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Distributional effects of education on mental health

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ABSTRACT

We leverage the exogenous variation in education induced by the implementation of a national compulsory schooling law (CSL) in China in 1986 to study the mean and heterogeneous effects of education on mental health. Regression discontinuity (RD) estimates suggest that on average CSL beneficiaries had better mental health and lower probability of being severely depressed. We combine the RD design with novel distributional analysis methods to demonstrate that this average effect is largely driven by improvements in the top half of the mental health distribution (higher scores indicating worse mental health). These findings not only add to the scant evidence on the effect of education on mental health in low- and middle- income contexts, but also suggest that looking beyond average effects might better inform how policies can be targeted to enhance their benefits. In terms of potential mechanisms, we find that CSL beneficiaries experienced better physical health, labor market outcomes and marital outcomes.

Mental health disorders contribute significantly to the global disease burden. Depression is one of the foremost mental health issues, and is the third leading cause of disability across the world (James et al., 2018), with an estimated 264 million people suffering from depression in 2017. Given the critical nature of this health challenge, it is important to find effective ways to mitigate mental health problems. Several studies have explored the possibility of education in promoting better health outcomes, especially mental well being (Jiang et al., 2020; Dursun and Cesur, 2016; Kondirolli and Sunder, 2022). However there is some evidence that education could negatively affect mental health (Avendano et al., 2020; Courtin et al., 2019). This suggests that the effect of education on mental health might go in either direction and the potential for the presence of significant heterogeneities in the effect of education on mental health across different individuals, which merits further exploration.

Past research has demonstrated that the effects of many policy interventions on mental health exhibit substantial variations across the mental health distribution, with more pronounced effects typically among those with worse mental health. For instance, Stillman et al. (2009) found stronger improvements in mental health from New Zealand's migration lottery for those with worse mental health in home country. Banerjee et al. (2015) observed positive effects on mental health in the lower and middle mental health spectrum in a multi-country asset-promotion intervention, while no such effects were found for individuals with good to best mental health. Ohrnberger et al. (2020) identified a fourfold larger positive impact of a cash transfer program in Malawi on individuals with the worst mental health compared to the average effect. These findings motivate our exploration of the presence of such heterogeneities in the effect of education on mental health. Understanding the treatment effect heterogeneities is important as it provides a deeper understanding of individual and societal benefits from a schooling intervention (as highlighted in Buhl-Wiggers et al., 2022).

We explore the education gradient of mental health and the associated heterogeneities in the context of China. We leverage the implementation of a large-scale national compulsory schooling law (CSL) in 1986 which mandated a minimum of nine years of schooling for all school-age children. We use a regression discontinuity design where we utilize differences in CSL exposure across individuals from different cohorts — those who were in (or about to start) the ninth schooling year at the time of policy implementation would have had to stay back in school for at least nine years, while those who were just older would not have been subject to this policy. Our identification rests on the assumption that cohorts born a few years apart on either side of the cut-off are conditionally similar to each other on most respects apart from their CSL status. Under this assumption, any mental health differences observed between these two groups can be attributed to the CSL policy.

Our analysis uses data from the China Family Panel Studies (CFPS), a nationally representative sample of mainland China, which contains rich information on birthplace, education and health outcomes at the individual level. The main mental health outcome we use is the Center for the Epidemiological Studies of Depression scale (CESD) (Radloff, 1977), which is an internationally validated measure of depressive

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symptoms and ranges from zero to 60 with higher numbers denoting worse mental health. Using the CESD-20 measure we create a categorical variable that measures the presence of potentially severe clinical depression – *Depressed20*, that takes a value of one if CESD-20 takes a value of 20 or more, and zero otherwise. We also use other measures of mental health to examine the robustness of our findings.[1]

Our results suggest that the CSL reform led to increased years of education among beneficiary cohorts, thus demonstrating that the reform was effective in shaping educational outcomes. This increase in education translated into improved mental health almost 25 years later. The findings suggest that CSL beneficiaries on average had a CESD20 score that was 1.07 points (8 percent of the control mean) lower than non-beneficiaries, while they were 6 percentage points (28 percent of the control mean) less likely to be diagnosed with severe depression. The distributional effect results suggest that these gains in mental health are largely concentrated in the top half of the CESD20 score distribution (worse mental health). In particular, we find that the effect size on the 90th percentile of the CESD20 score distribution is approximately double the average treatment effect.

Interestingly, the concentration of the impacts in the higher percentiles of the CESD20 score distribution is consistent with the pattern of mean treatment effects that we obtain. We observe that the mean treatment effect of the reform on being depressed is relatively large (28 percent of the control mean) as compared to the effect on the CESD20 score itself (8 percent of the control mean). This is possible because the gains from education are largely concentrated at the top part of the CESD20 distribution. This implies that the increased education leads the CESD measure of individuals to be below the threshold value of 20 — which is used to classify whether an individual is depressed or not. Therefore, the increased education is translating into a disproportionately greater impact on reducing the fraction of people being classified as depressed. The results using the distributional analysis reconcile the relative effect sizes of the mean impacts based on different measures.

Further, we conduct a distributional test to assess the statistical significance of the observed differences in mental health measures for the above median (worse mental health) and below median (better mental health) groups of the outcome distribution (following Shen and Zhang, 2016; Barcellos et al., 2021). This method estimates the distributional effects based on discontinuities in the cumulative distribution function (CDF) of mental health around the RD cutoff, and offers two key advantages – (1) it allows for an unconstrained examination of heterogeneities, and (2) this methodology is better powered to measure if the effects of education are concentrated on certain parts of the mental health distribution as compared to a difference in means approach (Barcellos et al., 2021). For the top half of the CESD distribution (worse mental health), the test rejects the null hypothesis that CSL policy led to no changes (p-value = 0.0007) or a worsening (p-value = 0.0004) of mental health, which implies improved mental health for the top part of the score distribution due to the CSL policy. On the other hand we are unable to reject the null of no effects for the bottom half of the CESD distribution (p-value = 0.9447) – which implies null effects of the compulsory schooling reform on mental health for the bottom half of the CESD distribution (better mental health). Put together, these findings suggest that the average treatment effect of education on mental health is largely driven by improvements on the top half of the CESD distribution (worse mental health). We corroborate these findings using another distributional approach (quantile RD) based on Frandsen et al. (2012).

In terms of potential mechanisms, we find suggestive evidence that the effects are plausibly driven by three different channels – (1) improved physical health, (2) better labor market outcomes (income and type of employment) and (3) enhanced marital outcomes.[2] Physical health can negatively affect mental well-being through the incidence of chronic pain and fears regarding health/mortality, among other factors (Ohrnberger et al., 2017). Improved health can enhance mental health indirectly by reducing catastrophic health expenditures, which can be a source of major stress for households in low- and middle-income settings (Wagstaff and Doorslaer, 2003; Sirag and Mohamed Nor, 2021). Employment outcomes have been demonstrated to be strongly associated with better mental health, via a direct income effect as well as an indirect effect of job security and fringe benefits (Baird et al., 2013b; Hamersma and Ye, 2021; Ridley et al., 2020). Marital outcomes, such as being married, smaller age differences between spouses, and greater intra-household decision making power, are found to be associated with lower rates of depression across a variety of contexts (Grundström et al., 2021; Heath and Tan, 2020; Kim et al., 2015). We present evidence that is suggestive of these channels being at work in the Chinese context.

This paper makes several valuable contributions. To our knowledge, this is the first study to explore the effect of education on mental health using a distributional approach. Focusing solely on average effects may conceal heterogeneous effects of the compulsory schooling policy along different parts of the mental health distribution. The importance of distributional analysis of education policies is highlighted in a recent study by Buhl-Wiggers et al. (2022). They note that most studies evaluating education interventions in developing countries focus almost entirely on average treatment effects; but a policy that works on average does not necessarily help everyone. If the return to education is higher for the relatively healthier people, then education may leave some behind. We complement the literature on education and mental health by demonstrating that the effects of education on mental health are concentrated among the higher quantiles of mental illness distribution (worse mental health).

