Development Economics III

Lecture 2

Konrad B. Burchardi
(IIES)

29th of March 2012

Today's Lecture

Yesterday, we saw models of why credit markets might fail, and which micro-finance might help to alleviate these problems.

Today:

- Discuss evidence that borrowers *are* credit constraint.
- Evidence that moral hazard can explain some of this. (In a very selected pool of borrowers, adverse selection does not.)
- We show that microcredit does seem to relieve credit constraints - for some.
- We look at empirical work which tries to test whether some of the models of micro-finance seen last time (and which) can explain these effects.
- We look at other aspects of micro-finance: other effects and mechanisms.

Table of Contents

1 Empirical Evidence: Credit
- Impact of Credit
- Digression: Testing Models of Credit

2 Empirical Evidence: Microcredit
- Impact of Microcredit
- Testing Models of Microcredit
- Food for Thought: Further Results

3 Summary and Outlook

Borrowers' Credit-Constraint: Empirical Evidence

We have seen several models of why credit markets might not provide (sufficient) credit to socially beneficial projects, e.g.:

Stiglitz and Weiss (1981): Some socially desirable projects do not obtain credit.

Stiglitz (1990): Borrowers obtain a smaller loan than optimal.

Is this a real issue, i.e. **are borrowers credit-constraint**?

Marginal Return to Capital
De Mel, McKenzie, Woodruff, 2008

One way to answer this question is to **estimate the marginal return to capital**, and compare it to the cost of funds.

Problem: The question is difficult to answer! Differences and changes in capital stock are likely correlated with ability, demand shocks, and other factors associated with the differences in the profitability of investments across firms.

Very convincing evidence on the marginal return to capital is from De Mel, McKenzie, and Woodruff, 2008. They **provide grants experimentally** and measure the effect on profits.

Marginal Return to Capital
De Mel, McKenzie, Woodruff, 2008: Setting

Setting:

- Three southern and southwestern districts of Sri Lanka.
- Sample of firms/microenterprises with invested capital of 100,000 LKR (about US$1,000) or less, excluding investments in land and buildings.

→ Treatments (below) large shock to business capital.

- Surveyed quarterly between 2005 and 2007.
- Full survey of 659 enterprises. After baseline survey data, 41 enterprises eliminated because they exceeded the 100,000 LKR maximum size or because a follow-up visit could not verify the existence of an enterprise. The remaining 618 firms constitute the baseline sample.
- In analysis excluding firms directly affected by Tsunami. Baseline sample of 408 enterprises. Of those 203 firms in retail sales and 205 in manufacturing/services.

Random Treatment: The prize consisted of one of 4 grants:

- 10,000 LKR ($\approx$ US$100) of equipment/inventories, or
- 20,000 LKR in equipment/inventories, or
- 10,000 LKR in cash, or
- 20,000 LKR in cash.

The 10,000 LKR treatment is equivalent to about three months of median profits reported by the firms in the baseline survey.

The median initial level of invested capital, excluding land and buildings, was about 18,000 LKR, implying that the small and large treatments correspond to approximately 55% and 110% of the median initial invested capital.

Marginal Return to Capital

De Mel, McKenzie, Woodruff, 2008: Reduced Form Treatment Effects

TABLE II
EFFECT OF TREATMENTS ON OUTCOMES

Impact of treatment amount on:	Capital stock (1)	Log capital stock (2)	Real profits (3)	Log real profits (4)	Owner hours worked (5)
10,000 LKR in-kind	4,793*	0.40***	186	0.10	6.06**
	(2,714)	(0.077)	(387)	(0.089)	(2.86)
20,000 LKR in-kind	13,167***	0.71***	1,022*	0.21*	−0.57
	(3,773)	(0.169)	(592)	(0.115)	(3.41)
10,000 LKR cash	10,781**	0.23**	1,421***	0.15*	4.52*
	(5,139)	(0.103)	(493)	(0.080)	(2.54)
20,000 LKR cash	23,431***	0.53***	775*	0.21*	2.37
	(6,686)	(0.111)	(643)	(0.109)	(3.26)
Number of enterprises	385	385	385	385	385
Number of observations	3,155	3,155	3,248	3,248	3,378

Notes: Data from quarterly surveys conducted by the authors reflecting nine survey waves of data from March 2005 through March 2007. Capital stock and profits are measured in Sri Lankan rupees, deflated by the Sri Lankan CPI to reflect March 2005 price levels. Columns (2) and (4) use the log of capital stock and profits, respectively. Profits are measured monthly and hours worked are measured weekly. All regressions include enterprise and period (wave) fixed effects. Standard errors, clustered at the enterprise level, are shown in parentheses. Sample is trimmed for top 0.5% of changes in profits.

Table II:

- The grants did increase the capital stock (first stage).
- The grants did increase profits (reduced form).

→ What we are interested in is: What is the marginal effect of an additional unit of business capital on profits?

→ The grants are an **instrument** for capital stock.

Marginal Return to Capital

De Mel, McKenzie, Woodruff, 2008: IV Estimates

TABLE IV
INSTRUMENTAL VARIABLE REGRESSIONS MEASURING RETURN TO CAPITAL FROM EXPERIMENT

	Real profits IV-FE (1)	Log real profits IV-FE (2)	Real profits 4 instruments (3)	Real profits adjusted (1) IV-FE (4)	Real profits adjusted (2) IV-FE (5)
Capital stock/log capital stock	5.85**	0.379***	5.16**	5.29**	4.59**
(excluding land & buildings)	(2.34)	(0.121)	(2.26)	(2.28)	(2.29)
First-stage					
Coefficient on treatment amount	0.91***	0.33***		0.91***	0.91***
F statistic	27.81	49.26	6.79	27.81	27.81
Observations	3,101	3,101	3,101	3,101	3,101
Number of enterprises	384	384	384	384	384

Notes: Data from quarterly surveys conducted by the authors reflecting nine waves of data from March 2005 through March 2007. Capital stock and profits are measured in Sri Lankan rupees, deflated by the Sri Lankan CPI to reflect March 2005 price levels. Profits are measured monthly. The estimated value of the owner's labor is subtracted from profits in columns (4) and (5), as described in the text. In column (4), the owner's time is valued by regression coefficients from a production function using baseline data; in column (5), we use the median hourly earnings in the baseline sample for each of six gender/education groups. A single variable measuring the rupee amount of the treatment is used as the instrument in columns (1) and (2) and (4) and (5). In column (3), we use four separate variables indicating receipt of each treatment type. Except in column (2), the coefficients show the effect of a 100-rupee increase in the capital stock. All regressions include enterprise and period (wave) fixed effects. Standard errors, clustered at the enterprise level, are shown in parentheses. The F statistic is the partial F statistic in the first-stage regression on the excluded instruments.
$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

Result:

- Table IV, Column (1): The instrumental variable estimate of the *monthly* gross return to capital is 5.85%. (More than 60% per year.)

