Pay Transparency and Mental Health*

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Abstract

This paper explores the impact of pay transparency on worker well-being. We exploit a pay transparency legislation in Denmark that requires firms with more than 35 employees to disclose pay information by gender. Using detailed administrative data on employee and mental health prescription drugs, we document the effect of the transparency policy using two empirical strategies, Difference-in-Differences and Regression Discontinuity Design. Our results suggest that the legislation led to a short-run decline in the anti-depressant use for women in affected firms. We consider two competing mechanisms behind this result: the potentially negative impact of horizontal (peer) comparisons and the potentially positive impact of organizational changes leading to the reduction of pay disparity. Taken together, our results are not consistent with a horizontal wage comparison mechanism and the evidence points to the second mechanism being the dominant force.

Keywords: Mental Health, Gender Pay Gap, Transparency

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1 Introduction

Gender pay gaps are prevalent in the economy. Governments often attempt to nudge firms to reduce these gaps by introducing pay transparency mandates. Despite the prevalence of those mandates, our understanding of their effect on workers has been limited to studying their effect on wages, where the impact has been generally to help decrease in gender pay gaps (Bennedsen et al.; 2022). Pay transparency may also have important implications on employees' well-being, though the direction of the effect is not clear ex-ante. Transparency can facilitate income comparisons, which may have a negative effect on job satisfaction (e.g. Card et al.; 2012) as differences in peers' salaries are often considered arbitrary and unfair. But as policies often lead to changes in – at least – relative and absolute pay, these and other positive firm responses might generate a correspondingly positive effect on employee well-being.

In this paper, we explore the impact of gender pay gap transparency legislation on worker well-being using detailed administrative data from Denmark. We exploit a 2006 law that required firms with more than 35 employees to report salary statistics by gender and occupation, highlighting the within-firm gender pay gap. A key empirical challenge in studying well-being is having a measure that can consistently compare employees' well-being across firms and over time. We overcome this challenge by using mental health drug prescription records for all employees in Denmark; this includes the population of prescriptions, as Denmark has a universal healthcare system. We use information on the type and dosage of drugs used to treat mental health conditions – henceforth, anti-depressant (AD) drugs – and assume that an increase in dosage over time is a proxy for worsening mental health conditions and vice-versa.

To examine the effect of transparency in AD use, we use a difference-in-differences (DiD) methodology in a narrow window around the 35 employee cutoff, defining firms with 35 to 50 employees as "treated" and firms with 20 to 35 employees as "control". We find a

significant impact of the pay transparency law on workers' anti-depressant use, especially for female workers. AD use by female workers in treated firms significantly decreased by about 3.4 percentage points relative to female workers in the control firms. The effect is, however, short-lived. There is an immediate reduction in year 1 that persists in year 2, but AD use reverts to the mean by year 3. This is consistent with the economics literature of the effect of life events on well-being, which tends to be short-lived (Oswald 2008), as well as the behavioral literature on the phenomenon of the "hedonic treadmill" (Brickman and Campbell; 1971). AD usage among male workers, in contrast, is not significantly affected by the reform. Comparing female and male workers, we find that female workers significantly decreased their anti-depressant use by approximately 4 to 6 percentage points relative to male workers. The granularity of our data allows us to control for time-invariant characteristics by including individual by firm fixed effects, so we can recover a causal effect of the law keeping the person-firm match constant, as well as time-varying individual characteristics such as worker age and experience. Although we consider a narrow window of firm sizes around the 35 employee cutoff, we additionally show our results are robust when controlling for firm size.

We attribute this short term reduction in AD use to the successful reduction in within-firm wage inequality, as well as a set of accompanying organizational changes that led to a more equitable work environment. We do not find evidence of horizontal peer comparisons negatively impacting morale (as in Card et al. (2012) and Cullen and Perez-Truglia (2022)); likely because firms seem to have addressed the inequality relatively quickly. The results are also not a result of a direct effect of the law on individual wages; the gap grew smaller due to slower male wage growth relative to no impact on female wages (Bennedsen et al.; 2022). As female wages did not rise in absolute terms, it was the reduction in *relative* wage gaps that seems to have mattered for employee well-being. Further, the transparency law also seems to have triggered broader positive organizational changes that led to a more equitable work environment. Beyond the reduction to the gender pay gap, treated firms also increased their

rates of hiring and promotion of women, including to managerial positions.

The impact of the law on gender pay gaps was heterogeneous within firms as well as across firms. Bennedsen et al. (2022) finds that the convergence in wages is more than twice as large at the bottom of the firm wage distribution as compared to the top and that the response to the transparency regulation was stronger for firms with larger gender pay gaps pre-treatment. As such, if the impact stems from a sentiment of fairness created by swift action on gender pay gaps, we should expect female workers in these more intensely affected firms to drive the improvement in mental health. To consider this, we first split the sample based on whether the worker's individual wage is above or below the firm median in the pre-period. We find that the reduction in AD usage is driven by lower-paid female workers. Second, we split firms based on their pre-period gender pay gap and find that female employees' AD use indeed drops more among firms characterized by larger gender pay gaps prior to the regulation. This is consistent with feelings of fairness driving the results.

A key concern to our baseline analysis is that a contemporaneous event is causing our results. To address this issue, we estimate a placebo analysis defining alternative treatment thresholds and show no significant effects. As such, our results are not driven by a contemporaneous event that might have affected differently larger and smaller firms, but the changes are specific to firms above and below the 35 employees cutoff. We verify this by employing a secondary identification strategy and estimate a regression discontinuity design, where we compare the AD use of men and women at the 35 employee threshold. Consistent with our DiD strategy, we find no effect at the discontinuity for male employees, and that female employees use less AD when employed in firms just above the 35 worker threshold.

A related concern is that health coverage changes for firms above 35 employees could be affecting their employees' general health, including their anti-depressant use. However, unlike the U.S. where health coverage varies by firm, healthcare in Denmark is government-funded and does not depend on employment. We confirm that the law change did not impact the

relative use of other medications such as cholesterol drugs, diabetes drugs, or antibiotics.

Our effect is economically important. To fix ideas, we examine the effect of other life events on individuals' anti-depressant use. The magnitude of the effect of transparency is, for example, about 20% that of divorce and very small relative to losing a spouse. Importantly, the short-lived effect on well-being is not unique to pay transparency legislation. We show that even more extreme life events, such as losing a spouse, have a temporary effect on anti-depressant use as well.

Our results contribute to two main strands of the pay transparency literature. One important question in this literature is quantifying the effect of pay transparency on employee job satisfaction and well-being. In the context of mandated pay disclosure in the public sector, Card et al. (2012) use a sample of government employees in California and show that after government employee salaries are published online, aggregate worker satisfaction drops. Using a sample of workers in an Indian manufacturing plant, Breza et al. (2018) show that information on peers' salaries leads to negative feelings, reduces job satisfaction, and negatively affects labor productivity. Using data from a bank in South East Asia, Cullen and Perez-Truglia (2022) find a negative impact on employees' morale of horizontal pay comparisons, which is though not present for vertical comparisons. More broadly, our paper is related to the literature on employee responses to income inequality (e.g., Perez-Truglia (2019), Cullen and Perez-Truglia (2018)). Our paper is distinct from these studies as we show how pay transparency impact employee well-being at the firm setting where mandates reduced inequality, thereby increasing fairness in the organization.

A related part of the pay transparency literature examines how it affects wage setting. Cullen and Pakzad-Hurson (2021) explores how transparency affects wage setting in organizations, and argue that higher transparency results in more equal but lower wages and higher profits for employers. Mas (2017) shows that top earners in municipal jobs experience a drop in wages following the public disclosure of wages, which he argues is due primarily to

public aversion to visibly exorbitant salaries. Bennedsen et al. (2022) and Baker et al. (2019) show pay transparency reduced the gender pay gap, with this effect driven primarily by lower growth of male wages. We contribute to this literature by showing that pay transparency can impact employee outcomes beyond wages, and specifically, their mental health.

2 Data and context

2.1 The Pay Transparency Law

The law requiring gender pay gap disclosure was passed on June 9, 2006 (Act no. 562), mandating disclosure of gender-based disaggregated statistics with the purpose of informing employees of within firm gender wage gaps. The law requires firms with more than 35 employees to report annually gender-segregated wage statistics for occupations (defined by six-digit DISCO code) with at least 20 employees of each gender.[1] This information, however, was not required to be disclosed to the wider public but only to the employee representatives who then convey the information the other employees.

In the empirical analysis we focus on the criterion that firms with 35 or more employee are required to report wage statistics and do not consider the second criterion that firms report only for occupations with more than 20 employees of each gender. We do this because most firms followed this criterion only. First, we investigated how the law was communicated to firms and find that the description of the law by the EU and the International Labor Organization (ILO) only mentions the first criterion.[2] Second and consistent with the way the law was communicated, many firms that met the first but not the second criterion reported

[1] The requirement does not extend to companies in the fields of farming, gardening, forestry, and fishery.

[2] The European Commission's Directorate General for Internal Policies issued a report entitled Gender Equality in Denmark that describes the law as follows: "Since 2007, companies with 35 employees or more should carry out gender disaggregated pay statistics and elaborate status reports on the efforts to promote equal pay in the workplace." (EC; 2015). ILO's description of the law reads as follows: "Employers employing 35 or more workers are required to prepare annually gender-disaggregated statistics or, alternatively, an equal pay report and action plan.".

wage statistics. According to a report by the Confederation of Danish Employers (DA), the largest employer association covering about a third of Danish employees, 35% of member firms that report gender-disaggregated wage statistics do not satisfy the second criterion but, all of them have more than 35 employees. Moreover, Bennedsen et al. (2022) provides a examples of implementation challenges that led firms to not follow the DISCO requirement as well as additional details about the law and its implementation.

The passage of the law was not anticipated. While the government had promised to enact a transparency law during the campaign for the snap election in February 2005, there was little discussion about fulfillment of this promise after winning the election.[3] This neglect was not surprising given that the same administration had tabled a similar proposal years earlier. However, eight months after re-election, in October 2005, the government announced that it would introduce a bill to amend the Equal Pay Act. The Ministry of Economics introduced the bill to Parliament in December 2005. The proposal was adopted on June 2006, and the new provisions came into force in January 2007.

2.2 Data Sources and Sample Description

We combine three primary data sources: data on individual workers' prescription use from the Danish Health Authority (SSI), employee work history from the Integrated Database for Labor Market Research (IDA) from Statistics Denmark, and firm data from the Danish Business Register (Experian).

2.2.1 Prescription Data: Danish Health Authority

Denmark's Health Authority compiles comprehensive dataset (SSI) on the distribution of pharmaceuticals throughout the country. This dataset started in 1994 and includes all records

[3]The snap election was announced on January 18, one year earlier than scheduled, and took place on February 8.

of drugs dispensed via prescription at a pharmacy and hospitals to individuals. Each record includes the individual's personal identification number (CPR number), drug name, date of purchase or date of delivery, the drug's Anatomical Therapeutic Chemical code (ATC) as defined by the World Health Organization (WHO), the strength of the active ingredient(s) included in the drug (for example, "500mg"), and number of units delivered.

Our main proxy of workers' mental health captures the intensive margin: the volume of anti-depressant medication an individual worker is prescribed. We identify antidepressants by their four-digit ATC code. Specifically we use the following categories of mental-health related drugs: antidepressants (N06A), anxiolytics (N05B), sedatives (N05C), and antipsychotics (N05A). We refer to these drugs used to treat mental health conditions, broadly, as anti-depressants (AD). Since the various antidepressant medications have different active ingredients and thus their quantity cannot be directly compared, we convert the amount of active ingredient into a standardized measure. For each drug, the WHO provides the Defined Daily Dosage (DDD), which is the average maintenance dose per day for a drug used for its main indication in adults. Our detailed information on prescriptions, allows us to define, for each individual, their annual antidepressant dosage in number of DDDs.

