In Aid We Trust:
Hearts and Minds And the Pakistan Earthquake 2005

Tahir Andrabi, Pomona College
(w/Jishnu Das)

PACDEV

March 2011

Hearts and Minds

- Heart and Minds in the Islamic World an acknowledged aim of US Policy
  - Bipartisan
    - Karen Hughes and Office of Public Diplomacy
    - Hillary Clinton and the current flood relief
- Foreign aid recognized as one potential vehicle for affecting “hearts and minds”

No direct evidence on relationship between foreign aid and “hearts and minds”

Why this is hard: 3 Reasons

- Want to look at how aid affects (say) trust
- Practically, this implies that we study how *variation* in aid affects *variation* in trust
- *Variation* in trust can be based on specialized surveys
- *Variation* in aid is difficult to obtain
  - Potentially across countries, but does the information trickle down?
- More importantly, severe selection biases in who gets aid (and who does not)

What we do

- Look at effect of humanitarian assistance following Pakistan earthquake in 2005 on trust, both in foreigners and in local populations

What we find

- Trust in foreigners goes up the closer you get to the fault line
- Posited Channel:
  - Foreign Aid and not a generalized disaster effect
    - Foreign Aid goes up closer you get to the fault line
    - Trust in locals does not change as you get closer to the fault line
- Causal effects

Outline

- Brief Review of literature
- Description of Earthquake
- Data
- Strategy
- Results

Current Literature

- Aid, Disasters, Trust
  - Berman and Shapiro (Iraq)
  - Fair and others (Pakistan)
  - Baez and Santos, Nicaragua and Hurricane Mitch
  - Wilder, Pakistan earthquake but mainly focus groups
  - Large, wider literature on trust (summary)
- Selection Bias
  - Aid not distributed randomly
  - Problem of choosing appropriate control group
    - Counterfactual: In the absence of aid, both groups need to have the same expected outcomes

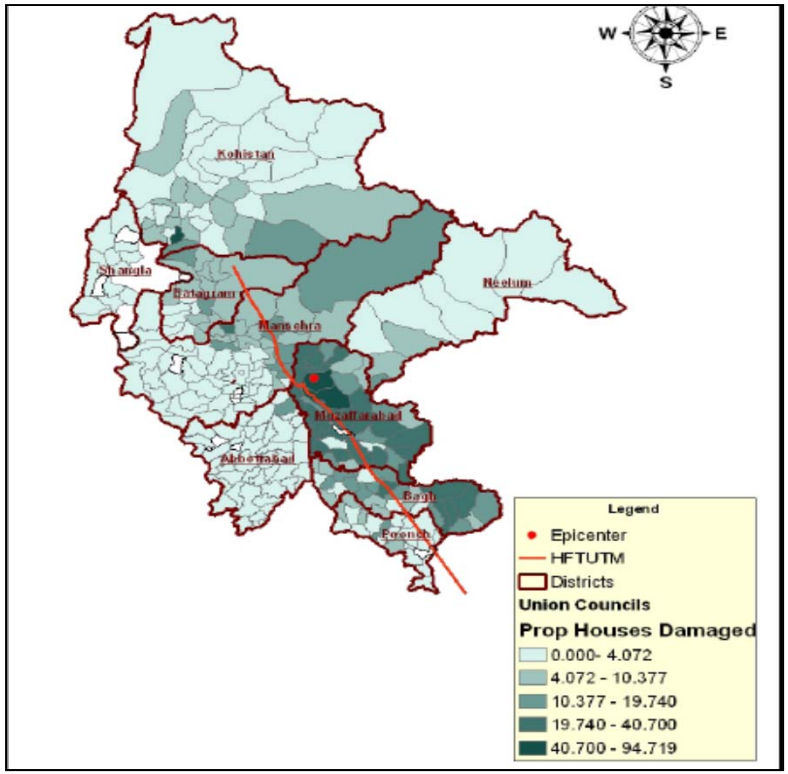
Pakistan Earthquake 2005

- Ideal case to test hearts and minds
  - Islamic country in midst of conflict
    - Current policy discussion on disaster aid
  - Lots of Destruction
  - Lots of Global Aid
  - Should have salience

Identification

✖ Cannot regress hearts and minds directly on aid

- Claim: Distance to the Fault Line provides exogenous variation



Identification, Contd.

- Exogeneity
  - Earthquake unanticipated
    - No history of major earthquakes in the immediate region
    - Unlike hurricane or floods, no warning
    - Many fault lines in the area, only one activated
  - Exogeneity vis-a-vis observables
    - Pre-earthquake population distribution uncorrelated with earthquake intensity

Identification, contd.

- ✖ Use distance to the fault line as a measure of intensity of earthquake shock
- Exclusion restriction
  - Earthquake intensity has an effect on outcomes independent of aid
- Argue that the impact of earthquake intensity independent of aid should be same on *trust in all population groups*

- Regress outcomes directly on distance
- Regress aid (as a channel) directly on distance
  - Reduced form effects
  - But still causal effects
- Argue that distance can be used as an instrument
  - Robustness and exclusion restrictions

The Pakistan Earthquake 2005

- 7.6 magnitude earthquake on October 2005
- 6 districts in North-eastern Pakistan
  - province of Khyber Pakhtunkhwa (formerly known as NWFP) and Azad Jammu Kashmir (AJK)
- Estimated 75,000 dead
- 2.4 million affected population

Global Aid Effort

- >$5 billion pledged aid by more than 40 countries and multilaterals
- Technical Assistance
  - Medical, Logistical,(helicopters), Excavation
  - Hundreds (Thousands?) of relief workers
    - Mainly health workers, rescue and relief specialists
- NGOs, Governments, Citizen Groups, Expatriates

Data From 2009

- 126 randomly selected villages in four districts including three hardest hit
  - 2 in KP, 2 in AJK
    - Abbottabad, Bagh, Mansehra, Muzaffarabad

Data, contd.

- 2009 Household census: 28,000 Households
  - Destruction, Aid, Household GPS Location
- ✖ 2009 Household survey: 2500 Households
  - ✖ Trust Module: Head of Household and spouse
  - ✖ Household Covariates
- 1998 Pakistan Population Census
  - Pre-Earthquake Village Characteristics
- Digital maps (provided by NESPAK)
  - Fault Line Coordinates, Epicenter Location, Slope of Union Council

Data: Attitudes

- Trust

  *Imagine you are walking down a street and dropped a Rs. 1000 note without noticing. _____ was walking behind you without you knowing and picked it up. How Likely are they to return it to you? (*Blank filled in with different population groups)

  (Responses coded 1-5 Likert Scale in changes and converted to binary)

  – Compare to World Values Survey:

    Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?

- Work Compatibility:

  Do you feel that the ability to different religions, nationalities, and races to work together for a common cause is: (Responses coded 1-5 Likert Scale and converted to binary)

- Kindness:

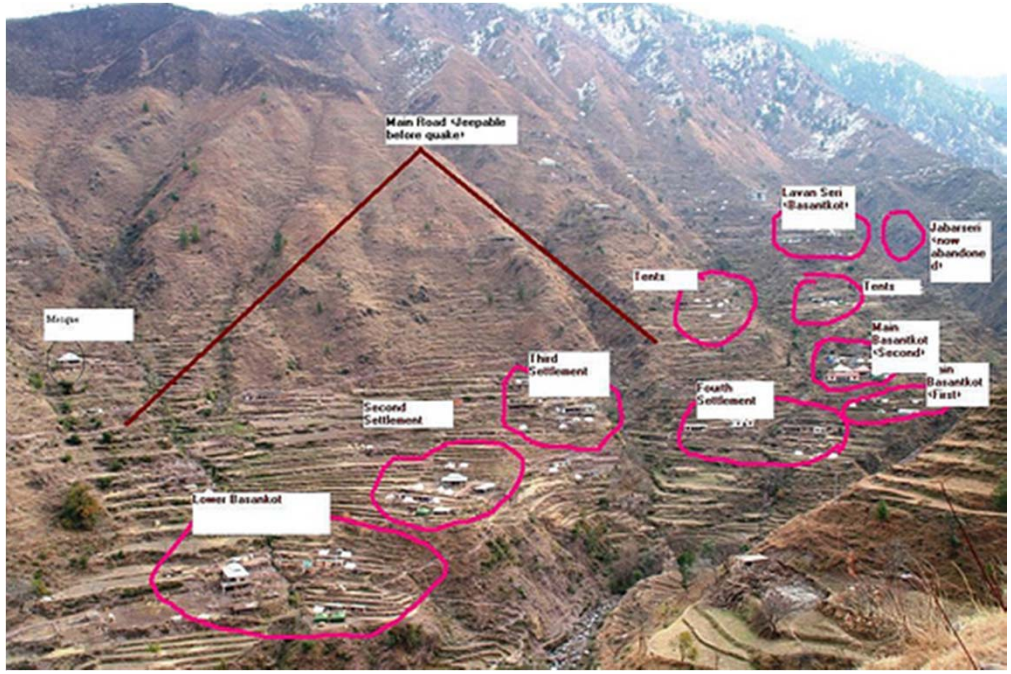
  After the earthquake, your opinion of the helpfulness and kindness of {name} is: (Responses coded 1-5 Likert Scale in changes and converted to binary)

Trust groups

- We ask for the respondent's attitudes towards the following groups:
  - 1) People in general
  - 2) Extended family
  - 3) People in your village
  - 4) People in your *qaum*/caste/clan/*biradari*
  - 5) People in your region
  - 6) Other Pakistani
  - 7) General foreigner
  - 8) European/American
  - 9) Islamic foreigner.
- We present results categories 3), 4), 5), 7) and 8) .

Data: Organizations

- Based on open-ended question asking about all organizations that came to help the household in the 3 months following the earthquake



Data

Variable	Observations	Mean	Standard Deviation
Distance to Fault-line (km)	4670	**18.89**	15.40
Distance to Epicenter (km)	4670	38.60	19.03
Slope (Degrees)	4670	21.39	6.39
Male	4670	0.50	0.50
Fraction with Any Education	4670	0.47	0.50
Fraction with any education (Male)	2324	0.65	0.48
Fraction with any education (Female)	2346	0.28	0.45
Age	4670	42.6	14.6
Fraction of the village reporting that a Foreigner Came (from Household Census)	126	0.25	0.28
Trust: Own Village	4670	0.25	0.43
Trust: Same Biradri/Quam (Caste/Clan)	4670	0.30	0.46
Trust: Same Region	4670	0.17	0.38
Trust: Other Pakistani	4670	0.29	0.45
Trust: Foreigners In General	4670	0.46	0.50
Trust: European/American	4670	0.48	0.50
Trust: Islamic Foreigners	4670	0.61	0.49
Kindness/Helpfulness: Own Village	4670	0.33	0.47
Kindness/Helpfulness: Same Biradri/Quam (Caste/Clan)	4670	0.36	0.48
Kindness/Helpfulness: Same Region	4670	0.28	0.45
Kindness/Helpfulness: Other Pakistai	4670	0.48	0.50
Kindness/Helpfulness: Foreigners In General	4670	0.54	0.50
Kindness/Helpfulness: Europeans or Foreign	4670	0.54	0.50
Kindness/Helpfulness: Islamic Foreigners	4670	0.66	0.47
Ability to Work Together	4670	0.40	0.49

Comparator: WVS trust question elicits positive response in 31% of respondents

Strategy

- Step 1: Argue that intensity of earthquake shock (geologically, the distance from the fault-line) *uncorrelated* to pre-existing socioeconomic characteristics
- Step 2: Show relationship between intensity of earthquake shock and trust
- Step 3: Show relationship between intensity of shock and aid
- Note: We are building towards an IV specification where Step 2 is the reduced form and Step 3 the first-stage with distance to the fault-line as the instrument
- Step 4/5: Discuss robustness issues

Step 1: Distance to The Fault Line Exogeneity

- Data Limitation
  - No pre-earthquake data
  - Use 1998 population census village level data
  - Use retrospective data on time-invariant characteristics *and* objective measurements (distance)
- Specification: Regress pre-quake characteristics on distance to fault-line
  - Add controls: Distance to epicenter, Slope, District Fixed Effects

Distance to The Fault Line
Exogeneity using census data

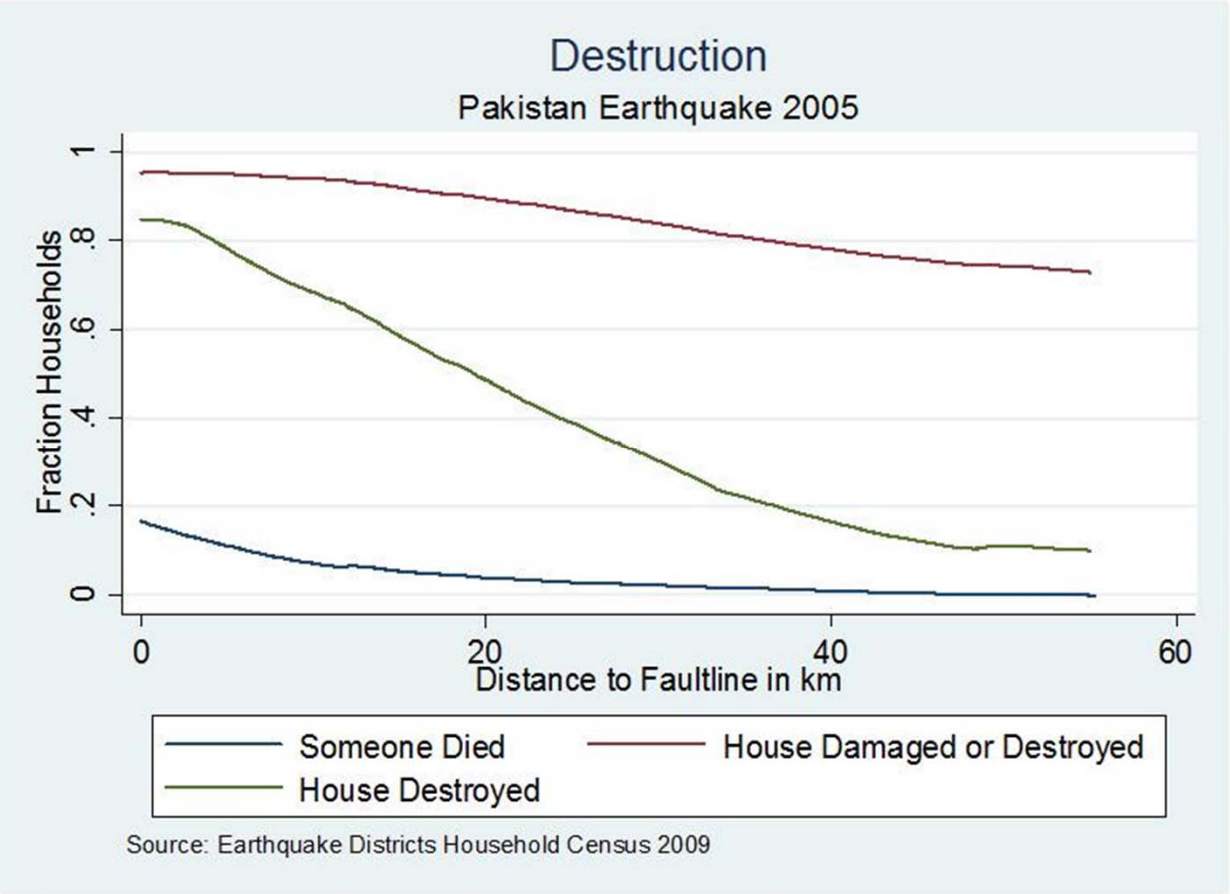
	(1)	(2)	(3)	(4)	(5)	(6)
	Total Population	**Male Population**	**Female Population**	**Literacy Rate**	**Fraction Females Secondary Educated**	**Village Infrastructure Index**
Distance to The Fault Line (km)	**-18.38**	**-9.41**	**-8.96**	**-0.02**	**-0.00**	**-0.01**
	(22.021)	**(11.142)**	**(10.908)**	**(0.109)**	**(0.000)**	**(0.009)**
Distance To The Epicenter (km)	-12.21	-6.57	-5.64	-0.18	-0.00	-0.00
	(20.693)	(10.470)	(10.250)	(0.102)*	(0.000)	(0.009)
Slope	58.59	28.61	29.98	-0.90	-0.00	-0.05
	(41.593)	(21.044)	(20.603)	(0.205)***	(0.000)**	(0.018)***
District: Bagh	-817.11	-394.72	-422.39	14.79	0.02	-0.95
	(1,075.476)	(544.144)	(532.737)	(5.297)***	(0.013)	(0.456)**
District: Mansehra	1,267.00	638.55	628.44	-17.80	-0.01	-0.72
	(663.066)*	(335.482)*	(328.449)*	(3.258)***	(0.008)*	(0.281)**
District: Muzaffarabad	-2,044.29	-1,010.67	-1,033.62	1.09	0.00	-0.42
	(726.129)***	(367.390)***	(359.688)***	(3.570)	(0.009)	(0.308)
Constant	2,036.30	1,042.29	994.01	74.23	0.06	2.34
	(1,187.380)*	(600.762)*	(588.168)*	(5.833)***	(0.014)***	(0.503)***
Observations	126	126	126	125	126	126
R-squared	0.186	0.182	0.189	0.401	0.143	0.161

Distance to The Fault Line
Exogeneity using household data

	Coefficient, Controlling for Distance to Epicenter	Standard Error
	(3)	(4)
Mother Primary Education	-0.000	0.001
Father Primary Education	-0.001	0.002
Distance to Closest Private School Before Earthquake	-0.035	0.025
Distance to Closest Public School Before Earthquake	-0.022	0.014
Had Electricity Before Earthquake	-0.008***	0.002
Had Inhouse Water Supply Before Earthquake	-0.003	0.002
Minutes to Get Water Before Earthquake	0.052	0.054
Distance to Closest Pump Before Earthquake	-0.004	0.017
Minutes to Closest Medical Facility Before Earthquake	-0.174	0.279
Minutes to Market Before Earthquake	0.133	0.330
Lived in a Permanent Structure Before Earthquake	-0.002	0.002

The Story in Five Figures

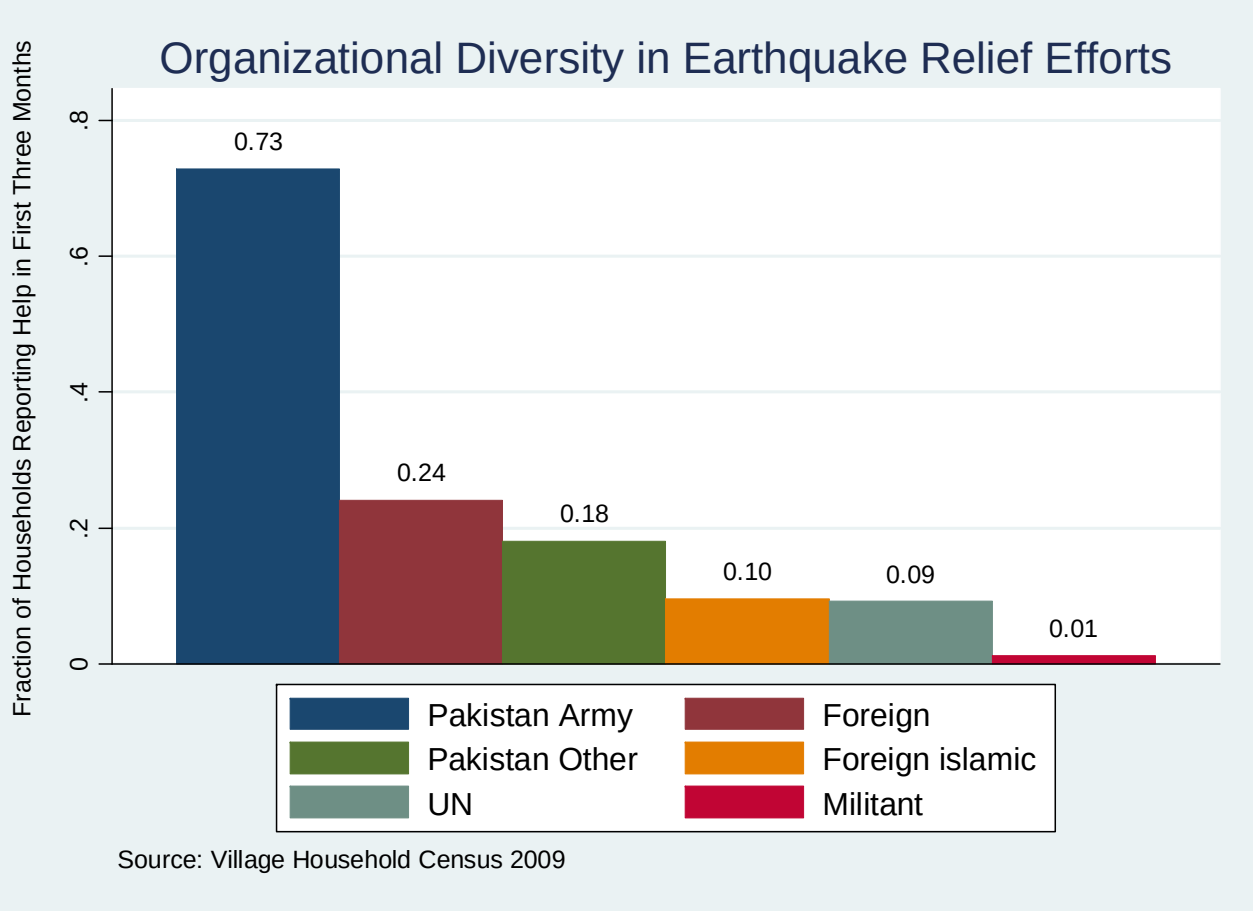
Earthquake Intensity and Destruction



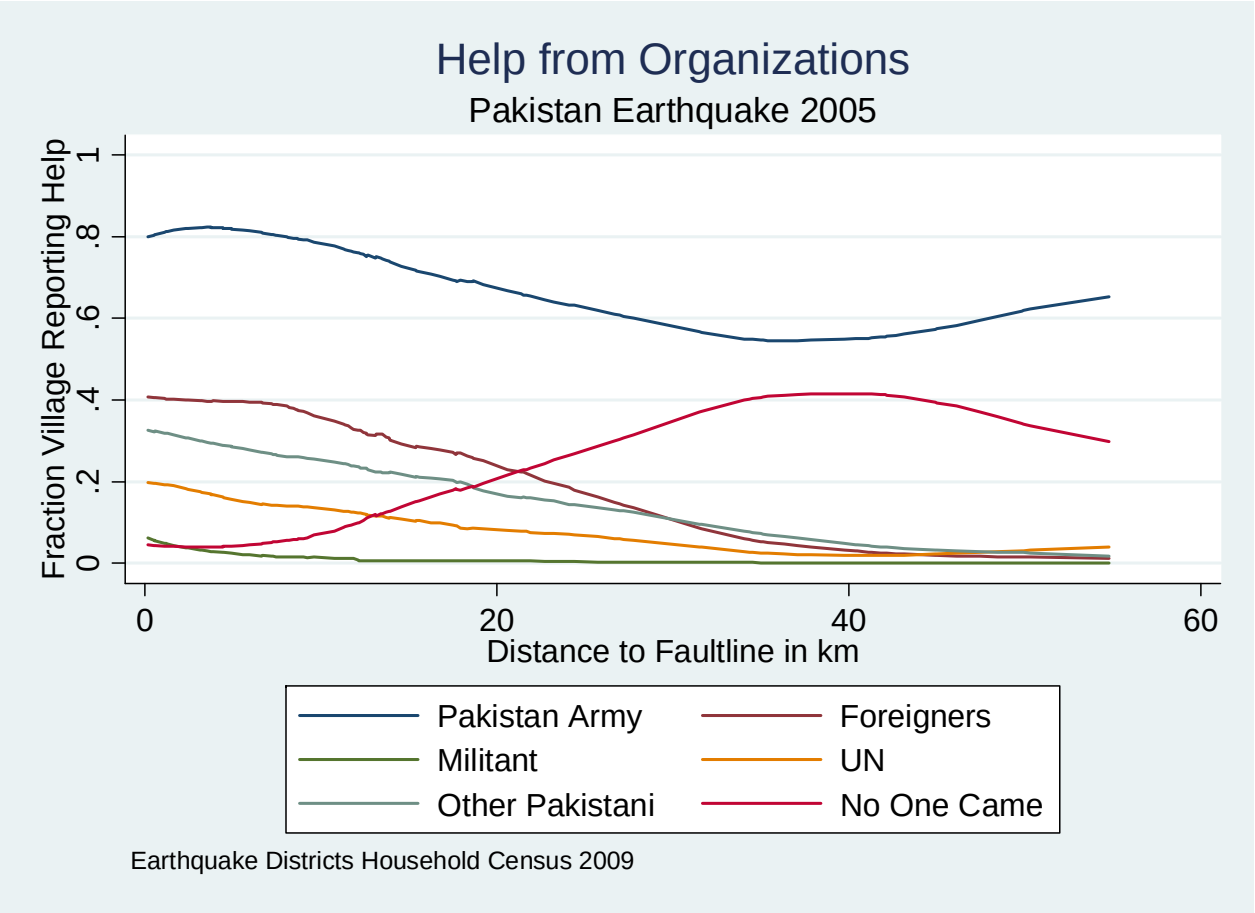
Aid

- Reported Aid:
  - Name organizations or people that came to your help within the first three months after the earthquake
    - Number of responses unrestricted
    - Focus on rescue, relief and rehabilitation not reconstruction
- 203 organizations or different groups recalled
  - Reports range from specific organizations such as Pakistan Army, Save the Children, Oxfam, UNHCR to Japanese NGO, some foreigners, etc.
  - Some villages report as many as forty five distinct answers

