# Are the Children of Uneducated Farmers Doubly Disadvantaged? Economic Reform, Structural Change and Intergenerational Educational Mobility in Rural China

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ABSTRACT

This paper relaxes the single factor model of educational mobility and analyzes heterogeneous effects of family background on children's education in rural China, with a focus on market reform and non-farm occupations. We use data on three generations in rural China spanning pre- and post-reform periods. Evidence from a battery of econometric approaches shows that the influence of family background remains stable for girls, but for boys, family background has become more important after market reform. The mean effects of parents education miss substantial heterogeneity across farm-nonfarm occupations. Having nonfarm parents, in general, has positive effects, but children of low educated non-farmer parents (with higher income) do not enjoy any advantages over the children of more educated farmer parents. Estimates of cross-partial effects without imposing functional form show little evidence of complementarity between parental education and non-farm occupation. We explore causality using three approaches: Rosenbaum sensitivity analysis, minimum biased IPW estimator and heteroskedasticity based identification. Our results suggest that the advantages of having more educated parents, especially with nonfarm occupations, are unlikely to be due solely to selection on genetic transmissions. However, the estimated positive effects of nonfarm over the farmer parents among the low educated households may be driven entirely by moderate selection on genetic endowment.

**Key Words**: Educational Mobility, Inequality, Rural China, Nonfarm, Education and Occupation, Family Background, Heterogeneity, Complementarity, Market Reform, Gender Gap
**JEL Codes:** O12, J62

[1]We would like to thank Matthew Lindquist, Forhad Shilpi, Samantha Rawlings and participants in the NEUDC 2014 at Boston University for helpful comments on earlier versions. Special thanks to Li Shi, Zhan Peng, and Song Jin for help with the CHIP survey data. An earlier version of the paper was circulated under the title: "Are the Children of Uneducated Farmers Doubly Disadvantaged? Farm, Nonfarm and Intergenerational Educational Mobility in Rural China". The current version supersedes the earlier versions. The standard disclaimers apply.
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## (1) Introduction

Increasing inequality over the last few decades in many developing countries has led to a renewed interest in understanding intergenerational economic mobility. However, progress has been slow, constrained by a lack of data required for measurement of permanent income (socio-economic status) across multiple generations. This paper provides an analysis of intergenerational educational mobility in rural China that makes progress with limited data, and brings the focus on the role of non-farm occupations in the reform period. A research design is developed that relaxes the single factor model of intergenerational educational mobility, and constructs a broader measure of socio-economic status by combining parents' education and occupation in the absence of long-term panel data on income.[2] We take advantage of a data set that covers three generations spanning pre- and post-reform periods, and provide evidence on the effects of market reform on educational mobility in rural China.

The economics literature on intergenerational educational mobility has focused on parental education as the relevant indicator of family background for understanding intergenerational linkages (see Bjorklund and Salvanes (2011) for a recent survey, and on China, see, among others, Knight et al. (2013), Sato and Li (2007), Emran and Sun (2011)). This emphasis on parental education is eminently appropriate when the goal is to estimate the causal effects of parental education on children's schooling (for a recent survey, see Holmlund et al. (2011)). However, this approach may be less than satisfactory in understanding intergenerational transmission of economic status, where the focus is on the role of family background in generating and sustaining educational inequality. Because it implicitly assumes that parents' education is effectively a sufficient statistic for family background relevant for children's education, and thus ignores the role of parents' occupation, regarded as the most important indicator of socio-economic status in a large literature on mobility in Sociology (see, for example, Grusky and Cumberworth (2010)).[3] This omission seems especially surprising in the context of villages in developing countries, given substantial evidence that non-farm occupations yield higher income, and non-farm income may

[2]For an interesting analysis of and evidence against the single factor model of intergenerational income correlation, please see Lefgren, Lindquist and Sims (2012).

[3]Grusky and Cumberworth (2010) note: "...sociologists typically carry out analyses of intergenerational mobility in terms of occupations....., because occupations are so deeply institutionalized in the labor market, they serve as a powerful omnibus indicator of life conditions and chances."

be an important source of increasing inequality in rural areas (for recent surveys, see Lanjouw and Feder (2001), Haggblade et. al. (2007), and on rural China see Benjamin et. al. (2005), Rozelle (1994), Yang and An (2002)).[4]

Like in many other developing countries, the rural economy in China has experienced fundamental structural transformation over the reform period, with impressive growth in non-farm employment and output in the recent decades. The share of non-farm sector in household income in rural China increased from 22 percent in 1980 to 51 percent in 2001.[5] There are potential interactions between education and non-farm occupations which may exacerbate inequality. Consistent with Schultz-Nelson view that education equips people to deal with disequilibria, the evidence indicates that educated families in rural China were the first responders to the incentives created by household responsibility system and related reforms, and were able to take advantage of the growing non-farm sector to reap higher income (Yang (2004), Yang and An (2002)). Positive feedbacks among education, nonfarm occupation, and income may lead to a bifurcation where the children born into parents with higher education and nonfarm occupation enjoy a clear and cumulative advantage in education, while the children of uneducated farmers are trapped in low educational attainment.

A broader conception of parental economic status that combines both education and occupation is specially desirable from a "measurement perspective", because it provides a more complete measure of permanent income of parents.[6] An important lesson from the rich literature on intergenerational income persistence in developed countries which partly motivates our analysis is that income data available from cross-section household surveys or short panels yield estimates which are severely biased downward (see Solon (1992), Mazumder (2005), Atkinson et al. (1983)). It is extremely difficult, in fact nearly impossible, to find long-term panel data on parents' income in developing countries.[7] A natural alternative, surprisingly ignored in the existing economics literature, is to combine two most salient indicators of parents' socio-economic status routinely

[4]The role of non-farm occupations in the recent rise in inequality in has also attracted increasing attention in India (Himanshu et al. (2013), Rama et al. (2014)).

[5]See National Bureau of Statistics of China, Statistical Year Book, 2003.

[6]Parents' education and occupation are also among the most salient 'circumstances' in the related but distinct literature on 'inequality of opportunity'. For recent contributions in that literature see, for example, Brunori, Ferreira, and Peragine (2013).

[7]Income data are especially prone to measurement error in villages in developing countries due to large informal and non-market economy (Deaton (1997)).

available in standard household surveys: education and occupation.[8]

We develop a research design in terms of binary indicators of parents' education and occupation: higher education ($E_i^p = 1$, if at least one parent of child $i$ has education more than a threshold such as primary or middle), and non-farm occupation ($O_i^p = 1$, if at least one parent's primary occupation is non-farm).[9] We thus split the sample into four mutually exclusive groups (see Figure (1) below).[10] This framework allows us to use recently developed matching and propensity score weighted estimators (Huber et al. (2013), Millimet and Tchernis (2013)) with appropriate subsamples as "treatment" and "comparison" groups to explore potentially heterogeneous effects of family background on children's schooling.[11]

The central challenge in causal interpretation of the estimated intergenerational links is whether the observed intergenerational persistence can be driven primarily by genetic transmissions of ability and preference across generations. In the absence of any credible exclusion restrictions, we explore the role of genetic transmissions in two steps.[12] First, we implement sensitivity analysis using Rosenbaum bounds under the assumption that the estimates effects of economic status are biased upward because of a positive correlation in genetic ability and preference.[13] Second, we take advantage of the recently developed econometric approaches that corrects for selection on unobservables without imposing any exclusion restrictions (Millimet and Tchernis (2013), Lewbel (2012)). We use data from the 2002 round survey of the Chinese Household Income Project (CHIP) for our empirical analysis.

[8]Many household surveys in developing countries contain income information only for a single year (the survey year). As shown by Mazumder (2005) in the context of USA, income data for a period of a decade and half may be required to tackle the measurement error. Since measurement error is likely to be a much more severe problem in income data from developing countries, one probably needs data over a longer period of time.

[9]A reader might wonder whether it would be best to include all of the available indicators of parental socio-economic status and create an index using statistical techniques such as principal components. It is, however, difficult to interpret the estimates based on a principal component index, because ranking according to, for example, the first principal component lacks any clear economic content.

[10]We focus on primary schooling as the education threshold to keep estimates comparable across three generations. The proportion of grandparents with of more than middle school is too small (4 percent) for any meaningful analysis. However, in an online appendix, we report estimates for parents-children sample using middle school as the relevant cut-off.

[11]Most of the existing analysis of intergenerational educational mobility in developing countries including China relies exclusively on the OLS estimator.

[12]Please see the discussion in section 4.2 below on the challenges in finding appropriate natural experiments to estimate heterogeneous effects of parent's economic status in our context.

[13]To the best of our knowledge this is the first paper in the literature on intergenerational mobility to exploit Rosenbaum bounds to understand whether the estimated persistence can be driven by selection on genetic endowments.

The substantive conclusions of this paper can be summarized as follows. First, we find evidence of substantial heterogeneity. Focusing on the children's generation, the standard specification with parental education as the sole indicator of economic status shows that a child of a parent with more than primary schooling gains about a year more of schooling. But our results reveal substantial heterogeneity across occupations within a given education group and across education levels within a given occupation group. For example, within the low education sub-sample, a son attains about 0.80 years of additional schooling when born into a non-farm household compared to a farm household, and the corresponding gain in schooling for a girl is about 0.60 years.[14]

Second, the research design allows us to test whether children's schooling function is supermodular in parental education and occupation (complementarity) without imposing arbitrary functional forms. We find little evidence of complementarity between parental education and occupation in determining the schooling attainment of the children. In fact, our evidence indicates that, if anything, for boys in the reform era, parents' non-farm occupation may be a substitute for parents' education.

Third, a comparison of parents' and children's generations shows that, for girls, the role of family background in schooling attainment remains largely unchanged across three generations. But for boys, family background has become more important in the reform era. A comparative analysis of parents' and children's generations is interesting because most of the parents completed schooling before the reform which allows us to compare and contrast the effects of family background in a socialist versus a more market oriented economy.[15]

Fourth, the evidence from multi-pronged analysis of the role of genetic transmissions shows the following. It is highly unlikely that the educational advantages that come with having better educated parents (i.e., more than primary or middle schooling), especially the educated nonfarmer parents, are driven by genetic correlations. In contrast, the estimated positive effects for the nonfarmer group within the low educated (primary or less) households by OLS and matching estimators may not reflect any causal impact. The evidence from Lewbel heteroskedasticity based

[14]The children from uneducated farming households constitute the comparison group.

[15]As we discuss later, the farm and nonfarm distinction carries different meanings before and after the reform, because the policies implemented during the Maoist era (in particular the Cultural Revolution) were aimed at enhancing the social position of peasants and improving educational mobility of their children (see, among others, Hannum and Xie (1994), Sato and Li (2007), and Hannum et al. (2008)).

identification confirms that the effects of family background has become much stronger for the sons in the reform era.

### (2) Related Literature

This paper contributes to a small but active literature on intergenerational economic mobility in developing countries. Recent contributions in this literature include, among others, Hertz et al. (2007), Binder and Woodruff (2002), Behrman et. al. (2001), Duncan (1996), Lillard and Willis (1995), Emran and Shilpi (2011, 2012), Bossuroy and Cogneau (2013), Maitra and Sharma (2010), Zhang et al. (2014)). However, none of the existing studies exploit both parents' education and occupation to construct a broader measure of a household's economic status, and thus do not analyze the implications of the dramatic expansion of non-farm occupations for educational mobility of children in rural areas where most of the poor households live in developing countries.

Our analysis is obviously related to multiple strands of literature on China: educational inequality, economic mobility, rural nonfarm economy and the interactions among them. A large part of the literature on inequality in China focuses on spatial differences between coastal and interior regions, and across rural and urban areas (see, among others, Fleisher et al. (2010), Kanbur and Zhang (2005), Park (2008)). For insightful analysis of inequality in post-reform period see, among others, Li et al. (2013), Benjamin et al. (2008). Benjamin et al. (2005) provide an in-depth and comprehensive analysis of the evolution of inequality during the transition in rural China. An important finding in Benjamin et al. (2005) which is partly responsible for our focus on non-farm occupations is that non-farm income has played an important role in worsening income distribution in rural China since 1995. For similar evidence on the inequalizing role of non-farm occupations in rural India, see Himanshu et al. (2013).

The effects of non-farm opportunities on educational attainment of rural children have been analyzed by de Brauw and Giles (2006) with a special focus on migration; they show that the urban migration opportunities affect schooling attainments of poor rural children adversely. The structural change within the rural economy from farm to non-farm in rural China has been the focus of a substantial literature (see, for example, Nyberg and Rozelle (1999), Mukherjee and Zhang (2007), de Baruw et. al. (2013)). According to the estimates reported in de Baruw et al. (2013), the rural economy in China has experienced significant structural change during the reform period; from 1991 to 2004 the proportion of households reporting positive time allocation to farm activities fell from 89 percent 70 percent, and the household engaged in farm work, the average total hours devoted to farm work declined dramatically from 3,528 in 1991 to 1,756 in 2004.