Figure A6 shows the number of DDDs of mental health drugs that employees in our sample take by categories. Antidepressants are the most commonly prescribed drug, with sedatives (often prescribed for sleep disorders and some types of anxiety) and anxiolytics (often prescribed for anxiety) as the next two most common prescription types.

2.2.2 Employee data: Integrated Database for Labor Market Research

We use the matched employer-employee dataset from the Integrated Database for Labor Market Research (IDA) from Statistics Denmark. This dataset includes the employer identification number (CVR), the employee identification number (CPR), compensation, demographic information (age, gender, education) and occupation (job type and hierarchy). This dataset

covers all employees and firms in Denmark and hence allows us to follow employees over time and across firms.

2.2.3 Firm data: Danish Business Register

Firm-level data comes from the Danish Business Register. This dataset covers all firms incorporated in Denmark and includes the information firms are required to file with the Ministry of Economics and Business Affairs, including the value of total assets, number of employees, and revenues. Even though most firms in this dataset are privately held, external accountants audit firm financial information in compliance with Danish corporate law.

2.3 Sample construction

Our starting point is the universe of the limited liability companies in Denmark and their employees. We focus the analysis around the 2006 reform, covering the period 2003 to 2008. The empirical analysis involves comparing firms affected by the law (treated firms) to firms that are not affected (control firms). To keep the treated group as comparable as possible to the control group, our sample includes firms within a narrow band around 35 employees, specifically from 10 to 50 employees. We exclude firms in industries not affected by the law (those in farming, gardening, forestry and fishery) and firms with missing financial information.

We focus on full-time workers and exclude CEOs. Most importantly, our preferred sample includes employees with no pre-existing mental health conditions as it avoids including workers with long-term chronic conditions, who are unlikely to be affected by the transparency. Specifically, our sample includes only workers who had not used anti-depressants from the time our data starts, 1995, to 2002, before our window of analysis. Our final analysis dataset includes over 163,000 person-year observations across over 46,000 unique individuals in over 3,400 unique firms.

2.4 Summary statistics

Table A1 reports summary statistics for treated and control firm in our sample. Treated firms are those that employ between 35 and 50 employees before the 2006 law and control firms are those that employ 10 to 34 employees in the same period. Panel A presents information on firm characteristics, while Panel B present employee-level information.

By design, treated companies are larger in terms of sales and number of employees. However, the gender composition and education level of their respective employees is very similar. Albeit the difference in age is statistically significant, the magnitude is quite small at only 0.4 years. The AD use in the pre-period is, however, not statistically different between employees in treated versus control firms.

3 Effect of Transparency on Mental Health

We first consider the effect of the transparency law on ad usage of employees using a differences-in-differences (DiD) framework.

3.1 Main results

We estimate the following OLS regression for individual i in firm j and year t, separately for male and female employees:

$$ln(AD_volume)_{ijt} = \alpha_{ij} + \alpha_t + \beta_1 I(Treated_{ij} \times Post_t) + \varepsilon_{ijt}, \tag{1}$$

where $Post_t$ is an indicator that takes a value of one for 2006, 2007, and 2008 and a value of zero for 2003, 2004 and 2005, $Treated$ is an indicator that takes a value of one for firms that employ 35 to 50 employees prior to the introduction of the law and zero for firms that

employ 20 to 34 employees. α_t are year fixed effects that absorb aggregate macroeconomic shocks. We also include individual $\times$ firm fixed effects, α_{ij}, to control for time-invariant individual characteristics, time-invariant firm characteristics, and the match between firms and employees.[4] With this specification, we compare the *same employee at the same firm* before and after the regulation, avoiding composition effects. We include controls for firm and employee time-varying characteristics (sales, age, and experience) as a robustness check. Standard errors are clustered at the individual and firm level.

We also test whether the difference between female and male employees is statistically significant using a triple difference (DiDiD) specification:

$$\begin{aligned} ln(AD_volume)_{ijt} &= \alpha_{ij} + \alpha_t + \beta_1 I(Treated_{ij} \times Post_t) \\ &+ \beta_2 I(Post_t \times Female_i) \\ &+ \delta\, I(Treated_{ij} \times Post_t \times Female_i) + \varepsilon_{ijt}, \end{aligned} \tag{2}$$

where the notation is the same as in Equation 1.[5] δ is the triple differences coefficient estimate that tests whether the effect of the transparency law is different between female and male employees. In this specification, we can further include firm $\times$ year firm fixed effects, α_{jt}, which controls for any firm level shocks. Standard errors are clustered at the individual and the firm level.

Table 1 reports our main results. Column (1) compares the change in AD usage in treated relative to control firms for all female employees and Column (2) for male employees. while Column. We observe the transparency law had a differential impact in men and women: we find that female employees' anti-depressant use in treated firms grows 3.4 percentage points

[4]Note that we omit $Treated_j$ and $Post_t$ as these coefficients are absorbed by the fixed effects.

[5]Note that we omit $Female_i$, $Treated_j$, $Post_t$, and $Treated_{ij} \times Female_i$ as these coefficients are absorbed by the fixed effects.

more slowly relative to control firms. Male employees' use in treated firms, however, grows at the same rate as in control firms. Column (4) reports the result of estimating the triple difference model presented in Equation 1. The triple-difference coefficient $Treated \times Post \times Female$ compares the effect of the law on female vs male employees. Female antidepressant usage growth is 4.3 percentage points lower than male usage growth, and the effect is statistically significant. In Column (5), we additionally control for interacted firm $\times$ year-fixed effects that absorb any time-varying changes at the firm level.[6] The triple-difference coefficient in this more stringent specification is stronger both in terms of magnitude and significance.

3.2 Event study

To understand the dynamics of the effect, we report the estimation of a linear panel model with dynamic effects (Figure 1), with 2005 as the $t-1$ base year. We show the dynamic plots of the event study coefficients and their associated confidence intervals with the log of anti-depressant use as the outcome variable for female workers (green solid triangles) and male workers (red hollow circles). There are no differential trends in antidepressant use for both female and male employees and, consistent with our main results in Table 1, women in treated firms experience a significant drop in the growth rate of their anti-depressant usage after the introduction of the transparency law (Panel A). Specifically, we observe a approximately 5 percentage points lower growth in the use of antidepressant medication in each of 2006 and 2007. However, the effect is relatively short-lived and it dissipates by 2008, which explains why we find a weaker effect on average. There is also a significant impact on the incidence of use in Panel (B), though even more short-lived.

This mean-reversion three years after the law introduction could be consistent with two explanations. First, this is consistent with a hedonic adaptation documented in the literature, where important life-changing events (e.g., learning about relative income as in Perez-Truglia

[6] Firm $\times$ year fixed effects subsume the coefficient on $TreatedxPost$.

(2020)) or positive unexpected effects on employee compensation ('gift-giving') as in **?** have a temporary effect on individuals' satisfaction. In the behavioral and psychology literature, this is called the "hedonic treadmill". Second, anticipatory effects of expected improvements could have not materialized, thereby resulting in a reversion to the prior status quo. Indeed, while there was a reduction in the pay gap, it did not substantially increase female wage growth.

3.3 Placebo tests

A possible concern is that a contemporaneous event is causing our results. To address this concern, we perform several placebo tests where the treatment threshold takes each of the values between 15 and 100 employees, excluding the range of 20-50 employees. We plot the average placebo coefficients in Figure 2 Panels (a) and (b) in gray hollow markers, together with our "real" effect coefficients in solid green and red markers (respectively). The average placebo coefficients are close to zero both before and after the law in all cases, suggesting that the observed effect is specific to the 35-employee threshold.

An alternative concern could be that health coverage changes for firms above 35 employees affecting their employees' general health, including their anti-depressant use. However, unlike the U.S. where health coverage varies by firm, healthcare in Denmark is government-funded and does not depend on employment. To mitigate this concern, we estimate our main model using as dependent variables alternative drugs (cholesterol, diabetes, and hormonal). Figure 2 shows the event study plots for female (Panel C) and male (Panel D) employees. These plots indicate that there is no effect on other medications for treated employees either before or after the transparency law.

3.4 Regression-discontinuity design

An additional identification concern with our empirical strategy might be that treated and control groups are not good counterfactuals for each other, given that the control group includes smaller firms by definition. To mitigate this concern, we implement a regression discontinuity approach (RDD), given that the law applies only for firms above the 35 employees threshold. For each worker, we compute the difference between their average antidepressant usage in 2006-2007 and 2004-2005 (in logs). For this analysis, we use a narrow window around the event (two years before and two years after), effectively excluding the mean-reversion year (2008) as shown in Figure 1. Figure 3 shows the results of fitting a third-degree polynomial on each side of the discontinuity.[7] In Panel (A) we observe a clear discontinuous drop at the 35-employee threshold for female employees (significant at the 1% level), while in Panel (B) we find no significant effect for male employees. These results are consistent with those in Table 1.

4 Mechanisms

Prior literature consistently documents that wage transparency in the private sector leads to a deterioration of employee morale and loss of productivity (Breza et al.; 2018; Cullen and Perez-Truglia; 2022). This literature would predict a negative effect on mental health for employees in our setting, but this is not what we find. In the setting of these two studies, researchers conducted an experiment in a single organization to study the effect of learning about peer's wages, though the firms did not address the – now revealed – pay inequality. Our effects instead capture the new equilibrium resulting from learning about inequality combined with actions to address it.

Indeed, the Danish transparency legislation was aimed at informing employees of gender

[7] In Appendix Figure A5 we show a McCrary test indicating the distribution of number of employees is continuous around the threshold (McCrary, 2008).

wage gaps and, as such, led firms to make organizational changes to address gender inequality. As noted, the pay gap was reduced via slower growth in male wages while female wages did not change.[8] As such, a direct effect of an increase in pay of female workers seems an unlikely driver of our results. We also explicitly rule out this channel in our data. In the Appendix Table, A2, we estimate our main tests, including employee wages as a control. While we find that the direct effect of wages is to reduce antidepressant use, including this control does not affect the magnitude or significance of the main coefficient estimates.

Instead, it seems organizational changes promoting a more equitable workplace seem to have driven the improvements in employee mental health. We document a greater impact for female workers in firms where there was more room for change, that is, lower-wage workers and firms with larger pre-period pay gaps. We estimate our benchmark model for the sample of female workers above and below their median firm wage. In Figure 4 Panel (A), the results indicate a larger antidepressant reduction in the sample of women at the bottom of the wage distribution. Panel (C), in turn, considers the impact on female workers in firms with above-median pre-period gender pay gap and shows a larger reduction in anti-depressant usage for women in these firms. We find no significant change for men in either group of firms or workers. As Bennedsen et al. (2022) documents that convergence in wages at the bottom of the wage distribution was twice as large as compared to the top, this suggests that employees' antidepressant use can be affected by a sentiment of fairness in the firm.

Firms implemented other organizational changes that created a seemingly fairer environment. Table 3 replicates and extends findings in Bennedsen et al. (2022), showing that treated firms promoted and hired more women after the law change. Column (1) shows the probability of promotion for women in treated firms increases by 1.8 percentage points, though women are not promoted any faster (in terms of tenure in the firm or age at pro-

[8]In Appendix Table A7, we replicate the results of Bennedsen et al. (2022) and also find that in our sample, there is no effect on female wages and that the reduction in the gender wage gap was entirely driven by a reduction in the growth rate of male wages.

motion) relative to control firms. This may be positive, however; it suggests firms did not simply promote women prematurely simply to favor the gender wage gap statistics. In terms of flows in and out of the firm, there was a significant increase in the hiring of women.

