Adolescent Environment and Noncognitive Skills

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Abstract

Human skills are formed throughout multiple stages over the life cycle. Different types of skills are manipulable at different ages. This paper investigates whether and how the adolescent environment affects individuals' noncognitive skills. We exploit the "Up to the Mountains and Down to the Countryside Movement" during China's Cultural Revolution, in which millions of urban educated youths were forced to work and live in rural areas. We identify the effect of the "send-down" experience on the youths' locus of control by regression discontinuity. Results show that rusticated youths have less external locus of control, i.e., are less likely to believe that external circumstances, such as luck or powerful others, control their lives. We interpret our findings as a long-run effect of the adolescent experience of adapting to adversity and expending effort that leads to reward. We also find evidence consistent with the dynamic complementarity hypothesis which holds that investments at different ages bolster each other.

Keyword: Noncognitive skills; Adolescent environment; Rusticated youth; China
JEL Classification: J13, O15, Z13

1 Introduction

Human skills are formed throughout multiple stages over the life cycle. Economists have increasingly demonstrated the importance of prenatal and early childhood investment in cognition, which is mostly documented by performance on achievement tests. More rarely investigated is how noncognitive skills—personality, core beliefs, preferences, sociability, etc.—are formed and shaped at different ages. Noncognitive skills are valued across cultures, religions, and societies, and are shown to be as important as cognitive skills in explaining socioeconomic and behavioral outcomes. Once formed, they are typically considered relatively stable across contexts. A central question in human development and policy design, therefore, is how environmental factors and interventions at different stages shape individuals' noncognitive skills.

In skill-formation models, individuals are endowed with abilities and inputs at each stage that produce skills in the next stage (Cunha and Heckman 2007). Different skills are manipulable at different ages. Numerous studies in economics and social psychology have shown that while cognitive skill measures (e.g., IQ scores) become stable by age 10, noncognitive skills develop and mature during the teen years and are malleable until late adolescence (Borghans et al. 2008; Heckman and Kautz 2013). Adolescence is also a period of transition, during which children shift their social worlds outward and are particularly responsive to their external environment, making it a sensitive and critical stage for noncognitive skills. One implication is that individuals' experiences and environments during their adolescent years may have a strong and persistent impact on their noncognitive skills.

This paper empirically demonstrates the effect of adolescent environment on noncognitive skills. Direct investigation of the relationship between environment and skill formation usually suffers from endogeneity issues. For example, people who undergo certain experiences or live in a particular neighborhood may have unobservable attributes that affect both their character skills and choice of environment. To overcome the identification problem, we exploit a large-scale, mandatory social movement in China. In December 1968, the then-leader of China, Mao Zedong, initiated a national movement to send urban junior and senior high school graduates to rural areas. Millions of urban youths were suddenly banished to the countryside, and their lives changed dramatically; they had to work and live with peasants, earned food by hard manual labor every day, and were not allowed to visit their families for years. By the end of the policy in the late 1970s, more than 17 million people had been rusticated (Zhou and Hou 1999; Pan 2002). While there is broad agreement on the social cost of the "send-down" movement, only a few studies have empirically documented the impact on the human capital of rusticated youths (e.g., Meng and Gregory 2002; Li, Rosenzweig

and Zhang 2010), and even fewer on their noncognitive skills.

This unexpected and mandatory movement provides us with a regression discontinuity (RD) design to estimate the impact of send-down on noncognitive skills. Starting in 1968 and ending after Mao's death in 1976, the mandate applied to all eligible urban youths who had just graduated from junior or senior high school. The first and the last sent-down cohorts were the cutoffs for being sent down. Specifically, the cohort born just after September 1947 (or before August 1960) was rusticated, whereas the cohort born just before September 1947 (or after August 1960) was not, and therefore constitutes a good counterfactual. Moreover, to deal with the cohort effect—that individuals born just before and just after the cutoffs could differ for reasons unrelated to send-down—we first control for birth-quarter dummies with the identifying assumption being that cohort effects are the same in different years. To accommodate the possibility that cohort effects at the margin are different from other years, we then use the rural sample (to whom the policy did not apply) to estimate the difference in noncognitive skills between rural individuals born before and after the cutoffs, and subtract it from the estimate using the urban sample—a combined RD and difference-in-difference (RD-DD) estimator.

We measure noncognitive skills by locus of control, which is the extent to which people believe that they have control over their lives (internal control) as opposed to the extent to which they believe that the environment controls their lives (external control). Locus of control has been commonly used in previous studies to analyze noncognitive skills and labor outcomes (Groves 2005; Heckman, Stixrud and Urzua 2006; Heckman and Kautz 2013), and is available in our primary data source, the China Family Survey Panel. The data contain individual-level information on send-down experience, date of birth, urban or rural status at teen years, and socioeconomic characteristics. Both anecdotal evidence and quantitative analysis support our identifying assumption. In particular, a density check of birth cohorts around cutoffs and a balancing test of predetermined characteristics for treatment and control groups both reveal that individuals did not manipulate their birth timing to avoid being sent down.

Our RD and RD-DD estimates show that sent-down individuals have less external locus of control: They tend to believe less in external circumstances—luck or their family's connections, social status or wealth—as the most important determinants for success. We find consistent evidence on all external control indicators, and RD and RD-DD estimations yield similar results. Our findings are also robust to a battery of robustness checks, including alternative bandwidth, parametric estimation, differential effects at two cutoff points, and the inclusion of covariates.

We interpret the change in locus of control as the result of the experience of adapting to

adversity and expending effort that leads to reward during the teen years. Sent-down youths were uprooted, separated from family, and exposed to a completely different and difficult environment, in which external supports and factors were of little help. They needed abilities and social skills to fit into rural society, and earned food and income only by exerting effort in agricultural work. The experience of one's own effort leading to reward is different from that of teenagers who remained in cities and fundamentally changed sent-down youths' views on their control over life. Because there were other events and environmental changes during the Cultural Revolution, we also check the relevance of disrupted education, escaping violence in the cities, and disciplined responses (i.e., bias in answering survey questions) in explaining our results. Both anecdotal and quantitative evidence confirm that these three alternatives are unlikely to drive our results. The estimated effect, therefore, can more likely be attributed to the youths' experiences and environments during the sent-down years.

Our findings support skill-formation models. As conjectured by Cunha and Heckman (2007) we find that environments during sensitive and critical stages—e.g., adolescence for noncognitive skills—have a strong and persistent influence on skills. We also find evidence that is consistent with the dynamic complementarity hypothesis, i.e., that investments at different ages bolster each other. In particular, we show that the documented effects of send-down on locus of control are stronger among individuals with parents who were more educated, parents who remained present throughout childhood, and children came from richer areas, which presumably indicate greater early-life investment in skills.

