Corporate Financial Frictions and Employee Mental Health

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The research for this publication was partly funded by the Open Data Infrastructure for Social Science and Economic Innovations (ODISSEI) in the Netherlands (www.odissei-data.nl).

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Keywords: Financial crisis, financial constraints, credit supply, mental health, job insecurity, job loss, depression, anti-depressants

JEL Classifications: G01, G21, I12

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Abstract

This paper argues that corporate financial frictions can have an adverse effect on employee mental health, an important determinant of employee productivity. To identify the causal effects of financial frictions, we exploit variation in firms' need to refinance their long-term debt in 2008, a period when refinancing became more difficult due to the credit crunch. Using administrative microdata, we find that antidepressant use grows significantly more among employees of firms in higher need of debt refinancing. Much of this effect occurs at employees keeping their jobs, pointing to decreased perceptions of job security as a transmission channel.

1. Introduction

A growing literature documents that financial constraints amplify the adverse effects of economic shocks on firms' human capital. Giroud and Mueller (2017) provide evidence that high-leverage firms decreased their employment more during the Great Recession in response to local demand shocks. The authors argue that leverage may impair firms' ability to retain temporarily unnecessarily employees (labor hoarding), a practice that firms may otherwise find optimal in order to preserve human capital and avoid hiring/rehiring costs. Caggese, Cuñat and Metzger (2019) find that financial constraints prompt firms experiencing economic distress to implement sub-optimal dismissal policies, firing short-tenured workers with high future expected productivity. Baghai, Silva, Thell, and Vig (2016) document that firms lose workers with the highest cognitive and noncognitive skills due to financial distress as they approach bankruptcy, whereas Brown and Matsa (2016) show that financial distress can discourage talented job applicants.

In this paper, we document a novel cost of financial constraints on firms' human capital: we provide evidence that financial constraints can exacerbate the adverse effects of economic shocks on employee mental health. Employees' mental well-being should be a primary concern for any firm, given its role in employee productivity, absenteeism, and employee turnover (Bubonya et al., 2017; Burton et al., 2008; Duijts et al., 2007). We argue that financial constraints contribute to a greater probability of job loss, and that decreased job security may trigger mental health problems also for employees who manage to keep their jobs.

To study the effects of financial constraints on employee mental health, we must overcome two empirical challenges. First, we need to establish a quantitative measure of mental health. To do so, we exploit rich administrative data from the Netherlands, in particular a population-wide medicine use register, which records annual binary indicators of medicine use grouped by 4-digit ATC (Anatomical Therapeutic Chemical)

codes. As a measure of mental health, we focus on the use of antidepressants (ATC: N06A), drugs that are predominantly prescribed to treat serious mental illnesses, such as depressive disorders, anxiety disorders, or bipolar disorders (Gardarsdottir et al., 2007; Simon et al., 2014). Although antidepressant use does not cover the complete spectrum of mental health problems, especially milder conditions, general practitioners in the Netherlands frequently employ antidepressants as the first line of treatment for mental health complaints.[1] Furthermore, as the medically unjustified use of antidepressants is reported to be low (Piek et al., 2011), patients prescribed these medicines indeed suffer from mental problems.

The second empirical challenge is how to disentangle the effects of financial constraints from the effects of economic distress that make these constraints bind. As the papers cited in the introductory paragraph also highlight, the adverse effects of financial constraints on human capital are the most pronounced in bad economic times. Yet, during economic distress, variables that could serve to measure a firm's financial health (such as profitability or firm leverage) are also likely correlated with the firm's sensitivity to the economic shock, the firm's labor demand, and ultimately the mental health of its employees.

Therefore, instead of focusing on contemporaneous measures of financial health, we identify a balance sheet vulnerability that made firms more likely to be financially constrained during a subsequent economic shock. In particular, we exploit the unforeseen credit supply shock presented by the Global Financial Crisis and employ an empirical strategy motivated by Almeida, Campello, Laranjeira and Weisbenner (2011)[2]. We consider the long-term debt maturity structure of 352 large Dutch companies that employed over 330,000 people on the 1st of January 2008, and identify firms as financially constrained if they had to refinance a large part (minimally 25% in our baseline model) of their long-term debt outstanding in 2008 (we call these firms the *high-repayment* or *treated* firms*)*.

The underlying idea of this identification strategy is that firms that had to repay a larger share of their outstanding long-term debt in 2008 faced refinancing difficulties due to the credit crunch. We offer two pieces of evidence in support. First, bank lending is the main source of external financing for Dutch firms (Kalara and Zhang, 2018), and the Netherlands experienced a strong negative bank credit supply shock in 2007-2008 (Duchi and Elbourne, 2016). As Figure 1 reveals, almost all Dutch banks tightened their lending standards (for large firms) *in each quarter* starting from end-2007, which contributed to the slowdown of business lending observed from mid-2008 (DNB, 2009; van der Veer and Hoeberichts, 2016). In the last quarter of 2008, the net borrowing of Dutch firms turned significantly negative for the first time in many years (Figure 2). Second, there is direct survey evidence indicating that firms experienced a negative credit

[1] In 2010, 30% of patients with any psychological diagnoses were prescribed antidepressants (Nuijen et al., 2012).
[2] A similar methodology was applied in several recent papers (e.g. Benmelech et al., 2019; Carvalho, 2015; Duval et al., 2020)

supply shock: in 2009Q1, 21% of the Dutch companies reported the unavailability of bank lending as the most important crisis-related problem they faced (56% among those firms that reported any problems).[3]

[Insert Figures 1 and 2]

As the maturity profile of long-term debt is the cumulative outcome of hard-to-reverse decisions made several years prior to 2008, it is unlikely that the 2008 repayment share is correlated with the sensitivity of the firm to the economic downturn or other unobservable factors. This is particularly true because in our regression models we control for time-invariant employee unobservables (employee fixed effects) and we allow for different flexible time trends for firms with distinct pre-crisis characteristics (controls * year fixed effects). The included control variables (industry, firm size, cash ratio, long-term debt to assets, and cash flow) aim to pick up any systematic differences in firms' long-term debt maturity structure that might have also affected the firm's economic perspectives and personnel policies, and thus its employees' mental health, during the crisis.

The results from the regression models suggest a significant and persistent effect of the credit supply shock on employee mental health. People employed on 1 January 2008 by firms with at least 25% of their long-term debt maturing in 2008 faced a 0.44 pp (percentage points) higher average probability of antidepressant use in the 2008-2012 period, which is an economically significant 9% increase with respect to the 5% unconditional prevalence. The 9% increase in the probability of antidepressant use is comparable in magnitude to the 7.5% rise in antidepressant prescription volume due to a 20% decline in US housing prices between July 2006 and February 2009 as estimated by Lin, Ketcham, Rosenquist and Simon (2013).

These results are qualitatively robust to variations in control variables, restricting or broadening the sample of firms, altering the 25% refinancing cut-off, and using pre-regression matching to remove any imbalances between employees of treated and control firms. We also perform placebo tests to verify that our results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock) and that the relation between financial constraints and mental health does not apply in firms where financial constraints are not expected to be binding because of internal capital markets.

The estimated 0.44 pp increase in antidepressant use is a weighted average treatment effect on employees who left their job during the sample period (*leavers*) and on those who stayed in their jobs (*stayers*). Based on the literature, we argue that a main transmission channel from refinancing difficulties to

[3] COEN Business Survey Netherlands 2009Q1, administered by Statistics Netherlands. The sample consists of establishments with more than 5 employees; the average sample size of the COEN surveys is approximately 6,000 establishments. The crisis-related questions were first added in 2009Q1. The question of interest asks about the *most important* effect of the economic downturn that the respondent experiences (problems acquiring credit, problems attracting equity, losses on deposits, value loss of investments, increased debtor risk or problems saving surplus funds), 62% mentions that none of these effects are important (i.e. no important effects or important effects are unlisted). 21% mentions problems acquiring credit (22% for establishments with over 100 employees), 11% mentions increased debtor risk.

employee mental health is job loss for *leavers* and decreased job security for *stayers*. Financial constraints may negatively affect firms' labor demand (Benmelech et al., 2011; Chodorow-Reich, 2014; Giroud and Mueller, 2017; Huber, 2018; Popov and Rocholl, 2018), and the ensuing job loss can have an adverse effect on employees' mental health (Browning and Heinesen, 2012; Ganster and Rosen, 2013; Schaller and Stevens, 2015). However, decreased job security can damage employee mental health even in the absence of actual job loss (Burgard et al., 2009; Kim and Von Dem Knesebeck, 2015; Reichert and Tauchmann, 2011; Witte, 1999). Green (2011) concludes that for an employee of average employability the mental health effect of extreme job insecurity is similar to the effect of unemployment.

We do indeed find that employees of high-repayment firms had a 6.2 pp higher probability of job separation in the 2008-2010 period. Although this estimate does not distinguish between involuntary job loss and voluntary job separation, we provide two supplementary analyses that show that employees of high-repayment firms likely faced greater job insecurity: These employees were more likely (i) to be dismissed with a permit from the Dutch Employee Insurance Agency, and (ii) to experience job separation followed by a gap in employment. For the period prior to 2008, we show that employee turnover was similar in high-repayment firms and in control firms, and that treated and control employees in our sample experienced similar trends in labor force attachment.

Can a greater propensity of job loss in treated firms completely explain the increase in antidepressant use? We argue that this is not the case, and that *stayers* also suffered from deteriorating mental health. First, in a back-of-the-envelope calculation, we multiply the job loss estimates (with an upper bound of 6.2 pp) with the effects of job loss on depression/anxiety reported by Schaller and Stevens (2015) (1.6 pp). From this calculation, it is clear that the 0.44 pp overall increase in antidepressant use may be rather high to be explained by greater job loss alone. Second, we restrict our sample to employees who kept their jobs at least till the end of the year in which we measure antidepressant use. In this sub-sample, we still find that the probability of antidepressant use in treated firms was 0.28 pp higher in the 2008-2012 period.[4]

Finally, we study treatment heterogeneity among *stayers* to test whether job insecurity is indeed a driver for greater antidepressant use for these employees. Based on the economics and psychology literatures, we identify five personal/household characteristics that are expected to increase the mental health burden of job insecurity: older age, being male, living without a partner, having children in the household, and having a salary that constitutes a large share of total household income. When we interact our treatment indicator with these moderator characteristics, we find statistically significantly larger treatment effects for employees

[4] The group of employees who keep their job is a selected sample and selection is possibly endogenous to changes in mental health outcomes. For example, employees who stayed with financially constrained firms might be in general more resilient to job insecurity. These employees might have reacted more mildly to increasing job insecurity due to the economic downturn even in the absence of financial constraints, introducing a downward bias in our estimates of the financial constraints' effects on these employees' mental health. As we lack any good instruments for job separation, we cannot claim a causal interpretation for our results on the sub-sample of employees who keep their job.

without a partner, for those with children in their household, and for employees whose salary constitutes a large share of their total household income. Treatment effects appear to be larger for employees who are at least 45 years old, but the difference is not statistically significant at any conventional level, while male and female employees appear to be similarly affected by corporate financial stress. We also find that the relation between corporate financial frictions and antidepressant use is higher for employees with medium tenure, and for employees whose hourly wage is in the top quartile of hourly wages in their firms (a proxy for managerial employees). This latter group of managerial employees are potentially more aware of the financial and economic difficulties of their firms and may be involved in resolving them, both of which can generate additional mental stress. Finally, antidepressant use increases more for treated employees who retain their jobs but work in business units with a higher increase in job separations. Reasons could be that the departure of colleagues induces an enhanced perception of job insecurity, tensions consequential to the reorganization of the work, as well an increased burden of work pressure. Furthermore, the severing of collegial or friendship ties may reduce work satisfaction. Taken together, these results provide support to our hypothesis that greater job insecurity is driving increased antidepressant use among employees who do not lose their jobs.

This paper relates to three strands of literature in finance and economics. First of all, as cited in the introductory paragraph, a growing literature in finance studies the effects of financial constraints on firms' human capital. We combine firm-level financial data with rich employee-level data on antidepressant use to document a novel cost of financial constraints, their detrimental effect on employee mental health. We show that the mental health toll of financial constraints is not restricted to dismissed employees but it is also substantial for employees who stay with the firm. As argued above, the mental health of employees, particularly of those not dismissed, should be a prime concern of firms due to mental illnesses' burden on employee productivity.[5]

Another strand of literature related to our work studies the health effects of financial and economic crises. Several papers in this field report a negative correlation between unemployment rates and mental health status (e.g. Bradford and Lastrapes, 2009; Charles and DeCicca, 2008; Tefft, 2011). We also study how employment relations contributed to the mental health of employees during a crisis period, but contrary to the previous literature, we use employer-employee matched data to disentangle the mental health effects of the financial crisis (credit supply shock) from the effects of the ensuing economic crisis (the Great Recession). Furthermore, we show that crisis periods may have an adverse mental health effect even on employees who manage to keep their jobs but who may suffer from decreased perceptions of job security.

[5] Another related strand of the literature studies the interaction of firm financial events, such as takeovers and buyouts, and employee health. Bach, Baghai, Bos and Silva (2021) document that takeovers increase the incidence of stress, anxiety, depression, psychiatric medication usage, and even suicide among affected employees. Garcia-Gomez, Maug and Obernberger (2020) investigate the health effects of private equity buyouts. Although the authors find no evidence that buyouts worsen employees' health, they report that health characteristics strongly predict job loss after buyouts.

Finally, this paper also relates to the literature on the health effects of job displacement. Findings of this literature generally indicate a negative causal relation between job loss and mental health (Browning and Heinesen, 2012; Schaller and Stevens, 2015), although not unequivocally (Salm, 2009). The key difference between these papers and our work is that while the job displacement literature's main interest is the effect of job loss per se, we focus on the effects of a firm-level financial shock that may be propagated by job loss, among other channels. We argue that it is not possible to infer the mental health effects of the financial constraints that we study directly from the job displacement literature, most importantly because the majority of employees in financially constrained firms do not lose their jobs, yet they may suffer from workplace stress and increased job insecurity.

The rest of this paper is organized as follows. Section 2 describes the data and the institutional setting, and presents our empirical strategy. Section 3 documents the baseline results, financial constraints' effects on employees' antidepressant use. Section 4 studies a transmission channel, increased job insecurity, and presents evidence that the increase in antidepressant use is not restricted to employees losing their jobs. Section 5 presents robustness and placebo tests. Section 6 concludes.

2. Data, institutional setting, and empirical specification

We use administrative data from the Netherlands. Our dataset combines medicine use and employment data at the individual level with financial data at the corporate (employer) level. All administrative data are provided by Statistics Netherlands (SN), and separate databases are linked using unique (pseudonymized) identifiers at the individual or firm level. Appendix A provides details on the databases used (Table A.1.) and on variable definitions (Table A.2.).

2.1 Firm-level financial data

Under the data framework of Statistics Netherlands, the definition of a firm is hierarchical, whereby the *enterprise group* stands on top of the hierarchy and is considered the center of financial decision making. All corporate financial data are provided at the (consolidated) enterprise group level. An enterprise group consists of one or more *business units*, which are characterized by independent production decisions and the ability to offer their products to external parties, and comprise one or more legal entities (e.g., BVs - private limited liability companies) over which the enterprise group has majority control. An enterprise group in our sample consists of on average 5.5 business units, although 115 of the 352 enterprise groups only have a single business unit. Hereafter we use firm and enterprise group interchangeably.

Firm-level financial data is from the Annual Statistics of Finances of Large Enterprises (SFLE, in Dutch: *Statistiek Financiën van Grote Ondernemingen, SGFO*), which contains information on the consolidated balance sheets and income statements of the largest Dutch enterprise groups. In 2007, all

enterprise groups with at least EUR 23m in total assets were surveyed, amounting to a sample of 1204 firms. The scope of consolidation is the Netherlands; foreign subsidiaries of Dutch internationals and Dutch subsidiaries of foreign internationals are not consolidated. Financial data is presented by calendar year; only for a small share of companies does the financial year not coincide with the calendar year.[6]

In most of our regression specifications, we add (industry * year) fixed effects. We use the first two digits of the 1993 version of the Dutch industry classification codes (SBI), which aligns with the European NACE Rev.1 classification at the 4-digit level.[7]

2.2 Employee-level labor data

Information on employer-employee links is provided to SN by the Employee Insurance Agency (EIA or, in Dutch, *UWV*), an administrative authority responsible for implementing employee insurances and recording labor market data. From these data, SN creates the databases *BAANKENMERKENBUS,* which records qualitative job characteristics (e.g., the type of the job such as regular employment or internship, and the start and end dates of an employment relation), and *BAANSOMMENTAB,* which records quantitative job characteristics (such as salaries). We use the information in these databases to link employees to firms, to construct our sample (e.g., excluding interns), and to determine when employees separated from their initial job (using the unique employment relationship identifier – *baanid*).