Finally, while the reform did not appear to have an effect on the share of female managers (top of the corporate hierarchy), we examine the role of having at laest one female managers towards implementing organizational changes at the bottom of the corporate hierarchy. To this end, we analyze the sample of firms that had no women managers before the regulation versus those that had at least one female manager pre-treatment. In Figure 4 panel (E), we show the results for women. We find that the antidepressant use of female workers decreased more in firms that had female representation at the top of corporate hierarchy. We instead find no results for men, as shown in Panel (F). These results suggest that female representation at the top of the organization can help push for positive change once an opportunity arises.

5 Comparison with Other Life Events

To put the magnitude of the effect into context, we compare the change in AD use to the magnitude of other life events that are potentially impactful for the mental health of individuals.

We first consider the change in antidepressant use around the death of a spouse, for women and men separately. For these tests we use the whole Danish population excluding those with pre-existing mental health issues. We plot the event study coefficients around the year of the death of the spouse (t=0). The green horizontal line in the plots of Figure 5 shows the absolute value of our baseline magnitude. Panel A shows a sharp increase of AD use for women following a partner death, twenty times larger than to our baseline estimate (and, of course, in the opposite direction). Notably the effect dissipates within a couple of years and

reverts to the mean by year 3. Panel B shows there is an impact in male anti-depressant use following a partner death, though their response is half relative to that of the women. Similarly, the economic magnitude gradually converges to zero by year four.

In Panels C and D we repeat the same exercise for divorces as the main event. As expected the magnitude of the effect is lower than death for both women (Panel C) and men (Panel D), and between 5 to 6 times higher relative to the magnitude of our baseline. We observe that for divorces the effect starts in t=-1 because in Denmark there is a mandatory two year period of living separately before a legal divorce can be issued. We also see a similar pattern here where the use of anti-depressants reverts to the mean two years after the event.

Taken together, these results suggest that actual or anticipatory organizational changes that are expected to benefit female employees are consistent with the positive mental health outcomes for female employees in treated firms.

6 Discussion

As mental health issues become more widespread and a growing concern in many countries, it is important to understand the potential contribution of workplace factors to people's well-being. We explored the impact of pay transparency legislation on worker well-being using detailed employee and medical prescription administrative data from Denmark using two empirical strategies. As documented elsewhere, the policy of pay transparency was effective in changing the structure of pay in Denmark. Our results suggest that it also led to a short-run decline in the relative growth rate of anti-depressant use for women in affected firms. As our measure of well-being is anti-depressant use, our results are likely an upper bound. Many employees suffering with stress or job-related anxiety may not resort to using anti-depressants as treatment in the short run or for "milder" cases. We consider two competing mechanisms behind this result: the potentially negative impact of horizontal (peer) comparisons and the

potentially positive impact of organizational changes leading to the reduction of pay disparity. Taken together, our results are not consistent with a horizontal wage comparison mechanism and the evidence points to the second mechanism being the dominant force.

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Tables and Figures

Table 1: Effects of transparency, first time users

	All	**Female**	**Male**	**All**	
Outcome var: ln(ADV)	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.003	-0.034**	0.010	0.009	
	(0.007)	(0.016)	(0.008)	(0.008)	
Female × Post				0.061***	0.066***
				(0.013)	(0.014)
Treated × Post × Female				-0.043**	-0.063***
				(0.018)	(0.020)
R^2	0.604	0.611	0.598	0.605	0.648
Year FE	✓	✓	✓	✓	
Firm × Year FE					✓
Person × Firm FE	✓	✓	✓	✓	✓
# Employee-Years	163096	49913	113183	163096	161574
# Employees	46584	14583	32001	46584	46269
# Firms	3435	3049	3293	3435	3220
Dep Var Mean	0.092	0.137	0.072	0.092	0.092

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log volume of antidepressant use (in DDD) by each year. Treated is a dummy variable that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. Post takes the value of zero for 2003-2005, and value of one for 2006-2008. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

Table 2: Mechanisms: Alternative drugs

	All workers				
	(1) ln(ADV)	(2) ln(cholesterol)	(3) ln(diabetes)	(4) ln(antibiotics)	(5) ln(hormonal)
Female X Post	0.062***	-0.102***	-0.024***	0.052**	-0.375***
	(0.013)	(0.021)	(0.009)	(0.026)	(0.032)
Treated X Post	0.009	0.016	-0.007	-0.029	0.004
	(0.008)	(0.023)	(0.010)	(0.024)	(0.002)
Treated X Post X Female	-0.043**	-0.013	0.008	-0.029	0.030
	(0.018)	(0.031)	(0.013)	(0.039)	(0.048)
R^2	0.605	0.828	0.931	0.484	0.887
Year FE	No	No	No	No	No
Firm × Year FE	Yes	Yes	Yes	Yes	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	163096	93127	93127	93127	93127
# Employees	46584	30582	30582	30582	30582
# Firms	3435	3351	3351	3351	3351
Dep Var Mean	0.092	0.294	0.129	0.904	0.987

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each column reports coefficients from the same specification but with different outcome variables. All drug usage volumes are in DDD. *Treated* is a dummy that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. *Post* takes a value of zero for 2003-2005, and a value of one for 2006-2008. Controls include employee work experience and age, and firm log sales. Column (1) is repeated from Table 1 for ease of comparison. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

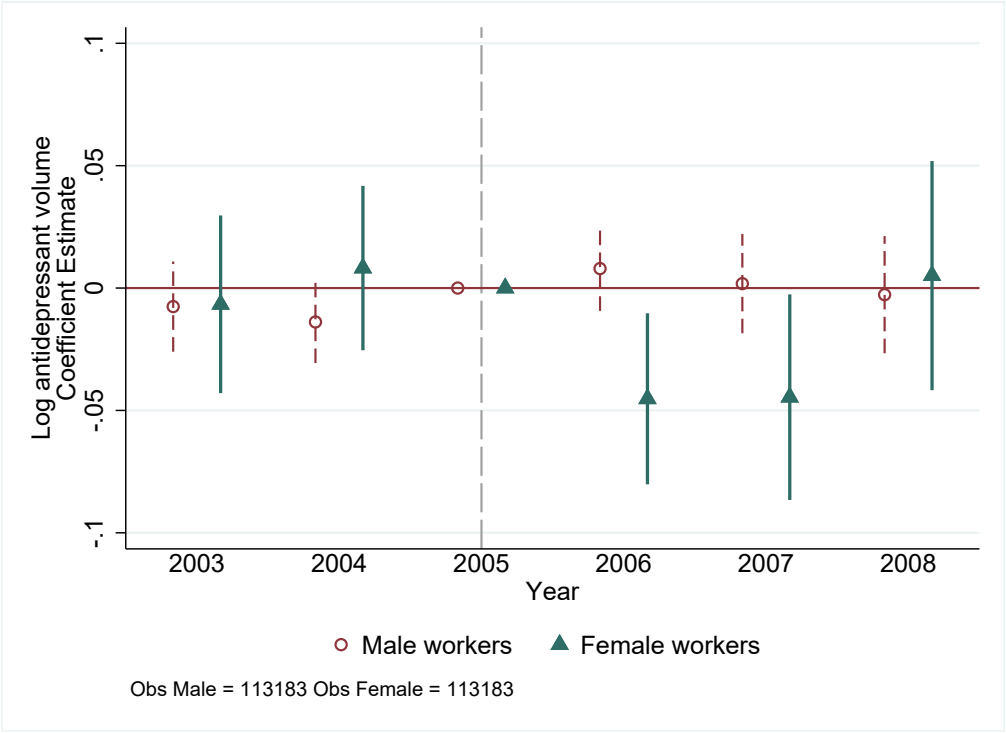
Table 3: Organizational changes

	Promotions			**Flows**	**Female managers**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Incidence	Speed	Age	% hired	Share	At least one
Treated × Post	0.018***	0.235	0.759	0.010***	0.002	0.023**
	(0.007)	(0.231)	(0.800)	(0.003)	(0.003)	(0.010)
N	62845	4432	4428	17656	14833	14833
R^2	0.322	0.519	0.381	0.399	0.745	0.747

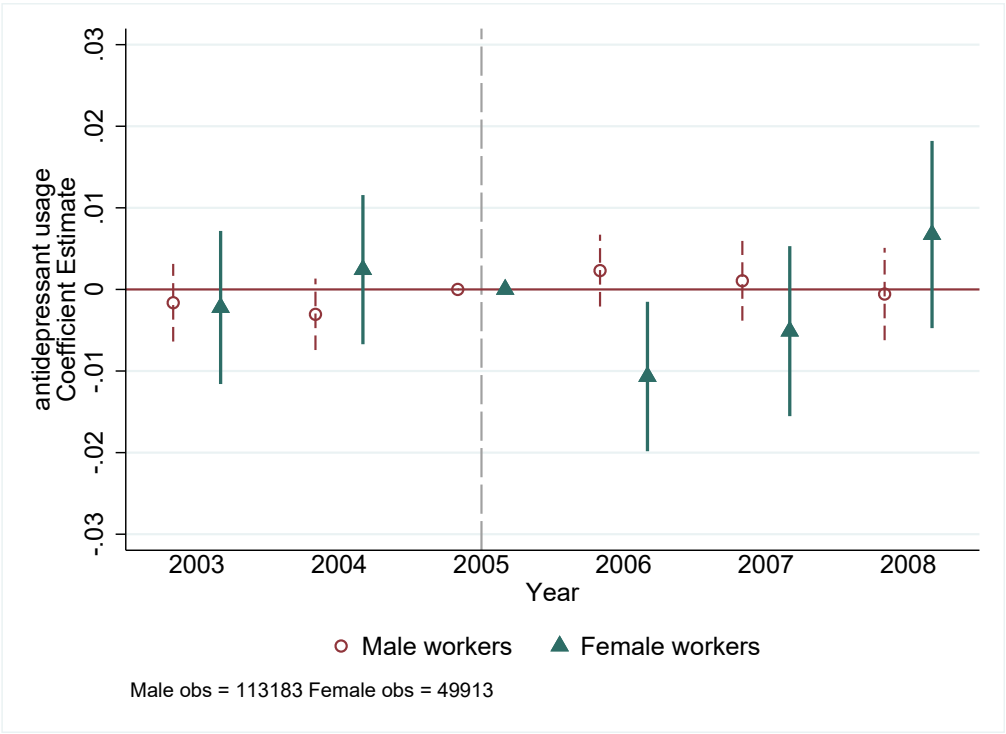
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table includes only female workers. Each column reports coefficients from the same specification but with different outcome variables. $Treated$ is a dummy that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. *Post* takes a value of zero for 2003-2005, and a value of one for 2006-2008. Incidence is a variable that takes a value of 1 if a female worker is promoted. Speed is the number of months it takes for a female worker to be promoted. Age is the female worker's age at promotion. % hired is the percentage of new hires in a firm that are women. Share (Column 5) indicates the share of female managers in a firm. At least one (Column 6) is an indicator that takes a value of 1 if the firm has at least one female manager. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

Figure 1: Effect of transparency policy on anti-depressant use

(a) Continuous log(ADV)

