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UNOBSERVED HOUSEHOLD AND COMMUNITY HETEROGENEI
AND THE LABOR MARKET IMPACT OF SCHOOLING:
A CASE STUDY FOR INDONESIA

by

Jere R. Behrman and Anil B. Deolaikar
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October 16, 1990

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A CASE STUDY FOR INDONESIA

Jere R. Behrman and Anil B. Deolalikar*

16 October 1990

Abstract

Previous estimates of the private and social economic rates of return to schooling in developing countries often suggest that schooling is an attractive investment opportunity. But most such estimates do not control for unobserved community and household factors relating to abilities, motivation, schooling quality, employment opportunities, and role models. If such unobserved characteristics are important in the determination of wage rates and if they are correlated with years of schooling, the standard estimation procedure results in biased estimates of the impact of schooling. This paper presents new estimates of the impact of schooling on Indonesian wage rates and hours supplied to the paid labor force. These estimates focus on the control for unobserved household and community characteristics. They indicate that standard estimates bias upwards substantially the estimated overall impact of schooling on wage rates, that they bias upwards the relative returns to lower schooling levels as opposed to higher schooling levels, and they bias upwards the returns to schooling for males in comparison to those for females. The standard estimates also bias upwards substantially the estimated impact of schooling on hours supplied to the paid labor market by males and females, particularly for the lower schooling levels once again.

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Recent advocacy of increasing schooling in the developing countries has been strong, both from applied and theoretical perspectives. In the more applied literature, the World Bank (1980, 1981, 1990), Colclough (1982), Eisemon (1988), Pscharopoulos (1985, 1988), the United Nations Development Program (1990) and many others have emphasized strongly the important role of schooling in attaining economic and other goals in developing countries. In the theoretical literature, the "new neoclassical economic growth" models of Romer (1986), Lucas (1988), Azariadis and Drazen (1990) and others have emphasized the possibly critical role of schooling in the development process from a theoretical perspective that they argue is more consistent with actual growth experience than are previous growth models.[1] The applied literature maintains that the rate of returns to such schooling investments are substantial. The World Bank (1980), for example, claims that the average social economic real annual rate of return to primary schooling in developing economies is over 24%, and the rates of returns on higher levels of schooling, though lower, still are considerable.[2] Based on such rates of return, schooling indeed appears to be an important investment for the development process. With complete reinvestment of the returns of an annual real rate of return of 24%, for example, one can double one's assets in less than three years.

Estimates of the economic rates of return to schooling in Indonesia, the country of empirical interest for this study, tend to be a little lower than the mean estimates for developing countries summarized by the World Bank, but still considerable. Table 1 gives previous estimates of private and social rates of return to investing in schooling in Indonesia. The estimates of the social rates of return to primary education in Indonesia cluster in the 17 to 22% range (though the one for urban males in 1986 is 9%). Most of these estimates suggest that schooling is a very attractive investment opportunity in Indonesia.

However there have been a number of criticisms of such calculations.[3] One major line of criticism has been that such studies typically attribute the association between years of schooling and wages[4] to reflect causality from the former to the latter without controlling for a host of other factors that plausibly may be correlated with years of schooling and affect wages. As a result the estimated impact of years of schooling on wages may be biased, with the direction of such bias dependent on the correlations of years of schooling with the omitted variables and the true impact of the omitted variables on wages. Examples of such possibly important omitted variables include schooling quality, prices, general community learning environment, individual ability and motivation, household and community role models, community job opportunities and home environment. A number of studies

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[1]This literature tends to emphasize externalities generated by knowledge that can lead to different phenomena that is more consistent with real world experience than are previous neoclassical growth models. For a survey and an evaluation, see Behrman (1990a).

[2]"Social" refers to the incorporation of direct public budgetary expenditures in addition to direct private costs (including time costs), and not to externalities nor to noneconomic returns.

[3]See Behrman and Birdsall (1987) or Behrman (1987, 1990a,b,c) for a summary of many of these criticisms and references to other related studies.

[4]Sometimes earnings are used instead of wage rates in such studies, which may confound the effect on hours worked with those on the value of time. Also while the wage rate may capture well the private return to schooling, it reflects productivity only under additional assumptions. However only a few studies have been able to use direct productivity measures instead of wages (e.g. Jamison and Lau, 1985).

find that failure to control for some of these factors apparently causes substantial biases in the estimated rates of returns to schooling in both developing and developed economies.[5]

One way to control simultaneously for a number of such possibly important unobserved (at least in most labor force surveys) community and household variables, at least to the extent that they enter the wage relation additively, is via household fixed-effect estimation. Perhaps surprisingly, we are unaware of any prior published studies of wage estimates for developing countries that control for such household fixed effects.[6] Our contribution in this study first is to present estimates of wage functions for Indonesia without and with fixed effects to see if such controls make a difference in the estimated impact of the impact of schooling on wages. Second, we examine a parallel question about the impact of schooling on hours supplied to the paid labor market. For both wage rates and hours supplied to the paid labor market we consider relations separately for males and females since there are widespread perceptions that such relations may differ by gender.[7] For both wages rates and hours supplied to the paid labor market, we explore the robustness of our results to possible sample selectivity and to the presence of random effects instead of fixed effects.

Our results suggest that estimates of the impact of schooling on wage rates and on hours supplied to the paid labor market in Indonesian that do not control for household fixed effects may be substantially misleading. But the estimated directions and magnitudes of the biases due to the failure to control for such fixed effects differ substantially between males and females, between wage rates and hours supplied to the paid labor force, and among schooling levels. For the wage rate estimates for lower schooling levels the failure to control for such fixed effects causes substantial upward biases in the estimated impact of schooling at lower schooling levels -- for instance, almost 100% for males and 27% for females with three years of schooling. But with more schooling, this bias declines for males (though it still is 15% at 12 years of schooling) and reverses in sign for females. For the hours supplied to the paid labor market estimates for both males and females the bias tends to be upward[8] (i.e., the OLS estimates are greater algebraically than the fixed effects estimates, whether both are positive or both are negative), though with lessening relative differences once again for more years of schooling.

1. DATA

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[5]For examples, see the references in note 3.

[6]The closest published study of which we are aware is Behrman and Wolfe (1984). They control for childhood household fixed effects shared by adult sisters in their estimates of the determinants of the sisters' own (as adults) household income, but they do not consider wage rates nor labor market earnings. As we were preparing this revision a preliminary current study by Khandker (1990) came to our attention; the Khandker study cites an earlier version of the present paper, and adopts a related approach.

[7]And since statistical tests indicate that the relations differ between males and females. See Section 4.

[8]The one exception to this statement is for males with 12 or more years of schooling, but for this group the differences due to controlling or not for fixed effects are not significant.

We use data from the 1986 Indonesian Labor Force Survey. This survey is a stratified national sample of about 250,000 individuals ten years old or older. We focus on the 30,253 individuals who received wages as paid employees. These individuals include 21,655 males and 8,598 females. The rates of participation in the paid labor force for males are about 19% and for females are about 7%. Therefore there may be a question of sample selectivity for both males and females, though perhaps it is more likely to be greater for the latter. We explore selectivity issues in Sections 3 and 4.

