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Regular Article

Under pressure: High-Stakes exams and media-reported student suicides in India

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A R T I C L E I N F O

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A B S T R A C T

We estimate the temporal association between high-stakes exam timing and media-reported student suicides in India using geocoded news data. Student suicides are 17.5 percent higher during months in which NEET-UG or JEE-Mains examinations are held, with no comparable pattern around lower-stakes board exams or among adults. The association is concentrated when national entrance exams coincide with board exams, suggesting the salience of cumulative exam burden. These patterns are stronger among girls, in states with higher exam participation, and in urbanised districts. Event-study estimates show suicides increasing immediately before and during exams, while movements around result announcements are weaker and less precisely estimated. We find no evidence that broad shifts in media attention during cyclone or election months account for the pattern, although exam-specific reporting bias cannot be ruled out. In the absence of mental health data, we generate an alternative database to address a policy question and spur future research.

1. Introduction

Youth mental health deterioration is a global concern with severe economic consequences, including low educational attainment, reduced productivity, earnings loss, and increased health costs that perpetuate poverty and inequality (Belfer, 2008; Kessler et al., 2007; Patel et al., 2007). Suicide, an extreme condition of mental health crisis, is a leading cause of death for those aged 15–29, and its reduction is a target in the United Nations Sustainable Development Goals.

Youth suicides in high-income countries are found to follow a cyclical pattern linked to the school calendar, rising when school is in session and declining during breaks (Chandler et al., 2022; Hansen and Lang, 2011; Hansen et al., 2024; Matsubayashi et al., 2016; Lahti et al., 2007). However, evidence from Low and Middle-Income Countries (LMICs), which account for 77% of global suicides (World Health Organization, 2022), remains scarce due to data limitations, mental health stigma, inadequate policies, criminalization of suicide attempts, and absent death registration systems (Guzmán et al., 2019; Kane et al., 2019). The data is often not available at high frequency and lower granularity, making it difficult to investigate the suicide trigger in contexts where a majority of the world's population resides. Existing literature attributes suicide seasonality to school-related issues but fails to identify specific triggers like academic stress, violence, bullying, and interpersonal conflicts. Academic pressure and high-stakes testing are associated with poor student mental health, including elevated anxiety, depression, and suicide ideation (Chen and Glaude, 2017; Deb et al., 2015; Yang et al., 2023).

We examine the association between high-stakes exams and the seasonality of student suicides in India using novel geocoded high-frequency data extracted from the media reports. We focus on post-high school national entrance exams for engineering and medical programs, which are mandatory screening tests for prestigious professional degrees. Focusing on post-high school competitive exams helps us isolate academic stress and the timing of the exams from other school-related factors. India represents nearly one-third of global youth, making this contribution particularly meaningful for policymakers in LMICs,

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with the unavailability of granular data on this extreme mental health outcome. We focus on student suicides because this is a vulnerable age group with significant potential for policy intervention.

We find that media-reported student suicides are 17.5 percent higher during months in which NEET-UG or JEE-Mains exams are held. This average association masks important heterogeneity across exam periods. The strongest and most precisely estimated association occurs when national entrance exams coincide with secondary board exams, suggesting that periods of cumulative examination burden may be particularly salient. Event-study estimates further show that suicides rise in the period immediately preceding examinations, peak during the examination month, and subsequently decline.

As our outcome is constructed from media reports, we interpret these estimates as temporal associations in media-reported student suicides rather than the causal effect of competitive examinations on underlying suicide incidence. We assess the validity of the media-based measure by comparing it with official National Crime Records Bureau (NCRB) data and conducting a range of robustness and falsification exercises that address selective reporting, geographic coverage, placebo suicide outcomes, and alternative exam timing. These exercises support the stability of the documented temporal pattern, although exam-specific reporting bias cannot be entirely ruled out.

We contribute to the literature on mental health and education in the following ways. First, according to our knowledge, this is a novel contribution explaining the association between high-stakes national entrance exam timing and student suicides in a developing country context. Using district-month panel data with high-dimensional fixed effects, we separate exam-month patterns from broader school-year seasonality and from time-invariant district characteristics. Some of our estimates also isolate the timing of exams from that of result announcements.

Second, we demonstrate that stakes and exam burden shape these patterns. Suicides increase most when national entrance exams coincide with secondary board exams and in high-participation states. This extends literature on determinants of youth suicide, including economic factors like unemployment and inequality (Breuer, 2015; Huikari and Korhonen, 2021; Lepori et al., 2024), psychosocial factors (Chen et al., 2012), and environmental factors (Persico and Marcotte, 2022; Woo et al., 2012), showing how high-stakes exams interact with other stressors affecting youth mental health.

Third, we discuss welfare costs, policy implications, and proximate stressors behind these suicides using scraped article text. High-stakes exams impose physiological and psychological burdens, such as gastroesophageal symptoms (Suriyayothin et al., 2023), elevated cortisol (Heissel et al., 2021; Medovnikova et al., 2018), and anxiety, depression linked to suicidal ideation (Melguizo-Ibáñez et al., 2023). These exams promote unproductive learning, increase stress for marginalized groups (French et al., 2023), deepen inequalities (Mangal, 2021, 2024), and have lasting negative educational and employment effects (Machin et al., 2020). In India, where prestigious programs represent socioeconomic mobility, understanding exam-related stress is crucial for policy intervention. We create an alternative database to address this key question when granular mental health data are unavailable.

The rest of the paper is structured as follows: Section 2 explains the context and economics of high-stakes exams in India. Section 3 discusses the data and the relevant variables. Section 4 sets up our estimation framework. Section 5 discusses the main results, robustness checks, subsample analyses, and falsification tests. Section 6 discusses potential channels. Section 7 discusses proximate stressors and policy relevance, and Section 8 concludes.

2. Context

India is the world's most populous country, with over 50% of the population younger than 25 and 65% below the age of 35 (United Nations Population Division, 2022). Out of the 1.2 billion youth aged 15-24 globally, one-third reside in India (United Nations Department of Economic and Social Affairs, 2020). Out of the 700,000 annual suicides worldwide (World Health Organization, 2022), India contributes approximately one-fifth, with 140,000 reported annually.[2] Student suicides constitute 7-9% of total suicides and are rising 4% annually, surpassing the national average. The states of Maharashtra, Tamil Nadu, and Madhya Pradesh account for one-third of these incidents.[3] These statistics make India an apt canvas for studying the cyclicality of student suicides in LMICs.

2.1. High-stakes exams

We focus on India's two largest national standardized examinations: National Eligibility cum Entrance Test for Undergraduate admissions (NEET-UG) and Joint Entrance Examination-Mains (JEE-Mains), taken post-high school for admission to medical, engineering, and some other natural sciences programs. These hyper-competitive exams hold significant societal value (Mangal, 2021, 2024) due to perceived economic returns, social capital, and socioeconomic mobility. India's industrialization fuelled a strong cultural emphasis on these degrees, and exam preparation culture persists despite economic diversification. Success opens gateways to lucrative careers, whereas failure causes significant psychological distress. Cut-throat competition, social isolation, and societal or familial expectations create anxiety and deteriorating mental health. Public discourse increasingly links rising student suicides to these exams (Parashar, 2024).

NEET-UG[4] is a single-level, annual, nationwide entrance exam mandatory for all undergraduate medical and nursing programs, and India's largest by number of applicants. 2,406,079 candidates competed for 56,000 government and 52,000 private college seats, yielding a success rate of less than 5% in 2024. The affordability and quality make government seats extremely competitive, as Government medical degrees cost INR 30,000-100,000 (USD 347-1157) annually, versus 10-20 times higher costs in private colleges.

JEE-Mains is the national competitive exam for undergraduate engineering, sciences, architecture, and planning degrees at several campuses of premier institutes like the National Institutes of Technology (NITs), Indian Institute of Technology (IITs), Indian Institute of Information Technology (IIITs), and government-funded technical institutes, through a centralized seat allocation process. Many other government institutes and private universities also use JEE-Mains ranks. It serves as an eligibility test for JEE-Advanced, which admits students to IITs, the country's most elite technical institutes (Ørberg, 2018), and the Indian Institute of Science. JEE-Mains directly influences institutions' quality and career trajectories. The JEE-Mains exam is conducted twice a year so that the candidates can improve their percentiles if they want. Candidates can sit for either one or both sessions (conducted in different months), with the best score being considered for the preparation of the Merit List or Ranking. In 2024, 1,170,048 candidates appeared in the two sessions of JEE-Mains. With 121 centrally funded institutes offering 56,548 seats total, the success rate was less than 4.8%.[5]

While our objective is to study extreme mental health outcomes of the youth, we do not consider any other national-level exams as high-stakes exams. The Secondary and Higher Secondary School Board examinations, including the one conducted by the Central Board of

[2] https://www.ncrb.gov.in/accidental-deaths-suicides-in-india-adsi.html, full citation in Appendix section D1.

[3] https://www.business-standard.com/india-news/student-suicides-in-india-soar-4-annually-exceeding-national-average-124082900397_1.html?.

[4] We use NEET(UG) and NEET alternatively throughout the paper.

[5] All the NEET and JEE statistics presented here are the authors' calculations from the extracted data using the published reports from the publicly available sources provided in detail in Appendix D1.