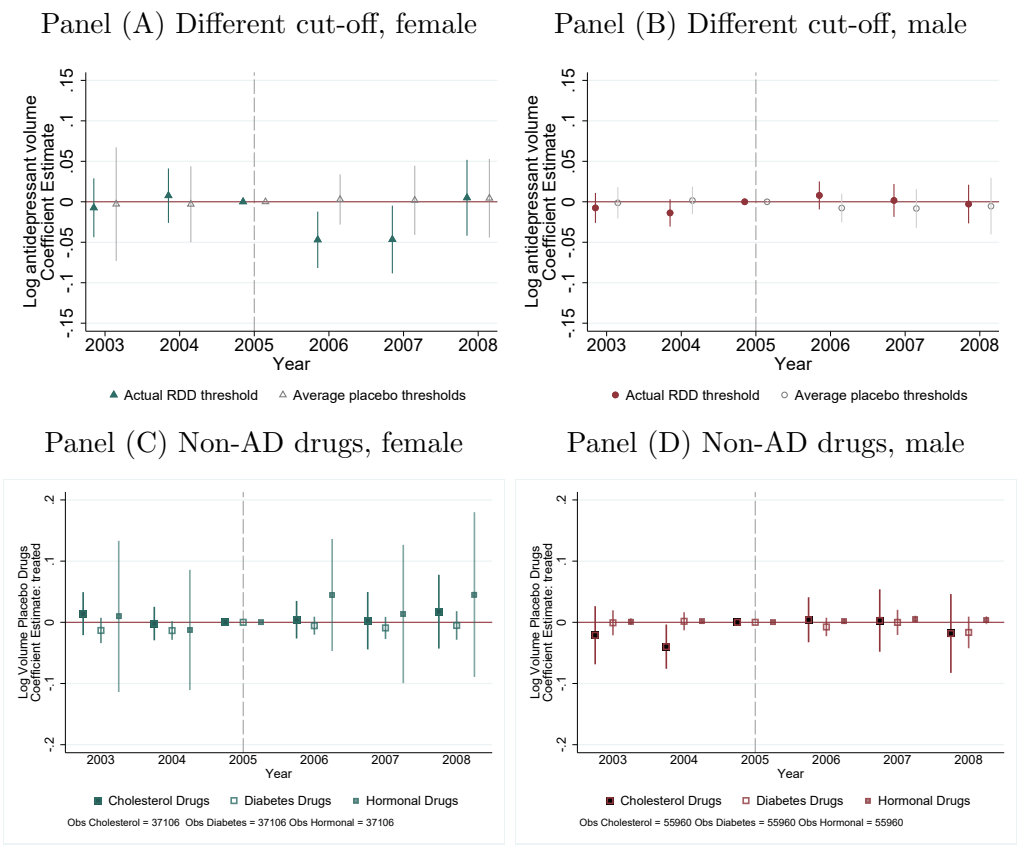


(b) Binary outcome (user = 1)



Note: Specification in these figures do not include any controls.

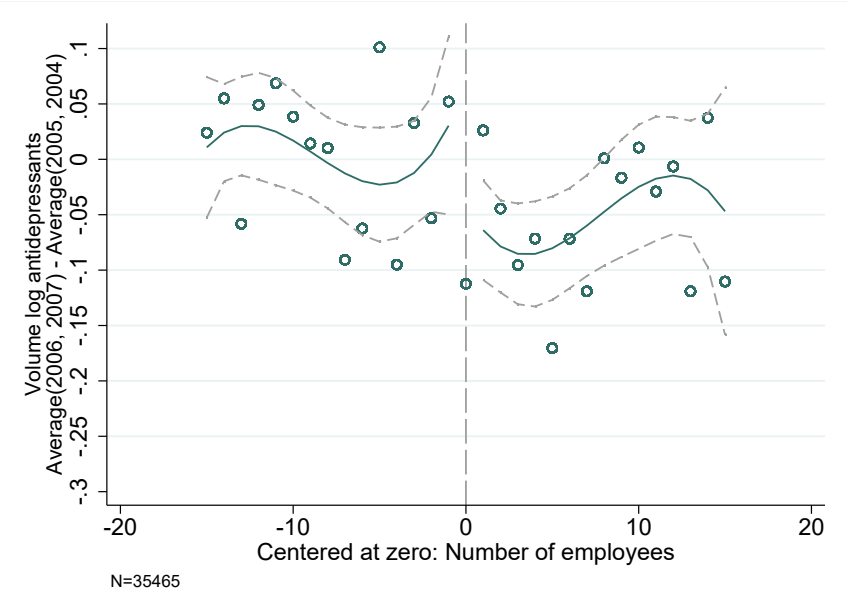
Figure 2: Placebo tests



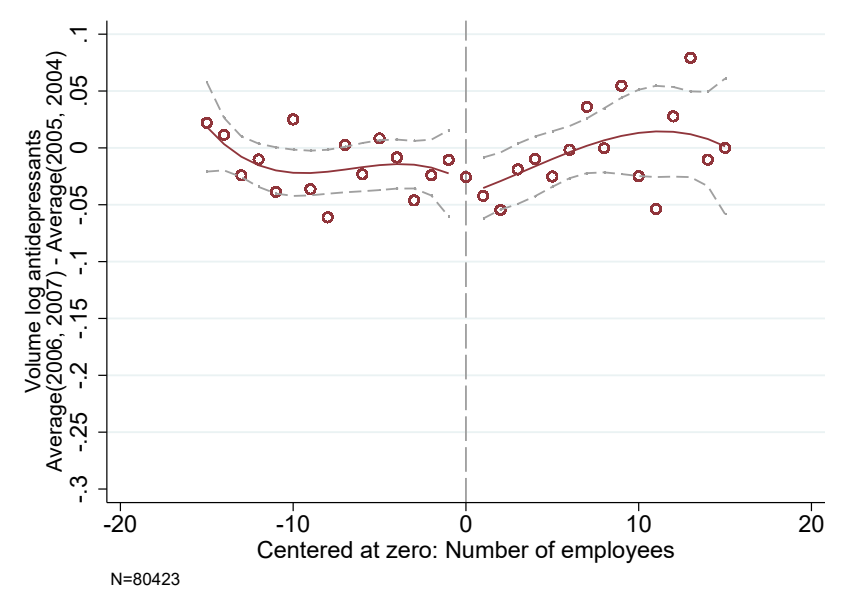
Note: For Panels A and B, we perform several placebo tests where the treatment threshold takes each of the values between 15 and 100 employees, excluding the range of 20-50 employees. We plot the **average placebo coefficients** together with our baseline coefficient estimates shown in Panels. For Panels C and D, we show an alternativce placebo test, where instead of anti-depressant volume we use volume of one available alternative drug (here, cholesterol). Table 2 shows the average results for 5 different alternative drugs.

Figure 3: RDD: Log of anti-depressant use

Panel (A) Female employees



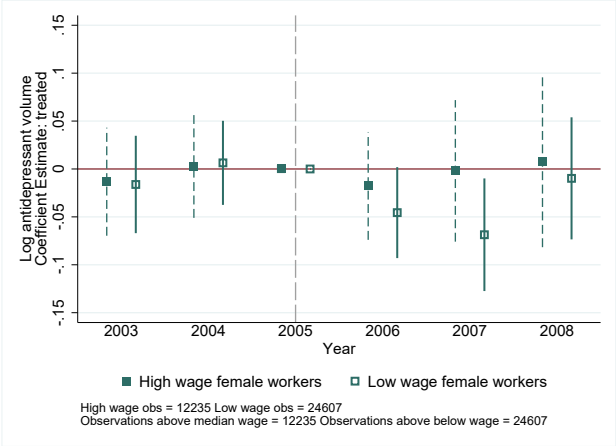
Panel (B) Male employees



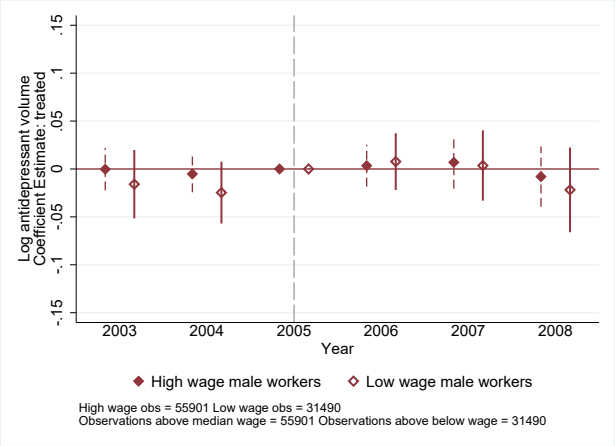
Note: The vertical axis in both panels shows the difference between employees' log usage of antidepressants in avg(2006, 2007) and avg(2004,2005). The horizontal axes show the number of employees in the firm minus 35. The employment number is based on year $t-1$. The blue center line corresponds to a third-order polynomial fit; the green dotted lines represent the 95% confidence interval. Scatter points are average changes in individual log usage between avg(2006, 2007) and avg(2004,2005) for firms with a given employee size. The p-value of the difference in Panel (a) is 0.57. The p-value of the difference in Panel (b) is 0.003.

Figure 4: Mechanisms: anti-depressant use volume, by category

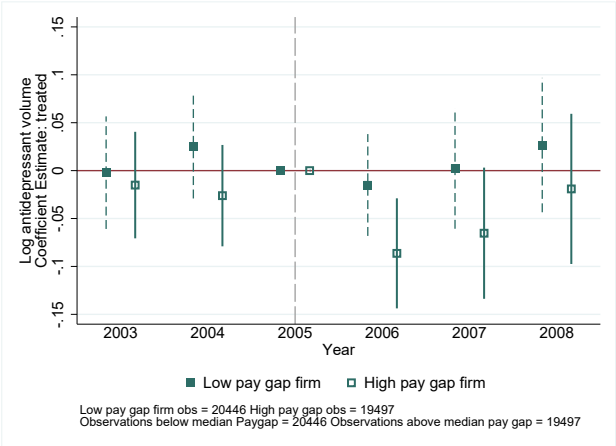
Panel (A) Median wage, female workers



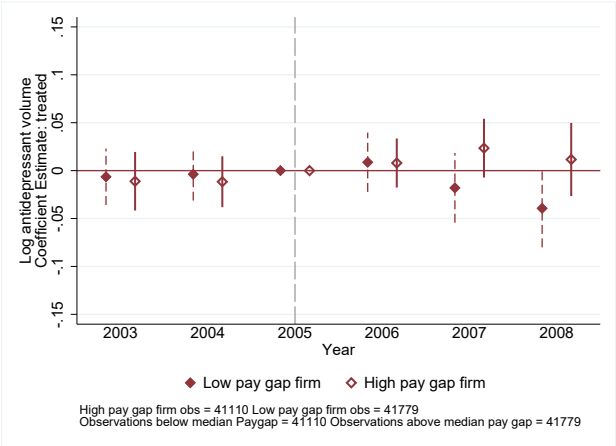
Panel (B) Median wage, male workers



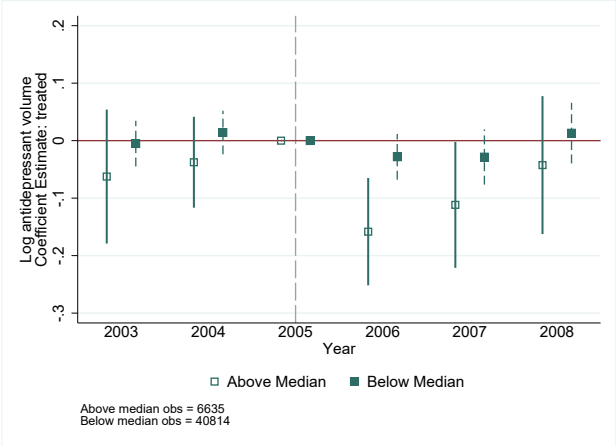
Panel (C) Median firm pay gap, female workers



