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THE EFFECTS OF SCHOOLING ON COSTLESS HEALTH MAINTENANCE: OVERWEIGHT ADOLESCENTS AND CHILDREN IN RURAL CHINA

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ABSTRACT

Obesity is an important global health problem. Although obesity is not directly related to access to health care or constrained by resource deprivation, overweight status is predominantly found in poor, less-educated populations. This paper seeks to identify the causal role of schooling in affecting obesity among children and adolescents, using new estimation methods that exploit unique panel data on young twins in China. The estimates indicate that higher levels of schooling negatively affect being overweight and positively affect healthy behavior, with a large component of the causal effects due to increased information on the benefits of maintaining a healthy weight. There is also evidence that the higher-income associated with increased schooling increases incentives to invest in health.

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This paper has two aims. The first is to revisit the question of whether increasing schooling causally increases health and health behaviors using unique panel data on twins from rural China. These data permits a new identification strategy that relaxes key assumptions of prior estimation strategies used to identify the causal effects of schooling on health. The second aim is to identify some of the causal mechanisms by which schooling affects health with estimates that have credible internal validity. We focus on the weight of children and adolescents as our principal health outcome, in particular the likelihood of being overweight, but also look at such health behaviors as (not) smoking and exercise as key dependent variables. We focus on weight and these behaviors for two reasons. First, obesity is a relatively new and increasingly important health problem (Swinburn *et al.*, 2019) around the world, especially for poorer populations. Second, weight and weight gain are not outcomes that are directly related to access to health care or are otherwise constrained by lack of resources. Nor is engaging in exercise or abstaining from smoking. Maintaining a healthy weight via exercise or diet is not a matter of affordability - simply put, a sugarless drink costs the same as a sugared one, low-calories foods are no more expensive than high-calories ones and eating less is cheaper than eating more.[1] The absence of roles for health care costs or resource constraints allows us to better identify the mechanisms by which schooling may affect health and health behavior.

Despite the affordability of maintaining a healthy weight for most people, across the world children and adolescents in lower-income households are more likely to be overweight than in higher incomes households. Figures 1-3 display the relationship between household or parental income and the fraction of children and adolescents who are overweight in our sample of twins in rural China, described below, in two rounds of the Indonesia Family Life Surveys, and in the United States. In all three countries, the relationship is an inverted U - while at the very lowest incomes, overweight incidence is low, likely due to limited ability to reach healthy weight levels, from the lowest incomes, rates of overweight reach their peak at just slightly higher levels of income, thereafter falling. It is unlikely that the decline in unhealthy weight with income can be explained by increased access to health care. Indeed, as displayed in Figure 4 the

[1] We distinguish a diet that does not lead to excessive weight from a "healthy" diet providing balanced nutrition, including micro-nutrients. Achieving an optimally healthy diet may not be costless.

adult male consumption of (costly) cigarettes by household income in our rural twins sample displays the same inverted-U pattern as seen for child and adolescent overweight status, with the incidence of smoking among males rising initially then falling as incomes rise - increasing health for the poor by refraining from smoking would save resources.

Given that budgetary cost cannot be a major reason behind the income patterns for unhealthy weight or smoking, one principal alternative hypothesis is that the less-educated poor are unaware of the future health costs of being overweight or any unhealthy behavior with long-term effects. Another is that even with full information the poor have less incentives to invest in health because they have low incomes and low expected life spans, so they take pleasure from consuming tasty but unhealthy foods, now made more accessible with the growth of fast food outlets, suffering less consequences, consistent with the findings in Jayachandran and Lleras-Muney (2009).[2] The opportunity cost of low health, and thus the incentives to improve health via sacrifice, are lower for those with limited resources. We seek to test both of these factors - information and incentives - that potentially underlie the observed patterns of schooling/income and affordable health outcome and inputs.

Almost all data sets in the world indicate that there is a significant positive correlation between education, morbidity, adult survival rates and healthy behavior, such as being smokeless and engaging in exercise. It is well-recognized, however, that these associations may not be causal. For example, as suggested by Fuchs (2004), those individuals who have a lower rate of time preference are more likely to invest in health and in schooling, or those born into families with more resources may have better access to health care and schooling. Even if the relationship is causal, there is little evidence on why increased schooling might induce greater health investments. Identifying the mechanisms by which schooling affects health behavior is important, as there are substantial resources allocated to programs designed to make health care "affordable" and/or to directly provide health information in order to reduce illness and

[2]The availability of tasty and convenient foods is accelerating in China. The CEO of KFC announced in 2008 a plan to open 20,000 outlets in China (Shen, 2008). KFC opened 28 outlets in 1994 (Jing, 2000) and had accumulated 5,910 outlets by 2018 (YumChina, 2018). In 2017, McDonald's announced a plan to increase its outlets in China from 2.500 to 4,500 within five years (Feng, 2017) and Starbucks in 2019 announced a plan to open 600 stores per year through 2022 (Sanchez, 2019).

mortality, particularly in low-income, low-schooling populations.

That schooling causally affects health is not settled.[3] A number of recent studies have attempted to identify the causal effect of schooling on health and health behaviors, although none of these studies focused on causation have so far identified the mechanisms by which schooling might affect health. There are three principal identification strategies that have been employed, two of them using changes in compulsory schooling laws. One method, first employed in Lleras-Muney (2005), uses changes in school-leaving laws as an instrument for schooling and links cohorts over time to census and administrative data using difference-in-differences. Identification rests on the assumption that the treated and untreated groups experience parallel shocks before and after the differential treatments, which may a strong assumption for mobile populations. Results are thus sensitive to the specification of time trends, as shown by Mazumdar (2008). A second method that exploits variation in school-leaving laws identifies schooling effects by comparing differences in the cohorts on either side of the law cut-off (Clark and Royer, 2013). This discontinuity design does not require any assumption about parallel trends. However, the method does require that there are no general-equilibrium spillover effects across cohorts, that an increase in schooling for an adjacent cohort does not affect the returns to the older cohort unaffected by the law (lack of substitution or complementarity in production).

Additional important limitations of the identification strategies based on compulsory schooling laws, even if they have internal validity, are that they have a narrow LATE (around one specific year of schooling attainment) and the treated groups, those that are forced to undertake more schooling, are precisely those persons who had anticipated receiving lower returns from schooling. If an important reason for health investment is to increase the returns from schooling investment, then the estimates will underestimate the degree to which schooling and health are complements in the general population.

The third method for identifying the causal effect of schooling on health and health behaviors makes use of differences across genetically-identical (monozygotic or MZ) twins. The

[3] For a recent review of the literature on the relationship between schooling, income and health, see Lleras-Muney (2018).

method clearly eliminates that part of the association between schooling and health due to genetic endowments and family resources. However, the results are mixed, with some studies indicating a causal effect of schooling on health or health behaviors and some not. For example, Amin *et al.* (2013) using a data set on MZ twins born in the United Kingdom found that none of the statistically significant cross-sectional associations in the data set between schooling and health or health behaviors were causal. On the other hand, Lunborg (2013) found that the within-twins estimates from US data indicated that schooling causally affected self-reported health and exercise frequency positively and chronic illness negatively, but had no effect on smoking behavior while Behrman *et al.* (2015) using data on a survey of adult MZ twins in China, found that schooling causally reduced smoking and chronic illness but had no effect on exercise. The main weakness of the twins methodology (Bound and Solon, 1999) is that MZ twins must be assumed to be identical in all endowments, while the salient differences in their birthweight and the strong effects of birthweight differences between twins on adult outcomes suggest that is unlikely (Behrman and Rosenzweig, 2004). Thus, the internal validity of such studies up until now is an open question.[4]

A major conceptual problem with all of these studies, which affects the interpretation of the results, is that they ignore the role of parents in directly affecting health investments. There is a growing literature that shows the importance of parental investments in children (reviewed, for example, in Francesconi and Heckman, 2016). If schooling and health investments are complements, and parents provide health inputs and health advice to their children as well as schooling resources, then even the IV estimates of schooling will not identify the specific role of schooling in augmenting health, since parental investments in health or health information may respond to the schooling increases.

Attempts to identify the mechanisms by which schooling might affect health have eschewed attempts to identify causal effects. The most comprehensive quantitative study of the health mechanisms of schooling (Cutler and Lleras-Muney, 2010) provides estimates based on

[4]The twins studies and the studies making use of changes in laws governing schooling attainment are identifying different schooling effects. The former studies provide an answer to the question of how increasing an individual's schooling might affect her health. The latter identify the consequences of an overall increase in a sub-population's schooling level, which incorporate general-equilibrium effects.

regressions of schooling on health outcomes and behaviors while controlling for the mediating variables by which schooling might affect health. For example, they examine how the schooling coefficient changes when income is included as an additional regressor. The reduction in the schooling coefficient indicates that some part of the schooling "effect" is due to increased income. However, as the authors are aware, schooling and income are not likely to be uncorrelated with the health error term, an issue that is the principal motivation behind studies aimed at identifying the causal effects of schooling on health.

