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How Do Mergers Affect the Mental Health of Employees?

Finance Working Paper N° 793/2021
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Abstract

We study employee mental health to assess the long-term, non-monetary consequences of mergers. Using employer-employee level data linked to individual health records, we document that the incidence of stress, anxiety, depression, and psychiatric medication usage increase following mergers. 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 skills are most affected. Mergers that generate more mental illness among employees perform worse after the transaction. A variety of tests address endogeneity concerns.

Keywords: Mergers and Acquisitions, Corporate Restructuring, Mental Health, Mental Illness

JEL Classifications: G34, I10, J81, L23

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How Do Mergers Affect the Mental Health of Employees?

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July 2024

Abstract

We study employee mental health to assess the long-term, non-monetary consequences of mergers. Using employer-employee level data linked to individual health records, we document that the incidence of stress, anxiety, depression, and psychiatric medication usage increase following mergers. 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 skills are most affected. Mergers that generate more mental illness among employees perform worse after the transaction. A variety of tests address endogeneity concerns.

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1 Introduction

As fiduciaries of corporations, directors must act in the interest of shareholders. However, this focus on shareholder value may not safeguard the welfare of all stakeholders if their contracts are incomplete. Therefore, a natural question to ask is how corporate actions affect stakeholders other than the firm's owners in dimensions likely to be disregarded in existing contracts. Recent debates about corporate purpose and calls for corporations to balance the interests of all stakeholders make this a timely question.[1]

In a merger, two firms combine to operate as a single legal entity to achieve certain strategic goals such as an increase in market share, a reduction in competition, or to benefit from economies of scale and scope. In these corporate transactions, the interests of shareholders may diverge from those of other stakeholders. 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 from the perspective of shareholders (Eckbo, 2014; Betton, Eckbo and Thorburn, 2008). This almost exclusive focus on shareholder value is not sufficient to understand whether mergers benefit society.[2] Mergers inevitably affect a wide range of stakeholders. For example, IO economists and antitrust regulators have long argued that anticompetitive mergers, even if beneficial to shareholders, may harm consumers. Our study focuses on another important set of stakeholders: workers.

Despite the importance of human capital for firms, few studies have examined the consequences of mergers for employees beyond the dimensions of employment and pay (Shleifer and Summers, 1988; Li, 2013*a*; Dessaint, Golubov and Volpin, 2017; Prager and Schmitt, 2021). Yet through mergers, firm boundaries are redrawn and internal work processes are reorganized. These changes may transform the scope and nature of jobs and, in some cases, lead to dismissals. The uncertainty, anxiety, and stress caused by such events can have a profound impact on the workers in the affected firms. In this paper, we investigate how mergers affect the mental health of employees.

Mental health is a key determinant of individual well-being and economies incur signifi-

[1]For example, in 2019, the Business Roundtable re-defined the purpose of a corporation from one that is exclusively focused on shareholders to one that benefits all stakeholders, including employees. In 2020, the World Economic Forum followed suit and declared that "shareholder primacy is giving way to a broader vision for stakeholder-driven capitalism." In addition, in a 2020 open letter to business leaders, Larry Fink (Chairman and CEO of BlackRock, Inc.) urged CEOs that "companies must be deliberate and committed to embracing purpose and serving all stakeholders–your shareholders, customers, employees, and the communities where you operate." Such efforts of "stakeholder governance" are not uncontroversial and have been criticized as misguided and counter-productive (e.g., Bebchuk and Tallarita, 2020).

[2]Indeed, common metrics of M&A success such as short-term stock price reactions may be misleading, calling for a more diverse set of performance measures (Bénabou and Tirole, 2010).

cant costs due to mental illness in the population.[3] While these costs are difficult to 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 may amount to up to four percent of GDP (Smetanin et al., 2011).

Existing evidence on the impact of M&As 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 a firm's decision to restructure and an 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 merger transactions in the Swedish economy between 2007 and 2015. This allows us to study not only mergers among large publicly-traded firms, which is often done due to data limitations, but also mergers among smaller firms that represent an important part of the economy but rarely receive attention in academic studies. Moreover, the employer-employee level data enable us to track the employment of workers before and after a merger. 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, and psychiatric medication usage and dosages.

Whether employees 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 improve workers' career prospects and provide a level of wage and employment stability that is beneficial for workers' mental health.

Using a wide array of measures, we find that mergers negatively impact workers' mental health. Relative to the pre-merger period, the likelihood of being diagnosed with a mental illness (including depression and anxiety) increases by 9%. The likelihood of being diagnosed

[3]Several studies document a strong and robustly negative correlation between life satisfaction and the presence of mental illness (e.g., Strine et al., 2009; Bray and Gunnell, 2006; 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."

with depression specifically increases by 8%, and the probability of taking antidepressants increases by 3% after the merger. Many of these effects remain elevated for three or more years after the merger.

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 larger than the effect of experiencing a change in management. It is quantitatively similar to the effect of marriage, but with the opposite sign: while business combinations 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 child.

Mergers are not random events. Therefore, a worsening of mental health following a merger may not reflect a causal effect on worker well-being. To address endogeneity concerns, we conduct a variety of identification tests that, taken as a whole, increase the confidence that the negative effect of mergers on mental health is indeed causal. Our baseline identification strategy consists of using a differences-in-differences design with staggered entry into the merger "treatment" (Borusyak and Jaravel, 2017). We consider only those individuals who have been involved in a merger event at some point in their career. This is relevant because we show that, before a merger takes place, workers of firms that merge are healthier than the rest of the population. The identifying assumption is that the timing of a merger is independent of the natural development of mental health issues among employees. Consistent with this assumption, we provide visual evidence that mental health among employees involved in a merger does not significantly change in the years prior to the event. To avoid well-documented biases generated by staggered designs (Baker, Larcker and Wang, 2022), we implement "stacked" differences-in-differences specifications in our entire analysis (Cengiz et al., 2019). That is, we compare members of a treated cohort only with employees who belong to a cohort that has not yet been affected by a merger.

To address the concern that the mental health of the workers in our sample could deteriorate even in the absence of a merger, we perform several additional tests. First, we conduct a placebo test in which we study the evolution of physical disorders among employees around merger events and find no effect. This evidence suggests that it is not the ailing condition of employees which triggers the merger. Second, we want to ensure that neither financial nor economic distress drive our results. This would be the case if a firm's distress jointly affects the likelihood of a merger and of worsening mental health conditions. To address this, we re-estimate our baseline regression using sub-samples of target firms that are profitable and growing prior to the acquisition (i.e., firms that are unlikely to be economically distressed),

as well as targets with low pre-merger leverage (i.e., firms unlikely to be experiencing financial distress). We find that even employees of such firms experience a worsening of their mental health following mergers.

Our last strategy to address 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, an event of which we find 38 occurrences involving 76 firms and 4,478 individuals during our sample period. We then 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 analyze whether there are differences in 'pre-trends' across the two groups. We observe that 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 mergers 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 impact of mergers on mental health 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.[4] At the same time, acquisitions typically involve a variety of restructuring measures that are not confined to employment cuts and that could also impair workers' mental health—e.g., expected promotions may be delayed or may not materialize, 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 experience negative mental health effects, we focus on a sub-sample of 'stayers', that is, workers who remain with the merging firms after the merger takes place. 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 of workers who experience wage cuts significantly deteriorates. Furthermore, we observe larger negative effects on the mental

[4]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).

health of workers who are assigned new tasks compared to workers who continue to perform similar tasks as before the merger. Taken together, these results suggest that takeover-related restructuring 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 view, we find that workers at the target firm are more likely to develop mental health problems than those in the acquiring firm. Horizontal mergers, in which firms that are active in the same industry are combined, may also plausibly create larger opportunities for employee cost savings and cultural clashes than vertical or conglomerate mergers, leading to more detrimental mental health effects for employees of firms involved in a horizontal merger. Indeed, we find that, relative to the average effect for all workers, employees involved in a horizontal merger are 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, especially when it comes to 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 workers in higher hierarchical positions. Consistent with these observations, workers with lower educational attainment experience more detrimental mental health effects than more educated employees.

We shed further light on the mechanisms behind our findings by studying 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., Mackenzie, Gekoski and Knox, 2006; 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 be diagnosed with a mental illness 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.[5] However, we obtain estimates that are small and statistically insignificant, suggesting that native-born and foreign-born workers experience broadly similar mental health effects following mergers. Finally, we find that workers older than 50 years are less likely to develop mental health problems following a merger. This may be because this group of people is particularly affected by mental health disorders to begin with, rendering problems occurring in the workplace of second-order importance.[6]

Mergers generally aim to enhance corporate value, yet the adverse effects on employees' mental health could lead to temporary declines in productivity, highlighting the complex balance firms must strike between strategic benefits and the well-being of their workforce. We investigate whether the deterioration of workers' mental health affects the post-merger operating performance of firms. We find that mergers that induce higher levels of mental illness among workers perform relatively worse after the business combination, as both profitability and labor productivity decline relative to mergers with lower mental health impact. A detailed exploration of the welfare implications of mergers is beyond the scope of our study; however, our findings suggest that firms may internalize, at least to some extent, the mental health consequences for employees.

Finally, we observe that mergers are experienced in different ways by employees at different levels of the firm hierarchy. In particular, while rank and file employees suffer negative mental health consequences after a merger, the mental health of C-suite executives as a group (encompassing CFOs, COOs, etc.) improves relative to the period before the merger.

This paper increases our understanding of the economic effects of acquisitions. Most of the existing 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*b*; Lagaras, 2019), and workplace safety (Cohn, Nestoriak and Wardlaw, forthcoming).[7] Our paper 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)

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

[6]Another possibility in this context is that because older workers have, on average, longer tenure they have more job security given Sweden's last-in-first-out dismissal rules. However, we do not find differences in mental health outcomes after the mergers when we explicitly compare workers with long and short tenures.

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

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 restructuring decisions affect the mental health of workers.

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 often 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 investigate the potential trade-off between the mental health costs of mergers and their monetary benefits for shareholders. Third, 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. Fourth, we shed light on what is driving the average deterioration in mental health after a merger and find that specific types of mergers (such as horizontal mergers) and personal characteristics (such as low skills and ability) lead to more detrimental health outcomes. We also show that certain groups of workers, like high-level executives, may experience an improvement in their mental health after the merger, suggesting that the mental health effects are quite unevenly distributed within firms. Fifth, we examine a 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.

Finally, our paper contributes to studies in the field of organizational psychology and occupational medicine that examine the effects of corporate restructuring on the well-being and mental health of workers. These studies tend to analyze a single restructuring event (typically not mergers) and its effect on workers in case-study type settings. They provide evidence that is anecdotal in nature and cannot differentiate across firm-specific contexts.

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 in different firms using a large sample of M&A events and a wide range of mental health indicators. Further, our setting enables us to link the mental health outcomes to the post-merger performance of the firm.

2 Empirical Setting, Data, and 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. The combined dataset allows us to track individuals and their characteristics over time and to study the evolution of health outcomes around merger events.[8] Our sample period is 2006 to 2015.

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. 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, and (ii) 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 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").[9]

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

[9] 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

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.[10] The transactions we study are those in which two or more firms combine to form a single entity. In the following sections, we will refer to these transactions interchangeably as mergers, acquisitions, or M&As. We do not study transactions in which one firm acquires an ownership stake in another firm and both entities continue to operate separately after the transaction.

