Written Exam for the M.Sc. in Economics Summer 2010

Advanced Development Economics: Micro Aspects

Final Exam

Date 31 May 2010

(3-hour closed book exam)

Please note that the language used in your exam paper must correspond to the language of the title for which you registered during exam registration. I.e. if you registered for the English title of the course, you must write your exam paper in English. Likewise, if you registered for the Danish title of the course or if you registered for the English title which was followed by “eksamen på dansk” in brackets, you must write your exam paper in Danish.

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Question 1:

The questions below refer to the analysis and results in Duflo (2001), "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment", *American Economic Review*, 91(4), 795-813. Between 1973 and 1978, the Indonesian government engaged in one of the largest school construction programs on record. Duflo (2001) evaluate this effect of building schools on education and earnings in Indonesia.

(a) Using Table 3, outline the basic idea behind the identification strategy followed in Duflo (2001).

TABLE 3—MEANS OF EDUCATION AND LOG(WAGE) BY COHORT AND LEVEL OF PROGRAM CELLS

	Years of education			Log(wages)		
	Level of program in region of birth			Level of program in region of birth		
	High (1)	Low (2)	Difference (3)	High (4)	Low (5)	Difference (6)
Panel A: Experiment of Interest						
Aged 2 to 6 in 1974	8.49	9.76	−1.27	6.61	6.73	−0.12
	(0.043)	(0.037)	(0.057)	(0.0078)	(0.0064)	(0.010)
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Difference	0.47	0.36	0.12	−0.26	−0.29	0.026
	(0.070)	(0.038)	(0.089)	(0.011)	(0.0096)	(0.015)
Panel B: Control Experiment						
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Aged 18 to 24 in 1974	7.70	9.12	−1.42	6.92	7.08	−0.16
	(0.059)	(0.044)	(0.072)	(0.0097)	(0.0076)	(0.012)
Difference	0.32	0.28	0.034	0.056	0.063	0.0070
	(0.080)	(0.061)	(0.098)	(0.013)	(0.010)	(0.016)

Notes: The sample is made of the individuals who earn a wage. Standard errors are in parentheses.

The basic idea behind the identification strategy can be illustrated using the simple two-by-two table (Table 3), which shows means of education and wages for different cohorts and program levels. Regions are separated in "high program" and "low program" regions. Panel A compares the educational attainment and the wages of individuals who had little or no exposure to the program (they were 12 to 17 in 1974) to those of individuals who were exposed the entire time they were in primary school (they were 2 to 6 in 1974), in both types of regions. In both cohorts, the average educational attainment and wages in regions that received fewer schools are higher than in regions that received more schools. This reflects the program provision that more schools were to be built in regions where enrollment rates were low. In both types of regions, average educational attainment increased over time. However, it increased more in regions that received more schools. The difference in these differences can be interpreted as the causal effect of the program, under the assumption that in the absence of the program, the increase in educational attainment would not have been systematically different in low and high program regions. The identification assumption should not be taken for granted. However, an implication of the identification assumption can be tested because individuals aged 12 or older in 1974 were not exposed to the program. The increase in education between cohorts in this age-group should not differ systematically across regions. Table 3,

panel B, presents this control experiment, by considering a cohort aged 18 to 24 in 1974 and a cohort aged 12 to 17 in 1974. The estimated differences in differences are very close to 0. These results provide some suggestive evidence that the differences in differences are not driven by inappropriate identification assumptions, although they are imprecisely estimated. In panel B, for example, the differences in differences are insignificantly different from 0 but also from the differences in differences in panel A.

Using Table 4, discuss the problem omitted time- varying and region-specific effects and explain how Duflo (2001) can conclude that the estimates are not upwardly biased by mean reversion or omitted programs.

