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Adolescent Adversity and Long-run Health

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Abstract

A central question in human development is what causes health inequalities over the life cycle. We link a harsh environment in the teen years to individuals' health conditions almost 40 years later, and employ Regression Discontinuity Design to make a causal inference between adolescent adversity and long-term health. Specifically, we exploit the mandatory "send-down" policy during China's Cultural Revolution, in which millions of urban youths were forced to move to rural areas. We find that rusticated youths were more likely to develop chronic diseases and mental problems. These effects are similar across gender, but stronger for individuals with fewer siblings.

Keyword: Adolescent environment; Health; Youth; China

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 in prenatal 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 programs have not been established 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). Moreover, many of the adolescent evaluations focus on labor market outcomes rather than non-cognitive skills, which are largely formed, developed, and shaped during the teen years.

In this paper, we exploit a large-scale, mandatory social movement to investigate the effect of adolescent adversity on long-run health. Specifically, we study the impact of the send-down movement in China, a mandatory urban-to-rural migration that forced 17 millions urban youths to live and work in the countryside for years. Employing survey data from 2010, we are able to examine the impact on their physical and mental health conditions 40 years after the experience for long-term follow-up.

In December 1968, the then-leader of China, Mao Zedong, initiated a national movement to send junior and senior high school graduates in the cities to rural areas. The eligible urban youths were suddenly exiled to the countryside and experienced a dramatic decline in their standard of living. In most cases, they lived without running water, electricity, or a proper sanitation infrastructure, and had to perform hard labor every day. Further, they were estranged from family for several years. By the late 1970s, more than 17 million people had been rusticated (Pan 2002).

This unexpected and mandatory movement provides us with a regression discontinuity (RD) design to estimate the impact of adolescent adversity on long-run 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, birth cohort 1947, was the cutoff for being sent down: The cohort born just after 1947 was forced to be rusticated, whereas the cohort born just before 1947 was not subject to the scheme and therefore constitutes a good counterfactual. Meanwhile, to deal with the cohort effect—that individuals born just before and just after 1947 could differ for reasons unrelated to the send-down experience—we use the policy-ineligible sample (urban individuals who did not complete junior high school and rural residents) to estimate the difference in health between cohorts born right before and right after 1947, and subtract it from the estimate using the policy-eligible sample—a combined RD and difference-in-difference (RD-DD) estimator.

We examine the impact of the send-down experience on a range of physical and mental health outcomes observed in 2010, approximately 40 years after the shock. 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 assumption of our RD-DD estimation. We found that the sent-down youths who were placed in a less modern environment (post-1947 birth cohorts) present poorer health status, especially for mental well-being: They are 44 percentage points more likely to develop chronic diseases and report a more severe level of mental problems including amnesia, distress, anxiety and restlessness. The results are robust to a battery of robust checks.

The large effects on mental health are consistent with social-psychology theories on early adulthood. Adolescence and early adulthood is a period of great mental plasticity, when non-cognitive 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; in our context, 40 years. In contrast, most indicators of physical health status, including BMI, hospitalization, and overall comfort, were not particularly worse compared to the non-sent-down groups.

We also test the importance of various subsequent pathways—educational attainment, income, marriage, and childbearing—that may lead to long-run 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 total years of schooling, income, marriage status in 2010, age of first marriage and childbirth, or the number of children. Hence we can reject the chain of causation that runs from the send-down experience to educational attainment, labor and marriage market outcomes, and finally to long-run health status.

Lastly, we examine the presence of heterogeneous effects in particular, the impact of the send-down experience across gender and number of siblings. Overall, we find that the effect on long-run health is quite general, with slightly stronger effects for (1) females to have chronic diseases and (2) youths with fewer siblings to have physical and mental problems.

This paper contributes to a better understanding of the relationship between adolescent 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 targeting 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 age 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 draught and civil war (Alderman et al. 2006) and famine (Meng and Qian 2009). In comparison, we examine a category of adolescent shocks that is more relevant to the lives of rural populations in developing countries today. For example, these findings will help us to understand the consequences and overall welfare effect of public policies that facilitate migration but leave behind the children of migrant workers. They will also shed light on the benefits of programs that target disadvantaged teenagers and their families.

Our study has several additional features that distinguish it from existing research. First, evaluations of adolescent programs usually follow participants for no longer than 20 years. Short-term follow-up could lead to biased estimates of returns—upward-biased if the b enefits

[1]Almond and Currie (2011) survey papers on the fetal origins hypothesis and discuss a broad range of fetal shocks and circumstances that have found a later-life impacts.

eventually dissipate or downward-biased if the effects take place later in life (Heckman and Mosso 2014). In contrast, we examine the long-term outcome, thereby revealing the long-lasting impact of adolescent conditions. To study the impact of early intervention over the life cycle, Gould et al. (2011) examine Operation Magic Carpet in which Yemenite 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, many programs target specific demographic or disadvantaged groups, while the event in our setting applied to all eligible youths at the time, regardless of family income or background. It thus allows us to avoid any selection concern—i.e.,that participants chose to enter or avoid the program—and also to apply the findings to a more general population. Third, we provide results for a battery of physical and mental health indicators—a major addition to evaluations in which measures of health outcomes are coarse, or non-cognitive skills are absent.

This paper is also related to the literature of the origins of health inequalities. Besides the studies of early-life conditions(Currie et al. (2010)), a line 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 activity, 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 in China and outlines the estimation strategy. Section 3 describes the data. Section 4 reports the main findings and estimates of the send-down effect, and describes several robustness checks. Section 5 discusses possible interpretations of our findings and the relevance of competing hypotheses. Section 6 explores the heterogeneity of the effects across individual characteristics. Section 7 concludes.

[2]Clark and Royer (2013) find little causal effect on health, and suggest caution as to the likely health returns on educational interventions.

2 Estimation Strategy

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 the Cultural Revolution and affected millions of urban youths until it ended in 1978.

A small-scale send-down movement started in the early 1950s, following Mao Zedong's rallying cry to develop remote regions. In 1955, Mao commented that "the countryside is a vast expanse of heaven and earth where we can flourish," an attempt to direct the urban unemployed to rural areas. 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 to the people, 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 colleges had been shut down nationally since 1966, 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 give up the better life 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)

The government relaxed enforcement after 1977, and brought some youths back to join the urban labor force or enter college. By 1979, Mao's successors had denounced the send-down policy and allowed all the 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 Life 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 at both material and spiritual levels. They encountered difficulties in adapting to the lower standard of living and sanitation, strenuous physical labor, lack of cultural and spiritual activities, and separation from their family.

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, the youths had to travel miles to fetch water in buckets for a minimum amount 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 time of the day to agricultural labor which was scarcely mechanized at the time. 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)

The monotony of life and the lack of cultural activities was also a huge change from life in the city. There were only limited social activities or entertainment after work. 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 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)

2.3 Estimation 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 adolescent adversity on long-run health outcomes. Specifically, we use the regression discontinuity (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 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 early adulthood; let Y_{i0} be the outcome in the absence of send-down;

and denote D_i as the send-down status, i.e., 1 if individual i was sent down and 0 otherwise. The sudden and mandatory send-down movement in 1968 implies that the probability of being sent to the rural areas is discontinuous at a cutoff point c_0 of the birth cohort (c_i), i.e., $\lim_{c\downarrow c_0} E[D_i|c_i = c] \neq \lim_{c\uparrow c_0} E[D_i|c_i = c]$. Assuming $E[Y_{i0}|c_i = c]$ is continuous in c at c_0 (later we will formally check this key identifying assumption), Hahn et al. (2001) show that γ can be identified as

$$\gamma = \frac{\lim_{c\downarrow c_0} E[Y_i|c_i = c] - \lim_{c\uparrow c_0} E[Y_i|c_i = c]}{\lim_{c\downarrow c_0} E[D_i|c_i = c] - \lim_{c\uparrow c_0} E[D_i|c_i = c]} = \frac{\hat{\beta}_{reduced}}{\hat{\alpha}_{first}} = \hat{\gamma}_{RD}. \quad (1)$$

Empirically, the RD estimation uses the sample of all urban junior high school or above graduates, as they were subject to the scheme. The assignment variable in our RD estimation, birth cohort (c), is a grade-based birth cohort—that is, students born in different months but in the same grade. Since its establishment, China has followed the former Soviet Union and used September as the school opening month. Therefore, in constructing our assignment variable, we define a birth cohort as those born between September (of the previous year) and August (of the current year). For example, cohort 1947 consists of students born between September 1946 and August 1947. The oldest cohort affected was those who graduated from senior high school in 1966. The school starting age was 7 in the 1950s-1980s, the completion of the primary grades and junior and senior high school took 6 years, 3 years, and 3 years, respectively. Hence, the first cohort affected is cohort 1947; accordingly, we set the cutoff point as $c_0 = 1947$.

Lee and Lemieux (2010) show that the RD estimator (1) is essentially an instrumental variable estimator. Specifically, the first stage of the instrumental variable estimation has the following specification

$$D_i = \alpha I[c_i \geq c_0] + g(c_i) + \mu_i, \quad (2)$$

and the reduced-form is

$$Y_i = \beta I[c_i \geq c_0] + f(c_i) + \varepsilon_i, \quad (3)$$

where $I[.]$ is an indicator function that takes a value of 1 if the argument in the bracket is true and 0 if it is false; $g(.)$ and $f(.)$ are flexible functions of c_i, controlling for the direct effect of birth cohort on outcome variables. Hence, the RD estimator is $\hat{\gamma}_{RD} = \frac{\hat{\beta}_{reduced}}{\hat{\alpha}_{first}}$.

One potential concern about the above RD estimator is that it may also capture the effect of birth cohorts on the margin—that is, people from grade-based birth cohorts on the margin (i.e., cohort 1947 vs. cohort 1946, given that $c_0 = 1947$) are inherently different for reasons unrelated to the send-down movement. In other words, $\hat{\alpha}_{first}$ and $\hat{\beta}_{reduced}$ become $\alpha + \lambda_{1947}$ and $\beta + \eta_{1947}$, and the RD estimator becomes

$$\hat{\gamma}_{RD} = \frac{\hat{\beta}_{reduced}}{\hat{\alpha}_{first}} = \frac{\beta + \eta_{1947}}{\alpha + \lambda_{1947}} \neq \frac{\beta}{\alpha} = \gamma.$$

Assuming that the effects of birth cohort on the margin are the same for the send-down eligible and ineligible groups (i.e., $\lambda_{1947} = \eta_{1947}$) , we address this concern by including all send-down ineligible groups as a control, or a combined RD and difference-in-differences (RD-DD) analysis. The ineligible group includes urban individuals who did not complete junior high school (including people with partial junior high education, primary completion or below) and rural residents. Since there is no send-down for the ineligible groups, $\hat{\alpha}_{first,ineligible}$ and $\hat{\beta}_{reduced,ineligible}$ should only capture cohort effects on the margin, i.e., $\hat{\alpha}_{first,ineligible} = \lambda_{1947}$, $\hat{\beta}_{reduced,ineligible} = \eta_{1947}$. The RD-DD estimator can then recover the effect of send-down

$$\hat{\gamma}_{RD-DD} = \frac{\hat{\beta}_{reduced,eligible} - \hat{\beta}_{reduced,ineligible}}{\hat{\alpha}_{first,eligible} - \hat{\alpha}_{first,ineligible}} = \gamma. \quad (4)$$

Note that our identifying assumption imposes no restriction on the effects of other birth cohorts (i.e., $\lambda_{t\neq 1947}$ and $\eta_{t\neq 1947}$). It is plausible that the ineligible groups, for example rural youths were affected by different policies, or had a different social and educational profile compared to the urban eligible people. But such differences are common to all rural cohorts and canceled out with the first differencing within rural population. What might generate bias, however, are the shocks specific to the 1947 send-down-eligible group but not affecting the 1947 send-down-ineligible group (i.e., $\lambda_{1947} \neq \eta_{1947}$). For people born in the narrow window of September 1947, the most plausible source of specific shocks is the Cultural Revolution, which unleashed great violence and chaos in cities but not in rural areas. In the empirical session, we provide anecdotal and quantitative evidence to waive the concern. In particular, the potential bias should be largest for areas with fierce violence, and the smallest for areas with little violence where there would be little difference between urban and rural shocks. We found no such heterogeneity across regions.

Two practical issues on estimation are worth noting here. The first is how to estimate $g(.)$ and $f(.)$. Lee and Card (2008) point out that when the assignment variable (birth cohort in our case) is discrete, one cannot use the 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. The second issue is how to estimate the standard error. Following the tradition in the literature (see Lee and Lemieux 2010), we use the standard errors clustered at the birth cohort level for the RD estimator. The standard errors of the RD-DD estimator $\hat{\gamma}_{RD-DD}$ are computed by bootstrapping using the birth cohort level as the resampling cluster. We obtain similar results for $\hat{\gamma}_{RD-DD}$ by the delta method, which assumes that estimators from both send-down-eligible and send-down -ineligible are uncorrelated.

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 (i.e., Gansu, Guangdong, Henan, Liaoning, and Shanghai) were chosen for initial oversampling (1,600 households in each) to achieve regional comparisons, and the remaining 8000 households were drawn through weighting from the other provinces/regions to make the overall CFPS sample representative of the country. The final sample included 15,717 households and 33,600 adult respondents in 2010.

