What do households respond to?

Esther Duflo

14.771

Do perceived returns affect education?

- Recall that

$$(\gamma w_t^H + (1-\gamma) w_t^L) s_1(h_{t+1}, h_t) =$$
$$\frac{1}{r_t}[w_{t+1}^H - (\gamma w_{t+1}^H + (1-\gamma) w_{t+1}^L) s_2(h_{t+2}, h_{t+1})].$$

- In this model, it is people's beliefs about the net returns that affect human capital investment:
  - costs
  - benefits
- How could we test this?

Do households respond to the cost of education?

- Direct costs: Uniform (Duflo, Dupas, Kremer, 2015), scholarships (Duflo, Dupas, Kremer, 2021)
- Indirect costs: Literature on Conditional Cash transfer, Distance to school.
- Duflo, 2001: Evidence on school construction–Standard Example of a difference in difference policy evaluation, which we will see plenty of.

Simplest setting:

- Individual i belong to one of groups $G = 1$, treated group, $G = 0$, non treated group.
- and is observed in one of two periods (or cohorts) $T = 1$ (post) and $T = 0$ (pre).
- Group $G = 1$ is treated when $T = 1$, not when $T = 0$.
- Identification Assumption: Potential outcome $Y_i(0)$ can be written:

$$Y_i(0) = \alpha + \beta T_i + \gamma G_i + \epsilon_i$$

  with $\epsilon_i \perp (T, G)$, i.e. ϵ_i is independent of the group indicator and its distribution does not change over time.
- What is the key identification assumption?

$$\tau_{DID} = (E[Y_i|G=1, T=1) - E[Y_i|G=1, T=0])$$
$$-((E[Y_i|G=0, T=1) - E[Y_i|G=0, T=0]))$$

Sample equivalent:

- Replace expectation by population averages:

$$\tau_{DID} = (\overline{Y_{11}} - \overline{Y_{10}}) - (\overline{Y_{01}} - \overline{Y_{00}})$$

  where $\overline{Y_{gt}} = \frac{1}{N_{gt}} \sum_{G_i=g, T_i=t} Y_i$

- Or equivalently estimate OLS on

$$Y_i = \alpha_1 + \beta_1 T_i + \gamma_1 G_i + \tau_{DID}(T_i * G_i) + \epsilon_i$$

Example: The impact of school building on education and earnings (Duflo,2001)

- Set-up:
  - Relatively swift school building construction campaign, financed by oil boom (1973)
  - Intensity of treatment depends on pre-campaign enrollment.
- Diff in Diff
  - Definition of treated and control cohorts
  - 12 or younger in 1973: treated.
  - Definition of treated and control regions
  - Program intensity below/above median
  - Results: Basic DID
- Testing the identification assumption
  - Old versus very Old Placebo experiment

Extension: Continuous treatment intensity across groups

- Suppose that we in fact have G groups and that the intensity of the treatment depend on the group. We can think about this as if it were several treatments: $Y_i(w)$, for $w = 0, 1, 2, G$.
- Alternatively, the treatment could take continous or discrete values, as in our case (number of schools): we control for district of birth dummies, and we interact post*number of schools per (1000) kids.
- With only two cohorts:

$$Y_i = \alpha + \beta T_t + \sum_{g=1}^{G} \gamma 1[G_i = g] + \tau_C(S_g * T_t) + \epsilon_i$$

Table

- with multiple periods, but still one "pre" and one "post" period, replace T_t by year of birth dummies.

$$Y_i = \alpha + \sum_{t=1}^{T} \beta_t 1[T_i = t] + \sum_{g=1}^{G} \gamma_g 1[G_i = g] + \tau(S_g * POST) + \epsilon_i$$

Extension: variable treatment intensity across periods

- Equivalent to have several treatments W_i^t, where W_i^t is equal to 1 for treated groups in year t

$$Y_i = \alpha + \sum_{t=1}^{T} \beta_t 1[T_i = t] + \sum_{g=1} \gamma 1[G_i = g] + \sum_{t=2}^{T} \tau_t W_i^t + \epsilon_i$$

(alternatively: compute a series of DID relative to one base period)

- Combine with different treatment intensity across groups:

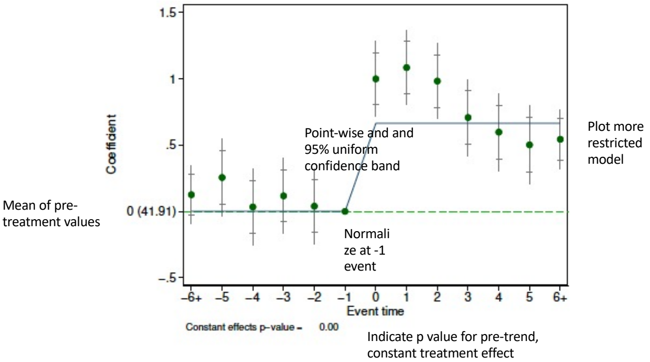
$$Y_i = \alpha + \sum_{t=1}^{T} \beta_t^T 1[T_i = t] + \sum_{g=1}^{G} \gamma_g 1[G_i = g] + \sum_{t=2}^{T} \tau_{Ct}(M_g * T_t) + \epsilon_i$$

