The Average and Distributional Effects of Teenage Adversity on Long-Term Health

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Abstract

A central question in human development is what causes health inequalities over the life cycle. This paper links adversity in the teen years to individuals' long-term health outcomes. We examine a mandatory rustication program, the "send-down" policy during China’s Cultural Revolution, and employ Regression Discontinuity Design to estimate the impact on people’s physical and mental outcomes 40 years later. Our results suggest that rusticated youths were more likely to develop mental disorders but not to have worse physical outcomes. Further assessing distributional effects through marginal treatment effect (MTE), we find strong heterogeneous treatment effects and selection on gains.

Keywords: Adolescent environment; health; marginal treatment effects

JEL Classification: D63, I15, J13

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1 Introduction

A key question in human development is what causes health inequalities over the life cycle. A growing literature shows that conditions during gestation and early childhood explain health inequalities in prime-age adults, and that investment before age 5 has large payoffs for future health (Currie and Almond 2011; Heckman and Kautz 2014). In contrast, adolescent and adult programs have not been found to be as effective as programs that target earlier ages, partly due to participants’ selection into the program and the evaluations’ short-term follow-ups (Heckman and Mosso 2014).

In this paper, we exploit a large-scale, mandatory urban-to-rural movement in the 1970s to investigate whether and how teenage adversity affects long-term health. Specifically, employing survey data from 2010, we are able to examine the impact on affected individuals’ physical and mental health conditions 40 years after the experience for long-term follow-up. Meanwhile, by applying the marginal treatment effect (MTE) generalized by Heckman and Vytlacil (1999, 2005, 2007), we further examine how the teenage adversity effect differs across individuals’ observed and unobserved characteristics.

In December 1968, the then-leader of China, Mao Zedong, initiated a national movement to send 17 million junior and senior high school students in the cities to rural areas. Eligible urban youths were suddenly exiled to the countryside and experienced a dramatic change in living environment. They were estranged from their families for years, and were compelled to perform hard labor every day.

This unexpected and mandatory movement provides a regression discontinuity (RD) design to estimate the impact of teenage adversity on long-term health. Starting in 1968, the scheme applied to all eligible urban individuals who would graduate from junior or senior high school. The first sent-down cohort was the cutoff for being sent down: The cohort born just after September 1946 was forced to be rusticated, whereas the cohort born just before September 1946 was not subject to the scheme and therefore constitutes a good counterfactual. Both anecdotal evidence and quantitative exercises (i.e., the density check and balancing tests of predetermined characteristics) support the idea that individuals did not manipulate their birth timing to avoid being sent down, which is the key identifying
Using a range of physical and mental health indicators, we found that sent-down youths present worse mental health status: The likelihood of having mental disorders is about 1.09 standard deviations higher than their non-sent-down counterparts. On the other hand, the send-down experience has little impact on long-term physical health outcomes. These results are robust to a battery of robustness checks, including RD-DD estimators that control for cohort effect using rural population (i.e., that was not affected by send-down).

Our RD estimates identify the local average treatment effect (LATE), which gives the average treatment effect on the send-down compliers in the cutoff cohort. To assess send-down effects on populations other than the compliers and the distributional effects, we apply the framework of Brinch et al. (2017) and Kowalski (2016) to estimate the MTE. Our findings show strong treatment effect heterogeneity and selection on gains. For instance, youths with more siblings or from less revolutionary families are more likely to be sent down and experience smaller adverse effects on health outcomes. Selection on unobserved characteristics reinforces this finding: Youths with unobserved characteristics that tend to incur larger costs from being sent down also suffered more on health outcomes. Using MTE to calculate the average treatment effect on the treated (ATT) and average treatment effect (ATE), we consistently find adverse effects on mental health, while the impact on physical health is either close to zero or statistically not significant.

By examining the source of the treatment heterogeneity, we show that the larger health impact for high- versus low-resistance children is driven by worse outcomes in the treated state, whereas outcomes in the untreated state are mostly similar. Our findings also suggest that high-resistance youths are more likely to come from more disadvantaged backgrounds in which the parents are more likely to be illiterate or to work in less prestigious occupations.

The large effects on mental health are consistent with social-psychology theories regarding the teen years. Adolescence and early adulthood is a period of great mental plasticity, during which noncognitive and personality skills are formed, developed, and shaped by experience (Alwin and Krosnick 1991). Hardships in the countryside and separation from family are crucial to the youths’ mental conditions and long-term development. In addition to the contemporaneous shocks, the effects of this adversity may still be felt many years later—
our context, 40 years.

We also test the importance of various subsequent pathways—educational attainment, income, marriage, and childbearing—that may lead to long-term health outcomes. By estimating the effect of send-down on several indicators of socioeconomic status, we do not find evidence that the send-down experience affects the individual’s education attainment or labor market outcomes. It is unlikely, therefore, that the send-down’s impact on long-term health conditions is mostly driven by the post-send-down life events.

This paper contributes to the literature on the relationship between teenage conditions and later life well-being. Heckman (2000) argues that early investments in human capital for children have a large payoff. A large number of studies evaluate programs that target early childhood, such as policies to extend maternity leave (Tanaka 2005; Carneiro et al. 2010; Rasmussen 2010; Dustmann and Schönberg 2012); Head Start which provides health and other social services to poor children ages 3 to 5 (Currie and Thomas 1995; Garces et al. 2002; Ludwig and Miller 2007); and the Moving to Opportunities (MTO) program, which moves low-income families to better residential neighborhoods (Katz et al. 2001; Ludwig et al. 2001; Kling et al. 2005; Sanbonmatsu et al. 2006; Kling et al. 2007; Ludwig et al. 2011, 2012, 2013). Another line of literature examines exogenous conditions and unusual shocks, such as pollution or disease breakouts at the fetal stage (Currie (2011), Almond 2006);\(^1\) environmental factors and economic circumstance at birth (Maccini and Yang 2009; Fenske et al. 2014); the loss of a parent (Adda et al. 2011); extreme drought and civil war (Alderman et al. 2006) and famine (Meng and Qian 2009).

Our study has several additional features that distinguish it from previous research. First, evaluations of programs that target teenage usually follow participants for no longer than 20 years. Short-term follow-up could lead to biased estimates of returns—upward-biased if the benefits eventually dissipate or downward-biased if the effects take place later in life (Heckman and Mosso 2014). In contrast, we examine long-term outcomes, thereby revealing the long-lasting impact of teenage conditions. To study the impact of early intervention over the life cycle, Gould et al. (2011) examine Operation Magic Carpet in which Yemenite

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\(^1\)Almond and Currie (2011) survey papers on the fetal origins hypothesis and discuss a broad range of fetal shocks and circumstances that have been found to have later-life impacts.
children were airlifted to Israel. Their study is close to ours in terms of the long-term nature of the effects, but we further innovate by employing an identification strategy closer to a random experiment. Second, we move beyond the commonly discussed LATE and estimate ATT and ATE from MTE of the send-down movement. This allows us to examine distributional effects and understand the external validity of the estimated treatment effects.

This paper is also related to the literature on the origins of health inequalities. In addition to the studies of early-life conditions (Currie et al. 2010), one strand of literature focuses on how socioeconomic status and conditions affect non-cognitive development and health conditions. Adams et al. (2003) and Adda et al. (2003) find that socioeconomic status appears to have stronger links with mental and chronic illnesses than with acute and sudden-onset health conditions. For more specific causes, Adda et al. (2009), Fiorini and Keane (2013) and Cornaglia et al. (2014) identify the effect on health of income shocks, children’s activities, and crime, respectively. There is also a well-established correlation between health and education (Grossman 2006; Cutler and Lleras-Muney 2010; Clark and Royer 2013).²

The remainder of the paper is organized as follows. Section 2 briefly describes the send-down movement. Section 3 describes the data and main variables. Section 4 presents RD and MTE frameworks and discusses estimation particulars. Section 5 reports RD estimates of the send-down effect, MTE estimates and treatment effect heterogeneity. Section 6 interprets the results. Section 7 concludes.

## 2 Research Background

### 2.1 The Send-Down Movement

The “Up to the Mountain and Down to the Countryside Movement” (also called the send-down movement) in China was a massive movement of educated youths who left their urban homes to live and work in rural areas. Beginning in the 1950s, as a policy response to urban employment and rural development problems, it evolved into a political movement during

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² Clark and Royer (2013) find little causal effect on health, and suggest caution regarding the likely health returns on educational interventions.
the Cultural Revolution and affected millions of urban youths until it ended in the late 1970s.

A small-scale send-down movement started in the early 1950s, following Mao Zedong’s rallying cry to develop remote regions. In 1955, in an attempt to motivate the urban unemployed to relocate in rural areas, Mao stated that “the countryside is a vast expanse of heaven and earth where we can flourish.” The early phase of the send-down movement was mostly voluntary.

On December 22, 1968, Mao suddenly asserted that “intellectual youth must go to the countryside, and will be educated from living in rural poverty,” and called for a nationwide mandatory movement of urban youth to the countryside. This 1968 directive marked the official beginning of the mandatory and large-scale send-down.

The policy came as a shock, forcing millions of youths out of the cities and exiling them to the countryside and remote regions. Specifically, the mandatory policy launched in 1968 applied to individuals who were registered as urban residents and graduating from junior or senior high school. As junior and senior high schools had been closed for much of the first two years of the Cultural Revolution, six cohorts of graduates (i.e., 1966-1968 cohorts of junior and senior high school graduates) were sent down together in 1968.

Though some youths were inspired by the revolutionary and patriotic propaganda, most did not want to be separated from their families or to give up the better living conditions and work opportunities in urban areas. Many families with eligible youths were forced, under political pressure, to cooperate; parents were often threatened with job loss. One sent-down individual recounted his experience:

I was only 15 when I was sent down. No one wanted to go, but no one could resist. When I refused to go, those in charge of the residential committee came to our home every day and asked us to study Chairman Mao’s instructions. A member of the worker’s propaganda team came to live in our home and organized a study team for my family. My father was a cadre. He was locked up in a study team in his workplace and was not allowed to return home until his children agreed to go to the rural area. In the end, my mother begged me to go to the rural area. (Deng, 1993, p. 60)
In 1977, the government relaxed enforcement and brought some youths back to work in the urban labor force or enter college\textsuperscript{3}. By 1979, Mao’s successors had denounced the send-down policy and allowed all affected youths to return to their home regions. From 1968 to 1978, roughly 17 million people, or 10.5\% of the non-farming population at that time, were sent down to rural areas (Pan 2002).

2.2 Lives of the Sent Down Youths

In the 1960s and 1970s, there was a wide gap in living environments between urban and rural areas in China due to the Big Push Development Strategy adopted in the 1950s (Naughton 2006). To the sent-down youths, moving to a rural area amounted to poverty on both material and spiritual levels. They encountered difficulties in adapting to the lower standard of living and poor sanitation, strenuous physical labor, lack of cultural and spiritual activities, and separation from their families.

A basic problem for the urban youths was to adapt to a lower standard of living—for instance, to live without electricity or running water. In the midwest, where rainfall was scarce, they had to travel miles to fetch water in buckets to maintain a minimum level of hygiene. Their rural diet mainly consisted of coarse grain and corn; at the time, vegetables were expensive on the free market and meat was difficult to find (Bonnin and Horko 2013). Harmful insects, such as mosquitos and whitmania pigra, were widespread in the countryside.