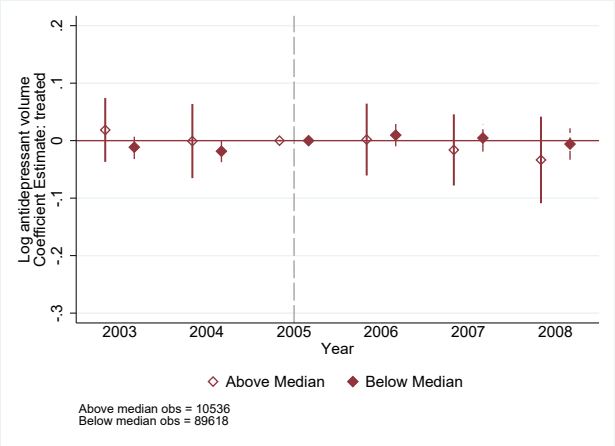
Panel (D) Median firm pay gap, male workers



Panel (E) Female manager tercile top vs bottom, female



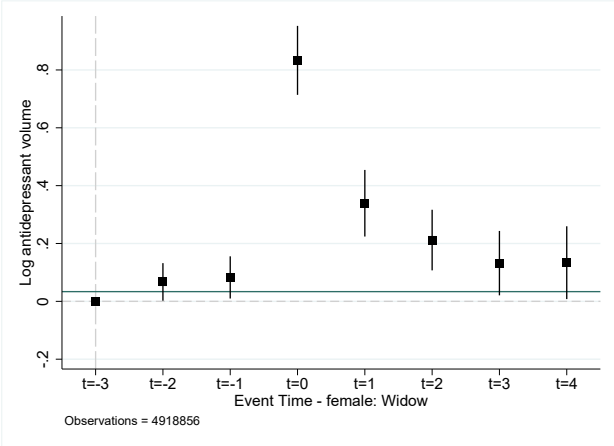
Panel (F) Female manager tercile top vs bottom, male



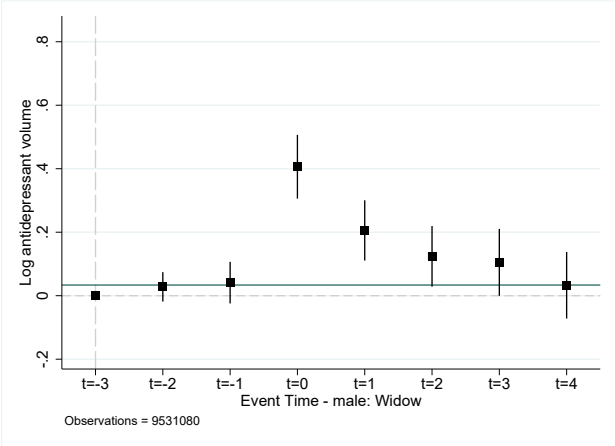
Note: All graphs have log antidepressant use as the outcome variable. Panel A and B split the sample of workers into above and below median (high wage and low wage, respectively). Panels C and D split the sample by firm-level pay gap in the pre-period. Panels E and F split the share of female managers in a firm by terciles and compares outcomes for workers in the top vs bottom terciles. Panels A, C and E show results for female workers. Panels B, D and F show results for male workers.

Figure 5: Magnitudes: comparison with other life events (Appendix?)

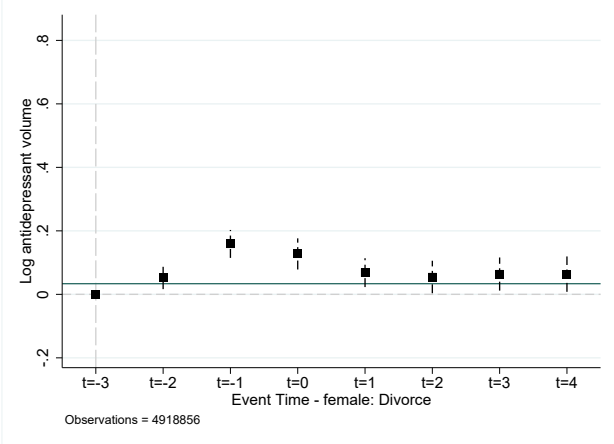
Panel (A) Partner death, female workers



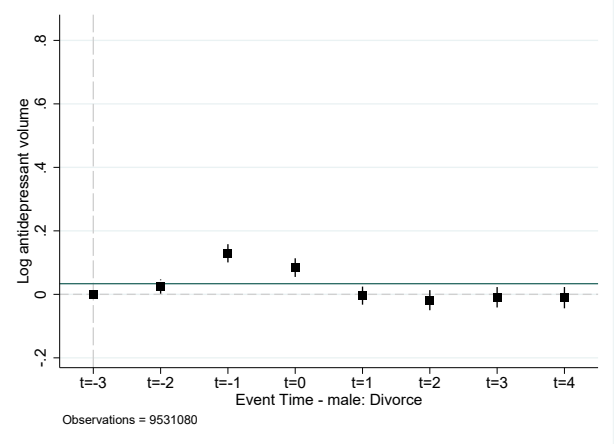
Panel (B) Partner death, male workers



Panel (C) Divorce, female workers



Panel (D) Divorce, male workers



Note: All graphs have log antidepressant use as the outcome variable. The green line denotes the benchmark result for the post-pay gap law period. Panels A and B show the results for change in anti-depressant use following a partner's death, for women and men respectively. Panels C and D show the results for change in anti-depressant use following a divorce, for women and men, respectively.

Appendices

A Appendix

Table A1: Descriptive statistics

Panel A: Firm statistics	All firms		Treated		Control		Difference
	Obs	Mean	Obs	Mean	Obs	Mean	T-Test
Turnover (US$)	3435	11.49	1172	13.98	2263	10.20	3.7837***
		(17.610)		(17.208)		(17.680)	(0.631)
Assets (US$)	3409	9.96	1162	10.81	2247	9.52	1.2834
		(41.628)		(33.089)		(45.419)	(1.504)
# Employees	3435	31.33	1172	41.70	2263	25.96	15.7325***
		(8.553)		(4.350)		(4.094)	(0.151)
Sales per employee (US$)	3435	0.34	1172	0.32	2263	0.35	-0.0327*
		(0.393)		(0.340)		(0.418)	(0.014)
% female workers	3435	33.21	1172	32.57	2263	33.54	-0.9669
		(25.066)		(23.683)		(25.752)	(0.902)
Panel B: Employee statistics	Obs	Mean	Obs	Mean	Obs	Mean	T-Test
Volume anti-depr use (DDD)	163096	3.34	72173	3.32	90923	3.36	-0.0431
		(37.488)		(37.540)		(37.446)	(0.187)
Share female employees	163096	0.31	72173	0.31	90923	0.31	0.0007
		(0.461)		(0.461)		(0.461)	(0.002)
Work experience (yr)	163096	19.91	72173	20.15	90923	19.72	0.4313***
		(10.041)		(9.975)		(10.088)	(0.050)
Age (yr)	163096	42.00	72173	42.23	90923	41.81	0.4264***
		(10.194)		(10.063)		(10.293)	(0.051)
Share w/ college deg	160627	0.14	71061	0.14	89566	0.14	0.0002
		(0.342)		(0.342)		(0.342)	(0.002)
Wages (US$)	163096	65810.39	72173	66034.20	90923	65632.74	401.4684**
		(25188.579)		(25413.685)		(25007.164)	(125.571)

Note: This table reports the summary statistics for the baseline sample of first time users. First time users is defined as employees whose usage of anti-depressants was zero prior to 2003. Panel A reports the summary statistics of the firms included in the sample. Panel B reports the summary statistics of the employees included in the sample. Standard deviation in parentheses.

Table A2: Effects of transparency, first time users including all controls

	All	**Female**	**Male**	**All**	
	(1) ln(ADV)	(2) ln(ADV)	(3) ln(ADV)	(4) ln(ADV)	(5) ln(ADV)
Treated × Post	-0.004	-0.034**	0.009	0.009	0.000
	(0.007)	(0.016)	(0.008)	(0.008)	(.)
Female × Post				0.061***	0.066***
				(0.013)	(0.014)
Treated × Post × Female				-0.043**	-0.062***
				(0.018)	(0.020)
Controls					
Log Wage	-0.023**	-0.024	-0.023*	-0.023**	-0.033***
	(0.011)	(0.023)	(0.012)	(0.011)	(0.012)
R^2	0.604	0.611	0.598	0.605	0.648
Year FE	Yes	Yes	Yes	Yes	No
Firm × Year FE	No	No	No	No	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	163096	49913	113183	163096	161574
# Employees	46584	14583	32001	46584	46269
# Firms	3435	3049	3293	3435	3220
Dep Var Mean	0.092	0.137	0.072	0.092	0.092

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log volume of antidepressant use (in DDD) by each year. Treated is a dummy variable that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. Post takes the value of zero for 2003-2005, and value of one for 2006-2008. Controls include employee work experience and age, and firm log sales, and log wages. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

Table A3: Effects of transparency, first time users including controls (except wage)

	All	**Female**	**Male**	**All**	
Outcome var: ln(ADV)	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.003	-0.034**	0.009	0.009	0.000
	(0.007)	(0.016)	(0.008)	(0.008)	(.)
Female × Post				0.062***	0.066***
				(0.013)	(0.014)
Treated × Post × Female				-0.043**	-0.063***
				(0.018)	(0.020)
Controls					
Age	✓	✓	✓	✓	✓
Experience	✓	✓	✓	✓	✓
Firm ln(sales)	✓	✓	✓	✓	✓
R^2	0.604	0.611	0.598	0.605	0.648
Year FE	✓	✓	✓	✓	
Firm × Year FE					✓
Person × Firm FE	✓	✓	✓	✓	✓
# Employee-Years	163096	49913	113183	163096	161574
# Employees	46584	14583	32001	46584	46269
# Firms	3435	3049	3293	3435	3220
Dep Var Mean	0.092	0.137	0.072	0.092	0.092

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log volume of antidepressant use (in DDD) by each year. Treated is a dummy variable that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. Post takes the value of zero for 2003-2005, and value of one for 2006-2008. Controls include employee work experience and age, and firm log sales, and log wages. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

Table A4: Effects of transparency, first time users including controls, binary outcome

	(1) ADV=1	(2) ADV=1	(3) ADV=1	(4) ADV=1	(5) ADV=1
Treated X Post	0.000	-0.005	0.003	0.003	
	(0.002)	(0.004)	(0.002)	(0.002)	
Female X Post				0.013***	0.015***
				(0.003)	(0.003)
Treated X Post X Female				-0.007*	-0.014***
				(0.004)	(0.005)
Controls					
Age	✓	✓	✓	✓	✓
Experience	✓	✓	✓	✓	✓
Firm ln(sales)	✓	✓	✓	✓	
R^2	0.533	0.534	0.529	0.533	0.585
Year FE	Yes	Yes	Yes	Yes	No
Firm × Year FE	No	No	No	No	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	163096	49913	113183	163096	161574
# Employees	46584	14583	32001	46584	46269
# Firms	3435	3049	3293	3435	3220
Dep Var Mean	0.023	0.035	0.018	0.023	0.023
Sample	**All**	**Female**	**Male**	**All**	**All**

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a binary variable that has a value of 1 if the worker used any positive value of anti-depressants in that year. Treated is a dummy variable that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. Post takes the value of zero for 2003-2005, and value of one for 2006-2008. Controls include employee work experience and age, and firm log sales, and log wages. Standard errors are two-way clustered at the individual and firm levels and reported in parentheses.