Specification check:

- the treatment effect should follow the pattern of the extension of the program. It should be be 0 for all the periods before the treatment starts; it should equal for all periods where the treatment intensity was the same.
- In the INPRES case, exposure depends on cohort of birth in a specific way
- We get this for the Graph coefficients: encouraging?
- Now e can force the earlier cohort to have zero treatment effect Table

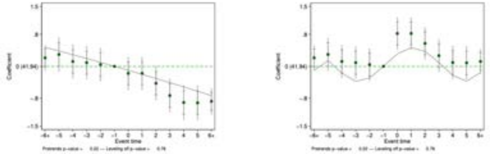
Econometrics issues with Diff in Diff

- Difference in difference/2 ways fixed effects have become very popular in applied economics and you will see a bunch in this class and elsewhere
- There is an active literature on how to do it "right"
- A few issues to keep in mind
  - Standard errors (Bertrand, Duflo, Mullainathan, 2004). At what level must we cluster them?
  - Staggered Design (Goodman-Bacon, 2020): if a reform is implemented differently at different time, one must "stack" the data appropriately [with respect to an "event" at zero]
  - More formal tests of pre-trends: Freyaldenhoven, Hansen and Shapiro (2019)
  - Best practice for event study graphs : Freyaldenhoven, Hansen, Perez Perez and Shapiro (2021)
  - Heterogenous treatment effect: with treatment effects that are different for different units or different time period, there is a risk for bias (due to negative weight placed on some of the DD). De Chaisemartin and D'Hautefeuille (2020), Sun and Abraham (2020), Boryusak, Jaravel, Spiess (2021)


1.5
1
.5
0 (41.91)
-.5
Coefficient
-6+
-5
-4
-3
-2
-1
0
1
2
3
4
5
6+
Event time
Constant effects p-value = 0.00
Point-wise and and 95% uniform confidence band
Normalize at -1 event
Plot more restricted model
Mean of pre-treatment values
Indicate p value for pre-trend, constant treatment effect

Go Back

Shapiro et al. event study graph suggestions



(a) "Smooth" event-time trend (b) "Jump" at the time of the event

Figure 6: Least "wiggly" path of confound. Exemplary event-study plot for two possible datasets. Relative to Figure 5, a curve has been added that illustrates the least "wiggly" confound that is consistent with the event-time path of the outcome, in accordance with Suggestion 6.

Do perceived returns affect education?

- Jensen 2010: The (Perceived) Returns to Education and the Demand for Schooling
- Setting:
  - Dominican Republic. 85% completion rate for primary school (up to 8th grade) but only 30% completion rate for high school
  - Students asked at baseline about perceived wages for those who had completed primary school vs. those who had completed high school
  - At baseline, perceived returns were about 2% per year (9% increase in wages from moving from 8th grade to 12th grade)
- Randomized experiment:
  - Students at random set of schools were simply informed about the "true" Mincerian returns (10% per year, so 40% for going from 8th grade to high school)
  - Shows that they update their beliefs
  - Then investigates the impact on education
  - Table

- Increased average education by 0.20 years of schooling
- Bigger effect for wealthier students, no effect for poor. Suggests credit constraints / cost of schooling may also be a factor
- Nguyen 2008 finds similar effects for primary school students in Madagascar, and find more specific evidence of a response to perceived returns:
    - Beliefs are correct on average
    - However those who overestimate reduce effort (results on test)
    - And those who underestimate increase effort

What about the actual returns to education?

- Foster and Rosenzweig (1995): HYV increases returns to education, which in turns increases education. [Problem: there may be a direct income effect on the family when their yield goes up]
- Atkin (2009) and Hernandez (2015), finds that export firm growth in Mexico and flower jobs in Colombia leads to more school dropout among girls. These are non-educated jobs for women, so idea is that this reduces the wage premium (and may also increase opportunity costs!)

- How can you modify the *actual* returns to education?
- Great idea: recruiting campaign for call centers (when they were relatively new) within 50km-150km of Delhi.
- "Our intervention provided three years of BPO recruiting services to women in randomly selected rural villages. By connecting the villages to experienced recruiters, the interventionwas designed to increase awareness of and access to BPO jobs, and thus in effect increase employment opportunities forwomen."
- 80 treatment villages, 80 control villages
- In treatment villages, recruiters conducted information sessions where the advertised BPO jobs (for young, unmarried women with at least high school education), and a booster shot 1-2 years later.
- Survey before and after, main group is 15-21 years old, also look at education decisions for younger kids.

Results

- "First stage": Women more likely to work in a BPO. ▸ table
- Human capital: Increase in education and health ▸ table
- Collateral benefit: Decline in age at marriage and fertility ▸ table

Does life span affect education?

- Returns are experienced over a life time
- So if this life time is longer, it is more valuable to invest!
- How would we test this?

Does life span affect education?

Jayachandran and Lleras-Muney (2010): Life Expectancy and Human Capital Investments: Evidence from Maternal Mortality Declines

- Empirical challenge:
  - Need an instrument that affects T – lifespan – without affecting wage premium or low skill wage
  - This is hard problem!
- Their idea:
  - Reductions in maternal mortality in Sri Lanka from the introduction of ambulances, which allow moms with at-risk childbirths to be rushed to hospitals
  - Introduced differentially across districts across time Figure
  - Key empirical advantage: this increases life expectancy T for girls, but not for boys
  - So they can use boys as a control group
  - This is therefore a "triple-difference": districts, time, and gender

Does life span affect education?