Like the peasants, the youths devoted most daytime hours to agricultural labor, which at the time was rarely mechanized. They performed hard manual work for 10 hours per day, and in harvest months, almost 16 hours. They were paid by “efficiency units” and could barely remain self-sufficient. A teacher wrote to Mao about his son’s situation:

My son graduated from middle school in 1968 and went to countryside in 1969… In mountain areas, he did agricultural labor for the whole year, but obtained not enough food to eat, nor one cent of income… when he was sick, he cannot even afford for seeing the doctor. (Gu and Hu, 1996, p. 116-117)

\textsuperscript{3}After Mao’s death in September 1976, it became clear that the Cultural Revolution would end and the enforcement of send-down was much relaxed. Furthermore, college admissions were reinstated in 1977, and high school graduates in 1977 were allowed to enter universities.
The monotony of life and the lack of cultural activities was also a huge change from life in the city. Only limited social activities or entertainment were available after work, and reading and writing were difficult by the light of an oil lamp and without a table. Most inhumanely, the experience amounted to deportation from their families and homes. Some of the teenagers were sent to remote areas or border regions, and were not allowed to visit their families for several years.

Cao [a sent down woman from Shanghai] is also tormented by the thought that she may have increased her parents’ suffering. Like Ma’s [another women sent down to the same village] family, Cao’s mother was attacked in the Cultural Revolution. She died soon after Cao’s return home. “I keep thinking I could have taken care of her if I’d been there. She might have lived longer,” she says. (Hille 2013)

3 Data and Variables

Data. Our primary data source is the China Family Panel Studies (CFPS) 2010, a nationally representative sample of Chinese communities, families, and individuals, that covers 25 of 31 provinces/regions (the six omitted provinces are Hainan, Inner Mongolia, Ninxia, Qinghai, Tibet and Xinjiang) and 95% of the total population of China in 2010. Sampling for the 2010 CFPS was drawn with implicit stratification through a multistage probability. Specifically, five provinces/regions (Gansu, Guangdong, Henan, Liaoning, and Shanghai) were chosen for initial oversampling (1,600 households in each, for a total of 8,000) to achieve regional comparisons, and another 8000 households were drawn through weighting from the other provinces/regions to make the overall CFPS sample representative of the country. The final sample covered 15,717 households and 33,600 adult respondents in 2010.

The 2010 CFPS consisted of four questionnaires (Community, Family, Adolescent, and Adult), which included most questions covered in four U.S. counterpart datasets (PSID, CDS, HRS, and NYLS). The dataset contains rich information on demographic and socioeconomic characteristics, such as gender, date of birth (month and year), ethnicity, marital status,
educational attainment, family background, registered residency (or hukou in Chinese), type of residency (rural or urban), employment status, etc.

**Health outcomes.** Most relevant to our study, the 2010 CFPS asked respondents multiple questions about their physical and mental health status. Four questions can be directly linked to an individual’s physical health conditions, from which we construct four 1/0 binary outcome variables that reflect the respondent’s physical health status. The first measure, *Abnormal BMI*, indicates whether an individual is underweight (BMI<19.5) or overweight (BMI>25). The second, *Chronic*, takes a value of 1 if the respondent answered “yes” to the survey question “During the past six months, have you had any doctor-diagnosed chronic disease?” and 0 otherwise. The third, *Hospitalized*, takes a value of 1 if the respondent answered “yes” to the question “Were you hospitalized last year due to illness/injury?” and 0 otherwise. The last variable, denoted *Uncomfortable*, takes a value of 1 if the respondent answered “yes” to the question “During the past two weeks, have you felt physically uncomfortable?” and 0 otherwise.

The 2010 CFPS asked six questions related to mental health conditions, all of which belong to the the 10-question Kessler Psychological Distress Scale (K10). Respondents were asked to rate, on a scale from 1 to 5, the frequency of certain symptoms. We construct six variables accordingly, all of which take values from 1 to 5, with 1 meaning never and 5 meaning almost every day: *Depressed, Nervous, Restless, Hopeless, Difficult,* and *Worthless.* Specifically: (1) *Depressed*: “How often did you feel depressed and cannot cheer up in the past month?”; (2) *Nervous*: “How often did you feel nervous in the past month?”; (3) *Restless*: “How often did you feel agitated or upset and could not remain calm in the past month?”; (4) *Hopeless*: “How often did you feel hopeless in the past month?”; (5) *Difficult*: “How often did you find it difficult to do everything in the past month?”; and (6) *Worthless*: “How often do you think life is meaningless?”

In Table 1, we list the health measures for the two categories (i.e., physical and mental health) and their corresponding survey questions. To capture the overall effect of teenage

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4 Results using only overweight or underweight are similar (available upon request).
5 The K10 was developed by Kessler and Mroczek in 1992 and has been widely adopted to measure anxiety-depression spectrum mental distress (Kessler et al. 2002).
adversity on long-term health, we construct a physical health index and a mental health index as our main outcome variables. Specifically, our index construction follows the one used by David et al. (2003): We first use principal component analysis to create an index, and $z$-standardize it with a higher score to indicate unhealthier conditions. By aggregating the health measures to a summary index, we also improve statistical power to detect effects that move in the same direction (Kling et al. 2007).

While our health outcomes are constructed based on self-reported responses, one could be concerned with the possible measurement error, especially when reporting errors are different across our treatment and control groups. Several threads of evidence suggest that this is less applicable to our setting. First, we essentially compare outcomes between two birth cohorts on the margin (i.e., the cohort born in September 1946 vs. the cohort born in August 1946). No prior theories argue that these two birth cohorts should report differently, especially since our construction of birth cohorts is based on grade (August versus September of the same year) rather than calendar year. Second, the CFPS is designed to “collect individual-, family- and community-level longitudinal data in contemporary China,” instead of specifically targeting the send-down experience. Hence, respondents should not be influenced by survey objectives in their replies to questions. Third, answers to the more factual questions suggest that measurement errors are limited. For example, to follow up on the main question under Chronic (“During the past six months, have you had any doctor-diagnosed chronic disease?”), there is a subquestion for respondents who answered “yes”: “For each of two main chronic diseases, when was the chronic disease diagnosed by the doctor?” We check the correlation between the response to the subquestion (i.e., whether the respondent replied to the subquestion) and Chronic, and find a correlation of 0.9051. Given that the subquestion requires detailed information on chronic diseases, the high correlation reduces concern about reporting errors.

*Send-down status.* The CFPS contains information on whether the person experienced

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6http://www.isss.edu.cn/cfps/EN/About/Introduction/
the send-down movement and his/her place of registered residence (hukou) at various ages. We use hukou status at the age of 12 to identify urban youths, assuming that during junior and senior high school the person lived in the region he or she lived in at 12. One concern is that people could have moved from urban to rural areas during that period, and thus avoided being sent down. However, in the 1960s and 1970s, the government strictly regulated urban-to-rural migration, and hukou status could not be manipulated (Naughton 2006).

Regression sample. In the empirical analysis, we restrict our analysis to individuals born between 1930 and 1958. This is because cohorts born before 1930 (i.e., citizens who were in their 80s when the survey was conducted) have very few observations in our data and could suffer from selection bias; those born from 1959 to 1961 experienced the three-year Great Famine (1959-1961) in China, which could also affect long-term health outcomes; and those born after 1961 were not eligible for the mandatory send-down movement. The remaining sample contains 11,933 individuals, including 1,916 urban individuals (with urban hukou at age 12) and 10,017 rural individuals (with rural hukou at age 12). The send-down ratio is about 30.8% for urban individuals and 1.2% for rural individuals. Descriptive statistics for our sample are presented in Appendix Table A1.

4 Estimation Strategy

4.1 RD Framework

The send-down policy moved millions of teenagers from urban to rural areas and turned their early adulthood upside down. The unexpected launch of the mandatory movement in December 1968 provides us with some randomness to identify the effect of teenage adversity on long-term health outcomes. Specifically, we use an RD framework, which is arguably the closest in the observational data analysis to an experimental design (e.g., Lee and Lemieux 2010).

As an illustration of the RD framework, consider the following Rubin causal model: Let

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7Since the college entrance examination was reinstated in 1977, the last cohort affected by the mandatory policy consisted of those who had graduated from junior high school in 1976, i.e., those born between June 1960 and August 1960.
\( Y_{i1} \) be the outcome (i.e., measures of physical and mental health status; see Section 3 for details) of individual \( i \) being sent down to the countryside and hence experiencing great hardship in his/her teen years; let \( Y_{i0} \) be the outcome in the absence of send-down; and denote \( D_i \) as send-down status, i.e., 1 if individual \( i \) was sent down and 0 otherwise. The effect of send-down is identified as

\[
\gamma = E \left[ Y_{i1} - Y_{i0} \right].
\]

(1)

However, as we cannot observe for individual \( i \) both her \( Y_{i1} \) and \( Y_{i0} \), the comparison of outcomes between the send-down group (i.e., \( D_i = 1 \)) and the non-send-down group (i.e., \( D_i = 0 \)) could be biased due to the selection issue, i.e., \( E \left[ Y_{i0} | D_i = 1 \right] \neq E \left[ Y_{i0} | D_i = 0 \right] \).

The sudden and mandatory send-down movement in 1968 implies that the probability of being sent to a rural area is discontinuous at cutoff point \( c_0 \) of the birth cohort (\( c_i \)), and the probability jumps by less than one because of imperfect compliance, i.e., \( \lim_{c_i \downarrow c_0} E \left[ D_i | c_i = c \right] \neq \lim_{c_i \uparrow c_0} E \left[ D_i | c_i = c \right] \). Assuming \( E \left[ Y_{i0} | c_i = c \right] \) is continuous in \( c \) at \( c_0 \) (later we will formally check this key identifying assumption), Hahn et al. (2001) show that \( \gamma \) can be identified as

\[
\gamma = \frac{\lim_{c_i \downarrow c_0} E \left[ Y_i | c_i = c \right] - \lim_{c_i \uparrow c_0} E \left[ Y_i | c_i = c \right]}{\lim_{c_i \downarrow c_0} E \left[ D_i | c_i = c \right] - \lim_{c_i \uparrow c_0} E \left[ D_i | c_i = c \right]} = \hat{\gamma}_{RD}.
\]

(2)

Lee and Lemieux (2010) show that the RD estimator (2) is essentially an instrumental variable estimator, send-down status \( D \) is instrumented by \( T = I \left[ c_i \geq c_0 \right] \), \( I \left[ \cdot \right] \) is an indicator function that takes a value of 1 if the argument in the bracket is true and 0 if it is false), and the treatment effect can be recovered by dividing the jump in the relationship between \( Y \) and \( c \) at \( c_0 \) by the fraction induced to take-up the treatment at the threshold. Specifically, the first stage of the instrumental variable estimation has the following specification

\[
D_i = \alpha T_i + g \left( \tilde{c}_i \right) + \mu_i,
\]

(3)

and the reduced-form is

\[
Y_i = \beta T_i + f \left( \tilde{c}_i \right) + \varepsilon_i,
\]

(4)
where \( \tilde{c}_i = c_i - c_0 \), and \( g(.) \) and \( f(.) \) are flexible functions of \( c_i \), which controls for the direct effect of birth cohort on outcome variables. Hence, using two-stage least-squares (TSLS) in this setting, the RD estimator is \( \hat{\gamma}_{RD} = \frac{\hat{\beta}}{\hat{\alpha}} \).