An important methodological exception to the existing literature on schooling effects on health and health behaviors is the Jensen and Lleras-Muney (2012) study that exploits an experiment in which information on the returns to schooling, randomly assigned, was used as an instrument for schooling attainment. They looked at the effects on smoking and alcohol consumption and found by the third round that the IV estimate of schooling on smoking was statistically significantly negative. They also found, however, that schooling *reduced* disposable income (both work hours, participation and wages) and had little effect on perceptions of the health consequences of smoking. The problem with the experimental design and identification strategy, however, is that a change in the perception of the income returns to schooling, which drives schooling investment decisions, directly affects expected lifetime income even if schooling is not changed. Thus, the exclusion restriction seems to be invalid - net of schooling there is a lifetime expected income effect of the treatment - and it is not clear therefore that a schooling effect is identified. The experiment does not replicate what would be the ideal, but likely not implementable, randomized control treatment - the direct random assignment of individuals to different schooling levels.[5]

We use data from a new panel survey of child and adolescent rural twins in China. These data permit a new identification strategy, one that combines the advantages of difference-in-difference methods and the advantages of twins data. Importantly, the method does not rely on the assumption, key in all cross-sectional twins studies, that post-conception monozygotic twins are identical. Rather, identification rests on two important assumptions: the parallel trends

[5]Because those who increased their schooling did so in the expectation of higher lifetime income returns yet schooling evidently reduced contemporaneous disposable income, it is difficult to identify the role of income in affecting smoking from the results.

assumption of difference-in-difference studies, an assumption that is arguably more credible for children born at the same time in the same household and followed over an 11-year period, and an assumption of the time-invariance of endowments effects, which can be directly verified. An advantage of using data on twins, as we show, is that differences across twins in schooling attainment are exhibited at many levels of schooling, so the LATE is more representative of the general population than from strategies relying on changes in a specific school-leaving age. We are also able to show for the first time that the "identical" twins assumption actually fails, using a placebo test both in our data and in another twins data set. We also show that our method exploiting a panel of twins, which does not rely on that assumption, passes the test as well as the test of the time-invariance of endowment effects.

Our data set has additional features that allow us to go beyond merely identifying schooling effects on arguably "costless" health or health behaviors. These include variables characterizing the frequency of advice, if any, given by parents to their children, parental assessments of each child's health, and information on self-reported happiness measures. Using a simple model of health, schooling and income, we show that by estimating the effects of the parental provision of diet advice on the health-related behaviors of the children by schooling level the information role of schooling in augmenting health can be identified. We also use the parental assessments of the health of each of the twin siblings at two points in time to quantify by how much increased schooling increases knowledge about the unhealthiness of being overweight. Finally, we are able, using information on the reported happiness of the parents, to explore the hypothesis that income and healthiness are complements in generating utility, consistent with the negative relationship between income and health outcomes, when such outcomes are virtually costless.

In section I, we briefly contrast four estimation methods to identify the causal effect of schooling on health in a setting in which children are heterogeneous in health endowments and parents invest resources in health and schooling, constrained by income and responsive to endowments and health shocks. We discuss the assumptions necessary for identification using OLS applied to a cross-section of children, within-twins methods that exploit differences within twin pairs in schooling, a child fixed-effect estimator that exploits changes in schooling over time from a panel of children, and an estimator that exploits changes in differences within twin pairs in schooling over time from a panel of twin pairs (DD twins). We show that the assumptions necessary for identification of a schooling effect on health are weakest for the DD twins method, with one necessary identification condition, the time-invariance of endowment effects, directly testable. We also establish a placebo test that reveals whether the estimate of the schooling effect has bias. The test is based on the assumption that the true effect of schooling on height is zero, so that an estimation method that yields an estimate of the schooling effect on height that rejects the null hypothesis fails identification.

Section II contains a description of the panel data on 1,128 child and adolescent twins aged 7-15 in 2002 from rural areas of Kunming district in China who were re-interviewed in 2013-14. We discuss sample attrition and its selectivity and provide descriptive statistics for the panel, including information relevant to identification and test power on the fraction of children whose schooling changed over the course of the panel and the fraction of twin pairs in each round with discordant schooling levels by schooling level. We show that because within-twin differences are located throughout the schooling distribution, the DD twins estimator provides a LATE that is relatively wide. In this relatively poor population, although with little access to fast food outlets, the fraction of overweight children is 18-19%.

In Section III we carry out tests of the internal validity of the DD twins estimator - the test of the time-invariance of endowments, using information on birthweights, and the placebo test, based on the relationship between schooling and height, to assess the credibility of DD twins estimates. We find that (i) we cannot reject the null that birthweight effects, based on within-twin variation, are constant over the life cycle of children, as in Figlio *et al.* (2014), and (ii) that only for the DD estimator can we not reject the null of no schooling effect on height. Section IV reports estimates of the effects of schooling on children's BMI and their overweight status. The results indicate that OLS and within-twins under-estimate the negative effects of schooling on both dependent variables. The preferred DD twins point estimate indicates that a one-year increase in schooling reduce the probability of a child being overweight by 29%.

In section V we set out a simple model in which an agent chooses a costless health input (e.g., exercise) and in which the returns to consumption depend on health. The model incorporates two different roles of schooling - an income effect, which affects the opportunity cost of bad health, and an effect on the perception of the consequences for health of the activity (information effect). We use the model to establish a test of the information role of schooling when both effects co-exist based on variation in information provided by an external source and in Section VI we carry out the test by estimating the determinants of exercise and smoking by the twins using the DD twins estimator. Following the model, we estimate interactions between the schooling of the child and the parental provision of diet information and the schooling of the twins' sibling, if higher. All of the interaction coefficient estimates are consistent with schooling affecting health by increasing health information, as both the higher schooling of the sibling and parental diet and exercise advice reduce the positive effect of own schooling on healthy behavior. In particular, provision of health information by the parents reduces the own effect of schooling on healthy behavior by a statistically significant 50% for exercise, and by a statistically significant 44% for smoking.

Section VII exploits parental assessment of each of the twin's health in the two rounds to further assess if schooling increases health knowledge. The test here is to see if, when a twin becomes overweight, the parent's assessment of the child's health is reduced. The maintained assumption is that, as the medical literature suggests, being overweight is a signal of bad health and informed parents will recognize this. The test then is whether the effect of a child's becoming overweight is more likely to change the health assessment downward for more educated parents, essentially a triple difference (changes between twins over time across parental schooling categories). Because identification of the effects of weight gain on the assessments, using DD twins, comes from time variation in the difference between twins's weight status, we eliminate bias due to heterogeneity in assessment criteria across parents by schooling and biases due to changes in those criteria over time. While the OLS estimates show no pattern, the DD twins estimates indicate that for parents with less than nine years of schooling, a child's becoming overweight does not affect her parental health assessment, while for parents with nine or more years of schooling a child's passing the overweight threshold is increasingly likely to be reported as less than in top health as the parent's schooling rises. In particular, the estimates indicate that 24% of parents with nine years of schooling report that an overweight child is in mediocre rather than excellent health while for a parent with 15 years of schooling the likelihood she reports her child as being in less than excellent health rises to 64%.

The results using the parents' provision of health information and from the parental health assessment of the children are consistent with the hypothesis that the less-educated are less informed about the health effects of being overweight. In the penultimate section of the paper we explore using information in the survey data on happiness whether being less healthy lowers the utility gains from increased income associated with higher schooling as embodied in the model. We find that for parents who are in "excellent" health (the top category), a one standard deviation increase in income increases the probability of being in the top happiness categories by 11 percentage points (41%). However, if the parent is not in the top health category there is essentially no increase in happiness from additional income. While these results are consistent with the hypothesis that the returns to health investments are lower for lower-income households and individuals, they are only suggestive, since for these estimates identification comes only from variation across parents. This is an area of health research that requires more attention. The final section summarizes our findings and discusses policy options for reducing the problem of obesity among the poor.

I. Estimating the Causal Effect of Schooling on Health: Methods

a. OLS estimator. To illuminate how panel data on twins overcome key identification problems of conventional estimators, we start with estimation of the effect of the schooling s_{ijt} at time t of person i in family j on her health H_{ijt} in a standard linear specification using a cross-section of individuals. We discuss below different measures of health, and how the measure may matter for identification. We assume that schooling and health are also affected by a history of parental inputs determined by a history of parental resources Y_{jk}, $k=0...t$, denoted by the vector $\boldsymbol{Y}_{jt}$, if health is a cumulative stock, and that all individuals are born with an initial health endowment H_{0ij}.

The cross-section regression of health on schooling across children in different households is:

$$H_{ijt} = \beta_1 s_{ijt} + \mu_{jt} + \varepsilon_{ijt}, \tag{1}$$

where we have divided up the error term into family μ_{jt} and individual ε_{ijt} components. The goal is to obtain an estimate of β_1 that mimics randomly assigning s_{ijt}. However, households differ in incomes and children differ in endowments, and these may affect the variation in both schooling and health and we only observe, as in most data sets, s_{ijt} and H_{ijt}. The unobservables are thus

(2) $$\mu_{jt} = \Sigma\beta_{jk}Y_{jk}, + \upsilon_{jt} = \mathbf{Y}_j\boldsymbol{\beta}_{2t} + \upsilon_{jt},$$

where υ_j reflects the preferences of the parents for child health as well as any household-specific shocks to health (illness). Note that we allow for period-*t* household income realizations to have different effects on health measured at different time periods.

(3) $$\varepsilon_{ijt} = \beta_{3t}H_{0ij} + \xi_{ijt},$$

where H_{0ij} = health endowment of child *i* in family *j* and ξ_{ijt} = *iid* random error term. Note that we permit the initial endowment to have a different effect on health in different time periods.

