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Mental health spillovers in primary schools

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Abstract: In this paper we use extensive Danish register data to study peer effects from students with mental health problems. In our analysis, mental health problems are defined from health information in registers on medical prescriptions, contact with general practitioners and psychologist and psychiatrists. We follow classes from 1st to 6th grade between 2007 and 2013 and study mental health trajectories of incumbent students when receiving a new student with mental health problems. We use a time to event design, controlling for individual and grade fixed effects and using never treated classes as the control group. We condition on students being observed from grade 1 until treatment and follow them after treatment irrespectively of the school or class they attend. Following the new difference-in-differences literature, we produce estimates that are robust to heterogeneity across cohorts and grades. As additional robustness check, we compare against classes that receive a new student without a mental health problem, and produce results adjusting for changes in diagnoses trends. We find that outcomes for incumbent students are affected by receiving a new student with mental health problems, an effect that is larger when first exposure occurs by grade 6. These effects are long-lasting irrespective of the gender of the mover and of the gender of the incumbent student. We are currently working with well-being data to identify whether the effect works through direct student-to-student contagion (behavioral effect) or through a higher likelihood of diagnosis due to parental awareness of the condition (information effect).

Keywords: Mental health problems, time-to-event, peer effects.

1. Introduction

Understanding the magnitude of peer influence in mental health has critical policy implications. Evidence about how students with low well-being and potential latent diagnoses might be directly or indirectly affected by increasing the number of students with mental health diagnoses in the classroom can inform and improve inclusive education policies, in addition to contributing to understand the positive externalities of mental health treatment and public health school interventions. The existence of peer effects triggers the so-called multiplier effects, which reflects the extent to which the scope of a policy, program or treatment is magnified by strategic complementarities in behaviours and conditions. The impact of any policy that targets the distribution of children with mental health problems will be larger in the presence of endogenous peer effects, as the policy will affect the individual not only directly but also indirectly through the influence on his/her peers. Thus, the social value attributed to mental health treatment, school policies, or preventive interventions will be largely dependent on the existence of peer effects.

This paper examines peer effects among Danish students in public primary schools on short- and medium-term measures of mental health and well-being. Our measures are short- and medium-term measures in the sense that they capture effects from being exposed to a diagnosed student on other students' mental health diagnosis up to five years from exposure. The aim of the paper is to understand how social interactions mediate the production of mental health and affect students' well-being and their likelihood of future diagnoses. Specifically, we explore the extent to which primary school children in a classroom are negatively or positively affected by being exposed to a new student with a pre-existent mental health diagnosis, and how student, class composition and school characteristics mediate this relationship. Negative peer effects could stem from class disruption or from the direct influence of students with mental health problems on other students' mental health. Positive peer effects could stem from parents or teachers being more able to identify or respond to certain underlying mental health condition once there is a child with a diagnosis of that condition in the classroom.

We take advantage of the panel nature of the data and use variation given by the entry of a new student in the class to identify the effect of the new peer on the trajectory of incumbent students' mental health diagnoses and wellbeing. Our approach compares "treated" school-grades (those receiving a new student with mental health problems) to "never treated" ones. We take into account potential selection effects in the comparison of treated school-grades with never treated ones by using a difference-in-differences design. Our critical assumption is that the evolution of new diagnosis would have been the same in the treated school-grades than in never treated ones, had the school-grade not been exposed to a diagnosed new student.

Our results indicate that incumbent students experience increases in the diagnosis of mental health problems upon the entry of a new student with mental health problems. The effects appear two or three years after the new student's entry and are largest for students first exposed at 6th grade, at the start of the adolescence. To analyze mechanisms, we are now exploring with additional data whether being exposed to a new student with mental health problems negatively affects incumbent students' wellbeing, suggesting class distortion or direct contagion channels, or if it just affects diagnoses either through higher awareness of mental health problems by parents and school staff, or through a reduction of stigma and an increase in the likelihood of seeking treatment. The analysis of the well-being data will allow us to understand whether peer effects are positive or negative. Further heterogeneity analysis (pending), will let us identify student and school characteristics that mediate peer influence.

2. Background literature and contribution

Our study relates to the literature analyzing mental health determinants and its consequences. Among the former, there is plenty of evidence that analyzes the effects on mental health of prenatal and early life conditions (Persson et al., 2016; Adhvaryu et al., 2019; Singhal, 2019), negative economic shocks (Marcus, 2013; Bubonya et al., 2017; Schaller et al., 2015), neighborhood conditions (Katz et al., 2000; Leventhal et al., 2003; Ludwing et al., 2008, Ludwing et al., 2012; Graif et al., 2016; Boje-Kovacs et al., 2018) and rising internet and social media usage (Braghieri et al., 2022; Donati et al., 2022; McDool et al., 2020).

In terms of its consequences, poor mental health has been associated with disruptions in educational attainment and choices (Fletcher, 2008; Fletcher, 2009; Currie et al., 2004; Currie et al., 2010; Cornaglia et al. 2012), poorer labor market outcomes (Fletcher, 2012; Fletcher, 2013; Banerjee et al., 2017; Biasi et al., 2021; Butikofer et al., 2020; Lundborg et al., 2014) and crime (Fletcher et al., 2009; Anderson et al., 2012; Blattman, 2017).

2.1 Peer effects on adults' emotional and mental health

Several papers report how mental states and emotions can transfer from one individual to another. Christakis and Fowler (2008) investigate whether happiness can spread from person to person in large social networks. Their results suggest that clusters of happy individuals result from the spread of happiness and not just from people's tendency to associate with similar individuals.[1] While this research has been contested for not accounting for common contextual variables or shared experiences, experimental or quasi-experimental studies using social networks have also provided evidence on emotional contagion. Coviello et al (2014) show that rainfall directly influences the emotional content of Facebook messages, and also affects the status messages of friends in other cities not experiencing rainfall. Similarly, Kramer et al (2014) vary experimentally the emotional

[1] Friends who live within a mile and become happy increase the probability that ego is happy by 25 percent. This correlation increases to 42 percent when the alter who becomes happy lives less than half a mile away.

content in Facebook's News Feed. They find that when positive expressions are reduced, people produce fewer positive posts and more negative posts; while when negative expressions are reduced, the opposite pattern occurs.

The evidence is more ambiguous when assessing peer influence in mental health disorders. A strand of research in the psychology and public health literature finds evidence of interpersonal convergence or contagion of depressive symptoms (Prinstein 2007, Rosenquist et al 2011, Kiuru et al 2012). Rosenquist, Fowler, and Christakis (2011), for example, find large associations between an individual's level of depression and that of her peers, associations that extend up to three degrees of separation (to one's friends' friends' friends). Boje-Kovacs et al. (2018) find that being exposed to a deprived neighborhood as an adult has a significant negative impact on mental health among vulnerable men. A problem with these studies is that they do not sufficiently account for correlated effects, such as the weather affecting all members of the same network. When scholars address this issue, results indicate weaker links. For example, Eisenberg, Golberstein, Whitlock, and Downs (2013) find no evidence of overall contagion of happiness and evidence of small peer effects for specific mental health measures (anxiety and depression) in an experiment exploiting the random assignment of first- year college students to roommates. Pachucki, Ozer, Barrat, and Cattuto (2015) find cross-sectional correlations in mental health status across students in the same network. However, they find no evidence that connected students' mental health status becomes more similar over time because of their network interactions. In line with these two studies, Ho (2017) finds no evidence of sibling spillovers in health symptoms that could have mental causes, such as loss of appetite or stomach pain, while finding spillovers due to the spread of contagious illnesses, like the stomach flu. Taking advantage of sibling pairs who attend different schools, Ho identifies potential spillovers by instrumenting the health of the sibling by that of his or her classmates.

