The Teachers Aren't Alright: School Effects on Teacher Mental Health*

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Abstract

We examine to what extent schools, as teachers' workplaces, explain differences in teachers' mental health. Using population-wide Danish register data on teachers' mental health care utilization, we document substantial and systematic differences across schools in an Empirical Bayes framework. Establishing causality through a quasi-experimental mover design, we show that the mental health of teachers moving to new schools gradually adjusts in the direction predicted by the destination school's mental health environment. Examining correlates of healthy and unhealthy school environments, we find that register-based characteristics explain only about one fifth of variation in school environments. The most robust correlates of a healthy school environment are the experience of principal and teaching staff, the psychological work environment reported by teachers, and the demands of difficult classroom situations. Based on an auxiliary within-teachers across-cohorts design, we establish causality between a higher share of girls and college-educated parents with better teacher mental health. Taken together, the results point to the school as a relevant unit for intervention, with potential leverage through organizational practices, leadership, and the management of psychosocial risks.

JEL Classification: I10, J81, I31, J24, I14, J45

Keywords: mental health, burnout, teachers, workplace conditions, sickness absence, public sector, Empirical Bayes, mover design, event study

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1 Introduction

Teachers are a central input into educational production, with lasting consequences for students' college attendance and earnings (Chetty et al., 2014b). Yet, teacher shortages have become a policy priority across OECD countries: In 2025, 39 percent of students attended schools whose principals reported that a lack of teaching staff hindered instruction (OECD, 2026). Mental health and perceived stress account strongly for difficulties in hiring and retaining teachers. In Denmark, the setting of this study, more than half of Danish teachers reported weekly or more frequent stress experience, and 34 percent had taken stress-related sick leave (Danmarks Lærerforening, 2026).[1] For 2025 alone, the Danish teacher union estimates associated wage costs at 67 million EUR (DKK 500 million), not accounting for substantial expected costs for hiring and retraining teachers, and costs of disrupting instruction and reducing student achievement (Gibbons et al., 2021; Herrmann & Rockoff, 2012).

Against this background, in this paper we examine to which degree schools as teachers' workplaces causally affect their mental health. Motivated by evidence that employers contribute substantially to differences in employee absenteeism and that firm-level working conditions affect workers' health (Bennedsen et al., 2019; Bíró & Elek, 2025; Hummels et al., 2025), we focus on Danish primary schools: In an Empirical Bayes framework, we describe differences in teacher mental health across schools, and establish causality between a teacher's work environment and their individual mental health based on a quasi-experimental mover design. We then describe the content of a healthy school environment by linking register data to survey reports by teachers and students.

We base our analysis on the universe of primary school teachers aged 25–65 in Denmark in the school years 2013/14 to 2019/20. These teachers, teaching grades 0–9, are decentrally hired by schools, while wages are collectively bargained with little variation between schools. They have access to universal healthcare, where general practitioners act as gatekeepers to subsidized specialized care for mental health issues by psychologists, psychiatrists, and in-hospital care. Teachers rank among the occupations with the highest use of mental health care in Denmark: primary and lower secondary school teachers ranked above the 89th percentile of occupations by the share of workers with doctor contacts due to mental-health-related problems in 2005, and at the 96th percentile in 2015 (Figure A1 in the appendix).

Our final sample consists of 68,351 teachers with 267,270 teacher-year observations across 1,271 schools. We link population register information on healthcare services, mental

[1] In the TALIS OECD teacher survey 2024, 10 percent of teachers reported strong impairment of their mental health, while 19 percent reported experiencing substantial work-related stress (OECD, 2025).

health-related diagnoses, drug doses, and illness-related absence and combine them via principal component analysis into a summary index of mental health care utilization. We further link the register data to two survey data sources: teacher reports on working conditions, workload, and support from colleagues and administration, and student well-being reports.

Our analysis unfolds in three steps. In a first step, we document differences in teacher mental health across schools using an Empirical Bayes (EB) framework following Walters (2024). We estimate school-level parameters of teacher mental health, and their associated standard errors, from a fixed effects regression. We then back out a latent distribution of school mental health parameters that we interpret as an empirically informed prior of the true variation between schools. Based on the empirical prior, we construct denoised EB posterior estimates by combining each school's estimated raw fixed effect with information from the estimated prior distribution, which will serve as inputs in the following quasi-experimental analysis.

In a second step, we examine to what extent the observed differences between schools reflect *causal effects* of workplaces. We apply a mover analysis to a sample of teachers moving at least once in the observation period. We compare two teachers moving between schools, similar in characteristics, previous mental health progression and origin environment, but who then end up in either a more or less healthy environment. We thus define the treatment as the difference in posterior estimates between destination and origin school, leaving out the focal moving teacher from both origin and destination parameter. The identification assumption we rely on is that teachers do not systematically sort into destination schools with respect to their own mental health and the destination school's mental health environment. We provide extensive empirical evidence for the validity of the assumption: moves to schools with worse mental health environments are as common as moves to schools with better mental health environments, move-induced changes in school environments cannot be predicted by important life events and personal constraints, and we find only a limited role for informal networks.

In a third step, we examine what observable characteristics distinguish healthier from less healthy schools. To this end, we draw on three complementary data sources: administrative register data on school-, student-, teacher-, and principal-level characteristics, and the linked survey data at the teacher and student level. For time-varying cohort composition, we establish causality through a within-teacher, across-cohort design.

This sequence of analyses produces three sets of results. First, the EB analysis reveals substantial heterogeneity across schools in teachers' mental health, with about half of the variance of raw school estimates identified as sampling noise. The empirical prior

distribution implies an interquartile range of schools accounting for 5.1 absence days per teacher and year, almost a third of the mean, and 68 percent of the mean in average specialist care utilization. Register-based school means of teacher mental health correlate with survey-reported stress, sick leave due to working conditions, and leave intentions. Teachers in less healthy schools move to other schools but do not leave the profession more often, and receive no wage compensation. We show that posterior estimates are close to forecast-unbiased predictors of out-of-sample school outcomes: regressing yearly means of teacher health on leave-one-year-out shrunk posterior means yields a coefficient of 0.91.

Second, the mover analysis supports a causal interpretation of the observed differences. While pre-trends are flat, after the move, teachers' mental health gradually adjusts in the direction predicted by the destination school's mental health fixed effect. Within three years, teachers take on about 60 percent of the difference in school mental health fixed effects into individual mental health care utilization. Changes in teachers' mental health are larger following moves between schools with greater differences in their mental health fixed effects, indicating a dose-response pattern. Estimated event patterns are insensitive to excluding moves coinciding with life events such as changing address, expecting a child, and changing cohabitation status, and to the inclusion of observable characteristics of origin and destination schools into the estimation of school fixed effects.

Third, examining correlates of healthy and unhealthy school environments, we find that register-based characteristics explain only about one fifth of the variation in school environments. The most robust correlates are the tenure of the principal and the experience of the teaching staff, the psychological work environment reported by teachers, and the demands of handling difficult classroom situations. In contrast, features that are readily visible from outside, such as school size, location, class size, and most dimensions of students' socio-demographic composition, are only weakly related to school mental health, supporting the identification assumption of the mover analysis: teachers may not easily foresee the health environment of their destination school. These analyses remain descriptive. Auxiliary within-teacher across cohorts analyses establish causality between a higher share of female students, better-educated parents and improved teacher mental health.

These findings speak directly to current policy debates on teacher shortages, working conditions, and retention. If heterogeneity in teacher mental health reflects workplace-specific conditions rather than mainly individual characteristics or student composition, policies focused solely on recruitment, training, or individual resilience are unlikely to be sufficient. In fact, the results point to the school as a relevant unit for intervention, with potential leverage through organizational practices, leadership, and the management of

psychosocial risks. More broadly, the analysis suggests that differences in school organization can generate substantial variation in teacher well-being even within a highly regulated public-sector profession. Improving workplace conditions in schools may therefore contribute to stabilizing the teacher workforce and mitigating mental health-related turnover.

Our results have practical implications for the identification of at-risk schools. Observable school characteristics explain only a limited share of differences in teacher mental health, making outcome-based measures potentially useful for targeting audits and support. Such measures, however, contain substantial estimation noise. In an illustrative exercise targeting the 10 percent of schools with the most adverse estimated mental health environments, using Empirical Bayes posterior estimates instead of raw school estimates changes more than one third of the selected schools. Our out-of-sample validation shows that posterior estimates provide better-calibrated predictions of subsequent outcomes, supporting their use to guide further assessment and targeted support.

We contribute to four strands of the literature. First, and most directly, we contribute to a growing literature showing that employers and workplace conditions affect workers' health. Recent studies use linked employer–employee and health records together with worker mobility to document persistent firm effects on health-care utilization and mental health (Ahammer et al., 2023; Bíró & Elek, 2025; Nekoei et al., 2024). Complementary evidence exploits organizational shocks, such as downsizing and mergers, to show that changes in the workplace can causally affect workers' physical and mental health (Ahammer et al., 2025; Bach et al., 2026). Related work documents substantial employer components in employee absenteeism (Bennedsen et al., 2019). We extend this literature by identifying workplace effects on mental health across organizational units within the same occupation and a highly regulated public-sector labor market. Using teacher moves across schools, we further show that these workplace differences are transmitted to workers' own mental health rather than solely reflecting systematic sorting across workplaces.

Second, we relate to the literature estimating persistent school effects on student outcomes. A large body of work shows that schools differ systematically in their effects on test scores, non-cognitive skills, and longer-run educational and labor-market outcomes (Angrist et al., 2016, 2017; Beuermann et al., 2023; Deming, 2014; Jackson et al., 2024; Kirkebøen, 2022). This literature typically treats students as the relevant outcome population and asks whether attending different schools changes students' trajectories. We apply the same organizational perspective to teachers: schools are not only production environments for students, but also workplaces for employees. We document systematic school-level heterogeneity in teacher mental health and show that these differences predict changes in teachers' own outcomes after moving between schools.

Third, we contribute to research on teachers' working conditions and their consequences for teacher health and labor supply. A substantial literature shows that teachers value workplace attributes such as class size, student achievement, and workload, and that these characteristics affect mobility and retention (Bonhomme et al., 2016; Falch & Strøm, 2005; Mont & Rees, 1996). More recent quasi-experimental evidence shows that student composition can affect teacher turnover and health (Jackson & Bruegmann, 2009; Karbownik, 2020; Karbownik et al., 2026), while other work studies the effects of accountability regimes, school leadership, and school-specific management practices on teacher burnout, absence, and retention (Bennedsen & Schlier, 2025; Svaleryd & Vlachos, 2023). Rather than isolating the effect of one particular workplace attribute, we estimate an omnibus measure of the mental-health environment of a school and subsequently ask which observable characteristics account for this heterogeneity. We find that observable differences in student composition, school characteristics, and teacher composition explain only a limited share of the substantial variation in workplace mental-health environments across schools.

Fourth, our results relate to work on management quality and non-pecuniary workplace amenities. A growing literature emphasizes that workers value dimensions of jobs beyond wages and that these amenities can generate economically meaningful differences across employers (Lamadon et al., 2022; Sockin, 2022; Sorkin, 2018). Management is one potentially important component of such workplace quality: managerial practices and people-management skills affect worker satisfaction, productivity, and retention (Hoffman & Tadelis, 2021). These dimensions are difficult for prospective employees to observe before joining an organization, potentially limiting sorting toward workplaces that better match workers' preferences. Consistent with this interpretation, we find that readily observable characteristics account for only a small fraction of the systematic differences in teacher mental health across schools, leaving scope for less directly observed organizational attributes, such as leadership style, collegial relations, and organizational climate.

The paper is structured as follows. Section 2 describes the setting of primary schools and mental health care in Denmark. The data sources and sample selection are described in Section 3. Section 4 outlines the empirical strategy and results of measuring systematic school-level heterogeneity in teacher mental health in an Empirical Bayes framework. The mover analysis is shown in Section 5 in addition to an extensive discussion and empirical evidence of its identification assumption. In Section 6 we address the question of what describes a healthy school, followed by Section 7 containing a causal analysis of the effect of student characteristics on teacher mental health. We examine the policy relevance of our findings in Section 8 and conclude in Section 9.

2 Setting

2.1 Primary schools in Denmark

Our study focuses on Danish public primary and lower secondary schools (*folkeskole*), which provide comprehensive education for children typically aged 6 to 16, covering grades 0 (kindergarten class) through 9.

Teachers in Danish public schools are employed by municipalities, while recruitment takes place at the school level. Teachers apply for positions at individual schools, where principals oversee recruitment and determine the competencies required for vacancies. Mobility between schools can occur through applications for vacancies or transfers within the employing municipality. Such transfers may be requested by teachers or initiated by the employer, for example to address staffing imbalances across schools.

Schools are organized by their principals, who decide autonomously within municipal budgets and applicable regulations. Especially following reformed working-time rules introduced in 2014/15, principals have extended discretion over teacher workload. Teacher pay, on the other hand, is primarily determined through national collective bargaining between municipal employers and teachers' unions. The salary structure provides experience-related progression and additional payments reflecting qualifications and specific responsibilities, with scope for locally negotiated supplements that generate some variation across municipalities and individual teachers.

Monthly salaried teachers receive continued full pay during sickness absence. Teachers must report absence according to workplace procedures, and employers may request medical documentation. Schools arrange cover for absent teachers through a combination of substitution by colleagues and substitute staff.

2.2 Mental Health Care in Denmark

The Danish healthcare system provides universal health coverage, primarily funded through taxation. General practitioner (GP) consultations are free of charge, as are publicly reimbursed consultations with office-based psychiatrists and psychiatric treatment in hospitals. GPs play a central role as the primary entry point into the mental health care system, providing initial assessments, treatment, referrals to specialists, and follow-up care. Access to publicly reimbursed specialist treatment generally requires a GP referral. Outpatient psychological treatment under the publicly subsidized scheme requires a GP

referral and is restricted to specified conditions and patient groups. Eligible patients generally face a copayment of 40% of the treatment fee.[2]

Mental health care utilization among teachers in Denmark is high and has increased over time. Figure A1 in the appendix shows that primary and lower secondary school teachers consistently ranked above the 89th percentile of occupational groups ranked by the share of individuals with mental-health-related health care contacts in 2005, and have increased to the 96th percentile in 2015. Recent Danish surveys provide complementary evidence of work-related psychological strain: In a 2026 membership survey by the Danish Union of Teachers, 52% of respondents reported feeling stressed at work daily or weekly, and 34% reported having taken sickness absence due to stress at some point during their teaching career.[3]

3 Data

To pursue our empirical exercise, we link three data sources: (1) Danish population register data on teachers, their students and their students' parents covering mental health care utilization, sickness absence, and socioeconomic background information, (2) surveys from the Danish Union of Teachers on teachers' work environments, and (3) data from the Danish Student Well-being Survey. In the following, we describe these data sources and the resulting sample in detail.

3.1 Danish Register Data

We draw from several Danish population registers to construct a yearly panel of teachers in public primary schools, linked to students and students' parents. Schools as teachers' workplaces are accordingly defined as all teachers and students being assigned to the same institution within a year.

For each teacher, we collect information from various registers on their mental health care utilization. The *Danish National Health Service Registry* (SSSY) documents healthcare services conducted by non-hospital practitioners in the universal public health care system. It records the specialty of the provider (psychiatrists or psychologists vs. dentists)

[2]Individuals may also obtain psychological treatment outside the publicly subsidized scheme, either paying out of pocket or through private health insurance. Fully privately financed psychologist consultations are not captured in the National Health Service Register. Private insurance may also cover copayments for publicly subsidized consultations, which remain recorded in the register.

[3]Danmarks Lærerforening (2026), *Stress blandt lærere og børnehaveklasseledere*, https://www.dlf.org/media/kvflvttt/stress-blandt-laerere-og-boernehaveklasseledere.pdf.

as well as treatment codes, allowing us to distinguish general practitioner (GP) contacts for mental health reasons from other GP contacts. The *Patient Registry and Psychiatric Central Research Registry* allow us to construct an indicator for having received a mental-health-related diagnosis in a hospital.[4][5] The *Register of Pharmaceutical Sales* (LMDB) contains information on all sales of medicines in pharmacies, or dispensed by doctors and hospitals. The register contains individual-level information on drug types (ATC codes), dispensing dates, and quantities. We specifically select ATC codes typically dispensed for mental health issues, namely psycholeptics and psychoanaleptics within the nervous system class. Prescribed medication can be linked to the individual. Finally, we draw information on teachers' absence from the *Periods of Absence Registry* (FRPE) containing the exact days as well as the cause of absence, allowing us to distinguish illness- from non-illness-related absence. We cannot distinguish by the exact diagnosis or type of sickness, including mental illness, that led to a specific absence. Both short- and long-term absence are included from the first day of an absence.

For our analyses, we combine the information from the registers described above into a single summary measure of teacher mental health. Specifically, for each school year, the index incorporates the following outcomes: numbers of GP visits for psychological reasons, visits to mental health specialists (psychologists and psychiatrists), hospital stays involving a mental health diagnosis, distinct mental health diagnoses recorded in hospital data, the defined daily doses of mental health medication, and days of illness absence. To aggregate these measures, we first standardize each component and then extract the first principal component from a principal component analysis. We standardize the resulting index to have mean zero and standard deviation one. Higher values of the index indicate worse mental health, reflected in greater mental-health-related health care utilization and illness absence.