Our hours worked variable is the hours worked in the paid-labor market during the week before the survey.[9] Our wage variable is the average wage calculated by dividing total wages received during the previous week by the hours worked as a paid employee during that week. The mean wage rates are 85% higher for males than for females, though there is substantial variance for both males and females (Table 2).

Schooling is recorded in the following categories: subprimary, completed primary, vocational junior secondary completed, general junior secondary completed, vocational senior secondary completed, general senior secondary completed, post-secondary diploma completed, and university completed. Table 2 gives the proportions of males and females in each of these schooling categories. This distribution tends to be higher for males than for females, with 93.9% of the males and 80.8% of the females having some formal schooling. Nevertheless there are a few categories beyond no schooling and subprimary schooling for which the share of female respondents is greater than the share of male respondents -- namely, vocational senior high school and diploma 1,2. We translate these schooling categories into years of schooling for the analysis presented below. In this translation we use data on school grade-specific repetition and dropout rates for 72 subsamples (defined by age, sex, region, and urbanization) in addition to the number of grades at each level to calculate the expected

[9]In the Indonesian economy many people who work do not work in the paid labor market, but work as unpaid family workers, self-employed or as employers. We consider only hours worked as employees in the paid labor market, which may cause a selectivity bias that we discuss in Sections 3 and 4.

years of school for an individual of a given age, sex, and location.[10]

2. NATIONAL OLS ESTIMATES OF WAGE RATE AND HOURS WORKED RELATIONS

We focus on semilog wage rate (W) and hours worked in the paid labor force (H) functions in which the right-side variables are quadratics in years of schooling (S) to capture possible nonlinear effects of schooling, quadratics in age (A) to represent standard life cycle phenomena, interactions between years of schooling and age to represent secular effects in the impact of schooling, interactions with dichotomous variables for vocational (V) and diploma 1,2 (D) schooling (the latter usually is teacher training) to allow for differences in the impact of general versus specialized schooling, and disturbance terms that in turn can be broken down into unobserved household fixed effects (f_h), unobserved community fixed effects (f_c), and random terms (e) distributed normally with means zero and constant variances:

$$\ln W = a_0 + a_1S + a_3S^2 + a_4A + a_5A^2 + a_6SA + a_7SV + a_8SD + f_h + f_c + e, \tag{1}$$

$$H = b_0 + b_1S + b_3S^2 + b_4A + b_5 A^2 + b_6SA + b_7SV + b_8SD + f'_h + f'_c + e', \tag{2}$$

where the a's and b's are the respective parameters to be estimated and the primes on the components

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[10]For details of our calculations and discussion of their implications, see Behrman and Deolalikar (1991). As discussed there, we consider the possibility that the repetition and dropout rates are homogeneous for each subsample and alternatively that they are heterogeneous within each subsample, with those who proceed to higher schooling levels not repeating nor dropping out of lower schooling levels. For the present purpose of investigating the impact of controlling for household and community fixed effects on the estimated impact of schooling, it does not make much difference whether we assume homogeneity or heterogeneity in repetition and dropout rates, so we present results that are based on the assumption of heterogeneity since that seems more plausible a priori to us.

The correction for repetition and dropout rates clearly is appropriate since resources (including time) are expended in schooling whether entry into a given grade culminates in promotion, repetition, or dropping out. (Dropping out of a grade presumably requires less resources (at least in terms of students' time) than completing the grade successfully (and therefore progressing) or completing the grade unsuccessfully (and therefore repeating the grade the following year), even though there are some fixed costs to beginning a grade. Therefore we have assumed that individuals who drop out of a given grade on the average spend half of that year in school.) But while such a correction clearly is appropriate, some readers may wonder whether it is worth the cost since it may seem to be a minor detail. However the repetition and dropout rates are high enough in the Indonesian case (and in the case of many other developing countries) to make a considerable difference. For example, for males age 20 - 49 in the urban areas of Java, the expected years in school for individuals who are in the completed primary school category is about eight. This is about a third larger than the six years that would be required to complete the six grades of primary school with neither repetition nor starting and dropping out either in primary or in junior secondary school (without completing the latter, or the individual would be in the completed junior secondary category instead of the completed primary category). Thus correcting for repetition and dropout rates in this case results in an increase of a third in the expected years of schooling -- and a reduction of about a third in the estimated returns to such schooling.

of the disturbance terms refer to the hours worked relation. Under the standard assumption that years of schooling and age are predetermined,[11] these two relations can be thought of as quadratic approximations to the reduced forms that determine an individual's wage rate and hours worked in the paid labor market.[12] The wage relation also often is given two more specific interpretations. First, it can be considered an hedonic index yielding weights on characteristics that affect the price of an unit of the individual's time, as suggested by Tinbergen (1951) and Rosen (1974). Second, it can be viewed as a generalization of the equilibrium relation between schooling and wages derived by Mincer (1974) in which the partial derivative of ln wage with respect to schooling is the estimate of the private rate of return to the time spent in school instead of in the labor market.[13]

Whether or not one wishes to give a Mincerian interpretation regarding such rates of return, the partial derivatives of expressions (1) and (2) with respect to schooling are of interest to characterize the impact of schooling on wages and on hours worked in the paid labor market. Because of the well-known impact of experience on wages, we consider the partial derivatives with respect to years of schooling holding post-schooling experience constant using the standard definition of such experience as age minus years of schooling minus six (the assumed age of entering schooling):

$$(3) \qquad d\ln W/dS = a_1 + a_4 + (2a_5 + a_6)A + (2a_3 + a_6)S + a_7V + a_8D$$

$$(4) \qquad dH/dS = b_1 + b_4 + (2b_5 + b_6)A + (2b_3 + b_6)S + b_7V + b_8D.$$

These expressions indicate that the impact of schooling holding constant post-schooling experience depends on the parameters of relations (1) and (2) and on age, schooling, and type of schooling. We use such expressions below to summarize our regression estimates for general schooling.

The standard procedures for obtaining schooling rate of return estimates use as a basic input the impact of schooling on wages. The procedures generally used to estimate this impact are equivalent to estimating relation (1), or some simpler version without the age controls, by OLS. Therefore we begin with OLS estimates of relations (1) and (2), and effectively thereby ignore the unobserved fixed effects

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[11]From a single-period lifetime point of view, years of schooling and wages can be considered to be determined simultaneously, as in Becker's (1975) and Mincer's (1974) seminal studies. In most empirical estimates of adult labor market relations, however, schooling is treated as predetermined. Griliches (1977) claims that empirically such treatment makes little difference in available estimates.

[12]Sometimes the returns to schooling are calculated by comparing the wage rates for pairwise groups of individuals with otherwise identical observed characteristics (typically age and sex). Such a procedure is very close to running the OLS regression here, and like the OLS regression here may cause biased estimates due to the failure to control for unobserved fixed effects that may differ systematically across such groups.