There is some research showing that peer influence may increase the search for mental health treatment. This is particularly important, because the lack of knowledge regarding signs and symptoms, ignorance about how to access treatment, and stigma are among the main reasons for undertreatment of mental health diseases. Golberstein, Eisenberg, and Downs (2016) use college housing-assignment data to study peer effects in the use of mental health services. The study finds evidence of peer influence when groups are defined at the hall level. The paper finds that exposure to a higher proportion of peers with a recent history of mental health treatment is associated with more positive beliefs about treatment effectiveness.

2.2 Peer effects on children and adolescents' mental health

Despite the importance of social relationships in children and adolescents' development, there is limited evidence on whether a child's mental health problems affect other peers' mental health during this stage. A strand of literature has studied the effects of disruptive peers on other students' test scores, risky behaviors, and mental health. Albeit related to our work, in these papers, disruptiveness is associated with a broad set of problematic peers and not only with mental health diagnosis or emotional problems.[2] Among this line of work, Carrell et al. (2011), Figlio (2007) and Xu et al. (2022) find negative effects of disruptive peers on students' achievement, in-class behaviors, school engagement, and educational expectations. Carrell et al. (2018) shows that the adverse effects persist into young adulthood, as manifested by a lower probability of college attendance and reduced earnings. Feng et al. (2021) identify a negative effect of disruptive peers on personality skills development, but no impact on academic performance. In terms of effects on mental health, Gutierrez et al. (2020) find that exposure to disruptive peers increases depression, isolation, and victimization from bullying. And Wang et al. (2021) show that classroom exposure to children who were left behind

[2] Disruptive peers are not only identified by their own characteristics, but also by parents' risk behaviors (alcohol or drug abuse, criminal records) or intelligence (proxied by educational attainment) which are not perfect measures of children characteristics or behaviors.

in Chinese rural areas by their migrant parents impacts negatively on their peers' mental health. The effect appears to work through poorer classroom climate and peer-to-peer interactions. Lavy et al. (2011, 2012) and Zhao et al. (2020) also suggest that the negative effect of disruptive peers could be explained by increasing in-class misbehavior and modifying teachers´ pedagogical practices. In contrast, Bietenbeck (2019) encounters positive spillovers of disruptive peers on test scores and on the probability of high-school graduation and taking college-entrance exams. The author attributes these effects to improved non-cognitive skills, as students exposed to disruptive peers show more effort and discipline levels.

Of special interest to our work are the studies by Kristoffersen et al. (2015) and Fletcher et al. (2010), who narrow down the definition of disruptive peers only to those with special needs. Still, the definition of special needs includes not only mental health or emotional disorders, but also a large number of other difficulties, such as learning disabilities, speech disabilities, or physical impairments. As most papers on the effects of disruptive peers, both Kristoffersen et al. and Fletcher et al., are focused on short-term effects on cognitive outcomes. Kristoffersen et al. (2015) leverage the variation in peer disruptiveness generated by children moving across schools in Denmark, to identify the effects of disruptive peers on test scores. They find negative effects, which are stronger for children exposed to disruptive peers with a psychiatric diagnosis or who have parents with a criminal history. Fletcher (2010) arrives at similar results when exploring the effects of peers with serious emotional problems (as reported by their teacher) in the United States.

More recently, Balestra et al. (2022) exploit within-school random assignment to classrooms in Switzerland to explore the effects of peers with special education needs on short- and medium-term education outcomes. Results show negative spillovers on test scores and on entering post-compulsory formal education, with larger effects for children in the lower end of the ability distribution. Contrary to this finding, Rujis (2017) for Netherland and Friesen (2010) for Canada find no effect of exposure to peers with special education needs on test scores. Both studies fail to find effects, even when considering different types of problems separately, including social, emotional, or behavioral difficulties. In the case of Friesen (2010), results are not too conclusive, as standard errors are quite large. Regarding non-cognitive outcomes, Gottfried (2014) finds that the exposure to peers with special needs increases behavioral problems and lowers self-control, learning, and interpersonal skills. The author also highlights the importance of differentiating disability categories, as they have asymmetric effects over different outcomes. On the opposite side, as in the disruptive-peers literature, there is also evidence that being exposed to special needs peers improves outcomes: using data on the proportion of students with special needs in Texas' schools, Hanushek et al. (2002) find positive spillovers of having special needs peers on test scores.

We are aware of only two studies that analyze directly the effects of classroom exposure to peers with mental health conditions on students' own mental health. In an analysis of Chinese Middle Schools that randomly assign students into classrooms, Zhang (2019) finds little evidence that the mental health condition of peers affects classmates' overall mental health, but some evidence of heterogeneity for specific mental health conditions and socioeconomic characteristics. On the contrary, using AddHealth data for the US, Giulietti et al. (2022) find that exposure to peers with mental health disorders significantly affects the likelihood of short- and long-term depression for females but not for males, while also negatively affecting college attendance and the likelihood of working. Moreover, they find that the effects are larger for girls from less advantageous socioeconomic backgrounds.

To sum up, there are few studies analyzing directly the spillovers of peers' mental health problems on other students' mental health during childhood and adolescence. Most of the evidence comes from the literature on disruptive and special-needs children, which includes other categories beyond mental health conditions, and focuses mainly on short-run education outcomes. Even within this broader literature, results are mixed, with some studies finding negative spillovers on test scores and mental health outcomes, while others finding no effects or even positive spillovers. Apart from identification strategies and sample representativeness of the population under study, the disparities in results could be explained by differences in the institutional settings or in the mental health problems considered,[3] as pointed out by Gottfried (2014). Moreover, in most studies, measures of mental health conditions are self-reported by teachers, parents or children, and not based on administrative data on psychiatric diagnosis. In fact, the work of Aizer (2008) on ADD underscores the importance of distinguishing between self-reported and diagnosed measures of mental health conditions. Aizer (2018) leverages the exogenous timing in diagnosis and treatment of ADD to evaluate its effects on peers' test scores. She finds negative and statistically significant effects of undiagnosed children on peers´ test scores, but this effect disappears once the children is diagnosed[4]. She also shows that diagnosed peers behave better than those not diagnosed, but do not improve their academic performance. The work of Aizer (2008) not only highlights the importance of taking into account the institutional setting when evaluating peer effects, but also the relevance of correctly understanding the entitlement behind professional psychiatric diagnosis in that setting. This consideration is also made by Rijus (2017) as a plausible explanation behind the disparity exhibited in the results encountered within the special needs literature.

2.3 Our contribution

Our study contributes to the literature in several ways. First, unlike most of the prior studies, it pins down the relevant explanatory variable to peers' mental health diagnosis, allowing us to identify directly the spillover effects of mental health conditions that are prevalent during childhood and adolescence, such as ADHD, autism, depression and anxiety. Our findings improve the understanding

[3] Externalizing behaviors could have more effect over academic performance while internalizing behaviors could impact personality or mental health.
[4] The result hint the channels for the spillover to other peers.

of multiplier effects associated with mental health conditions and have direct implications on the estimation of the value added of mental health treatment and public health preventive programs. Second, we provide new evidence on the spillover effects of mental conditions on peers' mental health dynamics. Most previous papers have focused on test scores and other schooling-related outcomes. We focus our analysis on the trajectories of mental health outcomes several years after first exposure. We are not aware of any other paper showing that spillovers do not appear immediately, but two to three years after first exposure. Third, the use of both administrative measures of mental health diagnosis and self-reported measures of well-being allows us to disentangle effects associated with disruption and contagion from those that have to do with awareness and stigma reduction [we are currently working on this]. Ours is one of a few studies to distinguish mechanisms behind peer effects in mental health.