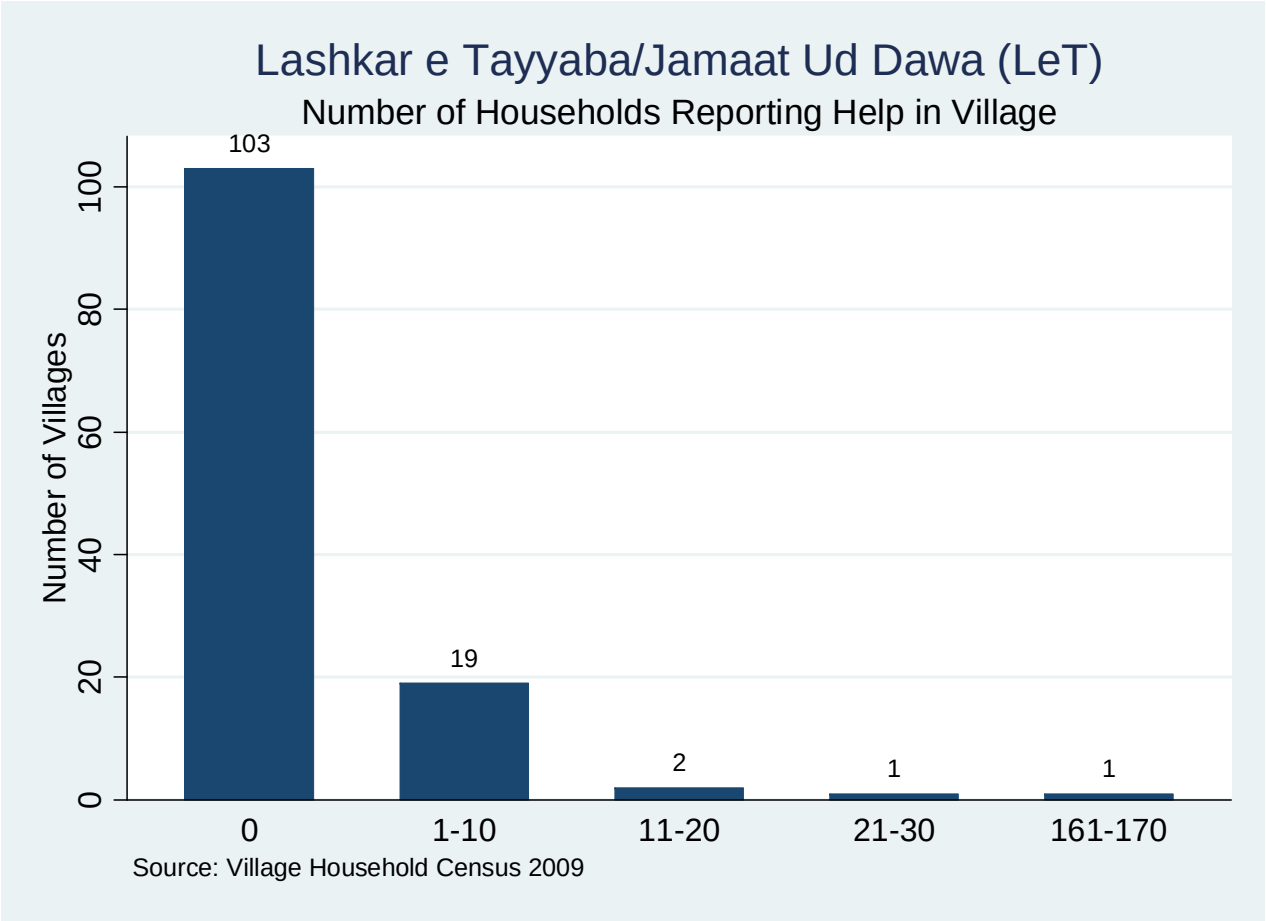
Earthquake Intensity and Aid



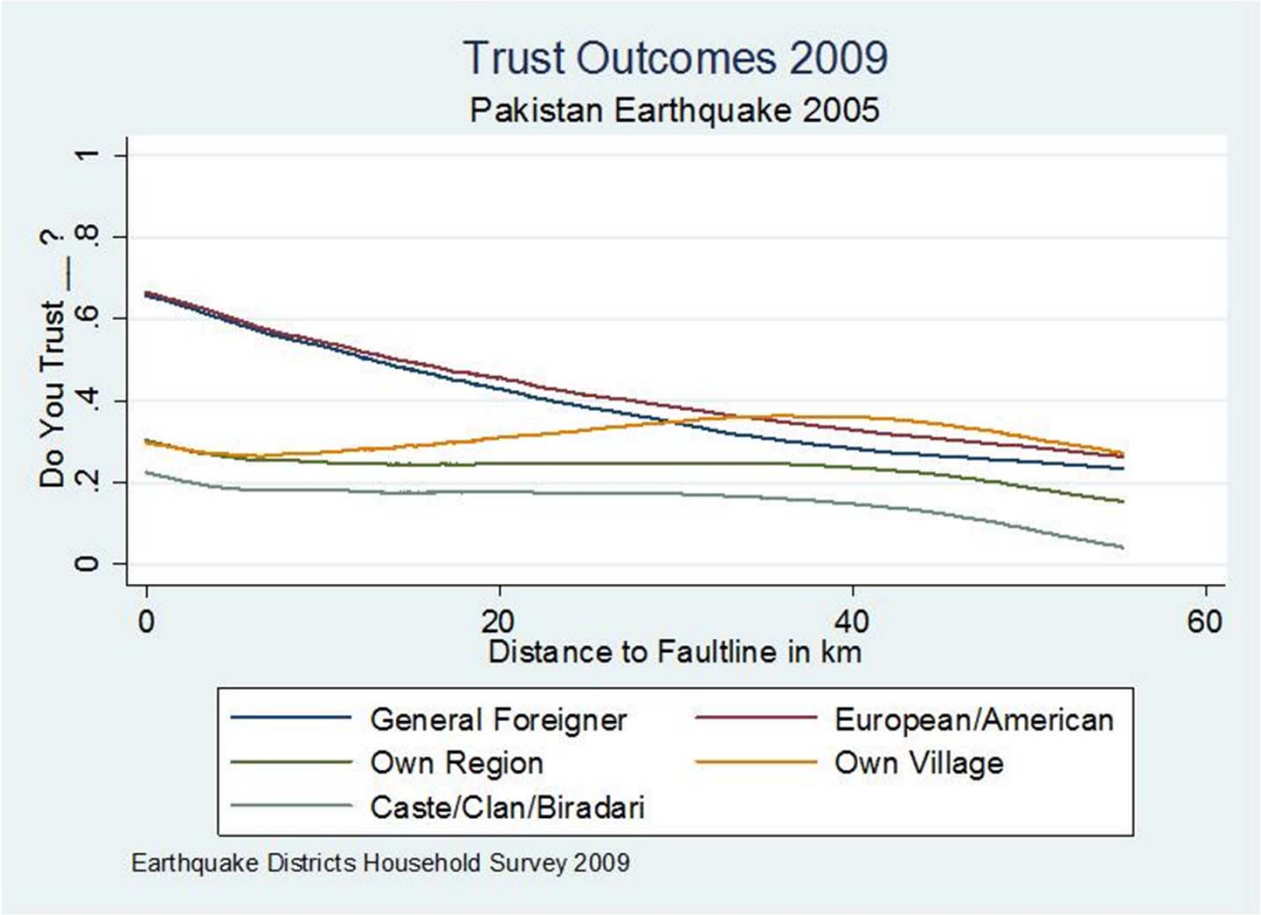
Earthquake Intensity and Aid



Earthquake Intensity and Aid: A footnote



Earthquake Intensity and Trust



Regressions: Earthquake intensity, aid and destruction

	(1)	(2)	(1)	(2)	(3)
VARIABLES	**Foreign Came (Household Response)**	**Foreign came (Fraction village reporting)**	**Mortality**	**House Damaged or Destroyed**	**House Destroyed**
Distance Fault-line (km)	**-0.004*** **	**-0.008*** **	**-0.001* **	**-0.004*** **	**-0.013*** **
	(0.000)	**(0.002)**	**(0.001)**	**(0.001)**	**(0.002)**
Distance Epicenter	0.000	0.002	-0.001	0.000	0.002
	(0.000)	(0.002)	(0.001)	(0.000)	(0.001)
Slope	0.011***	0.007*	0.003**	0.005***	0.014***
	(0.000)	(0.004)	(0.001)	(0.001)	(0.005)
District: Bagh	0.543***	0.057	0.048	-0.002	0.170**
	(0.012)	(0.100)	(0.030)	(0.027)	(0.077)
District: Mansehra	0.208***	0.076	0.032*	0.045**	0.123
	(0.007)	(0.061)	(0.019)	(0.020)	(0.075)
District: Muzaffarabad	0.515***	0.211***	0.053***	-0.002	0.227***
	(0.008)	(0.068)	(0.018)	(0.022)	(0.056)
Constant	-0.098***	0.068	0.019	0.836***	0.278**
	(0.013)	(0.011)	(0.033)	(0.039)	(0.128)
Observations	28,297	126	28297	6455	6455
R-squared	0.336	0.403	0.043	0.058	0.304

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

Regressions: Intensity and trust in foreigners

	(1)	(2)	(3)	(4)	(5)
	Other Pakistani	**Islamic Foreigner**	**General Foreigner**	**European/American Foreigner**	**Work Together**
Distance Fault-line (km)	**-0.007*** **	**-0.006*** **	**-0.006*** **	**-0.005** **	**-0.003** **
	(0.001)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.001)**
Educated	0.023	0.041**	0.057***	0.047**	0.072***
	(0.016)	(0.017)	(0.020)	(0.019)	(0.018)
Asset Index: Middle	-0.036**	-0.016	-0.019	-0.021	-0.038*
	(0.018)	(0.020)	(0.021)	(0.022)	(0.020)
Asset Index: Poor	-0.061***	-0.024	-0.073***	-0.074***	-0.055**
	(0.021)	(0.026)	(0.026)	(0.027)	(0.024)
Male	-0.005	0.114***	0.076***	0.060***	0.088***
	(0.019)	(0.018)	(0.018)	(0.021)	(0.028)
Distance Epicenter (km)	0.003**	0.002	0.001	0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Slope	0.002	0.003	0.002	0.003	0.010***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
District: Bagh	-0.103	-0.173**	0.101	0.061	0.066
	(0.068)	(0.078)	(0.081)	(0.075)	(0.058)
District: Mansehra	0.073***	-0.230***	-0.038	-0.094*	0.034
	(0.028)	(0.056)	(0.042)	(0.049)	(0.042)
District: Muzaffarabad	0.099***	-0.049	0.105**	0.071	-0.025
	(0.036)	(0.061)	(0.049)	(0.056)	(0.052)
Constant	0.257***	0.628***	0.397***	0.479***	0.238***
	(0.062)	(0.098)	(0.087)	(0.097)	(0.078)
Observations	4670	4670	4670	4670	4670
R-squared	0.072	0.090	0.109	0.091	0.074

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

Regressions: Intensity and trust in locals

	(1)	(2)	(3)	(4)
VARIABLES	**Own Village**	**Extended Family**	**Own Caste/Clan/ *Biradari***	**Own Region**
Distance Fault-line (km)	**0.000**	**0.002**	**0.001**	**-0.001**
	(0.001)	**(0.002)**	**(0.001)**	**(0.001)**
Educated	-0.005	0.026	0.027	0.006
	(0.016)	(0.018)	(0.018)	(0.015)
Asset Index: Middle	-0.020	-0.006	-0.013	-0.014
	(0.018)	(0.022)	(0.020)	(0.016)
Asset Index: Poor	-0.041*	-0.059**	-0.054**	-0.042**
	(0.022)	(0.028)	(0.024)	(0.018)
Male	0.026	0.073***	0.048**	0.023
	(0.018)	(0.020)	(0.020)	(0.015)
Distance Epicenter (km)	-0.001	-0.002	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Slope	-0.005**	-0.009***	-0.005**	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)
District: Bagh	0.035	0.066	-0.099*	-0.022
	(0.049)	(0.063)	(0.056)	(0.043)
District: Mansehra	0.001	-0.156***	-0.063	0.029
	(0.031)	(0.046)	(0.042)	(0.027)
District: Muzaffarabad	0.080**	0.017	-0.018	0.029
	(0.038)	(0.052)	(0.044)	(0.035)
Constant	0.376***	0.657***	0.425***	0.253***
	(0.065)	(0.088)	(0.076)	(0.053)
Observations	4670	4670	4670	4670
R-squared	0.025	0.046	0.036	0.030

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

Extensions

- Heterogeneity
  - Gender, Education, Age
  - Risk Aversion
- Robustness
  - Control for aid
  - Pakistani Groups aid
- IV

Policy Implications

- Trust is malleable
  - Trust responds to actions of foreigners
  - Trust not a function of deep rooted preferences and beliefs
- Aid Matters
  - Hillary Clinton: “Shared Humanity”
  - People matter
  - Humanitarian, Professional, Technical

Regressions: Trust Controlling for aid

	Trust: Local Own Village	Own caste	Own Region	Trust: Foreign General Foreigner	European/ American Foreigner	Work Together
	(1)	(2)	(3)	(4)	(5)	(6)
Distance Fault-line (km)	**0.000**	**0.000**	**-0.000**	**-0.003***	**-0.003**	**-0.002**
	(0.001)	**(0.001)**	**(0.001)**	**(0.002)**	**(0.002)**	**(0.001)**
% Village: Foreign Came	**0.013**	**-0.020**	**0.061**	**0.370*** **	**0.323*** **	**0.211*** **
	(0.054)	**(0.062)**	**(0.044)**	**(0.062)**	**(0.060)**	**(0.059)**
Educated	-0.006	0.028	0.004	0.045**	0.037*	0.065***
	(0.016)	(0.018)	(0.015)	(0.019)	(0.019)	(0.018)
Asset Index: Middle	-0.020	-0.012	-0.015	-0.025	-0.027	-0.042**
	(0.018)	(0.020)	(0.016)	(0.020)	(0.021)	(0.020)
Asset Index: Poor	-0.041*	-0.054**	-0.042**	-0.074***	-0.075***	-0.055**
	(0.022)	(0.024)	(0.018)	(0.025)	(0.027)	(0.024)
Male	0.026	0.048**	0.024	0.081***	0.064***	0.090***
	(0.018)	(0.020)	(0.015)	(0.018)	(0.021)	(0.027)
Distance Epicenter (km)	-0.001	0.000	-0.001	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Slope	-0.005**	-0.005**	-0.002	-0.000	0.001	0.008***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
District: Bagh	0.034	-0.098*	-0.027	0.074	0.038	0.051
	(0.050)	(0.057)	(0.042)	(0.071)	(0.071)	(0.061)
District: Mansehra	-0.000	-0.061	0.024	-0.067*	-0.120**	0.017
	(0.031)	(0.042)	(0.028)	(0.038)	(0.046)	(0.040)
District: Muzaffarabad	0.078**	-0.014	0.016	0.027	0.003	-0.069
	(0.036)	(0.045)	(0.034)	(0.047)	(0.056)	(0.051)
Constant	0.375***	0.426***	0.249***	0.374***	0.458***	0.225***
	(0.066)	(0.077)	(0.053)	(0.077)	(0.089)	(0.075)
Observations	4670	4670	4670	4670	4670	4670
R-squared	0.125	0.136	0.131	0.105	0.111	0.082

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

The IV Specification

	(1)	(2)	(3)	(4)
	Difference in Village trust: Local, General Foreigner	**Difference in Village trust: Local, European/American Foreigner**	**Difference in Village trust: Local, General Foreigner**	**Difference in Village trust: Local, European/American Foreigner**
Percent Village Reporting Foreigners came	**0.884*** **	**0.733*** **	**0.855*** **	**0.707** **
	(0.249)	**(0.243)**	**(0.285)**	**(0.279)**
Distance to Epicenter (km)	0.000	-0.000	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
Slope	0.001	0.002	0.002	0.003
	(0.004)	(0.004)	(0.005)	(0.005)
District: Bagh	-0.013	-0.045	0.010	-0.031
	(0.111)	(0.108)	(0.105)	(0.103)
District: Mansehra	-0.148**	-0.196***	-0.114	-0.171**
	(0.070)	(0.068)	(0.085)	(0.084)
District: Muzaffarabad	-0.184*	-0.190**	-0.159	-0.173*
	(0.095)	(0.093)	(0.099)	(0.097)
Percent Village Educated			-0.097	-0.040
			(0.252)	(0.247)
Village Average Asset Index			-0.032	-0.020
			(0.030)	(0.029)
Constant	0.057	0.140	0.032	0.109
	(0.109)	(0.106)	(0.174)	(0.171)
Observations	126	126	126	126
R-squared	0.233	0.246	0.264	0.266

Trust = f(aid, geographic controls), IV for aid using distance to fault-line

Regressions: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Trust: General Foreigner (Educated Only)	Trust: General Foreigner (Uneducated Only)	Trust: General Foreigner (Full Sample)	Trust: Euro/Amer. Foreigner (Educated Only)	Trust: Euro/Amer. Foreigner (Uneducated Only)	Trust: Euro/Amer. Foreigner (Full Sample)	Work Together (Educated Only)	Work Together (Uneducated Only)	Work Together (Full Sample)
Educated			**0.094*** **			**0.066** **			**0.106*** **
			(0.032)			**(0.032)**			**(0.027)**
Distance Fault-line (km)	**-0.007*** **	**-0.006*** **	**-0.006*** **	**-0.006** **	**-0.005** **	**-0.005** **	**-0.006*** **	**-0.002**	**-0.003** **
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.001)**	**(0.002)**	**(0.001)**
Educated* Distance-to-fault-line (km)			**-0.002* **			**-0.001**			**-0.002** **
			(0.001)			**(0.001)**			**(0.001)**

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

Regressions: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	**Trust: General Foreigner (Male Only)**	**Trust: General Foreigner (Female Only)**	**Trust: General Foreigner (Full Sample)**	**Trust: Euro/Amer. Foreigner (Male Only)**	**Trust: Euro/Amer. Foreigner (Female Only)**	**Trust: Euro/Amer. Foreigner (Full Sample)**	**Work Together (Male Only)**	**Work Together (Female Only)**	**Work Together (Full Sample)**
Male			**0.090*** **			**0.067***			**0.233*** **
			(0.030)			**(0.034)**			**(0.037)**
Distance To The Fault-Line (km)	**-0.007*** **	**-0.006*** **	**-0.006*** **	**-0.005** **	**-0.005** **	**-0.005** **	**-0.010*** **	**0.003**	**0.001**
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.001)**
Male* Distance To Fault-line (km)			**-0.001**			**-0.000**			**-0.008*** **
			(0.001)			**(0.001)**			**(0.001)**

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)

Robustness: General Aid Effect

	(1)	(2)	(3)	(4)	(5)	(6)
		Trust: Local			Trust: Foreign	
VARIABLES	Own Village	Own Caste	Own Region	General foreigner	European/American	Different people can work together?
Distance Fault-Line (km)	-0.000	0.001	-0.001	-0.006***	-0.005***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Percent Village Reporting Pakistani Organizations Came	-0.005	0.000	0.004	0.033	0.017	0.042

Is the trust in foreigners effect because *in general* there was greater aid in the villages closer to the fault-line?

Robustness: Risk Aversion?

	Trust: General Foreigner	Trust: General Foreigner	Trust: European or American Foreigner	Trust: European or American Foreigner	Work Together	Work Together
	(1)	(2)	(3)	(4)	(5)	(6)
Distance Fault-Line (km)	-0.007***	-0.008***	-0.006***	-0.008***	-0.005***	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Risk Aversion: Middle	-0.057**	-0.082**	0.005	-0.028	0.067***	0.146***
	(0.024)	(0.034)	(0.026)	(0.038)	(0.023)	(0.036)
Risk Aversion: Least	-0.033	-0.089*	0.017	-0.036	0.113***	0.165***
	(0.030)	(0.048)	(0.033)	(0.050)	(0.030)	(0.048)
Risk Aversion: Middle*Distance		0.002		0.002		-0.004***
		(0.002)		(0.002)		(0.002)
Risk Aversion: Least*Distance		0.003*		0.003*		-0.003*
		(0.002)		(0.002)		(0.002)

- Outcome=f(Distance to Fault Line, Distance to Epicenter, Slope, District Fixed Effects)
- Based on risk aversion game (problem: 25% of sample refused to play)

Trust groups

- We ask for the respondent's attitudes towards the following groups:
  - 1) People in general
  - 2) Extended family
  - 3) People in your village
  - 4) People in your *qaum*/caste/clan/*biradari*
  - 5) People in your region
  - 6) Other Pakistani
  - 7) General foreigner
  - 8) European/American
  - 9) Islamic foreigner.
- We present results categories 3), 4), 5), 7) and 8) .

An Investigation of the Psychological Impacts of Child Sponsorship

Phillip H. Ross
University of San Francisco

Paul Glewwe
University of Minnesota

Laine Rutledge
University of Washington

Bruce Wydick
University of San Francisco

What accounts for the large impacts of child sponsorship?