4.2 MTE Framework

Imprecise control over the assignment variable (i.e., the timing of birth) guarantees local random assignment, and thus the internal validity of RD estimates. However, due to imperfect compliance, the fuzzy RD design identifies the LATE; that is, the average treatment effect for compliers at the cutoff point. To understand treatment effects on other populations, such as ATT, ATUT and ATE, and the distributional effect, we apply the MTE framework introduced by Björklund and Moffitt (1987) and generalized by Heckman and Vytlacil (1999, 2005, 2007).

Specifically, the MTE framework relates heterogeneity in the treatment effect to observed and unobserved heterogeneity in the propensity for being sent down, and allows us to move beyond the LATE. Heckman and Vytlacil (1999, 2005, 2007) develop approaches to identify the MTE in settings with continuous instruments, under the standard IV assumptions of conditional independence and monotonicity. Recently, Brinch et al. (2017) provide approaches to estimate MTE in settings with discrete or binary instruments, whereas Kowalski (2016) applies the MTE method to examine treatment effect heterogeneity in experiments with binary interventions. As the send-down experience is a binary treatment and our instrument is binary, we follow the framework of Brinch et al. (2017) and Kowalski (2016) in estimating MTE.

For simplicity, we suppress \( i \) index in our exposition of the MTE framework. We specify potential outcomes as

\[
Y_1 = \mu_1(X) + U_1 \tag{5}
\]

\[
Y_0 = \mu_0(X) + U_0,
\]

where \( \mu_1(\cdot) \) and \( \mu_0(\cdot) \) are general functions; and \( X \) is a vector of covariates. \( U_1 \) and \( U_0 \) are random variables that are normalized such that \( E(U_1|X = x) = E(U_0|X = x) = 0 \).
$E(U_j^2 | X = x)$ is assumed to exist for $j = \{0, 1\}$ for all $x$ in the support of $X$, but no independence between $X$ and $(U_0, U_1)$ is required.

The net benefit of treatment, $I_D$, consists of an observed component $\mu_D(Z)$ and an unobserved component $U_D$:

$$I_D = \mu_D(Z) - U_D$$  \hfill (6)

where $U_D$ is a continuous random variable with a strictly increasing distribution function, capturing the costs of treatment participation; and $\mu_D(\cdot)$ is a (unspecified) function of $Z = (X; Z_-)$. $Z$ includes the same covariates as the outcome equations in (5) and an instrument $Z_-$ excluded from the outcome equations. Specifically, in our setting, $Z_- = T$ is an indicator of being born at or after the cutoff point. We further assume that $(U_0, U_1, U_D)$ is independent of $Z$, conditional on $X$.

An individual selects into the treatment iff the net benefit is non-negative, i.e.,

$$D = I[I_D \geq 0] \Leftrightarrow \mu_D(Z) \geq U_D.$$  \hfill (7)

Define $P(Z) \equiv Pr(D = 1 | Z) = F_{U_D}(\mu_D(Z))$, where $F_{U_D}(\cdot)$ is the cumulative distribution function of $U_D$. As the quartiles of any distribution are distributed uniformly between 0 and 1, we normalize $U_D \sim U(0; 1)$, and therefore, $P(Z)$ is a uniformly distributed propensity score. At a given propensity score $P(Z) = p^*$, an individual selects into treatment if $U_D < p^*$, while individuals with $p^* = U_D$ are indifferent between participating in the treatment or not.

The MTE is defined as the average treatment effect for individuals with covariate values $X = x$ and an unobserved cost of treatment $U_D = p$, i.e.,

$$MTE(x; p) = E(Y_1 - Y_0 | X = x, U_D = p),$$  \hfill (8)

Following Brinch et al. (2017), we assume the additively separability property; that is, $E(Y_j | X = x, U_D = p) = \mu_j(X) + E(U_j | U_D)$, for $j = \{0, 1\}$. Equation (5) implies that

$$MTE(x, p) = \mu_1(X) - \mu_0(X) + E(U_1 - U_0 | U_D = p)$$
So for the MTE curve as a function of $p$, its intercept is affected by the treatment effect heterogeneity resulting from observed characteristics $X$, but its slope does not depend on $X$.

Following Heckman and Vytlacil (1999, 2005, 2007), the LATE can be calculated as an integral over the MTE function. In particular, for a given binary instrument ($Z$) that shifts the propensity score from $p_B = Pr(D = 1|X = x; Z_− = 0)$ to $p_I = Pr(D = 1|X = x; Z_− = 1)$, the LATE is

\[
LATE(x) = \frac{\int_{p_B}^{p_I} \text{MTE}(x, p) \, dp.}
\]

Intuitively, an individual is treated if her cost of treatment $U_D$ is less than or equal to her potential benefits (represented by $p$). The sent-down individuals in the control group ($D = 1$ and $Z_− = 0$) are those with low costs of being sent down, $0 < U_D < p_B$, i.e., always takers. On the other hand, the non-sent-down people in the treatment group ($D = 0$ and $Z_− = 1$) have high costs of treatment, $0 < p_I < U_D$, and they are never takers. The remaining individuals, who have $p_B < U_D < p_I$, are compliers and select into the treatment according to random assignment, and their average treatment effect is the LATE.

Similar to calculation of the LATE, other conventional treatment effect parameters, including the ATE, ATT and ATUT, can be recovered by appropriately aggregating over the MTE curve, using the weights provided in Appendix B following Kowalski (2016).

### 4.3 Estimation Particulars

In this subsection, we provide details of our RD estimation and MTE estimation. In particular, for RD estimation, we describe how we construct assignment variable (birth cohorts), define cutoff points, select polynomial orders, and calculate standard errors; for MTE estimation, we introduce a separate estimation approach.
4.3.1 RD Estimation

**Assignment variable—birth cohorts.** The assignment variable in our RD estimation is a grade-based birth cohort. Following the system used in the former Soviet Union, schools in China start the academic year in September. The oldest students in a grade were born in September, and the youngest in August of the following year.\(^8\)

We sort students into bins of three birth months. The first bin contains students born between September and November, the second between December and February, the third between March and May, and the fourth between June and August. The assignment variable in our RD estimation, birth cohort \((c)\), is therefore a quarterly variable.\(^9\) For instance, the first youths affected were those who graduated from high school in 1966, and therefore those born between June and August 1946, were not affected by the mandate; those born between September and November 1946, were involuntarily sent down.

**Cutoff points.** As described previously, the send-down movement, which was launched in December 1968, required that junior and senior high school students go to the countryside. Because middle schools were closed for much of the first two years of the Cultural Revolution (1966 to 1968), the first youths affected were those who had graduated from senior high school in 1966. From the 1950s to the 1980s, children started school at the age of 7 and completed the primary grades in 6 years and junior and senior high school in 3 years each.\(^10\) Therefore, the first cohort \(c_0\) affected by the mandatory send-down policy were those born between

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\(^8\)Regarding school entry date, we are aware of studies that use different dates (e.g., Zhang 2014). Sources suggest that although classes were interrupted at the beginning of the Cultural Revolution and then resumed after the 1967 Spring Festival, there were no changes in the admission and entry dates for the fall semester. In particular, documents show that: (1) middle schools started the academic year on September 1 during the 1950s (Major Educational Events in the People’s Republic of China: Secondary Education, pp.62-63); (2) in 1966 (i.e., the beginning of the Cultural Revolution), the Ministry of Education announced that fall admission would continue as usual (Major Educational Events in the People’s Republic of China: 1949-1982, pp.403), but teaching activities were largely interrupted by revolutionary events within the schools; (3) in 1967, the Ministry of Education directed all schools to resume classes after the Spring Festival, and to prepare for admissions for the fall semester (same source as above, p. 415); and (4) in 1968, the government announced that the graduating class would graduate in July and that schools would have summer breaks as usual (same source as above, p. 419).

\(^9\)We also use birth year-month as the assignment variable and obtain similar estimated results, but standard errors are larger due to insufficient observations for each bin.

\(^10\)In 1951, the Government Administration Council issued the "Decision on the Education System Reform," which sets the age at which children start primary school to be 7 (see [http://xuewen.cnki.net/R2006090880000764.html](http://xuewen.cnki.net/R2006090880000764.html)).
September 1946 and November 1946.

*Polynomial functions.* As our assignment variable (birth cohort) is discrete, Lee and Card (2008) point out that one cannot use nonparametric estimation, even on data of infinite observations. Following their suggestion, we use a lower-order polynomial function, with various polynomial orders and with or without varying slopes across the cutoff point $c_0$. As a robustness check, we also calculate $\hat{\gamma}_{RD}$ using a nonparametric approach. Specifically, we use local linear regression, as suggested by Hahn et al. (2001).

*Standard errors.* Following the tradition in the literature (see Lee and Lemieux (2010)), we compute clustered standard errors at the assignment-variable level (i.e., birth cohort level), which allows us to capture random sampling errors and obtain conservative statistical inference.

### 4.3.2 Estimation of MTE

There are two approaches to estimate the MTE, i.e., local instrumental variable (LIV) estimation and separate estimation. As our instrument is binary, we adopt the separate estimation, which allows identification of richer MTE specifications when the instrument is discrete (Brinch et al. 2017).

As in Heckman et al. (2006) and Carneiro et al. (2011), we specify the average treated and untreated outcome functions as follows

\[
ATO (x, p) = E (Y_1 | X = x, D = 1) \\
= \mu_1 (X) + E (U_1 | U_D \leq p) \\
= X \beta_1 + K_1 (p)
\]  

(9)

and

\[
AUO (x, p) = E (Y_0 | X = x, D = 0) \\
= \mu_0 (X) + E (U_1 | U_D > p) \\
= X \beta_0 + K_0 (p)
\]  

(10)
The marginal treatment effect (MTE) is the difference between the marginal treated outcome (MTO) and the marginal untreated outcome (MUO):

\[ MTO(x, p) = E(Y_1|X = x, U_D = p) \]
\[ MUO(x, p) = E(Y_0|X = x, U_D = p) \]

and

\[ MTE(x, p) = MTO(x, p) - MUO(x, p) = X(\beta_1 - \beta_0) + k_1(p) - k_0(p), \]

where \( k_j(p) = E(U_j|U_D = p) \) can be obtained from \( K_j(p), j = \{0, 1\} \):

\[ K_1(p) = E(U_1|U_D \leq p) = \frac{1}{p} \int_0^p E(U_1|U_D = t) \, dt = \frac{1}{p} \int_0^p k_1(t) \, dt \]
\[ K_0(p) = E(U_1|U_D > p) = \frac{1}{(1 - p)} \int_p^1 E(U_0|U_D = t) \, dt = \frac{1}{(1 - p)} \int_p^1 k_0(t) \, dt. \]

Our MTE estimation consists three steps. First, we regress treatment indicator \( D \) on the instrument \( Z_- \) along with all covariates \( X \) in a probit model, and predict a propensity score \( \hat{p}_x \equiv P(D = 1|Z_-, X) \) for each individual.\(^{11}\) Second, we estimate the average treated outcome function \( ATO(x, p) \) by regressing outcome \( Y \) on the covariates \( X \) and a first-order polynomial of \( \hat{p}_x \) using the sample of sent down individuals (i.e., those with \( D = 1 \)).\(^{12}\) Similarly, we estimate the average untreated outcome function \( AUO(x, p) \) using the sample of non-sent-down individuals (i.e., those with \( D = 0 \)). Third, we construct estimates of the marginal treated outcome \( MTO(x, p) \) and marginal untreated outcome \( MUO(x, p) \) based on the functional forms and coefficients from the previous step, and calculate \( MTE(x, p) = \)

\(^{11}\)Specifically, \( X \) includes the baseline covariates \((\tilde{c}_i, \tilde{c}_i^2, T_i\tilde{c}_i, T_i\tilde{c}_i^2, Q_1 - Q_4)\) and all predetermined individual characteristics used in the validity checks of RD design (see Appendix Table A2). Also, using the logit model or the linear probability model generates similar results.