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 healthcare 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.[11]

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 for clinical purposes. The ICD is maintained by the WHO. We define a diagnosis of a mental illness as having a 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 to define depression, 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 are structured according to ATC

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.

[10] In our tests, we code the event year as one year *prior* to the year that a merger is registered in the Bolagsverket filings reported in Serrano. The reason is that workers are likely to respond to the announcement to the merger, rather than the official filing itself, which according to our news searches often occurs more than six months after the announcement of the merger.

[11] 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.

codes, which represent a system of unique codes 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 an individual takes 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.

Panel A of Table A2 reports definitions of the dependent variables, while Panel B of Table A2 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 regression coefficients as percentage point changes.

2.3 Sample selection

For a firm to be included in our sample, it must meet three criteria. First, we only include firms that are incorporated and thus are limited liability firms (as opposed to partnerships, for example). Second, the firms in the sample must complete a merger transaction (either as

a target or an acquirer) between 2007 and 2015. As mentioned in section 2.1, the transactions we consider are business combinations in which two or more firms combine to form a single entity. Third, we require a firm to have at least five employees one year before the merger (i.e., at event-time $t = -1$). An advantage of our data relative to the data sets typically used to study mergers is that we can observe transactions affecting employees in all firms, not only among the largest firms in the economy. This is relevant because small and medium-sized firms contribute significantly to economic activity and represent a large part of the workforce, but are rarely the subject of academic studies. 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).[12] After these restrictions, the sample underlying our analysis contains 573,771 unique individuals that we follow over a period of up to nine years; these individuals work at 152,644 firms of which 5,646 experience one or more mergers and jointly account for 3,236 unique merger events during our sample period.[13]

Figure A1 reports the prevalence of mergers in the economy. About one percent of limited liability companies in the Swedish economy are involved in a merger transaction every year, affecting around three to four percent of all employees in the Swedish labor force. Figure A2 compares distributional features of limited liability companies in Sweden to those in our sample. Panels (a) and (b) suggest that our sample covers around two-thirds of the merger 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 1 reports the mean and standard deviation for the main variables of interest. We present the summary statistics separately for the pre-merger period (event-time *t=[-5,-1]*)

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

[13] 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.

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 9% in the pre-merger period to 12% in the post-merger period. Furthermore, the share of employees who take psychiatric medication increases from 9% pre-merger to 11% in the post-merger period. We also observe that the incidence of depression increases (from 0.5% to 0.6%), and that employees who take antidepressants increase their dosage from 16 to 24 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, and hospitalization.

To understand how employees' mental health is affected by an acquisition, we use a variety of complementary mental health measures, ranging from diagnoses of stress and anxiety to the intake of psychiatric medication, and hospitalizations. 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.[14] Equation (1) reports our baseline specification:

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

[14]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.

As the dependent variables we employ the mental health measures introduced in Section 2 and tabulated in Table A2. 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 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[15] 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 A3 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 prior to the merger event. To avoid potential biases generated by staggered empirical designs (Baker, Larcker and Wang, 2022), we use a "stacked" difference-in-differences design in our analysis; that is, we compare members of a treated cohort only with employees who belong to a cohort that has not yet been affected by a merger (Cengiz et al., 2019). This requires duplicating the initial sample as many times as there are yearly cohorts of treated employees, with the exception that in each cohort-specific sample we remove observations belonging to employees who have already been treated before. Due to this duplication, all specifications referred to in the paper must also include interactions of all right-hand-side variables (other than the treatment effect) with a fixed effect identifying each cohort-specific sample.

Table 2 presents the main results. Panel A reports coefficients from regressions using various measures of mental health diagnoses as dependent variables, while Panel B reports results on the use of psychiatric medication following a merger. In Panel A, specification 1, which employs as dependent variable the summary measure *Composite mental illness*, we observe a statistically significant increase by 3 percent (relative to the pre-merger period) 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), by 9 percent relative to the pre-merger period. While some mental health disorders, such as bipolar disorder or schizophrenia, are unlikely to be caused by corporate events, anxiety

[15]For trends in mental health diagnoses in Sweden see Figure A3 in the Appendix; for evidence elsewhere, see OECD (2018).

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 7.5% increase in depression diagnoses following the merger and an 11% increase in anxiety diagnoses. Finally, we observe statistically significant increases in the propensity to access health services in a hospital (column 5) and ambulatory healthcare (column 6). In terms of magnitudes, the likelihood of accessing outpatient (ambulatory) care increases by 1% after the takeover relative to the pre-period, while the likelihood of becoming a patient in a hospital increases by 6%.

Panel B of Table 2 reports results on the use of psychiatric medication following a merger. We find that the probability of taking any type of psychiatric medication increases by 2.3%. The probability of taking antidepressants, specifically, increases by 3.2% after the takeover, while the prescribed dosage increases by 9.6%.

A potential concern with these results is that they may be driven by differential trends originating prior to the merger. While our sample contains only employees who will at some point be affected by a merger, and are therefore relatively 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 merger. To address this issue, in Figures 1 to 2, 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. These figures also show that some of the negative mental health effects of business combinations remain elevated for several years after the event.

Our main tests focus on workers who were present at acquiring or target firms in the year prior to the merger. However, in results reported in Table A3 in the Online Appendix, we also analyze the evolution of mental health of workers who join merging firms. Because joining a new firm may be associated with changes in mental health even in the absence of a merger, we compare the incidence of mental illness of workers who join firms during the merger restructuring period to those who join firms prior to the merger. We find that joining a firm outside of a merger event is associated with a reduction in the likelihood of suffering from a mental illness. However, joining during the merger restructuring leads to a deterioration in mental health, compared to the mental health of those workers before joining and that of the employees already working at the merging firms. Because there may be selection in the type of workers joining at different points in time, one must interpret these findings with the appropriate degree of caution.

Taken as a whole, the results in this section imply that restructuring through mergers

and acquisitions has a 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 suggest that some workers, and, in turn, firms, may bear significant costs associated with mergers.

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, protracted unemployment, and a change in management. 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 3. To conserve space, we focus on the summary measure *Composite mental illness*.

The negative effect of a takeover on mental health (Panel (a)) is quantitatively similar to the effect of marriage (see Panel (e)), 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 child (displayed in Panel (f)): 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.[16] Another striking event is divorce (Panel (g)): 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. In the case of unemployment (Panel (d)), the changes in the likelihood of developing mental health problems peak in the year prior to becoming unemployed and then subside.[17] In the first year after becoming unemployed, the likelihood of mental health problems increases 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

[16] 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.

[17] In the test in this panel, unemployment is defined as a case when a worker leaves their employer but does not have another employer in the transition year and the next.

merger.

We also study the evolution of mental health around other significant corporate events. In Panel (b) of Figure 3, we document that corporate bankruptcies are associated with reductions in the incidence of mental illness by a magnitude that is about twice that of mergers in absolute terms. More similar to mergers is the effect of a change in top management (Panel (c) of Figure 3). When there is a change in executive-level management, mental health deteriorates but by less than in the event of a merger.[18]

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), larger than the effect of a change in senior management, 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 M&A transactions 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. To keep the discussion concise, we primarily focus on the summary measure of mental health, *Composite mental illness*.

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. However, an examination of the characteristics of workers 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 pre-merger is only 2% (see Table 1), compared to 4–6% in the general population (see Figure A3). Furthermore, depression and anxiety diagnoses (in-sample means of both variables are

[18] Members of the executive-level management include "C-suite executives" like the CEO, CFO, COO, etc., and are defined by occupational (SSYK) codes starting with 121, 131, 111, or 112, following Tag (2013).

around 0.5%) are also less common in our sample during the pre-merger period than in the general population (during 2006-2015, the depression incidence in the population was 0.8% and the incidence of anxiety was 0.95%).

4.2 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.[19] Based on this argument, we develop a placebo test to 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.[20] During our sample period, 3,833 employees were diagnosed with one of these disorders. Table A4 in the Appendix reports the list of single-gene physical disorders that we consider 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 3. The estimated coefficient on the variable *Post* is 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.[21]

[19]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; Gilje and Wittry, 2021). Second, certain mental diseases—such as depression, mood disorders, 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; 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.

[20]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).

[21]Table A5 in the Appendix presents several other placebo regression specifications with additional dependent variables related to physical illnesses. In these specifications, the dependent variables are physical ailments that are defined following Garcia-Gomez, Maug and Obernberger (2020). As in Table 3, we find that a merger has no effect on most physical diseases such as diseases of the respiratory system, diseases of the eye, diseases of the ear, and certain types of leukemia. A minuscule effect can be observed for osteopathies, diseases of the musculoskeletal system, and diabetes mellitus. Stress and lifestyle factors are commonly cited factors in the onset and maintenance of such diseases (e.g., McFarlane (2007)); these factors may be affected by the merger and would be consistent with the mental health effects of mergers documented in Table 2.

4.3 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 may be simultaneously affecting the target firm, their employees, and the likelihood of an acquisition taking place. That is, our results may reflect endogenous selection. To alleviate concerns that target firm distress drives the results reported in Section 3, we conduct several tests which we report in Columns 2 – 4 of Table 3.

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 3, we find that workers in the acquiring firms, on average, experience a deterioration of their mental health by 2.5% relative to the period before the acquisition. This suggests that any negative, unobservable factors leading a firm to be an acquisition target do not explain our results.

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 regression using a sub-sample of target firms that are unlikely to be experiencing economic or financial distress prior to the merger. In specification 3 of Table 3, we select target firms that satisfy the following criteria in the two years prior to the acquisition: the two-year average profitability (defined as 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.[22] 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 2), 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. While the coefficient estimates reported in columns 3 and 4 are less precise, we note that this may be due to the significantly reduced sample size in these specifications. In sum, the results reported in columns 2 – 4 of Table 3 suggest that negative confounding shocks—contemporaneous with the acquisition—and endogenous selection of targets are unlikely to be the drivers of the worsening mental health of workers in the aftermath of acquisitions.

[22] We rank firms into leverage quartiles using the debt to asset ratio of firms in the year prior to the merger. Results are similar when we focus on the lowest leverage decile.

4.4 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.

We define failed mergers as those that have a merger initiation date (which is reported in 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 38 failed mergers (76 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 explicitly 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.[23]

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 4 reports characteristics of employees that work for firms that successfully merge and those whose merger fails. Even though employees in failed mergers look slightly different from employees in completed mergers in terms of health before the (planned) merger date, the crucial identifying assumption is that the mental health of both groups of workers moves in parallel over time. Figure 4 plots the annual event-time averages (and standard deviations) of various mental health variables for the workers of firms that successfully consummated the merger 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 $t0$—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.