- Time Preferences
- Social Trust
- Self Esteem
- Reference Point Shifts
  - Educational Attainment
  - Adult Occupation

Sample Frame

- Currently Sponsored
- Non-sponsored siblings and peers
- India (29), Kenya (90), Bolivia (151)
  - Total: 270
- Age: 10-19
- Longer Survey in Bolivia
- Intelligence Proxies
  - All in India and Kenya
  - 81 in Bolivia

- Time Preferences
  - Developed: Hasuman (1979), Lawrance (1991), and Harrison, Lau and Williams (2002)
  - Developing: Pender (1996), Nielsen (2001), Yesuf (2004), Tanaka et al. (2010)
- Social Trust
  - Knack and Keefer (1997), Zak and Knack (2001), Fafchamps (2006), Tabellini (2007), Algan and Cahuc (2011)
- Self-Esteem
  - Goldsmith, Veum and Darity (1997), Munane et al. (2001), Bénabou and Tirole (2003), Waddell (2006), Drago (2008), Darolia and Wydick (2011)

Time Preferences

	"If you had a large unpleasant chore to do either today or next week, when would you choose to do it?" 1=Today 0=Next Week	If a trustworthy person were to offer you 100 (currency) today/tomorrow or 110 (currency) next week, which would you prefer? 1=Next week 0=Tomorrow/Today	If a trustworthy person were to offer you 100 (currency) today/tomorrow or 150 (currency) next week, which would you prefer? 1=Next Week 0=Tomorrow/Today
CSP	-0.021 (0.013)* [0.014]*	-0.022 (0.063) [0.062]	-0.009 (0.074) [0.073]
Observations	268	249	249

Logit marginal effects, Controls for gender, age, age-squared, and village fixed effects,
Standard errors in parentheses, robust SE's in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

- Time Preferences
  - Developed: Hasuman (1979), Lawrance (1991), and Harrison, Lau and Williams (2002)
  - Developing: Pender (1996), Nielsen (2001), Yesuf (2004), Tanaka et al. (2010)
- Social Trust
  - Knack and Keefer (1997), Zak and Knack (2001), Fafchamps (2006), Tabellini (2007), Algan and Cahuc (2011)
- Self-Esteem
  - Goldsmith, Veum and Darity (1997), Munane et al. (2001), Bénabou and Tirole (2003), Waddell (2006), Drago (2008), Darolia and Wydick (2011)

Social Trust

	"Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?" 1=Most can be trusted 0=Can't be too careful	Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair? 1=Most would try to be fair 0=Most would try to take advantage	Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? 1=Most of the time people try to be helpful 0=People are mostly looking out for themselves
CSP	-0.024 (0.046) [0.046]	0.087 (0.077) [0.079]	0.086 (0.071) [0.072]
Observations	268	267	265

Logit marginal effects, Controls for gender, age, age-squared, and village fixed effects,
Standard errors in parentheses, robust SE's in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

- Time Preferences
  - Developed: Hasuman (1979), Lawrance (1991), and Harrison, Lau and Williams (2002)
  - Developing: Pender (1996), Nielsen (2001), Yesuf (2004), Tanaka et al. (2010)
- Social Trust
  - Knack and Keefer (1997), Zak and Knack (2001), Fafchamps (2006), Tabellini (2007), Algan and Cahuc (2011)
- Self-Esteem
  - Goldsmith, Veum and Darity (1997), Munane et al. (2001), Bénabou and Tirole (2003), Waddell (2006), Drago (2008), Darolia and Wydick (2011)

Self-Esteem

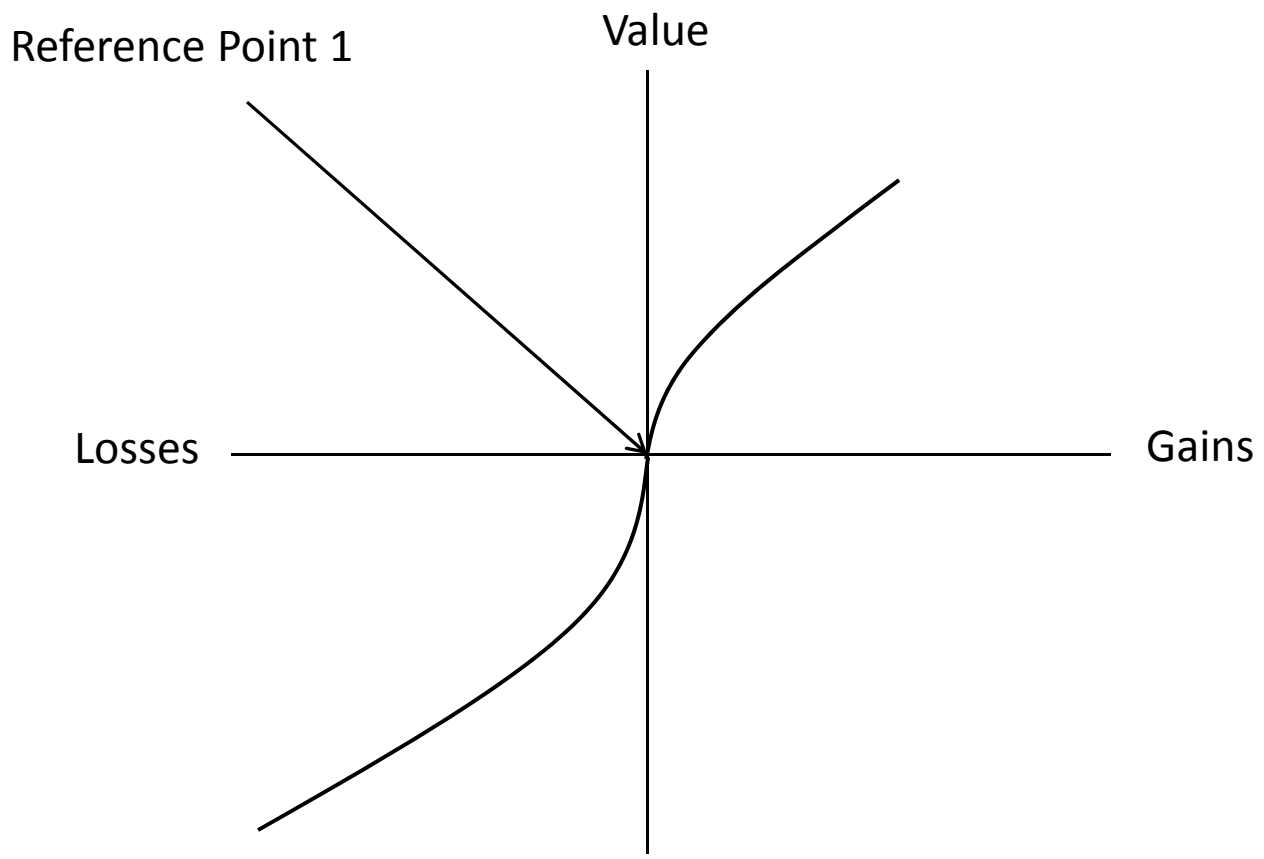
	I feel that I'm a person of worth, on an equal plane with others.	I am able to do things as well as most other people.	I feel I do not have much to be proud of.	On the whole, I am satisfied with myself.	At times I think I am no good at all.	Drawing Ranks 1=lower self-esteem 3=higher self-esteem
	SA=4, SD=1	SA=4, SD=1	SA=1, SD=4	SA=4, SD=1	SA=1, SD=4	
CSP	0.132 (0.173) [0.175]	-0.248 (0.165) [0.156]	0.120 (0.151) [0.166]	0.002 (0.178) [0.183]	0.130 (0.152) [0.160]	0.238 (0.355) [0.373]
Observations	269	269	266	268	263	54

Ordered probit coefficients, Controls for gender, age, age-squared, and village fixed effects,
Standard errors in parentheses, robust SE's in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

Reference Points

- Prospect Theory (Kahneman and Tverksy, 1979)
  - Tversky and Kahneman (1991, 1992)
  - Theoretical: Schmidt (2003), Schmidt, Starmer, and Sugden (2008), Koszegi and Rabin (2006, 2007)
  - Empirical: Bateman et al. (1997), Camerer et al. (1997), Barkan and Busemeyer (1999), Genesove and Mayer (2001), Kievtz (2003), Hart and Moore (2008), Farber (2008)
- Goals as Reference Points (Heath, Larrick, and Wu, 1999)
- Abeler, Gotte, Falk, and Huffman (2011)
  - Rational expectations shift reference point

Higher reference points (goals, aspirations) leave a person in the domain of losses longer where the marginal benefits of persisting are higher.



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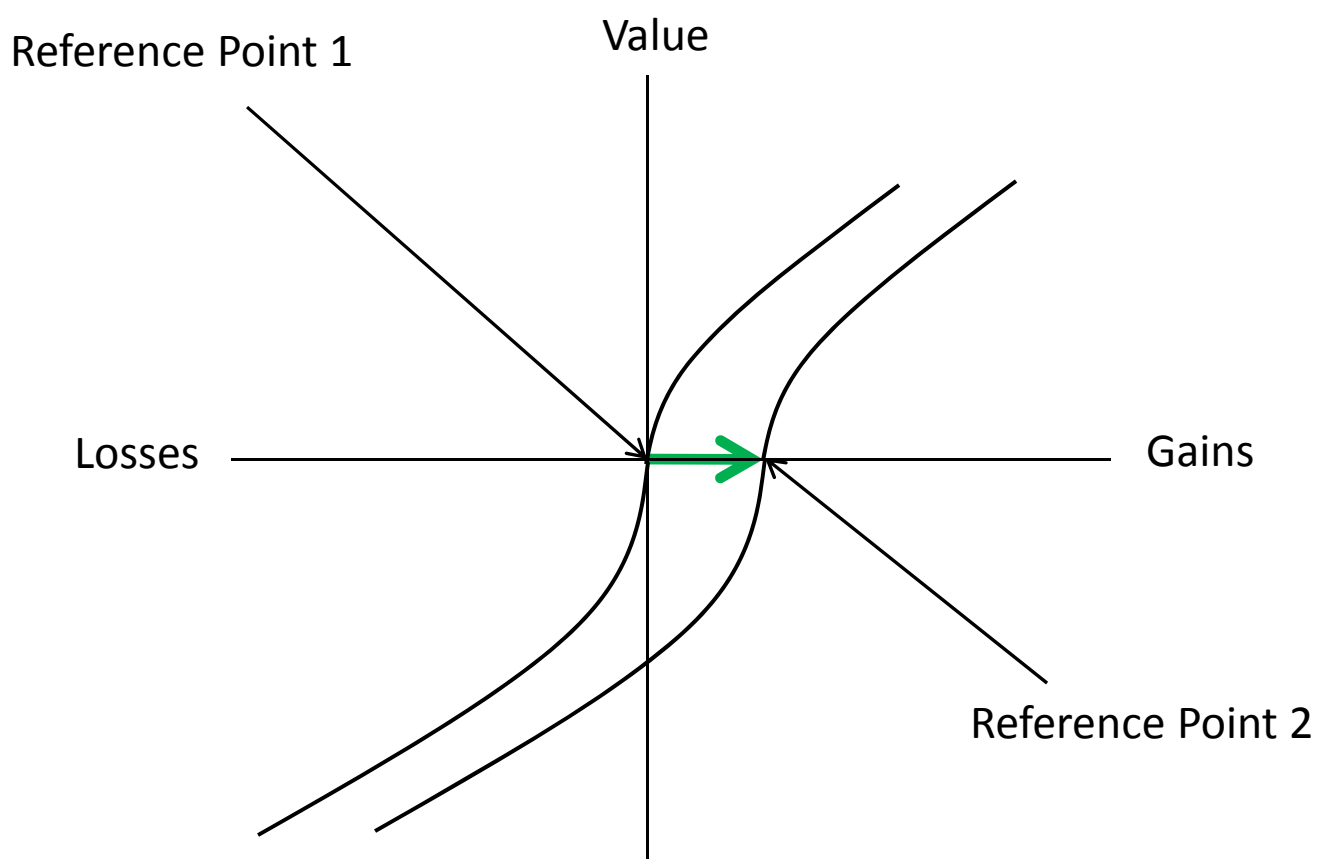

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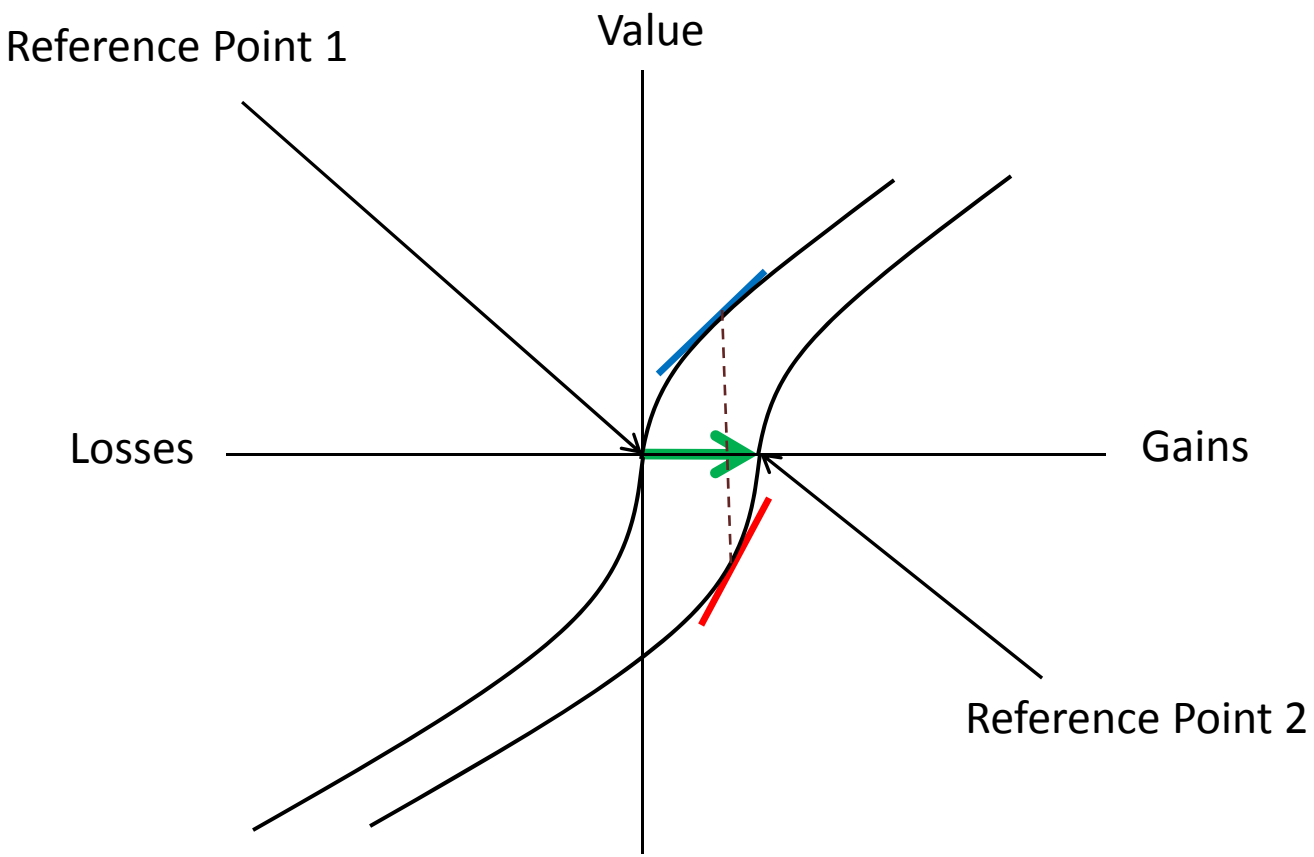

What level of education would you say is sufficient in order for one to be successful today?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS (Years of Education)	0.411 (0.259) [0.268]	0.461 (0.269)* [0.304]	0.560 (0.313)* [0.410]	0.798 (0.344)** [0.401]	1.091 (0.368)*** [0.447]*	1.140 (0.691) [1.257]	1.239 (0.533)** [0.977]	1.415 (0.562)** [1.147]
Ordered Probit Coefficients (Level of Grade Completion)	0.216 (0.174) [0.150]	0.249 (0.178) [0.153]	0.340 (0.215) [0.192]*	0.445 (0.277) [0.264]*	0.586 (0.244)** [0.223]***	0.525 (0.323) [0.310]*	0.625 (0.406) [0.381]	0.499 (0.412) [0.408]
Marginal Effect, highest level of education	0.066 (0.053) [0.046]	0.078 (0.056) [0.048]	0.084 (0.056) [0.050]*	0.172 (0.105) [0.100]*	0.225 (0.090)** [0.082]***	0.207 (0.124)* [0.119]*	0.243 (0.154) [0.146]*	0.195 (0.159) [0.159]*
Individual	X	X	X	X	X	X	X	X
Parent's Occupation		X	X	X	X	X	X	X
Intelligence			X			X	X	X
Parent's Education					X	X	X	X
Family/Peer Expectations				X	X		X	X
Sibling Occupation					X	X	X	X
Propensity Score								X
Countries	3	3	3	1	1	1	1	1
Villages	7	7	5	5	5	3	3	3
Observations	270	259	189	144	133	76	71	70

Village level fixed effects, Standard errors in parentheses, Robust SE's in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

Occupation

1= Professional occupation requiring some college 0= Otherwise	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
What occupation do you *realistically expect* in the future?	0.136 (0.069)** [0.068]**	0.119 (0.070)* [0.069]*	0.148 (0.075)** [0.073]**	0.121 (0.078) [0.076]	0.177 (0.104)* [0.098]*	0.251 (0.095)*** [0.094]***	0.229 (0.085)*** [0.090]**	0.236 (0.096)** [0.098]**
What occupation do you *hope* to have in the future?	0.065 (0.065) [0.069]	0.053 (0.065) [0.070]	0.047 (0.071) [0.072]	0.017 (0.072) [0.074]	0.110 (0.099) [0.103]	0.080 (0.110) [0.107]	0.085 (0.102) [0.104]	0.053 (0.117) [0.122]
Individual	X	X	X	X	X	X	X	X
Parent's Occupation		X		X	X	X	X	X
Intelligence			X	X		X	X	X
Parent's Education					X		X	X
Sibling Occupation					X	X	X	X
Propensity Score								X
Countries	3	3	3	3	1	1	1	1
Villages	7	7	5	5	5	3	3	3
Observations	270	259	200	189	139	81	76	75

Logit marginal effects, Village level fixed effects, Standard errors in parentheses, Robust SE's in brackets,
* significant at 10%; ** significant at 5%; *** significant at 1%

Findings

- No evidence of impact on time preferences and trust
- Mild evidence of an effect on self-esteem
- Moderate evidence of a positive association between sponsorship and a shift in the reference point for education and occupation
  - Educational Attainment: 0.461 to 1.415 years
  - Occupation Expectation: 11.9 to 25.1 percentage points

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  - Educational Attainment: 0.461 to 1.415 years
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Conclusions

- Emotional support contributes to internal reference point shift
- Complements tangible inputs

Further Investigation

- Kenya
  - Several programs rolled out ≈ 5 years ago
  - Limit of 1 sponsored child per household
- Mimic design of Wydick, Glewwe, and Rutledge (2010)
  - Regression Discontinuity
  - Eligibility IV

Does International Child Sponsorship Work? A Six-Country Study of Impacts on Adult Life Outcomes

Bruce Wydick*
Paul Glewwe**
Laine Rutledge***

November 20, 2010

Abstract: International child sponsorship is one of the leading forms of direct aid from households in wealthy countries to needy children in developing countries, where we estimate that 8.36 million children are currently supported currently through formal international sponsorship organizations. In this paper we present results from a six-country impact study of Compassion International, a leading child sponsorship organization, in which we obtain first-hand household survey data from 10,144 individuals in Uganda, Guatemala, the Philippines, India, Kenya, and Bolivia. We utilize an age-eligibility rule imposed as programs were introduced across different villages in the six countries from 1980 to 1992 to achieve statistical identification of impact on adult life outcomes of formerly sponsored children relative to their ineligible older siblings. Using household fixed-effects to control for genetics, family environment and household selection, and an instrumental variable that utilizes sibling order relative to program rollout to control for intra-household endogenous child selection, we find that sponsorship results in 2.42 additional years of formal education, and large and statistically significant impacts on the probability of employment, occupational choice, age of marriage and child-bearing, community leadership, and dwelling quality. We also find evidence of positive spillover effects on many of these variables to siblings and other village residents of the same age. We believe that the results of this study may have important policy insights: that the transformation of reference points and self-expectations among children in developing countries may be strong complements to the provision of educational and economic opportunities.

*Wydick, Professor, Department of Economics, University of San Francisco, 2130 Fulton Street, San Francisco, CA 94117-1080, e-mail: *wydick@usfca.edu*; **Glewwe, Professor, Department of Applied Economics, University of Minnesota, 1994 Buford Ave, St. Paul, MN 55108, e-mail: pglewwe@umn.edu; ***Rutledge, doctoral student, Department of Economics, University of Washington, 326 Savery Hall, Seattle, WA 98195-3330, e-mail: *lmrutledge@gmail.com*.

We would like to thank Wess Stafford, Joel Vanderhart, Scott Todd, Herbert Turyatunga, Jose-Ernesto Mazariegos, Ester Batz, Noel Pabiona, Rowena Campos, Sofia Florance, Catherine Mbotela, Sam Wambugu, Boris Zegarra, Marcela Bakir and other local Compassion staff and enumerators in Uganda, Guatemala, the Philippines, India, Kenya, and Boliva for logistical help and support in carrying out our field research. Thanks to graduate students Joanna Chu, Ben Bottorff, Jennifer Meredith, Phillip Ross, and Herman Ramirez for outstanding work in the field. We also appreciate support and extremely helpful comments from Christian Ahlin, Chris Barrett, Michael Carter, Alessandra Cassar, Pascaline Dupas, Giacomo De Giorgi, Alain de Janvry, Fred Finan, Pauline Grosjean, David Levine, Jeremy Magruder, Craig McIntosh, Ted Miguel, Jeff Nugent, Jon Robinson, Elizabeth Sadoulet, John Strauss, seminar participants at the University of California at Berkeley, Stanford University, the University of Southern California, the Georgia Institute of Technology, the Pacific Conference for Development Economics, and the Behavior and the Escape from Persistent Poverty (IBEPP) conference at Cornell University. We are grateful to BASIS/USAID for substantial funding for this project, to two generous South Korean donors, and to the University of San Francisco's graduate program in International and Development Economics.

1. Introduction

For millions of households in wealthy countries, international child sponsorship programs represent the most intimate and direct form of involvement with the poor in the developing world. Although we estimate that child sponsors currently give $3.2 billion to these programs each year, to date there has been no published research that has attempted to gauge the impact of these programs on the life outcomes of sponsored children. In this paper, we present an impact study on individuals formerly sponsored through Compassion International, a leading child sponsorship organization currently serving just over one million children in 25 countries. The data for our study on the adult life outcomes of 10,144 individuals was collected first-hand over a two-year period in six developing countries: Uganda, Guatemala, Philippines, India, Kenya, and Bolivia.

Child sponsorship programs involve a set of monthly financial contributions to a needy child in a developing country. Monthly contributions from sponsors usually range from US$25-$40. The Compassion program is fairly typical in its application of funds to sponsored children. Funds are applied either directly towards the child's education, food, and health expenses, or to support projects or programs in which the child participates and benefits. In the Compassion program, an important focus of the latter is the spiritual, social, and emotional development of sponsored children. Sponsors play a role in this process, and are encouraged to exchange letters and photos with their child. Sponsors often give their sponsored children gifts for birthdays and Christmas, and even visit them in their home countries. This sponsor-child relationship typically continues until the child finishes secondary school or is married.

There has been no previous accounting of the number of internationally sponsored children worldwide. A preliminary task of our research was to estimate this figure. Through internet searches in multiple languages and contact with industry personnel across countries, we tallied 207 organizations that we believe represent nearly all of the children sponsored through such organizations worldwide. From the level of sponsorship officially claimed by these organizations, we estimate that there are currently 8.36 million internationally sponsored children in the world today.[1] A large fraction of this total is made up of the sponsorship rolls of the largest several sponsorship organizations. Table 1 contains basic information about the ten largest organizations, including years of operation, countries served, monthly sponsorship fees, and sponsorship totals of these largest organizations that comprise the lion's share of the industry. Virtually all are based in the United

[1] Because the internet is so vital today for fundraising in the child sponsorship industry, posting pictures of children, and other contact between potential sponsors and potentially sponsored children, we assumed (correctly, we believe) that any such organization of significant scope must maintain a presence on the internet. This assumption is the basis on which we have accounted for almost all child sponsorship organizations and their respective numbers of sponsored children.

States and Europe, and two of the largest three are faith-based as are four of the largest ten. Based on the average family sizes of sponsored children in our study and of those in sponsoring countries, we estimate that 90 million people in the world today belong to households involved in child sponsorship, approximately 59 million people in households with a sponsored child, and approximately 31 million people in households that sponsor a child.

With the average monthly sponsorship level set at about $30 (not including other gifts sent to sponsored children), we estimate the flow of resources from wealthy countries to poor countries from international child sponsorships to be about $3.2 billion per year. Given these non-trivial flows of resources, it is surprising that so little work has been carried out to evaluate the impact of these programs. One exception is Kremer, Moulin, and Namunyu (2003), who used a randomized field experiment to analyze the short and medium-term impacts of a Dutch child-sponsorship program that funded new classroom construction and provided students in randomly selected schools a $6 uniform and $3.44 in textbooks. Kremer et. al. find that even these relatively low-cost interventions resulted in student beneficiaries attending school half a year longer than in control schools, and advancing a third of a grade farther in formal education.