We further enrich our data by a range of sociodemographic characteristics for teachers, students and their parents. Covariates at the teacher level are gender, age, non-Scandinavian nationality, years of labor market experience, and formal education as a teacher. We rely on a novel data source, *UDDLAERER*, that links teachers to their stu-

[4]As mental-health-related, we consider both primary and secondary diagnoses with the following ICD-10 (2019) codes: all F codes (mental and behavioural disorders), Z00.4 (general psychiatric examination), Z04.6 (general psychiatric examination requested by an authority), Z03.2 (observation for suspected mental and behavioural disorders), Z59–Z65 (problems related to socioeconomic and psychosocial circumstances), Z71.3–Z71.6 (counselling and surveillance concerning diet, alcohol, drugs, and tobacco), Z72–Z73 (problems related to lifestyle and life-management difficulties), and X60–X84 (intentional self-harm).

[5]In constructing measures of diagnoses, we need to account for a data break in 2018 (described in Plana-Ripoll et al. (2025)) removing a distinction between somatic (physical) hospitals (LPR2) and psychiatric hospitals, subsuming all hospital contacts in a single register (LPR3). Up to 2018, we combine the Danish Psychiatric Central Research Registry with the general Patient Registry (LPR2). From 2019 onwards, the source is the new LPR3.

dents on the subject-year level. On the student level, we include sex, non-Scandinavian nationality, and a measure of whether the student ever had a doctor contact for mental health reasons, similar to the teacher outcome, and test scores on the Danish National Tests in Danish (grades 2, 4, 6, and 8) and Math (grades 3, 6, and 8) during *folkeskole*. On the students' parents' level, we include an indicator that at least one parent has a college degree or more, and an indicator for parents' cohabitation status. At the school level, we construct measures of the average class size, school size, and school location.

3.2 Surveys from the Danish Union of Teachers

We complement the register data with survey data from the Danish Union of Teachers (DLF). This data allows us to examine how objective measures of mental health care usage relate to subjective perceptions of stress and experiences of working condition-related illness absence, and how these are related to teachers' perceptions of their work environment.

The DLF is Denmark's largest trade union and professional association for public school teachers and educators. Every two years, the union surveys teachers with respect to topics such as job satisfaction, working conditions, and perceived stress. Surveys in 2017 and 2019, covering 11,766 and 11,320 respondents, respectively, are available as school-year means for 1,026 schools with at least 5 respondents. We average these school-averages when two survey years were available (weighted by the number of respondents each year). The responses are linked to the register data via school names and municipality.

For the purpose of this study, we draw information on teachers' perceptions of their working conditions, workload, support from colleagues and administration, and overall job satisfaction (Table A1), as well as intentions to leave the profession and associated justifications (Table A1). We further use survey questions about teachers' perceived stress level and illness absence due to working conditions (Table A1).

3.3 The Danish Student Well-being Survey

We further complement the register data with data from the Danish Student Well-being Survey (DWS), a mandatory nationwide survey for students in public schools. Introduced in 2014 as part of the public school reform, the survey contains 40 items (administered to grades 4–9) covering topics such as student well-being, classroom climate, and academic support and progress. The data cover responses from 733,132 students at 1,268 schools. Table A2 in the appendix provides a full overview of the used items.

3.4 Sample Selection and Descriptive Statistics

From the linked registers, we select a sample covering the universe of primary school teachers during school years 2013/14 to 2019/20. We exclude teachers of atypical classes (such as special needs students, or covering several grade levels at once), and from schools with fewer than 10 teachers (for data protection reasons, affecting fewer than 0.1% of teachers). We focus on teachers aged 25–65, specifically to abstract from temporary teacher aid contracts during education, and retirement-induced moves. We further exclude teachers who teach at more than one public primary school, which accounts for less than 2% of our sample.

Table 1: Descriptive Statistics

	Teacher-Year Level				School-Year Level			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Teacher Outcomes								
Illness Absence Days	16.09	38.37	0.00	366.00	15.86	8.76	0.00	98.50
MH Contact	0.10	0.30	0.00	1.00	0.10	0.06	0.00	1.00
MH Medication	0.09	0.29	0.00	1.00	0.09	0.06	0.00	1.00
MH Index	−0.00	1.00	−0.32	28.44	−0.00	0.21	−0.32	3.69
Teacher Characteristics								
Female	0.70	0.46	0.00	1.00	0.71	0.09	0.00	1.00
Age	44.61	10.36	25.00	65.00	44.97	2.91	32.00	62.00
Non-Scandi. Immigrant	0.03	0.18	0.00	1.00	0.03	0.04	0.00	0.43
Experience	16.17	9.44	0.00	38.98	16.50	2.70	5.14	33.84
Formal Education as Teacher	0.80	0.40	0.00	1.00	0.80	0.11	0.00	1.00
Student Characteristics								
Female	0.49	0.08	0.00	1.00	0.49	0.04	0.00	1.00
Non-Scandi. Immigrant	0.13	0.16	0.00	1.00	0.12	0.14	0.00	1.00
MH Contact	0.03	0.03	0.00	0.59	0.03	0.02	0.00	0.23
College Edu. (Any Parent)	0.50	0.17	0.00	1.00	0.48	0.15	0.00	1.00
Parents Not Cohabiting	0.29	0.11	0.00	1.00	0.29	0.08	0.00	1.00
Test Scores								
Danish Test Score	0.02	0.39	−4.74	2.60	−0.02	0.30	−3.02	1.22
Math Test Score	0.00	0.42	−4.49	2.41	−0.01	0.33	−1.65	1.75
Class Danish Test Score SD	0.89	0.17	0.13	2.52	0.87	0.12	0.18	2.45
Class Math Test Score SD	0.90	0.16	0.09	2.02	0.88	0.13	0.07	2.01
School Characteristics								
Average Class Size					20.78	4.24	1.00	242.00
No. of Students this Year					423.28	239.12	1.00	1370.00
Urban Municipality					0.56	0.50	0.00	1.00
No. of Teachers this Year					35.37	17.89	1.00	108.00
No. Teachers in Total					69.22	34.28	10.00	230.00
No. Teacher-Year Observations					230.53	121.13	10.00	708.00
Observations (Year)	267,270				7,556			
Number of Teachers/Schools	68,351				1,271			

Note: The table reports descriptive statistics for our analysis sample at the teacher-year and school-year level: teachers aged 25–65 teaching at a single Danish public primary school per school year between 2013/14 and 2019/20. Student characteristics are averaged over a teacher's main cohort (the grade level in which they spend the most teaching hours). MH stands for mental health. See Section 3.1 for details on the construction of the mental health measures. For data confidentiality reasons, the reported minima and maxima are averages of the 30 smallest and largest observations, respectively.

This sample selection leads to a final sample of 68,351 teachers with 267,270 teacher-year observations across 1,271 schools. Table 1 summarizes the main characteristics of our estimation sample. Teachers are absent due to illness for an average of 16.1 days per

school year, and 10% have at least one doctor contact for mental health reasons. The teaching workforce is predominantly female (70%), with an average age of 44.6 years and about 16 years of teaching experience. Schools differ substantially in size: the average school enrolls 423 students, with a standard deviation of 239 and a range from fewer than 10 to more than 1,200 students. By contrast, average class size varies much less, with a mean of 20.8 students and a standard deviation of 3.4. This is well below the institutional class-size threshold of 28 students, which is therefore often not binding in practice.

4 School-Level Heterogeneity in Teacher Mental Health

4.1 Empirical Strategy

In this section, we document differences in teacher mental health across schools using an Empirical Bayes framework following Walters (2024). Conceptually, the question is whether teachers working at different schools experience systematically different mental health environments. Observed differences in average mental health may reflect systematic workplace characteristics that are potentially amenable to policy intervention. A direct comparison of school-level averages is problematic, however, because observed school means are noisy estimates of underlying mental health environments. The noise is largest in small schools and in schools whose teachers' outcomes vary strongly.

Setting up a standard Empirical Bayes framework, we assume that each school has a latent mental health parameter, θ_j, drawn from a common population distribution that does not depend on the precision of the school estimate,

$$\theta_j \mid s_j \sim \mathcal{N}(\mu_\theta, \sigma_\theta^2). \tag{1}$$

The observed school estimate, $\hat{\theta}_j$, is an unbiased estimate of this latent parameter with sampling variance s_j^2,

$$\hat{\theta}_j \mid \theta_j, s_j \sim \mathcal{N}(\theta_j, s_j^2), \tag{2}$$

where we treat the estimated sampling variance s_j^2 as known.

Ignoring sampling variation would mechanically exaggerate differences across schools be-

cause part of the observed dispersion simply reflects estimation noise. The Empirical Bayes approach explicitly accounts for this problem by estimating the latent prior distribution of school parameters and using this information to shrink noisy school-level estimates toward the population mean.

Empirically, we proceed in three steps. First, we estimate school-level conditional means of teacher mental health, $\hat{\theta}_j$, and their associated standard errors, s_j, from an OLS regression of the following equation:

$$y_{it} = \theta_{j(i,t)} + \tau_t + \varepsilon_{it}, \tag{3}$$

where y_{it} denotes the mental health index of teacher i in school year t (mean zero, standard deviation one; higher values indicate worse mental health), $j(i,t)$ denotes the school in which teacher i works in year t, $\theta_{j(i,t)}$ captures the corresponding school mental health parameter, and τ_t denotes year fixed effects. The resulting estimates $\hat{\theta}_j$ and their standard errors s_j constitute the raw school mental health fixed effects used in the subsequent Empirical Bayes procedure. We include a full set of school indicators and no constant, so that θ_j is the school intercept and τ_t is normalized to zero in the base year. Since no school serves as the omitted reference category, the sampling errors of the estimated school effects are approximately uncorrelated across schools.

Second, we estimate the latent distribution of school mental health parameters using an Empirical Bayes deconvolution procedure. We estimate the mean of the prior distribution as

$$\hat{\mu}_\theta = \frac{1}{J} \sum_{j=1}^{J} \hat{\theta}_j, \tag{4}$$

and its variance as

$$\hat{\sigma}_\theta^2 = \frac{1}{J} \sum_{j=1}^{J} \left[\left(\hat{\theta}_j - \hat{\mu}_\theta \right)^2 - s_j^2 \right]. \tag{5}$$

The subtraction of the sampling variance s_j^2 removes the excess dispersion in the raw school estimates attributable to estimation noise. The resulting prior distribution therefore summarizes our estimate of the population distribution of latent school mental health parameters, with $\hat{\sigma}_\theta$ measuring systematic between-school heterogeneity after accounting for sampling variation.

Third, we construct Empirical Bayes posterior estimates by combining each school's estimated raw fixed effect with information from the estimated prior distribution. The posterior estimate is given by

$$\hat{\theta}_j^* = \left(\frac{\hat{\sigma}_\theta^2}{\hat{\sigma}_\theta^2 + s_j^2} \right) \hat{\theta}_j + \left(\frac{s_j^2}{\hat{\sigma}_\theta^2 + s_j^2} \right) \hat{\mu}_\theta. \tag{6}$$

The posterior estimate is therefore a precision-weighted average of the raw school estimate and the overall population mean. Schools with relatively imprecise estimates are shrunk more strongly toward the population mean, whereas precise estimates are shrunk less. The resulting posterior estimates reduce dispersion attributable to sampling variation and provide de-noised estimates of the latent school mental health parameter.[6]

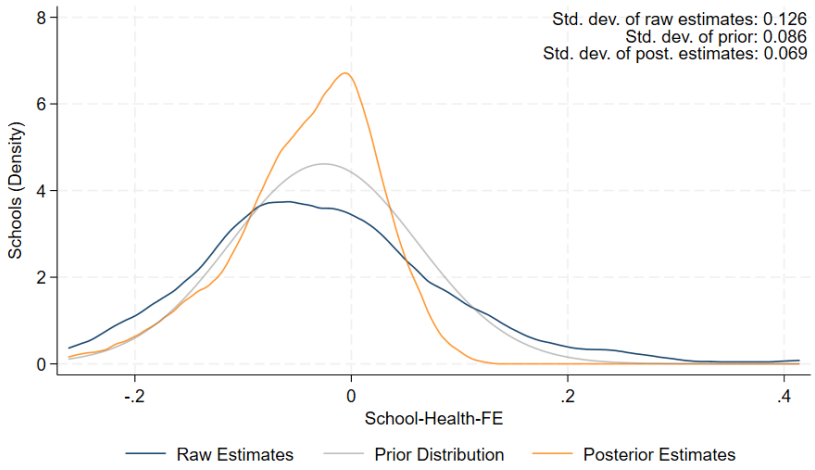
These posterior estimates capture systematic differences in teacher mental health across schools, but they should not be interpreted as causal effects of workplace environments. They may instead reflect a combination of causal effects and sorting of teachers across schools. The causal interpretation of these school-level differences is examined in the next section using teacher mobility between schools.

4.2 Results

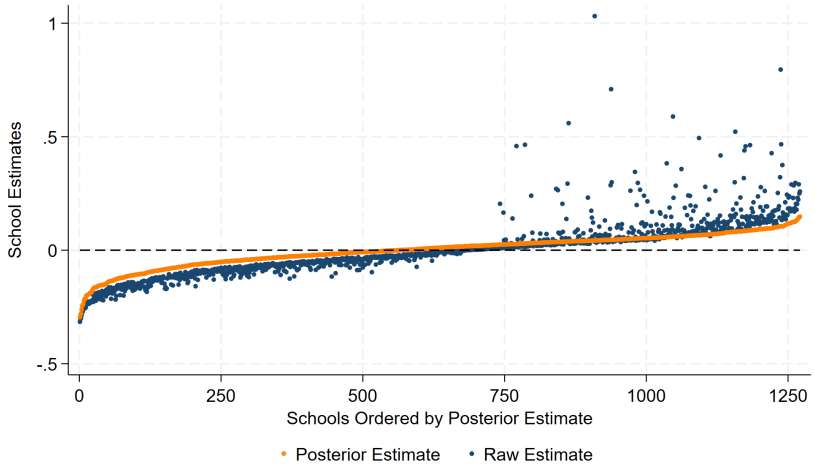
Panel (a) of Figure 1 displays the distributions of the raw school mental health fixed effects, the estimated normal prior, and the corresponding Empirical Bayes posterior estimates. The raw school mental health fixed effects, displayed in blue, are the school fixed effects estimated from Equation (3). The grey distribution shows the estimated prior, whose mean and variance are estimated according to Equations (4) and (5). The orange distribution shows the posterior estimates obtained by shrinking the raw estimates toward the estimated prior mean according to Equation (6).

Schools differ substantially in the average mental health of their teachers. The raw school mental health fixed effects have a standard deviation of 0.126, measured in teacher-level

[6]Sampling variances are positively related to estimated school means, because the components of the index are counts and doses (absence days, doctor visits, diagnoses, hospital stays and medication doses), whose variances rise with their means. By contrast, the pattern is not driven by school size: we show in Figure A2 in the appendix that raw school fixed effects are not systematically related to N_j, and raw estimates are similar across school size bins. Schools of different sizes are therefore drawn from a common latent distribution. Because sampling variances also depend on within-school outcome variance, however, θ_j and s_j are not fully independent as assumed in Equation (1). The prior mean and variance in Equations (4) and (5) are unweighted and remain consistent under such dependence (Walters, 2024), whereas the posterior estimates in Equation (6) shrink schools with high estimated effects more strongly, because these are estimated less precisely, and therefore compress the upper tail of the distribution.



(a) Distributions of School Mental Health Fixed Effects



(b) Raw and Posterior Estimates by School

Figure 1: Heterogeneity Across Schools

Note: The figure illustrates heterogeneity in teacher mental health care utilization across schools. Panel (a) displays estimated densities of raw (blue) and Empirical Bayes posterior (orange) school mental health fixed effects and the estimated prior distribution (gray) of the underlying school mental health parameters. Panel (b) plots the raw estimate (blue) and the posterior estimate (orange) of each school, with schools ordered by their posterior estimates; the dashed line marks zero. Mental health is measured with a mental health index, which is standardized to have mean zero and standard deviation one, with higher values indicating worse mental health (see Section 3.1). The raw school mental health fixed effects, the parameters of the prior distribution, and the Empirical Bayes posterior estimates are obtained from Equation (3), Equations (4) and (5), and Equation (6).

standard deviations of the index. This dispersion, however, reflects both systematic differences between schools and sampling noise, which is larger in small schools and in schools with high within-school outcome variance.[7] The Empirical Bayes procedure separates these two sources of variation by estimating the latent distribution of school mental health parameters. The estimated prior has a standard deviation of 0.086, substantially smaller than the standard deviation of the raw school mental health fixed effects.[8] The dispersion of the prior provides our estimate of the underlying between-school heterogeneity: About 47 percent of the variance of the raw school mental health fixed effects reflects systematic differences between schools; the remaining 53 percent is sampling noise. Finally, the standard deviation of the posterior estimates is 0.069, about 45 percent smaller than that of the raw estimates. The resulting posterior estimates, displayed in orange, provide de-noised estimates of individual school parameters. By construction, the posterior estimates are less dispersed than the latent school parameters, since shrinkage removes sampling noise but also part of the true variation.