More broadly, we add to the scant literature on the differential distributional effects of schooling interventions. As Glewwe and Muralidharan (2016) put it, understanding the treatment effect heterogeneity is "*likely to be a first-order issue*" in evaluating education interventions. There are only a few studies that examine treatment effect heterogeneity in education — for example, Jackson and Makarin (2018) employ a conditional quantile treatment regression method to demonstrate that the impact of lesson plans is more pronounced for educators with lower proficiency levels. Glewwe et al. (2009) find that textbooks yield improvements in academic performance primarily among the most academically accomplished students. Additionally, Barcellos et al. (2021) show that the influence of compulsory schooling laws in the UK on body size is primarily concentrated at the upper tail of the body size distribution. We join these existing works and provide a fuller picture of the impact of education policies across the mental health distribution.

The contribution of this study is enhanced due to the relative scarcity of evidence on the effects of education on mental health in developing country contexts. While there exist a few studies looking at the average treatment effect of education on mental health (Avendano et al., 2020; Jiang et al., 2020; Dursun and Cesur, 2016; Kondirolli and Sunder, 2022), we improve on these analyses in at least one of the following ways – (1) we capture mental health using a clinically validated measure of depressive symptoms, (2) we bring forth novel evidence on the distributional implications of enhanced education, and (3) we examine effects nearly three decades after the reform, which helps us quantify the long run mental health effects of a CSL policy.

[1] Other studies have used a subset of the CESD-20 index to create another measure of mental health called the CESD-10 (ranges from zero to 30, higher values denote worse mental health) – we use this measure and create an analogous measure of severe depressive symptoms called *Depressed10* (takes a value of one if CESD-10 is greater than 10, and zero otherwise).

[2] However, we are unable to definitively attribute the impacts we observe to these channels because the mechanisms identified in the study may be impacted by mental health itself. For instance, prior studies suggest that poor mental health has adverse effect on employment and earnings (Frijters et al., 2014; Kessler et al., 2008) and marital outcomes (Mojtabai et al., 2017).

This paper also adds to the growing literature on the determinants of adult mental health. We document the effects of a policy intervention in the late-childhood and early adolescence period. While early childhood is a critical (and perhaps the most important) period for the development of brain and personal character (Shonkoff et al., 2012; Adhvaryu et al., 2019), the early adolescent period is also key in shaping cognitive and non-cognitive skills (Cunha et al., 2010).

Our analysis also contributes to the evidence on the impacts of compulsory schooling laws on health outcomes in general. Several studies have explored this relationship based on data from developed countries, and have found mixed evidence (Hamad et al., 2018).[3] More recently there have been some studies analyzing the health impacts of the Chinese CSL reform on a number of different outcomes — such as own health (Huang, 2020), the health effects on the children of beneficiaries (Cui et al., 2019; Rawlings, 2015), the health and cognitive abilities of beneficiaries' parents (Ma, 2019), happiness and mental health (Liang and Dong, 2019; Jiang et al., 2020). We complement this literature by providing additional evidence on the distributional health effects of the CSL policy.

The rest of the paper is organized as follows. Section 1 provides the institutional background of the compulsory schooling law in China, while Section 2 explains the data sources and the key variables used in the analysis. We discuss the identification strategy used to estimate the average treatment effects and the associated results in Section 3. In Section 4, we discuss the approach used to estimate the distributional effects, the results we find and the associated robustness checks. And in Section 5 we discuss the potential mechanisms through which CSL impacts mental health. Section 6 presents concluding thoughts.

1. China's compulsory schooling law (1986)

The "Compulsory Education Law of the People's Republic of China", which took effect on July 1st, 1986, was the first nationwide law of its kind in China. It mandated that all school-age children must complete at least nine years of education. The nine-year compulsory schooling primarily consisted of six years of primary school and three years of junior-middle-school, while in some southern provinces it was five years of primary school plus four years of junior middle school. The CSL also required automatic promotion of students into junior middle school, without the need to qualify through any exams (which used to be the case before this policy). To further encourage enrollment, the law made nine years of education tuition-free for all the age-eligible children. This was one of the largest policies of its kind, and accounted for over 50 percent of the total educational public expenditure in China (China Educational Finance Statistical Yearbook, 1997). Although this expenditure was partially subsidized by the central government, most of the cost (and burden of implementation) was borne by local governments.

The CSL promotes the constructions of new schools to accommodate new students. Following the law, the government increased its investment in the education sector — capital investment in education (as a percentage of total capital investment in China) grew by around 8 percent between 1985 and 1988. A major portion of this increased investment was targeted towards the construction and maintenance of schools (page 118, Zhang, 2011). This guarantees the capacity of schools to accommodate new students.

[3] Positive (or protective) effects have been detected for body mass and physical activities for females (Brunello et al., 2013), health knowledge (Johnston et al., 2015), emotional self-regulation (Hahn and Truman, 2015) and mortality (Lleras-Muney, 2005; Lager and Torssander, 2012). On the other hand, there is evidence suggesting weak or negative health effects of education on outcomes such as mortality (Clark and Royer, 2013; Meghir et al., 2018), breast cancer risk and survival (Palme and Simeonova, 2015), long term illness for women and health-related behavior (Kemptner et al., 2011).

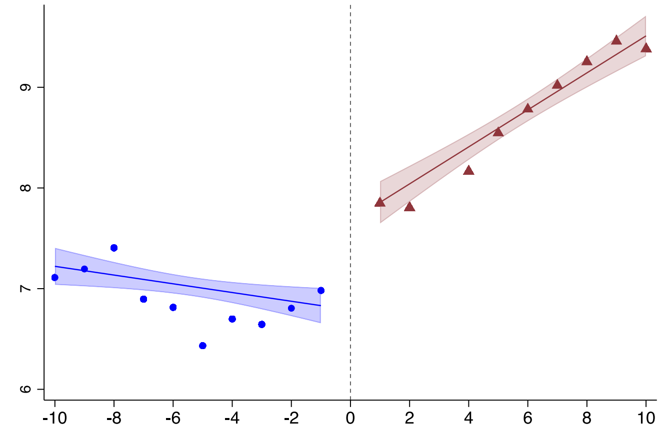

Fig. 1. Trends of years of education. *Note:* The horizontal axis indicates the distance in years from the first affected cohort in each province. The sample includes individuals born between −10 and +10 years around the cutoff in each province. The graph includes local linear fits on either side of the cutoff, and the 95% confidence intervals.

The CSL reform also promoted school-based changes, such as requiring teachers to hold teaching certificates, improving teachers' working status and welfare, formalizing curriculum. In the period between 1985 and 1990, the hiring of teachers led to a decline in the student–teacher ratio from 23 in 1985 to 20 in 1990. However, the newly hired teachers had similar qualifications to the teachers that were already employed in the Chinese school system — the percentage of junior middle school teachers (grades 6 to 9) who had completed a four-year college education only increased by 0.3 percent between 1985 and 1988.

Overall, the policy was fairly effective in achieving its goals and was considered a success. By the year 2000, China attained close to universal primary school enrollment. The rate of promotion into junior high school from primary school increased by 27 p.p., from 68 percent to 95 percent during 1985–2000. Simultaneously, there was a decline in drop-out rates — between 1990 and 2000, the dropout rates in primary and secondary schools decreased by around 77 and 41 percent (relative to 1990 levels), respectively (Education Statistics Yearbook of China, 1987 and 2000).

The timing of policy initiation varied across different provinces. Table A.1 presents the date when the CSL took effect in each province — this information is compiled from different administrative documents. Information in table A.1 clearly shows that while the law was implemented in a staggered manner, more than half of the provinces (16 out of 31) received this policy in 1986 (the year this policy was announced). By the year 1988, all but five provinces had implemented this policy. Since the implementation date is important in determining the CSL beneficiary cohorts in each province, we standardize the first cohort affected by the CSL to be zero in all provinces, and present graphical evidence of the education impacts of CSL in Fig. 1. The graph plots the binned (annual) average of years of education for each cohort. The graph shows a clear discontinuous jump in years of education around the cutoff, suggesting that the reform increased education for beneficiary cohorts (to the right of the cutoff). This graph provides strong evidence of the effectiveness of the CSL in increasing years of schooling for the beneficiary cohorts.