Result:

- Table IV, Column (1): The instrumental variable estimate of the *monthly* gross return to capital is 5.85%. (More than 60% per year.)

Digression: Is this really an estimate of the marginal return to capital?

Marginal Return to Capital

De Mel, McKenzie, Woodruff, 2008: Interpretation

Take a simple model of a firm: Profits are a multiplicative function of effort e and capital x, $\pi = p(e)q(x)$. Suppose effort has cost ηe. The entrepreneur will choose effort to maximize π:

$$p'(e)q(x) = \eta. \tag{1}$$

Hence equilibrium effort e is a function of x. Is this a problem?

Marginal Return to Capital
De Mel, McKenzie, Woodruff, 2008: Interpretation

Assume the functional form $p(e) = e^{\alpha}$ and $q(x) = Bx^{\beta}$. This suggests, to estimate the marginal return to capital (!), we *should* run the regression (instrumented):

$$\log \pi_i = \log B + \alpha \log e_i + \beta \log x_i + \epsilon_i.$$

However, we did see results from the regression (instrumented):

$$\log \pi_i = \log B + \beta \log x_i + \nu_i, \;\; \nu_i = \epsilon_i + \alpha \log e_i.$$

The problem: Equation 1 shows that whatever shocks x (the capital grants here) will also impact effort/labour supply. In other words: The exclusionary restriction is violated.

Marginal Return to Capital

De Mel, McKenzie, Woodruff, 2008: IV Estimates

Therefore the authors correct for the effect of additional labour supply in columns (4) and (5):

TABLE IV

INSTRUMENTAL VARIABLE REGRESSIONS MEASURING RETURN TO CAPITAL FROM EXPERIMENT

	Real profits IV-FE (1)	Log real profits IV-FE (2)	Real profits 4 instruments (3)	Real profits adjusted (1) IV-FE (4)	Real profits adjusted (2) IV-FE (5)
Capital stock/log capital stock	5.85^{**}	0.379^{***}	5.16^{**}	5.29^{**}	4.59^{**}
(excluding land & buildings)	(2.34)	(0.121)	(2.26)	(2.28)	(2.29)
First-stage					
Coefficient on treatment amount	0.91^{***}	0.33^{***}		0.91^{***}	0.91^{***}
F statistic	27.81	49.26	6.79	27.81	27.81
Observations	3,101	3,101	3,101	3,101	3,101
Number of enterprises	384	384	384	384	384

Notes: Data from quarterly surveys conducted by the authors reflecting nine waves of data from March 2005 through March 2007. Capital stock and profits are measured in Sri Lankan rupees, deflated by the Sri Lankan CPI to reflect March 2005 price levels. Profits are measured monthly. The estimated value of the owner's labor is subtracted from profits in columns (4) and (5), as described in the text. In column (4), the owner's time is valued by regression coefficients from a production function using baseline data; in column (5), we use the median hourly earnings in the baseline sample for each of six gender/education groups. A single variable measuring the rupee amount of the treatment is used as the instrument in columns (1) and (2) and (4) and (5). In column (3), we use four separate variables indicating receipt of each treatment type. Except in column (2), the coefficients show the effect of a 100-rupee increase in the capital stock. All regressions include enterprise and period (wave) fixed effects. Standard errors, clustered at the enterprise level, are shown in parentheses. The F statistic is the partial F statistic in the first-stage regression on the excluded instruments.
$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

It does not change much. Certainly not the basic take-away!

Back to our question: Are we actually interested in the marginal return to capital (in the production function)? No.

We want to know by how much the profits increase to an injection of capital - taking into account the endogenous effort response! Then the estimate from column (1) is just right.

Result:

- The instrumental variable estimate of the *monthly* gross return to capital is 5.85%. (More than 60% per year.)
- The average of two yearly deposit rates published by the central bank for April 2005 - an estimate of the cost of funds (excluding banks' administrative costs) - 8% per year.

Marginal Return to Credit

De Mel, McKenzie, Woodruff, 2008: Take-Away

Result:

- The instrumental variable estimate of the *monthly* gross return to capital is 5.85%. (More than 60% per year.)
- The average of two yearly deposit rates published by the central bank for April 2005 - an estimate of the cost of funds (excluding banks' administrative costs) - 8% per year.

→ Strong evidence that micro-entrepreneurs in Sri Lanka are credit constraint.

Marginal Return to Credit
De Mel, McKenzie, Woodruff, 2008: Further Results

They "find that there is considerable heterogeneity of the returns along measurable dimensions. [...] Returns to capital are generally higher for entrepreneurs who are more severely capital constrained – those with higher ability and with fewer other wage workers in the household who can provide liquidity."

"One important exception to this is that while the conventional wisdom holds that women are more severely credit constrained, [they] find that the returns are much higher in enterprises owned by males than in enterprises owned by females."

Other Studies:

McKenzie and Woodruff (2006) estimate returns to capital among the smallest urban microenterprises in Mexico of around 180% per year. Returns in the Mexican data fall to around 40%–60% per year above US$500 of capital stock.

McKenzie and Woodruff (2008) undertake a similar experiment among enterprises in Mexico with less than US$900 of capital stock. They find returns in the range of 250%–360% per year.

Udry and Anagol (2006) estimate returns of small-scale agricultural producers in Ghana to be 50% per year (amongst those producing traditional crops) and 250% per year (non-traditional crops).

Other Studies (cont.):

Banerjee and Duflo (2004) take advantage of changes in the criteria identifying firms eligible for earmarked credit from Indian banks. They derive estimates of returns for this set of firms of 74%–100% per year.

Burgess and Pande (2005) They show that branch expansion into rural unbanked locations in India significantly reduced rural poverty. Evaluated at the sample mean, they find that rural branch expansion can explain a 14 to 17 percentage point decline in rural headcount, about half the overall fall in the period.

Testing Models of Credit Market Failure

It seems that marginal returns to credit are a lot higher than the cost of funds. Why? Can the models of credit market failures we saw help to explain these facts? And which?

A clever experimental design which helps to shed some light on this question is the study by Karlan and Zinman, 2009.

Brain Storming: How would you design an experiment to test whether adverse selection or moral hazard are important for repayment rates?

Let us start to think about **adverse selection**. The story was...

Let us start to think about **adverse selection**. The story was...
...that a different pool of individuals is applying for credit when the interest rate is different, *and that alone* has implications for the repayment rates.

Testing Models of Credit Market Failure

Karlan and Zinman, 2009: Idea

Let us start to think about **adverse selection**. The story was...
...that a different pool of individuals is applying for credit when the interest rate is different, *and that alone* has implications for the repayment rates.

→ So what we need is an experiment in which the **interest rate that people see when they apply is different**, but everything else is the same.