Then, the cross-household estimated relationship between *s* and *H*, given that variations in *s* and *H* vary with family resources and person-specific endowments that are not observed in the data, will depend on how variation in these unobservables affect schooling and affect health. For example, if the source of variation in s_{ij} is due solely to variation in endowments, then

(4) $$\mathrm{d}H_{ijt}/ds_{ijt} = \beta_1 + \beta_{3t}\,[\mathrm{d}s_{ijt}\,/\mathrm{d}H_{0ij}]^{-1}$$

and the relationship between s_{ijt} and H_{ijt} will not identify β_1. But, of course, family resources will also affect schooling and health as well, and the bias in β_1 obtained from variation in the cross-section will be difficult to know in the absence of knowledge of both the effects of endowments and parental resources on schooling and health and the moments of the distributions of these omitted variables.

b. <u>Within-twins estimator</u>. Now assume we have a pair of children, 1*j* and 2*j* born at the same time in family *j*. The history of parental resources, parental preferences for human capital investment and common household shocks (contagious illnesses) in μ_{jt}, are thus identical for both children but the children's endowments differ: $H_{01j} \neq H_{02j}$. The difference (within-twin) estimating equation for is:

(5) $$H_{2jt} - H_{1jt} = \Delta H_{jt} = \beta_1(s_{2jt} - s_{1jt}) + \beta_{3t}(H_{02j} - H_{01j}) + \xi_{2t} - \xi_{1jt} = \beta_1\Delta s_{jt} + \beta_{3t}\Delta H_{0j} + \Delta\xi_{jt}$$

There is still bias:

(6) $$d\Delta H_{jt}/d\Delta s_{jt} = \beta_1 + \beta_{3t}[d\Delta s_{jt} / d\Delta H_{0j}]^{-1}$$

as the children's health endowment differences, contained in the residual, will likely be associated with the schooling difference (parents may favor the healthier child by investing more in schooling, in which case the bias will be positive).

c. Child fixed effects estimator. Suppose we have two observations at different times (t=2,1) for the same child ij, then differencing her health across the two periods ij yields:

(7) $$H_{ij2} - H_{ij1} = \beta_1(s_{ij2} - s_{ij1}) + (\beta_{32} - \beta_{31})H_{0ij} + \mathbf{Y}_{j2}\boldsymbol{\beta}_2 - \mathbf{Y}_{j1}\boldsymbol{\beta}_1 + \xi_{ij2} - \xi_{ij1}$$

The residual contains the time-differential effect of the child-specific endowment and the difference in the history of household income effects across the two periods. If higher household income in the second period induces greater health and schooling, then the child fixed effects schooling estimate will be positively biased.

d. Twins difference estimator (DD twins). Now suppose we have observations on twins at two points in time. Double differencing across the non-identical twins born at the same time in the same household eliminates the history of all common shocks and the history of (trends in) household time-varying income effects.[6] What remains is any time-differentiated effect of the difference in child-specific endowments and the iid time-varying, child-specific health shocks:

(8) $$\Delta H_{j2} - \Delta H_{j1} = \beta_1(\Delta s_{j2} - \Delta s_{j1}) + (\beta_{32} - \beta_{31})\Delta H_{0j} + \xi_{ij1} - \xi_{kj1} - \xi_{ij0} + \xi_{kj0}$$

From (8) we see that there are two principal threats to identification using the DD-twins estimator: 1. endowments may have time-varying effects on health and 2. past child-specific health shocks may affect current schooling and current health. We discuss each in turn.

The within-twins estimator assumes that $\beta_3 = 0$, there are no endowment effects, or there are no unmeasured differences in endowments across genetically-identical twins. The existence of large birthweight differences between monozygotic twins indicate that the latter assumption is untenable. And, studies using birthweight differences between twins have been used to show the

[6] In the panel data we use, described in detail below, all of the respondents are less than age 25 in the second round and thus most of the twin respondents reside at home. Twins who left their parents household tended to do so together and to reside in the same community. Thus, even among the 12% of older twins who left the home, it is more likely that both twins were experiencing similar shocks.

importance of variation in this endowment measure for adult outcomes (e.g. Behrman and Rosenzweig, 2004). In contrast, the twins difference estimator allows endowments to differ across twins but needs to impose a weaker restriction of endowment effects time-invariance, that $\beta_{3t} = \beta_3$. This restriction can be directly tested using, for example, birthweight information and outcome measures for twins of different ages (they need not be the same twins over time). Figlio *et al.* (2014) have carried out this test for measures of cognitive development. They cannot reject the hypothesis that the effects of birthweight on the outcome measures are identical for children in grades three through eight. We can also directly test for the time-homogeneity of birthweight effects using our data for health outcomes.

The importance of the existence of time-varying, child-specific shocks (all common-household shocks having been eliminated) are likely to depend on the measure of health. For example, if the measure of H_{ijt} is a short-term illness, it is unlikely that a past illness shock to a child many years ago has a major influence later in life, net of endowment effects and parental income histories (the permanent propensity to be ill is removed if endowment effects are not time-varying, thus leaving only the time-varying random components of illness). On the other hand if the measure of healthiness is height, histories matter, as height reflects the cumulation of inputs, particularly early-life inputs. Early child-specific shocks may thus cumulate. Here our focus is on weight and overweight status. We regard weight as more similar to a short-term illness than to height in terms of its relationship to past child-specific shocks. Weight can be changed in the relatively short run, net of a permanent propensity to be heavy or light, which is removed from the double differencing if the time-homogeneity assumption of endowment effects holds. Indeed in our two rounds of data, separated by 11 years, the inter-temporal correlation in weight is only 0.073 (p=0.10) while the inter-temporal correlation in height standardized for age is over three times as high, 0.234 (p=.000).

e. <u>A placebo test</u>. As noted, we can directly test the assumption of the time-invariant effects of endowments by estimating within-twin birthweight effects by age. We cannot directly test the susceptibility of weight to idiosyncratic shock histories. However, we can carry out a global test of whether any estimate of β_1 is biased if we know the true β_1. We believe it is credible that for height the true treatment effect of schooling is zero, that is, $\beta_1 = 0$ - any correlation between schooling and height is spurious, the result of the influence of omitted variables. We will apply each of the estimation methods to estimate the β_1 for height to see if they recover the true β_1. The test of bias is thus the null that $\beta_1 = 0$. As far as we know, no health study has looked at the "effect" of schooling on height; we think that is because it is universally believed that schooling cannot alter height. But by examining the relationship between schooling and height we can test an estimator's ability to identify a true schooling effect on health. We will thus test both the time-invariance of the endowment effects as well as carry out the placebo test for each of the estimators. We will show that the DD twins estimators passes both tests, and we will then proceed to estimate the effects of schooling on obesity and on health behaviors and test for the specific mechanisms using the preferred new estimation method.

II. The Data

The data that we use are from the two rounds of the Chinese Child Twins Survey (CCTS). The initial round of the survey was designed and overseen by the authors and carried out in 2002-3 by the Urban Survey Unit (USU) of the National Bureau of Statistics (NBS) in the Kunming district of China. The first-round CCTS includes a probability sample of households with twins aged between 7 and 18 in both rural and urban areas as well as a comparable sample of households with no twins. All households with twins in the relevant age range were initially identified by USU according to whether children had the same birth year and month in the age interval and the same relation with the household head using data from the 2000 population census for Kunming. The addresses of the eligible households were obtained from the census office and actual child twins were then determined by household visits. Because it was determined that many twins aged above 16 has left their parents' household, we restrict our analysis to twins aged 7-16 in the first round (Rosenzweig and Zhang, 2009).

In 2013-4 a follow-up survey was undertaken. The attrition rate for urban households was over 50%, while 73.5% of the original rural twin pairs (households) in the relevant age range were successfully interviewed in the second round. We thus restrict our analysis to rural twin pairs. The number of individual twins aged 7-16 in 2002 is 1,534, of which 1,128 have second-round data. For each of the twins in the panel we have two observations on anthropometrics, schooling attainment and attendance, and parental health assessments as well as repeated observations on the parent's employment and earnings and household incomes. The information also includes smoking histories for the twins and information on their exercise habits, the frequency of parental advice on diet, and parents' happiness.

While for most of our estimators internal validity is not threatened by the exit of twin pairs over time, based on a large set of observables in the first round attrition does not appear to be selective. Table A1 in the Appendix reports a probit regression of household and parent characteristics on the probability of interviewing a rural twins household in the second round. The set of variable, including individual parent's age and schooling, wage earnings, total household income in eight categories is only marginally statistically significant at the 0.12 level. None of the income variables are statistically significant by conventional levels and only father's age is individually statistically significant at the 0.05 level.

Tables 1 and 2 provide descriptive statistics on the parents and on the children, the latter by survey round. Despite the absence of major fast food outlets and lower availability of less healthy processed foods in the rural area, as seen in Table 3, using the World Health Organization (WHO) standards for being overweight by age and gender, 19.1% of the twins were overweight in the first round and 5.8% in the second round. If we use as the criterion for overweight a BMI of 23, which the medical literature suggests is more appropriate for the adult Asian Population (Wildman *et al.*, 2004; Hsu *et al.*, 2015) than the standard of 25, 17.7% of the twins are overweight in the second round.[7]

Tables 4 and 5 display OLS estimates, based on the first- and second-round data, respectively, of the relationships between parental characteristics and the height, schooling attainment, BMI and overweight status of the twins. Three findings exhibited in the tables stand out. First, parental income in the first round and parental height have persistent effects into the second round for child height. These estimates are consistent with the importance of early nutritional intakes and of genetics (endowments) for this measure of health. Second, while parental income is negatively related to a child being overweight in the first round, no parental characteristics appear to be significantly related to a child's BMI or weight status in the second round. Third, parental schooling rather than parental income are important correlates of the schooling attainment of the children in both the first and second rounds. This could reflect

[7] The criterion is based on at which levels of BMI significant health problems appear among adults; these include Type-2 diabetes or heart disease.

preferences and/or endowment effects.

Because our identification strategy rests on changes in differences in schooling within twin pairs, it is important that there be sufficient variation in schooling over time and across twins at a point in time. 85% of twins aged 7-15 experienced an increase in schooling over the interval of the survey. There is also significant within-twin variation within a round. Table 6 reports the distribution of the absolute value of the within-twin differences in schooling attainment in the first and second rounds. As can be seen, in the first round schooling attainment differed in only 12% of twin pairs, with more than half of those exhibiting a difference of only one year. By the second round, schooling differed in over 30% of twin pairs, with almost two-thirds of those pairs exhibiting a difference of more than two years. An important feature of the data is that the average schooling level of the twin pairs with heterogeneous schooling is distributed over most of the distribution of schooling levels. Table 7 reports the full distribution of years of schooling attainment for the twins in the second round in the first column, while the second column displays the distribution of average schooling attainment levels for the twins with discordant schooling attainment. These patterns indicate that the LATE obtained from variation in differences between twins is broad with respect to schooling levels.