[13]Mincer actually works with earnings under the assumption that total hours worked are fixed, but that is equivalent to working with wage rates. Mincer's equilibrium expression also implies that the coefficients on S^2, SA, SV, and SD in relation (1) are constrained to be zero. Finally, Mincer includes a quadratic in post-schooling experience rather then in age, but the two are equivalent in their implications for the interpretation of the estimates given his representation of post-schooling experience by age minus years of schooling minus age when starting school (usually assumed to be six).

in these two relations. The first columns for males and for females in the top panel of Table 3 give the estimated private rates of return from the wage rate relations for an additional year of schooling for four different total years of schooling (i.e., 3, 6, 9, and 12 years of schooling).

These estimates imply an impact of an additional year of schooling on wage rates of the magnitude of 6 to 11% for males and of 6 to 17% for females, with increasing effects with more years of schooling and with increasing differential favoring females with more years of schooling. Under Mincerian assumptions these imply private real rates of return to time spent in schooling (instead of in the labor market) of the same ranges. That the private returns are estimated to increase with the schooling level of course does not mean that the social returns increase with the schooling level because the public costs per student tend to increase with schooling level at least through the secondary school years (e.g., see IEES 1986).[14] The range of estimated private rates of return to schooling investments are lower than are those in Table 1, which is interesting in light of the fact that the social rates of return for comparison by construction should be lower than the private ones since the social rates in this table incorporate public subsidies for schooling (but not externalities). That our estimates are lower than those previously presented for Indonesia probably reflects the failure in the previous estimates to control for the costs of repetition and dropouts and for experience and hours worked.[15] Thus they imply less optimism about the gains to be expected from continuing schooling expansion in Indonesia than do the earlier estimates. Nevertheless they imply that schooling still is an attractive private investment and, if the publicly subsidized costs are not too large or the positive externalities are large, an attractive social investment.

The hours worked estimates imply differential effects of additional years of schooling depending particularly on the schooling level and secondarily on gender. At low schooling levels, additional schooling induces additional hours worked in the paid labor market. The reverse holds for higher schooling levels. Such a pattern is consistent with the textbook example of a backward-bending labor supply curve because of the dominance of the price effect at low schooling (wage rate) levels and the dominance of the income effect at high schooling (wage rate) levels. However in the Indonesian context the traditional textbook story has to be modified because the alternatives to working in the paid labor market are not only the leisure emphasized in the textbook story, but also work in such categories as unpaid family worker, self-employed and employer. Therefore the backward bending supply of hours worked in the paid employee labor market may reflect partly that those with more schooling are more likely to be members of households that have assets (e.g. land, shops, and other family enterprises) that permit higher returns to work time spent in nonpaid labor market activities.[16] Both the positive effects of additional time in school at low years of schooling and the negative effects at high

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[14]Here we use "social returns" as it is used in this literature to refer to the returns to private and public costs, but without any adjustment for possible externalities or noneconomic effects.

[15]Because of the tremendous expansion in Indonesian schooling, some of the difference between our estimates and for those for earlier years in Table 1 (but obviously not between our estimates and the ones for 1986 in Table 1) may be due to declining returns to schooling over time as a result of the tremendous expansion in the supply of schooled labor. The cross-country comparisons in World Bank (1980) and in Psacharopoulos (1985, 1988) support the existence of some such phenomenon, but suggest that it usually does not cause really large drops in the returns to schooling.

[16]Blau (1986) presents evidence consistent with such effects for a neighboring country, Malaysia.

years of schooling are estimated to be larger for females than for males. In some sense, thus, the sensitivity of female's time to both the price and the income effects appears greater than for males.

3. FIXED-EFFECTS ESTIMATES

The OLS estimates discussed in the previous section ignore the possibility of there being unobserved household and community characteristics that affect wage rates and hours worked in the paid labor market and that are associated with years of schooling, with the result that the estimated coefficient of the years of schooling may be contaminated by omitted variable bias. As is indicated in the introduction, examples of such possible characteristics are many: ability, motivation, role models, household opportunities, household assets, household demographic structure, schooling quality, relative prices, community employment opportunities, among others. As also is indicated in the introduction, the authors of a number of previous studies for both developed and developing countries interpret their results to mean that the usual failure to control for one or more of such factors has biased substantially the estimated impact of schooling on a range of outcomes.

What are the effects of controlling for such factors on the Indonesian estimates of the micro economic impact of schooling? We now turn to exploring this question. We do so by controlling for community and household effects by estimating relations (1) and (2) in the form of deviations from the household means.[17],[18] Such a procedure controls for all of the household and community characteristics that should be included additively in the wage rate and hours worked relations to avoid omitted variable bias in the estimated schooling coefficients and are shared among household members, but are not observed. Among these characteristics are many, if not all, of the characteristics that determine whether or not an individual participates in the paid labor force, so such estimates also limit,

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[17]We here are assuming that the household and community characteristics in the disturbance terms in relations (1) and (2) are fixed effects. In Section 4 we report on the investigation of random effects and on tests that indicate that these effects are fixed and not random.

[18]Bishop (1976), Griliches (1979), and others have noted that fixed effects procedures may result in greater random measurement error than in level estimates due to the differencing involved. This would result in a greater bias towards zero in the schooling coefficient estimates in the fixed effects than in the level estimates. Behrman (1984), however, notes that if there is systematic measurement error that is correlated across the observations for which fixed effects are explored (e.g., such as ignoring schooling quality that is likely to be correlated positively across household members and that Behrman and Birdsall 1983, 1985 find has important impacts on labor market outcomes in another developing country context), the bias may be greater in the level than in the fixed effects estimates. In the present study we find several cases for which the fixed effects estimates are further from zero than are the level estimates (e.g., for higher schooling levels for female wage rates and for most schooling levels for both male and female hours supplied to the paid labor force), so classical random measurement error due to the differencing in the fixed effects estimates emphasized by analysts such as Bishop and Griliches does not seem to be dominating our results.

or possibly eliminate, selectivity bias.[19] Through this procedure we are not able to control relevant individual characteristics that differ from household characteristics. To the extent that these are important our estimates still may have some omitted variable biases. However, many of the characteristics to which appeal is made to explain differential labor market behavior usually are represented in empirical studies by household characteristics (e.g. assets for household enterprises, demographic composition, unearned income) and the variance in many others (e.g. intelligence) apparently is greater between households than among household members due to a combination of genetic, micro environmental and assortative mating factors. Therefore controlling for household and community fixed effects is likely to control for a substantial share of the relevant unobserved factors.

The second columns for males and for females in the two panels in Table 3 give our fixed effects estimates for the impact of an additional year of schooling on Indonesian wage rates and hours worked in the paid labor market. The next column in each case gives the percentage differences between the OLS and the fixed effects estimates relative to the fixed effects estimates. Under the assumption that the fixed effects estimates are the "true" estimates, these percentages indicate the magnitudes of the biases in the OLS estimates. A priori such biases could be in either direction depending upon the sign of the correlation between years of schooling and the unobserved fixed effects and on the sign of the impact of the fixed effects on the outcome of interest.