3. Data

3.1 School Sample

Our main population consists of primary-school students while they attend public and private school in normal classes in Denmark. School records stem from the Danish Student Register (KOTRE), the Students' Class Identifier Register (UDKLASSE_ID, and the Special-Needs Education Register (UDSP). To be in the sample, the student must attend a grade corresponding to having begun 1st grade between 2007 and 2013. We define the year a student started grade 1 as the student's school cohort. By grade 7, some school-grades are merged, creating completely new classes not included in the data. For this reason, we only consider classes up to grade 6, although we follow students for additional years to assess the dynamic impacts of having been subject to peer effects in mental health up to grade 6. We observe children in our data from year 2007-2018.

Consequently, in order to consider the full primary school sequence for each cohort (grades 1 through 6), we restrict the sample to school cohorts 2007-2013.

3.2 Diagnosis Data

We use data on psychiatric diagnoses to create our treatment indicator. The measure of psychiatric diagnosis is cumulative in the sense that a child is categorized as diagnosed if ever had a diagnosis from age 2 until the moment the child is observed in the data. The term diagnosis always refers to a psychiatric diagnosis. Psychiatric diagnoses are defined by the ICD10 Classification of Diseases and Related Health Problems (WHO) and collected from psychiatric and somatic hospital records in the Danish National Patient Register (PSYK and LPR, respectively). We consider the following psychiatric conditions more common among children and teenagers: ADHD, autism, anxiety, and depression. Following VIVE,[5] we use the following ICD10 codes to define mental health diagnoses: F90 for ADHD, F84 for autism, F40-F42 for anxiety, and F32-F34 and F43 for depression.

3.3 Well-being data

We obtain measures of emotional well-being in the classroom from a large national survey, the "Danish Well-being Surveys" (DWS) administered since 2014 by the Danish Ministry of Education at schools during the first quarter of each calendar year (Andersen et al., 2015). We link the survey data to the diagnosis and school sample data and work with an anonymized version stored on a secured server by Statistics Denmark.[6] The DWS includes 20 questions for children in grades 0-3 and 40 questions for children in grades 4-9. [we are currently working on this data].

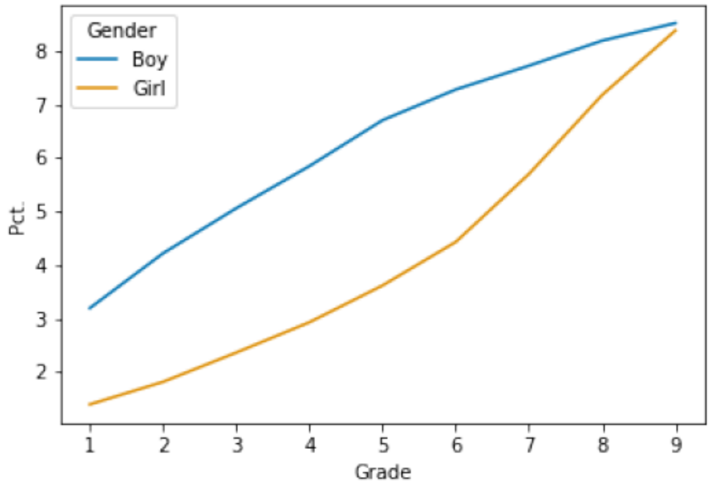
3.4 Descriptive statistics

[5] VIVE is a Danish knowledge-providing independent government organization that contributes to the development of the welfare society and the public sector.

[66] The Danish Data Protection Agency, Statistics Denmark and the Ministry of Education have granted access to the data for this research project.

Our explanatory variable of interest is captured by an indicator of exposure (at the school-grade level) to a new student with a mental health diagnosis. Our main outcome is the likelihood that the student is diagnosed with a mental problem. Figure 1 shows the prevalence of mental health diagnosis in our sample by gender and grade. While our treatment is only defined up to grade 6, we show statistics up to grade 9 as we follow students for some years after they have been exposed to a new student with a mental health diagnosis. While boys are initially more likely to be diagnosed, girls start catching up from grade 6 onwards. At grade 1, only 0.3% of girls are diagnosed with a mental health disorder, versus 1.2% of boys. At grade 6 the rates are 4.3% and 7.2% respectively. By grade 9, the prevalence of diagnosis converges to 8.5% for both genders. Boys are more likely to be diagnosed with autism ad Attention Deficit Hyperactivity Disorder (ADHD), conditions that are typically detected at late kindergarten and early primary school. Girls are, to a larger degree, diagnosed with depression and anxiety, which show at later ages.

Figure 1: Mental health diagnosis, by gender (2007-2013).



4. Methodology

4.1 Treatment status

A student is called a “treater” if this student moves to a new school and has been diagnosed with a psychiatric diagnosis at least one year before changing school.[7] A student is referred to as “treated” if this student is enrolled in a class where a child from another school with a psychiatric diagnosis is received (i.e., a student diagnosed the year before entering the class). Treatment is at the school grade level, in the sense that, if any class at the given school and grade is treated, all classes in this grade are counted as treated. This restriction prevents selection bias from headmasters and teachers who strategically could place the treater-children in the worse/better classes. When a child has been treated, we assume this child keeps being treated for the rest of the data period, even if this child moves out of the school or if the treater-child does.[8] Students that are not treated between 1st and 6th grade (i.e., that do not receive in their class a new student with a mental health diagnosis while in primary school) are considered never treated and are used to construct counterfactuals.

4.2 Balanced sample

A child is observed in the data when the child attends a public or a private school in a normal class. If this child moves to a class that either has no class ID, is a mixed grade class or is a special needs class, the child is no longer observed. Similarly, a child initially attending one of these classes will start being observed from the moment this child enrolls in a normal class. Because of these moves, we further restrict the data to include only children observed from grade 1 until potential treatment. For example, in the analysis of those first treated at grade 3, all children in either the treated or control group have to be observed from grades 1 to 3. Similarly, children in the "first treated at grade 6” analysis (both treated and control) have to be observed through grades 1 to 6. Conditioning

[7] We only observe classes on October 1st of each year, so any change of school in between will be marked as a change the year after. In order to assure that a diagnosis is not a response to the school change, we condition on the child being diagnosed in the year before the school change.

[8] This assumption could attenuate treatment effects, but assuming that the child would no longer be affected by the received treatment when changing school is also problematic.