2.3 Individual-level antidepressant use data

The Netherlands has a universal health care insurance system where taking out the basic health insurance is mandatory for all residents. Care consumers are free to choose among multiple nation-wide private health insurers who offer the same regulated basic insurance package for an annual premium of approximately 1000-1200 EUR (subsidies are available for low-income households). The package covers general practitioner (GP) care, maternity care, hospital care, home nursing care, pharmaceutical care, and mental healthcare, but does not cover for example dentistry or physical therapy which may be covered by supplementary insurance products. Care consumers must pay for their health consumption up to an annual deductible (EUR 150 in 2008 and EUR 385 as of 2016; the deductible can be voluntarily increased to lower

[6] We do not observe firms' financial years, but as per Bureau van Dijk Orbis data only 9% of the 900 firms in the Netherlands that meet our broadest sample selection criteria (Dutch-owned with at least EUR 23 million in assets) had their 2010 financial year ending *not* on the 31st of December. The financial year is labelled as the calendar year with which it has the most overlap (e.g. data of a financial year ending on 30 September 2019 are labelled as 2019 data).

[7] We use the codes provided in the General Business Register (in Dutch: *Algemeen Bedrijven Register* or *ABR*). The industry classification codes are registered at the Chamber of Commerce for each legal unit. In the General Business Register, SN provides a code at the business unit level by using the code of the legal unit within the business unit that has the most employees. Similar to this approach, we use the code of the business unit with the most employees within an enterprise group as the enterprise group-level code.

the insurance premium). Certain care products such as GP care and maternity care do not count towards the deductible.

The initial point of contact for most medical complaints is the general practitioner. All residents are registered with a local GP of their choice. GPs play a gatekeeper role; their referral is necessary for (non-urgent) hospital and specialist care. This holds for mental health problems as well; patients first approach their GP (or in rare cases a so-called first-line psychologist), who may refer them to the second-line specialist mental care in case of any serious problem. GPs frequently employ antidepressant medication as the first line of treatment for mental health complaints; in 2010, 30% of adult patients with *any* psychological diagnoses were prescribed antidepressants (Nuijen et al., 2012). Unjustified antidepressant use is reported to be low (Piek et al., 2011).

The individual-level medicine use database (*MEDICIJNTAB*) comprises annual binary indicators for the use of medicines that are reimbursed under the Dutch basic health insurance scheme. The indicators are grouped at the 4-digit ATC (Anatomical Therapeutic Chemical) level. Therefore, we observe if a person was reimbursed (any positive amount of) antidepressants (ATC-code N06A) in a particular year, but we do not observe the exact chemical substance (e.g., paroxetine, N06AB05) nor do we observe the exact amounts (e.g., defined daily doses, DDDs). As antidepressants are only available on prescription, and all antidepressants are reimbursed under the basic health insurance[8], the database gives a complete picture of antidepressant use.

2.4 Other databases

We use two additional databases provided by SN in the selection of our employee sample. First, we determine employees' age and gender using the Municipal Personal Records Database (or in Dutch: *Gemeentelijke Basis Administratie* or *GBA*). Second, we collect information on the position of each person in their household from the Income of People (in Dutch; *Integraal Persoonlijk Inkomen* or *IPI*) database.

In order to illustrate the strong (cross-sectional) correlation between worrying about job loss and antidepressant use, we also use answers to the question "Are you concerned of keeping your job?" from the National Labor Conditions Survey (in Dutch: *Nationale Enquête Arbeidsomstandigheden, NEA*), an annual survey on working conditions, accidents at work, work content, and industrial relations.

2.5 Attrition

[8] Reimbursements for medicines count towards the compulsory annual deductible. Some medicines are only partially reimbursed and a personal contribution must be paid. We could also have studied the use of anxiolytic drugs (N05B, such as benzodiazepines including alprazolam/Xanax); however, starting from 2009 these drugs are only reimbursed in rare cases and are consequently missing from the *MEDICIJNTAB* database. Furthermore, due to the side-effects of benzodiazepines, Dutch guidelines on pharmacological treatment of anxiety disorders recommend the use of antidepressants (Sonnenberg et al., 2012).

The administrative databases on medicine use and employment do not suffer from the attrition problems that surveys usually face (e.g., non-response). Yet, attrition might occur if someone leaves the Dutch population, for instance due to emigration or death. Using the Wealth of Households (*VEHTAB*) dataset, which lists all households and household members that belong to the Dutch population on the 1st of January of each year, we find that attrition is similar for treated and control employees.[9] Attrition affects the definition of our main outcome variable (antidepressant use): we assign a missing value to person-year observations where the given person was missing from *VEHTAB* (we do this because *MEDICIJNTAB* does not cover the antidepressant use of people who are not part of the Dutch population).

2.6 Sample composition

The starting point of our sample selection is the 1204 firms (enterprise groups) in the 2007 annual SFLE (Statistics of Finances of Large Enterprises). Because repayment obligations of local subsidiaries may have limited financial consequences as corporate groups can meet these obligations through their internal capital markets (e.g. Desai et al., 2004), we exclude Dutch subsidiaries of foreign internationals. We identify these subsidiaries as firms with more than 50% of the share capital owned by foreign-based companies or with an Ultimate Controlling Institutional Unit (UCI) located outside the Netherlands.

Following Almeida et al. (2011), we also exclude firms with a low long-term debt (excluding the current portion of long-term debt) to total assets ratio at the year-end of 2007. This is because our treatment classification aims to contrast firms with comparable debt profiles, for which long-term debt financing is a permanently important source of funds. In the baseline specification, we only consider firms with at least 10% of long-term debt to total assets. In robustness tests, we will vary this cut-off.

Finally, we exclude firms that operate in government-controlled and heavily regulated industries. These are government management (SBI code 7511), public transport via railway (6010), national post with universal service obligation (6411), and utilities (40-41). We further exclude outsourcing firms (74501, 74502) because we cannot observe the actual company where outsourced employees work. The resulting sample consists of 353 firms (enterprise groups).

We identify the business units of these enterprise groups on 1 January 2008 using the General Business Register (ABR). Subsequently, for the same date, we identify the people employed by each business unit. We restrict our sample to employees with a regular or on-call job contract[10] aged between 20 and 60

[9] Untabulated; by 2016 treated employees are 0.16% more likely to be in the Dutch population (t-stat 0.74).

[10] On-call employees only work when the employer calls them up, as they do not have fixed working hours. We exclude employees classified as interns and outsourced workers. We also exclude director-major shareholders, who are people with a considerable ownership in the firm they manage.

years in 2008[11], and who are the household head of their household or the partner of the household head[12]. The final sample consists of 328,229 employees. The steps of sample composition are presented in Table 1.

[Insert Table 1]

2.7 Treatment classification and summary statistics

We classify firms as "treated" or "control" based on the share of long-term debt that they were required to repay in 2008. Unlike most databases comprising European firms' financial data (e.g., Bureau van Dijk's Orbis), which report the current portion of long-term debt aggregated with all other current liabilities, the SFLE database reports these items separately. This is important because other current liabilities, such as short-term bank loans, may be correlated with the business outlook that the company faced preceding the crisis, and may thus fail to be exogenous to the outcomes we study. We calculate our "forcing" variable as

$$Share\ of\ current\ portion\ of LT\ debt = \frac{Current\ portion\ of\ LT\ debt}{Current\ portion\ of\ LT\ debt\ +\ Total\ LT\ debt}$$

where Total LT debt is the part of long-term debt maturing beyond one year.[13] In our baseline specification, we classify firms as treated if the "*Share of current portion of LT debt*" ratio is at least 25%. This results in 23 treated and 329 control firms. In robustness tests we will vary this cut-off point.

Table 2 presents summary statistics for treated and control firms (panel A) and for their employees included in our sample (panel B). The last three columns show a comparison between treated and control; the column "Raw Δ" presents the difference in means, the column "Adjusted Δ" presents the difference in means adjusted in a regression setting for industry fixed effects (and in Panel B also for the firm financial controls that we include in our baseline regressions: log total assets, liquid assets to total assets, long-term debt to total assets, and cash flow), and the last column presents the significance of the regression coefficient "Adjusted Δ" by means of a t-statistic.

[11] In the sample period, early retirement was widespread in the Netherlands with 80% of employees retiring before reaching the state pension age of 65, mostly at the age of 60. As labor market shocks have arguably limited impact on employees close to retirement, we exclude them.

[12] SN classifies people as either household head (person with the highest socio-economic position), partner (married or unmarried) of the household head, children of the household head, or other/unknown (e.g. children of the partner from a previous marriage). We only include people of the first two categories, excluding children and other/unknown household members because we aim to limit our sample to people for whom an employment shock (or a threat thereof) has high stakes.

[13] The SFLE database differentiates between five categories of long-term debt, (1) Debt to group companies, (2) Subordinated loans, (3) Bonds outstanding, (4) Loans from domestic financial institutions and (5) Other long-term debt (a residual category that includes, inter alia, loans from private individuals, derivatives, and lease obligations). Ideally, we would only consider bonds outstanding and bank loans (and the current portion thereof) because these financing forms are the hardest to renegotiate. However, the SFLE reports the current portion of all five debt categories combined. Although the scope of this problem is limited as bank loans constitute by far the largest share of long-term debt for most sample companies, in Table 8 we also execute a robustness test where we exclude firms with any long-term intercompany debt on their 2007 opening balance sheet (the type of long-term debt presumably the least binding).

[Insert Table 2]

As Panel A shows, treated firms (at the end of 2007) were slightly smaller in terms of total assets but larger in terms of number of employees (column Raw Δ). Treated firms had a somewhat lower cash ratio (liquid assets to total assets) and more long-term debt outstanding relative to their total assets, but they exhibited a more positive cash flow in 2007. As the "Adjusted Δ" and "t-stat" columns reveal, once controlling for industry composition, only this latter difference (and the difference in our forcing variable, share of current portion) is statistically significant at the 5% level. We control for any (remaining) difference in these variables in our regression models.

Turning to employee characteristics in Panel B, *antidepressant use*, our main outcome variable, is an annual binary indicator that takes the value 1 if a person was reimbursed for antidepressant medications in the given year (we multiply the indicator by 100 hence our results are in %). 4.1% of our sample used antidepressants in 2007, comprising 4.75% of treated employees and 3.99% of control employees. This difference practically disappears in the regression setting in column "Adjusted Δ". While a similar pre-treatment *level* of the dependent variable is not required for identification in a difference-in-differences setting, it is reassuring that the included control variables adequately explain any differences in treated and control employees' antidepressant use in 2007. Regarding other employee characteristics, treated and control employees have similar tenure, although treated employees are slightly younger and more likely to be female. Once controlling for industry fixed effects and the financial control variables, in column "Adjusted Δ", these difference in age and gender diminish and even reverse. In a robustness test we will also control for year effects interacted with these employee characteristics to account for any time trends that might depend on these characteristics (e.g., older employees might be more affected by the crisis).

While Table 2 presents the pre-treatment characteristics of treated and control employees, we present the mean of our binary outcome variables (antidepressant use, job separation) at the bottom of the respective regression tables.

2.8 Institutional detail on corporate information flows to employees

The validity of our research question hinges on employees who do not belong to the management being able to gain knowledge of the firm's (financial) situation and strategy. If the average employee is oblivious of the firm's financial health, corporate financial (di)stress cannot induce mental stress.

Although the Netherlands does not have a 'Mitbestimmung' (codetermination) corporate governance system as is the case in Germany (where half the supervisory board seats are reserved for union and employee representatives and employees can hence weigh on corporate decision making), employees in the Netherlands have strong rights to acquire corporate information and to be consulted on important corporate decisions. Furthermore, employees are also asked for approval in case of social corporate policies. The body to which corporate law grants these rights is the 'Ondernemingsraad' (OR), the Works Council. Every firm with at

least 50 employees is legally obliged to have an OR, and the employee representatives at the OR are elected by the employees. The number of OR members depends on firm size (for a small firm with 50 employees, the OR counts 5 members). The OR has several important rights: (i) Information rights – The law requires the management to provide the OR information on a range of financial/economic issues. On an ongoing basis, the management must provide information on the activities and financial results of the business, and on future prospects. The management is also obliged to give the OR copies of the annual report and accounts, including consolidated accounts of the group, and, where this is relevant, details of the specific results for the part of the business the works council covers (this would the case if the annual report only gives consolidated information). (ii) Consultation rights – Corporate law states that consultations with and representation of the employees are in the interests of the sound functioning of the enterprise in all its objectives. (iii) Social rights – The management is legally required to ask the approval of the OR when corporate decisions have social consequences. This would for example be the case when the corporation needs to be restructured, when changes in working conditions are to be introduced (e.g. more/less overtime, introduction of more labor flexibility, changes to a shift system, etc.)

The OR receives new information *timely* as the OR members meets once a month and every 2 months the OR meets the management (in about 20% of firms, the OR-management meets more frequently). At least twice a year, the OR and the management join for consultation meetings whereby the general strategy of the enterprise is to be discussed. More specifically, this includes decisions to attract large loans; to set up, take over, or sell other organizations; or to terminate some corporate activities. The information is dissipated to the employees through various information channels: e.g. every OR publishes a newsletter distributed to all employees that includes the agenda and the minutes of the OR meetings.

2.9 Empirical specification

Our empirical specification compares the time-trend of antidepressant use of employees of high-repayment firms (treated) and other sample employees (controls), accounting for employee fixed effects, industry-specific year effects, and year effects that depend on pre-crisis firm (or employee) characteristics. We estimate a linear probability model:

$$\text{Antidepressant Use}_{i,f,j,t} = \alpha_i + T_f\beta_t + \gamma_{j,t} + x'_{f,2007}\lambda_t + \epsilon_{i,f,j,t} \quad (1)$$

where $Antidepressent\ Use_{i,f,j,t}$ is a binary indicator variable capturing whether individual *i* who worked on 1 January 2008 for firm *f* belonging to industry *j* was reimbursed for any antidepressant use in year *t*, α_i are employee fixed effects, T_f is the treatment indicator that takes the value 1 for treated firms and 0 for control firms, $\gamma_{j,t}$ are (year * industry) fixed effects, and $x_{f,2007}$ is a 4-by-1 column vector of firm *f*'s 2007 financial characteristics, comprising log total assets, liquid assets to total assets, long-term debt to total assets,

and a measure of cash flow [=(net income plus depreciation and amortization) / total assets].[14] These financial characteristics are derived from Almeida et al. (2011), who argue that industry fixed effects and these financial characteristics[15] capture a lot of otherwise unobserved firm heterogeneity that is important both for the treatment classification (i.e., maturity structure of long-term debt) and firms' business conditions prior to and during the crisis. β_t are the differential year effects for the treated firms, our main coefficients of interest. We estimate Model (1) using data from 2006 to 2013, equivalent to 2 years prior to and 5 years following the 2008 financial crisis. Due to the presence of individual fixed effects we normalize β_{2007} to 0. We cluster standard errors at the firm (enterprise group) level because the treatment variation is at the firm level.

We quantify the average treatment effect over the 2008-2012[16] period using a difference-in-differences model:

$$\text{Antidepressant Use}_{i,f,j,t} = \alpha_i + T_f \; Post \; \beta + x'_{f,2007} \; Post \; \lambda + Post \; \gamma_j + \epsilon_{i,f,j,t} \qquad (2)$$

where $Post$ is an indicator for the post-treatment period (2008-2012), γ_j are industry fixed effects, $x_{f,2007}$ is a 4-by-1 column vector of the same 2007 firm financial characteristics as in Model (1), and consequently λ is a 4-by-1 column vector of coefficients. The included periods are 2006 to 2012.

