health of workers. In particular, the amalgamation of human and physical capital through a merger, and the associated impact on the work-life of employees, leads to an increase in anxiety and stress, and in more severe cases, to the intake of psychiatric medication and hospitalization. Given that mental illness is an important determinant of individual happiness and labor productivity, these findings represent significant costs associated with mergers, for the individual

workers, their employers, and society at large.

3.2 How does the effect of mergers on mental health compare to other important life events?

To contextualize the economic significance of our estimates, we compare the effects of business combinations on mental health to several other marked events that the workers in our sample may experience, such as divorce, marriage, and protracted unemployment. These life events are endogenous in nature so we do not intend to attribute a causal interpretation to these empirical patterns. Instead, these events allow us to better gauge the economic magnitudes of the effect of mergers on mental health. Indeed, a set of interesting patterns emerge, which we document in Figure 7. To conserve space, we focus on the summary measure *Composite mental illness*.

First, the negative effect of a takeover on mental health is quantitatively similar to the effect of marriage, but with the opposite sign: while business combinations induce mental health problems, marriage alleviates them. Another important life event is the birth of the first-born child: as in the case of marriage, this is an event that is associated with a significant reduction in mental health problems. The size of the effect, in absolute terms, is around three times that of a takeover.[13] Another striking event is divorce: the evolution of mental health problems follows an inverse-U shaped pattern centered around the year of divorce, when mental health problems peak. In that 'event-year,' the increase in mental health problems, relative to the pre-event period, is an order of magnitude larger than in the case of mergers. Finally, in the case of unemployment, the changes in the likelihood of developing mental health problems peak in the year prior to becoming unemployed and then subside. In the first year after becoming unemployed, the likelihood of mental health problems decreases by an amount that is around two to five times larger (in absolute terms) than the increase in mental health problems experienced by workers in the first year after a takeover.

[13] As in the case of marriage, the period of bliss lasts about three years; starting at age four of the first-born child, the likelihood of parents experiencing mental health problems begins to rise.

In sum, the effect of business combinations on the mental health of employees is of similar magnitude as the effect of marriage (but with the opposite sign), and smaller than the effect of other noteworthy events in workers' lives.

4 Addressing empirical challenges

The evidence presented in the previous section suggests that acquisitions negatively affect the mental health of workers. However, mergers are not random events, so the results discussed so far may not reflect a causal relationship between acquisitions and worker well-being. This section presents tests aimed at addressing various endogeneity concerns. In these tests, we primarily focus on the summary measure of mental health (*Composite mental illness*) to keep the discussion concise.

4.1 Differences in the mental health of workers in the sample versus the general population

A natural concern that may arise is that the workers in our sample could be particularly prone to mental health problems. In fact, the poor mental health condition of a firm's workforce may be precisely what makes it a takeover target, as the acquirer may see an opportunity to restructure the labor force to achieve higher productivity. If that was the case, it would be poor mental health that would be driving the merger events and not vice versa. Before we conduct additional tests, we compare our sample of workers to the general population. A comparison of the background characteristics shows that the employees of firms who will experience a merger are in better, not worse, mental health condition than the general Swedish population. In our sample, the prevalence of a mental illness diagnosis is only 2% (see Table 2), compared to 4–6% in the general population (see Figure A1). Furthermore, depression diagnoses (in-sample mean 0.5%) and anxiety diagnoses (in-sample mean 0.5%) are also less common in our sample during the pre-merger period than in the general population.

4.2 Permutation tests

Next, we address the concern that the effects we report in Section 3 might be driven by random unobservable variation rather than the merger event per se. We follow the procedure proposed by Chetty, Looney and Kroft (2009) and construct 'placebo-merger' events as follows. Each employee in our sample is randomly reassigned to a placebo-merger in a random year during the sample period. Keeping the sample size constant, the baseline regression (1) is re-estimated. The *Post* dummy variable, which in this analysis is designated *Placebo post*, is adjusted to reflect the placebo pre- and post-merger period. The regressions employ the full set of fixed effects (age, individual, and industry-year) and use the summary mental health measure *Composite mental illness* as the dependent variable. The coefficient on the variable *Placebo post* is stored, and the process repeats by randomly assigning another placebo merger event year for each employee. This procedure is repeated 1,300 times.[14]

In Figure 8, we plot the distribution of the point estimates from the 1,300 regressions and mark the original result from Table 3, column 1, with a vertical dark-blue line. The figure shows the cumulative distribution function for the permutation analysis. If an acquisition had a significant positive effect on the incidence of mental health problems among workers, we would expect the estimated coefficient from the main sample (indicated by the dark-blue line) to be in the right tail of estimated effects when we replicate the analysis for placebo mergers. The chart shows that the coefficient from the original regression is above the 99th percentile of the distribution, suggesting that the effect on mental health is indeed most likely driven by the merger event itself. In other words, once we remove the possible 'treatment' effects of the actual merger by reshuffling observations across time, the effect essentially disappears.

[14]The permutation test employs 1,300 iterations because this is the point when the distribution of estimates converged to a stable distribution.

4.3 Placebo test: Mergers and physical illnesses

While mergers plausibly affect the mental well-being of workers, such events should have no effect on physical illnesses attributable to genetic disorders with adult onset, such as muscular dystrophy and multiple sclerosis.[15] Based on this argument, we develop a placebo test, where we study the evolution of employees' genetic disorders around merger events. We estimate our baseline regression model (equation (1)) but use the outcome variable *Physical illness*, an indicator that takes the value of one if an employee is diagnosed with a single-gene physical disorder, and zero otherwise.[16] During our sample period, 3,833 employees were diagnosed with one of these disorders. Table A2 in the Appendix reports the list of single-gene physical disorders that we use and documents the frequency of diagnosis for each such disorder within our sample period.

We report the results of this test in column 1 of Table 6. The estimated coefficient on the variable *Post* is both economically small and statistically insignificant, suggesting that mergers are not associated with an increase in the incidence of physical illnesses attributable to genetic disorders. This supports our identifying assumption and suggests that our main results are not driven by differential secular health trends of employees at the merging firms.

4.4 Distressed target companies

Distressed companies may be attractive acquisition targets: their purchase price will reflect the difficulties they face and the urgency with which they need to raise capital or sell assets. In our analysis, the concern thus arises that fundamental economic or financial factors might be simultaneously affecting the target firm, their employees, and the likelihood of an

[15] Note that this argument does not hold for all physical illnesses. First, there is evidence that physical illnesses related to workplace accidents are correlated with the ownership structure and financial constraints of the firm (see, e.g., Filer and Golbe (2003), Cohn and Wardlaw (2016), and Gilje and Wittry (2021)). Second, certain mental diseases—such as depression, mood disorders in general, and anxiety—can play a causal role in the development of some physical illnesses. This is due to the adverse effects of mental illnesses on habits such as smoking, diet, over-eating, sedentary lifestyle, and their maladaptive effects on adherence to medical regimens (see, e.g., Balon (2006) and Robson and Gray (2007)). For these reasons, we focus on the diagnosis of inherited single-gene physical disorders that emerge in adulthood. Arguably this group of physical illnesses is less likely to be directly affected by either workplace safety or a mental illness.

[16] We base our selection of single-gene physical disorders with adult onset on the list of most common single-gene disorders in adults listed in Table 1 in Gilchrist (2002).

acquisition taking place. That is, our results could reflect endogenous selection. To rule out that target firm distress drives the results reported in Section 3, we conduct several tests which we present in Table 6.

Acquiring firms provide the financing for the takeover; as such, their financial health is likely to be better than that of the target. Therefore, if financial distress were driving the results, we would expect to find no effect in the sub-sample of acquirers' employees. Yet, in the second column of Table 6, we find that workers in the acquiring firms, on average, experience a deterioration of their mental health by 2% relative to the period before the acquisition. This suggests that any negative, unobservable factors leading a firm to be an acquisition target are unlikely to be the sole drivers of the results discussed in Section 3.

To further address the concern that economic shocks contemporaneous to the merger are causing the mental health decline among workers, we re-estimate our baseline equation 1 using a sub-sample of target firms that are not experiencing economic or financial distress prior to the merger. In specification 3 of Table 6, we select target firms that satisfy the following criteria in the two years prior to the acquisition: the two-year average profitability (operating income divided by assets), employment growth, and sales growth are (i) positive and (ii) rank above the median among all target firms in the sample. Based on these selection criteria, these target firms are unlikely to be economically distressed. In specification 4, we focus on a sub-sample of target firms in the lowest quartile of the leverage distribution.[17] Due to their low leverage, these target firms are unlikely to be financially distressed. Similar to the results from the main sample (reported in Table 3), we find that workers in target firms which are unlikely to be financially or economically distressed experience a worsening of their mental health following the acquisition. In sum, the results reported in columns 2 - 4 of Table 6 suggest that negative confounding shocks—contemporaneous with the acquisition—and endogenous selection are unlikely to be the sole drivers of the worsening mental health of workers in the aftermath of acquisitions.

[17] Results are similar when we focus on the lowest leverage decile.

4.5 Quasi-experimental evidence from failed mergers

Our most apt empirical strategy to address endogenous selection and other threats to identification is to consider a sample of successful mergers in comparison to mergers that fail. This analysis permits us to examine the mental health of workers in firms that attempt to but fail to merge, as a counterfactual for how the mental health of workers in successful mergers would have evolved absent the business combination.

Failed mergers are those that have a merger initiation date (according to the Serrano database), but where the merging firms both continue to exist as separate entities thereafter (we exclude cases where two firms initiate a merger but then either of these firms closes down within one year of the attempted merger date). Using this procedure, we identify 21 failed mergers (42 associated firms) during our sample period. As a caveat it should be noted that we are unable to verify the reasons for the failure of the merger because the data are anonymized. While we consider it implausible that the mergers failed because of the mental health status of the workers, this is something we cannot rule out. The failed merger events that satisfy our sample selection criteria (namely, limited liability companies with at least five employees in the year prior to the announcement) span the period 2009 to 2015.[18]

The fact that the firms involved in failed mergers intended to merge suggests that the underlying economic factors motivating the decision to merge were likely similar to those of the firms in our main sample. Indeed, Table 7 reports characteristics of employees that work for firms that successfully merged and those whose merger failed.[19] Even though the two groups of employees look slightly different in terms of health before the (planned) merger date, what is crucial is that their mental health moves in parallel over time to function as a counterfactual. Figure 9 plots the annual event-time averages of various mental health variables for the workers of firms that successfully consummated the merger

[18]Due to the shorter number of years in which we observe failed mergers, in this part of the analysis we restrict the event-window from three years before to four years after the attempted merger.

[19]Note that the event windows in Table 2 and Table 7 differ due to the limited number of years in which failed mergers occur.

and for workers of firms in which the merger failed. We observe that prior to the (planned) business combination—indicated in the graphs as event time t=0—the workers' mental health evolves similarly in both groups of firms. However, while the mental health of workers in firms that merge deteriorates after the deal, the mental health of workers in failed merger firms remains stable.

To show that in the absence of the merger workers do not experience a change in their mental health status, we first conduct a placebo test where the baseline regression (1) is re-estimated in the sample of employees from the 42 firms involved in the failed mergers. In these regressions, *Post* is an indicator variable that takes the value of one in the years after an attempted merger, and zero in the years before. We report the results in Table 8. We employ the summary mental health measure *Composite mental illness* in three specifications: column 1 reports coefficients from a regression using the pooled sample of failed merger targets and acquirers, while column 2 focuses on the sample of failed acquirers and column 3 studies employees in the sample of failed targets only. In all three specifications, we find that the coefficient on the variable *Post* is not statistically different from zero, indicating no significant change in the mental health status of workers of firms that attempt to but fail to merge. This finding further supports the causal interpretation of the tests reported in Section 3: it appears that the takeover itself, not other confounding factors, are indeed causing the adverse mental health outcomes experienced by workers.

Next, we employ standard difference-in-differences regressions where the mental health of workers from successfully merged firms (the 'treatment' group) is compared with the mental health of workers from firms that initiated a merger but end up not merging (the 'control' group), before versus after the (attempted) merger. In these regressions, *Post* is an indicator variable that takes the value of one in the years after the attempted or successful mergers, and zero in the years before; *Successful merger* is a dummy variable that takes the value of one for target and acquirer firms that successfully merge, while it is zero for firms that initiate but do not complete a merger. Tables 9 - 11 present the results, which are broadly comparable to those reported in Section 3. In Table 9, the summary mental

health indicator *Composite mental illness* is statistically significant at the 5 percent level. Quantitatively, the estimated coefficient on this variable represents a 7 percent increase in the incidence of mental health problems after the merger. This effect is economically larger than that in our baseline regressions, where we estimated a 2 percent increase in mental health problems (see Table 3). Decomposing the increase in mental health problems into its sub-components, it is clear from the coefficients in Tables 9 and 10 that the significant increase in the summary mental health variable is mainly driven by an increase in psychiatric medications rather than by an increase in mental illness diagnosis. For example, relative to the employees at the firms that initiate but fail to conclude a merger, employees who work at a firm that successfully completes a merger increase their daily doses of antidepressants by 3.8 percentage points, which corresponds to an increase of 21 percent relative to the pre-merger period. Overall, the conclusion remains that acquisitions result in a worsening of the mental health of workers.

