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Lecture 3

Poor Area Development Programs

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Poor-area development programs

- Public investment and/or private credit is targeted to poor areas with the aim of assuring a sustained reduction in poverty
- *Does this work in practice?* Maybe the problem of lagging poor areas is not lack of capital but low productivity of capital
 - Due to poor natural/geographic conditions
 - Or poor governance

Knowledge about long-term impacts

- There have been some evaluations of short-term impacts, but none on longer-term impacts.
- We know very little about the long-term effectiveness of a large class of development projects.
- Why? High cost; knowledge externalities (myopia).
- Methodological problems: Selection bias etc + serious concerns about spillover effects (violations of stable unit treatment value assumption)

Confounding spillover effects via local government responses to external aid

- Theoretical model: preferences over spending in project villages, non-project villages and other spending. Local spending constraint.
- Result: The external aid will displace local government spending in the project villages, increase spending in the comparison villages, but decrease total local government spending across both sets of villages.
- Standard method (DD) will underestimate long-term impact.

Does community participation make these programs more effective?

- New wave of participatory, community-based, poor-area development projects.
 - Emphasis on local beneficiary selection and local choice of how to spend the money.
- However, heterogeneity in impacts is plausible:
 - Some are credit constrained, some are not;
 - Some have complementary skills, some do not.
- Will the community-based selection process pick the “winners”?
 - Bias toward egalitarian allocations?
 - Potential trade-off with achieving maximum impact, including on poverty.

A case study: Evaluating a poor-area development program in China

Reading: Chen, Shaohua, Ren Mu and Martin Ravallion, 2009, “Are There Lasting Impacts of Aid to Poor Areas?” *Journal of Public Economics*, Vol. 93, pp. 512-528.

China: overall success against chronic poverty, but lagging areas

- Large reduction in absolute poverty over 1980-2000.
 - 53% poor in 1981; 8% in 2001 (Chen and Ravallion, 2007)
- But wide geographic disparities have emerged, notably between the coast and remote resource-poor inland.
- Southwest China: [Map=>](#)
 - Guangxi, Guizhou and Yunan: one of the poorest regions.
 - Population of 120 million and area of 800,000 square kilometers.



小湖村扶贫开发剪影

Are poor-area programs the answer?



当干部就要带头



大搞环境卫生整治



村民踊跃投工投劳



村容村貌变迁

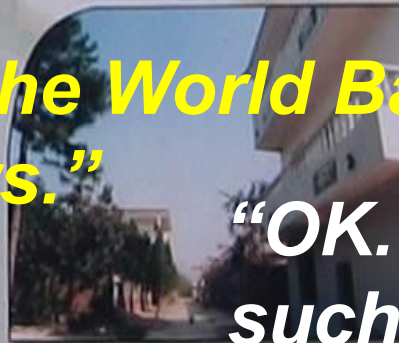


71岁的老人刘福林

"So, did we have any lasting impact?"

"I really don't know."

"Let's ask the World Bank's Researchers."



农民新居



调整产业结构

"OK." (Thinks: Is that such a good idea?)

China's poor-area programs

- Village and small-holder investment programs have been the Government of China's main direct intervention for fighting poverty.
- Government's emphasis on agriculture makes sense given that this is a major income component and generator of positive externalities.
- Adequate (human and physical) infrastructure is a pre-condition for growth in poor areas.
- Selected counties tend to be poorer
 - Evidence from probits based on RHS sample data
 - Evidence from the Yunnan poverty map

The Southwest Poverty Reduction Project

- Substantial aid-financed expansion of efforts to fight poverty in lagging poor villages of Southwest China.
- Rural development program targeted to poor areas.
- Aims to reduce poverty by providing:
 - resources to poor farm-households and
 - social services and rural infrastructure.
- 35 national poor counties
- \$US 460 million from a World Bank loan and Chinese government over 1995-2001.

The key components of SWP

1. Income-generating activities: methods for raising grain yields, animal husbandry, reforestation.
2. Off-farm employment: voluntary labor mobility and support for township-village enterprises.
3. Social services and infrastructure: tuition subsidy to children from poor families, upgrading village schools and health clinics, rural roads, safe drinking water supply system etc.
4. Institution building and poverty monitoring:
 - improving the management of the project and
 - establishing a poverty monitoring system.

Composition of SWP spending

	% of total investment
Education	8.6
Health	5.4
Labor mobility	9.7
Rural infrastructure	17.2
Agriculture	43.1
Rural enterprise development	11.5
Institution building	1.7
Project & poverty monitoring	2.8
Total	100.0

What is different about the SWP? (compared to the govt.'s programs)

1. Greater integration across sectors, esp., agriculture/(physical) infrastructure + human resource development.
2. Greater community/farmer participation in deciding what is done
3. Greater resources.
4. External donor funding (M&E).

Selection of participants

- Village selection: Poor villages in selected poor counties, picked by readily verifiable objective criteria, but not formulaic
 - All households in selected villages were expected to benefit from the local public goods
- Household selection: 60% also got individual loans, selected by pre-existing community groups (“farmers committee”)
 - Less transparent credit-worthiness criteria and successful past experience with such project activities played a role.