The 2010 CFPS consisted of 4 questionnaires (Community, Family, Adolescent, and Adult), which included most questions covered in four U.S. counterpart datasets (PSID, CDS, HRS, and NYLS). It 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 reflecting the respondent's physical health status. The first measure, denoted *Abnormal BMI*, indicates whether an individual is underweight (BMI$<$19.5) or overweight (BMI>25).[3] The second, denoted *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, denoted *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 seven questions related to mental health conditions, six of which belong to the the 10-question Kessler Psychological Distress Scale (K10).[4] Respondent were asked to rate, on a scale from 1 to 5, the frequency or severity of certain symptoms. We construct seven variables accordingly, all of which take values from 1 to 5: *Forgetful*, *Depressed*, *Nervous*, *Restless*, *Hopeless*, *Difficult*, and *Worthless*. Specifically, (1) *Forgetful* is derived from the question "Are you able to remember the important things that have happened to you in the past week?", with 1 meaning "able to remember all of them" and 5 meaning "able to remember a little bit". The other six variables reflect the frequency of corresponding symptoms, with 1 meaning never and 5 meaning almost every day: (2) *Depressed*:"How often did you feel depressed and cannot cheer up in the past month?"; (3) *Nervous*: "How often did you feel nervous in the past month?"; (4) *Restless*: "How often did you feel agitated or upset and could not remain calm in the past month?"; (5) *Hopeless*: "How often did you feel hopeless in the past month?"; (6) *Difficult*: "How often did you find it difficult

[3]Results using only overweight or underweight are similar (available upon request).

[4]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).

to do everything in the past month?"; and (7) *Worthless*: "How often do you think life is meaningless?" In Table 1, we list the health measures for the two categories (physical and mental health) and their corresponding survey questions.

[Insert Table 1 here]

Responses to these health-related questions could be highly correlated. For example, people who are depressed may also be likely to feel hopeless and worthless. Empirically, we find a high correlation among *Depressed*, *Nervous*, *Restless*, *Hopeless*, *Difficult*, and *Worthless* (the average correlation is 0.6961), but there is a low correlation between these six variables and *Forgetful* (the average correlation is 0.1153). In the empirical analysis, we report the estimates for each of these health outcomes, as well as the average effect size (AES) indices for the two health categories (physical and mental health) following Kling et al. (2004). Specifically, let $\hat{\gamma}_k$ be the estimated send-down coefficient for a health outcome variable Y_k, and let σ_k^2 denote the variance of outcome Y_k for the control group. The AES index is then defined as $\frac{1}{K}\sum_{k=1}^{K}\frac{\hat{\gamma}_k}{\sigma_k}$, where K is the total number of outcome variables for physical or mental health. Hence, we are drawing more general conclusions about the effect on long-term overall heath conditions instead of on a particular health problem.[5]

While our health outcomes are constructed based on self-reported responses, one could be concerned with the measurement error problem, 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., cohort 1947 vs. cohort 1946). No prior theories posit why 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",[6] instead of specifically targeting the send-down

[5] The AES index has two additional advantages over the individual estimates: First, while results regarding each outcome variable could potentially be due to chance (Type I error), this is less likely for the AES index when several outcome variables are simultaneously summarized. Second, the AES index reduces the risk of low statistical power (Type II error).

[6] http://www.isss.edu.cn/cfps/EN/About/Introduction/

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 the 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 (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 the reporting errors.

Send-down status. The CFPS contains information on whether the person experienced 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 was living in the region where he or she lived 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 1958 to 1961 experienced the three-year Great Famine (1958-1961) in China, which could also affect long-run health outcomes; and those born after 1961 were not eligible for the send-down movement.[7] The remaining sample contains 11,810 individuals. Descriptive statistics for our sample are presented in Table A1.

Overall, our sample contains 1,477 send-down-eligible individuals (with urban hukou at age 12 and at least junior high school education), and 10,333 send-down-ineligible individuals (with rural hukou at age 12 or urban hukou and education below junior-high school completion). The send-down ratio is 34.6% for eligible individuals and close to zero for ineligible

[7] As the send-down movement was officially terminated in 1978, the last cohort subject to the movement is cohort 1961.

individuals. Compared to their ineligible counterparts, eligible individuals on average have higher education attainment, higher birth weight, fewer siblings, more educated parents, and are less likely to belong to an ethnic minority, less likely to be separated from parents between age 0 and 12, and less likely to have migrated between age 0 and 12. Eligible and ineligible samples are similar in gender composition and family background during the Cultural Revolution. An average eligible individual in the survey was more likely to have chronic diseases and less likely to have abnormal BMI values, be hospitalized, or feel uncomfortable. Meanwhile, raw comparison of the means suggests that send-down-eligible people seem to have better mental health status.

4 Empirical Results

4.1 Potential Manipulation

The key identifying assumption of our RD-DD estimations is that $E\left[Y_{i0}|c_i = c\right]$ is continuous in c at c_0; in other words, people cannot fully manipulate the assignment variable, i.e., the timing of births.

Before testing the validity of our identification strategy, let us first point out that our estimation framework allows for a certain degree of manipulation within cohorts. That is, within the send-down-eligible cohorts, the selection of being sent down is allowed. For instance, middle school students with poor health could avoid being sent down, and our identification strategy allows for such selection. The rationale is similar to the case of randomized controlled trials (RCT) with some compliers and some noncompliers. Within the treatment group, people can choose to participate (compliers) or not (noncompliers). But as long as there is randomization across treatment and control groups, comparison of outcomes between the whole treatment and whole control groups can identify the intention-to-treat effect (ITT). Meanwhile, using randomization to instrument for the real status of treatment can identify the treatment-on-treated effect (TOT), the strategy used here.

Anecdotal evidence suggests that our identifying assumption is satisfied. First, households would not reasonable have been able to foresee the benefits or costs of birth timing decades before the Cultural Revolution was initiated. The cohorts on the margin are people born in 1945-1947, the period of the Chinese Civil War (the battle 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, notice that our assignment variable, birth cohort, is based on the school opening month. There was no fixed date for school opening in the 1930s-1940s in China; hence, it is unlikely that people would manipulate the birth months of their children to let them 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 (cohort 1947). Figure 1 reports the histogram of the birth cohort; clearly, there is no discontinuity at cohort 1947 (the cutoff point) in either the urban or rural population.

[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, it might be possible that this differential mortality rate cancels out the manipulation of childbirth timing, so we do not find any discontinuity in the observed density of birth cohort in Figure 1. To check this possibility, we report in Appendix Figures 1-3 the histogram of birth cohorts using China's population censuses in 1982,[8] 1990, and 2000, 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 finds any discontinuity at cohort 1947. Combined, these results suggest that there is no sample selection issue due to differential

[8]The 1982 population census does not include the information necessary to break the population into rural and urban; hence, we draw the histogram for the whole population.

mortality rates across cohorts on the margin and no evidence for the manipulation of birth timing.

A second check for the validity of our research setting is to examine whether individuals' predetermined socioeconomic characteristics are smooth at the cutoff point. Specifically, we examine the number of siblings, gender, ethnic group, low birth weight (birth weight <2500 gm), parents' education, parents' age at birth, weeks separated from parents from age 0 to 12, ever migrated from age 0 to 12, and family background during Cultural Revolution (i.e., revolutionary class, middle class, class enemies, or other class). Figures 2A-2C plot the differences in these socioeconomic variables between treatment (urban high school or above graduates after September 1946) and control cohorts (urban high school or above graduates born before September 1946), as well as the 95% confidence intervals against different window lengths. As shown in these figures, we cannot reject the null hypothesis that mean differences for these socioeconomic characteristics between treatment and control groups are zero at the 95% confidence interval up to a window of seven cohorts.

[Insert Figures 2A-2C here]

We then test whether our treatment and control groups are balanced among all these socioeconomic characteristics within the seven cohorts window in Table A2. Columns 1 and 4 report mean values for these socioeconomic characteristics for the seven control and seven treatment cohorts, while Columns 2 and 5 present standard deviations. Columns 7 and 8 report mean differences and p-values. Clearly, all these predetermined socioeconomic characteristics are balanced between treatment and control groups. Accordingly, in the regression analysis, we also present results based on this narrow window of seven cohorts on each side of the cutoff point (i.e., cohorts 1940-1953), as well as those based on the full window (cohorts 1930-1958).

4.2 Send-Down Probability and Birth Cohorts

We first present (Figure 3) the relation between send-down status (our regressor of interest) and birth cohort (our assignment variable) for send-down-eligible (urban junior high school or above graduates) and ineligible groups (urban individuals who did not complete junior high school and rural residents) separately. Clearly, there is a jump in the probability of being sent down at the cutoff cohort (i.e., cohort 1947) among the send-down-eligible group. In contrast, among the send-down-ineligible group, the probability of being sent down always remains close to zero.

[Insert Figure 3 here]

Table 2 reports the first-stage results—the effect of being born after September 1946 on send-down participation—using the RD-DD estimation. We use two regression samples: a full sample consisting cohorts 1930-1958 and a restricted sample of seven cohorts on each side of the cutoff cohort (i.e., cohorts 1940-1953). Meanwhile, we use various polynomial functions to control for the direct effect of birth cohort. Table 2 presents four specifications: Row 1 reports results using the full sample and cubic polynomial function of cohorts without varying slopes across the cutoff cohort; Row 2 reports results from the full sample and quadratic function with varying slopes across the cutoff; Row 3 uses the restricted sample and quadratic function without varying slopes; Row 4 uses the restricted sample and linear function with varying slopes. We also report the point estimate and bootstrapping standard errors.

We find that mandatory enforcement significantly increased the probability of being sent down, consistent with the pattern shown in Figure 3. The magnitude is about 0.203 to 0.257, the exact number depends on regression sample and choice of polynomial control function. The effect is enormous considering the fact that the average send-down probability for the eligible group is 0.34.

[Insert Table 2 here]

In summary, our results show that there is a discontinuity of being sent down for cohorts on the margin; the policy change in 1968 increased the probability of being sent down by 20%-26%. Therefore, this confirms the validity of our research design.

4.3 Physical Health

We first present the relation between physical health conditions and birth cohorts in Figure 4. For the send-down-ineligible group, we do not find visible discontinuities at the cutoff cohort among all four physical health outcomes. For the send-down-eligible group, at the cutoff cohort there is no clear discontinuity for *Abnormal BMI* (underweight or overweight) and *Hospitalized* (being hospitalized within a year due to illness or injury). There is sizable jump, however, in *Chronic* (any doctor-diagnosed chronic diseases in the past six months) and a modest drop in *Uncomfortable* (feeling physically uncomfortable in the past two weeks) at the cutoff cohort.

[Insert Figure 4 here]

Table 3 reports the RD-DD estimators corresponding to Equation (4) with four different specifications. Each column, from Column 1 to Column 4, reports the results for a health indicator. Each row corresponds to a specification. We find consistent estimates across all the regression specifications: (1) no significant effects of the send-down experience on *Abnormal BMI*, *Hospitalization*, or *Uncomfortable*, and in some specifications the estimates are close to zero; and (2) a positive effect of the send-down experience on *Chronic*, with estimates ranging from 0.448 to 0.802. Overall, the results for physical health indicators are in line with the observation in Figure 4.

[Insert Table 3 here]

To capture the overall effect on physical health, in Column 5 we report the AES index, which is the weighted average of the four individual health estimates. This is insignificant and small in magnitude in some specifications, suggesting a limited overall effect of the send-down experience on long-term physical health.

4.4 Mental Health

For the send-down effect on mental health, we found consistently adverse impacts across all seven indicators. Figures 5a-5b show the relation between mental health status and birth cohort. We find that for the send-down-ineligible group, there is no clear discontinuity at the cutoff cohort for any of the seven measures of mental health, whereas for the send-down-eligible group, there is a visible jump at the cutoff cohort for all seven mental health outcomes. These results suggest that the send-down experience has a negative effect on individuals' long-term mental health.

[Insert Figures 5a-5b here]

To corroborate the message in Figures 5a-5b, we report the RD-DD estimators corresponding to Equation (4) in columns 1-7 of Table 4 for the seven measures of mental health. Across all four regression specifications, we consistently find positive effects of the send-down experience on all seven measures of mental health, and most of these estimates are statistically significant. By definition, higher values of these measures mean worse mental health status; therefore, the estimates suggest that the send-down experience leads to worse mental well-being.

[Insert Table 4 here]

In Column 8 of Table 4 we report the AES index, which captures the overall effect on long-term mental health. The AES index is positive and statistically significant for all four regression specifications, suggesting that the adverse effects of the send-down are not due to any particular measure of mental health status.

To better appreciate these estimates, we compare them to health impacts from other early-life interventions. Our results show that the send-down experience increases mental problems by 0.9-1.9 percentage points, which is about 1.57-3.32 standard deviations, or 62.6% to 132.1% of the mean. These effects strike us in terms of magnitude compared to previous studies. For instance, Kling et al. (2007) found that the Moving to Opportunities (MTO) programs reduced psychological distress by 0.196 percentage point, or 3.4% of the mean.[9] Fenske et al. (2014) found that a one standard deviation rise in the price of cocoa

[9]Kling et al. (2007) use the K6 z-score scale to measure mental health status, which is comparable to the K10 scale we use here. See Kessler et al. (2002), Table 2, for K10 and K6 item pools.