→ What is everything else? Everything, including especially the interest rate they need to pay, and their knowledge about it from the moment after selection happened.

Then any difference between the two groups is attributable to adverse selection, i.e. a different pool of borrowers applies.

And how can one design an experiment to test whether **moral hazard** is important for repayment rates?

There are different forms moral-hazard can take: ex-ante moral hazard in project choice, ex-ante moral hazard in effort choice, and ex-post moral hazard (enforcement problem).

Always the story is that a **higher r induces behavior** during and after the loan period **which makes repayment less likely**.

Testing Models of Credit Market Failure

Karlan and Zinman, 2009: Idea

There are different forms moral-hazard can take: ex-ante moral hazard in project choice, ex-ante moral hazard in effort choice, and ex-post moral hazard (enforcement problem).

Always the story is that a **higher r induces behavior** during and after the loan period **which makes repayment less likely**.

So what we need is two groups which are exposed to different interest rates, but are similar on observable and unobservable characteristics before they receive the loan.

Then any difference between the two groups
is attributable to moral hazard.

Both of these ideas are very clean. On top, Karlan and Zinman (2009) combine them in one experiment elegantly[1]:

Group 1 is offered the low interest rate, and once signed up, given the low interest rate.

Group 2 is offered the high interest rate, and once signed up, given the low interest rate.

Group 3 is offered the high interest rate, and once signed up, given the high interest rate.

Comparing group 1 and group 2 allows to test for adv. selection. Comparing group 2 and group 3 allows to test for moral hazard.

[1] In fact, they have third intervention: In group 1 and 2, some borrower where offered a continued lower interest rate on future loans, if they remained in good standing. This should also induce moral hazard.

Setting:

- Large South African micro-lenders.
- Offers small, high interest, short-term, uncollateralized credit with fixed monthly repayment schedules.
- Cash loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to a typical borrowers income.
- The lenders normal 4-month rates, absent the experiment: 7.75% – 11.75% per month depending on observable risk. 75% of clients in the high-risk category.
- Repeat borrowers had default rates of about 15%; first-time borrowers defaulted twice as often.
- Sample: 57,533 former clients with good repayment histories. Everyone had borrowed from the lender within the past 24 months, and did not have a loan outstanding in the 30 days prior to the offer.

Testing Models of Credit Market Failure

Karlan and Zinman, 2009: Results

TABLE I

EMPIRICAL TESTS OF HIDDEN INFORMATION AND HIDDEN ACTION: FULL SAMPLE

	OLS							
Dependent Variable:	*Monthly Average Proportion Past Due*		*Proportion of Months in Arrears*		*Account in Collection Status*		*Standardized Index of Three Default Measures*	
Mean of Dependent Variable:	0.09	0.09	0.22	0.22	0.12	0.12	0	0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contract rate (Hidden Action Effect 1)	0.005	0.002	0.006*	0.002	0.001	−0.001	0.014	0.004
	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.005)	(0.011)	(0.013)
Dynamic repayment incentive dummy (Hidden Action Effect 2)	−0.019*	−0.000	−0.028**	0.004	−0.025**	−0.004	−0.080**	−0.000
	(0.010)	(0.017)	(0.011)	(0.021)	(0.012)	(0.020)	(0.032)	(0.057)
Dynamic repayment incentive size		−0.005		−0.009**		−0.006		−0.023*
		(0.004)		(0.004)		(0.005)		(0.013)
Offer rate (Hidden Information Effect)	0.005	0.004	0.002	0.002	0.007	0.007	0.015	0.015
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.011)	(0.012)
Observations	4348	4348	4348	4348	4348	4348	4348	4348
Adjusted R-squared	0.08	0.08	0.14	0.15	0.06	0.06	0.10	0.11
Probability(both dynamic incentive variables = 0)		0.06		0.00		0.06		0.01
Probability(all 3 or 4 interest rate variables = 0)	0.0004	0.0005	0.0003	0.0012	0.0006	0.0016	0.0000	0.0001

*significant at 10%; **significant at 5%; ***significant at 1%. Each column presents results from a single OLS model with the RHS variables shown and controls for the randomization condtions: observable risk, month of offer letter, and branch. Adding loan size and maturity as additional controls does not change the results. Robust standard errors in parentheses are corrected for clustering at the branch level. "Offer rate" and "Contract rate" are in monthly percentage point units (7.00% interest per month is coded as 7.00). "Dynamic repayment incentive" is an indicator variable equal to one if the contract interest rate is valid for one year (rather than just one loan) before reverting back to the normal (higher) interest rates. "Dynamic repayment incentive size" interacts the above indicator variable with the difference between the lender's normal rate for that individual's risk category and the experimentally assigned contract interest rate. A positive coefficient on the Offer Rate variable indicates hidden information, a positive coefficient on the Contract Rate or Dynamic Repayment Incentive variables indicates hidden action (moral hazard).

The dependent variable in columns (7) and (8) is a summary index of the three dependent variables used in columns (1)–(6). The summary index is the mean of the standardized value for each of the three measures of default.

Results:

- The data certainly cannot reject the null that adverse selection is not important. At this interest rate margin, in this pool of borrowers. And this is a very selected pool: former clients with good repayment!
- There is some evidence that **moral hazard is important**, especially from the dynamic incentives experiment.

[An aside: Check their little theory for the modeling technique.]

Table of Contents

1 Empirical Evidence: Credit
- Impact of Credit
- Digression: Testing Models of Credit

2 Empirical Evidence: Microcredit
- Impact of Microcredit
- Testing Models of Microcredit
- Food for Thought: Further Results

3 Summary and Outlook

There are various working papers reporting results from the randomized introduction of micro-credit lending:

- Banerjee, Duflo, Glennerster, Kinnan, 2010: "The Miracle of Microfinance: Evidence from a Randomized Evaluation."
- Crépon, Devoto, Duflo, Pariente, 2011: "Impact of Microcredit in Rural Areas of Morocco: Evidence from a Randomized Evaluation."
- Karlan, Dean S. and Jonathan Zinman, 2011: "Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation."

Impact of Microcredit

Banerjee, Duflo, Glennerster, Kinnan, 2010: *"The Miracle of Microfinance: Evidence from a Randomized Evaluation."*

Impact of Microcredit

Banerjee, Duflo, Glennerster, Kinnan (2010)

Setting/Randomization:

- 104 urban ‘slums’ in Hyderabad.
- Areas selected based on having no pre-existing microfinance presence, and having residents who were desirable potential borrowers. Poor, but not the poorest.
- **Half of 104 slums were randomly selected for opening of an MFI branch**.
- The baseline survey took place in 2005, the follow up 15-18 months after the introduction of micro-finance in an area.
- Household survey of on average of 65 households in a neighborhood, and a total of 6,850 households.