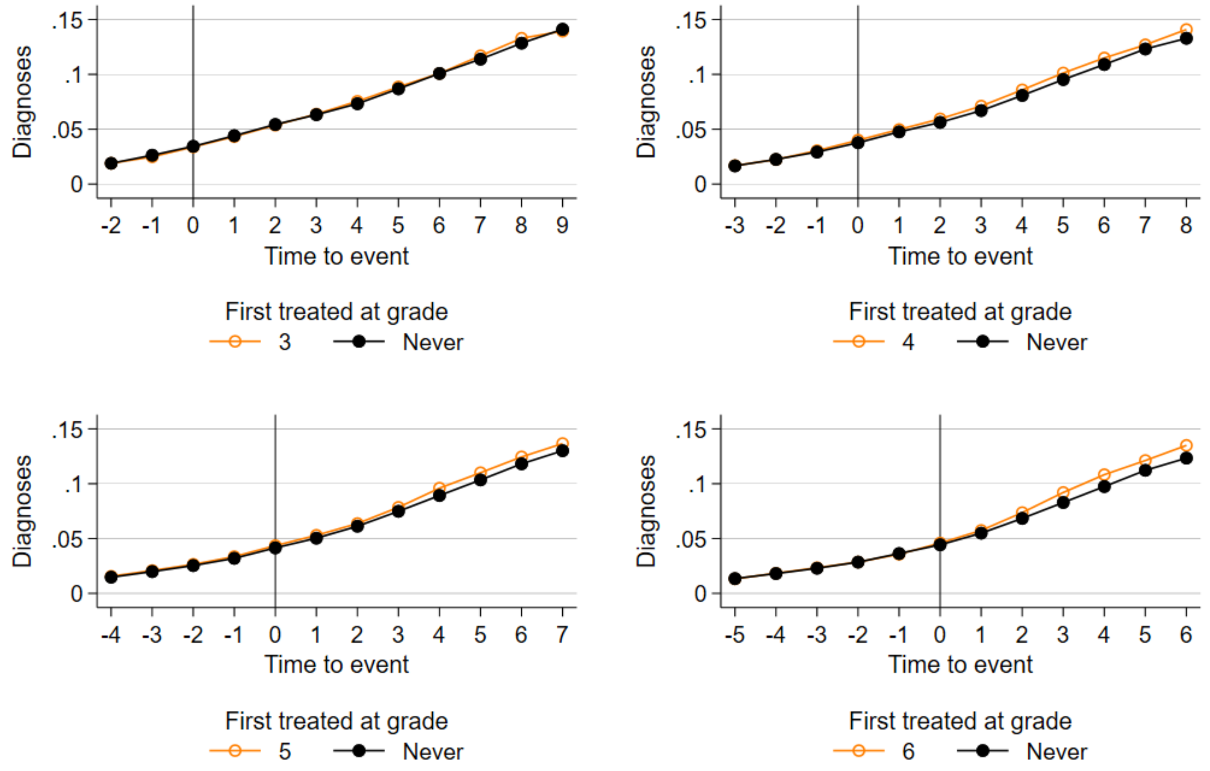
on being observed only until treatment solves the problem of selecting based on a potential reaction to treatment.[9] In addition, we drop a small share of classes outside the interval of 15-30 students.

Because we are able to observe diagnoses data until 2020, we follow children from grade 1 until 6 years after potential treatment, that is up to six years after they have been first exposed. When analyzing the effect of being first "treated" at grade 3, we have 193,697 individual-year observations on students exposed to the treatment group and 1,481,788 individual-year observations in the control (never-treated) group. When analyzing the effect of being first treated at grade 6, we have 249,922 observations in the treatment group and 1,311,345 observations in the control (never-treated) group.

Figure 2 plots the share of students with a diagnosis by grade and treatment group. Treaters are excluded from the sample from the year they become treaters and onwards.[10] The Figure suggests a small divergence in the trends of diagnoses after treatment, more pronounced for those treated in grade 6. In the next subsection, we formally define our strategy to identify the effects of the moving student on the dynamics of incumbent students' mental health diagnoses.

Figure 2. Share of diagnosed students by grade for treated and never-treated classes

[9] However, it does not solve the problem that children dropping out of the sample before potential treatment could be different from those staying until treatment, which is an external validity issue.
[10] Treaters will often occupy never treated classes before they become treaters.



4.3 Identification

To estimate the effects on classmates' mental health of receiving a child with a mental health diagnoses, we take an event study approach and express the dynamics of the outcome trajectory relative to time to treatment. An event study with a comparison group of never treated students is a dynamic difference-in-difference study, where we assume the never treated students are a good counterfactual to the treated students, conditional on a set of fixed effects. We rely on the assumptions of no anticipation (no student in the class responds to treatment before treatment in anticipation of exposure to the new student), and parallel trends, that is, that the outcome of the treated group would have evolved in the same way than that of the comparison (never treated) group had it not been exposed to treatment. Sun and Abrahams (2021) show that with heterogeneous dynamic treatment effect profiles, the coefficients of a given lead or lag obtained from a classic Two-Way Fixed Effects (TWFE) regression will be contaminated by leads and lags from other periods. Therefore, we choose to estimate cohort specific treatment effects and then manually aggregate these across treatment cohorts. We do this by following Cengiz et al. (2019) stacked regression approach, which is especially convenient given that we want to vary the comparison group with each treatment cohort. Specifically, we split the data into four datasets consisting of one treatment cohort each (those treated first in grade 3, 4, 5 or 6) and the corresponding group of never treated students. We call these datasets $h \in \{3,4,5,6\}$ and stack them together such that many students in the comparison group are now duplicated or even quadrupled. We then estimate the following regression:

$$y_{igh} = \sum_{k=-h+1}^{k=6} \delta_{kh} D_{igh}^{k} + f_{gh} + f_{ih} + e_{igh} \qquad (1)$$

where y_{igh} is the outcome of interest. Our first outcome variable is whether or not the student has a psychiatric diagnosis. We then repeat the analysis with well-being outcomes. The treatment indicator D_{igh}^{k} equals 1 for student i in grade-school g and dataset h if the student receives a new student with a psychiatric diagnosis in $k = 0$ and the outcome is measured k periods from treatment. We leave all event time periods k = −1 out as reference points. We control for dataset interacted with grade fixed effects f_{gh} and for dataset interacted with student fixed effects f_{ih}. After obtaining all $\hat{\delta}_{kh}$ we manually aggregate $\hat{\delta}_{k} = \sum_{h} \omega_{h}^{k} \hat{\delta}_{kh},$ with ω_{h}^{k} equal to dataset h's share of the treatment population in the given event time k. Since the treatment cohorts do not cover the exact same event time periods, the weight for some treatment cohorts/datasets will be zero in some event time periods. Standard errors are clustered at the level of the child's initial school to account for common shocks within schools.