Uninsured risk remains

- In common with other development projects, the SWP provided the capital and technical assistance, but it did not provide insurance
 - And many of the project activities are likely to entail non-negligible income risk. The income gains will depend on a number of contingencies:
 - the vagaries of the weather (given the role of agriculture)
 - uncertain demand for the new products
 - risks associated with out migration.
- => Precautionary saving

Evaluation issues

Impact evaluation: the counterfactual

- Recall: Impact is the difference between the relevant outcome indicator with the program and that under the counterfactual.
- Here the counterfactual includes pre-existing governmental programs; we can only identify the incremental impact of SWP.
- So we must also consider the responses of the local political economy to the external aid.

Impact evaluation: specific issues

- Time span of evaluation
 - Development projects may need longer evaluation periods than found in practice
 - Expensive longer tracking *or* more and better data?
 - How well do rapid appraisal methods work?
- Measurement of welfare impacts
 - Poor people are not myopic
 - Consumption may better reveal long-term impact
 - However, there may be great uncertainty about impact on permanent income
 - Lags in impacts on living standards cloud identification

Evaluation strategy:

Matching/weighting + diff-in-diff

- Propensity score matching/weighting:
 - Matching/weighting on observed initial characteristics likely to jointly influence poor-area targeting and how incomes evolve over time.
- Difference-in-difference:
 - Difference in gains over time between participants and non-participants.
 - This eliminates any time-invariant bias due to miss-matching, selection bias, omitted variables etc

Outcome measure for treatment group:

$$Y_{it}^T = \underset{\substack{\uparrow \\ \text{counterfactual}}}{Y_{it}^C} + \underset{\substack{\uparrow \\ \text{impact ("gain")}}}{G_{it}} \quad (i=1,n; t=0,1)$$

Selection bias: $B_t = E(Y_{it}^C | T_{i1} = 1) - E(Y_{it}^C | T_{i1} = 0)$

Diff-in-diff: $DD = E[\Delta(Y_{it}^T - Y_{it}^C)] = G_{i1}$

if (i) time-invariant selection bias (change over time for comparison group reveals counterfactual)

$$(B_1 = B_0)$$

and

(ii) baseline is uncontaminated by the program

$$(G_{i0} = 0)$$

Sources of time-varying selection bias in diff-in-diff estimators

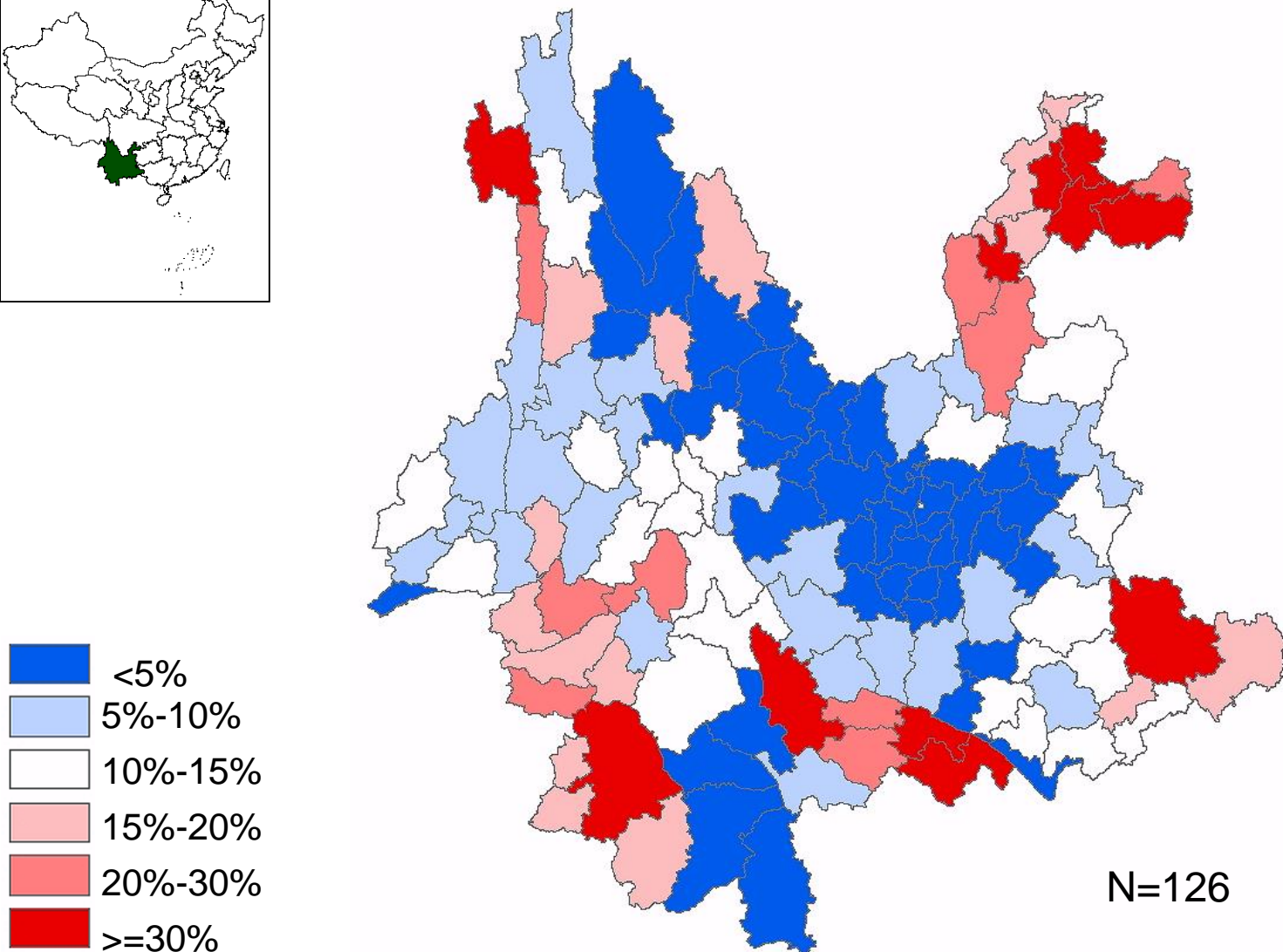
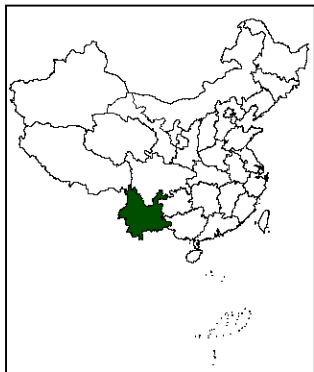
1. Interference between treatment and comparison groups.