Our paper mainly contributes to the understanding of skill formation and the effect of the environment at different ages. In the skill-formation literature, empirical evidence for the development of noncognitive skills is thin. The closest to our paper are Malmendier and Nagel (2011), Alesina and Fuchs-Schundeln (2007), and Giuliano and Spilimbergo (2014), who examine the effect of macroeconomic shocks on preference for risks, government intervention, and redistribution, respectively. In line with these studies, we find that economic and social environments have significant impact on the formation of core beliefs. We focus on locus of control as a common measure for noncognitive skills.

A larger body of literature evaluates policies and interventions that target children at different ages. Heckman and Kautz (2013) review the recent literature on measuring and boosting noncognitive skills. Early-intervention programs before formal schooling, such as the Nurse-Family Partnership and Perry Preschool Program, are shown to be effective in improving character skills, which are mostly measured by behavior (Kitzman et al. 2010; Olds et al. 2010; Eckenrode et al. 2010; Heckman, Pinto and Savelyev 2013). Adolescent programs and interventions have not been found to be as effective as programs that target earlier ages, partly due to the absence of measures of noncognitive skills and the relatively

shorter follow-ups.

We innovate in two ways. First, instead of inferring character skills from behavior, we elicit information from a set of locus-of-control survey questions. Second, we are able to follow the affected individual approximately 40 years after the intervention, and can therefore document a relatively long-term effect on their noncognitive skills.

Another line of literature studies the importance of environment, and mainly examines the effects on early childhood well-being and later-life outcomes (Heckman 2000; Currie 2001; Currie and Thomas 2001; Krueger and Whitmore 2001; Garces, Thomas and Currie 2002; Gould, Lavy and Paserman 2011; Carneiro, Løken and Salvanes 2015). Our findings on how adolescent environments shape noncognitive skills are not only of direct importance, but also provide a possible channel through which environments in earlier ages shape decisions, life events, and outcomes in adulthood.

Other research has examined the impact of send-down on individuals' education and life events. Li, Rosenzweig and Zhang (2010) use twin data and find that rusticated individuals did not have worse—and, in some cases, had better—outcomes for health, earnings, career, and social status. Meng and Gregory (2002) and Zhou (2013) find that sent-down individuals were more likely to upgrade their education after returning to the city. Our paper makes a valuable contribution by applying RD to estimate its impact on rusticated youths' noncognitive skills, which are seen as more stable, and may explain a broad range of later life outcomes.

The remainder of the paper is organized as follows. Section 2 briefly describes the skill-formation framework and the send-down movement. Section 3 discusses estimation strategy and particulars. Section 4 describes the data. Section 5 details the main findings and several robustness checks. Section 6 offers possible interpretations of our findings and the relevance of competing hypothesis. Section 7 presents evidence on dynamic complementarity between early-life investment and send-down effects. Section 8 concludes.

2 Framework and Background

2.1 A Skill-Formation Framework

We outline a skill-formation framework in the spirit of Cunha and Heckman (2007). A child's skills, both cognitive and noncognitive, are formed in a multistage technology. Each stage corresponds to a period in the life cycle—for instance, early childhood, adolescence, adulthood, etc.— and can have different technologies. At each stage, inputs such as parental investment, schooling, or policy interventions produce outputs—i.e., skills—at the next stage.

Formally, assume a child is born with initial conditions θ_1. The production function of skills when the child is t years old is

$$\theta_{t+1} = f_t(\theta_t, I_t),$$

where I_t are inputs at stage t. In other words, a child's skill is a function of his/her stock of skills and inputs from the previous stage. We can also write the stock of skills as a function of all past inputs:

$$\theta_{t+1} = g(\theta_1, I_1, I_2, \cdots, I_t)$$

Assume positive and diminishing returns from investment in the last stage, i.e., the production function f_t is strictly increasing and concave in I_t.

Past inputs may have different returns. In particular, some stages may be more productive for certain skills than other stages. such as early childhood for cognitive skills and adolescence for noncognitive skills, and these are referred to as "sensitive periods" for the respective skills (for a comprehensive discussion of interventions at different stages, see Cunha and Heckman 2008). If period t^* is a sensitive period for noncognitive skill θ^{nc}_{t+1}, then holding other conditions constant, interventions that affect noncognitive skills are more influential in stage t^* than in other stages $s \neq t^*$:

$$\frac{\partial \theta_{t+1}}{\partial I_s}\Big|_{I_1,\cdots I_t} < \frac{\partial \theta_{t+1}}{\partial I_{t^*}}\Big|_{I_1,\cdots,I_t}, \quad \text{for all} \quad s \neq t^*$$

An empirical implication is that intervention at the sensitive stage would have a relatively large and persistent effect on skills. This is the first-order effect, and we will test it by examining how interventions in adolescence affect noncognitive skills in the long run.

Meanwhile, the effect of any intervention may depend on the stock of skills and, therefore, inputs in previous stages. For instance, the effects of adolescent environment on noncognitive skills might be stronger for more able children. Specifically, Cunha and Heckman (2007) point out that dynamic complementarity—$\frac{\partial^2 \theta_{t+1}}{\partial \theta_t \partial I_t} > 0$—is essential to explain why children with greater early skills are more productive in later learning of both cognitive and noncognitive skills. This is the second-order effect, and can be related to heterogeneous effects across children with varying stock of skills, e.g., early-life environments and family investments. We will empirically examine the dynamic complementarity between early-life and adolescent environment.

2.2 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 initiative that forced educated youths out of cities to live and work in rural areas. Beginning in the 1950s as a policy response to urban employment problems, it evolved into a political movement during the Cultural Revolution and affected millions of urban youths until its end in the late 1970s.

A small-scale send-down movement started in the early 1950s, following Mao's rallying cry to develop remote regions. In 1955, he commented that "the countryside is a vast expanse of heaven and earth where we can flourish," in an attempt to direct urban unemployed youth to rural areas; the early phase of the send-down movement was mostly voluntary.

With the start of the Cultural Revolution in 1966, schools across the country closed during the first two years, leaving most teenagers idle. Many became Red Guards, whose mission was to harass and attack counter revolutionaries and intellectuals with capitalist leanings in an effort to make education conform to socialism (Bridghan 1967; Heaslet 1972). The Red Guards soon became a destructive force. They harassed ordinary citizens, raided homes, destroyed schools and factories, and engaged in robbery and other criminal behavior.

On December 22, 1968, Mao suddenly asserted that "The 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. The 1968 directive marked the official beginning of the mandatory, large-scale send-down.

The policy, which came as a shock to the people, uprooted millions of youths from their families and exiled them to the countryside and remote areas. The mandate was launched in 1968 and applied to individuals who were registered as urban residents and due to graduate from junior or senior high school. As junior and senior high schools had been closed for much of the first two years of Cultural Revolution, six classes of graduates (1966-1968 cohorts of junior and senior high school graduates) were sent down together in 1968.