To provide a more intuitive sense of magnitude of the observed heterogeneity, Table 2 reports the implied interquartile range of the estimated prior distributions separately for the components of the mental health index. For the mental health index, schools at the 75th percentile have outcomes 0.12 teacher-level standard deviations worse than schools at the 25th percentile. For the component of illness absence, the difference between schools at the 25th and 75th percentiles amounts to about 5.1 absence days per teacher and year, compared with a population mean of 16.1 days. Thus, the interquartile difference across schools corresponds to almost one-third of average annual illness absence. Relative to average GP contacts, differences in mental health care use are even larger. The interquartile range amounts to 0.08 GP visits for mental health reasons, relative to a population mean of 0.17, and 0.16 psychologist or psychiatrist visits, relative to a mean of 0.23. Hence, the difference between the 25th and 75th percentiles corresponds to roughly 48 percent and 68 percent of average utilization, respectively.

The EB shrinkage compresses estimated differences between schools most strongly at the unhealthy end of the distribution, where school effects are estimated less precisely. Panel (b) of Figure 1 illustrates the shrinkage for individual schools. Schools are ordered by their posterior estimate. For each school, the raw school estimate is displayed in blue and the corresponding posterior estimate in orange. For the majority of schools, raw and posterior estimates are close, as these schools have low estimated latent parameters, which are estimated precisely. For the remaining schools, roughly the 40 percent with the

[7] The number of underlying teacher-year observations ranges from 14 to 650 across schools, with an average of 230.

[8] The prior mean of -0.025 differs from zero because the school intercepts in Equation (3) refer to the base year and are averaged without weights. This normalization does not affect the dispersion of school effects or the shrinkage.

highest posterior estimates, raw estimates are substantially more dispersed and are shrunk strongly toward the prior mean. Thus, posterior estimates range from about -0.3 at the healthy end to only about 0.1 at the unhealthy end of the distribution. This asymmetry reflects the dependence of sampling variances on estimated school means discussed in Section 4.1: schools with high estimated latent parameters are estimated less precisely and are therefore shrunk more strongly.

Table 2: Comparing Population Means with School Heterogeneity

		School Health Fixed Effects			
	Pop. Mean	Raw SD	Prior SD	Posterior SD	Prior $Q_{75} - Q_{25}$
MH Index	-0.00	0.13	0.09	0.07	0.12
Days of Illness Absence	16.09	5.32	3.76	3.18	5.08
No. of GP Visits for MH Reasons	0.17	0.10	0.06	0.05	0.08
No. of Psychologist/Psychiatrist Visits	0.23	0.19	0.12	0.09	0.16
No. of Different MH Diagnoses	0.02	0.03	0.02	0.01	0.02
No. of Hospital Stays for MH Reasons	0.14	0.24	0.14	0.07	0.18
Total DDD of MH Medication	28.35	21.85	17.68	13.34	23.85

Note: The table reports heterogeneity in teacher mental health care utilization measured with the mental health index and the individual variables used to construct it. Column (1) reports the mean in the teacher population. Columns (2)–(4) report the standard deviations of the raw school mental health fixed effects, the estimated normal prior distribution, and the Empirical Bayes posterior estimates, respectively. Column (5) reports the implied interquartile range of the estimated prior distribution. For each variable, the Empirical Bayes procedure described in Equations (3) to (6) is applied separately, using the respective variable in levels as the outcome. See Section 3.1 for details on variable construction. DDD stands for defined daily doses.

We assess the validity of the Empirical Bayes shrinkage with the forecast-unbiasedness test of Chetty et al. (2014a). For each school year t, we re-estimate the prior means and the posterior estimates from all other years and regress teachers' mental health index in year t on the leave-one-year-out posterior of their school (equivalent to a regression of school-year means weighted by the number of teachers). Figure 2 plots this relationship in orange, together with the corresponding relationship for leave-one-year-out raw school estimates of year-adjusted outcomes in blue. For the posterior estimates, the coefficient is $\hat{\beta}_{eb} = 0.91$ (SE 0.03). It is statistically different from, but close to, the benchmark of one implied by forecast unbiasedness. A slope slightly below one is consistent with some drift in school effects over time, since the leave-one-year-out posteriors are based on data from other years. For the raw school estimates, the coefficient is $\hat{\beta}_{raw} = 0.56$ (SE 0.02). Raw school estimates thus overstate differences between schools by about 80 percent, and the shrinkage removes most of this forecast bias. We conclude that the posterior estimates are close to forecast-unbiased predictors of out-of-sample school outcomes, whereas raw school estimates are not.

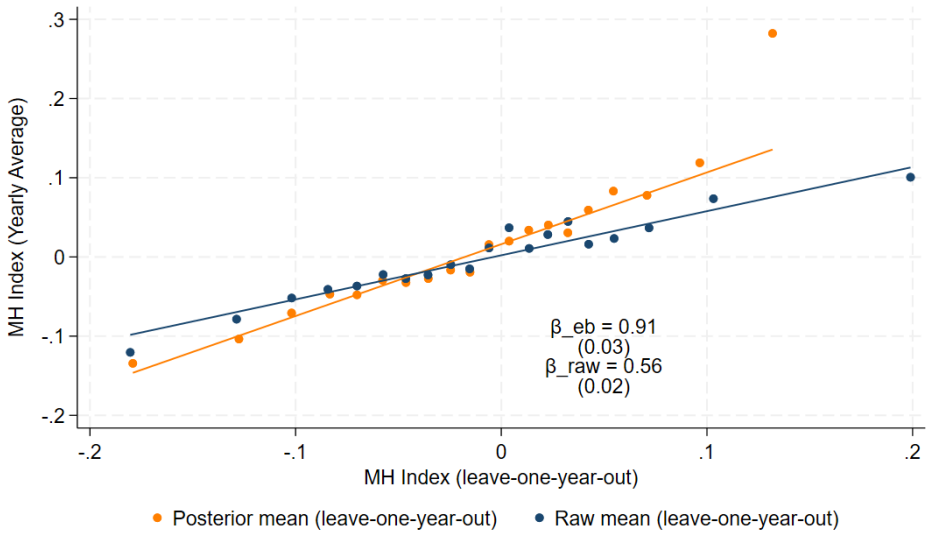


Figure 2: Forecast-Unbiasedness Test

Note: The figure reports the forecast-unbiasedness test for the posterior estimates. A teacher's mental health index in year t is plotted against two leave-one-year-out estimates for their schools, estimated using data from all other years: the Empirical Bayes posterior estimates (orange) and the raw school estimates of year-adjusted outcomes (blue). Each scatterplot is based on 20 quantile bins of the respective leave-one-year-out school estimates. The fitted lines and coefficients (β_{eb}, β_{raw}) are based on the corresponding linear regressions at the teacher-year level. Standard errors clustered at the school level are reported in parentheses. A coefficient of one indicates forecast-unbiased estimates (Chetty et al., 2014a); a coefficient below one indicates that the estimates overstate differences between schools.

4.3 School Health Effects and Teacher-Reported Stress

Beyond predicting outcomes, the register-based school mental health fixed effects reflect working conditions that teachers themselves perceive. Table 3 relates the school fixed effects to two items from the DLF teacher survey, available for 922 schools: the share of teachers reporting stress, and the share of teachers reporting sick leave due to working conditions in the previous year. Both shares are higher in schools with more mental health care usage. A 10 percentage point higher share of teachers reporting sick leave due to working conditions is associated with a school fixed effect that is higher by 0.013, about 15 percent of the prior standard deviation of school mental health fixed effects, and with 0.8 additional days of illness absence. A 10 percentage point higher share of teachers reporting stress is associated with a school fixed effect that is higher by 0.010, about 11 percent of the prior standard deviation, and with 0.6 additional days of illness absence. In contrast, we do not find a similar association with medication use. This pattern validates the objective register-based measures of school-level mental health with independently collected teachers' self-reports.

Table 3: School Mental Health Fixed Effects and Teacher-Reported Stress and Illness Absence

	(1) MH Index	(2) Illness Absence	(3) MH Contact	(4) MH Medication
Sick leave last year bc working conditions	0.129***	8.459***	0.032***	0.007
	(0.026)	(1.173)	(0.008)	(0.010)
Stress (some/high vs low)	0.096***	5.785***	0.023**	0.000
	(0.026)	(1.080)	(0.008)	(0.011)
Observations	922	922	922	922
Adj. R2 in multivariate	0.035	0.081	0.025	-0.002

Note: The table reports results from univariate regressions of raw school mental health fixed effects on school-level average responses to the teacher survey. Each coefficient is estimated in a separate regression. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. The adjusted R^2 refers to the regression including all survey items listed in the table jointly. See Section 3.1 for details on variable construction, Section 4.1 for the estimation of school mental health fixed effects, and Table A1 for details on the survey items. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$. Data sources: Danish register data and DLF teacher survey.

4.4 Teacher Mobility and Pay

School mental health fixed effects are also systematically associated with teachers' labor-market outcomes. In the teacher survey (Table 4), teachers in less healthy schools more often consider teaching in a different municipality because of working conditions, and more often name the psychological work environment as a reason for considering leaving, but they do not consider leaving the profession more often. Actual mobility in the registers shows the same pattern: teachers respond to less healthy workplaces by moving to other schools rather than by leaving the profession. Table 5 relates the estimated school mental health fixed effects to subsequent mobility and wages. Teachers in worse environments are more likely to move to another school, with the probability of school turnover increasing by 2.5 percentage points for a one-unit increase in the school fixed effect. In contrast, we find no association with exits from the teaching profession.

Less healthy schools do not compensate teachers through higher pay, in line with collectively bargained wages discussed in Section 2.1. Teachers working in schools with worse mental-health environments earn, if anything, somewhat lower annual wages: a one-unit increase in the school fixed effect is associated with a 0.7% reduction in wages and with 4.5 thousand DKK lower wages in levels. Neither estimate is statistically significant. Hence, there is no indication of a compensating wage differential for working in a less healthy school.

Table 4: School Mental Health Fixed Effects and Teacher-Reported Leave Intentions

	(1) MH Index	(2) Illness Absence	(3) MH Contact	(4) MH Medication
Considered leaving ...				
to school in different municipality bc work cond.	0.056**	5.018***	0.018***	-0.009
	(0.017)	(0.721)	(0.005)	(0.007)
to private/indep school bc work cond.	0.009	2.505**	0.005	-0.016
	(0.021)	(0.936)	(0.007)	(0.008)
the profession	0.002	1.758	0.003	-0.003
	(0.027)	(1.167)	(0.007)	(0.010)
Reasons for considering leaving:				
Psych. work environment	0.074***	2.990***	0.013**	0.013*
	(0.016)	(0.628)	(0.004)	(0.007)
Practical reasons	0.037	0.490	-0.006	-0.010
	(0.043)	(1.655)	(0.010)	(0.012)
Discrepancy btw tasks and resources	0.009	0.633	-0.001	0.007
	(0.017)	(0.731)	(0.005)	(0.006)
Personal reasons	0.015	1.082	0.010	-0.000
	(0.025)	(1.098)	(0.007)	(0.010)
Observations	919	919	919	919
Adj. R2 in multivariate	0.036	0.062	0.020	0.010

Note: The table reports results from univariate regressions of raw school mental health fixed effects on school-level average responses to the teacher survey. Each coefficient is estimated in a separate regression. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. The adjusted R^2 refers to the regression including all survey items listed in the table jointly. See Section 3.1 for details on variable construction, Section 4.1 for the estimation of school mental health fixed effects, and Table A1 for details on the survey items. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$. Data sources: Danish register data and DLF teacher survey.

Table 5: School Mental Health Fixed Effects and Labor Market Outcomes

	(1) School Turnover	(2) Profession Turnover	(3) Wage	(4) Log(Wage)
School Health-Fixed Effect	0.025**	-0.002	-4.510	-0.007
	(0.009)	(0.008)	(2.539)	(0.007)
Observations	222,119	267,270	238,812	238,808
Adj. R2	0.012	0.008	0.527	0.508
Year FE	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes

Note: The table reports estimates from regressions of teachers' turnover and wages on raw school mental health fixed effects based on the mental health index. The dependent variables in columns (1)–(4) are turnover to a different public primary school, turnover to a different profession (different DISCO code), annual wages in 1,000 DKK, and the natural logarithm of annual wages, respectively. See Section 3.1 for details on variable construction and Section 4.1 for the estimation of school mental health fixed effects. All regressions are estimated at the teacher-year level and include year fixed effects. The wage regressions additionally control for municipality wage allowance, experience, teacher education, and yearly hours of work. Standard errors clustered at the school level are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$.

5 Mover analysis

5.1 Sample & Empirical Strategy

In this section, we describe a quasi-experimental research design to examine in how far the differences between schools described in Section 4 reflect *causal* effects of workplaces. We employ a *mover analysis* comparing two teachers moving between schools, similar in characteristics, previous mental health progression and origin environment, but who then end up in either a more or less healthy environment. If the mental health of a teacher moving to an unhealthy school environment deteriorates after the move, relative to a teacher moving to a healthier school environment, we interpret this as the causal effect of the destination school.

We progress in this section as follows: we first describe estimation details, and present the arising event study patterns and underlying raw variation. We then explicitly discuss potential threats to identification and provide empirical evidence supporting its validity.

The necessary identification assumption for this interpretation is that teachers do not systematically sort into destination schools with respect to their own mental health and the destination school's mental health environment. Put differently, the difference in mental health environments between destination and origin schools must not be systematically related to unobserved determinants of teachers' post-move mental health. We will extensively discuss the validity of this assumption in Section 5.3: Moves to schools with worse mental health environments are as common as moves to schools with better mental health environments, move-induced changes in school environments cannot be predicted by important life events and personal constraints, and we find only a limited role for informal networks.

Noteworthy, our identification strategy does not entail a comparison of movers to non-movers. Such a comparison would unlikely produce causally interpretable results, as movers differ from the overall population of teachers, as displayed in Table A3: the teachers moving at least once within the observation period are more likely female and have less teaching experience (but are more qualified). Conditional on experience, age is slightly positively related to moving. While modest in economic terms, these differences are statistically significant. Further, moving teachers (regardless of destination) on average experience decreasing mental health prior to the move, and are more likely to expect a child and to start cohabiting.

A move occurs when a teacher works at one school in school year t and at a different school in school year $t+1$, with both schools in our sample of public primary schools.

Changes of the school identifier due to school mergers do not count as moves, and moves to schools outside our sample are not considered. For teachers who move more than once, we use the first move and keep all subsequent observations: teachers who move again retain the change in school environment of their first move and are not dropped from the sample. Teachers who are not observed at a primary school in the year or years between origin and destination school ("gap years") are not part of the main sample, since we do not observe the school environment they are exposed to in between. We later examine the robustness of our results to including these teachers. The final mover sample consists of 8,031 teachers with 36,361 teacher-year observations.

The event window covers three school years before and three school years after the move. Since our data cover the school years 2013/14 to 2019/20, the panel is unbalanced in event time, and coefficients are identified from different cohorts of movers. Table A4 in the appendix shows the number of teachers and teacher-year observations available for each event time point, as well as average teacher observables.

We define the change in mental health environment as the difference in the posterior school health fixed effects between origin and destination school:

$$\hat{\delta}_i = \hat{\theta}^*_{d(-i)} - \hat{\theta}^*_{o(-i)}, \tag{7}$$

where $\hat{\theta}^*_{d(-i)}$ is the school-level estimate of the destination school and $\hat{\theta}^*_{o(-i)}$ the school-level estimate at the origin school, shrunk according to Equation (6) to correct for sampling noise. In both cases, we rely on leave-teacher-out estimates: origin and destination school effects are computed from the year-adjusted outcomes of all other teachers at the school, excluding all observations of the focal teacher there. Consequently, variation in the estimated origin and destination school health fixed effects is driven entirely by other teachers' outcomes rather than by the focal teacher's outcome. By doing so, we avoid mechanically induced correlations between the explanatory variable and the outcome.

We estimate the following event-study specification,

$$y_{it} = \sum_r \beta_r \left(\hat{\delta}_i \times \mathbb{1}\{R_{it} = r\} \right) + \sum_r \kappa_r \mathbb{1}\{R_{it} = r\} + \alpha_i + \tau_t + \varepsilon_{it}, \tag{8}$$

where y_{it} denotes measures of teacher i's mental health in year t. The term R_{it} denotes event time relative to the school move. The coefficients β_r trace out the dynamic relationship between teacher mental health and the change in school health environment, ranging from three years before to three years after the move. Observations outside the event window are dropped. The coefficients for pre-move periods provide a visual test

for common pre-trends, with the year immediately preceding the move ($r = -1$) serving as the omitted reference period. Event-time fixed effects κ_r capture changes in mental health around the move that are common to all movers, irrespective of the direction of the move, such as the deterioration in mental health that precedes moves (Figure 4). The coefficients β_r are therefore identified only from differences between teachers whose moves differ in $\hat{\delta}_i$. Teacher fixed effects α_i absorb time-invariant individual characteristics, while year fixed effects τ_t account for aggregate shocks common to all teachers. The error term ε_{it} captures idiosyncratic variation in outcomes.