The comparison of the OLS and the fixed effects estimates for the Indonesian national sample are striking! For the estimated wage rates for males, OLS overestimate the impact of schooling of from 15 to 99%, depending on the schooling level. For women, OLS overestimate the impact of schooling by 27% at three years of schooling, but the bias appears to be in the opposite direction for higher levels of schooling -- e.g., -12% for 12 years. Thus schooling in the OLS estimates appears to represent in part not only the effects of schooling per se, but also the effects of unobserved household and community factors that are associated with schooling and with wage rates. Possible examples of such unobserved household and community factors are given above: motivations for work, abilities that are rewarded in the labor market, role models, employment options, and production factors that are complementary with skilled labor in household enterprises and in the local community. The simple bivariate association between years of schooling and wage rates represents in part such unobserved household and community characteristics. The results are a (1) substantial upward bias in the estimated

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[19]This observation that fixed effects procedures may control for selectivity has been made by others. For example, Heckman and Macurdy (1980) state: "The similarity of the estimated fixed effects and the structural coefficients for the unconditioned and the conditioned systems suggest that the sample selection bias that might arise in estimating equations on samples of women who have worked at least once does not appear to be an empirically important phenomenon in hours of work and wage functions once fixed effects are introduced into labour supply and wage equations." For another example, Pitt and Rosenzweig (1990) state: "A second advantage [in addition to reducing the number of parameters that they estimate in their model] of a fixed effects procedure is that differencing eliminates the sample selection problem. Given a selection rule, the residuals ... can be rewritten as the sum of residuals having zero mean and another term which adjusts for the truncation of the distribution of behaviors. As is well known, in the case of normality, this term is proportional to the Mills ratio.... The important point is that the term which adjusts the residual for its truncation is household-specific.... These additive adjustment terms vanish when household member type equations are differenced...."

impact of schooling on wage rates,[20] (2) a substantial upward bias in the estimated returns to primary as opposed to higher schooling levels for both males and females, and (3) a substantial upward bias in the estimated returns to male relative to female schooling.

These are important biases, each of which merit some further comment. (1) The overall upward bias means that the World Bank (1980, 1981, 1990), Colclough (1982), Eisemon (1988), Pscharopoulos (1985, 1988) and other strong schooling investment advocates that have based their advocacy in part on standard estimates have a weaker bases for the strength of their advocacy than it would appear from the standard estimates. The point is not that there are not likely to be reasonable private and perhaps social returns to schooling. Real private rates of return of 5 to 10% for primary schooling are likely to be fairly attractive given the options for most individuals. But the standard estimates are likely to exaggerate substantially the returns to such investments and, if believed, possibly result in unrealistic expectations and distortions in the allocation of resources.[21]

(2) The relative upward bias in estimated returns to primary school also is important because studies and surveys such as those mentioned in point (1) also have emphasized strongly the relatively high returns to investments in primary schooling as opposed to higher schooling levels, in part on the basis of calculations such as in the standard OLS estimates.[22] Our results suggest that for Indonesia, the control for unobserved fixed effects increases substantially the estimated private impact of higher schooling levels relative to lower levels.[23]For males, for example, the estimated impact of a marginal year of schooling after 12 years is 1.37 (=10.7%/7.8%) times that of a marginal year of schooling after six years of schooling (with an absolute difference of 2.9%), but the fixed effects estimates imply a relative effect of 1.79 (=9.3%/5.2%) and an absolute difference of 4.1%. For females the parallel changes are 1.66 to 1.92 for the relative estimates and 6.6% to 9.0% for the absolute differences. The standard estimates, thus, embody a substantial distortion regarding the returns to different levels of schooling. The pattern of distortions across schooling levels suggests that for higher schooling levels, labor markets are integrated more geographically so community effects are not so important and the

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[20]This is the case despite the possible downward bias in the estimated returns to females at higher schooling levels because the proportion of the population that is female and that had attained such schooling levels in the sample is very small. See Table 2.

[21]As noted above, neither the standard estimates nor the fixed effects estimates discussed here address the issue of whether there are efficiency reasons, such as externalities, for having policies that subsidize schooling. However both sets of estimates do address in part the question of how effective schooling investments are likely to be in pursuing distributional goals since the answer to this question depends importantly on what are the private returns to schooling.

[22]Another major component of such advocacies is that the public cost per pupil in primary schooling typically is much lower than the public cost per pupil at higher schooling levels. Such a subsidy structure across schooling levels is a separate issue from the one that we are addressing in this paper (though we are not aware of persuasive empirical evidence that supports efficiency or equity-related distributional reasons for such a subsidy structure).

[23]And, of course, the direction of this effect would carry over to standard calculations of social returns based in part on these same data since this effect does not change the calculation of public costs but does change the estimated social gains parallel to the change in the estimated private gains.

more extensive schooling has weakened the relative influence of family background so household effects are not so important. Control for the distortions due to unobserved household and community fixed effects at least raises questions about the conventional wisdom that the returns are much higher for primary than for higher schooling levels.

(3) The apparent upward bias in the estimated relative returns to male versus female schooling also has important implications for understanding developing country labor markets and possibly for policy. In his review of labor market effects of women's schooling, Schultz (1989, 1991) suggests that if anything such returns are at least as high for females as for males.[24] The results of this study reinforce Schultz's conclusion if they hold for other developing countries since the studies that he summarizes do not control or control only partially for household and community effects. The failure to control for unobserved household and community factors in standard OLS estimates[25] tends to bias upwards the estimated impact of schooling on wage rates at lower schooling levels for both males and females in Indonesia, but much more for the former. And at higher schooling levels, the fixed effects controls, while implying smaller biases in the OLS estimates than for lower schooling levels, suggest biases in the opposite directions -- i.e., upwards for males and downwards for females. For both males and females at lower schooling levels and for males at higher schooling levels, therefore, years of schooling is associated with unobserved household and community characteristics that have labor market returns, but the associations are stronger for males than for females. For females with higher schooling levels, however, these associations are inverse. That is, for a given higher level of years of schooling females with more of these productivity-related household and community attributes are more likely to work in household enterprises or other non wage activities so that the failure to control for such attributes causes a downward bias in their estimated labor market returns. The overall effect of the patterns of estimates for males and females, thus, is to bias upwards considerably the estimated wage rate impact of years of schooling for males relative to females.

We do not discuss the estimates for the hours supplied to the labor market as extensively as those for the wage rates because the emphasis in the literature is on the impact of schooling on wages, not on hours spent in paid labor market activities. But we do note that our results indicate some substantial effects of the control for unobserved household and community effects on hours supplied to the paid labor market in addition to those on wage rates. *First*, the OLS estimates generally overstate algebraically (i.e., yield a more positive estimate) the response of hours supplied to the paid labor market to years of schooling (though not for males with high schooling levels). That is, the income

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[24]He also suggests that returns in "noneconomic" activities are likely to be greater for females than for males. See Behrman (1990c) for a survey of the returns to women's schooling in "noneconomic" activities such as health, nutrition, child schooling, and fertility and some doubts whether the empirical basis for the conventional wisdom that such returns are quite high is as strong as often seems to be assumed.