To survey formerly sponsored children and their families, we obtained enrollment lists of children sponsored during the initial years of operation of each of our 19 program sites across our six countries. With the aid of local village[2] residents we were able to locate 93.5% the families of these formerly sponsored children (now adults) to conduct an interview with their family members, consisting of questions about basic life outcomes of the sponsored child and his or her siblings. We obtained this data on formerly sponsored children and all of their siblings: years of formal education, type of current employment, remittances sent to other family, marriage and childbearing, community leadership positions, basic information on the quality of their homes and dwelling construction, and ownership of major consumer goods such as a cell phone, bicycle, motorcycle, or automobile, and purchases of land. We also asked these questions to a random sample of other households in the same villages, and to a random sample of households in 12 similar, nearby non-program villages. Overall, our six-country survey obtained data on 1,860 formerly sponsored children, 3,704 of their unsponsored siblings, 2,136 individuals from non-Compassion families in treated villages, and 2,444 individuals from similar and nearby non-program villages.

Two potential challenges with obtaining unbiased impact estimates of this type of program are the selection of households into the program based on unobservable characteristics that may be

[2] We will use the term villages for the site of Compassion projects for the sake of brevity, although a few of the projects were located in urban areas such as Bangalore, India and Cochabamba, Bolivia.

correlated with impact measures, and the possibility of non-random selection of children within a particular household for sponsorship. We address the issue of endogenous household selection through the use of a household fixed effect in our estimations, which also allows us to control for genetics, parental characteristics, and general household environment in our impact identification.

To address the issue of endogenous child selection within families we make use of two program eligibility rules in order to identify impacts. As Compassion rolled out projects between 1980 to 1992 in all of our six countries, there was an age-eligibility rule stipulating that only children 12 years and younger at the time of project rollout were eligible for sponsorship (11 years and under in Uganda and Guatemala). This arbitrary age-eligibility rule suggests the use of a regression discontinuity design to help identify the causal impacts of the program on adult life outcomes of formerly sponsored children relative to the life outcomes of their ineligible older siblings, whose age happened to lie on the other side of the eligibility rule when the program entered their village.

But the use of regression discontinuity still leaves open the possibility of an endogenous choice between all of the eligible children within a family. However, in order that a maximum number of households might benefit from sponsorship, a second rule imposed by Compassion country directors limited sponsorship to a given number of children per household. In response to the child-limit-per-family rule, we find that in practice the *oldest age-eligible siblings* were the individuals far most likely to be chosen for sponsorship in a family. Thus we are able to utilize a child's birth order around the time a project was introduced in his or her village as an instrument to address potential endogeneity issues of intra-household child selection.

Our estimations find large and statistically significant effects of child sponsorship on a broad array of these life outcomes. Using an estimation that accounts for possible spillovers to younger siblings (but not potentially endogenous child selection) we find that child sponsorship results in an average of 1.53 additional years of formal education to sponsored children (t = 11.08). Instrumental variable estimates that account for endogenous child selection show an impact on the sponsored child of 2.42 years (t = 6.41). Using a pure eligibility instrument that includes spillover impacts to other children of eligible age we obtain an impact estimate of 2.85 years of additional schooling per sponsored child (t = 7.40), such that we find there to be a positive spillover of slightly less than half a year of education from the program spread across eligible, non-sponsored children.

The educational impact on sponsored children across our six countries appears to be driven largely by counterfactuals. In the countries where existing (counterfactual) levels of formal schooling were low, we find bigger impacts of the sponsorship program than we do in places where the counterfactual was high. In places where schooling was higher among boys, we find bigger impacts from the program among girls. Where it was higher among girls, we find bigger impacts on boys.

We believe that provision of educational inputs and subsidies in tandem with the program's emphasis on the nurture of children and the re-orientation of their self-expectations and reference points drive many of the impacts of child sponsorship on life outcomes.

We find many other impacts on adult life outcomes across our six countries that are both large and statistically significant. Our OLS and instrumental variable estimations find that child sponsorship resulted in a 19.6 and 32.6 percentage point increase, respectively, in the probability of secondary school graduation, with significant spillovers to younger siblings, a 7.1 and 17.3 percentage point estimated increased probability of white collar employment, a 7.3 and 8.0 percentage point increase in the probability of sending remittances back to family, marriage and childbearing moved significantly to later in age, a greater probability of living in a house with electricity and an improved floor, and roughly double the probability of being a church, community or village leader.

To better understand the mechanisms that underlie the impacts of the sponsorship program, we carried out a smaller survey among 278 currently sponsored teenagers in India, Kenya, and Bolivia, siblings of these sponsored children, and random children of the same age in the same villages. Our research with the existing sponsored children sought to ascertain whether levels of self-esteem, delayed gratification, hopes and aspirations, and educational and career-based reference points differed between sponsored children, siblings and peers. Through this exercise we find few differences between sponsored children and others with one major exception: reference points and self-expectations about education and vocation among sponsored children are markedly higher.

The remainder of our paper continues as follows. Section 2 of the paper reviews some of the previous findings in the literature relevant to our study. Section 3 explains our fieldwork methodology and data collection. Section 4 describes our estimation strategy, and section 5 presents our impact results and compares them with the impact results of CCT (conditional cash transfer) programs. Section 6 provides a summary of our research results of our smaller study on currently sponsored children, and section 7 summarizes and reflects on our findings. We find that the child sponsorship program we analyze is approximately three times as cost effective at generating added years of formal education as the impacts that have been recently estimated for CCT programs in development countries. We believe that the significant difference between the impacts we observe from child sponsorship and CCT programs is that whereas the approach of conditional cash transfers is primarily to relieve economic constraints, a key component of child sponsorship programs is the release of inward psychological constraints associated with poverty and the intentional reorientation of a child's reference points with regard to achievements in education, vocation, and leadership.

2. Existing Research and Literature

A growing literature has sought to ascertain cost-effective methods to induce households to increase investment in children's education. Different programs have used cash transfers, the lure of free meals, the provision of school uniforms, deworming treatments, or free medical treatment in order to provide incentives for families to keep (or send) their children to school.

The widely celebrated (and widely evaluated) CCT program, *Progresa* (later renamed *Oportunidades*), was implemented in 1997 by the Mexican government to create financial incentives for families to boost school attendance in economically poor regions. *Oportunidades* provides cash transfer payments to mothers conditioned on children continuing in school. The rollout of *Oportunidades* was undertaken randomly to facilitate its evaluation by researchers. Impact evaluations have shown that access to the program is associated with higher school enrollment rates, lower grade repetition and better grade progression, lower dropout rates, and higher school reentry rates among dropouts (Behrman, Sengupta, and Petra, 2005).

For example, Behrman, Parker and Todd (2005) estimate that receiving cash transfers from the *Oportunidades* program in Mexico for two years increased years of schooling by 0.20 years. Schultz (2004) estimates that *Oportunidades* program in Mexico increased formal schooling by an average of 0.66 years (0.72 for girls, and 0.64 yrs for boys). CCT payments in his study ranged anywhere from $4.62 to $21.67 per month, and are received from the third to the eleventh years of school.

Schultz finds enrollment rates to be higher in villages with *Oportunidades*, where its impact was positive from grades one through eight. Using a difference-in-difference estimation, he finds that, averaging over all children, enrollment increased by 3.4 percent. He also finds that the impact was much higher in later grades, and among children in those later grades it was larger for girls (14.8 percent) than for boys (6.5 percent). Bobonis and Finan (2008) observe a 5 percent increase in enrollment in *Oportunidades* program communities, even among those ineligible for the program. Barerra-Osorio et. al. (2008) implement a randomized field experiment in the context of the *Conditional Subsidies for School Attendance* program in Bogota, Colombia, finding that the overall average effect was to increase school attendance by 2.8 percentage points.

Other programs have sought to subsidize different kinds of inputs to schooling. These inputs have ranged from providing free or subsidized school meals to providing uniforms, textbooks, school construction, and teachers. Drèze and Kingdon (2001) find that providing a mid-day meal increased female attendance by 15 percent in a study in northern India. Similarly, Kremer and Vermeersch (2004) estimate that school attendance increased by 8.5 percentage points in preschools that provided free meals, affecting both current students and new students who had never before attended school.

Handa and Peterman's (2007) work in South Africa measured the effect of poor nutrition on schooling, finding that educational attainment of children was strongly affected by nutritional status.

Evans, Kremer, and Ngatia, (2008) randomly selected children by lottery to receive free uniforms in a program administered by an NGO operating in Kenya. They find that receiving a school uniform reduced overall school absenteeism by 39 percent and by 64 percent for poorer students, who did not previously own a uniform. In a similar geographic area in Kenya, a de-worming medical intervention was implemented in randomly selected schools. Miguel and Kremer (2004) discover that this intervention not only decreased overall disease transmission but also helped to reduce school absenteeism by 7 percentage points in the treatment schools. In addition, they find positive spillover effects to children who attended nearby schools that did not receive the de-worming intervention.

In a randomized experiment that provided girls in the sixth grade merit scholarships of approximately $20 to pay for school fees and school supplies, Kremer, Miguel, and Thornton (2008) estimate that the intervention increased student attendance by 5 percentage points, and in successful districts, it led to a significant increase in both girls' *and* boys' test scores.

Still other experimental studies have attempted to provide incentives to teachers to improve education quality. In response to high teacher absenteeism rates, Glewwe, Ilias, and Kremer (2010) carried out an experimental intervention in Kenya that provided monetary bonuses to teachers based on student test scores. Despite the incentives, teacher attendance did not improve; instead teachers held additional prep sessions prior to the exams on which the incentives were based, which led only to a short-term increase in test scores.

Methodologically, the empirical strategy used in this paper is similar to that of Duflo (2001) in the sense that we use the age of former students and geographic placement of a schooling treatment as instruments for impact identification. Her study examines the impact of a dramatic expansion of school construction financed by the Indonesian government from 1973-1979. In that time span, over 61,000 schools were constructed. Duflo uses an individual's exposure to the program, which was measured by the number of schools built in his or her region of birth, along with age at the time of program inception, to identify impacts on education and wages. She finds that the program increased the probability of primary school completion by 12%, and that each new school constructed per 1,000 children contributed to an increase in formal education of 0.12 to 0.19 years. This implies an average increase of between 0.25 and 0.40 years per child beneficiary, which then resulted in an increase of between 3.0 and 5.4 percent in wages, suggesting an economic return to education of 6.8 to 10.6 percent. Moreover, she also finds that those who benefited were among the poorest, and would not have had access to a primary school education otherwise.

3. Program, Area of Study, and Data Collected

3.1 The Compassion Child Sponsorship Program

Compassion International is a large, faith-based, non-profit organization whose stated goal is to "release children from spiritual, economic, social, and physical poverty." The benefits children receive through sponsorship vary somewhat by country, and even within countries, and Compassion's approach has evolved over time. Table 2A gives a concise description of the benefits sponsored children received by country during the time of enrollment in the program. In Uganda, Kenya, and in three of the projects we studied in Bolivia, Compassion operated student centers, where sponsored children gathered several days during the week after attending their own school or on Saturdays. Students participated in a structured program at the student centers where they received academic tutoring, spiritual instruction, healthcare, nutritious meals, school supplies, and where they participated in a wide array of games and activities. In most of the projects we studied with student centers, Compassion children received subsidized school fees and school uniforms.

In Guatemala and the Philippines, Compassion operated its program through (Protestant) Christian schools, where students would receive a similar array of benefits, although a tutoring program was not generally an explicit component of sponsorship. In India and one of our projects in Bolivia, Compassion partnered with government programs that gave direct monetary aid to families conditional upon the sponsored child's continuity in school. However, these programs differed from the standard conditional cash transfer program in that children received most of the same benefits received at the other types of projects, as well as individual nurturing and care via Compassion's partnership with local Protestant churches.

In all projects children received basic healthcare benefits. This included regular physical examinations at Compassion schools and student centers by local nurses and physicians. These benefits also included a form of catastrophic health insurance paid through a separate fund administered from the organization's headquarters in Colorado. If a Compassion child suffered from a serious illness or needed surgery, the full cost of the procedure and hospitalization was covered through this fund. For those who needed such care, this benefit was often reported by formerly sponsored children to be the greatest source of support offered by the program.

All children sponsored through Compassion write letters several times a year to their sponsor and most receive correspondence from their sponsor in return (71.8% in our study). Along with the letters, roughly once a year sponsors receive a picture of the child and updated news from local Compassion staff about the child's progress in school. Most children (83.7% in our study) also received birthday gifts from their sponsor. Sponsors are also able to travel on organized overseas trips to visit their sponsored children and their families; while not uncommon, this was not the norm.

As part of our survey, we included an open-ended question asking which component of the Compassion program formerly sponsored children believed had been most beneficial to them. The most common answer given was educational support (38.5%). (Within this category the payment of school fees and the helpfulness of tutoring were cited almost equally.) The second-most common response related to spiritual and character development (29.4%), followed by the economic aid of the program (9.5%--a figure which was no higher in the countries in which direct cash payments were made to families), the benefits of the healthcare program (2.8%), and the gifts given to them by sponsors (0.8%).

In all of Compassion's international projects, selection of children into the sponsorship program is determined at the local level. The official Compassion manual instructs its program staff to work with local families to select children according to the following criteria:

1) Sponsored children are to be from needy, low-income families. The official selection criteria writes, "When only a percentage of the children are sponsored from an institution, the school or parent committee should choose children among the neediest families for sponsorship."

2) Children who are orphaned, living with a widowed parent or other family member, or who are refugees are given special priority.

3) The child cannot have been sponsored by another agency.

4) Both children from Christian families and non-Christian families may participate equally, but families must allow children to participate in the Christian religious instruction of the program.

5) Children of kindergarten age, first grade, second grade, and third grade will receive priority, with older children (still aged 12 and under) a second priority.

This final rule was implemented to lengthen the number of years that a child is able to be sponsored. The mean duration of sponsorship in our study was 9.25 years.

3.2 Survey and Fieldwork

The survey work over the six countries in our study took place from June 2008 to August 2010. A list of villages in each country, rollout years for each project, sample sizes for each country, and the dates of each survey can be seen in Table 2B. Our sample frame included formerly sponsored children who had been enrolled in the years of the local Compassion project and all of their siblings. In addition, we randomly surveyed 50 to 75 households in each Compassion village, conditional on the presence of an individual in the household born in the ten years before the Compassion project began operation, along with their own siblings.

To obtain our sample of treated individuals, we obtained enrollment lists in the years village projects began during 1980 to 1992. In some projects these lists were kept on an electronic database at the country office, and in others the lists had been filed away at the projects themselves and kept in hard copy. We used enrollment lists of children in the first two to three years of the program. Some

projects started on a large-scale, enrolling up to 100 children in the first year. Other projects started with a smaller number of children, enrolling only 20 to 30 children in the first year. For the larger projects, we randomly selected individuals from the enrollment lists to be surveyed. For the smaller projects, we surveyed a population sample of the children enrolled in the first years. Two of the 19 projects we studied were no longer sponsoring children through Compassion, but did from 1980-92.

In order to locate formerly sponsored individuals on the early enrollment lists, we hired local assistants. These individuals were typically recommended by project staff, and were known to be responsible, well-respected members of the community. They were also individuals who had been raised in the village and were knowledgeable about the community. Through the help of these local assistants, we were able to locate all but a few households in Uganda, Guatemala, and Kenya, and a slightly lower fraction in the other three countries. Overall, we were able to locate 93.5% of the families of formerly sponsored children with names on enrollment lists when Compassion began its program in their village.

We also obtained a random sample of non-Compassion families within the treatment villages and in one village adjacent to or nearby to each Compassion village without a sponsorship program. These households were selected through the following randomization rule: A starting point in the village was randomly chosen, and then every third household was selected to survey. The household was surveyed if an individual in the house met the aforementioned age criteria. When we reached the end of the street or block, we turned left and continued with every second or third household, then turned right and proceeded in this way, beginning with a new random point in the village on different days. This same randomization technique was used in the control villages.

Our survey obtained basic information regarding level of formal schooling, type of employment, age at marriage, number of children, whether or not the sibling holds a church or community leadership position, construction material of his or her home, and ownership of basic consumer durables such as a cell phone, bike, motorcycle, or automobile, as well as home and land ownership. Basic statistics on this data and control variables can be seen in Table 3. All questions were designed to be simple and discrete, in line with our empirical approach that sought to identify the basic life outcomes of adult individuals that would be common knowledge between family members. Except for our income data, we eschewed questions that asked for finely tuned or continuous variable data on individuals, realizing that with this kind of follow-up methodology such data may be unavailable to family members, or inaccurate. Many of our questions were obtained from modules in the World Health Organization general survey. We also borrowed from the World Bank's Living Standards Measurement Study (LSMS) education modules. In the case of formerly sponsored children, we interviewed all available family members jointly in the household regarding

the life outcomes of the formerly sponsored child and siblings. We obtained data on the primary person answering the questions--the mother or father 36.6% of the time, the formerly sponsored child 35.8% of the time, a sibling (22.4%), or another relative (5.2%). We found no statistically significant effects on impact variables by category of main respondent.

4. Empirical Methodology

We employ a variety of estimation techniques to identify the impact of Compassion's sponsorship program: non-parametric estimation, regression discontinuity, OLS (ordinary least-squares) household fixed-effects estimation, and an IV (instrumental variables) estimation that incorporates the eligibility rules of the program with household fixed effects.

Figure 1 shows a non-parametric estimation (Epanechnikov kernel; bandwidth = 1) of the discontinuity in the probability of a child being sponsored for treated households given his or her age when the program was introduced in the local village. As can be seen in the diagram, a child had a 40 to 50 percent probability of sponsorship if he or she was 12 or under when the program entered the village, but virtually no chance of being sponsored if over 12 years old. (According to Compassion staff, a few children over 12 were sponsored because children's photos had been taken and they were placed up for sponsorship when of eligible age, but it took longer than expected to obtain a sponsor.)

Figures 2a through 2f show kernel density functions of all six countries that give probability densities over varying numbers of years of formal schooling for Compassion-sponsored individuals, their ineligible older siblings, their younger siblings, and untreated members of their village. While there is some heterogeneity across countries, the differences in density functions between formerly sponsored children and the other groups is striking; elementary school dropout is virtually eradicated by the program and the vast majority of density is massed over secondary school completion or greater. Younger siblings of sponsored children in many cases appear to have better educational outcomes than their non-sponsored older siblings or non-treated individuals. Figures 2g and 2h show kernel density functions for all treated individuals, and non-treated individuals, respectively. The diagrams illustrate how the density function of educational outcomes clearly lies to the right for sponsored children over non-sponsored children, although without controls.

Figures 3 and 4 again use non-parametric estimations to illustrate two important differences in life outcomes between children based on their age around the eligibility rule when the program was introduced in their village. Figure 3 illustrates a jump of approximately one year in education among the eligible population in treated households. Given that the fraction of treated children and treated households is equal to 0.341, this suggests an impact on formal education of approximately three years, which may include spillover effects to younger siblings of former Compassion children.

Figure 4 illustrates the discontinuity in the probability of white-collar employment around the eligibility rule. It suggests a jump by about 4 percentage points in the probability of a child in a treated family who is eligible for the program having a white collar job as an adult relative to older siblings who were ineligible for the program. Given the approximately one-third of these former children that were treated, the discontinuity suggests about a 12 percentage point increase in the probability of white-collar employment.

A primary concern with identifying the impact of the sponsorship program is to account for potential endogeneity in *a*) the selection of households into the program; and *b*) the selection of children with a particular household into the program. To address the former concern, we use an OLS estimator with household fixed effects, where we control for age, age-squared, gender, birth order, and a dummy variable for oldest child, variables that we believe concisely describe the child's position within the family. We can thus estimate

$$y_{ij} = \alpha_j + \gamma T_{ij} + \boldsymbol{X}'_{\boldsymbol{ij}}\boldsymbol{\beta} + e_{ij}, \qquad (1)$$

where T indicates a dummy variable for former sponsorship, which we label CSP (Compassion Sponsorship Program) of individual i in family j, α_j is a household fixed effect, $\boldsymbol{X}_{\boldsymbol{ij}}$ is the previously described vector of sibling-level control variables, and e_{ij} is the error term. In most of our OLS household fixed effect estimations we include a variable YS_{ij}, for younger sibling, which can potentially estimate spillovers from the program to unsponsored younger siblings while creating a purer counterfactual to treatment consisting of solely older, nearly all ineligible siblings:

$$y_{ij} = \alpha_j + \gamma T_{ij} + \delta YS_{ij} + \boldsymbol{X}'_{\boldsymbol{ij}}\boldsymbol{\beta} + e_{ij}, \qquad (1')$$

Thus, controlling for family birth order, gender, and (polynomials of) age, our estimations of (1') implicitly use the life outcomes of older ineligible siblings as our counterfactual for the treatment outcomes associated with child sponsorship. Of course the main objection to this would be the possible existence of externalities from sponsorship affecting older siblings of sponsored children. But while there are good reasons to expect that positive externalities trickle down from sponsored children to younger unsponsored siblings, there are also good reasons to believe externalities to older siblings are much smaller, if they exist at all.[3] First, older siblings tend to be less influenced by the choices of their younger siblings than vice versa. More importantly, certain opportunities such as education are accessible predominantly within a time window of age range, beyond which older

[3] We test for educational differences for older siblings to sponsored children using parental education attributes, our controls, and *village*-level fixed effects. We find that these older siblings of sponsored children have about 0.3 years of education more than they "should have" given parental education. Whether this difference results from selection of households into sponsorship based on unobservables or upward spillovers from younger sponsored children is difficult to determine. While the former possibility is addressed via our household fixed effect in (1') and (3), if it stemmed from upward spillovers, it would cause us to somewhat *understate* the effects of the sponsorship program on sponsored children.

siblings have often passed even if they desired to emulate younger siblings. The household fixed-effect estimation allows us to ascertain the degree to which sponsored children and their younger unsponsored siblings stand out significantly from their older siblings more than other children of their own age, gender, and sibling order stand out statistically from their own older siblings.

In practice what we observed in the field was that Compassion staff often selected a family for sponsorship, but the individual child or children chosen for sponsorship were heavily influenced by the household. Endogeneity in the choice of children within a family could operate in two different directions; children could have been selected over others because they seemed to be of higher ability and might realize bigger results from the program, or children might have been selected because they were needier, with parents believing that they required more assistance than other siblings. It appeared to us in the field that the latter tended to be more operational in practice than the former. Therefore, if anything, we believe the OLS fixed-effect estimator is more likely to have a slight downward than upward bias.

To account for this we employ IV estimations to correct for any endogenous choice of children that may exist. The main instrument that we employ takes into account two Compassion rules regarding child sponsorship, the first of which is that children needed to be 12 or under (11 or under in Uganda and Guatemala) to be eligible for the program. But each country also had a second rule, stipulating that only a limited number of children per family could be sponsored: one in Kenya and Uganda, two in the Philippines and India, three in Guatemala, and less than half of the total number of siblings in the family in Bolivia. What we discovered in the data was that it was most likely that the oldest children in the family that met these criteria would be the ones chosen for sponsorship. This tendency was strongest in the two African countries, where social norms often dictate that older children should receive many types of benefits before younger children, and weakest but still existent in the Asian countries. To generalize these rules across our six countries, we use a vector of instrumental variables that identify one's sibling order relative to the program rollout. For example, those who were the oldest siblings to meet the age eligibility requirement in the year the program was introduced into the village receive a one for "first-eligible-sibling" dummy, but a zero for all others. The second oldest eligible sibling receives a one for the "second-eligible-sibling" dummy variable, and a zero for all others, and so forth. All age-ineligible siblings were identified by a single dummy variable; those who ranked lower than the sixth eligible sibling were also identified by a single dummy variable. The probability of sponsorship in either of these groups was virtually zero.