Next, we employ standard difference-in-differences regressions to estimate the effect of mergers on the mental health of workers. We compare the mental health of workers from successfully merged firms (the 'treatment' group) to the mental health of workers from firms that initiated a merger but end up not merging (the 'control' group), before versus after the

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

(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. Table 5 presents the results, which are broadly consistent with those reported in Section 3. Panel A reports coefficients from regressions using measures of mental health diagnoses as dependent variables, while Panel B reports results on the use of psychiatric medication following a merger. In column 1 of Panel A (a specification employing *Composite mental illness* as the dependent variable), the coefficient on *Post* × *Successful merger* is statistically significant at the five percent level. Quantitatively, the estimated coefficient on this interaction term represents a seven percent increase in the incidence of mental health problems after the merger, compared to before. This effect is economically larger than that in our baseline regressions, where we estimated a three percent increase in mental health problems (see Table 2). Decomposing the increase in mental health problems into its sub-components, it emerges from the coefficients in Table 5 that this increase is driven by an increase in the prescription of psychiatric medications and an increase in anxiety diagnoses. 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 4 percentage points, which corresponds to an increase of 22 percent relative to the pre-merger period. Although our sample of failed mergers is small and some of the difference-in-differences coefficients are imprecisely estimated, overall, the conclusion remains that mergers 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:

$$\begin{aligned} \textit{Composite mental illness}_{i,t} = \beta_1 \times \textit{Post}_{i,t} \times \textit{Characteristic}_i + \beta_2 \times \textit{Post}_{i,t} \\ + \beta_3 \times \textit{Characteristic}_i + \omega_{FE} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

The unit of observation continues to be a worker (i) in the years (t) around the merger event. To preserve space, the dependent variable employed in this analysis is the summary mental

health measure *Composite mental illness*, which accounts for both mental illness diagnoses and medication. *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, together with the fixed effect interactions required to implement the "stacked" differences-in-differences approach. 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 (different production facility or office within the merged firm), may experience a change in 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 experience negative mental health effects, in Table 6, we focus on a sub-sample of 'stayers', that is, workers who remain with the merged firm for at least one year after the merger event (they have to be at one of the merging firms in $t = 0$ and $t = 1$). While we continue to record the mental health status of the workers thereafter, to be categorized as a stayer in columns 1 to 5, we do not require a worker to be at either of the merging firms starting in year two after the acquisition. However, as discussed below (see discussion of column 6), this choice of post-merger tenure is inconsequential.

In column 1 of Table 6, we report the coefficients from a regression examining the effects of the acquisition on this sample of workers who 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 workers who do not lose their job when the merger occurs. The magnitude of the effect (an increase of around four percent relative to the pre-merger period) is comparable to the effect observed in the main sample (as reported in Table 2).

The other specifications reported in Table 6 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 or stays the same after the merger, the mental health among workers who experience a wage cut significantly deteriorates, as measured using the variable *Composite mental illness*. Indeed, among workers who are paid less after the merger, the incidence of mental health problems increases by 13 percent relative to the pre-merger period, while workers who are paid more after the merger experience a marginal improvement of their mental health status relative to the pre-period. Wage cuts may 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 6, we focus on stayers who do not register any sick leave during the year of the merger and the year thereafter. Relative to specification 2, the coefficient of interest is unchanged: 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.

Next, we investigate the effects of two common M&A restructuring activities on the workers. First, we consider the reallocation of workers to different tasks. Workers who maintain their status quo within the merging firm may experience less merger-induced uncertainty and anxiety than workers who are assigned to different tasks, which typically involves a change of colleagues and work habits. The variable *Change task* is a dummy variable indicating whether an individual's occupational code has changed in its first digit between $t = 0$ and $t = 2$, which represents a significant change in the types of tasks an employee performs.[24] Second, corporate restructuring brought about by the M&A event may entail a change in the place of work. We test whether merger-induced geographic relocation (within the merged firm) leads to a larger incidence of mental illness among the affected workers. The variable *Change location* is a dummy variable indicating whether the county code for the establishment at which an individual works differs between $t = 0$ and $t = 2$. This variable takes the value of one if there is a location change, regardless of whether the change is to an establishment that belonged to the acquirer or target prior to the transaction. The results from these two tests are reported in columns 4 and 5 of Table 6. We find that workers who are affected by the restructuring of the production process experience a larger increase in mental health disorders than workers who are not affected by a change in job tasks or locations. The coefficients are positive for both restructuring proxies, and statistically significant at the 1% level for *Change task*. We find that workers whose roles and responsibilities on the job change experience a 13 percent increase in mental health problems, relative to other workers.

[24]The occupational code (SSYK code) is a four-digit code with the first digit indicating the coarsest definition of occupational tasks. A change in the first digit would occur, for example, if a worker's tasks change from technical to clerical, or vice versa.

Finally, in the regressions reported in columns 1 – 5, a stayer is defined as a worker who works at one of the merging firms in $t = 0$ and $t = 1$. However, our results are similar when, in column 6, we require workers to remain within the merged firm for at least three years post-transaction (instead of one year) to be categorized as stayers.

In sum, our results illustrate that takeover-related restructuring is associated with the worsening of workers' mental health, even in the absence of job loss.

5.2 Mergers and mental health outcomes: the role of firm and transaction characteristics

In this section, we focus on specific characteristics of the transactions and the merging firms to shed more light on how the depth of internal reorganizations determines post-merger mental disorders among the employees. We report the coefficient β_1 on the interaction term $Post_{i,t} \times Characteristic_i$ from the regression model described in equation (2). First, we compare workers in the takeover targets to those in the acquirer firms. It is plausible that a takeover target may be subject to more intense restructuring and reorganization activities than the acquiring firm, leading to more pronounced effects experienced by the target firm's workers. Consistent with this conjecture, in the test reported in Table 7 under the heading 'Target versus acquirer', we find that workers at the target firm are indeed more likely to develop mental health problems than those in the acquiring firm.

What is the role of firm size in the merger? On the one hand, 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 drastically following a merger. Workers of small target firms may thus experience more uncertainty and loss of control, and may therefore 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 report two specifications, one comparing small targets to mid-size targets and one comparing small targets to large targets. We find no evidence 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. At the same time, conditional on retaining the job in the merged firm, one may argue that workers are less likely to face new jobs and assignments since the merging firms may engage in broadly similar activities in the case of horizontal mergers; this may lessen the impact of the merger on workers' mental health. We examine the question whether horizontal mergers are more detrimental than other mergers in the tests reported in Table 7 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 estimate a positive coefficient, suggesting that horizontal mergers are associated with more adverse mental health outcomes for employees than other mergers. This appears to be especially the case when the activities are more similar, as is evidenced by the larger magnitude and statistical significance of the coefficient associated with the most granular industry definition (*Horizontal 5*).

While we have examined how various transaction and firm characteristics influence post-merger mental disorders among employees, many other deal characteristics could affect employee mental health in different and interesting ways. These include acquisitions of struggling firms, disciplining mergers to eliminate inefficiencies, and acquisitions of family firms that experience internal conflicts. Exploring these distinct effects on mental health would be an interesting direction for future research.

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

Does the impact of takeovers on mental health depend on a worker's innate abilities and skills acquired on-the-job? 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 endowed with better 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 8 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 skills are 0.2 percentage points more likely to endure a deterioration of their mental health, although this difference is not statistically significant at conventional levels. The effect is larger (and statistically significant) for noncognitive skills. Workers who rank below the median level of noncognitive skills experience a 0.5 percentage point higher likelihood of suffering from mental illness than those above the median.[25]

While the previous tests focus on measures of innate ability, we also study the role of skills acquired through education or on the job. We find that workers with lower educational attainment (less than a college degree) experience about 0.4 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 (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.

Workers also acquire skills on the job. Using tenure as a proxy for firm-specific human capital, we do not observe a statistically significant difference in health outcomes between high and low tenure workers. The lack of precision of the estimate associated with worker tenure may reflect the ambiguity of using tenure as a proxy for firm-specific human capital.[26]

In sum, our estimates support the conjecture that skilled workers are less prone to suffer from merger-induced mental illness than workers who have lower skill endowments.

5.4 Personal sensitivity to work-related changes

Do personal characteristics determine the extent to which the mental health of employees is affected by an M&A event? We shed light on this question in Table 8, under the heading 'Demographics'. As before, we report the coefficient β_1 on the interaction term $Post_{i,t} \times Characteristic_i$ from the regression model described in equation (2). The first specification

[25]The share of individuals in our sample who have below median IQ and non-cognitive skills is only 15% and 13%, respectively. This is because the military tests (from which these data were sourced) were only mandatory for males until 2010. Therefore, these scores are missing for women, non-Swedes, and younger cohorts.

[26]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.

examines 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 or death when the pressure on them becomes overwhelming (see, e.g., Mackenzie, Gekoski and Knox, 2006). Our data confirm this. In Table 8, we observe that the likelihood of women suffering from mental health problems after the merger is 0.6 percentage points higher than that of men.[27].

The second set of estimates focuses on the country of origin of employees. On the one hand, the mental health of employees born abroad may be more dependent on occurrences 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 if they experience mental health problems, or it may not be in line with their culture of origin to use certain medication. Consistent with this ambiguity, we obtain estimates that are small and statistically insignificant, suggesting that native and foreign-born workers experience broadly similar mental health dynamics around 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, if workers in this age group already experience more mental health problems to begin with, they may be less affected by work-related events. Furthermore, the last-in-first-out labor laws in place in Sweden protect workers with longer tenure at the firm (who also tend to be older) from being fired, which may insulate older workers more from restructuring-related stress. Their careers tend to be also more settled and they may face less uncertainty about career prospects after a merger. We find that the effect of mergers on mental health appears to manifest itself less for older workers: workers above the age of 50 experience less negative mental health consequences from mergers than younger workers.

6 The implications of merger-induced mental illness for firm performance

In the previous sections, we documented that workers experience an increased likelihood of suffering from mental illness after a merger, especially workers with low skills and in case of more profound restructuring of the merging firms. In this section, we study the broader

[27]In untabulated results, we find that women are significantly less likely to die or commit suicide following a merger than men are (note that the annual risk of dying before the M&A is around 0.14%, so this is a rare event and these estimates have to be interpreted with caution)

implications of these findings for firms.

Mergers entail costs and benefits to different stakeholders. A key consideration is the extent to which firms internalize the mental health repercussions on employees. Specifically, the question arises whether the financial performance of firms reflects the negative mental health outcomes experienced by their workforce. This would be the case, for example, if a merger triggered significant compensation claims by employees affected by mental illnesses. However, while employers can face litigation for inducing occupational stress (DeFrank and Ivancevich, 1998), reorganizations and layoffs often do not qualify for such claims (Mishiba, 2022). Firms may also internalize the mental health effects of mergers through ex ante wage premia, compensating employees for the heightened risk of takeovers and possible negative effects of these events on employment and mental health. This would align with evidence that high-pressure jobs carry an earnings premium (Nagler, Rincke and Winkler, 2023). Detecting such premia in our sample is difficult due to the positive selection on mental and physical attributes of workers in merging firms (see Section 4.1) and due to the difficulties in measuring ex ante takeover risk using observable firm characteristics. Nevertheless, in Table A6 in the Online Appendix, we compare the salaries of workers in firms that will merge in the near future (but before they do so) to those of workers whose firms are not involved in a merger during the sample period. We find that workers in (future) merging firms enjoy higher wages than workers in non-merging firms. While this is consistent with workers being (potentially partially) compensated for health-related risks associated with future mergers, the fact that workers of merging and non-merging firms differ in many dimensions other than merger exposure prevents us from drawing definite conclusions.