TABLE 4—EFFECT OF THE PROGRAM ON EDUCATION AND WAGES: COEFFICIENTS OF THE INTERACTIONS BETWEEN COHORT DUMMIES AND THE NUMBER OF SCHOOLS CONSTRUCTED PER 1,000 CHILDREN IN THE REGION OF BIRTH

		Dependent variable					
		Years of education			Log(hourly wage)		
	Observations	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest: Individuals Aged 2 to 6 or 12 to 17 in 1974 (Youngest cohort: Individuals ages 2 to 6 in 1974)							
Whole sample	78,470	0.124	0.15	0.188			
		(0.0250)	(0.0260)	(0.0289)			
Sample of wage earners	31,061	0.196	0.199	0.259	0.0147	0.0172	0.0270
		(0.0424)	(0.0429)	(0.0499)	(0.00729)	(0.00737)	(0.00850)
Panel B: Control Experiment: Individuals Aged 12 to 24 in 1974 (Youngest cohort: Individuals ages 12 to 17 in 1974)							
Whole sample	78,488	0.0093	0.0176	0.0075			
		(0.0260)	(0.0271)	(0.0297)			
Sample of wage earners	30,225	0.012	0.024	0.079	0.0031	0.00399	0.0144
		(0.0474)	(0.0481)	(0.0555)	(0.00798)	(0.00809)	(0.00915)
Control variables:							
Year of birth*enrollment rate in 1971		No	Yes	Yes	No	Yes	Yes
Year of birth*water and sanitation program		No	No	Yes	No	No	Yes

Notes: All specifications include region of birth dummies, year of birth dummies, and interactions between the year of birth dummies and the number of children in the region of birth (in 1971). The number of observations listed applies to the specification in columns (1) and (4). Standard errors are in parentheses.

To exploit the variation in treatment intensity across regions and cohorts, this strategy can be generalized to a regression framework. Consider first the difference between the average education of a young cohort exposed to the program and that of an older cohort not exposed to the program. If additional schools led to an increase in educational attainment, the difference will be positively related to the number of schools constructed in each region. Table 4 (columns 1-3) presents estimates of this exercise. Panel A compares children aged 2 to 6 in 1974 with children aged 12 to 17 in 1974. In column 1, the specification controls only for the interaction of a cohort of birth dummy and the population aged 5 to 14 in 1971. The suggested effect is that one school built per 1,000 children in- creased the education of the children aged 2 to 6 in 1974 by 0.12 years for the whole sample, and by 0.20 years for the sample of wage earners.

This interpretation relies on the identification assumption that there are no omitted time-varying and region-specific effects correlated with the program. The allocation of schools to each region was an explicit function of the enrollment rate in the region in 1972. Therefore, the estimate could potentially confound the effect of the program with mean reversion that would have taken place even in its absence. The identification assumption will also be violated if the allocation of other governmental programs initiated as a result of the oil boom (and potentially affecting education) was correlated with the allocation of schools.

Duflo (2001) present specifications that control for the interactions between cohort dummies and the enrollment rate in the population in 1971, as well as for interactions between cohort dummies and the allocation of the water and sanitation program, the second largest program centrally administered at the time. Controlling for both the enrollment rate and the water and sanitation program makes the estimates higher (columns 2 and 3), suggesting that the estimates are not upwardly biased by mean reversion or omitted programs.

Panel B of Table 4 shows the results of the control experiment (comparing the cohort aged 12 to 17 to the cohort aged 18 to 24 in 1974). If, before the program was started, education had increased faster in regions that received more schools, panel B would show (spurious) positive coefficients. But the impact of the "program" is very small and never significant. The coefficients are statistically different from the corresponding coefficients in panel A. Although this is not definitive evidence (education level could have started converging precisely after 1973), it is reassuring.

Even if the identification assumption is satisfied, the coefficient may slightly overestimate the effect of the program on average education. Note that such a large program could potentially have affected the returns to education by increasing the stock of primary school graduates. Individuals' education choices could then have responded to this decrease in the returns to education. To the extent that Indonesia is an integrated labor market, the returns to education would have declined in the entire country. The estimates do not take this negative effect of the program into account because it is common to all regions. This effect, however, is not likely to be very large. Its size ultimately depends on the elasticity of the demand for educated labor (which is likely to be low in a rapidly growing economy), the sensitivity of educational choice to perceived returns to education, and the extent of integration in the Indonesian labor market.