- Focus on those who are aged -2 to 11 in 1946 (so age $5-19$ in 1953), since they are young enough to respond to MMR declines, old enough so that literacy is observed in 1953
- Empirical specification:

$$\begin{aligned} literacy_{atdg} &= \beta_0 + \beta_1 LaggedMMR_{dt} \times female_g + \\ &\quad \mu_{dg} + \gamma_{dt} + \nu_{gt} + \gamma_{ga} + \theta_{ta} + \varepsilon_{adgt} \end{aligned}$$

- What is identifying this regression?
- Results: Life expectancy literacy

- Estimate that MMR fell by 70% between 1946-1953. Increased female life expectancy by 1.5 years on average (4.1% increase in life expectancy conditional on being age 15).
- Caused literacy to increase by 1 percentage point (2.5%) and schooling to increase by 0.2 years (4%)

What if parents miss-perceive costs and returns?

- People have completely distorted view on returns: Overestimate returns to secondary, underestimate returns to primary
  ▸ real and perceived occupation
- And ability as well
  Rebecca Dizon-Ross (2018) designed a very clever experiment and data collection method to show that parents have distorted beliefs about their children's ability (or even how well they are doing in school: a less fundamental measure of ability), and that this affects their investment decisions.

  The experiment takes place in Malawi.

  Basic experimental design: Select 3,464 households with at least 2 school age children, and select 2 school age children per family. Select half of those families randomly (the treatment group), and provide to the treatment group information about their children's achievement: the school report card, explained in detail.

Data collection: How to elicit beliefs and investments?

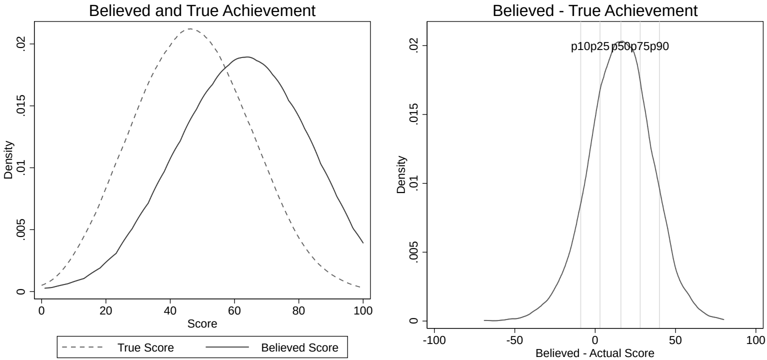
1. Ask them .... However, there may be a problems with that. Parents may not remember their investment or may want to please the surveyor ("Social desirability bias").
2. "Put your money where your mouth is": little "lab in the field" experiments to force parents to make choices which have some consequences.
   - Willingness to pay for a remedial textbook in English and Math, using Becker-DeGroot-Marshak method.
   - Each child is given two workbooks: one in math, and one in english.Parents must chose among 3 levels (easy, medium, hard).
   - Secondary school lottery: one in every 100 child in the sample will get secondary school fees paid. Each parent is given 9 tickets and must allocate them between the two children
3. Administrative data on school participation and end-of-year grades
4. Actual investment decisions one year later.

Becker-DeGroot-Marshak

Surveyor: For each row, say: "At the end of the interview, if the randomly selected textbook is the **math** book for [*NAME*] and the randomly selected price is [*PRICE*] MWK, will you purchase the book?"					
a)	**1900**MWK	☐ 1. YES	*or*	☐	2. NO
b)	**1700**MWK	☐ 1. YES	*or*	☐	2. NO
c)	**1500**MWK	☐ 1. YES	*or*	☐	2. NO
d)	**1300** MWK	☐ 1. YES	*or*	☐	2. NO
e)	**1100** MWK	☐ 1. YES	*or*	☐	2. NO
f)	**900**MWK	☐ 1. YES	*or*	☐	2. NO
g)	**700**MWK	☐ 1. YES	*or*	☐	2. NO
h)	**500**MWK	☐ 1. YES	*or*	☐	2. NO
i)	**300**MWK	☐ 1. YES	*or*	☐	2. NO

Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

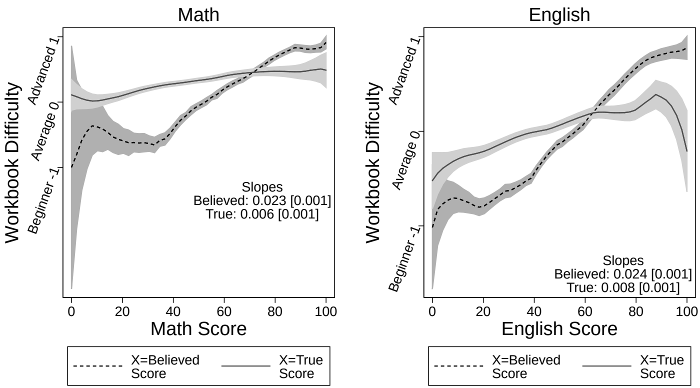
parents have inaccurate perceptions about their children's achievement



Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

As a result they pick the wrong workbooks, for their own preferences

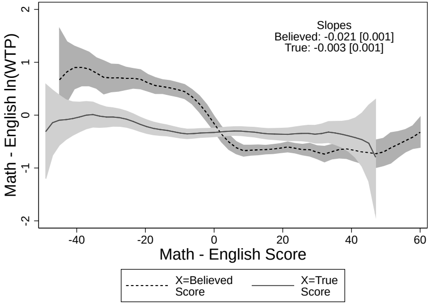
(a) **Workbooks (Complements)**



Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

...they don't want to pay for the right textbook

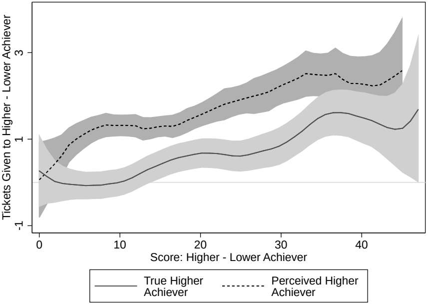
(b) **Textbook WTP (Substitute)**



Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

...they give more ticket to the "wrong" child

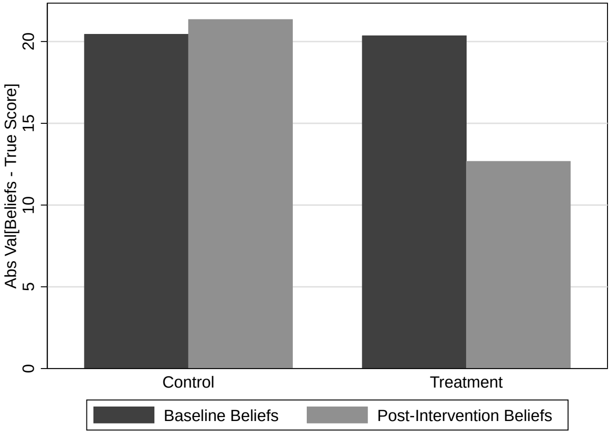
(c) **Secondary School Lottery**



Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

The information treatment affect beliefs

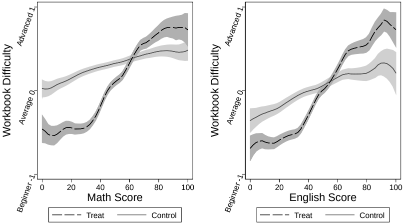
Figure 5: Information shifts parents' beliefs towards their children's true achievement



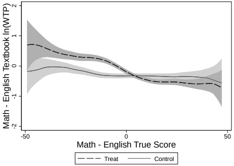
Source: Dizon-Ross (2014) "Parents' Perceptions and Children's Education: Experimental Evidence from Malawi"

And it makes decisions more sensitive to true achievement

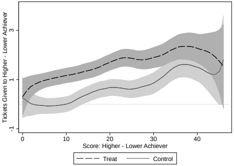
(a) **Workbooks (Complements)**



(b) **Textbook WTP (Substitute)**



(c) **Secondary School Lottery**



Source: Dizon-Ross (2014) “Parents’ Perceptions and Children’s Education: Experimental Evidence from Malawi”

To sum up

- At a broad, qualitative level, parents do respond to (perceived() costs and benefits when making education decision.
- But the order of magnitudes can be entirely wrong.
- Both because perceptions are often completely off
- And because people seems to be too sensitive to prices: for example, in the uniform study in Kenya, one find large impact of a $5 uniform... which is not directly a response to the opportunity costs.

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.

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Duflo (2001)

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.

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Duflo (2001)

TABLE 5—EFFECT OF THE PROGRAM ON EDUCATION AND WAGES: COEFFICIENTS OF THE INTERACTIONS BETWEEN DUMMIES INDICATING AGE IN 1974 AND THE NUMBER OF SCHOOLS CONSTRUCTED PER 1,000 CHILDREN IN REGION OF BIRTH