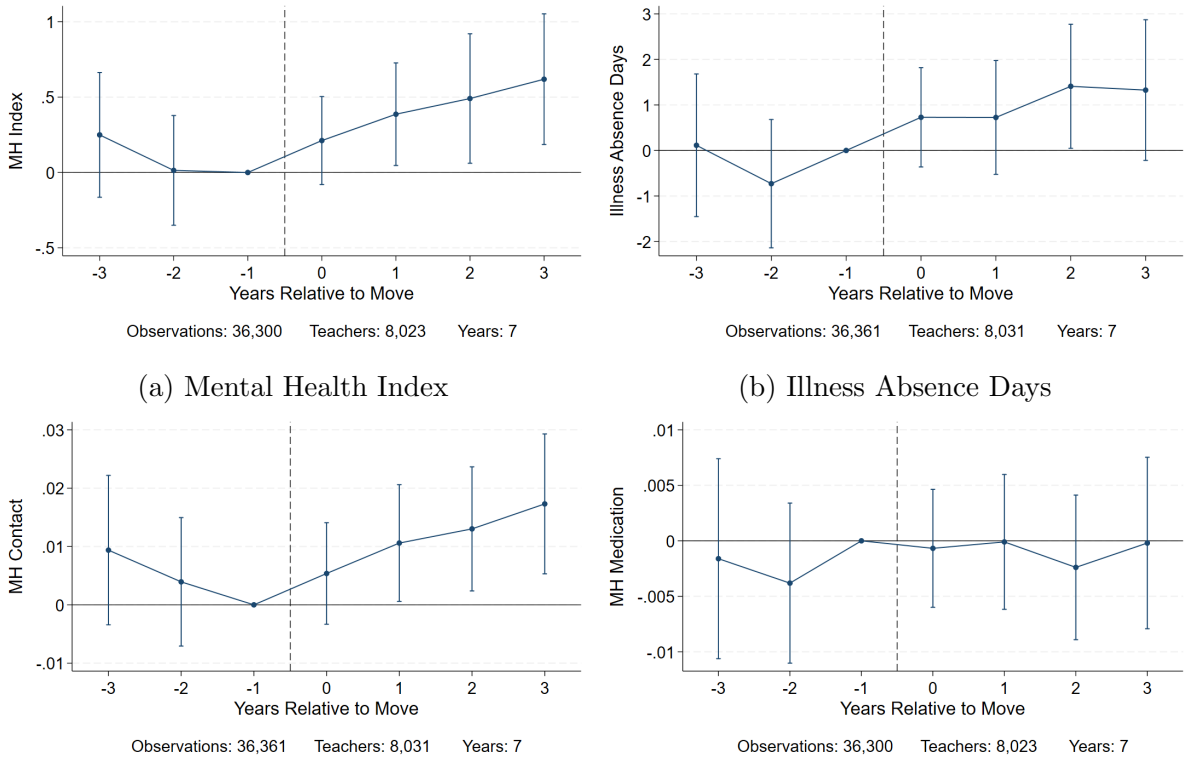
We estimate the specification separately for the mental health index and its components: days of illness absence, contact to a doctor for mental health reasons, and mental health medication usage. In case the outcome is the mental health index itself, both outcome and treatment $\hat{\delta}_i$ are measured on the same scale. Event-study coefficients β_r can accordingly be interpreted as transmission rates. A coefficient of one implies that teachers fully converge to the mental health environment of the destination school, while a coefficient of zero implies no transmission. In this case the differences across schools would completely stem from selection. Values between zero and one indicate partial convergence to the new mental health environment.

In case the outcome is one of the index' components, outcomes are measured as numbers of days of absence, and indicators for any doctor contact for MH reasons, or any MH-related medication. Outcome and treatment are then measured on different scales. For ease of interpretation, we therefore divide the treatment intensity $\hat{\delta}_i$ by the prior standard deviation of school health fixed effects in the mental health index, $\hat{\sigma}_\theta = 0.086$ (Equation (1)). Since the posterior means are close to forecast-unbiased (Figure 2), the coefficients then measure the effect of moving to a school with a one-standard-deviation higher (less healthy) school health fixed effect. In the event-study sample, the standard deviation of $\hat{\delta}_i$ is 0.084.

5.2 Results

Figure 3 displays the event-study estimates from Equation (8), using both teachers' mental health index and its individual components as outcomes.

Pre-trends appear to be flat across all outcomes, with coefficients in the pre-move periods being small and statistically indistinguishable from zero (joint F-tests of $\beta_{-3} = \beta_{-2} = 0$: $p = 0.45$ for the mental health index, 0.68 for illness absence, 0.35 for doctor contacts, and 0.54 for medication use). This pattern supports the assumption that teachers who eventually move to either healthier or less healthy schools would have exhibited similar



(a) Mental Health Index

(b) Illness Absence Days

(c) Mental Health Contact

(d) Mental Health Medication

Figure 3: Event Study of Teacher Mental Health Around School Moves

Note: The figure plots the estimated event study coefficients $\hat{\beta}_r$ from Equation (8). The sample consists of teachers who change schools at least once between the school years 2013/2014 and 2019/2020 without an interruption in employment at a public primary school during the transition. For each teacher, we consider only the first school move. Treatment intensity $\hat{\delta}_i$ is defined as the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index and leaving out the moving teacher from both estimates. The dependent variables in Panels (a)–(d) are the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. In Panels (b)–(d), treatment intensity is divided by the estimated prior standard deviation of school mental health fixed effects ($\hat{\sigma}_\theta = 0.086$), such that the coefficients capture the effect of moving to a school with a one-standard-deviation higher school mental health fixed effect. Event time 0 denotes the first year at the destination school, and event time -1 serves as the reference period. All regressions are estimated at the teacher-year level and include individual and year fixed effects. See Section 3.1 for details on variable construction and Section 4.1 for the estimation of school mental health fixed effects. Whiskers indicate 95% confidence intervals based on robust standard errors.

outcome trajectories had they moved to schools with the same mental health environment.

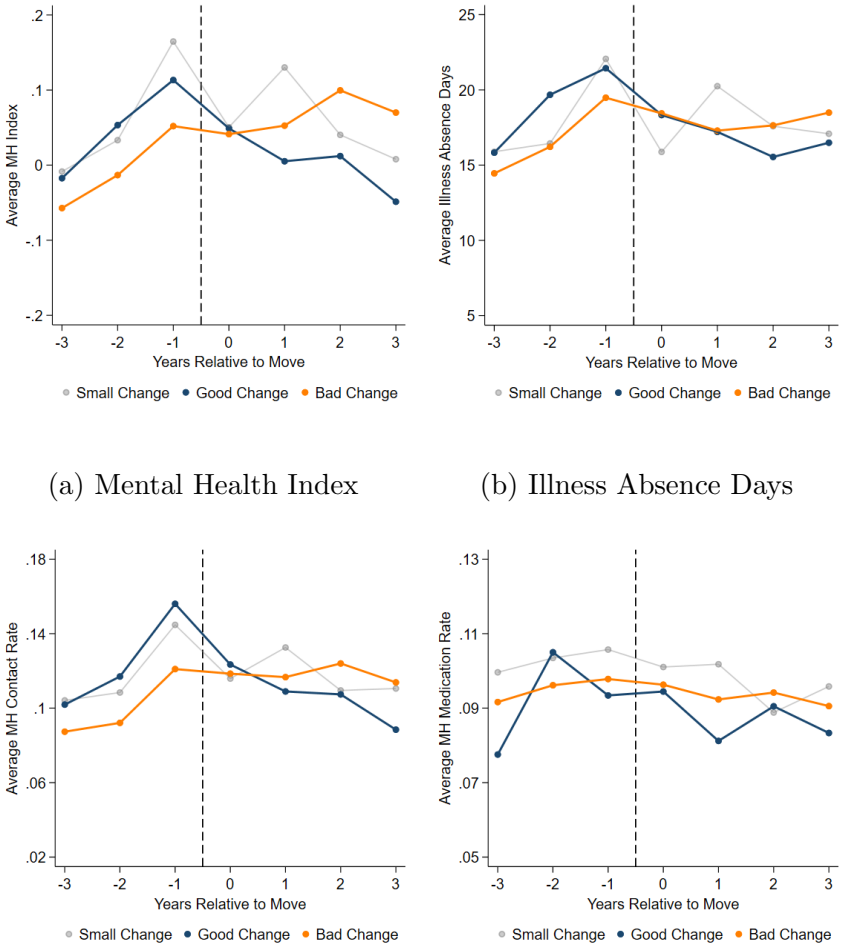
After the move, teachers' mental health gradually adjusts in the direction predicted by the destination school's mental health fixed effect (Panel a). The coefficient reaches 0.49 two years and 0.62 three years after the move (95 percent confidence interval 0.19 to 1.05). Within three years, teachers thus take on about 60 percent of the difference in school mental health fixed effects between destination and origin school, although the estimate is imprecise. This is a substantial adjustment of teachers' own mental health to the new environment.[9]

Focusing on the individual components of the index, we find similar patterns for days of illness absence and doctor contacts for mental health reasons. After three years, moving to a school with a one-standard-deviation higher school mental health fixed effect is associated with about 1.3 additional days of illness absence, an increase of about 8 percent relative to the mean of 16.1 days. This estimate is imprecise: it is statistically significant at the 5 percent level two years after the move, but not three years after the move. The probability of a doctor contact for mental health reasons rises by 1.7 percentage points, an increase of 17 percent relative to the mean. We do not find a corresponding pattern for mental health medication use.

Changes in teachers' mental health are larger following moves between schools with greater differences in their mental health fixed effects, indicating a dose-response pattern. Figure A5 in the Appendix relates changes in teachers' mental health index directly to the continuous difference in mental health school fixed effects between destination and origin schools, with larger improvements in the school environment being associated with larger subsequent improvements in teachers' mental health and vice versa. This positive gradient in treatment intensity becomes gradually more pronounced over the post-treatment period.

We next turn to raw outcome trajectories to examine teachers' common mental health trends before moving and their subsequent evolution across destination school environments. Figure 4 plots raw outcome trajectories for teachers moving to substantially healthier schools (blue lines), substantially less healthy schools (orange lines), and schools associated with only minor changes in the mental health environment (gray lines). The

[9]We examine heterogeneity in event patterns by gender, teacher experience, family status or move distance. Figure A3 presents separate estimates by gender, above- versus below-median teaching experience, the presence of children below age 18, and a distance between destination and origin school of more versus less than 20 km. Across all four dimensions, post-move trajectories are closely aligned between subgroups, and we find little evidence of systematic differences in either the magnitude or timing of the response. Due to substantially smaller samples, the estimates are overall noisier than in our main analysis. In Figure A4, we further show that relaxing our sample restrictions (Section 5.1) to include an additional 4,284 teachers for whom we observe a "gap year" between destination and origin school preserves the main event pattern.



(a) Mental Health Index

(b) Illness Absence Days

(c) Mental Health Contact

(d) Mental Health Medication

Figure 4: Raw Variation in Outcomes by Direction of School Move

Note: The figure plots average teacher outcomes around school moves, separately by the magnitude and direction of the change in the school mental health environment. The sample consists of teachers who change schools at least once between the school years 2013/2014 and 2019/2020 without an interruption in employment at a public primary school during the transition. For each teacher, we consider only the first school move. Moves are grouped based on the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. A "Good Change" ("Bad Change") corresponds to the lower (upper) quartile of the distribution of these differences, with negative values indicating a healthier destination school. A "Small Change" corresponds to a difference that deviates from the median of this distribution by no more than half the interquartile range. The outcomes in Panels (a)–(d) are the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. Event time -1 denotes the last year at the origin school, and event time 0 denotes the first year at the destination school. The vertical dashed line marks the school move. Outcomes are plotted as raw means by event time and move type, without adjusting for covariates or fixed effects. See Section 3.1 for details on variable construction and Section 4.1 for the estimation of school mental health fixed effects.

latter group experiences moving schools but barely a change in mental health environments. Therefore, it can serve as a control group that allows us to visually separate mental health changes corresponding to a school move from changes corresponding to a change in mental health environments. Three features stand out.

First, teachers experience a gradual deterioration in mental health prior to moving. Across all three groups, both days of illness absence and doctor contacts for mental health reasons increase in the years leading up to the move, but similarly across groups, in line with common pre-trends. This pattern is consistent with worsening mental health acting as one push factor for teacher mobility.

Second, prior to switching schools, the mental health care outcomes of teachers moving to a healthier school are larger than those of teachers moving to a less healthy school. This is a mechanical pattern, because for teachers at healthier origin schools, which are also better for their own mental health, it is statistically more likely that the destination school is less healthy and that they will, therefore, experience a "bad change" and vice versa for teachers at a less healthy origin school.

Third, the event pattern is mainly driven by improving mental health among teachers moving to a healthier environment, and less by worsening mental health of teachers moving to a less healthy environment. After the move, teachers with small changes and teachers moving to healthier schools recover from their pre-move deterioration, and to a similar degree. Teachers moving to less healthy schools show little recovery: their index remains at about its pre-move level, and absence only slightly falls.[10]

5.3 Causal Identification

The identifying assumption stated in Section 5.1 does not require teachers to choose destination schools at random, but requires that teachers whose mental health would evolve differently after the move do not systematically end up in schools with different mental health environments. The two small and jointly insignificant pre-move coefficients already presented in Figure 3 directly support this assumption. In this section, we complement this evidence with tests of whether teachers can and do select destination schools based

[10]Differential retention at the destination school could also shape these trajectories. Teachers moving into less healthy school environments may be more likely to leave these schools again. Since teachers who move again keep the change in school environment of their first move (Section 5.1), this could mechanically attenuate the estimated deterioration at longer horizons through selective attrition. Figure A6 provides little evidence that this mechanism is quantitatively important. Kaplan-Meier survival curves for teachers experiencing favorable, unfavorable, or only small changes in school environments remain closely aligned over the six years following the move. While retention is slightly lower following moves toward less healthy environments, the differences across groups are small.

on their mental health environment.

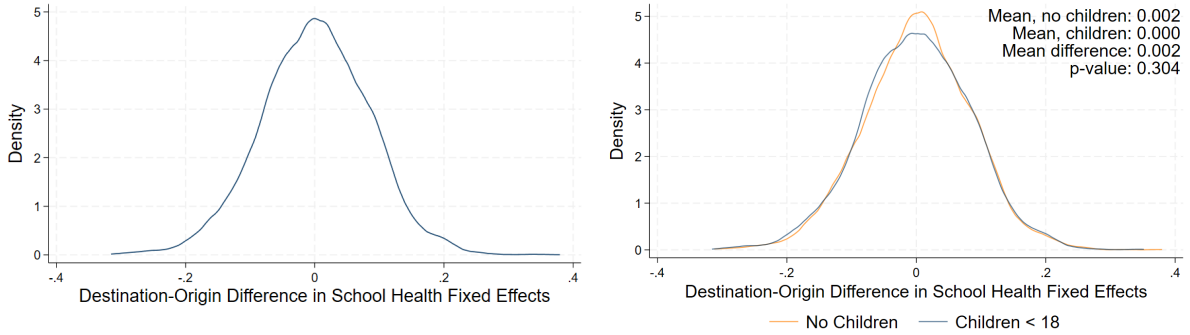
A key rationale behind the identifying assumption is that, unlike neighborhood, room conditions or faculty composition, features such as management quality, collegial climate, or workplace stress are neither contractible nor directly advertised. They may therefore be considerably better known to insiders than to prospective teachers. Recent evidence shows that workers hold systematically inaccurate beliefs even about salient dimensions such as wages (Caldwell et al., 2025; Jäger et al., 2024). Information may be much more difficult to obtain for less readily observable job amenities becoming apparent only through workplace experience (Lamadon et al., 2022; Sockin, 2022; Sorkin, 2018), a reason why reputation intermediaries such as Glassdoor are valued for providing such information before joining an employer (Huang et al., 2026).

To empirically support the plausibility of the identifying assumption, we pursue several complementary exercises. First, we show that teachers move as often to good as to bad schools (Figure 5, Panel a): The distribution of changes in school environments experienced by movers is symmetric around zero. If teachers would both value and anticipate the mental health environment of destination schools, we would instead expect selective mobility to better schools, especially in the absence of wage differentials. However, this is not what we observe in the data.

Second, teachers who are constrained for family reasons (approximated by having care-dependent children), move to similar schools as non-constrained teachers (Figure 5, Panel b). These constrained teachers may face a more restricted set of feasible destination schools because of residential immobility, childcare arrangements, or a partner's employment, yet are no more likely to move toward less healthy schools. This pattern thus provides little evidence that greater flexibility in destination choice leads teachers to systematically sort into healthier school environments.