2. Data & key variables

A bulk of the analysis uses the China Family Panel Studies (CFPS), a nationally representative sample covering 29 out of 31 provinces in mainland China.[4] It is a panel survey of about 14,000 households

[4] The two omitted provinces are Inner Mongolia and Tibet.

that has been conducted every alternate year since 2010. The survey is focused on the economic and non-pecuniary wellbeing of the Chinese population, with rich information covering such topics as economic activities, education outcomes, family dynamics and health outcomes at the household and individual levels.

We measure education by total years of formal schooling attended. The main outcomes of interest in our analysis are different measures of adult psychological well-being. We primarily use the Center for the Epidemiological Studies of Depression scale (CESD) (Radloff, 1977). The CESD is a validated screening tool for detecting depressive symptoms, and is suitable to be used across different racial, gender and age groups (Weissman et al., 1977). Furthermore, this scale has been used widely not only in developed countries (Assari et al., 2016; Courtin et al., 2019), but also in a variety of low and middle income settings such as China (Zhang et al., 2017; Scheffel and Zhang, 2019), Ecuador (Paxson and Schady, 2010), South Africa (Fernald et al., 2008) and Vietnam (Singhal, 2018). While the original index is based on 20 items, there is a shorter 10-item version that has also been shown to have similar predictive accuracy (Andresen et al., 1994). We use both the CESD–10 and CESD–20 measures. Data for the complete CESD–20 scale was collected only in the 2012 round of the CFPS, and hence the main results are based on this wave of the survey.

Every component of the CESD–20 (CESD–10) index is listed in Table A.3. Individuals are asked about the frequency with which they experienced each item during the past week. The responses are coded on a four-point scale, ranging from zero ("never or less than one day") to three ("most of the time (5 to 7 days)"). The responses to each item are then aggregated to get the CESD index for an individual. The CESD–20 index ranges from zero to 60, while the CESD–10 index ranges from 0 to 30. Higher scores indicate increased levels of depressive symptoms or worse mental health.[5] Table 1 provides some summary statistics related to these indices. The average CESD–20 score in our sample is 12.8, while the average CESD–10 score is 6.4.[6]

In addition, we also create a categorical variable for severe stress/clinical depression, *Depressed20* (*Depressed10*), which takes a value of one if the individual score is above the threshold of 20 (10) for the CESD–20 (CESD–10) index (see Figure A.1 in the appendix for the distributions). As shown in Table 1, 17 percent of the CSL beneficiaries reported experiencing clinically depressive symptoms as opposed to 21 percent in the control group, clearly showing lower prevalence of depression in the beneficiary group.

We primarily probe three potential channels through which the CSL could have shaped mental health. The first channel is physical health, which we proxy using the following measures — self-reported health (ranging from 1 to 5, with higher values indicating better health), a categorical variable each for smoking and drinking, and scores based on a cognition test involving basic math and word skills.[7] The second channel is labor market outcomes, which we measure using the individual's personal income in the past year, current financial assets, total value of all household assets. We also consider other measures of employment quality in developing country context — whether the individual works in a formal sector job or not (measured by whether they work in non-agricultural industries), an indicator for being covered by employer-provided medical insurance, and a categorical variable for being covered by employer-provided pension insurance. The third channel we explore is marital outcomes, where we use the following variables — whether the individual is currently married, if so whether they were married before age 18 (child marriage), the age difference between the individual and their spouse, and their decision making power in the household (this is constructed by aggregating responses to five questions asking whether the subject could make decisions in each scenario in the household. It is measured on a scale from 0 to 5, with higher values denoting more decision making power). Table A.2 provides some summary statistics related to these channel variables.

3. CSL and mental health (Mean effects)

3.1. Empirical strategy

Our empirical strategy leverages the age-specific exposure of different cohorts to the CSL reform. The oldest affected cohort within each province consists of individuals in (or about to start) the ninth schooling year at the time of CSL implementation. Based on the province-wise implementation date (from table A.1) and the school starting age in China (equal to seven years[8]), we calculate the date of birth for this cohort in each province. Children born after this cutoff date (younger than the first affected cohort) had to complete nine years of education and are thus CSL beneficiaries. However, those born before this cutoff date cohort did not fall under the ambit of this law, and hence had the option of completing less than nine years of schooling.

For each birth cohort we construct a measure of distance from the oldest affected cohort (called $Cohort_i$) which takes a value of zero for individuals in the first (oldest) affected cohort in each province, and subsequently takes integer values in increments of one for each cohort born in successive years. Analogously, individuals born before the oldest affected cohort (hence not impacted by CSL) would be assigned negative integer values. Note that we use a person's residential province at age 12 as a proxy of their residential location in adolescence, the period when they are most likely to be affected by CSL — this is akin to the strategy used in other similar studies (Cui et al., 2019; Erten and Keskin, 2018, 2020). Our identification strategy rests on the assumption that individuals born a few years on either side of the cutoff date were similar (conditional on covariates), except their exposure to the CSL.[9] This cutoff defines reform exposure and allows for an RD design. We primarily focus on the reduced form estimates (Sharp RD), as is the case in prior work estimating the effects of CSL policies in other contexts (Oreopoulos, 2006; Erten and Keskin, 2018, 2020; Gulesci et al., 2019; Clark and Royer, 2013).

The RD setup is as follows:

$$\begin{aligned} Y_i =& \alpha + \beta t_i + f(Cohort_i) + X_i\beta_1 + \epsilon \\ & \forall Cohort_i \in (0-h, 0+h), \end{aligned} \tag{1}$$

where Y_i represents the outcomes of individual i, t_i is the treatment status of individual i (equal to one for CSL beneficiaries), $Cohort_i$ is the running variable (described above), and $f(Cohort_i)$ is a polynomial function of the running variable. In our main results we use local linear functions, and control for a vector of covariates (X_i) for individual i, including gender, Hukou status at age 3, categorical variable for being Han ethnicity and a set of dummies for the province of residence at age

[5] For the positive questions, the coding is reversed so that a higher score indicates poorer mental health.

[6] The Cronbach's α statistics is 0.85 for the CESD–20 scale and 0.77 for the CESD–10 scale in our sample, indicating high internal consistency.

[7] The survey asks individuals to report whether they consumed any alcoholic beverage more than three times in the past month — we create a categorical variable based on this question.

[8] In the year 1951 the federal government issued the "Decision on Education System Reform" that stipulated the school entry age to be seven years. The choice of seven years as the school starting age is also consistent with other work in the Chinese context (Cui et al., 2019).

[9] Fertility rates dropped significantly in the early 1970s (even prior to the enforcement of the one-child policy in 1978) under the influence of the "Later, Longer, Fewer" policies (LLP) (Chen and Fang, 2021). There might be a concern that the efforts to lower birth rates may lead to healthier and more educated individuals. If such efforts coincide with the CSL, then it will lead to an overestimation of the true effect of the CSL in our paper. However, a comparison of table A.1 and the policy implementation years in Chen and Fang (2021) demonstrates that the rollout patterns of these two policies did not significantly overlap.

Table 1
Summary statistics.