Secondary Education (CBSE) across the country, and the ones conducted by various state boards taken in grades 10 and 12, are not considered high-stakes exams for the following reasons. First, they are non-competitive and non-standardized, and award percentage marks. Second, they do not ensure clear post-high school career trajectories like the NEET or the JEE. Third, their importance for college admission has diminished over the years. Fourth, they span multiple months to conduct exams on more than 100 subjects, and the state boards follow separate exam schedules, making it difficult to isolate specific stress periods.[6] Thus, the NEET and JEE-Mains timing provides an ideal setting to explore the relationship between intense academic pressure from high-stakes exams and student suicide seasonality in previously uncharted contexts.

2.2. Economics of high-stakes exams

Aspirants invest substantially in high-stakes exam preparation. Leading private coaching institutes serving over 250,000 students across more than 200 centers in India charge INR 32,804-136,526 (USD 380-1580) annually. With an estimated annual income of INR 396,000 (USD 4584) for urban lower-middle-class consumers in 2024, the upper bound of these coaching fees exceeds one-third of that income.[7] This has spawned a massive shadow education industry, and students commonly migrate to cities with high concentrations of coaching institutes. Kota (a city in the state of Rajasthan), known as the 'Coaching Capital,' enrolled over 200,000 students in 2021, representing 20% of its approximately 1 million population, and its economy is significantly dependent on coaching-related businesses. Although institutions admit students based on JEE-Main or NEET ranks, board exams require a minimum percentage (75% for IIT admissions). Many enroll in 'dummy schools'[8] meeting board exam prerequisites without class attendance, spending substantial time in coaching centers instead. Common stressors among them include isolation, homesickness, academic pressure from continuous testing, and discrimination based on caste, economic status, religion, and performance.[9] Alarming suicide rates over the past decade earned Kota the 'Suicide Capital' tag.

3. Data

We construct our dataset from the Global Database of Events, Language, and Tone (GDELT), an open-source platform that monitors global news in real time. GDELT uses NLP and machine-learning tools to transform news text into structured records with event dates, locations, and source metadata. Through geocoding and post-processing, events are mapped to specific cities or the nearest landmarks. Each record includes the original article URL (SOURCEURL), enabling article-level verification and transparency. Although GDELT also assigns CAMEO event codes, this taxonomy is not designed to classify civilian suicides. Therefore, unlike prior economics applications that rely on built-in event categories (Siddique, 2022; Vasishth, 2022), we identify suicide incidents from headline text using token-based classification rules (Dell, 2024).

We construct a corpus of suicide-related news articles from the GDELT Events database by filtering the article URL field (SOURCEURL) for linguistic markers of death by self-harm from January 2017 to October 2025. The working assumption, consistent with common GDELT practice when full text is unavailable, is that Indian news outlets typically use descriptive URL slugs that mirror headlines, enabling topical identification from the URL string alone.

We identify candidate suicide coverage using an ex-ante URL dictionary that captures generic English/Hinglish suicide phrases, common Indian reporting terms including romanized Indic equivalents, and method-specific markers frequently used in Indian headlines (e.g., hanging, toxin ingestion, jumping, self-immolation, and drowning). To reduce false positives and focus on completed deaths by self-harm, we exclude URLs reflecting terrorism/warfare usage, entertainment or idiomatic uses, and prevention, non-fatal attempts, or rescue/survival framing. URL-based selection necessarily trades recall for precision. Non-descriptive slugs and paywalled links may be missed, so the resulting data should be interpreted as a systematically identified sample of media-reported suicides rather than a census. For youth/student linkage, we implement a transparent, rule-based high-recall screening indicator in Python that standardizes the exported schema, deterministically normalizes URLs, and applies case-insensitive regular expressions with word boundaries to flag articles containing student language, school grade/board markers, competitive exam or coaching ecosystem terms, or explicit youth/age tokens. This screen is intended to define a plausibly youth/student-relevant universe for downstream classification and robustness checks. Reproducibility is ensured by logging the exact query text, versioning the regex dictionaries, and exporting the deduplicated article-level corpus, so that the pipeline can be regenerated identically in future runs.[10] After applying these filters, we arrive at 4423 geocoded incidents of student suicides reported by the print media between January 1, 2017, and October 31, 2025. On average, this translates to approximately 42 incidents per month.

Next, we compare this data with official statistics on student suicides from the Accidental Deaths and Suicides in India (ADSI) reports published annually by the NCRB of the Government of India. To facilitate the comparison, we aggregate the data from GDELT to state boundaries for each year, as that is the lowest level of granularity in the official statistics on student suicides in India. This enables us to assess the degree of overlap and patterns of discrepancies between media-reported student suicides and official records. Appendix Figure A1 shows that the queried GDELT data has a strong positive association with ADSI totals across states and Union Territories (UTs) over 2017-2023[11]. In pooled totals, the Pearson correlation in levels is $r = 0.724$ (95% CI [0.486, 0.862]) and the Spearman rank correlation is $\rho = 0.793$ (95% CI [0.602, 0.899]), indicating substantial concordance in both levels and cross-state ranking. As expected, the media-based counts are uniformly lower than ADSI totals.

To reduce the influence of scale differences and heteroskedasticity arising from very high-count states, we also compare the two sources in log space using $\log 10(1 + \text{count})$. The association tightens substantially (Panel 1, Fig. A2). The Pearson correlation rises to $r = 0.838$ (95% CI [0.681, 0.922]), and the linear fit explains a large share of the cross-state variation ($R^2 \approx 0.70$). The second panel (Fig. A2) further compares state rankings implied by the two sources. The rank-rank plot lies close to the 45-degree line, reinforcing that the media-based series preserves much of the relative ordering across states/UTs even when absolute levels

[6] The Union Public Service Commission (UPSC) exam for Indian Administrative Services (IAS), mandatory for Class 1 government bureaucratic positions, requires a minimum undergraduate degree, resulting in older applicants. Many engineers and doctors take it post-graduation; thus, failure doesn't stall career growth. Unsuccessful candidates can reappear for three years. The multi-stage selection process (preliminary, mains, interviews) spanning months makes concentrated stress periods difficult to pinpoint.

[7] Additional references about India's private coaching industry are in Appendix D2.

[8] Dummy schools enroll students in higher secondary classes, where no regular classes are conducted, but students can use that affiliation to sit for the mandatory school-leaving board examination. This spares students the time spent at school, allowing them to attend private coaching classes to prepare for the JEE and NEET exams. https://www.thehindu.com/data/who-goes-to-kotas-coaching-classes-and-why-data/article67504252.ece#:~:text=Nearly%20half%20the%20students%20(46,to%20Kota%20(Table%201.

[9] https://www.thehindu.com/data/the-stressful-lives-of-students-in-kota-data/article67513277.ece.

[10] More details in the appendix Section B.

[11] ADSI data is available only up to 2023.

differ.

These validation exercises suggest that media-reported student suicides contain meaningful geographic signal and are not dominated by random noise, while also highlighting that measurement is plausibly non-classical, with coverage varying systematically across states and UTs.[12] This motivates our empirical strategy that relies on within-location comparisons over the calendar year.

We acknowledge the limitations of our data. Media coverage is prone to bias, selective reporting, and regional disparities in attention. Furthermore, media reporting can be affected by sensationalism and may not capture actual incidents. We address some of these concerns in the robustness and falsification sections, providing additional details in Appendix Section B.

We plot the evolution of media coverage of student suicides from 1 January 2017 to 31 October 2025 (Appendix Figure A4). The x-axis denotes the time, and the y-axis denotes the number of media-reported suicides. The dotted vertical lines represent the months in which a high-stakes exam (NEET-UG or JEE-Mains) was held. We find visual evidence of a cyclical pattern in media coverage of student suicides. The peaks tend to coincide with or closely follow high-stakes exam months. This is indicative of a potential relationship between high-stakes exams and student suicides, possibly driven by academic stress. As we have geo-coded data on student suicides and the date they occurred, using Census 2011 district boundaries, we map the incidents to the districts and create a district-month-year panel. Thus, we have a panel of 641 districts over 106 months, which results in a final dataset of 67,946 observations. The average number of student suicides reported in the media is 0.065 suicides per district-month with a standard deviation of 0.548. There are 256 districts in our data in which no student suicide occurred during our study period. Appendix Figure A4 suggests that high-stakes exams may be associated with a trigger for student suicides that we explore formally in the next few sections.

4. Estimation strategy

We estimate the following regression specification:

$$Y_{dsmt} = \beta_0 + \beta_1 HighStakeExam_{mt} + \gamma_{d\times t} + \delta_{s\times m} + X'_{dmt}\beta_3 + \epsilon_{dsmt} \quad (1)$$

The dependent variable is the number of student suicides that were reported in the media in the district d of state s in month m in the calendar year t. The key independent variable of interest $HighStakeExam_{mt}$ is a dummy variable that equals 1 if there was a high-stakes exam conducted in the month m during year t, and 0 otherwise. Table A1 presents the distribution of these exams across different months during our sample period. The table demonstrates that these exams are not confined to specific months across all years. Therefore, we exploit this variability in exam timing to estimate the association between high-stakes exams and mental health of students, potentially triggered by exam-related stress. The media-reported student-suicide incidents are used as a proxy for mental health. The coefficient β_1 captures the association between high-stakes exams and the number of student suicides. $\gamma_{d\times t}$, an indicator for district-year fixed effects, controls for all district-specific time-variant unobservables that vary over the years, potentially affecting suicide incidences or reporting of student suicides. Youth suicides could also be triggered by economic stress, such as economic shocks arising from crop failure.