→ They randomly assign microcredit across areas (not individuals). What advantages/drawbacks does this have?

Credit product offered by the MFI:

- A group has 6-10 women; self-selected; jointly liable.
- Eligible are females, between 18-59 years, have lived in area for a year, and at least 80% of women own home.
- First loan is Rs. 10,000, ≈ $1,000 at PPP exchange rates.
- 50 weeks to repay principal and interest; interest rate: 12%.
- If all members of a group repay loan, eligible for second loans of Rs. 10,000-12,000; and later up to Rs. 20,000.
- MFI does not require its clients to borrow to start a business (atypical). 'First generation' group loan product.

→ Standard **'first generation'** micro-finance loan.

Take-Up:

To interpret differences between treatment and control areas as due to microcredit, need that MFI borrowing is higher in treatment areas (First Stage).

Table 2: First stage

	(1) Borrows from Spandana	(2) Borrows from any MFI	(3) Borrows on credit	(4) Spandana borrowing (Rs.)	(5) MFI borrowing (Rs.)	(6) Borrowing on credit (Rs.)
Treatment	0.133***	0.083***	-0.093***	1406.814***	1250.504**	-390.956
	[0.023]	[0.030]	[0.034]	[261.568]	[477.956]	[1168.656]
Control Mean	0.052	0.186	.441	592.47	2404.7	8757.9
Control Std Dev	0.222	0.389	.497	2826.855	6698.2	32786.0
N	6651	6651	6638	6651	6651	6638

Note: Cluster-robust standard errors in brackets. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .1, ** means statistically significant at .05, *** means statistically significant at .01.

Result:

- The expansion of the MFI did indeed increase micro-loan borrowing from some 18.6% in the control group to 27% in the treatment areas.
- Over 70% of households do not take micro-loans. The authors write: "In short, microcredit is not for every household, or even most households in Hyderabad[...]."

Subsequently focus on intent to treat (ITT) estimates.

Impact of Microcredit
Banerjee, Duflo, Glennerster, Kinnan (2010): Results

Effects on expenditure:

Table 4: Impacts on monthly household expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Rs per capita per month								
	Total PCE	Nondurable PCE	Food PCE	Durable PCE	Durables used in a business	"Temptation goods"	Festivals (not weddings)	Any home repair>Rs 500 last year	75th percentile of home repair value (Rs)
Treatment	9.863	-6.689	-12.674	19.575*	6.832*	-8.859*	-22.217**	0.03	-1000
	[37.231]	[31.857]	[11.618]	[11.308]	[3.519]	[4.885]	[10.620]	[0.020]	[1320.07]
									75th percentile
Control Mean	6821	6775	6821	6775	6817	6857	6857	0.495	in control is
Control Std Dev	1419.229	1304.786	520.51	116.174	5.335	83.88	119.489	0.501	8000
N	978.299	852.4	263.099	332.563	89.524	130.213	161.522	2189	2189

Note: Cluster-robust standard errors in brackets. "Temptation goods" include alcohol, tobacco, gambling, and food and tea outside the home. Durables include assets for household or business use. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

→ No effect of access to microcredit on average monthly expenditure per capita after 15-18 months. (Delayed response?)
→ Expenditure on durable goods increased in treated areas.

Effects on business creation:

Table 3a: Impacts on business creation and business outcomes

	All households		Existing business owners					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New business	Stopped a business	Profit	Inputs	Revenues	Employees	Wages (Rs per month)	Value of assets used in businesses
Treatment	0.016**	-0.003	475.15	2391.534	2866.683	-0.028	-100.937	857.876
	[0.008]	[0.004]	[2326.340]	[4441.696]	[3187.618]	[0.084]	[136.518]	[979.533]
Control Mean	0.054	0.031	550.494	13193.81	13744.304	0.384	411.477	6675.911
Control Std Dev	0.252	0.173	46604.8	59769.3	47025.5	1.656	2977.457	16935.123
N	6735	6650	2362	2362	2362	2365	2365	2360

Note: Cluster-robust standard errors in brackets. Profits, inputs and revenues are monthly, measured in Rs. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

→ Number of new businesses increased by one third.

Impact of Microcredit
Banerjee, Duflo, Glennerster, Kinnan (2010): Results

Effects on welfare measures:

Table 5: Treatment effects on empowerment, health, education

	Women's empowerment: All households				HHs with loans	Health: HHs w/ kids 0-18
	(1)	(2)	(3)	(4)	(5)	(6)
	Woman primary decision-maker	Woman primary decision-maker (non-food spending)	Health expenditure (Rs per capita/mo)	Index of social outcomes	Woman primary decision-maker on loans	Child's major illness
Treatment	0.014	0.024	-2.608	0.008	0.009	0.017
	[0.035]	[0.032]	[12.431]	[0.023]	[0.017]	[0.032]
Control Mean	0.662	0.516	140.253	-0.002	0.281	0.420
Control Std Dev	0.473	0.500	455.740	0.457	0.396	0.659
N	6849	6849	6821	6856	6028	5871

→ No effect on measures of health, education, or women's decision-making after 15-18 months. (Delayed response?)

The **aggregate effects of micro-finance** 15-18 months after its introduction, are mixed and inconclusive: New business open, durable consumption increases, consumpt. of temptation goods decreases, but aggregate expenditure does not change, and we see no effect on measures of welfare.

→ Will we see delayed effects?

But: average results mask interesting **heterogenous effects**.

Heterogeneous effects:

Table 8: Effects by business status: borrowing and expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Borrowing		Monthly PCE				Business outcomes		Social index
	Borrows from any MFI	Non-MFI loan age (years)	Durable expenditure	Business durables	Nondurable expenditure	"Temptation goods"	Started new business	Stopped business	
Main effects									
New biz propensity	0.00	-.281**	4.49	-7.58	201.94***	-25.03***	.046**	-0.08	.127***
(no old biz)	(0.03)	(0.13)	(19.68)	(7.62)	(57.56)	(8.10)	(0.02)	(0.11)	(0.039)
Any old biz	.125***	-.309**	50.13**	1.74	202.42***	-10.58	.0395**	-0.15	.158***
	(0.03)	(0.14)	(22.08)	(9.20)	(51.13)	(7.97)	(0.02)	(0.09)	(0.038)
Interaction w treatment									
No old biz	.095**	-0.31	-46.72**	-5.10	213.30**	19.90*	-0.02	0.02	0.065
	(0.05)	(0.20)	(23.10)	(9.33)	(99.12)	(12.06)	(0.02)	(0.16)	(0.057)
New biz propensity	-0.02	0.24	67.40**	7.45	-260.24**	-32.87***	.0424*	0.04	-0.064
	(0.04)	(0.20)	(29.17)	(8.63)	(102.29)	(12.35)	(0.02)	(0.18)	(0.053)
Any old biz	.085*	-0.09	55.42**	18.90**	65.12	-14.71*	0.01	0.00	0.001
	(0.05)	(0.12)	(24.53)	(8.86)	(56.03)	(8.86)	(0.01)	(0.01)	(0.028)
Control mean of LHS var	0.19	0.85	116.17	5.34	1,304.79	83.88	0.05	0.04	-0.001
Control Std Dev	0.39	1.41	332.56	89.52	852.40	130.21	0.25	0.19	0.456
N	5996	6037	6141	6179	6141	6183	6183	2299	6183

Similar rates of take up of MFI loans across groups (column 1). Therefore columns 3-6 show different **uses** of MFI loans!