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

> I was only 15 when I was sent down. No one wanted to go, but no one could resist. When I refused to go, those in charge of the residential committee came to our home every day and asked us to study Chairman Mao's instructions. A member of the worker's propaganda team came to live in our home and organized

> a study team for my family. My father was a cadre. He was locked up in a study team in his workplace and was not allowed to return home until his children agreed to go to the rural area. In the end, my mother begged me to go to the rural area. (Deng 1993, p. 60)

In 1977, the government relaxed enforcement and brought some youths back to work in the urban labor force or enter college.[1] By 1979, Mao's successors had denounced the send-down policy and allowed all affected youths to return to their home regions.

3 Estimation Strategy

3.1 Estimation Framework

The send-down movement in China uprooted millions of urban teenagers and banished them to rural areas, which completely altered their adolescent lives. We exploit this event to examine the effect of adolescent experience on noncognitive skills. Specifically, we use the regression discontinuity (RD) framework, which is arguably the closest in the observational data analysis to 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 (noncognitive skills—specifically, locus of controls; see Section 4 for details) of individual i being sent down to the countryside and Y_{i0} be the outcome in the absence of send-down, and denote D_i as the status of send-down, i.e., 1 if individual i was sent down and 0 otherwise. The effect of send-down is identified as

$$\beta = E\left[Y_{i1} - Y_{i0}\right]. \tag{1}$$

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

The mandatory send-down movement implies that the probability of being sent down is discontinuous at a cutoff point c_0 of the birth cohort (c_i), i.e., $\lim_{c \downarrow c_0} E\left[D_i|c_i = c\right] \neq \lim_{c \uparrow c_0} E\left[D_i|c_i = c\right]$. Assuming $E\left[Y_{i0}|c_i = c\right]$ is continuous in c at c_0, Hahn, Todd and Van der Klaauw (2001) show that β can be identified as

[1] After Mao's death in September 1976, it became clear that the Cultural Revolution would end and the enforcement of send-down was much relaxed. Furthermore, college admissions were reinstated in 1977, and high school graduates in 1977 were allowed to enter universities.

$$\beta = \frac{\lim\limits_{c\downarrow c_0} E\left[Y_i|c_i=c\right] - \lim\limits_{c\uparrow c_0} E\left[Y_i|c_i=c\right]}{\lim\limits_{c\downarrow c_0} E\left[D_i|c_i=c\right] - \lim\limits_{c\uparrow c_0} E\left[D_i|c_i=c\right]} = \hat{\beta}_{RD}. \tag{2}$$

We estimate $\hat{\beta}_{RD}$ using a nonparametric approach, i.e., local linear regression, as suggested by Hahn et al. (2001). Specifically, $\alpha_1 \equiv \lim\limits_{c\downarrow c_0} E\left[Y_i|c_i=c\right] - \lim\limits_{c\uparrow c_0} E\left[Y_i|c_i=c\right]$ is estimated from

$$\min_{\alpha_1,\gamma_1,\tau_1,\delta_1} \sum_{i=1}^{N} K\left(\frac{c_i - c_0}{h_1}\right)\left[Y_i - \delta_1 - \gamma_1\left(c_i - c_0\right) - \alpha_1 E_i - \tau_1 E_i\left(c_i - c_0\right)\right]^2, \tag{3}$$

where E_i takes a value of 1 if $c_i \geq c_0$ and 0 otherwise; h_1 is the bandwidth; and $K\left(.\right)$ is a kernel function. Similarly, $\alpha_2 \equiv \lim\limits_{c\downarrow c_0} E\left[D_i|c_i=c\right] - \lim\limits_{c\uparrow c_0} E\left[D_i|c_i=c\right]$ is estimated from

$$\min_{\alpha_2,\gamma_2,\tau_2,\delta_2} \sum_{i=1}^{N} K\left(\frac{c_i - c_0}{h_2}\right)\left[D_i - \delta_2 - \gamma_2\left(c_i - c_0\right) - \alpha_2 E_i - \tau_2 E_i\left(c_i - c_0\right)\right]^2. \tag{4}$$

Once we obtain $\hat{\alpha}_1$ and $\hat{\alpha}_2$, $\hat{\beta}_{RD}$ is then calculated as $\hat{\beta}_{RD} = \frac{\hat{\alpha}_1}{\hat{\alpha}_2} = \frac{\alpha_1}{\alpha_2} = \beta$. As only urban cohorts were affected by the send-down movement, we focus on the urban sample in the RD estimation.

As a robustness check, we also calculate $\hat{\beta}_{RD}$ using a parametric approach. Specifically, we use a second-order polynomial function following the suggestion by Gelman and Imbens (2014).

3.2 Estimation Particulars

In this subsection, we provide some particulars of our RD estimations—specifically, the construction of the assignment variable (i.e., birth cohorts), the definition of cutoff points, the selection of optimal bandwidth, and the calculation of standard errors. We also discuss some estimation issues—specifically, seasonality and the local average treatment effect (LATE) vs. the average treatment effect (ATE).

Assignment variable—birth cohorts. The assignment variable in our RD estimation is a grade-based birth cohort—that is, students born in different quarters but in the same grade. Following the system used by the former Soviet Union, schools in China start the academic year in September. The oldest students in a grade were born in September, and the youngest in August of the following year. We sort students into bins of three birth months. The first bin contains students born between September and November, the second between December and February, the third between March and May, and the fourth between June

and August. The assignment variable in our RD estimation, birth cohort (c), is therefore a quarterly variable.[2]

Cutoff points. As described previously, the send-down movement, which was launched in December 1968, required that junior and senior high school students go to the countryside. Because middle schools were closed for much of the first two years of Cultural Revolution (1966 to 1968), the first youths affected were those who had graduated from senior high school in 1966. From the 1950s to the 1980s, children started school at the age of 7 and completed the primary grades in 6 years and junior and senior high school in 3 years each. Therefore, the first cohort c_0^1 affected by the mandatory send-down policy were those born between September 1946 and November 1946. Meanwhile, since the colleague entrance examination was reinstated in 1977, the last cohort c_0^2 affected by the mandatory policy consisted of those who had graduated from junior high school in 1976, i.e., those born between June 1960 and August 1960.

Optimal bandwidth. We calculate the optimal bandwidth h using the method developed by Imbens and Kalyanaraman (2012). Specifically, we separately calculate the optimal bandwidth for equations (3) and (4), then take the minimal of these two to apply to both equations, as suggested by Imbens and Lemieux (2008)—i.e., $h^* = \min\left(h_1^{IK}, h_2^{IK}\right)$. To check whether our results are sensitive to the selected optimal bandwidth, we conduct analyses with bandwidth from $h^* - 8$ to $h^* + 8$ (for a similar exercise, see, e.g., Carneiro, Løken and Salvanes 2015).

As we have two cutoff points in the assignment variable (c_0^1 for the first cohort affected and c_0^2 for the last cohort affected), we center birth cohorts to the cutoff points so that $c \geq 0$ indicates the cohorts affected by the send-down movement and $c < 0$ the cohorts unaffected. We calculate the optimal bandwidth for the two cutoff points separately and pool them together to conduct a local linear estimation. This approach implicitly restricts a common effect at the two cutoff points. As a robustness check, we relax this assumption and allow the effects to differ between the two cutoff points.