Table A5: Effects of transparency, all sample

	(1) ln(ADV)	(2) ln(ADV)	(3) ln(ADV)	(4) ln(ADV)	(5) ln(ADV)
Treated X Post	0.003	-0.025	0.017*	0.017*	
	(0.008)	(0.016)	(0.009)	(0.009)	
Female X Post				0.046***	0.054***
				(0.013)	(0.015)
Treated X Post X Female				-0.042**	-0.068***
				(0.019)	(0.021)
R^2	0.759	0.762	0.754	0.759	0.782
Year FE	Yes	Yes	Yes	Yes	No
Firm × Year FE	No	No	No	No	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	200939	66542	134397	200939	199703
# Employees	58778	19947	38831	58778	58518
# Firms	3478	3232	3355	3478	3318
Dep Var Mean	0.216	0.321	0.165	0.216	0.216
Sample	**All**	**Female**	**Male**	**All**	**All**

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effects of transparency, all sample (with controls)

	(1) ln(ADV)	(2) ln(ADV)	(3) ln(ADV)	(4) ln(ADV)	(5) ln(ADV)
Treated X Post	0.003	-0.025	0.017*	0.017*	
	(0.008)	(0.017)	(0.009)	(0.009)	
Female X Post				0.046***	0.055***
				(0.013)	(0.015)
Treated X Post X Female				-0.042**	-0.068***
				(0.019)	(0.021)
Age control	✓	✓	✓	✓	✓
Experience control	✓	✓	✓	✓	✓
ln(sales) control	✓	✓	✓	✓	
R^2	0.759	0.762	0.754	0.759	0.782
Year FE	Yes	Yes	Yes	Yes	No
Firm × Year FE	No	No	No	No	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	200939	66542	134397	200939	199703
# Employees	58778	19947	38831	58778	58518
# Firms	3478	3232	3355	3478	3318
Dep Var Mean	0.216	0.321	0.165	0.216	0.216
Sample	**All**	**Female**	**Male**	**All**	**All**

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Replication of Bennedsen et al. (2022) wage results with first-time users sample

	(1) ln(Wage)	(2) ln(Wage)	(3) ln(Wage)	(4) ln(Wage)	(5) ln(Wage)
Treated X Post	-0.012***	-0.002	-0.016***	-0.016***	
	(0.003)	(0.005)	(0.004)	(0.004)	
Female X Post				-0.003	-0.001
				(0.004)	(0.004)
Treated X Post X Female				0.014**	0.016***
				(0.005)	(0.006)
R^2	0.876	0.846	0.872	0.876	0.896
Year FE	Yes	Yes	Yes	Yes	No
Firm × Year FE	No	No	No	No	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes
# Employee-Years	163096	49913	113183	163096	161574
# Employees	46584	14583	32001	46584	46269
# Firms	3435	3049	3293	3435	3220
Dep Var Mean	12.929	12.771	12.998	12.929	12.929
Sample	**All**	**Female**	**Male**	**All**	**All**

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

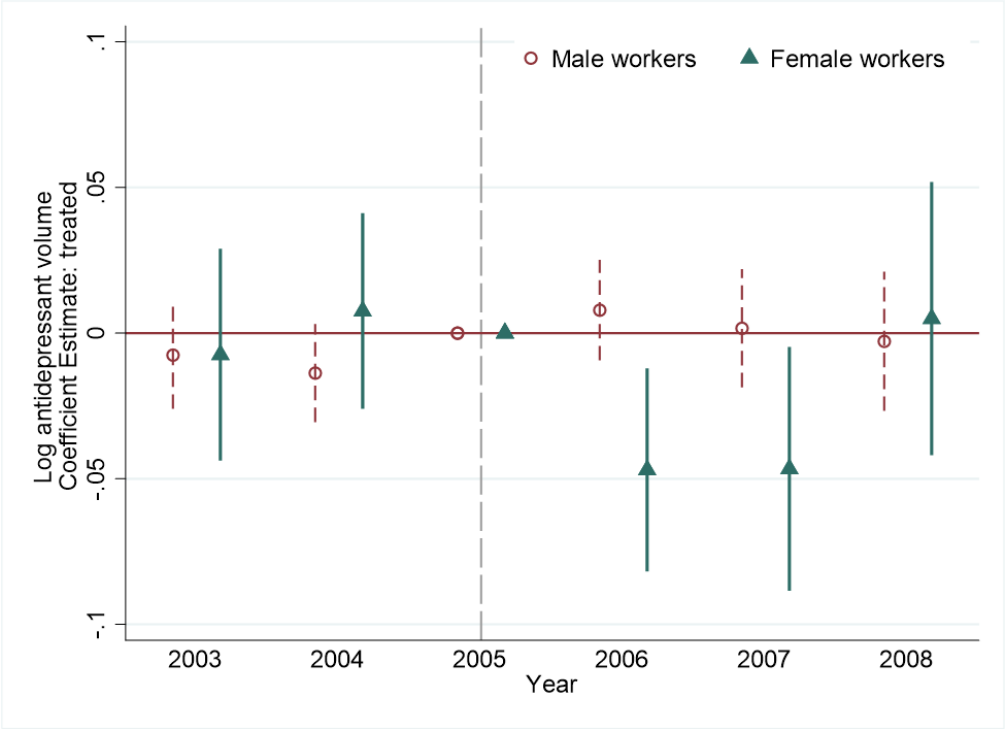
Table A8: Placebo test (alternative drugs), male and female workers

Outcome variables:	(1) ln(ADV)	(2) ln(anxiety)	(3) ln(cholesterol)	(4) ln(diabetes)	(5) ln(antibiotics)	(6) ln(hormonal)
Panel A: Male workers						
Treated X Post	0.009	0.000	0.016	-0.007	-0.029	0.002
	(0.008)	(0.003)	(0.023)	(0.010)	(0.024)	(0.002)
R^2	0.598	0.457	0.836	0.938	0.486	0.890
Number of observations						
# Employee-Years	113183	113183	56004	56004	56004	56004
# Employees	32001	32001	18698	18698	18698	18698
# Firms	3293	3293	3080	3080	3080	3080
Dep Var Mean	0.072	0.072	0.387	0.166	0.881	0.007
Panel B: Female workers						
Treated X Post	-0.034**	-0.007	0.004	0.001	-0.059*	0.035
	(0.016)	(0.007)	(0.021)	(0.009)	(0.030)	(0.048)
R^2	0.611	0.426	0.795	0.907	0.481	0.834
Number of observations						
# Employee-Years	49913	49913	37123	37123	37123	37123
# Employees	14583	14583	11884	11884	11884	11884
# Firms	3049	3049	2892	2892	2892	2892
Dep Var Mean	0.137	0.137	0.153	0.074	0.938	2.465
Fixed effects						
Year FE	No	No	No	No	No	No
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

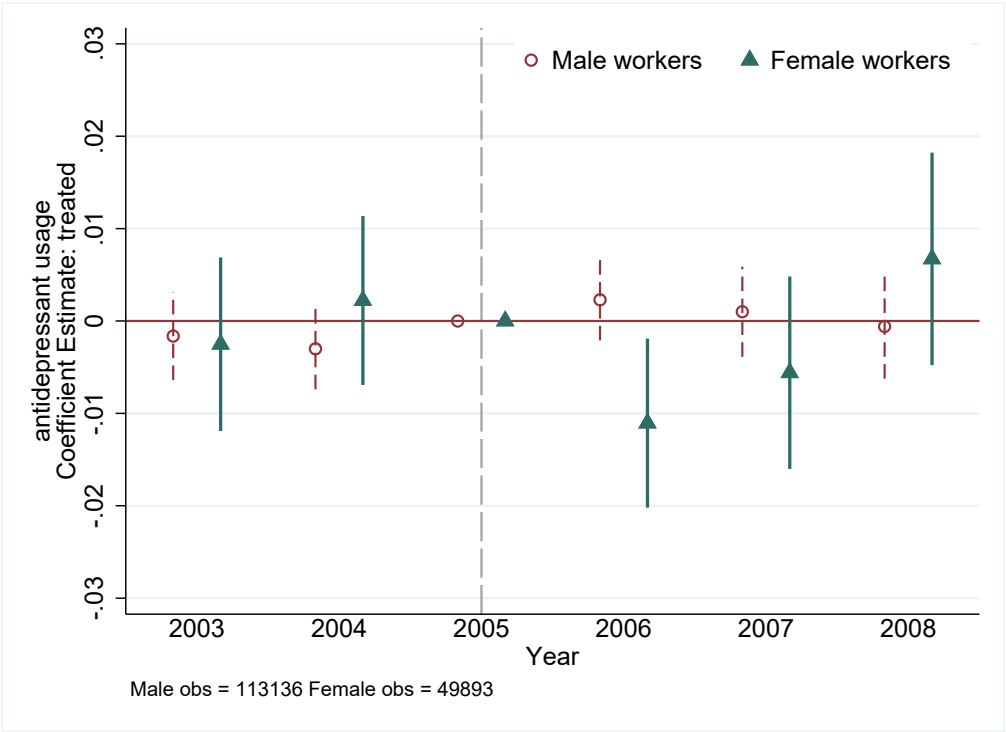
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table uses a sample of all male employees in the dataset, considering not only those who started taking mental health drugs after 2003. The dependent variable is the log volume of mental health drug use (in DDD) by each year. Treated is a dummy variable that takes the value of one for individuals working in firms with 35 to 50 employees before the introduction of the law and zero for employees in firms with 20 to 34 employees. Post takes the value of zero for 2003-2005, and value of one for 2006-2008. Controls include employee work experience and age, and firm log sales

Figure A1: Effect of transparency policy on anti-depressant use (first time users, with controls)

(a) Continuous log(ADV)

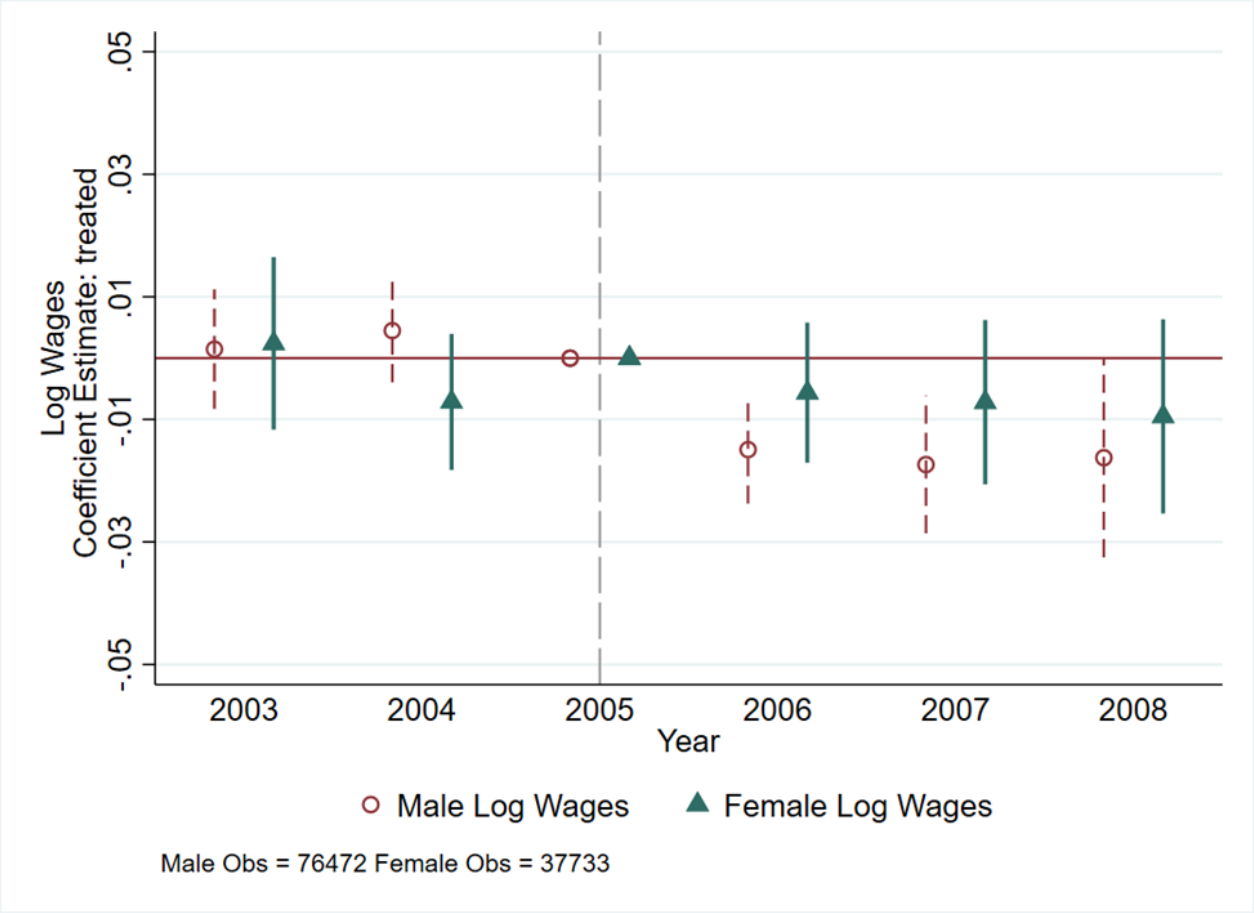


(b) Binary outcome (user = 1)



Note: Specification in these figures include controls for age, experience and log of sales.