	Dependent variable: years of education						Dependent variable: log(hourly wage)		
	Whole sample			Sample of wage earners					
Age in 1974	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
12	−0.035	−0.025	0.002	−0.040	−0.010	0.009	0.016	0.019	0.027
	(0.047)	(0.048)	(0.054)	(0.077)	(0.078)	(0.091)	(0.013)	(0.013)	(0.015)
11	0.011	0.025	0.018	0.008	0.014	−0.003	−0.014	−0.013	−0.009
	(0.046)	(0.047)	(0.051)	(0.073)	(0.074)	(0.083)	(0.012)	(0.013)	(0.014)
10	0.059	0.049	0.078	0.10	0.092	0.13	0.0036	0.0042	0.0059
	(0.047)	(0.049)	(0.054)	(0.075)	(0.076)	(0.090)	(0.013)	(0.013)	(0.015)
9	0.14	0.14	0.15	0.067	0.063	0.17	0.0095	0.010	0.018
	(0.039)	(0.041)	(0.044)	(0.065)	(0.066)	(0.077)	(0.011)	(0.011)	(0.013)
8	0.088	0.11	0.11	0.19	0.20	0.28	0.019	0.021	0.027
	(0.049)	(0.050)	(0.054)	(0.078)	(0.079)	(0.089)	(0.013)	(0.013)	(0.015)
7	0.12	0.14	0.16	0.11	0.13	0.16	−0.0095	−0.0049	0.0066
	(0.044)	(0.046)	(0.051)	(0.072)	(0.073)	(0.084)	(0.012)	(0.012)	(0.014)
6	0.14	0.17	0.26	0.23	0.23	0.32	0.011	0.013	0.018
	(0.042)	(0.044)	(0.049)	(0.070)	(0.070)	(0.084)	(0.012)	(0.012)	(0.014)
5	0.10	0.13	0.13	0.14	0.16	0.27	0.021	0.023	0.052
	(0.043)	(0.045)	(0.050)	(0.075)	(0.075)	(0.088)	(0.013)	(0.013)	(0.015)
4	0.11	0.12	0.18	0.19	0.19	0.29	0.019	0.020	0.038
	(0.039)	(0.041)	(0.046)	(0.069)	(0.069)	(0.082)	(0.012)	(0.012)	(0.014)
3	0.11	0.14	0.20	0.15	0.17	0.30	0.0079	0.013	0.027
	(0.044)	(0.046)	(0.053)	(0.079)	(0.080)	(0.097)	(0.013)	(0.014)	(0.016)
2	0.14	0.19	0.19	0.20	0.22	0.25	0.016	0.023	0.040
	(0.041)	(0.043)	(0.049)	(0.073)	(0.074)	(0.088)	(0.012)	(0.013)	(0.015)
Control variables:[a]									
Year of birth*enrollment rate in 1971	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year of birth*water and sanitation program	No	No	Yes	No	No	Yes	No	No	Yes
F-statistic[b]	4.03	5.18	6.15	2.70	2.74	4.38	1.13	1.29	2.05
R^2	0.19	0.19	0.17	0.14	0.14	0.13	0.14	0.15	0.13
Number of observations	152,989	152,495	143,107	60,633	60,466	55,144	60,633	60,466	55,144

◂ Go Back

Duflo (2001)

TABLE 7—EFFECT OF EDUCATION ON LABOR MARKET OUTCOMES: OLS AND 2SLS ESTIMATES

Method	Instrument	(1)	(2)	(3)	(4
Panel A: Sample of Wage Earners					
Panel A1: Dependent variable: log(hourly wage)					
OLS		0.0776	0.0777	0.0767	
		(0.000620)	(0.000621)	(0.000646)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0675	0.0809	0.106	0.0
		(0.0280)	(0.0272)	(0.0222)	(0.0
		[0.96]	[0.9]	[0.93]	[0.9
2SLS	(Aged 2–6 in 1974)*program intensity in region of birth	0.0752	0.0862	0.104	
		(0.0338)	(0.0336)	(0.0304)	
		(0.0338)	(0.0336)	(0.0304)	
Panel A2: Dependent variable: log(monthly earnings)					
OLS		0.0698	0.0698	0.0689	
		(0.000601)	(0.000602)	(0.000628)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0756	0.0925	0.0913	0.1
		(0.0280)	(0.0278)	(0.0219)	(0.0
		[0.73]	[0.63]	[0.58]	[0.7
Panel B: Whole Sample					
Panel B1: Dependent variable: participation in the wage sector					
OLS		0.0328	0.0327	0.0337	
		(0.00311)	(0.000311)	(0.000319)	
2SLS	Year of birth dummies*program intensity in region of birth	0.101	0.118	0.0892	
		(0.0210)	(0.0197)	(0.0162)	
		[0.66]	[0.93]	[1.12]	
Panel B2: Dependent variable: log(monthly earnings), imputed for self-employed individuals					
OLS		0.0539	0.0539	0.0539	
		(0.000354)	(0.000354)	(0.000355)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0509	0.0745	0.0346	
		(0.0157)	(0.0136)	(0.0138)	
		[0.68]	[0.58]	[1.16]	
Control variables:					
Year of birth*enrollment rate in 1971		No	Yes	Yes	Y

Duflo (2001)

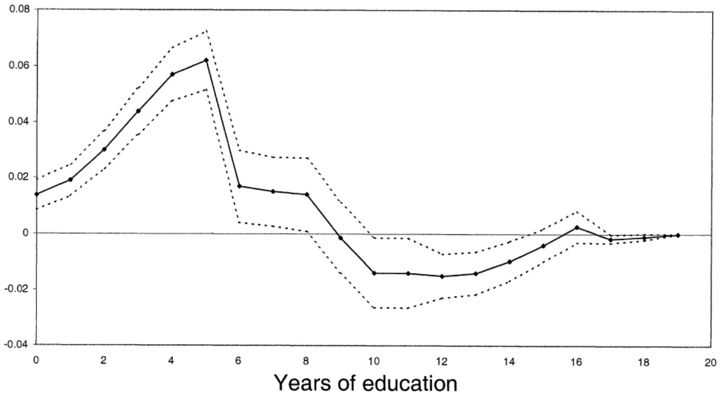

FIGURE 2. DIFFERENCE IN DIFFERENCES IN CDF (ESTIMATED FROM LINEAR PROBABILITY MODEL) WITH 95-PERCENT CONFIDENCE INTERVAL

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Duflo (2001)