Firms may also internalize a merger's mental health impact on workers because of the importance of workers as a fundamental production input. Indeed, the merging firms' bottom lines may be negatively affected due to temporary drops in labor productivity if workers' mental health suffers significantly in the aftermath. This does not negate the potential benefits of mergers as, overall, welfare-enhancing corporate strategies. However, it is conceivable that a firm may experience temporary declines in operating performance if their workers are particularly negatively affected by the merger.

We study this question in Table 9. In these tests, we track the evolution of operating performance before and after the merger for target and acquirer firms. We test whether firms whose workers experience the largest increase in mental illness around the merger are the firms where performance is most affected by the M&A event. We employ a similar stacked difference-in-differences estimation method as in the individual employee regressions reported in previous tables; however, the regressions reported in Table 9 are run at the firm-level. The dependent variable is the return on assets (EBITDA divided by assets) in column

1, profit margin (EBITDA divided by sales) in column 2, and sales per employee in column 3. In these regressions, we compare post-merger outcomes for the merged firm to a blended measure representative of the performance of the merging firms pre-merger. For example, in the case of the return on assets, in each year before the merger, we sum EBITDA for target and acquirer and divide this by the sum of total assets for target and acquirer. We follow a similar procedure to calculate the evolution of profit margin and sales per employee for the combined firm prior to the merger. We indicate large increases in mental health problems among the workforce after the merger using the dummy variable *Above median change share MI*. To construct this variable, we first calculate the share of workers who experience mental health problems in each firm and year (these are workers for whom the variable *Composite mental illness* takes the value of one); pre-merger, for each transaction, we pool the workers of future targets and acquirers. For each merger, we then average the annual share of workers with mental health problems for all pre- and post-merger years, respectively, and calculate the change in this average share from before to after the merger; this is a firm-level summary measure of how the mental health status of the employees of the merging firms changes after the merger relative to before. Finally, *Above median change share MI* takes the value of one if the increase in the share ranks above the sample median.

We find a negative correlation between merger-induced mental illness and firm performance. Mergers in which a larger fraction of workers experience mental health problems after the merger, relative to before, suffer a decline in the return on assets (Table 9, column 1), profit margins (column 2), and sales per employee (column 3), when compared to cases in which workers' mental health is less negatively affected by the merger.

One of the ways through which mental illness may affect firm performance is an increase in the rate of employee absenteeism. We measure absenteeism at the firm level as the total number of sick days registered by employees of the firm during the year divided by the number of employees at the firm. In the regression reported in column 4 of Table 9, we find that the post-merger rate of health-related absenteeism is significantly larger in cases where the business combination was associated with a larger increase in mental illness than in mergers with limited impact on employee mental health. While these correlations may indicate that merger-driven mental illness impacts the success of the merger, it is also possible that the causality is in the opposite direction; that is, when mergers are financially unsuccessful, employees' mental health problems may increase. Therefore, one must exercise caution when interpreting these suggestive findings.

Next, we ask whether the managers of the merging firms also bear significant psychological costs of the merger. If uncertainty is a big driver of a decline in mental health, senior managers may be less affected as they have more control and are better informed about

the merger. On the other hand, the burden of responsibility falls disproportionately on the senior executives of firms, which may elevate stress and anxiety around the merger event. We examine this question in Table 10. We estimate our baseline regression model (equation (1)) but focus on two subsamples of senior executives: specification 1 focuses on all executive-level managers (including chief executive officer, chief financial officer, chief operating officer etc.), while the sample underlying specification 2 studies all senior staff members.[28] We find that top managers do not experience a deterioration of their mental health around the merger. If anything, members of the executive suite experience an improvement in their mental health after the merger, although the precision of these estimates is low.

Finally, to ascertain the full extent of the mental health costs caused by mergers, we analyze the persistence of the effects. If mental illness is a transitory phenomenon and workers recover quickly after the merger, the cost for society may be small. If instead, merger-related mental health problems tend to persist for a protracted period of time, the mental health costs associated with mergers can be substantial even beyond the window of eight years over which we evaluate the effect of mergers. To quantify the persistence of mental health disorders, we estimate the likelihood of an individual having a mental illness depending on past mental illness. Specifically, for the workers in our sample, we test whether mental health disorders one, two, three, four, five, or six years ago are associated with an elevated likelihood of still suffering from mental health problems today. The findings, presented in Table A7, suggest that mental illness is indeed very persistent. When a worker is diagnosed with a mental disorder, the likelihood of still being treated for that illness one, three, and five years later is elevated by 41%, 10%, and 6%, respectively, suggesting that individual workers who experience an increase in mental illness following the merger suffer lasting negative effects on their well-being.

In sum, mergers impose a significant and persistent toll on workers' mental health. In cases where mergers are associated with large negative mental health effects among workers affected by the merger, we observe a significant deterioration in firm performance. However, there is no evidence that the mental health of senior executives is negatively affected.

7 Conclusion

Mental health is a key determinant of individual well-being and economic development. Recently, the WHO estimates that depression and anxiety disorders alone cost the global

[28]The grouping follows Tag (2013), where members of the C-Suite are defined by occupational codes starting with 121, 131, 111, or 112; members of the senior staff are defined by occupational codes starting with 122 or 123.

economy $1tn each year in lost productivity (Bloom et al., 2011; EU, 2014).

In this paper, we ask whether mergers affect the mental health of employees. 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 and acquiring firms, 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.

The mental health problems that workers experience during the corporate restructuring process of a merger are negatively correlated with firm performance, suggesting that firms may at least partly internalize these effects on workers. Specifically, mergers with larger increases in mental illness among the worker population are associated with larger reductions in profitability and labor productivity compared to mergers in which the mental health of workers is not severely affected.

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, such as mergers, on employees' well-being and on overall welfare.

Different corporate restructuring events, including mergers, IPOs, and bankruptcies, are likely to affect employee mental health in diverse ways, with each event bringing unique challenges, opportunities, and outcomes to different groups of employees within the firm. The study of how corporate actions other than mergers impact workers' well-being constitutes a fruitful area for future research.

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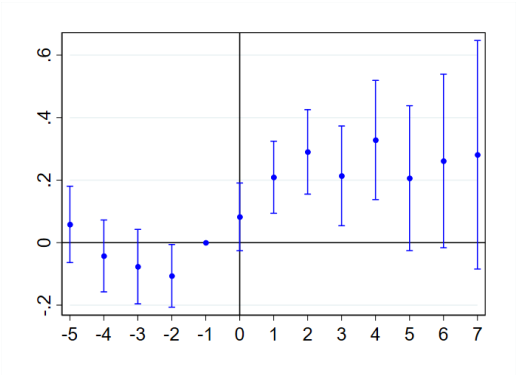
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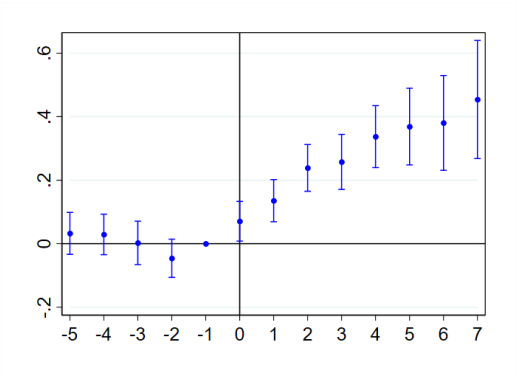
Figure 1: **Event time graphs: Mental health and contacts with healthcare providers**

This figure shows the evolution of several mental health measures 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 in Panels (a) to (d) corresponds to different measures of mental health. In Panels (e) and (f), we consider as dependent variables *Inpatient* and *Outpatient*. $t = -1$ is the reference event time (corresponding to the omitted category in the regression equation above). The plotted confidence intervals are based on standard errors that are clustered at the level of the firm at which an employee worked before the merger event (that is, target or acquirer firm).

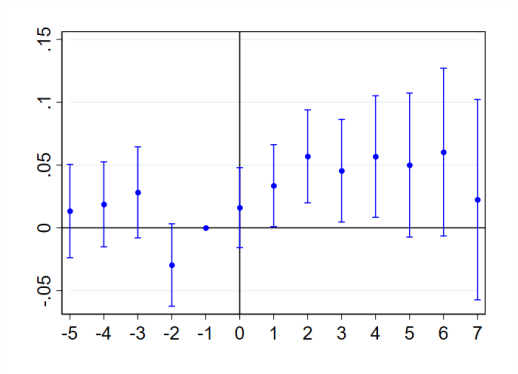
(a) Composite mental illness



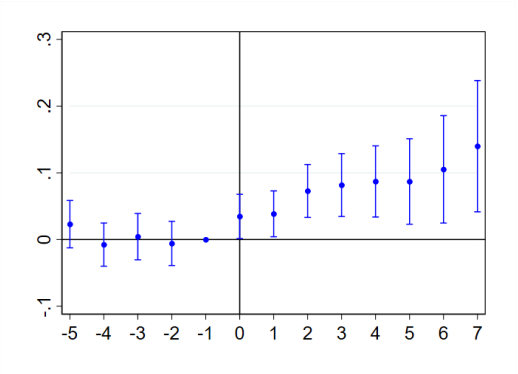
(b) Mental illness diagnosis



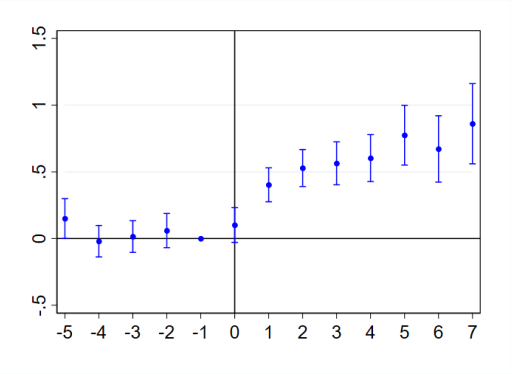
(c) Depression diagnosis



(d) Anxiety diagnosis



(e) Inpatient



(f) Outpatient

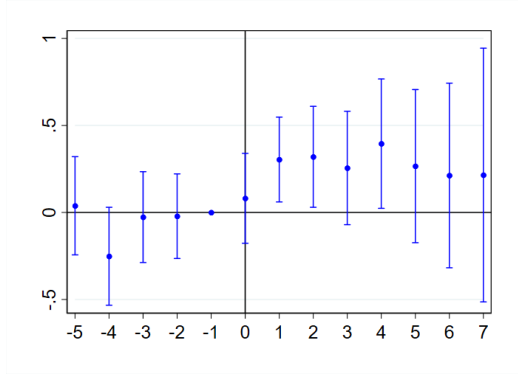
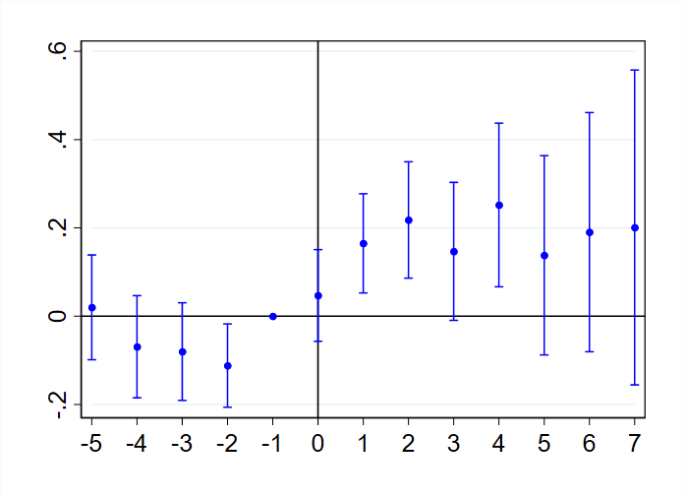