(a main source of income fluctuation) at the time of birth decreases the likelihood of severe mental distress in adulthood by 3 percentage points, or half the mean prevalence. One possible explanation for our relatively large effect is that compared to changes due to moving to a better environment or better economic circumstances, the shock from the send-down experience was more severe. Another possibility is that adolescence and early adulthood is a period when mental status is more sensitive to shocks in the environment than during early childhood, which previous studies target.

4.5 Robustness Checks

The Cultural Revolution effect. As discussed in Section 2.3, as there is no evidence of birth timing or clumping, and that households would not reasonably have been able to foresee the benefits or costs of birth timing decades before the send-down movement, the primary threat to the identification are other channels generating differential 1947 cohort effects between send-down-eligible and send-down-ineligible groups. The most plausible source of such cohort-specific shocks is the Cultural Revolution, which unleashed great violence and chaos in cities but not in rural areas, and involved mostly students in the schools in cities. Hence, cohort 1946 (who had graduated at the time of the Cultural Revolution) and cohort 1947 (who were still in the schools at the time of the Cultural Revolution) in our send-down-eligible group could be exposed to the violence and chaos differentially while cohort 1946 and cohort 1947 in the send-down-ineligible groups were largely unaffected, causing possible estimation bias.

To address this concern, we exploit geographic heterogeneity in the level of violence and chaos. Specifically, we divide our sample provinces into two groups: provinces with fierce violence and provinces with less violence.[10] The premise is that send-down cohorts from different areas were largely blended in the same rural areas, suggesting similar send-down effects across these two groups. However, if our aforementioned estimates were driven by the city violence and chaos, the estimated send-down effect would be larger for areas with more

[10] A province is classified as a fierce-violence province if at least two major violent events occurred between May 1966 and December 1968, and as a less-violence province otherwise. See the Memorabilia of the Red Guards (Jiang 1994) for information on major violent events during this period.

violence than for areas with less violence. Regression results are reported in Table A3a and Table A3b. It is clear that all of our estimates are statistically similar between the fierce-violence group and the less-violence group and many differences are small in magnitude, implying that city violence did not have differently affected cohort 1946 and cohort 1947 in the cities.

Including predetermined socioeconomic characteristics. Our RD-DD estimators require that cohorts on the margin (i.e., cohort 1947 vs. cohort 1946) be balanced along all dimensions except for facing mandatory send-down. If this identifying assumption was satisfied, including socioeconomic controls should have little effect on our estimator $\hat{\gamma}_{RD-DD}$ for both statistical significance and estimated magnitude. As shown in Table A4, we find that the inclusion of controls (e.g., gender, ethnicity, number of siblings, and political identity of the family during the Cultural Revolution) barely changes our RD-DD estimators in either statistical significance or magnitude, implying the validity of our identification strategy.

Placebo test using different send-down-ineligible groups. Note that there are two groups of send-down-ineligible individuals: urban individuals who did not complete junior high school and rural residents. Because both groups were immune to the mandatory send-down movement, comparisons of them using the same estimation strategy as above should not produce any significant differences; otherwise, it would indicate the existence of some omitted variables. In Table A5, we report the RD-DD estimations, in which the treatment group is urban individuals who were born after 1947 and did not complete junior high school, the control group is urban individuals who were born before 1947 and did not complete junior high school, and the sample of rural residents is used to subtract birth-cohort effects. We find that none of the RD-DD estimators is statistically significant. Meanwhile, these estimators are of much smaller magnitude than our baseline RD-DD estimators in Tables 3-4. Taken together, these findings imply that our research design is valid.

5 Mechanism

In the previous section we documented a significant adverse effect of the send-down experience on long-term mental health, but only a limited effect on long-term physical health.

Because sent-down youths experienced a great deal of hardship during their send-down period, our findings suggest that adversity in adolescence and early adulthood creates long-term mental health problems but has much less impact on the physical level. In this section, we check two major hypothesis that shed light on the underlying mechanism of our findings. First, mental health problems developed during the send-down period and persisted to the late-life stage. Second, the send-down experience may have changed people's post-movement life trajectory, which in turn affects their mental health.

5.1 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-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, which described 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 men 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)

Sent-down youths were more likely to get sick or be injured due to the low standard of sanitation, extreme fatigue from labor and malnutrition. Harmful insects such as mosquitoes and whitmania pigra were widespread in the countryside and infected many youths with malaria. Hepatitis and rabies were endemic in some regions among the sent-down youths (Bonnin and Horko 2013). Certain tasks they performed in the field were high risk. For instance, long hours of working in the paddy field could lead to rheumatism and chronic arthritis. Many sent-down women suffered from abnormal menstruation due to living and working conditions. A 1972 survey shows that around 70% of the sent-down women had gynecological problems, as they were forced to carry out farm work during their menstrual periods (Gu, 2009, p. 103-104).

In addition to being more likely to fall sick in the countryside than in the city, health care was far inferior in rural areas. In areas without proper transportation, youths had to travel to health centers or ill-equipped hospitals miles away. Like the peasants, sent-down youths did not have insurance, and had to rely on the low wages they earned from farm work.

At the level of mental health, the monotony of life and absence of any social or spiritual activities affected their morale and beliefs. During leisure time, well-educated youths were hungry for books and to write, which is common in the cities but difficult to do by the light of an oil lamp and without a table. 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)

5.2 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, labor market, and marriage outcomes. Specifically, we construct *Years of Schooling* to capture educational attainment, *Total 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.

Regression results from the same RD-DD specification as before are reported in Table 5. While we found that the send-down experience reduced educational attainment and age at first marriage and increased total income, probability of being single, probability of getting divorced, age at first birth, and number of children, yet none of these estimates is statistically significant and most are small in magnitude. Combined, these results provide limited evidence that our findings can be explained by changes in education, labor market, or marriage outcomes caused by the send-down experience.

[Insert Table 5 here]

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 hypothesis about post-send-down life should be interpreted with caution.

6 Heterogeneous Effects

We further investigate whether the effect of adolescent adversity on long-term health differs across individuals with different characteristics, and specifically, across gender and different numbers of siblings.

6.1 Gender Difference

Recent literature has uncovered substantial gender differences in economic outcomes such as risk preference (Holt and Laury 2002), social preference (Brown-Kruse and Hummels 1993; Nowell and Tinkler 1994; Cadsby and Maynes 1998; Eckel and Grossman 1998; Andreoni and Vesterlund 2001) and attitudes toward competition (Gneezy et al. (2003); Niederle and

Vesterlund (2007)).[11] If men and women cope with stress in different ways, there could be differences in send-down effects on health.

Regression results for male and female samples and their estimate differences are reported in Table 6.[12] Interestingly, we find that females who experienced send-down are more likely to have chronic diseases than males, but are similar to males on other three measures of physical health. Meanwhile, there is no significant gender difference across all of the mental health outcomes. These results imply that females handle mental problems as well as males when facing hardships in their early adulthood lives.

[Insert Table 6 here]

6.2 Sibling Difference

Our main results show overall worse health for sent-down individuals. When a household has both sent-down and non-sent-down children, our findings reveal differences in skills and human capital across children after the send-down years, and parents may reinforce or compensate for such differences. On one hand, parents may behave "efficiently" and invest more in the non-sent-down child because he or she is healthier and will potentially have larger returns, thereby increasing the adverse effects on the health of the sent-down child. On the other hand, parents may want to compensate the sent-down child by allocating more health inputs to him or her after returning to the city, thereby reducing the gap between the sent-down and non-sent-down children. Studies have found evidence for both reinforcing and compensating investment (Li et al. 2010; Conti et al. 2010; Heckman et al. 2013).

Here, we explore how send-down effects differ across individuals with different numbers of siblings. In households with more siblings, resources are scarcer per person; this reduces magnitude for both reinforcing and compensating investment. If parents invest to make efficient returns, more siblings will reduce this effect and cause a smaller gap between sent-down and non-sent-down individuals. If, instead, parents invest to compensate the child

[11] For recent reviews on this literature, see Eckel and Grossman (2008); Croson and Gneezy (2009); Bertrand (2011); Niederle and Vesterlund (2011).

[12] Appendix Table A6a and A6b report the results of a full set of specifications.

experiencing difficulties, having more siblings will also reduce the effect and therefore lead to larger health effects from the send-down experience.

Regression results for samples with more siblings and fewer siblings (defined as above and below the sample median) and their estimate differences are reported in Table 7.[13] We find that the effects are larger for the sample with fewer siblings than the sample with more siblings, for both physical and mental health outcomes, and most of the mental health effects are statistically significant. These results suggest that the reinforcing investment mechanism is more in effect than the compensating investment mechanism in our setting.

[Insert Table 7 here]

7 Conclusion

This paper exploits a unique episode in which 17 million urban youths were exiled to rural China in the late 1960s and 1970s, following Mao's sudden directive. Teenagers were uprooted from their urban lives, and lived in a harsh environment for years. We use this mandatory movement to estimate the effect of adversity in adolescence on long-term physical and mental outcomes. Our focus on long-term impacts lasting almost 40 years and the identification strategy from Regression Discontinuity are the paper's main distinguishing features.

Our findings show that youths who were rusticated—and thus lived in a less established environment—were more likely to develop chronic disease and mental problems in their later life. It is worth noting that the estimated effect of the adversity in teen years is quite substantial. Spending many years in a backward environment led to a 44 percentage point increase in the likelihood of chronic disease, and raised the level of mental distress by 1.57 to 3.32 standard deviations, or 62%-132% from the mean. By comparison, Gould et al. (2011) examined the long-term social and economic effects of "Operation Magic Carpet," in which Yemenite children were airlifted to Israel in 1949. They found that growing up in a city lowers the probability of reporting a health problem by 6.2 percentage points, about 15.5% from the mean. In the Moving to Opportunity experiment, Kling et al. (2007) found that moving

[13] Appendix Table A7a and A7b report the results of a full set of specifications.

to less distressed neighborhoods led to a reduction in psychological distress (K6 z-score) by 0.2 standard deviations for adults and 0.5 standard deviations for female youth. The type of treatment that we analyze is not perfectly comparable to interventions that bring young people to modernized or better environments; adversity and benefit programs may not have symmetric effects on health, and the exact treatment is difficult to quantity. Nevertheless, taken together, we have a better understanding of the importance of adolescence and early adulthood, especially the long-lasting impacts on mental well-being. The results thus shed light on the benefits of programs targeting disadvantaged teenagers.

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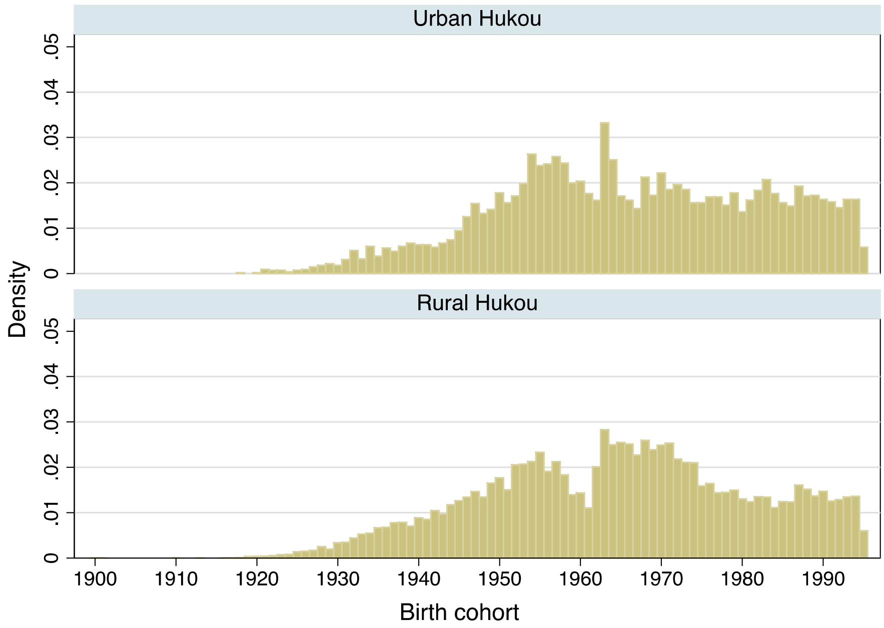
Figure 1. Distribution of Birth Cohort by Hukou Status at Age 12



Notes: This figure plots the density of birth cohort in the sample. The upper panel shows individuals with Urban Hukou at the age of 12 and the lower panel shows individuals with Rural Hukou at the age of 12.