Therefore, we also control state-specific seasonality by including state-month fixed effects. X'_{dmt} is a vector of district-calendar month level temperature and precipitation that could potentially affect the stress levels. This is our preferred specification. Since the exam indicator varies at the calendar month-year level and is common across districts, we cluster standard errors at the calendar month-year level. Although the timing of high-stakes exams is common across locations within a calendar month-year, retaining the district-month structure is useful for outcome measurement and conditioning. In particular, district-by-year fixed effects absorb annual changes specific to each district, including changes in local economic conditions, population composition, educational infrastructure, media presence, and the general intensity of suicide reporting. A more aggregated state-month specification with state-by-year fixed effects would absorb only annual changes common to all districts within a state. The district-level panel also allows us to incorporate district-level monthly weather conditions and examine heterogeneity in local socioeconomic and geographic characteristics. Identification comes from changes in national exam timing across the 106-calendar month-year periods in the sample.

Our identifying comparison is therefore temporal rather than cross-sectional. Conditional on district-by-year fixed effects and state-specific seasonality, we compare monthly media-reported student suicides within a district between months that contain a high-stakes exam and months that do not. The relevant counterfactual is therefore a month in which the exam is not scheduled, and not a world without any exam. The patterns we document are best interpreted as showing that student suicides are disproportionately concentrated in months when high-stakes exams take place and in the weeks leading up to those exams. Therefore, we estimate the association of exam timing and salience with the temporal distribution of suicides, conditional on competitive exams being the mechanism through which students are selected into higher-education institutes. We also address whether the association with suicides is driven by the cyclical timing of exams or their unique stakes when we disentangle these two in the next section.

5. Results

5.1. Main results

Table 1 reports the estimation results. Column 1 reports the association between periods of high-stakes exams and student suicides reported in the media in a year fixed effects model, and without controlling for any other covariates. The year-fixed effects model helps us to control the year-specific shocks, such as those of Covid-19 (when exam time changed) or any other unobserved year-specific covariates. The coefficient associated with high-stakes exam months on student suicides is positive and statistically significant (at the 10% level), suggesting that high-stakes exam months are associated with an increase in student suicides when accounting for variation across years. In column 2, we add district-fixed effects to account for time-invariant district-specific factors that might influence student suicides. We note that this does not change the effect size.

Column 3 reports the results after controlling for all district-specific unobservables that may also vary over the years. The estimated coefficient is again very similar. We include state time trends in column 4 to control for state-specific linear trends in student suicides. The effect size is almost the same (varying at four decimal places only). We note that the association between the high-stakes exam months and student suicide incidents persists even after controlling for differential trends across states. However, the coefficients increase slightly when we add month-fixed effects in column 5, and State-Month fixed effects in column 6. In column 7, we also add district-month level weather controls for temperature and precipitation.

These results provide strong evidence that high-stakes exams are associated with an increase in student suicides, even after accounting for a host of potential confounding factors. Furthermore, the magnitudes of the coefficients for the high-stakes exam variable remain stable across columns 1-7. Using the estimated coefficient from our preferred specification, we find that high-stakes exam months are associated with an increase of 0.011 in student suicides per district-month, relative to a

[12] Fig. A3 shows the positive relationship between the number of students registering for NEET and official student suicides. States that have had a high number of NEET registrations in 2021 and 2022 also tend to have a high number of student suicides, as per the NCRB data.

Table 1
Estimates of the association between exam months and student suicide incidences from OLS specification.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome: Student Suicides							
High-Stakes Exam	0.009*	0.009**	0.009**	0.009*	0.011**	0.011***	0.011***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)
Mean of dep. var.	0.065	0.065	0.065	0.065	0.065	0.065	0.065
Effect size (coef/mean)	0.136	0.136	0.136	0.136	0.172	0.172	0.175
Observations	67,946	67,946	67,946	67,946	67,946	67,946	67,946
No. of districts	641	641	641	641	641	641	641
Adj R-squared	0.0022	0.2136	0.2843	0.2843	0.2846	0.2872	0.2872
Year Fixed Effects	Yes	Yes	No	No	No	No	No
District Fixed Effects	No	Yes	No	No	No	No	No
District × Year Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	No	No	No	No	Yes	No	No
State × Month Fixed Effects	No	No	No	No	No	Yes	Yes
State time trends	No	No	No	Yes	Yes	No	No
Weather controls	No	No	No	No	No	No	Yes

The table reports the estimation results from the specification 1. The dependent variable is the count of student suicides reported in the media. High-Stakes Exam is a dummy variable that equals 1 if there was a high-stakes exam conducted in calendar month m during year t, and 0 otherwise. Column 7 reports the results of our preferred specification. Bootstrapped standard errors clustered at the calendar month-year level in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

mean of 0.065 suicides per district-month. This corresponds to a 17.5% increase in student suicides during these months.

5.2. Disentangling the associations with timing and stake

A natural question is whether observed spikes in student suicides reflect generic exam-season stress or the high stakes of particular exams. Therefore, we exploit two additional sources of variation in stakes versus timing: i) cumulative stress buildup through multiple exams in a calendar year; and ii) cross-state differences in exam participation intensity, captured by a pre-period measure of students' participation in these exams in each state.

First, Indian students face multiple assessments annually: high-stakes national entrance exams (NEET and JEE-Mains) that largely determine access to professional degree courses, and secondary school board exams that are important but typically less decisive for long-run opportunities. If our results were driven mainly by academic calendar cyclicality, such as end-of-year fatigue or seasonal factors, then we would expect similar patterns around both exam types. Conversely, if the intensity of the exam-related pressure matters, we should observe stronger associations around high-stakes exams and in months when multiple major exams are concentrated.

To investigate, we use board exams as a lower-stakes benchmark, distinguishing between months with: (i) no major exam, (ii) only board exams, (iii) only high-stakes national exams, and (iv) both exam types. Of the 106 month-year periods in our sample, 27 contain at least one national high-stakes exam. Nine of these 27 months (33.3 percent) coincide with one or more board exams, representing periods of exam congestion. Congested months, therefore, account for 8.5 percent of all month-year periods in the sample. We first compare these categories over the full year, then restrict the sample to exam months so that identification comes from variation in examination type rather than differences between examination and non-examination periods. Table 2 reports the results.

Columns 1 and 2 compare different exam environments relative to non-exam months. Column 1 shows that high-stakes examination months are associated with significantly more student suicides, while board-exam months are not statistically different from non-exam months. Column 2 decomposes the pooled high-stakes category into high-stakes-only, board-only, and congested months containing both exam types. Neither high-stakes-only nor board-only months differs significantly from the omitted non-exam category. In contrast, months in which national entrance exams coincide with board exams exhibit a substantially larger and precisely estimated association. Thus, the pooled estimate in Table 1 masks important heterogeneity and appears to be disproportionately concentrated in periods of cumulative examination burden.

Columns 3 and 4 restrict the sample to exam-containing months, using board-only months as the reference category to compare high-stakes and mixed months against lower-stakes exams within the exam season. In column 3, high-stakes exam months have 0.026 more student suicides per district-month than board-only months. Column 4 further decomposes exam months: relative to board-only months, student suicides are higher in high-stakes-only months and highest when both exams coincide. While within-exam contrasts are estimated imprecisely, particularly for high-stakes-only months, the coefficient pattern across all columns consistently reveals a stakes gradient. Board-only months show little difference from baseline, high-stakes months are associated with harmful outcomes, and months with coinciding high- and low-stakes exams exhibit the largest suicide increases.

Second, we proxy state-level exposure to national entrance examinations using a predetermined NEET-UG registration index. We average the number of state-level NEET-UG registrations over 2017–2019 and standardise this pre-period measure across states to have mean zero and standard deviation one. The index is therefore based on registration levels rather than a per-capita participation rate. Because larger states may mechanically have more registrations, we estimate an augmented specification that additionally controls for the state-level eligible youth population and its interaction with the examination-month indicator. The interaction between examination months and the registration index remains positive after including these controls. We nevertheless interpret the index cautiously as a proxy for pre-period examination exposure that may capture both participation intensity and population scale, rather than as a pure measure of the propensity to participate. The index Z_s is time-invariant within state-years and, therefore, absorbed in our fixed effects.

Specifically, we estimate:

$$Y_{dsmt} = \beta_0 + \beta_1 HighStakeExam_{mt} + \beta_2 HighStakeExam_{mt} \times Z_s + \gamma_{d\times t} + \delta_{s\times m} + \epsilon_{dsmt} \tag{2}$$

Columns 1 and 2 of Panel A in Table 3 interact the index with the exam-month indicator in the district-month panel, controlling for district-year and state-month fixed effects as well as weather. In column

Table 2
High-stakes vs lower-stakes exams: stakes versus timing.