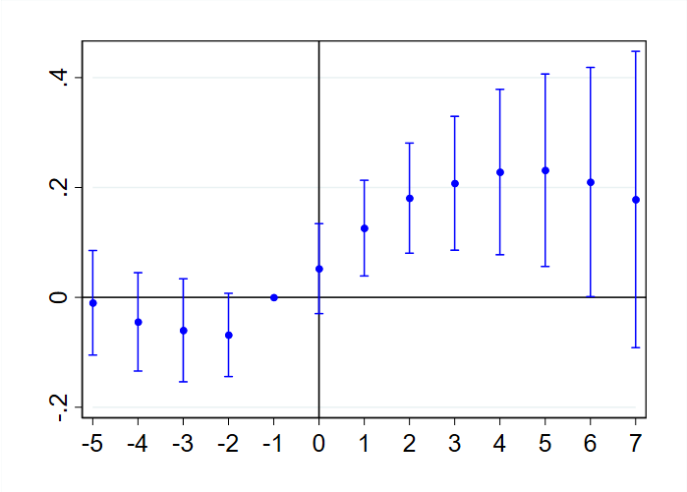
Figure 2: **Event time graphs: Psychiatric medication**

This figure shows the evolution of employees' intake of psychiatric medication 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 is *Mental illness medication* in Panel (a), *Antidepressants medication* in Panel (b), and *Antidepressant dosage* in Panel (c). $t = -1$ is the reference event time (corresponding to the omitted category in the regression equation above). The plotted confidence intervals are based on standard errors that are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm).

(a) Mental illness medication



(b) Antidepressants medication



(c) Antidepressant dosage

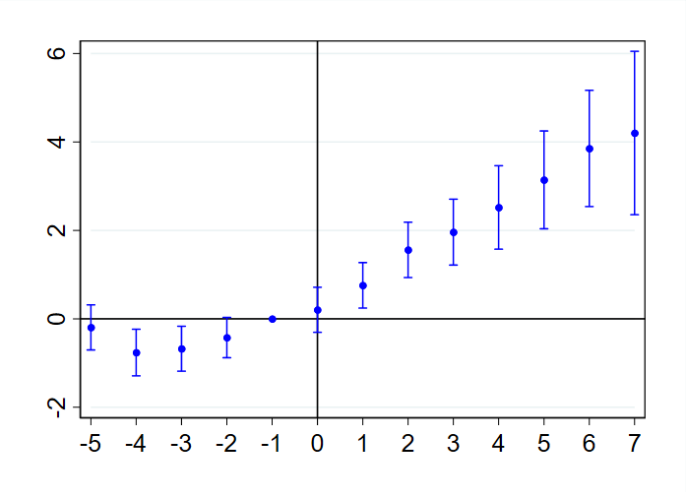
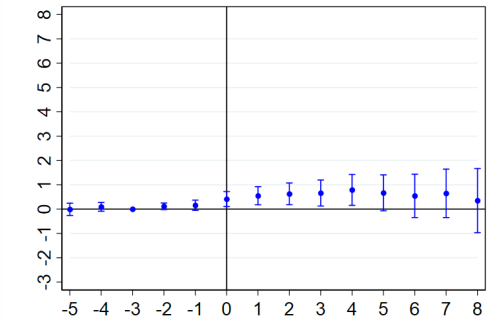


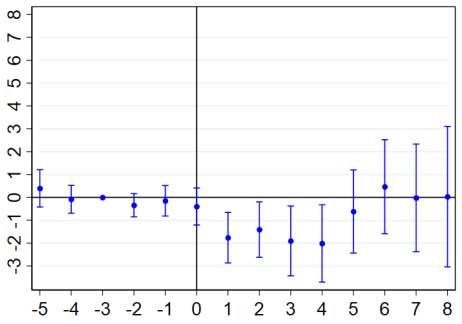
Figure 3: **Merger event compared to other events**

We examine the mental health implications of various events for employees that work at a merger firm at $t = -1$: mergers, Panel (a); corporate bankruptcy, Panel (b); change in executive-level management, Panel (c); unemployment, Panel (d); marriage, Panel (e); birth of first child, Panel (f); divorce, Panel (g). We plot the coefficients β_j and 95% confidence intervals based on the following regression: $\textit{Composite mental illness}_{it} = \sum_{j=-5}^{8} \beta_j \times 1(\textit{Event time} = j) + \omega_{FE} + \varepsilon_{it}$. ω_{FE} contains age and sector-year fixed effects. The event time dummy for year $t = -3$ is omitted (reference year). Standard errors are clustered at the level of the firm at which an employee worked in the year before the merger (panel (a)); the current firm of employment (panels (b) and (c)); and at the individual-level in the other panels.

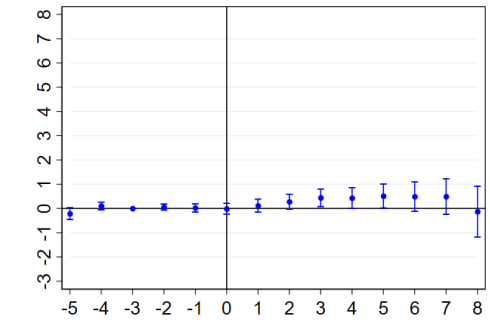
(a) Mergers



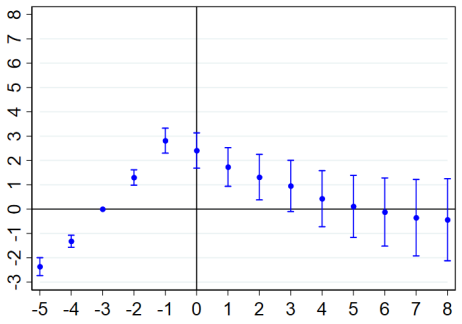
(b) Bankruptcy



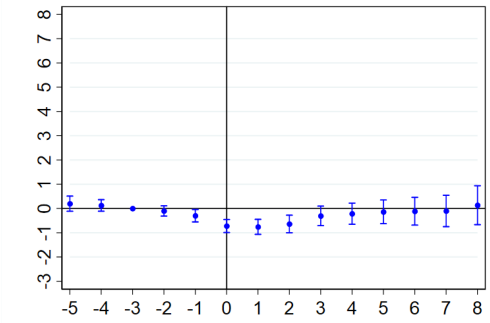
(c) C-Suite change



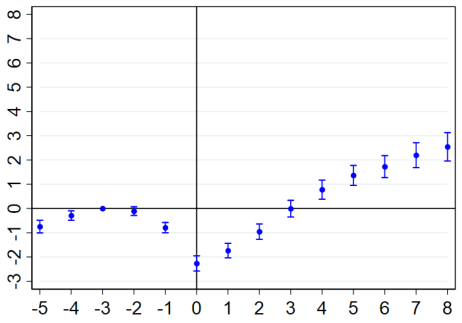
(d) 1+ years of unemployment



(e) Marriage



(f) First-born



(g) Divorce

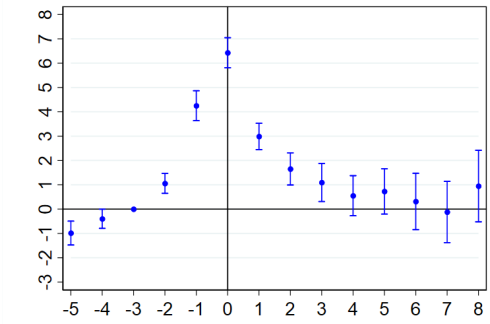
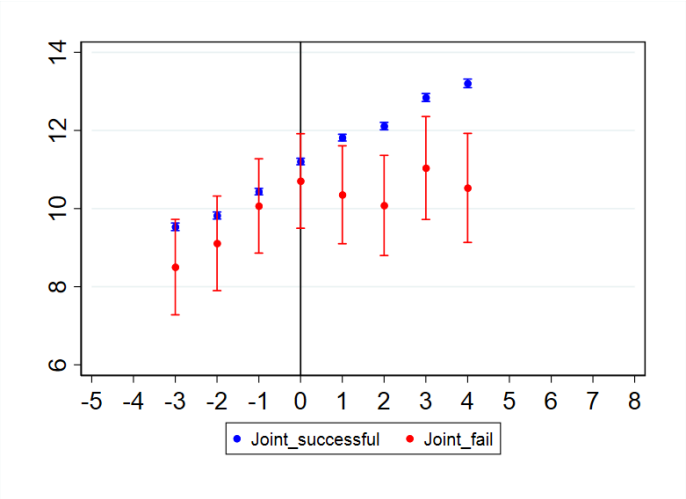


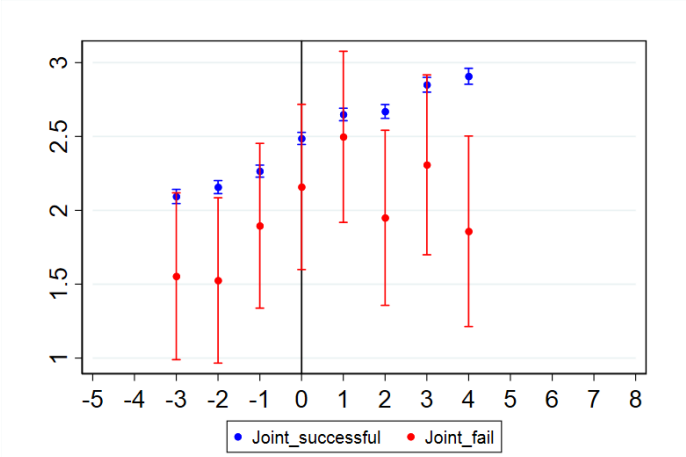
Figure 4: **Pre-trend graphs: Successful vs. failed mergers**

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). In all panels, we plot the mean and the standard deviation around the mean. 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

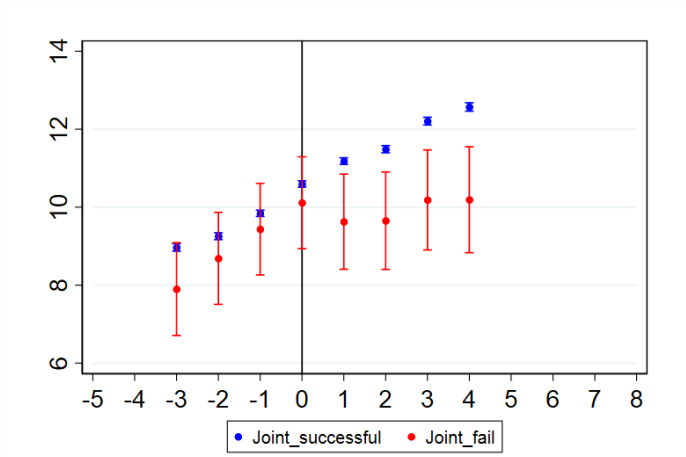


Table 1: **Summary statistics: main sample**

In this table, we report the mean and standard deviation of the main variables of interest in the paper. 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 binary variables, the mean and standard deviation are expressed in percent. There are 573,771 individuals included in the sample (as of $t = -1$) that work for 5,646 merging firms (3,236 unique mergers).