Standard errors. We compute clustered standard errors at the assignment-variable level, which allows us to capture random sampling errors and obtain conservative statistical inference.

Seasonality and cohort effect. A potential concern about the RD estimator is that it may also capture the cohort effect—that is, that people born in different quarters are inherently

[2]Our data report the month and year of birth for each individual. We aggregate to quarterly bins to ensure sufficient observations for each bin.

different. In other words, $\hat{\alpha}_1$ and $\hat{\alpha}_2$ becomes$\alpha_1 + \theta^{c_0}_{cohort}$ and $\alpha_2 + \phi^{c_0}_{cohort}$, respectively, where $\theta^{c_0}_{cohort}$ and $\phi^{c_0}_{cohort}$ are cohort effects at cutoff point c_0. As a result, $\hat{\beta}_{RD} = \frac{\hat{\alpha}_1}{\hat{\alpha}_2} \neq \beta$.

We address this issue in two ways. First, we add four quarter dummies in the regressions—specifically, Q_1 (born September–November), Q_2 (December–February), Q_3 (March–May), and Q_4 (June–August). This approach implicitly assumes that the quarter effects are the same across years—in other words, $\theta^{c_0}_{cohort} = \theta_{cohort}$ and $\phi^{c_0}_{cohort} = \phi_{cohort}$. Given that these quarter dummies control for cohort effects, we then have $\hat{\beta}_{RD} = \beta$.

To further accommodate the possibility that the cohort effect at the cutoff point might differ from other years, we include data on rural individuals—who were ineligible for the send-down movement—and combine the RD framework with a DD analysis to obtain an RD-DD estimation. Specifically, by estimating equations (3) and (4) for the urban sample with the inclusion of quarter dummies, we obtain $\hat{\alpha}_{1,urban} = \alpha_1 + \theta^{c_0}_{cohort,urban} - \theta_{cohort,urban}$ and $\hat{\alpha}_{2,urban} = \alpha_2 + \phi^{c_0}_{cohort,urban} - \phi_{cohort,urban}$. Applying the same estimations to the rural sample, we obtain $\hat{\alpha}_{1,rural} = \theta^{c_0}_{cohort,rural} - \theta_{cohort,rural}$ and $\hat{\alpha}_{2,rural} = \phi^{c_0}_{cohort,rural} - \phi_{cohort,rural}$. Therefore, $\hat{\beta}_{RD-DD} = \frac{\hat{\alpha}_{1,urban} - \hat{\alpha}_{1,rural}}{\hat{\alpha}_{2,urban} - \hat{\alpha}_{2,rural}} = \beta$, as long as $\theta^{c_0}_{cohort,urban} - \theta_{cohort,urban} = \theta^{c_0}_{cohort,rural} - \theta_{cohort,rural}$ and $\phi^{c_0}_{cohort,urban} - \phi_{cohort,urban} = \phi^{c_0}_{cohort,rural} - \phi_{cohort,rural}$. In other words, the identifying assumption in the RD-DD estimation becomes that the deviation of cohort effects at the cutoff point from the average cohort effect in the urban sample is the same as that in the rural sample.

LATE vs.ATE. The RD and RD-DD estimators essentially capture the LATE—that is, the effect on the cohort at the cutoff points. To show the generality of our findings, we conduct a DD analysis to calculate the ATE in one robustness check. Specifically, the two DD estimation equations are

$$\begin{aligned} Y_{ic} &= \delta_1 + \alpha_1 Urban_{ic} \times T_{ic} + \gamma_1 Urban_{ic} + \lambda_c + \eta_{ic} \\ D_{ic} &= \delta_2 + \alpha_2 Urban_{ic} \times T_{ic} + \gamma_2 Urban_{ic} + \lambda_c + \varepsilon_{ic}, \end{aligned} \tag{5}$$

where $Urban_{ic}$ is a dummy variable indicating whether individual i from birth cohort c had urban status (*urban hukou*) at the age of 12; λ_c is the birth cohort fixed effects; and T_{ic} is an indicator of birth cohorts subject to the mandatory send-down movement—specifically, individuals born between September 1946 and August 1960. The DD estimator $\hat{\beta}_{DD}$ is then calculated as $\hat{\beta}_{DD} = \frac{\hat{\alpha}_1}{\hat{\alpha}_2}$. There might be autocorrelation across (urban) cohort so that a two-way clustering approach (by place of registered residence at age 12 and cohort) is used to calculated the standard errors for this specification (as suggested by Bertrand, Duflo and Mullainathan 2004 and Cameron, Gelbach and Miller 2011).

3.3 Potential Manipulation

The key identifying assumption of our RD and RD-DD estimations is that $E[Y_{i0}|c_i = c]$ is continuous in c at c_0. As discussed by Lee (2008), this means that people cannot fully manipulate the assignment variable, i.e., the timing of birth. We provide both qualitative and quantitative evidence to show that this identifying assumption has been satisfied in our research setting.

Selection within the cohort. Before discussing the validity of our identification strategy, it is worth noting that our estimation framework allows for a certain degree of manipulation *within* cohorts. Specifically, within the send-down-eligible cohorts, the selection of being sent down is allowed. For instance, students with certain characteristics may have avoided being sent down by deliberately injuring themselves or failing to complete school, and our identification strategy allows for such selection. The rationale is similar to the case of randomized controlled trials that include both compliers and non-compliers: Within the treatment group, people can choose to participate in the treatment (compliers) or not (non-compliers). As long as there is randomization across the treatment and control groups, however, the comparison of outcomes between the whole treatment group and the whole control group can identify the intention-to-treat effect (ITT). In addition, using randomization to instrument for the real status of treatment can identify the LATE.

Anecdotal evidence. Several threads of anecdotal evidence suggest that people cannot fully manipulate the timing of birth, in particular, to avoid the mandatory send-down movement. First, no one could foresee that roughly 20 years later, Mao would issue his mandate in 1968; it is well documented that this came as a shock to most people (e.g., Bernstein 1977; Li, Rosenzweig and Zhang 2010). Similarly, no one could predict that roughly 20 years later, the send-down movement would end in 1977.

Second, using birth quarter as the assignment variable means that to avoid being sent down, people would be able to select precisely in which quarter the child would be born—specifically, to choose between policy-ineligible (June–August) and policy-eligible (September–November) quarters. Cesarean sections were not widely available across China in the 1940s to 1960s, making it difficult to manipulate the timing of birth. Moreover, anecdotal records suggest that there was no fixed day that schools opened in the 1930s in China; therefore, it is unlikely that parents would have adjusted the timing of childbirth so that their children could enter school earlier or later.