\(^{12}\)Results are similar with second-order polynomials.
\[ MTO(x, p) - MUO(x, p). \]

4.4 Potential Manipulation

Our key identifying assumption in RD design is that \( E[Y_i|c_i = c] \) is continuous in \( c \) at \( c_0 \); in other words, people cannot fully manipulate the assignment variable, i.e., the timing of births.

Anecdotal evidence suggests that our identifying assumption is satisfied. First, households would not reasonably have been able to foresee the benefits or costs of birth timing decades before the Cultural Revolution was launched. The cohort on the margin contains people born in 1946, the period of the Chinese Civil War (between the Kuomintang and the Communist Party). No one at that time could predict that the Chinese Communist Party would win the war in 1949 and establish a new government, nor that roughly 20 years later, Mao would launch a large-scale send-down movement. Indeed, it is well documented that the mandatory policy came as a shock to most people (Bernstein 1977; Li et al. 2010). Second, it is difficult to manipulate the timing of childbirth, as cesarean sections were not widely available at the time. Further, our assignment variable, birth cohort, is based on the school opening month. However, there was no fixed date for school opening in the 1930s and 1940s in China; hence, it is unlikely that people would manipulate their children’s birth months to allow them to enter school earlier or later.

To further support our identifying assumption, we provide two sets of quantitative analyses suggested by Lee and Lemieux (2010). First, if there is no strategic timing of birth, we will not find discontinuity in the density of the birth cohort at the cutoff point \( c_0 \). Figure 1 reports the histogram of the birth cohort. We also formally test the significance of the discontinuity using the number of observations in each cohort as the dependent variable in our RD estimation, and plot the fitted lines and present the estimated jump in Figure 1. Clearly, there is no statistically and economically significant discontinuity in the density of the birth cohort at September 1946 (the cutoff point)

[Insert Figure 1 here]
However, a concern about this density check is that our data come from a survey conducted in 2010, when the relevant cohorts were in their 60s. If the probability of surviving to 2010 is affected by the send-down experience and changes discontinuously at the cutoff point, this differential mortality rate might cancel out the manipulation of childbirth timing, and we would not find any discontinuity in the observed density of birth cohort in Figure 1. To check this possibility, we report in Appendix Figures A1-A3 the histogram of birth cohorts using China’s population censuses in 1982, 1990, and 2000\textsuperscript{13}, when the cohorts on the margin were, respectively, in their 30s, 40s, and 50s—ages at which the mortality rate is relatively low. It is clear that none of these figures displays any discontinuity at our cutoff cohort. Combined, these results suggest that there is no sample selection issue due to differential mortality rates across cohorts on the margin and no evidence for the manipulation of birth timing.

A second check is to directly examine whether individuals’ predetermined socioeconomic characteristics are smooth at the cutoff point. If there had been full manipulation of birth timing, we would find discontinuities in these predetermined characteristics at the cutoff point. To this end, we go through 11 predetermined individual variables that can be identified in the data—gender, ethnicity, urban or rural status at the age of 3, number of siblings, political identity measured by family’s revolutionary level during the the Cultural Revolution\textsuperscript{14}, whether lived in birthplace at age 3 and age 12 (2 variables), whether had lived away from father by age 3 and when the child was 4-12 years old (2 variables), and whether had lived away from mother by age 3 and when the child was 4-12 years old (2 variables). First, we test whether these predetermined variables are jointly smooth at the cutoff point. Specifically, we examine the predicted probability of being sent down, calculated as the fitted value from an OLS regression of send-down indicators on all predetermined covariates along with birth quarter dummies. Figures 2 plots the predicted probability against birth cohorts for the RD analysis, showing no particular jump or drop around the cutoff point. Second, we examine the smoothness of these predetermined variables separately. Appendix Table A2

\textsuperscript{13}We use the household type (agricultural or non-agricultural) in the 1982 and 1990 Chinese census and registration status (agricultural or non-agricultural) in the 2000 Chinese census to break the population into rural and urban, and draw the histogram for the urban population.

\textsuperscript{14}I.e., 3 for revolutionary class, 2 for middle class, 1 for class enemies.
reports RD regression results. For all the predetermined socioeconomic characteristics, we do not find any statistically and economically significant discontinuities at the cutoff points.

[Insert Figure 2 here]

In summary, our exercises in this subsection suggest that there was no full manipulation of the assignment variable related to the mandatory send-down movement, which implies that our RD estimation strategy is internally valid.

5 Main Results

5.1 First Stage: Send-Down Probability and Birth Cohorts

Figures 3a and 3b show the relation between the send-down experience (the regressor of interest) and birth cohort (the assignment variable) for urban and rural samples, respectively. The figures plot average send-down ratio for every 4 birth quarters (bins) and the fitted values from the quadratic regression. Clearly, for the urban sample (Figure 3a), send-down probability jumps at the cutoff cohort, whereas for the rural sample (Figure 3b), the probability is close to zero for all birth cohorts, and there is no discontinuity at any cohort.

[Insert Figure 3a and 3b here]

Table 2 reports RD estimates of first-stage results, i.e., the effects of being born at and after September 1946 on send-down participation. All regressions control for flexible second-order polynomial functions of assignment variable \( \tilde{c} \) and four quarter dummies. Results suggest that mandatory enforcement significantly increased the probability of being sent down, consistent with the pattern shown in Figure 3a. The magnitude is about 17.5 percentage points, which is enormous given that the average send-down probability for the urban group is 31%.

[Insert Table 2 here]

Together, graphical and regression results suggest that there is a discontinuity of send-
down probability for cohorts on the margin, which confirms the validity of our research design.

5.2 The LATE: RD Estimates

Table 3 reports RD estimates of send-down effects on long-term physical (column 1) and mental (column 2) health outcomes. We include the same set of control variables as in Table 2.

[Insert Table 3 here]

Results show that the send-down experience has limited effects on physical health, but significantly adverse effects on mental health conditions. Specifically, the RD estimate in Table 3, column 1 suggests that the send-down experience reduces the level of unhealthy physical symptoms by around 0.07 standard deviation, but the effect is not statistically insignificant. For mental health, the RD estimate in column 2 suggests that send-down increases the likelihood of having mental disorders by approximately 1.09 standard deviation, the magnitude of which is statistically and economically significant.

Figures 4a and 4b plot physical and mental health indices by birth cohort using the urban sample, without controlling for quarterly dummies. We find no significant change in physical health, but a clear discontinuity in mental health at the cutoff point, consistent with the RD estimates.

[Insert Figures 4a and 4b here]

Robustness Checks. Our findings are robust to a battery of robustness checks, including

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15 Appendix Table A3 reports the send-down effect on individual indicators of physical health. Results show that send-down renders the individuals less likely to have abnormal BMI values and physically uncomfortable feelings during the past two weeks and more likely to have chronic disease and hospitalization experience in the previous year, and the latter two are marginally significant.

16 Appendix Table A4 reports estimates on individual measures of mental health. Effects are similar to the summary index: Sent-down individuals are significantly more likely to feel nervous and agitated in the past month, to feel hopeless about the future, and to feel that everything is difficult and life is meaningless.

17 As a complement to the summary indices, we also present similar figures for individual measures (components of the index). Consistent with the regression results in Appendix Tables A4 and A5, the figures show that while the send-down experience renders individuals more likely to have mental disorders, the effect on physical health is mixed and relatively small.

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using alternative polynomial orders, calculating RD estimates using a nonparametric approach, controlling for cohort effects with the rural sample in an RD-DD estimation, and calculating the intention-to-treat effect using Chinese Census 2005. Results remain similar to the baseline RD estimates. Details of robustness checks are provided in Appendix A.

5.3 MTE Estimates and Treatment Effect Heterogeneity in Unobservables

Our RD estimates identify the LATE for send-down compliers in the cutoff cohort. To assess treatment effects on other subpopulations and the distributional effect, we apply the framework from Brinch et al. (2017) and Kowalski (2016) to estimate the MTE.

We first estimate first-stage probit selection equation (7), and, for interpretation, report the marginal effects in Table 4, column 1. Clearly, our instrument is a strong predictor of the treatment (rustication). There are other noteworthy features of selection into treatment. For instance, individuals with one more sibling were 2 percentage points more likely to be sent down, and those from more revolutionary families were less likely to be sent down. Children who had lived away from the father between ages 0-3 were around 10.8 percentage points more likely to be sent down.

Figures 5a and 5b plot the MTE as a function of \( p \) measured in percentiles for physical and mental health, respectively.\(^{18}\) On the horizontal axis, we depict the baseline treatment probability \( p_B \equiv P(D = 1 | E = 0) \) and the intervention treatment probability \( p_I \equiv P(D = 1 | E = 1) \). By definition, \( p_B \) is the share of \textit{always takers}, \( p_I - p_B \) is the share of \textit{compliers} (\( p_I < U_D < p_B \)), and \( 1 - p_I \) is the share of \textit{never takers}.\(^{19}\) Three MTE curves are plotted: (i) the sample marginal treatment effect (SMTE), which is \( MTE(x, p) \) with \( X \) evaluated at the sample mean; (ii) \( \min MTE(x, p) \) which is the \( MTE(x, p) \) evaluated at the minimum

\(^{18}\)The common support of our estimated propensity scores, as reported in Appendix Figure A6, is between \((0.03, 0.68)\). To use all of the data, we follow Kowalski (2016) and estimate \( MTE(x, p) \) outside the common support using the global polynomial for extrapolation.

\(^{19}\)The underlying assumption is that distribution of the unobserved net cost of treatment \( U_D \) is the same for both treatment and control groups. Therefore, the shares of always takers, compliers, and never takers are the same for the two groups. This assumption holds given the internal validity of RD estimation.
values of $X$; and (iii) $\text{max } MTE(x, p)$ which is the $MTE(x, p)$ evaluated at the maximum values of $X$.

[Insert Figures 5a and 5b here]

We focus on the $MTE(x, p)$ evaluated at the sample means of $X$, i.e., SMTE, for illustration. The $MTE(x, p)$ curves for both physical and mental health are upward-sloping, suggesting a selection on gains in terms of unobserved characteristics. Given that $p = U_D$, these results suggest that individuals with larger costs of participating in the treatment (rustication) face more severe effects on long-term health. The magnitude of the heterogeneity of send-down effects is substantial. For physical health, effects can vary from $-1.167$ standard deviations (for low $U_D$ individuals, who would gain from rustication) to $4.219$ standard deviations (for high $U_D$ individuals, who would lose from rustication). For mental health, although across all $U_D$ individuals would lose, the magnitude of the effects varies from $0.850$ standard deviations to $6.108$ standard deviations.