FIGURE 3. COEFFICIENTS OF THE INTERACTIONS AGE IN 1974* PROGRAM INTENSITY IN THE REGION OF BIRTH IN THE WAGE AND EDUCATION EQUATIONS

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Duflo (2004)

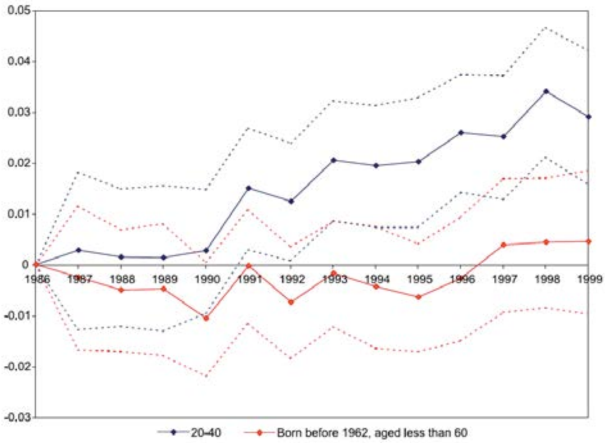

Fig. 2. Coefficients of the interactions of program intensity and survey year dummies. Dependent variable: % of primary school graduates.

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Duflo (2004)

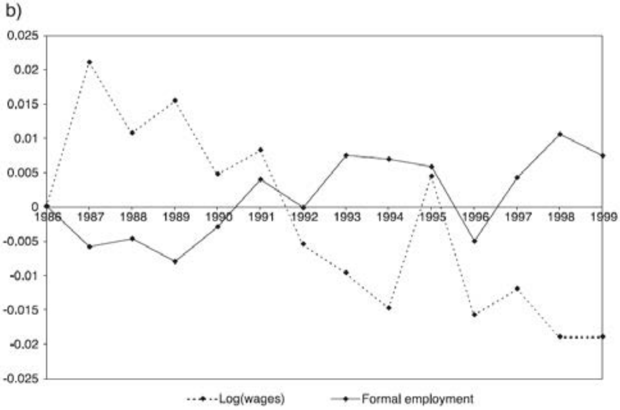

Fig. 4. (a) Coefficients of the interactions of program intensity and survey year dummies. Dependent variables: log(wage) and formal sector employment (individuals born before 1962 and aged less than 60). Sample: urban and rural regions. (b) Coefficients of the interactions of program intensity and survey year dummies. Dependent variables: average log(wage) and average formal sector employment among individuals born before 1962 and aged less than 60. Sample: rural regions.

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Duflo (2004)

Table 6

2SLS estimates of the impact of average education on individual wages

	Independent variable: % of primary school graduates in the 20–40 sample		Independent variable: % of primary school graduates in the 20–60 sample	
	Sample: rural and urban areas	Sample: rural areas only	Sample: rural and urban areas	Sample: rural areas only
	(1)	(2)	(3)	(4)
Panel A: years 1986–1999				
Log (wage)	−0.204 (0.443)	−0.834 (0.701)	−0.208 (0.615)	−0.871 (0.837)
Log (wage) residual	−0.292 (0.355)	−0.633 (0.431)	−0.379 (0.512)	−0.994 (0.556)
Skill premium	−0.434 (0.916)	−0.982 (1.408)	−0.596 (1.197)	−0.636 (1.645)
Formal employment	0.441 (0.159)	0.454 (0.203)	0.661 (0.238)	0.745 (0.352)
Formal employment among educated workers	0.432 (0.197)	0.501 (0.259)	0.543 (0.264)	0.713 (0.406)
Formal employment among uneducated workers	0.379 (0.203)	0.409 (0.232)	0.510 (0.354)	0.318 (0.318)
Panel B: years 1986–1997				
Log (wage)	−0.358 (0.493)	−0.710 (0.821)	−0.451 (0.716)	−0.480 (0.801)
Log (wage) residual	−0.330 (0.412)	−0.588 (0.529)	−0.437 (0.618)	−0.902 (0.602)
Skill premium	−0.225 (1.033)	−0.635 (1.461)	−0.291 (1.488)	0.536 (1.576)
Formal employment	0.463 (0.183)	0.442 (0.233)	0.716 (0.282)	0.694 (0.379)
Formal employment among educated workers	0.428 (0.229)	0.473 (0.301)	0.530 (0.317)	0.622 (0.479)
Formal employment among uneducated workers	0.478 (0.249)	0.449 (0.277)	0.624 (0.415)	0.263 (0.319)

Men aged 20–60 and born before 1962.

1. Survey year dummies, region dummies, interactions between survey year dummies and the enrollment rate in 1971, and interactions between survey year dummies and the number of children are included in the regressions. 2. Regression run using kabupaten-year averages, weighted by the number of observations in each kabupaten-year cell. 3. The instruments are interactions between survey year dummies and the program intensity. 4. The standard errors are corrected for auto-correlation within kabupaten.