Based on Figure 1 explain why the identification strategy in Duflo(2001) is considered reasonable.

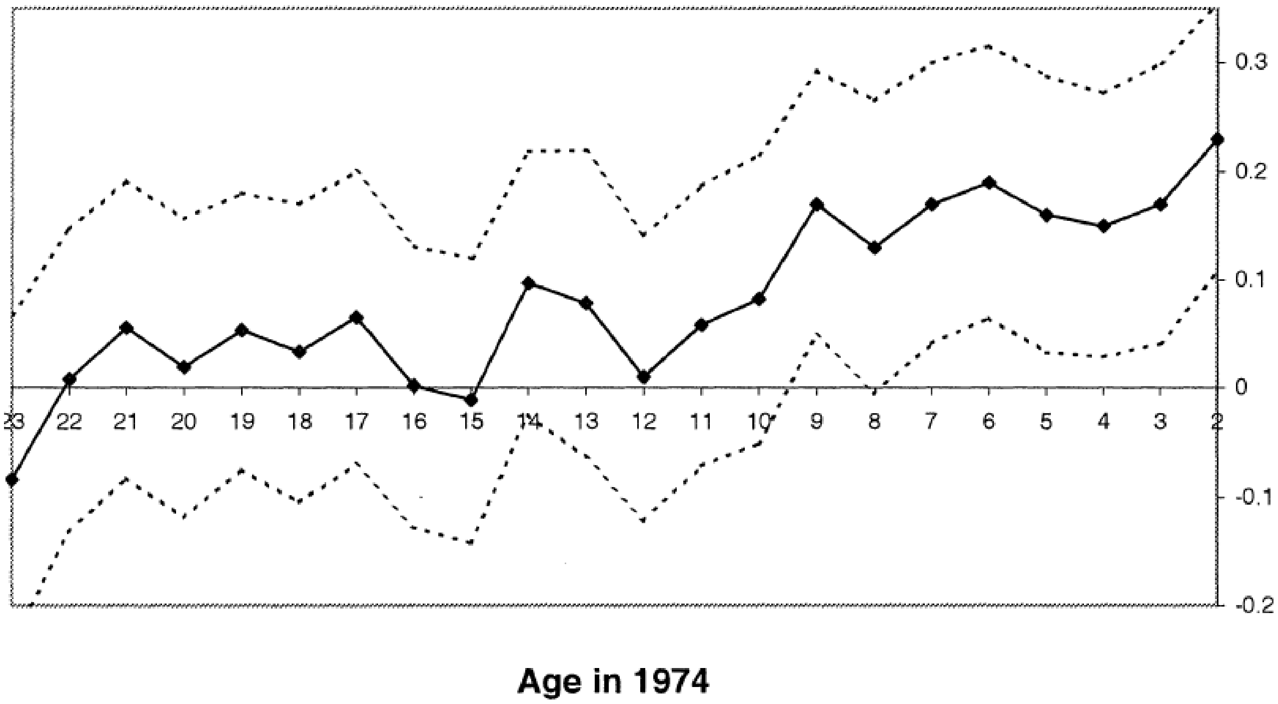

FIGURE 1. COEFFICIENTS OF THE INTERACTIONS AGE IN 1974* PROGRAM INTENSITY IN THE REGION OF BIRTH IN THE EDUCATION EQUATION

The identification strategy can be generalized to an <u>interaction terms analysis</u>, measuring the time dimension of exposure to the program with 22 year-of-birth dummies. Individuals aged 24 in 1974 form the control group, and this dummy is omitted from the regression. Each coefficient can be interpreted as an estimate of the impact of the program on a given cohort. There is a testable restriction on the pattern of the coefficients. Because children aged 13 and older in 1974 did not benefit from the program, the coefficients should be 0 for *l* > 12 and start increasing for *l* smaller than some threshold (the oldest age at which an individual could have been exposed to the program and still benefit from it). Figure 1 plots the coefficients. Each dot on the solid line is the coefficient of the interaction between a dummy for being a given age in 1974 and the number of schools constructed per 1,000 children in the region of birth (a 95-percent confidence interval is plotted by broken lines). These coefficients fluctuate around 0 until age 12 and start increasing after age 12. As expected, the program had no effect on the education of cohorts not exposed to it, and it had a positive effect on the education of younger cohorts. All coefficients are significantly different from 0 after age 8. These figures show that the identification strategy is reasonable and that the program had an effect on education.