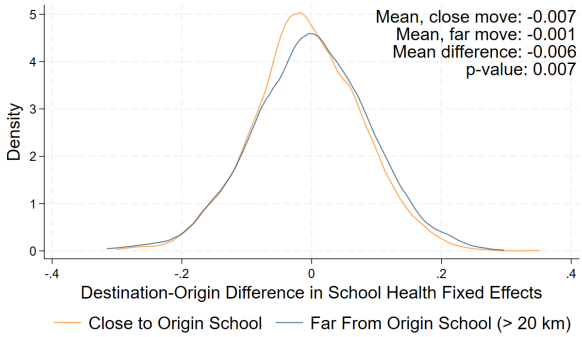
Third, we analyze whether teachers moving to schools within 20 km of their origin school move to similar schools than teachers moving farther (Figure 5, Panel c). We find that these close school moves are slightly healthier, which speaks to informal "whisper" networks that transmit information about school working conditions and plausibly operate locally. The difference in means is statistically significant ($p = 0.007$) but small: for close moves, the change in school environment is on average 0.006 more favorable, about 7 percent of the prior standard deviation of school mental health fixed effects, and the two distributions largely overlap. We therefore cannot rule out that local information affects destination choices, but it accounts for little of the variation in $\hat{\delta}_i$ that identifies our estimates. In the heterogeneity analysis (Figure A3), we also split the sample into teachers moving to closer versus farther away schools and do not find that event study

patterns differ between the two groups.



(a) All First Moves

(b) Separately by Having Children



(c) Separately by Move Distance

Figure 5: Differences Between Destination and Origin Schools

Note: The figure plots estimated densities of differences in school mental health fixed effects between teachers' destination and origin schools. The sample consists of teachers who change schools at least once between the school years 2013/2014 and 2019/2020 without an interruption in employment at a public primary school during the transition. For each teacher, we consider only the first school move. Differences are calculated as the Empirical Bayes posterior estimate of the destination school's mental health fixed effect minus that of the origin school, estimated using the mental health index. Panel (a) displays the density for all moves in the sample. Panel (b) displays separate densities for teachers with and without children under the age of 18. Panel (c) displays separate densities for moves involving distances of more than 20 km and distances of 20 km or less between the origin and destination school. In Panels (b) and (c), the group-specific means and the p-value of a t-test of equality of means are reported in the upper right corner. See Section 3.1 for details on variable construction and Section 4.1 for the estimation of school mental health fixed effects.

Fourth, the event pattern does not change when school mental health fixed effects are estimated conditional on observable school, student, and municipality characteristics (Figure A7). These factors may be easily observable by teachers prior to moving, and may be taken as signals for the latent mental health environment (we examine correlations between these characteristics and the mental health fixed effects below in Section 6). Yet, our main results persist when identification relies only on variation in school mental-health environments that is orthogonal to these observable school characteristics. Besides school characteristics, teacher characteristics also do not seem to play a major role in sorting into healthier or less healthy schools. In the upper part of Table A3, column 3, we show regression results of treatment intensity $\hat{\delta}_i$ on teacher characteristics. Only formal

teacher education is statistically significant, and its coefficient of 0.008 is very small, about 9 percent of the prior standard deviation of school health fixed effects.

Finally, we show that major life events coinciding with school moves do not confound the estimated event pattern. Identification would be threatened if events that likely affect mental health care utilization, such as the end of cohabitation, expecting a child, residential moves, or changes in general practitioner, systematically coincided with treatment intensity. We show that excluding teachers experiencing coinciding life events keeps the event pattern intact (Figure 6). Among the initial 8,023 teachers, for 12% of teachers the school move coincides with address changes, for 13% with a GP change, for 3% with expecting a child, and for 3% with the end of cohabitation. In addition, we lose 2%, 13%, 0.001%, and 0.002% of teachers, respectively, due to missing data. Excluding these teachers does not change the original event pattern in sign, magnitude, and statistical significance. This finding is in line with the lack of predictive power of these events for sorting into healthier or less healthy schools demonstrated in the lower part of Table A3, column (3).

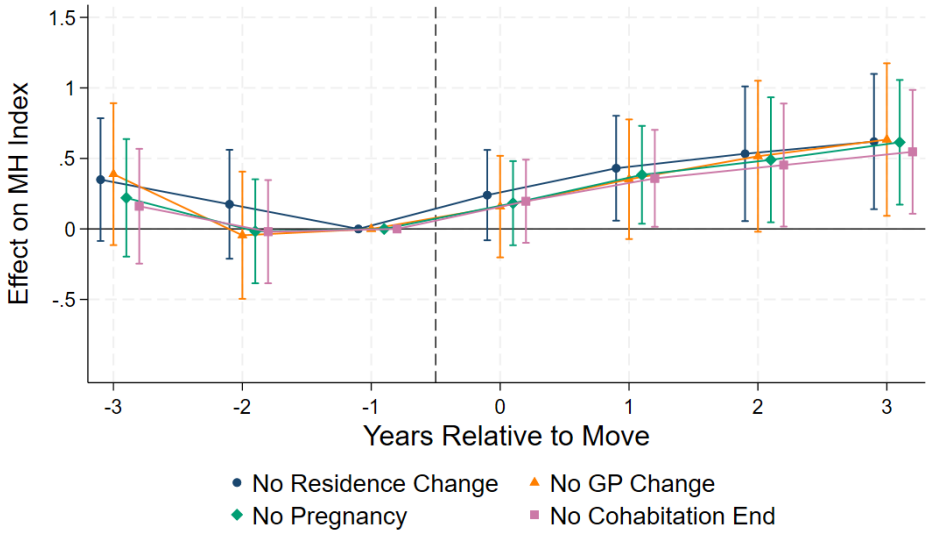


Figure 6: Event Study Excluding Coinciding Life Events

Note: The figure plots the estimated event study coefficients $\hat{\beta}_r$ from Equation (8), using the mental health index as the dependent variable, analogous to Panel (a) of Figure 3. Each series corresponds to an alternative sample restriction excluding teachers who change residence (blue), change their general practitioner (orange), expect a child as mother or father (green), or end cohabitation (pink) in the year of the move. Teachers with missing information on the respective life event are also excluded from the corresponding sample. Treatment intensity $\hat{\delta}_i$ is defined as the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. Event time 0 denotes the first year at the destination school, and event time -1 serves as the reference period. All regressions are estimated at the teacher-year level and include individual and year fixed effects. Whiskers indicate 95% confidence intervals based on robust standard errors.

6 What Describes a Healthy School?

So far, we have established substantial variation between schools in terms of teachers' mental health, and that these differences are at least partly transmitted to the mental health of teachers moving into new environments. We now examine what observable characteristics on the school-, teacher-, and student-level distinguish healthier from less healthy schools. All associations reported are correlational. For the student observables, we additionally analyze in Section 7 to what extent they causally determine teacher mental health.

We draw on three complementary data sources: administrative register data on school-, student-, teacher-, and principal-level characteristics; teachers' own perceptions of their workplaces, collected through surveys by the teacher association; and students' perceptions of the school environments, collected through a mandatory nationwide well-being survey. We thus observe both employees' and students' perceptions of the same organization. This allows us to examine whether schools that provide healthier workplaces for teachers also provide better environments for students, or whether these dimensions instead trade off against one another. Where measures capture conceptually distinct characteristics, we report multivariate associations conditional on the remaining observables. Where measures capture closely related dimensions of the same underlying construct, we report univariate correlations.

Two findings stand out. First, no single observable factor accounts for the differences between schools. Jointly, the register-based characteristics explain about 9 percent of the variance of the raw school mental health fixed effects (adjusted R^2 in Table 6), which translates into about a fifth of the "true" variance as expressed by the prior distribution. This means that features that might be readily visible from outside, such as school size, location, class size, and most dimensions of students' socio-demographic composition, are only weakly related to school mental health. This supports the argument in Section 5.3 that the mental health environment of a school is difficult to observe for prospective teachers. Second, healthier workplaces for teachers coincide with better environments for students.

6.1 Register-Based Characteristics

Table 6 displays results of a multivariate regression of raw school mental health fixed effects on observable school-, student-, teacher-, and principal-level characteristics from the registers. Many of these covariates, in particular school size, location, and student

composition, are readily observable to prospective teachers and may guide selection into schools with better working conditions. Each covariate is standardized with mean zero and a standard deviation of one, and all characteristics enter jointly. The regression covers the 966 schools with information on all characteristics.

The observable characteristics are only weakly related to school mental health. In terms of school and student characteristics, neither the number of students nor average class size is related to the mental health fixed effects; school size is associated only with more days of illness absence. Schools located in urban municipalities tend to exhibit worse mental health outcomes. Schools with higher-performing students display better teacher mental health outcomes, although this holds for Danish but not for math test scores. Schools with a larger share of immigrants from non-Scandinavian countries display better outcomes as well, an association that the within-teacher analysis in Section 7 does not confirm. The share of students with college-educated parents is unrelated to the mental health fixed effects, as are the shares of students with mental health contacts and with parents not cohabiting. The weak role of students' socio-demographic composition contrasts with evidence that teachers are more likely to leave schools with disadvantaged students (Falch & Strøm, 2005; Scafidi et al., 2007) and that a disadvantaged student composition increases teachers' sickness absence (Karbownik et al., 2026). As these studies apply quasi-experimental identification strategies, we return to this question in Section 7 to review the causal link between student composition and teacher mental health.

The strongest register-based correlates are characteristics of a school's staff and leadership, which are less readily visible from outside. In terms of teacher composition, the clearest correlate is teaching experience: conditional on age, schools with a more experienced teaching staff exhibit better outcomes, while conditional on experience, schools with older teaching staff exhibit worse outcomes. Since teacher turnover is higher in less healthy schools (Section 4.4), a less experienced staff may partly be a consequence rather than a cause of a less healthy environment. Neither the share of female teachers nor the share of teachers with a formal teaching education is related to the mental health index; a higher share of female teachers is associated only with more days of illness absence.

Principal experience, measured as the number of years working as a primary school principal, emerges as a most robust and a significant predictor of the mental health environment: Besides Danish test scores, it is the only characteristic that is significant for all four outcomes. The magnitude of the correlation is similar to that of Danish test scores. Principals are the local managers of schools, responsible for, e.g., timetables, class assignments, decisions on staffing. In addition, they can affect the culture at a school. As with the experience of the teaching staff, the association may partly reflect reverse causality if principals leave less healthy schools more often. We explore this role of school

Table 6: School Mental Health Fixed Effects and Register-Based School Characteristics

	(1) MH Index	(2) Illness Absence Days	(3) MH Contact	(4) MH Medication
School Characteristics				
Average Class Size	0.007	0.940	0.000	-0.004
	(0.015)	(0.598)	(0.004)	(0.006)
Number of Students	0.005	0.475*	0.002	0.002
	(0.005)	(0.218)	(0.002)	(0.002)
Urban Municipality	0.011*	0.523**	0.004**	-0.002
	(0.005)	(0.194)	(0.001)	(0.002)
Student Characteristics				
Female	-0.000	-0.201	0.000	0.001
	(0.005)	(0.211)	(0.002)	(0.002)
Non-Scandi. Immigrant	-0.024***	-1.022***	-0.006***	-0.003
	(0.006)	(0.237)	(0.002)	(0.002)
MH Contact	0.004	-0.026	0.002	0.004
	(0.007)	(0.270)	(0.002)	(0.003)
College Edu. (Any Parent)	0.002	-0.122	0.002	0.002
	(0.008)	(0.270)	(0.002)	(0.003)
Parents Not Cohabiting	-0.001	0.332	-0.002	-0.002
	(0.006)	(0.218)	(0.001)	(0.002)
Danish Test Score	-0.024*	-1.062**	-0.006*	-0.010**
	(0.009)	(0.367)	(0.003)	(0.004)
Math Test Score	-0.001	-0.274	-0.001	0.003
	(0.010)	(0.417)	(0.003)	(0.004)
Class Danish Test Score SD	0.004	0.010	0.000	-0.003
	(0.007)	(0.280)	(0.002)	(0.002)
Class Math Test Score SD	0.008	0.340	0.002	0.003
	(0.005)	(0.217)	(0.001)	(0.002)
Teacher Characteristics				
Female	0.012	0.709**	0.003	0.002
	(0.007)	(0.226)	(0.002)	(0.002)
Age	0.026*	1.404**	0.010**	0.005
	(0.012)	(0.524)	(0.004)	(0.004)
Non-Scandi. Immigrant	0.015*	0.635**	0.002	0.001
	(0.006)	(0.199)	(0.001)	(0.002)
Experience	-0.038**	-1.689**	-0.015***	-0.003
	(0.012)	(0.534)	(0.004)	(0.004)
Formal Education as Teacher	-0.003	-0.344	0.000	-0.002
	(0.005)	(0.192)	(0.001)	(0.002)
Principal Chars.				
Number of Principals	-0.005	-0.126	-0.002	-0.002
	(0.003)	(0.137)	(0.001)	(0.001)
Experience as Principal	-0.019***	-0.521**	-0.005***	-0.004**
	(0.004)	(0.159)	(0.001)	(0.001)
Female Principal	-0.006	-0.101	-0.001	-0.001
	(0.004)	(0.166)	(0.001)	(0.002)
Principal ever teacher	-0.002	0.056	-0.000	0.002
	(0.004)	(0.156)	(0.001)	(0.001)
Principal/Family MH Contact	-0.004	0.042	0.000	-0.001
	(0.003)	(0.149)	(0.001)	(0.001)
Constant	-0.025***	14.770***	0.092***	0.095***
	(0.004)	(0.159)	(0.001)	(0.002)
Observations	966	966	966	966
Adj. R2	0.092	0.140	0.087	0.031

Note: The table reports results from multivariate regressions of raw school mental health fixed effects on observable school-, student-, teacher-, and principal characteristics from administrative registers. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. All regressions are estimated at the school level and cover the 966 schools with information on all characteristics. Characteristics are aggregated to the school level using simple averages. All explanatory variables are standardized to have mean zero and standard deviation one and are included jointly in each regression. See Section 3.1 for details on variable construction and Section 4.1 for the estimation of school mental health fixed effects. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$.

leadership below by analyzing teacher perceptions of their workplace, including teachers' assessment of their collaboration with school management.

6.2 Working Conditions Reported by Teachers

We next turn to teachers' perceptions of their workplace from the DLF survey. Table 7 reports separate univariate regressions of the raw school mental health fixed effects on school-level average responses, and Table A5 in the Appendix reports the corresponding multivariate regression. Teachers in less healthy schools rate almost every dimension of their workplace worse, most of all being able to provide support to students with special needs and the psychological work environment, followed by their influence on their own working conditions, opportunities for further training and development, the physical work environment (light, air, noise), and, only marginally, their collaboration with school management. They also more often experience threats or physical violence. Other plausible channels, such as interruptions of preparation time and insufficient preparation time within working hours, in contrast, are related to illness absence but not to the other components of the index.

Several of these dimensions are tangible conditions that school management can influence. The support for students with special needs and physical violence remain associated with school mental health when all items enter jointly (Table A5). Both relate to the demands of difficult classroom situations, which school management can address through resources and school-wide rules. Teachers' influence on their own working conditions, that is, their autonomy, has previously been linked to better mental health (Belloni et al., 2022; Spearing, 2025), but in our data this association does not survive conditioning on the other dimensions. The effect of the quality of collaboration with school management changes sign once the other items are included, so the survey does not allow us to attribute these conditions to management itself. Overall, the pattern of results does not confirm a critical role of management quality as measured in the teacher survey, but rather points toward a broader psychological work environment as a key correlate of the mental health index.

6.3 Student Perceptions

We further examine to what extent students' perceived well-being and classroom climate are related to the mental health environment for teachers. Table 8 relates school mental health fixed effects to survey-based student perceptions of the school environment for

Table 7: School Mental Health Fixed Effects and Teacher-Reported Workplace Perceptions

	(1) MH Index	(2) Illness Absence	(3) MH Contact	(4) MH Medication
Can support special needs kids	-0.161***	-11.302***	-0.038**	-0.026
	(0.043)	(1.794)	(0.014)	(0.019)
Psychological work environment	-0.148***	-11.229***	-0.041***	-0.019
	(0.033)	(1.515)	(0.011)	(0.014)
Influence on own working conditions	-0.096*	-7.978***	-0.038**	-0.003
	(0.040)	(1.760)	(0.012)	(0.018)
Opportunities for further training	-0.086**	-7.749***	-0.039***	0.004
	(0.029)	(1.254)	(0.008)	(0.012)
Physical work environment	-0.081*	-8.347***	-0.027**	0.015
	(0.032)	(1.264)	(0.009)	(0.012)
Collaboration quality with management	-0.081*	-6.339***	-0.028*	-0.010
	(0.039)	(1.827)	(0.011)	(0.016)
Interruptions in preparation time	-0.001	2.070*	0.007	-0.002
	(0.020)	(0.817)	(0.005)	(0.008)
Sufficient time to prepare	0.017	-2.447	-0.008	0.023
	(0.032)	(1.256)	(0.009)	(0.015)
Received threats	0.056**	4.728***	0.011*	0.008
	(0.019)	(0.801)	(0.005)	(0.007)
Suffered physical violence	0.079***	4.458***	0.015**	0.011
	(0.019)	(0.756)	(0.005)	(0.007)
Observations	922	922	922	922
Adj. R2 in multivariate	0.052	0.140	0.029	0.014

Note: The table reports results from univariate regressions of raw school mental health fixed effects on school-level average responses to the teacher survey. Each coefficient is estimated in a separate regression. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. All regressions are estimated at the school level and cover 922 schools. Most survey items are measured on a scale from 1 to 10, with higher values indicating better working conditions. School-level averages of these items are divided by 10 to facilitate comparison with the binary items. The adjusted R^2 refers to the regression including all survey items jointly, with the corresponding results reported in Table A5. See Table A1 for details on the survey items and response scales, Section 3.1 for details on mental health variable construction, and Section 4.1 for the estimation of school mental health fixed effects. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$. Data sources: Danish register data and DLF teacher survey.