In order to study treatment heterogeneity, we also use a version of Model (2) where we interact the treatment indicator with pre-treatment employee characteristics:

$$\begin{aligned}\text{Antidepressant Use}_{i,f,j,t} &= \alpha_i + \; z'_{i,2007} \; T_f \; Post \; \beta + T_f * Post \; \delta_1 + \; z'_{i,2007} * Post \; \delta_2 \; + \; Post \; \gamma_j \\ &+ x'_{f,2007} \; \text{Post} \; \lambda + \epsilon_{i,f,j,t}\end{aligned} \qquad (3)$$

where $z_{i,2007}$ is an n-by-1 column vector of 2007 employee characteristics such as age, gender or having a partner and β is an n-by-1 column vector of coefficients.

3. The effect of financial constraints on employee mental health

Figure 3 presents the estimated treatment effects (β_t) from Model (1). Employees of the treated firms, relative to employees of the control firms, experience an increase in antidepressant use starting from 2008; the treatment effect reaches its peak in 2011 and it is not statistically significantly different from zero in 2013 anymore.

[Insert Figure 3]

The relatively fast increase in antidepressant use in 2008 may reflect increased job insecurity (as discussed in Section 4), and is in line with the findings of Schaller and Stevens (2015) who document that

[14] In a robustness test (column 4 of Table 4) we also include in x a set of 2007 employee characteristics to account for the differences between treated and control employees reported in Panel B of Table 2.
[15] Almeida et al. (2011) further control for Tobin's Q and credit ratings, variables unavailable in our dataset.
[16] We opt to quantify the average treatment effect for the 2008-2012 period as the estimation of Model (1) will reveal that treatment effects in 2013 are not statistically significant anymore.

displaced US workers exhibit depression or anxiety within months after the loss of their jobs. The immediacy of the treatment effect is further supported by results from the psychology literature. Kendler et al. (1999) study 15 different stressful life events and find that 11 of them, including job loss, and financial or housing problems, are significantly associated with the onset of major depression *in the month of occurrence*. The onset of depression may have a swift effect on antidepressant use due to the prescription preferences of Dutch general practitioners: Van Marwijk, Bijl, Adèr, and De Haan (2001) report that in 1998 Dutch GPs prescribed antidepressants in 73% of the *first* consults for depressive symptoms.

The persistence of the treatment effect can be partially explained by the persistence of depression. Depression (medical term: major depressive disorder) is a lifelong illness that is categorized by recurrent depressive episodes. The majority of patients recover (i.e., are no longer symptomatic) within 12 months following a depressive episode; however, long-term recovery (lack of recurrence) is low, approximately 30% at a 6-year horizon, and almost 80% of patients experience at least one further episode in their lifetime. Furthermore, a large proportion (up to 27%) of patients never recover and develop chronic depressive illness (Malhi and Mann, 2018). The long-lasting nature of depression is also supported in our medicine use data, 57% of people in our sample who used antidepressants in 2006 continued to do so in 2012.

We also estimate treatment effects for 2006, to investigate "parallel trends" before the treatment. Ideally, we would present trends for multiple pre-treatment periods, but the medicine use database is only available from 2006. As Figure 3 illustrates, treated and control employees demonstrated a similar change in antidepressant use between 2006 and 2007, conditional on the control variables.

The coefficient estimates from Figure 3 and the corresponding standard errors are presented in Table 3. The table also shows the estimates on the control variables (interacted with the year indicators). Employees of firms with higher long-term debt to total assets ratio in 2007 exhibited a greater increase in antidepressant use during the crisis, whereas employees of larger firms exhibited a smaller increase. Both the 2007 cash ratio (liquid assets to total assets) and cash flow appear to diminish growth in antidepressant use, although these estimates are mostly not statistically significant.

[Insert Table 3]

Table 4 presents the average 2008-2012 treatment effects from Model (2), a difference-in-differences model. All specifications control for employee fixed effects. The baseline specification in column (1) further controls for (industry * year) fixed effects and year fixed effects that depend linearly on 2007 firm financial characteristics. Columns (2) to (4) present variations on these additional controls. Column (2) drops the (2007 firm financials * year) fixed effects, while column (3) defines industries at a coarser (sectoral) level instead of using 2-digit Dutch SBI93 industry codes. Finally, because the descriptive statistics in Table 2 show that treated and control employees exhibited some pre-treatment differences in age, salary, and tenure, column (4) includes the interaction of these characteristics with year dummies. All four specifications show

qualitatively similar results, with 2008-2012 treatment estimates ranging between 0.26 pp and 0.47 pp (a 5.4% to 9.7% effect relative to the 4.8% baseline probability of antidepressant use).

[Insert Table 4]

To better understand the magnitude of these treatment effects, we can compare them to estimates from the literature on the mental health effects of wealth and employment shocks. Our main result of a 5.4% to 9.4% relative increase in the probability of antidepressant use due to firm-level (re)financing difficulties is similar to the effect of the 2006-2009 US housing price shock (7.51% rise in antidepressant prescription volume) reported by Ketcham, Rosenquist and Simon (2013), but smaller than the effect of job loss (22% increase in the probability of depression/anxiety) calculated by Schaller and Stevens (2015) or the effect of losing on average USD 220,000 during the October 2008 market crash (35% relative increase in the probability of antidepressant use) estimated by McInerney, Mellor and Nicholas (2013). This benchmarking exercise shows that firm-level (re)financing difficulties had a serious impact on employee mental health, although not directly comparable to the effects of job loss.

4. The transmission channel of job insecurity

4.1 Increased job insecurity of treated employees

How can firm-level refinancing difficulties lead to an increase in employees' antidepressant use? The estimated 0.44 pp increase in antidepressant use is a weighted average treatment effect on employees who left their job during the sample period (*leavers*) and on those who stayed in their jobs (*stayers*). We argue that an important transmission channel from refinancing difficulties to employee mental health is job loss for *leavers* and decreased job security for *stayers*.

Previous work demonstrated that financial constraints can negatively affect firms' labor demand. Chodorow-Reich (2014) finds evidence, among 2,000 non-financial US firms, that companies reduced employment more in the 2008-2009 period if they had pre-crisis relationships with banks that were in a less healthy condition during the financial crisis. Huber (2018) shows that German firms fully dependent on Commerzbank, a bank severely affected by the 2008 financial crisis, reduced their employment on average by 5.3% between 2009 and 2012 compared to firms with no Commerzbank relationship. Giroud and Mueller (2017) report that employment in more highly levered US firms was more sensitive to declines in local consumer demand during the Great Recession. Giroud and Mueller argue that financing constraints may dampen labor demand by impairing firms' ability to engage in *labor hoarding*, a practice of retaining temporarily unnecessary employees to preserve firm-specific human capital and to avoid firing/re-hiring costs.

During the first crisis years, labor hoarding was widespread in the Netherlands; indeed, several studies credit to this phenomenon the relatively mild increase in Dutch unemployment rates between 2008 and 2010 (e.g. van den Berge et al., 2014). Nonetheless, as highlighted by Giroud and Mueller (2017), labor

hoarding requires financial resources, which are scarcer for financially constrained firms. In the Netherlands, financial resources are particularly important for labor hoarding due to the inflexible employment terms regarding both working hours and wages. Over 80% of Dutch employees are covered by collective labor agreements (CLAs), which largely prevent companies from adjusting nominal wages downwards. Adjustments in the number of working hours are also not straightforward to implement because Dutch CLAs, unlike for instance German ones, do not contain provisions for temporary shorter working hours (Tijdens et al., 2014).

Given these observations, we hypothesize that firms that had to repay a larger share of their long-term debt in 2008 had relatively fewer resources to engage in labor hoarding, and consequently employees of these firms suffered from decreased job security. The adverse mental health effects of job loss are well-documented (Browning and Heinesen, 2012; Ganster and Rosen, 2013; Schaller and Stevens, 2015), which could explain treatment effects on *leavers*. However, decreased job security can damage employee mental health even in the absence of actual job loss (Burgard et al., 2009; Kim and Von Dem Knesebeck, 2015; Reichert and Tauchmann, 2011; Witte, 1999).[17] Green (2011), for instance, concludes that for an employee of average employability the mental health effect of extreme job insecurity is similar to the effect of unemployment. Therefore, decreased job security could also explain treatment effects on *stayers*.

In line with this hypothesis, we find evidence that employees in treated firms were more likely to separate from their job during the crisis period. Panel A of Table 5 presents the treatment effects on the (cumulative) probability of job separation. We assume that an employment relationship ended in a given year ("job separation") if the job is not registered anymore in the "Quantitative characteristics of employment relationships" database (*BAANSOMMENTAB*) in the following year. We only consider the initial employment relationships that existed on 1 January 2008. The results in columns (1), (2) and (3) show that treated employees had a 4.6, 6.7 and 6.2 pp higher probability of job separation by the end of 2008, 2009 and 2010, respectively. This is an economically significant increase compared to the unconditional means of job separation, for example 30% by end-2010. Job separation is statistically significantly lower in larger firms, whereas it appears to be larger in firms with higher end-2007 long-term debt to total assets ratio and cash ratio. In columns (4) to (6) we further control for the same pre-treatment employee characteristics that we also controlled for in column (4) of Table 4. After adding these additional controls, the treatment effects increase slightly with treated employees facing a 7.2 pp greater probability of having separated from their job by the end of 2010. Age and tenure statistically significantly decrease the probability of job separation.

[Insert Table 5]

[17] We also illustrate this negative association between job insecurity and mental health in the Netherlands, using the National Working Conditions Survey. Although we cannot establish causality, Table B.1 in Appendix B shows that employees answering "yes" to the question "Are you concerned of keeping your job?" are substantially (~2 pp or 44% relative to the 4.5% baseline) more likely to use antidepressants, even after controlling for a broad range of personal and firm characteristics.

Job separation, the dependent variable in Panel A of Table 5, does not differentiate between voluntary and involuntary departures. Arguably, involuntary job separation would be a stronger indicator of decreased job insecurity, although in practice employees might also quit 'voluntarily' when they face a poor work environment. In Panels B and C, we perform two additional analyses that suggest that the increased rate of job separation among employees of treated firms is at least partly due to dismissals. In Panel B, we study the treatment effects of dismissal for economic reasons with the permit granted to an employer by the Employee Insurance Agency (UWV). In the Netherlands, employers have multiple legal possibilities to dismiss employees for economic reasons, the three main ways being: (i) by mutual consent, (ii) with a dismissal permit from the UWV, and (iii) with the permit of the sub-district court. Based on the study of Hoevenagel and Engelen (2013) on dismissal routes in the Netherlands, we estimate that dismissals via a UWV permit capture around one half of all dismissals for economic reasons. The estimates in Panel B show that employees in treated firms faced an increased probability of being dismissed for economic reasons with an UWV permit in the 2008-2010 period. As UWV dismissals only represent a part of all dismissals for economic reasons, these numbers likely provide a lower bound for the treatment effects on such dismissals.

In Panel C, we define involuntary job separation as any separation where there is a gap between the end date of the terminating contract and the start date of any new employment contract of the given person. Although this definition might capture some voluntary departures as well, we find that it is a strong predictor of receiving any unemployment benefits in the year of job separation.[18] The estimates in Panel C also suggest that employees in treated firms suffered from greater involuntary job separation.

Finally, we address two potential concerns to the estimated treatment effects on job separation. First, it might be that treated firms in general have higher employee turnover, even after controlling for industry fixed effects and 2007 firm characteristics. To address this issue, we study the 2005-2007 job separation rate of *2005* employees of the treated and control firms. First, we match the 352 firms in our sample to their employees on 1 January 2005.[19] We then estimate three regressions for (cumulative) job separation up until end-2005, end-2006, and end-2007 using the same controls as in columns (1)-(3) of Table 5 (industry fixed effects and firm financial characteristics, but measured in 2007). Table B.2 of Appendix B presents the results from this placebo test. If anything, the employees of treated firms in 2005 were less likely to be separated from their jobs than employees of control firms, although the estimates are economically and statistically close to zero.

[18] Among the about one million 20- to 60-year-old individuals with a job separation in 2011, about 60% had a job separation with a gap in employment. 31.5% of these individuals received unemployment benefits vs. only 9% among those who had job separation without a gap in employment.

[19] We can match 325 of the 352 enterprise groups to their business units and employees on 1 January 2005. Out of the 27 non-matched enterprise groups, 24 could not be matched because their identifier in the General Business Register changed between 2005 and 2008 due to restructuring, split, re-starting or mergers. Two enterprise groups were created in the period, and we find no information on one remaining enterprise group.

Second, it might also be possible that employees with a generally weaker labor force attachment select into the treated firms, for any reason. The placebo test discussed above cannot address this potential concern as the employees in our sample, by definition, are with their firm on the 1st of January 2008. To establish that treated and control employees exhibited parallel trends in labor force attachment before the crisis, we study the differences in the (log of) the annual calendar days worked (summed across all employers) for the two groups. Working fewer calendar days may indicate that an employee is less attached to the labor market (has gaps in employment) or that he or she had suffered from job loss (which often leads to gaps in employment). We regress the log of the annual calendar days worked on the same controls, firm characteristics interacted with year fixed effects and employee fixed effects, as in our baseline antidepressant regression. The results, presented in Figure B.1 of Appendix B, reveal that there were no differential trends in the labor force attachment of treated and control employees before the crisis. In contrast, starting from 2008, treated employees experienced a drop of about 2 to 3% in the number of calendar days worked. This latter result is in line with the increasing job separation in treated firms during the crisis that we document above.

The fact that treated and control employees show parallel trends in the number of calendar days worked before the crisis is also supportive of the parallel trends assumption for our main outcome variable, antidepressant use. This is because unemployment and job loss are two of the most important employment-related causes of mental health problems (e.g., Schaller and Stevens 2015, Tefft 2011).

In summary, the results in Table 5 and the additional analyses in Table B.2 and Figure B.1 provide evidence that while the job security of treated and control employees was similar prior to the crisis, during the crisis employees in treated firms experienced increasing job insecurity.

4.2 Increased antidepressant use of employees who kept their jobs

Can the greater propensity of job loss and its negative effects on employees who lost their jobs fully explain the deteriorating mental health of employees of firms with refinancing difficulties? We argue that this is not the case, and that employees who managed to keep their jobs also suffered from an increased prevalence of mental health problems.

First, in a back-of-the-envelope calculation, we multiply the job loss estimates (with an upper bound of 6.2 pp) in Table 5 with the effects of job loss on self-reported depression/anxiety estimated by Schaller and Stevens (2015) (1.6 pp). The resulting treatment effect (6.2 pp * 1.6 pp = 0.1 pp) is clearly smaller than the 0.44 pp overall increase in antidepressant use we estimate in Table 4. This suggests that it is not merely mental illness caused by job loss that is driving our results.

Next, in Table 6 we restrict our sample to employees who kept their jobs at least till the end of the year in which we measure antidepressant use. In the first seven columns we repeat the analysis of Table 3 for this restricted sample of employees, while in the last column we estimate the average 2008-2012 treatment effect as in column (1) of Table 4. We observe a similar trend in treatment effects as for the complete sample,

the excess probability of antidepressant use rapidly increases in 2008 and remains statistically significant until 2013. The average 2008-2012 treatment effect amounts to 0.28 pp, or 6.5% compared to the unconditional mean antidepressant use of 4.3% in this sample. Although we cannot interpret these results causally due to the possibly endogenous nature of job separation, discussed in footnote 4, they support a negative effect of refinancing constraints on the mental health of employees who kept their jobs.

[Insert Table 6]

Finally, we study the moderating effect of personal, household, and employment characteristics that are expected to increase the effects of firm-level refinancing difficulties on experienced job insecurity and the effects of job insecurity on employee mental health. We consider eight moderating characteristics: age, gender, partnership status, having children in the household, the share of salary in total household income, job tenure, wage, and job loss experienced by peer employees. Personal characteristics (age and gender) may influence re-employability upon job loss. In the Netherlands, Deelen et al. (2018) show that, following a dismissal, older (ages 45-54 in their sample) men are more negatively affected in terms of reemployment probability than either prime-age men (age 35-44) or older women. The mental health effects of unemployment and job loss also appear to be stronger for men than for women (e.g., Kuhn et al., 2009; Paul and Moser, 2009). This suggests that job insecurity may be more stressful for older and male employees. Having no partner may represent a lack of a familial support and could increase the risk of developing mental illness (Teo, Choi and Valenstein, 2013), whereas having (a) child(ren) could indicate that job loss is more consequential due to a higher number of dependents. Earning a salary that represents a greater share of the total household income (conditional on having a partner or not) may imply a more detrimental effect of an eventual job loss on the family budget. Indeed, Marcus (2013) suggests that the mental health effects of job loss are worse if the dismissed employee had a higher pre-dismissal share of household income. The potential moderating role of tenure is motivated by Caggese et al. (2019), who find that financially constrained firms may find it optimal to dismiss short-tenured employees when facing economic distress. Regarding wage, we consider an indicator that takes the value of 1 if an employee's hourly wage is in the top quartile of all hourly wages in the firm. The idea here is to create a proxy for managerial employees, who are likely more aware of the financial and economic circumstances of the firm.[20] Finally, we also consider the job insecurity experienced by the peers of *stayer* employees. We calculate the difference in job separation rates (as defined in Panel A of Table 5) in the business unit of the employee between 2005-2007 and 2008-2010. We expect stronger adverse effects on the mental health of employees who work in business units that experienced a greater increase in job separation rates.