Although the research on economic mobility in China has focused primarily on urban areas (see, for example, Deng et al. (2013), Gong et al. (2012)), there is a small but growing strand that focuses on rural China. Some of the recent contributions, closer to our interest, analyze intergenerational educational mobility in rural China; see, for example, Knight et al. (2013), Sato and Li (2007), Emran and Sun (2011). However, all of the available economic research on educational mobility in developing countries including China relies on the standard single factor representation of parents' economic status, where parents' education is the sole indicator.

We also take advantage of a rich literature on educational policy and returns to education in China to interpret our results (see, among others, Hannum and Park ed. (2007), Hannum et. al. (2008), Fleisher and Wang (2005), Hannum and Xie (1994), Debrauw and Rozelle (2008), Fang et. al. (2012), Tsang (2000)). The recent literature on intergenerational educational mobility identifies a close link between an increase in returns to education and intergenerational educational persistence. For example, in the context of USA, Aaronson and Mazumder (2008) and Mazumder (2012) find that the periods with high returns to education are also characterized by high intergenerational persistence (i.e, low mobility).[16] The evidence from the literature on China shows that while private returns to education was very low before and during the early period of the economic reform, the returns have been increasing over the reform era (Hannum et al. (2008)). The recent estimates by Fang et al. (2012) show that the over-all returns to one more year of schooling between 1997 and 2006 is about 20 percent for individuals 35 years of age or younger in 2000. The available evidence also indicates that the returns to education is higher in non-farm activities and there is a gender penalty against girls (de Brauw and Rozelle (2008)). The literature on the changes in educational policy in rural China and its implications for inequality is rich with many interesting and insightful contributions (see, for example, Hannum and Xie (1994), Hannum and Park (2007), Tsang (2000), Ma and Ding (2008)). Hannum and Xie (1994) discuss

[16]For the relevant theory underlying the link between increasing returns to education and lower mobility, see, for example, Solon (2004).

the role played by the conflicting objectives of efficiency and equity in changing educational policy in China, before and after the reform. The implications of fiscal decentralization starting from mid 1980s for educational inequality has been underscored by many authors (see, for example, Brown and Park (2002), Hannum and Park (2007), Hannum et al. (2008)).

**(3) Conceptual Framework**

A large literature on intergenerational educational mobility, both in developed and developing countries, analyzes the persistence in educational attainment across generations, where parents' education is used as a measure of economic status. The standard regression specification used almost universally is as follows:

$$E_i^c = \alpha_0 + \alpha_1 E_i^p + \Omega' X + \varepsilon_i \tag{1}$$

where $E_i^c$ is the years of schooling of children $i$ , $E_i^p$ is an indicator of parental education of child $i$ , $X$ is a vector of exogeneous control variables. A substantial body of evidence based on this or variant of this specification shows that parental education is strongly correlated with children's education. However, there are at least two features of the above specification which requires scrutiny. First, it is implicitly assumed that parent's education is effectively a sufficient statistic for the socio-economic status a child is born into, and second, the effect of parental education is usually assumed to be constant for all households. When one allows for heterogeneous effects, $\alpha_1$ can be interpreted as an average of the effects.

There is a parallel literature that focuses on intergenerational occupational mobility, the corresponding regression specification is:

$$O_i^c = \gamma_0 + \gamma_1 O_i^p + \Theta' X + \epsilon_i \tag{2}$$

where $O_i^c$ is an occupation dummy (it takes on the value of 1 when children's occupation is non-farm in our analysis), and $O_i^p$ is the corresponding occupation dummy for parents.

The recent economics literature on intergenerational mobility in developing countries, has focused on education and occupational linkages separately. Since parents' occupational choices depend on their education, among other things, the standard specification as in equation (1) partly captures the effects of occupation on children's education. In fact, there is a substantial literature that finds a significant positive effect of education on the probability of non-farm participation in villages in developing countries (Lanjouw and Feder (2001)). It is, however, important to appreciate that educational attainment of children may depend on parental occupation in rural areas (agriculture vs. non-farm) even after the 'total effects' (i.e., total derivative) of parental education are accounted for. First, parents engaged in non-farm occupation are likely to have higher permanent income, even when their educational attainment is low.[17] The role of high income non-farm occupations may have become increasingly important after the fiscal decentralization in China, because of the importance of fees collected by the schools, especially in poor counties (Brown and Park (2002)). Second, the returns to education is higher in non-farm sector compared to agriculture in most of the developing countries. The expected higher returns to education leads to higher investment in children's education, given the cost, and thus strengthen the link between parental economic status and children's schooling, according to Becker and Tomes (1979) model.

Once we acknowledge that both parental education and occupation are important for a more complete measure of economic status, an immediate question arises about the nature of interaction between them: are they complementary or substitutes?[18] These considerations may lead one to the following specification of the effects of family background on children's education:

$$E_i^c = \beta_0 + \beta_1 E_i^p + \beta_2 O_i^p + \beta_3 \left(E_i^p \times O_i^p\right) + \Upsilon' X + \upsilon_i \tag{3}$$

In this framework, $\beta_3 > 0$ implies complementarity between parental education and occupation, while $\beta_3 < 0$ implies substitutability, and $\beta_3 = 0$ suggests separability. While the above specification is intuitive and useful, it suffers from some limitations. First, we have to estimate three parameters ($\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$), which precludes the use of a rich array of econometric approaches developed for a binary treatment, for example, matching and propensity score weighted estimators (Busso et al. (2014)). Second, it is restrictive in testing potential complementarity, because it imposes strong functional form assumption. To get around these limitations, we split

[17]In the context of rural China, non-farm occupations are positively correlated with higher household income in the post reform period (see the evidence in Table 1A). But it may not be the case for the pre-reform period, especially during Cultural Revolution. Unfortunately, we do not have any income information for the grandparents.

[18]The linear specification that ignores possible interactions is almost universal in the literature on intergenerational mobility, both in economics and sociology.

the sample in four mutually exclusive groups in terms of binary indicators of parents' educational and occupational status:

$$D_i^{00} \equiv \left(E_i^p = 0, O_i^p = 0\right), D_i^{01} \equiv \left(E_i^p = 0, O_i^p = 1\right); D_i^{10} \equiv \left(E_i^p = 1, O_i^p = 0\right), D_i^{11} \equiv \left(E_i^p = 1, O_i^p = 1\right)$$

This $2 \times 2$ education and occupation classification provides a rich portrayal of socio-economic heterogeneity and allows us to use appropriate groups as "treatment' and "comparison" in a binary treatment set-up where there is only one parameter of interest. One can also re-specify equation (3) above as follows which can be estimated by OLS:

$$E_i^c = \theta_0 + \theta_1 D_i^{01} + \theta_2 D_i^{10} + \theta_3 D_i^{11} + \Pi' X + \varepsilon_i \tag{4}$$

where $D_i^{00}$ is the omitted category (the "comparison" group). The parameters of equations (3) and (4) are related as follows: $\beta_1 = \theta_1; \beta_2 = \theta_2; \beta_1 + \beta_2 + \beta_3 = \theta_3$. Note that in this framework, complementarity implies the following inequality: $\theta_3 > (\theta_1 + \theta_2)$. Intuitively, the intersection has a stronger effect than the union, i.e., the sum of individual effects.

Since non-farm occupations, in general, yield higher permanent income in developing countries, a reasonable ranking of the four different groups in terms of both parental income and human capital is: $D_i^{11} \succ \left[D_i^{01}, D_i^{10}\right] \succ D_i^{00}$, i.e., we can rank the groups in relation to each other, except for the two groups in the middle. The relative ranking of $D_i^{10}$and $D_i^{01}$is not unambiguous, as higher education is also correlated with higher income. In the context of most developing countries, it is reasonable to posit that $D_i^{01} \succ D_i^{10}$ if one is interested in a measure of permanent income, because the non-farm households enjoy higher income compared to the farm households even when the farm households are better endowed in terms of human capital (education). The evidence on per capita income of parents in our data set confirms this ranking for the parents-children sample, but there is no income information available for the grandparents.[19] Note that if the focus is on the transmission of parental human capital alone, then the appropriate ranking is reversed: $D_i^{01} \prec D_i^{10}$. This reversal of the ranking of these two groups in terms of parental income and education provides us with an excellent opportunity to study the relative roles of family resources

[19]As noted before, nonfarm occupational status of grandparents may not necessarily imply higher income in rural China before the reform.

compared to parental education as a direct influence over and above the income effect. Under the null hypothesis that low schooling attainment is due to parents' low income alone, parents education would matter only in so far as it affects income. In this case, we would expect that the effect of being born into a low education non-farm household $\left(D_i^{01}\right)$ should boost children's schooling much more than it would for a child born into a farm household with high education.

The research design with four mutually exclusive groups also enables us to implement an approach to potential complementarity between parental education and occupation that does not depend on the parametric specification as in equation (3) above. To fix ideas, consider the general specification of children's years of schooling:

$$E_i^c = F\left(E_i^p, O_i^p, X\right) \tag{5}$$

In this formulation, parental education and occupation are complementary only if the function $f(.)$ is supermodular in $E_i^p, O_i^p$ which implies the following cross-partial derivative (assuming twice differentiability):

$$\frac{\partial^2}{\partial E_i^p \partial O_i^p} F(.) > 0$$

The advantage of our framework is that we can estimate a discrete analog of the above cross-partial derivative without assuming any functional form, if we exploit matching methods (see below for different econometric approaches). A specific approach implemented in this paper estimates the partial derivative $\frac{\partial}{\partial E_i^p} F(.)$ separately for the farm $(O_i^p = 0)$and non-farm $(O_i^p = 1)$ sub-samples using an appropriate matching method, and then estimates the discrete analog of the cross-partial effect as the difference between these two estimates. The recent econometric literature points out that estimating the cross-partial effects from equation (3) may lead to incorrect conclusions when the functional form assumption is violated (Greene (2005)).

**(4) Econometric Approach**

The available economics (and sociology) literature on intergenerational educational mobility in developing countries relies mostly on the OLS estimator. We use a number of alternative estimators suggested in the recent econometrics literature on program evaluation. This may be valuable for making progress on two issues. First, to provide some evidence on the robustness of standard OLS estimates. Second, to understand whether the estimated effects could be due primarily (or exclusively) to genetic transmissions of ability and preference across generations which is central to any causal interpretation of the estimates.

**(4.1) Robustness of the OLS Estimates**

We use two matching estimators, and a propensity score weighting estimator to evaluate the robustness of the OLS estimates. Compared to standard OLS estimator, the matching estimators have two advantages: (i) they relax the linear functional form assumption and (ii) impose the common support condition to address potential lack of overlap between the treatment and comparison. The matching estimators used are: (i) bias corrected nearest neighborhood matching due to Abadie and Imbens (2002) (henceforth called 'A-I Matching'), and (ii) distance weighted bias corrected radius matching (henceforth called 'BC-RM') due to Lechner et al. (2011). The propensity score weighting estimator we implement is the Normalized Inverse Propensity Score Weighted (NIPW) estimator due to Hirano and Imbens (2001). There is substantial monte-carlo evidence in favor of these estimators when working with non-experimental data. Busso et al. (2014) provide evidence that NIPW performs best among a large set of matching and propensity score estimators in estimating a binary treatment effect, and Huber et al. (2013) provide extensive evidence from empirical Monte-carlo that the BC-RM estimator due to Lechner et al. (2011) performs very well among a wide set of estimators. We emphasize here that the matching and propensity score weighting estimators are used here to check robustness of the OLS estimates, no claim of causality can be made from these estimates.[20]

**(4.2) Causal Analysis and Estimates**

As noted earlier, a central issue in the literature has been whether most of the observed persistence can be accounted for by correlations in genetic endowments, leaving little room for economic forces to shape the opportunities faced by the children (see the discussion in Solon (1999) and Black and Devereux (2011)). Consider the following triangular model of a child's

[20]This also means that we do not present any analysis of covariate balance which is important for matching estimates to have causal interpretation.

schooling attainment and her/his parents' economic status:

$$E_i^c = \lambda_0 + \lambda_1 D_i^P + X_i\Phi + \vartheta_i \quad (6); \quad D_i^P = \delta_0 + X_i\Psi + \nu_i \quad (7)$$

Where $D_i^P$ is an indicator parents' socioeconomic status, in our case, defined by education and occupation, and the error terms capture unobservables such as ability and preference. If high ability parents produce high ability children, we expect that the correlation between the error terms is positive, $\rho = Corr\left(\nu_i, \vartheta_i\right) > 0$. This gives rise to classic "ability bias" where the effect of parental economic status $\hat{\lambda_1}$ is overestimated by OLS or matching methods.