Heterogeneous effects:

- Households with an existing business at the time of the program invest more in durable goods, while their nondurable consumption does not change.
- Households with high propensity to become new business owners increase their durable goods spending and see a decrease in nondurable consumption, consistent with the need to pay a fixed cost to enter entrepreneurship.
- Households with low propensity to become business owners increase their nondurable spending.

Their conclusion: "Our results suggest that microcredit is an important financial tool for some households:

- for households already engaged in entrepreneurship, it allows expansion of the household business;
- for those with high returns to entrepreneurship, but rates of time preference high enough that they did not become entrepreneurs in the absence of microcredit, access to microcredit makes it possible to pay the fixed cost of starting a business;
- and for households with low returns to entrepreneurship and high rates of time preference, microcredit facilitates borrowing against future income to finance current consumption."

Similar Evidence:
Crépon, Devoto, Duflo, Pariente, 2011: *"Impact of Microcredit in Rural Areas of Morocco: Evid. from a Random. Evaluation."*

The also investigate the aggregate effects of the random expansion of a MFI in to some areas, and not to other.

However, their study is set in an **rural** environment in which previously **no micro-lender** was active.

Findings:

- The program increased access to credit significantly, but again the vast majority does not take a micro-credit.
- They find **no effect on average consumption** as well as on other outcomes such as health and education.
- **Existing self-employment activities** expand their scale, for both non-livestock agriculture and livestock activities. They have higher profits, but part of the effect on income is offset by lower wage earnings. They decrease their non-durable consumption and consumption overall and save more.
- For those without an own activity at baseline: positive but not significant effect on overall consumption; significant increase in expenditure on food, and on durable goods.

Impact of Microcredit

Take-Away

- Relatively low take-up of micro-credit, and little to no 'transformational' effect.
- Existing or potential business owners seem to be credit constraint, and micro-credit helps them to expand/start their business.
- For some other borrowers micro-credit might help borrow against future income (smooth consumption).

→ Overall impact might be smaller than thought, but might be an effective tool to provide credit for small credit constraint entrepreneurs.

Another research design to estimate the impact of micro-finance is to randomly assign **individuals** to treatment and comparison groups amongst applicants.

Upside: Might give more precise estimates of effects, or - said differently - requires smaller sample size.[2]

Downside: (i) Estimates are valid only for a very specific subgroup (those that applied) and
(ii) in the presence of spillovers, the comparison between treatment and comparison would be biased.

Leading Example: Karlan and Zinman (2011) *"Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation."*

[2] Random program placement typically requires larger sample sizes because of clustering issues and low take-up rates.

Setting/Randomization:

- For-profit lender that makes small, 3-month loans at 60% annualized interest rates to micro-entrepreneurs in the outskirts of Manila.
- Worked with lender and build a quantitative model that distinguishes creditworthy or not creditworthy applicants from marginal ones (1601 observations were classified "marginal", corresponding to 74% of the sample frame).
- Marginal applicants get approved for a loan according to some preassigned probability.

Credit product:

- First-time borrowers. Requirements: 18-60 years, in business > 1 year, in residence > 1 year if homeowner or > 3 years if renter, and daily income > 750 pesos.
- Loan sizes of 5000-25,000 pesos. Substantial relative to borrower income. For example, the median loan size made, 10,000 pesos ($220), was 37% of the median borrowers net monthly income.
- Loan maturity was 13 weeks, with weekly repayments.
- Effective annual interest rate around 60%.
- Individual liability loans.

Impact of Microcredit
Karlan and Zinman (2011)

Table 2. Microcredit in theory: Intention-to-treat effects of credit access on widely hypothesized outcomes. For the full sample and for females, data are OLS results for the independent variable "assigned a loan"; Huber-White SEs and control group means for the dependent variable listed in each row are also shown. The incremental effect on males is shown as an estimate for the interaction between "assigned a loan" and "male." Variation in sample sizes is due to survey question nonresponse. The summary index is in standard deviation units of the average outcome of its components. All estimates control for probability of assignment to treatment and for timing of treatment assignment and survey measurement. Borrowing measures do not count the 1% of loans that are too large (>50,000 pesos) to be plausibly affected by the treatment.

	Full sample			Females			Incremental effect on males		
	OLS result	SE	Control group mean	OLS result	SE	Control group mean	Estimated interaction	SE	Control group mean
Borrowing									
Number of loans from financial institutions in month before survey	0.094**	0.045	0.359	0.080	0.051	0.385	0.039	0.095	0.244
Number of loans from friends, family, or moneylenders in month before survey	–0.011	0.042	0.286	–0.011	0.045	0.279	0.010	0.104	0.317
Business size									
Number of businesses in household	–0.102*	0.060	1.378	–0.057	0.062	1.354	–0.265	0.181	1.488
Number of paid employees (not including in-kind contributions) in all household businesses	–0.273**	0.123	0.878	–0.214	0.130	0.801	–0.272	0.417	1.220
Subjective well-being									
Life satisfaction (scale: 1–4, 1 = not at all, 4 = very)	0.016	0.063	2.818	–0.024	0.067	2.855	0.209	0.168	2.659
Job stress (scale: –12 to 0: 0 = no stress, –12 = always stressed)	–0.190	0.227	–6.725	0.033	0.254	–6.912	–1.189**	0.513	–5.925
Summary index of above outcomes, optimism, calmness, worry, job satisfaction, decision power, and socioeconomic status	–0.053*	0.030	0.000	–0.043	0.032	–0.014	–0.042	0.082	0.064
	N = 1062–1113						N(male) = 160–165		

*P < 0.10, **P < 0.05, ***P < 0.01.

Impact of Microcredit
Karlan and Zinman (2011)

Table 3. Microcredit in practice: Intention-to-treat effects on household risk management. For the full sample and for females, impacts on trust outcomes are estimated using ordered probit, and other data are OLS results for the independent variable "assigned a loan"; Huber-White SEs and control group means for the dependent variable listed in each row are also shown. The incremental effect on males is shown as an estimate for the interaction between "assigned a loan" and "male." Variation in sample sizes is due to survey question nonresponse. All estimates control for probability of assignment to treatment and for timing of treatment assignment and survey measurement.