[20] Non-managerial employees can also learn about their employer's financial and economic circumstances, either by observing the outcomes of their peer employees (e.g., dismissals) or from information shared by the employee representatives. Firms in the Netherlands that employ at least 50 employees are required to establish a 'Works Council', an employee representative body, which has extensive information and consultation rights.

Table 7 shows the moderating effects of the above characteristics on the 2008-2012 average treatment effect of antidepressant use, based on Model (3). The table presents the coefficient estimates of the triple interactions Post*Treated*Characteristic and of the double interactions Post*Treated and Post*Characteristic. Columns (1) to (8) show results from eight separate univariate specifications where we interact Post*Treated with a single characteristic measured pre-treatment (during 2007 or on 1 January 2008), while Column (9) presents results from a model where all the eight triple interactions are included. As the results reveal, treatment effects are larger both in the univariate and in the multivariate regressions for employees who have at least one child in their households (column 2), whose salary constitutes a large share of their total household income (column 4), who have 5 to 10 years of tenure[21] (column 6), and who work in a business unit where job separation increased more during the crisis (column 8). Employees without a partner (column 1) and those with an hourly wage in the top quartile (column 7) also appear to be more affected, although these differences are only statistically significant in the multivariate and the univariate specification, respectively. Finally, we do not find differences in treatment effects for employees of different age and gender.[22]

Taken together, these treatment heterogeneity estimates provide suggestive evidence that job insecurity may be a potential factor behind the increase in antidepressant use for employees who did not lose their jobs. On the other hand, the causal interpretation of these treatment heterogeneity tests needs to be made cautiously as other (omitted) moderators might drive some of the results. It is possible that other factors related to stress at work, and not related to job separations, are responsible for the adverse effects on mental health. For example, Popov (2014) shows that credit constraints are associated with significantly lower investment in on-the-job training, which could also result in lower productivity and mental strain.

In summary, our results suggest that the adverse mental health effects of refinancing constraints are present both for employees who lose their jobs due to these constraints and for employees who manage to

[21] The estimated treatment effect heterogeneity in tenure merits further discussion as it is slightly different from what we would hypothesize based on Caggese et al. (2019). Although we do find that employees with more than 10 years of tenure are less affected than employees with 5 to 10 years of tenure, it is employees with less than 5 years of tenure whose mental health is the least affected. A potential explanation is that although short-tenured employees do indeed face a higher risk of job loss (in untabulated regressions we do find evidence of this), they already internalize this risk, e.g., because they work in fixed-term contracts, and their mental health is less sensitive to increasing job insecurity.

[22] We also study treatment heterogeneity in education. Higher-educated employees may face better prospects of re-employability; they may in general serve in different positions (e.g., more likely in managerial roles) than lower-educated employees. We have information on education for a subset of the employees in our sample (about 16,000 employees out of the total 330,000) from the GEMON 2012 survey. We code if someone is college educated by means of a binary variable, and interact this variable with the Post*Treated indicator. Among stayers, higher educated employees appear to be less affected by the debt refinancing shock, but this difference is not statistically significant. We also study treatment heterogeneity in medicine use: we create an indicator variable capturing if a person has used any medicines (apart from antidepressants) in 2007, and interact it with our treatment variable. Our results show that employees who were in worse physical health (as proxied by the use of medication other than antidepressants) were more affected by the treatment, but this difference is not statistically significant.

keep their jobs albeit possibly suffer from greater job insecurity. From the firm's perspective the mental illness of non-dismissed employees is of particular importance as it may negatively affect firm productivity. Therefore, our results illustrate that the mental health costs of financial constraints are not restricted to dismissed employees but also affect the firm.

[Insert Table 7]

5. Robustness and placebo tests

There are several assumptions regarding treatment specification and sample selection that underpin our results of a greater post-2007 increase of antidepressant use in high-repayment share firms. In this section, we present estimates where we relax/alter these assumptions. We also perform placebo tests to verify that our results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock) and to assert that the treatment effect does not apply in firms where the repayment share is not expected to be binding because of internal capital markets. We also address possible ex ante sorting by employees in control and treated firms prior to the 2008 shock. In addition, we show evidence that the negative effects of financial difficulties on employee mental health also hold in a much larger sample where we proxy for financial difficulties by means of a high leverage ratio. Finally, as Collective Labor Agreements (CLAs) may be an additional source of uncertainty, we control for renegotiations of CLAs in the treatment period.

5.1. Sample selection criteria and the definition of treated firms

We present robustness tests based on changes in the sample selection criteria (Table 8). Column (1) presents the baseline estimate of the 2008-2012 average treatment effect on antidepressant use from Table 4. Column (2) excludes firms with any long-term debt resulting from intra-group loans on their opening 2007 balance sheet. Ideally, we would restrict our 2008 repayment share variable to the repayment of long-term debt that is most probably binding and hard-to-renegotiate, such as bank loans and bonds. Due to data limitations this is not possible, but excluding firms with intercompany loans would alleviate concerns that our repayment share variable picks up non-binding repayment obligations within the group. The point estimate from column (2) is very close to the baseline estimate, although the sample size decreases.

Columns (3) to (5) investigate alterations on the long-term debt to total assets selection criterion to capture the degree to which firms rely on long-term debt. When we include firms with lower long-term debt to total assets ratios (columns (3) and (4)), the treatment effects become smaller, while restricting the sample to firms with at least 15% long-term debt to total assets increases the estimate. This is as expected, as the size of the refinancing shock is arguably proportional to the share of long-term debt on the balance sheet.

Column (6) retains all industries (i.e., the state-controlled and heavily regulated industries such as utilities are not excluded). Adding firms that belong to regulated industries or form part of the state

administration yields a slightly lower (0.34 pp vs 0.44 pp) 2008-2012 treatment estimate, indicating that for such firms the refinancing problems may be more easily addressed without repercussions on the job security of the employees.

Finally, in columns (7) and (8), we re-estimate the treatment effects after controlling for a firm size effect. We exclude the largest 5% and 10% of firms, respectively, where we measure size as the number of employees in the sample. Excluding these large firms yields almost identical treatment effects, indicating that the results are not driven by a handful of the largest firms.

[Insert Table 8]

Next, we turn to alternative thresholds for the current portion of long-term debt to define treated and control firms. Table 9 repeats the analyses of Table 3 and column (1) of Table 4, and presents the estimated treatment effects on antidepressant use when we use a lower (20%) or higher (30%) cut-off value. The effects for a higher (30%) cut-off, which results in only 10 treated firms, are slightly larger than the baseline estimates. If we choose a lower cut-off value (20%), our estimation results are expectedly attenuated due to the fact that the refinancing (di)stress may be somewhat lower.[23]

[Insert Table 9]

Finally, we address a set of additional potential concerns (stratifying replacement shares, industry effects, eliminating very low repayment share benchmarks, repayment share definitions), and discuss the results (tables are not shown for reasons of parsimoniousness but are available upon request).

First, we study the treatment effects using two non-overlapping treatment indicators: having a repayment share between 25 and 30% and a repayment share over 30%. In both specifications, control firms are those with a lower than 25% repayment share (as in the above analysis). Our results in Table B.3 of Appendix B show that both of these groups of treated firms have experienced increasing antidepressant use in the 2008-2012 period, and thus that the results are not confined to only those firms that had to repay at least 30% of their long-term debt.

Second, given the possible concern that facing high repayment obligations may only have an effect in specific industries, we exclude industries one by one (from both treated and control firms) and estimate very similar treatment effects in all sub-samples.[24]

Third, another potential concern is that firms with low repayment shares might be systematically different from other firms, for instance, because they have negotiated debt contracts with bullet-type

[23] In line with a smaller treatment effect on antidepressant use, a lower cut-off also results in a smaller effect on the probability of job separation (4.4 pp higher job separation in treated firms by end-2010 based on a cut-off of 20% compared to 6.2 pp based on the baseline cut-off of 25%). The financial distress that the firms with high repayment shares face results in a slight (albeit not statistically significant) increase in the probability of bankruptcy as well. By 2010, the cumulative probability of bankruptcy among the business units of high-repayment firms is about 6 pp higher than among the business units of the other firms (t=0.41). As expected, this effect also shrinks (to 4 pp) when we choose a lower (20%) cut-off value.

[24] Table available upon request.

repayment and/or use different debt instruments. Many firms in our sample have very low (<5%) 2008 repayment shares. We therefore exclude in two regression models the firms with the lowest repayment shares: a. those with a zero repayment share, and b. firms a repayment share lower than 5%. We find that the baseline results on anti-depressant are upheld in these alternative models.[25]

Fourth, there are two main ways of defining the threshold of the long-term debt that needs to be refinanced. In our baseline specification, we define firms with a high refinancing need based on the ratio of the repayment obligation and total outstanding long-term debt. Although we had already excluded firms from our sample that have a low debt to total assets ratio (below 10% in our baseline), it might be possible that a firm with a lower share of long-term debt to total assets and a large fraction of it needing to be refinanced is classified as treated, while it may not really be affected by the refinancing needs. We therefore study another possibility to define the refinancing needs threshold and consider the ratio of repayment obligations and total assets. Under this definition, we classify firms as 'treated' if the ratio of their 2008 repayment obligations to their total assets reached at least 7.5% (case 1), 10% (case 2), or 12.5% (case 3). Using total assets as the denominator has the advantage that we do not need to restrict our sample to firms that have a larger share of long-term debt to total assets on their balance sheets to ensure that the refinancing needs are significant compared to the size of the firm. Abandoning this restriction increases the size of our sample to 568 firms. The results of this analysis show that higher repayment obligations in 2008 as a proportion of total assets are also related with an increase in antidepressant use among employees.[26]

5.2 Economic recessions with and without financing constraints

We perform a placebo test to verify that our results are not driven by the excess sensitivity of the treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock). We exploit the fact that after a short-lived recovery in 2010-2011 the Dutch economy fell back into recession in the second half of 2012 ("double-dip recession"). Importantly, Duchi and Elbourne (2016) show that the effect of credit supply shocks on corporate lending growth and corporate investments is negligible in this period, noting that "when we look at the double-dip recession in 2012, adverse credit supply shocks play no role" (p. 65). Therefore, the 2012 recession presents a negative economic shock without a strong corporate credit supply component: if our results are indeed driven by disruptions in corporate credit supply, we would expect to find no positive treatment effects in this period. Indeed, we demonstrate in Table 10 that, following the 2012 recession, growth patterns in antidepressant use are similar between employees of firms that had to repay a large share (>25%) of their long-term debt in *2012* and employees of other firms in our sample. If anything, employees of high-repayment firms exhibited a slight decrease in antidepressant use. Column (2) repeats the job separation analysis from Table 5 in this placebo setting. The results reveal that employees of 2012 high-repayment firms did not suffer from increased job

[25] Table available upon request
[26] Table available upon request.

insecurity. In summary, the placebo test shows that repayment of a high share of long-term debt has no detrimental effects on employees during an economic downturn when credit constraints were not binding.

[Insert Table 10]

5.3 Additional controls capturing financial constraints

It is possible that firms that had to repay a larger share of their long-term debt in 2008 might differ in terms of e.g., debt maturity structure because a shorter debt maturity could (mechanically) lead to a larger annual repayment share. Firms with shorter debt maturities might have been differently/more strongly affected by the financial crisis.[27] So, this could suggest that treated firms could be weaker or, more broadly, choose a shorter debt maturity within their long-term tranches for other endogenous reasons. Consequently, it could be that employees sort themselves towards the treated firms. Let us first point out that, following Almeida, Campello, Laranjeira and Weisbenner (2011), we have already controlled for flexible time trends in pre-crisis firm characteristics that aim to capture firm heterogeneity. In our baseline estimation (Table 4), these characteristics include two-digit industry codes, size (log total assets), long-term debt to total assets, cash flow, and liquid assets (cash) to total assets. Almeida et al. (2011) argue that "it is commonly accepted that these covariates capture a lot of otherwise unobserved firm heterogeneity".[28] Still, we further examine potential concerns that so far *unobserved (quality) differences* between treated and controls firms, and not financing frictions, drive our results.

First, we extend the set of control variables using 2007 accounting information in Panel A of Appendix Table B.4. Column 1 of presents the average treatment effect on antidepressant use in our baseline specification, while columns 2 to 8 add to the baseline regressions the following flexible time trends based on additional 2007 firm characteristics: firm profitability measured as ROA (column 2), leverage measured as total debt to total assets (column 3), interest coverage defined as EBIT divided by interest expense (column 4), a binary indicator capturing dividend payments in 2007 (equal to 1 in case of payments) (column 5), a binary indicator capturing whether the firm has any bonds outstanding (equal to 1 in case of bonds) (column 6), an indicator equal to 1 if the firm is a public liability company[29] (column 7), and finally all these controls together (column 8). We choose these characteristics of firm heterogeneity based on the literature on measuring financial constraints (Kaplan and Zingales 1997, Lamont, Polk, and Saa-Requejo 2001, Hadlock

[27] Alternatively, theoretical and empirical evidence suggests that firms with shorter debt maturity may have higher investment opportunities and may signal higher quality by choosing more short-term debt (Myers 1977; Flannery 1986). Barclay and Smith (1995) estimate that firms with more growth options have, ceteris paribus, less long-term debt and find a negative, albeit economically small, correlation between a measure of firm quality and debt maturity. Stohs and Mauer (1996) document that debt maturities are on average shorter for higher quality firms (proxied by more positive future earnings surprises).

[28] Almeida et al. (2011) also controls for firms' market-to-book ratio (Q); however, this control cannot be used for the private firms in our sample (and is not observed for the public firms in our dataset).

[29] This is a necessary but not sufficient condition for the firm to be publicly traded – it should be noted that given the confidential nature of the data, Statistics Netherlands does not reveal which firms are in the sample of treated and control firms.

and Pierce 2010, Whited and Wu 2006). As the results in Panel A of Table B.4 reveal, our baseline estimate of a 0.44 percentage points increase in antidepressant use among treated employees in the 2008-2012 period hardly changes (it remains between 0.39 pp and 0.51 pp) when we include these additional controls (interacted with the Post indicator).

Second, as for a subsample of firms additional past (financial) information is available, we control for these characteristics in Panel B of Appendix Table B.4.[30] While column 1 repeats the results of our baseline specification, columns 2 and 3 control for two measures of firm age, a characteristic often used in measures of financial constraints (e.g., in the Hadlock and Pierce (2010) index).[31] Column 4 controls for revenue growth, a component of the Whited and Wu (2006) index of financial constraints, during the 2005-2007 period. As this measure requires balance sheet data from 2005, our sample is reduced to 257 firms. Column 5 controls for a set of binary indicators (as usual interacted with the year fixed effects) of business events that affected the firm in 2007. The source of this information is the National Working Conditions Survey, which is filled out by about 30,000 employees in the last quarter of each year.[32] The question we rely on asks if "any of the following changes occurred in your company (plant/location) in the past 12 months? (a) large reorganization, (b) takeover by another firm, (c) takeover of another firm, (d) downsizing without forced dismissals, (e) downsizing with forced dismissals, (f) merger with another firm, (g) outsourcing of support services (h) relocation of business activities abroad, and (i) automation of business operations. Among these additional control variables, only the indicators for "takeover by another firm" (0.25 pp) and "takeover of another firm" (0.27 pp) are statistically significant.[33] Column 6 controls for the share of long-term debt that the firm had to repay in *2007*. If treated firms indeed have a systematically shorter debt maturity structure, their 2007 repayment share should also be higher on average. Therefore, by controlling for the 2007 repayment share we can partially control for the maturity of corporate debt in our sample. We find that adding this additional control variable does not affect the estimated treatment effect. Finally, column 7 combines all these preceding controls, while column 8 also includes the control variables that we added in Panel A of Table B.4. The results in Panel B of Table B.4 show that our baseline estimate of a 0.44 percentage

[30] Past financial information is not available for all sample firms due to two main reasons, (1) firms are not part of the "Annual Statistics of Finances of Large Enterprises" if they are not considered to be a large enterprise in the given year, (2) data on some firms cannot be traced back to earlier years because the firm's unique identifier changes (e.g., due to mergers, reorganizations or a change in the tax unit structure).