Although the potential upward bias in the estimates of intergenerational persistence because of positive correlation in ability has been a central focus in the economics literature, the relative role of genetic factors in a child's development outcomes has been controversial since the publication of Galton's study in 1886 where he claimed the primacy of genetics. The pendulum has swung back and forth in Behavioral Genetics over the years, with the recent work attributing 50 to 60 percent variation in performance in academic test in UK (GCSE) to genetic differences (Shakeshaft et al. (2013)).[21] In contrast, Fryer and Levitt (2013) report that controlling for age, number of siblings, and other environmental factors, the partial correlation between socioeconomic status and cognitive ability is small and statistically insignificant. A child born into a family in the highest socioeconomic quintile can expect to score only 0.02 standard deviations higher on a test of cognitive ability than an average child, while one born into a family in the lowest socioeconomic quintile can expect to score about 0.03 standard deviations lower. These are small in magnitude, and are not statistically significantly different effects.[22]

There are a number of important recent contributions in the literature on developed countries which breaks the potential genetic correlation by exploiting natural experiments, but all of them (at least what is known to us) are concerned with estimating causal effects of parent's education

[21]The conclusions of Shakeshaft et al. (2013) are based on twins data as is standard in Behavioral Genetics. However, it is not clear to what extent these estimates can be treated as relevant for the wider population.

[22]It is also important to reiterate Goldberger's observation that even when the differences in academic performances are due to genetic factors, it does not imply that there are no appropriate policy responses (Goldberger (1979)). If academic performance of some children are adversely affected by genetically inherited poor eye sights, it does not necessarily mean immutable disadvantages, policies that ensure equal access to eye care and eye glasses may be sufficient to address this.

(for a survey see Black and Devereux (2011). Note that it is extremely difficult, if not impossible, to find credible natural experiments that can enable us to estimate the causal effect of four different socioeconomic status defined in the conceptual framework. Finding a natural experiment that mimics a randomized allocation of children into four different groups is most likely impossible. For example, consider the case of policy reforms that exogeneously change educational attainment of parents used in many recent studies to estimate causal effect of parental education (for example, Black et al. (2005)). If we use such an exogeneous shift as an instrument, it will give us reliable estimate of the effects of parental education alone. But it cannot identify the effects separately for two groups differentiated by farm or nonfarm occupation within a given education level which is a central focus of the paper. In the absence of any conventional sources of identification, we implement three alternative approaches to understand whether the estimated effects could be driven solely by genetic correlations across generations.

**Rosenbaum Bounds: Sensitivity Analysis**

The standard matching estimates assume that $\rho = Corr\,(\nu_i, \vartheta_i) = 0$. We estimate Rosenbaum bounds under the assumption that the estimates from matching estimators are biased upward due to a positive correlation in genetic endowments, i.e., $\rho > 0$. Note that Rosenbaum bounds are particularly useful in this context because the sign of $\rho$ is known with a measure of confidence from independent evidence. For example, the correlation between IQ of parents and children is close to 0.50 (Plomin et al (2008)). It is, however, important to appreciate that we are concerned with only a subset of genetic transmissions which not only affect the probability that the parents belong to a certain socioeconomic group, but also have substantial influence on the probability that a child attains higher schooling. The Rosenbaum bounds provide estimates of the effects of the alternative socio-economic status under different assumptions regarding how the genetic endowments affect the odds ratio of treatment, denoted as $\Gamma$. The baseline is $\Gamma = 1$ which implies that unobservable genetic factors have no influence on the selection. Since $\rho > 0$, we focus only on the upper bound estimates, i.e, under the assumption that the estimates with $\Gamma = 1$ are biased upward. With $\Gamma = 1.2$, for example, the estimation is done under the assumption that genetic endowment alone increases the odds for selection into treatment by 20 percent.

**Minimum Biased IPW Estimator (MB-NIPW)**

We implement the minimum biased NIPW estimator because it relaxes the CIA assumption of the NIPW estimator. While the NIPW reduces biases in the estimates compared to the OLS by using appropriate weighting based on propensity score of treatment, the MB-NIPW estimator is especially useful, because it minimizes the biases arising from selection on unobservables such as ability and preference. The bias is minimized by trimming the sample to a radius around the bias minimizing propensity score, which is equal to 0.50 as established by Black and Smith (2004).[23] Millimet and Tchernis (2013) provide evidence that the MB-NIPW estimator is able to correct for the biases due to selection on unobservables and yield more reliable estimates of the causal effects. According to the Monte Carlo evidence reported by Millimet and Tchernis (2013), the MB-NIPW estimator performs particularly well when the estimating equation suffers from omitted variables bias as is the case in our application. It is, however, important to appreciate that the advantage of MB-NIPW estimator in terms of causal interpretation comes with a price: the estimates are relevant for only a subset of the population defined by an interval around the bias minimizing propensity score of 0.50. In other words, the estimates provide local average treatment effect, similar to instrumental variables and regression discontinuity designs (for an extended discussion see Millimet and Tchernis (2013)). Thus the MB-NIPW estimates may be different from the other estimates in this paper, simply because they provide estimates for a sub population. This also brings focus on the trade-off between external and internal validity across different estimates which is being increasingly appreciated in the recent literature.[24]

**Heteroskedasticity Based Identification: Lewbel Approach**

The challenge of finding credible exclusion restrictions required for a standard instrumental variables approach have long been appreciated in the literature on the effects of family background on children's academic and labor market outcomes. There is a now a substantial econometric literature that shows that when no exclusion restriction is available, one can exploit heteroskedasticity for identification. As noted by Rigobon (2003), heteroskedasticity in the selection equation can be thought of as exogeneous probabilistic shifter similar to the standard instruments that helps

[23] At a propensity score of 0.50, a household is indifferent between the treatment statuses in terms of the observables, and the role of unobservables can be thought of like flipping a fair coin that determines the actual status observed in the data.

[24] For an excellent discussion on the trade-offs between internal and external validity in different econometric approaches, please see Dehejia (2015).

to trace out the causal relation in the outcome equation.

We implement an approach developed by Lewbel (2012). Recent applications of Lewbel's heteroskedasticity based identification include Umberger et. al. (2015), Mallick (2012), Emran and Hou (2013), Emran and Shilpi (2011). Lewbel (2012) shows that when there is significant heteroskedasticity in equation (7) above, one can generate instruments by exploiting restrictions on the second moment, even though there are no external instruments available. The Lewbel two stage approach relies on two conditions. In the context of the model in equations (6) and (7), they imply the following: (i) $\nu_i$ is heteroskedastic in equation (7), and (ii) there is a vector $Z \subseteq X$ that satisfies $Cov\left(Z, \nu_i \vartheta_i\right) = 0$. As Lewbel (2012) shows that the second covariance restriction is satisfied in many models, most importantly for us when the correlation between $\nu_i$ and $\vartheta_i$ is driven by a common third factor which is the common genetic endowment in our application. Following Lewbel, we test the first condition (i.e., existence of heteroskedasticity in (7)) by using Breusch-Pagan test of scale heteroskedasticity.

The construction of the heteroskedasticity based instruments involves two steps. First, the selection equation is estimated and the residuals $\hat{\nu_i}$ are retrieved. Second, the set of instruments are constructed as $\left(Z_i - \bar{Z_i}\right)\hat{\nu_i}$. Lewbel (2012) notes that a subset of or the full vector of $X$ can be used as $Z$. One worry about the approach is that when heteroskedasticity is not strong enough, the generated instruments will be weak and the IV estimates would be of little value. We use indicators of cohort (fathers age and its square) and of geographic location (province fixed effects) as $Z$ to increase the amount of heteroskedasticity and thus avoid weak identification. The choice of the subset is determined by an inspection of the determinants of heteroskedasticity in the selection equation.

**(5) Data**

We use data from the 2002 round of Chinese Household Income Project (CHIP 2002) for our analysis. CHIP survey was collected by Chinese Academy of Social Science and a group of international researchers.The CHIP 2002 data have some important advantages for understanding the role played by family background in educational inequality in rural China. First, unlike the standard household surveys in many developing countries, we have the relevant data on education and occupation on three generations: grandparents-parents-children. This allows us to understand whether the "children's generation" who grew up mostly during the reform period face significantly different educational opportunities compared to their parents who mostly grew up before the reform. Second, for the parents generation (i.e., the grandparents-parents sample), the CHIP 2002 survey includes all of the grandparents irrespective of their residency. Thus the grandparents-parents sample does not suffer from any coresidency bias.

The sample on parents-children from the rural survey is also much richer compared to standard household surveys in many developing countries; because, in addition to the coresident children, the rural survey includes information on a significant proportion of the non-resident children. All of the non-resident children who had not been away for more than six months were included as household members. Among those who had been away for more than six months, the survey counted a child as part of the household if he/she had significant economic connection with the household.[25]

The sampling procedure for the rural survey consists of two steps: first, sample villages are selected in each province, and then approximately 10 households are drawn from each village. The 2002 CHIP rural sample include 9200 households in 22 provinces .[26]

Another important advantage of 2002 CHIP survey in this regard is that it also includes a migrant household survey of 2000 households that covers people residing in the urban areas with rural *Hukou*. This allows us to add a random sample of the individuals who may be missing from the rural survey because of migration. The final sample of 'parents-children' analysis thus ensures that potential biases from coresidency is much lower compared to a standard household survey in developing country such as living standard measurement surveys of the World Bank (for more details, see below).[27]

**The Parents-Children sample**

Our parents-children sample is composed of two parts. The first subsample is extracted from the CHIP 2002 rural survey. Adult children in this paper are defined as individuals 18 years of age or older. We have a total of 5909 rural adult children-parents pairs, with adult children's age

[25] For an excellent discussion on the 2002 CHIP survey, please see Shi et. al. (2013).

[26] The 22 provinces are Beijing, Hebei, Shanxi, Liaoning, Jilin, Jiangsu, Zhejiang, Anhui, Jiangxi, Shandong, Henan, Hubei, Hunan, Guangdong, Guangxi, Chongqing, Sichuan, Guizhou, Yunnan, Shaanxi, Gansu, and Xinjiang.

[27] We are grateful to Li Shi for many clarifications on the CHIP 2010 survey used in this paper.

and education, parents' age, education and occupation identified.

The second parents-children subsample comes from the rural-urban migrant survey which captures the long-term migrants who have left their rural home and live in urban areas at the time of the survey. For the migrant survey, we are interested in household heads/spouses and their parents. We focus on the household heads/spouses 18 years of age or older. To capture the missing long-term migrants from the rural survey, we set four criteria for our migrant subsample. First, the household head/spouse must have an identified rural *Hukou*. Second, they have stayed in the urban area for at least one year or longer by the time of the survey, which helps to identify the long-term rural migrants possibly missed in the rural survey. Third, they still have family member(s) living in the rural area. This ensures that the the families that are now fully urban are not included. Fourth, they did not have a strong economic tie with their original home back in the village. This is to avoid double counting (by both rural survey and migrant survey) and to prevent over-sampling of the migrants. We use remittances sent back to the village as a measure of the strength of their relationship with the household of origin. We examine the distribution of their income and the remittances. The 80th percentile of the remittance rate is 20.8 percent of income. We then set the cutoff for the remittance rate at 20.8 percent and limit the migrants to those who had remitted less than 20.8 percent of their urban income back to rural home. The low remittance rate represents a weak economic tie and it is most likely that these rural migrants are not be captured in the original rural survey. Using the four criteria, we identified 1355 adult household heads/spouses and their parents along with the necessary information on household heads/spouses' age, education,and their parents' age, education and occupation. By reclassifying the migrants according to the province of their agricultural *Hukou*, and adding them to the rural survey sample, we have a total of 7264 valid children-parents pairs (5909 rural pairs plus 1355 migrant pairs). In our sample, most of the children (84 percent of daughters and 83 percent of sons) went to school after the reform was implemented in 1978. We thus can call them the 'children of reform'.

**Parents-grandparents**

In 2002 CHIP rural data, there is a specific module providing the information about the parents of household heads and spouses. Furthermore, this is the module for the complete parental information of the household heads and spouses, including the grandparents co-residing with the family, grandparents not co-residing, and also the grandparents who had passed away. We have identified 14777 parents-grandparents pair with the required information on parents' age and education, grandparents' age, education and grandparents. In our sample, most of the parents (95 percent of fathers and 92 percent of mothers) completed their schooling before the economic reform in 1978. Also, 59 percent of fathers' education and 64 percent of mothers' education were affected by cultural revolution.