	(1)	(2)	(3)	(4)
	High-stakes vs boards	By exam type	High-stakes vs boards (exam months only)	By exam type (exam months only)
High-stakes exam month	0.011**		0.026**	
	(0.005)		(0.010)	
Board exam month	0.009			
	(0.007)			
High-stakes *only* month		0.003		0.018
		(0.006)		(0.011)
Board exam *only* month		−0.001		
		(0.009)		
High-stakes and Board month		0.033***		0.041***
		(0.009)		(0.014)
Mean of dep. var.	0.065	0.065	0.071	0.071
Observations	67,946	67,946	27,554	27,554
Year Fixed Effects	No	No	No	No
District Fixed Effects	No	No	No	No
District × Year Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	No	No	No	No
State × Month Fixed Effects	Yes	Yes	Yes	Yes
State time trends	No	No	No	No
Weather controls	Yes	Yes	Yes	Yes

The dependent variable is the monthly count of media-reported student suicides at the district level. All specifications include district-by-year fixed effects, state-by-calendar-month fixed effects, and district-month temperature and precipitation controls. Columns 1 and 2 use the full district-month panel. Column 1 includes separate indicators for high-stakes examination months and board examination months. Column 2 uses mutually exclusive indicators for high-stakes-only months, board-examination-only months, and months in which both examination types coincide; months containing neither examination type form the omitted category. Columns 3 and 4 restrict the sample to months containing at least one major examination, with board-examination-only months as the omitted category. Column 3 compares all months containing a high-stakes examination with board-only months. Column 4 separately compares high-stakes-only months and months containing both high-stakes and board examinations with board-only months. The restricted-sample specifications therefore compare higher- and lower-stakes examinations within the examination season, while Column 4 also captures the additional association with examination congestion. Bootstrap standard errors clustered at the calendar month-year level are reported in parentheses. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).The state board exams coincide with the central board exams, leaving barely any variation across months. Hence, we are not considering State board exams separately in any of our analyses.

1, the coefficient for exam-month is small and insignificant, but the interaction with participation intensity is positive and precisely estimated. Moving from one standard deviation below to above the mean participation associates with roughly 0.024 additional student suicides per district-month in exam months. Column 2 also controls for state-level youth population and its interaction with exam months, leading to a slightly larger coefficient for the interaction term. Thus, at the district level, exam-month increases in student suicides are systematically steeper in states with higher pre-period NEET participation, independent of population scale. Columns 3 and 4 collapse data to the state-month level to reduce noise and weight each state-month-year cell equally. These specifications include state-year and month-year fixed effects, so the association with the exam-month is absorbed, and we capture the association with cross-state variation in participation intensity within the month-year. Interaction coefficients remain sizeable and significant.

Table 3
Heterogeneity in exam-months by participation index.

	(1)	(2)	(3)	(4)
Panel A *Outcome: Student Suicides*				
Exam month	0.004	0.002		
	(0.005)	(0.005)		
Exam month × Z_s	0.012***	0.016***	0.315**	0.401**
	(0.004)	(0.006)	(0.152)	(0.196)
Observations	67,946	67,946	3710	3710
Panel B *Outcome: Student Suicides*				
NEET	0.004	0.003		
	(0.005)	(0.006)		
NEET× Z_{neet_s}	0.016**	0.018**		
	(0.006)	(0.007)		
JEE-Mains	0.003	0.002		
	(0.005)	(0.005)		
JEE-Mains × Z_{jee_s}	0.006*	0.009*		
	(0.003)	(0.004)		
Observations	67,946	67,946		
Sample	District-month panel	District-month panel	State-month panel	State-month panel

In Panel A, Z_s is the standardised pre-period state-level NEET-UG registration index. It is constructed by averaging the number of NEET-UG registrations in each state over 2017–2019 and standardising the resulting measure across states to have mean zero and standard deviation one. The index is based on registration levels and should not be interpreted as a per-capita participation rate. Column 2 additionally controls for the state-level eligible youth population and its interaction with the examination-month indicator to account for differences in population scale. Columns 3 and 4 are regressions at the state-month level. Controlling for temperature, precipitation, population (eligible), and absorbing state× year and year×month fixed effects. In Panel B, the NEET-UG index is constructed as above, while the JEE-Mains index is based on available state-level registration data for 2020 and is similarly standardised. Column 1 includes controls for temperature and precipitation and absorb district×year and state-×month fixed effects. Column 2 includes controls for temperature, precipitation, population (state-level eligible) and absorb district×year and state×month fixed effects. Bootstrap standard errors clustered at the calendar month-year level in parentheses.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

As an additional check, we use state-wise JEE-Mains candidate counts available for 2020 from the National Testing Agency, to construct a similar index (see Annexure 1), finding stronger associations with higher participation when allowing exam-wise differences (Panel B of Table 3).[13]

These exercises provide only a partial answer to the exam-specific participation test. Panel A uses a multi-year NEET participation measure but interacts it with a pooled indicator covering both NEET-UG and JEE-Mains months. Panel B more directly matches JEE-Mains participation to JEE-Mains months, but this analysis is restricted to 2020 because comparable annual state-level JEE participation data are not publicly available. We therefore interpret the results as suggestive evidence that exam-month associations are stronger in states with greater exposure to the relevant examination, rather than as a fully symmetric multi-year comparison of NEET- and JEE-specific participation. Our exam-participation intensity measure is based on pre-period state-year NEET registrations, standardized across states. Although predetermined relative to suicides in our main sample, it is an aggregate proxy that may also capture broader differences in exam culture, aspirations, and coaching markets, and it masks within-state variation in exam pressure.

[13] Annual state-wise JEE-Mains information is not publicly available. However, 2020 data are available, which we use as a robustness check in Panel B of Table 3.

We therefore interpret the positive interaction between exam months and participation intensity as evidence that exam-related suicide increases are larger in historically high-participation states, rather than as a causal effect of marginal participation changes. Finally, while estimates are robust to flexible controls for state youth population, we cannot fully disentangle intensity from other correlated state characteristics.

5.3. Robustness exploiting disruptions in exam timing

As an additional check, we exploit COVID years when high-stakes exams were rescheduled to atypical months (Table A2). Restricting the sample to 2020–2022 and re-estimating our baseline specification, months when JEE-Mains/NEET are actually held remain associated with approximately 0.007 additional student suicides per district-month (about 10–12% of the sample mean). We then construct an indicator for "usual" exam months, namely January, April, and May of 2020–2022, which host national exams in normal years but were often not held during COVID. When both indicators enter a specification with (district$\times$year) fixed effects, actual exam months show positive, statistically significant associations with suicides, whereas traditional exam-season months without exams show negative deviations (Table A2). In the joint specification, the difference between the actual-examination-month and usual-examination-month coefficients is 0.0231, and equality is rejected using both a conventional Wald test ($p < 0.001$) and wild-cluster-bootstrap inference ($p = 0.0002$). These patterns suggest suicide spikes during COVID move with rescheduled exams rather than fixed calendar months. We emphasize that the COVID period involves many concurrent shocks (lockdowns, school closures, macroeconomic uncertainty), so we do not treat these shifts as a clean natural experiment. Instead, we view the COVID evidence as suggestive that our estimated timing is tied to when exams are actually held rather than to a pre-determined calendar exam season.

5.4. Other robustness checks

5.4.1. Alternate estimation methods

Our dependent variable is the count of media-reported student suicides in a district-month-year, whereas the estimates from our OLS regressions assume that the dependent variable is continuous and normally distributed, which is not the case for count data. Appendix Table A3 reports robustness checks to alternative estimators for the count-dependent variable. We first present OLS with high-dimensional fixed effects (HDFE) as a transparent benchmark in levels, with inference based on cluster-robust standard errors. Because linear models for counts can yield negative fitted values and may be sensitive to the heteroskedasticity typical of count outcomes, we additionally estimate Poisson Pseudo-Maximum Likelihood (PPML) models with the same fixed-effects structure. To facilitate comparability across estimators, we re-estimate the OLS specification on the PPML estimation sample.

Column 1 reports the baseline OLS (HDFE) estimate of the full sample. Exam months are associated with an increase of 0.011 suicides per district-month, which corresponds to a 17.5% increase relative to the full-sample mean (0.065). Column 2 reports PPML (HDFE) estimates. The exam-month coefficient is 0.326, implying a 38.5% increase in expected suicides during exam months. Evaluated at the PPML-sample mean (0.352), this corresponds to approximately 0.135 additional suicides per district-month. Column 3 reports OLS (HDFE) estimates on the PPML estimation sample; the exam-month coefficient is 0.0759, equivalent to a 21.5% increase relative to the restricted-sample mean (0.352). Overall, the association with exam-month remains positive and statistically significant across specifications, supporting robustness to modelling choices for count outcomes.

5.4.2. Potential threats to identification

One potential threat to our estimation strategy is that media attention to suicides might be higher during exam months. This could bias our results through differential reporting rates due to media agenda-setting, creating a misleading correlation between exam months and student suicides. We address this concern in two ways.

First, we test robustness using subsamples where media attention is expected to be lower by excluding big cities and urban hubs, or education epicenters.[14] These areas likely have more frequent, detailed suicide reporting, especially during exam season when media interest in education-related stories intensifies. Focusing on less prominent areas tests whether the relationship between high-stakes exam months and student suicides persists when media attention is less likely to drive the pattern. The relationship holds even for this subsample (Table A4), strengthening the case that our results reflect real changes in student suicides during high-stakes exam months rather than media reporting bias.