III. Tests of the Internal Validity of the DD Twins Estimator

a. <u>Testing the persistence of endowment effects over time</u>. As noted, a key identification assumption of the twin differenced method is that endowment *effects* do not vary over time. In Table 8 we report estimates of birthweight effects on BMI, overweight status and schooling by round using the within-twins method (as in Figlio *et al.*, 2014). As they found for cognition test scores, we cannot reject the hypothesis for any of the three dependent variables that the birthweight effects are identical over time, in this case across the 11-year interval of the panel. This is not because the birthweight coefficients are imprecise, as they are individually statistically significant at at least the 0.1 level for all but the schooling dependent variable. The statistical significance of the individual birthweight effects rejects a key assumption of the within-twin method, that there are no persistent individual endowment effects.[8]

[8]Many twins studies include birthweight as a control. But, of course, birthweight is just one endowment measure. As we will see below inclusion of birthweight is not sufficient to pass the placebo test applied to the within-twin method estimates.

b. The height placebo test. Under the reasonable assumption that schooling has no true causal effect on height,[9] we can assess the validity of different estimation methods by examining the estimates each yields of the relationship between schooling and height. Rejection of the null $\beta_1 = 0$ is rejection of the assumptions of the estimation method. We begin with a replication of a published study using adult twins from China (Behrman *et al.*, 2015). In that study, the within-twins MZ method was used to estimate the effect of schooling on a variety of health measures and behaviors. The study did not include height as one of the health measures. However, the authors included birthweight in the specification to control for endowment effects. So do we. In the first two columns of Table 9 we report OLS and within-MZ twin estimates from those data of the effect of schooling attainment on height. We see that for both methods we reject the null, and thus reject the hypothesis that the within-MZ estimator is unbiased. This is the first test that we know of to reject the assumptions of the within-MZ twins method based on outcome estimates.

We next use our child twins data and apply four methods: OLS, within-MZ twin, child fixed effects, and finally the DD twins method that exploits the panel. These are reported, respectively, in the last four columns of Table 9. As can be seen, we can reject the null for all but the DD twins method. In part this is due to the coefficient standard error becoming larger using the latter method, but the within-twin schooling coefficient is 50% higher than that obtained using the DD twins method. One interpretation of this result is that the DD twins method does not have enough power to identify a schooling effect. As we will see, however, using the same data the method yields statistically significant schooling effects for measures of weight. Finally, we regard the height placebo test as a strong test in that we expect that there are important persistent effects from individual-specific time-varying shocks for height. As noted, we think these are much less likely to be an issue for weight measures, as evidenced by the small degree of individual-specific persistence of weight compared with that of height.

IV. The Effects of Schooling on Weight using the Twins Panel

We now apply the set of estimation methods - OLS, within-twin, within MZ twin, and DD twins - to obtain estimates of the effects of schooling on BMI and overweight status for the

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[9] Of course a parent's schooling can effect a child's height, via income and health-behavior effects, such as smoking (Currie and Moretti, 2003; Thomas *et al.*, 1991).

twins. The top panel of Table 10 reports the estimates for the determinants of BMI, where all estimates are obtained from the second round except those using the DD twins method, which uses both rounds of data. In the first column, the OLS estimate indicates that, controlling for parental income and birthweight, a one-year increase in schooling is associated with statistically significant 0.11 drop in BMI. In the second column we add to the specification a variable that takes on the value of one if the parents provided diet and health advice to the child; the coefficient for that variable indicates that if the parents provide diet advice BMI is lower by a statistically significant 0.61. The coefficient on schooling drops by 29%, while still remaining statistically significant. The fall in the schooling coefficient when the parent is providing health advice hints at the informational role of schooling. However, as we have shown, the OLS estimates are unlikely to provide unbiased estimates. We will more rigorously test for the informational roles of schooling in the next sections.

The set of estimates in the remaining columns in the top panel of Table 10, which rely on twin differences, provide schooling effects that are net of parental advice effects because in the data such advice does not differ across the twins. And of course the schooling estimates are net of parental income effects. For all three of the estimation methods that rely on twin differences, the schooling coefficient is larger in absolute magnitude than the OLS estimate. And, in contrast to the results for height, the estimate of the schooling effect is statistically significant using the preferred DD twins method, as reported in the last column. The DD twins estimate of the schooling effect is 50% larger than the OLS estimate, suggesting that BMI would be 3.2% lower for a college graduate compared with a high school graduate at the mean value of BMI.

While the effects of schooling on BMI appear to be modest across all estimation methods, estimated schooling effects on the probability of being overweight, obtained exploiting twins differences are not. The bottom two panels of Table 10 report the estimates of the effects of schooling on the probability of the child being overweight by estimation method using the Who and Asian standards. As for BMI, the methods exploiting differences between twins indicate a stronger schooling effect on the probability of being overweight compared with the OLS estimate for both overweight criteria. The DD twins estimates are statistically significant and indicate that a one-year increase in schooling reduces the probability of being overweight by 30% using the WHO criterion and by 11% using the lower-threshold Asian standard at the relevant sample means.

V. Testing the Mechanisms Using Causal Estimates: Theory

a. Affordable health, income and schooling. Having found evidence that increased schooling lowers the likelihood of being overweight we now seek to understand some of the mechanisms. We focus on two mechanisms: (a) an income effect and (b) an effect of schooling on health information. We begin by constructing a simple model incorporating the two roles of schooling in augmenting health to guide the subsequent empirical analysis. In particular, the model is used to indicate whether and how, if at all, we can identify each of the mechanisms from the reduced-form estimates. While we want to keep the model simple, it should fit the observed facts. These are that virtually costless health maintenance (not avoiding obesity) is lower for those with lower incomes and schooling and costless health-related behavior is positively associated. To fix ideas about the affordability of maintaining a healthy weight, we assume that health inputs have a market price of zero. In particular, we let h be an unhealthy but utility-enhancing behavior (eating tasty food, smoking, foregoing exercise) that imposes no budgetary burden or even saves resources (smoking). On the other hand, bad health H (e.g., obesity) reduces the utility of consumption.[10]

The agent, with given schooling s, chooses the amount of unhealthy consumption according to the program:

$$(9) \qquad \max_{h} \; \mathrm{U} = U(h) + (1 - H)V(C), \;\; H<1$$

where $C = \omega s + y$, y = exogenous non-earnings income, and ω = skill rental price. The agent derives utility both from the costless unhealthy consumption good h and from consumption good C, which has a unit price but has no effect on health.

The agent has a belief about how her behavior affects ill health, given by:

$$(10) \qquad H = \eta f(h), \;\; f'>0, f''<0$$

[10]In Appendix A we discuss a model in which unhealthy behaviors impose direct costs. The net result is to introduce ambiguity into the model; e.g., income effects can be positive and schooling effects on health are weaker.

where η, $1>\eta>0$, is a scalar indicating the perceived strength of the relationship. η is affected by information, which may be provided in school, and is thus a function of schooling level, and by other sources e (information from parents or from a health campaign), such that

$$(11) \qquad \eta = \eta(s,e),$$

s and e, if they both provide information about the consequences of unhealthy behavior, increase η (η_s, $\eta_e > 0$). They are thus substitutes, so that $\eta_{se} <0$. The model then accommodates two roles for schooling in augmenting health: (a) schooling augments income ($\omega>0$) and (b) schooling increases the perception of how strongly h affects H ($\eta_s>0$, information).

Despite the fact that changes in h have no budgetary impact, the model delivers an income "effect" consistent with what is observed worldwide - there is more unhealthy behavior among those with lower income. The FONC is:

$$(12) \qquad U' - \eta f'V = 0$$

where $\eta f'V$ is the shadow price of h, the loss in utility of consumption. The income comparative static is:

$$(13) \qquad dh/dy = \eta f'V'/[U'' - \eta f''V] < 0,$$

higher income leads to less bad behavior because higher income raises consumption C and thus the costliness (shadow price) of unhealthy behavior. The income effect is really an opportunity cost effect - health can be increased at no budgetary cost, but bad health lowers the utility of consumption. In this set-up, the poor have a lower incentive to augment health by giving up utility-enhancing unhealthy activities. A rise in income does not increase the affordability of good health, as is the usual interpretation of the income effect on health when health maintenance requires budgetary expenditures.

b. Schooling and health information. Increased schooling reduces bad health behavior and thus bad health due to its effects on both income and information:

$$(14) \qquad dh/ds = [\eta_s f'V + \eta f'V'\omega]/[U'' - \eta f''V] = \omega dh/dy + \eta_s f'V/[U'' - \eta f''V] < 0,$$

where $U'' - \eta f''V <0$ by second-order conditions. The model thus captures the two facts about unhealthy weight that we have documented: a negative association between income and ill-health and the fact that increased schooling attainment causally lowers bad health (overweight status). The issue we now address is whether we can separately identify the informational and opportunity-cost roles of schooling. The model shows that it is possible, using information on alternative sources of health information.

First, the model delivers the comparative static result that direct information (e.g., from parents) indicating that *h* adversely affects health reduces unhealthy behavior:

(15) $$\mathrm{d}h/\mathrm{d}e = \eta_e f'V/[U'' - \eta f''V] < 0.$$

If we assume that schooling itself provides no health information (η_s, η_{se}=0), then the effect of increased information on the relationship between schooling and *h* is

(16) $$\mathrm{d}^2h/\mathrm{d}s\mathrm{d}e = \{U''f'\eta_e V'\omega)/[U'' - \eta f''V]^2 < 0.$$

The negative schooling effect on unhealthy behavior is enhanced when health information is made available - the direct provision of information on the (higher) returns to health behavior steepens the negative gradient between schooling and unhealthy behavior. This is because the information augments the perceived effect of schooling on the opportunity cost of ill-health.