Question 2:

Consider a rural credit market where borrowers and lenders are risk neutral. Each individual in a village has access to the same amount of land, and can farm this land at a fixed cost (equal to 1). The farm yields 0 if there is harvest failure, and *R>1* otherwise. The probability of a successful farming season is $\pi(e)$, where *e* represents effort of the farmer. $\pi(e)$ is strictly increasing and concave. The utility cost to the farmer of working is given by *D(e)*, which is increasing and strictly convex. Assume no land market (no wealth), and the farmer therefore has to borrow the necessary working capital. If a lender offers an interest factor of $i \leq R$, the returns to the farmer and lender are as follows:

	Borrower	Lender
Success	*R-i-D(e)*	*i*
Failure	*D(e)*	0

Assume that lenders have access to a risk-free capital market with a return (ρ) of $R > \rho \geq 1$. Assume also that, if the borrower does not involve herself in farming, she can receive a return (W) of $R > W \geq 0$ in alternative employment. Based on the above we have that;

The expected utility of a borrower: $U(i,e) = \pi(e)(R-i) - D(e)$

The expected utility of a lender: $\Pi(i,e) = \pi(e)i$

(a) In compiling the above table two assumptions are made. Describe and discuss these assumptions.

All questions should be answered using Chapter 7 in Bardhan and Udry (1999).

1) The loan contract has limited liability: If the borrower's harvest fails, she has no funds to repay the loan and the lender receives nothing.

2) No problems of enforcement: If the harvest is successful, the borrower cannot renege on her commitment to repay the loan if the project is successful.

A very good answer will include a discussion about these assumptions being to simplified (page 79-80 in Bardhan and Udry, 1999)

(b) Assume that a lender cannot observe the input of effort by the borrower. Describe in context of the above model setting the consequences of this moral hazard problem.

We are in model (ii) in Bardhan and Udry (1999): Competitive equilibrium and moral hazard. The perfect answer follows the description in page 80-85 in Bardhan and Udry (1999) including Figure 7.1.

(c) Explain and illustrate graphically how the equilibrium in (b) compares with the (i) Competitive equilibrium with complete markets, (ii) Equilibrium with a fully informed monopolist, and (iii) Equilibrium where there is competition between an informed local moneylender and uninformed outside lenders.

The perfect answer follows the description in page 80-85 in Bardhan and Udry (1999) including Figure 7.1.

(d) Outline an example of how the consequences of moral hazard can be neutralized.

The consequences of moral hazard in the credit market can be neutralized by the use of collateral, when both borrowers and lenders are risk-neutral. Suppose that each borrower owns some assets with value greater than *R*. If the project fails, the borrower transfers the collateral pledged for the loan to the lender. The borrower absorbs the entire risk of the transaction, and the return to the lender no longer depends on the choice of effort by the borrower (collateral removes the moral hazard problem). The loan is now riskless to the lender, so the interest rate is at the level of the riskless rate. Borrowers are induced to put the optimal effort into the project. Lenders make zero profits, and borrowers achieve the same utility as they achieve in the complete info equilibrium. The result depends crucially on the assumed risk neutrality of both parties. If the borrower were risk-averse the use of collateral could not entirely alleviate the difficulities induced by moral hazard, because the borrower would not be willing to absorb the entire risk of the transaction without some compensation from the lender.

Question 3:

Technological changes in developing countries are often characterized as a process where a given technology is invented in developed countries and then imported and adapted for local use. However, technological change is often more complex than transfers of blueprints and machines from rich countries.