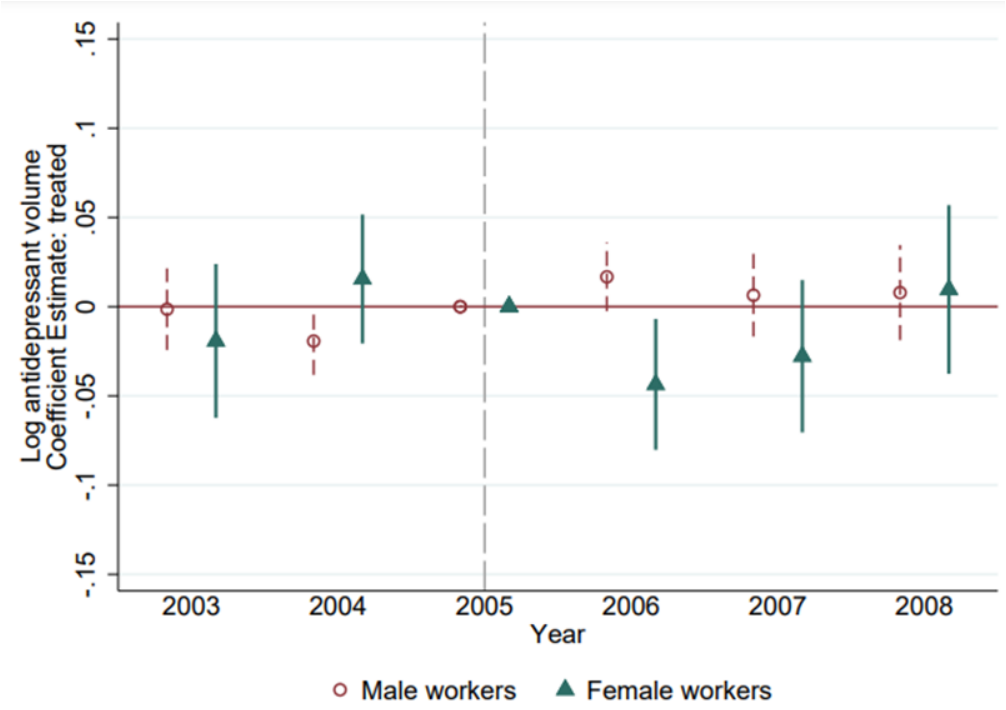
Figure A2: Effect of transparency on wages



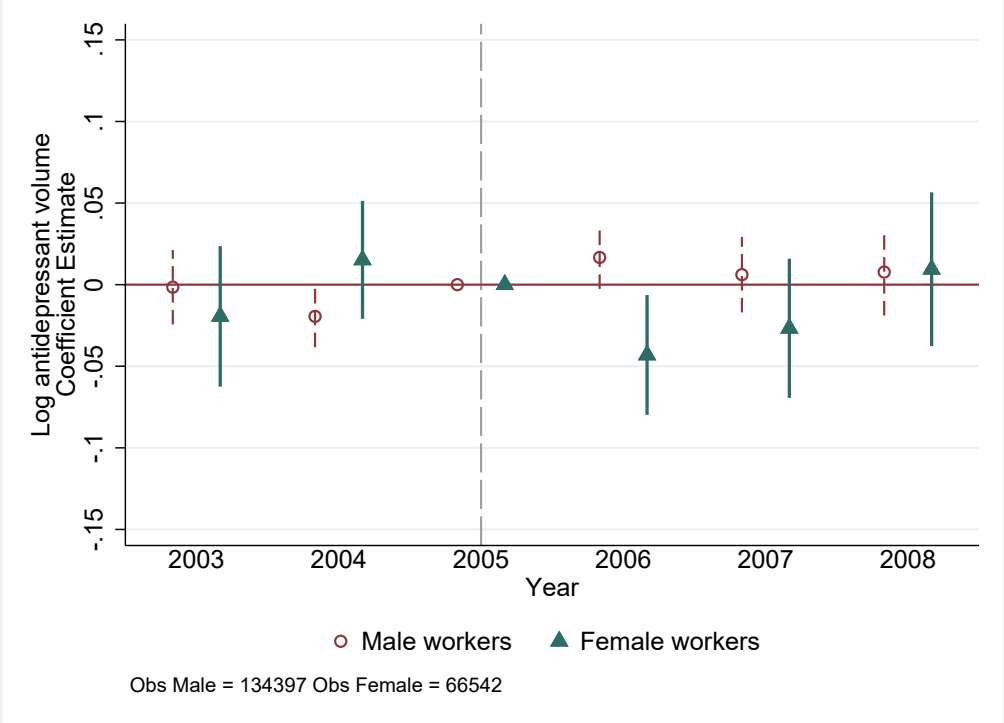
Note:

Figure A3: Robustness checks, log(ADV)

(a) Baseline result with "stayers" only



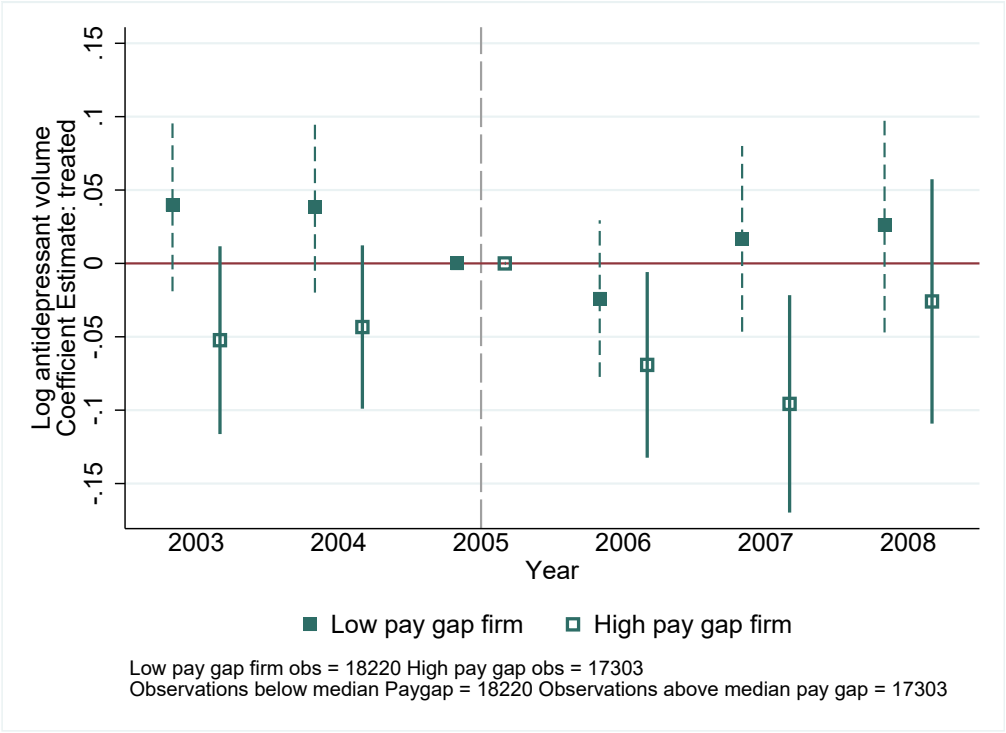
(b) Baseline result with "full sample"



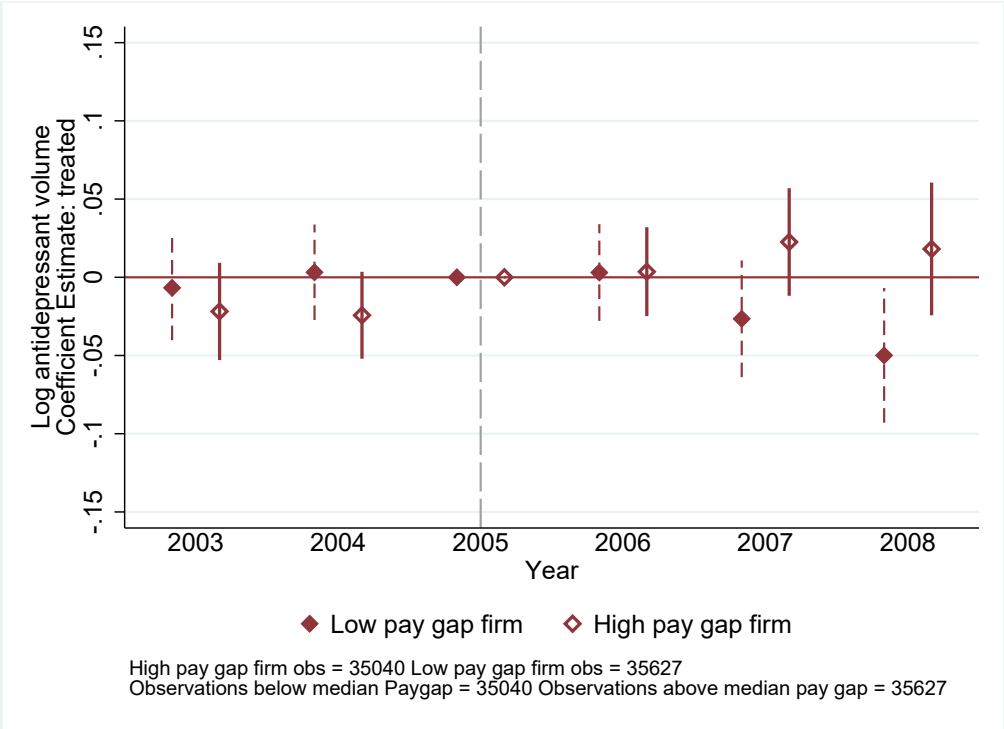
Note: Panel (a) includes only workers who stayed within the firms they were employed in before the policy change. Panel (b) includes only all workers, not just those who were first time drug users. Neither panel includes controls beyond fixed effects.