Now assume schooling also provides health information. Then the negative effect of schooling on unhealthy behavior *can* be attenuated when health information is provided directly (η_{se}<0, schooling and other sources of information are strictly substitutes):

(17) $$\mathrm{d}^2h/\mathrm{d}s\mathrm{d}e = \{U''f'(\eta_{se}V + \eta_e V'\omega) + V^2 f''f'(\eta_e\eta_s - \eta\eta_{se})\}/[U'' - \eta f''V]^2$$

While the second term is negative, the first term in (17) may be positive, so that the cross-derivative may not be negative. This result leads to the following proposition:

> <u>Proposition:</u> *If and only if schooling and other sources of health information are strictly substitutes* (*the effect of schooling is attenuated by having alternative sources of information*) *can the cross-derivative between schooling and alternative sources of information on health be positive.*

The role of schooling in providing health information can be identified from the cross-derivative of schooling and health information provision, but may not be. From (16), the direct provision of information on the unhealthiness of h increases the effect of schooling on the opportunity cost of bad health. This is offset only if schooling also provides information and this redundancy effect must be sufficiently strong such that it outweighs the opportunity cost effect for the cross-derivative (17) to be positive. Thus, we can infer whether schooling has an information role by testing if the negative schooling effect on unhealthy behavior is reduced when parental health advice is provided.

VI. Testing for the Information Role of Schooling Using Causal Estimates: Evidence using Alternative Family Information Sources

In this section we implement the test of the existence of the informational role of schooling when schooling also increases the returns to health investments, as implied by the model, based on plausibly causal estimates. There are three potential sources of health information for respondent twins in the data: (a). A twin's own schooling, (b). Parent's health-related advice provided to the children (measured by its frequency) (c). Information from the more educated twin's sibling (the twin's "peer"). The relevant sibling is the one with strictly higher schooling than the twin, as she potentially has more information to provide. We assume the information from siblings with equal or less schooling is redundant. This assumption allows us to identify a sibling peer effect using the DD estimator.

The survey elicited information from the parents on the frequency with which they provided health and diet advice to their children and asked the children, independently, whether such parental advice affected their own behavior. 35% of the parents reported that they provided this advice "always" and "usually." And, over 35% of their children reported that their parents advice had actually "always" and "usually" influenced their behavior. Unsurprisingly we can resoundingly reject that parents frequency of attempts at influencing their children's behavior and their children's reports of the influence of their effects are independent. Thus, the data clearly indicate that parents may play an important informational role in affecting their children's health. Moreover, the data indicate that mother's schooling is strongly and positively related to both the frequency of health advice and to the children's schooling attainment. Thus any association between a child's schooling and health will likely overstate the role of own schooling in directly affecting health behavior if this informational role of parents is ignored.

The model assumes that schooling, income and external information vary exogenously.

However, all of these variables in reality may be correlated with unobservables, such as child endowments, parental resources and preferences for health, as we discussed in the section on identifying schooling effects on health. For example, parents may provide more frequent advice to lower-endowment children or children with bad health habits, parents who care more about their children's human capital or have higher permanent-income may provide both more advice and subsidies to (or virtual taxes on) health-related inputs (e.g., an exercise mat, confiscation of cigarettes).We thus use the same estimation procedures we employed to identify the health effect of schooling that exploit the panel data on twins. The specification of the reduced-form health behavior equation incorporating parental advice and the influence of a better-educated sibling is:

$$(18) \qquad h_{ijt} = \gamma_1 S_{ijt} + \gamma_2 \iota_j + \gamma_3 S_{kjt}\,(S_{kjt} > S_{ijt}) + \gamma_4 S_{ijt}\iota_j + \gamma_5 S_{ijt} S_{kjt}(S_{kjt} > S_{ijt}) + \kappa_j + \kappa_{jt} + e_{ij} + \zeta_{ijt},$$

where h_{ijt} is *healthy* behavior by twin *i* in family *j* (not smoking, exercising) at time *t*, ι_j = whether parents regularly provide diet information,[11] S_{kjt} = the schooling of the "superior" twin peer at time *t*, κ_j = time-invariant parent preferences for their children's health and parent resource endowment effects, κ_{jt} = time-varying parent preference and resource effects, and e_{ij} = child-specific endowment effects. The model indicates that γ_1, γ_2, $\gamma_3 > 0$ and, iff schooling provides information (the proposition), the coefficients on the interaction terms, which correspond to (17), $\gamma_{4,}\,\gamma_5 < 0$, appropriately reversing sign because we are using healthy behaviors empirically. Note that if $\gamma_{4,}\,\gamma_5 \geq 0$, we cannot rule out an information role for schooling, we just cannot identify it. The model implies that information suggesting that a behavior has adverse health consequences increases the effect of schooling on the opportunity cost of bad health and thus can make the effect of an increase in schooling on healthy behavior more positive.

We estimate the interactive equation using both the within-twin estimator from the second-round data, which eliminates the effects of parental preferences and resources, and the DD estimator, using the information from both rounds, the latter to eliminate any correlation between schooling, parental advice and the unobservable components, including child-specific

[11] We are specifying parental health information provided to the children as common across sibling because in less that 4% of the twin pairs is the frequency of advice different. As a consequence we cannot identify γ_2, the direct effect of parental information on the twin's behavior using any of the methods that rely on differences within twins.

endowment heterogeneity and changing parental human capital endowments and preferences over the life-cycle. We select twins who were aged 11-16 in the first round, when at least some twins were already smoking. Appendix Figure 1 displays the proportion of these twins who smoke by age; about 25% are smoking in the second round. About half of the twins also engaged in exercise in the second round.

The first two columns of Table 11 report the estimates of equation (18) using the within-twins estimator applied to the second-round twins (aged 21-26) for both exercise and non-smoking, respectively. The effects of own schooling on both behaviors are positive and statistically significant, indicating in the absence of parental health advice a one-year increase in schooling increases the probability of exercising by 11 percentage points (22% increase) and increases the likelihood of not smoking by 6.5 percentage points, an 8% increase.

As indicated in the model, equation (14), the positive schooling effect on healthy behavior is not informative about whether the schooling effect mechanism is via an improvement in understanding how behavior affects health or via an increase in the utility loss from bad health associated with increased income. However, the estimate of the interaction coefficient indicate that if the parents regularly provide information on diet and exercise, the effects of own schooling on exercise are cut in half and the effect on the probability of smoking is reduced by 63%. These results are thus consistent with the effect of own schooling having a strong information component, as indicated by the model proposition. Information provision is not, however, the only mechanism by which schooling increases healthy behavior because not all of the schooling effect is wiped away by the information provided by parents. This is consistent with schooling also increasing the incentive to increase health when income is increased by the additional schooling.[12] Finally, the within-twins estimates do not indicate any effect of the twin's sibling schooling on either behavior, although the sign of the cross-effect is consistent with the sibling providing some useful health information.

The results from the DD estimator using the full panel are qualitatively similar to the within-twin estimates: increased schooling increases both measures of healthy behavior but does

[12]Of course the remaining effect of schooling could still be an information effect if parent advice is incomplete. We directly test in section VIII below that the opportunity cost of health increases with income.

so less if the parents are providing health advice. However, the DD-twins estimate of the own schooling effect is higher by 11% for exercise and is almost double that of the within-twin estimate for smoking. As was seen from the within-twin estimates, provision of health information by the parents reduces the own effect of schooling on healthy behavior by a statistically significant 50% for exercise, and by a statistically significant 44% for smoking. We can now also reject the hypothesis from the DD-twins estimates that the schooling of the twin's sibling does not reduce the own effect of schooling for both exercise and smoking at the 0.032 level, treating the behaviors as jointly affecting health behavior. The sibling schooling cross effect point estimates are small, however.

The interaction coefficient estimates, as indicated by the model proposition, are consistent with schooling affecting health by increasing health knowledge, as parental diet and exercise advice provided by the parents substantially reduces the positive effect of own schooling on healthy behavior. Is there additional evidence that schooling affects the information set of adolescents and children? The survey elicited information in both rounds on the reading and web surfing habits of the respondent children. Table 12 reports within-twin and DD-twins estimates of the effects of own schooling on the probabilities of the twins reading a newspaper and surfing the web. Both sets of estimates indicate that schooling and information acquisition are positively related. The larger DD-twins point estimates indicate that a one-year increase in schooling increases the likelihood of reading a newspaper by 6.4 percentage points and surfing the web by 7.2 percentage points, almost a doubling of the probabilities.

VII. Testing for the Information Role of Schooling Using Parent's Health Assessments of their Overweight Children

In the previous section we tested whether schooling improves knowledge about the returns to health behavior based on the implication of the model that parent information provision reduces the impact of own schooling on health-related behavior. In this section we test more directly, using the DD-twins estimator, whether the more schooled are better informed about the unhealthiness of being overweight using information on the respondent parent's assessments of each of her twin's health based on her twin's anthropometric statuses in both survey rounds. Parent's health assessments embody their beliefs about healthiness. Medical science indicates that being overweight is unhealthy. If parents are aware of this information, our maintained assumption is that no parent who observes a child who is (becomes) overweight should rate that child's health in the top category. Thus, we test if the respondent parent's health assessment of a child is below the top category ("excellent") when the child is overweight by the level of *respondent* schooling. If more schooled parents are better informed, we expect that they are more likely to rate a child more negatively if she becomes overweight. We can implement this test because the data provide the identity of the parent who is the survey respondent.

The standard problem with using subjective health assessment data in conventional cross-sectional data is that parents may differ in their standards of health, and these may vary systematically with their preferences for their children's health and with their own schooling. Moreover, a parent's assessment criteria may change over time along with their own or their children's health so that panel data on a single child may not identify true changes in *perceived* health from information on parental health characterizations. Our data, which provides parental assessments at two points in time across each of the twin siblings, eliminates both of these problems when we use the DD-twins estimator. We just need the weak assumption that parental standards do not differentially change over time across the twin siblings as a function of their health status.