1,220 schools, covering dimensions such as well-being, classroom order, safety, and perceived educational quality. Results suggest a strong link between healthier workplaces for teachers and higher reported student well-being: all ten measured dimensions are negatively associated with the teacher fixed effect, nine of them significantly. Classroom order is among the strongest of these associations, and safety is significant as well. Together with teachers' reports of physical violence and of insufficient support for students with special needs, three different measures thus point to the demands of difficult classroom situations. The demands that matter thus appear to stem from classroom behavior and the support available for it. Students' socio-demographic background nonetheless affects teachers' mental health, as we will show in the next section (Section 7), plausibly through this channel, even where it is not correlated with school mental health across schools. Taken at face value, this evidence points toward complementarity rather than a trade-off between student and teacher well-being: we find no indication that healthier working conditions for teachers come at students' expense, or vice versa, in line with the higher Danish test scores in healthier schools (Table 6).

Table 8: School Mental Health Fixed Effects and Student Perceptions of the School Environment

	(1) MH Index	(2) Illness Absence Days	(3) MH Contact	(4) MH Medication
Student Well-Being	-0.019***	-1.086***	-0.003**	-0.002
	(0.004)	(0.167)	(0.001)	(0.001)
Student Mental Health	-0.020***	-0.991***	-0.003*	-0.004*
	(0.004)	(0.177)	(0.001)	(0.001)
Student Belonging	-0.018***	-1.070***	-0.003**	-0.000
	(0.004)	(0.167)	(0.001)	(0.001)
Participation	-0.005	-0.443*	-0.001	-0.002
	(0.004)	(0.173)	(0.001)	(0.002)
Classroom Order	-0.018***	-1.208***	-0.003*	-0.001
	(0.004)	(0.162)	(0.001)	(0.001)
Educational Quality	-0.014***	-1.079***	-0.002	-0.002
	(0.004)	(0.167)	(0.001)	(0.001)
Facilities	-0.015***	-1.087***	-0.003*	0.000
	(0.004)	(0.160)	(0.001)	(0.001)
Safety/No Bullying	-0.013**	-0.806***	-0.000	-0.002
	(0.004)	(0.188)	(0.001)	(0.001)
Student Social Cohesion	-0.016***	-1.034***	-0.001	-0.000
	(0.004)	(0.172)	(0.001)	(0.001)
Student Perceived Progress	-0.009*	-0.505**	0.000	-0.003*
	(0.004)	(0.179)	(0.001)	(0.001)
Observations	1220	1220	1220	1220

Note: The table reports results from univariate regressions of raw school mental health fixed effects on school-level measures of students' perceptions of the school environment. Each coefficient is estimated in a separate regression. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. All regressions are estimated at the school level and cover 1,220 schools. All explanatory variables are standardized at the school level to have mean zero and standard deviation one. See Section 3.1 for details on mental health variable construction and Section 4.1 for the estimation of school mental health fixed effects. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$. Data sources: Danish register data and Danish Well-Being Survey (DWS).

7 Causal Effects of Student Composition

7.1 Empirical Strategy

The previous section presents correlations between mentally healthy school environments, school characteristics, and student and teacher perceptions. These associations may reflect selection and therefore cannot be interpreted as causal effects. To nonetheless examine the causal effect of a subset of these factors, specifically student composition, on teachers' mental health, we develop an additional quasi-experimental strategy based on an exact year- and subject-specific link between teachers and their students.

Specifically, we identify the cohort with which a teacher spends the largest number of teaching hours within a school year, i.e., the "main cohort". The strategy exploits the fact that teachers largely progress through grades according to predetermined teaching regimes and capacity constraints. Figure A8 in the appendix shows that most teachers either follow their cohort from year to year within a grade tier (grades 1–3, 4–6, or 7–9) and take over a new cohort when the tier starts again, or teach the same cohort in the next grade. Within-teacher variation in cohort composition stems from this cohort assignment and from students entering or leaving a cohort.

We exploit the resulting within-teacher variation in assigned student cohorts over time using a fixed effects model that accounts for teacher, year, and grade fixed effects, and linear school trends:

$$Y_{itsg} = \boldsymbol{\beta}\mathbf{X}_{itsg} + \omega_s t + \lambda_i + \gamma_t + \phi_g + \varepsilon_{itsg}, \tag{9}$$

where Y_{itsg} denotes the outcome of teacher i in school year t while teaching grade g at school s. The vector $\mathbf{X}_{itsg}$ captures average characteristics of students in the teacher's main cohort. The term $\omega_s t$ represents school-specific linear time trends. The term λ_i represents teacher fixed effects, γ_t year fixed effects, and ϕ_g grade fixed effects.

Identification relies on changes in cohort composition across grades and years within the same teacher, net of common time shocks, systematic differences across grades, and school-specific linear trends. In particular, the identifying assumption is that, conditional on the fixed effects and school trends in Equation (9), the composition of a teacher's main cohort in a given year is unrelated to time-varying unobserved determinants of the teacher's mental health. This assumption would, for example, be invalidated if teachers

were systematically assigned to cohorts on the basis of contemporaneous or anticipated shocks to their own mental health.

While this identification assumption is not directly testable, we can examine whether the composition of a teacher's main cohort is systematically related to the teacher's mental health in the previous year. If teachers were assigned to cohorts based on their own mental health, we would expect to find a correlation between last year's mental health and this year's cohort characteristics. Table A6 in the appendix instead shows no systematic relation. In regressions of the teacher's mental health index in the previous year on the characteristics of the current main cohort, which include the fixed effects of Equation (9), only one of 28 coefficients is statistically significant at the 5 percent level (the share of non-Scandinavian immigrant students in column 1), and it loses significance once school trends are included (column 2). Moreover, the joint F-tests do not reject the null of no systematic relation between all student characteristics and teachers' mental health in the previous year.

7.2 Results

Only a few student characteristics causally affect teachers' mental health, and they differ from those suggested by the correlations across schools. Table 9 summarizes results from estimating Equation (9), using the mental health index as outcome. Results indicate that a higher share of female students in a teacher's main cohort causes significantly less mental health care usage in all specifications. A higher share of students with college-educated parents reduces mental health care usage as well once year fixed effects are included (columns 2 to 5), in contrast to the absence of a correlation across schools in Table 6. Based on the estimates in column (4), an increase in either share by 10 percentage points lowers the index by 0.007 and 0.010, respectively, about 8 and 12 percent of the standard deviation of latent school mental health parameters (0.086, Section 4). We do not find that the share of non-Scandinavian immigrant students improves mental health, as indicated by the correlation across schools in Table 6: once year fixed effects are included, the coefficient is small and statistically insignificant. Neither the share of students with mental health contacts nor the share of students whose parents do not cohabit has a robust effect; the latter is significant in columns 1 to 3, but not once school trends are included. Among the test-score measures, only the dispersion of math scores matters: teachers whose main cohort is more heterogeneous in math use more mental health care (column 5, which adds test scores, available from grade 3 onwards). Note that test scores might be affected by teachers' mental health and sickness absence. Therefore, we use the score from the latest available test *before* this school year.

Table 9: Effect of Student Composition on Teacher Mental Health Care Usage

	(1) MH Index	(2) MH Index	(3) MH Index	(4) MH Index	(5) MH Index
Student Characteristics					
Female	-0.059*	-0.057*	-0.060*	-0.067**	-0.096**
	(0.025)	(0.025)	(0.025)	(0.025)	(0.031)
Non-Scandi. Immigrant	0.066*	-0.018	-0.014	-0.025	-0.010
	(0.030)	(0.031)	(0.031)	(0.037)	(0.054)
MH Contact	0.119	0.034	0.058	0.079	0.072
	(0.067)	(0.067)	(0.068)	(0.068)	(0.082)
College Edu. (Any Parent)	0.033	-0.073**	-0.084**	-0.101***	-0.111**
	(0.024)	(0.026)	(0.027)	(0.028)	(0.037)
Parents Not Cohabiting	0.059*	0.051*	0.058*	0.037	0.008
	(0.026)	(0.026)	(0.027)	(0.027)	(0.035)
Test Data					
Last Danish Test Score					-0.019
					(0.012)
Last Math Test Score					0.005
					(0.011)
SD Last Danish Test Score					-0.033
					(0.018)
SD Last Math Test Score					0.041*
					(0.019)
Adj. R2	0.358	0.359	0.359	0.363	0.374
Observations	251,864	251,864	251,864	251,862	157,390
Teacher FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Grade FE	No	No	Yes	Yes	Yes
Linear School Trends	No	No	No	Yes	Yes

Note: The table reports estimated coefficients from regressions based on Equation (9), exploiting within-teacher variation over time in assigned cohort characteristics. The dependent variable in all columns is the mental health index. All regressions are estimated at the teacher-year level and include teacher fixed effects. Columns (2)–(4) successively add year fixed effects, grade fixed effects, and school-specific linear time trends. Column (5) additionally controls for students' prior academic performance. The sample size decreases in column (5) because lagged test scores are only available from grade 3 onwards. Student characteristics are aggregated at the level of the teacher's main cohort, defined as the grade in which the teacher teaches the most hours during the school year. All student characteristics except test scores are measured contemporaneously. *Female* is the share of female students, *Non-Scandi. Immigrant* is the share of students with a non-Scandinavian immigration background, *MH Contact* is the share of students with doctor contact for mental health reasons, *College Edu. (Any Parent)* is the share of students with at least one college-educated parent, and *Parents Not Cohabiting* is the share of students whose parents do not cohabit. *Last Danish/Math Test Score* is the cohort average of the latest available test scores prior to the current school year, standardized at the year-by-grade-by-test level. *SD Last Danish/Math Test Score* is the corresponding standard deviation within the cohort, capturing heterogeneity in academic performance. See Section 3.1 for details on variable construction. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$.

Taken together, we find only limited evidence that student composition causally explains the stark differences in school mental health documented in Section 4. Within teachers, a higher share of girls and of students with college-educated parents improves teachers' mental health. The direction of the parental-education effect matches Karbownik et al. (2026), who use a similar within-teacher design for Swedish secondary school teachers and find that a more disadvantaged student composition raises stress-related sickness absence, operating through classroom conditions. Students' background thus matters for teachers' mental health through the classroom, in line with the role of classroom demands in Section 6.3, even though this effect is not visible in the cross-section. The effects are modest, however: increasing either the share of female students or college-educated parents by 10 percentage points decreases the teacher MH index by about 1 percent of a standard deviation. Together with the limited explanatory power of observable characteristics in Section 6, this suggests that the primary drivers of heterogeneity lie in factors that are not captured in our data, such as leadership style, collegial atmosphere and organizational culture.

8 Targeting Unhealthy Schools

So far, we have used the Empirical Bayes framework to estimate the distribution of latent school mental health parameters, and thus to describe the variance in teacher mental health attributable to schools. We now turn to the practical relevance of our results: the same framework may also help to identify schools at risk for particularly adverse mental-health environments, e.g., for resource allocation or audits.

Three results from the previous sections make such targeting both worthwhile and difficult. First, differences between schools are largely causal. Teachers who move to a school with a worse posterior estimate experience a deterioration of their own mental health index of about half to three fifths of the difference between destination and origin school (Figure 3). Targeting schools by their posterior estimates therefore selects environments that harm teachers' mental health, not only schools that attract less healthy teachers. Second, observable characteristics are a poor substitute for outcome-based measures. Jointly, register-based school, student, teacher, and principal characteristics explain only about one fifth of the true variation in school mental health (Section 6), so a regulator cannot identify at-risk schools from such characteristics. Third, outcome-based measures are noisy: only about half of the variance of raw school estimates reflects true differences between schools (Section 4). A ranking based on raw estimates therefore partly reflects chance, in particular for small schools. Shrinkage toward the prior mean reduces this noise, and the remainder of this section asks how much this shrinkage changes which

schools a regulator would select.

Consider a regulator who defines at-risk schools as the upper decile of the latent effect distribution, $\theta_j > q_{0.90}$, where higher values indicate worse mental health, and who can subject these 10 percent of schools to costly audits and interventions. As latent effects are not directly observable, the regulator has to rank schools by an estimate. A naive rule flags the 10 percent of schools with the highest raw school estimates $\hat{\theta}_j$. This rule will misclassify some schools due to estimation noise: Some schools enter the upper tail because their raw estimates are imprecise rather than because their latent mental-health environments are particularly bad. The Empirical Bayes rule instead flags the 10 percent of schools with the highest posterior estimates $\hat{\theta}^*_j$.[11]

Figure 7 compares the two rules. Each dot represents a school, plotted by its raw school mental health fixed effect $\hat{\theta}_j$ and the standard error s_j of this estimate. The raw rule flags all schools to the right of a vertical cutoff. Under the Empirical Bayes rule, the cutoff depends on precision: a school is flagged if its posterior estimate is at least c, the lowest posterior estimate among the flagged schools, which by Equation (6) requires $\hat{\theta}_j \geq \hat{\mu}_\theta + (c - \hat{\mu}_\theta)(\hat{\sigma}^2_\theta + s^2_j)/\hat{\sigma}^2_\theta$. The less precise the raw estimate, the higher it has to be for the school to be flagged. Both rules flag 127 schools and agree on 79 of them. The raw rule additionally flags 48 schools with high but imprecise raw estimates, most of them with standard errors of 0.1 or more. The empirical Bayes rule replaces these with 48 precisely estimated schools whose raw estimates lie just below the raw cutoff; since both rules flag the same number of schools, the two groups are equally large by construction. More than a third of the schools selected once estimation noise is taken into account (48 of 127) would thus not be selected by the raw rule.

This comparison relies on the assumption that latent school effects are independent of the sampling variances (Equation (1)). Figure 7, however, shows that schools with higher raw estimates tend to have larger standard errors, in line with the count-based components of the index (Section 4). If schools with worse environments are noisier because their outcomes are higher, some of the schools flagged only by the raw rule are truly at risk,

[11] As a recent literature on ranking and selection with noisy unit-level estimates points out, relying on posterior estimates alone does not use all available posterior information (Gu & Koenker, 2023; Kline et al., 2024; Walters, 2024). Consider two schools with posterior estimates below, but close to, the cutoff. If one posterior estimate is estimated less precisely, its latent school effect may still lie above the threshold with substantial probability. Posterior tail probabilities $P(\theta_j > q_{0.90} \mid \hat{\theta}_j, s_j)$ therefore depend not only on the posterior estimate, but also on posterior uncertainty, and ranking schools by these probabilities provides a direct measure of how likely each school is to belong to the adverse tail of the latent effect distribution. Under a normal posterior, flagging all schools with a tail probability above one half is equivalent to flagging all schools with a posterior estimate above $q_{0.90}$. The two rankings differ only if the number of flagged schools is fixed, as in our exercise, or if the regulator applies a different probability threshold, for example to reflect the relative costs of unnecessary audits and of missed at-risk schools.