[31] Although there is no direct information on firm age in the datasets of Statistics Netherlands, we can still match 326 of the 352 firms in our sample by means of the historical General Firm Register. The register contains in some cases the year of establishment and in others the year of the first appearance in the register. As the register starts in 1994, age is right censored at 14 years for many sample firms. Consequently, in column 2 we control for a binary indicator if the firm is at least 14 years old (about 60% of the sample firms).[31] Another proxy for firm age is the longest tenure among all employees in the firm (available for all sample firms) (column 3).

[32] We use data from the 2007 survey wave. There are 218 firms in our sample in which at least one employee filled out the questionnaire.

[33] For reasons of parsimoniousness, we do not show all these parameter estimates in Table B.4, but they are available upon request. The significance of the takeover coefficients suggests that experiencing a takeover, either on the target or on the acquirer side, can be detrimental for employee mental health. This is in line with the results of Bach et al. (2021), discussed in the introduction.

points increase in antidepressant use among treated employees in the 2008-2012 period is robust to the addition of these further control variables. The estimates range between 0.37 pp and 0.57 pp.

5.4 Matching

Third, as we want to address possible ex ante sorting by employees in control and treated firms before the 2008 shock and alleviate any concerns that these differences drive our results, we control for employee traits in our models and apply additional matching strategies. The summary statistics presented in Table 2 have revealed some differences between the employees of treated and control firms. For example, employees of treated firms were more likely to use antidepressants before the crisis and are a bit younger. Importantly, these unconditional differences either disappear or are substantially reduced once we control for the same characteristics that we use in our regression models: industry fixed effects, firm size (total assets), cash flow, cash ratio, and long-term debt to total assets ratio. Table B.5 of Appendix B presents the differences in pre-crisis employee characteristics between treated and control employees unconditionally in Panel A, and conditional on the included control variables (Panel B). For example, although the unconditional mean of antidepressant use is 0.76 pp higher among treated employees (column 1), once we account for differences in industry composition, size, cash flow, cash ratio and long-term debt ratio, the difference disappears. Unconditionally, treated employees are younger (column 2), more likely female (column 3), less likely to have a partner (column 4), less likely to have dependent children (column 5). Also, they take on average 0.15 more types of medications (ATC-4 code groups) (column 6), earn a lower salary (column 7), and have a lower household income (column 8). However, most of these differences arise from the different industry composition and other differences in firm characteristics of treated and control firms. Once accounting for the control variables in Panel B, these differences disappear or greatly shrink. Although treated employees still earn a lower salary (column 7), the difference is only weakly statistically significance (at the 10% level). We only still observe some differences in age and the probability of having dependent children.

To account for possible imbalances between the characteristics of employees in treated and control firms, we have also performed the regression analysis on a matched sample of treated and control firms. Panels C and D of Table B.5 present the differences in characteristics of treated and control employees after applying matching.[34] The results in Panel C show that, even without adjusting for the firm-level characteristics in a regression setting, matching eliminates almost all (mean) differences between treated and control employees. An exception is the difference in tenure, which slightly increases. The results in Panel D

[34] There are different ways to perform matching, based on how to calculate the similarity between observations and how many similar observations (neighbors) to use. We exact match on the industry codes and use nearest-neighbor matching based on the Mahalanobis distance measure on the four financial characteristics (cash ratio, log total assets, long-term debt to total assets and cash flow). We use matching with replacement and for each treated firm we select the 3 "nearest" neighbor control firms. We estimate the regression models on these 'matched sample' that consists of the treated firms (with any matched controls) and the nearest neighbor control firms. We also implement matching with 5 nearest neighbors and/or based on propensity scores.

show that controlling for the firm-level characteristics, there remain no significant differences between treated and control employees, including differences in tenure. We obtain similar results with different matching methodologies, using the five instead of the three nearest neighbors and/or matching on the propensity score. We then estimate treatment effects on 2008-2012 average antidepressant use in the matched sample(s). The estimates, presented in Table B.6 of Appendix B, range between 0.42 pp and 0.46 pp and are all statistically significant at the 1% or 5% levels. Because matching greatly reduces the pre-treatment differences between treated and control firms and their employees, these results alleviate potential concerns that the estimated treatment effect on antidepressant use is driven by imbalances between treated and control employees.

5.5 Refinancing frictions and internal capital markets

We implement an additional placebo test relating repayment share with antidepressant use on firms for which the channel of refinancing difficulties is not expected to be binding. In this test, we study the treatment effects for foreign-controlled firms. When such firms have to repay a large share of (intra-group or external) debt, they may not face refinancing difficulties because their corporate groups could meet repayment obligations through their internal capital markets.[35] The results, available upon request, show that employees of foreign firms that had to repay more than 25% of their outstanding long-term debt in 2008 did not exhibit higher antidepressant use during the crisis (the coefficient estimate is -0.06 with a t-value of -0.4) compared to employees of other foreign firms.

5.6 Leverage

Studying the repayment share of long-term debt is not the only way to investigate the effects of employer firms' financial difficulties on employees' mental health during the crisis. We had argued that focusing on the repayment share offers the possibility of identifying the causal effects of refinancing difficulties on employee mental health because repayment obligations are pre-determined by debt contracts. A drawback of focusing on repayment share is that our sample is reduced to the largest Dutch firms for which we have information on the repayment share. Adopting leverage (long-term debt plus short-term debt over total assets) as a measure of financial vulnerability substantially increases the sample size: leverage information is available for most non-financial firms in the Netherlands such that our sample increases to about 94,000 firms and more than 2,000,000 employees.[36] On the negative side, a firm's leverage does not

[35] For instance, Almeida, Kim, and Kim (2015) argue that internal capital markets of Korean business groups helped them mitigate the negative effects of the Asian crisis on investment and performance. Desai, Foley, and Hines Jr. (2004) study the internal capital markets of multinational corporations and find that multinational firms employ internal capital markets to overcome the imperfections in external capital markets. Gopalan, Nanda, and Seru (2007) study Indian business groups and find that intragroup loans are being used as a means of support for firms in distress.

[36] While our baseline study is based on firms present in the SFGO (Annual Statistics of Finances of Large Enterprises) dataset, these analyses are based on the much larger NFO (Annual Statistics of Finances of Non-Financial Enterprises) dataset. We apply the same sample selection criteria as in our baseline analysis with two differences. We do not exclude firms with a low long-term debt to total assets ratio (as we consider all forms of leverage).

necessarily capture only its financial position. Leverage might be correlated with the firm's ownership and governance structure (e.g., family firms), its recent economic performance, its future growth opportunities, and other factors. Employees of firms with a high leverage ratio might have behaved differently even in absence of the credit supply shock presented by the Global Financial Crisis. Due to these limitations, we regard this analysis as a robustness tests on our main specification. The motivation for this robustness analysis is found in the study of Giroud and Mueller (2017) who provide evidence that high-leverage firms decreased their employment more during the Great Recession in response to local demand shocks. Financing constraints may dampen labor demand by impairing firms' ability to engage in labor hoarding, a prevalent practice in the Netherlands during the crisis (van den Berge et al., 2014).

Our results show that employees of firms with a high leverage ratio (at the year-end of 2007) increased their use of antidepressants more during the 2008-2012 period. In all specifications, our key independent variable is an indicator capturing whether the firm had higher than median (0.47) leverage ratio at end-2007.[37] This indicator is statistically significantly (at the 1% level) correlated with the increase in antidepressant use in the 2008-2012 period. The size of the effect (0.08 pp in the baseline specification) is smaller than in our main specification. This is as expected because having a high leverage ratio does not necessarily lead to refinancing difficulties. Our results also provide evidence that one of the possible transmission channels of high leverage to worsening mental health is higher job uncertainty as employees in treated firms were also more likely to be separated from their jobs in 2008/2009.[38]

5.7 Collective Labor Agreements

Collective Labor Agreements (CLAs) could affect the degree of protection of employees in relation to their working hours and conditions. If a new CLA were to be negotiated in the context of an economic recession and financial distress at the corporate level, additional uncertainty could weigh on employees, bringing about additional stress. A status quo (the non-negotiation) of a CLA may hence provide stability to employees and eliminate a source of uncertainty. Ouimet and Simintzi (2021) state that "firms […] locked in by wage agreements during the crisis outperform their peers. Implicit in our findings is the assumption that managers of the firms not bound by the agreements made decisions that ex post were not value maximizing."

We would expect that the renegotiation of firm-level CLAs would have less impact in the Netherlands than in the US or UK for two reasons. First, in the Netherlands, most firms and employees fall

[37] In a first set of regressions, we include the same control variables (cash flow, liquid assets to total assets, and log total assets) as our baseline specification (with exception of the long-term debt to total assets ratio as this is used to defined leverage ratio and hence the treatment). In subsequent regressions, we include additional firm-level (ROA, paying any dividends, interest coverage ratio, being a public limited company) and employee-level (age, gender, tenure, pre-tax salary) characteristics as controls (interacted with the Post indicator).
[38] Table available upon request.

under a sectoral CLA and only a minority of firms (mainly the larger ones) have a firm-level CLA. Indeed, in our sample about 60% of employees are covered by a sectoral CLA, even though our sample firms are large. Even if a sectoral CLA were to be renegotiated in 2008, employees of a treated firm may react less to this uncertainty because the outcome of the renegotiation will be affected by the (financial) situation of the whole sector and not by the heath of their own firm. Second, reducing what is considered by employees as 'acquired rights' would be very rare. For instance, CLAs would not agree to reduce salaries, pay packages, fringe benefits. In addition, many employment conditions are legally determined: e.g. employers in the Netherlands are not allowed to fire employees for reasons of illness for a period of two years and are required to offer gradual re-integration tracks. Laying off people is strictly regulated and in the wake of a restructuring an employer faces limitations (see above). Consequently, the CLAs in the Netherlands are mainly the result of negotiations between employers and unions about wage increases, potential bonuses, or labor flexibility. Finally, we should point out that in our analyses, we control for industry-time fixed effects, which should mostly account for the effects of changes in sectoral CLAs.

We test the impact of CLAs that are (not) renegotiated in the year of financial stress, considering both firm-level and sectoral CLAs. We have collected the list of company and sectoral CLAs that were agreed upon prior to September 2008 and that have not expired prior to January 2010.[39] Of the about 330,000 employees in our sample, 23% fell under a CLA that was renegotiated just before the crisis (sectoral CLAs or firm-level CLAs). In our basic model, we add the CLA variable interacted with the year fixed effects. Controlling for the pre-2008 renegotiation of firm and sectoral CLAs only slightly diminishes the positive relation between antidepressant use and repayment share (from 0.44 pp to 0.37 pp). Interestingly, the CLA indicator has a positive effect on 2008-2012 antidepressant use (albeit only weakly significant - at the 10% level): Employees of firms/sectors that had renegotiated their CLAs just before the crisis experienced a slightly higher increase in antidepressant use during the crisis (Table B.7 of Appendix B). A possible explanation could be that CLAs agreed upon before the crisis reduced the firms' abilities companies to respond to the crisis by cutting working hours or introducing flexible labor.

6. Conclusion

This paper argued that corporate financial constraints can have adverse effects on employee mental health, and that these effects are not restricted to employees who lose their job due to these constraints. To identify the causal effects of financial constraints, we exploited the plausibly exogenous variation in firms' need to refinance their long-term debt in 2008, a period when refinancing became more difficult due to a severe tightening of bank lending standards. Using administrative data from the Netherlands on the

[39] We follow the treatment definition of Ouimet and Simintzi (2021): "Our treated firms include firms that agreed to a multiyear settlement before September 2008 and this settlement expired only after January 2010."

antidepressant medicine use of 330,000 employees in 352 firms, we estimated that employees of firms that were facing the repayment of at least 25% of their long-term debt in 2008 were 0.44 pp more likely to consume antidepressants in the 2008-2012 period. This is an economically significant 9% increase relative to the 5% unconditional prevalence of antidepressant use, comparable to the 7.5% rise in antidepressant prescription volume following the 20% decline in US housing prices between July 2006 and February 2009 estimated by Lin, Ketcham, Rosenquist and Simon (2013).

These results are qualitatively robust to alternative industry classifications, variations in control variables, restricting or broadening the sample of firms, altering the 25% refinancing cut-off, and using pre-regression matching to remove any imbalances between employees of treated and control firms. Placebo tests suggest that the results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock) and that the relation between financial constraints and mental health does not apply in firms where financial constraints are not expected to be binding because of internal capital markets.

Although the estimated effects can be partially explained by higher job loss in constrained firms, much of the increase in antidepressant use occurs at employees who manage to keep their jobs. Studies of employee-level heterogeneity in the treatment effect, among employees who keep their jobs, suggest that antidepressant use grows more for employees who may experience a larger increase in job insecurity, or for whom job insecurity may represent a greater mental health burden: employees with children, employees without a partner, employees whose salary constitutes a greater share of family budget, and employees who work in business units where job separation increased more during the crisis. Although we lack direct data on employee perceptions of job security, these results suggest that increased perception of job insecurity is a transmission channel for deteriorating mental health.

Given the important role of mental health in employee productivity, these results provide evidence that deteriorating mental health represents a hitherto undocumented cost of financial constraints for firms. Furthermore, they also illustrate that crisis periods can have an adverse mental health effect even on employees who manage to keep their jobs, as these employees may still suffer from decreased perceptions of job security.

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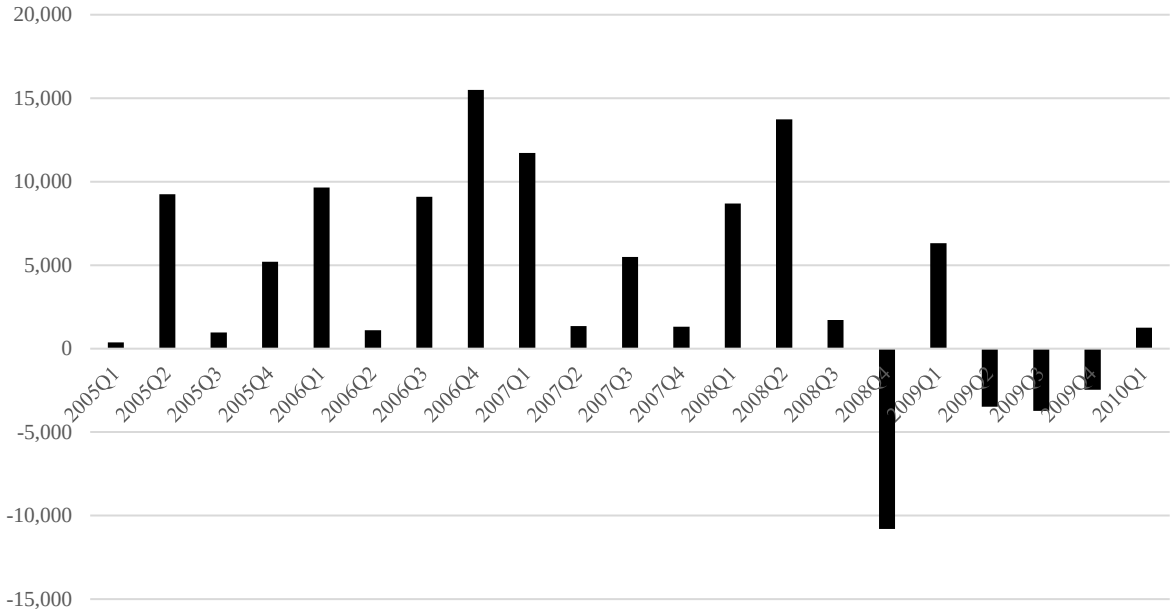
Figures:

Figure 1: Credit standards of Dutch banks on loans to large enterprises

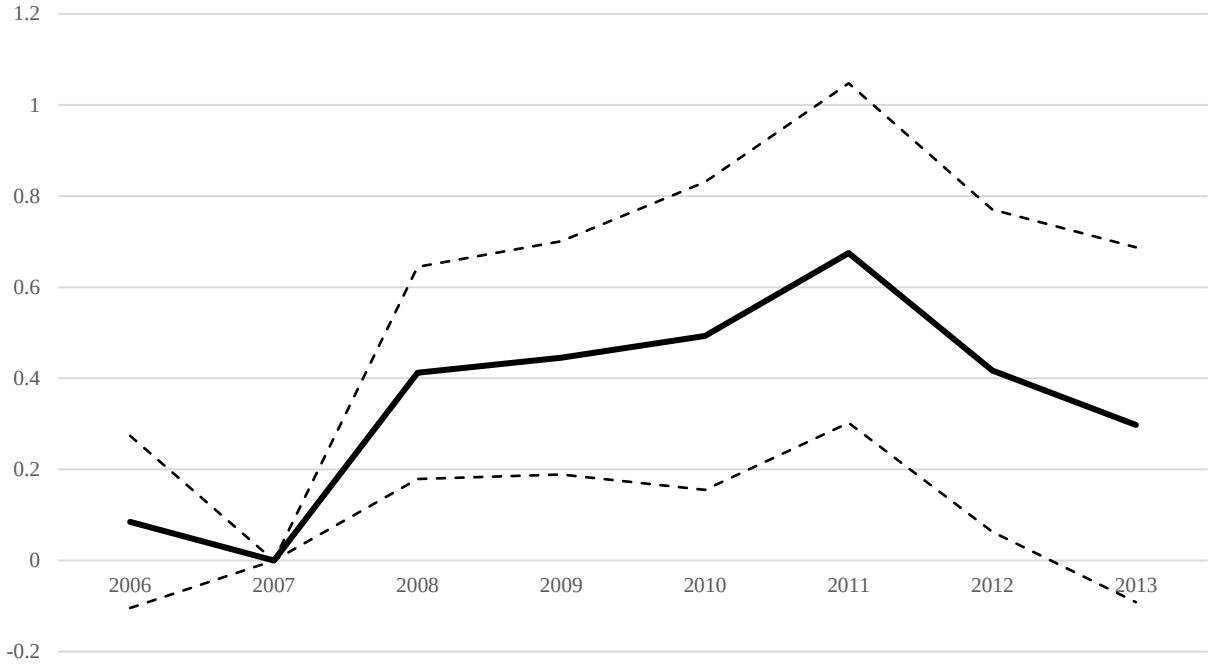
Notes: Net percentages of banks tightening and easing their credit standards (overall) in the preceding quarter, weighted by loans outstanding; Source: ECB (SDW item BLS.Q.NL.ALL.O.E.Z.B3.ST.S.BFNET)

Figure 2: New loans minus retired bank loans of Dutch non-financial companies (EURm)



Notes: Retired bank loans minus new loans of Dutch non-financial companies (EUR mln), Source: Statistics Netherlands - Quarterly sectoral accounts (CBS - *Kwartaalsectorrekeningen*)

Figure 3: Treatment effects on antidepressant use (percentage points)



Notes: Estimated treatment effects on antidepressant use (percentage points) and 95% confidence interval. The corresponding coefficient estimates and t-statistics are also presented in Table 3.

Tables:

Table 1: Steps of sample composition

	# enterprise groups	# business units	# employees
Total in 2007 SFLE	1,204		
Excluding foreign-owned firms	609		
Excluding firms with <10% LT debt on total assets	378		
Merging with business units	378	3,018	
Merging with employees	376	2,106	801,297
Excluding government-controlled and regulated industries	353	1,936	464,447
Restricting to age 20-60 years	352	1,917	388,539
Restricting to household head and partner	352	1,914	331,899
Excluding interns, outsourced employees and director-major shareholders	352	1,899	328,229

Notes: The table presents the steps taken to arrive at the final sample of firms and employees.

Table 2: Pre-treatment summary statistics

	Treated						Control						Raw Δ	Adjusted Δ	t-stat
Panel A: Firm characteristics	**N**	**mean**	**sd**	**p10**	**p50**	**p90**	**N**	**mean**	**sd**	**p10**	**p50**	**p90**			
Liquid assets to TA, 2007	23	0.03	0.04	.	0.00	.	329	0.05	0.08	0.00	0.01	0.12	-0.02	-0.01	-0.93
LT debt to TA, 2007	23	0.43	0.19	.	0.43	.	329	0.32	0.19	0.14	0.27	0.59	0.11	0.07	1.55
Cash flow, 2007	23	0.17	0.13	.	0.14	.	329	0.11	0.09	0.03	0.10	0.20	0.06	0.06	2.12
Total assets, 2007 (EURm)	23	489	1753	.	82	.	329	663	2457	33	92	1139	-174	51	0.11
Share of current portion of LT debt	23	0.34	0.09	.	0.30	.	329	0.06	0.06	0.00	0.03	0.15	0.28	0.27	13.41
No. of employees in sample	23	1552	5554	.	242	.	329	889	1920	70	285	1993	663	909	0.74
Industry composition:															
Wholesale and retail trade	12						88								
Other	11						233								
Panel B: Employee characteristics															
Antidepressant user, 2007 (%)	35692	4.75	21.26	0.00	0.00	0.00	292537	3.99	19.57	0.00	0.00	0.00	0.76	0.00	0.01
Tenure in years, 2008	35692	9.26	8.80	0.00	7.00	22.00	292537	8.79	8.85	0.00	6.00	21.00	0.47	0.99	1.91
Age, 2008	35692	39.60	10.85	24.00	40.00	55.00	292537	42.10	9.98	28.00	42.00	56.00	-2.50	1.46	2.50
Female	35692	0.50	0.50	0.00	0.00	1.00	292537	0.32	0.47	0.00	0.00	1.00	0.18	-0.07	-1.50

Notes: The table reports pre-treatment descriptive statistics for the treated and control firms, and their employees. The column Raw Δ presents the difference in means. The column Adjusted Δ presents the difference in means estimated in a regression where we control for 2-digit SBI 1993 industry fixed effects, and in case of Panel B also for the four financial variables included in our main specification (liquid assets to TA, LT debt to TA, cash flow, and log TA). The column t-stat presents the t statistic on the regression coefficient reported in column Adjusted Δ. No 10^{th} and 90^{th} percentiles are reported for treated firms, following Statistics Netherlands guidelines, because these values would refer to fewer than 10 companies. Variable definitions are presented in Table A.2 of Appendix A.

Table 3: Effects on antidepressant use over time

Dependent variable: antidepressant use (binary, x100)	**2006**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Treated	0.0849	0.412***	0.445***	0.486***	0.667***	0.407**	0.295
	(0.88)	(3.47)	(3.39)	(2.81)	(3.52)	(2.25)	(1.47)
Liquid assets to TA, 2007	-0.501	-0.317	-0.505	0.156	-0.892	-1.028	-0.751
	(-1.34)	(-0.97)	(-1.25)	(0.28)	(-1.31)	(-1.34)	(-1.10)
LT debt to TA, 2007	0.0199	0.301	0.731***	0.745***	0.858***	0.606**	0.625**
	(0.12)	(1.53)	(3.17)	(3.23)	(3.55)	(2.20)	(2.27)
Log total assets, 2007	0.00556	-0.0236	-0.0205	-0.0710***	-0.0780***	-0.0766***	-0.120***
	(0.32)	(-1.25)	(-0.91)	(-2.72)	(-2.71)	(-2.74)	(-3.87)
CF, 2007	-0.180	-0.436	-0.723	-0.661	-0.566	-0.397	-0.884
	(-0.45)	(-1.15)	(-1.42)	(-0.86)	(-0.64)	(-0.44)	(-0.99)
Unconditional mean (%)	3.72	4.31	4.58	4.83	5.14	5.33	5.60
Employee f.e.	Yes						
Industry * year f.e.	Yes						
# Firms (=clusters)	352						
# Observations	2,603,121						

Notes: The table shows estimates of the effect of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (1). *All columns belong to a single regression*; each column shows coefficient estimates on the year * Treated and year * Controls interactions for the given year. 2007 is the omitted year. As specified in Model (1), the regression includes employee fixed effects as well as 2-digit SBI93 industry*year fixed effects. Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. Antidepressant use is only defined for people who lived in the Netherlands on the 1st of January of the given year. The row 'unconditional mean' presents the sample mean of the dependent variable for the given year. Variable definitions are presented in Table A.2 of Appendix A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Average treatment effect for 2008-2012

	Antidepressant use (x100)			
Post * ...	Baseline	No covariates	Coarser industry	Employee covariates
Treated	0.440***	0.260***	0.333***	0.473***
	(3.09)	(3.54)	(2.91)	(3.35)
Liquid assets to TA, 2007	-0.261		-0.141	-0.326
	(-0.56)		(-0.33)	(-0.72)
LT debt to TA, 2007	0.637***		0.627***	0.618***
	(3.48)		(4.12)	(3.40)
Log total assets, 2007	-0.0565***		-0.0538***	-0.0486**
	(-2.66)		(-2.85)	(-2.41)
CF, 2007	-0.470		0.0912	-0.473
	(-0.67)		(0.17)	(-0.72)
Age, 2008				-0.0014
				(-0.50)
Female				0.288***
				(3.81)
Tenure, 2007 (years)				-0.011***
				(-3.41)
Unconditional mean (%)	4.84	4.84	4.84	4.84
Employee f.e.	Yes	Yes	Yes	Yes
Industry * Post f.e.	SBI93	SBI93	Sections	SBI93
# Firms (clusters)	352	352	352	352
# Observations	2,282,057	2,282,057	2,282,057	2,282,057

Notes: The table shows mean 2008-2012 treatment effect estimates of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (2). Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. The interaction of the Treated treatment indicator and the control variables with the Post indicator (which takes the value 0 in 2006-2007 and the value 1 in 2008-2012) are tabulated. All models also control for employee fixed effects and 2-digit SBI93 industry*Post fixed effects. Column (1) presents the baseline specification. Column (2) does not include the Post*firm-level control variables. Column (3) uses a coarser, sectoral-level, industry classification. Finally, compared to Column (1), Column (4) also include pre-treatment employee characteristics interacted with the Post indicator. The row 'unconditional mean' presents the sample mean of the dependent variable. Variable definitions are presented in Table A.2 of Appendix A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Treatment effects on cumulative job separation

	Firm controls			*Firm and employee controls*		
	2008	**2009**	**2010**	**2008**	**2009**	**2010**
Panel A: Cumulative job separation (binary, x100)						
Treated	4.60***	6.66**	6.20**	5.60***	7.93***	7.47**
	(2.97)	(2.29)	(2.01)	(3.95)	(2.92)	(2.58)
Liquid assets to TA, 2007	4.50	9.26	14.0*	4.50	8.74	13.24*
	(0.84)	(1.22)	(1.69)	(1.01)	(1.36)	(1.85)
LT debt to TA, 2007	3.41	8.64*	8.29	3.38	8.57**	8.25*
	(1.16)	(1.90)	(1.64)	(1.31)	(2.10)	(1.80)
Log total assets, 2007	-0.529*	-0.991**	-1.24**	-0.150	-0.524	-0.769*
	(-1.90)	(-2.10)	(-2.52)	(-0.61)	(-1.19)	(-1.65)
CF, 2007	-3.53	-9.31	-11.4	-7.86	-13.29	-14.33
	(-0.63)	(-0.93)	(-1.02)	(-1.65)	(-1.49)	(-1.43)
Age, 2008				-0.356***	-0.344***	-0.270***
				(-6.51)	(-4.38)	(-2.94)
Female				-0.091	1.443*	1.974**
				(-0.18)	(1.77)	(2.01)
Tenure, 2007 (years)				-0.489***	-0.681***	-0.755***
				(-7.37)	(-7.55)	(-6.93)
Unconditional mean (%)	14	23	30	14	23	30
Panel B: Cumulative UWV dismissal (binary, x100)						
Treated	0.224	0.667**	1.25**	0.220	0.662**	1.24**
	(1.34)	(2.15)	(2.42)	(1.31)	(2.12)	(2.37)
Panel C: Cumulative job separation (based on an employment gap) (binary, x100)						
Treated	3.07***	5.03**	4.83**	3.53***	5.53***	5.17**
	(3.32)	(2.50)	(2.19)	(3.95)	(2.85)	(2.39)
Industry f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Age, gender, tenure	No	No	No	Yes	Yes	Yes
# Firms (clusters)	352	352	352	352	352	352
# Observations	328,229	328,229	328,229	328,229	328,229	328,229

Notes: The table reports treatment effects on cumulative job separation (Panel A), UWV dismissal (Panel B), and Cumulative job separation with a gap (Panel C). *Cumulative job separation* is a binary indicator that takes a value 1 if an employee's initial (1 January 2008) job ended, for any reason, by the end of the year in consideration (the year indicated in the header of the column). *Cumulative UWV dismissal* is a binary indicator that takes the value 1 if an employee's initial (1 January 2008) job ended with a dismissal permit from the Dutch Employee Insurance Agency (UWV), by the end of the year in consideration. *Cumulative bob separation with a gap* is also a binary indicator that takes the value 1 if an employee's initial (1 January 2008) job ended, for any reason, by the end of the year in consideration and there was any gap (>1 day) between the end date of that job and the beginning date of any following job. We multiply all dependent variables by 100 and therefore all coefficients in the table are expressed in %. In all panels, columns (1) to (3) control for 2007 firm financial characteristics (including industry fixed effects), whereas columns (4) to (6) further control for 2007 employee characteristics (age, gender, tenure). As on 1 January 2008 all employees – by definition – worked at their initial job, the regressions do not include employee fixed effects. In Panel A, the row 'unconditional mean' presents the sample mean of the dependent variable (in %). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Treatment effects on employees keeping their job (stayers)

Dependent variable: antidepressant use (binary, x100)	**2006**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**	**2008-2012**
Treated	0.0849	0.323***	0.326**	0.362**	0.342	0.406**	0.144	0.278**
	(0.88)	(2.60)	(2.03)	(1.98)	(1.63)	(2.27)	(0.73)	(2.15)
Liquid assets to TA, 2007	-0.501	-0.226	-0.181	0.129	-0.704	-0.688	-0.899	-0.0897
	(-1.34)	(-0.71)	(-0.37)	(0.22)	(-0.96)	(-0.82)	(-1.51)	(-0.20)
LT debt to TA, 2007	0.0199	0.282	0.745***	0.974***	0.965***	0.469	0.368	0.636***
	(0.12)	(1.30)	(3.18)	(3.63)	(2.98)	(1.35)	(1.08)	(3.24)
Log total assets, 2007	0.00556	-0.00615	-0.00388	-0.0326	-0.0378	-0.0215	-0.0632*	-0.0170
	(0.32)	(-0.30)	(-0.16)	(-1.26)	(-1.19)	(-0.73)	(-1.90)	(-0.81)
CF, 2007	-0.180	-0.273	-0.668	-0.430	-0.155	-0.760	-0.914	-0.280
	(-0.45)	(-0.70)	(-1.10)	(-0.56)	(-0.18)	(-0.94)	(-1.25)	(-0.47)
Unconditional mean	3.72	4.10	4.22	4.30	4.50	4.53	4.67	4.31
Employee f.e.	Yes							Yes
Industry * Year f.e.	Yes							Yes
# Firms (=clusters)	352							352
# Observations	1,986,249							1,817,359

Notes: The table shows estimates of the effect of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use. Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. Contrary to Tables 3 and 4, which follow employees over time even if they leave their initial job, this table only considers employees who stay in their initial job (the job on 1 January 2008) for at least the end of the year of the observation. The first seven columns present the treatment effects over time, based on Model (1), in a similar manner as in Table 3. The last column presents the average 2008-2012 treatment effect, based on Model (2), in a similar manner as in Table 4 (a difference is that this specification controls for industry * post fixed effects instead of industry * year f.e.). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Treatment heterogeneity among *stayers*