Second, to assess whether aggregate news attention swings could mechanically generate our results, we exploit months dominated by major national stories plausibly crowding out other reporting. We focus on months with severe tropical cyclone landfalls in India and parliamentary or state assembly elections. We augment the baseline specification with indicators for cyclone-landfall and election months, interacting each with the high-stakes exam-month indicator as below:

$$
\begin{aligned}
Y_{dsmt} = {} & \alpha_0 + \alpha_1 HighStakeExam_{mt} + \alpha_2 MajorEvents_{mt} \\
& + \alpha_3 (HighStakeExam_{mt} \times MajorEvents_{mt}) + \gamma_{d\times t} + \delta_{s\times m} + \varepsilon_{dsmt}
\end{aligned}
\tag{3}
$$

To capture the cyclone events, we use data from the Indian Meteorological Department (IMD), Ministry of Earth Sciences, Government of India, on cyclonic storms over the years and the months of their occurrence. Following IMD's (See IMD data in appendix section D1) classification of cyclonic storms, we classify a month as having a cyclone (taking the value 1) if a regular, severe, very severe, extremely severe cyclonic storm or a super cyclonic storm occurred in that month, and 0 otherwise. Information on election months for state assembly elections and national elections are accessed from the website of the Election Commission of India (see ECI data in Appendix section D1).

Across specifications (Table 4), the estimated exam-month coefficient remains unchanged (approximately 0.01 and statistically significant), while news-event indicators and their interactions with exam months are small and statistically indistinguishable from zero. These patterns suggest the exam-month spike in media-reported student suicides is not driven by simple displacement from contemporaneous national news shocks nor confined to "quiet" months when outlets have more space for exam-related coverage.

We emphasize that cyclones and elections are not used as sources of identification for media attention, but are used solely for robustness checks. Controlling for these high-salience events and allowing differential effects in exam months does not attenuate the association with exam months. However, these events show limited detectable effects on overall reporting, consistent with being, at best, weak shifters of aggregate coverage.

5.4.3. Jackknife robustness

Next, we assess whether the estimated association was driven disproportionately by a single recurring calendar month, particularly April or May, when several high-stakes exams are often concentrated. We implement a leave-one-month-out jackknife to test this. For each calendar month, we drop all observations from that month across all years (e.g., all April) and re-estimate the baseline specification on the remaining data. Within each jackknife sample, we estimate the model

[14] We use National Crime Records Bureau's classification of major cities for this purpose. The list is available at https://www.ncrb.gov.in/accidental-deaths-suicides-in-india-table-content?year=2022&category.

Table 4
The association between high-stakes exam and cyclones/elections as media attention diverters.

	(1)	(2)
	Student Suicides	Student Suicides
High-Stakes Exam	0.011**	0.010**
	(0.005)	(0.004)
High-Stakes Exam × Cyclone Month	−0.001	
	(0.010)	
Election Month		−0.013
		(0.008)
High-Stakes Exam × Election Month		0.014
		(0.014)
Observations	67,946	67,946
Year Fixed Effects	No	No
District Fixed Effects	No	No
District × Year Fixed Effects	Yes	Yes
Month Fixed Effects	No	No
State × Month Fixed Effects	Yes	Yes
State time trends	No	No
Weather controls	Yes	Yes

The dependent variable is count of student suicides reported in media. High-Stakes Exam is a dummy variable that equals 1 if there was a high-stakes exam conducted in month m during year t, and 0 otherwise. Cyclone Month takes value 1 if a regular, severe, very severe, extremely severe cyclonic storm or a super cyclonic storm occurred in that month-year, and 0 otherwise. Election Month takes value 1 is a national or state election happened that month-year. Bootstrapped standard errors clustered at the calendar month-year level in parentheses.
$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

using a bootstrap procedure with 499 replications clustered at the calendar month–year level, recording the resulting point estimate and bootstrap standard error for the exam-month coefficient. Fig. A5 reports these twelve jackknife point estimates (dots) and their 95% confidence intervals (lines).

Across jackknife samples, all exam-month coefficients remain positive, though magnitude and precision vary. Excluding months other than April yields estimated coefficients close to the full-sample estimate with largely overlapping confidence intervals. Dropping April, a main high-stakes exam month, produces a smaller coefficient and wider confidence interval, indicating April contributes meaningfully to identification. Even in this case, however, the point estimate remains positive and of comparable magnitude. This exercise suggests the overall exam-month pattern is not driven solely by a single calendar month, while acknowledging peak exam months such as April play a disproportionate role.

5.4.4. *Robustness of inference to two-way clustering*

Although the bootstrap standard error in our primary specification already addresses the small cluster count, we also check the robustness of our inference when standard errors are two-way clustered by state and calendar month-year. This specification permits arbitrary residual correlation across districts within the same calendar month-year, while allowing for correlation within states over time, including persistent state-level differences in media coverage and reporting. appendix Table A5 shows that the substantive conclusions remain unchanged when allowing for dependence along both dimensions.

5.5. *Heterogeneity*

5.5.1. *Heterogeneity across regional sub-samples*

India's substantial regional diversity may generate differences in social norms, family structure, youth expectations, social support systems, and other unobserved factors that contribute to geographic variation in baseline student suicide rates. If these factors moderate exam-related stress, we would expect differential associations between high-stakes exam months and student suicides across regions with varying baseline rates. We therefore explore heterogeneity by estimating our specification within regional subsamples stratified by historical suicide levels.

The first column of Table 5 presents the estimates from our preferred specification for the Hindi-belt states subsample. High-stakes exam months are associated with a positive and significant increase in student suicides, equivalent to 21.6% of the subsample mean. These states have been shown to exhibit lower baseline suicide numbers that could also stem from poor registration of suicides due to a lack of development and literacy. In peninsular states below the Hindi belt, the estimate is larger, corresponding to 31.6% of the mean (column 2). Previous empirical work has documented high numbers of suicides being recorded in these states, even though they might have better development outcomes compared to northern states (Arya et al., 2018; Snowdon, 2019). This is also substantiated by the subsample mean of 0.12 student suicides per district-month, a figure about triple that of the subsample of Hindi belt states. Excluding Union Territories leaves the baseline pattern unchanged (column 3). In North-Eastern states, the coefficient is positive but imprecisely estimated and not statistically different from zero. Restricting to states with historically high official student-suicide counts yields a large and significant estimate (0.034, $p < 0.01$), equal to 37.0% of the subsample mean. Overall, the association is strongest in areas with higher baseline suicide prevalence.

5.5.2. *Heterogeneity across gender*

For a subset of our dataset, which is about half of the articles reporting student suicides, we are able to identify the gender of the students. Therefore, we treat suicides by boys and girls as separate outcomes (Table 6), enabling a more nuanced analysis of the gender-

Table 5
Heterogeneity by regions.

	(1)	(2)	(3)	(4)	(5)
	Hindi Belt	Below Hindi Belt	Dropping Union territories	North East	High Student Suicide States
Outcome: Student Suicides					
High-Stakes Exam	0.010***	0.040**	0.012***	0.004	0.034***
	(0.003)	(0.016)	(0.004)	(0.004)	(0.008)
Mean of dep. var.	0.045	0.126	0.060	0.012	0.090
Effect size (coef/mean)	0.216	0.316	0.192	0.330	0.370
Observations	28,408	14,204	63,282	8692	23,108
Year Fixed Effects	No	No	No	No	No
District Fixed Effects	No	No	No	No	No
District × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	No	No	No	No	No
State × Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
State time trends	No	No	No	No	No
Weather controls	Yes	Yes	Yes	Yes	Yes

The table reports the estimation results from our preferred specification over different subsamples. Columns 1 – 4 look at various geographical subsamples. Column 5 looks at the subsample of states that have had historically high official student suicide numbers as per NCRB. The dependent variable is count of student suicides reported in media. High-Stakes Exam is a dummy variable that equals 1 if there was a high-stakes exam conducted in month m during year t, and 0 otherwise. Bootstrapped standard errors clustered at the calendar month-year level in parentheses.
$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6
Heterogeneity by gender.

Panel A	(1)	(2)	(3)
	Student suicides by girls	Student suicides by boys	Combined student suicides
High-Stakes Exam	0.004***	0.006**	0.011**
	(0.001)	(0.003)	(0.004)
Effect Size	0.200	0.136	0.169
Observations	67,946	67,946	67,946
Panel B			
Only NEET	0.008***	0.006	0.015**
	(0.003)	(0.004)	(0.006)
Effect Size	0.400	0.136	0.230
Observations	67,946	67,946	67,946
Panel C			
Only JEE-Mains	0.003	0.005*	0.008*
	(0.002)	(0.003)	(0.005)
Effect Size	0.150	0.129	0.123
Observations	67,946	67,946	67,946

The dependent variables are counts of student suicides reported in media by girls (in Column 1), by boys (in Column 2), and the combined numbers (in Column 3). Panel A reports the association with both the high-stakes exams, Panel B reports the association of only NEET exam month and Panel C reports the association of only JEE-Mains exam months. High-Stakes Exam is a dummy variable that equals 1 if there was a high-stakes exam conducted in month m during year t, and 0 otherwise. Only NEET is a dummy variable that equals 1 if NEET was conducted in month m during year t, and 0 otherwise. Only JEE-Mains is a dummy variable that equals 1 if JEE-Mains was conducted in month m during year t, and 0 otherwise. Bootstrapped standard errors clustered at the calendar month-year level in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

based heterogeneity. The dependent variables of media-reported student suicides are the counts of student suicides of girls (column 1), the count of boys (column 2), and the combined count (column 3). Panel A reports the association with high-stakes exams, Panel B reports for NEET only exam months, and Panel C reports the JEE-Mains (only) exam months. Since the gender-specific regressions include district-by-year fixed effects, the annual size and gender composition of the exam-eligible student population are absorbed and cannot be included separately. Identification instead comes from variation between exam and non-exam months within the same district-year.