Quantitative evidence. To lend further support to our identifying assumption, we provide two sets of quantitative analyses suggested by Lee and Lemieux (2010). First, if there was full

manipulation of birth timing to avoid or participate in the mandatory send-down movement, the distribution of individual characteristics on the two sides of the cutoff points would be different. A mixture of discontinuities in individual characteristics would further imply that the aggregate distribution of the assignment variable is discontinuous at the cutoff points. To this end, we follow McCrary (2008) in conducting an RD analysis of density. Column 1 of Table A1 in Online Appendix A reports the regression results. We do not find any statistically and economically significant discontinuity in the density of birth cohort at the cutoff points for either urban or rural samples. However, a concern about this density check is that we draw data from a survey conducted in 2010, when the relevant cohorts were in their sixties (first cutoff) and fifties (second cutoff). If the probability of surviving to the time of survey changes discontinuously at the cutoff point, it might be possible that this differential mortality rate cancels out the manipulation of childbirth timing, and so we would not find any discontinuity in the observed density of birth cohort in column 1. To check this possibility, we report (column 2) the same regression results using China's population census in 1990,[3] when the cohorts at the two cutoff points were, respectively, in their forties and thirties—ages at which the mortality rate is relatively low. We still do not find economically significant discontinuity in the density of birth cohort at the cutoff points for the 1990 census.

A second check is to directly examine whether individuals' predetermined socioeconomic characteristics are smooth at the cutoff point. If there was full manipulation in our research setting, caused by either manipulation of birth timing or differential mortality rate, we would find discontinuities in these predetermined characteristics at the cutoff points. To this end, we go through 16 predetermined variables that can be identified in the data—gender, weight at birth, ethnicity, urban or rural status at the age of 3, migration history ages 0-12, whether s/he was the first child, number of siblings, family's political identity during the the Cultural Revolution (i.e., 4 for revolutionary class, 3 for middle class, 2 for class enemies, and 1 for other classes), parents' education (2 variables), parents' ages at first childbirth (2 variables) and whether the father/mother was absent when the child was 3 years old and 4-12 years old (4 variables). Table A2 reports the RD and RD-DD regression results. For all the predetermined socioeconomic characteristics, we do not find any statistically and economically significant discontinuities.

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

[3]Note that China had a population census in 1982. However, the 1982 census did not include information on birth month or residence (which is required to break the population into rural and urban), which prevents us from conducting the density test.

4 Data and Variables

Our data come from the 2010 China Family Panel Studies (CFPS), a nationally representative sample of Chinese communities, families, and individuals, that contains data on 15,717 households and 33,600 adult respondents in 2010.[4] The survey includes most questions covered in four U.S. counterpart datasets: the PSID, CDS, HRS, and NYLS. In addition to standard demographic and socioeconomic information, the survey asked about individuals' views and beliefs, and in particular about their locus (sense) of control.

Locus of control. We use survey items regarding locus of control as our noncognitive measures.[5] Locus of control indicates the extent to which people feel that they have control over their lives through self-motivation or self-determination (internal control), as opposed to the extent to which they believe that the environment controls their lives (external control). This psychological concept captures "a generalised attitude, belief or expectancy regarding the nature of the causal relationship between one's own behaviour and its consequences" Rotter (1966).

Locus of control has been commonly used in previous studies of noncognitive skills and labor outcomes (Groves 2005; Heckman, Stixrud and Urzua 2006; Heckman and Kautz 2013). It has been well established that locus of control affects key economic outcomes, including earnings and employment (Andrisani 1977; Goldsmith, Veum and Darity 1997; Groves 2005; Semykina and Linz 2007; Heineck and Anger 2010; Becker et al. 2012), educational attainment (Coleman et al. 1966; Coleman and DeLeire 2003; Barón and Cobb-Clark 2010; Piatek and Pinger 2010) and life satisfaction (Becker et al. 2012). Heckman et al. (2006) show that together with self-esteem, locus of control affects labor-market outcomes and social performance in adulthood, and appears to be as strong as cognitive skills in its effects.[6]

Moveover, locus of control is considered one of the fundamental personality traits that persist across situations and remain stable during adulthood. Cobb-Clark and Schurer (2013) show that changes in locus of control are modest, and are concentrated among the young or very old. This is important to our study, because if these beliefs were more adaptive to environment throughout one's adulthood, they would carry the marks from multiple—and possibly unobservable—events, making it hard to distinguish the source of the effects.

We draw the information on locus of control from eight questions in the CFPS 2010 data.

[4]The survey was conducted biennially from 2010 to 2012; only the 2010 wave is complete and ready for use by researchers.

[5]In an earlier version of the paper we also examined other core values and preferences, including attitudes toward family and relationship, success, and egalitarian society. Results are reported in Online Appendix C.

[6]In the range of psychosocial traits that are prominent in the economic literature, locus of control captures beliefs that correlate with other noncognitive skills, such as self-esteem and personality (big five; Judge et al. 2002), and are complementary to risk, time, and social preference (Becker et al. 2012).

Respondents were asked to rate how much they agree with the following statements on a determinant for success on a scale from 1 (*strongly disagree*) to 5 (*strongly agree*).

1. In today's society, hard work is rewarded.
2. The most important factor affecting one's future success is his/her effort.
3. The higher the level of education one receives, the higher the probability of his/her future success.
4. The most important factor affecting one's future success is his/her talent.
5. The most important factor affecting one's future success is his/her luck.
6. The most important factor affecting one's future success is whether his/her family has connections.
7. The higher a family's social status is, the greater the child's future achievement will be; the lower a family's social status is, the smaller the child's achievement will be.
8. A child from a rich family has a better chance of succeeding in the future; a child from a poor family has a worse chance of succeeding in the future.

The eight questions explore views on the determinants of success and achievement, and in particular are linked to expectations about the future, as opposed to aspects of the current environment.[7] The first four items—hard work, effort, education, and talent— concern internal factors that one can control, while the other four—luck, family relations, social status, and wealth—focus on chance and external circumstances that are beyond one's control. Compared to the general notion of luck versus factors one can control in the Rotter Internal-External Locus of Control Scale (Rotter 1966), the set of questions we use here list concrete factors of success, which allows us to examine people's views on the importance of each factor individually, as well as overall tendencies for internal or external control. It is important to note, however, that internality and externality represent two ends of a continuum, rather than an either/or typology (Rotter 1975).

In Table 1, we list the variable names for the corresponding survey questions. In the empirical analysis, we report the estimates for each of these variables, as well as the average effect size (AES) indices for internal and external control, following Kling et al. (2004).

[7]Psychologists distinguish between locus of control and attributional style, as the former is linked with expectations about the future and the latter to explanations for past outcomes. This distinction is important in our study, because the survey items ask whether people believe they *can* control events affecting them, instead of whether past events were the outcomes of their own actions.

Specifically, let β_k be the estimated send-down coefficient for the outcome variable k, and let σ_k^2 denote the variance of outcome Y_k for the control group. Then the AES index is defined as $\frac{1}{K}\sum_{k=1}^{K}\frac{\beta_k}{\sigma_k}$, where K is the total number of outcome variables. While the individual estimates are interpreted as the send-down effect on the belief about a particular factor of success, the AES index yields more general conclusions about the direction in which send-down shapes locus of control.[8]

[Insert Table 1 here]

Send-down status. The CFPS reports whether the person experienced the send-down movement and his or her place of registered residence (*hukou*) at various ages. We use hukou status at age 12 to identify urban youth, assuming that during junior and senior high school the person lived in the same region he or she lived in at age 12. One concern is that people could have moved from urban to rural areas during that period, and thereby avoided being sent down. In the 1960s and 1970s, however, the government strictly regulated urban-to-rural migration, and status was unlikely to be manipulated.