Figure 2A. Difference between Send-down-eligible and Send-down-ineligible Cohorts with Various Windows: Family Characteristics and Ethnicity

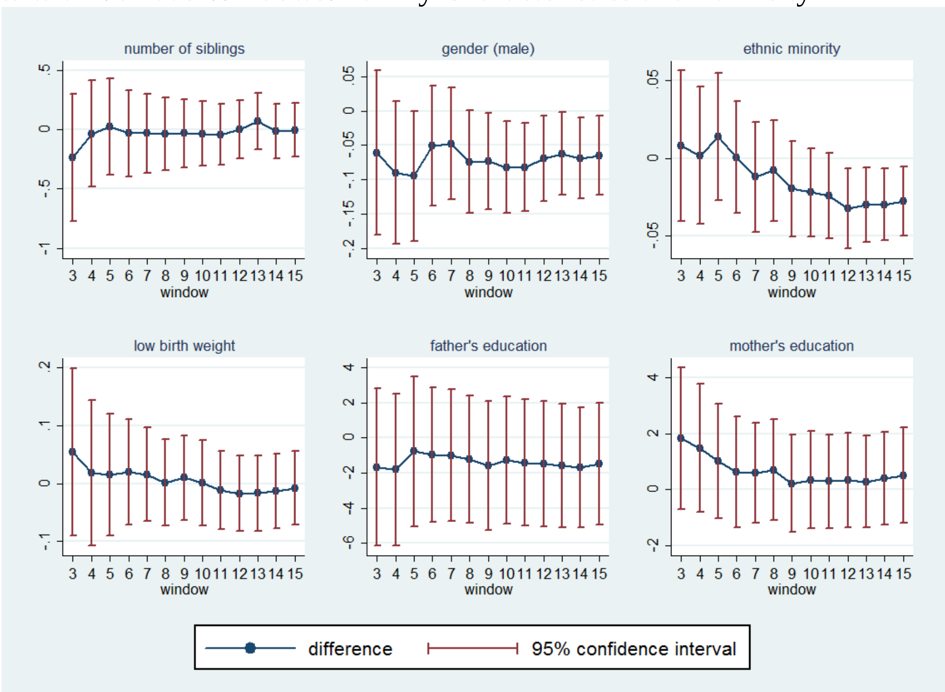


Figure 2B. Difference between Send-down-eligible and Send-down-ineligible Cohorts with Various Windows: Family Characteristics and Early Experience

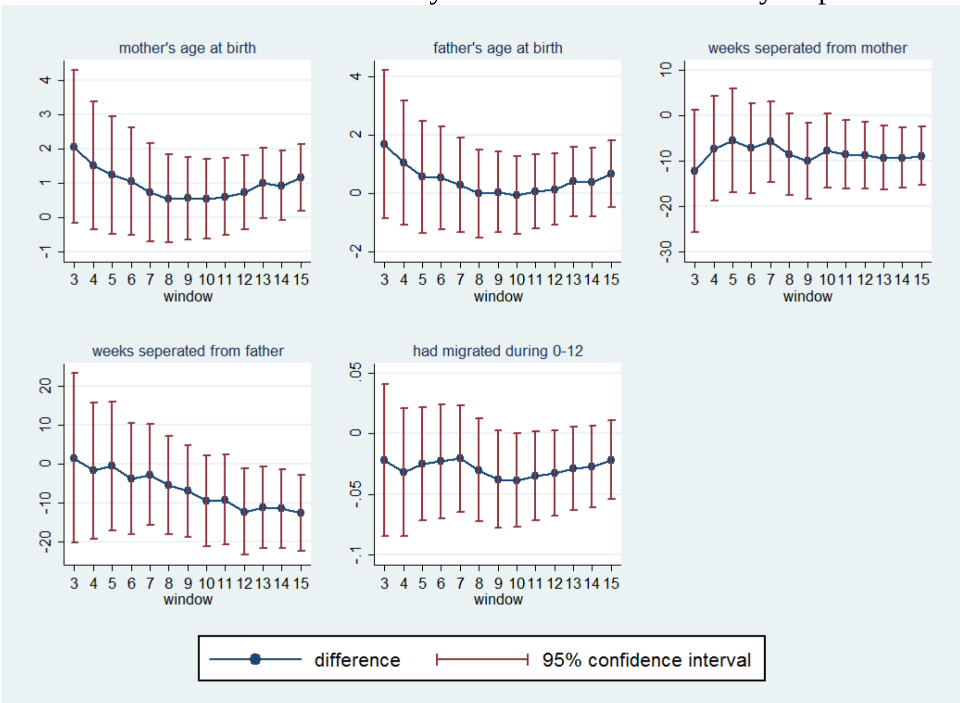
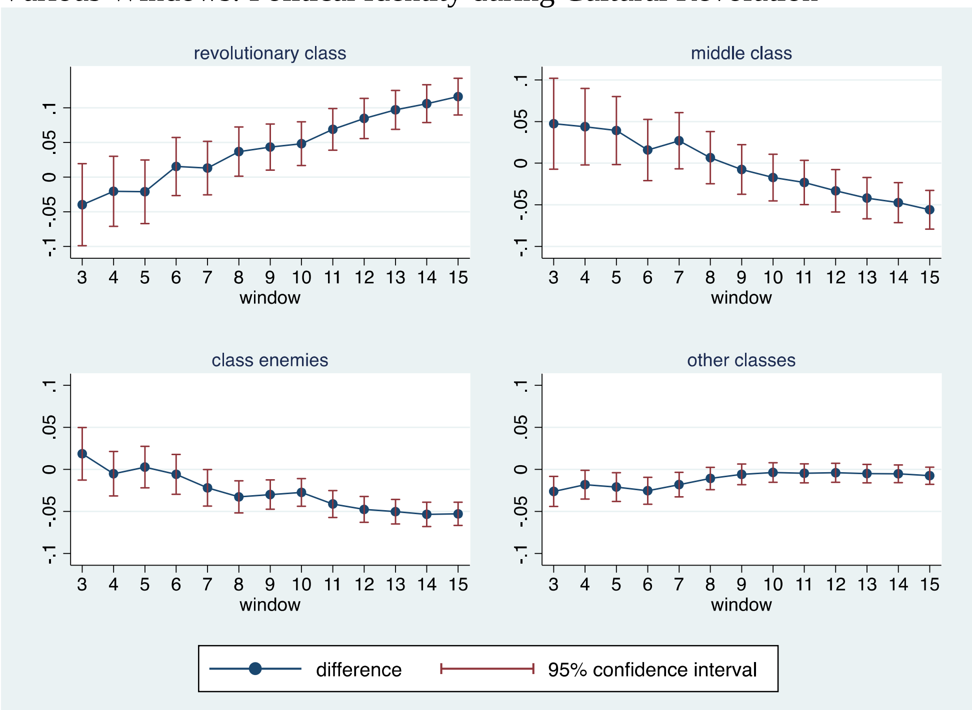
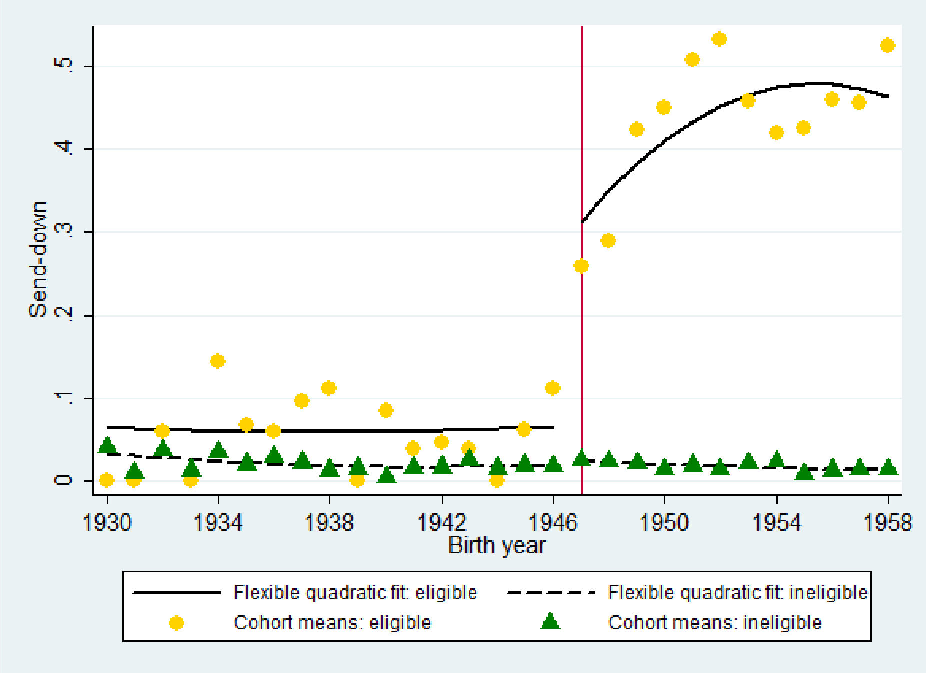


Figure 2C. Difference between Send-down-eligible and Send-down-ineligible Cohorts with Various Windows: Political Identity during Cultural Revolution



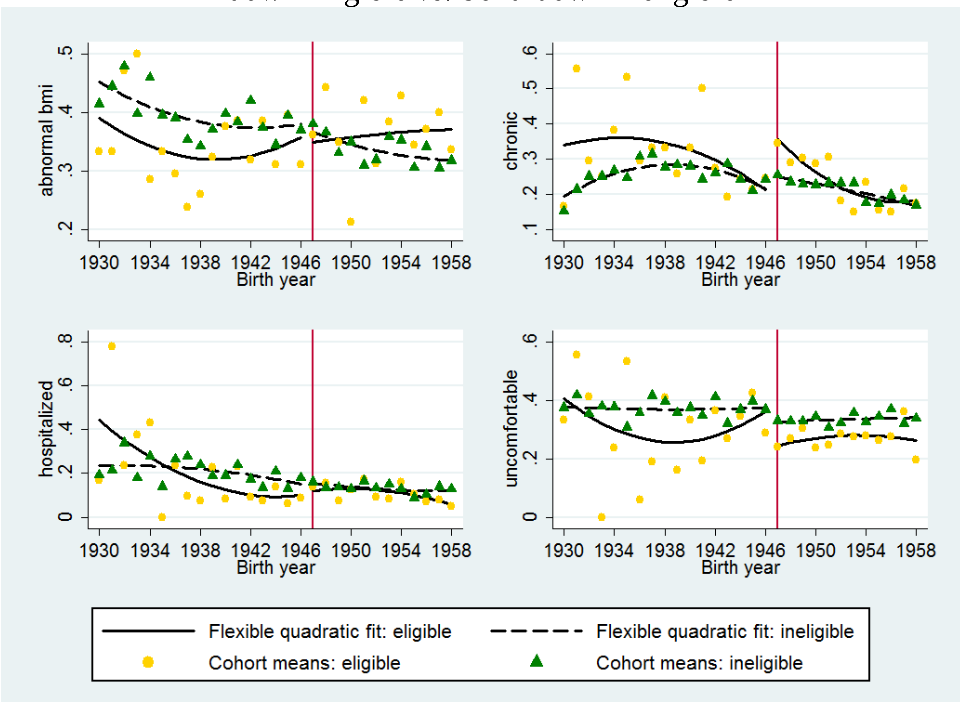
Note: Figures show differences in predetermined characteristics between send-down-eligible and send-down-ineligible cohorts. In each graph, the horizontal axis is the window around the cutoff cohort (birth cohort 1947). For instance, points corresponding to window 7 are calculated differences between cohorts 1947-1953 (send-down eligible) and cohorts 1940-1946 (send-down ineligible).

Figure 3. Cohort Means of Send-down: Send-down Eligible vs. Send-down Ineligible



Note: This figure shows the discontinuity in the send-down probability at the cutoff birth cohort 1947 among send-down-eligible cohorts. Circles and triangles represent send-down probability for each cohort, and the lines show fitted values from flexible quadratic regression.

Figure 4. Estimated Discontinuities at Cohort Cutoff 1947 in Physical Health: Send-down Eligible vs. Send-down Ineligible



Note: These figures show the estimated discontinuities in four physical health measures at the cutoff birth cohort 1947 among send-down-eligible cohorts and send-down-ineligible cohorts. Circles and triangles represent the mean values for each cohort, and the lines show fitted values from flexible quadratic regressions.