	Full sample			Females			Incremental effect on males		
	OLS or ordered probit result	SE	Control group mean	OLS or ordered probit result	SE	Control group mean	Estimated interaction	SE	Control group mean
Financial instruments									
Any health insurance	–0.035	0.038	0.658	–0.018	0.043	0.646	–0.101	0.094	0.707
Any other type of insurance	–0.079**	0.039	0.486	–0.066	0.043	0.475	–0.072	0.101	0.537
Any savings in household	0.002	0.039	0.591	–0.009	0.043	0.594	0.063	0.099	0.575
Family/community networks									
Trust that you would not be taken advantage of (1 = people would take advantage, 10 = people would be fair)	–0.060	0.082	7.685	–0.087	0.092	7.725	0.150	0.192	7.512
Trust in your neighborhood (–4 = no trust, –1 = complete trust)	0.209**	0.090	–2.215	0.203**	0.101	–2.219	0.064	0.196	–2.195
Trust in people you know personally (–4 = no trust, –1 = complete trust)	0.036	0.093	–1.895	0.001	0.102	–1.882	0.215	0.237	–1.951
Trust in your business associates (–4 = no trust, –1 = complete trust)	0.101	0.089	–2.184	0.080	0.101	–2.175	0.117	0.186	–2.225
Could get financial assistance from family or friends in an emergency	0.010	0.027	0.883	–0.003	0.030	0.888	0.080	0.068	0.861
Could get 10,000 pesos' worth of financial assistance from family or friends in an emergency	0.102***	0.040	0.370	0.091**	0.044	0.379	0.062	0.102	0.333
Could get unlimited financial assistance from family or friends in an emergency	0.090**	0.035	0.254	0.074*	0.039	0.267	0.091	0.087	0.194
	N = 995–1113						N(male) = 151–165		

$^*P < 0.10$, $^{**}P < 0.05$, $^{***}P < 0.01$.

Results:

- Net borrowing increased in the treatment group relative to controls.
- The number of business activities and employees decreased.
- Subjective well-being declined slightly.
- Little evidence that treatment effects were more pronounced for women.
- Micro-credit seem to increase ability to cope with risk, strengthen community ties, and increase access to informal credit.

They conclude: "Thus, microcredit here may work, but through channels different from those often hypothesized by its proponents." **What do you think?**

How does this square up with the other results?

How does this square up with the other results?

Estimate the effect of micro-credit on **marginal applicants**. Maybe the infra-marginal effect of 'credit-worthy' applicants is quite different? Applicants are 'marginal' for a reason...

The credit product is different from the 'first generation' loans studied in the previous experiments. It is not individual liability, and it is short-term (13 weeks). The latter might hinder business investments with longer term payoffs.

Effect for marginal applicants policy relevant effect? This is obviously just one margin on which to expand micro-credit. Another is to expand to areas where no micro-credit exists.

This does obviously not mean that the findings are not **thought provoking**, they very much are!

Testing Models of Microcredit

We saw that micro-finance has some effects.
Does any of the models we saw yesterday explain these?

To my mind, there are two levels to this question:

Testing Models of Microcredit

We saw that micro-finance has some effects.
Does any of the models we saw yesterday explain these?

To my mind, there are two levels to this question:

(a) Does micro-finance help by alleviating credit constraints?
  - $\rightarrow$ Micro-finance has many aspects, and potentially many effects. We will come back to some of this in a second.

(b) If it helps by alleviating credit constraints, does any of the models analyzed yesterday explain why it helps?

We will now focus on (b) and assume that a least one effect of micro-finance is to alleviate credit constraints. [The evidence seen before on business expansion suggests this is true.]

Testing Models of Microcredit

Two very different approaches to address this question:

Ahlin and Townsend, 2007: Derive, creatively, several auxiliary predictions of each of the joint liability models. (Step 1) Differential predictions allow to test the models against each other. (Step 2)

Giné and Karlan, 2011: Run an experiment where after groups have been formed, joint liability is randomly transformed into individual liability.

Step 1: Ahlin and Townsend (2007) derive (with assumptions) for each of Stiglitz (1990), Besley and Coate (1995), Banerjee et al. (1994) and Ghatak (1999)[3] the **comparative statics they imply** for the rate of repayment with respect to:

- the joint liability payment,
- correlation of returns,
- cooperative behavior,
- productivity,
- the interest rate, and
- the loan size (amongst others),

treating the interest rate as exogenous.

[3] This is similar in spirit to the paper we have seen, Ghatak (2000), but has continuous types.

Is keeping the interest rate fixed innocuous? No. Some of the predictions might change when it is endogenised.

Example: They find for Ghatak (1999) that "[a] higher joint liability payment makes borrowing relatively less attractive. Thus the higher a groups [c], the smaller and more risky the pool from which it is drawn." (Proposition 10)

This is the opposite of the main take-away of Ghatak (2000).

Is keeping the interest rate fixed innocuous? No. Some of the predictions might change when it is endogenised.

Example: They find for Ghatak (1999) that "[a] higher joint liability payment makes borrowing relatively less attractive. Thus the higher a groups [c], the smaller and more risky the pool from which it is drawn." (Proposition 10)

This is the opposite of the main take-away of Ghatak (2000).

What is the "**correct**" way of doing this?

The correct way of doing this depends on the empirical setting:

- If you have data from a setting where - for some reason - the interest rate is fixed/exogenous, deriving the predictions of the model with r kept fixed is appropriate. They argue this is true for their setting (Thai villages).
- If you have data from a setting where you cannot think of r as fixed - most cases of competitive or monopolistic credit markets - then one would need to derive predictions of a model which endogenises r.

[**An aside:** The nature of the competitive environment is often not analysed in this literature, but object of recent debates.]

Testing Models of Microcredit: Step 1

Ahlin and Townsend, 2007: Derived Comparative Statics

Table 1

Repayment Implications

An entry marked with a "‡" corresponds to a variable not included in the original model.

Variable	Effect on Repayment			
	Stiglitz	BBG	BC	Ghatak
liability payment q	↓[a]	↑		↓
positive correlation	↑‡[b]		↓‡[b]	↑‡[b]
cooperative behavior	↑‡	↓‡[c]	↓‡[d]	
cost of monitoring		↓		
official penalties			↑	
unofficial penalties			↑	
screening				↑
productivity H	↑‡	↑‡	↑‡	↑‡
interest rate r	↓	↓	↓	↓
loan size L	↓	↓‡		↗ ↘‡

Under assumption A2, section 2.1.1.
All correlation results rely on general, symmetric parametrizations of the correlation.
If the marginal cost of penalizing is less than one.
If unofficial penalties are larger than the loss to a borrower due to his partner's default.