Regression Sample. We focus on individuals born between 1930 and 1992, because cohorts born before 1930 (i.e., older than 80 years when surveyed) have limited observations in our data and could suffer from selection bias; those born after 1992 (i.e., younger than 18 years when surveyed) were still adolescents, and personality traits were still malleable. The main sample contains 31,506 individuals. Table 2 presents the descriptive statistics for send-down experience, locus of control, and predetermined demographic and socioeconomic characteristics separately for urban and rural populations. The sent-down ratio is 12% for urban people and close to zero for rural people. Mean levels of locus of control, reported separately for each item in the survey, range from 2.81 to 3.86.

[Insert Table 2 here]

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

5 Empirical Findings

5.1 Send-Down Probability and Birth Cohorts

Figures 1a and 1b plot the relation between the sent-down experience (the regressor of interest) and birth cohort (the assignment variable) for urban and rural samples, respectively. Circles represent the ratio of being sent down for each birth cohort; lines indicate the fitted values from the local linear regression, with optimal bandwidth calculated using the method of Imbens and Kalyanaraman (2012); and vertical lines indicate the two cutoff points in the assignment variable.

[Insert Figures 1a and 1b here]

We find a clear jump in the probability of being sent down at the first cutoff (the first cohort subject to mandatory send-down, i.e., individuals born between September and November 1946) and a clear drop at the second cutoff (the last cohort subject to send-down, i.e., individuals born between June and August 1960) in the urban sample. In contrast, for the sample of rural individuals, the probability of being sent down always remains close to zero, and there is no discontinuity at any cohort. These results reflect the effectiveness of the policy changes in 1968 (the beginning of the movement) and 1977 (the end of the movement), and support our research design (i.e., RD and RD-DD estimations).

Table 3 reports the regression results of equation (4), with RD estimates in column 1 and RD-DD estimates in column 2. All regressions control for a linear term of assignment variable c (centered at the cutoff points); an interaction between c and an indicator of being on the right side of the cutoff points $E_i \equiv I[c_i \geq c_0]$; and four quarter dummies, the coefficients of which are suppressed to save space. We find consistent evidence, as seen in Figure 1, that the mandate significantly increased individuals' probability of being sent down by about 20 percentage points.

[Insert Table 3 here]

5.2 The Effect of Send-Down on Locus of Control

In Table 4, we examine whether the send-down experience affects locus of control. Panel a presents the RD estimates and Panel b the RD-DD estimates. Columns 1 to 4 report estimates for outcomes that concern external locus of control, and column 5 reports the AES. Similarly, columns 6 to 9 report the estimates for outcomes concerning internal locus of control, and column 10 reports the AES. We include the same set of control variables as in Table 3, whose coefficients are omitted to save space.

[Insert Table 4 here]

Locus of control—external control. We consistently find that being sent down has negative effects on all four measures of external locus of control. The coefficients are similar for RD and RD-DD estimations, with the latter having more statistical power, presumably due to the larger sample. These estimates suggest that the send-down experience causes people to be (1) less likely to believe in luck as the most important factor of success, (2) less likely to view the family's social status as a factor that determines future achievement, (3) less likely to believe that wealth reflects achievement, and (4) less likely to believe that family wealth increases one's chance of success in the future.

Figures 2a-2d, respectively, display the relation between birth cohort—our assignment variable—and four external-locus-of-control variables for the urban sample, without controlling for quarterly dummies. We find modest drops at the first cutoff point and modest jumps at the second cutoff point, consistent with the findings in Panel a of Table 4.

[Insert Figures 2a-2d here]

Meanwhile, for both the RD and RD-DD estimations, we find that AES indices of external control are negative and statistically significant, further confirming that the send-down experience causes people to have less external locus of control.

To gauge the economic magnitude, we rely on the RD-DD estimates. We find economically meaningful effects: The send-down experience reduces beliefs about *Luck* by 13 percent, *Family Relations* by 16 percent, *Social Status* by 23 percent, and *Family Wealth* by 30 percent.

Locus of control—internal control. For measures of internal locus of control, we find weak and mixed evidence. Coefficients of *Hard Working* are persistently positive but statistically insignificant; coefficients of *Education* and *Talent* are persistently negative, with the latter being statistically significant; and coefficients of *Effort* are small and switch signs between RD and RD-DD estimations. As a result, we find the AES indices statistically insignificant and small in magnitude for both RD and RD-DD estimations.

Figures 3a-3d display the relation between our assignment variable and four internal-locus-of-control variables, respectively. Consistent with the regression results in Panel b of Table 4, we do not find large discontinuities at the two cutoff points, except for the outcome *Talent*. These results suggest that while the send-down experience makes people less likely to believe in talent as the key determinant of future success, the overall effect on internal

locus of control is small.

[Insert Figures 3a-3d here]

5.3 Robustness Checks

In this subsection, we present a battery of robustness checks. Specifically, we check sensitivity to different bandwidths, use a parametric approach to calculate RD and RD-DD estimators, allow for different effects around the two cutoff points, use send-down duration as the regressor of interest, conduct DD estimation, and include predetermined covariates.

Alternative bandwidth. To check whether our findings are sensitive to the optimal bandwidth that we chose using the method of Imbens and Kalyanaraman (2012), we experiment with alternative bandwidth from $h^* - 8$ to $h^* + 8$. Figures 4a and 4b in Online Appendix B report the estimates for external and internal control, respectively. We find stable estimates among all the outcomes, suggesting that our results are not driven by a particular bandwidth.

Parametric estimation. In the baseline estimations, RD and RD-DD estimators are calculated using the nonparametric approach. To check whether the results are sensitive to this method, we conduct a parametric approach (i.e., second-order polynomial function) to calculate the RD and RD-DD estimates. Estimation results are reported in Table B1 in Online Appendix B. We find similar patterns and estimates close to those in the nonparametric estimation, suggesting that our findings are robust.

Differential effects at the two cutoff points. Our research design contains two cutoff points, and in the baseline estimation we assume that the discontinuities in the probability of being sent down are the same at both cutoff points. To relax this restriction, we allow the magnitudes of the discontinuities to be different across cutoff points—specifically, we add the interaction term between treatment and an indicator of the first cutoff point. Estimation results are reported in Table B2, which shows that the our estimates remain similar.

Send-down duration as an alternative regressor of interest. Thus far, we have used a binary variable to characterize the send-down experience. As a robustness check, we use time spent in the countryside during the send-down movement as an alternative measurement. Estimation results are reported in Table B3. We find similar patterns for both RD and RD-DD estimations.