Figure 5A. Estimated Discontinuities at Cohort Cutoff 1947 in Mental Health: Send-down Eligible vs. Send-down Ineligible

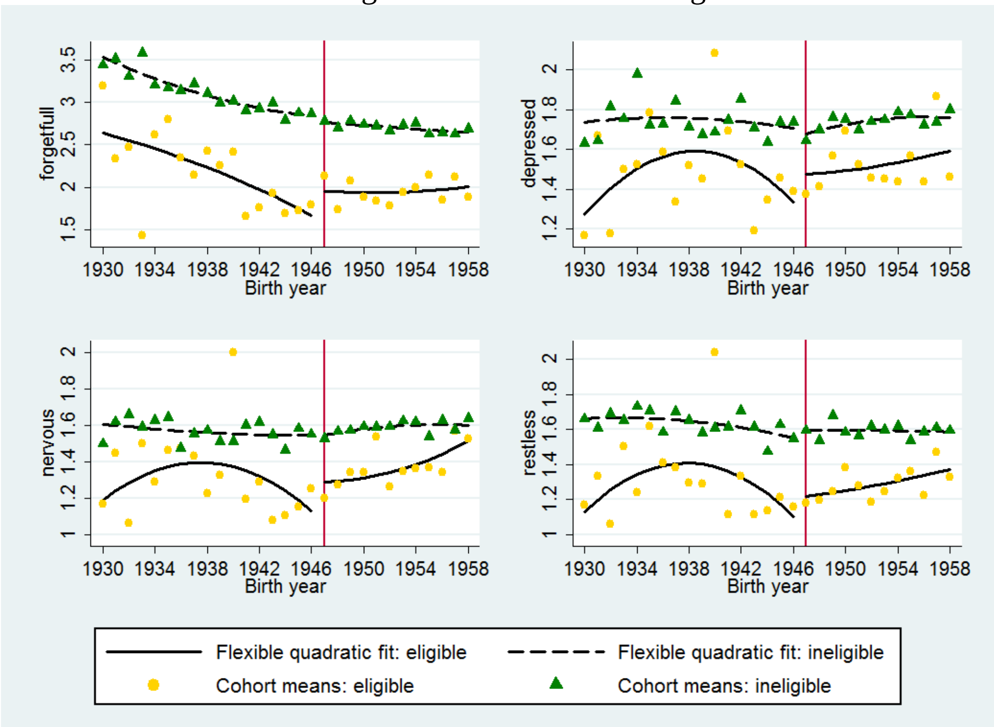
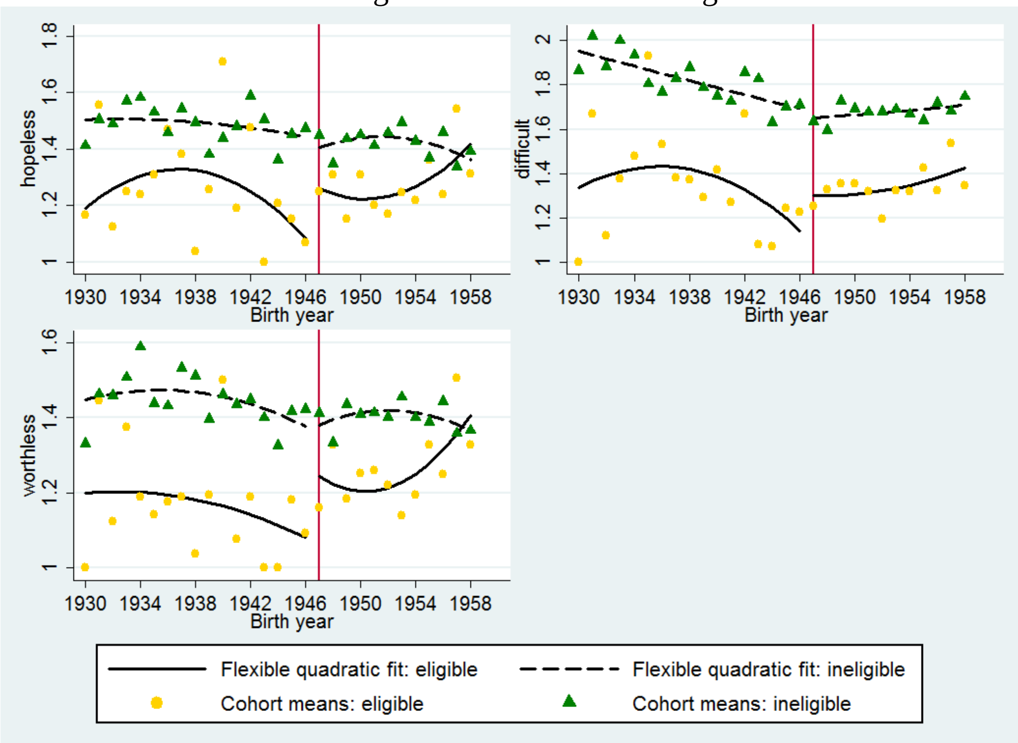


Figure 5B. Estimated Discontinuities at Cohort Cutoff 1947 in Mental Health: Send-down Eligible vs. Send-down Ineligible



Note: These figures show the estimated discontinuities in seven mental health measures at the cutoff birth cohort 1947 among send-down-eligible cohorts and send-down-ineligible cohorts. Circles and triangles represent the mean values for each cohort, and the lines show fitted values from flexible quadratic regressions.

Table1. Outcome Variables and Corresponding Survey Questions

Category	Variable	**Survey Questions**
Physical health (1-yes;0-no)	**Abnormal BMI**	(1) Being underweight (BMI<18.5) or overweight (BMI>25), based on current height and weight.
	Chronic	(2) During the past six months, have you had any doctor-diagnosed chronic disease?
	Hospitalized	(3) Were you hospitalized last year due to illness/injury?
	Uncomfortable	(4) During the past two weeks, have you felt physically uncomfortable?
Mental health (rate on a scale of 1 to 5)	**Forgetful**	(1) Are you able to remember the important things that have happened to you within a week? (1-all; 5-a little bit)
	Depressed	(2) How often have you felt depressed and could not cheer up in the past month? (1-never; 5-almost every day)
	Nervous	(3) How often have you felt nervous in the past month? (1-never; 5-almost every day)
	Restless	(4) How often have you felt agitated or upset and could not remain calm in the past month? (1-never; 5-almost every day)
	Hopeless	(5) How often have you felt hopeless about the future? (1-never; 5-almost every day)
	Difficult	(6) How often have you felt that everything is difficult? (1-never; 5-almost every day)
	Worthless	(7) How often do you think life is meaningless? (1-never; 5-almost every day)

Table 2. First Stage Results

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	First Stage	
				Send-down
1930-1958 cohorts	Cubic	No	I(cohort>=1947)	0.257***
				(0.080)
				11,810
1930-1958 cohorts	Quadratic	Yes	I(cohort>=1947)	0.242***
				(0.088)
				11,810
1940-1953 cohorts	Quadratic	No	I(cohort>=1947)	0.203**
				(0.096)
				6,347
1940-1953 cohorts	Linear	Yes	I(cohort>=1947)	0.224**
				(0.093)
				6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 3. Effects on Physical Health

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Cubic	No	0.000	0.448**	0.070	-0.283	0.123
			(0.210)	(0.208)	(0.141)	(0.202)	(0.252)
			11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	0.014	0.488**	-0.001	-0.389	0.057
			(0.269)	(0.196)	(0.229)	(0.282)	(0.359)
			11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	0.180	0.802***	0.373	-0.223	0.693
			(0.497)	(0.305)	(0.234)	(0.286)	(0.495)
			6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	0.159	0.724***	0.342	-0.210	0.625
			(0.382)	(0.247)	(0.209)	(0.228)	(0.524)
			6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 4. Effects on Mental Health

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Nervous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Cubic	No	1.324*	0.578	0.649**	0.252	0.706	0.818*	0.492	0.908**
			(0.780)	(0.390)	(0.269)	(0.332)	(0.525)	(0.426)	(0.490)	(0.381)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	1.724**	0.933*	0.956**	0.551	1.103	1.058**	0.646	1.337***
			(0.800)	(0.508)	(0.422)	(0.391)	(0.734)	(0.521)	(0.549)	(0.459)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	2.020*	1.472**	1.177*	0.884	1.601*	1.076*	1.128	1.871***
			(1.069)	(0.667)	(0.710)	(0.729)	(0.944)	(0.628)	(0.751)	(0.560)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	1.778**	1.474***	1.177*	0.900	1.505*	0.977*	1.043*	1.801***
			(0.725)	(0.554)	(0.690)	(0.693)	(0.807)	(0.509)	(0.605)	(0.505)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 5. Post-send-down Outcomes

			[1]	[2]	[3]	[4]	[5]	[6]	[7]
			Post-send-down Outcomes						
Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	Schooling years	Total income	Being single	Being divorce	Age at marriage	Age at birth	No. of children
1930-1958 cohorts	Cubic	No	-1.819	15.121	0.041	0.153	-0.578	2.641	0.711
			(1.947)	(20.747)	(0.043)	(0.101)	(3.241)	(2.100)	(0.688)
			11,802	11,810	11,806	11,806	10,607	11,503	11,810
1930-1958 cohorts	Quadratic	Yes	-2.945	16.275	0.038	0.206**	-1.239	2.567	0.985
			(2.493)	(15.190)	(0.052)	(0.101)	(3.980)	(3.462)	(1.004)
			11,802	11,810	11,806	11,806	10,607	11,503	11,810
1940-1953 cohorts	Quadratic	No	-4.714	15.752	0.026	0.166	0.437	4.433	0.546
			(3.477)	(25.862)	(0.073)	(0.169)	(3.993)	(3.884)	(1.032)
			6,342	6,347	6,345	6,345	5,712	6,188	6,347
1940-1953 cohorts	Linear	Yes	-4.600	-2.114	0.031	0.157	0.298	3.844	0.423
			(3.069)	(2.040)	(0.062)	(0.133)	(2.892)	(3.562)	(0.814)
			6,342	6,347	6,345	6,345	5,712	6,188	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Income is in thousand yuan

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 6. Heterogeneous Effects by Gender

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1]	[2]	[3]	[4]	[5]
			Physical Health Measures				
			Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Quadratic	Yes	0.038	-0.420	-0.506	-0.861	-1.017*
Males			(0.479)	(0.559)	(0.681)	(0.535)	(0.601)
			5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Quadratic	Yes	0.046	1.170**	0.347	-0.057	0.770
Females			(0.277)	(0.468)	(0.368)	(0.425)	(0.510)
			5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2							
χ2			0.000	5.551	1.483	2.427	
Prob > χ2			0.983	0.019	0.223	0.119	

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
			Mental Health Measures							
			Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Quadratic	Yes	0.904	1.101	1.459**	0.572	0.923	0.894	0.706	1.072
Males			(1.383)	(0.997)	(0.700)	(1.039)	(1.362)	(1.209)	(1.161)	(1.036)
			5,936	5,936	5,936	5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Quadratic	Yes	2.309**	0.750	0.564	0.579	1.240*	1.191*	0.651	1.546***
Females			(1.020)	(0.647)	(0.808)	(0.658)	(0.702)	(0.703)	(0.450)	(0.594)
			5,874	5,874	5,874	5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2										
χ2			0.858	0.197	0.884	0.000	0.194	0.067	0.005	
Prob > χ2			0.354	0.657	0.347	0.993	0.660	0.796	0.941	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 7. Heterogeneous Effects by No. of Siblings

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1]	[2]	[3]	[4]	[5]			
			Physical Health Measures							
			Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES			
1930-1958 cohorts	Quadratic	Yes	-0.212	0.178	0.103	-0.628	-0.315			
More siblings			(0.288)	(0.236)	(0.247)	(0.384)	(0.398)			
			7,914	7,914	7,914	7,914	7,914			
1930-1958 cohorts	Quadratic	Yes	0.585	1.508	-0.127	0.185	1.304			
Fewer siblings			(5.121)	(5.091)	(1.100)	(3.412)	(7.628)			
			3,896	3,896	3,896	3,896	3,896			
Test Ho: ß1=ß2										
$\chi2$			1.037	4.003	0.128	3.729				
Prob > $\chi2$			0.309	0.0454	0.721	0.0535				
Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
			Mental Health Measures							
			Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Quadratic	Yes	0.686	-0.035	0.392	-0.127	0.601	0.892	0.526	0.685
More siblings			(0.851)	(0.781)	(0.457)	(0.459)	(0.706)	(0.594)	(0.676)	(0.578)
			7,914	7,914	7,914	7,914	7,914	7,914	7,914	7,914
1930-1958 cohorts	Quadratic	Yes	5.010	3.684	2.499	2.367	2.594	1.343	1.053	3.216
Fewer siblings			(23.327)	(13.852)	(3.996)	(7.746)	(11.806)	(6.134)	(5.220)	(15.738)
			3,896	3,896	3,896	3,896	3,896	3,896	3,896	3,896
Test Ho: ß1=ß2										
$\chi2$			2.965	5.637	5.525	6.339	3.523	0.184	0.334	
Prob > $\chi2$			0.085	0.018	0.019	0.012	0.061	0.668	0.564	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Appendix