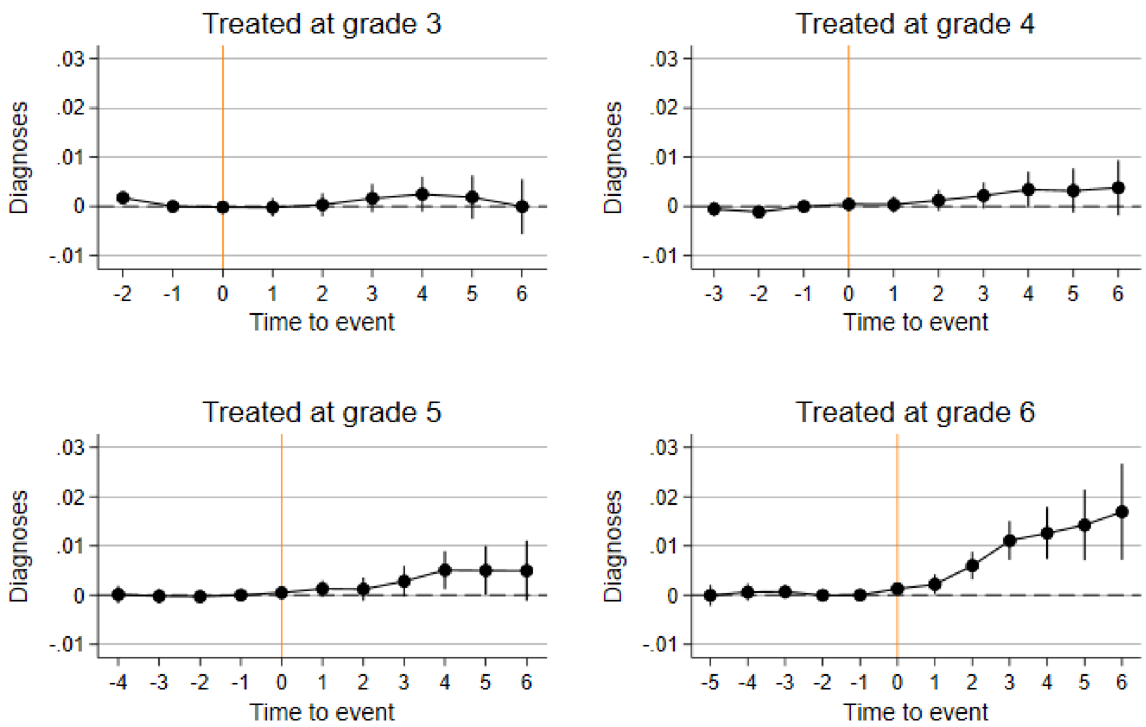
5. Results

5.1 Effects on Mental Health Diagnoses

5.1.1 Main results

Figure 3, panels (a) and (b), show the event study estimates of children first treated at grade 3 when compared to never treated students as control units. The treatment effects are very small in magnitude and non-statistically significant. As the grades increase (panels (c) and (d)), there is evidence of some statistically significant effects, but only in the longer run. Students exposed to a newcomer with a mental health condition increase their likelihood of diagnosis by 0.5 percentage points 4 to 6 years after first exposure. For those first treated in grade 6, exposure increases diagnosis by 0.6 percentage points after 2 years, and the effect increases even more as time progresses. Six years after treatment, a student is 1.8 percentage points more likely to be diagnosed with a mental health problem, a large effect (15%) when compared to a 12 percent likelihood of being diagnosed in the never treated group.

Figure 3. Effects of exposure to a new student with a mental health diagnosis. Event study.



The event study allows us also to test the assumption of parallel pre-trends. We find no statistically significant coefficient prior to treatment, nor can we reject the hypothesis of zero effects when testing all lead coefficients jointly. To make the analysis cleaner, we test for pre-trends considering only untreated observations (never treated students and treated students before treatment). Results in Figure 4 show that we are unable to reject the hypothesis of differential pre-trends, providing evidence in favor of the comparison being the right counterfactual, and hence, evidence in favor of a consistent treatment effect.

Figure 4. Testing the parallel pre-trends assumption with only pre-treatment data.

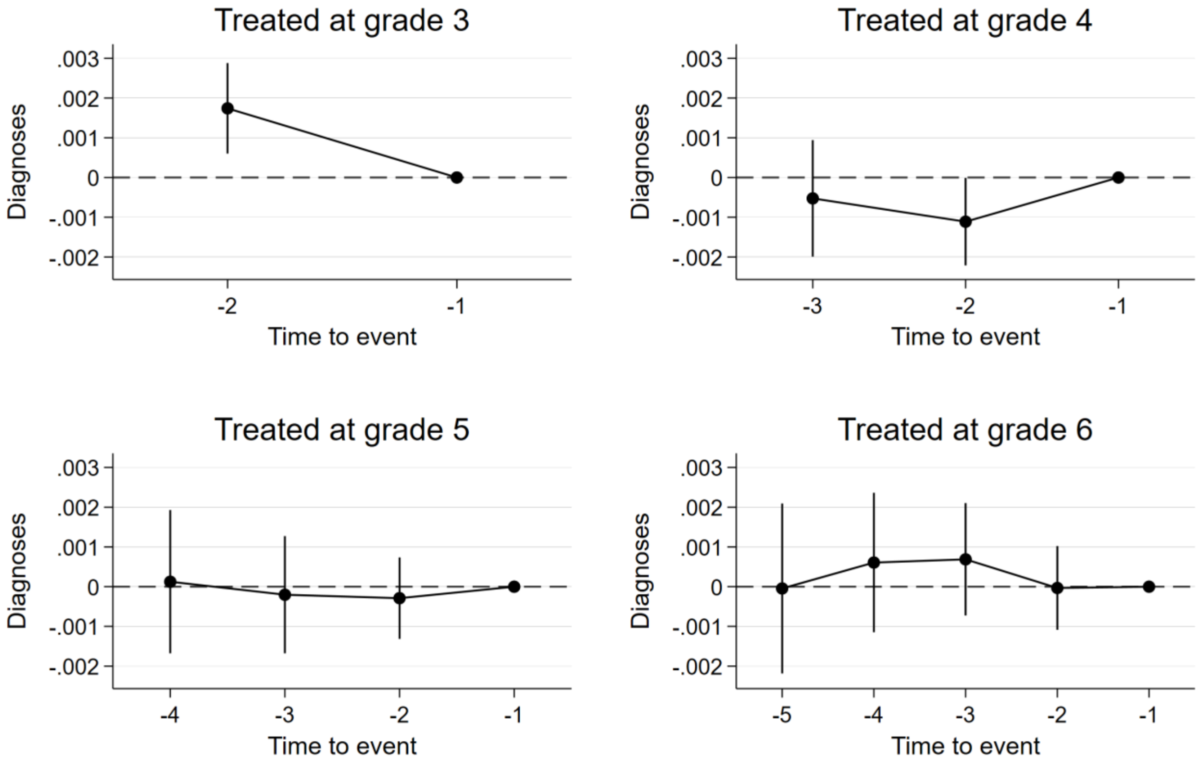
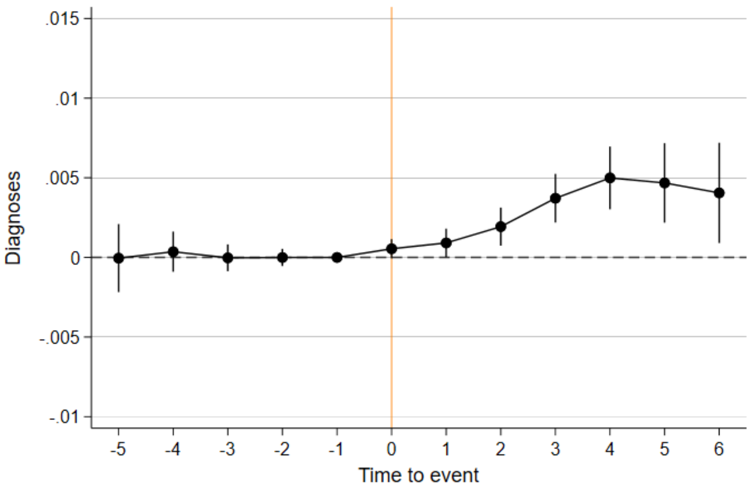


Figure 5 shows the event study when manually aggregating treatment effects across grade of first treatment (cohort). Here, the weights equal the treatment grade's share of the treated population at the given time-to-event. Given that we follow the students treated at grade 6, five periods before treatment, and those treated at grade 3 only two periods before treatment, the pre-trend coefficients reflect different cohort compositions - as do the weights used for aggregation. Similarly, we are only able to follow students treated in grade 6 until time-to-event 6 if they belong to school cohort 2007, while students treated in grade 3 can be followed until time-to-event 6 also if they belong to school cohort 2008-2010. Since the aggregated treatment effects are driven by students treated in grades 5 and 6, the aggregated treatment effects start to diminish after period 4 reflecting that those treated at grade 3 take up a larger share of the treated students in 6th grade compared to 3rd grade. However, the cohort composition does not seem very important in general. The TWFE estimated by treatment cohort are not sensitive to whether the sample is restricted to school cohort 2007-2010 or 2007-2013.