DD estimates. RD and RD-DD estimators essentially capture the LATE. As a comparison with the ATE, we conduct a DD estimation via equation (5). Estimation results are reported

in Table B4. DD estimates have similar patterns as RD and RD-DD estimates—that is, the send-down experience leads to less external locus of control, but has a limited effect on the degree of internal control. The magnitude of DD estimates is generally smaller than that of RD and RD-DD estimates, though they are not statistically different. These results suggest that our findings on the send-down effect on locus of control are not specific to neighborhood cohorts around the cutoff points.

Inclusion of covariates. As a further robustness check, we follow the suggestion of Lee and Lemieux (2010) by including predetermined socioeconomic characteristics as additional controls. Given that the research design is valid, the inclusion of socioeconomic controls should have little effect on our estimates. However, as there are many missing values in the control variable *Birth Weight*, we conduct two exercises—one with all the predetermined variables except for *Birth Weight*, and one with a further control for *Birth Weight*. Regression results are reported in Table B5. We find that our results remain robust to these additional controls, implying the validity of our identification strategy.

6 Interpretation

We documented a significant effect of send-down experience on individuals' noncognitive skills—that is, sent-down people have less external locus of control. In this section, we provide interpretations of this estimated effect. Specifically, inspired by skill-formation models (e.g., Cunha and Heckman 2007) and the impressionable-years hypothesis (e.g., Krosnick and Alwin 1989; Alwin and Krosnick 1991; Alwin et al. 1991), which holds that core beliefs are malleable until the late adolescent years, we interpret our findings as a result of adolescents' adaptation to difficulties and the experience of expending effort that leads to reward. We also check three alternative explanations arising from other shocks during the Cultural Revolution: disrupted education, city violence, and disciplined responses.

6.1 Environment and Experience during the Send-down Years

Theories and empirical findings suggest that the skill-formation process is governed by a multistage technology (Cunha and Heckman 2007). Individuals are endowed with abilities and environmental inputs at each stage that produce output—cognitive and noncognitive skills—at the next stage. Relatedly, the impressionable-years hypothesis in social psychology suggests that individuals' core beliefs form, develop, and mature during a period of great mental plasticity in adolescence and early adulthood (generally considered age 12-18) and remain largely unchanged thereafter (Merelman 1972; Meadow 1982; Krosnick and Alwin

1989; Alwin and Krosnick 1991). This hypothesis is well supported by evidence from neuroscience that the prefrontal cortex remains malleable until the early twenties (Dahl 2004). To the extent that individuals are highly susceptible to attitude changes during adolescence and early adulthood, and that susceptibility drops precipitously thereafter, interventions during these critical years should have a strong and long-lasting effect on one's core beliefs.

Consistent with the models and hypothesis, our findings show that being sent down—that is, a dramatic change in an adolescent's environment—influenced people's beliefs about their control over events and life outcomes. Specifically, we attribute less external control to the youths' adaptation, during their send-down years, to the adversity and the experience of expending effort that leads to reward.

Sent-down youths were forced to develop necessary life and social skills to adapt to a completely different environment. During the 1960s and 1970s, living conditions were much more stringent in the countryside than in the cities. The youths lived with peasants, most of whom had no electricity or running water at home. They were not allowed to visit family for many years, and could not receive support from family or friends in the cities. In addition to the harsh environment, fitting into rural society was also a challenge. Peasants were typically less educated, if not completely illiterate, and only spoke the local dialect. Sent-down youths, therefore, needed social skills to communicate, live, and work with peasants and establish good relationships with local cadres.[9] The youths' well-being in the countryside essentially depended on their own actions and ability to adapt to the environment, which in turn influenced their beliefs about their control over life. Interviews and documents have revealed that many sent-down people associated the difficult experience with developing confidence in their ability to overcome hardship.

> Song: For me, I am rather thankful for that experience because it really tempered me. It built the foundation of my character and made everything else possible for me. It really was a reeducation.
>
> Tan: After that experience, you got a bottom line because you went through so much difficulty and hardship so you develop a certain confidence, that no matter what, you can face anything seemingly insurmountable and you can be persistent in your pursuit no matter what you wanted to do in life. (Rene 2013, p. 174)

In addition, sent-down youths earned their living from manual labor—i.e., they expended effort to obtain rewards. During the same period, urban workplaces based earnings and promotion mainly on seniority; it is hard to control one's career outcomes when advancement

[9] In some areas, local cadres played an important role in deciding when a youth could return to the city (Chen and Cheng 1999).

depends on other workers' tenure in the same organization. Sent-down youths faced a different incentive system. Right out of junior or senior high school, most had little work experience, let alone hard agricultural work. Once sent down, they had to do manual labor every day, earning work points to purchase food and basic living supplies; all rewards and promotions depended on their effort in the field—where work outcomes were easier to predict and control.

> Ding (who was sent down to a rural village near Guangdong) explained the system, which was used throughout the rural villages: "Work points directly affected your food ration, or one portion of your allocated food amount, which was called the labor grain ration.[...] So if I don't work it would mean that I will have to starve because I won't have enough to eat [without the extra portion earned from the labor ration]." (Rene 2013, p. 135)

Sent-down youths were exposed to a completely different environment than those who remained in the cities. Their efforts to adjust to the harsh environment and earn a living from manual labor improved their life and social skills, and altered their views on what determines life and work outcomes. We believe that the experience of living independently and being rewarded by effort rendered them less likely to attribute success or failure to external causes, such as luck or powerful others.

6.2 Competing Hypotheses

In this subsection, we discuss three alternative hypotheses that might also explain our findings.

Disrupted education. Schools and universities throughout the country closed down during the first phrase of the Cultural Revolution. As a result, sent-down individuals may have either delayed or completely forgone the opportunity for higher education. For instance, while cohorts just affected by the send-down policy were unable to attend college after they completed high school, cohorts who just avoided the mandate had about one year of college. If education affects individuals' locus of control, our findings could then be explained by disrupted education rather than the environment and experiences in the countryside.

To assess the relevance of this competing hypothesis, we use data on education attainment. First, before the schools were closed, there were 9.3 college students for every 10,000 people, and the percentage of college students among all registered students was as low as 0.5 percent in 1965. Therefore, the effect of disrupted eduction would be negligible, with such a small share of the population.

Second, when the government abandoned the send-down movement and reopened the college entrance exam in 1979, all students—either sent-down youths who had returned from the countryside or concurrent high school graduates—were eligible to take national college entrance exams and were admitted on an equal footing. The Ministry of Education also established vocational and adult learning institutions, where sent-down individuals could acquire further education. Through these alternative-education programs, the sent-down individual could make up for a disrupted education. Indeed, we find no statistically significant effects of the send-down movement on either total years of schooling or the probability of completing college.[10] Detailed results are presented in Table D1, columns 1 and 2 in Online Appendix D. The fact that sent-down individuals have similar education attainment rejects the chain of causation that runs from send-down to education and, finally, to noncognitive skills. Altogether, we believe that disrupted education is unlikely to be the main channel through which the send-down experience affects people's noncognitive skills.