**(6) Empirical Results**

Table 1A presents the summary statistics for a set of economic and demographic characteristics of six different groups of households for the parents-children sample. Table 1B reports the corresponding estimates for four different groups for the grandparents-parents sample. The evidence in Table 1 shows that the households in farm and low education (less than or equal to primary) group $\left(D_i^{00}\right)$ are characterized by unfavorable socio-economic characteristics including lowest education levels in both the parents' and children's generations, and lowest per capita income (income data are only for the parents-children sample). The non-farm and high education group $\left(D_i^{11}\right)$ occupies the other extreme, both parents and children have highest levels of educational attainment and also per capita household income is highest.

**(6.1) Parents Economic Status and Children's Schooling: Heterogeneous Effects**

In this section, we use a dummy for more than primary schooling for parental education for both generations to ensure comparability. However, we also provide a set of estimates later for the parents-children sample using the middle school as the cut-off for the education dummy for parents, because about 60 percent of households have at least one parent with more than primary schooling.

**Family Background and Schooling in Children's Generation**

The estimates for the parents-children sample from alternative estimators (OLS, A-I Matching, BC-RM, NIPW) are reported in Table 2, separately for daughters (panel A) and sons (panel B).

Following Solon (1992), all of the regressions include age and age squared of father, mother, and the child.[28] The first column reports the estimate of the effects of parents' education dummy alone, as is standard in most of the recent economics literature, i.e., it provides an estimate of the parameter $\beta_1$ in equation (1) above. This is a useful benchmark to assess the results from the $2 \times 2$ education and occupation research design. The last three columns in Table 2 report the estimates for the three different "treatment" groups: column 2 for the households with low education and non-farm parents (parameter $\theta_1$), column 3 for the high education and farming households (parameter $\theta_2$), and column 4 for the high educated and non-farm households (parameter $\theta_3$).

The OLS estimates in column 1 of Table 2 show that the average effects of parents' education on sons and daughters samples are similar in magnitude: having at least one parent with more than primary schooling increases schooling attainment by approximately one year for a child. These estimates, however, do not account for differences in parents' occupations, and can be interpreted as average effects in a model where the effect of parental education is heterogeneous across farm and non-farm occupations. A look at the last three columns in Table (2) shows that all of the coefficients across groups and estimators are statistically significant at the 10 percent level or lower, and also numerically substantial. This provides a clear answer to the question posed in the title of the paper: the children born into the low educated farmer parents (the comparison group) are in fact saddled with the lowest educational attainment among the four different groups.

The evidence also suggests that the heterogeneity across the other three groups is substantial. Having a parent with more than primary schooling provides an advantage of about 1.5 years of more schooling for a daughter, and about 1.7 years for a son, when at least one parents' primary occupation is non-farm. But the corresponding effects of better parental education among the farming households are much smaller: about 1 years of additional schooling irrespective of gender. Within the low educated subsample, having a non-farm parent increases schooling by about 0.60 year for daughters and by about 0.80 year for sons. These estimates bring into focus the importance of non-farm occupations in educational inequality which is masked by the average estimate in column (1). A comparison of the estimates across gender reveals interesting pattern: once at least one of the parents have more than primary schooling, the effects of family background

[28] As discussed in the data section above, 96 percent of household heads are male. We divide the sample into fathers and mothers by including the spouse of female headed households as fathers.

do not show any appreciable gender bias (compare the estimates across sons and daughters in last two columns in Table 2). The flip side of this is that gender bias seems to persist even with higher income when we focus on the subset of primary or lower schooled parents. The results thus suggest that gender bias in educational attainment may be primarily driven by parents' lack of education in so far as children who grew up during the reform period are concerned.[29]

The ranking between the two intermediate groups (i.e., educated farmer parents vs. uneducated non-farmer parents) is also instructive. Under the null hypothesis that parents' income is the only constraint on children's schooling, we should expect the estimate for the children in low educated nonfarm households to be significantly higher, because they have higher per capita income than the educated farmer households. The evidence, however, is exactly opposite; the point estimates for children born into more educated farmer parents are substantially larger.[30]

**Parents as Children: Estimates from the Grandparents-Parents Sample**

Table 3 reports the estimates of the effects of grandparent's occupational and educational status on the schooling of parents (household head and spouse). We call the children in this generation 'parents' (mothers and fathers) and the parents are called 'grand-parents'. This helps keep track of three generations when we compare the effects of family background over time. Similar to Table 2, the education dummy is defined with primary schooling as the threshold.

The OLS estimates of $\beta_1$ in equation 1 reported in column (1) of Table 3 show that the average impact of grandparents education depends on the gender: it is higher for mothers (1.03) compared to that for fathers (0.76). This is in contrast to the apparent gender neutrality in the average effects in children's generation reported in column (1) of Table 2. All of the estimated effects for mothers are positive, numerically substantial and statistically significant at the 1 percent level, implying that, the girls (mothers) from the households with low educated farmer parents (i.e., grandparents) had faced the most disadvantages in schooling among the four different groups. Among the other groups, the mothers who were born into households with both higher education and non-farm occupation have the highest schooling attainment; compared to those born into low educated farming households, they attain about 1.50 years of more schooling. This is a 50

[29] This, however, is not true in grandparents-parents sample. Please see below.

[30] Compared to the estimates for daughters, the difference between the point estimates between these two groups for sons is smaller, implying that income may have played a more important role for the sons.

percent larger effect compared to the average effect of grandparents education in mothers sample reported in column 1 (1.03). Interestingly, the advantage for a girl (mother) of being born into a business (non-farm) family with less than primary education is much smaller than the advantage derived from a household with better educated farmer grandparents. This strengthens the finding from the children's generation above about the importance of educated parents and rejects the null that only income matters for educational attainment.

The estimated effects of grandparents' economic status are smaller in magnitude across the board for the fathers' sample when compared to the mothers' estimates. Thus the boys from poor economic background faced relatively less constraints on educational mobility in the parents' generation. The pattern of estimates across different groups for fathers is also different. First, the advantage from being born into low educated grandparents involved in business is much smaller compared to the corresponding estimates for mothers (again, the low educated farmers' are the comparison group). Second, the difference between the estimates in last two columns is small, implying that birth into parents with better education and non-farm occupation did not confer any significant advantages for a son over the sons of farmer parents with better education. This is in contrast to the clear advantage for the non-farm higher educated group in the case of mothers, and also sons and daughters in children's generation, as discussed above.

### (6.2) Do the Children of Reform Have More Educational Opportunities than Their Parents?

As noted earlier, most of the parents completed their schooling before the reform began in 1978, and most of the children completed their schooling during the reform period. In our grandparents-parents sample, 95 percent of fathers and 92 percent of mothers completed education before 1978. In contrast, 84 percent of daughters went to school after 1978, and the corresponding estimate for sons is 83 percent. A comparison between the grandparent-parents and parents-children samples can thus provide us with useful evidence on the effects of market-oriented reform on children's educational opportunities in rural China.

The first striking observation that comes across from a comparison of Tables 2 and 3 is that, for girls, the estimated effects of family background remained broadly similar across the two generations. The point estimates (OLS) of the average effects of parent's education on girls are very close to each other between mothers (1.03) and daughters (1.10) generations. For girls, the stubborn persistence is observed not only in the average estimates, the estimates across three groups in Tables 2 and 3 are also similar in magnitude across two generations (grandparents-mothers and parents-daughters), and this is true for all four estimators. The changes in economic and educational policies following the economic liberalization in 1978 seem to have made little difference to the roles played by family background in girls' education.

In contrast, the estimates for sons show an increase in the importance of family background across generations; according to the estimates in column (1), the average effects of parental education increased from 0.76 for fathers to 1.02 for sons, a substantial increase in magnitude (about 30 percent). The estimates for three groups in columns (2)-(4), however, suggest a more nuanced interpretation; while each group has experienced an increase in the impact of family background in the sons generation compared to their fathers, the magnitude of increase is the least for the children of better educated farmers. The sons born into the parents in non-farm occupation have gained in schooling attainment irrespective of the parental education level which suggests that higher income has played an increasingly important role in their educational attainment (recall that the low educated non-farm households have higher income than the better educated farmer households). This is consistent with the evidence that non-farm occupations have contributed significantly to the increase in rural inequality in 1990s (Benjamin et. al. (2005)).

One might worry that our results can be influenced by the fact that some non-resident children may be missing from the parents-children sample, while the grandparents-parents sample do not suffer from such problem. In the data section, we explain in details the advantages of the CHIP survey and the steps we took to minimize the problem of missing non-resident children from parents-children sample. The recent analysis of the biases arising from coresidency restriction in the estimates of intergenerational persistence is useful in understanding the implications for our conclusions (see Emran, Greene and Shilpi (2015)). The evidence reported by Emran, Greene and Shilpi (2015) shows that when some children are missing from the sample due to coresidency criteria used to define the household in a survey, the estimated intergenerational regression coefficients are biased *downward*. The downward bias is due to truncation of the sample by the imposition of coresidency criteria. Under the assumption that our parents-children sample is missing some nonresident children, the estimates are biased downward, while the estimates for grand parents-parents are unbiased. This implies that we should treat the estimates for parents-children sample (post reform generation) as lower bounds. This strengthens the conclusion that educational mobility has worsened for sons who grew up in post-reform period, and for daughters, it remained stagnant at the least, but might have worsened as well.

The increasing importance of parents' economic status in sons educational attainment probably reflects the consequences of a host of factors during the reform era. First, the evidence indicates that the returns to education is higher and increasing over the reform era, especially for the boys (de Baruw and Rozelle (2008), Fang et. al. (2012)). According to the theory of intergenerational economic mobility (see Solon (2004)), this is expected to strengthen the effects of family background on son's educational attainment as richer parents invest more in the education of their sons to take advantage of the rising returns. This investment in sons education may be reinforced by son preference and the reliance on a son for old age support which became more important with the market reform that dismantled the socialist safety net. The urban migration opportunities opened up by the relaxation of *Hukou* restrictions from mid 1980s can adversely affect the schooling of poor children in rural areas. The role played by parental resources has increased in rural schools due to fiscal decentralization which started in mid 1980s and culminated in a comprehensive reform in 1994 (Hannum and Park (2002)). Fiscal decentralization compelled the schools in poorer counties to impose a varieties of fees on the households. Brown and Park (2002) report that the children were not allowed to attend the school if their parents had not paid the fees (see also the discussion in Hannum and Park (2007)). The importance of parental resources also explains the evidence that the sons of nonfarm parents have experienced significant positive impact on their schooling, because nonfarm households enjoy significantly higher income, irrespective of parental education.

The evidence that the effects of family background on girls did not change in any significant way from mothers' to daughters' generations may seem puzzling. The persistence implies that the strength and pattern of the effects of parents' educational and occupational status are driven primarily by factors that do not change easily over time even in the face of dramatic changes in economic policies, significant income growth, and impressive poverty reduction. The literature on girls educational attainment in China points to a possible resolution of this apparent puzzle. The literature emphasizes that whether a girl progresses through the school depends largely on her academic aptitude and grades, and may not be very sensitive to family economic conditions (Zhang, Kao, and Hannum (2007)). Since the distribution of academic abilities is persistent across generations, this may provide a partial explanation for the persistence across generation for girls found in Tables 2 and 3. Other factors that might have also played a role include (i) lower returns to education for girls, about 12 percent lower according to the estimates of de Brauw and Rozelle (2008), and (ii) low elasticity of parents' demand for girls schooling with respect to labor market returns, because the girls leave their parental home after marriage.

We also underscore an important implication of the results in Tables 2 and 3. The structural change in the rural economy from agriculture to non-farm activities has helped narrow the gender gap in education in parental generation, because it had a significant positive effect on mothers education, while the impact on the fathers was weak. The results on children's generation show that the pattern has reversed: the effects of family background on sons have increased over the reform period to catch up with the effects observed for the girls, resulting in gender parity in the effects of family background, both in the mean effect and also across different groups.

**(7) Causal Analysis: Can the Effects be Due to Genetic Correlation Alone?**

The evidence presented in Tables 2-3 provide interesting evidence of heterogeneous effects of family background on children's education across generations which are robust to a number of alternative estimators. The estimates, however, capture the effects of genetic transmissions from parents to children along with the effects of parents economic status. The estimates are useful in understanding cross-sectional inequality in observed economic status and are often the focus of media and policy makers. However, it is important to understand whether the estimated effects can be due largely to the genetic transmissions from parents to children which are subsumed in the error terms in the equations (6) and (7) above. In this section, we provide evidence from alternative approaches that suggests that the observed effects of economic status can be accounted for by genetic transmissions alone in the case of low educated nonfarm group, but for the other two groups, the evidence points to significant causal effects. The discussion below focuses on the results for the case when parent's (and grandparent's) schooling cut-off is primary. We report in an online appendix the estimates using more than junior middle (9 years) as the cut-off (only for parents-children sample), and do not discuss the results in the text for the sake of parsimony.