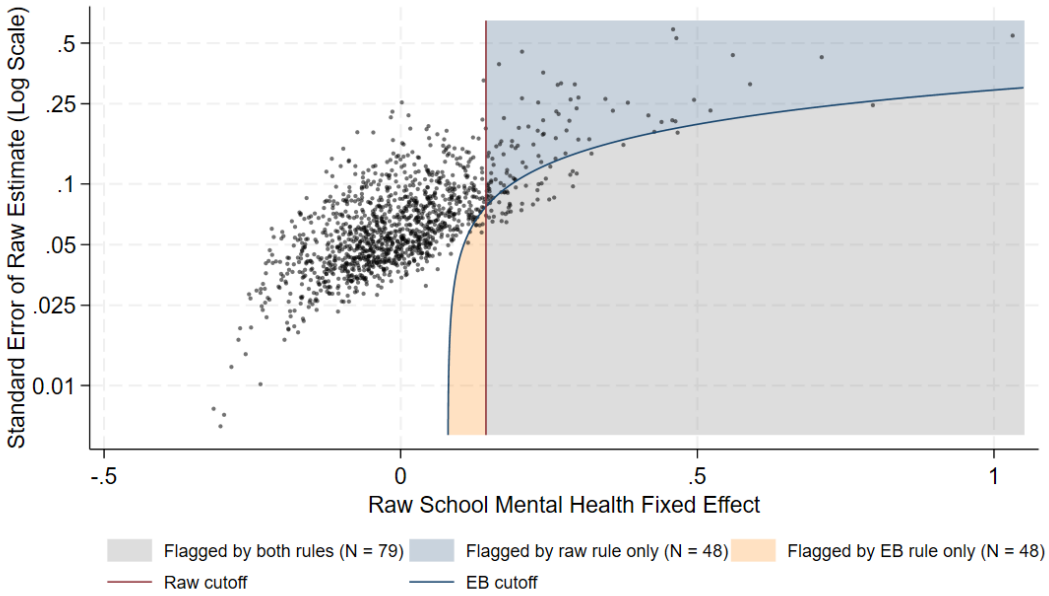


Figure 7: Flagging Schools by Raw Estimates and Empirical Bayes Posteriors

Note: The figure plots the raw school mental health fixed effects $\hat{\theta}_j$ (horizontal axis) against their standard errors s_j (vertical axis, logarithmic scale), estimated using the mental health index. Each dot represents a school, and higher values of $\hat{\theta}_j$ indicate worse mental health. The raw rule flags the 10 percent of schools with the highest raw estimates, while the EB rule flags the 10 percent of schools with the highest Empirical Bayes posterior estimates $\hat{\theta}_j^*$ (Equation (6)). The vertical line (raw cutoff) marks the lowest raw estimate among schools flagged by the raw rule. The curve (EB cutoff) marks, for each standard error, the raw estimate needed to reach the corresponding EB rule threshold. This threshold increases with s_j because less precise estimates are shrunk more strongly toward the prior mean. The shaded areas indicate schools flagged by both rules (gray), by the raw rule only (blue), or by the EB rule only (orange). The corresponding numbers of schools are reported in parentheses. See Section 4.1 for details on the estimation of school mental health fixed effects.

and the comparison overstates how many of the schools selected by the raw rule are misclassified.

We cannot directly test whether choosing schools by an EB rule is truly superior to the naive rule based on raw estimates. The EB rule is, however, supported by our leave-one-year-out validation exercise (Section 4): Empirical Bayes posteriors are nearly forecast-unbiased for held-out outcomes ($\beta_{eb} = 0.91$, SE 0.03), whereas raw school estimates overstate differences between schools by about 80 percent ($\beta_{raw} = 0.56$, SE 0.02). This suggests that regulators interested in choosing schools for audits and interventions should rely on EB posterior information rather than raw estimates.

9 Conclusion

This paper studies heterogeneity in teacher mental health across schools and asks whether schools, as workplaces, constitute a meaningful source of this variation. Using comprehensive Danish register data, we document large and systematic differences in teachers' mental health between schools. A quasi-experimental mover analysis suggests a causal link between school environments and individual health: Teachers moving between schools internalize the destination school's environment into individual mental health care utilization, by about three-fifths of the difference within three years. We characterize healthy school environments by register characteristics and survey reports: healthier schools have more experienced teaching staff and principals, and teachers report better psychological work environments and less difficult classroom situations, such as less noise, sufficient support for special needs students. Teacher and student well-being do not trade off against each other: students report higher levels of well-being in schools with less mental health care utilization of teachers.

The findings matter for policy debates on teacher mental health and retention. The results indicate that mental health burdens among teachers are shaped by differences across schools as workplaces rather than solely by individual characteristics or student composition. This points to the school as a relevant level at which mental health risks emerge and persist. Since less healthy schools lose teachers to other schools rather than to other occupations, better working conditions in these schools could reduce turnover between schools, which disrupts instruction (Ronfeldt et al., 2013). Our correlates point to the demands of difficult classroom situations, and the support available to handle them, as candidate levers. Identifying the schools that need such support requires outcome-based measures, because observable school characteristics do not single them out. These measures are noisy: Empirical Bayes posterior estimates are close to forecast-unbiased,

and using them instead of raw school estimates changes more than a third of the schools selected in our targeting exercise.

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A Supplementary Tables & Figures

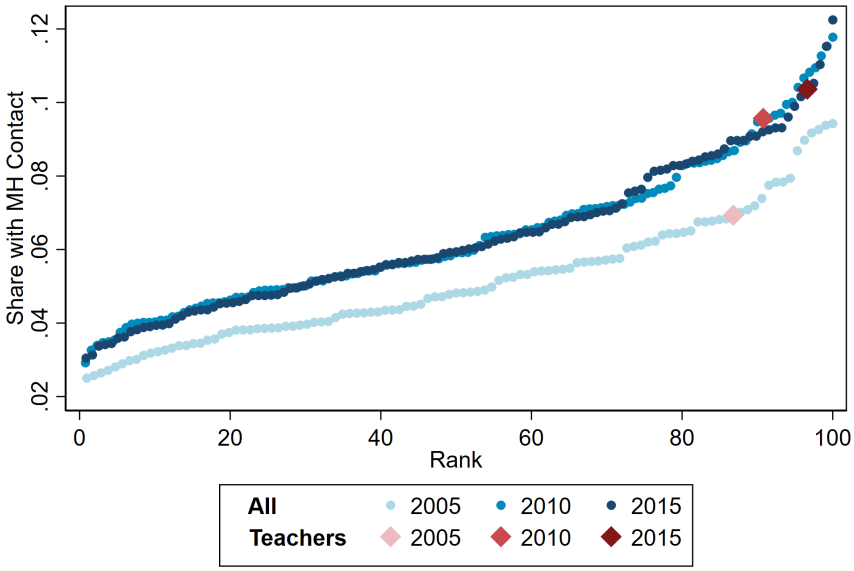


Figure A1: Occupational Rankings by Mental Health Contact

Note: The figure plots the share of workers with at least one doctor contact for mental health reasons (vertical axis) against the percentile rank of occupations based on this share (horizontal axis), separately for 2005, 2010, and 2015. Primary school teachers are highlighted by red diamonds. The sample consists of the Danish working population aged 25–65 in the respective years. Occupations are defined using the three-digit DISCO code of each individual's main occupation on November 1 of the respective year. Only occupations with at least 1,000 workers in the respective year are included. Occupations are ranked separately for each year. See Section 3.1 for details on the construction of the mental health contact indicator. The graphical representation is adapted from Svaleryd and Vlachos (2023).

Table A1: DLF Teacher Survey Items

Domain	**Survey Questions**	**Response Scale**
Stress and Sick Leave	1. Within the past year, have you been on **sick leave as a result of the working conditions** at your school?	Yes/No
	2. (Stress means a situation in which you feel tense, restless, nervous or anxious, or are unable to sleep at night because you are thinking about work-related problems.) **Do you feel this form of stress at the moment?**	1 = "Really a lot / quite a lot / some", 0 = "only a little / not at all"
Leaving Intentions	1. Within the past year, have you considered **applying for a job** as a teacher/kindergarten class leader in **another municipality** because of the working conditions in your current municipality of employment?	Yes/No
	2. Within the past year, have you considered **applying for a job** as a teacher/kindergarten class leader at **a private/free school** because of the working conditions in your current municipality of employment?	Yes/No
	3. Within the past year, have you considered leaving the profession?	Yes/No

Continued on next page

Table A1 (continued)

Domain	Survey Questions	Response Scale
	4. *Conditional on answering yes to leaving the profession:* What is the reason for your thoughts about leaving the profession? – Practical reasons (relocation, shorter commute, etc.) – Lack of alignment between tasks and resources/time – The psychological working environment, including collaboration problems – Personal reasons	Tick boxes
Work Environment	1. How are your options for providing the necessary **support to children with special needs**?	Scale from 1 to 10, where 1 is very poor and 10 is very good. Average recoded to 0–1.
	2. How are your options for **further training**/competency development?	1–10
	3. How is the **physical working environment** at the school (light, air, noise) overall?	1–10
	4. How is the **psychological working environment** at the school overall?	1–10
	5. How good is your **collaboration with the school's management**?	1–10
	6. How much **influence** do you assess that the school's teachers and kindergarten class leaders have over their **own working conditions**?	1–10
	7. Do you feel that, within the bounds of your working hours, you have **sufficient time to prepare** your teaching?	Yes/No
	8. How often within the past 14 days have you experienced unforeseen (suddenly arising) tasks eating into your **preparation time**?	1 = Several times a week/Daily, 0 = Weekly / less often than weekly / I have not experienced this.
	9. Have you, within the past 12 months, been subjected to **threats of violence** at your workplace? (Threats of violence can be verbal and expressed e.g. as threats against you, your family or friends, threats against property, but also silent threats in the form of clenched fists, drawings, etc.)	1 = any, covering "Yes, daily / weekly / monthly / more rarely", 0 = "No, never"
	10. Have you, within the past 12 months, been subjected to **physical violence** at your workplace? (Physical violence is an assault on the body, e.g. kicking, hitting, pushing, tripping, throwing objects, biting, pinching, scratching and spitting.)	1 = any, covering "Yes, daily / weekly / monthly / more rarely", 0 = "No, never"

Note: The table lists the survey items used in our analysis from the 2017 and 2019 waves of the general member survey conducted by the Danish Union of Teachers (DLF). The items are grouped into three domains: stress and sick leave, leaving intentions, and work environment. The last column reports the response scales and, where applicable, the coding of responses used in our analysis. All survey questions are translated from Danish by the authors.

Table A2: DWS Measures and Underlying Survey Items

Measure	**Survey Questions**
Student Well-Being	1. Are you happy in your school? 2. Are you happy in your classroom?
Student Mental Health	1. Do you feel lonely? 2. How often does your stomach hurt? 3. How often do you have a headache? 4. Do you fear being made fun of in school?
Student Belonging	1. I feel that I belong in my school 2. I like the breaks in my school
Participation	1. Are you and your classmates consulted on what you work on in the classroom? 2. The teachers make sure that the pupils' ideas are used in the lessons
Classroom Order	1. Can you concentrate during lessons? 2. If the classroom is noisy, the teachers are quick to restore order. 3. Is it easy to hear what the teacher is saying during class? 4. Is it easy to hear what the other students say during class?
Educational Quality	1. Are the lessons exciting? 2. Do your teachers show up on time for class? 3. Do you manage to learn what you are supposed to (want to) learn at school? 4. Do your teachers help you learn effectively? 5. The instruction makes me want to learn more 6. The teachers are good at supporting and helping me in school when I need it
Facilities	1. I like the outdoor areas in my school 2. I like the classroom (facilities) in my school 3. I think the toilets are clean and neat at my school
Safety / No Bullying	1. How often do you feel safe in school? 2. Have you been bullied this school year? 3. Have you bullied anyone at school this school year?
Student Perceived Progress	1. What do your teachers think about your progress in school? 2. I am doing well academically in school 3. I am making good academic progress in school
Student Social Cohesion	1. Most of the pupils in my classroom are kind and helpful 2. The other pupils accept me as I am

Note: The table lists the survey items from the Danish Student Well-being Survey (DWS) used to construct the ten measures of students' perceptions of the school environment. The first column lists the measures, and the second column presents the underlying survey questions, translated from Danish by the authors. The individual items are coded such that higher values are more positive, then standardized to have mean zero and standard deviation one, after which they are averaged within the measures, which is itself then also standardized.

Table A3: Correlates of Move Probability, Timing and Direction

	(1) Ever Moving	(2) Timing of Move	(3) Direction of Move
Characteristics			
Female	0.014***		−0.001
	(0.003)		(0.002)
Formal Education as Teacher	0.091***		0.008**
	(0.003)		(0.003)
Age	0.001***		0.000
	(0.000)		(0.000)
Experience	−0.005***		0.000
	(0.000)		(0.000)
Non-Scandi. Immigrant	−0.011		0.003
	(0.008)		(0.005)
Life Events			
Illness Absence Days, Last Year		0.001***	−0.000
		(0.000)	(0.000)
MH Contact, Last Year		0.058***	−0.005
		(0.012)	(0.003)
MH Medication, Last Year		0.028	0.003
		(0.018)	(0.003)
Pregnancy This or Next Year		0.094***	0.002
		(0.022)	(0.005)
Starts Cohabiting This or Last Year		0.041**	−0.002
		(0.015)	(0.004)
Stops Cohabiting This or Last Year		−0.027	0.004
		(0.015)	(0.004)
Mean Outcome	0.187	0.259	0.001
Adj. R2	0.021	-0.146	0.003
Observations	68,125	32,108	7,973
Year FE	—	Yes	Yes
Teacher FE	—	Yes	—

Note: The table reports results from regressions examining the probability, timing, and direction of teachers' moves between schools. Column (1) regresses an indicator for moving between schools at least once during the observation period on individual teacher characteristics averaged over the observation period. The regression is estimated at the teacher level and includes all teachers in the main sample. Column (2) regresses an indicator for a school move occurring in a given school year on lagged mental health outcomes and life events. The regression is estimated at the teacher-year level, restricted to teachers who move between schools, and includes teacher and year fixed effects. Column (3) regresses the difference in school mental health fixed effects between the destination and origin school on individual teacher characteristics measured in the year of the move, lagged mental health outcomes, and life events. The difference is calculated using the Empirical Bayes posterior estimates of school mental health fixed effects based on the mental health index. Positive values indicate moves to less mentally healthy schools. The sample is restricted to observations from the year of each teacher's first school move, and the regression includes year fixed effects. See Section 5.1 for details on the definition of school moves and the construction of the mover sample. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$.

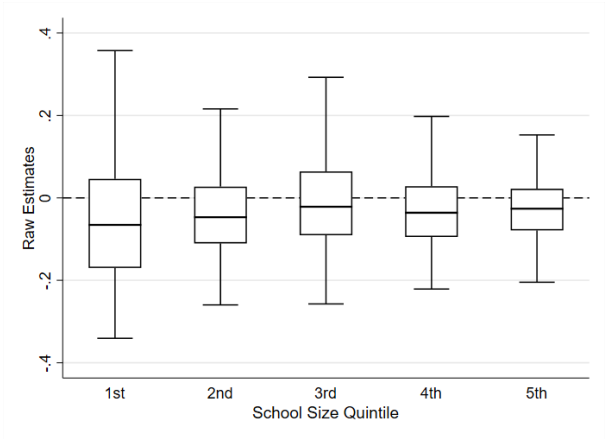


Figure A2: Raw School Mental Health Fixed Effects by School Size

Note: The figure plots the distribution of raw school mental health fixed effects estimated using the mental health index, separately by school size quintile. School size is measured by the total number of teacher-year observations per school over the observation period. The boxes indicate the interquartile range, and the horizontal lines within the boxes indicate the median. The whiskers extend to the most extreme values within 1.5 times the interquartile range from the respective quartile. The horizontal dashed line marks zero. See Section 4.1 for details on the estimation of school mental health fixed effects.

Table A4: Teacher Characteristics per Event Time

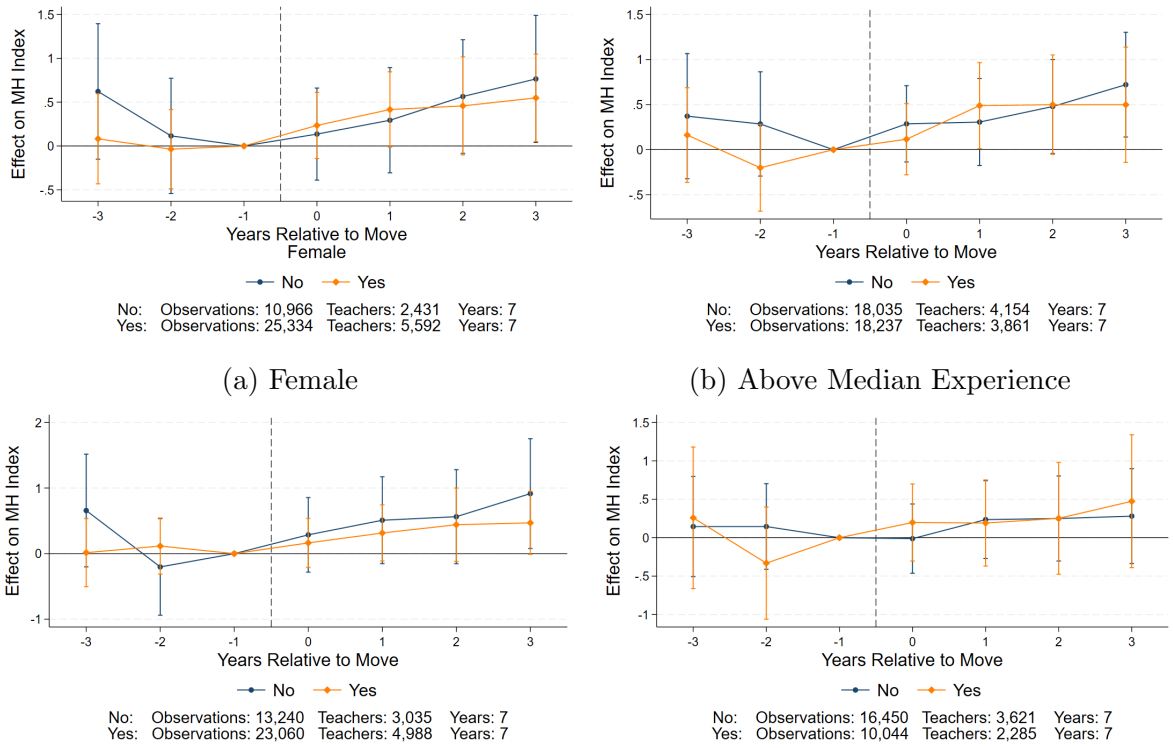
	-3	-2	-1	0	1	2	3
Female	0.70	0.70	0.70	0.70	0.70	0.70	0.70
Age	42.22	42.06	41.36	42.36	43.32	44.03	44.45
Non-Scandi. Immigrant	0.03	0.04	0.04	0.04	0.04	0.04	0.04
Experience	13.82	13.66	12.74	13.56	14.58	15.34	15.72
Formal Education as Teacher	0.83	0.83	0.82	0.83	0.85	0.86	0.87
Observations	3,004	4,289	8,031	8,031	5,557	4,403	3,046

Note: The table reports the number of teachers and average teacher characteristics by event time for the sample underlying the event studies in Figure 3. Event time ranges from −3 to 3, with event time 0 denoting the first year at the destination school. The number of teachers varies across event time because the panel is unbalanced. See Section 5.1 for details on the construction of the mover sample.