Figures 5a and 5b show these probabilities based on sibling order relative to program rollout in each country, where the variable is indexed so that the oldest eligible sibling when the program

began in the village =1, the second oldest =2, the youngest ineligible sibling = 0, the, second youngest ineligible = -1, and so forth. The first stages of our IV estimations are thus given by

$$T_{ij} = a_j + \boldsymbol{X}'_{ij}\boldsymbol{\varphi_1} + \boldsymbol{Z}'_{ij}\boldsymbol{\lambda_1} + u_{ij}, \tag{2}$$

$$\text{and } YS_{ij} = a_j + \boldsymbol{X}'_{ij}\boldsymbol{\varphi_2} + \boldsymbol{Z}'_{ij}\boldsymbol{\lambda_2} + \tilde{u}_{ij}, \tag{2'}$$

where T_{ij} again is the probability of treatment for individual i in family j (of being sponsored), $\boldsymbol{Z}$ is a vector of instrumental variable dummies reflecting sibling order relative to program rollout that yield the predicted probability of CSP or a younger sib to a CSP. Our second-stage equation is thus

$$y_{ij} = \alpha_j + \gamma\hat{T}_{ij} + \varsigma\hat{YS}_{ij} + \boldsymbol{X}'_{ij}\boldsymbol{\beta} + \varepsilon_{ij} \tag{3}$$

where y is a relevant impact indicator, α_j is again the household fixed effect, $\hat{T}$ is the instrumented probability of CSP, $\hat{YS}$ is the instrumented probability of being the younger sibling of a sponsored child, and ε_{ij} is the error term.

Because the number of instruments (seven) exceeds the number of potentially endogenous variables (two) in our estimation of (3), this allows us to carry out Sargan tests of over-identifying restrictions to help ascertain the validity of our instruments. Because of our large number of impact variables, we carried out tests on a sub-sample of ten of our variables which appear to capture some of the strongest impacts of the program.[4] Using the specification for over-identifying restrictions, where $nR^2 \sim \chi^2(d.f.=5)$, tests failed to reject the validity of the instruments at the 95% level for all ten six-country estimations and in 56 of the 60 tests in individual country samples.

5. Empirical Results

5.1 Estimates of Impact on Education

We find large and statistically significant impacts of the program on formal education. Table 4 presents estimates of impacts on formal schooling that are averages over all countries, using a wide variety of estimation methods. Table 5 presents country specific results for the preferred methods.

The first column of Table 4 estimates equation (1) and presents results for a simple household fixed effects estimate, in which the impact of the program is the coefficient on a dummy variable that equals one for the sponsored child (children) and equals zero for all other children (both children who were too old to be eligible and children who were not too old but were not selected for

[4] The impact variables used in over-identifying restrictions tests were total years of education, secondary school completion, employment, white collar employment, remittances to family, electrification of the household in adulthood, leadership at the church, community, and village levels, and cell phone ownership.

the program by the family). The estimated impact of the child sponsorship program, which is highly statistically significant, is to increase completed years of schooling by 1.28 years.

While this household-fixed-effect estimate of the impact of the program addresses the issue of non-random selection of households, it could still be biased because parents choose, from among their children who are eligible for the program, which child or children will participate. Another problem with the household fixed effects in column 1 is that they assume that there are no spillover effects onto other children in the household. Yet if one child is selected to participate in the program parents may reallocate resources among their children to optimize the distribution of child outcomes among their children. Perhaps the most likely possibility is that parents divert resources from the program child to other children in the family, to make the distribution of outcomes more equal (relative to the case where no diversion occurs). For education outcomes this is most likely to affect younger siblings who were eligible for the program but were not chosen to participate for two reasons. First, parents' concept of fairness may make them more willing to divert resources to a child who was eligible but was not chosen to participate in the program than to an older child who was not eligible in any case. Second, many of the children who were too old to participate in the program may have already left school, and the window of opportunity for more education has already passed.

To examine whether spillover effects occurred, column 2 of Table 4 estimates (1') and adds a variable indicating whether a child is a younger sibling of the sponsored child. The estimates indicate that the impact of the program on the sponsored child was somewhat higher, 1.53 years of schooling, and that the impact on the eligible younger siblings was about 0.51 years of schooling (both $p < 0.01$). Thus it either appears to be that needier children were chosen for sponsorship, or there exists a spillover effect onto younger siblings, perhaps through a role-modeling effect or that either parents reallocate resources from the sponsored child to younger siblings. Note that the increased impact (from 1.28 to 1.53 years) on the sponsored child is consistent with this; the estimate in column 1 compared the sponsored child both to older, ineligible siblings and to younger, eligible siblings, implicitly assuming no spillover effects for either (positive spillovers lead to underestimation of effects when comparing within households), while this estimate compares the sponsored child only to the older, ineligible siblings.

Another useful estimation method using household fixed effects is to measure the impact of each year that a child was sponsored, *i.e.* via the internal margin. This is done in column 3 of Table 4. The estimates indicate that each year of sponsorship increases completed schooling by 0.15 years. Given that the average sponsored child was supported for 9.25 years (see Table 3), this implies an average impact of 1.39 years, which lies between these two estimates.

A final approach using household fixed effects estimation is to estimate not the impact of participating in the program, but the impact of being eligible for the program. This "intention to treat" (ITT) estimate is given in column 5. In effect, this estimate is an average impact from the program of being eligible for the program, averaging over eligible children who participate and eligible children who do not participate. Since only about one third of eligible children participated (see the notes to Table 4), the total impact per sponsored child should be about three times higher, about 2.88 years of schooling, which would include externalities to other eligible children in a program area.

Column 4 presents a direct estimation of education impact via a regression discontinuity design (RDD), which estimates program impact by comparing individuals who narrowly missed being eligible for the program with individuals who were narrowly eligible by a small margin. Here this implies comparing children who are just below the 12-year-old age cut-off point for eligibility with those who are just above that cut-off. The intuition behind this estimation method can be seen in Figure 3. Students who just meet the age 12 eligibility rule (and thus who are eligible for the program) attained about 11.3 years of schooling, averaging over all six countries. On the other hand, students who are just *above* the age 12 eligibility rule (and thus are not eligible for the program) attained about 10.4 years of schooling, again averaging over all six countries. This suggests that the program had an impact of about 0.9 years of schooling on children of eligible age in a program village. This matches closely with the RDD estimate in column 4 of Table 4, which estimates an impact of 0.97 years of schooling. Given that only about one third of eligible children participated, the impact per treated child should be about three times higher, so about 2.9 years of schooling.

The last four columns in Table 4 present instrumental variable estimates of the impact of participating in the program. These estimates also include household fixed effects; by using instrumental variables they address the second selection problem, that parents may choose (from among their eligible children) a child who is either a "weaker" student or a "stronger" student.

In column 6, program eligibility only (as determined strictly by meeting the age requirement) is used to predict program participation. The estimated impact of the program is to increase years of schooling by 2.845 years. Column 7 uses our more refined instrumental variable that gives the probability of treatment based on sibling order relative to program rollout, accounting for the diminished probability of sponsorship among some eligible children, e.g. fourth- and fifth-oldest eligible siblings and younger. The benefit of using this more precise instrumental variable is that while also satisfying the exclusion restriction, it yields a stronger first-stage estimation and thus more precise second-stage estimates, yet the point estimate is nearly identical to that of column 6; program participation increases years of schooling by 2.857 years. The next set of estimates, in column 8,

estimates (2), (2') and (3) and allows for the possibility of spillovers to younger siblings who did not participate, yet we find no spillovers in this estimation and the estimated impact for eligible children who did participate is essentially unchanged.

In the presence of positive spillovers to non-treated children outside treated households, the IV estimates in columns 6, 7 and 8 could overestimate the impact of the child sponsorship program on sponsored individuals because they assume no effect of the program on children in non-Compassion households in the same community. For example, column 6 gives a local average treatment effect (LATE), measuring the effect of the participating in the program for those people whom the instrument induces to participate. Mechanically, this estimate is the ratio of two terms: the difference in years of schooling between children who are eligible and children who are not eligible, divided by the difference in treatment rates between children who are eligible and children who are not eligible. Yet this assumes that there are no spillover effects of the program onto ineligible children, which may be incorrect since, within a community, ineligible children may be induced to stay in school longer when they see their eligible classmates doing the same.

Column 9 is our preferred IV estimation for ascertaining the impact of the program on treated individuals and their younger siblings. In this estimation, we remove non-treated households in treated villages from the sample. Any positive spillovers to younger siblings are captured in our coefficient for younger siblings in treated families, and we would not expect educational peer effects to reach all the way to individuals in the non-program villages that we include in this estimation. Here we find program impacts of the program on sponsored children to result in 2.42 years of additional formal schooling, and we can interpret the difference between (9) and (6) of 0.43 years as a spillover effect to non-treated households. Results are robust to the inclusion of added age polynomials, the use of village-level fixed effects instead of at the household level, fixed effects for age, and the inclusion of other controls. They are also robust to the use of village fixed effects and the inclusion of parental attributes, ruling out false impacts resulting from negative effects on older siblings.

We carried out Hausman tests for endogeneity with the null hypothesis that our OLS estimates are efficient relative to our IV estimates, the alternative hypothesis being that the OLS estimations are inconsistent relative to IV. Tests were carried out on the same ten key impact variables used in our test of over-identifying restrictions. We find that Hausman tests cannot reject the null of OLS efficiency at the 95% level for six of the ten variables in combined six-country estimations, and in 55 out of 60 estimations for these variables when we estimate parameters by each country separately. We thus present OLS (household fixed-effect) estimates for our individual

country estimations in our remaining tables, then including both OLS and IV estimates in the final two columns that give aggregate impacts of the program for the six-countries collectively.

Tables 5 presents education estimates across our six countries that utilize both columns (2) and (9) of Table 4. To avoid clutter, we only display program impact coefficients. (Other explanatory variables are gender, age, age-squared, a dummy variable for the oldest child, and birth order). We present our OLS household fixed-effect results for each country and then six-country totals for our OLS and IV estimations. The first panel shows the impact on completed years of schooling aggregated in Table 4. Examining the six countries separately, all six of the estimated effects on the sponsored child are significantly positive and range from 0.96 years (the Philippines), 1.11 years (India), 1.25 years (Guatemala), 1.27 years (Bolivia), 1.75 years (Kenya), to 3.12 years (Uganda), significant at the 1% level in all countries. Clearly the impact of the program is greater in the African countries, and lower but still significant in the Asian countries, with the Latin American countries in the middle. This variation in impact across countries is not surprising; the Philippines already has a high untreated mean years of completed schooling (12.1 years), the highest among the six countries, while Uganda has the second lowest untreated mean years of completed schooling (8.4 years). Estimates of the impact on the sponsored child's younger siblings are also all positive, but significantly so for only three of the six countries (Kenya, the Philippines and Uganda). In our IV results, the estimated impacts for the sponsored child are positive for all six countries, and significantly so for three countries (Bolivia, Kenya and Uganda). In contrast, the IV estimates of the impact on younger siblings' education is, in general, ambiguous; four countries have positive impacts but two have negative impacts, and the two that are statistically significant have opposite signs.

The second and third panels show the impact on years of formal education by gender. In each country, educational impact is typically greater on the gender for which the mean among the untreated is lower. In Uganda, Guatemala, and Bolivia the impact on female schooling is greater, but in India and the Philippines, where the mean level of schooling for girls is higher, point estimates indicate a higher impact on boys' schooling. In Kenya, there is little difference across genders in either counterfactual outcomes or level of impact.

The remaining four panels in Table 5 show linear probability estimations on the impact of the sponsorship program on completing primary, middle (lower secondary), secondary and university education. Averaging across all six countries, the effects are smallest (although still statistically significant) on the probability of completing primary, which reflects that most children in these countries already complete primary even in the absence of the program. Counterfactuals in untreated individuals' primary completion also explain the heterogeneity across countries in the results. There is no effect for either estimation method in the Philippines, where 99 percent of untreated individuals

in our study complete primary school even in the absence of the program, while in Uganda, the country with the largest estimated effects for the sponsored child, has the second lowest rate of primary school completion (79 percent). The impacts on younger siblings are even smaller and not significant at the 5 percent level for either estimation method.

Turning to the impact of the program on the probability of completing middle school and secondary school, the estimated impacts on the sponsored child are larger than the impacts at the primary level, and are significant for most countries for both estimation methods (and almost always significant for the OLS household fixed effects estimates). Averaging over all countries, the impact on the sponsored child is to increase the probability of completing middle school by 10.3 to 14.4 percentage points over a baseline of 79 percent (the higher estimate being the IV estimate) and to increase the probability of completing secondary school by 19.6 to 30.3 percentage points over a baseline of 44 percent (the higher estimate again being the IV estimate). The estimated impacts for younger siblings are smaller but for the most part are statistically significant over the six country sample. Again, heterogeneity of impacts across countries reflects their initial levels of education: The Philippines, where the program had little or no effect, has the highest rates of completion of middle and secondary school. Uganda, which has the lowest levels of completion, has the highest estimated impacts.

Finally, the magnitudes of estimated impacts on completing university education are relatively small, not surprising given the low percentage of the population that achieves this level of education, yet they are statistically significant. Averaging over all countries, the impact on the sponsored child is to increase the probability of completing university education by 2.6 to 5.1 percentage points (the higher estimate being the IV estimate). Although seemingly small point estimates, this is in fact reflects a strong impact given that over all countries only about 4.2 percent of the untreated population in the villages of our sample completes a university education.

5.2 Impact on Employment, Vocation, and Economic Outcomes

Along with educational goals, a major goal of the Compassion program is to prepare sponsored children for employment. We estimate the impact of the program on life outcomes related to work in three major areas. We look at program impact on 1) the probability that an individual is gainfully employed with a steady salary at the time of the survey; 2) the probability that an individual is employed in a "white collar" job; and 3) significant differences in the types of career chosen. Our first impact variable, salaried employment, we defined as being "currently employed at a steady salary." This definition ruled out itinerant laborer work, although we do calculate wage earnings for these types of informal jobs. To answer the second questions we divided employment into 14 categories: 1-agriculture, 2-construction, 3-clerical & sales, 4-blue collar work, 5-personal

services, 6-teaching, 7-public administration, 8-small business, 9-career-based ministry, 10-finance & large business, 11-police/army/security, 12-professional (doctors, lawyers, engineers, professors, etc), 13-less-skilled technical work (such as call centers), and 14-nursing & public health. We defined a "white collar" job as pertaining to categories 3, 6, 7, 9, 10, 12, 13, and 14.

The baseline level of "salaried employment" by our definition for untreated individuals in our survey was about 0.36. As seen in Table 6, we find large and significant impacts from the Compassion program in both OLS and IV estimations, particularly the latter. Coefficients on CSP are positive in virtually every specification and in all six countries. OLS estimations indicate an impact of about 7 percentage points, while the IV estimate is much larger, 20.6 percentage points. Results in Kenya—our largest study country--are significant at the 1% level in all of the key estimations. We view the program's impact on salaried employment as one of the most substantial benefits of the program, responsible for the increases in earned income which we describe shortly. The movement into employment appears to occur primarily through greater levels of formal education, but not entirely; when total years of education are included as an explanatory variable, the impact of the sponsorship program remains significant.

The impact on white collar employment is also large, carrying a positive sign in every specification and country estimation and largest where there is more opportunity for white collar employment (Guatemala, Philippines, India). OLS and IV estimations for the six-countries respectively find an impact of 7.1 and 17.3 percentage points over a baseline of 18.5% for untreated households. The mean for treated households is 27.3%, so as with the employment outcomes, we are inclined toward the OLS estimates, but they may understate results since they do not account for the tendency for needier children in households to be chosen for sponsorship.

As Table 7 illustrates, the movement into white collar work is not made up of a large increase in movement into high-paying jobs. It is principally made up of the sizable impact of the program on formerly sponsored children becoming relatively modestly-paid white-collar workers, particularly teachers. Table 7 presents multinomial logit estimations of logit coefficients and marginal effects for selection into one of our 14 categories that include all of the previously mentioned controls plus a control for "Compassion household." The baseline category is "unemployed" and so nearly all categories have a positive coefficient, though many are insignificant. The baseline level of teachers among untreated (non-Compassion) individuals is 5.3%. Thus with a marginal effect of 4.1 percentage points, Compassion sponsorship nearly doubles the probability that a sponsored child becomes a teacher. This result seems plausible given that many Compassion children come from families in which there is little role modeling of white-collar work, and so staff from the program and

teachers may serve as primary role models. We see very few consistent effects on employment spillovers to younger siblings, except perhaps in Kenya, which had the poorest villages in our study.

We estimate increases in income in Table 8. The dependent variable is estimated monthly salary, converted into U.S. dollars using exchange rates at the time of the survey. While identifying a family member's occupation was usually easy for the households in the survey, in about a quarter of responses family members did not know a particular individual's income. To estimate an individual's income in these instances, we substituted mean levels for the 14 work categories by country, which we believe capture the important differences in salary between occupations. Individuals not receiving a salary received a zero for income. OLS estimations with household fixed effects, indicate an increase in salary of about US$16.65 per month; IV estimates show US$37.88. In the lower panel we see that conditioning the estimation on salaried employment yields a near zero effect of the program on income. As a result we conclude that essentially all of the gains in income result from people being drawn into salaried employment who in the counterfactual would have remained outside the formal labor force.

Although we do not take our income data to have the precision of our other life-outcome data, we calculate an internal rate of return to Compassion child sponsorship counting only the increase in income as benefit, although clearly there are other benefits from the program. We use 9.25 as the mean number of years of sponsorship and a $28 per month cost of sponsorship during this time. Assuming a US$16.65 increase in salary at mean age 26.3 remains constant throughout an individual's life (which undoubtedly understates the increase), we calculate an internal rate of return of 5.16% on Compassion sponsorship. Using the larger IV estimate, the rate of return is 9.2%.

The results in Table 8 also show that formerly sponsored children are much more likely to send remittances to parents and other siblings than non-sponsored children. OLS fixed-effect estimations show that sponsored children are 7.3 percentage points more likely to send remittances; the IV estimates show an 8.0 increase. Both of these figures indicate a more than doubling of the 6.7% baseline propensity for untreated individuals to send remittances back to family.

5.3 Impact on Marriage and Fertility

It is well known that increases in schooling tend to increase age at marriage and reduce the number of children born to individuals. Estimates for the impact of the program on these variables are shown in Table 9. The IV estimations indicate that, conditional upon marriage, Compassion sponsorship leads to a 1.71 increase in age (in years) at first marriage ($p < 0.01$). The OLS fixed-effect estimates are lower, at 0.65 years ($p < 0.05$). Adding years of schooling to the estimation cuts the Compassion impact in half, but does not eliminate it. To see the impact of the sponsorship program on marriage more specifically, we examine the probability of marriage at age 20, 24, and 28.

What is clear from the estimations is that the program reduces early-age marriage, with the probability of marriage by age 20 falling by about 5 percentage points, by age 24 falling by only 3 percentage points, but the probability of marriage by age 28 actually increasing by 7.7 percentage points. Thus the overall marriage rate may be higher, but marriage is shifted to a later age.

We see almost exactly the same pattern in female fertility in the fourth panel of Table 9. Being a sponsored child does not reduce fertility, but it clearly delays childbearing from the teens and early 20s toward the middle and late 20s. While both OLS and IV estimates show significantly lower fertility rates in the early 20s, point estimates show that by the late 20s women have almost exactly the same number of children as non-program individuals, with children merely born at a later age of the mother.

5.4 Impact on Community Leadership

Compassion's sponsorship program emphasizes leadership development, especially among older sponsored children. We defined "church leadership" as someone having any kind of leadership role in their community of worship, not merely as priest or pastor. Not surprisingly, OLS estimations with household fixed effects indicate that former Compassion children are roughly half again as likely to be church leaders as non-Compassion children, a result which is statistically significant at the 1% (10%) level in OLS (IV) estimations. There also appear to be significant spillover effects to younger siblings of sponsored children, where in both OLS and IV estimations the impacts on younger siblings are comparable to those of the sponsored children. Based on the vocational results in Table 7, we see little impact on sponsored individuals entering into pastoral work as a paid vocation; most of this movement into church leadership appears to be on a volunteer basis.

The "community leadership" variable is based on a question that asked respondents whether they hold any type of secular leadership position in the local community. This includes membership on a school board, water board, or any other local leadership council or body. The baseline rate of community leadership is small among non-Compassion individuals (2.9%), yet all of our estimations indicate a positive impact in this area, with estimates ranging from 1.9 (OLS) to 10.0 (IV) percentage point increases ($p < 0.05$) with the strongest results in Uganda and Bolivia. The "village leadership" variable sets a higher bar for leadership, defined as one who sits on the town council or holds an authoritative position with the larger community. The baseline for this variable was only 1.5%, yet we find statistically significant impacts of just less than one percentage point in OLS and slightly less than four percentage points in IV estimations.

5.5 Impact on Dwelling

Our survey collected data on the basic building materials used in the construction of the houses inhabited by each sibling from the families of our survey households. Categories for wall

material construction include leaves, grass, bamboo, mud (adobe), wood, brick, and concrete. The exact definition of "improved walls" varies slightly between the six countries, depending on context, but in general reflects wall construction of brick or concrete rather than unprocessed natural materials. The main floor categories were dirt, cardboard, leaves, grass, wood, concrete, carpet, and tile; an "improved floor" was considered to be one of the latter four categories. An improved roof was generally considered to be constructed of high-quality iron, tile, or concrete as opposed to low-quality corrugated iron, a plastic sheet, cardboard, leaves, thatch, or straw. We also asked if siblings' houses had electricity and if they contained indoor toilets. Even if the latter are determined largely by surrounding infrastructure, we believed those with higher incomes might relocate to these areas.

Table 11 shows modest impacts on dwelling improvements, with significant impacts of the program on some measures, and little or no impact on others. We find no evidence for significant impacts on wall or roof construction in any of our countries, but we do find evidence for impacts on formerly sponsored children living in houses with higher quality floors; given an untreated baseline of 62% on our "improved floors" variable, we find positive impacts of 3.0 percentage points ($p < 0.05$) and 8.5 percentage points ($p < 0.05$), respectively, from our OLS and IV estimates (both with household fixed effects).

Our OLS fixed-effect estimations show small impacts on formerly sponsored children now living in a house with an indoor toilet: 1.5 percentage points, and significant only in OLS estimations ($p < 0.10$) over an untreated baseline of 81%.

Impacts on having a house with electricity were the largest of all the dwelling improvements. Because the baseline for household electrification was nearly 100% in the non-African countries, we restricted our overall estimate to the two African countries--Uganda and Kenya--where the untreated baseline is 30% or lower. Here we find impacts of 3.7 percentage points and 9.2 percentage points (both with $p < 0.10$), respectively, from our OLS and IV estimations, perhaps our best evidence that child sponsorship results in improved living conditions in adulthood.

5.6 Impact on Consumer Good Ownership

The last component of the quality of life that we measured was the impact of the sponsorship program on consumer durables. We attempted to include five goods in our study for which ownership by a family member would be common knowledge among other family members: cell phone, car, motorcycle, bike, and land (of any type). Table 12 we reveals little evidence of any impacts on the purchase of these consumer goods in any of our six countries, with one glaring exception: cell phone ownership. In the three countries where baseline ownership of cell phones was lowest (Uganda, Guatemala, and Kenya), we find that being sponsored during childhood results in a significantly higher probability of owning a cell phone in adulthood. Over an untreated baseline

(averaged over the six countries of) 77%, we find average impacts of 11.0 and 15.6 percentage points from OLS and IV estimations, respectively ($p < 0.01$). There also appear to be spillover effects of significant magnitude onto younger siblings. Because cell phone ownership is likely to be correlated with education, income, social connectedness, and leadership, the results on cell phones may not be surprising. We suspect that the cell phone results may collectively capture many of the other impacts from sponsorship.