**Evidence from Sensitivity Analysis**

Table 4 reports the estimated effects of parents economic status under the assumption that the estimates in Tables 2-3 are biased upward due to genetic correlations. Following Aakvik (2010), we carried out the sensitivity analysis for a range of odds ratios, from $\Gamma = 1$ (no selection on unobservables) to $\Gamma = 2$ (genetic endowment alone results in a 100 percent increase in the probability that a household switches to the treatment group; the treatments are the three groups in Tables 2-3). So a $\Gamma = 1.10$ , for example, implies that the estimation is done under the assumption that genetic endowments alone increase by 10 percent the probability that a household falls into a certain socioeconomic group (the comparison group is low educated farmer parents). Although we used 10 equally spaced values of $\Gamma$, starting at 1.10, for the sake of brevity, we report estimates for five values of the odds ratio $\Gamma = 1.00, 1.30, 1.50, 1.80, 2.00$. The discussion below, however, draws on the full set of results (available from the authors).

The estimates for parents-children sample are reported in Table 4A and those for the grandparents-parents in Table 4B. We use 10 percent significance level if not noted otherwise. According to the evidence in Table 4A, the estimated effect of family background remains numerically substantial and statistically significant for the children of educated nonfarmers even when we allow for very high level of selection on genetic endowments, both for A-I Matching and BC-RM estimators ($\Gamma = 2$, implying a 100 percent increase in the odds ratio). The evidence is thus very strong that the estimated effects for this group represent significant causal impact of parental socio-economic status. The estimates for the children of educated farmers are equally strong according to the A-I Matching (remains significant at $\Gamma = 2$ for both daughters and sons), but somewhat weaker according to BC-RM (significant at $\Gamma = 1.60$ for daughters and at $\Gamma = 1.50$ for sons). In contrast, the estimated positive effects for the children of nonfarm but low educated parents depend on the gender; while for daughters the estimate can be driven entirely by low to moderate genetic influence (becomes insignificant at $\Gamma = 1.4$ (A-I Matching) and at $\Gamma = 1.10$ (BC-RM )), the effect is more robust for sons (insignificant at $\Gamma = 1.7$ (A-I Matching) and at $\Gamma = 1.4$ (BC-RM)). The sensitivity analysis leads to four important conclusions. (i) The estimated effects of better educated farmer and nonfarmer groups are unlikely to be driven solely by selection on ability, but the estimates for the low educated nonfarmer group may well be due to genetic transmissions, and may not represent any causal effect, especially for the daughters. (ii) The effects of parent's non-farm occupation is stronger on the sons. (iii) Compared to low educated nonfarmer group, the positive effects of being born into high educated farmer parents are stronger, and are unlikely to be driven by genetic correlations alone. This provides strong support to the proposition that the role played by money and resources may be secondary to that played by parents' education, because the low educated non-farmers have higher income.

In parents' generation, the results from the sensitivity analysis for mothers are very similar to those for the daughters discussed above (see Table 4B). This reinforces the conclusion that there has not been any significant change for women across generations. In sharp contrast to sons, for fathers, the sensitivity analysis shows that except for the group composed of educated farmers, the estimated effect can be accounted for by low to moderate selection on ability and preference. Thus, for boys, having nonfarmer parents did not constitute an advantage in parental generation.

**Estimates from Minimum-Biased NIPW (MB-NIPW) Estimator**

The estimates from MB-NIPW are reported in Table 5.[31] The estimated effects are interesting, and in some cases different from the estimates in Tables 2 and 3. In children-parents sample, the magnitudes of the estimates are broadly similar to the earlier estimates for daughters, but the magnitudes are larger for sons, especially for the low educated nonfarm households.

The MB-NIPW estimates for grandparents-parents sample show two things. First, the estimated effect of low educated nonfarm status for fathers is very low and not significant at the 10 percent level, implying that, for fathers, non-farm occupation of grandparents did not matter for educational attainment. This is in sharp contrast of a numerically larger effect found for the sons in this group. Interestingly, the irrelevance of occupational status is also observed for the educated subsample in parents' generation: the estimates are very similar for the educated farmer and educated non-farmer groups. The non-farm occupation, however, played a relatively larger role in mothers' education.

[31]We reemphasize here that these estimates are not comparable to the estimates in Tables 2-3 and the results of the sensitivity analysis. The sensitivity analysis pertains to the estimates for the full sample and refers to the average effects (ATE). The MB-NIPW estimates, in contrast, provide LATE; effects on the households that fall in the neighborhood of bias minimizing propensity score (0.50), but the advantage is that we have a point estimate.

**Estimates from Lewbel's Heteroskedasticity Based Identification**

The estimates from the Lewbel approach are reported in Table 5.[32] The evidence is broadly consistent with the main conclusions reached above on the basis of sensitivity analysis and the MB-NIPW estimates, but there are also some differences. The effects of family background have increased substantially over the reform period for the men, as found above from a host of other approaches. The larger effect of high educated farmer parents compared to the low educated nonfarmers remain valid for the women in both generations, but does not hold for the sons. The estimates also imply that the effects of family background on women may have increased somewhat for the reform generation, in contrast to the finding from other econometric approaches that they remain stubbornly similar across generations. We, however, note that since these are IV estimates they are not relevant for the whole population, and part of the difference in the results may reflect that fact that they are local average treatment effect, rather than average treatment effect estimates in Tables 2-3.

**(8) Parent's Education and Non-Farm Occupation: Substitutes, Complements or Separable?**

There are some plausible arguments in favor of the proposition that positive feedbacks between education and occupation may be important, especially in the post-reform period; education helps in finding better quality non-farm jobs, and the expected returns to education for children may be higher as nonfarm jobs depend on network. Given higher income, the ability to finance education may also be higher for parents involved in non-farm activities.[33] Another important factor is that educated parents may be more effective in providing homework help and a home environment conducive to learning. However, it is also possible that resources are primarily substitutes for parental education in producing children's schooling. For example, higher income may enable the parents to buy the required educational inputs (such as better schools, tutors etc.). If the market

[32] Note that we do not report any Hansen's J test even though the models are overidentified. It is unlikely that the effect of parents' socio-economic status is constant within a group defined by parents' education and occupation. The effects may vary, for example, by ethnicity (Hahn or minority), by political connections (party affiliation). With heterogenous effects, it is not meaningful to use overidentification test for validity of the instruments (Angrist and Pischke (2009)).

[33] In our rural survey data for parents-children sample, among the low educated nonfarmers, 29 percent have high skilled nonfarm jobs, while among the parents with more than primary schooling 43 percent are engaged in skilled nonfarm economic activities.

for educational inputs is well developed, low educated non-farming parents may be able to offset some of the disadvantages of their own educational deficit (inability to help with homework, for example). Also, the relation between a parents education and the time devoted to children may not be monotonic. A highly educated parent may not have time to spend with his kids, especially given the higher price of labor in the market. This may create a negative correlation between direct educational inputs provided by a parent at home and his labor market opportunities, especially at the right tail of schooling distribution. Table 6 reports evidence on the existence and nature of interaction between parents education and non-farm occupation using alternative econometric approaches.

Column (1) in Table 6 reports OLS estimates of the parameter $\beta_3$ in equation (3) which shows the effect of the interaction between two dummies: more than primary schooling and non-farm occupation. The evidence across gender and generations in general suggests the lack of a significant interaction effect; out of the four cases, only in the case of fathers the effect is statistically significant at the 10 percent level. More important, the sign is negative in this case, implying substitutability, rather than complementarity. One might however have reservations about this simple test, as the conclusions may be affected by the linearity in parameters assumption.

We present a more robust test of possible complementarity by using the the methodology described earlier in section 3 above. Columns (2) and (3) in Table 6 provide estimates of the marginal effect of better education in two subsamples: parents in farming and in non-farm occupation respectively. The last column reports the test of the null hypothesis that the marginal effects in columns (2) and (3) are equal, i.e, the null that there is no interaction effect. The evidence clearly shows that there is no evidence of complementarity between parental education and occupation in so far as children's schooling attainment is concerned. This conclusion is valid for both the parents-children and grandparents-parents samples. In fact, there is some evidence that in children's generation, especially for boys, non-farm occupation has become a substitute for parental education. This is consistent with the finding discussed above that the role played by higher income in the non-farm sector has assumed greater importance for the sons during the reform era.

We also check if there is complementarity between non-farm occupation and higher education (more than 9 years schooling). We estimate the effects of having more than 9 years of schooling (complete middle school) compared to the parents with less than or equal to primary schooling as the comparison. The results again show that there is little or no evidence of complementarity. The estimates are omitted for the sake of brevity (see the online appendix).

Note that if the estimates are biased upward due to genetic transmissions, the conclusion about lack of complementarity is strengthened. The estimates for the marginal effect of education in non-farm occupation should be biased upward more, if genetically transmitted ability and preference affect positively the selection into both education and nonfarm occupation.

### (9) Conclusions

This paper provides evidence on the effects of parental economic status on children's educational attainment in rural China using a rich data set that covers three generations spanning pre and post reform periods. In the absence of long panel data on income, we develop a simple yet versatile research design to relax the single factor characterization of parental economic status standard in the economics literature on intergenerational educational mobility. We use two most salient markers of socio-economic status: parents' education and occupation to understand the effects of family background on children's schooling. We take advantage of a rich menu of econometric approaches recently developed for analyzing non-experimental data and provide evidence on (i) heterogeneous effects of family background, (ii) potential complementarity between parental education and non-farm occupation in promoting children's schooling, (iii) gender differences in educational mobility, (iv) the evolution of intergenerational educational mobility across the pre and post reform generations.

The evidence shows that the current focus on the mean effect (intergenerational regression coefficient) misses substantial heterogeneity important for understanding educational inequality. The effects of family background vary significantly with parents' occupation within a given educational group, and across parents' education within a given occupation group. Our evidence indicates that once at least one parent has more than primary schooling, there is a gender convergence in the effects of family background in the children's generation most of whom went to school during the reform era. In contrast, among the low educated families, the parents' nonfarm occupation has a much larger positive effect on sons schooling, which is expected to widen the gender gap. The evidence thus indicates that gender bias may not be primarily a poverty problem. While having a non-farmer parent, in general, improves educational attainment, it does not confer any advantages over the more educated farmer parents, even though nonfarm households have significantly higher income. Evidence from three econometric approaches shows that the effects of having better educated parents, especially with nonfarm occupations, cannot be explained solely by genetic correlations across generations; the results suggest a causal effect on children's schooling.

Estimates of cross-partial effects without imposing arbitrary functional forms show that there is little evidence of complementarity between parental education and non-farm occupation in children's educational mobility which contradicts widely held perceptions. In fact, there is some evidence that nonfarm occupation may be a substitute for parents' education for sons educational attainment during the reform period. A comparison of parents and children's generations shows that for girls, the role of family background in schooling attainment remains largely unchanged across generations, but for boys, family background has become more important over the reform period.