	CSL beneficiaries		Non-beneficiaries		Full sample	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Male	0.49	0.50	0.49	0.50	0.49	0.50
Ethnicity (=1 if Han)	0.90	0.30	0.93	0.26	0.92	0.28
Hukou status at age 3 (rural hukou = 1)	0.88	0.32	0.89	0.31	0.89	0.32
Age	37.95	2.04	42.70	2.04	40.56	3.12
CESD10 score	6.21	4.08	6.63	4.59	6.44	4.38
CESD20 score	12.51	7.27	13.07	8.12	12.83	7.76
Depressed10 (CESD $\geq$ 10)	0.19	0.4	0.24	0.43	0.22	0.42
Depressed20 (CESD $\geq$ 20)	0.17	0.37	0.21	0.4	0.19	0.39
Years of schooling Attended	8.01	4.15	7.10	4.26	7.51	4.23
CESD–20 Items						
1. I was bothered by things that usually don't bother me.	0.78	0.76	0.78	0.80	0.78	0.78
2. I did not feel like eating; my appetite was poor.	0.52	0.67	0.58	0.73	0.55	0.71
3. I felt that I could not shake off the blues even with help from my family or friends.	0.41	0.67	0.45	0.70	0.43	0.69
4. I felt I was just as good as other people.	1.91	1.06	1.88	1.07	1.90	1.07
5. I had trouble keeping my mind on what I was doing.	0.56	0.72	0.62	0.77	0.59	0.75
6. I felt depressed.	0.61	0.68	0.66	0.75	0.64	0.72
7. I felt that everything I did was an effort.	0.55	0.73	0.63	0.80	0.60	0.77
8. I felt hopeful about the future.	1.12	1.01	1.18	1.03	1.15	1.02
9. I thought my life had been a failure.	0.48	0.72	0.54	0.81	0.51	0.77
10. I felt fearful.	0.28	0.55	0.30	0.61	0.29	0.58
11. My sleep was restless.	0.63	0.80	0.68	0.84	0.65	0.82
12. I was happy.	1.17	0.97	1.22	1.00	1.20	0.98
13. I talked less than usual.	0.60	0.79	0.62	0.81	0.61	0.80
14. I felt lonely.	0.37	0.65	0.39	0.69	0.38	0.68
15. People were unfriendly.	0.31	0.56	0.32	0.58	0.31	0.57
16. I enjoyed life.	1.08	0.96	1.10	0.99	1.09	0.98
17. I had crying spells.	0.33	0.55	0.31	0.58	0.32	0.57
18. I felt sad.	0.40	0.60	0.40	0.63	0.40	0.62
19. I felt that people dislike me.	0.30	0.55	0.29	0.55	0.29	0.55
20. I could not "get going."	0.16	0.47	0.19	0.54	0.17	0.51

Note: We restrict the sample to individuals within a four year window around the cutoff birth cohorts.

12 (μ_p).[10] ϵ_i represents the error term. The parameter of interest in these regressions is β, which gives us the effect of the CSL policy. For our main results we determine the optimal bandwidth (h) for each outcome separately using the algorithm outlined in Imbens and Kalyanaraman (2012).

In sensitivity analyses, we show that our findings are largely unchanged to using other methodologies of determining the optimal bandwidth (h), namely Mean Squared error (MSE) approach and the Coverage Erorr Rate (CER) approach (Calonico et al., 2014, 2017, 2019). As we use data close to the threshold, we only have a few different values of the running variable on each side. We cluster standard errors at the province-$Cohort_i$ level, a strategy similar to other studies (Heinesen, 2018; Fredriksson and Öckert, 2014). In addition, we conduct a robustness check using *honest* confidence intervals that are generated based on the method outlined in Kolesár and Rothe (2018) (presented in Appendix C) – they suggest that in cases when the running variable used in an RD analysis is discrete, it may lead to confidence intervals having poor coverage properties.

Note that the impact estimates we provide are intent-to-treat (ITT) estimates of the effect of the policy, akin to other studies of a similar nature (Cook and Kang, 2016; Malamud and Pop-Eleches, 2011; Palme and Simeonova, 2015; Meghir et al., 2018). In Appendix B, we provide further checks to validate the use of the RD design in this setting, which include the McCrary density test (to establish that there was no manipulation around the RD cutoff), and a balance check showing that other pre-determined covariates were no different across the RD cutoff (McCrary, 2008).

[10] Hukou is a family registration system in China that is used to distinguish rural and urban citizens. We use the status at three years of age since later life status might be endogenous with education (and the CSL reform).

Table 2
Effect of the Chinese CSL on years of education.

Outcome = Years of education	(1) Full sample	(2) Rural	(3) Urban
RD estimate	0.70***	0.93***	0.28***
	[0.06]	[0.08]	[0.03]
N (within bandwidth)	4941	4302	639
Bandwidth (in years)	4	4	4
Control mean	6.37	5.73	10.95

Note: The optimal bandwidth is computed using the IK approach outlined in Imbens and Kalyanaraman (2012) with a triangular kernel. All specifications include the following controls: gender, hukou status at age 3, ethnicity dummies and categorical variables for province of residence at age 12. Standard errors are clustered at the province-$Cohort_i$ level.

3.2. Effect of CSL on education

We use the RD specification in (1) to estimate impacts of the CSL policy on educational outcomes. Using the algorithm outlined in Imbens and Kalyanaraman (2012), we find the optimal bandwidth to be four years. Running a local linear regression using observations within this bandwidth yields an RD estimate of 0.7 years (column 1 in Table 2). This corresponds to an around 11 percent increase relative to the mean of the control group.

Further, we calculate the impact estimate for rural and urban subsamples. The reform effect is relatively higher among rural residents (0.93 years) than for the urban sample (0.28 years). This higher effect for rural residents could be because of their low initial educational attainment before CSL implementation — the average number of years of schooling for rural residents unaffected by the reform is more than five years lower than the corresponding average for the urban non-beneficiaries. The finding that rural residents benefited more from CSL is consistent with previous findings in China (Cui et al., 2019) and those in other settings (Erten and Keskin, 2020).

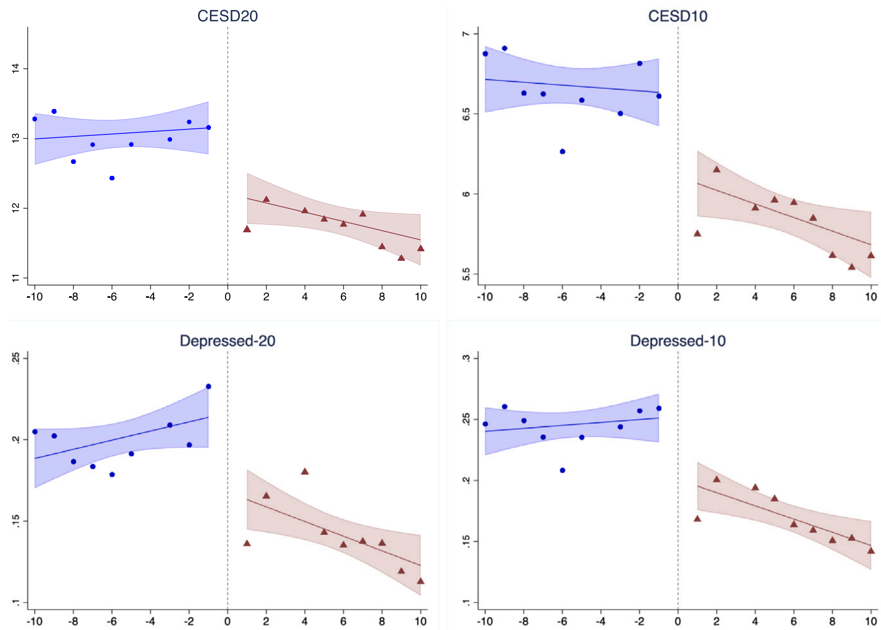

Fig. 2. Effect of the Chinese CSL on mental health. *Note:* The shaded regions represent the corresponding 95 percent confidence intervals.

3.3. Effect of CSL on mental health

We begin by presenting graphical evidence of the effects of CSL on mental health. Fig. 2 plots the local averages of adult mental health against the running variable ($Cohort_i$) – each graph represents a different outcome. The graphs in the left panel present results for CESD-20 index and the associated categorical variable for clinical depression (Depressed-20), while the right panel presents results for CESD-10 and Depressed-10 measures. Note that a higher score on each of the indices indicates worse mental health. All the graphs show a clear and robust fall in the corresponding value of the measure among cohorts just to the right of the cutoff (CSL beneficiaries) as compared to those to the left of the cutoff (non-beneficiaries), which suggests improved mental health for the beneficiary cohorts (as a fall in CESD score and probability of being depressed represents mental health benefits).

In Table 3, we present point estimates from a regression analysis exploring the effects of the CSL on long-run mental health outcomes. Columns 1 and 2 present impact estimates for the different variations of the CESD–20 index, and columns 3–4 present results based on the CESD–10 index. The results presented in Table 3 suggest that the CSL led to a statistically significant reduction in CESD scores, thus engendering a positive effect on mental health of the beneficiaries. In the RD specification with the full set of controls, exposure to the policy reduced CESD–10 and CESD–20 scores by 0.47 and 1.07 points respectively (columns 1 and 3 of Table 3). As higher values of CESD indices denote poorer mental health, the results suggest that exposure to the CSL improved mental health among beneficiary cohorts. For the CESD–20 index, the magnitude of the effect corresponds to around 8 percent of the control group mean, which is a substantial effect, especially considering the facts that (1) the CSL policy was not directly targetted towards improving mental health (this is a positive spillover of the CSL), and (2) these effects on mental health are measured almost three decades after the policy was implemented, and thus represent fairly longer term effects. In the appendix C, we show that this estimate is robust to the change of different model specifications, such as the choice of kernel functions and bandwidth selection methods.