High-stakes exams are associated with statistically significant increases in suicides for both girls and boys, with a slightly higher relative effect for girls. Disaggregating by exam type reveals NEET months have a pronounced association with girls' suicides, likely due to higher female participation, while the relationship for boys is not statistically significant. Conversely, JEE-Mains months show a stronger association with boys' suicides, consistent with higher male participation, while the association for girls is minimal and not statistically significant. Findings are similar for PPML models (Table A6).

NEET covers Physics, Chemistry, and Biology with higher weights on Biology, focusing on human anatomy, physiology, and life sciences. NEET is the gateway to medical and allied health sciences, including nursing, attracting many female applicants. JEE-Mains, the gateway to engineering and technical courses, covers Physics, Chemistry, and Mathematics (no Biology) and requires advanced problem-solving. Research shows that fewer girls pursue advanced math courses due to cultural and family influences; girls from boy-biased families (Dossi et al., 2021) or high-achievement countries with ingrained gender norms often show reduced math interest (Eriksson, 2020). Gender stereotypes lower confidence, as many girls believe boys are inherently better at math (Herts and Levine, 2020). Despite strong early performance, these factors discourage girls from advanced math courses, emphasizing the role of enjoyment and confidence in educational choices (Owusu-Amoah, 2015). This explains higher female registration for NEET and higher male registration for JEE-Mains. Fig. A6 shows male candidates consistently outnumber female candidates for JEE-Mains, while the opposite is true for NEET.

5.6. Falsification tests

We perform two falsification tests to lend further credibility that the observed relationship is not driven by spurious correlations or unrelated time trends.

First, to assess whether the exam-month pattern we document is specific to student suicides, we replicate our baseline specification using non-student suicides as placebo outcomes. Panel A of Table 7 uses farmer suicides (see Hebous and Klonner, 2014, for details on why farmer suicide may be considered as a major reason for all suicides in India), while Panel B uses all adult suicides (including non-farmers). Across all columns, the exam-month coefficients for farmer and adult suicides are small and statistically indistinguishable from zero. For farmer suicides (Panel A), the estimates are around −0.001. For all adult suicides (Panel B), the coefficients range from −0.008 to −0.001 and are again never statistically different from zero. These null patterns stand in sharp contrast to the positive and precisely estimated effects we observe for student suicides in the same specifications.

Second, we also perform a randomization-inference test based on random permutations of exam months. In each of 1000 iterations, we assign the exam-month indicator to a randomly selected set of calendar months, holding fixed the number of exam months in each year and the underlying district-month-year structure, and re-estimate our preferred. Fig. 1 plots the empirical distribution of exam-month coefficients generated by these permutations. The distribution is centered close to zero, while the actual exam-month estimate from the data (0.011) lies in the extreme right tail. Only 6 out of 1000 placebo coefficients have an absolute value as large as the observed estimate, implying a two-sided randomization-inference p-value of 0.007 (one-sided p-value of 0.003).

5.7. Robustness through oster bounds

As omitted variable bias is a potential source of endogeneity in the relationship between high-stakes exams and student suicides, we estimate Oster bounds to assess robustness. Building on Altonji et al. (2005), which assumes selection on observables can inform selection on unobservables, Oster bounds provide a systematic approach to assess robustness to omitted variable bias. Under equal selection on observables and unobservables, the estimated treatment effect is bounded between 0.011 and 0.013, excluding zero, indicating the observed relationship is robust to omitted variable bias (Table A7). Results suggest the uncontrolled model underestimates the effect. We also calculate the degree of selection on unobservables required to reduce the observed effect to zero, revealing it would need to be over 8 times stronger than selection on observables to fully explain away the association (column 3). Notably, Oster (2019) finds that in 86% of cases, selection on unobservables is less than or equal to that on observables. These findings provide robust evidence of a positive association between high-stakes exams and student suicides, even with potential omitted variable bias.

6. Potential channels

6.1. Urban concentration and social pressures

The existing literature establishes education as a positional good that affects social prestige (Gallice and Grillo, 2019; Nikolaev, 2016). Peer pressure influences academic effort (Bursztyn and Jensen, 2015), and peer comparisons can amplify psychological distress in high-stakes circumstances. Economic incentives stemming from career concerns fuel academic pressure, consistent with signalling and human capital models that posit that higher schooling yields higher economic returns (Becker, 1962, 1975; Riley, 2001; Spence, 1973). The convergence of economic, social, and psychological stressors makes exam periods particularly

Table 7
Falsification tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Farmer Suicides							
High-Stakes Exam	−0.001	−0.001	−0.001	−0.001	0.001	0.001	0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Mean of dep. var.	0.014	0.014	0.014	0.014	0.014	0.014	0.014
Effect size (coef/mean)	−0.050	−0.050	−0.050	−0.050	0.037	0.037	0.024
Observations	67,946	67,946	67,946	67,946	67,946	67,946	67,946
Panel B: Adult Suicides							
High-Stakes Exam	−0.008	−0.008	−0.008	−0.008	−0.003	−0.003	−0.001
	(0.015)	(0.011)	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)
Mean of dep. var.	0.231	0.231	0.231	0.231	0.231	0.231	0.231
Effect size (coef/mean)	−0.036	−0.036	−0.036	−0.036	−0.011	−0.011	−0.004
Observations	67,946	67,946	67,946	67,946	67,946	67,946	67,946
Year Fixed Effects	Yes	Yes	No	No	No	No	No
District FE	No	Yes	No	No	No	No	No
District × Year FE	No	No	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	No	No	Yes	No	No
State × Month FE	No	No	No	No	No	Yes	Yes
State time trends	No	No	No	Yes	Yes	No	No
Weather controls	No	No	No	No	No	No	Yes

Each column reports coefficients from regressions of suicides per district–month on an indicator for high-stakes exam months. Panel A uses farmer suicides as the dependent variable; Panel B uses all adult suicides (non-students). Columns (1) - (7) sequentially add fixed effects and controls following the main specification. The "Effect size" row reports the coefficient divided by the sample mean of the dependent variable.
Bootstrapped standard errors clustered at the calendar month-year level in parentheses.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

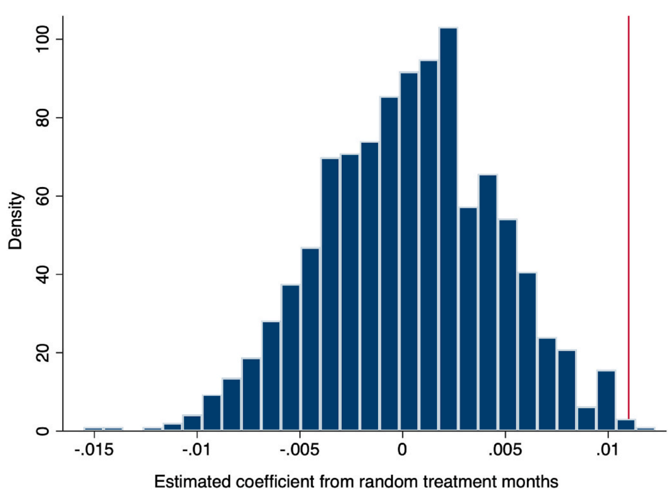

Fig. 1. Randomization-inference distribution of exam-month effects. The histogram shows the distribution of estimated exam-month coefficients obtained from 1000 placebo regressions in which exam months are randomly reassigned to calendar months, holding fixed the number of exam months per year. Each regression uses the baseline specification with district×year and state×month fixed effects and weather controls for temperature and precipitation. The vertical red line marks the actual exam-month coefficient estimated from the true exam schedule ($\beta = 0.011$). Only 6 of 1000 placebo coefficients are at least as large in absolute value as the observed estimate, implying a two-sided randomization-inference p-value of 0.007. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

taxing for mental health.

Pressures surrounding high-stakes exams are likely heightened in areas with more aspirants. Exam centers and coaching industries concentrate in cities, and students migrate specifically to prepare for these exams (see Appendix Figure A7 for Kota, a small city in the state of Rajasthan). Cities also offer greater career opportunities tied to exam qualifications. We therefore examine whether the exam-month association concentrates in more urban and connected districts.

We proxy baseline urban connectivity and economic intensity using district-level measures from AidData's GeoQuery (GeoAid) raster products, aggregated as district (ADM2) zonal means: mean travel time to the nearest city in 2015 (minutes), mean distance to the nearest road (meters), and gridded GDP (higher values indicate greater economic intensity). For each proxy, we define an "urban" indicator using the sample median (above-median economic intensity; below-median travel time and road distance) and estimate our preferred specification allowing exam-month coefficients to differ across groups via interactions.

Table 8 (Panel A) shows that the association with the exam month is larger in more urban-connected districts. The coefficient on *Exam month* captures the association with the reference group (urban indicator = 0) and is small for economic intensity and city proximity, but positive and marginally significant for road access. The interaction term (*Exam month × Urban)*, measuring the differential between urban and non-urban

Table 8
Mechanism by urban connectivity.