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TABLE V

EFFECTS OF THE INTERVENTION ON EXPECTED RETURNS AND SCHOOLING

	Full sample				Poor households				Least poor households			
	(1) Returned next year	(2) Finished school	(3) Years of schooling	(4) Perceived returns	(5) Returned next year	(6) Finished school	(7) Years of schooling	(8) Perceived returns	(9) Returned next year	(10) Finished school	(11) Years of schooling	(12) Perceived returns
Treatment	0.041*	0.023	0.20**	367***	0.006	−0.01	0.037	344***	0.072*	0.054*	0.33***	386***
	(0.023)	(0.020)	(0.082)	(28)	(0.034)	(0.026)	(0.11)	(41)	(0.038)	(0.031)	(0.12)	(41)
Log	0.095**	0.23***	0.79***	29.0	0.054	0.26***	0.69***	188**	0.047	0.10	0.51	23
(inc. per capita)	(0.040)	(0.044)	(0.16)	(47)	(0.068)	(0.062)	(0.23)	(87)	(0.12)	(0.13)	(0.45)	(133)
School	0.011	0.019**	0.086**	0.74	0.001	0.015	0.064	−9.5	0.025*	0.024*	0.10**	8.2
performance	(0.010)	(0.009)	(0.034)	(14)	(0.014)	(0.012)	(0.048)	(13.5)	(0.013)	(0.012)	(0.048)	(22)
Father	0.074**	0.050*	0.26**	−24	0.056	0.019	0.16	−29.1	0.096**	0.096**	0.36**	−3.8
finished sec.	(0.030)	(0.030)	(0.12)	(32)	(0.045)	(0.043)	(0.18)	(62)	(0.038)	(0.038)	(0.14)	(40)
Age	−0.010	0.004	−0.006	−42*	−0.042	0.002	−0.071	−46	0.005	0.005	0.025	−35
	(0.016)	(0.015)	(0.059)	(21)	(0.030)	(0.019)	(0.088)	(32)	(0.025)	(0.035)	(0.087)	(29)
R^2	.016	.040	.049	.090	.007	.019	.014	.094	.020	.020	.029	.090
Observations	2,241	2,205	2,074	1,859	1,055	1,055	1,007	920	1,056	1,056	1,002	939

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Documenting declines in MMR

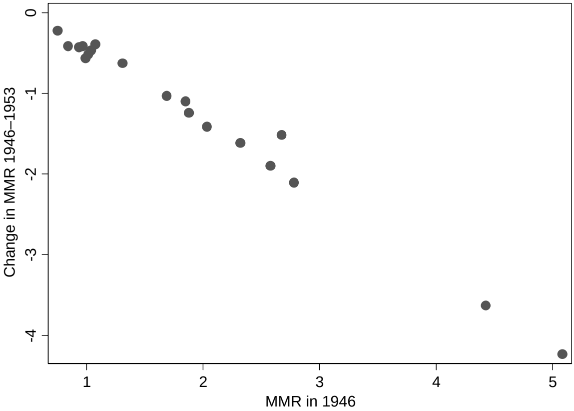

FIGURE II

Declines in Maternal Mortality across Districts

Note. Each dot represents a district. Maternal mortality is the number of deaths per 100 live births. In a univariate regression of maternal mortality changes between 1946 and 1953 on the initial 1946 level, the coefficient on initial MMR is −0.70 and is statistically significant at the 5% level.

only if there are returns to education that accrue over time. Although no solid causal estimates of the returns to education exist

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Impact of MMR on life expectancy

TABLE IV
EFFECT OF MATERNAL MORTALITY ON LIFE EXPECTANCY AND INFANT MORTALITY

	(1) Basic	(2) Add malaria death rates	(3) Add nutritional diseases death rates	(4) Add nutritional diseases and malaria death rates
		e(15–65)		
MMR × female	−1.204***	−1.302***	−1.214***	−1.373***
	[0.198]	[0.302]	[0.183]	[0.330]
R^2	0.97	0.97	0.98	0.98
		e(45–65)		
MMR × female	0.054	−0.033	0.078	−0.043
	[0.089]	[0.120]	[0.119]	[0.180]
R^2	0.94	0.95	0.97	0.97
		e(0–15)		
MMR × female	−0.088*	−0.081	−0.064*	−0.018
	[0.050]	[0.065]	[0.033]	[0.055]
R^2	0.99	0.99	0.99	0.99
		IMR		
MMR × female	0.133	0.081	0.265*	0.213
	[0.164]	[0.192]	[0.145]	[0.265]
R^2	0.99	0.99	0.99	0.99

Note. All regressions include district-year, district-gender, and gender-year fixed effects. Additional controls are measured in changes. The notation e(15–65) is the expected years of life between ages 15 and 65, conditional on surviving until age 15, and so forth. MMR is the maternal mortality ratio, and IMR is the infant mortality rate. Both are measured as deaths per 100 live births and are measured contemporaneously. Nutritional diseases are helminths, anemia, diarrhea, and vitamin deficiencies. Standard errors (reported in brackets) are clustered at the district level. Each cell reports the coefficient from a separate regression. $N = 76$ (19 districts, 2 genders, 2 years).