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**Table 1A: Descriptive Statistics on Parents-Children Sample**

| Sample | Full sample | | Means of Variables for Different Groups | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | Primary school as the cut-off | | | | Junior middle as the cut-off | |
| | Mean | Standard deviation | Farm low edu | Off-farm low edu | Farm high edu | Off-farm high edu | Farm high edu | Off-farm high edu |
| ***Daughters*** | | | | | | | | |
| Daughters' age | 24.3 | 7.1 | 28.5 | 24.0 | 22.4 | 21.2 | 22.1 | 20.7 |
| Daughters' years of schooling | 8.6 | 2.6 | 7.4 | 8.5 | 9.0 | 9.6 | 9.2 | 9.9 |
| Dummy = 1 if daughter more than primary schooling | 0.8 | 0.4 | 0.7 | 0.8 | 0.9 | 0.9 | 0.9 | 1.0 |
| ***Sons*** | | | | | | | | |
| Sons' age | 25.6 | 7.2 | 29.0 | 26.1 | 24.0 | 22.5 | 23.8 | 21.7 |
| Sons' years of schooling | 8.9 | 2.4 | 7.9 | 8.9 | 9.1 | 9.8 | 9.4 | 10.2 |
| Dummy = 1 if son more than primary schooling | 0.9 | 0.3 | 0.8 | 0.9 | 0.9 | 1.0 | 0.9 | 1.0 |
| ***Father*** | | | | | | | | |
| Father's age | 53.2 | 8.4 | 58.4 | 53.9 | 50.8 | 49.0 | 50.1 | 48.0 |
| Father's years of schooling | 6.4 | 3.0 | 3.6 | 4.4 | 8.1 | 8.6 | 9.5 | 9.8 |
| Dummy = 1 if father more than primary schooling | 0.5 | 0.5 | 0 | 0 | 0.9 | 1.0 | 1.0 | 1.0 |
| Dummy = 1 if father more than middle schooling | 0.2 | 0.4 | 0 | 0 | 0.2 | 0.4 | 0.8 | 0.8 |
| Dummy = 1 if father in off-farming | 0.3 | 0.5 | 0 | 0.9 | 0 | 0.9 | 0.0 | 0.9 |
| ***Mother*** | | | | | | | | |
| Mother's age | 51.0 | 8.4 | 55.8 | 51.2 | 48.8 | 47.2 | 48.1 | 46.2 |
| Mother's years of schooling | 4.5 | 3.1 | 2.4 | 3.2 | 5.5 | 6.3 | 6.4 | 7.1 |
| Dummy = 1 if mother more than primary schooling | 0.3 | 0.4 | 0 | 0 | 0.4 | 0.5 | 0.6 | 0.6 |
| Dummy = 1 if mother more than middle schooling | 0.1 | 0.3 | 0 | 0 | 0.1 | 0.2 | 0.3 | 0.4 |
| Dummy = 1 if mother in off-farming | 0.1 | 0.3 | 0 | 0.3 | 0 | 0.3 | 0.0 | 0.3 |
| ***Household per capita** income (yuan)* | | | | | | | | |
| income in 2002 | 2725 | 2239 | 2189 | 3159 | 2449 | 3445 | 2551 | 3692 |
| 3-year average income (2000-2002) | 2561 | 1857 | 2111 | 2947 | 2318 | 3170 | 2411 | 3369 |
| 5-year average income (1998-2002) | 2376 | 1680 | 1972 | 2730 | 2155 | 2918 | 2236 | 3102 |

***Notes***. 1) Observations: 7264 for full sample: 2916 for daughters and 4348 for sons; 2) Observations for the four groups in the primary school cutoff: farm and low edu (2494), off-farm and low edu (621), farm and high edu (2403), off-farm and high edu (1746); 3) Observations for the junior middle school cut-off: farm and high edu (636 ), off-farm and high edu (735 ); 4) For primary school cut-off, "High edu" stands for more than primary schooling and "low edu" stands for primary schooling or less; 5) For junior middle school cut-off, "High edu" stands for more than junior middle schooling

**Table 1B: Descriptive Statistics on Parents-Grandparents Sample**

| Sample | Full sample: Mean | Full sample: Standard deviation | Mean Values of Variables, Primary school cut-off: Farm low edu | Off-farm low edu | Farm high edu | Off-farm high edu |
|---|---|---|---|---|---|---|
| ***Parents*** | | | | | | |
| Parents' age | 44.8 | 10.0 | 45.6 | 45.9 | 40.5 | 41.9 |
| Parents' years of schooling | 6.6 | 2.8 | 6.4 | 6.9 | 7.6 | 7.7 |
| Dummy = 1 if parents more than primary schooling | 0.6 | 0.5 | 0.5 | 0.6 | 0.7 | 0.8 |
| ***Grandfathers*** | | | | | | |
| Grandfather's age | 74.5 | 12.3 | 75.7 | 76.4 | 68.3 | 70.1 |
| Grandfather's years of schooling | 2.9 | 2.7 | 2.0 | 2.9 | 7.2 | 7.3 |
| Dummy = 1 if grandfather more than primary schooling | 0.2 | 0.4 | 0 | 0 | 1.0 | 0.9 |
| Dummy = 1 if grandfather in off-farming | 0.1 | 0.3 | 0 | 1.0 | 0 | 1.0 |
| ***Grandmothers*** | | | | | | |
| Grandmother's age | 71.8 | 11.8 | 72.9 | 73.4 | 66.2 | 68.1 |
| Grandmother's years of schooling | 1.2 | 1.9 | 0.8 | 1.1 | 3.2 | 3.0 |
| Dummy = 1 if grandmother more than primary schooling | 0.1 | 0.2 | 0 | 0 | 0.5 | 0.3 |
| Dummy = 1 if grandmother in off-farming | 0.0 | 0.1 | 0 | 0.2 | 0 | 0.2 |
| ***Grandparents in general*** | | | | | | |
| Grandparents' average years of schooling | 2.2 | 2.1 | 1.5 | 2.1 | 5.5 | 5.5 |
| Dummy = 1 if at least one grandparent more than primary schooling | 0.2 | 0.4 | 0 | 0 | 1 | 1 |
| Dummy = 1 if at least one grandparent in in off-farming | 0.1 | 0.3 | 0 | 1 | 0 | 1 |

***Notes***

1) Observations: 14777 for full sample;

2) Observations for the groups in the primary school cutoff: farm and low edu (11571), off-farm and low edu (690), farm and high edu (2204), off-farm and high edu (312);

3) For primary school cut-off, "High edu" stands for more than primary schooling and "low edu" stands for primary schooling or less;

.

## Table 2: Impact of Family Background on Children's Years of Schooling
### (primary schooling as the threshold for parental education)

| Estimates | | (1) Parents' education Full sample | (2) Parents education and Occupation: Non-farm, low edu | (3) Farm, high edu | (4) Non-farm, high edu | (5) Test of equality $\hat{\theta}_1-\hat{\theta}_2=0$ | (6) $\hat{\theta}_2-\hat{\theta}_3=0$ |
|---|---|---|---|---|---|---|---|
| | | $\hat{\alpha}_1$ | $\hat{\theta}_1$ | $\hat{\theta}_2$ | $\hat{\theta}_3$ | Z score | Z score |
| ***Daughters*** | | | | | | | |
| | OLS | **1.10**<br>10.78*** | **0.57**<br>3.32**** | **1.05**<br>8.26*** | **1.53**<br>11.07*** | -2.24* | -2.56** |
| **Matching Estimators** | A-I matching | | **0.72**<br>3.62*** | **1.04**<br>7.18*** | **1.31**<br>6.25*** | -1.30 | -1.06 |
| | BC-RM | | **0.66**<br>3.03*** | **1.00**<br>7.03*** | **1.40**<br>9.11*** | -1.31 | -1.91* |
| **Propensity score weighted estimators** | NIPW | | **0.59**<br>(0.31, 0.87) | **1.01**<br>(0.76, 1.22) | **1.58**<br>(1.33, 1.83) | -1.97* | -2.87** |
| ***Sons*** | | | | | | | |
| | OLS | **1.02**<br>12.72**** | **0.8**<br>5.42*** | **1**<br>10.74*** | **1.67**<br>15.42*** | -1.14 | -4.69*** |
| **Matching Estimators** | A-I matching | | **0.70**<br>4.25*** | **1.05**<br>10.31*** | **1.72**<br>12.38*** | -1.69 | -3.87*** |
| | BC-RM | | **0.62**<br>3.54*** | **0.91**<br>8.21*** | **1.74**<br>11.63*** | -1.39 | -4.45*** |
| **Propensity score weighted estimators** | NIPW | | **0.82**<br>(0.60, 1.09) | **1.04**<br>(0.86, 1.19) | **1.74**<br>(1.53, 1.92) | -1.17 | -4.91*** |

***Notes***

1) Dependent variable=children's years of schooling;
2) For the column (1), the interest variable=parents' education dummy, dummy =1 if at least one of parents more than primary schooling, 0=otherwise;
3) For all columns, the control variables are children's age, children's age squared, mother's age, mother's age squared, father's age, father's age squared;
4) For the columns (2) - (4), the interest variable is parents' education and occupation status dummy. The dummy=1 if treatment group, =0 otherwise; base group is (farm, low edu)
5) A-I matching=Abadie and Imbens (2002)
6) BC-RM = Biased corrected radius matching ;
7) NIPW = normalized inverse probability weighted propensity score estimator of Hirano and Imbens (2001);
8) The computation for NIPW uses STATA program written by Millimet and Tchernis (2013);
9) Estimates and t statistics are shown. For NIPW, 90% confidence interval is given in parentheses, which is boostrapped using 250 replications;
10) "high edu" stands for more than primary schooling, "low edu" stands for primary schooling or less;
11) ***, **, and * Denotes statistical significance at the 1%, 5%, and 10% level respectively.

**Table 3: Impact of Family Background on Parents' Years of Schooling (primary schooling as the threshold for Grandparent's education)**

| Estimates | | (1) Grandparents' education Full sample | (2) Grandparents education and Occupation: Non-farm, low edu | (3) Farm, high edu | (4) Non-farm, high edu | (5) Test of equality $\hat{\theta}_1-\hat{\theta}_2=0$ | (6) $\hat{\theta}_2-\hat{\theta}_3=0$ |
|---|---|---|---|---|---|---|---|
| | | $\hat{\alpha}_1$ | $\hat{\theta}_1$ | $\hat{\theta}_2$ | $\hat{\theta}_3$ | Z score | Z score |
| ***Mothers*** | | | | | | | |
| | OLS | **1.03** | **0.58** | **0.99** | **1.48** | -2.22* | -2.03* |
| | | 12.05*** | 3.66*** | 10.48*** | 6.69*** | | |
| **Matching estimator** | A-I matching | | **0.67** | **0.98** | **1.63** | -1.57 | -2.48** |
| | | | 4.07*** | 9.14*** | 6.85*** | | |
| | BC-RM | | **0.78** | **1.01** | **1.47** | -1.08 | -1.85* |
| | | | 4.33*** | 8.99*** | 6.67*** | | |
| **Propensity score weighted estimator** | NIPW | | **0.62** | **1.00** | **1.51** | -2.02* | -2.11** |
| | | | (0.39, 0.89) | (0.87, 1.15) | (1.05, 1.88) | | |
| ***Fathers*** | | | | | | | |
| | OLS | **0.76** | **0.42** | **0.78** | **0.80** | -2.46** | -0.09 |
| | | 10.64*** | 3.31*** | 10.04*** | 4.15*** | | |
| **Matching estimator** | A-I matching | | **0.44** | **0.91** | **0.77** | -2.94*** | 0.58 |
| | | | 3.35*** | 10.09*** | 3.44*** | | |
| | BC-RM | | **0.55** | **0.89** | **0.73** | -1.89* | 0.75 |
| | | | 3.57*** | 9.73*** | 3.83*** | | |
| **Propensity score weighted estimator** | NIPW | | **0.41** | **0.78** | **0.81** | -2.41** | -0.11 |
| | | | (0.22, 0.62) | (0.66, 0.92) | (0.51, 1.24) | | |

***Notes***

1) Dependent variable=parents' years of schooling;
2) For the column (1), the interest variable=grandparents' education dummy, dummy =1 if at least one of grandparents more than primary schooling, 0=otherwise;
3) For all columns, the control variables are parents' age, parents' age squared, grandmother's age, grandmother's age squared, grandfather's age, grandfather's age squared;
4) For the columns (2) - (4), the interest variable is grandparents' education and occupation status dummy. The dummy equals 1 if household belongs to 'treatment' group, and 0 otherwise; the base group is (farm, low edu)
5) A-I matching=Abadie and Imbens (2002)
6) BC-RM = Biased corrected radius matching;
7) NIPW = normalized inverse probability weighted propensity score estimator of Hirano and Imbens (2001);
8) The computation of NIPW uses STATA program written by Millimet and Tchernis (2013);
9) Estimates and t statistics are shown. For NIPW, 90% confidence interval is given in parentheses, which is boostrapped using 250 replications;
10) "high edu" stands for more than primary schooling, "low edu" stands for primary schooling or less;
11) ***, **, and * Denotes statistical significance at 1%, 5%, and 10% level.