5.4 Treatment Effect Heterogeneity in Observed Characteristics

Comparing $\text{min } MTE(x, p)$ and $\text{max } MTE(x, p)$, we find that treatment effect varies with covariates. For instance, in the physical health curve (Figure 5a), $\text{min } MTE(x, p)$ shows $MTE(p)$ for female minorities who were born between January and Feburary 1930, rural residents at age 3, and those who had no siblings, were from the least revolutionary families, lived in regions other than birthplace between ages 3-12, and had never lived away from their parents between ages 0-12. On the other hand, $\text{max } MTE(p)$ reports the $MTE(p)$ for Han (ethnic majority) males, those who were born in December 1958, urban residents at age 3, and those who have twelve siblings, were from the most revolutionary families, lived in birthplace between ages 3-12, and had lived away from their parents in childhood. When $p = U_D = 0.2$, $\text{min } MTE(0.2) < 0$ but $\text{max } MTE(0.2) > 0$, indicating that send-down leads to fewer health symptoms for people from the former group, but more health problems for the latter group.

To further assess how treatment effect varies with each covariate, we estimate the average treated and untreated outcome functions, i.e., equations (9) and (10), in Table 4, columns
2-5. Specifically, $\beta_0$ can be interpreted as the differences in health outcomes in the untreated state across covariates, and $\beta_1 - \beta_0$ measures the differences in send-down effects across covariates. The estimated results reveal treatment effect heterogeneity and selection on gains in terms of observed characteristics.

As shown in Table 4, there is strong heterogeneity in the send-down effect across covariates. For example, in the untreated state, people with one more sibling are physically less healthy—i.e., about 0.07 standard deviations more likely to have physical problems—but they experience 0.112 standard deviation less send-down effects on physical health and 0.076 standard deviation less send-down effects on mental health (calculated by $\beta_1 - \beta_0$). Meanwhile, people with more siblings are also more likely to be sent down to the countryside. A similar pattern for mental health emerges with respect to political identity (e.g., family’s revolutionary level) and father absence in early childhood. Individuals from more revolutionary families or who had never lived away from the father by age 3 suffer more from the send-down experience: $\beta_1 - \beta_0$ is positive, suggesting a larger adverse effect on mental health. Also, they have lower propensity to treatment. Overall, the observations imply treatment effect heterogeneity and selection on gains—that is, youths who suffer less from send-down have higher propensity to be rusticated.

5.5 ATT, ATUT and ATE

For physical health outcomes, we find that the estimated $MTE(x, p)$ is negative for always takers, positive for never takers, and mixed for compliers. $MTE(x, p)$ changes from negative to positive when the fraction treated increases to $p^* = 0.217 < p_I$, which may explain why the average effect for compliers is close to zero. For mental health outcome, $MTE(x, p)$ is positive for all $p$ at the means of $X$, and the magnitude of the effects for always takers is smaller than for compliers and never takers. Our finding that the send-down experience leads to more mental disorders can therefore be generalized to all individuals in the cutoff cohort.

We calculate standard treatment effect parameters—LATE, ATT, ATUT, and ATE—at the mean values of $X$ by aggregating over the MTE curve, using the weights provided by Kowalski (2006). Table 5, columns 1-4, report the results.
First, reassuringly, the LATEs calculated from the MTE are similar to our RD estimates; that is, the send-down’s effect is close to zero on physical health and significantly positive on mental health (more mental disorders, worse conditions). Second, ATTs (the common estimator in the literature) suggests that on average, sent-down youths have better physical health—although the difference is not statistically significant—while their mental health is significantly worse. Third, ATUTs suggest adverse effects on both physical and mental health outcomes, although the estimates are not statistically significant. Last, ATEs are positive for both physical and mental health outcomes but estimates are noisy: ATE is statistically insignificant for physical health and only marginally significant for mental health. These results imply that for a person randomly drawn from the population, the send-down experience would cause worse health conditions, both physically and mentally, but the effects are noisy.

To better appreciate these estimates, we compare them to the health impacts estimated from other early-life interventions. Our ATT shows that the send-down experience increases mental disorders by 1.654 standard deviations, which is 612.6% of the control mean. These effects are strikingly large compared to previous studies. For instance, Kling et al. (2007) estimates that the ATT of Moving to Opportunities (MTO) program is about 0.196 standard deviation lower in psychological distress, which is 392% of the control mean.20 Fenske et al. (2014) find that a one standard deviation increase in cocoa price, which closely relates to income in Ghana, at the time of birth decreases the likelihood of severe mental distress during adulthood by 3 percentage points, or half of the mean prevalence. One possible explanation for our relatively large effect is that the send-down experience is a more severe and traumatic shock than moving to better neighborhoods or being born in better economic circumstances. Further, adolescence is a time when mental status is more sensitive to shocks and environment, compared to the time of birth and childhood that previous studies focus

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20Kling et al. (2007) use the K6 z-score scale to measure the mental health status, which is comparable to the mental health index constructed from K10 z-score scales we use here. See Kessler et al. (2002), Table 2, for K10 and K6 item pools.
Our empirical findings suggest that the send-down experience on average had a significantly adverse effect on long-term mental health, but only a limited effect on physical health. Meanwhile, there is substantial heterogeneity in the effects across individuals, and also evidence of selection on gains, i.e., youths with lower resistance to rustication suffer less from the send-down experience.

In this section, we shed light on the interpretation of our findings. We first examine two major hypotheses regarding the underlying mechanism: (i) the send-down’s effects on health mainly work through the rustication experience, and (ii) send-down affects certain later life events such as schooling and marriage, which in turn influence health conditions. Next, we investigate the sources of treatment heterogeneity. In particular, we check whether treatment heterogeneity is driven by differences in the outcome in the untreated state, differences in the outcome in the treated state, or a combination of both. We also check how treatment resistance varies with youths’ family background, such as parental education and occupation.

6.1 Mechanisms

6.1.1 Health Conditions during the Send-Down Period

Testing the hypothesis that mental health problems originated during the send-down period requires data on health conditions in the 1960s and 1970s in China. However, the unavailability of such data prevents us from examining this hypothesis quantitatively. Instead, we look at anecdotal evidence documented by Chinese historians and sociologists to provide suggestive information on this hypothesis.

As mentioned in Section 2.2, sent-down youths encountered difficulties in adapting to rural life. When they arrived in the countryside, they found that the reality was a long way from official propaganda, such as Mao’s description of the countryside as a “vast expanse of heaven and earth where we can flourish.” To them, the villages did not offer an appropriate
future, and they felt deprived compared to their non-sent-down counterparts, who were enjoying the comforts of city life (Bernstein 1977). A young man from Beijing described their shock upon arriving in a village in Heilongjiang, Northeast China:

We were in the same high school and rather pleased to be going off to live together among friends in a new place in the middle of the countryside. But when we arrived and discovered how filthy the peasants were, and the desolation and backwardness of that dump, and realized that we would have to spend the rest of our lives there, we felt a terrible anguish and as soon as we were left alone we burst into tears together. The girls especially, were sobbing loudly. (Bonnin and Horko, 2013, p. 236)

The monotony of life and absence of any social or spiritual activities also affected the youths’ morale and beliefs. During leisure time, well-educated youths were hungry for books and to write, which was common in the cities but difficult in the countryside. The writer Wang Xiaobo described this feeling:

The sent-down life was difficult, we did not eat to the full, we couldn’t get acclimated to the local environment, and many people fell sick. But the greatest pain was the lack of books... I believe that I’m not alone. As the night drew closer, you sit under the roof, watch the sky get darker slowly, feeling immensely lonely and miserable, as if someone deprived our lives. I was young at the time, but I was daunted by the idea that I had to live and grow old like that. I think this is more terrifying than death. (Li and Zheng, 1999, p. 22)

6.1.2 Post-Send-Down Life Outcomes

The send-down experience may significantly affect people’s lives even after the send-down movement ended, causing later mental health problems. To check the feasibility of this hypothesis, we investigate the effect of send-down on education, the labor market, and marriage outcomes. Specifically, we use Years of Schooling and College Completion to capture educational attainment; Being Retired, Wage before Retirement, and Total Monthly Income to
capture the labor market outcome; and Being Single, Being Divorced, Age at First Marriage, Age at First Birth and Number of Children to capture the marriage outcome.

Table 6 reports RD estimates using the same specifications as used to estimate health effects. While the send-down experience slightly increased the probability of being single and the age at first child’s birth, we find no statistically significant effects of the send-down movement on either educational attainment or labor market outcomes. It is unlikely that our findings on health outcomes are mainly driven by changes in education, labor market, or marriage outcomes caused by the send-down experience. However, due to data limitations, we cannot exhaust all potentially important events that happened after the send-down movement. For example, when returning to cities, the send-down youths may have taken up different occupations or had difficulty fitting into the new environment. We admit this possibility, and therefore our argument about post-send-down life should be interpreted with caution.

[Insert Table 6 here]

6.2 Sources of Treatment Heterogeneity

Our MTE estimates reveal heterogeneous treatment effects across individuals. Specifically, the upward sloping $MTE(x, p)$ curves in Figures 5a and 5b indicate that people with larger costs of rustication also encounter greater health impacts from the adversity. Note that $MTE(x, p) = MTO(x, p) - MUO(x, p)$, and the MTE’s slope does not depend on the covariates $X$. Hence, we examine whether treatment effects’ increasing with participation resistance is driven by the outcome in the untreated state ($MUO$’s slope), the outcome in the treated state ($MTO$’s slope), or a combination of both. To this end, we plot the curves for $MTO(x, p)$ and $MUO(x, p)$ for physical and mental health indices in Figures 6a and 6b, respectively.

[Insert Figures 6a and 6b here]

For physical health, the curve for marginal treated individuals, $MTO(x, p)$, is upward sloping, whereas the marginal untreated curve, $MUO(x, p)$, is slightly downward sloping. For mental health, $MTO(x, p)$ is upward sloping, while $MUO(x, p)$ is flatter.
As untreated individuals did not have the send-down experience, any changes in physical and mental health outcomes (along with the fraction treated \( p \)) reflect the selection. \( MUO (x, p) \) curves indicate that, in the untreated state, individuals with higher resistance to the send-down movement had similar physical conditions as, but worse mental conditions than, their lower-resistance counterparts. However, the slopes of \( MUO (x, p) \) curves are small, suggesting that differences between high- and low-resistance individuals are small, or small selection into the send-down movement.

The marginal treated outcome \( MTO (x, p) \) captures both treatment effect heterogeneity and selection into treatment. Given the relatively small selection effect, the substantial treatment heterogeneity across individuals (i.e., variations of \( MTE (x, p) \) with \( p \)) is primarily driven by differences in health outcomes across individuals in the treated state. Specifically, because youths with high resistance to treatment suffer more when treated, they have higher MTEs than their low-resistance counterparts.

To further understand how individuals with high resistance to treatment differ from those with low resistance, we examine whether resistance to treatment is related to family background. Specifically, we check standard family background characteristics available in our data, such as parental education and occupational status, and plot their \( MTO (x, p) \) and \( MUO (x, p) \) curves. Note that covariates \( X \) only influence the intercepts of \( MTO (x, p) \) and \( MUO (x, p) \) curves, and their slope patterns can provide insights into the differences between high and low resistance youths.