Step 2: The idea of the empirical work is to see how, given cross sectional data on group repayment R and characteristics $X = (X_1, \ldots, X_M)$, the frequency of repayment R varies across groups with different characteristics X.

Potentially one could determine the shape of the entire probability of repayment surface $P(R = 1|X)$ in each of the theories. To estimate this too little data is available. Instead they focus on estimating the partial $\partial P(R = 1|X)/\partial X_m$.

Even this requires further assumptions. They make two:

Assumption 1: $P(R = 1|X^g)$ can be written as $P(\beta X)$. This restricts covariates to enter repayment probabilities as a linear combination, leaving P unrestricted.

Assumption 2: The probability function P is logistic.

→ This gives a standard logit model, which can be estimated by maximum likelihood.

Potential problems?

Even this requires further assumptions. They make two:

Assumption 1: $P(R = 1|X^g)$ can be written as $P(\beta X)$. This restricts covariates to enter repayment probabilities as a linear combination, leaving P unrestricted.

Assumption 2: The probability function P is logistic.

→ This gives a standard logit model, which can be estimated by maximum likelihood.

Potential problems? Endogeneity, 'selection into models'.

I spare you going in detail through the empirics. They depend on finding some measures for all the above variables, which can be interpreted in various ways.[4]

Their conclusion: "We find that the Besley and Coate model of social sanctions that prevent strategic default performs remarkably well, especially in the low-infrastructure northeast region. The Ghatak model of peer screening by risk type to overcome adverse selection is supported in the central region, closer to Bangkok. [...] Social structures that enable penalties can be helpful for repayment, while those which discourage them can lower repayment."

[4] And I do not think they are the most interesting bit about the paper.

A different approach to testing the models of joint liability lending is the experiment by Giné and Karlan. What they do is to randomize the contractual terms. Two experiments.

1. Half of the MFI's existing group-lending centers in a region were randomly converted to individual liability (but maintained group meetings) *after the screening took place*.
2. Villages were randomly assigned to be offered *new centers* with either group liability, centers with individual liability or centers with phased-in individual liability.

The first experiment allows to see whether, after peer screening, group liability has any "**effect on the mitigation of moral hazard through improved monitoring or enforcement**".

Testing Models of Microcredit
Giné and Karlan, 2011

Explanatory variable:	(1) Proportion of missed weeks	(2) Indicator for having at least one missed week	(3) Proportion of past due balance, at maturity date	(4) Indicator for having past due, at maturity date	(5) Proportion of past due balance, 30 days after maturity date	(6) Indicator for having past due, 30 days after maturity date	(7) Total excess savings	(8) Loan size
Panel A: Baseline clients								
All Loans								
Individual liability	0.005	-0.004	-0.001	0.008	-0.000	0.011	-309.973**	-924.722***
	(0.014)	(0.034)	(0.001)	(0.012)	(0.001)	(0.011)	(131.414)	(317.470)
Observations	14333	14333	14333	14333	14182	14182	14333	14333
R-squared	0.102	0.099	0.036	0.227	0.024	0.243	0.303	0.166
Mean of dependent variable	0.075	0.430	0.002	0.045	0.001	0.031	842.3	6844.4
`Hump' loans only: disbursed before and matured after the conversion date								
Individual liability	0.003	0.012	-0.001	0.006	-0.000	-0.000	-51.803*	-540.902
	(0.015)	(0.052)	(0.001)	(0.009)	(0.000)	(0.000)	(28.772)	(359.792)
Observations	2,985	2,985	2,985	2,985	2,985	2,985	2,985	2,985
R-squared	0.158	0.130	0.010	0.033	0.006	0.006	0.061	0.202
Mean of dependent variable	0.073	0.445	0.001	0.010	0.000	0.000	248.3	7947.0

No change in repayment for centers converted to individual liability, and nil effect is estimated accurately.

Firstly, they claim that this is evidence that joint liability was not important to mitigate moral hazard in enforcement. True?

→ Besley and Coate (1995) explicitly stress that the total effect of joint liability is ambiguous. It might - for the parameter values in this setting - be just zero. Hence no strong evidence that this mechanism is not at work.

Firstly, they claim that this is evidence that joint liability was not important to mitigate moral hazard in enforcement. True?

→ Besley and Coate (1995) explicitly stress that the total effect of joint liability is ambiguous. It might - for the parameter values in this setting - be just zero. Hence no strong evidence that this mechanism is not at work.

Secondly, is this evidence that joint liability is not important through its effect on adverse selection?

Thirdly, removing joint liability does not mean that there is no cost to non-repayment (e.g. shame). It just reduces the cost.

So what do the results show?

Interesting additional results:

- The group process helps lenders lower their transaction costs (by consolidating and simplifying loan disbursal and collection logistics). See Banerjee's model.

Now look at the second experiment. Here new areas obtain randomly a branch which gives individual or group liability loans. This allows to capture the **total effect of joint liability**, including the selection effect, for which the previous experiment controlled.

Testing Models of Microcredit

Giné and Karlan, 2011

Table II B

Institutional Impact At the Loan Cycle Level, New Areas

All regressions use fixed effect for credit officers and months of maturity dates. Panel A reports on all loan cycles, Panel B uses the first loan while Panel C uses subsequent loans. Robust standard errors clustered by lending centers in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Proportion of missed weeks	Indicator for having at least one missed week	Proportion of past due balance, at maturity date	Indicator for having past due, at maturity date	Proportion of past due balance, 30 days after maturity date	Indicator for having past due, 30 days after maturity date	Loan size
Panel A: All cycles							
Individual liability	-0.004	0.002	-0.005	-0.018	-0.002	-0.018	-139.556
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.014)	(177.596)
Phased-in individual liability	-0.001	0.067	-0.004	-0.010	-0.004	-0.015	-237.521
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.013)	(179.535)
Number of observations	4869	4869	4869	4869	4704	4704	5356
R squared	0.151	0.227	0.115	0.138	0.123	0.187	0.138
Mean of dependent variable	0.098	0.493	0.023	0.122	0.014	0.068	4390.067
Panel B: All cycles, controlling for baseline loan size							
Individual liability	-0.002	0.003	-0.005	-0.017	-0.002	-0.017	35.678
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.014)	(128.479)
Phased-in individual liability	0.001	0.068	-0.004	-0.010	-0.003	-0.014	31.713
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.013)	(140.579)
Number of observations	4869	4869	4869	4869	4704	4704	5356
R squared	0.153	0.227	0.115	0.138	0.123	0.187	0.470
Mean of dependent variable	0.098	0.493	0.023	0.122	0.014	0.068	4390.067
Panel C: First cycle only							
Individual liability	-0.002	0.023	0.002	-0.013	0.002	-0.011	-139.239
	(0.015)	(0.053)	(0.007)	(0.035)	(0.005)	(0.016)	(144.602)
Phased-in individual liability	0.002	-0.005	-0.003	-0.011	-0.004	-0.009	-232.650**
	(0.017)	(0.062)	(0.006)	(0.030)	(0.005)	(0.014)	(110.370)
Number of observations	2137	2137	2137	2137	2112	2112	2207
R squared	0.274	0.332	0.258	0.211	0.254	0.258	0.236
Mean of dependent variable	0.086	0.420	0.024	0.125	0.015	0.072	3685.998

No statistically or economically significant difference in repayment rates across any of the three groups.