The estimating equation is:

$$A_{ijt} = \delta_1 w_{ijt} + \delta_2 S_j + \delta_3 w_{ijt} S_j + e_{\mathrm{ij}} + \eta_{jt} + \varsigma_{ijt}, \tag{19}$$

where $A_{ijt} = 1$ if the parent j rates her child i below the top health category at time t, $w_{ij} = 1$ if the child is overweight at time t, e_{i} = the child-specific health endowment, and η_{jt} = the parent's subjective time-varying health threshold . We expect that $\delta_3 > 0$, the higher is S_j (= parental schooling) the more likely the parent of a child who is overweight will report the child's health as less than excellent. We can apply the DD-twins estimator because we have two assessments for each twin - this importantly eliminates biases due to differences in assessment criteria across parents in different households, the influence of differences in the permanent health endowments of the twins on their parent's assessment, and any changes in (common) parental assessment criteria over time. Using either the DD-twins or within-twin estimator we cannot, however, identify δ_2, the direct effect of parental schooling on the assessment of a child's health.

The first two columns of Table 13 report the OLS estimates of (19) without (first column) and with (second column) the interaction between the parent's schooling and an indicator if the child is overweight using the stricter (and thus more visible) WHO standard. Neither the child being overweight nor the parent's schooling or their interaction are statistically significant. In the third and fourth columns, we report the within-twin estimates, which eliminate differences in parental health criteria η_{jt}. Here, we see whether parents of different schooling assess differentially their children depending on each child's individual overweight status, thus eliminating any different standards for health assessments across parents. In contrast to the cross-sectional OLS estimates, the set of overweight coefficients are jointly statistically significant, with a parent respondent with higher schooling more likely to report the overweight twin as being of mediocre health.

Finally, the last two columns of Table 13 report the DD-twins estimates, which eliminate the influence of both the child endowment e_{ij} and parental time-varying health criteria η_{jt}. The set of overweight coefficients remain statistically significant, with higher schooled parents more likely to report that a child is less healthy if the child becomes overweight over time while her sibling does not. Evidently the differing health criteria of parents by parental schooling and parent's assessment biases by child health endowments obscures arguably true significant differences by parental schooling in their health assessments of their children's nutritional status.

With respect to the pattern of assessments by parent schooling implied by the estimates, while the OLS estimates show no relationship, both the within-twin and DD-twin estimates indicate that for parents with less than nine years of schooling, a child's being overweight does not affect her parental health assessment.[13] However, for parents with nine or more years of schooling a child's being overweight is increasingly likely to be reported as less than in top health as the parent's schooling rises. The DD-twins estimates indicate that only 24% of parents with nine years of schooling report that an overweight child is in mediocre rather than excellent health while for a parent with 15 years of schooling the likelihood she reports her child as being in less than excellent health rises to 64%.

VIII. Does Being Overweight Lower the Utility of Consumption?

[13]Note that the schooling level is below nine years for 50% of the mothers and fathers in the sample.

In this section we return to the model, exploring the realism of the assumption that being less healthy lowers the returns from higher income by exploiting the happiness information in the second round of the survey. As embodied in the model, complementarity between income and health can explain why there is a negative income gradient, apart from information, for virtually-costless healthy behaviors such as seat belt use or being smoke free or for a measure of health such as obesity that does not require costly inputs to prevent.

The parents were asked in the second round of the survey a battery of standard happiness questions. The happiness indicator we used was based on the question: *"All things considered, how satisfied are you with your life these days?"* The answer scale ranged across nine categories, from "very dissatisfied" (1) to very "satisfied" (9). We define the respondent as "happy" if the respondent chose a number in the top three categories. 27 percent of the respondents were happy by this definition. The results were not sensitive to altering the categorization of the dependent variable by one or two ranks. We estimate whether (a). income is associated with greater happiness (b). respondents below the top health category are less happy, and (c). the positive association between income and happiness is smaller for respondents below the top health category, as assumed in the model.

The specification we estimate is:

$$U_{ij} = \gamma_1 Y_{ij} + \gamma_2 \mathrm{a}_{\mathrm{ij}} + \gamma_3 Y_{ij} a_{ij} + \boldsymbol{Z}_{ij} \boldsymbol{\gamma} + v_{\mathrm{ij}}, \tag{20}$$

where $U_{ij} = 1$ if the respondent reports a happiness category in the top three, Y_{ij} = income of the parent i, $a_{\mathrm{ij}} = 1$ if the respondent's own health assessment is below the top category, and $\boldsymbol{Z}$ is a vector of control variables, including gender, age, and whether the respondent smokes. If more resources increase consumption and thus utility we expect that $\gamma_1 > 0$ and if health and consumption are complements we expect that $\gamma_3 < 0$.

The first column of Table 14 reports the happiness equation estimates for parents aged 40-59 in the second round without the interaction between income and the health variable. In this specification, income has a positive and statistically significant effect on the happiness indicator while being below the top health category statistically significantly lowers the probability of being in the top happiness categories. When we add the interaction between income and health, as reported in the second column, the positive income coefficient more than doubles in magnitude and remains statistically significant and the interaction coefficient is negative and statistically significant. The linear health coefficient, however, loses its statistical significance. Thus we find results consistent with the model assumptions: increases in income significantly increase the likelihood of being happy and the increase in happiness from additional income is significantly less if the respondent is less healthy. The point estimates indicate that for parents who are in "excellent" health (the top category), a one standard deviation increase in income increases the probability of being in the top happiness categories by 11 percentage points (41%) while if the parent is not in the top health category there is essentially no increase in happiness from additional income. These results are thus consistent with health gains being more valuable for those with higher incomes - health matters only instrumentally by permitting the enjoyment of having more resources. These findings are only suggestive, of course, as we have shown OLS can yield misleading results when examining health outcomes and behaviors.

IX. Conclusion

Obesity is a growing problem worldwide, especially among children and younger adults with low levels of schooling and family incomes. This relatively new health phenomenon poses a unique challenge for health policy because it is unlikely caused by lack of access to medical care and lack of family resources, since inhibiting obesity does not significantly adversely affect a family's budget nor require medical inputs. In this paper we have used new and unique panel data on twins in a poor rural area to identify the causal effect of schooling on unhealthy weight and the mechanisms by which schooling affects costless (or budget-increasing) health-related behaviors such as exercise and foregoing smoking.

Our data permit us to relax critical and untestable identification assumptions required using existing data sets, including that twins are identical, that treated and untreated populations experience common shocks, and that health assessment criteria do not differ by schooling level or change over time. In particular, our estimation method obtains identification of schooling effects and some of the mechanisms by which schooling affects health based on changes in differences in schooling and in health behaviors and outcomes between twins over time. While we can never know if the new estimates are completely without bias without a full understanding of the remaining sources of variation in schooling over time and across twins, using a placebo test that assumes there is no causal effect of one's own schooling on own height, we were able to reject the assumptions underlying within-twin MZ estimators while our new estimator passed this test. In addition, we were able to verify a key identification assumption of our estimation method, the time-invariance of endowment effects, consistent with previous findings in the literature. Our estimates based on our new method, indicate that increased schooling significantly lowers the probability of being overweight among children and adolescents. Thus, the observed negative association between schooling and being overweight appears robust to an estimation method that relaxes critical assumptions about sources of bias.

Beyond providing estimates that imply a causal effect of schooling on "costless" health, we also sought to identify the causal mechanism. Using a simple model in which variation in behaviors with health consequences, such as diet, exercise and being smoke-free, are costless but that delivers comparative static results consistent with the observed negative relationships between income and obesity and smoking, we showed how one can identify the information role of schooling using data on health information provided by parents, even when schooling increases healthy behavior because it increases the opportunity cost of low health. Using our new estimation method, we found that approximately 50% of the causal effect of schooling on smoking and exercise is due to lack of health information. We also estimated, using the same estimation procedure, the effects of changes in the overweight status between twins over time on the changes in the differential parental assessments of their health. We found, also consistent with the information role of schooling, that parents with less than nine years of schooling were evidently unaware of the adverse consequence of being overweight. For schooling attainment above that level, additional parental schooling increased linearly the probability of a correct health assessment of the children's overweight status. Using data on the self-reported happiness of parents, we were also able to confirm the hypothesis, embedded in the model, of a rising opportunity cost of lower health with income.

Our findings thus suggest that higher levels of obesity among the poor and less educated are due in part to ignorance about the health consequences of overeating and other unhealthy behaviors as well as a lower incentive to be healthy associated with lack of alternative utility-enhancing options that require higher incomes to exploit. These results have implications for policies aimed at increasing "costless" health maintenance activities among the poor. For example, the Lancet Commission on World Obseity (Swinburn *et al.*, 2019) recommended increasing incomes and insuring that all foods were labeled with nutrient and calorie information. The former recommendation is obviously correct, but is only a long-run solution. However, our findings suggest that food nutrition labeling will be ineffective because among the relevant populations the adverse consequences of consuming excess calories are less strong and there is lack of awareness of why calories can be unhealthy. Information on the health consequences of weight gain, of smoking and of exercise targeted to low-education populations as well as initiatives that increase the incentives for the poor to forego pleasurable, cheap but unhealthy activities, in the absence of income gains, may be more appropriate.