	Mean	SD
Pre-period		
Firms		
Number of employees (Acquirer)	255.146	898.509
Number of employees (Targets)	43.414	167.378
Profitability	9.440	49.619
Profit margin	-110.06	7,464.16
Sales per employee	3,182.67	10,500.28
Sick days per employee	4.315	8.240
Employees		
Age	39.099	13.705
Female	39.411	48.870
Foreign	17.454	38.957
Blue-collar	56.985	49.510
Composite mental illness	9.325	29.078
Mental illness diagnosis	2.055	14.147
Depression diagnosis	0.499	7.045
Anxiety diagnosis	0.512	7.135
Inpatient	5.954	23.663
Outpatient	31.488	46.447
Mental illness medication	8.762	28.275
Antidepressants medication	5.411	22.623
Antidepressants dosage	16.133	134.619
Post-period		
Firms		
Number of employees (Merged Firms)	330.612	957.785
Profitability	8.142	75.281
Profit margin	-34.521	2,132.71
Sales per employee	3,384.00	9,476.99
Sick days per employee	4.920	10.180
Employees		
Composite mental illness	12.013	32.511
Mental illness diagnosis	2.641	16.036
Depression diagnosis	0.595	7.693
Anxiety diagnosis	0.702	8.351
Inpatient	6.796	25.168
Outpatient	34.976	47.690
Mental illness medication	11.387	31.766
Antidepressants medication	7.032	25.569
Antidepressants dosage	23.813	195.110

Table 2: **The effect of mergers on mental health**

We study the effect of mergers on the mental health of workers using the following stacked OLS regression model: $y_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. y denotes one of the measures of mental health, described in detail in Table A2. We report the coefficient on the variable *Post*. Panel A reports coefficient estimates for the main mental health outcomes, while Panel B considers measures of psychiatric medication as dependent variables. Because the outcome variables (except for the variable *Antidepressants dosage* in Panel B) 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, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A

	(1) Composite mental illness	(2) Mental illness diagnosis	(3) Depression diagnosis	(4) Anxiety diagnosis	(5) Inpatient	(6) Outpatient
Post	0.244***	0.190***	0.037***	0.057***	0.376***	0.280**
	(0.060)	(0.030)	(0.014)	(0.016)	(0.049)	(0.110)
Observations	22,891,165	22,891,165	22,891,165	22,891,165	22,891,165	22,891,165
Number of clusters	5,646	5,646	5,646	5,646	5,646	5,646
Percentage change	2.6	9.3	7.5	11	6.3	0.89
Pre-period mean of dep. var.	9.325	2.055	0.499	0.512	5.954	31.488
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

Panel B

	(1) Mental illness medication	(2) Antidepressants medication	(3) Antidepressants dosage
Post	0.198***	0.176***	155.0***
	(0.059)	(0.047)	(25.700)
Observations	22,891,165	22,891,165	22,891,165
Number of clusters	5,646	5,646	5,646
Percentage change	2.3	3.2	9.6
Pre-period mean of dep. var.	8.762	5.411	1613.284
Age F.E.	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes

Table 3: **The effect of mergers on mental health: robustness tests**

We study the effect of mergers on the health of workers using the following stacked 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*. Because 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 while the other specifications employ sub-samples. 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 in-sample leverage distribution. Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Physical illness	Composite mental illness		
Post	0.007	0.230***	0.561*	0.344*
	(0.007)	(0.068)	(0.297)	(0.182)
Subsample	Full sample	Acquirers	Growth targets	Low leverage targets
Observations	22,891,165	17,593,529	792,157	2,879,250
Number of clusters	5,646	2,378	449	1,616
Percentage change	3.9	2.5	5.7	3.9
Pre-period mean of dep. var.	0.172	9.350	9.766	8.917
Age F.E.	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes

Table 4: **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 ($0 \leq t \leq 4$). There are 573,771 individuals in the sample of successful mergers, and 4,478 individuals in the sample of failed mergers (as of $t = -1$). There are 5,646 firms involved in successful mergers, of which 3,236 are target firms. There are 76 firms involved in failed mergers, of which 40 are target firms. Failed mergers are those where the target firm continues to exist as an independent entity in the Serrano database for more than 365 days after merger announcement. For detailed descriptions of the variables tabulated below, see Table A2.

	Successful merger		Failed merger	
	Mean	SD	Mean	SD
Pre-period				
Firms				
Number of employees (Acquirer)	267.353	957.831	150.086	261.228
Number of employees (Targets)	44.383	181.573	48.546	135.695
Employees				
Age	38.820	13.887	41.907	12.733
Female	40.003	48.990	36.713	48.208
Foreign	18.227	38.607	14.783	35.497
Blue-collar	57.442	49.443	34.658	47.593
Composite mental illness	9.620	29.486	8.909	28.489
Mental illness diagnosis	2.278	14.920	1.634	12.678
Depression diagnosis	0.527	7.243	0.420	6.469
Anxiety diagnosis	0.586	7.635	0.389	6.225
Inpatient	5.966	23.685	5.664	23.117
Outpatient	32.282	46.755	30.314	45.963
Mental illness medication	9.007	28.628	8.466	27.838
Antidepressants medication	5.512	22.821	5.042	21.882
Antidepressants dosage	16.984	137.937	14.858	84.339
Post-period				
Firms				
Number of employees (Merged Firms)	330.612	957.785	147.810	187.859
Employees				
Composite mental illness	12.293	32.836	10.531	30.696
Mental illness diagnosis	2.751	16.355	2.100	14.338
Depression diagnosis	0.609	7.777	0.574	7.555
Anxiety diagnosis	0.742	8.582	0.454	6.722
Inpatient	6.860	25.277	6.228	24.167
Outpatient	35.497	47.851	34.715	47.610
Mental illness medication	11.648	3.080	10.022	30.030
Antidepressants medication	7.199	25.847	6.113	23.957
Antidepressants dosage	24.719	200.895	18.386	93.747

Table 5: **Difference-in-differences analysis: Successful versus failed mergers**

This table reports the coefficient estimates from the following "non-stacked" 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 a measure of mental health (see Table A2 for detailed variable descriptions). We report the coefficient on the variable β_1. Panel A reports coefficient estimates for the main mental health outcomes, while Panel B considers measures of psychiatric medication as dependent variables. Because the outcome variables (except for the variable *Antidepressants dosage* in Panel B) 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 the Serrano database more than 365 days after merger announcement. Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the (planned) merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A

	(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

Panel B

	(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 6: **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 stacked OLS regression model: $\textit{Composite mental illness}_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. We report the coefficient on the variable *Post*. Because 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 in both years $t = 0, 1$. *Lower wage* is a binary variable that takes the value of one if the real wage (taking into account an annual inflation rate of 2%) at event times $t = 0, 1$ is lower than the wage at $t = -1$; it is zero otherwise. *Change task* is a dummy variable indicating whether the first digit of an individual's occupational code has changed between $t = 0$ and $t = 2$. *Change location* is a dummy variable indicating whether the county code for the establishment at which an individual works differs between $t = 0$ and $t = 2$. Specification (6) is similar to specification (1) except that stayers' are defined as workers who remain with the merged firm during the year of the merger and the three years thereafter. Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Composite mental illness					
Post	0.398***	-0.043	-0.043	0.386***	0.387***	0.263***
	(0.072)	(0.076)	(0.074)	(0.075)	(0.237)	(0.080)
Post × Lower wage		1.270***	1.270***			
		(0.135)	(0.134)			
Post × Change task				1.160***		
				(0.283)		
Post × Change location					0.166	
					(0.339)	
Subsample	Stayers	Stayers	Stayers (no sick days)	Stayers	Stayers	Stayers (3 years)
Observations	10,966,761	9,910,949	9,862,211	9,910,949	10,926,465	5,813,811
Number of clusters	3,638	3,418	3,416	3,418	3,517	2,704
Percentage change	4.6	13	13	13	1.6	3.3
Pre-merger mean of dep. var.	8.628	9.611	9.603	8.723	10.211	7.97
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 7: **Effect of mergers on worker mental health: firm characteristics**

We report the stacked 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*. Because 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 employ the following firm characteristics in the interaction with *Post*. *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, reported in parentheses next to the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). 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	0.182*	(0.105)	23	61
Relative size: Targets				
Small vs mid	0.041	(0.236)	10	37
Small vs large	0.165	(0.214)	10	37
Merger type				
Horizontal 2	0.126	(0.105)	42	54
Horizontal 3	0.141	(0.109)	38	47
Horizontal 5	0.195*	(0.109)	31	39

Table 8: **Effect of mergers on worker mental health: individual characteristics**

We report the coefficients from the following stacked 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 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. We employ the following worker characteristics in the interaction with *Post*. *Below median IQ (♂)* is a dummy variable equal to one if a man's IQ is strictly less than the median IQ across sample cohorts. *Below median 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. Note that data on cognitive and noncognitive scores stem from mandatory military conscription tests carried out for men in Sweden. These data are not available for women, non-Swedish citizens, and for younger cohorts; as a consequence, cognitive and noncognitive scores are available for 40% and 37% of all individuals in our sample, respectively. *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 first three digits of the SSYK (occupational) code of the individual is within the 400-999 range at $t = -1$ (see Tag, 2013). *Above med. tenure* is a dummy variable equal to one if an individual has above median tenure at the firm (measured at $t = -1$). *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. 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, reported in parentheses next to the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). 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 (♂)	0.181	(0.198)	15	78
Below median noncog. (♂)	0.510**	(0.226)	13	79
No higher educ.	0.368***	(0.088)	69	98
Blue-collar	0.385***	(0.083)	57	89
Above med. tenure	0.089	(0.096)	34	87
Demographics				
Female	0.572***	(0.102)	40	89
Foreign	-0.025	(0.109)	18	74
Age 18-30	0.112	(0.091)	30	86
Age 31-50	0.025	(0.076)	44	98
Age 51-	-0.232***	(0.089)	26	87

Table 9: **Profitability, mergers, and mental health**

This table reports coefficients from a stacked merger-level regression estimating the effects of mergers on profitability variables. *Return on assets* is EBITDA divided by assets; *Profit margin* is EBITDA divided by revenue; *Sales per employee* is revenue divided by the number of employees; *Sick days per employee* is the total number of sick days during the year divided by the number of employees. We compare post-merger outcomes for the merged firm to a blended target and acquirer measure pre-merger (in each year before the merger, we, e.g., sum EBITDA for target and acquirer and divide it by the sum of total assets for target and acquirer). We indicate large increases in mental health problems among the workforce after the merger using the dummy variable *Above median change share MI*. To construct this variable, we first calculate the share of workers that experience mental health problems in each firm and year (these are workers for whom the variable *Composite mental illness* takes the value of one); pre-merger, for each transaction, we pool the workers of future targets and acquirers. For each merger, we then average the annual share of workers with mental health problems for all pre-merger years and post-merger years, respectively, and calculate the change in this average share from before to after the merger. We rank transactions by the change in this share of workers affected by mental health problems. Finally, *Above median change share MI* takes the value of one if the increase in the share ranks above the sample median. The outcome variables are winsorized at the 1% and 99% levels. Standard errors, reported in parentheses below the coefficients, are clustered at the merger event level. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Return on assets	(2) Profit margin	(3) Sales per employee	(4) Sick days per employee
Post	0.036***	0.058***	2,981***	3.839***
	(0.008)	(0.009)	(318.5)	(0.178)
Post × Above median change share MI	-0.021**	-0.025***	-873.1**	1.979***
	(0.009)	(0.009)	(339.2)	(0.221)
Observations	270,745	270,745	270,745	270,745
Number of clusters	2,060	2,060	2,060	2,060
Pre-merger (average)	0.148	0.097	4,191	20.780
Merger F.E.	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes

Table 10: **The effect of mergers on the mental health of senior management**

This table reports the coefficient estimates from the following stacked OLS regression model: $Composite\ mental\ illness_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. ω_{FE} contains age, individual, and sector-year fixed effects. We report the coefficient on the variable *Post*. Because 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 variable (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 two different sub-samples of senior employees, where the employee type is measured at $t = 0$ (the year of the merger). We follow Tag (2013) and define members of the C-suite by SSYK occupational codes starting with 112 (CEOs), 121 (economic and financial managers), 131 (IT managers); members of senior staff are defined by occupational codes starting with 122 (staff and HR managers) or 123 (Administration and planning managers). Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

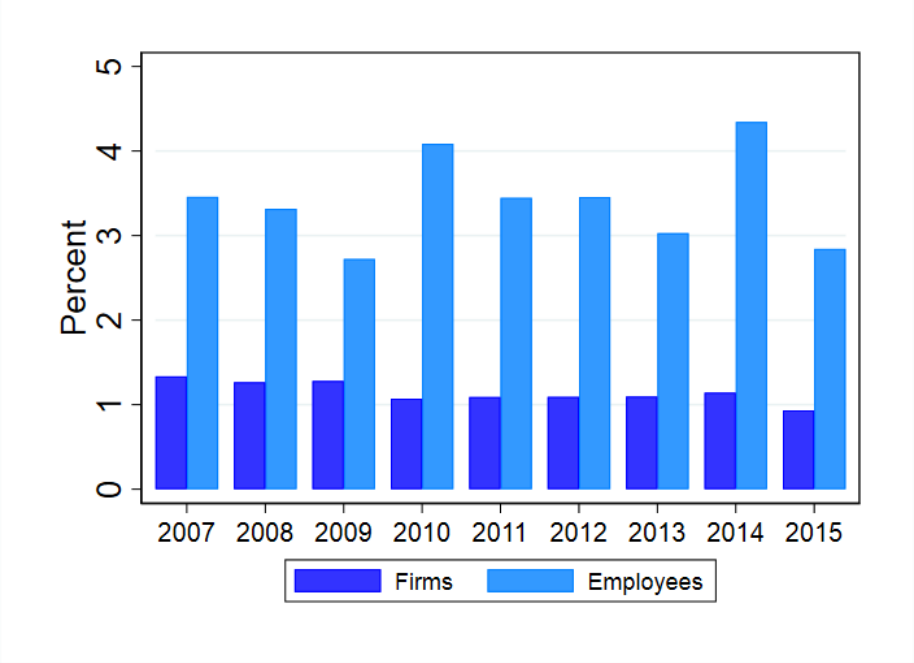
	(1)	(2)
	Composite mental illness	
Post	-1.000**	-0.004
	(0.432)	(0.207)
Subsample	C-suite	Senior staff
Observations	273,287	816,373
Number of clusters	2,734	2,605
Percentage change	-13	-0.057
Pre-merger mean of dep. var.	7.951	6.556
Age F.E.	Yes	Yes
Individual F.E.	Yes	Yes
Sector-year F.E.	Yes	Yes

A Online Appendix to:

"How Do Mergers Affect the Mental Health of Employees?"

Appendix Figure A1: **Prevalence of mergers**

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

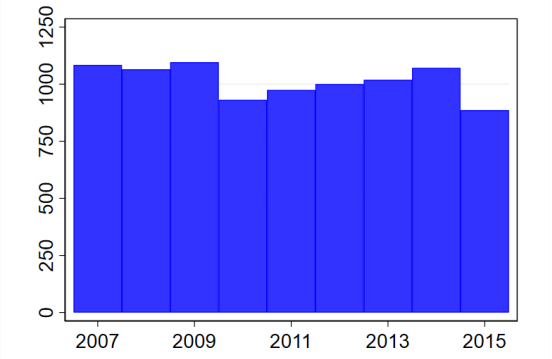


Appendix Figure A2: **Sample mergers versus all Swedish mergers**

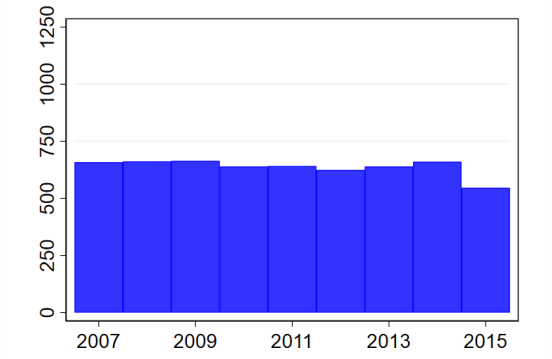
This figure compares the mergers included in our sample to all merger events in Sweden during the sample period. Panel (a) reports the number of mergers among limited liability firms in Sweden. Panel (b) shows the subset of such mergers where the acquirer and/or target firm has five or more employees 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 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

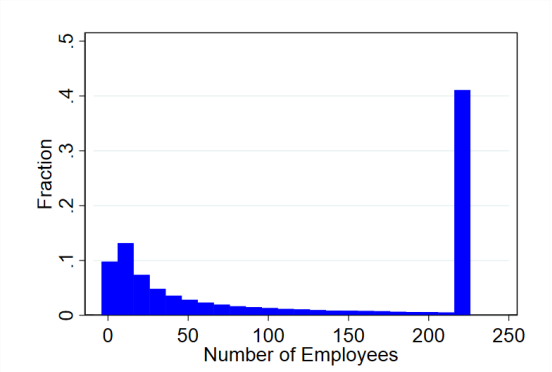


(b) Limited liability firms in the sample

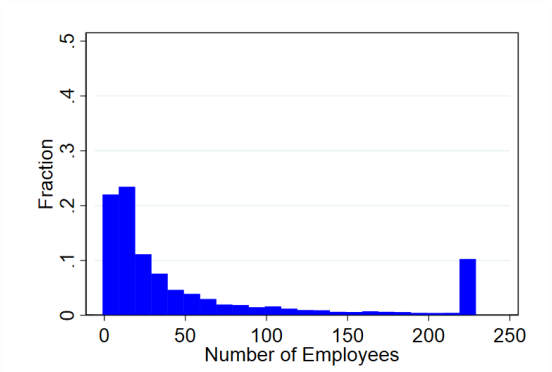


Employee distributions

(c) Employees at limited liability firms

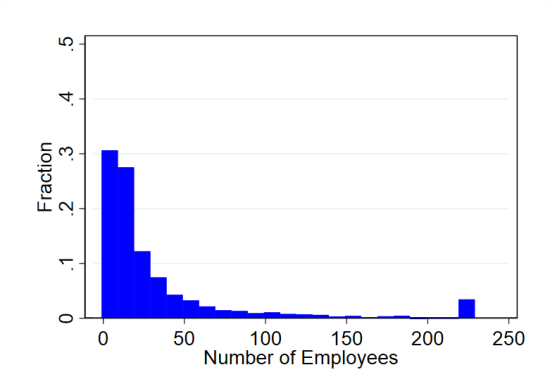


(d) Employees in sample firms

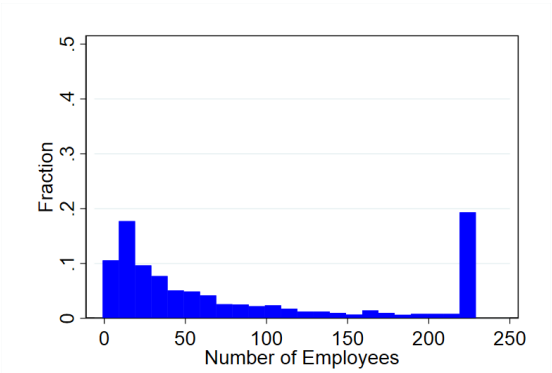


Employee distributions: target vs. acquirer

(e) Employees in target firms



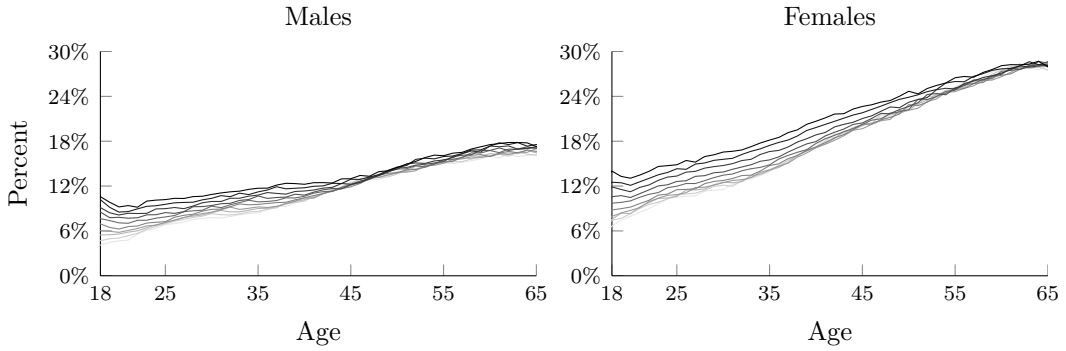
(f) Employees in acquirer firms



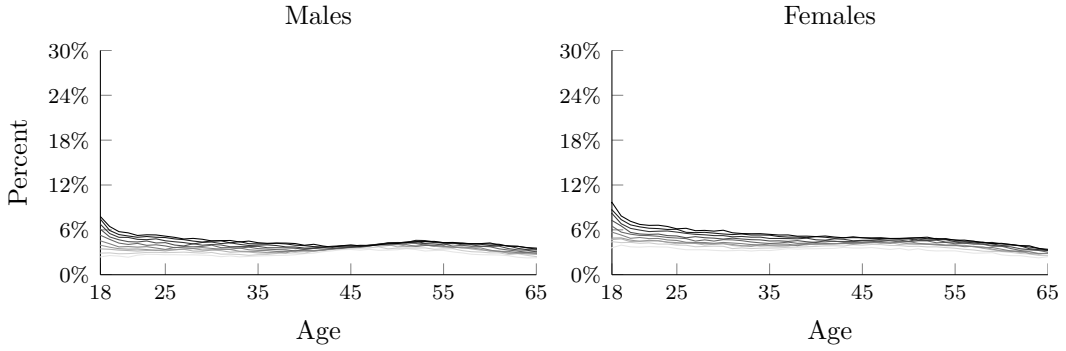
Appendix Figure A3: **Share of individuals with mental health problems in the general population**

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).