Figure 5. Event study with manual aggregation of effects across grade of first treatment



We also test for parallel pre-trends in the aggregated data, both by using all the pre- and post-treatment data and by using only the untreated observations. We find no statistically significant coefficients prior to treatment.

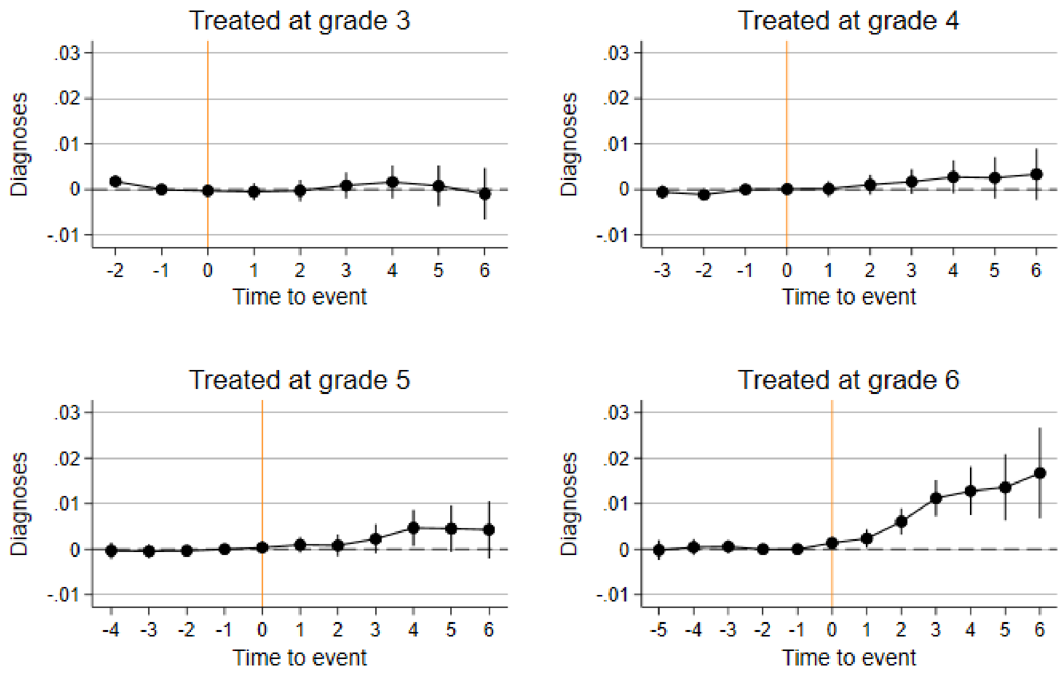
5.1.2 *Robustness I: Using never-treated school-grades that received a new student as comparison group*

We consider an alternative comparison group that includes, as before, never treated students observed from 1st grade until potential treatment, but is now constrained to school-grades that received a new student without a mental health diagnosis at the same grade level as the treatment group receives their treater.[11] The control of never treated students is only reduced by 25.000-40.000 students at each grade (for those first treated at 3rd grade it is reduced from 180.000 to

[11] We do not require that this be the first time the control group receives a new student.

140.000 students). Both the descriptive evidence and the event study estimates are very similar to the baseline results. Results are reported in Figure 6.

Figure 6. Effects of exposure to a new student with mental health diagnosis, when using as comparison group never treated students in school-grades receiving a new student.

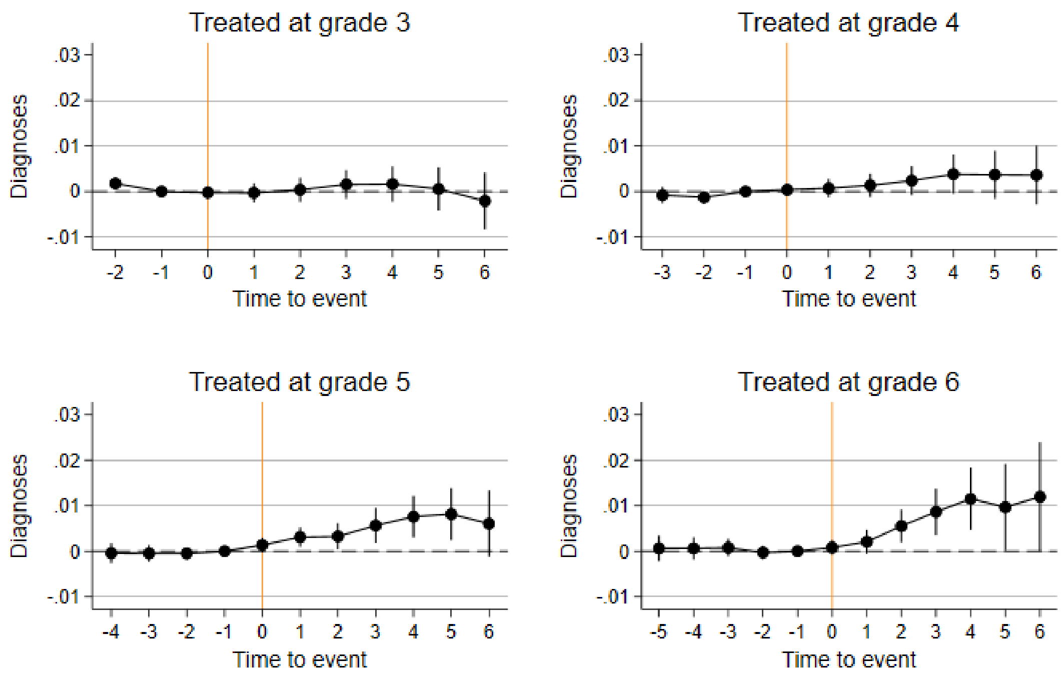


5.1.3 Robustness II: assessing SUTVA

Another concern is that the control group may also be exposed to some sort of treatment coming from sending this treater. If this were the case, the Stable Unit Treatment Value Assumption (SUTVA) would be violated, and we would end up capturing the aggregate effect of letting go of a treater and receiving one. In practice, however, both the treated and the control groups send treaters to other schools. To check how much our results may be affected by a "sender" effect, we first exclude school-cohorts sending a treater before potential treatment if they belong to the never treated control group, and in a following exercise, we exclude all school-cohorts sending a treater before potential treatment, regardless of their belonging to the treated or control groups. We do not exclude school-cohorts sending a treater after the potential treatment, as we follow children in the post treatment period regardless of what type of classes they attend. A school sends a treater if this child attends a school cohort at the time in point when the treater is sent or later. If this school sends a treater in grade 5, the student is still part of the treatment/ control group for those potentially treated in grade 3 and 4, but not for those potentially treated in grade 5 and 6.

When excluding senders from control school-cohorts only, results are almost identical to those in our core specification (those in Figure 3). Figure 7 shows event study results when school-cohort senders of treaters are excluded from both the treated and control groups. While results remain qualitatively similar to those in Figure 3, the magnitude of the effects at grade 6 is somehow smaller, stabilizing around 1 percentage point by the 4th year after first exposure.