- Both come from poor counties. (To assess impact on top of national/provincial programs.)
- However, there could be interference through local funding choices. Displacement.

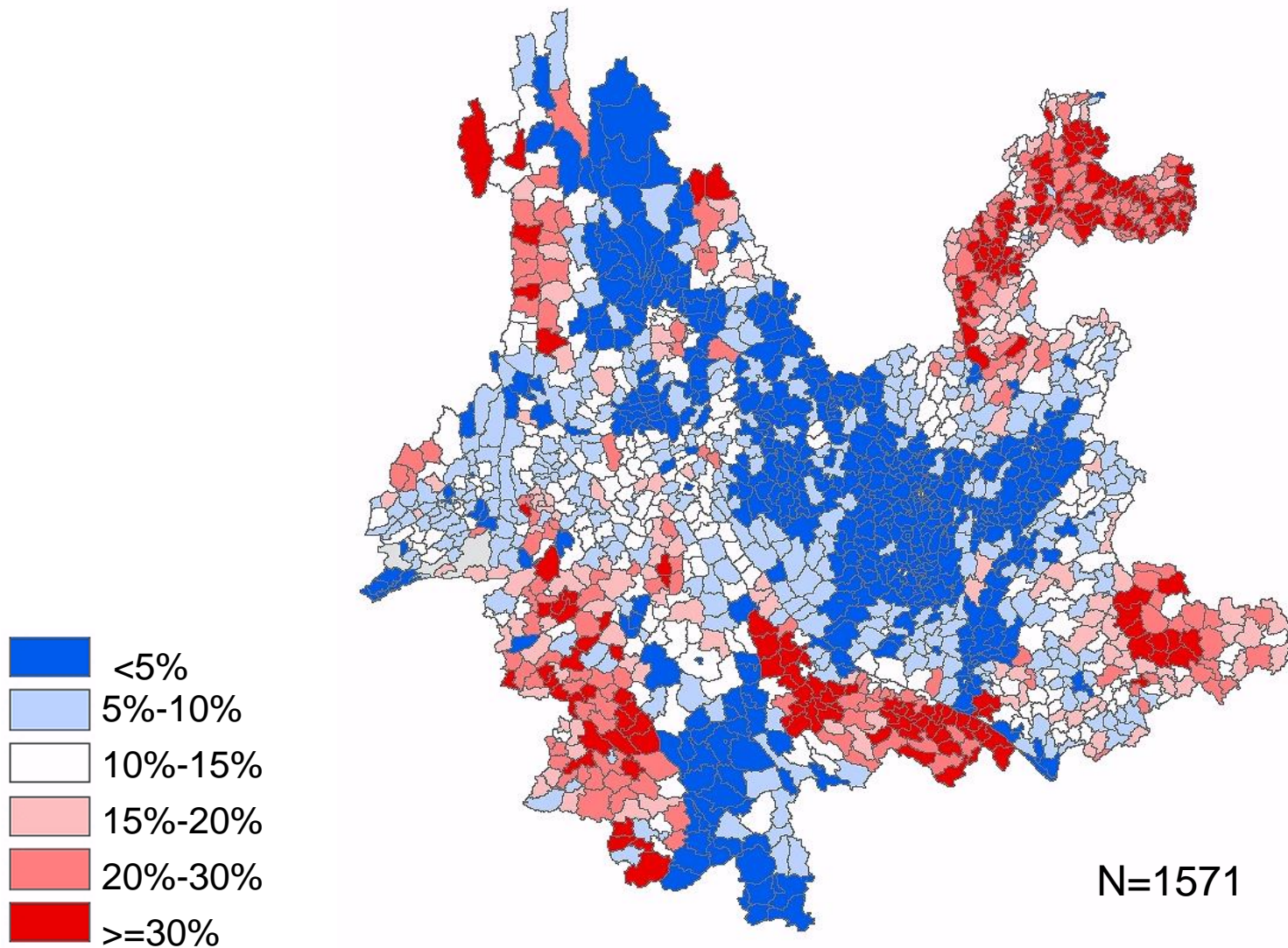
2. Convergent or divergent growth processes:

- If subsequent outcome changes are a function of initial conditions that influence program assignment.
- This is known to be a serious concern (Jalan and Ravallion, 1998; same region of rural China).
- Heterogeneity within poor counties; [Yunnan poverty map=>](#)

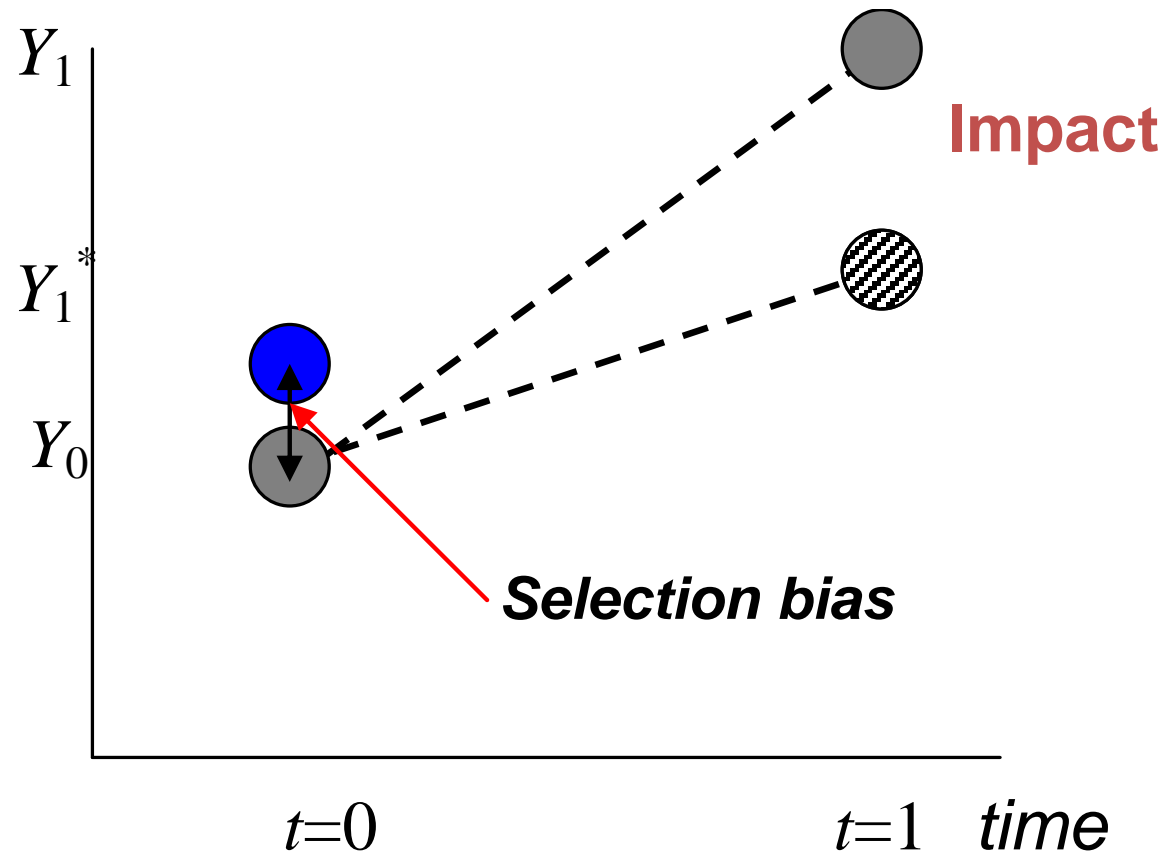
Yunnan: County poverty incidence



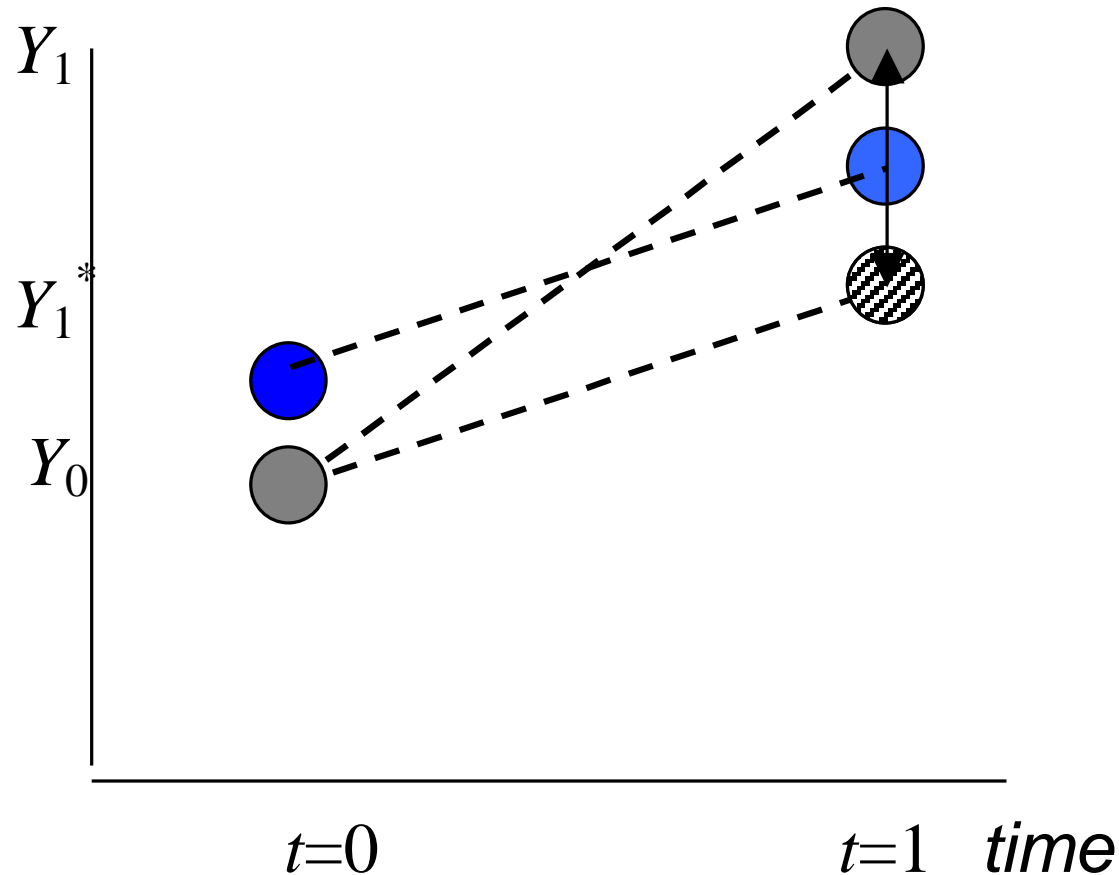
Township poverty incidence



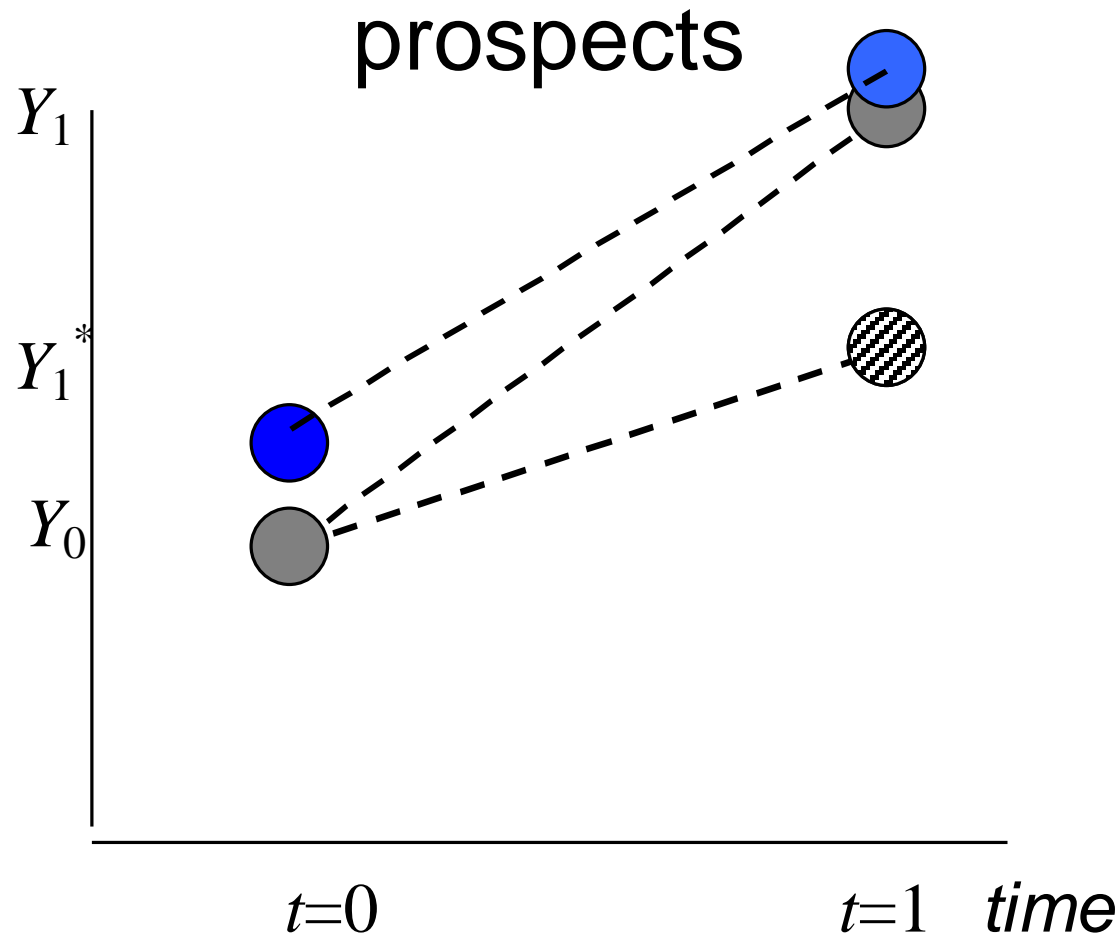
Given purposive targeting, we can expect selection bias