Figures 7a and 7b plot \( MTO (x, p) \) and \( MUO (x, p) \) curves using parents’ schooling when the child was 14 as the dependent variable. In both untreated and treated states, we find that youths with less educated parents had higher resistance to send-down than those with more educated ones. In Figures 8a and 8b, we confirm a similar patterns using parent’s occupational prestige.\(^{21}\)

\(^{21}\) The CFPS-2010 records father’s and mother’s exact occupation codes when the child was 14 according to the Chinese Standard Classification of Occupations (CSCO) and the International Standard Classification of Occupation (ISCO-88). Moreover, the CFPS-2010 provides Treiman’s Standard International Occupational Prestige Scale (Treiman’s SIOPS) as socioeconomic index for each occupation, based on Ganzeboom et al. (1992).
Our findings in Figures 7 and 8 suggest that youths with higher resistance to send-down are more likely to come from a more disadvantaged backgrounds. This is in line with social and political environments during the Cultural Revolution. Specifically, people from privileged family backgrounds, such as intellectual or cadre families, were out of political favor and their children were deprived of access to higher education and job opportunities (Xie et al. 2008). Considered to come from “bad origins”, children from more privileged familiers had stronger incentives, or lower resistance, to be sent down because compliance showed their support of the political agenda. As Bonnin and Horko (2013) wrote, “since children with bad class origins were especially vulnerable, they did not dare resist, so frequently it was those with good class backgrounds who put up the most resistance.” A sent-down youth from a cadre family recounted that:

> My family background was bad and my father was labeled a “rightist.” So when Chairman Mao’s instruction was announced, my father “voluntarily” applied for going to rural areas on behalf of his three children. (Deng, 1993, p. 60)

7 Conclusion

This paper exploits China’s send-down movement, in which 17 million urban youths were exiled to the countryside. We use this mandatory movement to estimate the effect of adversity in adolescence on long-term physical and mental health outcomes. We contribute to the literature by focusing on long-term impacts that have lasted almost 40 years and using RD as the identification strategy. We also analyze MTE and therefore extend from LATE to ATT and ATE.

Our findings suggest that rusticated youths were more likely to have mental disorders in later life, whereas their physical health conditions were not significantly affected by their
teenage experiences. The MTE estimates show strong heterogeneous treatment effects across individuals and selection on gains: Those with higher resistance to rustication also suffered more from the send-down experience. We also investigate the mechanisms of the adverse effects on mental health, as well as the source of treatment heterogeneity. It is worth noting that the type of treatment we analyze here is not perfectly comparable to interventions that bring young people to modernized or better environments, such as the Moving to Opportunities program; adversity and benefit programs may not have symmetric effects on health outcomes. Nevertheless, taken together, our results shed light on the importance of adolescence and early adulthood, and especially their long-lasting impacts on mental well-being.

References


8 Appendix A. Robustness Checks

In this subsection, we present a battery of robustness checks. Specifically, we check sensitivity to different polynomial orders, use a nonparametric approach to calculate RD, use the RD-DD estimation, and calculate the intention-to-treat effect using data from China’s 2005 Population Census. We focus on summary indices for physical and mental health in our robustness checks.
Alternative polynomial order. To check whether our findings are sensitive to the second order polynomial function we adopted in the benchmark regression, we also use third- and fourth- order polynomial functions and report the estimates in columns 1-4 of Appendix Table A5. We find stable estimates for both health indices, suggesting that our results are not driven by a particular functional form.

Nonparametric estimation. In the baseline estimations, RD estimators are calculated using the parametric approach. To check whether the results are sensitive to this method, we employ a nonparametric approach, i.e., local linear regression using the optimal bandwidth calculated by method of Imbens and Kalyanaraman (2012). Estimates are reported in Appendix Table A5, columns 7 and 8. We find similar patterns with relatively larger standard errors due to the smaller sample size.

RD-DD estimation. To further accommodate the possibility that the cohort effect at the cutoff point might differ from other years, we include data on rural individuals—who were ineligible for the send-down movement—and combine the RD framework with a DD analysis to obtain an RD-DD estimation.

Estimating equations (??) and (??) for the urban sample with the inclusion of quarter dummies, we obtain \( \hat{\alpha}_{1,\text{urban}} = \alpha_1 + \theta_{\text{cohort,urban}} - \theta_{\text{cohort,urban}} \) and \( \hat{\alpha}_{2,\text{urban}} = \alpha_2 + \phi_{\text{cohort,urban}} - \phi_{\text{cohort,urban}} \). Applying the same estimations to the rural sample, we obtain \( \hat{\alpha}_{1,\text{rural}} = \theta_{\text{cohort,rural}} - \theta_{\text{cohort,rural}} \) and \( \hat{\alpha}_{2,\text{rural}} = \phi_{\text{cohort,rural}} - \phi_{\text{cohort,rural}} \). Therefore, \( \hat{\beta}_{\text{RD-DD}} = \hat{\alpha}_{1,\text{urban}} - \hat{\alpha}_{1,\text{rural}} - \hat{\alpha}_{2,\text{urban}} + \hat{\alpha}_{2,\text{rural}} = \beta \), as long as \( \theta_{\text{cohort,urban}} - \theta_{\text{cohort,rural}} = \theta_{\text{cohort,urban}} - \theta_{\text{cohort,rural}} \) and \( \phi_{\text{cohort,urban}} - \phi_{\text{cohort,urban}} = \phi_{\text{cohort,rural}} - \phi_{\text{cohort,rural}} \). In other words, the identifying assumption in the RD-DD estimation becomes that the deviation of cohort effects at the cutoff point from the average cohort effect in the urban sample is the same as that in the rural sample. RD-DD estimates are presented in Appendix Table A5, columns 5 and 6. We find similar patterns, in which the send-down experience has limited impact on physical health but affects mental health negatively, which suggests that our findings are robust.

City violence. During the Cultural Revolution, the Red Guard unleashed frequent violence and chaos in cities, but less so in the countryside. Therefore, our findings could be driven by escaping from violence and chaos rather than by experiences of teenage adversity. However, both anecdotal and analytical evidence suggest that this is not the main reason.
First, one widely held conjecture about Mao’s motive for ordering send-down for all urban youths is that the Red Guards, who were mostly teenagers, became a destructive force in the cities (e.g., destroying schools and factories, harassing ordinary citizens, and engaging in robbery and other criminal behavior), and moving students to the countryside would defuse the Red Guards and reduce the chaos. In other words, the urban youths in both our treatment and control groups largely experienced similar levels of violence and chaos, which therefore should not be the main driving force in the differences in health outcomes.

Second, to provide quantitative evidence, we control for the level of violence in our RD baseline specifications. Specifically, we use death casualties between 1968 and 1971, collected by Walder and Su (2003) from County Annals (Xian Zhi) and, divide them by total population in each province at the beginning of the Cultural Revolution (year 1966) to calculate the average death count. As shown in columns 9 and 10 of Appendix Table A5, the estimates are quite similar to our baseline results, implying that city violence cannot explain our findings.

ITT using Census 2005. Thus far, we have used the CFPS-2010 for our analysis. To achieve a larger sample size and gain more representation, we further use China’s 2005 census to repeat the baseline analysis as a robustness check. The 2005 census recorded self-reported health status, with 1 meaning healthy, 2 meaning normal, and 3 meaning disabled. The census also contains information on the respondent’s birth year and birth month, but no information on send-down experience. Therefore, we are only able to calculate the intention-to-treat (ITT) effects instead of LATE. Based on RD estimations, the same patterns are found for self-reported health status: Send-down cohorts tend to have worse health conditions in the long run.

9 Appendix B. Treatment Effects and Estimands as Weighted Average of the MTE

MTE unifies common treatment effect parameters including ATE (average treatment effect), TT (treatment effect on the treated), TUT (treatment effect on the untreated), and LATE
(local ATE). These treatment effect parameters can be recovered by the weighted average of the MTE curve:

\[
ATE(x) = \int_0^1 MTE(x, p_x) \cdot \varpi_{ATE}(x, p_x) \, dp_x \\
TT(x) = \int_0^1 MTE(x, p_x) \cdot \varpi_{TT}(x, p_x) \, dp_x \\
TUT(x) = \int_0^1 MTE(x, p_x) \cdot \varpi_{TUT}(x, p_x) \, dp_x \\
LATE(x) = \int_0^1 MTE(x, p_x) \cdot \varpi_{LATE}(x, p_x) \, dp_x
\]

where \( p_x = p(D = 1|Z_-, X = x) \) and the weights are given as follows:

<table>
<thead>
<tr>
<th>( \varpi_{ATE}(x, p_x) )</th>
<th>( \varpi_{TT}(x, p_x) )</th>
<th>( \varpi_{TUT}(x, p_x) )</th>
<th>( \varpi_{LATE}(x, p_x) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>if ( 0 \leq p_x \leq p_{Bx} )</td>
<td>1</td>
<td>( \frac{1}{p_{Bx} + s(p_I)(p_{Ix} - p_{Bx})} )</td>
<td>0</td>
</tr>
<tr>
<td>if ( p_{Bx} \leq p_x \leq p_{Ix} )</td>
<td>1</td>
<td>( \frac{s(p_I)}{p_{Bx} + s(p_I)(p_{Ix} - p_{Bx})} )</td>
<td>( \frac{s(p_B)}{1 - s(p_I) p_{Ix} - s(p_B)p_{Bx}} )</td>
</tr>
<tr>
<td>if ( p_{Ix} \leq p_x \leq 1 )</td>
<td>1</td>
<td>0</td>
<td>( \frac{1}{1 - s(p_I) p_{Ix} - s(p_B)p_{Bx}} )</td>
</tr>
</tbody>
</table>

where \( p_{Bx} = p(D = 1|Z_- = 0, X = x) \); \( p_{Ix} = p(D = 1|Z_- = 1, X = x) \); \( s (p_B) = P(Z_- = 0) \) represents the share of individuals in the control group; and \( s (p_I) = P(Z_- = 1) \) is the share of individuals in the treatment group.

\[\text{---}^{22}\]

\( p_{Bx} \) and \( p_{Ix} \) are estimated using predicted probabilities in the first stage selection equation.
Figures and Tables

Figure 1. Density of Birth Cohort

Note: Figure 1 displays the density of birth cohort for urban sample, without controlling for quarterly dummies. Bars represent the average birth density for each bin (birth quarter); the solid lines indicate the fitted values from the second order polynomial regression; the vertical line indicates the cutoff point (normalized to 0) in the assignment variable, and the dashed lines are the 95 percent confidence intervals.
Figure 2. Smoothness of Predetermined Covariates

Note: Figures 2 displays the smoothness of predicted Send-down probability, which is calculated by OLS using predetermined covariates, including gender, ethnicity, urban or rural status at the age of 3, number of siblings, family's political identity measured by the revolutionary level during the Cultural Revolution, whether lived in birthplace at age 3 and age 12 (2 variables), whether had lived away from father by age 3 and when the child was 4-12 years old (2 variables), and whether had lived away from mother by age 3 and when the child was 4-12 years old (2 variables). Circles represent the average predicted sent-down ratio for 4 bins (birth quarters) of urban samples; the solid lines indicate the fitted values from the local linear regressions; the vertical line indicates the cutoff point (normalized to 0) in the assignment variable, and the dashed lines are the 95 percent confidence intervals.
Figure 3a. Cohort Means of Send-down: Urban Sample