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

Second, to provide quantitative evidence, we divide the provinces in our sample into two groups: those who suffered from fierce violence in the cities and those who experienced less violence. Specifically, we use death casualties between 1968 and 1971, collected by Walder and Su (2003) from County Annals (*Xian Zhi*), to calculate each province's death count per county and distinguish fierce- and less-violent provinces by sample median. Since youths from different provinces were largely blended into the same rural areas, any differential estimates would indicate the effect of violence in their home cities. Regression results are reported in

[10]It is worth emphasizing that our RD and RD-DD estimation compares the education attainment of cohorts on the margin, i.e., born just before and just after the cutoff. Studies that examine a broader sent-down population—for instance, Meng and Gregory (2002) and Zhou (2013)—find that sent-down people were more likely to upgrade their education after the schools reopened.

Online Appendix Table D2. Most of our estimates are statistically indifferent between the fierce-violence group and the less-violence group, and the differences in AES indices are also small in magnitude. These results imply that city violence cannot explain our findings.

Disciplined responses. While the initial purpose of the send-down movement was to defuse the Red Guards and discipline urban youths, it is possible that sent-down individuals may have perceived and answered survey questions differently from non-sent-down people. Since it was largely a political movement, send-down is more likely to influence correspondents' answers to questions related to politics. We examine how sent-down and non-sent-down people rate the performance of their county/district government, with a higher value indicating a better evaluation. If the send-down movement made individuals more politically sensitive or disciplined, we expect that their rating scores would differ from those of the non-sent-down people. As shown in Online Appendix Table D1, column 3, we do not see any significant differences, and the magnitude of estimated coefficients is close to zero. This suggests that the disciplined-response explanation is not likely to be a driving force in our findings.

7 Dynamic Complementarity

A key feature of skill formation is dynamic complementarity (Cunha and Heckman 2007). In particular, the levels of skill investment at different ages—e.g., early childhood versus adolescence—bolster each other. To test dynamic complementarity in the formation of noncognitive skills, we explore whether the send-down's effect on locus of control differs across individuals with varying skill stocks. Following Heckman and Kautz (2013), who demonstrate the impact of parents, schools, and social environments on skill formation at different ages, we examine whether and how the send-down's effects vary by parents' education, father's or mother's absence when the child was 4-12, and economic conditions during childhood.[11]

Parents' education. Parents' human capital influences the skills and abilities of their children. Children of more educated parents tend to have better health, cognition, education, and labor market outcomes (e.g., Haveman and Wolfe 1995; Holmlund, Lindahl and Plug 2011; Lundborg, Nilsson and Rooth 2014; etc.). One possibility is that more educated parents invest more in their children at early stages, which further bolsters the return from subsequent investments. Along the same lines, we examine the heterogenous effects of

[11]Note that because our treatment and control groups have similar predetermined socioeconomic characteristics, there is no sample selection issue in dividing the samples based on these characteristics.

parents' education. Specifically, we compare the send-down's effects on individuals whose parents are both literate with individuals who have at least one parent who is illiterate.

Table 5 presents the estimation results. The effects on external locus of control tend to be stronger and more statistically significant for the *Literate Parents* group than for the *Illiterate Parents* group. Using the RD estimates, for instance, the AES index for external locus of control is -0.625 and statistically significant for the *Literate Parents* group, compared to an estimate of 0.115, which is statistically insignificant, for the *Illiterate Parents* group. These results indicate that individuals with more educated parents were more responsive to the send-down experience than those with less educated parents.

[Insert Table 5 here]

Parental absence in early stage. Parental absence often has adverse effects on the child's human capital development. Potential mechanisms include a lack of parenting inputs, loss of local earnings and labor, and the psychological costs associated with family separation (e.g., Antman 2013; Zhang et al. 2014). If parents are absent and invest less when the child is very young, it can make later intervention less productive. In our context, we expect smaller send-down effects on noncognitive skills for individuals who were separated from their parents in early years.

Table 6 reports the results by individuals' early experience (age 4-12) of family separation. The estimates show that, compared to sent-down individuals who had at least one parent who was absent during ages 4 to 12, sent-down youths without parental absence tend to have less external locus of control. In other words, the send-down's effect—less external—is stronger among youths whose parents were present in early years. One explanation is that those who had never been separated from their parents had more challenges in adapting to the environment and living independently. The send-down experience was therefore a more intense "treatment" for them, and had a stronger effect on their locus of control.

[Insert Table 6 here]

Economic conditions in early stages. It has been well documented that early life environments (e.g., neighborhood quality) affects adult social and economic outcomes (e.g., Kling, Liebman and Katz 2007; Gould, Lavy and Paserman 2004; Gould, Lavy and Paserman 2011). Considering the surrounding economic conditions as an indicator of early-life environment, we examine whether adolescent environment has a different influence on youths from richer

versus poorer areas. Specifically, we looked at the GDP of one's home province (registered at age 12) in 1952 and divided the provinces by sample median. The idea is that individuals who lived in a wealthier province at age 12 were nurtured in a better environment than those who lived in a poorer province at 12.

As shown in Table 7, we find that youths from richer provinces experience stronger send-down effects on the tendency for external control, while the impact on individuals from poorer provinces is relatively smaller and insignificant.

[Insert Table 7 here]

Throughout these three exercises, we consistently find evidence supporting the dynamic complementarity conjecture of Cunha and Heckman (2007): The send-down's effect on locus of control is stronger when the individual had a better social environment and more parental investment throughout childhood—in particular, those who had more educated parents, whose parents were present during childhood, and who came from richer areas.

8 Conclusion

In this paper we investigate how a large-scale urban-to-rural migration in China affects individuals' noncognitive skills. Using Regression Discontinuity Design, we show that people forced to live and work in the countryside had less external locus of control. Because the sudden and mandatory change in environment occurred during adolescence, when core beliefs and values are highly sensitive to external changes and interventions, we interpret our findings as evidence of the impact of adolescent environment on one's character skills.

We present new evidence for the multistage technology of skill formation, in that inputs during critical and sensitive stages have strong and long-lasting impact on skills, and returns can be fostered by investment in earlier stages. The findings also support the conventional wisdom that environment matters for human development. Our demonstration that environment is important in explaining character differences helps to explain why early-life environment, such as one's neighborhood, affects earnings and well-being in adulthood. Environmental factors and experiences can shape an individual's character, which either directly affects their achievements or indirectly affects their decisions and choices in schooling, the labor market, and other domains.

One implication is for policies that target adolescents. While the sudden and mandatory send-down movement offers us a clean empirical setting, one should be cautious when comparing it to returns from voluntary programs that target disadvantaged children. Returns

from the latter might be higher given that children with larger potential gain are more likely to sign up or lower, if the changes associated with the intervention are relatively mild. Future studies could improve our understanding of this issue by directly and consistently measuring pre- and post-intervention noncognitive skills and following up with individuals at multiple stages of the life cycle.

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