[25]Schultz is dubious about the value of standard OLS estimates due to their failure to control for possible selectivity bias, though he appears less concerned about the failure to control for unobserved household and community characteristics other than those that enter in through the selectivity decision. As is discussed below in Section 4, at least for the sample used in this study the control for paid labor force participation selectivity with observed variables does not change the estimated impact of schooling significantly in the relations for paid labor force outcomes, though the control for unobserved fixed effects does.

effect is overestimated relative to the price effect, so that the OLS estimates imply a more positive (or less negative) impact of years of schooling on hours supplied to the paid labor market than do the fixed effects estimates. *Second*, the differences between the OLS and the fixed effects estimates tend to lessen relatively (and absolutely for males, but not for females) with higher schooling levels. *Third*, the absolute extent to which the control for fixed effects reduces the estimated impact of years of schooling on hours supplied to the paid labor market is much greater for females than for males at all schooling levels (e.g., for three years of schooling the reduction is 0.25 for males and 0.79 for females, while for 12 years of schooling is -0.01 for males and 0.79 for females). This suggests that for females more than for males those who participate in the paid labor market tend to be those for whom the nonmarket alternatives are less attractive, so that control for those nonmarket alternatives (e.g., assets related to household enterprises) reduces more the estimated impact of years of schooling on hours supplied to the labor market.

4. SOME EXPLORATIONS OF THE ROBUSTNESS OF THE BASIC RESULTS

The results that are summarized in the previous two sections suggest that the control for unobserved household and community fixed effects alters importantly some dimensions of the estimated impact of schooling on Indonesian labor market outcomes. Therefore it is of value to explore some dimensions of the robustness of those results. In this section we report on some statistical tests that we undertook in order for this purpose. In this discussion we refer to the estimates and the tests that are summarized in Tables 4-7.

Table 4 presents four sets of estimates for males and four sets of estimates for females for ln wage rate relations. The four sets of relations refer to (1) full-sample of all paid labor force participants OLS estimates, (2) full-sample OLS relations with selectivity control, (3) OLS on fixed effects sample, and (4) fixed household and community effects estimates. Table 5 presents the first, third and fourth relations for males and for females for hours supplied to the paid labor force. Table 6 presents five sets of estimates for males and for females for ln wage rate relations, all using the subsamples in which there are at least two male wage recipients per household or two female wage recipients per household, respectively. These five sets of estimates are (1) OLS, (2) OLS with selectivity control, (3) household and community fixed effects, (4) random effects, and (5) random effects with selectivity control. Table 7 presents the first, third and fifth of these estimates for both males and females for hours supplied to the labor market.

1. <u>F tests reject combining the samples for males and females</u>: At the bottom of Tables 4 and 5 are given F tests for combining the samples for males and for females for ln wage rate and hours supplied to the paid labor force relations for both OLS and fixed effects estimates. The critical F value at the one per cent level is about 2.2. All of the F values are substantially above this level. Therefore there are some important statistical differences between the relations for males and female, and we do not present any estimates with the male and female samples combined.

2. <u>Selectivity controls in the OLS wage rate estimates indicate some evidence of selectivity for females, but not for males and not substantially different estimates for females</u>: Selectivity bias may contaminate the estimates for at least two reasons. First, there is the limited participation in the paid labor force for both males and females in our sample that is noted in Section 1, and the participation decision is not likely to be random with regard to the included variables, but based on unobserved productivity characteristics that enter into the comparison between the returns to spending time in the

paid labor force versus time in other activities.[26] Second, to undertake our household and community fixed effects estimates we must further limit our sample to individuals who are from households with at least two participants in the paid labor force.[27] While there is a lack of complete agreement about the importance of selectivity and the usefulness of the various procedures that have been developed to deal with selectivity,[28] efforts to correct for it are enough part of standard procedures that we think it important to attempt to do so within the limitations of the Indonesian Labor Force Survey data.

As we argue at the start of Section 3, our use of household fixed effects procedures controls for selectivity in so far as labor force participation for one or more household members depends on household and community characteristics, which in fact are the types of characteristics that usually are used to construct empirical controls for selectivity (e.g., the number of small children, income from nonlabor sources, and productive assets for use in household enterprises). As we also note there, previous studies (e.g., Heckman and Macurdy 1980, Pitt and Rosenzweig 1990) have argued that the use of fixed effects controls for selectivity.

But to obtain further insight into possible selectivity problems, we also use the Olsen (1980) least squares correction for selectivity bias in some estimates, and we compare OLS estimates between samples of all paid labor force participants and subsamples of these participants that we use for fixed effects estimates.

The Olsen control involves the OLS estimation of a probability of receipt of wages relation and the subsequent inclusion of the predicted probability minus one from this relation in the estimated relations of interest. Table 8 gives the first-stage OLS wage rate receipt prediction relations separately for males and females. The right-side variables include quadratics in age and in years of schooling, schooling interactions terms with vocational and diploma schools (as contrasted with general

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[26]As noted above, these activities include working in own farm/firm activities and in household production. For Malaysian males and females, for example, Blau (1986) claims that the more productive individuals in terms of these characteristics tended to work in own enterprises rather than enter the paid labor force.

[27]This is not such a strong limitation as it would be for many societies since many teenagers participate in the labor force and since many households are extended vertically or horizontally.

[28]Manski's (1989: 358-9) recent survey of the "Anatomy of the Selection Problem," for example, concludes:

> Fifteen years ago few economists paid attention to the fact that selective observation of random sample data has implications for empirical analysis. Then the profession became sensitized to the selection problem. The heretofore maintained assumption, conditional mean independence of y and z, became a standard object of attack. For a while the normal-linear latent variable model became the standard 'solution' to the selection problem. But researchers soon became aware that this model does not solve the selection problem. It trades one set of assumptions for another. Today there is no conventional wisdom.... I find the current diversity of opinion unsurprising. Moreover, I expect it to persist. Selection creates an identification problem. Identification always depends on the prior knowledge a researcher is willing to assert in the application of interest. As researchers are heterogeneous, so must be their perspectives on the selection problem.

schooling), and two indicators of the demographic composition of the household (the number of household members ten years old or older and the number less than ten years old).[29] The estimates indicate a positive but diminishing impact of age on receipt of wages for both males and females. For both males and females there is a tendency for the impact of general schooling on labor force participation to be negative, apparently since the returns to such schooling are higher for own-enterprise activities (e.g., as found in Malaysia by Blau 1986). But the opposite is the case for vocational training and for diploma-level schooling (the latter primarily is teacher training). For both males and females there is a negative impact of the number of other household members over ten on receipt of wages, presumably because more such other household members increase the possible sources of income for the household. For females the number of household members under ten years of age reduces the probability of receipt of wages, presumably due to child care needs, but for males the number of household members under ten years of age increases the probability of receipt of wages apparently because of the higher dependency ratio.[30]

There are Olsen selectivity controls in the second sets of estimates in Tables 4 and 6. For males, the point estimates for the coefficients of the selectivity control are negative, but with large standard errors so that the larger of the two t values does not indicate significance even at the 25% level. For females, the point estimate for the coefficient of selection into the paid labor force is positive and significantly nonnegative at the 10% (but not at the standard 5%) level and the point estimate for the coefficient of selection into the fixed effects subsample is significantly positive at the standard 5% level. Thus, these estimates suggest some possibility of sample selectivity for the females. But neither for males nor females does the inclusion of the Olsen selectivity control change substantially nor significantly the estimated impact of schooling on wage rates.[31] The contrast between the effects of controlling for selectivity and of controlling for unobserved household and community effects suggests that, at least in this and similar data sets, there may be some important selectivity occurring that controls based on observed household characteristics may be inadequate to represent.