As long as the bias is additive and time-invariant, diff-in-diff will work



But diff-in-diff hides true impact when targeted areas have lower growth



Targeted poor counties in China may have lower growth rates in the absence of intervention (divergence)

Propensity-score weighting

- Hirano, Imbens and Ridder (2003): weighting the control observations according to their propensity score yields a fully efficient estimator.
- Regression implementation:

$$Y_{it} = \alpha + DD.T_{i1}t + \beta T_{i1} + \delta_t + \varepsilon_{it}$$

with weights of unity for the treated units and

$\hat{P}(X)/(1 - \hat{P}(X))$ for the controls where $P(X_i) = \Pr(T_i = 1|X_i)$ is the propensity score.

Further reading: Hirano, Keisuke, Guido Imbens, and Geert Ridder, 2003, "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score," *Econometrica* Vol. 71(4): 1161-1189.

Equivalence to fixed effects model

- Fixed effects model on balanced panel:

$$Y_{it} = \alpha^* + DD.T_{i1}t + \delta_t + \eta_i + \nu_{it}$$

where

$$\eta_i = \eta_i^T T_{i1} + \eta_i^C (1 - T_{i1}) = \beta T_{i1} + \bar{\eta}^C + \mu_i$$

$$\beta = \bar{\eta}^T - \bar{\eta}^C$$

- Thus βT_{i1} picks up any differences in time-mean latent factors.
- However, our specification does not require balanced panel and is thus robust to selective attrition.

Data

NBS Rural Household Survey

- Good quality budget and income survey (care in reducing both sampling and non-sampling errors).
- Sampled households maintain a daily record on all transactions + log books on production.
- Local interviewing assistants (resident in the sampled village, or nearby) visit each household at roughly two weekly intervals.
- Inconsistencies found at the local NBS office are checked with the respondents.
- Sample frame: all registered agricultural h'holds.

Consumption aggregates

- Measure of consumption based on the RHS method includes all “living expenditures:”
 - cash spending on food, clothing, housing, rents, utilities, durable consumer goods, services etc
 - imputed values of all in-kind spending
- Excludes:
 - “transfer expenditure” (remittances out, insurance, transaction costs on loans/land-use changes)
 - production costs, investments

Income aggregates

- The income variable includes
 - cash income from all sources
 - imputed values for in-kind income from various sources (household production which includes farming, forestry, animal husbandry, handicrafts etc.)
 - income received as a gift