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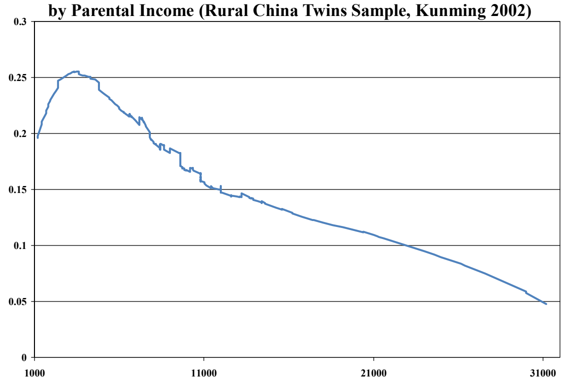
Figure 1: Fraction of Overweight Children Aged 7-16 (WHO Child Standards), by Parental Income (Rural China Twins Sample, Kunming 2002)



1

Figure 2: Fraction of Overweight Children Aged 7-16 by Year (WHO Child Standards), by Parental Income (Rural Indonesia)

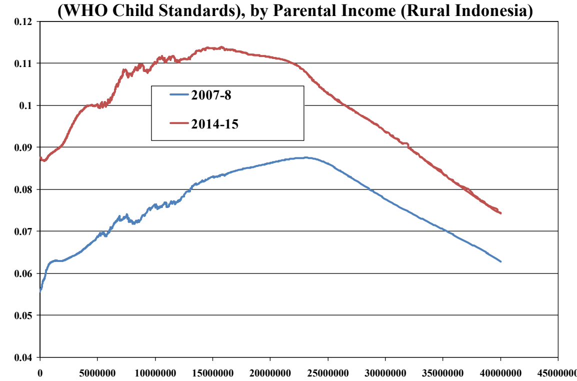


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Figure 3: Fraction of Overweight Children Aged 7-16 (WHO Child Standards), by Household Income (United States, 2013-14)

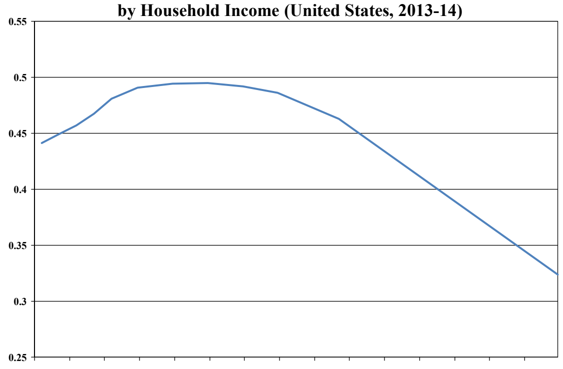

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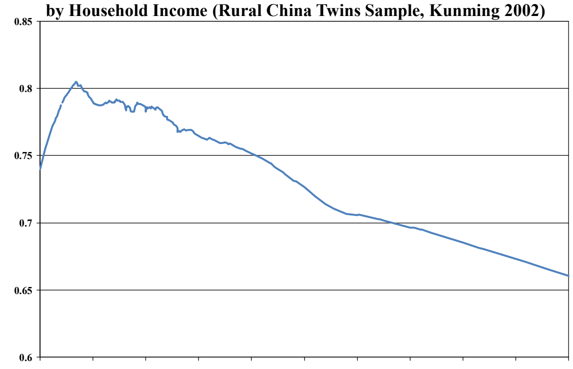
Figure 4: Fraction of Male Parents Who Smoke, by Household Income (Rural China Twins Sample, Kunming 2002)



4

Table 1
Characteristics of the Parents of the Rural Twins in the Panel

Variable/parent	Mother	Father
Schooling (years)	6.97	7.85
	(2.27)	(2.40)
Age, second round	47.7	49.4
	(4.63)	(5.58)
Monthly income, first round	297.5	519.7
	(299.1)	(485.6)
Monthly income, second round (2002 RMB)	986.8	1490
	(1137)	(1501)
Height (cm)	159	167
	(5.09)	(5.16)
BMI	22.4	23.0
	(3.13)	(2.91)
Overweight (BMI $\geq$ 25)	0.181	0.213
	(0.385)	(0.410)
Below top health category	0.673	0.648
	(0.469)	(0.478)
Smoke, first round	0.020	0.814
	(0.139)	(0.389)
N	542	538

Standard deviation in parentheses.

Table 2
Characteristics of the Rural Twins in the Panel, by Round

Variable/round	First	Second
Schooling (years)	4.78	11.6
	(2.74)	(2.81)
In school	0.888	0.211
	(0.315)	(0.408)
Age	11.1	22.1
	(2.96)	(3.14)
Birthweight (kg)	2.48	2.48
	(0.441)	(0.441)
Height (cm)	134	164
	(19.4)	(7.35)
BMI	17.7	20.9
	(3.32)	(2.49)
Below top health category	0.866	0.360
	(0.341)	(0.480)
Smoke	0.010	0.194
	(0.0977)	(0.396)
Male	0.506	0.506
	(0.500)	(0.500)
N	1,094	1,094

Standard deviation in parentheses.

Table 3
Mean BMI and Percent Overweight
Rural Twins, by Round and Definition

Health variable	Mean BMI (SD)	Percent Overweight		
Definition/Round	Kg/(m^2)	WHO Adult (BMI$\geq$25)	Asian Adult (BMI$\geq$23)	WHO Child and Adult
First (N=1,096)	17.7 (3.38)	3.01	5.57	19.1
Second (N=1,004)	20.9 (2.49)	5.78	17.7	5.78

Mean age in the first round = 11.1 (2.99). Mean age in the second round = 22.1 (3.17).

Table 4
OLS: Parent Characteristics and Child Height, Schooling and Body Mass in the First Round,
Rural Twins Aged 7-15 in 2002

Variable	Height		Schooling		BMI		Overweight	
Parent income x 10^{-4}	2.12	1.53	0.227	0.0405	-0.160	-0.170	-0.0434	-0.0474
first round	(0.571)	(0.584)	(0.224)	(0.224)	(0.173)	(0.188)	(0.0212)	(0.0233)
Father's height	0.251	0.211	0.0280	0.0323	-0.0505	-0.0516	-0.00600	-0.00646
	(0.0976)	(0.0978)	(0.0376)	(0.0359)	(0.0350)	(0.0357)	(0.00369)	(0.00372)
Mother's height	0.153	0.151	0.00465	-0.00714	0.0166	0.0127	0.000157	-0.000174
	(0.0994)	(0.101)	(0.0367)	(0.0371)	(0.0337)	(0.0344)	(0.00368)	(0.00371)
Father's schooling	-	0.0987	-	0.160	-	-0.0631	-	-0.0122
		(0.225)		(0.0812)		(0.0919)		(0.00836)
Mother's schooling	-	0.719	-	0.0769	-	0.0393	-	0.0118
		(0.240)		(0.0885)		(0.0743)		(0.00792)
Child is male	0.594	0.625	-0.259	-0.172	0.536	0.573	0.0885	0.0885
	(0.837)	(0.827)	(0.326)	(0.325)	(0.291)	(0.294)	(0.0306)	(0.310)
Age	5.73	5.67	0.380	0.395	0.233	0.242	-0.0457	-0.0461
	(0.170)	(0.172)	(0.0650)	(0.0662)	(0.0572)	(0.0588)	(0.00575)	(0.00588)
N	724	714	724	714	714	714	724	714

Robust standard errors clustered at the household level in parentheses.

Table 5
OLS: Parent Characteristics and Child Height, Schooling and Body Mass in the Second Round, Rural Twins Aged 7-15 in 2002

Variable	Height		Schooling		BMI		Overweight	
Parent income x 10^{-4} first round	0.743 (0.272)	0.577 (0.272)	0.513 (0.175)	0.243 (0.170)	-0.0163 (0.0140)	-0.145 (0.145)	-0.00866 (0.0184)	-0.00236 (0.0198)
Parent income x 10^{-4} second round	0.0560 (0.0500)	0.0386 (0.0498)	0.0801 (0.0381)	0.0517 (0.0355)	-0.00519 (0.0253)	-0.00194 (0.0264)	-0.00504 (0.00309)	-0.00425 (0.00325)
Father's height	0.333 (0.0401)	0.340 (0.0391)	0.0597 (0.0275)	0.0562 (0.0271)	-0.0129 (0.0220)	-0.0119 (0.0224)	-0.00196 (0.00324)	-0.00243 (0.00324)
Mother's height	0.338 (0.0431)	0.328 (0.0433)	0.0415 (0.0259)	0.0211 (0.0258)	-0.0192 (0.0205)	-0.0191 (0.0206)	-0.00092 (0.00313)	-0.00114 (0.00318)
Father's schooling	-	0.0782 (0.0901)	-	0.207 (0.0527)	-	0.00159 (0.0439)	-	-0.00119 (0.00717)
Mother's schooling	-	0.0987 (0.100)	-	0.170 (0.0575)	-	-0.0298 (0.0502)	-	-0.00795 (0.00761)
Child is male	9.75 (0.403)	9.78 (0.403)	-0.840 (0.221)	-0.821 (0.214)	1.40 (0.187)	1.44 (0.190)	0.150 (0.0277)	0.0230 (0.00551)
Age	0.129 (0.0698)	0.136 (0.00689)	-0.0657 (0.0447)	-0.0719 (0.0442)	0.201 (0.0343)	0.207 (0.0351)	0.0227 (0.00539)	0.0230 (0.00552)
N	834	818	890	874	825	809	825	809

Robust standard errors clustered at the household level in parentheses.

Table 6
Distribution (Percent) of the Absolute Value of Within-Pair Differences in Schooling Years, by Round

Abs(schooling years difference)	First Round	Second Round
1	7.87	2.59
2	2.38	8.87
3	0.36	6.28
4	0.55	3.51
5	0.37	3.70
6	0	2.22
7	0.18	1.48
8	0	0.55
9	0	0.55
10	0	0.18
11	0	0
12	0	0.37
0	88.3	69.7

Table 7
Distribution (Percent) of Schooling of Rural Twins in the Second Round

Schooling years	All Twins	Twins in Twin Pairs with Schooling Differences
3	1.10	1.20
6	7.95	7.49
9	1.19	0.90
10	41.0	24.9
12	19.0	25.8
13	9.96	6.89
14	4.94	8.38
15	5.58	11.1
16	3.75	5.69
17	5.39	7.49
20	0.09	0.30
N	1,094	334

Table 8
Test of Equality of Birthweight Effects Across Rounds, by Variable

Variable	BMI		Overweight		Schooling	
Round	First	Second	First	Second	First	Second
Birthweight	0.844 (0.297)	0.679 (0.373)	0.0511 (0.0296)	0.0745 (0.0388)	0.186 (0.122)	0.151 (0.353)
H_0: equality, robust $\lvert t \rvert$ [p]	0.36 [0.722]		0.48 [0.632]		0.10 [0.919]	
N	423		423		475	

Robust standard errors in parentheses.