5 The pathology of mergers: which workers are most affected?

What are the specific mechanisms through which mergers impact the mental health of workers? To answer this question, we investigate the extent to which the effect varies across different types of workers and firms. We examine the treatment effect heterogeneity by estimating regression models of the following type:

$$\textit{Mental Health}_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t} \tag{2}$$

As in the previous sections, the unit of observation continues to be a worker (i) in the years (t) around the merger event. As the dependent variable, we focus on the summary measure *Composite mental illness* in our analysis, though we report coefficients from regressions using additional measures of mental health (which were discussed in Section 3)

in the Appendix. *Characteristic* denotes the worker or firm attribute used to analyze the heterogeneity in treatment effects. As in previous sections, the matrix ω_{FE} contains sector-year fixed effects, individual worker fixed effects, and fixed effects for employee age. β_1, the main coefficient of interest in these tests, measures the treatment effect heterogeneity along various dimensions discussed below.

5.1 Internal reorganization and mental health outcomes for non-departing workers

Because acquisitions tend to be associated with job displacement, our results could, in principle, be entirely driven by job loss. However, acquisitions often encompass restructuring measures which are not limited to employment cuts. For example, workers in the post-merger firm may be assigned different tasks, may have to change work location, may experience a change of leadership or team composition, or may be promoted or demoted. Such 'on-the-job' changes may lead to stress and other adverse mental health effects. To study whether non-departing workers of the merging firms also experience negative mental health effects, we focus on a sub-sample of 'stayers', that is, workers who remain with the merged firm during the year of the merger and the year thereafter. While we continue to record the mental health status of the workers thereafter, to be categorized as a stayer we do not require a worker to be at either of the merging firms starting in year two after the acquisition.

In column 1 of Table 12, we report the coefficients from a regression examining the effects of the acquisition on this sample of workers that are not immediately affected by job loss. The coefficient on the variable *Post* is positive and statistically significant (at the 1% level), suggesting an increase in the incidence of mental health problems among this group of workers. The magnitude of the effect (an increase relative to the pre-merger period of 3 percent) is comparable to the effect observed in the main sample (as reported in Table 3).

The other specifications reported in Table 12 shed further light on the mental health effects of restructuring during mergers using the sample of stayers. In column 2, *Post* is

interacted with *Lower wage*, a variable that indicates that a worker receives a lower wage after the merger than before. A wage decrease may be driven by a demotion, a wage cut, or a reduction in working hours. We find that relative to workers whose wage increases after the merger, the mental health among workers who experience a wage cut significantly deteriorates. Indeed, among such workers, the incidence of mental health problems, as measured using the variable *Composite mental illness*, increases by 19 percent relative to the pre-merger period. Wage cuts may of course reflect poor performance and/or absenteeism, which, in turn, may be a reflection of mental health problems that may not be merger-related. To alleviate this concern, in column 3 of Table 12, we focus on stayers who do not register any sick leave during the year of the merger and the year thereafter. We continue to find that the mental health among the group of stayers who experience a wage cut deteriorates more than among stayers who do not experience wage cuts. However, relative to the effects reported in column 2, the economic magnitude of the effects in column 3 is smaller.[20]

Next, we focus on specific characteristics of the merging firms to further test how the depth of internal reorganizations determines post-merger mental disorders among the employees. We report these tests in Table 13. First, we compare workers in the takeover target to those in the acquirer firm. It is plausible that a takeover target may be subject to more intense restructuring and reorganization activities than the acquiring firm, which may lead to more pronounced effects experienced by the target firm's workers. Consistent with this conjecture, in the test reported in Table 13 under the heading 'Target versus acquirer', we find that workers at the target firm are more likely to develop mental health problems than those in the acquiring firm—the negative effect on mental health among target firm workers is about twice as large as that among acquirer workers. Smaller takeover targets may be more flexibly integrated into a bigger entity with large financial resources. At the same time, employees at smaller companies may have to change their organization more

[20]While focusing on workers who do not go on sick leave helps alleviate the concern that the wage cut itself is driven by mental health problems, selecting on workers who are potentially more resilient introduces the possibility that we underestimate the effect of wage cuts.

drastically following a merger. Workers of small target firms may thus experience more uncertainty and loss of control, and may hence be subject to more severe mental health traumata. We measure relative size as the ratio of number of employees at the target over that at the acquirer, as of the year prior to the business combination. We find no evidence in our sample that the relative size of the merging firms is a significant determinant of the effect of the M&A on the employees' mental health.

Finally, we investigate whether horizontal mergers are associated with a more pronounced increase in mental illness than diversifying (vertical or conglomerate) mergers. In the case of horizontal mergers, the combination of similar activities may be associated with more corporate cultural clashes. Such mergers may also plausibly create larger opportunities for employee cost savings than vertical or conglomerate mergers, where operations are more naturally segmented, existing collaboration structures are more likely to remain intact, and old working habits less likely to be altered by the merger. In the tests reported in Table 13 under the heading 'Merger type', we define a horizontal merger as one where the merging companies share the same industry code (Swedish Standard Industrial Classification code, SNI code for short). For example, the indicator variable *Horizontal 5* denotes mergers of two firms that are within the same 5-digit SNI code (the most narrow industry definition within the Swedish industry classification). However broadly or finely we define horizontal mergers, we find that horizontal mergers are associated with more adverse mental health outcomes for employees than other mergers.

Taken together, the results reported in this subsection show that takeover-related restructuring, and not only job loss, is associated with the worsening of workers' mental health.

5.2 How do skills affect the onset of mental illness after mergers?

Next, we study whether the impact of takeovers on mental health depends on a worker's innate abilities (e.g., IQ or psychological traits) and skills acquired on-the-job. For example, some workers may have made significant investments in firm-specific human capital to

master their job. A merger may render some (if not all) of that human capital obsolete. Furthermore, individuals with fewer marketable skills, be it lower IQ or lower educational attainment, may have less control over their tasks on-the-job, which has been shown to be a determinant of depression risk (Karasek Jr (1979)).

We first evaluate the possibility that workers who are better endowed with cognitive (such as IQ) and noncognitive abilities (such as empathy, perseverance, and intrinsic motivation) may be less exposed to the risk of depression following a merger. Highly skilled workers may easily adapt to and even thrive in a changing environment, while workers with a more modest skill endowment may be more exposed to the risk of becoming redundant or seeing their status within the firm diminish (Ma, Ouimet and Simintzi (2016)). Are more skilled workers less prone to suffer from merger-induced mental illness? Table 14 reports the regression results using the dependent variable *Composite mental illness* under the heading 'Skill level'. Relative to high ability workers, we find that workers with below-median cognitive (noncognitive) skills are 0.5 (0.9) percentage points more likely to endure a deterioration of their mental health. Figure A4 in the Appendix reports the coefficients from similar regressions with a variety of additional mental health outcomes. Overall, we find that workers with lower innate abilities suffer a larger deterioration in their mental health following the merger, in particular in the case of workers with low noncognitive skills.

While the previous tests focus on measures of innate ability, we also study the role of skills acquired through education or on the job. First, according to the regression employing the summary measure *Composite mental illness* reported in Table 14, we find that workers with lower educational attainment (less than a college degree) experience about 0.6 percentage points worse mental health effects than more educated employees following mergers. Next, we make use of information on the occupation of workers within firms and categorize each worker into 'blue-collar' or 'white-collar.' Clerks, service workers, plant and machinery operators, and other elementary occupations are classified as blue-collar workers. The remaining occupations are categorized as white-collar (see Tag (2013)). We find that blue-collar workers experience a more pronounced deterioration of their mental health

status than white-collar workers. Quantitatively, the effect is similar to the comparison between high and low education workers. Figure A5 in the Appendix presents the coefficients from various other mental health measures and corroborates the evidence from the summary measure: workers with low educational attainment and low skills tend to suffer worse mental health outcomes.

Workers also acquire skills on the job. Using tenure as a proxy for firm-specific human capital and using *Composite mental illness* as the mental health measure, in Table 14, we do not observe a statistically significant difference in health outcomes between high and low tenure workers. This finding is corroborated in specifications using other mental health measures as dependent variables (see Figure A5 in the Appendix): the coefficients are imprecisely estimated for most mental health measures employed. This may reflect the ambiguity of using tenure as a proxy for firm-specific human capital.[21]

In sum, our estimates support the conjecture that skilled workers are less prone to suffer from merger-induced mental illness. We find that low-skill workers are significantly more likely than high-skill workers to be diagnosed with depression, to use antidepressants, and to die following a merger.

5.3 Personal sensitivity to work-related changes

A variety of other personal characteristics could also determine the extent to which the mental health of employees is affected by an M&A event. Table 14 reports the regression results using the dependent variable *Composite mental illness* under the heading 'Demographics'. The first set of estimates details the marginal impact of being a female employee rather than a male employee subject to a merger. It is well-documented that faced with events affecting their mental health, women are more prone to seeking help and medication, while men tend to take such actions less promptly at the cost of a higher likelihood of suicide

[21] Long tenure in the firm may indicate the existence of firm-specific human capital which has little value outside of the current employment relationship. However, workers with long tenure may also be 'legacy' workers who are unmotivated. Another interpretation is that short tenure workers are more likely to be fired due to Sweden's last-in-first-out dismissal laws, which may explain why we observe some tentative evidence that workers with short tenure are more negatively affected by the merger.

or death when the pressure on them becomes overwhelming (see, e.g., Mackenzie, Gekoski and Knox (2006)). Our data confirm this: in Table 14 we observe that the likelihood of women suffering from mental health problems after the merger is 1.3 percentage points higher than for men. Furthermore, according to the additional regression results reported in the Appendix (Figure A6), across all medication and mental health outcomes observed by third-parties, women seem to be more affected by mergers than men. This is particularly dramatic regarding the usage of psychiatric medication and the diagnosis of mental health disorders, which are at least four times more likely to happen due to a merger when the employee is female relative to their male counterparts. On the other hand, women are considerably less likely to die or commit suicide following a merger than men are.

The second set of estimates focuses on the origin of employees. On the one hand, the mental health of employees born abroad may be more dependent on what is going on at the workplace because of their more recent integration in the local community and lack of extended family nearby. On the other hand, it may be more difficult for them to access the Swedish health system in case of a depression, or it may not be in line with their culture of origin to use certain types of medication. Consistent with this ambiguity, we obtain estimates that are small and only marginally statistically significant, suggesting that native-born and foreign-born workers experience broadly similar mental health effects following mergers.

Worker age may also influence the mental health consequences of mergers. On the one hand, older workers may be more prone to experiencing mental health problems and may be more likely to have difficulty adapting to a changing environment with a new corporate culture. On the other hand, last-in-first-out labor laws protect workers with longer tenure at the firm (which will also tend to be older) from being fired. Using the variable *Composite mental illness* as an encompassing measure of mental health status we do not find significant differences in the effects of mergers on the mental health of workers of different ages (see Table 14). Figure A7 in the Appendix reports a variety of mental health outcomes and corroborates the inference based on our summary mental health measure.

6 Conclusion

Mental health is a key determinant of individual well-being and economic development. Recently, the WHO estimates that depression and anxiety disorders cost the global economy $1tn each year in lost productivity (Bloom et al. (2011), EU (2014)).

In this paper, we ask whether acquisitions affect the mental health of employees. We find that, on average, workers exposed to a merger experience a significant increase in the likelihood of suffering from mental health problems. In absolute terms, the effects are of similar magnitude to the (positive) mental health impact of marriage. While the effects are felt by workers in target as well as acquiring firms and leavers as well as stayers, the negative effects of mergers are most pronounced for women, 'blue-collar' workers, employees with lower levels of cognitive and noncognitive skills, and employees who see their employment status change in the aftermath of the merger.

Our evidence suggests that labor market outcomes (employment and wages), which are the typical measures of worker welfare used in studies in economics and finance, may not capture the full impact of corporate actions on employees' well-being. The anxiety and stress that workers face during corporate restructuring processes, such as in the case of a merger, may lead to detrimental effects on workers' mental health.

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Figure 1: **Prevalence of mergers**

This figure shows the prevalence of mergers and acquisitions in Sweden during the period 2007-2015. The dark blue columns show the share of Swedish limited liability companies with at least four employees that are involved in a merger during a given year. The light blue columns show the number of employees of limited liability companies (with four or more employees) involved in a merger relative to all employees in limited liability companies (with four or more employees). We measure employees in the year prior to the merger.