**Table 4A: Rosenbaum Sensitivity analysis for the Impact of Family Background on Children's Years of schooling (primary schooling as the threshold for parental education)**

| Gamma $\Gamma$ | Non farm, low edu | | Farm, high edu | | Non farm, high edu | |
|---|---|---|---|---|---|---|
| | **Mantel-Haenszel statistics** | **P value** | **Mantel-Haenszel statistics** | **P value** | **Mantel-Haenszel statistics** | **P value** |
| **Abadie and Imbens matching** | | | | | | |
| *Daughters* | | | | | | |
| 1.0 | 0.50 | 0.00 | 1.00 | 0.00 | 1.63 | 0.00 |
| 1.3 | 0.25 | 0.05 | 0.75 | 0.00 | 1.38 | 0.00 |
| 1.5 | 0.13 | 0.26 | 0.63 | 0.00 | 1.13 | 0.00 |
| 1.8 | -0.13 | 0.72 | 0.38 | 0.00 | 1.00 | 0.00 |
| 2.0 | -0.25 | 0.90 | 0.25 | 0.00 | 0.88 | 0.00 |
| *Sons* | | | | | | |
| 1.0 | 0.75 | 0.00 | 1.00 | 0.00 | 1.63 | 0.00 |
| 1.3 | 0.38 | 0.00 | 0.75 | 0.00 | 1.38 | 0.00 |
| 1.5 | 0.25 | 0.03 | 0.63 | 0.00 | 1.25 | 0.00 |
| 1.8 | 0.13 | 0.31 | 0.38 | 0.00 | 1.00 | 0.00 |
| 2.0 | 0.00 | 0.62 | 0.25 | 0.00 | 0.88 | 0.00 |
| **Biased corrected radius matching** | | | | | | |
| *Daughters* | | | | | | |
| 1.0 | 0.50 | 0.03 | 1.00 | 0.00 | 1.50 | 0.00 |
| 1.3 | 0.00 | 0.44 | 0.50 | 0.00 | 1.00 | 0.00 |
| 1.5 | 0.00 | 0.79 | 0.50 | 0.00 | 1.00 | 0.00 |
| 1.8 | -0.50 | 0.98 | 0.00 | 0.08 | 0.50 | 0.00 |
| 2.0 | -0.50 | 1.00 | 0.00 | 0.36 | 0.50 | 0.00 |
| *Sons* | | | | | | |
| 1.0 | 0.50 | 0.00 | 1.00 | 0.00 | 1.50 | 0.00 |
| 1.3 | 0.50 | 0.03 | 0.50 | 0.00 | 1.50 | 0.00 |
| 1.5 | 0.00 | 0.24 | 0.50 | 0.00 | 1.00 | 0.00 |
| 1.8 | 0.00 | 0.76 | 0.00 | 0.25 | 1.00 | 0.00 |
| 2.0 | -0.50 | 0.94 | 0.00 | 0.74 | 0.50 | 0.00 |

***Notes:***

1. Mantel-Haenszel test statistics gives the upper bound under the assumption of overestimation of treatment effects;
2. Gamma=1 implies the absence of unobserved selection bias and both matched individuals have the same probability of participating;
3. Gamma=2 implies the two individuals are identical on matched covariates but one might be twice as likely as the other to receive the treatment because they differ in terms of unobserved covariates.

**Table 4B: Rosenbaum Sensitivity Analysis for the Impact of Family Background on Parents' Years of Schooling (primary schooling as the threshold for grandparents' education)**

| Gamma | Non farm, Low Edu | | Farm, High Edu | | Non farm, High Edu | |
|---|---|---|---|---|---|---|
| Γ | Mantel-Haenszel statistics | P value | Mantel-Haenszel statistics | P value | Mantel-Haenszel statistics | P value |
| **Abadie and Imbens Matching** | | | | | | |
| *Mothers* | | | | | | |
| 1.0 | 0.75 | 0.00 | 1.00 | 0.00 | 1.63 | 0.00 |
| 1.3 | 0.38 | 0.02 | 0.63 | 0.00 | 1.38 | 0.00 |
| 1.5 | 0.13 | 0.17 | 0.50 | 0.00 | 1.25 | 0.00 |
| 1.8 | -0.13 | 0.65 | 0.25 | 0.01 | 1.00 | 0.00 |
| 2.0 | -0.25 | 0.88 | 0.13 | 0.18 | 0.88 | 0.00 |
| *Fathers* | | | | | | |
| 1.0 | 0.38 | 0.01 | 0.88 | 0.00 | 0.50 | 0.00 |
| 1.3 | 0.00 | 0.47 | 0.63 | 0.00 | 0.25 | 0.07 |
| 1.5 | -0.13 | 0.87 | 0.38 | 0.00 | 0.13 | 0.25 |
| 1.8 | -0.38 | 1.00 | 0.25 | 0.00 | 0.00 | 0.61 |
| 2.0 | -0.50 | 1.00 | 0.13 | 0.07 | -0.13 | 0.80 |
| **Biased Corrected Radius Matching** | | | | | | |
| *Mothers* | | | | | | |
| 1.0 | 1.00 | 0.00 | 1.00 | 0.00 | 1.00 | 0.00 |
| 1.3 | 0.50 | 0.04 | 0.50 | 0.00 | 1.00 | 0.00 |
| 1.5 | 0.00 | 0.23 | 0.50 | 0.00 | 0.50 | 0.01 |
| 1.8 | 0.00 | 0.72 | 0.00 | 0.39 | 0.50 | 0.09 |
| 2.0 | -0.50 | 0.91 | 0.00 | 0.86 | 0.00 | 0.21 |
| *Fathers* | | | | | | |
| 1.0 | 0.50 | 0.07 | 0.50 | 0.00 | 0.50 | 0.00 |
| 1.3 | 0.00 | 0.77 | 0.50 | 0.00 | 0.50 | 0.09 |
| 1.5 | -0.50 | 0.97 | 0.00 | 0.02 | 0.00 | 0.27 |
| 1.8 | -0.50 | 1.00 | 0.00 | 0.67 | 0.00 | 0.63 |
| 2.0 | -1.00 | 1.00 | 0.00 | 0.97 | 0.00 | 0.81 |

***Notes:***

1. Mantel-Haenszel test statistics gives the upper bound under the assumption of overestimation of treatment effects;
2. Gamma=1 implies the absence of unobserved selection bias and both matched individuals have the same probability of participating;
3. Gamma=2 implies the two individuals are identical on matched covariates but one might be twice as likely as the other to receive the treatment because they differ in terms of unobserved covariates.

**Table 5: Causal Analysis: Estimates from MB-NIPW and Lewbel Estimators (Primary Schooling as the Threshold for Cut-off)**

| Estimates | | (2) | (3) | (4) |
|---|---|---|---|---|
| | | **Parent's Education and Occupation** | | |
| | | **Non-farm, low edu** | **Farm, high edu** | **Non-farm, high edu** |
| **Impact of Family Background on Children's Schooling** | | | | |
| ***Daughters*** | | | | |
| **MB-NIPW** | Coefficient | **0.63** | **0.93** | **1.68** |
| | 90% confidence interval | (0.18, 1.02) | (0.62, 1.23) | (1.32, 1.97) |
| **Lewbel** | Coefficient | **0.57** | **0.98** | **1.34** |
| | t statistics | 2.87** | 2.34** | 5.59*** |
| | Breusch-Pagan heteroskedasticity test | 191 | 315 | 185 |
| | Kleibergen-Paap rk Wald F statistic | 427.52 | 120.11 | 324.05 |
| ***Sons*** | | | | |
| **MB-NIPW** | Coefficient | **1.19** | **1.12** | **1.82** |
| | 90% confidence interval | (0.17, 1.57) | (0.77, 1.34) | (1.57, 2.11) |
| **Lewbel** | Coefficient | **0.89** | **0.59** | **1.49** |
| | t statistics | 3.08*** | 2.21** | 6.74*** |
| | Breusch-Pagan heteroskedasticity test | 160.56 | 371.69 | 138.39 |
| | Kleibergen-Paap rk Wald F statistic | 150.42 | 169.03 | 347.95 |
| **Impact of Family Background on Parent's Schooling** | | | | |
| ***Mothers*** | | | | |
| **MB-NIPW** | Coefficient | **0.66** | **1.04** | **1.24** |
| | 90% confidence interval | (0.22, 1.08) | (0.82, 1.22) | (0.91, 1.92) |
| **Lewbel** | Coefficient | **0.49** | **0.81** | **0.85** |
| | t statistics | 1.62 | 4.25*** | 2.73** |
| | Breusch-Pagan heteroskedasticity test | 327.31 | 173.83 | 873.24 |
| | Kleibergen-Paap rk Wald F statistic | 103.52 | 369.49 | 64.88 |
| ***Fathers*** | | | | |
| **MB-NIPW** | Coefficient | **0.21** | **0.76** | **0.73** |
| | 90% confidence interval | (-0.04, 0.65) | (0.61, 0.95) | (0.42, 1.17) |
| **Lewbel** | Coefficient | **-0.1** | **0.13** | **0.61** |
| | t statistics | -0.44 | 0.71 | 2.65** |
| | Breusch-Pagan heteroskedasticity test | 385.04 | 296.81 | 1537.28 |
| | Kleibergen-Paap rk Wald F statistic | 153.91 | 371.53 | 109.98 |

***Notes:***

1. BM-NIPW=minimum-biased normalized inversed probability weighted estimator;
2. The computation of MB-NIPW uses STATA program written by Millimet and Tachernis (2010);
3. The MB-NIPW estimator uses boostrapping of 250 applications for computing the confidence interval.
4. ***, **, and * Denotes statistical significance at 1%, 5%, and 10% level.

**Table 6: Test of Complementarity Between Parent's Education and Occupation in Children's Years of Schooling**

| | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| | | Standard specification | Marginal effect of parents' education in farm and non-farm households | | Test of equality between Columns (2) and (3) |
| | | $\hat{\beta}_3$ | Farm | Non-farm | Z-score |
| **Panel 6A: Children-Parents** | | | | | |
| ***Daughters*** | | | | | |
| | OLS | **-0.04**<br>-0.23 | **1.05**<br>8.26*** | **0.88**<br>5.02*** | 0.77 |
| **Matching estimator** | A-I matching | | **1.05**<br>7.18 | **0.83**<br>4.27 | 0.9 |
| | BC-RM | | **1.00**<br>7.03*** | **0.96**<br>4.93*** | 0.16 |
| **Propensity score weighted estimator** | NIPW | | **1.01**<br>(0.76, 1.22) | **0.88**<br>(0.55, 1.14) | 0.27 |
| | MB-NIPW | | **0.93**<br>(0.62, 1.23) | **1.01**<br>(0.44, 1.57) | -0.15 |
| ***Sons*** | | | | | |
| | OLS | **-0.15**<br>-0.86 | **1**<br>10.74*** | **0.7**<br>4.64*** | 1.71* |
| **Matching estimator** | A-I matching | | **1.05**<br>10.31 | **0.66**<br>3.81 | 1.94** |
| | BC-RM | | **0.91**<br>8.21*** | **0.86**<br>5.52*** | 0.26 |
| **Propensity score weighted estimator** | NIPW | | **1.04**<br>(0.86, 1.19) | **0.63**<br>(0.34, 0.91) | 2.12** |
| | MB-NIPW | | **1.12**<br>(0.77, 1.34) | **0.59**<br>(0.05, 1.04) | 1.74* |

***Notes***

1) For the column (1), the dependent variable is years of schooling of children generation, the regression includes the family background occupation dummy, family education dummy, the interaction of the two dummies, and the controls; but the table only reports the coefficient of the interaction of two dummies;
2) For the column (1), family background occupation dummy = 1 if off-farm, =0 otherwise, family background education dummy = 1 if more than primary schooling, 0=otherwise;
3) For all columns, the control variables are children's age, children's age squared, mother's age, mother's age squared, father's age, father's age squared;
4) Column (2) and (3) break the full sample into two subsamples based on family occupation background, for each subsample, the treatment is family education background high, which is more than primary schooling;
5) A-I matching=Abadie and Imbens (2002)
6) BC-RM = Biased corrected radius matching;
7) NIPW = normalized inverse probability weighted propensity score estimator of Hirano and Imbens (2001);
8) MB-NIPW = minimum-biased normalized inversed probability weighted estimator;
9) The computation uses STATA program written by Millimet and Tchernis (2013);
10) For NIPW and MB-NIPW, 90% confidence interval is given in parentheses, which is boostrapped using 250 replications;
11) ***, **, and * Denotes statistical significance at 1%, 5%, and 10% level;