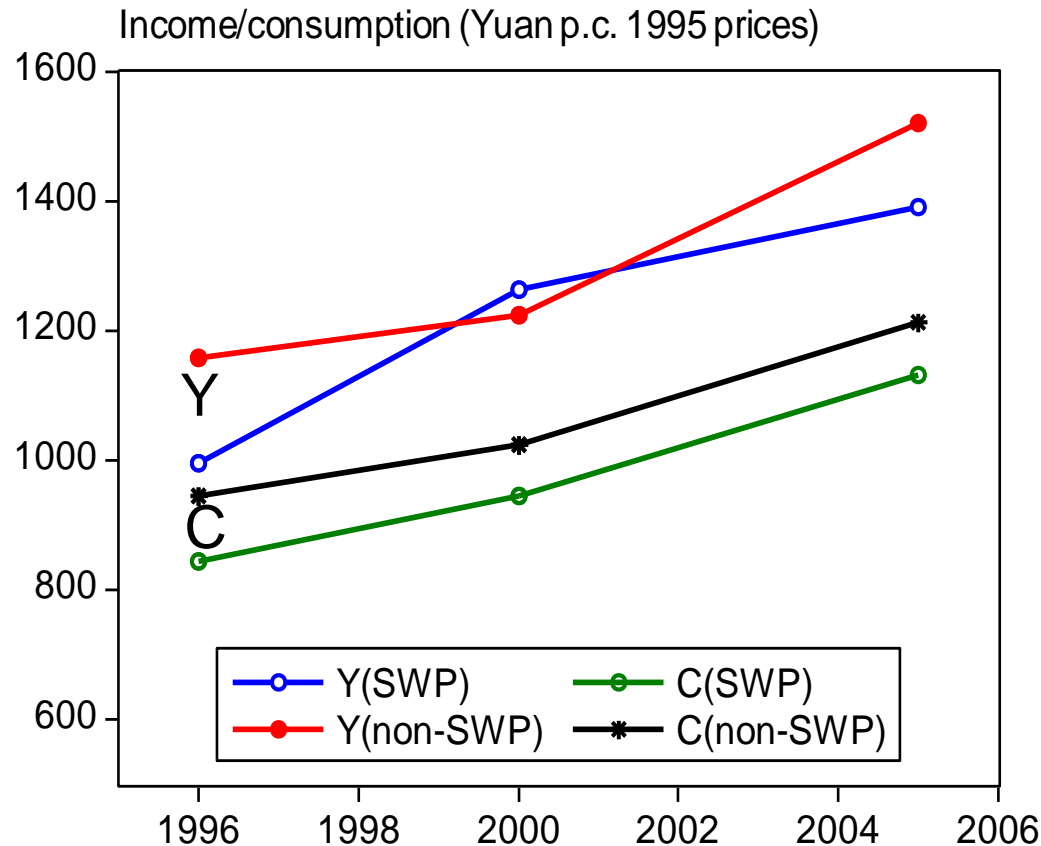
SWP survey data

- Based on RHS
- Community, household and individual data
- Time period: 1995-2001 (annual) + 2004
- 112 Project villages + 86 comparison villages
- Problem with baseline survey; 1996 instead
- Attrition bias (probit test)
- But replacement households look OK (probit for replacement in pooled sample).

Descriptive statistics

By the end of the disbursement period, SWP villages had caught up in mean income, but not consumption
=> the project's income gains were saved.

Project villages started worse off on average than non-project villages.

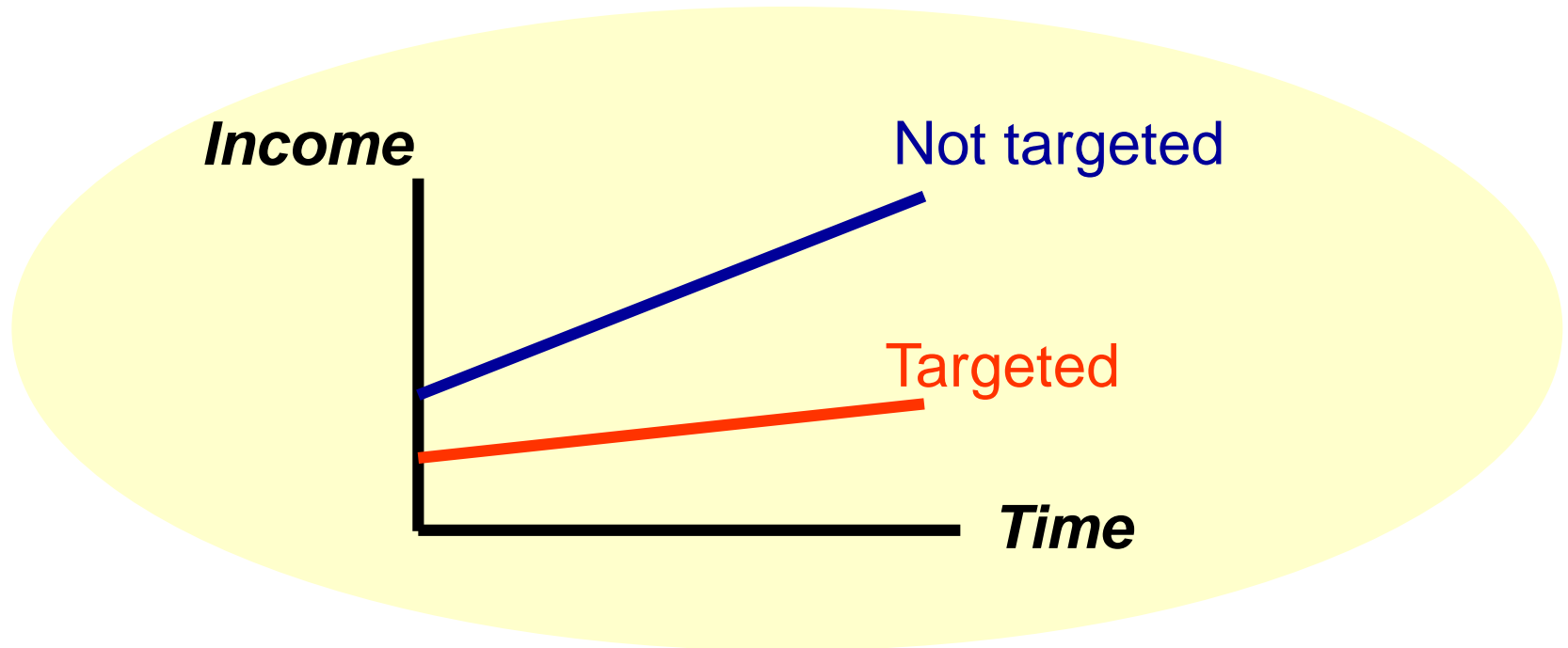


SWP villages had not caught up by 2004

Descriptive statistics...

- ...suggest that the project had little or no lasting impact.
- However, we need to allow for selection bias arising from the program's purposive targeting and displacement of non-SWP spending.