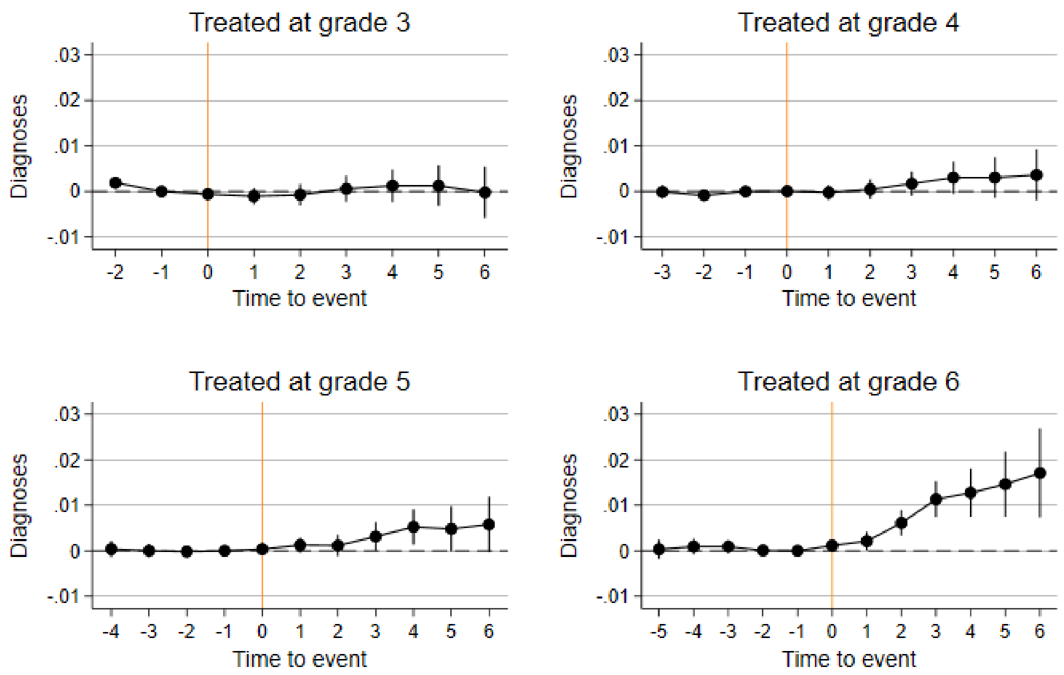
Figure 7. Event study: senders of treaters are excluded from the sample.



5.1.4 Robustness III: controlling for trends in diagnoses

To make sure that the results are not driven by a trend in diagnoses over time, we include cohort times year fixed effects to account for this trend and enable the trend to differ across cohorts. We show results in Figure 8. Aside from a slight change in the estimates of those treated in grade 3 (which remain insignificant), results are indistinguishable from those in Table 3.

Figure 8. Effects of exposure to a new student with a mental health diagnosis. Controlling for time trends that differ across cohorts.

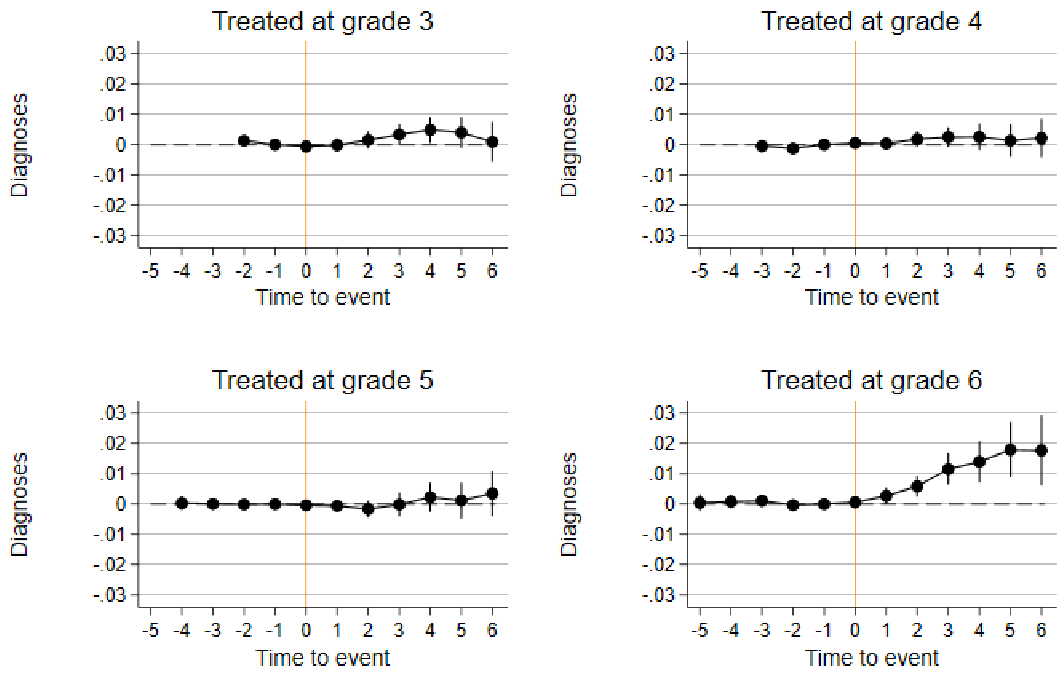


5.1.5 Heterogeneity by the gender of the treater

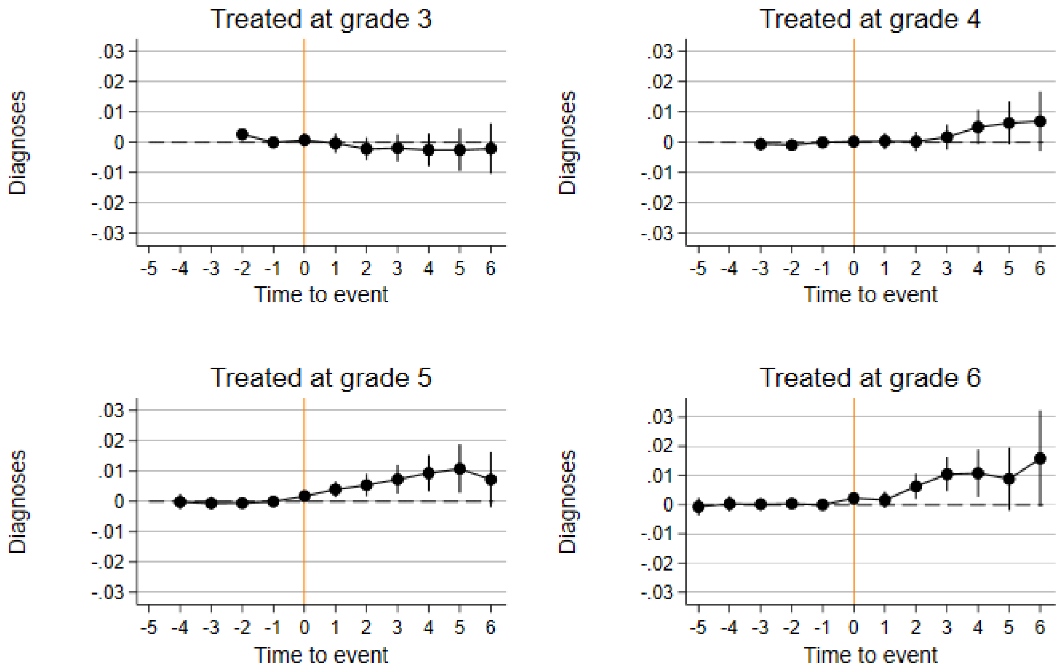
Figure 9 shows results of our main specification (equation (1)), when the treater is a boy (panel a) or a girl (panel b). Boy treaters do not have much impact on peers until grade 6, although at grade 3, boys appear to impose some sort of temporary small treatment effect that may reflect an information mechanism where some peers are diagnosed more quickly due to this new peer. If the treater is a girl, there seems to be a long-lasting effect on peers already from treatment at grade 5. These gender-specific treatment effect patterns, could be caused by anxiety and depression being more contagious than other conditions, or it could be related to the fact that girls are in general diagnosed later on, and thus, the treatment from girls could be stronger and the effect on them easier to detect. [We are in the process of investigating this further by running an analysis by type of diagnosis].