The first and third columns in Tables 4 and the first and second columns in Table 5 provide OLS estimates of the wage rate and hours supplied to the paid labor force market for the full samples of paid labor force participants and for the subsamples used for the fixed effects estimates. The first columns in Tables 6 and 7 gives similar estimates for the subsamples for which there are two or more members of the relevant sex participating in the paid labor force. Comparison of the point estimates for a particular variable involving schooling for a given outcome (i.e., either the wage rate or the hours supplied to the paid labor market) for a given sex indicates that the estimation of the same relations for

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[29]We use age ten for this division because that is the age that the Indonesian Labor Force Survey uses as its lower bound for potential labor force participants.

[30]The number of individuals under ten years of age in the household arguably may be determined simultaneously with the paid labor force participation decisions. However if this variable is dropped from these estimates, no substantive changes occur in the other estimates of interest in this paper.

[31]Though the selection control for the fixed effects sample does result in noticeable increases in the schooling-vocational school and the schooling-diploma, and an apparent shift from nonsignificance to significance for the latter.

the different subsamples does not result in estimates that differ significantly at standard levels.[32]

3. Hausman m specification tests suggest that the fixed effects estimates are preferable to the random effects estimates: Unobserved household and community effects are indicated in the relations to be estimated. These effects may be either fixed or random. As is well known, if these effects are fixed and correlated with included right-side observed variables, failure to control for them causes omitted variable biases in the coefficient estimates for the variables with which they are correlated. We give such an interpretation in the discussion in Section 3. As also is well known, if these effects are random, they cause biases in the estimated standard errors and therefore nullify standard statistical tests. We compare fixed effects and random effects estimates for the subset of individuals coming from two-male wage receiver households and from two-female wage receiver households. We use Hausman (1978) m-statistic specification tests to test the fixed effects versus the random effects specifications. This test indicates that the fixed effects specification is preferred for males and for females, for the ln wage rate and for the hours supplied to the paid labor market relations, and whether or not there is selectivity control for the random effects estimates (see Tables 6 and 7). On a priori grounds this result is not very surprising to us since there would seem to be important unobserved household and community effects that are correlated with the right-side variables in the relations of interest, but the random effects estimates assume that the unobserved effects are orthogonal to the included right-side variables.[33]

5. CONCLUSIONS

Schooling investments, particularly in primary schooling, are claimed to have high rates of return in developing countries by the World Bank (1980, 1981, 1990), Colclough (1982), Eisemon (1988), Psacharopoulos (1985, 1988), and many others. Such estimates have been questioned by some because of the failure to control for factors such as schooling quality, ability, motivation, role models, and repetition and dropout rates. Yet there have not been previous efforts, to our knowledge, to control for all of the unobserved household and community effects. The failure to control for these may affect substantially the estimates of schooling impacts on paid labor market outcomes.[34]

We compare OLS and fixed-effects estimates for ln wage and hours worked relations in the paid labor force in Indonesia. The results are striking. The OLS estimates of the impact of schooling appear have substantial biases that result in (1) an overall overestimate of the returns to school, (2) an overestimate of the relative returns to the lower schooling levels, (3) an overestimate of the relative

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[32]Though, particularly for females for the hours supplied to the paid labor market, there are some noticeable, if not statistically significant at standard levels, changes in the magnitudes of estimated coefficients.

[33]Hausman (1978: 1269) reports a similar finding and comments: "...this finding may be quite general [, and that]...the uncorrelated random effects model is not well suited to many econometric applications. The two requirements of exchangeability and orthogonality are not likely to be met in many applied problems."

[34]Though, as noted in Section 3, there still may be biases due to our inability to control for unobserved individual effects, though the control for household fixed effects may capture much of what sometimes is attributed to individual effects.

returns to schooling for males relative to females, and (4) an overestimate of the positive impact of schooling on hours supplied to the paid labor market. If such results hold for other societies, therefore, advocates for schooling investments based on the standard estimates may create unrealistic expectations regarding the positive impact of schooling on wage rates and on productivity, overemphasize the gains to be expected from investments in primary as opposed to higher schooling levels, and underestimate the economic returns to investing in female versus male schooling. Estimates that controlled for usually unobserved household and community characteristics would provide a better bases for positive analysis of the impact of schooling investments and for analyzing related policy issues.

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Table 1. Estimated annual rates of return to schooling in Indonesia[a]

		Private Rates of Return			Social Rates of Return		
Year	Source	Primary	Secondary	Higher	Primary	Secondary	Higher
1976	Payaman (1981)[b]		14.5%	19.7%			
1977	Psacharopoulos (1982)	25.5%	15.6%				
1978	Psacharopoulos (1982)				21.9%	16.2%	14.8%
1978	Clark (1983)[c]					25.0%	
1982	IEES (1986)[d] -male				19%	22.3%	10%
	-female				17%	10.5%	10%
1986	McMahon and Boediono (1988)[e] -male				9%	15%	16%
	-female				19%	21%	15%

a. Based on summary and estimates in IEES (1986) except for the 1986 estimates that are from McMahon and Boediono (1988). The social rates of return incorporate public costs in addition to the private costs (a major component of which is time costs) that are incorporated in the private rates of return.

b. The higher education figure is the unweighted average of the figures for a BA and a MA.

c. This is the unweighted average for SMA and STM.

d. All occupation averages (including self employed, farmers, and homemakers as well as employees). Secondary in the unweighted average for four types of junior and secondary schooling. Higher is the unweighted average of the two types of schooling.

e. Urban employees only. Primary in the unweighted average of some primary and completed primary. Secondary is the unweighted average of four types of secondary schooling. Higher is the unweighted average for two types of schooling.