The magnitudes of the negative effects on the probability of being depressed are around 6 percentage points (columns 2 and 4 of Table 3). In relative magnitude, these effects are substantially larger as they translate to an around 28 percent decline in the chances of depression in adult life (relative to the non-beneficiary group mean). We hypothesize that the disproportionately high effect on the probability of being depressed as compared to the effect on the CESD scores is potentially because of a large effect of the policy on the upper tail of the CESD distribution (and around the threshold values). We explore this in greater detail in the next section, where we discuss the distributional consequences of this reform.

Table 3
Effect of the Chinese CSL on mental health.

	(1) CESD20	(2) Depressed20	(3) CESD10	(4) Depressed10
CSL policy (=1)	−1.07***	−0.06***	−0.47***	−0.06***
	[0.21]	[0.01]	[0.08]	[0.01]
N (within bandwidth)	5561	1950	3228	1961
Bandwidth (in years)	5	2	3	2
Control mean	12.97	.21	6.63	.24

Note: The optimal bandwidth is computed using the Imbens–Kalyanaraman approach outlined in Imbens and Kalyanaraman (2012) with a triangular kernel. All specifications include the following controls: gender, hukou status at age 3, ethnicity dummies and categorical variables for province of residence at age 12. Standard errors are clustered at the province-$Cohort_i$ level.

Overall, the impact estimates we find are comparable to other studies. For example, Dursun and Cesur (2016) find that exposure to a Turkish compulsory schooling reform (extending minimum schooling from five to eight years) increased the composite life satisfaction index by 0.17 standard deviations for female adults. As another example, Erten and Keskin (2020) find that the probability of inter-generational transmission of domestic violence against children fell by 27 percent if women were exposed to the same Turkish CSL policy. Our results are also comparable to other studies that explore the link between exposure to early life shocks and mental health. For instance, Singhal (2018) finds that a one percent increase in bombing during childhood leads to a 16 percentage point increase in the probability of reporting severe mental distress as an adult. In another study, Adhvaryu et al. (2019) find that a one standard deviation increase in cocoa prices during early life led to a reduction in mental stress in adulthood by around 50 percent. This finding is also consistent with previous studies that have found protective effects of compulsory schooling reforms on other measures of psychological well-being in different contexts (examples include Oreopoulos, 2007; Dursun and Cesur, 2016; Crespo et al., 2014; Kondirolli and Sunder, 2022).

4. CSL and mental health – Distributional effects

4.1. Empirical strategy

The average treatment effects estimated earlier might conceal differential effects of CSL across the distribution of mental health status. We investigate whether this is the case by probing the following question — were the effects of CSL on mental health concentrated at particular parts of the mental health (CESD indices) distribution? To answer this question, we estimate the distributional treatment effect (DTE) based on the method outlined in Shen and Zhang (2016). In particular, we calculate the effect of the CSL (differences across RD cutoff) for the entire support of the CESD distribution. For every possible level of mental health m in the support of our distribution, we compute (and plot) the difference (and its 95 percent C.I.) between the beneficiary and non-beneficiary's conditional cumulative density functions (CCDF) for the CESD index (Y_i). This provides a graphical representation of how the distribution of mental health changed between CSL beneficiaries and non-beneficiaries. This approach rests on the assumption that without the policy intervention, the distributions (at particular percentiles) of the outcome variable(s) would be continuous and not shift around the cutoff. Although direct testing of this assumption is not feasible (as stated in Barcellos et al., 2021), the absence of any significant discontinuities in numerous predetermined variables (figure B.2 in the appendix) supports its validity. Also note this assumption does not invalidate the use of discrete outcome variables (Shen and Zhang, 2016).

For a particular level of mental health m, the non-beneficiary's cumulative distribution function (CDF) of the CESD index is defined as the CDF for non-beneficiaries in the limit when the date of birth variable is converging to the cutoff from the left (i.e. $Cohort < 0$):

$$F_{Y_i(0)|(Cohort=0)}(m) = \lim_{x\to 0^-} Pr(Y_i \leq m|Cohort_i = x) \tag{2}$$

Similarly, the beneficiaries' CDF is defined as the CDF for beneficiaries in the limit when the date of birth variable is converging to the cutoff from the right (i.e. $Cohort > 0$):

$$F_{Y_i(1)|(Cohort=0)}(m) = \lim_{x\to 0^+} Pr(Y_i \leq m|Cohort_i = x) \tag{3}$$

At a given value of m, the discontinuity of the CESD index is then equal to the difference of these two CDF:

$$\delta(m) = F_{Y_i(1)|(Cohort=0)}(m) - F_{Y_i(0)|(Cohort=0)}(m) \tag{4}$$

Another way of representing $\delta(m)$ is to examine it graphically — this would be done by presenting both $F_{Y_i(1)|(Cohort=0)}(m)$ and $F_{Y_i(0)|(Cohort=0)}(m)$ on a graph and then examining the difference between them. In this case $F_{Y_i(0)|(Cohort=0)}(m)$ is estimated using individuals who lie to the left of the RD cutoff (the non-beneficiaries of the CSL policy). Similarly, $F_{Y_i(1)|(Cohort=0)}(m)$ is estimated using people who lie to the right of the RD cutoff (the beneficiaries of the CSL policy).

The above methodology enables us to examine the differences between CCDF's but does not provide any information on whether these differences are statistically significant. For that purpose we conduct distributional tests (Kolmogorov–Smirnov test), which confirm whether the observed effects are concentrated in the top or bottom half of the distribution (and differences in which part of the distribution are statistically significant). In particular, we conduct three separate tests, denoted as follows:

$$\begin{aligned}
&H_0^2 : \delta(m) = 0 \ \forall m \in R, \ \ H_a^2 : \exists m \in R \ \ s.t. \ \ \delta(m) \neq 0, \\
&H_{0,+}^1 : \delta(m) \geq 0 \ \forall m \in R, H_{a,+}^1 : \exists m \in R \ \ s.t. \ \ \delta(m) < 0, \\
&H_{0,-}^1 : \delta(m) \leq 0 \ \forall m \in R, H_{a,-}^1 : \exists m \in R \ \ s.t. \ \ \delta(m) > 0.
\end{aligned} \tag{5}$$

The first test, labeled H_0^2, is a two-sided distributional test where the null hypothesis is that the CDFs before and after the reform are *equal*. A rejection of this null hypothesis indicates that the CCDFs for the two groups are statistically significantly different from each other. The null hypothesis of the second test ($H_{0,+}^1$) is that after the CSL reform the probability of observing a CESD score (Y_i) smaller than a given value (m) increases (or stays the same). Alternatively stated, the null hypothesis implies that the CSL decreases (or does not change) the conditional distribution of CESD index for the beneficiaries (thus making mental health better or unchanged). The last test, labeled $H_{0,-}^1$, has the following null hypothesis: the probability of observing a CESD score (Y_i) smaller than a given value (m) decreases (or stays the same) after the reform. In other words, the null hypothesis here is that the CSL policy increases (or does not change) the conditional distribution of CESD indices – a rejection of this null hypothesis indicates that after the CSL reform the CESD scores went down (mental health improved).

4.2. Results – Distributional effects of CSL policy

Fig. 3 shows the differential effects of the CSL along the mental health distribution. In Fig. 3 the CCDF for the beneficiary cohorts lies to the left of the CCDF of the non-beneficiary cohorts, which suggests that the compulsory schooling reform reduced the CESD20 scores, thus engendering better mental well-being. If the treatment effects of the policy were homogeneous then one would observe effects across the whole distribution, but the largest effects would be concentrated around the mode of the distribution (Barcellos et al., 2021), which in our case is a CESD-20 value of nine.[11] However, our findings do not support this – Fig. 3 shows that the difference in the CDFs is always positive and this difference becomes largely statistically significant (at the 5 percent level) above the CESD value of 14. This implies that the gains are heterogeneous across the CESD distribution, and being statistically significant for the top part of the CESD distribution.