Figure 9. Dynamic effects of exposure to a new student with a mental health diagnosis, by gender of the treater.

a) Effects when treated by a boy



b) Effects when treated by a girl

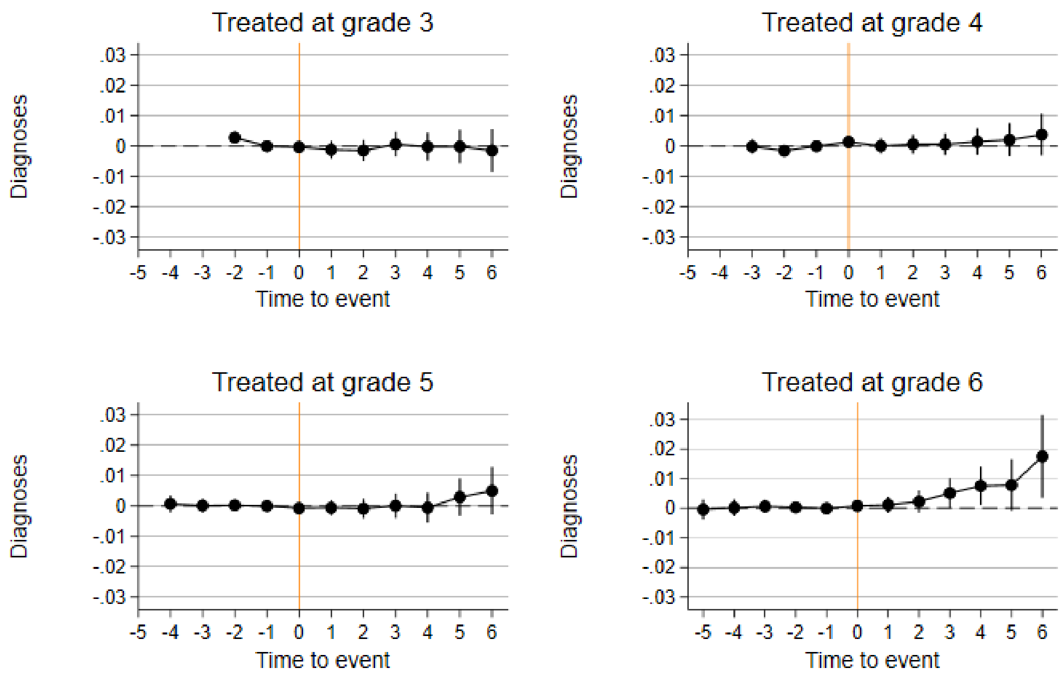


5.1.6 Heterogeneity by the gender of the treated

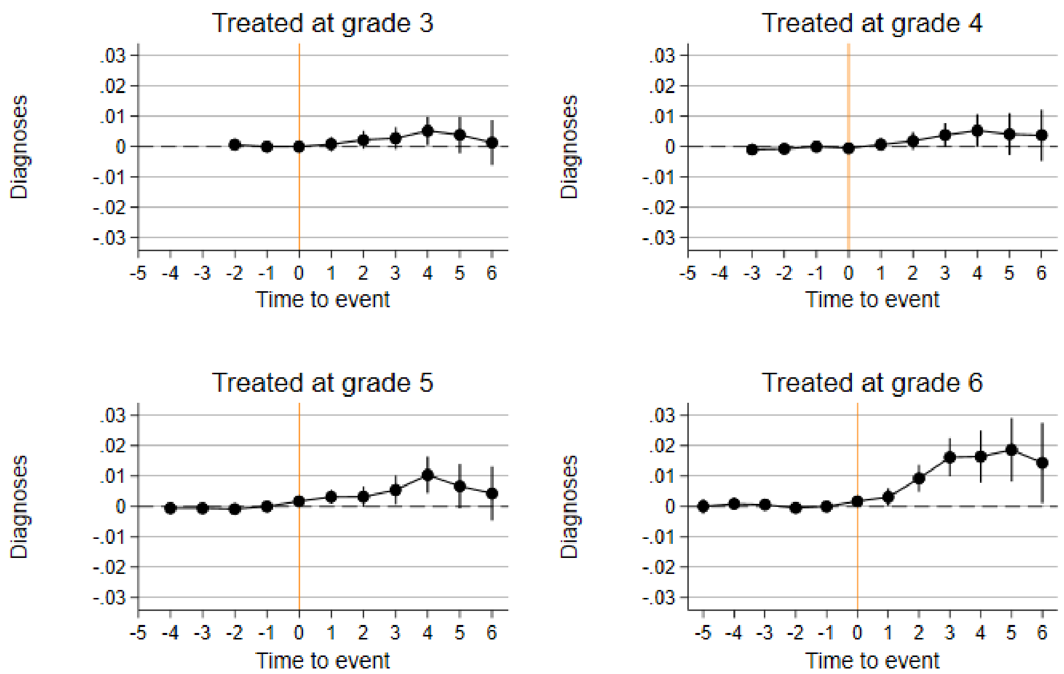
In general, girls seem to be more sensitive to peers' diagnoses than boys. The treatment effect on boys is small in magnitude and non-existing for treatment initiated in grade 3-5 but borderline significant and long lasting for boys treated at grade 6 (see Figure 10 panel a)). Girls are more sensitive to treatment but the effect is only temporary significant in grades 3 and 5, while the effects are long lasting and significant for those treated at grade 6 (see Figure 10 panel b)).

Figure 10. Dynamic effects of exposure to a new student with a mental health diagnosis, by gender of the treated.

a) Effects on boys' mental health



b) Effects on girls' mental health



5.2 Well-being outcomes

[We are currently working on this part of the paper, with a similar identification approach].

6. Conclusion

In this paper we have analyzed whether students with mental health problems impose spillovers on their peer students' mental health. We do this by leveraging the variation in peer mental health status generated by children moving across Danish primary schools. Using student and grade fixed effect time-to-event studies, we find a positive treatment effect on incumbent students' likelihood of being diagnosed with a mental health condition. The effects are small and statistically insignificant when exposure occurs before grade 5, but become larger in grades 5 and 6. Students first exposed to peer with a mental health diagnosis in grade 6 are about 2 percentage points more likely to be diagnosed in the future, compared to a prevalence of diagnosis of 12 percentage point in the comparison group. Effects do not occur right away, but take from 4 to 6 years to manifest.

We are currently working on the school well-being data, that will allow us to further understand the mechanisms underlying the peer effects identified: whether they are due to contagion or class-disruption or whether they respond to a higher awareness of parents and teachers about underlying mental health problems, and a higher likelihood of seeking diagnosis and treatment. We are also undertaking more heterogeneity analysis by type of diagnoses, and students' and school characteristics, to better understand the features that contribute to mitigate or exacerbate peer effects.

We believe our results contribute to the understanding of social interactions at school and of the determinants and consequences of mental health conditions. By shedding light on the spillovers of mental health treatment, and school preventive interventions, they can help inform and improve inclusive education policies.