6. Inside the Compassion Black Box: Exploring Causal Factors of Impact

What accounts for the impacts of the Compassion child sponsorship program? In order to investigate this question, we carried out a smaller study among 278 teenage children in our final three countries. We began with a short initial study of 30 children in India, then among 90 children in Kenya, and finally among 158 children in Bolivia. Among this group of teenagers, 127 were *currently* sponsored children, 70 were siblings of sponsored children, and 81 were children from unsponsored households of the same age in the same villages. Here we present a summary of the findings from this research; a more detailed analysis of the results is contained in an unpublished working paper (Wydick, Glewwe, Rutledge, and Ross, 2010). We take these results to be less conclusive than our results on adult life outcomes. While they do appear to give some insight into the impacts we observe in our larger study, they serve primarily as a guide for future research.

The Compassion program places a heavy emphasis on self-esteem building, character development, and enhancement of self-expectations. Thus we carried out a short survey to test for differences between currently sponsored children and non-sponsored children in delayed gratification and discount rates, sociality, self-esteem, reference points for schooling completion, and reference points for future vocation, variables in our larger study in which we had seen large impacts.

The first two of three hypothetical questions we asked our teen-aged subjects regarding delayed gratification and discount rates were oriented around a preference for money today rather than in the future. The first question was "If a trustworthy person were to offer you 100 (local currency units) tomorrow or 110 next week, which would you prefer?". The second substituted 150 currency units for 110 in an identical question. To both of these questions we found means and (OLS, village-fixed effect) point estimates that were nearly identical between sponsored and non-sponsored children. A third question asked "If you had a large unpleasant chore to do either today or next week, when would you choose to do it?" In response to this question, the Compassion children displayed an even somewhat stronger desire to put off the chore than other children. As a result, we found no evidence of lower discount rates among the sponsored children.

Our questions on social trust included the three commonly administered questions from the General Social Survey, that seek to measure different aspects of sociality in conceptions of trust, fairness, and helpfulness.[5] For none of these three questions did we find currently sponsored children or their siblings to indicate different levels of sociality that were significantly different from siblings or other teenagers in their village.

We included five questions related to self-esteem to our subjects.[6] Although the mean answers for sponsored children tended to be slightly higher than the answers by non-sponsored children, regressions controlling for age, gender, and village effects reveal no statistically significant differences in responses to the self-esteem questions between sponsored and unsponsored children.

As part of our study in Bolivia, subjects were asked to "Draw a picture of yourself in the rain." This exercise was carried out by 54 subjects in triads that included a currently sponsored child, a sibling of a sponsored child, and a peer from a non-sponsored family. Blind to which drawing was done by which type of subject, a psychologist trained in the psychoanalysis of children's drawings then ranked each triad of drawings in order of self-esteem implied by the pictures. While the sponsored children's drawings were more often ranked highest (in 8 out of the 18 triads; five of each of the other two groups ranked highest), differences in self-esteem rankings were not statistically significant in either simple *t*-tests or regressions controlling for standard control variables.

The uniquely significant differences we find between currently sponsored and unsponsored children are in regard to reference points and self-expectations in educational achievement and future vocation. In response to our educational reference point question, "What level of education would you say is sufficient in order for one to be successful today?" sponsored children gave an answer approximately 0.40 years higher than non-sponsored children ($p < 0.10$). Regressions controlling for age, gender, and village effects as well as reported educational expectations for parents, siblings and peers, find differences of 0.72 to 0.86 years ($p < 0.05$) between sponsored and non-sponsored children, with siblings' reference points being somewhat closer to sponsored children, but still significantly lower.

We asked three questions regarding reference points for vocation. One question asked "Would you be satisfied as an adult if you had your parent's job?" In response to this question, 35.1

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[5] We used three GSS questions in each country that included the question on *trust* "Generally speaking, would you say that most people can be trusted or that you can't be to careful in dealing with people?", the question on *fairness*, "Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair?", and the question on *helpfulness*, "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?".

[6] Q1: "I feel that I'm a person of worth, on an equal plane with others." (Respondent asked to answer Strongly Agree, Agree, Disagree, Strongly Disagree to each question); Q2: "I am able to do things as well as most other people." Q3: "I feel I do not have much to be proud of." Q4: "On the whole, I am satisfied with myself." Q5: "At times I think I am no good at all."

of children from non-sponsored families responded affirmatively, 41.1% of siblings of sponsored children responded affirmatively, but only 28.6% of sponsored children did so. A second question asked "What kind of job do you *hope* that you can have when you grow up?" This question was intentionally administered early in the survey. We then categorized questions based on whether or not the aspired occupation was a professional, white-collar job that required post-secondary education. Here, the difference was quite close between sponsored and unsponsored children: 73.8% of sponsored children said they hoped to be a doctor, nurse, engineer, teacher, or other professional compared to 69.8% of unsponsored children ($p = 0.19$). At the end of the survey, we then asked respondents "What kind of job do you *expect* to have when you grow up?" With this question the difference was far more substantial between the two groups: 69.8% of sponsored children indicated that they *expected* to have a professional job as an adult (a drop-off of only 4 percentage points from what they hoped for) compared to only 56.6% of non-sponsored children (a drop-off of 13.2 percentage points from what they hoped for). While regression results show no significant difference between the two groups in their *hopes*, they indicate an 11.8 to 12.6 percentage point difference in *self-expectations* about future vocation ($p < 0.10$).

Based on what we find from this modest study of currently sponsored children, we believe that a significant aspect of the impact of Compassion's child sponsorship program on adult life outcomes lies in a strong complementarity between the re-orientation of reference points of impoverished children and the simultaneous release of binding economic constraints that impede higher levels of formal schooling. In this respect we conceive of the impact results from our main study in a theoretical framework in which the provision of educational inputs such as school uniforms, tutoring services, and the payment of school fees are strongly complimentary to a child's endogenously chosen level of effort to persist in formal schooling. If children's own views of self-expectation or educational reference points are low, this reduces persistence in school, which in turn may dampen the marginal impact of economic subsidies to schooling.

Seen in the broad picture of behavioral economics, the Compassion sponsorship program appears to create a decade-long "nudge" that over this long period of time, alters self-expectations of children about their futures. Over the years of weekly participation in the program, sponsored children continually receive both overt and subtle messages from sponsors and program staff that are designed to augment self-assessment of own capability and self-expectations. This prolonged nurturing appears to significantly influence children's reference points regarding what constitutes a satisfactory level of schooling and achievement. Thus in the context of this more holistic approach to child development, in which the socio-emotional nurturing of children is carried out in tandem with subsidies and economic incentives, the marginal impact of the latter may increase substantially.

7. Conclusion and Reflections on Impact Results

Although international child sponsorship may be the most widespread form of personal contact between households in wealthy countries with the poor in developing countries, there have been no published studies to date that have analyzed the life outcomes of beneficiaries of these programs. This research presents a first attempt at measuring these impacts. After obtaining initial enrollment lists of children sponsored through nineteen Compassion International projects started in six countries during 1980-1992, we located formerly sponsored children and use the program's arbitrary age-eligibility and children-per-family-eligibility rules to statistically identify causal impacts of the program on adult life outcomes: years of formal education, employment, income, leadership, age of marriage, fertility, current dwelling quality, and durables consumption. Through household fixed-effect estimations, we attempt to control for household self-selection, household environment, genetics, and other factors common to siblings in order to isolate the impact of sponsorship in reference to the counterfactual of older siblings who failed to meet age-eligibility requirements for the program. The household fixed-effects estimator also allows us to address potential endogeneity issues of household selection into the program, and an instrument derived from siblings' birth order relative to project implementation in the local village allows us to address potential issues of intra-family endogenous child selection. We directly estimate impacts on education using a regression discontinuity estimation and an intention-to-treat estimator, finding that these estimates are nearly identical to our IV estimates that include positive spillover effects.

We find large and statistically significant impacts across most of our impact measures from the child sponsorship program. Our somewhat more modest OLS household-fixed-effect impact estimates indicate that over the six countries and 10,144 individuals in our study, sponsorship lead to 1.53 years of additional schooling, 19.6 percentage points greater probability of secondary school graduation, 7.1 percentage points greater probability of white collar employment, a $16.65 increase in monthly income, a 7.3 percentage point increase in the probability of sending remittances back to family, reduced early-age marriage and female fertility, significantly positive impacts on the probability of being an adult community leader, and increases in construction quality and the probability of electrification of the house in adulthood. Instrumental variable, household fixed-effect estimations that take into account that needier children within a family may have been more likely to be selected for the program indicate larger impacts: 2.42 years of additional schooling, 32.6 percentage points greater probability of secondary school graduation, 17.3 percentage points greater probability of white collar employment, $37.88 increase in monthly income, 8.0 percentage point greater probability of sending remittances back to family, slightly bigger impacts on delayed marriage and childbearing and on the probability of community leadership than OLS estimations, yet slightly

lower impacts than the OLS estimations on dwelling quality.

We find evidence in some of these variables of significant spillovers to younger, unsponsored siblings, particularly in the area of secondary school completion, where younger siblings of sponsored children are about 12 percentage points more likely to complete secondary school in both OLS and IV estimations. We also find evidence of positive educational spillovers to other children in Compassion villages from non-Compassion families that appear to account for an additional 0.43 years of education per sponsored child.

This resulting increase in formal education we believe drives many of our remaining results on life outcomes, although when education is included as a right-hand side variable of its own in estimations on many of the other variables such as employment, leadership, and so forth, (not included in our tables here) the impact of the Compassion program, isolated from its effect on education, typically remains a positive and significant explanatory variable. This leads us to believe that child sponsorship's impacts lie mostly, but not solely, through its impact on formal education.

We cannot make general claims about the positive impact of all child sponsorship programs based on the evidence we present in this study. Other large programs, such as those operated by World Vision and Plan International, direct funding given in the name of a sponsored child more broadly to village-based projects and the creation of village-level public goods rather than to sponsored children directly. While these programs also foster a relationship between a sponsor and a child, the less-targeted nature of the funding in these programs makes potential impact more diffuse and difficult to assess.

Our estimates of the impact of this direct form of child sponsorship on formal schooling are larger than those found by Kremer, Moulin, and Namunyu (2003), who report about 0.30 years of additional schooling as a result of sponsorship. But their impact results examine a shorter and far less costly intervention than that undertaken by the child sponsorship program we analyze. Whereas the program they analyze mandated basic provisions to sponsored children, principally uniforms and school textbooks, the program we study provided students with a much more comprehensive support structure: not only basic school provisions, but also tutoring services, school fees, health education and healthcare, spiritual mentoring, program retreats, and other opportunities for psychological and social development. The cost of these services during the 1980-1992 period was $28 per month to the sponsor, and the mean number of years of sponsorship in our sample was 9.25 years. Thus the total (non-discounted) cost of sponsoring a single child to the average sponsor therefore was approximately US$3,108.

Using our IV estimate of 2.42 additional years of schooling on sponsored children, this puts the average cost of an additional year of schooling from the program at $1,284 per year per

sponsored child. Our estimation that includes village spillovers to outside children finds a local average treatment effect of 2.85 years of education created per sponsored child, implying a cost of only $1,091 per year of additional schooling generated from the program.

While it is clear that the impacts of the child sponsorship extend beyond added years of schooling, it is useful to compare this particular result to CCT (conditional cash transfer) programs, which have been introduced now in 26 developing countries in recent years with considerable financial support from the World Bank. (See World Bank, 2009, for a recent review of these programs.) Behrman, Parker and Todd (2005) estimate that receiving transfers from the *Oportunidades* program in Mexico for two years increased years of schooling by 0.20 years. Interestingly, the monthly cost of *Oportunidades* is similar to that of the Compassion sponsorship program: $12-$23 per month at the primary level and $34-$43 at the secondary level (not counting an additional annual amount of $23 for primary school children and $29 for secondary school children; see World Bank (2009, p.268) for these figures). Given the mean number of years of sponsorship in our study and using our impact estimate of 2.85 on education that includes village spillovers, the per year effect of child sponsorship is 0.31 years of final schooling, about three times higher than the roughly 0.10 year effect of the CCT program as evaluated by Behrman et al.

Schultz (2004) estimates that in the *Oportunidades* program, CCT payments in 1999 were an average of approximately $25.68 per month, received from the third to the eleventh years of school. With an average of seven years of conditional cash transfers equal to $2,157, and an impact he estimates at 0.66 added years of schooling, the cost per additional year of schooling per child would be equal to US$3,268, almost exactly three times the cost per year of additional formal schooling as our child sponsorship estimates.

Even if we compare our results most closely with the impact studies carried out on CCT programs by restricting our estimates to the two Latin American countries in our study, Guatemala and Bolivia (n = 3,229), we still obtain an estimate of 2.69 years of additional education from the program in these two countries (t = 4.41), quite close to our result from entire the six-country sample of 2.85 years. Indeed each country in our study generates point estimates that reflect a more cost effective impact on formal education than these current estimates for CCT programs. The advantage in impact across the six countries holds similarly for OLS, IV, ITT, non-parametric, and regression discontinuity estimations.

While child sponsorship appears to be more cost effective than the estimates for CCT programs, they remain far higher than the $3.50 per added year of schooling estimated from child de-worming treatments (Miguel and Kremer, 2007), or the estimated $43.50 cost per year of additional schooling through providing school uniforms (Evans, Kremer, and Ngatia, 2008). However, these

latter estimates are based on increased attendance during a given schooling year rather than higher levels of grade completion, and the two are not strictly comparable, one reason being that the opportunity cost of child schooling increases with age.

Moreover, when addressing issues of cost efficiency, it is important to understand that the development of international child sponsorship programs fundamentally arose from their usefulness as a marketing tool for mobilizing resources in rich countries to fight poverty in poor countries. As administrators in these programs have recognized for decades, contact with an individual child creates a commitment device to help donors contribute a fraction of their monthly income to alleviating world poverty. In fact, it is likely that many of these resources may not have been mobilized at all if it were not for the ability of international child sponsorship to foster this type of commitment device to poverty alleviation via a relationship with a particular *child*.

In this way, international child sponsorship programs may mobilize these additional resources by drawing heavily upon the same group of psychological and moral instincts people possess to care for their own children. Even in difficult economic times, the commitment of donors to the wellbeing of "their child" is likely to be much greater than their commitment to a large, well-intentioned yet relatively faceless, non-profit organization. There is at least anecdotal evidence of this: During the first year of the 2008-09 recession when giving to most U.S. charities declined sharply, World Vision reported that the percentage of those who remained faithful with their monthly donations to sponsored children showed no sign of decline during this period (Kennedy, 2009). In summary, even apart from issues of cost-effectiveness as a method to increase child schooling, child sponsorship programs may be among the most effective methods to *mobilize* resources that significantly increase child schooling.

Much current research in development economics has explored the impact of psychological factors, nudges, and reference points on credit and savings decisions (Bertrand et al., 2010), health (Dupas, 2010), technology adoption (Duflo, Kremer, and Robinson, 2010), and education (Kremer, Miguel, and Thornton, 2009). We find modest evidence that the development of these psychological and behavioral attributes is a key component to the positive impacts we find on adult life-outcomes from child sponsorship. In the end, these internal constraints of the poor may present as great a barrier to escape from poverty as the more tangible and external economic constraints. Consequently the provision of economic subsidies and incentives without an effort to address these more subtle, and perhaps more profound, internal reference points and constraints may result in chronically low to modest impacts from such programs. Further observational and experimental research should seek to better understand the internal constraints of the poor and study how development efforts can work effectively in these areas as a complement to purely economic interventions.

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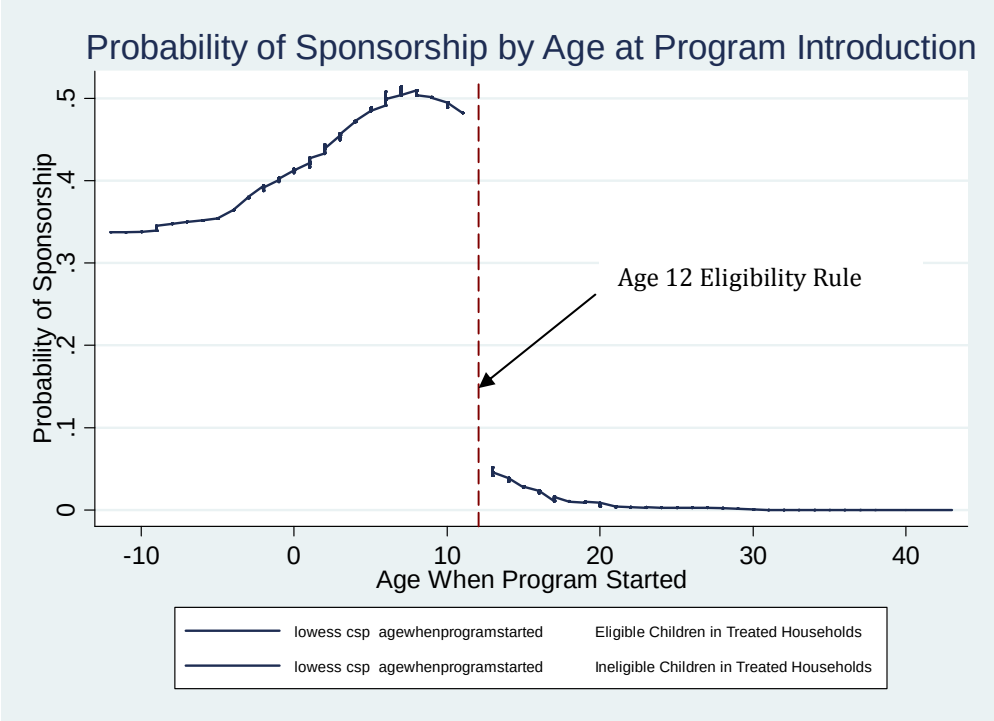