	(1) Economic intensity	(2) Travel time to city	(3) Distance to roads
Panel A			
High-Stakes Exam	0.0020	0.0038	0.0068*
	(0.0032)	(0.0031)	(0.0039)
Exam month x Urban indicator (Diff.)	0.0179***	0.0145*	0.0084
	(0.0060)	(0.0082)	(0.0079)
Observations	66,886	66,886	66,886
Panel B			
Board Exam	−0.0022	0.0071	0.0059
	(0.0048)	(0.0054)	(0.0048)
Board Exam x Urban indicator (Diff.)	0.0164	−0.0022	0.0003
	(0.0101)	(0.0110)	(0.0078)
Observations	66,886	66,886	66,886

The table reports results from subsample analyses. We have complete information on travel time, distance to roads and night-time lights for 631 districts. The data is extracted from AidData (Goodman et al., 2019). In panel A, High-Stakes Exam is a dummy variable that equals 1 if there was a high-stakes exam conducted in month m during year t, and 0 otherwise. In panel B, Board Exam is a dummy variable that equals 1 if there was a CBSE board exam conducted in month m during year t, and 0 otherwise. Bootstrapped standard errors clustered at the calendar month-year level in parentheses.*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

groups, is sizeable and statistically significant for economic intensity, marginally significant for city proximity, but smaller and imprecisely estimated for road access.

A potential concern is that Panel A's urban concentration reflects differential media reporting rather than underlying suicide differences, as incidents in more urban-connected districts may be more likely to be reported. To assess this, we use board exam months as a placebo in Panel B. Board exams are school-leaving exams taken across urban and rural areas, administered at a common time with widely distributed exam centers nationwide, unlike high-stakes entrance exams, which concentrate in cities.

Panel B provides little support for this explanation. Across all three proxies, which are economic intensity, city proximity, and road access, we find no statistically significant association between board exam months and youth suicides, with small and imprecisely estimated interaction terms. The absence of systematic urban concentration around board exams suggests reporting bias is unlikely to be the main driver of stronger high-stakes exam associations in more connected and economically intense districts in Panel A. Instead, the evidence is consistent with high-stakes entrance exams generating distinct pressures that are disproportionately salient in urban settings, potentially due to the spatial concentration of exam-related activity and higher perceived returns to exam success. As board exams and high-stakes exams differ along other dimensions (stakes, preparation markets, migration to exam hubs), this exercise should be interpreted as suggestive evidence against pure reporting-bias explanations rather than a definitive test.

Tables 3 and 8 provide complementary evidence consistent with a common interpretation. The examination-month association is stronger both in states with greater pre-period examination participation and in districts with greater economic intensity and urban connectivity. These patterns suggest that examination-related pressure may be amplified where more students compete and where the educational and employment rewards attached to examination success are especially salient. Because both sets of measures may capture other correlated characteristics, we interpret this evidence as suggestive of intensified economic incentives, peer comparisons, and social pressure rather than as separately identifying these mechanisms.

6.2. *Dynamics and timing of exam-related stress*

Finally, to examine dynamics more finely, we estimate stacked event studies centered on each high-stakes exam using a ±K-month window and absorbing exam fixed effects in addition to baseline fixed effects. The coefficients trace a clear pattern. Media-reported student suicides start rising several months before the exam, increase sharply in the month immediately preceding it, remain elevated during the exam month, and gradually return toward baseline within a few months afterward. In Fig. 2, we find no evidence of pre-trends beyond a few months before the exam, and the profile is robust to alternative window lengths and fixed-effects structure.

To probe potential channels, we examine whether exam-related suicides are driven by announcements or results rather than the exam period itself. We estimate analogous event studies centered on announcement months (Fig. A8) and results months (Fig. A9). In contrast to exam-centered profiles, which display marked increases in suicides in the month before and during high-stakes exams, announcement-centered event studies are flat, with coefficients close to zero and statistically indistinguishable from the pre-period.

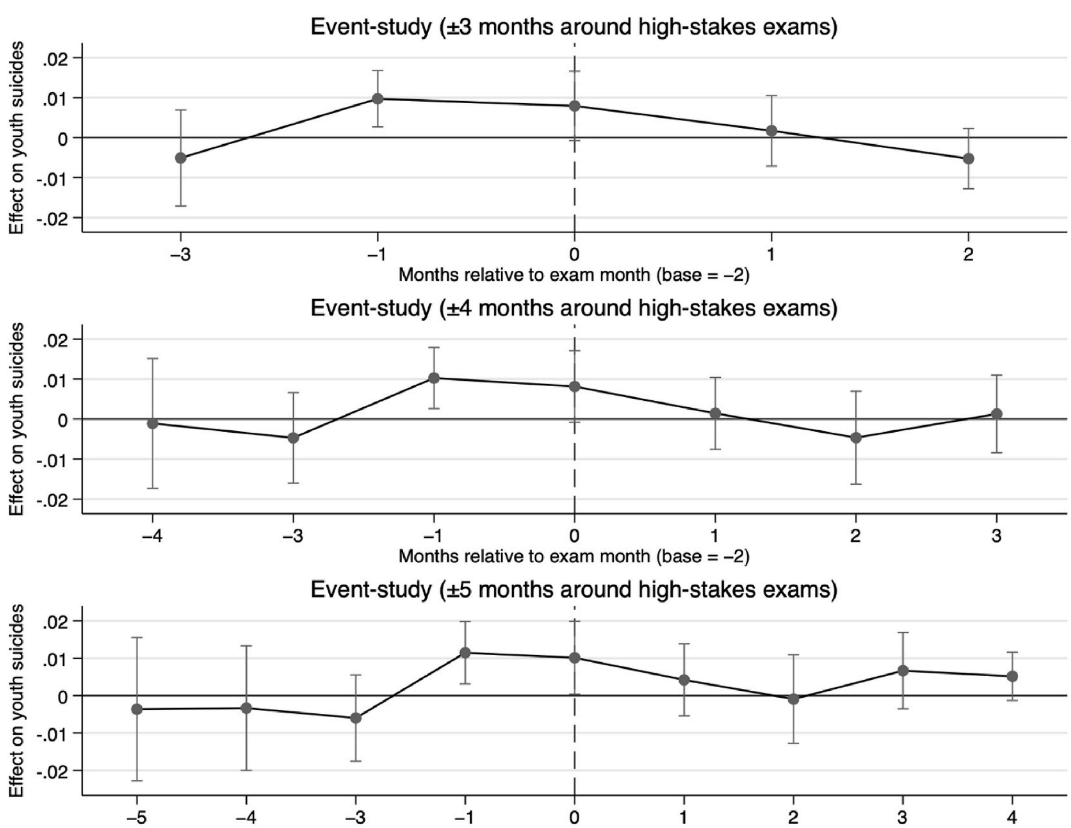

Fig. 2. Stacked event studies centered on each high-stakes exam around a ±K month The figure reports stacked event-study estimates centered on each high-stakes examination (NEET-UG or JEE-Mains). Event time 0 denotes the examination month, and month −2 is the omitted reference period. The three panels use ±3-, ±4-, and ±5-month windows, respectively. The dependent variable is the monthly count of media-reported student suicides at the district level. All specifications include district-by-year fixed effects, state-by-calendar-month fixed effects, district-month temperature and precipitation controls, and exam-event fixed effects. Points report coefficient estimates relative to month −2; vertical bars report 95% confidence intervals.

We re-estimate baseline specifications, replacing the exam-month indicator with a dummy for results-release months. Results-month coefficients are positive and statistically significant across specifications (Table A8) but are consistently smaller in magnitude than the corresponding exam-month associations. The implied increase in student suicides during results months is approximately 13–16% of the mean, compared with 14–18% for exam months. This pattern is consistent with institutional timing: results are often announced in the same month as the exam or immediately after, and answer keys are typically released soon after, so students can infer performance before official results are released. In line with event-study evidence centered on results months, which shows no sharp pre-result build-up, these OLS estimates suggest that while result disclosure is associated with elevated suicides, larger and more concentrated increases are tied to the exam period itself and anticipatory stress leading up to it.

These findings indicate that the main timing effect operates through pre-exam and exam-period stress rather than sharp shocks at announcements, as the pre-release of answer keys dilutes the shock. Our monthly data cannot rule out very short-run effects at the daily level around specific announcement or result dates. However, at a monthly frequency, we detect no systematic shifts in suicides around these events.

7. Policy relevance

7.1. Illustrative scale of the mortality burden

7.1.1. Implied mortality burden

We first express the magnitude of the estimated association in terms of its implied mortality burden. Our regression estimates the temporal association between high-stakes exam months and media-reported student suicides, whereas the exercise below extrapolates this proportional association to official national student-suicide totals.

Our preferred specification indicates that media-reported student suicides are approximately 17.5 percent higher during months in which NEET-UG or JEE-Mains exams are held. Since this association applies only during high-stakes exam months, we account for the number of such months in each year rather than applying the estimated association to the full annual suicide total. Specifically, for each year, we calculate $E_y = S_y \frac{M_y}{12} r$. Where E_y is the implied mortality burden in year y, S_y is the official number of student suicides recorded by the National Crime Records Bureau (NCRB) in that year, M_y is the number of months containing at least one NEET-UG or JEE-Mains examination, and ($r = 0.175$) is the estimated examination-month association relative to the mean of the dependent variable. A month is counted only once when both NEET-UG and JEE-Mains are held during the same month.

Official NCRB student-suicide totals are currently available through 2024. We therefore restrict this exercise to 2017–2024, although our media-based estimation sample extends into 2025. Across these eight years, the NCRB recorded 97,458 student suicides, and 24 of the 96 calendar months contained at least one NEET-UG or JEE-Mains examination. Applying the year-specific calculation gives a cumulative illustrative estimate of approximately 4374 deaths associated with the concentration of student suicides during high-stakes examination months. This is equivalent to approximately 4.5 percent of all officially recorded student suicides over 2017–2024, or an average of approximately 547 implied deaths per year. In broad terms, the estimates therefore imply an order of magnitude of approximately 4400 deaths over the eight-year period. For illustration, the NCRB recorded 10,355 student suicides in 2019, when three months contained at least one high-stakes examination. Applying the estimated 17.5 percent examination-month association gives an illustrative estimate of approximately 453 deaths associated with the temporal concentration of student suicides during these months. This corresponds to approximately 4.4 percent of all officially recorded student suicides in 2019. The implied number varies across years because both the official number of student suicides and the number of high-stakes examination months vary (see Table 9).