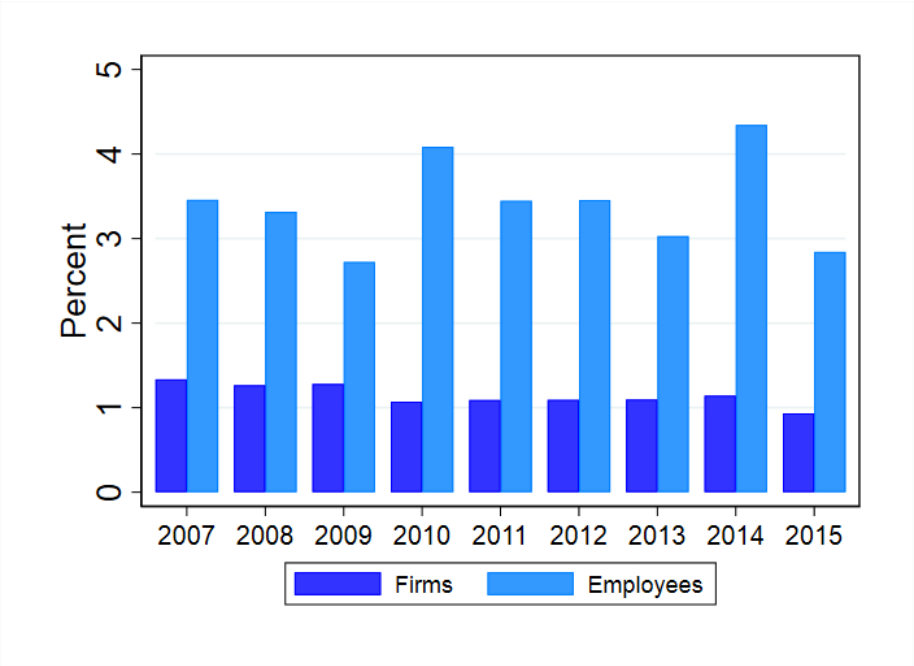
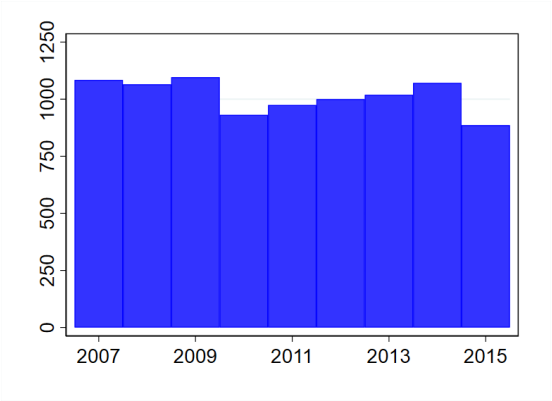


Figure 2: **Sample mergers versus all Swedish mergers**

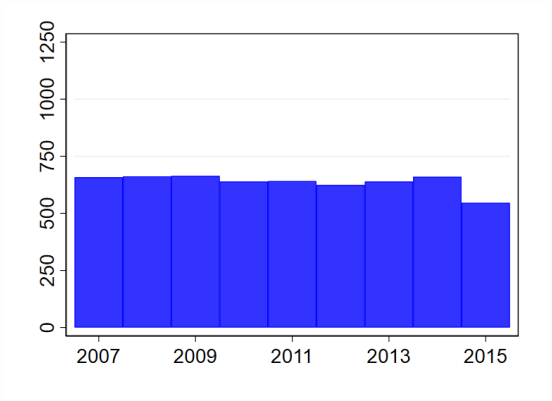
This figure compares the mergers included in our sample to all merger events in Sweden during the sample period. Panel (a) reports the number of mergers among limited liability firms in Sweden. Panel (b) shows the subset of such mergers where acquirer and/or target firm has more than four employees in the year prior to the merger (a pre-condition to enter our sample). Panel (c) shows a histogram of the number of employees in Swedish limited liability firms, while Panel (d) reports reports the employee distribution in the sample firms. Panels (e) and (f) report the employee distribution in the target and acquirer firms of the sample, respectively (employees are recorded in the year prior to the merger announcement). The sample period is 2007 to 2015.

Merger frequency

(a) All limited liability firms

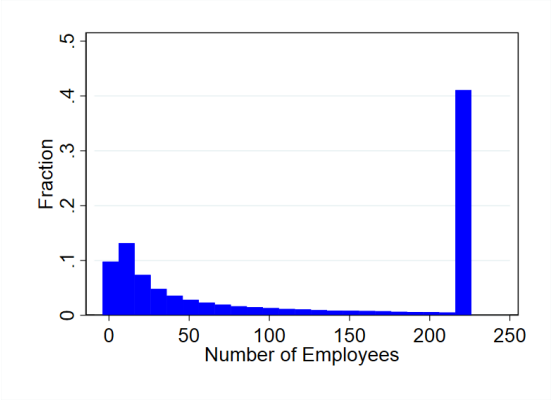


(b) Limited liability firms in the sample

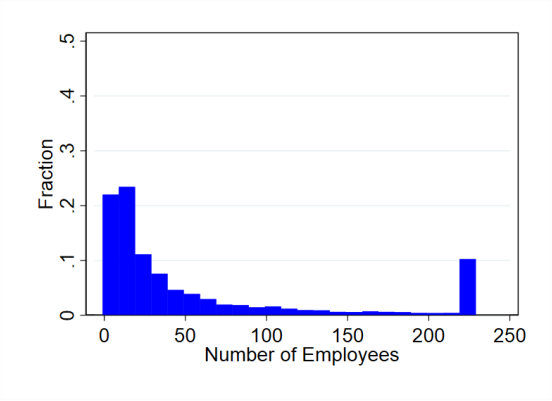


Employee distributions

(c) Employees at limited liability firms

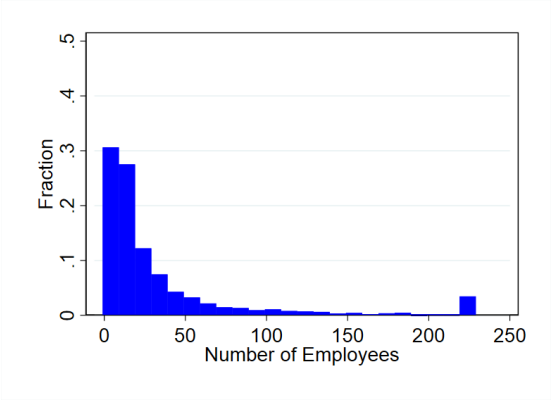


(d) Employees in sample firms



Employee distributions: target vs. acquirer

(e) Employees in target firms



(f) Employees in acquirer firms

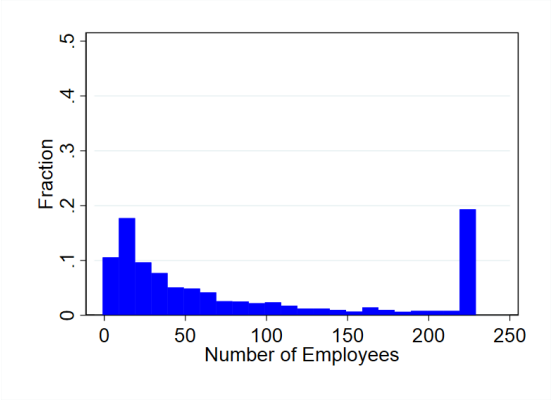
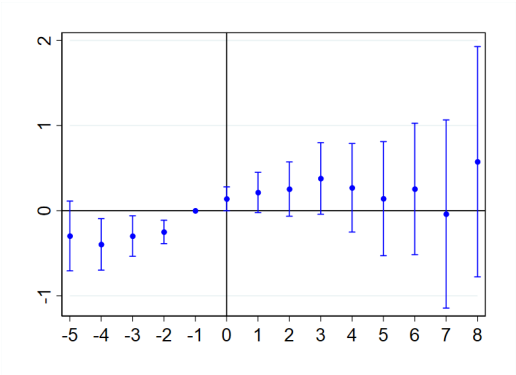


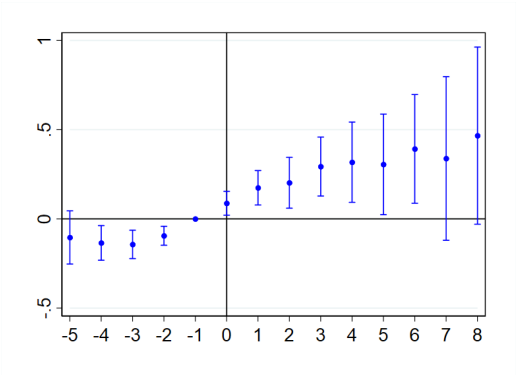
Figure 3: **Event time graphs: Mental health**

This figure shows the evolution of several individual mental health outcomes around merger events. It plots the coefficients β_j (including 95% confidence intervals) estimated using the following OLS regression model: $y_{it} = \sum_{j=-5}^{8} \beta_j \times 1(Event\ time = j) + \omega_{FE} + \varepsilon_{it}$, where ω_{FE} indicates age and sector-year fixed effects. The dependent variable y corresponds to different measures of mental health. Standard errors are clustered at the merger firm level. $t = -1$ is the reference event time.

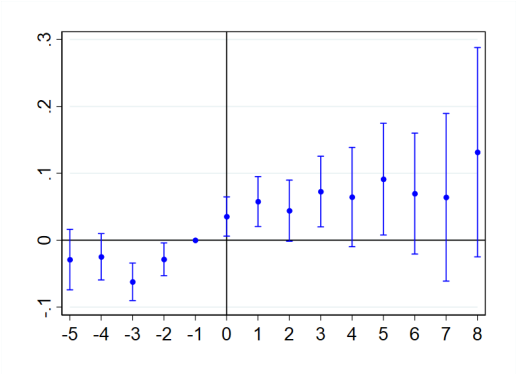
(a) Composite mental illness



(b) Mental illness diagnosis



(c) Depression diagnosis



(d) Anxiety diagnosis

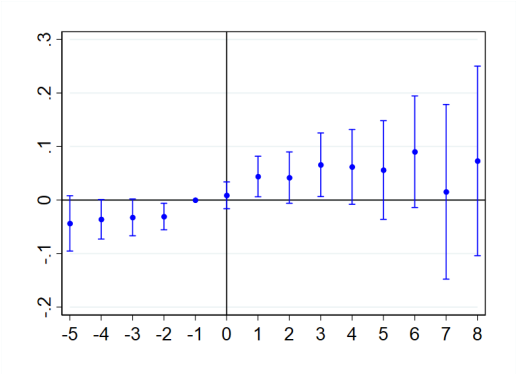
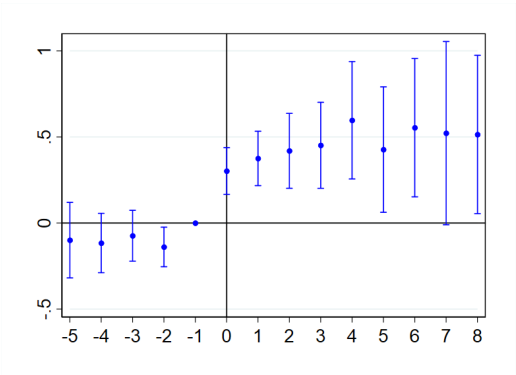


Figure 4: **Event time graphs: Contacts with healthcare providers**

This figure shows the evolution of employees' contacts with healthcare providers around merger events. The figures show the coefficients β_j (including 95% confidence intervals) estimated using the following OLS regression model: $y_{it} = \sum_{j=-5}^{8} \beta_j \times 1(Event\ time = j) + \omega_{FE} + \varepsilon_{it}$, where ω_{FE} indicates age and sector-year fixed effects. The dependent variable y is *Inpatient* in Panel (a) and *Outpatient* in Panel (b). Standard errors are clustered at the merger firm level. $t = -1$ is the reference event time.