Table 9
Placebo Test: Own Schooling and Height, by Survey and Estimation Method

Sample	Chinese Adult Twins Survey		Chinese Child Twins Survey, Rural Sample[a]			
Method	OLS	Within Twin MZ	OLS[b]	Within Twin MZ[b]	Individual FE	Twin DD
Schooling	0.226 (0.042)	0.070 (0.042)	0.00196 (0.000445)	0.00146 (0.000729)	0.00441 (0.000323)	0.000995 (0.00102)
Birthweight	1.701 (0.263)	1.046 (0.292)	0.00426 (0.00259)	0.00248 (0.00436)	-	-
Male	11.2 (0.263)	-	-0.0150 (0.00246)	-	-	-
N	2,866	1,746	1,007	321	2,087	1,007

[a] Dependent variable = height/WHO height age standard. [b]Second round of the survey. Robust standard errors clustered at the household level in parentheses.

Table 10
Schooling, BMI and Being Overweight, Rural Twins, by Estimation Method

Variable/method	OLS		Within Twin	Within Twin MZ	DD Twin
			BMI		
Schooling	-0.111 (0.0363)	-0.0793 (0.0381)	-0.140 (0.0522)	-0.181 (0.162)	-0.169 (0.0645)
Parental income (x10^{-6})	-0.950 (2.68)	-3.39 (2.59)	-	-	-
Parents provide health advice	-	-0.610 (0.244)	-	-	-
			Overweight (BMI $\geq$25,WHO)		
Schooling	-0.00357 (0.00332)	-0.00218 (0.00335)	-0.0110 (0.00455)	-0.0272 (0.0191)	-.0171 (0.00587)
Parental income (x10^{-6})	-0.104 (0.208)	-0.354 (0.148)	-	-	-
Parents provide health advice	-	-0.0426 (0.0162)	-	-	-
			Overweight (BMI $\geq$23, Asian)		
Schooling	-0.0157 (0.00548)	-0.0137 (0.00593)	-0.0228 (0.0110)	-0.0164 (0.0303)	-0.0193 (0.0105)
Parental income (x10^{-6})	-0.495 (0.305)	-0.699 (0.255)	-	-	-
Parents provide health advice	-	-0.119 (0.289)	-	-	-
N	996	888	996	322	996

All estimates from the second round except DD Twin estimates. Robust standard errors in parentheses.

Table 11

Health Behaviors, Schooling and Alternative Information Sources: Twins Aged 11- 16 in 2002

Estimation method	Within-Twin[a]		DD Twin	
Behavior	Exercise	Does not smoke	Exercise	Does not smoke
Own schooling	0.114 (0.0459)	0.0645 (0.0294)	0.101 (0.0364)	0.128 (0.0421)
"Superior" twin's schooling	0.0455 (0.0372)	0.0162 (0.0137)	0.0420 (0.0218)	0.0359 (0.0253)
Own schooling x S. twin's schooling	-0.00228 (0.00250)	-0.000767 (0.000894)	-0.00210 (0.00151)	-0.00138 (0.00175)
Own schooling x Parent's advice	-0.0572 (0.0274)	-0.0406 (0.0232)	-0.0606 (0.0289)	-0.0563 (0.0335)
H_0: Parent advice = 0, $\chi^2(2)$ [p]	7.68 [0.0215]		7.64 [0.0220]	
H_0: Twin's schooling = 0, $\chi^2(2)$ [p]	5.17 [0.159]		10.53 [0.0323]	
N	166		332	

[a]Second round of the survey. All within-twin specifications include child birthweight and gender. Robust standard errors in parentheses.

Table 12

Acquisition of Information and Schooling, by Source: Twins Aged 7- 16 in 2002

Estimation method	Within-Twin[a]		DD Twin	
Information Source	Read the Newspaper	Web Surf	Read the Newspaper	Web Surf
Own schooling	0.0258 (0.0155)	0.0260 (0.0127)	0.0636 (0.00300)	0.0718 (0.00301)
N	788		1536	

[a]Second round of the survey. All within-twin specifications include child birthweight and gender. Robust standard errors in parentheses.

Table 13
Child Overweight Status, the Respondent Parent's Assessment of their Child's Health and the Respondent Parent's Schooling
Dependent Variable = Below the Top Health Category

Variable/method	OLS[a,b]		Within Twin[b]		DD Twin	
Child overweight (BMI>=25)	-0.0404 (0.0690)	-0.0716 (0.00820)	0.121 (0.114)	-0.315 (0.321)	0.146 (0.105)	-0.353 (0.321)
Respondent schooling (*S*)	-0.00213 (0.0480)	-0.00230 (0.0164)	-	-	-	-
S x child overweight	-	0.00418 (0.283)	-	0.0573 (0.0361)	-	0.0659 (0.0371)
H_0: overweight coefficients = 0	-	F(2,496) 0.19	-	F(2,473) 2.45	-	F(2,470) 3.36
Overweight effect at *S* = 3 years	-	-0.0590 (0.140)	-	-0.143 (0.222)	-	-0.155 (0.218)
Overweight effect at *S* = 6 years	-	-0.0465 (0.0782)	-	0.0291 (0.138)	-	0.0419 (0.129)
Overweight effect at *S* = 9 years	-	-0.0339 (0.0841)	-	0.201 (0.110)	-	0.240 (0.103)
Overweight effect at *S* = 12 years	-	-0.0214 (0.150)	-	0.373 (0.169)	-	0.437 (0.172)
Overweight effect at *S* = 13 years	-	-0.0172 (0.176)	-	0.431 (0.198)	-	0.503 (0.203)
Overweight effect at *S* = 15 years	-	-0.00883 (0.229)	-	0.545 (0.262)	-	0.635 (0.271)
Overweight effect at *S* = 16 years	-	-0.00464 (0.256)	-	0.603 (0.295)	-	0.701 (0.305)
N	958	958	988	988	988	1,016

[a]Specification also includes both parent's schooling, household income, child age, whether the child is male and whether the respondent is the father. [b]Second round. Robust standard errors clustered at the household level in parentheses.

Table 14
Health, Income and Happiness: Rural Parents Aged 40-59, Second Round

Income (x 10^{-4})	0.147	0.341
	(0.0756)	(0.0761)
Income (x 10^{-4}) x health below excellent	-	-0.363
		(0.0837)
Health below excellent	-0.103	-0.0218
	(0.0382)	(0.0417)
Age	-0.00662	-0.00627
	(0.0040)	(0.00397)
Smokes	0.0206	0.0125
	(0.0467)	(0.0462)
Male	0.0401	0.0252
	(0.0405)	(0.0400)
N	779	779

Robust standard errors clustered at the household level in parentheses.

FOR ONLINE PUBLICATION

Appendix A

What happens if increasing h requires a per-unit payment of p? This makes unhealthy behavior more like a normal good. The consumption equation is now

$$C = \omega s + y - ph$$

The second-order condition requires that

$$\Phi = U'' - \eta f'' V + p[p(1 - H)V'' + 2\eta f' V'] < 0.$$

The income effect is now

$$\mathrm{d}h/\mathrm{d}y = [\eta f' V' + p(1 - H)V'']/\Phi$$

The first term reflects the effect of increases in y on the shadow price of unhealthy behavior, and leads to less unhealthy behavior. But the second term is the opposite sign - increases in income now also diminish the marginal utility of consumption, and thus the shadow price of bad health. Thus, the income effect is ambiguous, but empirically we see that higher income and unhealthy behaviors are negatively correlated, which is predicted by the model in which unhealthy behaviors are costless.

The effect of additional information e on h is now:

$$\mathrm{d}h/\mathrm{d}e = \eta_e[f' V - fV'p]/\Phi$$

The effect of schooling on unhealthy behavior consists of the information effect and the income effect - schooling increases the shadow price of unhealthy behavior, increases income and augments the perceived bad-health effect of increasing h:

$$\mathrm{d}h/\mathrm{d}s = \omega \mathrm{d}h/\mathrm{d}y + \eta_s[f' V - fV'p]/\Phi$$

The first bracketed term is the effect of schooling on unhealthy behavior due to its raising income and thus the shadow price of unhealthy behavior, and the second term arises due to schooling increasing the *perceived* effect of h on bad health. Increases in schooling lower unhealthy behavior due to these effects. Because an increase in unhealthy behavior lowers consumption directly due to its financial cost (p>0), the schooling effect is attenuated relative to the case in which unhealthy behavior has negligible effects on the budget constraint - the increase in consumption and thus the shadow price due to increases in schooling is less.

Table A1
ML Probit: Probability of Finding a Rural Household in the Second Round with Twins Aged 7-16 in 2002

Variable	Coefficient	SE
Father’s schooling	-0.0397	0.0229
Mother’s schooling	0.0349	0.0250
Father’s age	0.0356	0.0178
Mother’s age	0.0367	0.0198
Parents’ total monthly wage income x 10^{-4}	0.665	1.10
Household income < 3000	-	-
Household income 3000-4999	-0.172	0.195
Household income 5000-9999	-0.00425	0.190
Household income 10000-19999	-0.168	0.211
Household income 20000-29999	0.254	0.273
Household income 30000-49999	-0.509	0.491
Household income >50000	-1.05	1.02
N	730	
All variables ÷^{2}(11) [p]	16.6 [0.12]	
Household income ÷^{2}(6) [p]	7.98 [0.24]	
Parents’ age ÷^{2}(2) [p]	4.11 [0.13]	
Parents’ schooling ÷^{2}(2) [p]	3.76 [0.15]	

.05 level of significance, two-tailed test. .10 level of significance, two-tailed test.

Appendix Figure 1. Percent of Twins Who Smoke, by Age