These calculations rest on several assumptions and should be interpreted as order-of-magnitude illustrations rather than estimates of preventable deaths. First, they assume that the proportional association estimated using media-reported incidents can be extrapolated to official national student-suicide totals. Second, they apply the same proportional association across years despite changes in examination schedules and other institutional conditions. Third, they do not identify what would have occurred under a different examination calendar or tertiary-admission system. Finally, because monthly media data cannot fully distinguish changes in underlying suicide incidence from examination-specific changes in reporting, the calculation may also reflect some temporal concentration in media coverage. The estimates should therefore be understood as illustrating the potential scale of the mortality burden represented by the observed association, rather than as a causal accounting of lives that would necessarily have been saved in the absence of high-stakes examinations.

7.1.2. Monetary benchmarks

As a secondary exercise, we translate the implied mortality estimates into monetary benchmarks. These calculations are not valuations of the individuals who died, nor are they our principal measure of the burden. They are intended only to provide transparent, order-of-magnitude comparisons under alternative assumptions.

We first construct a resource-cost benchmark using existing estimates of the economic burden of suicide in India (Nigam et al., 2019; Poduri, 2016; Sarma, 2018; Doran and Kinchin, 2020). Combining Nigam et al.'s (2019) aggregate estimate with official suicide counts implies an average accounting burden of approximately USD 119,932 per suicide. Applying this benchmark to the 2019 illustrative estimate of 453 deaths gives an implied resource cost of approximately USD 54 million. Applying the same benchmark to the cumulative 2017–2024 estimate gives approximately USD 525 million.

Following León and Miguel (2017), we also present a

Table 9
Illustrative mortality and welfare benchmarks.

Panel A: Implied mortality burden		
Measure	2019	2017–2024
Officially recorded student suicides	10,355	97,458
High-stakes examination months	3	24
Implied deaths associated with examination-month concentration	453	4374
Implied deaths as a share of official student suicides	4.4%	4.5%
Panel B: Monetary Benchmarks		
Valuation Assumption	**2019**	**2017–2024**
Resource-cost benchmark: USD 119,932 per death	USD 54 million	USD 525 million
VSL benchmark: USD 0.5 million per statistical life	USD 0.23 billion	USD 2.19 billion
VSL benchmark: USD 1 million per statistical life	USD 0.45 billion	USD 4.37 billion
VSL sensitivity range	USD 0.23–0.45 billion	USD 2.19–4.37 billion

Notes: Panel A reports illustrative mortality estimates obtained by applying the estimated examination-month association to annual NCRB student-suicide totals while accounting for the number of months containing at least one NEET-UG or JEE-Mains examination. The calculation extrapolates an association estimated using media-reported student suicides to official national totals and should not be interpreted as a causal estimate of preventable deaths. Official NCRB student-suicide totals are currently available through 2024; cumulative estimates therefore cover 2017–2024. Panel B reports secondary monetised sensitivity exercises. The VSL benchmarks are external to this setting and are not specific to Indian adolescents; the resulting range is illustrative and should not be interpreted as a point estimate or as a valuation of individual lives.

value-of-a-statistical-life sensitivity exercise. Available estimates of the Value of a Statistical Life (VSL) are external to our setting and are not specific to Indian adolescents. We therefore do not treat them as context-appropriate point estimates. Instead, we apply benchmark values of USD 0.5 million and USD 1 million per statistical life to illustrate the sensitivity of the implied welfare magnitude. For 2019, these assumptions produce estimates ranging from approximately USD 0.23 billion to USD 0.45 billion. Applied to the cumulative 2017–2024 mortality estimate, the corresponding range is approximately USD 2.19 billion to USD 4.37 billion. Table 9 provides these ranges.

7.2. Proximate stressors

We exploit the richness of media reports by scraping full article text for each media-reported student suicide in our dataset. We successfully retrieved article text for about 67 percent of articles. Articles for which text could not be retrieved were typically blocked by paywalls, had links removed, or were dynamic webpages that prevented scraping. We then construct a multi-label coding scheme for stressors mentioned in the reports (e.g., exam failure, academic pressure, family conflict, financial distress, relationship issues, bullying/ragging, mental-health mentions, discrimination). Our coding pipeline is two-stage. First, we apply high-precision regex dictionaries to identify explicit mentions of each stressor and record short evidence snippets around matched phrases. We manually reviewed a random subset to refine dictionaries and reduce obvious false positives. Second, to reduce the number of uncoded cases, we train a supervised multi-label text classifier using the dictionary-coded articles as training data and apply it only to articles with no dictionary match. This approach yields transparent, auditable cause flags while substantially reducing cases where no stressor can be inferred from the text. These measures should be interpreted as media narrative indicators rather than causal determinants of suicide, since they capture what is mentioned in reporting rather than verified causes of death.

Media narratives overwhelmingly rely on police statements (86%), with fewer reports attributing contextual details to family members (12%) or suicide notes (10%), highlighting that the richness of reported stressors partly reflects journalistic sourcing rather than direct observation (Figure C1, Appendix C).

Figure C2 reports the prevalence of stressor categories mentioned in the scraped article text. Categories are non-mutually exclusive, so an incident may contribute to multiple bars. Academic stress and exam-related cues are mentioned in 41% of incidents, while harassment/discrimination cues appear in 39%. Cues related to family conflicts and relationship issues are mentioned in 17% and 11% of articles, respectively. Peer pressure and ragging cues are included in 2% and 3% of articles, respectively. About 14% of articles have cues for underlying mental health issues being faced by the suicide victim. Overall, the figure summarizes media-described stressors rather than definitive causal attribution.

Figure C3 reports the distribution of the number of distinct stressor categories mentioned in each scraped article. The distribution peaks at two categories (29.5%), followed by one (23.2%) and three (22.3%), indicating that most reports describe suicides as multi-factor events rather than attributing them to a single reason. Higher counts (four or more) are progressively less common, with a thin right tail. Only 0.07% of articles contain no detectable stressor category.

8. Conclusion

While cyclicality in student suicides has been observed in developed countries, evidence for developing countries is lacking due to data limitations. Youth suicide is a major public health concern, with LMICs accounting for most cases worldwide. We address this gap by studying the association between high-stakes exams and the seasonality of student suicides in India, while investigating the association with academic stress. Using novel, geocoded, high-frequency media-reported data on student suicides, we examine the association between hyper-competitive post-high-school examination periods and youth mental health. We find congruence between media-reported and official aggregated patterns. High-stakes exam months are associated with a 17.5% increase in student suicides over the sample mean. Results are robust to alternative estimation techniques and show geographical and gender heterogeneity. We address concerns about differential reporting rates during exam months due to media agenda-setting through falsification tests and subsample analyses.

A limitation of our analyses deserves emphasis. Although we compare exam months with result months, estimate announcement and result-centered event studies, and examine broad media-attention shifts using cyclone and election months, these exercises cannot fully distinguish between genuine changes in suicide incidence and exam-specific changes in media reporting. In particular, if news outlets systematically devote greater attention to student suicides during exam periods than during result periods, monthly media data alone cannot fully adjudicate this alternative explanation. Our findings should therefore be interpreted as evidence of robust temporal associations in media-reported student suicides around high-stakes exam periods rather than definitive evidence on the underlying incidence of suicides.

Our findings underscore the need to address the mental health implications of high-stakes exams on Indian youth. The cyclical pattern calls for immediate interventions in education policy, mental health support, and cultural reform. A cultural shift is needed to broaden acceptance of diverse career paths beyond conventional fields, reducing pressure on students. Our evidence also suggests that the mental-health burden is not uniform across exam periods. The strongest associations occur during months when national entrance exams coincide with board examinations, indicating that periods of cumulative assessment pressure are particularly salient. Policies that improve coordination across exam calendars, reduce the concentration of major assessments, and strengthen mental-health support during overlapping exam periods may consequently warrant particular attention. Additionally, reducing the stakes associated with government exams while promoting socio-emotional learning programs will equip adolescents with stronger coping mechanisms. This paper contributes to the debate surrounding the government's "one nation, one exam" policy, which could streamline admissions, but careful consideration of potential psychological consequences is warranted.

CRediT authorship contribution statement

Subarna Banerjee: Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Gitanjali Sen:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Ethical approval

This research did not involve human subjects, animals, or identifiable data requiring ethical committee review.

Originality and prior publication

By submitting this manuscript, the authors confirm that.

- The work described has not been published previously, except in the form of a preprint, abstract, or academic thesis.
- The manuscript is not under consideration for publication elsewhere.
- All co-authors have approved the manuscript and consented to this submission.

- If accepted, the paper will not be published elsewhere in the same form, in English or any other language, without written consent of the copyright-holder.

AI-assisted technology disclosure

No AI-assisted technologies were used in the preparation of this manuscript. The tools used for data extraction are given in appendix section B.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2026.103931.

Data availability

Please see supplementary files in appendix section D for sources to all data as all are publicly available. See appendix section B for detailed instructions on how to construct our working sample

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