(a) Inpatient



(b) Outpatient

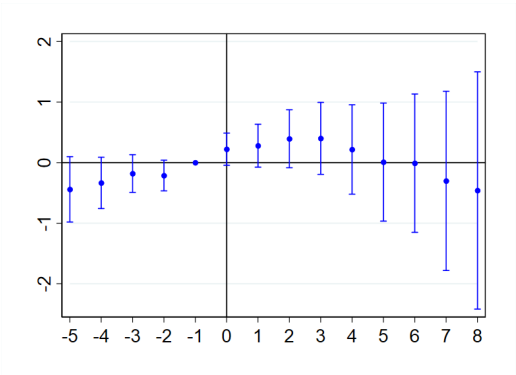
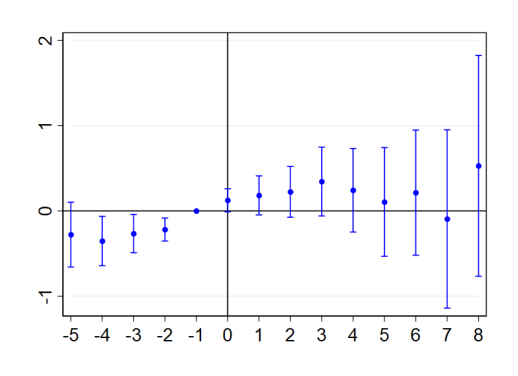


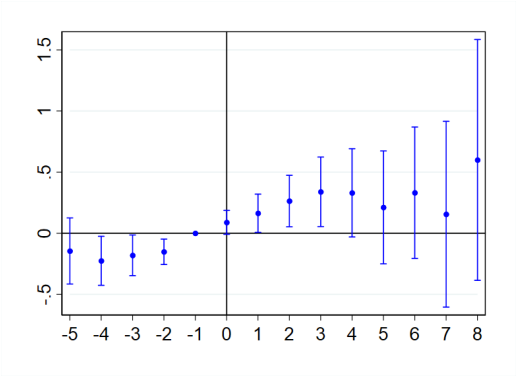
Figure 5: **Event time graphs: Psychiatric medication**

This figure shows the evolution of employees' intake of psychiatric medication around merger events. The figures show the coefficients β_j (including 95% confidence intervals) estimated using the following OLS regression model: $y_{it} = \sum_{j=-5}^{8} \beta_j \times 1(Event\ time = j) + \omega_{FE} + \varepsilon_{it}$, where ω_{FE} indicates age and sector-year fixed effects. The dependent variable y is *Mental illness medication* in Panel (a), *Antidepressants medication* in Panel (b), and *Antidepressant dosage* in Panel (c). Standard errors are clustered at the merger firm level. $t = -1$ is the reference event time.

(a) Mental illness medication



(b) Antidepressants medication



(c) Antidepressant dosage

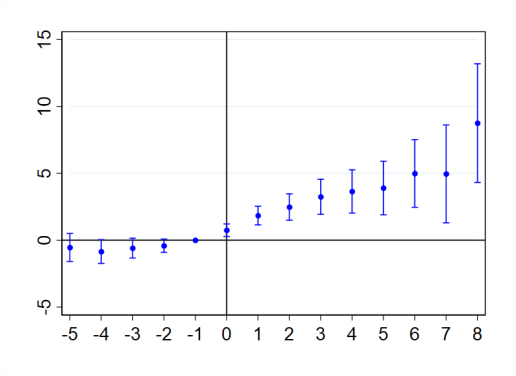
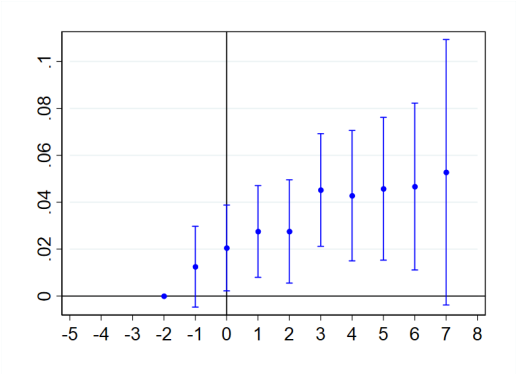


Figure 6: **Event time graphs: Death and suicide**

This figure shows the evolution of employees' death and suicide around merger events. The figures show the coefficients β_j (including 95% confidence intervals) estimated using the following OLS regression model: $y_{it} = \sum_{j=-2}^{8} \beta_j \times 1(Event\ time = j) + \omega_{FE} + \varepsilon_{it}$, where ω_{FE} indicates age and sector-year fixed effects. Standard errors are clustered at the merger firm level. Individuals included in the sample for these regressions are required to be alive and employed by the merger firm at $t = -3$. $t = -2$ is the reference event time in the graph.

(a) Death



(b) Suicide

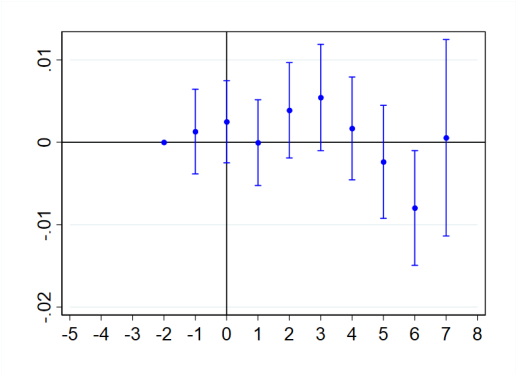
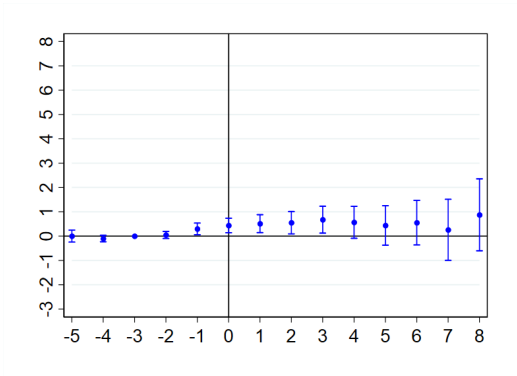


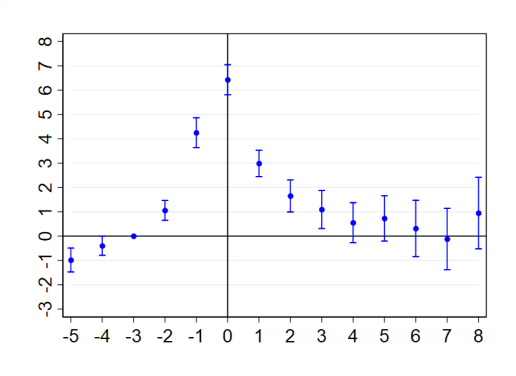
Figure 7: **Merger event compared to other events**

These figures examine the mental health implications of mergers and other events for employees that work at a merger firm at $t = -1$. We consider several events: mergers in Panel (a); divorce in Panel (b); marriage in Panel (c); birth of the first-born child in Panel (d); when an individual starts collecting unemployment benefits in Panel (e); and when a worker leaves their employer but does not have another employer in the transition year and the next in Panel (f). We plot the coefficients β_j and 95% confidence intervals based on the following regression model: $\textit{Composite mental illness}_{it} = \sum_{j=-5}^{8} \beta_j \times 1(\textit{Event time} = j) + \omega_{FE} + \varepsilon_t$. ω_{FE} contains age and sector-year fixed effects. Standard errors are clustered at the level of a worker's employer in a given year.

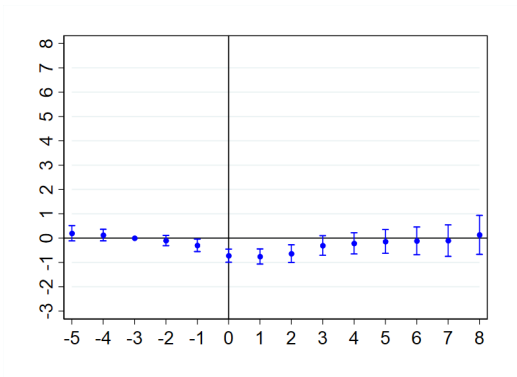
(a) M&A



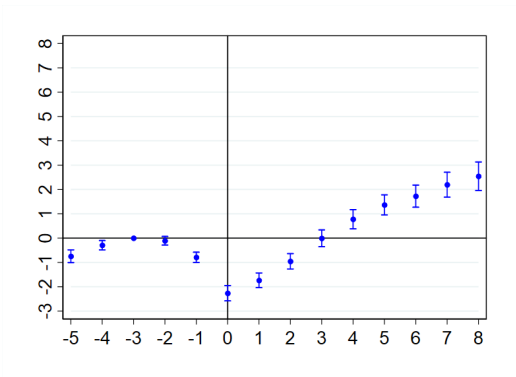
(b) Divorce



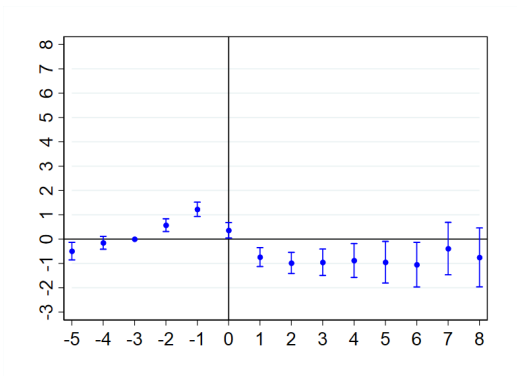
(c) Marriage



(d) First-born



(e) Unemployment benefits



(f) No employer

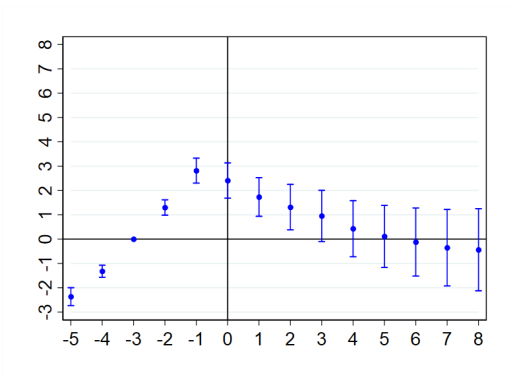


Figure 8: **Permutation test**

This figure shows the results of permutation tests following the procedure proposed by Chetty, Looney and Kroft (2009). We repeat the following process 1,300 times: (i) Randomly assign new hypothetical event times to all individuals in the sample; (ii) rerun the main regression model (equation (1)): $Composite\ mental\ illness_{i,t} = \beta \times Placebo\ Post_{i,t} + \omega_{FE} + \varepsilon_{i,t}$, where $Placebo\ Post_{i,t}$ is an indicator variable taking the value of zero during the pre-(hypothetical) merger period and a value of one during the post-(hypothetical) merger period. ω_{FE} is a vector of age and sector-by-year fixed effects. We plot the distribution of the 1,300 estimated regression coefficients β together with the regression coefficient for the actual merger events (marked by the vertical dark blue line).

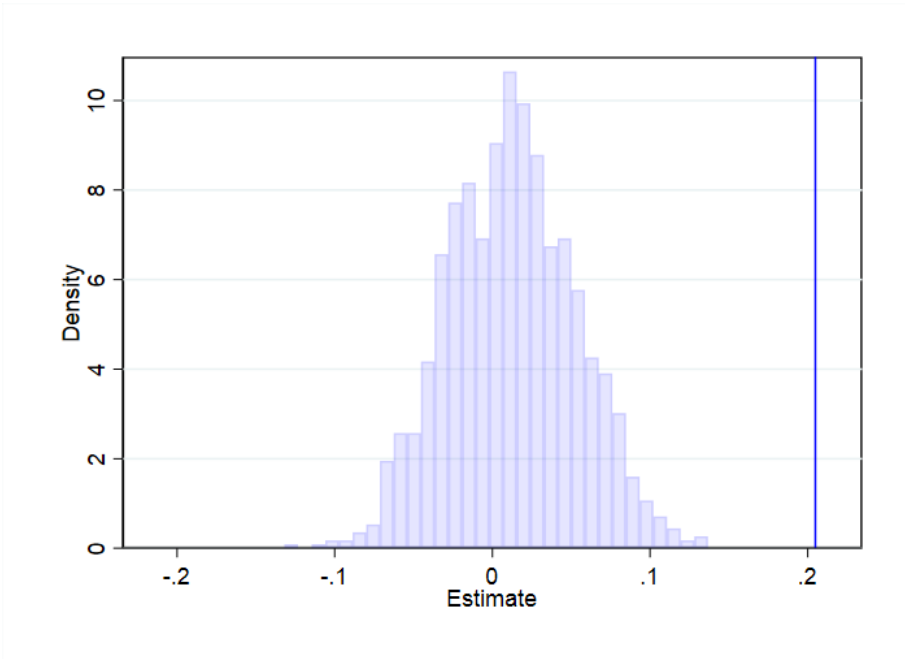
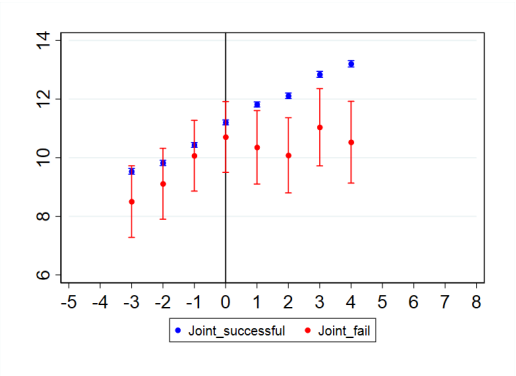


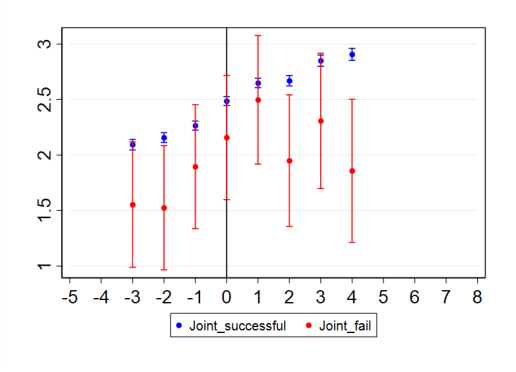
Figure 9: **Pre-trend graphs: Successful vs. failed mergers**

This figure shows the evolution of mental health outcomes for employees that work at firms that experience a successful merger (in blue) and for those that work at firms where the merger fails (in red). Panel (a) plots the evolution of *Composite mental illness* around (attempted) merger events, while Panel (b) plots the evolution of *Mental illness diagnosis*, and Panel (c) plots the evolution of *Mental illness medication*.