Figure A1. Distribution of Birth Cohort Using 1982 Census Data

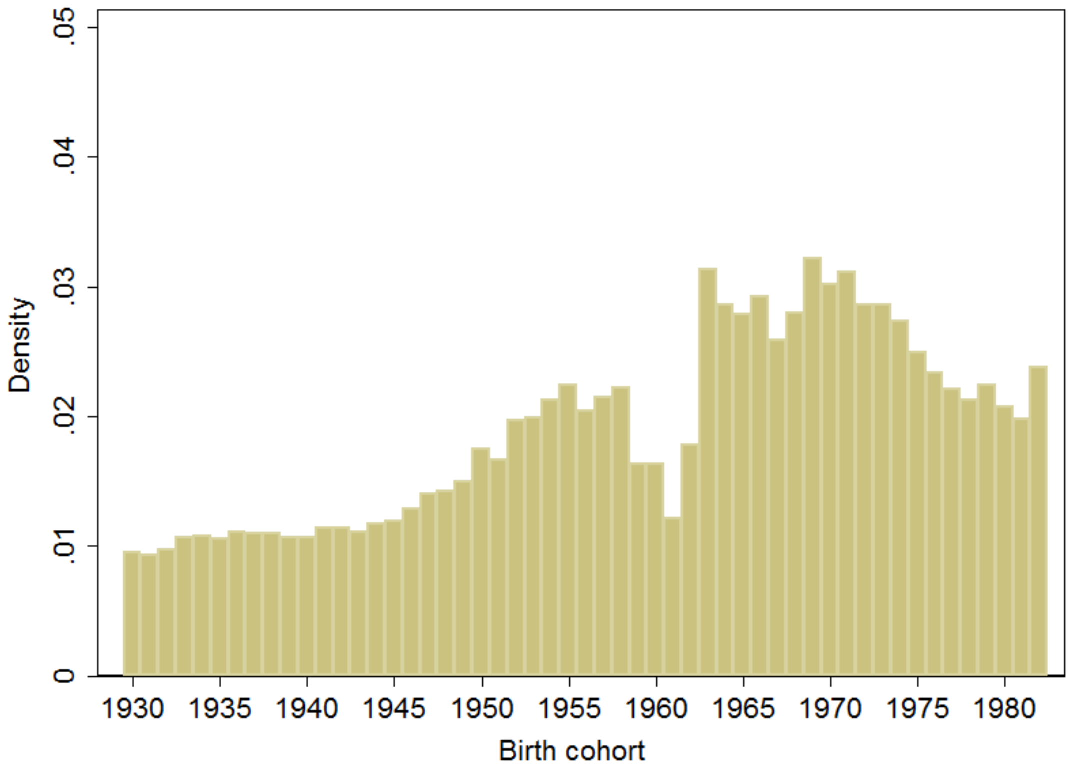


Figure A2. Distribution of Birth Cohort Using 1990 Census Data

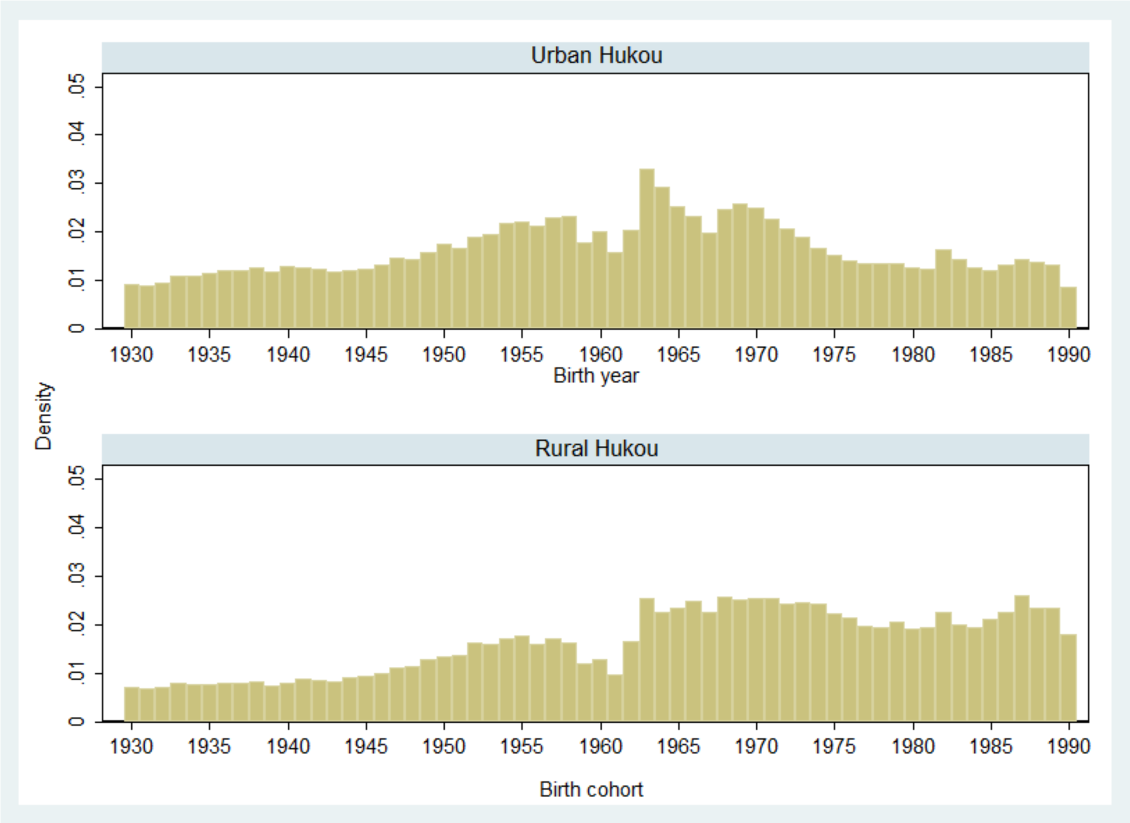


Figure A3. Distribution of Birth Cohort using 2000 Census Data

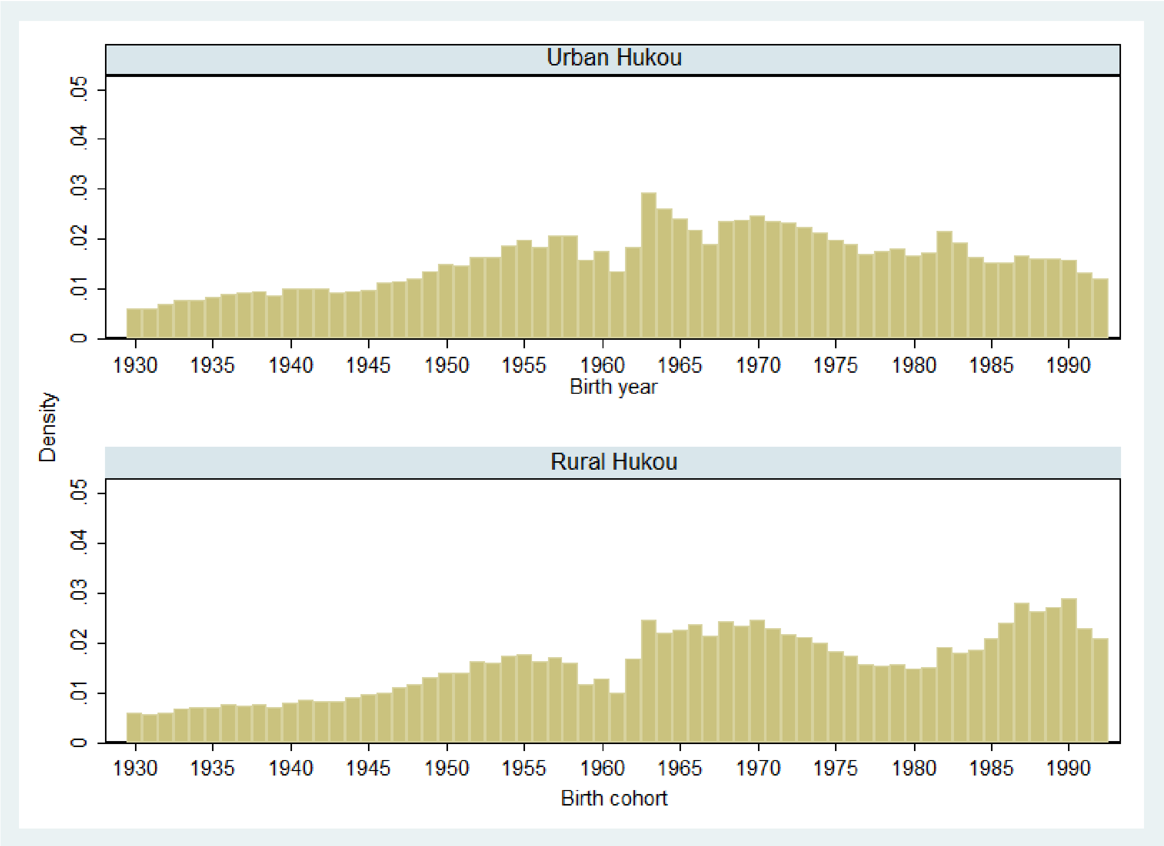


Table A1. Summary Statistics (1930-1958 cohorts)

	[1]	[2]	[3]	[4]	[5]	[6]	[5]	[6]
	Send-down Eligibles			**Send-down Ineligibles**			**Difference**	
Total number of observations	**1477**			**10333**			**[1]-[4]**	
Variables	**Mean**	**S.D.**	**# Obs**	**Mean**	**S.D.**	**# Obs**	**Diff.**	**s.e.**
Send-down	0.346	0.476	1,477	0.018	0.132	10333	0.328***	(0.012)
Physical health measures								
Abnormal BMI (1-yes; 0-no)	0.358	0.480	1,477	0.355	0.478	10333	0.004	(0.013)
Chronic (1-yes; 0-no)	0.240	0.427	1,477	0.227	0.419	10325	0.014	(0.012)
Hospitalized (1-yes; 0-no)	0.118	0.420	1,477	0.158	0.514	10328	-0.039***	(0.012)
Uncomfortable (1-yes; 0-no)	0.278	0.448	1,477	0.349	0.477	10330	-0.070***	(0.013)
Mental health measures								
Forgetful (1-all; 5-a little bit)	1.990	1.147	1,464	2.834	1.401	10270	-0.844***	(0.033)
Depressed (1-never; 5-almost every day)	1.521	0.873	1,466	1.741	1.004	10232	-0.220***	(0.025)
Nervous (1-never; 5-almost every day)	1.366	0.767	1,467	1.576	0.893	10261	-0.210***	(0.022)
Restless (1-never; 5-almost every day)	1.301	0.706	1,466	1.603	0.924	10260	-0.302***	(0.021)
Hopeless (1-never; 5-almost every day)	1.279	0.723	1,464	1.443	0.872	10224	-0.164***	(0.021)
Difficult (1-never; 5-almost every day)	1.348	0.776	1,468	1.722	1.064	10262	-0.374***	(0.023)
Worthless (1-never; 5-almost every day)	1.244	0.663	1,466	1.416	0.849	10238	-0.172***	(0.019)
Other variables:								
Education (years)	10.020	3.361	1,469	3.213	4.156	10333	6.807***	(0.097)
Total income (1000 *yuan*)	13.448	30.443	1,477	5.396	10.820	10333	8.052***	(0.799)
Marriage status			1,477			10329		
-never married	0.007	0.082	10	0.011	0.104	114	-0.004*	(0.002)
-married	0.884	0.320	1,306	0.843	0.363	8712	0.041***	(0.009)
-cohabitating	0.002	0.045	3	0.001	0.033	11	0.001	(0.001)
-divorced	0.040	0.196	59	0.008	0.088	80	0.032***	(0.005)
-widowed	0.067	0.250	99	0.137	0.344	1412	-0.070***	(0.007)

Table A1. Summary Statistics (1930-1958 cohorts) (cont.)

	[1]	[2]	[3]	[4]	[5]	[6]	[5]	[6]
	Send-down Eligibles			**Send-down Ineligibles**			**Difference**	
Total number of observations	**1477**			**10333**			**[1]-[4]**	
Variables	**Mean**	**S.D.**	**# Obs**	**Mean**	**S.D.**	**# Obs**	**Diff.**	**s.e.**
Age at marriage	25.536	4.113	1,421	22.129	4.487	9186	3.406***	(0.119)
Age at birth	27.576	4.087	1,416	24.752	4.623	10087	2.825***	(0.118)
No. of children	1.539	1.063	1,477	2.746	1.402	10333	-1.207***	(0.031)
Family background during Cultural Revolution:			1,469			10282		
-revolutionary class	0.699	0.459	1,025	0.717	0.450	7375	-0.018	(0.013)
-middle class	0.205	0.404	301	0.219	0.414	2251	-0.014	(0.011)
-class enemies	0.063	0.243	92	0.058	0.235	601	0.004	(0.007)
-others	0.033	0.178	48	0.005	0.073	55	0.027***	(0.005)
Birth weight (k.g.)	3032.9	559.89	434	2833.3	571.85	1872	199.6***	(29.933)
Number of siblings	3.295	1.961	1,425	3.425	2.008	10177	-0.129**	(0.056)
Gender (male=1)	0.508	0.500	1,477	0.502	0.500	10333	0.006	(0.014)
Ethnic minority	0.043	0.202	1,477	0.075	0.264	10304	-0.033***	(0.006)
Father's education (years)	5.334	5.056	196	2.338	3.664	798	2.996***	(0.383)
Mother's education (years)	2.413	3.983	363	0.48	1.812	1677	1.934***	(0.213)
Father's age at first birth	30.305	7.521	940	29.897	8.00	5815	0.409	(0.267)
Mother's age at first birth	27.095	6.610	981	27.226	7.141	5832	-0.131	(0.231)
Weeks separated from father during age 0-12	25.418	87.455	1,419	27.012	94.211	9816	-1.594	(2.508)
Weeks separated from mother during age 0-12	11.710	60.313	1,444	15.959	72.860	9979	-4.249**	(1.746)
Had ever migrated during age 0-12	0.913	0.281	1,467	0.986	0.117	10290	-0.073***	(0.007)

Table A2. Mean Comparison for Other Covariates

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Had Urban Hukou at Age 12							
	1940-1946 Cohorts			**1947-1953 Cohorts**				**Ho: diff=0**
Outcome	**Mean**	**S.D.**	**# Obs**	**Mean**	**S.D.**	**# Obs**	**Difference**	**P-value**
Family background during Cultural Revolution:								
-revolutionary class	0.680	0.468	203	0.691	0.463	492	-0.011	0.771
-middle class	0.187	0.391	203	0.215	0.412	492	-0.028	0.404
-class enemies	0.089	0.285	203	0.067	0.250	492	0.022	0.321
-other classes	0.044	0.206	203	0.026	0.161	492	0.018	0.221
Number of siblings	3.507	2.166	201	3.475	1.948	476	0.033	0.847
Gender (male=1)	0.541	0.499	205	0.494	0.500	496	0.048	0.253
Ethnic minority	0.059	0.235	205	0.046	0.210	496	0.012	0.502
Had low birth weight	0.058	0.235	52	0.073	0.262	150	-0.016	0.704
Father's education (years)	6.750	4.743	8	5.754	5.150	65	0.996	0.605
Mother's education (years)	1.773	3.023	22	2.372	4.064	125	-0.599	0.511
Father's age at first birth	30.220	8.036	132	30.489	7.892	309	-0.269	0.745
Mother's age at first birth	26.635	6.612	126	27.367	7.174	330	-0.732	0.320
Weeks separated from father during age 0-12	22.914	69.792	198	20.123	81.030	479	2.791	0.672
Weeks separated from mother during age 0-12	13.856	63.203	201	8.057	49.790	487	5.798	0.201
Had ever migrated during age 0-12	0.936	0.246	202	0.915	0.279	495	0.020	0.363

Notes: 1.Data include 1940-1953 cohorts who had urban Hukou status at age 12.