**Panel 6B:**
**Parents-Grandparents**

| | | (1) Standard specification | (2) Marginal effect of parents' education in farm and non-farm households | (3) | (4) Test of equality between column (2) and (3) |
|---|---|---|---|---|---|
| | | $\hat{\beta}_3$ | **Farm** | **Non-farm** | **Z-score** |
| ***Mothers*** | | | | | |
| | OLS | **-0.09** | **0.99** | **1.04** | -0.18 |
| | | -0.36 | 10.48*** | 4.07*** | |
| **Matching estimators** | A-I matching | | **0.98** | **0.96** | 0.07 |
| | | | 9.14 | 3.64 | |
| | BC-RM | | **1.01** | **1.25** | -0.73 |
| | | | 8.99*** | 4.09*** | |
| **Propensity score weighted estimators** | NIPW | | **1.00** | **1.07** | -0.23 |
| | | | (0.87, 1.15) | (0.51, 1.55) | |
| | MB-NIPW | | **1.04** | **1.10** | -0.11 |
| | | | (0.82, 1.22) | (0.18, 1.94) | |
| ***Fathers*** | | | | | |
| | OLS | **-0.39** | **0.78** | **0.4** | 1.56 |
| | | (-1.75)* | 10.04*** | 1.77* | |
| **Matching estimators** | A-I matching | | **0.9** | **0.46** | 1.76* |
| | | | 10.09 | 1.91 | |
| | BC-RM | | **0.89** | **0.24** | 2.36** |
| | | | 9.73*** | 0.91 | |
| **Propensity score weighted estimators** | NIPW | | **0.78** | **0.44** | 1.45 |
| | | | (0.66, 0.92) | (0.05, 0.08) | |
| | MB-NIPW | | **0.76** | **0.37** | 0.91 |
| | | | (0.61, 0.95) | (-0.28, 1.06) | |

**Figure 1: Economic Status Based on Parent's Education and Occupation**

| | Parent's Low Education | Parent's High Education |
|---|---|---|
| **Farmer Parents** | **2494**<br>(children-parents sample)<br><br>**11,571**<br>(parents-grandparents sample) | **2403**<br>(children-parents sample)<br><br>**2204**<br>(parents-grandparents sample) |
| **Parents in off-farm** | **621**<br>(children-parents sample)<br><br>**690**<br>(parents-grandparents sample) | **1746**<br>(children-parents sample)<br><br>**312**<br>(parents-grandparents sample) |

## ONLINE APPENDIX: NOT FOR PUBLICATION

### Middle School as Parent's Schooling Threshold

The results in the main text of the paper are based on a measure of parental education that uses primary schooling as the relevant threshold. This is motivated by the fact that there are only a low proportion (4 percent) of grandparents with more than middle school (9 years of schooling), and for comparability across generations, we set primary schooling as the cut-off. However, in children's generation, at least one of the parents has more than primary schooling in 60 percent households. The evidence also indicates that having more than middle schooling makes a significant difference in non-farm occupations (de Baruw and Rozelle (2008)). This implies that middle school may be a more discriminatory cut-off for the parents in the children's generation. In this appendix, we report estimates of the effects of family background for the children's generation using middle school as the relevant threshold for parental education.

Using middle school (9 years schooling) as the relevant cut-off, however means that the sample of low educated (less than or equal to 9 years of schooling) farmers now includes the subset of farming parents with 7-9 years of schooling. If we use this subsample as the comparison group, then the estimates are not comparable to the estimates reported earlier in Tables 2 and 3 in the text where the comparison group is composed if farmers with primary or less schooling. For the sake of consistency, we thus use the subsample of farmers with primary or less schooling as the comparison group, and estimate the effects of having at least one parent more than middle schooling for the farmer and non-farmer parents. The results are reported in Tables A1, A2, and A3 in this appendix.[34] Table A1 reports the estimates from OLS, matching, and NIPW estimators, similar to Tables 2 and 3 in the text. Table A2 provides the estimates from sensitivity analysis and Table A3 reports the causal estimates from MB-NIPW and Lewbel estimators.

The estimates in Table A1 are consistent with a priori expectations: the effects of having at least one parent with more than middle school education on the schooling of children are larger compared to the effects in Table 2 where the cut-off is primary schooling. Consistent with the results in Table 2, having higher educated non-farmer parents yields the most advantages. The estimated effects are larger in magnitude for sons compared to the daughters. The estimates

[34]We also estimated the effects using the subsample of farmers with 9 years or less schooling as the comparison group. The results are available from the authors.

show that having parents with more than middle school education and nonfarm job increases the schooling attainment by more than two years for a son.

The Rosenbaum sensitivity analysis in Table A2 shows that, similar to results for the primary school cut-off for parental education, the positive effects of being born into the better educated nonfarming parents cannot be explained away by even very strong positive selection on genetic endowments of ability and preference. The evidence also confirms the finding that the children of better educated farmer parents have significantly higher schooling attainment when compared to the children of low educated nonfarmer parents, and this cannot be accounted for by genetic correlations. These conclusions are strengthened by the causal estimates from MB-NIPW and Lewbel heteroskedasticity based idetification results in Table A.3.

**Appendix Table A.1: Impact of Parents' Family Background on Children's Years of Schooling (junior middle school as the threshold for parents' education)**

| Estimates | | (1) Parents' Education | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| | | | Parents' Education and Occupation | | | Test of equality | |
| | | Full sample | Non-farm, low edu | Farm, high edu | Non-farm, high edu | $\hat{\theta}_1 - \hat{\theta}_2 = 0$ | $\hat{\theta}_2 - \hat{\theta}_3 = 0$ |
| | | $\hat{\alpha}_1$ | $\hat{\theta}_1$ | $\hat{\theta}_2$ | $\hat{\theta}_3$ | z score | z score |
| ***Daughters*** | | | | | | | |
| | OLS | **1.36**<br>9.74*** | **0.57**<br>3.32*** | **1.19**<br>6.21*** | **1.78**<br>9.99*** | -2.41** | -2.25** |
| **Matching estimator** | A-I matching | | **0.72**<br>3.62*** | **1.69**<br>6.42*** | **2.17**<br>6.94*** | -2.94*** | -1.17 |
| | BC-RM | | **0.66**<br>3.03*** | **1.22**<br>5.37*** | **1.93**<br>10.74*** | -1.78* | -2.45** |
| **Propensity score weighted estimator** | NIPW | | **0.59**<br>(0.31, 0.87) | **1.26**<br>(0.96, 1.61) | **1.85**<br>(1.53, 2.14) | -2.46** | -2.13** |
| ***Sons*** | | | | | | | |
| | OLS | **1.42**<br>12.21*** | **0.81**<br>5.42*** | **1.31**<br>8.31*** | **2.08**<br>14.37*** | -2.36** | -3.59*** |
| **Matching estimator** | A-I matching | | **0.70**<br>4.25*** | **1.29**<br>6.33*** | **2.07**<br>8.26*** | -2.25** | -2.41** |
| | BC-RM | | **0.62**<br>3.54*** | **1.41**<br>8.22*** | **2.54**<br>14.76*** | -3.22*** | -4.65*** |
| **Propensity score weighted estimator** | NIPW | | **0.82**<br>(0.60, 1.09) | **1.41**<br>(1.16, 1.66) | **2.21**<br>(1.92, 2.44) | -2.64** | -3.87*** |

***Notes***

1) Dependent variable=children's years of schooling;
2) For the column (1), the interest variable=parents' education dummy, dummy =1 if at least one of parents more than junior middle school, 0=otherwise;
3) For all columns, the control variables are children's age, children's age squared, mother's age, mother's age squared, father's age, father's age squared;
4) For the columns (2) - (4), the interest variable is parents' education and occupation status dummy. The dummy=1 if treatment group, =0 otherwise; base group is parents (farm, low edu)
5) A-I matching=Abadie and Imbens (2002)
6) BC-RM = Biased corrected radius matching;
7) NIPW = normalized inverse probability weighted propensity score estimator of Hirano and Imbens (2001);
8) The computation of NIPW uses STATA program written by Millimet and Tchernis (2013);
9) Estimates and t statistics are shown. For NIPW, 90% confidence interval is given in parentheses, which is boostrapped using 250 replications;
10) "high edu" stands for more than junior middle schooling, "low edu" stands for junior middle schooling or less;
11) ***, **, and * Denotes statistical significance at 1%, 5%, and 10% level.

**Appendix Table A.2: Rosenbaum sensitivity analysis for the impact of family background on children's years of schooling (junior middle school as the threshold for parental education)**

| Gamma | Non farm, low edu | | Farm, high edu | | Non farm, high edu | |
|---|---|---|---|---|---|---|
| $\Gamma$ | Mantel-Haenszel statistics | P value | Mantel-Haenszel statistics | P value | Mantel-Haenszel statistics | P value |
| **Abadie and Imbens matching** | | | | | | |
| *Daughters* | | | | | | |
| 1.0 | 0.50 | 0.00 | 1.13 | 0.00 | 1.88 | 0.00 |
| 1.3 | 0.25 | 0.05 | 0.88 | 0.00 | 1.63 | 0.00 |
| 1.5 | 0.13 | 0.26 | 0.69 | 0.00 | 1.50 | 0.00 |
| 1.8 | -0.13 | 0.72 | 0.50 | 0.00 | 1.25 | 0.00 |
| 2.0 | -0.25 | 0.90 | 0.38 | 0.01 | 1.13 | 0.00 |
| *Sons* | | | | | | |
| 1.0 | 0.75 | 0.00 | 1.38 | 0.00 | 2.13 | 0.00 |
| 1.3 | 0.38 | 0.00 | 1.13 | 0.00 | 1.88 | 0.00 |
| 1.5 | 0.25 | 0.03 | 1.00 | 0.00 | 1.75 | 0.00 |
| 1.8 | 0.13 | 0.31 | 0.75 | 0.00 | 1.50 | 0.00 |
| 2.0 | 0.00 | 0.62 | 0.63 | 0.00 | 1.44 | 0.00 |
| **Biased corrected radius matching** | | | | | | |
| *Daughters* | | | | | | |
| 1.0 | 0.50 | 0.03 | 1.00 | 0.00 | 2.00 | 0.00 |
| 1.3 | 0.00 | 0.44 | 0.50 | 0.00 | 1.50 | 0.00 |
| 1.5 | 0.00 | 0.79 | 0.50 | 0.01 | 1.50 | 0.00 |
| 1.8 | -0.50 | 0.98 | 0.00 | 0.10 | 1.00 | 0.00 |
| 2.0 | -0.50 | 1.00 | 0.00 | 0.25 | 1.00 | 0.00 |
| *Sons* | | | | | | |
| 1.0 | 0.50 | 0.00 | 1.00 | 0.00 | 2.00 | 0.00 |
| 1.3 | 0.50 | 0.03 | 1.00 | 0.00 | 2.00 | 0.00 |
| 1.5 | 0.00 | 0.24 | 0.50 | 0.00 | 1.50 | 0.00 |
| 1.8 | 0.00 | 0.76 | 0.50 | 0.01 | 1.50 | 0.00 |
| 2.0 | -0.50 | 0.94 | 0.00 | 0.04 | 1.50 | 0.00 |

***Notes:***

1. Mantel-Haenszel test statistics gives the upper bound under the assumption of overestimation of treatment effects;
2. Gamma=1 implies the absence of unobserved selection bias and both matched individuals have the same probability of participating;
3. Gamma=2 implies the two individuals are identical on matched covariates but one might be twice as likely as the other to receive the treatment because they differ in terms of unobserved covariates.

**Appendix Table A.3: Causal analysis: estimates from MB-NIPW and Lewbel estimators (junior middle school as the threshold for parental education)**

| Estimates | | (2) | (3) | (4) |
|---|---|---|---|---|
| | | **Parents education and Occupation** | | |
| | | **Non-farm, low edu** | **Farm, high edu** | **Non-farm, high edu** |
| **Impact of family background on children's schooling** | | | | |
| ***Daughters*** | | | | |
| **MB-NIPW** | Coefficient | **0.63** | **1.24** | **1.87** |
| | 90% confidence interval | (0.18, 1.02) | (0.75, 1.49) | (1.47, 2.24) |
| **Lewbel** | Coefficient | **0.55** | **0.80** | **1.71** |
| | t statistics | 2.72** | 2.95*** | 7.07*** |
| | Breusch-Pagan heteroskedasticity test | 191.01 | 193.45 | 201.51 |
| | Kleibergen-Paap rk Wald F statistic | 433.12 | 285.63 | 278.42 |
| ***Sons*** | | | | |
| **MB-NIPW** | coefficient | **1.19** | **1.61** | **2.23** |
| | 90% confidence interval | (0.62, 1.57) | (1.26, 1.94) | (1.26, 1.94) |
| **Lewbel** | coefficient | **0.89** | **1.24** | **1.99** |
| | t statistics | 3.08*** | 5.29*** | 9.85*** |
| | Breusch-Pagan heteroskedasticity test | 160.56 | 239.08 | 296.16 |
| | Kleibergen-Paap rk Wald F statistic | 150.39 | 531.69 | 404.56 |

***Notes:***

1. BM-NIPW=minimum-biased normalized inversed probability weighted estimator;
2. The computation of MB-NIPW uses STATA program written by Millimet and Tachernis (2010);
3. The MB-NIPW estimator uses boostrapping of 250 replications for computing the confidence interval.
4. ***, **, and * Denotes statistical significance at 1%, 5%, and 10% level.