(a) Composite mental illness



(b) Mental illness diagnosis



(c) Mental illness medication

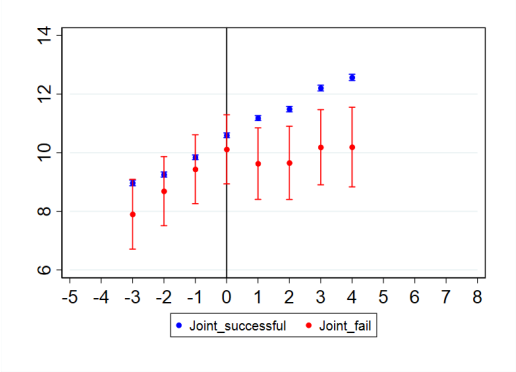


Table 1: **Definitions of variables**

This table reports definitions of variables used in the article. Panel (a) reports the definitions of dependent variables (alphabetic order). Panel (b) reports definitions of variables used in cross-sectional tests (alphabetic order). All indicator variables in Panel (a) are multiplied by 100.

Panel (a): Dependent variables

Variable	Definition	Registry
Antidepressants dosage	Number of predefined daily doses of antidepressant medication.	NPD
Antidepressants medication	A dummy variable that takes the value of one if there is an intake of antidepressant medication (that is, if ATC = N06A).	NPD
Anxiety diagnosis	A dummy variable that takes the value of one if an individual is diagnosed with anxiety (i.e., if the diagnosis code starts with F40 or F41).	NPR
Composite mental illness	A dummy variable that takes the value of one if an individual has either been diagnosed with a mental illness (variable *Mental illness diagnosis* $\neq 0$) or has been prescribed drugs to treat a mental illness (*Mental illness medication* $\neq 0$).	NPR/NPD
Death	A dummy variable that takes the value of one if the individual is registered in the National Death Registry.	NDR
Depression diagnosis	A dummy variable that takes the value of one if an individual is diagnosed with depression (that is, the diagnosis belongs to a code listed in Appendix Table A1).	NPR
Inpatient	A dummy variable that takes the value of one if the individual has a diagnosis or surgical procedure registered within the inpatient registry, zero otherwise.	NPR
Mental illness diagnosis	A dummy variable that takes the value of one if an individual is diagnosed with a mental illness (i.e., a diagnosis from the ICD10, F-chapter, is registered).	NPR
Mental illness medication	A dummy variable that takes the value of one if there is an intake of any mental health medication (that is, if ATC = N05A, or N05B, or N05C, or N06A, or N06B, or N03AE).	NPD
Outpatient	A dummy variable that takes the value of one if the individual has a diagnosis or surgical procedure registered within the outpatient registry, zero otherwise.	NPR
Physical illness	A dummy variable that takes the value of one if an individual is diagnosed with a single-gene disorder (that is, the diagnosis belongs to a code listed in Appendix Table A2).	NDR
Suicide	A dummy variable that takes the value of one if the cause of death code in the National Death Registry is between X60-X84 (indicating suicide).	NDR

Table 1 (continued)

Panel (b): Variables used in cross-sectional tests

Variable	Definition	Registry
Above median tenure	A dummy variable that takes the value of one if an individual has above median tenure at the firm at $t = -1$.	LISA
Age	Current year minus birth year plus one.	LISA
Age x-y	A dummy variable that takes the value of one if the individual is (or, if dying in pre-period, would be) of age between x and y at the time of merger.	LISA
Below median IQ (♂)	A dummy variable that takes the value of one if the cognitive skill score is strictly less than median cognitive skill score across cohorts. Available for males only.	Riksarkivet, Rekryteringsmyndigheten
Below median noncog. (♂)	A dummy variable that takes the value of one if the noncognitive skill score is strictly less than the median noncognitive skill score across cohorts. Available for males only.	Riksarkivet, Rekryteringsmyndigheten
Blue-collar	A dummy variable that takes the value of one if the occupation code of the individual (variable Ssyk3 in the individual registry) is within the 400-999 range at $t = -1$.	LISA
Female	A dummy variable that takes the value of one if female, zero if male.	LISA
Foreign	A dummy variable that takes the value of one if either the individual is born outside of Sweden or both of the individual's parents are born outside of Sweden.	LISA
Horizontal n	A dummy variable that takes the value of one if both target and acquirer have the same n first digits in their SNI industry codes (variable bransch_sni1 in Serrano) at $t = -1$.	Serrano
Leaver	A dummy variabke that takes the value of one if an individual does not work at the same firm from $t = -1$ to $t = 1$.	LISA
Leaver to no employer	A dummy variable that takes the value of one if an individual is a 'leaver' (as defined above) and has no employer at $t = 0$ and/or $t = 1$.	
Lower wage	A dummy variable that takes the value of one if the wage at event times $t = 0, 1$ is lower than the wage at $t = -1$; it is zero otherwise.	
No higher educ.	A dummy variable that takes the value of one if the individual has not attended higher education post high school.	
Stayer	A dummy variable that takes the value of one if an individual works at the same firm from $t = -1$ to $t = 1$ (with a possible switch between the target or acquirer firm at $t = 0$).	LISA
Successful merger	A dummy variable that takes the value of one if a merger is successfully consummated, zero if the merger fails. Failed mergers are those where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement.	Serrano
Target	A dummy variable that takes the value of one if the individual works at the target firm at $t = -1$.	Serrano

Table 2: **Summary statistics: main sample**

In this table, we report the mean and standard deviation of the main variables of interest. We report both the pre-merger period statistics ($-5 \leq t < 0$), as well as statistics for the post-merger period ($0 \leq t \leq 8$). For the variables *Death* and *Suicide*, the pre-period means and standard deviations are calculated for the interval $t = -2, -1$. For binary variables, the mean and standard deviation are expressed in percent. There are 564,675 individuals included in the sample (as of $t = -1$) that work for 5,268 merging firms (of which 3,227 are targets). Will change formatting of numbers slightly.

	Mean	SD
Pre-period		
Age	39.030	13.710
Female	39.660	48.920
Foreign	17.610	38.090
Blue-collar	64.180	47.950
Composite mental illness	9.710	29.600
Mental illness diagnosis	2.140	14.500
Depression diagnosis	0.516	7.170
Anxiety diagnosis	0.541	7.330
Inpatient	6.050	23.800
Outpatient	32.000	46.600
Mental illness medication	9.140	28.800
Antidepressants medication	5.630	23.100
Antidepressants dosage	17.030	141.900
Death	0.143	3.780
Suicide	0.012	1.100
Post-period		
Composite mental illness	12.600	33.100
Mental illness diagnosis	2.760	16.400
Depression diagnosis	0.613	7.810
Anxiety diagnosis	0.735	8.540
Inpatient	6.960	25.400
Outpatient	35.700	47.900
Mental illness medication	11.900	32.400
Antidepressant medication	7.370	26.100
Antidepressants dosage	25.630	208.700
Death	0.222	4.700
Suicide	0.012	1.090

Table 3: **The effect of mergers on mental health: Main variables**

We study the effect of mergers on the mental health of workers using the following OLS regression model: $y_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. y denotes one of the measures of mental health from Table 1. We report the coefficient on the variable *Post*. Since the outcome variables are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Composite mental illness	(2) Mental illness diagnosis	(3) Depression diagnosis	(4) Anxiety diagnosis	(5) Inpatient	(6) Outpatient
Post	0.205***	0.055*	0.040***	0.007	0.311***	0.475***
	(0.051)	(0.030)	(0.015)	(0.014)	(0.0478)	(0.105)
Observations	4,962,978	4,962,978	4,962,978	4,962,978	4,962,978	4,962,978
Number of clusters	5,268	5,268	5,268	5,268	5,268	5,268
Percentage change	2.1	2.6	7.8	1.4	5.1	1.5
Pre-period mean of dep. var.	9.714	2.138	0.516	0.541	6.052	31.977
Age F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: **The effect of mergers on mental health: Psychiatric medication**

We study the effect of mergers on the mental health of workers using the following OLS regression model: $y_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. y denotes one of the measures of mental health from Table 1. We report the coefficient on the variable *Post*. Since the outcome variables in columns (1) and (2) are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Mental illness medication	(2) Antidepressants medication	(3) Antidepressants dosage
Post	0.182***	0.133***	0.010
	(0.049)	(0.036)	(0.328)
Observations	4,962,978	4,962,978	4,962,978
Number of clusters	5,268	5,268	5,268
Percentage change	2	2.4	0.06
Pre-period mean of dep. var.	9.141	5.634	17.030
Age F.E.	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes

Table 5: **The effect of mergers on mental health: Death and suicide**

We study the effect of mergers on the passing of workers using the following OLS regression model: $y_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. y denotes the variable *Death* in specification 1 and *Suicide* in specification 2. We report the coefficient on the variable *Post*. Since the outcome variables are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). The analysis is restricted to employees working at the firm three years prior to the merger. The pre-period mean outcomes are calculated using only event times $t = -2, -1$. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Death	(2) Suicide
Post	0.043***	0.005***
	(0.007)	(0.002)
Observations	4,288,698	4,288,698
Number of clusters	3,798	3,798
Percentage change	30	44
Pre-period mean of dep. var.	0.143	0.012
Age F.E.	Yes	Yes
Individual F.E.	Yes	Yes
Sector-year F.E.	Yes	Yes

Table 6: **The effect of mergers on mental health: robustness tests**

We study the effect of mergers on the health of workers using the following OLS regression model: $y_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. y, the dependent variable, is *Physical illness* in specification (1) and *Composite mental illness* in specifications (2) - (4). We report the coefficient on the variable *Post*. Since the outcome variable is defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). Specification (1) employs the whole sample of workers and uses the dependent variable *Physical illness*. The other specifications employ sub-samples and the dependent variable *Composite mental illness*. Specification (2) focuses on the acquirer firms; specification (3) analyzes a sub-sample of growing and profitable target firms; specification (4) employs a sample of target firms in the lowest quartile of the leverage distribution. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Physical illness	Composite mental illness		
Post	0.002	0.196***	0.411**	0.410**
	(0.006)	(0.061)	(0.208)	(0.186)
Subsample	Full sample	Acquirers	Growth targets	Low leverage targets
Observations	4,963,081	3,727,878	201,860	342,446
Number of clusters	5,268	2,016	448	806
Percentage change	0.9	2.0	4.2	4.4
Pre-period mean of dep. var.	0.184	9.775	9.914	9.340
Age F.E.	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes

Table 7: **Summary statistics: Successful versus failed mergers**

In this table, we report the mean and standard deviation of the main variables of interest in the difference-in-differences analysis comparing successful and failed mergers. We report both the pre-merger period statistics ($-3 \leq t < 0$), as well as statistics for the post-merger period ($0 \leq t \leq 4$). For the variables *Death* and *Suicide*, the pre-period means and standard deviations are calculated for the interval $t = -2, -1$. There are 562,301 individuals in the sample of successful mergers, and 2,374 individuals in the sample of failed mergers (as of $t = -1$). There are 5,230 firms involved in successful mergers, of which 3,208 are target firms. There are 42 firms involved in failed mergers, of which 19 are target firms. Failed mergers are those where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement. Will change formatting of numbers slightly.