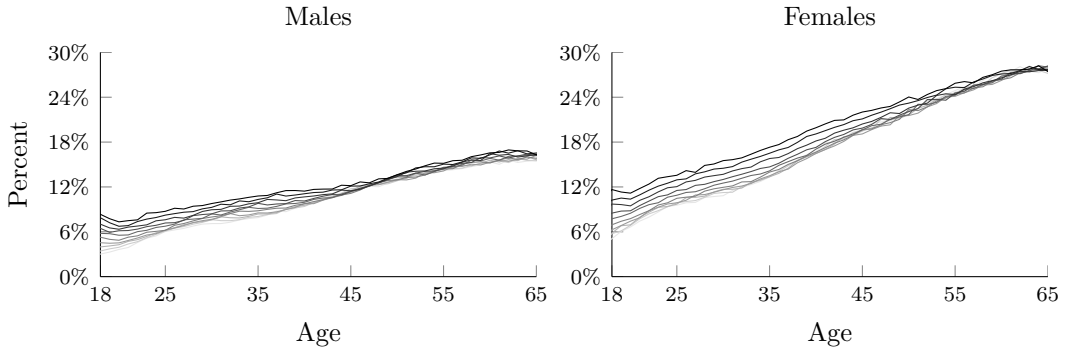
(a) Individuals diagnosed with a mental illness or with psychiatric medication intake

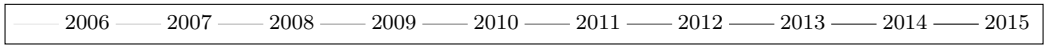


(b) Individuals diagnosed with a mental illness



(c) Individuals taking psychiatric medication





Appendix Table A1: **Depression definition**

The diagnosis codes reported below are used to define the variable *Depression diagnosis*. ICD-10 is the 10^{th} 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: **Definitions of variables**

This table reports definitions of variables used in the article, in alphabetic order. Panel A reports the definitions of dependent variables. Panel B reports definitions of variables used in cross-sectional tests. All indicator variables in Panel A are multiplied by 100 in the tests.

Panel A: Dependent variables

Variable	Definition	Registry / source
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* = 1) or has been prescribed drugs to treat a mental illness (*Mental illness medication* = 1).	NPR/NPD
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.	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.	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 A4).	NDR
Profit margin	EBITDA divided by sales	Serrano
Return on assets	EBITDA divided by assets	Serrano
Sales per employee	Revenue divided by the number of employees	Serrano
Sick days per employee	Number of sick days during the year divided by the number of employees	LISA, Serrano

Table 1 (continued)

Panel B: Variables used in cross-sectional tests

Variable	Definition	Registry / source
Above median change share MI	A dummy variable that takes the value of one if the increase in mental health problems (as measured by the variable *Composite mental illness*) among the merging firms' workers exceeds the sample median.	NPR/NPD
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 first three digits of the occupation code (SSYK) of the individual is within the 400-999 range at $t = -1$.	LISA
Change task	A dummy variable indicating whether an individual's occupational (SSYK) code has changed in its first digit between $t = 0$ and $t = 2$.	LISA
Change location	A dummy variable indicating whether the county code for the establishment at which an individual works differs between $t = 0$ and $t = 2$.	LISA
Female	A dummy variable that takes the value of one if the worker is 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 variable 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 $t = 1$.	LISA
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 $t = -1$.	LISA
No higher educ.	A dummy variable that takes the value of one if the individual has not attended higher education post high school.	LISA
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

Appendix Table A3: **The effect of joining firms on mental health**

This table reports ("non-stacked") OLS regression models analyzing the evolution of the mental health of workers who join firms outside of, that is, before, a merger event (Panel A) and those who join during the merger restructuring period (Panel B). Specifically, in Panel A, we focus on workers who joined target or acquirer firms in the five years prior to the merger; we compare their mental health status before and after joining with that of workers already employed at the target and acquirer during this period. In Panel B, we focus on workers who joined the merged firms in the five years after the merger; we compare their mental health status before and after joining with that of employees already at the merged firms who joined before the merger and stayed afterwards. We employ the following variables in our analysis. *Mental illness diagnosis* is 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). *Mental illness medication* is 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). *Composite mental illness* is a dummy variable that takes the value of one if an individual has either been diagnosed with a mental illness (variable *Mental illness diagnosis* is one) or has been prescribed drugs to treat a mental illness (*Mental illness medication* is one). *Antidepressants dosage* is the number of predefined daily doses of antidepressant medication. *Post join* is a dummy variable that takes the value of one after a worker joins a merging firm, and it is zero before joining the merging firm. Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm that an employee is joining. Statistical significance at 1%, 5%, and 10% is market with ***, **, and *, respectively.

Panel A: Workers joining firms in the pre-merger period (within five years prior to the merger)

	(1) Composite mental illness	(2) Mental illness diagnosis	(3) Mental illness medication	(4) Antidepressants dosage
Post join	-0.006***	-0.003***	-0.006***	-1.573***
	(0.001)	(0.001)	(0.001)	(0.335)
Observations	1,214,778	1,214,778	1,214,778	1,214,778
Percentage change	-6.7	-14	-6.9	-10
Mean of dep. var.	0.092	0.021	0.086	15.722
Age F.E.	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes
Sector - year F.E.	Yes	Yes	Yes	Yes

Panel B: Workers joining firms in the post-merger period (within five years after the merger)

	(1) Composite mental illness	(2) Mental illness diagnosis	(3) Mental illness medication	(4) Antidepressants dosage
Post join	0.002***	0.001***	0.002***	0.276
	(0.000)	(0.000)	(0.000)	(0.213)
Observations	3,790,790	3,790,790	3,790,790	3,790,790
Percentage change	1.9	5.6	2	1.5
Mean of dep. var.	0.101	0.023	0.095	18.043
Age F.E.	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes
Sector - year F.E.	Yes	Yes	Yes	Yes

Appendix Table A4: **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 10^{th} 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 sample period 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

Appendix Table A5: **"Placebo" regressions: the effect of mergers on the physical health of workers**

The regressions reported in this table study the effect of mergers on physical illnesses. The following stacked OLS regression model is estimated: $Physical\ illness_{it} = \beta \times Post_{it} + \omega_{FE} + \varepsilon_{it}$. ω_{FE} contains age, individual, and sector-year fixed effects. The dependent variables are various physical illnesses that are defined following Garcia-Gomez, Maug and Obernberger (2020). We report the coefficient on the variable *Post*. Because 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). Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm at which an employee worked in the year before the merger event (that is, target or acquirer firm). The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Diseases of the musculoskeletal system and connective tissue	(2) Osteopathies and chondropathies	(3) Diseases of the respiratory system	(4) Diseases of the eye and adnexa	(5) Diseases of the ear and mastoid process	(6) Diabetes mellitus	(7) Leukemia subset of: Malignant neoplasms
Post	0.001**	0.000**	-0.000	0.000	-0.000	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regression	Stacked	Stacked	Stacked	Stacked	Stacked	Stacked	Stacked
Sample	Successful mergers	Successful mergers	Successful mergers	Successful mergers	Successful mergers	Successful mergers	Successful mergers
Observations	22,891,165	22,891,165	22,891,165	22,891,165	22,891,165	22,891,165	22,891,165
Number of clusters	5,646	5,646	5,646	5,646	5,646	5,646	5,646
% Change	2.3	13	-.85	1.1	-1.3	4.1	8.2
Outcome pre-mean	0.045	0.002	0.020	0.018	0.009	0.008	0.000
Age F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix Table A6: **The merger wage premium**

This table reports regression models estimating the ex-ante wage premium for workers working in firms that merge in the future. The dependent variable is *Ln(Wage)*, the natural logarithm of the labor income obtained by an individual from the main employer in a given year. *Pre-merger* is a time-invariant dummy variable that takes the value of one for workers that will experience a merger during our sample period; it takes the value of zero for workers that never experience a merger during our sample period. *IQ* measures the cognitive skill of male workers; it takes values from one (low IQ) to nine (high IQ) on the Stanine scale. *Noncognitive skills* measures psychological abilities of workers (the score is also mapped onto the Stanine scale). In some specifications, we also employ the following firm-level variables which are winsorized at the 1% and 99% levels: *Ln(Assets)* is the natural logarithm of total firm assets; *ROA* is operating profit divided by total assets; *Leverage* is the ratio of total interest-bearing debt to assets; *Ln(Sales per employee)* is natural logarithm of net sales divided by number of employees. In the context of fixed effects employed, years of schooling denotes, for each individual, the number of scheduled schooling years required by an individual to obtain his/her highest earned degree, regardless of how many years it actually took the person to complete the degree (the latter information is unavailable): 12 years for a high school graduate, 15 years for an individual with a bachelor's degree, etc. Industry is categorized into 22 sectors. The sample covers the years 2006 - 2015; it includes all workers of limited liability firms with at least four workers that eventually merge (but excludes observations from the merger year onward) as well as workers from similar firms that never merge. Standard errors, reported in parentheses below the coefficients, are clustered at the level of the firm that an employee works at in a given year. Statistical significance at 1%, 5%, and 10% is market with ***, **, and *, respectively.

	(1)	(2)	(3)	(4)
	Ln(Wage)			
Pre-merger	0.130***	0.096***	0.062***	0.037***
	(0.019)	(0.010)	(0.009)	(0.009)
Female		-0.289***	-0.293***	-0.316***
		(0.004)	(0.003)	(0.009)
IQ				0.012***
				(0.001)
Noncognitive skills				0.042***
				(0.001)
Ln(Assets)			0.019***	0.016***
			(0.002)	(0.002)
Profitability			0.038*	0.035**
			(0.023)	(0.018)
Leverage			-0.171***	-0.157***
			(0.017)	(0.014)
Ln(Sales per employee)			0.075***	0.078***
			(0.006)	(0.006)
Observations	24,505,845	24,261,168	24,261,168	9,146,688
Year F.E.	Yes	No	No	No
Sector F.E.	Yes	No	No	No
Age - year F.E.	No	Yes	Yes	Yes
Sector - year F.E.	No	Yes	Yes	Yes
Years of schooling F.E.	No	Yes	Yes	Yes

Appendix Table A7: **Persistence of mental health problems in employees**

The regression model in this table is aimed at studying the persistence of mental health problems in individuals using a regression model with multiple lags of the dependent variable, *Composite mental illness*. The following regression model is estimated using the employees in our main sample: $Y_{i,t} = \beta_0 + \beta_1 Y_{i,t-1} + ... + \beta_6 Y_{i,t-6} + \epsilon_{i,t}$, where $Y_{i,t}$ represents mental health of employee i at time t, β represents the vector of coefficients listed in the table. Standard errors, reported in parentheses below the coefficients, are heteroskedasticity-consistent. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Composite mental illness
Composite mental illness ($t = -1$)	0.406***
	(0.002)
Composite mental illness ($t = -2$)	0.149***
	(0.002)
Composite mental illness ($t = -3$)	0.102***
	(0.002)
Composite mental illness ($t = -4$)	0.080***
	(0.002)
Composite mental illness ($t = -5$)	0.061***
	(0.002)
Composite mental illness ($t = -6$)	0.064***
	(0.002)
Number of employees	520,586
Number of observations	1,502,822

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