	Antidepressant use (x100)								
*Post * ...*	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Treated	0.252**	0.090	0.333**	0.186	0.262*	-0.016	0.224*	0.284*	-0.359
	(2.04)	(0.61)	(2.27)	(1.48)	(1.82)	(-0.10)	(1.68)	(1.82)	(-1.59)
No partner	-0.288***								-0.330***
	(-3.28)								(-2.97)
Treated * No partner	0.114								0.398**
	(0.87)								(2.33)
Has child(ren)		0.171***							0.072
		(2.86)							(0.89)
Treated * Has child(ren)		0.330***							0.394***
		(3.27)							(3.79)
Female			0.341***						0.379***
			(4.06)						(3.98)
Treated * Female			-0.0681						-0.083
			(-0.60)						(-0.62)
High share in household income				-0.142**					0.072
				(-2.42)					(0.94)
Treated * High share in household income				0.223***					0.229**
				(2.49)					(2.22)
Age above 44					-0.0988*				-0.015
					(-1.93)				(-0.22)
Treated * Age above 44					0.0512				0.032
					(0.47)				(0.26)
Tenure 5-10 years						0.0231			-0.069
						(0.26)			(-0.63)
Tenure 10+ years						-0.206***			-0.199*
						(-2.91)			(-1.96)
Treated * Tenure 5-10 years						0.731***			0.710***
						(5.45)			(4.70)
Treated * Tenure 10+ years						0.213**			0.087
						(2.01)			(0.66)
Wage in top 25%							-0.404***		-0.300
							(-7.21)		(-3.75)
Treated * Wage in top 25%							0.219**		0.171
							(2.32)		(1.17)
Differential job separation in business unit								0.00	0.198
								(0.00)	(0.65)
Treated * Diff. job separation in business unit								2.046**	2.186**
								(2.13)	(2.26)

Industry * Post f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2007 firm variables * Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Firms (=clusters)	352	352	352	352	352	352	352	316	315
# Observations	1,817,338	1,817,338	1,817,338	1,777,198	1,817,338	1,817,338	1,777,242	1,237,772	1,217,531

Notes: The table reports estimates of treatment heterogeneity for the 2008-2012 average treatment effect, based on Model (3). All specifications include the same controls as Column (1) of Table 4. As in Table 6, we restrict observations to *stayers*, employees who keep their jobs at least till the end of the year in consideration. Columns (1) to (8) present results from five univariate specifications where we interact Post*Treated with a single characteristic measured pre-treatment (during 2007 or on 1 January 2008). Column (9) presents a multivariate specification where Post*Treated is interacted with each characteristic. *No partner* is 1 if a person lived without a partner (unmarried or married). *Has child(ren)* is 1 if a person had at least one child in his/her household. *High share in household income* is 1 if the share of a person's salary in his/her total household income was in the top half of the distribution (conditional on having or not having a partner). *Age above 44* refers to the age of a person in 2008. *Tenure 5-10 years* and *Tenure 10+ years* are indicators of having a tenure between 5 and 10 years or above 10 years, respectively. Employees with a tenure lower than 5 years serve as the omitted category. *Wage in top 25%* is an indicator if the employee's hourly wage was in the highest 25% of all wages within the firm in 2007. *Differential job separation in business unit* refers to the difference of job separation rates (see Table A.2 in Appendix A for a definition) between 2005-2007 and 2008-2010 in the business unit of the employee. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Variations on sample selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	**Excluding firms with group lending**	**LT debt to TA >0%**	**LT debt to TA >=5%**	**LT debt to TA >=15%**	**No industry restrictions**	**Excluding top 5% firms**	**Excluding top 10% firms**
Average treatment effect 2008-2012 (binary, x100)	0.440***	0.377**	0.225**	0.357***	0.497**	0.338***	0.410**	0.406**
	(3.09)	(2.47)	(2.48)	(3.03)	(2.50)	(4.04)	(2.57)	(2.28)
Employee fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Post f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2007 firm variables * Post f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Treated firms	23	22	41	31	20	25	22	21
# Control firms	352	304	485	408	301	375	335	317
# Observations	2,282,057	1,279,136	3,092,926	2,622,039	1,944,757	3,210,309	1,205,793	855,764

Notes: The table reports 2008-2012 average treatment effects (from Model 2) when the sample selection criteria are changed. All specifications include the same controls as Column (1) of Table 4. Column (1) shows the baseline (repeats column (1) in panel B of Table 4). Column (2) excludes firms that had any long-term group lending on their 2007 opening balance sheet. Columns (3) to (5) vary the minimum long-term debt to total assets ratio (excluding the current portion). Column (6) also includes firms from government-controlled and highly-regulated industries. Columns (7) and (8) exclude the top 5% and 10% largest firms (based on the number of employees in our sample), respectively. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Alternative treatment classifications

Dependent variable: antidepressant use (binary, x100)	**(1)**	**(2)**	**(3)**
	25% (baseline)	**20%**	**30%**
Panel A: dynamic effects			
2006	0.0849	0.133	0.0459
	(0.88)	(1.32)	(0.38)
2008	0.412***	0.291**	0.362***
	(3.47)	(2.04)	(3.34)
2009	0.445***	0.270*	0.400**
	(3.39)	(1.66)	(2.56)
2010	0.486***	0.339*	0.561**
	(2.81)	(1.88)	(2.53)
2011	0.667***	0.487***	0.696***
	(3.52)	(2.63)	(2.76)
2012	0.407**	0.274	0.488**
	(2.25)	(1.42)	(1.99)
2013	0.295	0.163	0.198
	(1.47)	(0.80)	(0.77)
Panel B: average effects			
2008-2012	0.440***	0.265*	0.479**
	(3.09)	(1.78)	(2.52)
Employee fixed effects	Yes	Yes	Yes
Industry * Post f.e.	Yes	Yes	Yes
2007 firm variables * Post f.e.	Yes	Yes	Yes
# Treated firms	23	37	10
# Control firms	329	315	342
# Observations (Panel B)	2,282,057	2,282,057	2,282,057

Notes: The table presents alternative treatment specifications, variations on the 2008 repayment threshold of long-term debt. Panel A shows the treatment effects over time; column (1) of Panel A corresponds to the first row of Table 3. Panel B shows the average 2008-2012 treatment effect; the estimate in column (1) corresponds to the treatment estimate in column (1) of Table 4. All specifications include the same controls as Table 3 and column (1) of Table 4. In column (2) we classify firms as treated if they had to repay at least 20% of their long-term debt in 2008, whereas in column (3) if they had to repay at least 30% of their long-term debt. The changing number of treated and control firms is presented at the bottom of the table. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Placebo test – firms with high *2012* debt repayment share

	(1)	(2)
	Antidepressant use	**Cumulative job separation**
2010	-0.125	
	(-1.35)	
2012	-0.107	-0.0146
	(-1.30)	(-0.43)
2013	-0.252**	0.000868
	(-2.38)	(0.03)
2014	-0.188	-0.0193
	(-1.34)	(-0.54)
2015	-0.355**	-0.0361
	(-2.58)	(-0.96)
2016	-0.284	
	(-1.43)	
Employee fixed effects	Yes	No
Industry * year fixed effects	Yes	Yes
2011 firm variables * year f.e.	Yes	Yes
# Firms (clusters)	406	406
# Observations	2,485,867	1,433,912

Notes: The table presents the results of a placebo test where we define financially constrained (treated) firms as those that had to repay at least 25% of their long-term debt in *2012*. Column (1) presents treatment effect estimates on antidepressant use, as defined in Table 3; the coefficient estimates on the treatment indicator * year interaction terms are shown. The omitted year, due to employee fixed effects, is 2011. Column (2) presents treatment estimates on cumulative job separation, as defined in Table 5. Control variables are similar to those in Table 3 and Table 5, respectively, but are defined using 2011 data. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix A:

Table A.1: Statistic Netherlands datasets used

Name in English	SN name	Description
Annual Statistics of Finances of Large Enterprises, SFLE	Statistiek Financiën van Grote Ondernemingen, SGFO	Annual survey on the finances (balance sheet, income statement) of the largest non-financial enterprises in the Netherlands. As of 2007, all enterprises are sampled with total assets over EUR 23 million. Close to 100% response rate for the largest 300 enterprises.
General Business Register, GBR	Algemeen Bedrijven Register, ABR	Continuously updated database of companies registered in the Netherlands, with information on corporate/legal structure (enterprise group, business units, legal entities), industry classification codes and events (e.g. mergers, liquidation).
Qualitative characteristics of employment relationships	BAANKENMERKENBUS	Information on, inter alia, start and end date of employment relationship, type of employment (e.g. regular employee, on-call, outsourcing, manager-large shareholder), social security insurance indicators (e.g. insured for unemployment benefits).
Quantitative characteristics of employment relationships	BAANSOMMENTAB	Information on, inter alia, taxable salary, calendar days worked and payroll tax withheld.
Annual dispensations of medicines per ATC-4 code per person	MEDICIJNTAB	All medicines dispensed that are reimbursed under the basic health insurance policy to persons who are registered in the Municipal Personal Records Database (GBA). No quantities are recorded; merely the 4-digit ATC codes (e.g. N06A) are listed that were dispensed for a given person in the statistical year.
Extract from the Municipal Personal Records Database	Gemeentelijke Basis Administratie, GBAPERSOONTAB	Demographic background data (that do not or hardly change) of all persons who appear in the Municipal Personal Records Database from 1 January 1995 (e.g. gender, year of birth, migration background).
Income of People, Income of Households	IPI / IHI	Annual income components (such as labor income, subsidies, income from entrepreneurship) of people resident in the Netherlands on the 1st of January of the statistical year, and their households. Information on the position of the person within the household with respect to the head of the household.
Wealth and household composition	VEHTAB / KOPPELTABELVEHTAB	Annual wealth components (assets and liabilities) of households in the Netherlands on the 1st of January of the statistical year. *KOPPELTABELVEHTAB* contains information on household members.
National Labor Conditions Survey	*Nationale Enquête Arbeidsomstandigheden*, NEA	Annual survey of workers (excluding self-employed) between 15 and 74 years old on working conditions, work content, labor relations and employment conditions.

Table A.2: Variable descriptions

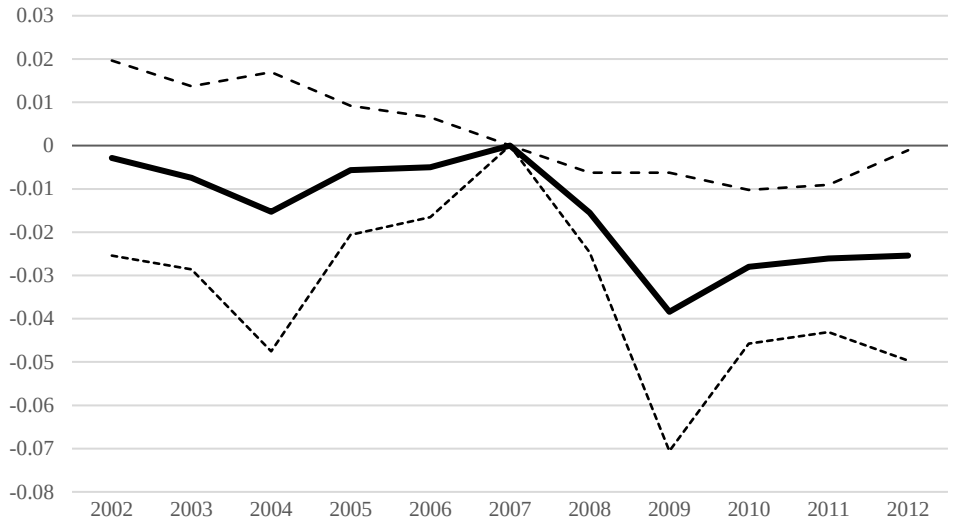
	Variable name	Additional note	Source/variable
Long-term debt structure	Long-term debt to group companies	Both in the Netherlands and abroad, maturity>1 year	SFGO /B65
	Subordinated loans	Maturity>1 year	SFGO /B67
	Bonds outstanding	Maturity>1 year	SFGO /B69
	LT bank loans	Loans from domestic financial institutions, including mortgages, maturity>1 year	SFGO /B71
	Other long-term debt	Other unclassified long-term debt, including loans from private parties, financial leasing, derivatives, member loans (for cooperatives)	SFGO /B73
	Current portion of long-term debt	Repayment obligation of long-term debt (including bonds and other debt) due within one year	SFGO /B85
	Total long-term debt	= Long-term debt to group companies + Subordinated loans + Bonds outstanding + Loans from domestic financial institutions + Other long-term debt	
	Total long-term debt including its current portion	= Current portion of long-term debt + Total long-term debt	
	Share of current portion of LT debt	= (Current portion of long-term debt) / (Total long-term debt including its current portion)	
Firm characteristics	(Log of) total assets		SFGO/B37
	Liquid assets to total assets ratio	Liquid assets are the sum of Cash and cash equivalents, Term deposits with financial institutions, and Receivables from financial institutions (current account). The ratio is defined as (Liquid assets)/(Total assets)	SFGO/B31-B35; SFGO /B37
	Long-term debt to total assets	= Total long-term debt / Total assets	
	Cash flow	= (Net income + depreciation and amortization) / Total assets	SFGO/R20, R05
	SBI93 - 1993 version of the Dutch industry classification codes	The industry classification codes are registered at the Chamber of Commerce for each legal unit (e.g. B.V.). In the GBR, SN provides a code at the business unit level by using the code of the legal unit within the business unit that has the most employees. Similar to this approach, we use the code of the business unit with the most employees within an enterprise group as the enterprise group level code.	ABR / RBE_SBI93
	ROA (return on assets)	Return on assets (=Net income divided by Total assets)	SFGO / R20, B37
	Leverage ratio	= (Long-term debt incl. current portion + Short-term debt) / Total assets	SFGO / B65-B87
	Interest coverage	= EBIT / Interest expense	SFGO / R07, R12
	Paid dividends	Indicator if the firm paid any dividends during the year	SFGO / R21, R22
	Has bonds	Indicator if the firm had any bonds outstanding at the end of the year	SFGO / B69
	Public limited company	Indicator if the firm's legal form is public limited company (N.V.)	ABR / RECHTSVORMCODE
	Firm at least 14 years old	Indicator that the firm's age is at least 14 years based on Statistics Netherlands' firm registry	ABR
	Firm age (from tenure)	The tenure (in years) of the employee with the longest tenure in the enterprise group	BAANKENMERKENBUS
	Revenue growth, 2005-2007	Percentage change in the revenues of the enterprise group between 2005 and 2007	SFGO / R01
	Differential job separation	For a given business unit (=a part of the firm/enterprise group that carries out similar economic activities according to Statistics Netherlands), the share of 1 January 2008 employees who separated from their jobs by 1	BAANSOMMENTAB, ABR

	Variable name	Additional note	Source/variable
		January 2011 *minus* the share of 1 January 2005 employees who separated from their jobs by 1 January 2008. Job separation includes both voluntary and involuntary separation, see the definition below.	
Employee characteristics	Initial job	The job (employment relationship) that existed on 1 January 2008 and based on which the employee was selected into the sample (employees with multiple jobs on 1 January 2008 are excluded)	
	Tenure in years	Integer part of number of days since the employment relationship exists (on 1 January 2008) divided by 365 (e.g. tenure in days = 400, tenure in years = 1).	BAANKENMERKENBUS / DATUMAANVANGBAANID
	Has a partner	Takes the value 1 if person i is recorded as household head with (married or unmarried) partner, or as partner of the household head in the 2007 Income of Households dataset; otherwise takes 0.	IPI / POSHHK
	Dependent child	Takes the value 1 if there is an underage child in the household of the individual on 1 January 2008.	IPI
	Number of medicines	The number of different medicines (=ATC4 codes) the individual was reimbursed for under the basic health insurance policy in 2007.	MEDICIJNTAB
	Salary	Pre-tax salary from the employment relation (which forms the basis of being selected into the sample) of the individual in 2007.	BAANSOMMENTAB
	Wage in top 25%	Takes the value 1 if the employee's hourly wage was in the highest 25% of all wages within the firm in 2007.	BAANSOMMENTAB
	Household income	Pre-tax household income; sum of all income components (such as labor income, subsidies, income from entrepreneurship) of all members of the individual's household.	IHI
Outcome variables	Antidepressant use indicator	Takes the value 1 if person i is listed as an antidepressant (ATC4 code: N06A) user in year t. Takes the value 0 if person i is not registered as antidepressant user and person i is in the supplementary table (KOPPELTABELVEHTAB) of the Wealth of Households (VEHTAB) dataset, which contains all residents on 1 January. The variable is set to missing otherwise.	MEDICIJNTAB, KOPPELTABELVEHTAB
	Cumulative job separation	Takes the value 1 if the initial job of person i terminated by the end of the given year. A job is considered terminated in year t if there is no salary received from the job in year t+1 (more precisely if the job identifier *baanid* cannot be matched to year t+1's BAANSOMMENTAB datafile); otherwise equal to 0.	BAANSOMMENTAB / BAANID
	Calendar days worked	The sum of all days in the year when the employee had an employment contract. In rare cases, if the employee has multiple jobs, the value could exceed 365/366.	BAANSOMMENTAB / KALDG
	Cumulative UWV dismissal	Takes the value 1 if an employee's initial (1 January 2008) job ended with a dismissal permit from the Dutch Employee Insurance Agency (UWV), by the end of the year in consideration	UWVONTAANVTAB
	Cumulative job separation with a gap	Takes the value 1 if an employee's initial job ended, for any reason, by the end of the year in consideration and there was any gap (>1 day) between the end date of that job and the beginning date of any following job.	BAANSOMMENTAB, BAANKENMERKENBUS

Notes: This table reports the description of all the variables used in the analysis. The Annual Statistics of Finances of Large Enterprises (SFLE) contains both opening and closing balance sheet values; in the main analysis we use 2007 closing values, therefore we refer to these variables in the table below. "Initial job" refers to the employment relationship that existed on 1 January 2008, the date on which employees were matched to employer firms in our sample. All monetary values are in nominal EURs.