In particular, we find that the CSL increased the fraction of individuals with a value of CESD20 smaller than 20 (the threshold for being classified as *Depressed*) from 82 percent to 87 percent, implying that the odds of being depressed reduced by around 5 percentage points after the reform (which is very similar in magnitude to the average treatment effect on *Depressed20* shown in Table 3). Moreover, the 90th percentile of the CESD-20 measure among the beneficiary cohorts decreases from 24 to 22, which represents a change that is nearly twice the average treatment effect (1.07 points) presented in Table 3. In essence, we detect important distributional differences in the effect of CSL on mental health along the CESD distribution.

The difference in the CCDFs is highest right around the critical threshold value of 20 — this suggests that the reform effectively reduced the fraction of the population with large or extreme values of the CESD20 score. Recall that the threshold of being classified as depressed is 20 for the CESD20 measure, this implies that a reduction in the mean CESD score led to a disproportionately larger fall in the probability of being depressed. This explains our findings in Section 3.3 that an 8 percent drop of the CESD score (relative to the control group mean) corresponds to a larger fall of depression (28 percent relative to the control mean). Therefore, results in Fig. 3 have demonstrated that the mean effect of the CSL is almost entirely driven by the top part of the CESD distribution.[12] The pattern of the results are similar when we examine the CESD-10 measure (see figure A.2).

[11] Homogeneous treatment effects do not mean that the shift in the CCDFs of beneficiaries and non-beneficiaries is parallel (that is, a constant value for $\delta(m)$ in Eq. (4)) across the distribution of mental health.

[12] Note that our distributional effect results are informative about effects on distributions, and not on individuals. If we assume rank invariance (that is, if everyone maintains the same rank across potential outcome distributions with and without the CSL), then our results could be interpreted as "the policy reduces the chance of depression for those with potential (or counterfactual) worse mental health" (Dong and Shen, 2018). If the rank preservation assumption were not to hold, then the distributional results only indicate that the higher quantiles of the CESD distribution are reduced after the CSL policy, regardless of who are in higher quantiles and whether their potential mental health status is in the top or the bottom quantiles of the CESD distribution.

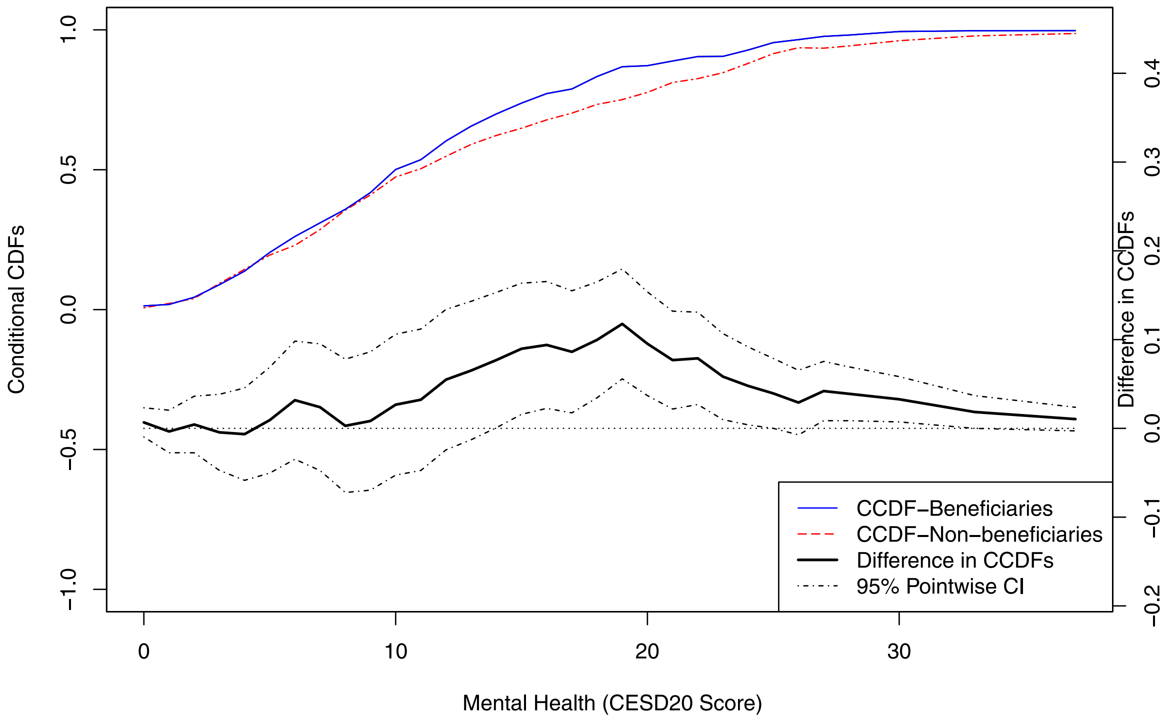

Fig. 3. Distributional effects of the CSL on mental health. *Note:* The figure plots the distributional effects of the CSL on CESD–20. Higher values of CESD indices imply lower mental well-being. Nonparametric local linear estimations are conducted using a triangular kernel and the rule-of-thumb bandwidth as described in Shen and Zhang (2016). The "CCDF-beneficiaries" line is the estimated outcome distribution for individuals born just to the right of the cutoff; the "CCDF-non-beneficiaries" line is the estimated outcome distribution for those who barely missed out benefitting from the CSL. The solid line in the lower half is the difference between "CCDF-beneficiaries" and "CCDF-non-beneficiaries". The dotted lines are 95 percent confidence regions of the distributional effects.

Table 4
Distributional effects of the CSL on mental health.

	CESD–20		
	(1) Equality of CDFs (H_0^2)	(2) Decrease in CESD ($H_{0,+}^1$)	(3) Increase in CESD ($H_{0,-}^1$)
Panel A: Full sample			
p-value of KS test	0.0007***	0.9760	0.0004***
Panel B: Above median			
p-value of KS test	0.0000***	0.8905	0.0000***
Panel C: Below median			
p-value of KS test	0.9447	0.5748	0.9448

Note: The table reports the p-values of the different distributional tests. The null hypothesis in column 1 is equality of beneficiaries' and non-beneficiaries' CDFs, while that in column (2) is that CESD-20 decreased among beneficiaries (or stayed the same), and in column (3) it is that the CESD-20 increased among beneficiaries (or stayed the same).

Further, we conduct tests (Kolmogorov–Smirnov based) to assess the statistical significance of the observed differences in the beneficiary and non-beneficiary distributions. As outlined in the empirical strategy section, we start by testing the null hypothesis of equality of the beneficiary and non-beneficiary CCDFs. Results in panel A of Table 4 imply that we are strongly able to reject the null hypothesis of equality of the CDFs (panel A, column 1) – this implies that the distribution of the outcome (CESD-20) was different between the beneficiaries and non-beneficiaries. However, this does not say anything about the direction of change — to explore this we conduct one-sided distributional tests to examine whether the CSL reform led to a decrease ($H_{0,+}^1$) or an increase in CESD scores ($H_{0,-}^1$). The results indicate that we are able to reject the null hypothesis that the CESD score went up or stayed the same among CSL beneficiaries (Table 4 panel A, column 3), implying a fall in CESD scores (better mental health) among beneficiaries. This is further corroborated by the results in panel A of column 2 (Table 4) — we are unable to reject the null that CESD scores went down (p-value = 0.98).

We conduct another check to confirm our earlier findings of the effect being concentrated at the top half of the mental health distribution (worse mental health). To do this, we conduct the aforementioned KS tests separately for observations below and above the median CESD-20 score (score = 12) in our sample. The results (presented in panels B and C of Table 4) suggest that the difference in the CCDFs between beneficiaries and non-beneficiaries is solely driven by the top half of the CESD distribution, whereas there are no statistically significant differences in the below-median half, further reaffirming the pattern we observe in Fig. 3. We obtain similar results when doing the Kolmogorov–Smirnov based distribution tests using CESD-10 scores (table A.4 in the appendix).