Testing Models of Microcredit

Giné and Karlan, 2011

Table II B

Institutional Impact At the Loan Cycle Level, New Areas

All regressions use fixed effect for credit officers and months of maturity dates. Panel A reports on all loan cycles, Panel B uses the first loan while Panel C uses subsequent loans. Robust standard errors clustered by lending centers in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Proportion of missed weeks	Indicator for having at least one missed week	Proportion of past due balance, at maturity date	Indicator for having past due, at maturity date	Proportion of past due balance, 30 days after maturity date	Indicator for having past due, 30 days after maturity date	Loan size
Panel A: All cycles							
Individual liability	-0.004	0.002	-0.005	-0.018	-0.002	-0.018	-139.556
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.014)	(177.596)
Phased-in individual liability	-0.001	0.067	-0.004	-0.010	-0.004	-0.015	-237.521
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.013)	(179.535)
Number of observations	4869	4869	4869	4869	4704	4704	5356
R squared	0.151	0.227	0.115	0.138	0.123	0.187	0.138
Mean of dependent variable	0.098	0.493	0.023	0.122	0.014	0.068	4390.067
Panel B: All cycles, controlling for baseline loan size							
Individual liability	-0.002	0.003	-0.005	-0.017	-0.002	-0.017	35.678
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.014)	(128.479)
Phased-in individual liability	0.001	0.068	-0.004	-0.010	-0.003	-0.014	31.713
	(0.016)	(0.054)	(0.006)	(0.026)	(0.004)	(0.013)	(140.579)
Number of observations	4869	4869	4869	4869	4704	4704	5356
R squared	0.153	0.227	0.115	0.138	0.123	0.187	0.470
Mean of dependent variable	0.098	0.493	0.023	0.122	0.014	0.068	4390.067
Panel C: First cycle only							
Individual liability	-0.002	0.023	0.002	-0.013	0.002	-0.011	-139.239
	(0.015)	(0.053)	(0.007)	(0.035)	(0.005)	(0.016)	(144.602)
Phased-in individual liability	0.002	-0.005	-0.003	-0.011	-0.004	-0.009	-232.650**
	(0.017)	(0.062)	(0.006)	(0.030)	(0.005)	(0.014)	(110.370)
Number of observations	2137	2137	2137	2137	2112	2112	2207
R squared	0.274	0.332	0.258	0.211	0.254	0.258	0.236
Mean of dependent variable	0.086	0.420	0.024	0.125	0.015	0.072	3685.998

Credit officers less likely to create groups under individual liability. A bit puzzling...

Interesting additional results:

- Repeat loans under individual liability have a lower probability of defaulting by 3 percentage points. (Though this is the only significant result out of six measures of default, and two sample frames.)
- Lenders spend more time on enforcing repayment in the individual loan group.
- Interaction between demand and the competitive setting:
  - In barangays in which competitors are group lenders, baseline MFI clients are more likely to borrow from them after their group is switched to individual liability.
  - On the other hand, when the competition only offers individual liability, likelihood that baseline clients seek a loan is reduced (although not robustly significant).

Food for Thought

1. We focused on the theoretical and empirical effects of **joint liability** and **self-selection of borrowing groups** on repayment rates (and very occasionally welfare). This is restrictive in two ways:
   (a) It only captures a subset of potential effects of joint liability and self-selection. Maybe being jointly liable for some time fosters social interactions?
   (b) There are many other aspects to first generation micro finance loans. For example: regular meetings, regular repayment schedules, loans are given mainly to females, and they have dynamic incentive schemes. What is their effect?

2. Questions other than the *effects of microfinance* are important. How is it best introduced, what are the effects of for-profit status of MFIs, what is the effect of the competitive environment?

Food for Thought

Feigenberg, Field, Pande. 2011. "The Economic Returns to Social Interaction: Experimental Evidence from Microfinance"

Feigenberg, Field, Pande (2011) randomly change the **meeting frequency** of microfinance groups. They show that more frequent meeting is associated with long-run increases in social interaction and lower default.

"Experimental and survey evidence suggests that the decline is driven by improvements in informal risk-sharing that result from more frequent social interaction outside of meetings. These findings constitute the first experimental evidence on the economic returns to social interaction, and provide evidence on an alternative theory for the success of the classic group lending model in reducing default risk."

Field, Pande, Papp, Rigol (2011) examines how **repayment structure** of a debt contract influences entrepreneurship. They randomly assign contracts which require repayment to begin immediately after loan disbursement and contracts that include a *two-month grace-period*.

Having a grace-period increased short-run business investments and long-run profits. "Alongside, variance of profits and default rates increase. [This suggests] that liquidity constraints imposed by debt structure inhibit investment in high-return but illiquid investment opportunities. Debt contracts that require early repayment discourage risky investments but limit the potential impact of microfinance on entrepreneurship and household poverty."

Food for Thought

Fischer, Greg. 2010. "Contract Structure, Risk Sharing, and Investment Choice"

Fischer (2010) argues that joint liability in micro-finance contracts has **two effects for risk taking**:

(i) It increases risk-taking, since joint liability partially insures against default.

(ii) Providing this insurance for others reduces incentives for risk-taking.

He tests these (and other) predictions in a lab experiment: When partners have full information about the others' projects, and approval rights, risk taking falls below autarchy levels. Hence the structure of existing micro-finance contracts may discourage risky but high-expected return investments.

Food for Thought

Banerjee, Chandrasekhar, Duflo, Jackson, 2011

They investigate how participation in a micro-finance program **diffuses through social networks**. They collected demographic and social network data before micro-finance was introduced and tracked eventual participation.

Reduced Form: Micro-finance participation is higher when the injection points (first informed about the program) have higher eigenvector centrality (more important in network sense).

Structural Estimation: They estimate structural models of diffusion. Participants are more likely to pass information to friends than informed non-participants. (But non-participants contribute to passing information, too). Conditional on being informed, own participation is not significantly affected by participation of friends.

Table of Contents

1 Empirical Evidence: Credit
- Impact of Credit
- Digression: Testing Models of Credit

2 Empirical Evidence: Microcredit
- Impact of Microcredit
- Testing Models of Microcredit
- Food for Thought: Further Results

3 Summary and Outlook

Summary

Tomorrow