Figure 3b. Cohort Mean of Send-down: Rural Sample

Note: Figures 3a and 3b plot the relation between the send-down experience (the regressor of interest) and birth cohort (the assignment variable) for urban and rural samples, respectively. Circles represent the average sent-down ratio for 4 bins (birth quarters); the solid lines indicate the fitted values from the local linear regressions; the vertical line indicates the cutoff point (normalized to 0) in the assignment variable, and the dashed lines are the 95 percent confidence intervals.
Figure 4a. Effect of Send-down on Physical Health

Figure 4b. Effects of Send-down on Mental Health

Note: Figures 4a and 4b display the discontinuities in physical and mental health indices without controlling for quarterly dummies, respectively. Circles represent the average sent-down ratio for 4 bins (birth quarters) of urban samples; the solid lines indicate the fitted values from the local linear regressions; the vertical line indicates the cutoff point (normalized to 0) in the assignment variable, and the dashed lines are the 95 percent confidence intervals.
Figure 5a. Marginal Treatment Effects of Send-down on Physical Health

Figure 5b. Marginal Treatment Effects of Send-down on Mental Health

Note: Figure 5a and 5b depict the linear MTE(x,p) as a function of p measured in percentiles, for physical and mental health indices, respectively. SMTE is MTE (x, p) when all baseline covariates X is evaluated at the sample mean; min MTE (x, p) is the MTE (x, p) evaluated at the minimum values of X; max MTE (x, p) is the MTE (x, p) evaluated at the maximum values of X. PB indicates the baseline treatment probability, and PI is the intervention treatment probability.
Figure 6a. Marginal Treated and Untreated Outcome: Physical Health

Figure 6b. Marginal Treated and Untreated Outcome: Mental Health

Note: Figure 6a and 6b depict the linear $\text{MTO}(x,p)$ and $\text{MUO}(x,p)$ as a function of $p$ measured in percentiles, for physical and mental health indices, respectively. PB indicates the baseline treatment probability, and PI is the intervention treatment probability.
Figure 7a. Marginal Treated and Untreated Outcome: Father’s Years of Schooling When Child was 14

Figure 7b. Marginal Treated and Untreated Outcome: Mother’s Years of Schooling When Child was 14

Note: Figure 7a and 7b depict the linear MTO(x,p) and MUO(x,p) as a function of p measured in percentiles, for father’s and mother’s years of schooling when the youth was 14, respectively. PB indicates the baseline treatment probability, and PI is the intervention treatment probability.
Figure 8a. Marginal Treated and Untreated Outcome: Father’s Occupational Prestige Scale When Child was 14

Figure 8b. Marginal Treated and Untreated Outcome: Father’s Occupational Prestige Scale When Child was 14

Note: Figure 8a and 8b depict the linear $\text{MTO}(x,p)$ and $\text{MUO}(x,p)$ as a function of $p$ measured in percentiles, for father’s and mother’s occupational prestige scale when the youth was 14, respectively. PB indicates the baseline treatment probability, and PI is the intervention treatment probability.
### Table 1. Items used to construct the Measures of Physical and Mental Health

<table>
<thead>
<tr>
<th>Variable</th>
<th>Construction</th>
<th>Survey Question</th>
</tr>
</thead>
</table>
| **Physical Health Index** | The principal components analysis was performed to create the physical health index which is then z-standardized: the higher the score, the physically healthy. | (1) Being underweight (BMI<18.5) or overweight (BMI>25), based on current height and weight. (1-yes; 0-no)  
(2) During the past six months, have you had any doctor-diagnosed chronic disease? (1-yes; 0-no)  
(3) Were you hospitalized last year due to illness/injury? (1-yes; 0-no)  
(4) During the past two weeks, have you felt physically uncomfortable? (1-yes; 0-no) |
| **Mental Health Index** | The principal components analysis was performed to create the mental health index which is then z-standardized: the higher the score, the mentally unhealthier the individual. | (1) How often have you felt depressed and could not cheer up in the past month? (1-never; 5-almost every day)  
(2) How often have you felt nervous in the past month? (1-never; 5-almost every day)  
(3) How often have you felt agitated or upset and could not remain calm in the past month? (1-never; 5-almost every day)  
(4) How often have you felt hopeless about the future? (1-never; 5-almost every day)  
(5) How often have you felt that everything is difficult? (1-never; 5-almost every day)  
(6) How often do you think life is meaningless? (1-never; 5-almost every day) |
Table 2. First Stage

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>ESTIMATE</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Send-down</td>
</tr>
<tr>
<td>I(cohort ≥ C₀)</td>
<td>0.175***</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,908</td>
<td></td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.0221</td>
<td></td>
</tr>
</tbody>
</table>

Notes: 1. The coefficient presents the estimated discontinuity in the Send-down probability; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
Table 3. Effect of the Send-down on Health

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>ESTIMATE</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Send-down</td>
<td>-0.065 (0.686)</td>
<td>1.094*** (0.418)</td>
</tr>
<tr>
<td>Observations</td>
<td>1.908</td>
<td>1.895</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.337</td>
<td>-0.270</td>
</tr>
</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the outcomes as a result of the Send-down Movement; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
Table 4. Selection Equation and Health Outcome Equation

<table>
<thead>
<tr>
<th>ESTIMATE</th>
<th>VARIABLE</th>
<th>Send-down</th>
<th>Physical Health</th>
<th>Mental Health</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$\hat{\beta}_0$</td>
<td>$\hat{\beta}_1$</td>
<td>$\hat{\beta}_0$</td>
</tr>
<tr>
<td>IV: I(cohort≥C₀)</td>
<td></td>
<td>0.236***</td>
<td>-0.825</td>
<td>4.561</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.079)</td>
<td>[0.592, 5.594]</td>
<td>[-2.311, 17.624]</td>
</tr>
<tr>
<td>Propensity Score</td>
<td></td>
<td>-0.006</td>
<td>-0.119</td>
<td>-0.203*</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.021)</td>
<td>[-0.27, 0.05]</td>
<td>[-0.48, 0.035]</td>
</tr>
<tr>
<td>Male</td>
<td></td>
<td>0.077</td>
<td>-0.085</td>
<td>-0.597</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.065)</td>
<td>[-0.491, 0.312]</td>
<td>[-1.509, 0.487]</td>
</tr>
<tr>
<td>Ethnicity-Han</td>
<td></td>
<td>0.016</td>
<td>-0.122</td>
<td>0.089</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.044)</td>
<td>[-0.359, 0.156]</td>
<td>[-0.396, 0.576]</td>
</tr>
<tr>
<td>Urban Hukou at Age 3</td>
<td></td>
<td>0.020***</td>
<td>0.070**</td>
<td>-0.047</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>[0.032, 0.11]</td>
<td>[-0.196, 0.173]</td>
</tr>
<tr>
<td>No. of Siblings</td>
<td></td>
<td>-0.041**</td>
<td>-0.062</td>
<td>0.145</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.018)</td>
<td>[-0.18, 0.055]</td>
<td>[-0.206, 0.524]</td>
</tr>
<tr>
<td>Revolutionary Background</td>
<td></td>
<td>-0.006</td>
<td>0.006</td>
<td>-0.184</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.044)</td>
<td>[-0.198, 0.223]</td>
<td>[-0.842, 0.272]</td>
</tr>
<tr>
<td>Lived in Birthplace at Age 3</td>
<td></td>
<td>-0.033</td>
<td>-0.067</td>
<td>0.139</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.038)</td>
<td>[-0.235, 0.081]</td>
<td>[-0.36, 0.612]</td>
</tr>
<tr>
<td>Lived away from Father by Age 3</td>
<td></td>
<td>0.104**</td>
<td>0.237</td>
<td>-0.165</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.051)</td>
<td>[-0.177, 0.604]</td>
<td>[-0.802, 0.848]</td>
</tr>
<tr>
<td>Lived away from Father when Aged 4-12</td>
<td></td>
<td>0.017</td>
<td>0.035</td>
<td>-0.08</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.040)</td>
<td>[-0.275, 0.307]</td>
<td>[-0.68, 0.558]</td>
</tr>
<tr>
<td>Lived away from Mother by Age 3</td>
<td></td>
<td>-0.049</td>
<td>0.556**</td>
<td>0.382</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.071)</td>
<td>[0.02, 1.107]</td>
<td>[-0.532, 1.269]</td>
</tr>
<tr>
<td>Lived away from Mother when Aged 4-12</td>
<td></td>
<td>-0.034</td>
<td>-0.021</td>
<td>0.215</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.062)</td>
<td>[-0.275, 0.304]</td>
<td>[-0.591, 0.792]</td>
</tr>
<tr>
<td>Observations</td>
<td></td>
<td>1,701</td>
<td>1,157</td>
<td>544</td>
</tr>
</tbody>
</table>

Notes: 1. Standard errors in parentheses are clustered at cohort level; 2. Bootstrapped 95% confidence intervals are in brackets; 3. Significance level: *** p<0.01, ** p<0.05, * p<0.1
## Table 5. Treatment Effects from MTEs

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Mental Health</td>
<td>2.155** [0.415, 3.939]</td>
<td>1.654** [0.261, 2.783]</td>
<td>4.336 [-1.079, 8.964]</td>
<td>3.479* [-0.262, 6.872]</td>
</tr>
</tbody>
</table>

Notes: 1. The Table reports, for physical and mental health indices, the local average treatment effect (LATE), the treatment effect on the treated (TT), treatment effect on the untreated (TUT), and the average treatment effect (ATE) calculated from MTEs; 2. 95% confidence intervals for treatment effects from MTE are obtained by bootstrapping: *** p<0.01, ** p<0.05, * p<0.1.
Table 6. Impact of the Send-down on Post-Send-Down Life Outcomes

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Years of Schooling</td>
<td>Send-down</td>
<td>0.351</td>
<td>-0.167</td>
<td>0.248</td>
<td>0.689</td>
<td>0.410</td>
<td>0.114*</td>
<td>0.108</td>
<td>1.962</td>
<td>7.614*</td>
<td>0.140</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(4.900)</td>
<td>(0.302)</td>
<td>(0.270)</td>
<td>(1.418)</td>
<td>(0.683)</td>
<td>(0.064)</td>
<td>(0.091)</td>
<td>(3.961)</td>
<td>(4.138)</td>
<td>(0.747)</td>
</tr>
<tr>
<td>Observations</td>
<td></td>
<td>1,907</td>
<td>1,907</td>
<td>1,627</td>
<td>1,098</td>
<td>1,588</td>
<td>1,908</td>
<td>1,908</td>
<td>1,815</td>
<td>1,824</td>
<td>1,908</td>
</tr>
<tr>
<td>Control Mean</td>
<td></td>
<td>8.104</td>
<td>0.147</td>
<td>0.988</td>
<td>5.617</td>
<td>7.294</td>
<td>0.004</td>
<td>0.015</td>
<td>23.890</td>
<td>26.150</td>
<td>2.503</td>
</tr>
</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the outcomes as a result of the Send-down Movement; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
Appendix Figures and Tables

Figure A1. Density of Birth Cohort: 1982 Chinese Census

Figure A2. Density of Birth Cohort: 1990 Chinese Census

Figure A3. Density of Birth Cohort: 2000 Chinese Census
Figure A4a. Effect of Send-down on Physical Health: Abnormal BMI

![Graph showing the effect of send-down on abnormal BMI vs. birth cohort.]

Figure A4b. Effect of Send-down on Physical Health: Chronic Disease

![Graph showing the effect of send-down on chronic disease vs. birth cohort.]