4.3. Robustness checks of the distributional effects

The CCDF-based approach that we have used above is closely related to a quantile approach (estimating RD impacts at different quantiles), which we use to corroborate the heterogeneous effects that we find. In particular we use the process described in Frandsen et al. (2012) to implement the quantile RD approach. One key difference from the approach used earlier is that the CDF approach is based on the vertical distance between the pre- and post-reform CCDFs whereas a quantile approach estimates the horizontal distance between the CCDFs. Despite this difference, it would be reassuring to us if the two methods generally lead to similar substantive conclusions (as the object of interest estimated in either case is similar in nature). The results from the quantile RD analysis are presented in Table 5, and suggest that there are no statistically significant effects for values below the median (50th percentile), while most of the effects are concentrated between the 70th and 90th percentiles of the CESD score distribution. These patterns are observed across all the outcomes of interest, and are similar in nature

Table 5
Quantile RD effects of CSL on mental health.

	20th percentile	30th percentile	50th percentile	60th percentile	70th percentile	80th percentile	90th percentile
CESD–10	0	0	−1**	−1*	−2***	−3***	−2***
	(0.389)	(0.442)	(0.485)	(0.533)	(0.595)	(0.581)	(0.730)
log(CESD–10)	0.000	0.000	−0.154**	−0.134*	−0.223***	−0.288***	−0.154**
	(0.110)	(0.138)	(0.071)	(0.068)	(0.062)	(0.061)	(0.060)
CESD–20	0	−1	−1	−1	−3**	−3**	−3*
	(0.641)	(0.745)	(0.832)	(0.981)	(1.246)	(1.293)	(1.822)
log(CESD–20)	0.000	−0.118	−0.087	−0.074	−0.182**	−0.147**	−0.123**
	(0.102)	(0.088)	(0.071)	(0.073)	(0.073)	(0.065)	(0.059)

Note: Based on data from the 2012 round of CFPS. Each row of the table shows the effects at different quantiles for a given outcome variable. These estimations are based on the process described in Frandsen et al. (2012). All estimations use the triangular kernel, with a bandwidth of four years. ***, ** and * denote significance at the 1, 5, and 10 percent levels, respectively.

to the results observed in the previous section, and hence validate the findings from the CDF analysis and the associated hypothesis testing.

Another way of understanding whether the effect of education on mental health is concentrated on the top half of the CESD distribution (worse mental health) is to divide the mental health distribution into parts based on different percentiles. We create four such samples based on whether the observations were below/above the 25th, 50th, 75th and 90th percentiles respectively.

To supplement the evidence presented in Fig. 3 we conduct another analysis. Based on different percentiles (25th, 50th, 75th and 90th), we create an indicator variable that takes the value of one if the CESD index is smaller or equal to the corresponding percentile of the CESD distribution. Then in a traditional RD plot, we verify whether there are any differences across the cutoff for the four variables that we have created. Based on our distributional analysis, we should observe statistically significant differences in the above median group, whereas we should observe minimal differences in the below median part of the distribution. In Fig. 4, we present the graphs for the 25th, 50th, 75th and 90th percentiles of the CESD distribution (following Barcellos et al., 2021). An upward jump indicates that a higher fraction of individuals have better mental health after the reform. We observe a more significant difference in mental health status across the RD cutoff in the graphs pertaining to the 75th and 90th percentiles of the CESD distribution, while the differences across the RD cutoff are very small for lower percentiles (25th and 50th). The results are similar when using the CESD-10 index (figure A.3 in the appendix). This corroborates our main finding that the top part of the CESD distribution benefited more from the CSL reform.

5. Mechanisms

We now investigate the potential channels through which China's CSL led to improved long-term mental health outcomes.[13]

Physical Health: Physical health is a strong predictor of mental health (Ohrnberger et al., 2017), especially among poorer individuals. Das et al. (2012) find that individuals with worse physical health are around 10 to 18 percent more likely to have worse mental health. Improved physical health can also have a positive effect on mental health indirectly by reducing catastrophic health expenditures, which can be a source of major stress for households in low- and middle-income settings (Wagstaff and Doorslaer, 2003; Sirag and Mohamed Nor, 2021). Therefore, we probe the role of enhanced physical health as a possible mediator of the mental health effects we observe in our main analysis.

To understand the potential role played by physical health in the context we study, we look at the effect of Chinese CSL on self-reported health (ranging from 1 to 5, with higher values denoting better health), health behavior (smoking and alcohol consumption) and cognitive function (math and reading scores). We find that beneficiaries report better subjective health status (column 1, panel A, Table 6), and were less likely to consume alcohol (column 3, panel A). They also experienced better cognitive functioning as measured by word memory and math tests (columns 4–5, panel A). These findings are in line with other recent studies that have found a positive education-health gradient in China (Huang, 2020), and with Adhvaryu et al. (2019) who also find that physical health is a potential mediator of the effect of an early life income shock on later life mental health.

Labor Market: The association between income and mental health is well documented — negative economic shocks and poverty are strongly associated with higher rates of mental illness (Sareen et al., 2011; Ridley et al., 2020), while an increase in income has been shown to reduce psychological distress (Baird et al., 2013a). We examine whether exposure to CSL affected mental health through the channel of improving individual earnings. We find evidence in support of this — CSL beneficiaries earned higher incomes later in life, and their households contain more assets, in terms of assets (panel B).

Studies have found that by reducing uncertainty surrounding catastrophic health expenditures, the provision of health insurance (and pension) in one's job can reduce the prevalence of depression (Hamersma and Ye, 2021; Tian et al., 2012; Chen et al., 2019), while other studies have found a negative relationship between informal employment and individual well-being (Ludermir and Lewis, 2003; Silva-Peñaherrera et al., 2021). In our study, we find that individuals who benefited from the Chinese CSL have a higher probability of being employed in a formal sector job, and were likelier to have employer-provided pension and medical insurances (panel B, Table 6). Put together, these results imply that the increased levels of education had a significant effect on employment and income outcomes in adulthood in our sample, which potentially had downstream mental health benefits.

Marital Outcomes: There is evidence on the strong association between being married and lower depression/anxiety (Jace and Makridis, 2021; Grundström et al., 2021). Studies have also demonstrated negative associations between earlier marriage, especially the practice of child marriage, and psychological well-being in later life (John et al., 2019). Larger age differences between couples has also been documented to increase the extent of depressive symptoms among partners (Kim et al., 2015), while higher intra-household bargaining power improves subjective well-being, especially for women (Ma and Piao, 2019; Heath and Tan, 2020). Therefore we probe different marital outcomes as potential channels for the effects we observe. The results in panel C of Table 6 indicate that CSL beneficiaries were more likely to be married than CSL non-beneficiaries, but were also less likely to marry before the age of 18. The spousal age gap was 0.38 years lower

[13] The mechanism results should be interpreted with caution due to the concern that the mechanisms identified in the study may be impacted by mental health itself. We would like to highlight that this approach to conducting mechanism analysis is prevalent in the literature linking education and mental health, as exemplified in studies such as Jiang et al. (2020), Dursun and Cesur (2016) and Kondirolli and Sunder (2022).

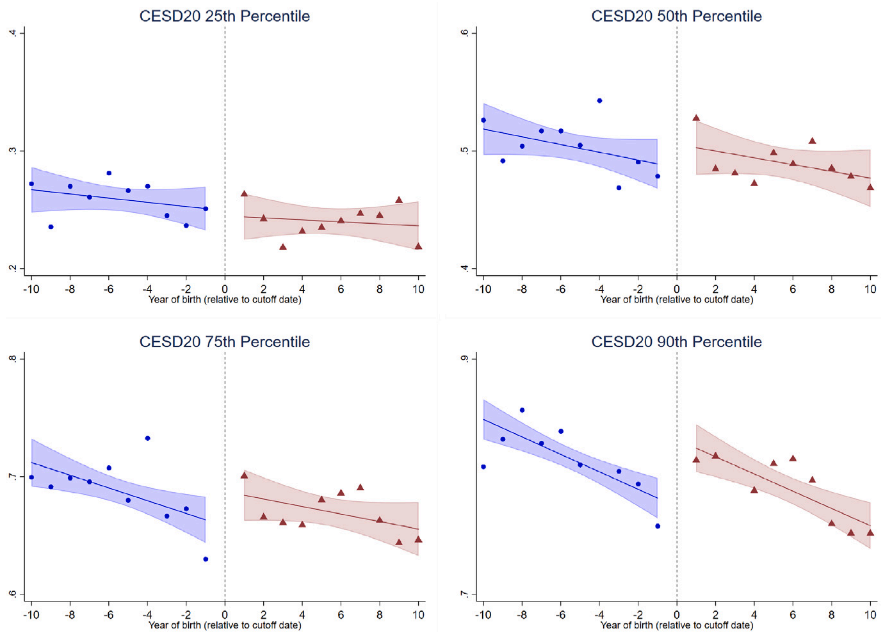

Fig. 4. Effect on percentiles of the distribution of mental health index (CESD-20). *Note:* The figure plots the RDD graphs for the 25th, 50th, 75th and 90th of the CESD-20 distribution. The vertical axis shows the fraction of individuals with a mental health index below the corresponding percentile of the CESD distribution. An upward jump indicates that a higher fraction of individuals have better mental health after the reform (lower CESD scores). The sample includes individuals born between −10 and +10 years around the cutoff in each province. The graph includes local linear fits on either side of the cutoff, and the 95% confidence intervals.