Figure A4: Median firm pay gap, 2005 base year

(a) Female



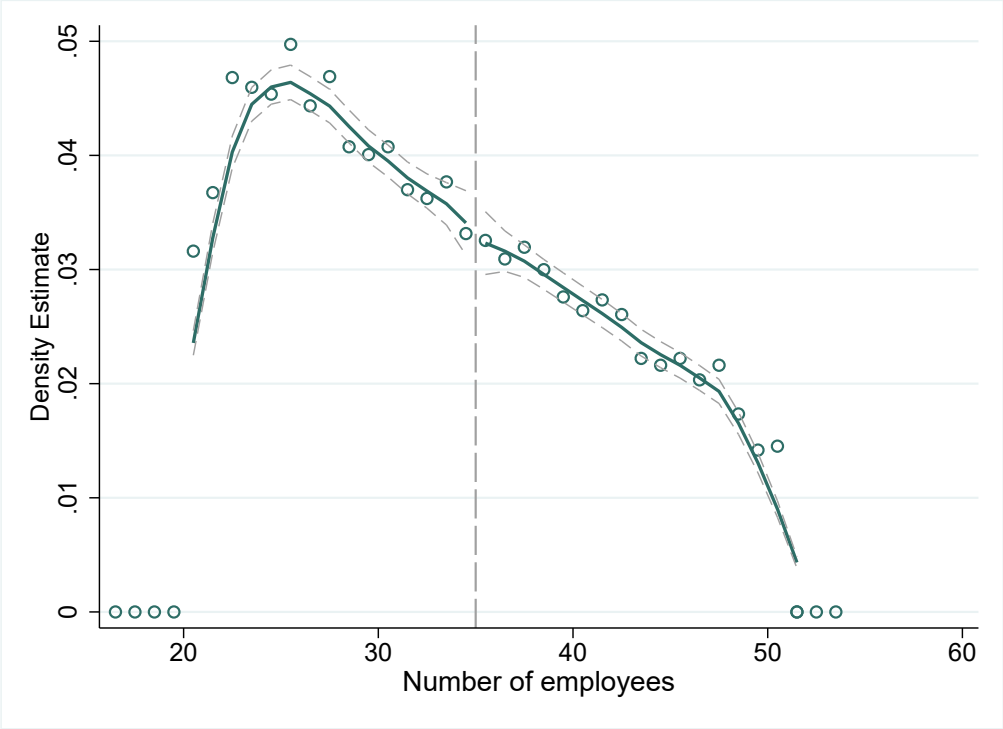
(b) Male



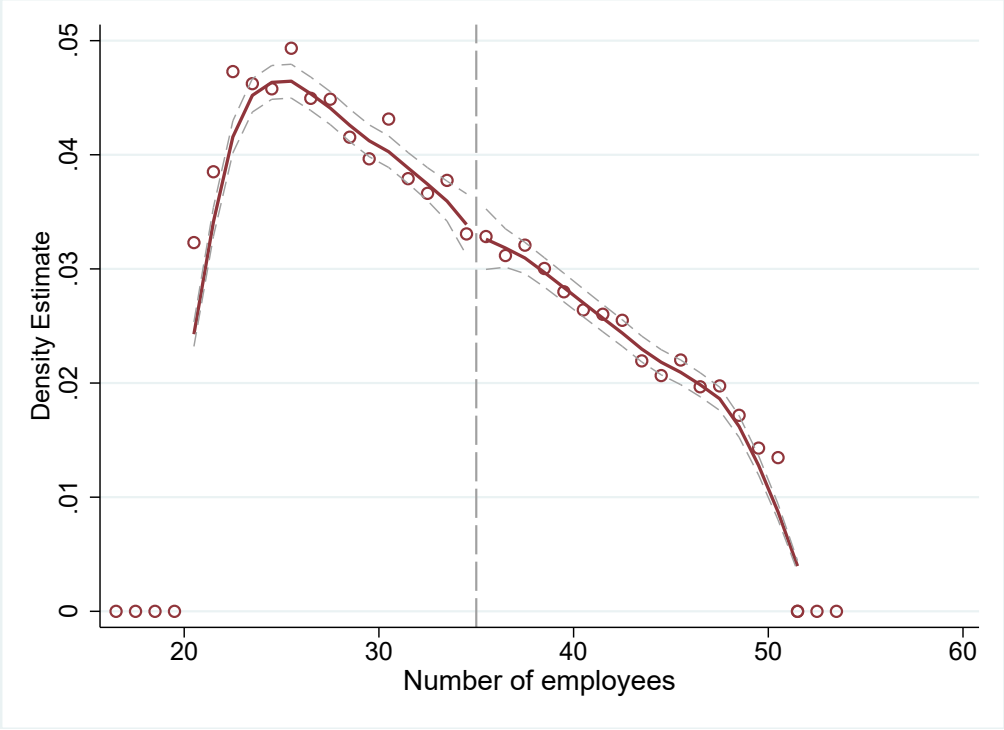
Note: Figures with no control

Figure A5: McCrary test

(a) Female workers

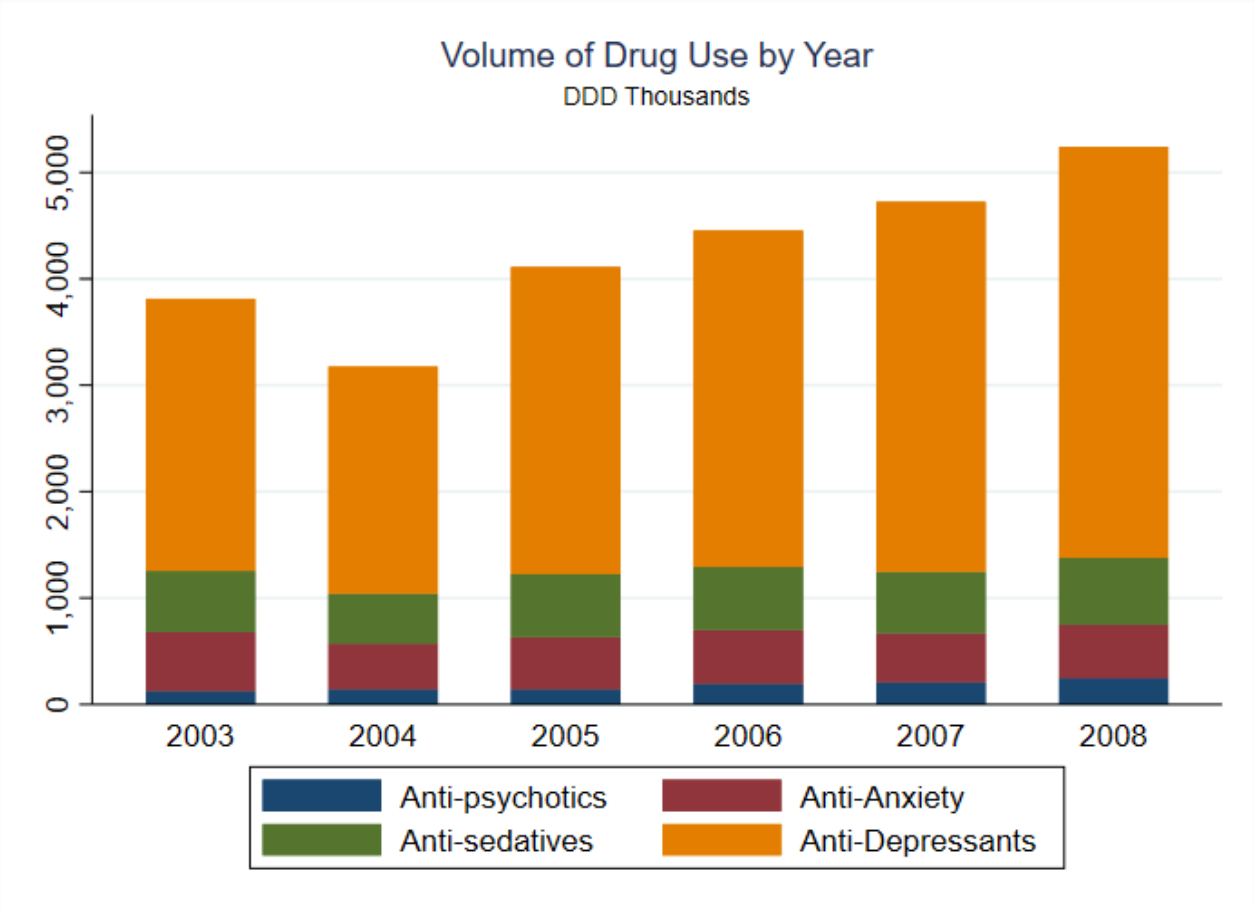


(b) Male workers



Note: Panel (a) shows the McCrary test for female workers. Panel (b) shows the McCrary test for male workers.

Figure A6: Volume of mental health drug use over time



Note: This figure includes the population of employees in Denmark and reports the volume of drug use per year for the four main drugs classified as mental health drugs.

Table A9: Panel A: Firm-Level Summary Statistics

	All Obs	Mean (All)	Treated Obs	Mean (Treated)	Control Obs	Mean (Control)	T-Test
Volume AD (DDD)	82099	1.94	36821	2.15	45278	1.78	0.3694*
		(25.980)		(28.353)		(23.876)	(0.182)
Female (%)	82099	0.30	36821	0.30	45278	0.30	0.0009
		(0.460)		(0.460)		(0.460)	(0.003)
Work Experience (years)	82099	19.19	36821	19.36	45278	19.05	0.3150***
		(9.876)		(9.792)		(9.942)	(0.069)
Age (years)	82099	41.28	36821	41.49	45278	41.10	0.3898***
		(10.190)		(10.033)		(10.312)	(0.071)
College Degree (%)	80808	0.13	36219	0.14	44589	0.13	0.0021
		(0.341)		(0.342)		(0.340)	(0.002)
USD Wages	82099	58813.41	36821	59180.81	45278	58514.63	666.1799***
		(21644.976)		(21869.229)		(21456.486)	(151.875)

Table A10: Panel A: Firm-Level Summary Statistics

	Obs	Mean	Obs	Mean	Obs	Mean	T-Test
(mean) usTurnover	3226	9.13	1104	11.39	2122	7.95	3.4430***
		(10.526)		(11.117)		(10.008)	(0.386)
(mean) usAssetsDst	3199	8.22	1093	9.10	2106	7.76	1.3344
		(38.164)		(33.762)		(40.260)	(1.423)
(mean) N_employees_no_mo_0305	3226	31.39	1104	41.76	2122	25.99	15.7765***
		(8.571)		(4.339)		(4.087)	(0.155)
(mean) us_sales_per_emp	3226	0.29	1104	0.27	2122	0.30	-0.0290*
		(0.326)		(0.273)		(0.350)	(0.012)
(mean) female_prop	3226	32.77	1104	32.03	2122	33.16	-1.1257
		(25.443)		(23.826)		(26.241)	(0.944)

Table A11: Panel A: Firm-Level Summary Statistics

	Obs	Mean	Obs T M	Mean T M	Obs T F	Mean T F	Obs C M	Mean C M	Obs C F	Mean C F	T-Test M	T-Test F
log_volume_antidep	82099	0.07	25603	0.05	11218	0.10	31524	0.05	13754	0.10	0.0022	0.0032
		(0.532)		(0.485)		(0.654)		(0.467)		(0.631)	(0.004)	(0.008)
Work Experience (years)	82099	19.19	25603	19.94	11218	18.04	31524	19.51	13754	17.98	0.4291	0.0592
		(9.876)		(10.032)		(9.081)		(10.253)		(9.101)	(0.085)	(0.116)
Age (years)	82099	41.28	25603	41.58	11218	41.28	31524	41.11	13754	41.08	0.4739	0.1978
		(10.190)		(10.188)		(9.668)		(10.514)		(9.833)	(0.087)	(0.124)
USD Wages	82099	58813.41	25603	63509.08	11218	49302.35	31524	62436.42	13754	49525.94	1072.6589	-223.5948
		(21644.976)		(22567.399)		(16344.469)		(22048.147)		(16892.582)	(187.463)	(211.803)