Appendix B:

Figure B.1: Treatment effects on log calendar days worked



Notes: Estimated treatment effects on (the log of) calendar days worked and 95% confidence interval. The dependent variable is (the log of) the sum of all days in the year when the employee had an employment contract.

Table B.1: Worrying about job loss and antidepressant use

Dependent variable: antidepressant use (%)	**(1)**	**(2)**
Concerned about keeping job	2.190***	1.843***
	(9.36)	(7.88)
Female		2.375***
		(11.99)
Age		0.519***
		(8.30)
Age squared		-0.00447***
		(-5.89)
Has partner		-2.354***
		(-9.25)
Tenure in years		-0.0492***
		(-4.15)
Constant	4.118***	-8.332***
	(41.52)	(-6.86)
2-digit SBI 93 industry fixed effects	Yes	Yes
Unconditional mean antidepressant use (%)	4.53	4.53
# Firms (clusters)	21,436	21,436
# Observations	61,575	61,575

Notes: The table presents the relation between antidepressant use and the indicator "Concerned about keeping job" that is based on the question "Are you concerned about keeping your job? (yes/no)" from the National Labour Conditions Survey (NLCS). Antidepressant use is defined as in Table 4. In both specifications a pooled cross-sectional regression is estimated with data from 2007 to 2010. Column (1) only controls for industry fixed effects, whereas column (2) further controls for gender, age, age squared, an indicator if the person lived with a (married or unmarried) partner, and the length of the person's current employment relationship (on the 1st of January of the year). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.2: Job separation in treated firms between 2005 and 2007

	Job separation 2005	Job separation 2006	Job separation 2007
Treated	-0.00585	-0.00891	0.00374
	(-0.23)	(-0.36)	(0.14)
Liquid assets to TA, 2007	-0.0668	-0.0497	-0.0463
	(-1.42)	(-0.75)	(-0.51)
LT debt to TA, 2007	0.0432	0.0964**	0.151**
	(1.43)	(2.24)	(2.11)
Log total assets, 2007	-0.000283	-0.00690**	-0.0112**
	(-0.11)	(-2.37)	(-2.56)
CF, 2007	0.0578	0.101	0.0812
	(0.90)	(1.16)	(0.73)
Industry fixed effects	Yes	Yes	Yes
Unconditional mean of outcome	0.133	0.232	0.325
# Enterprise groups (clusters)	325	325	325
# Observations	275,714	275,714	275,714

Notes: This table presents estimates of differential job separation rates between firms defined as Treated in our baseline specification (23 firms that had to repay at least 25% of their long-term debt in 2008) and firms defined as Control, controlling for 2-digit SBI93 industry fixed effects and financial variables measured year-end 2007. The sample includes 1 January 2005 employees of the Treated and Control firms who meet the same sample selection criteria as for Table 4 (on-call or regular job, between 20 and 60 years old, household head or partner of household head). The dependent variable (job separation) is defined as in Table 6. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.3: Non-overlapping definitions of the treatment indicator

	Antidepressant use (x100)		
	(1)	(2)	(3)
Post * ...	>25%	25-30%	>30%
Treated	0.440***	0.347*	0.473**
	(3.09)	(1.85)	(2.46)
Liquid assets to TA, 2007	-0.261	-0.300	-0.287
	(-0.56)	(-0.64)	(-0.61)
Cash flow, 2007	-0.470	-0.489	-0.448
	(-0.67)	(-0.60)	(-0.57)
LT debt to TA, 2007	0.637***	0.613***	0.691***
	(3.48)	(3.32)	(3.63)
Log total assets, 2007	-0.0565***	-0.0588**	-0.0618***
	(-2.66)	(-2.40)	(-2.72)
Employee f.e.	Yes	Yes	Yes
Industry * Post f.e.	Yes	Yes	Yes
2007 firm variables * Post f.e.	Yes	Yes	Yes
# Firms (clusters)	352	342	339
# Observations	2,282,057	2,077,913	2,237,766

Notes: This table presents the 2008-2012 treatment effect estimates on antidepressant use in regressions where we redefine our treatment indicator. Column 1 contains our baseline estimate. In column 2 we define Treated firms as those firms that had to repay between 25 and 30% of their long-term debt in 2008. In column 3 we define Treated firms as those firms that had to repay more than 30% of their long-term debt in 2008. In both columns 2 and 3, Control firms are those firms that had to repay less than 25% of their long-term debt in 2008 (i.e., in column 2 we exclude firms that had to repay more than 30%, while in column 3 we exclude firms that had to repay between 25 and 30% of their long-term debt in 2008). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.4: Average treatment effect on antidepressant use for 2008-2012, controlling for additional variables

	Antidepressant use (x100)							
Post * …	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Controlling for 2007 firm characteristics								
Treated	**0.440*** (3.09)**	**0.433*** (3.18)**	**0.441*** (3.04)**	**0.395*** (2.88)**	**0.513*** (2.85)**	**0.402*** (2.94)**	**0.460*** (3.14)**	**0.494*** (2.75)**
ROA, 2007		0.779 (0.54)						1.400 (0.87)
Leverage ratio, 2007			0.0199 (0.09)					-0.0541 (-0.29)
Interest coverage, 2007				-0.00739* (-1.69)				-0.0100** (-2.14)
Paid dividends, 2007					0.129 (1.21)			0.177 (1.47)
Has bonds, 2007						0.0708 (0.57)		0.0702 (0.53)
Public limited company, 2007							-0.0503 (-0.80)	-0.172 (-1.58)
# Firms (clusters)	352	352	352	347	352	352	352	347
# Observations	2,282,057	2,282,057	2,282,057	2,274,954	2,282,057	2,282,057	2,282,057	2,274,954
Panel B: Controlling for additional firm characteristics								
Treated	**0.440*** (3.09)**	**0.517*** (2.62)**	**0.440*** (3.10)**	**0.580*** (3.30)**	**0.366*** (3.03)**	**0.529*** (3.09)**	**0.484*** (2.93)**	**0.447*** (2.64)**
Firm at least 14 years old, 2007		-0.0317 (-0.34)					-0.148 (-1.56)	-0.152 (-1.54)
Firm age (from tenure), 2007			0.00188 (0.32)					
Post * …	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Revenue growth, 2005-2007				-0.0409 (-0.26)			-0.232 (-1.17)	-0.238 (-1.16)
Firm events, 2007					Yes		0.151 (1.25)	0.141 (1.33)
L-T debt repayment share, 2006						-0.388 (-1.01)	-0.401 (-0.95)	-0.776 (-1.30)
Controls from Panel A								Yes
# Firms (clusters)	352	326	352	257	218	269	159	158
# Observations	2,282,057	1,897,997	2,282,057	1,659,176	2,129,483	1,889,218	1,512,430	1,510,697
Employee fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Post fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2007 firm variables * Post f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows mean 2008-2012 treatment effect estimates of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (2). Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. The interaction of the Treated treatment indicator and the control variables with the Post indicator (which takes the value 0 in 2006-2007 and the value 1 in 2008-2012) are tabulated. All models also control for employee fixed effects, and the 2007 firm characteristics (including the industry fixed effects) from Table 4 interacted with the Post indicator. **In Panel A,** we additionally control for 2007 firm characteristics. Column 1 presents the baseline specification (as in Table 4). Columns 2 to 7 control for additional 2007 firm characteristics interacted with the Post dummy. ROA is the firm's return on assets (=net income divided by total assets); the Leverage ratio is the ratio of total long-term and short-term debt to total assets; Interest coverage is the ratio of EBIT to interest expenses; Paid dividends is an indicator if the firm paid any dividends in 2007; Has bonds is an indicator if the firm had any bonds outstanding end-2007; Public limited company is an indicator if the firm's legal form is public limited company (N.V. in Dutch). Column 8 controls for all these characteristics. **In Panel B,** we additionally control for other firm characteristics. Some of these characteristics are based on pre-2007 data, which might be missing because some firms are not present every year in the 'Annual Statistics of Finances of Large Enterprises', while other firms might change their unique identifiers over time (e.g., due to mergers). Missing data results in reduced sample size. Column 1 repeats the baseline specification. Columns 2 to 7 control for additional firm characteristics interacted with the Post dummy. Firm at least 14 years old is an indicator that the firm's age is at least 14 based on Statistics Netherlands' firm registry (firm age is censored at 14 thus we cannot control for the exact firm age); Firm age (from tenure) is an estimate of firm age that equals the tenure (in years) of the employee with the longest tenure in the firm; Revenue growth 2005-2007 is the % change in total revenues between the years 2005 and 2007; Firm events are a set of indicators of firm events (such as takeovers) in 2007 based on the National Working Conditions Survey; L-T debt repayment share 2006 is the share of long-term debt repayment obligations for the year 2007 divided by total long-term debt outstanding year-end 2006. Column 7 control for all these characteristics (besides the tenure-based firm age estimate), while Column 8 also controls for the characteristics in Panel A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.5: Differences in employee-level characteristics before/after regression adjustment and matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of tenure	Age	Female	Antidepr. use	Dependent child	No. of medicines	Salary (EUR)	Household income (EUR)
Panel A: no matching / no regression adjustment								
Treated	0.469	-2.496**	0.177**	0.758**	-0.0245*	0.152**	-11644***	-12291***
	(0.83)	(-2.45)	(2.12)	(2.12)	(-1.82)	(2.25)	(-4.27)	(-4.99)
Panel B: no matching / regression adjustment								
Treated	0.987*	1.463**	-0.0678	0.00429	0.036**	0.048	-4637*	-6965**
	(1.91)	(2.50)	(-1.50)	(0.01)	(2.06)	(0.42)	(-1.89)	(-2.20)
Number of firms	352	352	352	352	352	352	352	352
Observations	328,229	328,229	328,229	328,229	310,936	328,229	320,309	311,318
Panel C: matching / no regression adjustment								
Treated	1.840***	-0.0206	0.0431	-0.00569	-0.01	0.006	-1087	376
	(3.11)	(-0.02)	(0.37)	(-0.01)	(-0.36)	(0.63)	(-0.31)	(0.13)
Panel D: matching / regression adjustment								
Treated	0.266	0.894	-0.0355	-0.37	0.00	-0.02	-158	1348
	(0.43)	(1.21)	(-0.79)	(-1.15)	(0.12)	(-0.29)	(0.16)	(0.69)
Number of firms	68	68	68	68	68	68	68	68
Observations	69,469	69,469	69,469	69,469	65,673	69,469	67,848	65,783
Unconditional mean	8.84	41.83	0.341	4.071	0.54	1.94	35164	73501

Notes: The table shows differences between employees in treated and control firms in eight employee characteristics, pre-crisis (2007 or end-2007). In each panel, the characteristic is regressed on the treatment indicator 'Treated'. **Panel A** is based on the whole sample and there are no additional control variables in the regression (=mean differences). **Panel B** controls for the same firm-level characteristics (liquid assets to total assets, long-term debt to total assets, cash flow, log total assets, industry code) that we use in our baseline regression specifications. **Panel C** is based on a matched sample where we match treated firms to up to 3 control firms (with replacement). We exact match on the same industry codes that we use in our baseline regressions and use nearest-neighbor matching based on the Mahalanobis distance measure on the four firm financial characteristics. The regressions in Panel C does not control for these characteristics. **Panel D** is based on the same matched sample as Panel C, but it also controls for the four firm characteristics and the industry fixed effects, as a form of regression adjustment. 'Dependent child' (col 5) is an indicator that takes the value 1 if there is an underage child in the household of the individual, 'No. of medicines' (col. 6) refers to the number of distinct medicines the individual was reimbursed for in 2007, 'Salary' (col 7) refers to the pre-tax salary from the employment relation of the individual in 2007, 'Household income' (col. 8) is also pre-tax. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.6: Treatment effects on antidepressant use in matched samples

	Antidepressant use (x100)				
	(1)	(2)	(3)	(4)	(5)
Post * ...	Baseline	Mahala. 3	Mahala. 5	PSM 3	PSM 5
Treated	**0.440*** **	**0.443*** **	**0.460*** **	**0.420** **	**0.449*** **
	(3.09)	**(3.49)**	**(3.79)**	**(2.28)**	**(2.68)**
Liquid assets to TA, 2007	-0.261	-3.009	-4.596***	-3.068	-3.222
	(-0.56)	(-1.13)	(-2.93)	(-1.19)	(-1.38)
Cash flow, 2007	-0.470	-0.148	0.915**	0.785	0.165
	(-0.67)	(-0.13)	(2.14)	(0.85)	(0.18)
LT debt to TA, 2007	0.637***	0.701*	0.937**	0.867**	0.960***
	(3.48)	(1.71)	(2.11)	(2.44)	(2.90)
Log total assets, 2007	-0.0565***	-0.0189	-0.0560	-0.085**	-0.0274
	(-2.66)	(-0.27)	(-1.14)	(-2.44)	(-0.71)
Employee f.e.	Yes	Yes	Yes	Yes	Yes
Industry f.e. * Post	Yes	Yes	Yes	Yes	Yes
Number of firms	352	68	92	69	91
Observations	2282057	483779	670544	493670	595438

Notes: The table shows mean 2008-2012 treatment effect estimates of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (2) of the paper. Column 1 repeats the baseline results. Columns 2 to 5 are estimated on matched sample of treated and control firms. In these matched samples, all treated firms (with any matched controls) and the matched controls of these treated firms are included. In all cases we exact match on 2-digit industry codes. Column 2 applies nearest-neighbor matching on the four firm-characteristics (liquid assets to total assets, log total assets, long-term debt to total assets, and cash flows) based on the Mahalanobis distance metric. The sample includes the 3 nearest neighbor control firms of each treated firm (with replacement). Column 3 includes the 5 nearest neighbors. Columns 4 and 5 are based on propensity score matching and include the 3 and 5 nearest neighbors of each treated firm, respectively. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.7: Treatment effects controlling for Collective Labor Agreements

	Antidepressant use (x100)	
*Post * ...*	(1)	(2)
Treated	**0.440*** **	**0.369*** **
	(3.09)	**(3.07)**
Liquid assets to TA, 2007	-0.261	-0.123
	(-0.56)	(-0.27)
Cash flow, 2007	-0.470	-0.437
	(-0.67)	(-0.72)
LT debt to TA, 2007	0.637***	0.609***
	(3.48)	(3.67)
Log total assets, 2007	-0.0565***	-0.0540***
	(-2.66)	(-2.71)
Collective Labor Agreement		0.304*
		(1.92)
Employee f.e.	Yes	Yes
Industry * Post f.e.	Yes	Yes
Number of firms	352	352
Observations	2,282,057	2,282,057

Notes: The table shows the mean 2008-2012 treatment effect estimates of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (2). Antidepressant use is a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. The interaction of the Treated treatment indicator and the control variables with the Post indicator (which takes the value 0 in 2006-2007 and the value 1 in 2008-2012) are tabulated. Column 1 repeats our baseline analysis from Table 4. Column 2 further controls for the interaction of the Post indicator with an indicator of falling under a Collective Labor Agreement (CLA) (in 2008) that had been agreed upon prior to September 2008 and had not expired prior to January 2010. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

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