Table A5: School Mental Health Fixed Effects and Teacher-Reported Workplace Perceptions: Multivariate Regressions

	(1) MH Index	(2) Illness Absence	(3) MH Contact	(4) MH Medication
Can support special needs kids	-0.139*	-5.485	-0.002	-0.057*
	(0.061)	(2.780)	(0.019)	(0.023)
Psychological work environment	-0.224***	-15.210***	-0.045*	-0.043
	(0.058)	(2.689)	(0.020)	(0.026)
Influence on own working conditions	0.064	6.307*	0.000	0.012
	(0.071)	(3.141)	(0.021)	(0.031)
Opportunities for further training	-0.032	-3.905*	-0.030**	0.014
	(0.033)	(1.634)	(0.010)	(0.015)
Physical work environment	-0.011	-3.790*	-0.005	0.031*
	(0.029)	(1.588)	(0.011)	(0.013)
Collaboration quality with management	0.135*	9.372**	0.032	0.011
	(0.065)	(3.164)	(0.020)	(0.028)
Interruptions in preparation time	-0.027	-0.293	-0.001	-0.001
	(0.019)	(0.805)	(0.006)	(0.009)
Sufficient time to prepare	0.060*	0.448	0.005	0.033*
	(0.027)	(1.252)	(0.008)	(0.015)
Received threats	-0.010	2.157	-0.003	-0.001
	(0.027)	(1.135)	(0.007)	(0.011)
Suffered physical violence	0.075*	2.071	0.013*	0.010
	(0.030)	(1.054)	(0.006)	(0.010)
Constant	0.044	19.259***	0.112***	0.100***
	(0.041)	(1.697)	(0.010)	(0.018)
Observations	922	922	922	922
Adj. R2	0.052	0.140	0.029	0.014

Note: The table reports results from multivariate regressions of raw school mental health fixed effects on school-level average responses to the teacher survey, analogous to the univariate regressions in Table 7, but including all survey items jointly in each regression. The dependent variables in columns (1)–(4) are school fixed effects estimated for the mental health index, illness absence days, an indicator for doctor contact for mental health reasons, and an indicator for mental health medication, respectively. All regressions are estimated at the school level and cover 922 schools. See Table 7 for details on variable construction and scaling. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$. Data sources: Danish register data and DLF teacher survey.



(a) Female

(b) Above Median Experience

(c) Having Children

(d) Large School Move Distance

Figure A3: Event Study Heterogeneity

Note: The figure plots the estimated event study coefficients $\hat{\beta}_r$ from Equation (8) separately for different groups of teachers. As in Panel (a) of Figure 3, the dependent variable is the mental health index. Panel (a) distinguishes between female and male teachers, Panel (b) between teachers with teaching experience above the median and those at or below the median, Panel (c) between teachers with and without children under the age of 18, and Panel (d) between teachers moving distances of more than 20 km and those moving distances of 20 km or less. Teaching experience and parental status are measured in the year of the move, and the median teaching experience is calculated within the mover sample. Move distance is measured as the distance between the origin and destination school. Treatment intensity $\hat{\delta}_i$ is defined as the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. Event time 0 denotes the first year at the destination school, and event time -1 serves as the reference period. Regressions are estimated separately for each subgroup at the teacher-year level and include individual and year fixed effects. See Section 5.1 for details on the construction of the mover sample. Whiskers indicate 95% confidence intervals based on robust standard errors.

Table A6: Student Composition and Lagged Teacher Mental Health Care Usage

	(1) MH Index	(2) MH Index	(3) MH Index	(4) MH Index
Student Characteristics				
Female	-0.021	-0.018	-0.015	-0.027
	(0.028)	(0.028)	(0.037)	(0.037)
Non-Scandi. Immigrant	-0.082*	-0.066	-0.088	-0.063
	(0.038)	(0.044)	(0.052)	(0.062)
MH Contact	-0.055	-0.053	-0.062	-0.027
	(0.069)	(0.070)	(0.083)	(0.083)
College Edu. (Any Parent)	-0.024	-0.009	-0.047	-0.057
	(0.031)	(0.031)	(0.041)	(0.041)
Parents Not Cohabiting	-0.011	-0.014	-0.020	-0.026
	(0.033)	(0.032)	(0.043)	(0.040)
Test Data				
Last Danish Test Score			-0.022	-0.020
			(0.013)	(0.013)
Last Math Test Score			-0.006	-0.006
			(0.013)	(0.013)
SD Last Danish Test Score			-0.010	-0.016
			(0.019)	(0.019)
SD Last Math Test Score			0.031	0.027
			(0.021)	(0.021)
Observations	172,866	172,849	110,825	110,797
Adj. R2	0.361	0.368	0.367	0.380
Joint F-Test P-Value	0.294	0.632	0.314	0.418
Teacher FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes
Linear School Trends	No	Yes	No	Yes

Note: The table reports estimated coefficients from regressions based on Equation (9), using the lagged mental health index as the dependent variable. The analysis examines whether the assignment of teachers to cohorts with different student characteristics is related to teachers' mental health in the previous school year. All regressions are estimated at the teacher-year level and include teacher, year, and grade fixed effects. Columns (2) and (4) additionally include school-specific linear time trends. Columns (3) and (4) additionally control for students' prior academic performance. The sample size decreases in these columns because lagged test scores are only available from grade 3 onwards. Student characteristics are aggregated at the level of the teacher's main cohort, defined as the grade in which the teacher teaches the most hours during the school year. All student characteristics except test scores are measured contemporaneously. *Female* is the share of female students, *Non-Scandi. Immigrant* is the share of students with a non-Scandinavian immigration background, *MH Contact* is the share of students with doctor contact for mental health reasons, *College Edu. (Any Parent)* is the share of students with at least one college-educated parent, and *Parents Not Cohabiting* is the share of students whose parents do not cohabit. *Last Danish/Math Test Score* is the cohort average of the latest available test scores prior to the current school year, standardized at the year-by-grade-by-test level. *SD Last Danish/Math Test Score* is the corresponding standard deviation within the cohort, capturing heterogeneity in academic performance. The reported F-test p-value refers to the test of joint significance of all student characteristics included in the respective regression. See Section 3.1 for details on variable construction. Robust standard errors are reported in parentheses; $^{*}p < .05$,$^{**}p < .01$,$^{***}p < .001$.

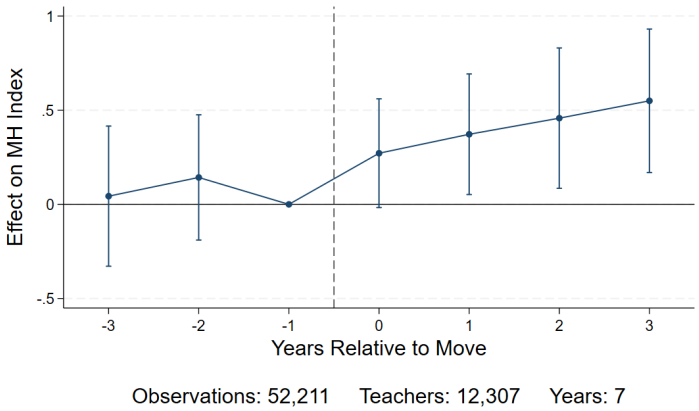


Figure A4: Event Study Including Teachers With Gap Years

Note: The figure plots the estimated event study coefficients $\hat{\beta}_r$ from Equation (8), using the mental health index as the dependent variable, analogous to Panel (a) of Figure 3. In contrast to the main specification, the sample additionally includes teachers with interruptions in employment at public primary schools between their origin and destination school (gap years). Treatment intensity $\hat{\delta}_i$ is defined as the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. For teachers with gap years, this difference does not account for the mental health environment experienced during these years. Event time 0 denotes the first year at the destination school, and event time -1 denotes the immediately preceding year and serves as the reference period. Observations at event time -1 are unavailable for teachers with gap years. All regressions are estimated at the teacher-year level and include individual and year fixed effects. See Section 5.1 for details on the construction of the mover sample. Whiskers indicate 95% confidence intervals based on robust standard errors.

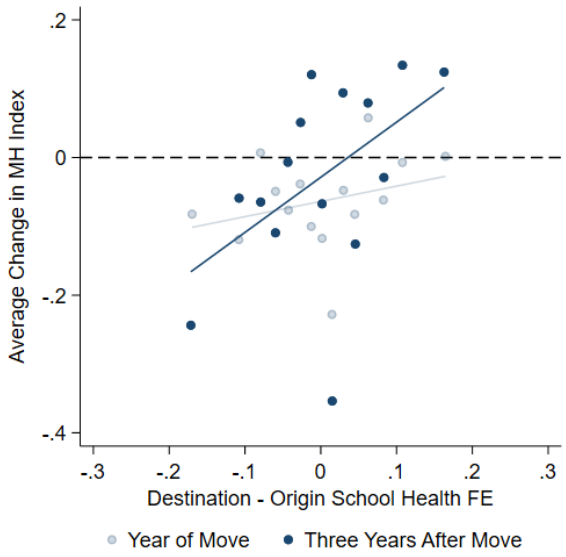


Figure A5: Heterogeneity by Treatment Intensity

Note: The figure plots changes in teachers' mental health index following school moves against treatment intensity. Treatment intensity $\hat{\delta}_i$ is defined as the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. Teachers are grouped into 15 bins based on treatment intensity. The vertical axis shows the average within-teacher change in the mental health index between event time -1 and event time 0 or 3, respectively. Event time 0 denotes the first year at the destination school, and event time -1 denotes the last year at the origin school. Changes are plotted as raw means within each bin, without adjusting for covariates or fixed effects. Each series is based on all available observations at the respective event time. Positive values on either axis indicate a deterioration in the respective measure of mental health. See Section 5.1 for details on the construction of the mover sample and treatment intensity.

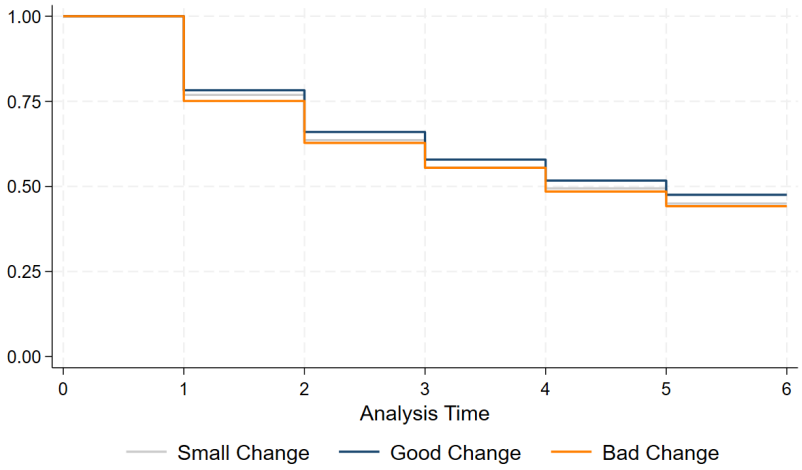


Figure A6: Kaplan-Meier Survival at School Post Move

Note: The figure plots Kaplan–Meier survival estimates for teachers at their destination school, separately by the magnitude and direction of the change in the school mental health environment. Survival is defined as remaining employed at the destination school. Both moving to another school and leaving the teaching profession count as failure events. Teachers who remain at the destination school until the end of the observation period are right-censored. The sample consists of teachers who change schools at least once between the school years 2013/2014 and 2019/2020 without an interruption in employment at a public primary school during the transition. For each teacher, we consider only the first school move. Moves are grouped based on the difference between the Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. A "Good Change" ("Bad Change") corresponds to the lower (upper) quartile of the distribution of these differences, with negative values indicating a healthier destination school. A "Small Change" corresponds to a difference that deviates from the median of this distribution by no more than half the interquartile range. Event time 0 denotes the first year at the destination school. See Section 5.1 for details on the construction of the mover sample and Section 4.1 for the estimation of school mental health fixed effects.

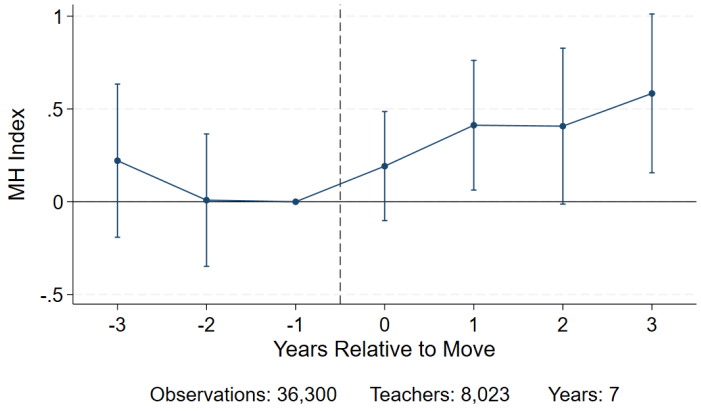


Figure A7: Event Study With Conditional Posterior School Estimates

Note: The figure plots the estimated event study coefficients $\hat{\beta}_r$ from Equation (8), using the mental health index as the dependent variable, analogous to Panel (a) of Figure 3. In contrast to the main specification, school mental health fixed effects are estimated controlling for observable school characteristics in addition to year fixed effects. These characteristics include the number of students at the school, the share of non-Scandinavian immigrant students, the share of students with at least one college-educated parent, and a proxy for municipality-level supply of and demand for mental health care, measured by the share of individuals aged 25–65 in the municipality with doctor contact for mental health reasons, excluding public primary school teachers. Treatment intensity $\hat{\delta}_i$ is defined as the difference between the resulting Empirical Bayes posterior estimates of the school mental health fixed effects of the destination and origin school, estimated using the mental health index. Event time 0 denotes the first year at the destination school, and event time -1 serves as the reference period. All event study regressions are estimated at the teacher-year level and include individual and year fixed effects. Whiskers indicate 95% confidence intervals based on robust standard errors.

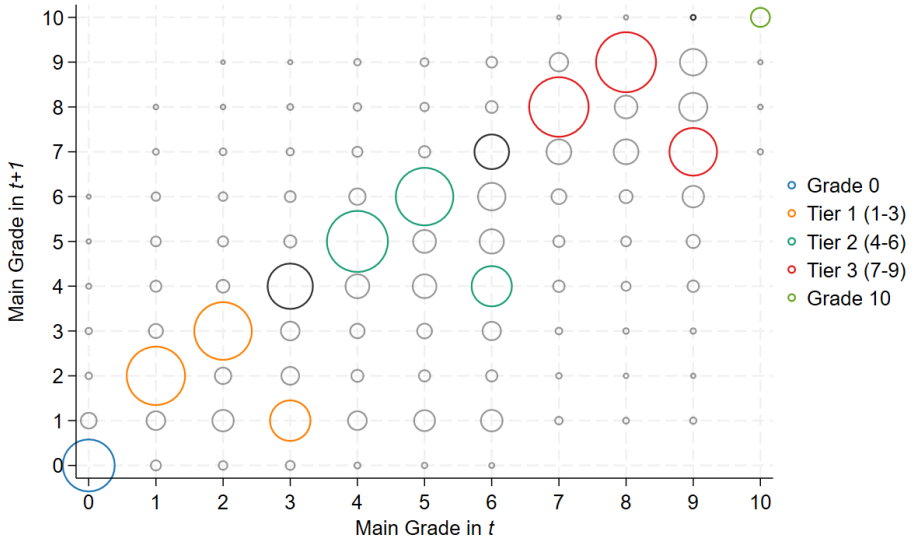


Figure A8: Teacher-Cohort Assignment from One Year to the Next

Note: The figure plots the frequencies of transitions between teachers' main grades in consecutive school years. The main grade is defined as the grade in which a teacher teaches the most hours during the school year. The horizontal axis indicates the main grade in year t, and the vertical axis indicates the main grade in year $t+1$. Bubble size reflects the frequency of the respective grade combination. Colors highlight transitions consistent with the tier system: *Tier 1* comprises grades 1–3, *Tier 2* comprises grades 4–6, and *Tier 3* comprises grades 7–9. Transitions in which teachers follow their cohort within a tier are highlighted accordingly. *Grade 0* and *Grade 10* are shown as separate categories.