Figure 1: Discontinuity in Sponsorship by Age

Kernel Density Functions over Total Years of Formal Education

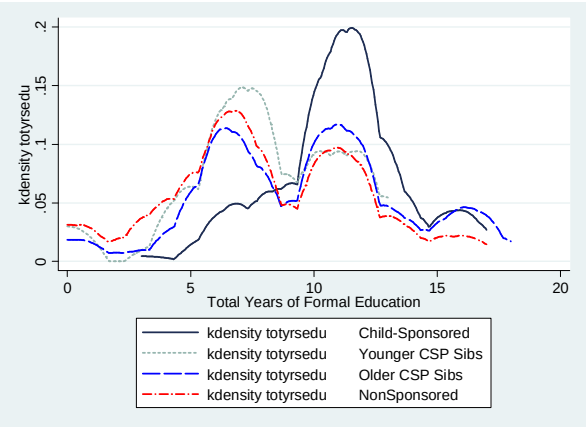

Figure 2a: Uganda

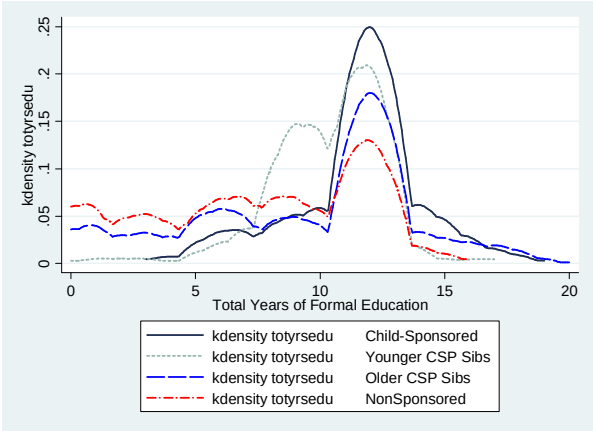

Figure 2b: Guatemala

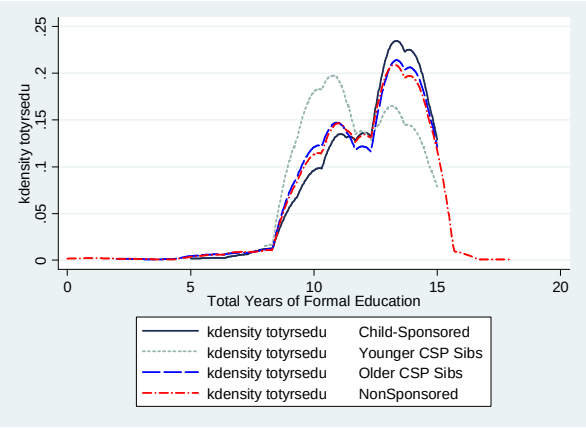

Figure 2c: Philippines

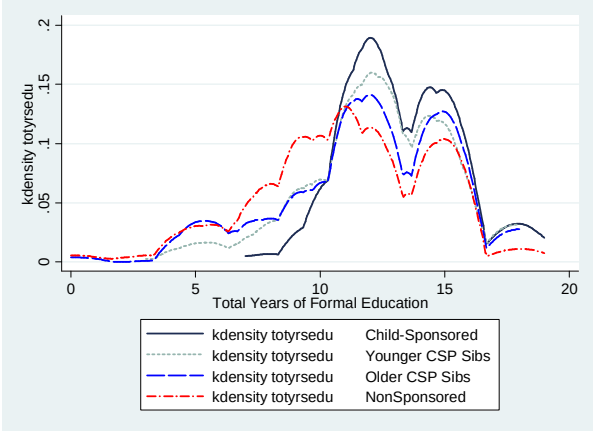

Figure 2d: India

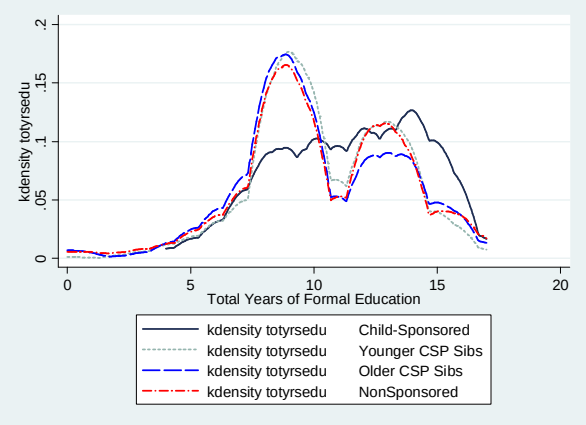

Figure 2e: Kenya

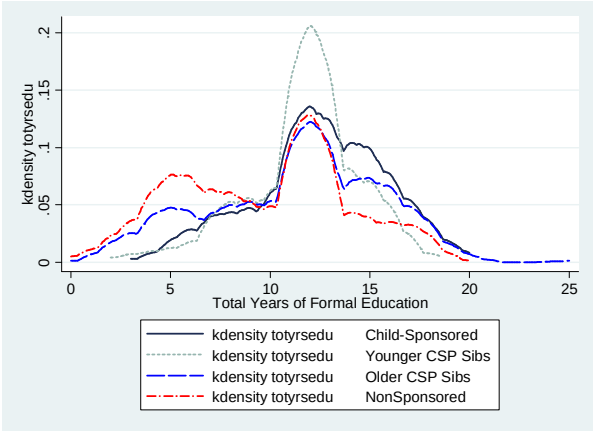

Figure 2f: Bolivia

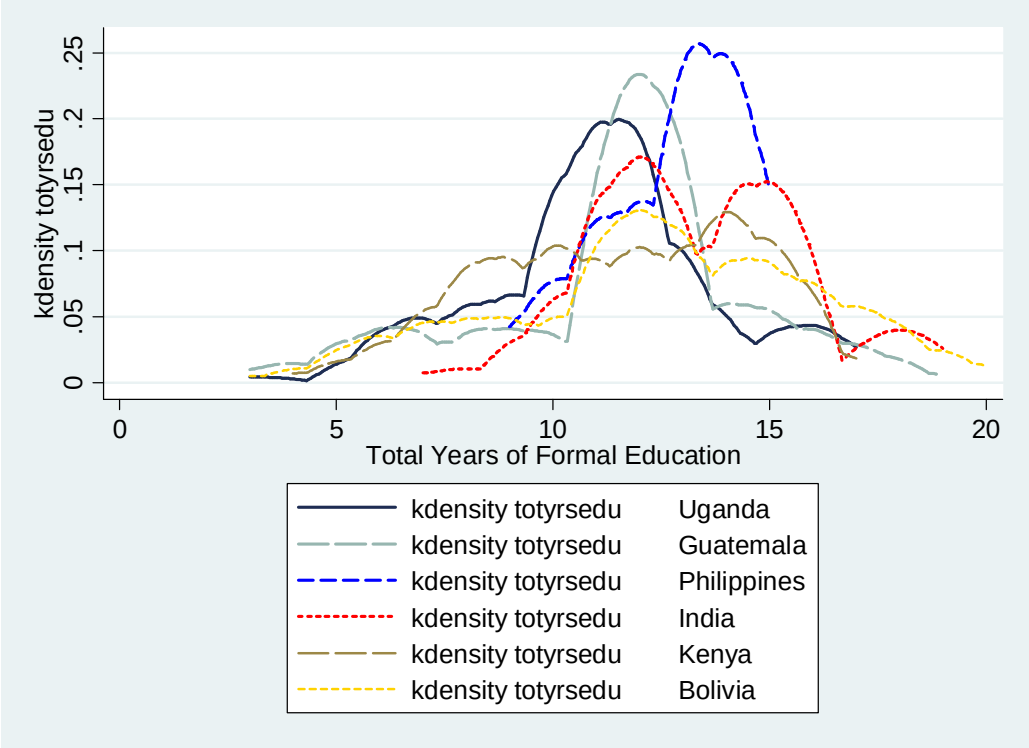

Figure 2g: Sponsored Children, All Countries

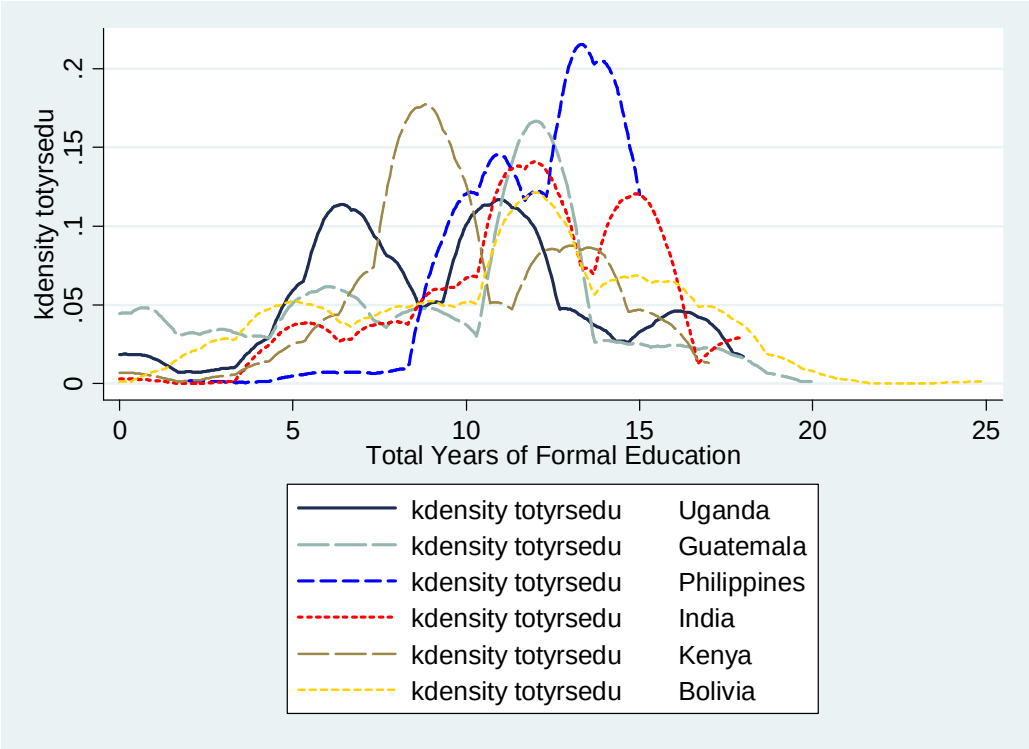

Figure 2h: Unsponsored Older Siblings, All Countries

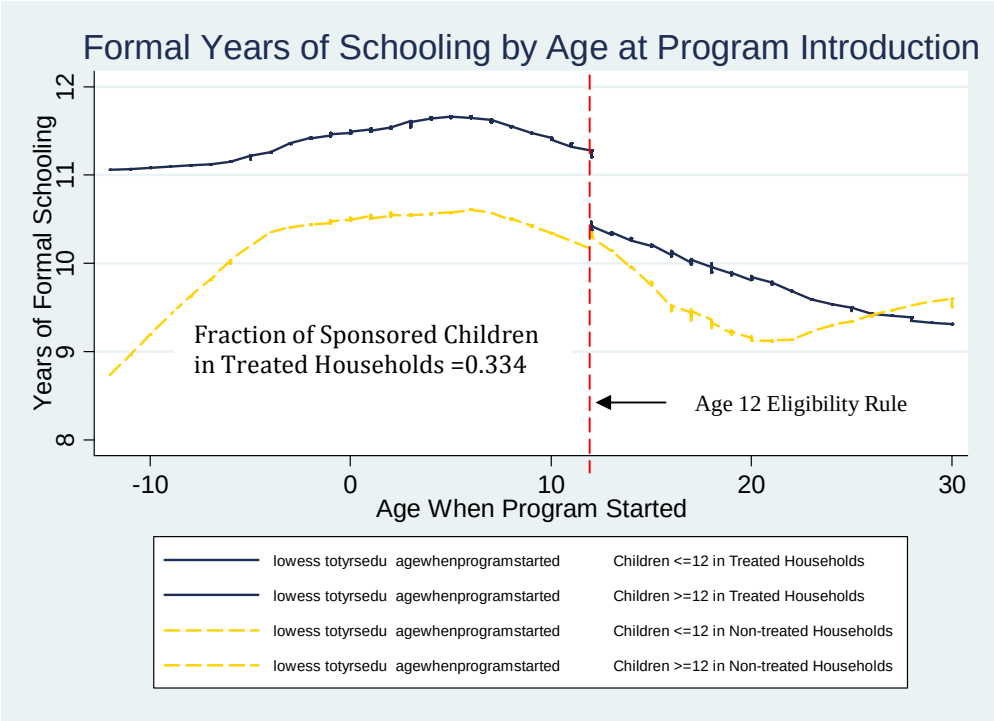

Figure 3: Total Years of Formal Education around Eligibility Rule

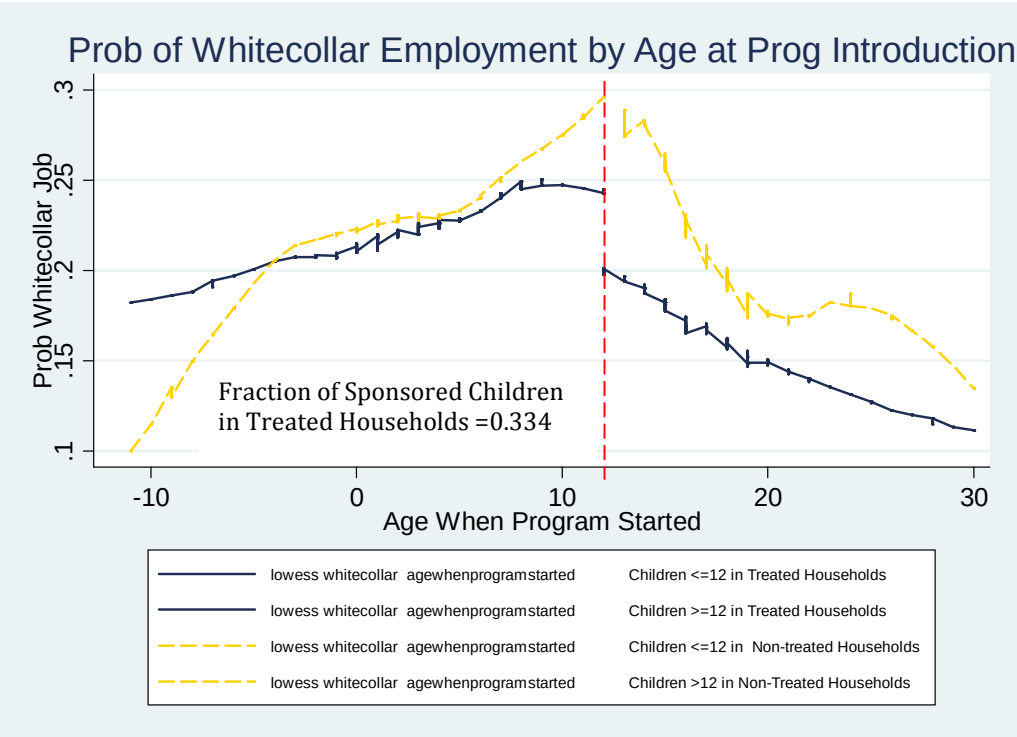

Figure 4: Probability of White Collar Employment around Eligibility Rule

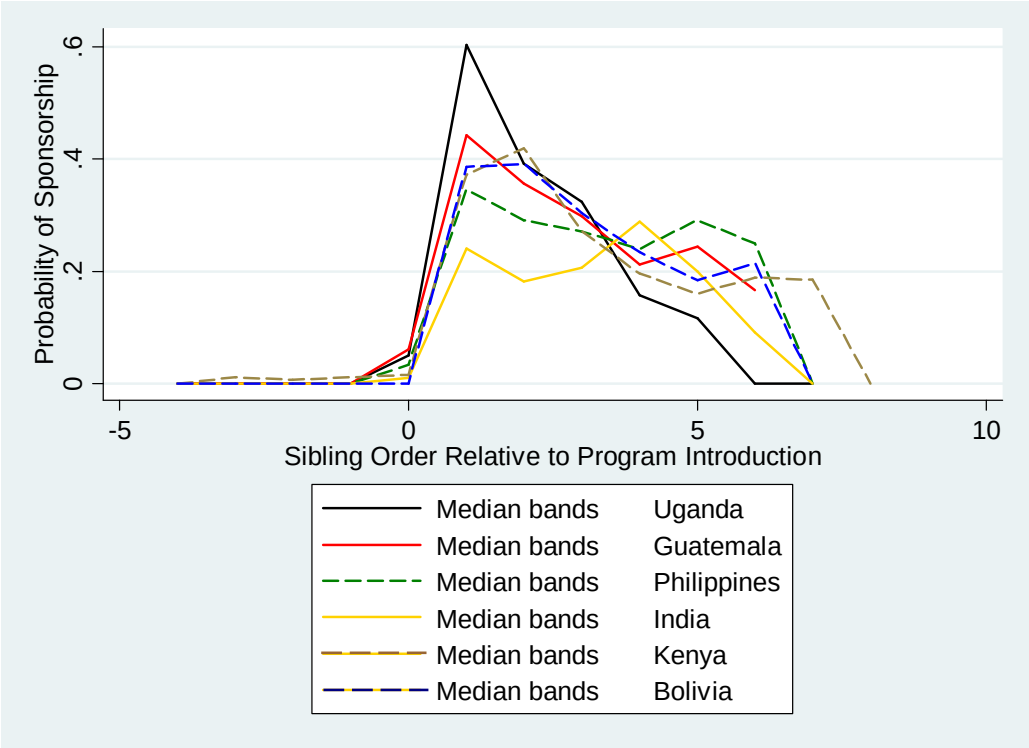

Figure 5a: Instrumental Variable (Y-axis) for Sponsored Children
(Oldest eligible sibling = 1, Second oldest =2 . . .; Youngest ineligible sibling = 0, Second youngest = -1…)

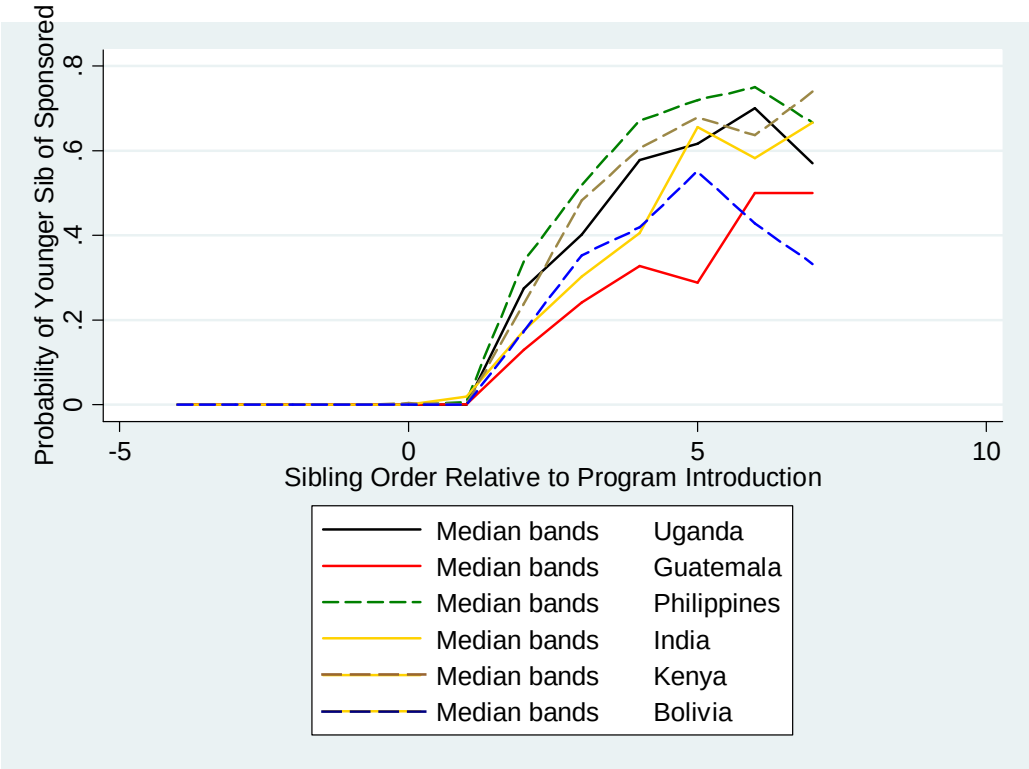

Figure 5b: Instrumental Variable (Y-axis) for Younger Siblings of Sponsored Children
(Oldest eligible sibling = 1, Second oldest =2 . . .; Youngest ineligible sibling = 0, Second youngest = -1…)

Table 1: The Ten Leading International Child Sponsorship Programs

Organization:	International Headquarters	Year Founded	No. of Countries	Contribution per month	No. of Sponsored Children*
1. World Vision†	USA	1953	100	$30	3,600,000
2. Plan USA	USA	1937	49	$24	1,500,000
3. Compassion International†	USA	1952	25	$38	1,050,000
4. ChildFund International	USA	1938	31	$24	510,000
5. Children International	USA	1980	11	$22	335,000
6. CFCA†	USA	1981	23	$30	291,262
7. Kindernothilfe†	Germany	1959	28	$30	145,814
8. Save the Children	USA	1932	50	$28	120,000
9. SOS Children's Villages	USA	1949	132	$28	80,000
10. Bornefonden	Denmark	1972	5	$34	72,473
Others* (197)					657,053
Total					**8,362,000**

*Child sponsorship organizations by donating country: USA (43), UK (41), France (18), Canada (10), Italy (10), Australia (9), Denmark (7), Spain (7), Norway (6),Germany (5), Sweden (4), Others (16).

† Faith-Based Organization. CFCA is the Christian Foundation for Children and Aging.

Table 2A: Compassion Program Benefits by Country

Country	Uniforms	Tutoring	School Materials	Spiritual Instruction	Healthcare	Gifts from Sponsors	Cash to Family
Uganda	Yes	Yes	Yes	Yes	Yes	Yes	No
Guatemala	Yes	Limited	Yes	Yes	Yes	Yes	No
Philippines	Limited	Limited	Yes	Yes	Yes	Yes	No
India	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kenya	Yes	Yes	Limited	Yes	Yes	Yes	No
Bolivia	Limited	Yes	Yes	Yes	Yes	Yes	Limited

Table 2B: Survey Information by Country

Country	Treatment Villages and Cities	Non-Treatment Villages and Cities	Sample Size	Time of Survey
Uganda	Jinja (1980), Bugiri (1981), Masaka (1989)	Kakooge, Bombo	809	June-August 2008
Guatemala	San Pedro La Laguna (1991), San Juan La Laguna (1992), San Pedro Necta (1992)	San Pablo La Laguna, Santiago Chimaltenango	1,762	May-July 2009
Philippines	Quezon City (1986), Bacolod (1986)	Skybag, Handumanan	1,428	Nov.-January 2009-2010
India	Tuticorn (1980), Sawyerpuram (1980), Bangalore (1986)	Eral, Bangalore	1,622	February-April 2010
Kenya	Cianthia (1986), Cierria (1986), Nderu (1990), Thigio (1990)	Riakingenyi, Kerwa, Rusigeti	3,056	April-June 2010
Bolivia	Chulla (1992), Los Olivios (1990), Puntiti (1991), Pongonhuyo (1980)	Pairumani-Iscaypata, Igrana	1,467	June-August 2010
Six Countries	**19 Compassion Programs**	**13 Non-Treatment Areas**	**10,144**	**June 2008-August 2010**

Table 3: Summary Statistics (Means; Standard Deviations in Parentheses)

	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**Six Countries**
Sponsored as a Child	0.232	0.212	0.168	0.136	0.179	0.197	**0.183**
	(0.423)	(0.409)	(0.374)	(0.343)	(0.384)	(0.398)	**(0.387)**
Years Sponsored	11.325	6.635	7.496	11.065	10.187	9.422	**9.257**
	(3.067)	(2.412)	(4.636)	(3.230)	(3.350)	(3.707)	**(3.795)**
Age	28.968	26.684	29.980	32.562	30.817	29.106	**29.546**
	(8.642)	(6.438)	(8.759)	(8.806)	(7.917)	(7.895)	**(8.296)**
Gender	0.476	0.506	0.504	0.500	0.515	0.500	**0.504**
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	**(0.500)**
Number of Siblings	4.639	4.497	4.242	3.981	6.109	4.661	**4.899**
	(2.984)	(2.218)	(2.166)	(1.953)	(2.272)	(1.992)	**(2.375)**
Total Years of Education	9.185	8.723	12.166	11.677	10.4365	10.739	**10.525**
	(4.003)	(4.337)	(1.996)	(3.342)	(3.078)	(4.150)	**(3.678)**
Mother's Education	5.508	1.830	10.382	7.063	3.856	2.723	**4.454**
	(4.286)	(2.802)	(3.027)	(3.712)	(4.118)	(3.045)	**(4.319)**
Father's Education	7.268	3.201	10.425	7.708	5.516	4.642	**5.838**
	(4.564)	(3.602)	(3.250)	(3.741)	(4.443)	(3.411)	**(4.408)**
Marriage Age	21.990	21.057	25.136	25.345	24.123	24.279	**24.045**
	(4.690)	(4.046)	(4.553)	(3.637)	(3.852)	(4.764)	**(4.316)**
Number of Children	3.700	2.231	2.254	2.025	2.800	2.599	**2.567**
	(2.544)	(1.569)	(1.377)	(0.965)	(1.708)	(1.667)	**(1.693)**
Formally Employed	0.551	0.322	0.504	0.647	0.138	0.366	**0.369**
	(0.498)	(0.467)	(0.500)	(0.478)	(0.345)	(0.482)	**(0.483)**
White-Collar Employed	0.213	0.225	0.324	0.368	0.059	0.157	**0.202**
	(0.409)	(0.418)	(0.468)	(0.482)	(0.236)	(0.363)	**(0.401)**
Remittance	0.268	0.034	0.085	0.066	0.060	0.073	**0.078**
	(0.443)	(0.182)	(0.279)	(0.248)	(0.237)	(0.260)	**(0.268)**
Has Indoor Toilet	0.090	0.866	0.999	0.957	0.996	0.393	**0.810**
	(0.287)	(0.341)	(0.038)	(0.203)	(0.061)	(0.489)	**(0.393)**
Has Electricity	0.253	0.919	0.988	0.997	0.292	0.988	**0.706**
	(0.435)	(0.272)	(0.107)	(0.051)	(0.455)	(0.111)	**(0.456)**
Improved Walls	0.794	0.468	0.963	0.810	0.353	0.551	**0.550**
	(0.405)	(0.499)	(0.190)	(0.392)	(0.478)	(0.498)	**(0.498)**
Improved Roof		0.349	0.098	0.082	0.004	0.224	**0.126**
		(0.477)	(0.297)	(0.275)	(0.061)	(0.417)	**(0.332)**
Improved Floors		0.787	0.982	0.950	0.407	0.175	**0.623**
		(0.409)	(0.133)	(0.217)	(0.491)	(0.380)	**(0.485)**
Community Leader	0.095	0.022	0.024	0.022	0.017	0.058	**0.032**
	(0.293)	(0.148)	(0.153)	(0.148)	(0.130)	(0.234)	**(0.176)**
Church Leader	0.160	0.145	0.102	0.100	0.071	0.075	**0.100**
	(0.367)	(0.352)	(0.302)	(0.300)	(0.257)	(0.264)	**(0.300)**
Village Leader	0.046	0.004	0.008	0.016	0.004	0.044	**0.016**
	(0.209)	(0.067)	(0.089)	(0.126)	(0.066)	(0.206)	**(0.125)**
Estimated Salary	55.93	202.42	320.87	192.85	230.91	206.36	**209.62**
(If Salary >0)	(60.28)	(104.354)	(81.29)	(183.99)	(590.61)	(130.08)	**(209.59)**

Table 4: Total Years of Formal Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS HH FE	OLS HH FE	OLS HH FE	Regression Discont. Estimation	OLS HH FE (ITT)	12&Undr Eligibility IV	Sib Order Relative to Rollout IV	Sib Order Relative to Rollout IV	Sib Order RR IV--no Externalt.
CSP (Former Sponsored Child)	**1.282*** **	**1.533*** **	**-0.089**			**2.845*** **	**2.857*** **	**2.858*** **	**2.423*** **
	(0.099)	**(0.138)**	**(0.224)**			**(0.384)**	**(0.326)**	**(0.409)**	**(0.377)**
Younger Sibling of Sponsored		**0.508*** **						**0.004**	**0.305**
		(0.163)						**(0.290)**	**(0.245)**
Number of years sponsored			**0.153*** **						
			(0.023)						
Eligible for Program				**0.970*** **	**0.989*** **				
				(0.318)	(0.168)				
Sex: Male =1	0.159**	0.152**	0.166**	0.083	0.082	0.168***	0.167***	0.167***	0.146**
	(0.071)	(0.071)	(0.070)	(0.087)	(0.083)	(0.061)	(0.059)	(0.064)	(0.070)
Age of Individual	0.115***	0.136***	0.117***	1.71	0.219***	0.078**	0.080**	0.080**	0.073*
	(0.037)	(0.037)	(0.036)	(0.336)	(0.045)	(0.033)	(0.032)	(0.037)	(0.041)
Age Squared	-0.002**	-0.003***	-0.002***	-0.002**	-0.004**	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Oldest Child = 1	0.162*	0.177**	0.167*	0.125	0.121	0.094	0.097	0.098	0.116
	(0.086)	(0.086)	(0.086)	(0.107)	(0.103)	(0.073)	(0.079)	(0.079)	(0.084)
Birthorder:1=old	0.058	0.030	0.059	0.067	0.016	0.046	0.048	0.048	0.026
	(0.050)	(0.052)	(0.050)	(0.062)	(0.061)	(0.045)	(0.044)	(0.040)	(0.044)
Constant	8.745***	8.336***	8.668***	-27.4***	7.116***	8.882***	8.842***	8.839***	8.964***
	(0.812)	(0.809)	(0.808)	(7.744)	(1.019)	(0.700)	(0.699)	(0.661)	(0.791)
Observations	10,052	10,052	10,052	6,950	7,545	10,035	10,052	10,052	7,930
R-squared	0.06	0.06	0.07	0.04	0.04	0.05	0.05	0.06	0.06

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in (1) through (6). Bootstrapped clustered standard errors in parentheses in (7) through (9). First-stage estimations: Coefficient on (9) for CSP on ineligible siblings =-0.285 (0.048), on first eligible sibling = 0.136 (0.044), on second eligible sibling =0.180 (0.042), on third eligible sibling = 0.109 (0.040), on fourth eligible sibling = 0.026 ((0.0399), on fifth eligible sibling = -0.024 (0.041), sixth eligible sibling excluded, coefficient on eligible siblings greater than sixth = -0.040 (0.055). Joint *F*-statistic on instruments: 101.30, $p < 0.001$. Coefficient for younger sibling of CSP on ineligible siblings = -0.773 (0.035), on first eligible sibling = -0.787 (0.031), on second eligible sibling =-0.528 (0.030), on third eligible sibling = -0.287 (0.029), on fourth eligible sibling = -0.146 ((0.028), on fifth eligible sibling = -0.032 (0.029), sixth eligible sibling excluded, coefficient on eligible siblings greater than sixth = 0.057 (0.039). Joint *F*-statistic on instruments: 294.48, $p < 0.001$.