* Cohort Mean  Quadratic Fit
Figure A4c. Effect of Send-down on Physical Health: *Hospitalization*

Figure A4d. Effect of Send-down on Physical Health: *Uncomfortable*

Note: Figures A4a-A4d, respectively, display the relation between birth cohort—our assignment variable—and four physical health variables. Circles represent the conditional mean values for 4 bins (birth quarters); lines indicate the fitted values from the second order polynomial regressions; and vertical line indicates the normalized cutoff point in the assignment variable.
Figure A5a. Effect of Send-down on Mental Health: *Depressed*

Figure A5b. Effect of Send-down on Mental Health: *Nervous*
Figure A5c. Effect of Send-down on Mental Health: *Restless*

Figure A5d. Effect of Send-down on Mental Health: *Hopeless*
Figure A5e. Effect of Send-down on Mental Health: *Difficult*

![Graph A5e](image)

Figure A5f. Effect of Send-down on Mental Health: *Worthless*

![Graph A5f](image)

Note: Figures A5a-A5f, respectively, display the relation between birth cohort—our assignment variable—and six mental health variables. Circles represent the conditional mean values for 4 bins (birth quarters); lines indicate the fitted values from the second order polynomial regressions; and vertical line indicates the normalized cutoff point in the assignment variable.
Figure A6. Common Support of Propensity Score
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Had Urban Hukou at Age 12</th>
<th></th>
<th>Had Rural Hukou at Age 12</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>S.D.</td>
<td># Obs</td>
<td>Mean</td>
</tr>
<tr>
<td>Send-down</td>
<td>0.308</td>
<td>0.46</td>
<td>1916</td>
<td>0.012</td>
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<tr>
<td>Physical Health Index</td>
<td>0.187</td>
<td>1.09</td>
<td>1916</td>
<td>0.286</td>
</tr>
<tr>
<td>Abnormal BMI</td>
<td>0.359</td>
<td>0.48</td>
<td>1916</td>
<td>0.353</td>
</tr>
<tr>
<td>Chronic</td>
<td>0.235</td>
<td>0.42</td>
<td>1916</td>
<td>0.226</td>
</tr>
<tr>
<td>Hospitalization</td>
<td>0.096</td>
<td>0.29</td>
<td>1916</td>
<td>0.118</td>
</tr>
<tr>
<td>Uncomfortable</td>
<td>0.278</td>
<td>0.45</td>
<td>1916</td>
<td>0.351</td>
</tr>
<tr>
<td>Mental Health Index</td>
<td>-0.229</td>
<td>0.93</td>
<td>1903</td>
<td>0.117</td>
</tr>
<tr>
<td>Depressed</td>
<td>1.528</td>
<td>0.88</td>
<td>1905</td>
<td>1.746</td>
</tr>
<tr>
<td>Nervous</td>
<td>1.378</td>
<td>0.77</td>
<td>1905</td>
<td>1.580</td>
</tr>
<tr>
<td>Restless</td>
<td>1.332</td>
<td>0.74</td>
<td>1907</td>
<td>1.608</td>
</tr>
<tr>
<td>Hopeless</td>
<td>1.294</td>
<td>0.73</td>
<td>1904</td>
<td>1.447</td>
</tr>
<tr>
<td>Difficult</td>
<td>1.361</td>
<td>0.78</td>
<td>1907</td>
<td>1.729</td>
</tr>
<tr>
<td>Worthless</td>
<td>1.267</td>
<td>0.68</td>
<td>1906</td>
<td>1.418</td>
</tr>
<tr>
<td>Other variables</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender (Male=1)</td>
<td>0.491</td>
<td>0.50</td>
<td>1916</td>
<td>0.507</td>
</tr>
<tr>
<td>Ethnic Han (Han=1)</td>
<td>0.963</td>
<td>0.19</td>
<td>1913</td>
<td>0.923</td>
</tr>
<tr>
<td>Had Urban Hukou at Age 3</td>
<td>0.911</td>
<td>0.28</td>
<td>1892</td>
<td>0.009</td>
</tr>
<tr>
<td>Number of Siblings</td>
<td>3.247</td>
<td>1.99</td>
<td>1840</td>
<td>3.446</td>
</tr>
<tr>
<td>Revolutionary Background during Cultural Revolution</td>
<td>2.623</td>
<td>0.65</td>
<td>1896</td>
<td>2.653</td>
</tr>
<tr>
<td>Lived in Birthplace at Age 3</td>
<td>0.902</td>
<td>0.30</td>
<td>1903</td>
<td>0.978</td>
</tr>
<tr>
<td>Lived in Birthplace at Age 12</td>
<td>0.766</td>
<td>0.42</td>
<td>1910</td>
<td>0.960</td>
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<tr>
<td>Had Lived Away from Father by Age 3</td>
<td>0.088</td>
<td>0.28</td>
<td>1851</td>
<td>0.083</td>
</tr>
<tr>
<td>Had Lived Away from Father When Aged 4-12</td>
<td>0.114</td>
<td>0.32</td>
<td>1873</td>
<td>0.149</td>
</tr>
<tr>
<td>Had Lived Away from Mother by Age 3</td>
<td>0.029</td>
<td>0.17</td>
<td>1876</td>
<td>0.045</td>
</tr>
<tr>
<td>Had Lived Away from Mother When Aged 4-12</td>
<td>0.058</td>
<td>0.23</td>
<td>1894</td>
<td>0.090</td>
</tr>
</tbody>
</table>
Table A2. Smoothness of Predetermined Socioeconomic Characteristics

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Gender-Male Male</th>
<th>Ethnic Han</th>
<th>Hukou at Age 3</th>
<th>No. of Siblings</th>
<th>Revolutionary Background</th>
<th>Lived in Birthplace at Age 3</th>
<th>Lived Away From Father by Age 3</th>
<th>Lived Away From Father When Aged 4-12</th>
<th>Lived Away From Father by Age 3</th>
<th>Lived Away From Father When Aged 4-12</th>
</tr>
</thead>
<tbody>
<tr>
<td>I(cohort ≥ C₀)</td>
<td>-0.040</td>
<td>0.007</td>
<td>0.002</td>
<td>-0.090</td>
<td>-0.147</td>
<td>-0.006</td>
<td>-0.028</td>
<td>0.044</td>
<td>-0.010</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.059)</td>
<td>(0.032)</td>
<td>(0.045)</td>
<td>(0.287)</td>
<td>(0.100)</td>
<td>(0.030)</td>
<td>(0.061)</td>
<td>(0.043)</td>
<td>(0.046)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,908</td>
<td>1,905</td>
<td>1,884</td>
<td>1,832</td>
<td>1,888</td>
<td>1,896</td>
<td>1,902</td>
<td>1,843</td>
<td>1,865</td>
<td>1,868</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.510</td>
<td>0.958</td>
<td>0.884</td>
<td>3.086</td>
<td>2.560</td>
<td>0.921</td>
<td>0.768</td>
<td>0.0816</td>
<td>0.129</td>
<td>0.0298</td>
</tr>
</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the predetermined characteristics; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
## Appendix Table A3. Effects on Physical Health

<table>
<thead>
<tr>
<th></th>
<th>Physical Health Measures</th>
<th></th>
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<th></th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Abnormal BMI</td>
<td>Chronic Disease</td>
<td>Hospitalization</td>
<td>Uncomfortable</td>
</tr>
<tr>
<td>Send-down</td>
<td>-0.980</td>
<td>1.513*</td>
<td>0.299</td>
<td>-1.633**</td>
</tr>
<tr>
<td></td>
<td>(0.879)</td>
<td>(0.854)</td>
<td>(0.751)</td>
<td>(0.699)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,908</td>
<td>1,908</td>
<td>1,908</td>
<td>1,908</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.0666</td>
<td>0.382</td>
<td>0.212</td>
<td>0.0750</td>
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</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the outcomes as a result of the Send-down Movement; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
Appendix Table A4. Effects on Mental Health

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Depressed</td>
<td>Nervous</td>
<td>Restless</td>
<td>Hopeless</td>
<td>Difficult</td>
<td>Worthless</td>
</tr>
<tr>
<td>Send-down</td>
<td>0.334</td>
<td>0.903*</td>
<td>0.777</td>
<td>1.067*</td>
<td>0.929**</td>
<td>1.083**</td>
</tr>
<tr>
<td></td>
<td>(0.483)</td>
<td>(0.481)</td>
<td>(0.475)</td>
<td>(0.554)</td>
<td>(0.457)</td>
<td>(0.535)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,897</td>
<td>1,897</td>
<td>1,899</td>
<td>1,896</td>
<td>1,899</td>
<td>1,898</td>
</tr>
<tr>
<td>Control Mean</td>
<td>-0.228</td>
<td>-0.259</td>
<td>-0.207</td>
<td>-0.120</td>
<td>-0.252</td>
<td>-0.164</td>
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</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the outcomes as a result of the Send-down Movement; 2. We use second order polynomial regressions; 3. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 4. Control means are the means of the outcomes for the prereform sample; 5. Each regression includes four quarter of birth dummies.
Appendix Table A5. Other Specifications

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Physical Third Order</th>
<th>Mental</th>
<th>Physical Fourth Order</th>
<th>Mental</th>
<th>Physical Nonparametric Estimation</th>
<th>Mental</th>
<th>Physical RD-DD Estimation</th>
<th>Mental</th>
<th>Physical Control for Violence</th>
<th>Mental</th>
<th>Self-reported Health Status</th>
<th>ITT-Census 2005</th>
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</thead>
<tbody>
<tr>
<td>ESTIMATE</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Send-down</td>
<td>-0.004</td>
<td>0.753**</td>
<td>0.023</td>
<td>0.745**</td>
<td>0.149</td>
<td>0.740*</td>
<td>-0.357</td>
<td>1.222*</td>
<td>0.085</td>
<td>1.289***</td>
<td>0.016**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.522)</td>
<td>(0.323)</td>
<td>(0.542)</td>
<td>(0.340)</td>
<td>(0.608)</td>
<td>(0.381)</td>
<td>(0.882)</td>
<td>(0.719)</td>
<td>(0.772)</td>
<td>(0.490)</td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1,908</td>
<td>1,895</td>
<td>1,908</td>
<td>1,895</td>
<td>916</td>
<td>908</td>
<td>11,799</td>
<td>11,628</td>
<td>1,878</td>
<td>1,866</td>
<td>227,929</td>
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</tr>
<tr>
<td>Control Mean</td>
<td>0.337</td>
<td>-0.270</td>
<td>0.337</td>
<td>-0.270</td>
<td>0.0791</td>
<td>-0.147</td>
<td>0.407</td>
<td>0.0957</td>
<td>0.337</td>
<td>-0.270</td>
<td>1.367</td>
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</tr>
</tbody>
</table>

Notes: 1. Each cell presents the estimated discontinuity in the outcomes as a result of the Send-down Movement; 2. We use third and fourth order polynomial regressions without interaction term in Columns 1-4, and local linear regressions in Columns 5 and 6; 3. Regressions in Columns 7 and 8 further include the rural sample to difference out the cohort effects; 4. Regressions in Columns 9 and 10 controls for violence level measured by average death counts per county per capita provided by Walder and Su (2003); 5. Standard errors in parentheses are clustered at cohort level: *** p<0.01, ** p<0.05, * p<0.1; 6. Control means are the means of the outcomes for the prereform sample; 7. Each regression includes four quarter of birth dummies.