2. Urban observations with below junior high school education levels are excluded from the sample.

Table A3a. The Cultural Revolution Effect

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1] Abnormal BMI	[2] Chronic	[3] Hospitalized	[4] Uncomfortable	[5] AES
			Physical Health Measures				
1930-1958 cohorts	Cubic	No	-0.039	0.566	0.244	-0.307	0.233
Fierce violence			(0.414)	(0.501)	(0.424)	(0.339)	(0.632)
			7,107	7,107	7,107	7,107	7,107
1930-1958 cohorts	Cubic	No	0.321	0.239	-0.379	-0.467	-0.172
Fewer violence			(0.490)	(0.270)	(0.392)	(0.541)	(0.623)
			4,703	4,703	4,703	4,703	4,703
Test Ho: ß1=ß2							
χ2			0.424	0.397	1.224	0.0814	
Prob > χ2			0.515	0.529	0.269	0.775	
1930-1958 cohorts	Quadratic	Yes	0.044	0.518	0.038	-0.444	0.071
Fierce violence			(0.502)	(0.444)	(0.452)	(0.483)	(0.611)
			7,107	7,107	7,107	7,107	7,107
1930-1958 cohorts	Quadratic	Yes	0.248	0.311	-0.318	-0.537	-0.169
Fewer violence			(0.533)	(0.462)	(0.474)	(0.624)	(0.602)
			4,703	4,703	4,703	4,703	4,703
Test Ho: ß1=ß2							
χ2			0.139	0.213	0.320	0.018	
Prob > χ2			0.709	0.645	0.571	0.894	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Violent provinces: Beijing, Shanghai, Shaanxi, Zhejiang, Chongqing, Hubei, Hunan, Tianjin, Heilongjiang, Hebei, Shandong, Guizhou, Liaoning, Anhui, Qinghai, and Henan

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A3a. The Cultural Revolution Effect (cont.)

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1]	[2]	[3]	[4]	[5]
			Physical Health Measures				
			Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1940-1953 cohorts	Quadratic	No	0.374	0.987**	0.617	-0.022	1.215
Fierce violence			(0.708)	(0.481)	(0.478)	(0.422)	(0.863)
			3,843	3,843	3,843	3,843	3,843
1940-1953 cohorts	Quadratic	No	0.076	0.382	-0.051	-0.813	-0.168
Fewer violence			(32.517)	(3.165)	(26.954)	(10.479)	(24.035)
			2,504	2,504	2,504	2,504	2,504
Test Ho: ß1=ß2							
χ2			0.168	1.039	0.981	1.177	
Prob > χ2			0.682	0.308	0.322	0.278	
1940-1953 cohorts	Linear	Yes	0.302	0.866**	0.537	-0.038	1.040*
Fierce violence			(0.525)	(0.382)	(0.361)	(0.354)	(0.591)
			3,843	3,843	3,843	3,843	3,843
1940-1953 cohorts	Linear	Yes	0.124	0.397	-0.006	-0.799	-0.098
Fewer violence			(0.741)	(0.514)	(0.704)	(0.910)	(0.836)
			6,347	6,347	6,347	6,347	6,347
Test Ho: ß1=ß2							
χ2			0.073	0.678	0.821	1.419	
Prob > χ2			0.787	0.41	0.365	0.234	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Violent provinces: Beijing, Shanghai, Shaanxi, Zhejiang, Chongqing, Hubei, Hunan, Tianjin, Heilongjiang, Hebei, Shandong, Guizhou, Liaoning, Anhui, Qinghai, and Henan

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A3b. The Cultural Revolution Effect

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Nervous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Cubic	No	0.578	0.297	0.625	0.222	0.586	0.614	0.446	0.964
Fierce violence			(1.103)	(0.587)	(0.459)	(0.376)	(0.699)	(0.709)	(0.398)	(0.609)
			7,107	7,107	7,107	7,107	7,107	7,107	7,107	7,107
1930-1958 cohorts	Cubic	No	2.654**	0.753	0.607	0.147	0.981	0.780	0.479	0.871
Fewer violence			(1.215)	(0.735)	(0.478)	(0.740)	(0.931)	(1.051)	(0.846)	(0.632)
			4,703	4,703	4,703	4,703	4,703	4,703	4,703	4,703
Test Ho: ß1=ß2										
χ2			6.677	0.395	0.001	0.011	0.166	0.020	0.003	
Prob > χ2			0.010	0.530	0.977	0.916	0.684	0.888	0.960	
1930-1958 cohorts	Quadratic	Yes	0.933	0.506	0.878	0.446	1.090	0.665	0.516	1.355**
Fierce violence			(0.891)	(0.804)	(0.642)	(0.513)	(0.871)	(0.680)	(0.545)	(0.586)
			7,107	7,107	7,107	7,107	7,107	7,107	7,107	7,107
1930-1958 cohorts	Quadratic	Yes	2.942	1.133	0.942	0.477	1.183	1.046	0.703	1.228
Fewer violence			(1.958)	(0.881)	(0.840)	(0.741)	(1.016)	(1.368)	(0.949)	(0.860)
			4,703	4,703	4,703	4,703	4,703	4,703	4,703	4,703
Test Ho: ß1=ß2										
χ2			4.841	0.669	0.009	0.002	0.011	0.151	0.091	
Prob > χ2			0.0278	0.413	0.926	0.963	0.919	0.698	0.764	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Violent provinces: Beijing, Shanghai, Shaanxi, Zhejiang, Chongqing, Hubei, Hunan, Tianjin, Heilongjiang, Hebei, Shandong, Guizhou, Liaoning, Anhui, Qinghai, and Henan

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A3b. The Cultural Revolution Effect (cont.)

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Nervous	Restless	Hopeless	Difficult	Worthless	AES
1940-1953 cohorts	Quadratic	No	1.179	1.127	0.889	0.685	1.504*	1.077*	0.831*	1.818***
Fierce violence			(1.136)	(0.964)	(0.854)	(0.944)	(0.808)	(0.617)	(0.505)	(0.640)
			3,843	3,843	3,843	3,843	3,843	3,843	3,843	3,843
1940-1953 cohorts	Quadratic	No	3.296	1.315	1.295	0.644	1.627	0.507	1.171	1.574
Fewer violence			(93.385)	(2.149)	(29.131)	(8.218)	(6.026)	(98.795)	(39.267)	(52.895)
			2,504	2,504	2,504	2,504	2,504	2,504	2,504	2,504
Test Ho: ß1=ß2										
χ2			3.625	0.052	0.189	0.002	0.012	0.224	0.221	
Prob > χ2			0.057	0.820	0.664	0.960	0.911	0.636	0.638	
1940-1953 cohorts	Linear	Yes	0.929	1.099*	0.925	0.654	1.332**	0.913**	0.722*	1.675***
Fierce violence			(0.758)	(0.667)	(0.760)	(0.817)	(0.648)	(0.385)	(0.410)	(0.463)
			3,843	3,843	3,843	3,843	3,843	3,843	3,843	3,843
1940-1953 cohorts	Linear	Yes	3.184*	1.519	1.365	0.804	1.778	0.638	1.247	1.746
Fewer violence			(1.888)	(2.071)	(1.584)	(0.931)	(1.517)	(3.490)	(1.438)	(1.286)
			2,504	2,504	2,504	2,504	2,504	2,504	2,504	2,504
Test Ho: ß1=ß2										
χ2			4.378	0.207	0.235	0.045	0.191	0.074	0.584	
Prob > χ2			0.036	0.649	0.628	0.833	0.662	0.785	0.445	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Violent provinces: Beijing, Shanghai, Shaanxi, Zhejiang, Chongqing, Hubei, Hunan, Tianjin, Heilongjiang, Hebei, Shandong, Guizhou, Liaoning, Anhui, Qinghai, and Henan

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A4. Control for Predetermined Characteristics

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Cubic	No	-0.082	0.387*	0.083	-0.300	0.043
			(0.184)	(0.201)	(0.131)	(0.205)	(0.221)
			11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	-0.089	0.386**	-0.005	-0.430	-0.078
			(0.219)	(0.186)	(0.193)	(0.292)	(0.313)
			11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	0.045	0.741**	0.330	-0.294	0.515
			(0.638)	(0.318)	(0.353)	(0.330)	(0.629)
			6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	0.030	0.651**	0.299	-0.272	0.448
			(0.448)	(0.268)	(0.310)	(0.255)	(0.504)
			6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels;see Lee and Card (2008)

2. Pre-determined controls include family background during Cultural Revolution, no. of siblings, gender, race

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A4. Control for Predetermined Characteristics (cont.)

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Cubic	No	1.374*	0.583	0.614**	0.172	0.665	0.732*	0.457	0.852**
			(0.799)	(0.421)	(0.293)	(0.314)	(0.511)	(0.415)	(0.437)	(0.400)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	1.799*	0.914*	0.862**	0.443	0.996	0.882*	0.619	1.242***
			(0.919)	(0.519)	(0.410)	(0.357)	(0.701)	(0.499)	(0.516)	(0.482)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	2.175*	1.346	1.098	0.789	1.494	0.908*	1.122	1.787***
			(1.126)	(0.879)	(0.677)	(0.792)	(0.987)	(0.539)	(0.775)	(0.547)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	1.929***	1.370**	1.093*	0.818	1.403	0.797*	1.050*	1.732***
			(0.739)	(0.668)	(0.633)	(0.704)	(0.859)	(0.418)	(0.629)	(0.502)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Predetermined controls include family background during the Cultural Revolution, no. of siblings, gender, and race

3. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A5. Placebo Test

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Cubic	No	-0.207	-0.096	0.075	-0.059	-0.158
			(0.156)	(0.066)	(0.111)	(0.068)	(0.111)
			11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	-0.234	-0.123	0.046	-0.067	-0.204
			(0.188)	(0.079)	(0.135)	(0.084)	(0.144)
			11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	-0.125	-0.086	-0.035	-0.091	-0.183
			(0.111)	(0.099)	(0.204)	(0.066)	(0.140)
			6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	-0.103	-0.103	-0.032	-0.094	-0.181
			(0.126)	(0.103)	(0.234)	(0.078)	(0.177)
			6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A5. Placebo Test (cont.)

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Cubic	No	0.331	0.069	0.009	-0.004	0.057	0.139	0.060	0.092
			(0.255)	(0.185)	(0.238)	(0.144)	(0.120)	(0.259)	(0.155)	(0.146)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1930-1958 cohorts	Quadratic	Yes	0.344	0.027	0.065	0.017	0.144	0.272	0.105	0.143
			(0.352)	(0.308)	(0.284)	(0.165)	(0.135)	(0.282)	(0.184)	(0.169)
			11,810	11,810	11,810	11,810	11,810	11,810	11,810	11,810
1940-1953 cohorts	Quadratic	No	0.316	-0.127	-0.087	-0.121	-0.004	0.094	-0.051	-0.033
			(0.302)	(0.259)	(0.238)	(0.159)	(0.148)	(0.275)	(0.222)	(0.178)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347
1940-1953 cohorts	Linear	Yes	0.297	-0.127	-0.101	-0.133	0.004	0.105	-0.044	-0.036
			(0.300)	(0.285)	(0.295)	(0.192)	(0.173)	(0.271)	(0.250)	(0.201)
			6,347	6,347	6,347	6,347	6,347	6,347	6,347	6,347

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A6a. Heterogeneous Effects by Gender

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Cubic	No	0.077	-0.221	-0.280	-0.649*	-0.623
Males			(0.353)	(0.348)	(0.497)	(0.334)	(0.450)
			5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Cubic	No	-0.055	0.971*	0.340	-0.009	0.637
Females			(0.357)	(0.550)	(0.452)	(0.367)	(0.524)
			5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2							
χ2			0.092	3.706	0.736	2.125	
Prob > χ2			0.762	0.054	0.391	0.145	
1930-1958 cohorts	Quadratic	Yes	0.038	-0.420	-0.506	-0.861	-1.017*
Males			(0.479)	(0.559)	(0.681)	(0.535)	(0.601)
			5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Quadratic	Yes	0.046	1.170**	0.347	-0.057	0.770
Females			(0.277)	(0.468)	(0.368)	(0.425)	(0.510)
			5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2							
χ2			0.000	5.551	1.483	2.427	
Prob > χ2			0.983	0.019	0.223	0.119	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A6a. Heterogeneous Effects by Gender (cont.)