Table 5: Educational Outcomes and Achievements

	---------- Household Fixed Effect Estimations, Ordinary Least Squares ----------							IV Estim. 2SLS: Six Countries HH FE
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**Six Countries HH FE**	
Total Years of Education:								
CSP	3.121***	1.254***	0.958***	1.109***	1.748***	1.269***	**1.533*** **	**2.423*** **
	(0.632)	(0.412)	(0.216)	(0.340)	(0.203)	(0.415)	**(0.138)**	**(0.377)**
Younger Sibling of CSP	1.285*	0.166	0.720**	0.462	0.754***	0.506	**0.508*** **	**0.305**
	(0.713)	(0.494)	(0.316)	(0.426)	(0.226)	(0.483)	**(0.163)**	**(0.245)**
mean, untreated =	***8.37***	***7.96***	***12.10***	***11.42***	***10.21***	***10.31***	***10.19***	***10.19***
Boys, Years of Education:								
OLS, HH Fixed Effects: CSP	2.624**	0.468	1.083**	0.972	1.665***	0.652	**1.275*** **	**2.066*** **
	(1.118)	(0.835)	(0.446)	(0.829)	(0.333)	(0.795)	**(0.251)**	**(0.504)**
Younger Sibling of CSP	1.295	-0.251	0.913	0.080	0.517	-0.209	**0.251**	**-0.052**
	(1.355)	(0.912)	(0.635)	(0.953)	(0.383)	(0.936)	**(0.292)**	**(0.463)**
mean, untreated =	***8.39***	***8.36***	***11.85***	***11.21***	***10.29***	***10.55***	***10.26***	***10.26***
Girls, Years of Education:								
OLS, HH Fixed Effects: CSP	3.309***	1.791***	0.542	0.747	1.561***	1.188	**1.474*** **	**2.502*** **
	(1.080)	(0.645)	(0.353)	(0.597)	(0.365)	(0.775)	**(0.249)**	**(0.469)**
Younger Sibling of CSP	1.113	0.090	0.305	0.458	0.965**	0.542	**0.566** **	**0.685**
	(1.095)	(0.893)	(0.467)	(0.799)	(0.378)	(0.796)	**(0.278)**	**(0.458)**
mean, untreated =	***8.35***	***7.55***	***12.36***	***11.66***	***10.14***	***10.06***	***10.11***	***10.11***
Completed Primary:								
OLS, HH Fixed Effects: CSP	0.190***	0.044	0.013	0.055**	0.056***	0.071**	**0.058*** **	**0.087*** **
	(0.058)	(0.040)	(0.013)	(0.024)	(0.014)	(0.036)	**(0.011)**	**(0.032)**
Younger Sibling of CSP	0.121*	-0.012	0.019	0.042	0.024	0.018	**0.006**	**-0.011**
	(0.071)	(0.048)	(0.015)	(0.041)	(0.018)	(0.045)	**(0.015)**	**(0.025)**
mean, untreated =	***0.79***	***0.73***	***0.99***	***0.92***	***0.94***	***0.81***	***0.88***	***0.88***
Completed Middle:								
OLS, HH Fixed Effects: CSP	0.387***	0.077*	0.021	0.066**	0.110***	0.089**	**0.103*** **	**0.144*** **
	(0.079)	(0.045)	(0.016)	(0.027)	(0.026)	(0.041)	**(0.015)**	**(0.039)**
Younger Sibling of CSP	0.194*	0.037	0.045**	0.016	0.071**	0.034	**0.042*** **	**0.040**
	(0.100)	(0.058)	(0.022)	(0.041)	(0.029)	(0.051)	**(0.019)**	**(0.029)**
mean, untreated =	***0.51***	***0.59***	***0.99***	***0.89***	***0.86***	***0.72***	***0.79***	***0.79***
Completed Secondary:								
HH Fixed Effects OLS: CSP	0.305***	0.223***	0.158***	0.166***	0.203***	0.120**	**0.196*** **	**0.326*** **
	(0.074)	(0.052)	(0.050)	(0.054)	(0.034)	(0.054)	**(0.119)**	**(0.045)**
Younger Sibling of CSP	0.133	0.088	0.071	0.147**	0.138***	0.122*	**0.119*** **	**0.114*** **
	(0.087)	(0.071)	(0.064)	(0.073)	(0.038)	(0.073)	**(0.025)**	**(0.037)**
mean, untreated =	***0.19***	***0.39***	***0.72***	***0.56***	***0.31***	***0.50***	***0.44***	***0.44***
Completed University:								
HH Fixed Effects OLS: CSP	0.068	0.001	0.014	0.024	0.014	0.071*	**0.026*** **	**0.051* **
	(0.057)	(0.034)	(0.019)	(0.028)	(0.015)	(0.039)	**(0.011)**	**(0.027)**
Younger Sibling of CSP	-0.020	-0.027	0.018	0.042	0.004	0.013	**0.002**	**0.001**
	(0.065)	(0.030)	(0.025)	(0.037)	(0.018)	(0.046)	**(0.012)**	**(0.018)**
mean, untreated =	***0.054***	***0.015***	***0.019***	***0.041***	***0.037***	***0.101***	***0.042***	***0.042***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 6: Employment Impacts

	---------- Household Fixed Effect Estimations, Ordinary Least Squares ----------							Instr. Var
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Salaried Employment:								
CSP	0.019	0.084	0.082	0.082	0.100***	-0.029	**0.070*** **	**0.206*** **
	(0.072)	(0.064)	(0.068)	(0.054)	(0.028)	(0.057)	**(0.021)**	**0.045**
Younger Sibling of CSP	-0.062	-0.071	-0.030	-0.132*	0.083***	-0.113	**-0.010**	**-0.061**
	(0.117)	(0.078)	(0.088)	(0.077)	(0.030)	(0.075)	**(0.025)**	**(0.041)**
mean, untreated =	***0.54***	***0.28***	***0.48***	***0.64***	***0.13***	***0.35***	***0.36***	***0.36***
White-Collar Employed:								
CSP	0.073	0.132**	0.137**	0.021	0.053***	0.023	**0.071*** **	**0.173*** **
	(0.092)	(0.058)	(0.069)	(0.065)	(0.019)	(0.050)	**(0.019)**	**(0.045)**
Younger Sibling of CSP	-0.067	-0.085	0.013	-0.118	0.040*	-0.061	**-0.018**	**-0.057**
	(0.097)	(0.071)	(0.084)	(0.084)	(0.020)	(0.067)	**(0.022)**	**(0.035)**
mean, untreated =	***0.183***	***0.176***	***0.297***	***0.358***	***0.056***	***0.148***	***0.185***	***0.185***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 7: Impacts on Vocation: Marginal Effects, Multinomial Logit Estimations

Occupational Category	**Six Countries MN Logit Coefficients**	**Six Countries Marginal Effects**	**Mean Untreated**	**Occupational Category**	**Six Countries MN Logit Coefficients**	**Six Countries Marginal Effects**	**Mean Untreated**
1 Agriculture	0.535***	0.0156*	***0.051***	8 Small Business	0.070	-0.0082	***0.048***
	(0.145)	(0.0083)			(1.62)	(0.0062)	
2 Construction, Day Labor	0.890***	0.0105**	***0.024***	9 Ministry, Pastoral	0.593**	0.0023	***0.007***
	(0.221)	(0.0044)			(0.311)	(0.0024)	
3 Clerical, Sales	0.646***	0.0235***	***0.049***	10 Finance and Large Business	0.592***	0.0116*	***0.028***
	(0.139)	(0.0087)			(0.182)	(0.0065)	
4 Blue Collar	0.484***	0.0147*	***0.069***	11 Police, Army, Security, Fire	0.990***	0.0084**	***0.011***
	(0.126)	(0.0080)			(0.300)	(0.0042)	
5 Personal Services	0.326**	0.0033	***0.052***	12 Professional, Doctor., Lawyer	-0.032	-0.0043	***0.019***
	(0.150)	(0.0073)			(0.270)	(0.0035)	
6 Teaching	0.848***	0.0407***	***0.053***	13 Less-Skilled Tech, Call Centers	0.636***	0.0082*	***0.020***
	(1.22)	(0.0090)			(0.188)	(0.0043)	
7 Government	0.977***	0.0066	***0.007***	14 Nursing, Public Health, Hospital	0.536**	0.0028	***0.011***
	(0.363)	(0.0041)			(0.270)	(0.00295)	

Marginal effects, *dy/dx*, are from corresponding multinomial logit estimations; control variables are gender, age, age^2, birth order, oldest child and a dummy variable for household with sponsored child. Number of observations = 9,003. Psuedo R^2 = 0.0464, Chi-squared $p < 0.0001$.

Table 8: Wage & Remittance Impacts

	---------- Household Fixed Effect Estimations, Ordinary Least Squares ----------							Instr. Var
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Monthly Income: Est. Increase in $U.S. (Including Unemployed)								
CSP	-4.765	28.997**	32.195	37.799	13.245	1.826	**16.653****	**37.879***
	(12.914)	(14.373)	(23.726)	(25.412)	(17.379)	(18.457)	**(8.698)**	**(21.854)**
Younger Sibling CSP	-14.595	-18.014	-0.736	-14.442	13.549	-24.097	**-8.270**	**-29.603*****
	(15.165)	(18.153)	(30.180)	(28.379)	(13.342)	(23.191)	**(5.922)**	**(10.794)**
mean, untreated =	***34.10***	***59.40***	***158.50***	***126.60***	***67.80***	***120.60***	***97.40***	***97.40***
Monthly Income: Est. Increase in $U.S. (Excluding Unemployed								
CSP	-11.843	2.037	14.801	14.316	-19.003	11.525	**0.965**	**-38.992**
	(17.491)	(13.528)	(18.500)	(31.481)	(89.468)	(24.785)	**(21.242)**	**(44.494)**
Younger Sibling CSP	-24.792	-2.709	17.950	-32.601	13.231	5.821	**-7.723**	**-40.270**
	(20.131)	(27.097)	(28.183)	(45.371)	(55.879)	(40.755)	**(21.433)**	**(26.058)**
mean, untreated =	***55.90***	***212.40***	***320.80***	***192.80***	***230.90***	***206.30***	***209.60***	***209.60***
Sends Remittances to Parents or Siblings:								
CSP	0.210***	0.010	0.020	0.023	0.051***	-0.020	**0.073*****	**0.080*****
	(0.053)	(0.015)	(0.028)	(0.018)	(0.015)	(0.025)	**(0.008)**	**(0.028)**
Younger Sib of CSP	-0.051	-0.004	0.013	0.002	0.021	-0.034	**0.041*****	**-0.035**
	(0.068)	(0.021)	(0.037)	(0.024)	(0.018)	(0.034)	**(0.009)**	**(0.023)**
mean, untreated =	***0.221***	***0.020***	***0.038***	***0.057***	***0.054***	***0.072***	***0.067***	***0.067***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 9: Demographic Effects

	-------- Household Fixed Effect Estimations, Ordinary Least Squares --------							Instr. Var.
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Marriage Age: (Among all Married)								
CSP	3.010***	0.990	1.911*	0.453	0.330	-0.223	**0.654****	**1.706*****
	(1.104)	(0.645)	(1.019)	(0.383)	(0.315)	(0.657)	**(0.280)**	**(0.529)**
Younger Sib of CSP	-0.402	0.496	1.838	0.494	0.184	-0.751	**0.146**	**-0.701**
	(1.354)	(1.221)	(1.440)	(0.531)	(0.423)	(0.949)	**(0.363)**	**(0.594)**
mean, untreated =	***21.5***	***20.9***	***25.2***	***25.4***	***24.2***	***24.4***	***24.1***	***24.1***
Marriage by Age X (LP):								
Age 20, CSP	-0.036	-0.109***	-0.047***	-0.009	-0.015***	-0.059**	**-0.049*****	**-0.115*****
	(0.034)	(0.030)	(0.015)	(0.010)	(0.004)	(0.024)	**(0.007)**	**(0.014)**
Age 24, CSP	-0.059	-0.023	-0.018	-0.007	-0.020**	-0.027	**-0.030*****	**-0.059**
	(0.040)	(0.039)	(0.023)	(0.027)	(0.008)	(0.028)	**(0.010)**	**(0.040)**
Age 28, CSP	-0.029	0.107*	-0.033	-0.029	0.185***	0.044	**0.077*****	**0.222*****
	(0.080)	(0.057)	(0.045)	(0.042)	(0.024)	(0.048)	**(0.017)**	**(0.035)**
Fem. Fertility (Number Children)								
CSP	-0.681	0.224	0.045	-0.023	0.114	-0.350	**-0.013**	**0.097**
	(0.733)	(0.417)	(0.490)	(0.305)	(0.253)	(0.408)	**(0.167)**	**(0.291)**
Younger Sib of CSP	0.138	1.317*	0.758	-0.050	-0.033	-0.212	**0.161**	**0.614****
	(0.920)	(0.734)	(0.653)	(0.485)	(0.277)	(0.522)	**(0.214)**	**(0.297)**
mean, untreated =	***2.4***	***1.2***	***1.1***	***1.3***	***1.9***	***1.5***	***1.6***	***1.6***
Fem. Fertility by Age X: (Number Children, LP)								
Age 20, CSP	0.037	-0.054	-0.052	0.015	-0.041**	-0.038	**-0.033****	**-0.118*****
	(0.140)	(0.050)	(0.036)	(0.022)	(0.017)	(0.036)	**(0.014)**	**(0.022)**
Age 24, CSP	0.085	-0.123	-0.064	-0.008	-0.226***	0.027	**-0.112*****	**-0.476*****
	(0.127)	(0.115)	(0.083)	(0.118)	(0.045)	(0.121)	**(0.038)**	**(0.089)**
Age 28, CSP	-0.328	0.230	-0.076	-0.230	0.040	0.252	**0.027**	**-0.002**
	(0.339)	(0.325)	(0.283)	(0.144)	(0.135)	(0.333)	**(0.094)**	**(0.163)**
Num. of Children: (Given children in family)								
CSP	-0.563	0.359	-0.238	0.032	-0.003	0.050	-0.009	**0.118**
	(0.391)	(0.223)	(0.315)	(0.144)	(0.148)	(0.214)	(0.094)	**(0.205)**
Younger Sib of CSP	0.190	0.985*	-0.173	0.087	0.121	0.324	0.233*	**0.709*****
	(0.627)	(0.596)	(0.428)	(0.281)	(0.195)	(0.294)	(0.140)	**(0.205)**
mean, untreated =	***3.9***	***2.4***	***2.3***	***2.0***	***2.9***	***2.7***	***2.7***	***2.7***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 10: Leadership Impacts

	-------- Household Fixed Effect Estimations, Ordinary Least Squares --------							Instr. Var.
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Church Leader								
CSP	0.236***	0.055	0.043	0.008	0.055***	0.068*	**0.063*** **	**0.050* **
	(0.061)	(0.039)	(0.041)	(0.040)	(0.020)	(0.040)	**(0.014)**	**(0.030)**
Younger Sib of CSP	0.208***	0.042	-0.010	0.006	0.031	0.098**	**0.049*** **	**0.043* **
	(0.068)	(0.053)	(0.044)	(0.049)	(0.022)	(0.040)	**(0.016)**	**(0.024)**
mean, untreated =	***0.118***	***0.132***	***0.086***	***0.096***	***0.061***	***0.062***	***0.086***	***0.086***
Community Leader								
CSP	0.100**	0.007	-0.017	-0.005	0.012	0.051*	**0.019** **	**0.100** **
	(0.047)	(0.021)	(0.016)	(0.017)	(0.010)	(0.028)	**(0.008)**	**(0.047)**
Younger Sib of CSP	0.109**	-0.000	-0.016	-0.026*	0.013	0.035	**0.012**	**0.109** **
	(0.054)	(0.017)	(0.024)	(0.015)	(0.008)	(0.035)	**(0.008)**	**(0.054)**
mean, untreated =	***0.086***	***0.015***	***0.020***	***0.021***	***0.016***	***0.056***	***0.029***	***0.029***
Village Leader								
CSP	0.042	0.009	0.003	-0.014**	0.007	0.019	**0.009* **	**0.039*** **
	(0.034)	(0.010)	(0.003)	(0.007)	(0.009)	(0.018)	**(0.005)**	**(0.011)**
Younger Sib of CSP	0.051	0.008	0.011	0.002	-0.002	0.005	**0.006**	**0.005**
	(0.036)	(0.007)	(0.012)	(0.012)	(0.004)	(0.026)	**(0.005)**	**(0.010)**
mean, untreated =	***0.047***	***0.003***	***0.007***	***0.017***	***0.002***	***0.046***	***0.015***	***0.015***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 11: Dwelling Impacts

	-------- Household Fixed Effect Estimations, Ordinary Least Squares --------							Instr. Var.
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Improved Walls:								
CSP	0.003	0.027	0.013	-0.009	0.035	0.007	**0.022**	**-0.007**
	(0.035)	(0.047)	(0.014)	(0.075)	(0.031)	(0.046)	**(0.018)**	**(0.033)**
Younger Sib of CSP	-0.037	-0.007	0.019	-0.000	0.025	-0.005	**0.003**	**-0.034**
	(0.047)	(0.066)	(0.021)	(0.107)	(0.038)	(0.051)	**(0.023)**	**(0.034)**
mean, untreated =	***0.78***	***0.43***	***0.96***	***0.81***	***0.37***	***0.52***	***0.54***	***0.54***
Improved Floor:								
CSP		0.015	0.003	-0.000	0.092***	-0.039	**0.030****	**0.085****
		(0.024)	(0.004)	(0.018)	(0.031)	(0.039)	**(0.015)**	**(0.037)**
Younger Sib of CSP		0.019	0.002	-0.008	0.061*	-0.044	**0.013**	**-0.014**
		(0.026)	(0.006)	(0.027)	(0.036)	(0.043)	**(0.019)**	**(0.030)**
mean, untreated =		***0.73***	***0.98***	***0.95***	***0.41***	***0.17***	***0.62***	***0.62***
Improved Roof:								
CSP		-0.013	0.008	-0.006	0.000	-0.004	**-0.002**	**-0.029**
		(0.038)	(0.013)	(0.005)	(0.001)	(0.039)	**(0.009)**	**(0.022)**
Younger Sib of CSP		-0.051	0.009	-0.020	0.003	-0.022	**-0.009**	**-0.027**
		(0.049)	(0.017)	(0.020)	(0.002)	(0.044)	**(0.010)**	**(0.017)**
mean, untreated =		***0.32***	***0.10***	***0.083***	***0.004***	***0.209***	***0.118***	***0.118***
Indoor Toilet:								
CSP	0.045	0.039*	0.004	0.027		0.021	**0.015***	**0.009**
	(0.034)	(0.023)	(0.006)	(0.022)		(0.044)	**(0.008)**	**(0.015)**
Younger Sib of CSP	0.001	0.021	0.007	0.036		0.018	**0.010**	**0.020**
	(0.025)	(0.026)	(0.010)	(0.030)		(0.054)	**(0.010)**	**(0.015)**
mean, untreated =	***0.60***	***0.84***	***0.99***	***0.96***		***0.37***	***0.81***	***0.81***
Home has Electricity:								
CSP	0.107**	0.018	0.005	-0.004	0.080***	-0.007	**0.037*****	**0.091*****
	(0.051)	(0.022)	(0.004)	(0.008)	(0.026)	(0.008)	**(0.011)**	**(0.029)**
Younger Sib of CSP	0.008	-0.004	0.008	-0.003	0.027	-0.004	**0.004**	**-0.007**
	(0.062)	(0.025)	(0.006)	(0.006)	(0.034)	(0.009)	**(0.016)**	**(0.025)**
mean, untreated =	***0.21***	***0.90***	***0.98***	***0.99***	***0.30***	***0.98***	***0.71***	***0.71***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.

Table 12: Consumer Goods

	-------- Household Fixed Effect Estimations, Ordinary Least Squares --------							Instr. Var.
	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**OLS: Six Countries HH FE**	**2SLS: Six Countries HH FE**
Owns Mobile Phone:								
CSP	0.264***	0.132***	0.023	0.044	0.121***	0.058*	**0.110*** **	**0.156*** **
	(0.073)	(0.035)	(0.035)	(0.032)	(0.032)	(0.034)	**(0.016)**	**(0.033)**
Younger Sib of CSP	0.043	0.118**	0.025	-0.031	0.155***	0.061	**0.090*** **	**0.059* **
	(0.089)	(0.058)	(0.046)	(0.038)	(0.037)	(0.045)	**(0.021)**	**(0.032)**
mean, untreated =	***0.47***	***0.63***	***0.90***	***0.89***	***0.72***	***0.87***	***0.77***	***0.77***
Owns Automobile:								
CSP	-0.018	-0.019	0.067*	0.018	-0.003	-0.050	**-0.002**	**-0.041* **
	(0.054)	(0.031)	(0.036)	(0.028)	(0.008)	(0.049)	**(0.011)**	**(0.025)**
Younger Sib of CSP	-0.031	-0.020	0.073	-0.005	0.008	-0.048	**-0.000**	**-0.026**
	(0.065)	(0.034)	(0.046)	(0.027)	(0.010)	(0.050)	**(0.012)**	**(0.019)**
mean, untreated =	***0.016***	***0.029***	***0.068***	***0.062***	***0.023***	***0.0149***	***0.055***	***0.055***
Owns Motorcycle:								
CSP	0.008	-0.019	0.048	0.030	0.016	-0.006	**0.010**	**0.023**
	(0.045)	(0.025)	(0.042)	(0.052)	(0.011)	(0.031)	**(0.012)**	**(0.025)**
Younger Sib of CSP	-0.021	0.002	0.084	-0.050	0.007	-0.040	**0.002**	**0.006**
	(0.052)	(0.029)	(0.055)	(0.059)	(0.009)	(0.035)	**(0.013)**	**(0.017)**
mean, untreated =	***0.039***	***0.043***	***0.110***	***0.499***	***0.017***	***0.056***	***0.121***	***0.121***
Owns Bicycle:								
CSP	0.045	0.026	-0.006	0.048	0.033	-0.040	**0.027* **	**-0.002**
	(0.051)	(0.020)	(0.033)	(0.045)	(0.025)	(0.051)	**(0.015)**	**(0.028)**
Younger Sib of CSP	0.016	0.029	-0.023	0.051	0.024	0.004	**0.024**	**0.001**
	(0.080)	(0.025)	(0.044)	(0.057)	(0.028)	(0.065)	**(0.018)**	**(0.027)**
mean, untreated =	***0.21***	***0.04***	***0.09***	***0.35***	***0.14***	***0.31***	***0.18***	***0.18***
Purchased Land:								
CSP	-0.037	0.023	-0.009	0.037	0.021	-0.096**	**-0.004**	**-0.023**
	(0.077)	(0.023)	(0.040)	(0.031)	(0.024)	(0.042)	**(0.014)**	**(0.029)**
Younger Sib of CSP	-0.059	0.044	0.000	0.023	0.017	-0.093*	**-0.004**	**-0.052** **
	(0.094)	(0.027)	(0.047)	(0.035)	(0.026)	(0.055)	**(0.017)**	**(0.026)**
mean, untreated =	***0.269***	***0.083***	***0.084***	***0.130***	***0.126***	***0.246***	***0.142***	***0.142***

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in OLS estimations Bootstrapped clustered standard errors in IV estimations. First stage estimations: Refer to Table 4.