Table 2
Means and Standard Deviations: Sakernas Sample, Indonesia, 1986

Variable	Mean for Males	Mean for Females
Average hourly wage rate	741 (14,336)	400 (9,241)
Age in years	33.8 (11.25)	31.0 (12.07)
Proportion of individuals having completed:		
Subprimary schooling	0.178 (0.383)	0.223 (0.416)
Primary schooling	0.298 (0.457)	0.222 (0.416)
General Junior High School	0.116 (0.321)	0.056 (0.230)
Vocational Junior High School	0.028 (0.166)	0.013 (0.112)
General Senior High School	0.122 (0.327)	0.077 (0.266)
Vocational Senior High School	0.137 (0.344)	0.181 (0.385)
Diploma 1, 2	0.010 (0.102)	0.013 (0.114)
Diploma 3	0.026 (0.160)	0.022 (0.148)
University	0.024 (0.152)	0.011 (0.105)

Notes: Standard deviations are in parentheses beneath means. The average hourly wage rates are in Rupiah. In 1986, 1,641 Rupiah equaled one U.S. dollar.

Table 3: Rates of Return to Schooling and Marginal Effects of Schooling on Market Labor Supply, Based on OLS and Household Fixed-Effects Estimates, Indonesia, 1986

RATES OF RETURN

Years of Schooling	MALES OLS	MALES Fixed effects	MALES % upward bias in OLS	FEMALES OLS	FEMALES Fixed effects	FEMALES % upward bias in OLS
3	6.4%	3.2%	98.9%	6.7%	5.3%	26.9%
6	7.8%	5.2%	48.9%	10.0%	9.8%	2.3%
9	9.2%	7.3%	27.0%	13.3%	14.3%	-6.8%
12	10.7%	9.3%	14.6%	16.6%	18.8%	-11.5%

MARGINAL EFFECTS ON MARKET LABOR SUPPLY

Years of Schooling	MALES OLS	MALES Fixed effects	MALES % upward bias in OLS	FEMALES OLS	FEMALES Fixed effects	FEMALES % upward bias in OLS
3	0.27	0.02	1025.7%	0.36	-0.43	-184.0%
6	-0.11	-0.27	-60.2%	-0.10	-0.89	-88.2%
9	-0.48	-0.55	-13.8%	-0.57	-1.36	-58.2%
12	-0.85	-0.84	0.8%	-1.03	-1.82	-43.5%

Notes: All figures are calculated from estimates reported in Tables 4 and 5. The OLS estimates used are those reported in the "OLS on Fixed-Effects Sample" column.

Table 4: OLS, Selectivity-corrected OLS, and Fixed-Effects Estimates of Ln Wage Equations, Indonesia, 1986

MALES

Independent Variable	Full-sample OLS Parameter	Full-sample OLS T Ratio	Full-sample OLS with selectivity Parameter	Full-sample OLS with selectivity T Ratio	OLS on fixed-effects sample Parameter	OLS on fixed-effects sample T Ratio	Fixed-effects Parameter	Fixed-effects T Ratio
Intercept	4.462	90.1	4.316	29.9	4.396	82.7		
Age	0.046	19.0	0.053	7.5	0.048	18.6	0.036	11.2
School years	-0.036	-6.5	-0.037	-6.6	-0.029	-4.8	-0.070	-8.9
Age squared	-0.001	-19.8	-0.001	-8.2	-0.001	-19.0	0.000	-11.2
Schooling squared	0.003	10.2	0.003	9.3	0.002	8.7	0.003	9.1
Age * Schooling	0.002	23.8	0.002	23.8	0.002	21.5	0.002	18.3
Schooling * Vocational dummy	0.004	3.0	0.004	3.1	0.002	1.6	0.004	2.5
Schooling * Diploma dummy	-0.005	-2.5	-0.004	-1.7	-0.005	-2.6	0.001	0.4
Selectivity variable			-0.098	-1.1				
F-Ratio	1529.278		1338.274		1356.283		440.272	
R-Squared	0.330		0.330		0.360		0.220	
degrees of freedom	21,646		21,645		17,029		11,071	

FEMALES

Independent Variable	Full-sample OLS Parameter	Full-sample OLS T Ratio	Full-sample OLS with selectivity Parameter	Full-sample OLS with selectivity T Ratio	OLS on fixed-effects sample Parameter	OLS on fixed-effects sample T Ratio	Fixed-effects Parameter	Fixed-effects T Ratio
Intercept	4.288	61.1	4.658	20.5	4.197	52.1		
Age	0.023	6.4	0.010	1.1	0.025	6.0	0.009	1.6
School years	-0.047	-5.3	-0.041	-4.3	-0.039	-3.9	-0.078	-5.6
Age squared	0.000	-5.4	0.000	-0.8	0.000	-4.8	0.000	-0.9
Schooling squared	0.006	12.3	0.006	12.3	0.006	10.8	0.008	10.5
Age * Schooling	0.002	14.8	0.002	14.9	0.002	12.7	0.003	11.1
Schooling * Vocational dummy	0.023	10.5	0.017	4.0	0.024	10.4	0.013	3.7
Schooling * Diploma dummy	-0.001	-0.3	-0.009	-1.6	0.001	0.3	-0.006	-1.1
Selectivity variable			0.323	1.7				
F-Ratio	1008.007		882.569		889.195		246.382	
R-Squared	0.450		0.450		0.510		0.320	
degrees of freedom	8,589		8,588		5,977		3,677	
F-Test for Equivalence of Parameters Across Males and Females	251.1						70.6	

Table 5: OLS and Fixed-Effects Estimates of Weekly Hours Worked, Indonesia, 1986

MALES

Independent Variable	Full-sample OLS Parameter	Full-sample OLS T Ratio	OLS on fixed-effects sample Parameter	OLS on fixed-effects sample T Ratio	Fixed-effects Parameter	Fixed-effects T Ratio
Intercept	40.329	44.3	40.784	40.7		
Age	0.269	6.1	0.253	5.2	0.169	2.8
School years	0.649	6.4	0.628	5.6	0.125	0.8
Age squared	-0.004	-8.0	-0.004	-6.9	-0.004	-4.7
Schooling squared	-0.061	-12.9	-0.062	-11.9	-0.048	-6.9
Age * Schooling	0.001	0.5	0.002	0.8	0.006	2.5
Schooling * Vocational dummy	-0.234	-10.5	-0.220	-9.1	-0.113	-3.6
Schooling * Diploma dummy	-0.102	-2.9	-0.094	-2.5	0.002	0.0
F-Ratio	113.221		93.213		40.108	
R-Squared	0.040		0.040		0.020	
degrees of freedom	21,646		17,029		11,071	

FEMALES

Independent Variable	Full-sample OLS Parameter	Full-sample OLS T Ratio	OLS on fixed-effects sample Parameter	OLS on fixed-effects sample T Ratio	Fixed-effects Parameter	Fixed-effects T Ratio
Intercept	54.769	34.0	57.285	29.1		
Age	-0.663	-8.2	-0.676	-6.7	-0.202	-1.7
School years	0.509	2.5	0.295	1.2	0.268	0.9
Age squared	0.005	4.9	0.005	3.7	0.000	-0.2
Schooling squared	-0.074	-6.9	-0.077	-6.2	-0.078	-5.1
Age * Schooling	0.016	4.6	0.020	4.7	-0.007	-1.3
Schooling * Vocational dummy	-0.420	-8.5	-0.454	-8.0	-0.115	-1.5
Schooling * Diploma dummy	-0.117	-1.5	-0.117	-1.3	-0.043	-0.4
F-Ratio	97.966		82.782		56.012	
R-Squared	0.070		0.090		0.100	
degrees of freedom	8,589		5,977		3,677	
F-Test for Equivalence of Parameters Across Males and Females	81.2				18.2	