Table 6
Potential mechanisms.

	Panel A: Physical health					
	Self rated health	Smoked last month	Consumes alcohol	Math score	Reading score	
CSL policy (=1)	0.25***	0.00	−0.02***	0.74**	0.53***	
	[0.02]	[0.01]	[0.00]	[0.36]	[0.15]	
Bandwidth	3	2	2	5	4	
Obs. in bandwidth	3582	1988	1988	5495	4377	
Mean	2.87	.31	.17	17.62	9.69	
	Panel B: Labor market outcomes					
	Log (Personal Income)	log(Household Financial Asset)	log(Total Asset)	Formal job	Pension provided	Medical insurance
CSL policy (=1)	0.07**	0.28***	0.05	0.06***	0.02***	0.03***
	[0.03]	[0.06]	[0.04]	[0.00]	[0.00]	[0.00]
Bandwidth	3	3	3	2	2	2
Obs. in bandwidth	1970	3559	3405	2019	2019	2019
Mean	9.51	8.45	11.99	.58	.14	.14
	Panel C: Marital outcomes					
	Currently married	Early marriage	Spousal age gap	HH decision making		
CSL policy (=1)	0.003***	−0.017***	−0.51***	0.20***		
	[0.001]	[0.001]	[0.06]	[0.02]		
Bandwidth	2	2	3	3		
Obs. in bandwidth	2193	1977	3234	3531		
Mean	.94	.03	1.70	2.42		

Note: Panel A reports RD estimates on adult physical health. *Self rated health* is measured as a discrete variable that ranges from one to five, with higher values indicating better health. *Smoked last month* and *Consumes alcohol* are indicator variables that take a value of one if the individual smoked cigarettes or consumed alcohol in the last month. *Math score* and *Reading score* are test scores based on assessment of basic math and language skills. Panel B reports RD estimates on labor market outcomes. *Formal job* is a dummy variable that takes a value of one if a person's main job is in the non-agricultural sector. *Medical insurance* (*Pension provided*) are indicators that take the value of one if the subject has employer-provided medical insurance (pension), and zero otherwise. Panel C reports RD estimates on marriage outcomes. *Early marriage* is an indicator variable that takes the value of one if the subject got married before the age of 18, and zero otherwise. *Spousal age gap* is the difference in the ages of the individual and their spouse. *HH decision making* proxies the respondent's decision making power in the household; this is constructed by aggregating responses to five questions asking whether the subject could make decisions in each scenario in the household. It ranges from zero to five. All estimations use the triangular kernel. The optimal bandwidths are generated using the Imbens–Kalyanaraman approach outlined in Imbens and Kalyanaraman (2012), but results are largely the same if using a uniform bandwidth of four years for all regressions. All specifications include the following controls: gender, hukou status at age 3, ethnicity dummies and categorical variables for province of residence at age 12. Standard errors are clustered at province-$Cohort_i$ level. Data are based on CFPS 2012 and CFPS 2014.

among CSL beneficiaries, and they also reported having more decision making power in the household.[14] These findings suggest that marital outcomes can also be a potential channel for the CSL-induced improved mental health.

6. Conclusion

Depression is a critical mental health challenge, and is further exacerbated in developing country contexts where treatments are considerably harder to obtain due to several factors, including lack of adequate health infrastructure, shortages of medical staff (like psychiatrists and psychiatric nurses) and higher societal stigma. It is in this context, that we probe the impact of a compulsory schooling law on long run mental health outcomes in China. We find that CSL implementation led to enhanced educational outcomes among beneficiaries, and to improvements in mental health three decades after the schooling reform — based on a widely used clinical screening tool, we find that CSL beneficiary cohorts displayed lower levels of depressive symptoms and a reduced chance of being depressed.

We also explore the distributional effects of the policy and find that the top part of the outcome distribution (worse mental health) gained the most from the CSL policy. In particular, the effect size at the 90th percentile is around two times the mean treatment effect. Using methods described in Shen and Zhang (2016) we establish statistical significance of the impact differences for the top (worse mental health) and bottom (better mental health) halves of the outcome distribution. Thus the policy disproportionately benefitted the top tail of the mental illness distribution, which has important implications for policymakers.

We find that the mental health gains are plausibly mediated by improvements in physical health among CSL beneficiaries. This result complements previous findings of the non-pecuniary benefits of education, especially on physical health (Oreopoulos and Salvanes, 2011). We also find that labor market outcomes and marital outcomes are potentially important in explaining our findings.

Our analysis adds to the relatively small literature focusing on childhood and early adolescent circumstances and its effect on long run outcomes. Although the fetal origins literature focuses on early childhood environment and its effect on various outcomes later in life, early adolescent (ages 10 to 14) environment is also important in determining adult outcomes. According to the life cycle skill formation theory proposed by Cunha et al. (2010), investments made to remedy early disadvantage at later stages of childhood (e.g. ages 10–14) could be as cost-effective as doing them in early life (e.g. below age 5). This opens a window for intervention in the early adolescent period. In addition, the importance of early adolescent circumstances is further evidenced by studies that show cognitive and non-cognitive traits measured at around 14 years of age have significant impact on adult physical and mental health (Kaestner and Callison, 2011). Compulsory schooling, as an intervention during early adolescent period (or childhood), could serve as a cost effective measure for fostering adult long-run non-cognitive skills, including subjective well-being.

The conclusions that we reach from this analysis are subject to the caveat that the Chinese CSL policy improved educational outcomes among the compliers, who are individuals that would have otherwise dropped out of the schooling system before getting nine years of education. Thus the policy largely changed education for those at the relatively lower end of the education distribution. Therefore, the impact estimates are applicable to a specific sub-population — those individuals who in the absence of this policy might have dropped out before nine years of schooling. This may limit the external validity of the results from this study. Thus, it is an open question as to what the impact of education on mental health would be at other parts of the educational distribution. Would we observe similar benefits, or would they be smaller or larger in magnitude? Additionally, we only present evidence for one middle income country — it is unclear if such benefits would be seen in other contexts. These provide potential avenues for future research in this domain.

Mental health has been highlighted as a key area of reform in the United Nations' 2015–30 Sustainable Development Goals (Target 3.4). Despite this, investments in mental health have been limited, especially in low- and middle-income countries. Data from WHO's Mental Health Atlas (2014) suggests that these countries spend less than USD 2 per year per capita on treatment and prevention of mental disorders, compared with an average of around USD 50 per capita that is spent by high-income countries. This has created a substantial gap between the needs and the availability of treatment. Our findings suggest that in addition to direct investment in mental health services, indirect investments (like those in education) might play a key preventative role in the long run.

[14] Decision making is measured using responses to questions asking whether the individual was involved in making decisions related to the following items: household expenditure, savings and investment decisions, buying a house, educating the children, buying durable goods.

CRediT authorship contribution statement

Yanan Li: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Software, Validation, Visualization, Writing – original draft, Writing – review & editing. **Naveen Sunder:** Conceptualization, Formal analysis, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

Data availability

Data will be made available on request.

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Appendix A. Supplementary materials

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