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1] Abnormal BMI	[2] Chronic	[3] Hospitalized	[4] Uncomfortable	[5] AES
			Physical Health Measures				
1940-1953 cohorts	Quadratic	No	0.185	-0.249	-0.249	-0.516	-0.566
Males			(9.769)	(9.744)	(18.033)	(5.757)	(8.790)
			3,233	3,233	3,233	3,233	3,233
1940-1953 cohorts	Quadratic	No	0.148	1.415*	0.723	-0.078	1.277
Females			(0.510)	(0.833)	(1.153)	(0.701)	(1.171)
			3,114	3,114	3,114	3,114	3,114
Test Ho: ß1=ß2							
χ2			0.010	3.461	0.808	0.533	
Prob > χ2			0.921	0.063	0.369	0.465	
1940-1953 cohorts	Linear	Yes	0.073	-0.265	-0.212	-0.433	-0.559
Males			(1.882)	(2.151)	(3.551)	(1.234)	(1.714)
			3,233	3,233	3,233	3,233	3,233
1940-1953 cohorts	Linear	Yes	0.194	1.288**	0.642	-0.117	1.162*
Females			(0.345)	(0.525)	(0.477)	(0.483)	(0.618)
			3,114	3,114	3,114	3,114	3,114
Test Ho: ß1=ß2							
χ2			0.159	4.47	0.971	0.333	
Prob > χ2			0.69	0.0345	0.324	0.564	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A6b. Heterogeneous Effects by Gender

Regression Specification	Function of Adjusted Cohort	Flexible Polynomial	[1] Forgetful	[2] Depressed	[3] Neverous	[4] Restless	[5] Hopeless	[6] Difficult	[7] Worthless	[8] AES
			Mental Health Measures							
1930-1958 cohorts	Cubic	No	0.621	0.880	1.116***	0.412	0.576	0.683	0.522	0.774
Males			(0.961)	(0.731)	(0.363)	(0.601)	(0.784)	(0.837)	(0.690)	(0.675)
			5,936	5,936	5,936	5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Cubic	No	1.849	0.256	0.182	0.096	0.776	0.918	0.517	1.003
Females			(1.480)	(0.543)	(0.690)	(0.526)	(0.556)	(0.720)	(0.480)	(0.735)
			5,874	5,874	5,874	5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2										
$\chi2$			0.787	0.666	1.478	0.246	0.123	0.070	0.000	
Prob > $\chi2$			0.375	0.414	0.224	0.620	0.726	0.791	0.994	
1930-1958 cohorts	Quadratic	Yes	0.904	1.101	1.459**	0.572	0.923	0.894	0.706	1.072
Males			(1.383)	(0.997)	(0.700)	(1.039)	(1.362)	(1.209)	(1.161)	(1.036)
			5,936	5,936	5,936	5,936	5,936	5,936	5,936	5,936
1930-1958 cohorts	Quadratic	Yes	2.309**	0.750	0.564	0.579	1.240*	1.191*	0.651	1.546***
Females			(1.020)	(0.647)	(0.808)	(0.658)	(0.702)	(0.703)	(0.450)	(0.594)
			5,874	5,874	5,874	5,874	5,874	5,874	5,874	5,874
Test Ho: ß1=ß2										
$\chi2$			0.858	0.197	0.884	0.000	0.194	0.067	0.005	
Prob > $\chi2$			0.354	0.657	0.347	0.993	0.660	0.796	0.941	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A6b. Heterogeneous Effects by Gender (cont.)

			[1]	[2]	[3]	[4]	[5]	[5]	[5]	[5]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Neverous	Restless	AES	Difficult	Worthless	AES
1940-1953 cohorts	Quadratic	No	0.790	1.315	1.389	0.740	1.536	1.027	1.370	1.442
Males			(10.236)	(5.688)	(13.645)	(14.017)	(33.183)	(28.454)	(25.312)	(12.957)
			3,233	3,233	3,233	3,233	3,233	5,936	5,936	5,936
1930-1958 cohorts	Quadratic	No	2.642	1.226	0.744	0.853	1.534	1.155	0.970	2.077**
Females			(1.767)	(1.777)	(1.235)	(1.317)	(1.266)	(1.022)	(0.949)	(0.999)
			3,114	3,114	3,114	3,114	3,114	5,874	5,874	5,874
Test Ho: ß1=ß2										
χ2			1.404	0.009	0.321	0.011	0.000	0.009	0.235	
Prob > χ2			0.236	0.923	0.571	0.917	0.998	0.926	0.628	
1940-1953 cohorts	Linear	Yes	0.695	1.439	1.381	0.848	1.538	0.894	0.706	1.072
Males			(2.174)	(1.491)	(2.765)	(2.844)	(1.525)	(1.209)	(1.161)	(1.036)
			3,233	3,233	3,233	3,233	3,233	5,936	5,936	5,936
1930-1958 cohorts	Linear	Yes	2.276**	1.159	0.763	0.800	1.308	1.191*	0.651	1.546***
Females			(1.119)	(1.259)	(1.123)	(1.131)	(0.899)	(0.703)	(0.450)	(0.594)
			3,114	3,114	3,114	3,114	3,114	5,874	5,874	5,874
Test Ho: ß1=ß2										
χ2			1.242	0.114	0.297	0.002	0.042	0.000	0.433	
Prob > χ2			0.265	0.735	0.586	0.962	0.838	0.996	0.511	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A7a. Heterogeneous Effects by No. of Siblings

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1930-1958 cohorts	Cubic	No	-0.292	0.144	0.147	-0.548	-0.311
More siblings			(0.398)	(0.169)	(0.206)	(0.357)	(0.349)
			7,914	7,914	7,914	7,914	7,914
1930-1958 cohorts	Cubic	No	0.613	1.294	0.017	0.285	1.323
Fewer siblings			(1.290)	(1.129)	(0.842)	(0.613)	(1.451)
			3,896	3,896	3,896	3,896	3,896
Test Ho: ß1=ß2							
$\chi2$			1.213	3.811	0.0488	6.902	
Prob > $\chi2$			0.271	0.051	0.825	0.009	
1930-1958 cohorts	Quadratic	Yes	-0.212	0.178	0.103	-0.628	-0.315
More siblings			(0.288)	(0.236)	(0.247)	(0.384)	(0.398)
			7,914	7,914	7,914	7,914	7,914
1930-1958 cohorts	Quadratic	Yes	0.585	1.508	-0.127	0.185	1.304
Fewer siblings			(5.121)	(5.091)	(1.100)	(3.412)	(7.628)
			3,896	3,896	3,896	3,896	3,896
Test Ho: ß1=ß2							
$\chi2$			1.037	4.003	0.128	3.729	
Prob > $\chi2$			0.309	0.045	0.721	0.054	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A7a. Heterogeneous Effects by No. of Siblings (cont.)

			[1]	[2]	[3]	[4]	[5]
	Function of	Flexible	**Physical Health Measures**				
Regression Specification	Adjusted Cohort	Polynomial	Abnormal BMI	Chronic	Hospitalized	Uncomfortable	AES
1940-1953 cohorts	Quadratic	No	-0.096	0.364	0.344	-0.387	0.137
More siblings			(0.531)	(0.382)	(0.220)	(0.403)	-0.508
			4,272	4,272	4,272	4,272	4,272
1940-1953 cohorts	Quadratic	No	0.882	2.092	0.417	0.135	2.548
Fewer siblings			(4.662)	(3.924)	(1.697)	(1.122)	(6.565)
			2,075	2,075	2,075	2,075	2,075
Test Ho: ß1=ß2							
χ2			0.751	3.566	0.009	2.361	
Prob > χ2			0.386	0.059	0.927	0.124	
1940-1953 cohorts	Linear	Yes	-0.049	0.355	0.301*	-0.364	0.147
More siblings			(0.401)	(0.307)	(0.171)	(0.305)	(0.353)
			4,272	4,272	4,272	4,272	4,272
1940-1953 cohorts	Linear	Yes	0.650	1.824	0.441	0.134	2.253
Fewer siblings			(8.365)	(7.835)	(1.306)	(3.624)	(12.025)
			2,075	2,075	2,075	2,075	2,075
Test Ho: ß1=ß2							
χ2			5.684	0.0426	2.318	1.852	
Prob > χ2			0.0171	0.836	0.128	0.174	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A7b. Heterogeneous Effects by No. of Siblings

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1930-1958 cohorts	Cubic	No	0.556	-0.164	0.210	-0.204	0.336	0.650	0.486	0.468
More siblings			(0.973)	(0.690)	(0.302)	(0.359)	(0.483)	(0.519)	(0.629)	(0.413)
			7,914	7,914	7,914	7,914	7,914	7,914	7,914	7,914
1930-1958 cohorts	Cubic	No	3.219	2.272	1.659*	1.215	1.577	1.045	0.452	1.843
Fewer siblings			(3.005)	(2.065)	(0.860)	(1.313)	(1.851)	(1.244)	(1.147)	(1.946)
			3,896	3,896	3,896	3,896	3,896	3,896	3,896	3,896
Test Ho: ß1=ß2										
χ2			3.454	6.635	4.927	2.908	4.824	0.194	0.003	
Prob > χ2			0.063	0.010	0.026	0.088	0.028	0.660	0.953	
1930-1958 cohorts	Quadratic	Yes	0.686	-0.035	0.392	-0.127	0.601	0.892	0.526	0.685
More siblings			(0.851)	(0.781)	(0.457)	(0.459)	(0.706)	(0.594)	(0.676)	(0.578)
			7,914	7,914	7,914	7,914	7,914	7,914	7,914	7,914
1930-1958 cohorts	Quadratic	Yes	5.010	3.684	2.499	2.367	2.594	1.343	1.053	3.216
Fewer siblings			(23.327)	(13.852)	(3.996)	(7.746)	(11.806)	(6.134)	(5.220)	(15.738)
			3,896	3,896	3,896	3,896	3,896	3,896	3,896	3,896
Test Ho: ß1=ß2										
χ2			2.965	5.637	5.525	6.339	3.523	0.184	0.334	
Prob > χ2			0.085	0.018	0.019	0.012	0.061	0.668	0.564	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table A7b. Heterogeneous Effects by No. of Siblings (cont.)

			[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Function of	Flexible	**Mental Health Measures**							
Regression Specification	Adjusted Cohort	Polynomial	Forgetful	Depressed	Neverous	Restless	Hopeless	Difficult	Worthless	AES
1940-1953 cohorts	Quadratic	No	1.207	0.617	0.644	0.206	0.905	1.214	0.816	1.234**
More siblings			(1.122)	(0.847)	(0.602)	(0.753)	(0.713)	(0.786)	(0.667)	(0.595)
			4,272	4,272	4,272	4,272	4,272	4,272	4,272	4,272
1940-1953 cohorts	Quadratic	No	4.334	4.190	2.819	3.134	3.996	0.459	2.416	4.028
Fewer siblings			(8.583)	(5.772)	(2.537)	(2.698)	(5.007)	(3.185)	(3.088)	(3.956)
			2,075	2,075	2,075	2,075	2,075	2,075	2,075	2,075
Test Ho: ß1=ß2										
$\chi2$			1.656	3.824	3.480	7.989	4.336	0.465	1.886	
Prob > $\chi2$			0.198	0.051	0.062	0.005	0.0373	0.495	0.170	
1940-1953 cohorts	Linear	Yes	1.053	0.634	0.662	0.220	0.862	1.075**	0.752	1.172**
More siblings			(0.827)	(0.608)	(0.576)	(0.656)	(0.621)	(0.538)	(0.541)	(0.515)
			4,272	4,272	4,272	4,272	4,272	4,272	4,272	4,272
1940-1953 cohorts	Linear	Yes	4.024	4.256	2.814	3.230	3.841	0.563	2.361	4.097
Fewer siblings			(25.478)	(24.367)	(10.067)	(13.427)	(21.572)	(4.620)	(14.700)	(20.129)
			2,075	2,075	2,075	2,075	2,075	2,075	2,075	2,075
Test Ho: ß1=ß2										
$\chi2$			4.458	3.273	3.273	8.368	4.701	0.331	2.356	
Prob > $\chi2$			0.035	0.070	0.070	0.004	0.030	0.565	0.125	

Notes: 1. All regressions are clustered by cohort levels; see Lee and Card (2008)

2. Bootstrapped standard errors in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$