	Successful merger		Failed merger	
	Mean	SD	Mean	SD
Pre-period				
Age	39.020	13.710	42.160	12.910
Female	39.710	48.930	28.100	44.960
Foreign	17.610	38.090	16.980	37.550
Blue-collar	64.320	47.910	34.160	47.430
Composite mental illness	9.980	29.970	9.230	28.950
Mental illness diagnosis	2.180	14.610	1.660	12.780
Depression diagnosis	0.515	7.160	0.397	6.290
Anxiety diagnosis	0.561	7.470	0.511	7.130
Inpatient	6.100	23.940	05.700	23.190
Outpatient	32.410	46.800	29.870	45.770
Mental illness medication	9.400	29.190	8.680	28.160
Antidepressants medication	5.780	23.340	5.080	21.950
Antidepressants dosage	17.690	145.260	15.100	86.710
Death	0.143	3.780	0.182	4.260
Suicide	0.012	1.090	0.020	1.420
Post-period				
Composite mental illness	12.090	32.610	10.540	30.710
Mental illness diagnosis	2.680	16.160	2.170	14.560
Depression diagnosis	0.610	7.790	0.537	7.310
Anxiety diagnosis	0.719	8.450	0.393	6.260
Inpatient	6.890	25.330	6.280	24.250
Outpatient	35.090	47.720	35.380	47.820
Mental illness medication	11.470	31.860	9.950	29.930
Antidepressants medication	7.100	25.680	5.910	23.590
Antidepressants medication	24.020	195.800	17.940	93.960
Death	0.205	4.520	0.176	4.190
Suicide	0.013	1.140	0.009	0.937

Table 8: **The effect on mental health in the subsample of failed mergers**

We present the results from the following OLS regression model: $Composite\ mental\ illness_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. We report the coefficient on the variable *Post*. Since the outcome variable is defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). The regressions focus on a sub-sample of failed mergers, which are defined as mergers where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement. Specification (1) focuses on both acquirer and target firms involved in failed mergers; specification (2) analyzes a sub-sample of failed acquirer firms; specification (3) employs a sample of failed target firms. Note that there are 42 firms involved in the failed merger analysis but we exclude two attempted deals from regression specification 2 and 3, respectively, because the associated firms cannot be unambiguously identified as acquirer or target. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	Composite mental illness		
Post	0.117	-1.539	1.592
	(0.729)	(1.118)	(1.251)
Subsample	Failed mergers	Failed acquirers	Failed targets
Observations	17,474	8,643	8,075
Number of clusters	42	19	19
Percentage change	1.3	-17.5	16.6
Pre-merger mean of dep. var.	9.233	8.779	9.595
Age F.E.	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes

Table 9: **Difference-in-differences analysis using failed mergers: Main variables**

This table reports the coefficient estimates from the following OLS regression model: $y_{it} = \beta_1 \times Post_{it} \times Successful\ merger_i + \beta_2 \times Post_{it} + \omega_{FE} + \varepsilon_{it}$ where ω_{FE} contains age, individual, and sector-year fixed effects. y denotes one of the measures of mental health from Table 1. We report the coefficient on the variable β_1. Since the outcome variables are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * β_1 / pre-period mean of dependent variable). Failed mergers are those where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Composite mental illness	(2) Mental illness diagnosis	(3) Depression diagnosis	(4) Anxiety diagnosis	(5) Inpatient	(6) Outpatient
Post × Successful merger	0.700**	-0.088	-0.128	0.231**	0.128	-1.09
	(0.356)	(0.170)	(0.110)	(0.114)	(0.417)	(0.870)
Post	-0.531	0.125	0.158	-0.225**	0.181	1.55*
	(0.353)	(0.171)	(0.110)	(0.114)	(0.417)	(0.868)
Observations	3,660,749	3,660,749	3,660,749	3,660,749	3,660,749	3,660,749
Number of clusters	5,268	5,268	5,268	5,268	5,268	5,268
Percentage change	7	-4	-24.9	41.2	2.1	-3.4
Pre-merger mean dep. var. (successful mergers)	9.980	2.182	0.515	0.561	6.103	32.413
Age F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: **Difference-in-differences analysis using failed mergers: Psychiatric medication**

This table reports the coefficient estimates from the following OLS regression model: $y_{it} = \beta_1 \times Post_{it} \times Successful\ merger_i + \beta_2 \times Post_{it} + \omega_{FE} + \varepsilon_{it}$ where ω_{FE} contains age, individual, and sector-year fixed effects. y denotes one of the measures of mental health from Table 1. We report the coefficient on the variable β_1. Since the outcome variables in columns (1) and (2) are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * β_1 / pre-period mean of dependent variable). Failed mergers are those where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Mental illness medication	(2) Antidepressants medication	(3) Antidepressants dosage
Post × Successful merger	0.658**	0.387*	3.798***
	(0.325)	(0.223)	(0.835)
Post	-0.514	-0.316	-3.633***
	(0.323)	(0.222)	(0.831)
Observations	3,660,749	3,660,749	3,660,749
Number of clusters	5,268	5,268	5,268
Percentage change	7	6.7	21.5
Pre-merger mean dep. var. (successful mergers)	9.402	5.783	17.69207
Age F.E.	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes

Table 11: **Difference-in-differences analysis using failed mergers: Death and suicide**

This table reports the coefficient estimates from the following OLS regression model: $y_{it} = \beta_1 \times Post_{it} \times Successful\ merger_i + \beta_2 \times Post_{it} + \omega_{FE} + \varepsilon_{it}$ where ω_{FE} contains age, individual, and sector-year fixed effects. y denotes the variable *Death* in specification 1 and *Suicide* in specification 2. We report the coefficient on the variable β_1. Since the outcome variables are defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * β_1 / pre-period mean of dependent variable). Failed mergers are those where the target firm continues to exist as an independent entity in Serrano more than 365 days after merger announcement. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Death	(2) Suicide
Post × Successful merger	0.055	0.001
	(0.047)	(0.015)
Post	-0.014	0.002
	(0.047)	(0.015)
Observations	3,205,624	3,205,624
Number of clusters	3,798	3,798
Percentage change	38.3	6.1
Pre-merger mean of dep. var. (successful mergers)	0.143	0.0120
Age F.E.	Yes	Yes
Individual F.E.	Yes	Yes
Sector-year F.E.	Yes	Yes

Table 12: **Mental health and internal reorganization: evidence from 'stayers'**

We study the effect of merger-related reorganization on the mental health of workers using the following OLS regression model: $Composite\ mental\ illness_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. We report the coefficient on the variable *Post*. Since the outcome variable is defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. We also report the pre-period mean of the dependent variables (in percent), as well as the percentage change of the outcome variable from pre- to post-period (calculated as 100 * *Post* / pre-period mean of dependent variable). The analysis focuses on a sample of 'stayers,' i.e., workers who remain with the merged firm during the year of the merger and the year thereafter. The sub-sample underlying specification (3) consists of 'stayers' who do not take any sick leave at both $t = 0, 1$. *Lower wage* is a binary variable that takes the value of one if the wage at event times $t = 0, 1$ is lower than the wage at $t = -1$; it is zero otherwise. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	Composite mental illness		
Post	0.249***	-0.566***	-1.043***
	(0.065)	(0.078)	(0.079)
Post × Lower wage		1.94***	0.227***
		(0.109)	(0.076)
Subsample	Stayers	Stayers	Stayers (no sick days)
Observations	2,574,884	2,574,884	2,163,666
Number of clusters	3,291	3,291	3,236
Percentage change	2.8	19	3
Pre-merger mean of dep. var.	8.892	9.953	7.319
Age F.E.	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes

Table 13: **Effect of mergers on worker mental health: firm characteristics**

We report the regression coefficients from the following specification: $Composite\ mental\ illness_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains age, individual, and sector-year fixed effects. In the table below, we report the coefficient estimate β_1 in the column *Interaction Coefficient*. Since the outcome variable is defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. *Target* is a dummy variable that takes the value of one for target firms and zero for acquirer firms. *Small vs mid* is a dummy variable that takes the value of one for small target firms (corresponding to a relative size ratio below 50%) and zero for mid-size target firms (corresponding to targets with relative size ratios between 50-150%). *Small vs large* is a dummy variable that takes the value of one for small target firms and zero for large ones (relative size ratio above 150%). Relative firm size for each merger is calculated as the ratio of number of employees at the target over number of employees at the acquirer measured at $t = -1$. *Horizontal 2* is a dummy variable that takes the value of one if target and acquirer firm belong to the same two-digit SNI industry code, zero otherwise; *Horizontal 3* and *Horizontal 5* are defined analogously. Columns *% of individ.* and *% of firms* report, respectively, the percentage of individuals and firms at $t = -1$ in the subsample where the relevant $Characteristic = 1$. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	Composite mental illness			
	Interaction Coefficient	Standard Error	% of individ.	% of firms
Target versus acquirer				
Target	0.197**	(0.095)	23	61
Relative size: Targets				
Small vs mid	0.152	(0.172)	10	37
Small vs large	-0.130	(0.180)	10	37
Merger type				
Horizontal 2	0.261***	(0.099)	42	54
Horizontal 3	0.284***	(0.106)	38	47
Horizontal 5	0.343***	(0.112)	31	39

Table 14: **Effect of mergers on worker mental health: individual characteristics**

We report the coefficients from the following OLS regression: $Composite\ mental\ illness_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains age, individual, and sector-year fixed effects (age fixed effects are excluded from the regression in models studying age as an individual characteristic). In the table below, we report the coefficient estimate β_1 in the column *Interaction Coefficient*. Since the outcome variable is defined on a 0-100 scale, coefficients can be interpreted as the percentage point change between the pre- and post-period. *Female* is a dummy variable equal to one for women, zero for men. *Foreign* is a dummy variable equal to one if the individual (or both parents) is born outside of Sweden. *Age x - y* is a dummy variable equal to one if an individual is of age between x and y in the merger announcement year. *Below med. IQ (♂)* is a dummy variable equal to one if a man's IQ is strictly less than the median IQ across sample cohorts. *Below med. noncog. (♂)* is a dummy variable equal to one if a man's noncognitive skill score is strictly less than the median noncognitive skill score across sample cohorts. *No higher educ.* is a dummy variable equal to one if an individual has at most completed high school. *Blue-collar* is a dummy variable equal to one if the SSYK code of the individual is within the 400-999 range at $t = \hat{a}1$. *Above med. tenure* is a dummy variable equal to one if an individual has above median tenure at the firm (measured at $t = -1$). Columns *% of individ.* and *% of firms* report, respectively, the percentage of individuals and firms at $t = -1$ in the subsample where the relevant $Characteristic = 1$. Standard errors are clustered at the merger firm level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

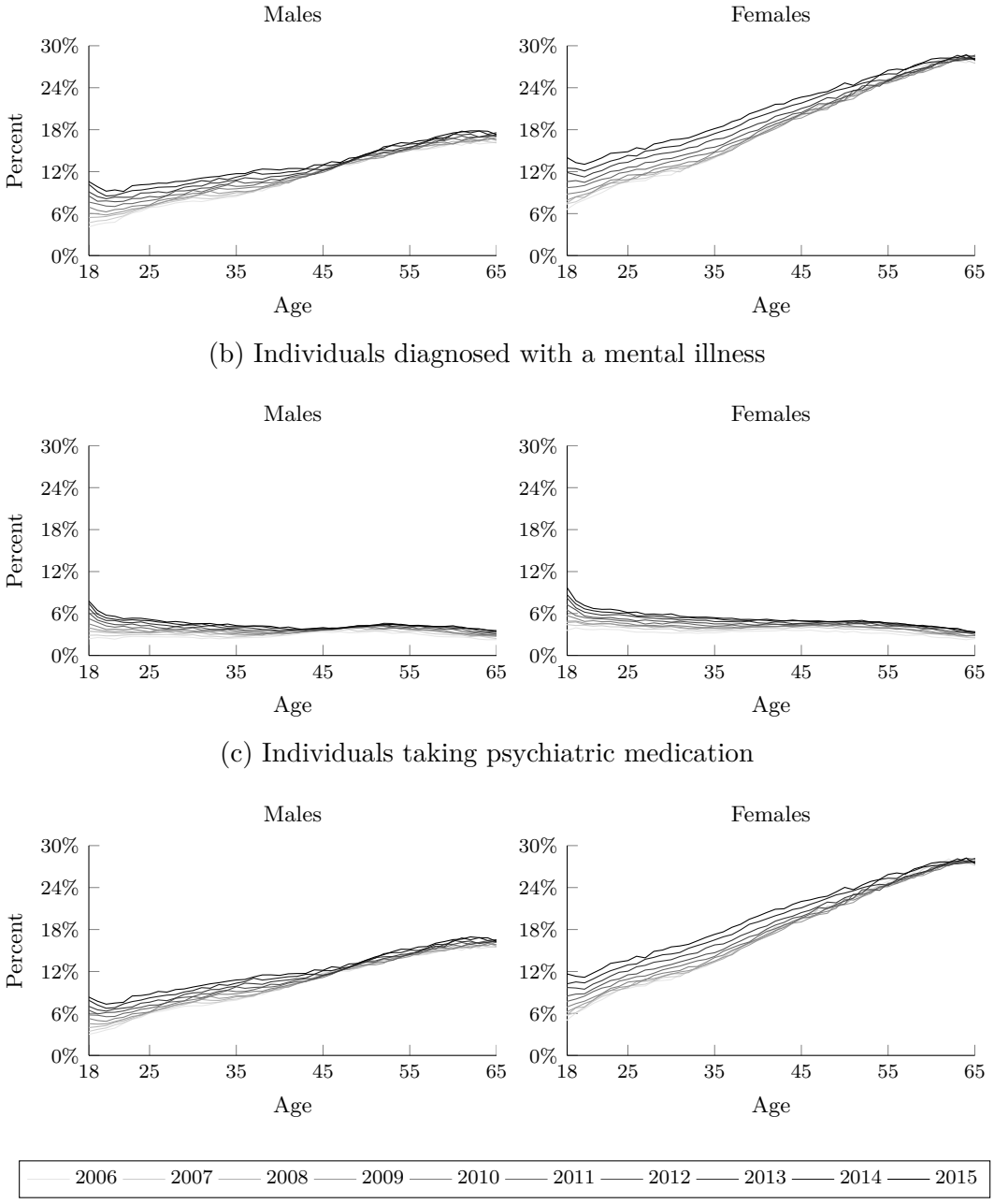
Dependent variable:	Composite mental illness			
	Interaction Coefficient	Standard Error	% of individ.	% of firms
Skill level				
Below median IQ (♂)	0.509***	(0.084)	15	77
Below median noncog. (♂)	0.931***	(0.096)	13	78
No higher educ.	0.615***	(0.071)	69	98
Blue-collar	0.640***	(0.077)	57	89
Above med. tenure	-0.066	(0.075)	34	87
Demographics				
Female	1.390***	(0.075)	40	89
Foreign	-0.160	(0.110)	18	74
Age 18-30	0.874***	(0.075)	30	86
Age 31-50	-0.092	(0.062)	44	98
Age 51-	-0.828***	(0.093)	26	86

A Internet Appendix

Appendix Figure A1: **Share of individuals with mental health problems in the general population**

These figures plot the share of men and women in the Swedish working age population, by age and cohort, during the sample period (2006-2015) that are either diagnosed with a mental illness or take psychiatric medication (Panel a); are diagnosed with a mental illness (Panel b); take psychiatric medication (Panel c).