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

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Impact of MMR on literacy

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TABLE V

EFFECT OF MATERNAL MORTALITY ON LITERACY AND PERCENTAGE IN SCHOOL

	DDD (district, year, gender)				Older coh
	(1) Basic	(2) Add nutritional diseases and malaria death rates	(3) 1946 level as IV	(4) Drop district FE and control for male $e(0–65)$	(5) DDDD (district, year, gender, cohort) D
Coefficient reported	Lagged MMR × female	Lagged MMR × female	Lagged MMR × female	Lagged MMR × female	Lagged MMR × female × treated
	Panel A: Literacy of treated cohorts aged 5–19				
	−0.879*	−1.652**	−1.008**	−1.07	−0.728
	[0.453]	[0.656]	[0.470]	[1.763]	[0.745]
Obs	228	228	228	228	532
	Panel B: Placebo test, literacy of controls cohorts aged 25–44				
	−0.151	0.273	−0.149		
	[0.469]	[0.450]	[0.476]		
Obs	304	304	304		
	Panel C: Percent of 5- to 24-year-olds who are in school				
	−0.904*	−0.686	−0.979**		
	[0.458]	[0.995]	[0.460]		
Obs	76	76	76		

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Impact of BPO campaign on job opportunities

TABLE II

EFFECT OF THE INTERVENTION ON EMPLOYMENT, BY AGE AT ROUND 2

	BPO employment			Works for pay away from home		
	(1)	(2)	(3)	(4)	(5)	(6)
	18–24	25–44	45–60	18–24	25–44	45–60
Panel A: Women						
Treatment	0.046***	0.003	~	0.024**	0.0029	−0.006
	(0.008)	(0.003)		(0.011)	(0.0089)	(0.014)
Observations	1,278	2,233	1,029	1,278	2,233	1,029
Control group mean	0.004	0.002	0.00	0.21	0.24	0.22
R^2	0.022	0.000	~	0.054	0.001	0.000
Panel B: Men						
Treatment	−0.007	0.002	~	0.003	0.007	−0.004
	(0.005)	(0.004)		(0.011)	(0.024)	(0.035)
Observations	1,442	2,469	1,104	1,442	2,469	1,104
Control group mean	0.008	0.003	0.00	0.47	0.56	0.52
R^2	0.001	0.000	~	0.000	0.001	0.000

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Impact of BPO campaign on human capital

TABLE IV

EFFECT OF THE INTERVENTION ON HUMAN CAPITAL

	(1) Enrolled in training (18–24)	(2) Enrolled in school (6–17)	(3) BMI for age (5–15)	(4) Height for age (5–15)
Panel A: Women				
Treatment	0.028***	0.050***	0.24***	0.063
	(0.008)	(0.015)	(0.070)	(0.066)
R^2	0.010	0.004	0.007	0.001
Observations	1,278	2,264	2,031	2,031
Control group bean	0.005	0.76	−1.25	−2.02
Panel B: Men				
Treatment	0.003	0.010	−0.020	0.005
	(0.004)	(0.011)	(0.076)	(0.052)
R^2	0.000	0.001	0.000	0.000
Observations	1,442	2,511	2,295	2,295
Control group mean	0.004	0.81	−1.29	−1.99

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Impact of BPO campaign on marriage and fertility

TABLE V

Effect of the Intervention on Marriage and Fertility, Ages 18–24 in Round 2

	(1) Married	(2) Had child	(3) Desired fertility
Panel A: Women			
Treatment	−0.051**	−0.057**	−0.35***
	(0.024)	(0.026)	(0.078)
R^2	0.003	0.003	0.018
Observations	1,278	1,278	1,226
Control group mean	0.71	0.43	3.0
Panel B: Men			
Treatment	−0.002	−0.009	0.027
	(0.025)	(0.018)	(0.066)
R^2	0.000	0.000	0.000
Observations	1,442	1,442	1,437
Control group mean	0.44	0.15	3.3

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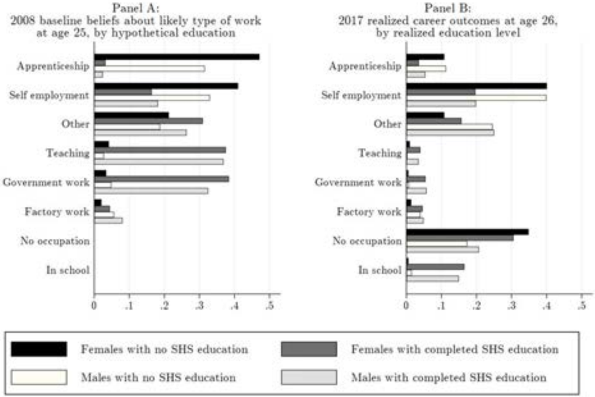
Figure 1: Type of work, by education level: Baseline Expectations vs. Realizations



Notes: Data from 2008 in-person baseline survey of participants (Panel A) and 2017 phone survey (Panel B). SHS stands for Senior High School. In Panel A, respondents (aged 17 on average at the time) were asked in 2008: "If you never go to SHS or continue any other higher education in the future, what types of work do you think you would do when you are 25 years old?" and "Imagine that you complete Senior High School in the future, what types of work do you think you would do when you are 25 years old?" In Panel B, data from the 2017 phone survey on the realized career outcomes of students who did and did not complete SHS is shown. We plot answers separately by respondent gender, pooling treatment and control groups.

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