Questions can be answered using Chapter 12 in Bardhan and Udry (1999) and/or Foster and Rosenzweig (1995)

a) Explain why the value of international technology spillovers may be limited and define the concept of social learning.

   Technological change in developing countries often characterized as a process where a given technology is invented in developed countries and then imported and adapted for local use. However, technological change often more complex than transfers of blueprints and machines from rich countries. Techniques of production are often characterized as being tacit and circumstantial sensitive (CS): Apparently identical techniques of production used quite differently across producers and non-tradable inputs (land) vary in characteristics in ways that affect the performance of different technologies. The "tacitness" and CS limit the value of international spillovers. When technology is tacit or circumstantial sensitive local investment in learning and innovation must take place.

   a. Learning-by-doing (uncertainty)
   b. Learning from others (uncertainty + information spillovers)

   Learning-by-doing + Learning from others = "Social learning".

b) Describe the "Target Input" model of social learning and explain the testable implication of this model.

   The profit of a producer declines with the distance between the input used and the unknown target level of input use. After input has been applied and output realized, the producer can deduce the target level of inputs. Each application of technology is an experiment which yields information about the distribution of the unknown random parameter. Inputs are costless = output equals profits. Model described in detail in section II in Chapter 12 of Bardhan and Udry (1999) – pages 154-157.

c) According to the model, how may many expected neighbor technology adoptions affect own adoption?

   Assume "traditional" technology with riskless return q(a). Let μ=1 if new technology and μ=0 otherwise. The value of future profits are given by:

   $$V(I_{t-1}, N_{t-1}) = \max(1-\mu_t)q_a + \mu_t E_t q_t(I_{t-1}, N_{t-1}) + \delta V_{t+1}(I_t, N_t)$$

   The farmers expectations about the neighbors use of the new technology have direct effect on the value of the flow of profits expected by the farmer. The higher the expectation of neighbor experiments, the higher the expected profit. Many expected neighbor adoptions may delay own adoption as the value of the information received from experimenting with technology is lower the more other farmers experiment.

   This can be seen from:

   $$q_0 - E_0 q(0, N_0) \le \delta\left[V_1(1, N_0) - V_1(0, N_0)\right]$$

   LHS: "Expected" profit of sticking to traditional technique.

   RHS: Expected increase in profits from experimenting.

RHS: Decreasing in N(0). If more farmers use new technology, less additional information is gained by own experiments.

The spread of technology depends on social interactions in the following ways:

Direct effect: Information flow within community.

Indirect effect: Externality.

Game effect: Pattern of adoption depends on the nature of the game that is played within the community.

d) How is the relative usefulness of information gathered from own and neighbor technology experiments, respectively? Is this assumption plausible? How does unobserved farmer characteristics affect estimation of different types of learning? (Hint: Discussion of consistency versus efficiency).

The simple target input model illustrates two important characteristics of innovation in the context of social learning: First, innovation is a form of investment. Producers experiment, sacrificing current profits for knowledge which will improve future profits. Second, innovation generates spillovers. Technology is non-rival – If one learns about the new technology others can also benefit from that knowledge. In the model information from a neighbor is therefore as useful as the information that the grower receives himself. However, the fact that the technology worked well for a neighbor does not necessarily imply that it will work well for you. Simple rule of thumb: Individuals learn from similar neighbors only, a fact that generally slows down the rate of diffusion. By conditioning for differences between his own and his neighbors' observed characteristics when learning from them the farmer can improve performance. BUT what about unobserved characteristics? The prospects for social learning decline. Consistent but inefficient estimates of the expected yield would be obtained with *individual* learning. Efficient estimates with *social* learning. More information is being utilized, but BIAS because the farmer cannot control for unobserved characteristics (affecting yield) of neighbors. Therefore trade-off between bias and efficiency when choosing between individual or social learning. Testable prediction: Farmer choose individual learning if the population is heterogeneous and yield is sufficiently sensitive to unobserved characteristics, otherwise social learning preferred.