(a) Individuals diagnosed with a mental illness or with psychiatric medication intake

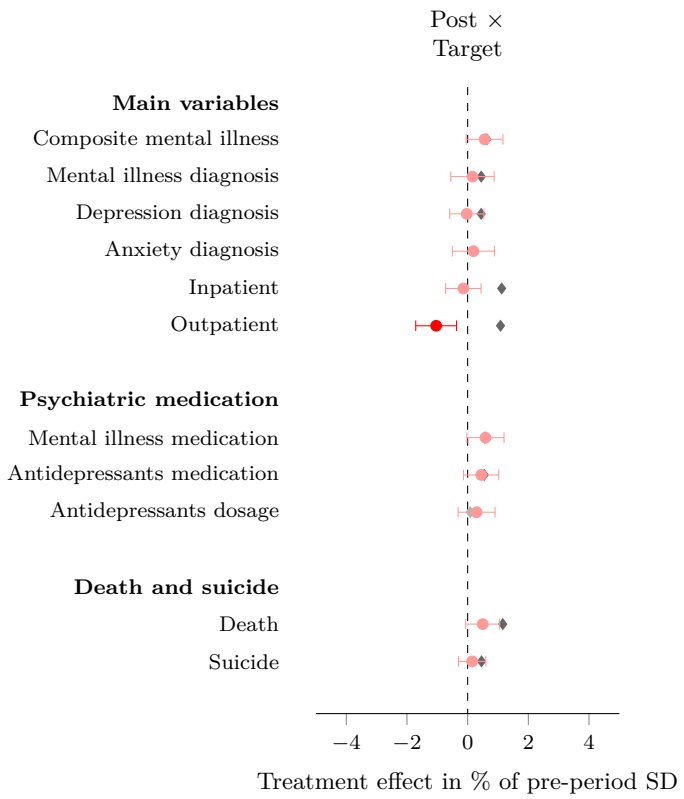


(b) Individuals diagnosed with a mental illness

(c) Individuals taking psychiatric medication

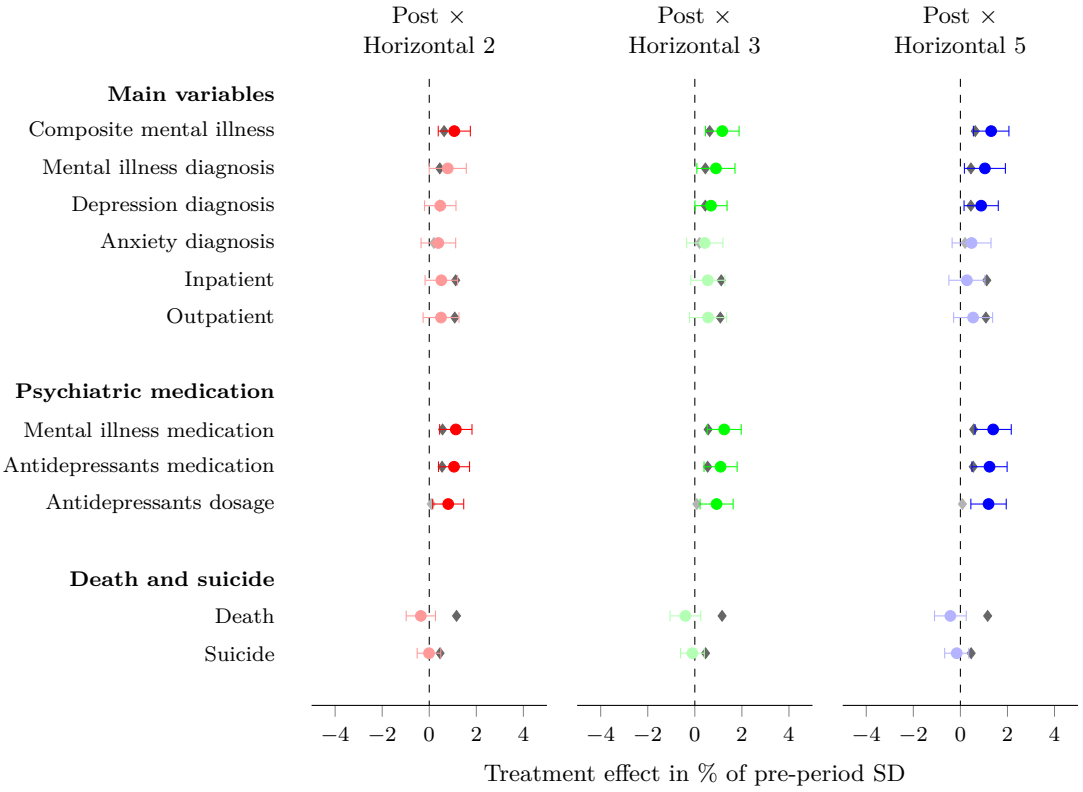
Appendix Figure A2: **Heterogeneous effects: Target employees**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual, age, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristic in the analysis. *Target* is a dummy variable taking the value of one if the individual works at the target firm at $t = -1$. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation*. The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



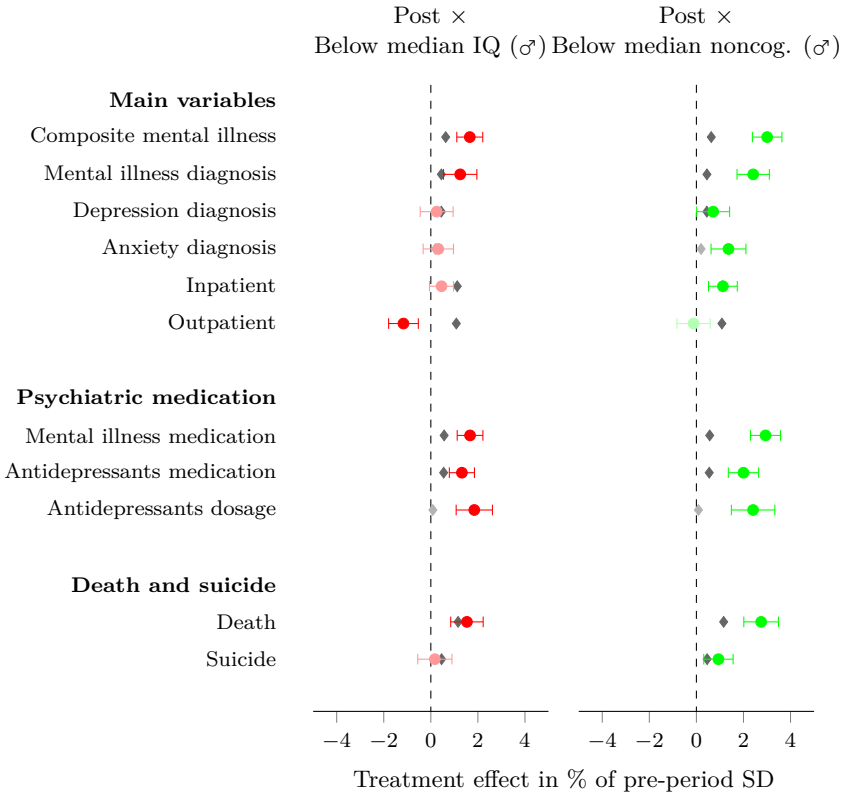
Appendix Figure A3: **Heterogeneous effects: Horizontal mergers versus vertical and conglomerate mergers**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual, age, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following firm characteristics in the analysis. *Horizontal n* is a dummy variable that takes the value of one if both target and acquirer have the same n first digits in their SNI industry codes at $t = -1$. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation*. The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



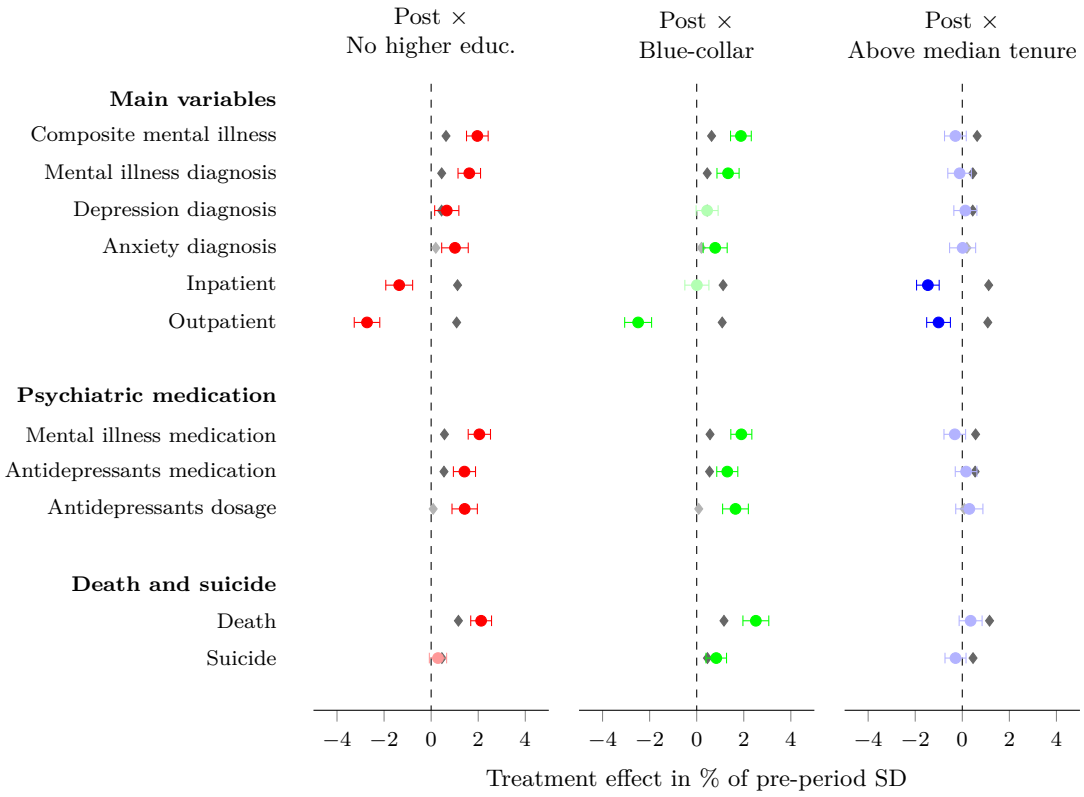
Appendix Figure A4: **Heterogeneous effects: Cognitive and noncognitive skills**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual, age, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristics in the analysis. *Below median IQ (♂)* is a dummy variable that takes the value of one if the cognitive skill score is strictly less than median cognitive skill score across cohorts. *Below median noncog. (♂)* is a dummy variable that takes the value of one if the noncognitive skill score is strictly less than the median noncognitive skill score across cohorts. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation.* The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