Table 6: OLS, Selectivity-corrected, Fixed-Effects and Random-effects estimates of Ln Wage Equations for All Individuals from Two-Salaried Member Households, Indonesia, 1986

MALES

Independent Variable	OLS w/o selectivity Parameter	T Ratio	OLS w/ selectivity Parameter	T Ratio	Fixed effects Parameter	T Ratio	Random effects w/o selectivity Parameter	T Ratio	Random effects w/ selectivity Parameter	T Ratio
Intercept	4.480	50.1	4.344	16.8			2.025	51.1	1.950	15.2
Age	0.045	10.3	0.051	4.1	0.020	3.0	0.033	7.3	0.042	2.8
School years	-0.029	-3.0	-0.029	-3.1	-0.112	-6.6	-0.073	-6.8	-0.074	-6.9
Age squared	-0.001	-11.2	-0.001	-4.5	0.000	-3.5	0.000	-8.0	-0.001	-3.2
Schooling squared	0.002	4.8	0.002	4.3	0.005	6.1	0.004	7.9	0.004	7.1
Age * Schooling	0.002	14.0	0.002	14.0	0.002	8.5	0.003	13.1	0.003	13.1
Schooling * Vocational dummy	0.005	2.7	0.006	2.6	0.003	0.8	0.005	2.0	0.005	2.0
Schooling * Diploma dummy	-0.006	-1.8	-0.005	-1.3	0.000	0.0	-0.004	-1.0	-0.002	-0.5
Selectivity variable			-0.092	-0.6					-0.124	-0.6
F-Ratio	559.223		489.312		51.895		278.334		243.568	
R-Squared	0.353		0.353		0.092		0.214		0.214	
Degrees of freedom	7,162		7,161		3,578		7,162		7,161	
Hausman's m statistic of misspecification							117.70		117.26	

FEMALES

Independent Variable	OLS w/o selectivity Parameter	T Ratio	OLS w/ selectivity Parameter	T Ratio	Fixed effects Parameter	T Ratio	Random effects w/o selectivity Parameter	T Ratio	Random effects w/ selectivity Parameter	T Ratio
Intercept	4.212	36.1	3.475	9.0			1.949	33.8	1.584	7.8
Age	0.025	4.3	0.051	3.6	0.003	0.3	0.015	2.3	0.044	2.6
School years	-0.027	-2.0	-0.039	-2.6	-0.072	-2.6	-0.048	-3.0	-0.062	-3.5
Age squared	0.000	-3.4	-0.001	-3.3	0.000	0.2	0.000	-1.5	0.000	-2.3
Schooling squared	0.005	7.0	0.005	7.0	0.008	5.5	0.006	7.6	0.006	7.6
Age * Schooling	0.002	8.8	0.002	8.5	0.002	4.0	0.002	7.5	0.002	7.3
Schooling * Vocational dummy	0.021	6.9	0.033	5.0	0.006	0.9	0.016	4.2	0.029	3.6
Schooling * Diploma dummy	0.003	0.6	0.020	2.0	-0.011	-1.1	-0.003	-0.5	0.016	1.4
Selectivity Variable			-0.644	-2.0					-0.737	-1.9
F-Ratio	490.479		430.097		41.603		252.452		221.510	
R-Squared	0.526		0.526		0.159		0.363		0.364	
Degrees of freedom	3,098		3,097		1,546		3,098		3,097	
Hausman's m statistic of misspecification							20.30		19.92	

Table 7: OLS, Fixed-Effects, and Random-effects Estimates of Weekly Hours Worked for All Individuals from Two-Salaried Member Households, Indonesia, 1986

MALES

Independent Variable	OLS		Fixed Effects		Random Effects	
	Parameter	T Ratio	Parameter	T Ratio	Parameter	T Ratio
Intercept	37.988	23.4			15.711	22.1
Age	0.380	4.8	0.435	3.6	0.414	5.0
School years	0.716	4.1	0.212	0.7	0.466	2.4
Age squared	-0.005	-5.6	-0.006	-4.2	-0.006	-5.9
Schooling squared	-0.061	-7.8	-0.043	-2.9	-0.052	-5.8
Age * Schooling	-0.002	-0.7	0.002	0.3	0.000	-0.1
Schooling * Vocational dummy	-0.279	-7.7	-0.106	-1.7	-0.194	-4.8
Schooling * Diploma dummy	-0.106	-1.9	-0.016	-0.1	-0.063	-1.0
F-Ratio	49.698		7.638		28.534	
R-Squared	0.046		0.015		0.027	
Degrees of freedom	7,162		3,578		7,162	
Hausman's m statistic of misspecification					3.0	

FEMALES

Independent Variable	OLS		Fixed Effects		Random Effects	
	Parameter	T Ratio	Parameter	T Ratio	Parameter	T Ratio
Intercept	53.463	20.2			17.172	18.7
Age	-0.583	-4.4	-0.170	-0.9	-0.313	-2.4
School years	0.753	2.4	0.454	0.9	0.457	1.4
Age squared	0.004	2.5	-0.001	-0.2	0.001	0.6
Schooling squared	-0.099	-6.0	-0.089	-3.5	-0.083	-4.8
Age * Schooling	0.017	3.1	0.002	0.2	0.009	1.6
Schooling * Vocational dummy	-0.458	-6.6	-0.048	-0.4	-0.232	-3.0
Schooling * Diploma dummy	-0.004	0.0	0.004	0.0	-0.032	-0.3
F-Ratio	45.767		13.532		30.898	
R-Squared	0.094		0.058		0.065	
Degrees of freedom	3,098		1,546		3,098	
Hausman's m statistic of misspecification					24.5	

Table 8: OLS Market Labor Force Participation Equations, Indonesia, 1986

Independent Variables	Females		Males	
	Parameter	T-Ratio	Parameter	T-Ratio
Intercept	-0.057	-5.7	-0.323	-45.7
Age	0.040	97.5	0.068	229.3
Age squared	0.000	-104.6	-0.001	-219.5
Schooling years	-0.019	-9.8	-0.006	-4.6
Schooling squared	0.000	-0.4	-0.001	-13.7
Age X Schooling	0.000	-0.4	0.000	8.1
No. of hh. members under 10	-0.013	-20.6	-0.023	-45.0
No. of hh. members over 10	-0.016	-17.0	0.003	4.2
Schooling X Vocational dummy	0.018	23.9	0.007	15.3
Schooling X Diploma dummy	0.025	15.8	0.010	12.0
F-Ratio	2,167		10,387	
R-Squared	0.14		0.45	
No. of obs.	116,523		114,093	

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