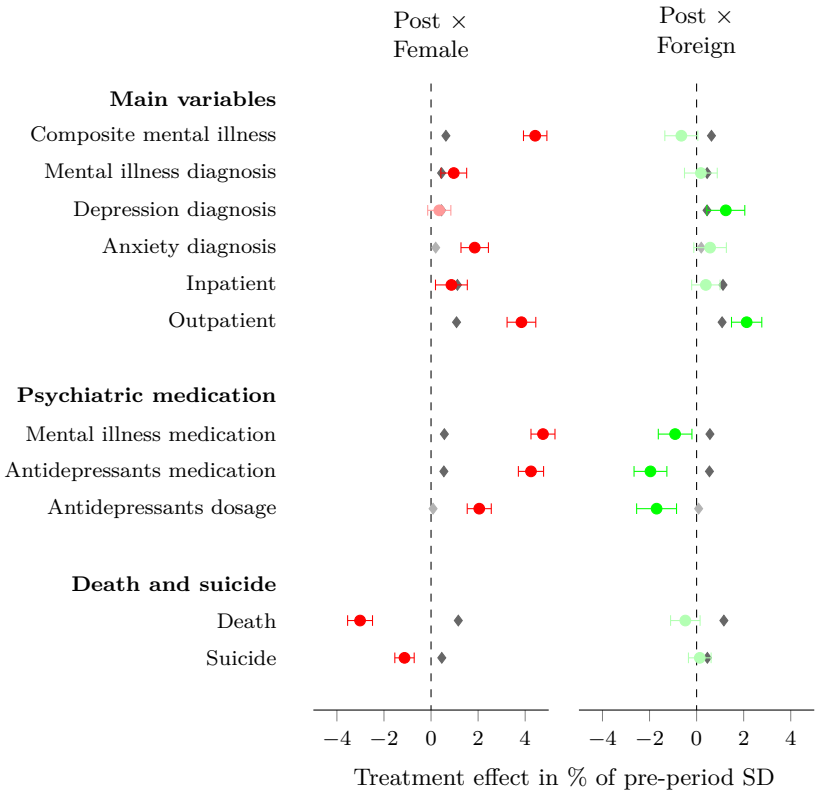
Appendix Figure A5: **Heterogeneous effects: Education, occupation, and tenure**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual, age, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristics in the analysis. *No higher educ.* is a dummy variable that takes the value of one if the individual has not attended higher education post high school. *Blue-collar* is a dummy variable that takes the value of one if the SSYK code of the individual is within the 400-999 range at $t = -1$. *Above median tenure* is a dummy variable that takes the value of one if an individual has above median tenure at the firm at $t = -1$. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation*. The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



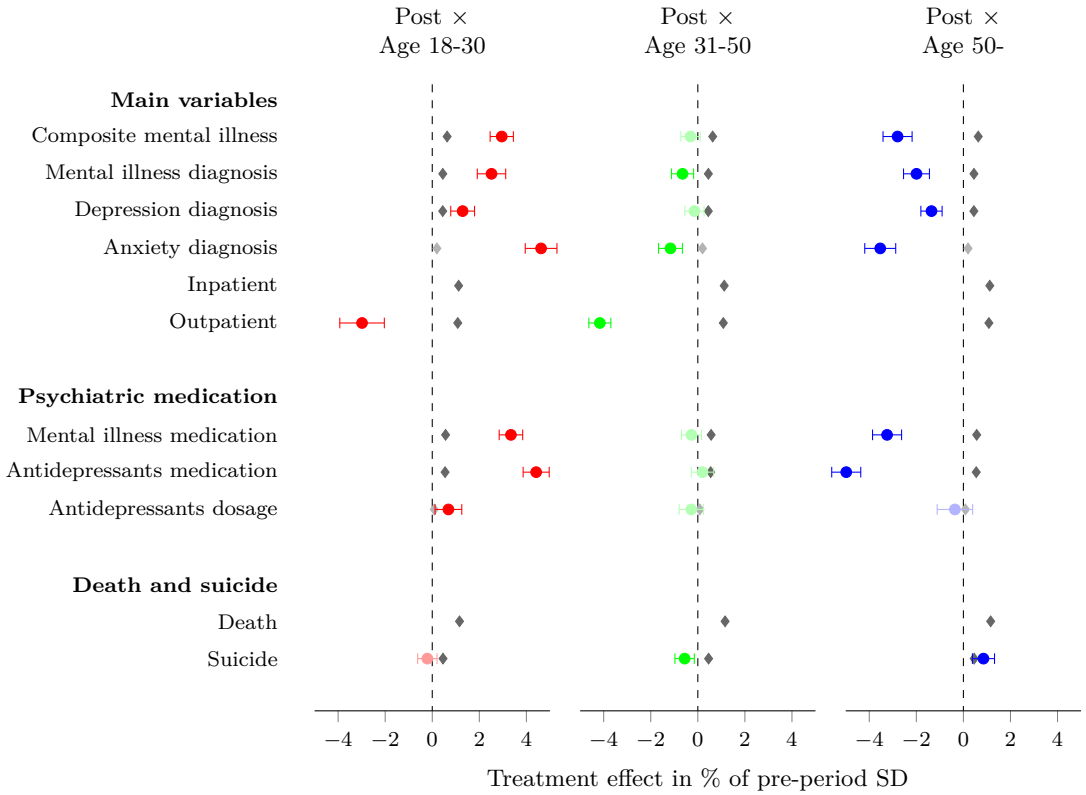
Appendix Figure A6: **Heterogeneous effects: Gender and country of origin**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains age, individual, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristics in the analysis. *Female* is a dummy variable that takes the value of one if female, zero if male. *Foreign* is a dummy variable that takes the value of one if either the individual is born outside of Sweden or both of the individual's parents are born outside of Sweden. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation*. The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



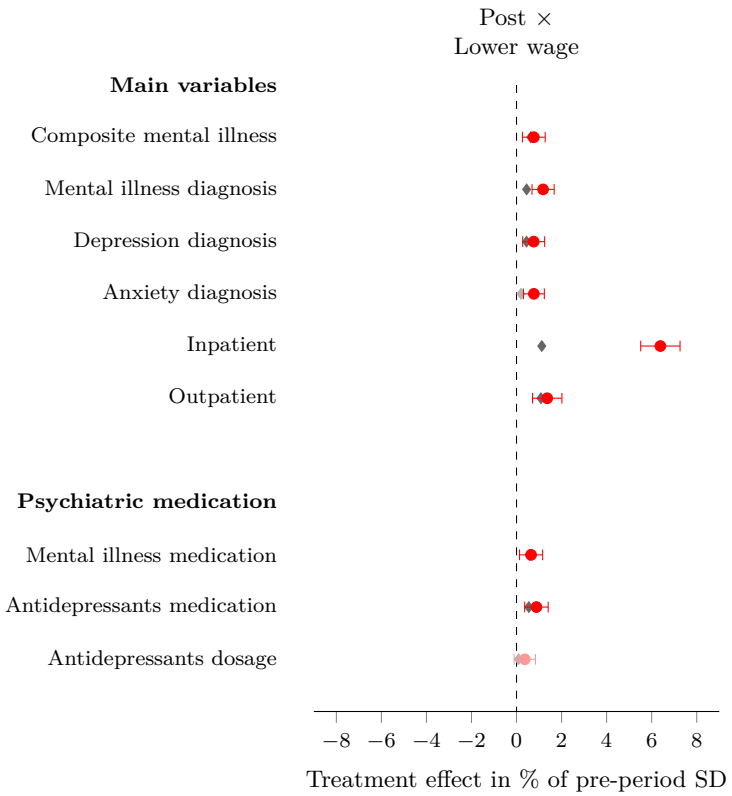
Appendix Figure A7: **Heterogeneous effects: Worker age**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual and sector-year fixed effects (age fixed effects are not used in these specifications). In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristics in the analysis. *Age x-y* are dummy variables that takes the value of one if the individual is (or, if dying in pre-period, would be) of age between x and y at the time of merger. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation*. The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$.



Appendix Figure A8: **Heterogeneous effects: Wage reduction for stayers**

We study the following OLS regression model: $y_{i,t} = \beta_1 \times Post_{i,t} \times Characteristic_i + \beta_2 \times Post_{i,t} + \beta_3 \times Characteristic_i + \omega_{FE} + \varepsilon_{i,t}$ where ω_{FE} contains individual, age, and sector-year fixed effects. In the figure below, we report the coefficient estimate β_1 from different specifications using several different dependent variables y (Table 1 contains variable definitions). We employ the following worker characteristic in the analysis. *Lower wage* is a binary variable that takes the value of one if the minimum wage at event times $t = 0, 1$ is lower than the wage at $t = -1$; it is zero otherwise. The treatment effect is expressed in percent of the pre-period standard deviation: *100 * coefficient / pre-period standard deviation.* The horizontal lines around the point estimates indicate 95% confidence intervals. Standard errors are clustered at the merger firm level. To be included in the sample, a worker must work at one of the merging firms at time $t = -1$ relative to the merger (except in the case of death and suicide, where workers are required to be employed by the merger firm at $t = -3$). In the case of death and suicide, the pre-period standard deviation is calculated using only event times $t = -2, -1$. For these graphs we only consider 'stayers' (individuals who work at the same firm from $t = -1$ to $t = 1$, with a possible switch between the target or acquirer firm at $t = 0$), and for the right-hand plot we restrict our subsample further to stayers with no sick leave at $t = 0$ and $t = 1$.



Appendix Table A1: **Depression definition**

The diagnosis codes reported below are used to define the variable *Depression diagnosis*. ICD-10 is the 10th revision of the International Statistical Classification of Diseases and Related Health Problems, a medical classification list by the World Health Organization.

ICD-10-CA	**Definition**
F31.31	Bipolar affective disorder, current episode mild or moderate depression
F31.32	Bipolar affective disorder, current episode mild or moderate depression
F31.4	Bipolar affective disorder, current episode severe depression without psychotic symptoms
F31.5	Bipolar affective disorder, current episode severe depression with psychotic symptoms
F31.6x	Bipolar affective disorder, current episode mixed
F31.9	Bipolar affective disorder, current episode unspecified
F32.0	Mild depressive episode
F32.1	Moderate depressive episode
F32.2	Severe depressive episode without psychotic symptoms
F32.3	Severe depressive episode with psychotic symptoms
F32.4	Depressive disorder, single episode, in partial remission
F32.5	Depressive disorder, single episode, in full remission
F32.8	Other depressive episodes
F32.9	Depressive episode, unspecified
F32.9	Depressive episode, unspecified
F33.0	Recurrent depressive disorder, current episode mild
F33.1	Recurrent depressive disorder, current episode moderate
F33.2	Recurrent depressive disorder, current episode severe without psychotic symptoms
F33.3	Recurrent depressive disorder, current episode severe with psychotic symptoms
F33.41	Recurrent depressive disorder, currently in remission
F33.42	Recurrent depressive disorder, currently in remission
F33.8	Recurrent depressive disorder, other
F33.9	Recurrent depressive disorder, unspecified
F34.1	Dysthymia
F34.8	Other persistent mood disorders
F34.9	Persistent mood disorder, unspecified
F38.0	Other single mood disorders
F38.1	Other recurrent mood disorders
F38.8	Other specified mood disorders
F39	Unspecified mood disorder
F41.2	Mixed anxiety and depressive disorder
F43.2	Adjustment Disorders
F43.2	Adjustment Disorders
F45.0	Somatization disorder
F45.1	Undifferentiated somatoform disorder
F45.9	Somatoform disorder, unspecified
F48.8	Other specified neurotic disorders
F48.9	Neurotic disorder, unspecified
F60.6	Anxious [avoidant] personality disorder
F60.8	Other specific personality disorders
F61	Mixed and other personality disorders
F62.8	Other enduring personality changes
F68.8	Other specified disorders of adult personality and behaviour
F99	Mental disorder, not elsewhere specified

Appendix Table A2: **Physical illness definition**

The diagnosis codes reported below are used to define the variable *Physical illness*; our definition follows Gilchrist (2002) closely. ICD-10 is the 10th revision of the International Statistical Classification of Diseases and Related Health Problems, a medical classification list by the World Health Organization. We list the number of individuals who at any point during the panel have been diagnosed.

ICD-10-CA	**# Individuals**	**Definition**
G71.0	117	Muscular dystrophy
G11.8	2	Other hereditary ataxias
G11.9	14	Hereditary ataxia, unspecified
G11.0	2	Congenital nonprogressive ataxia
G11.2	28	Late-onset cerebellar ataxia
G60.0	109	Hereditary motor and sensory neuropathy
G60.8	16	Other hereditary and idiopathic neuropathies
G24.1	9	Idiopathic familial dystonia
G30.0	140	Alzheimer disease with early onset
G35	1,201	Multiple sclerosis
G12.2	114	Motor neuron disease
Q85.0	157	Neurofibromatosis (nonmalignant)
Q61.2	221	Polycystic kidney, autosomal dominant
N00	84	Acute nephritic syndrome
N01	59	Rapidly progressive nephritic syndrome
D58.2	5	Other haemoglobinopathies
D68.5	775	Primary thrombophilia
E84.9	38	Cystic fibrosis, unspecified
M06.1	21	Adult-onset Still disease
E88.0	132	Disorders of plasma-protein metabolism, not elsewhere classified
I42.2	227	Other hypertrophic cardiomyopathy
E83.1	372	Disorders of iron metabolism
E72.1	12	Disorders of sulfur-bearing amino-acid metabolism
I78.0	28	Hereditary haemorrhagic telangiectasia
E31.2	0	Multiple endocrine neoplasia [MEN] syndromes
Single-gene disorder	3,833	Any of the diagnosis above