Time-varying selection bias: Areas not targeted = biased counter-factual



The growth process in non-treatment areas is not indicative of what would have happened in the targeted areas without the program

Correcting for selection bias

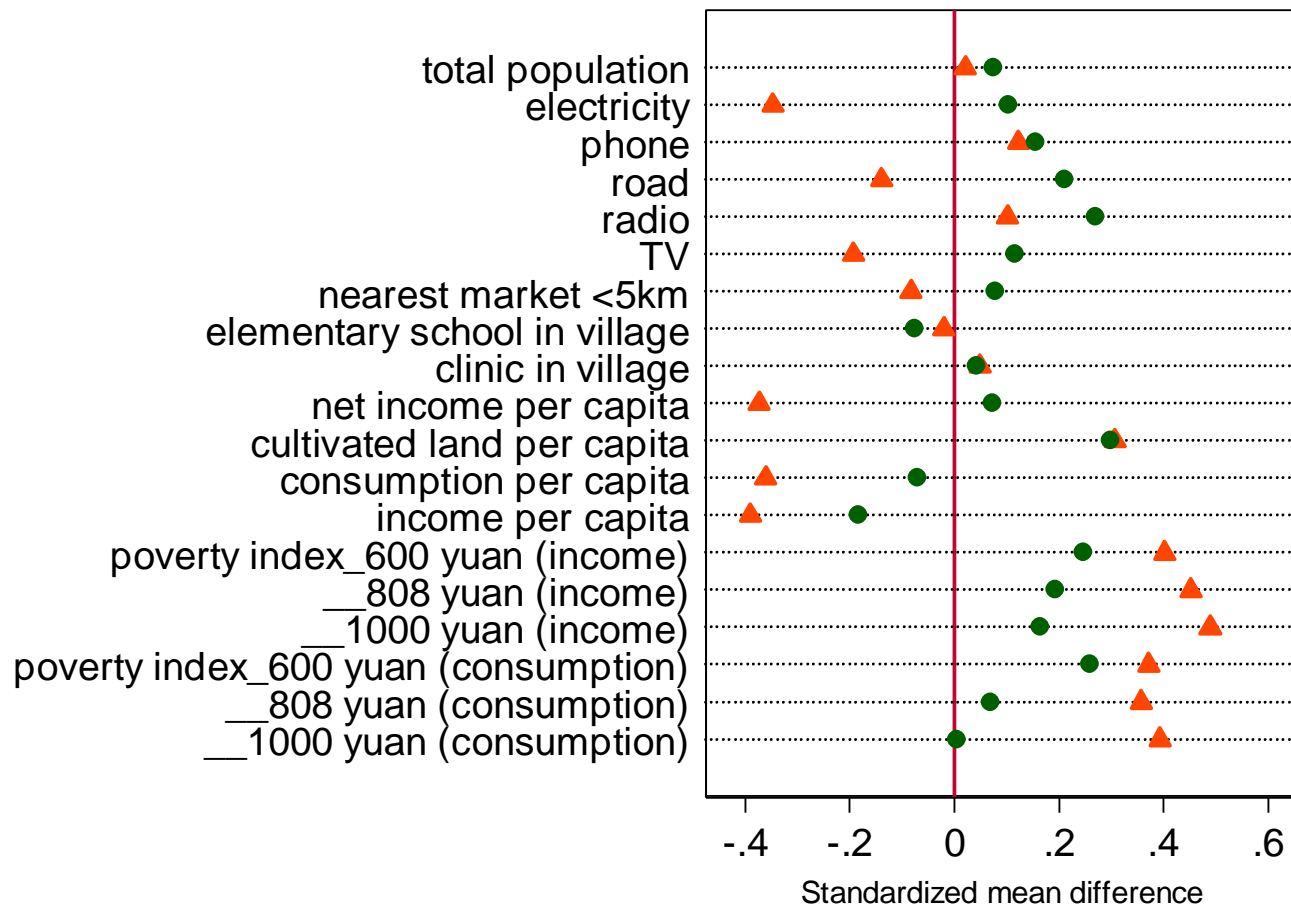
Covariates of participation

- SWP villages tend to be:
 - in more mountainous remote areas,
 - less likely to have electricity,
 - less likely to have a school in the village,
 - more likely to have a health clinic.
- The project villages tend to have:
 - higher populations,
 - lower mean income and
 - more land per capita, reflecting lower pop. density.
- Consistent with targeting poor villages within poor counties

Balancing the placement covariates

- Standardized differences between the SWP villages and matched or weighted non-SWP villages in terms of means of each covariate (Abadie and Imbens, 2006).
- Good balance between village covariates once re-weighted.
- Also, balancing tests for the 1996 outcome measures based on the household survey data for that year.
- Even though these were not used as covariates in estimating the propensity scores, very good balance is achieved.

Balancing test on selected variables



Note: Standardized mean = (sub-group mean *minus* mean for full sample)/standard deviation for full sample

Impacts?

Impacts on mean income and consumption

2000:

- sizeable and statistically significant impacts on income, but not consumption;
- the bulk of the income gain during the project's disbursement period was saved

2004:

- much lower impacts on incomes and not statistically significant.
- Some signs of consumption gains, but with wide confidence interval, which includes zero!

Simple diff-in-diff estimate of the impact of SWP on household income and consumption

		1996 mean in SWP villages	Gain in SWP project	Gain in non-SWP villages	Diff-in- Diff	t-ratio
2000	income	971.4	206.8	66.0	140.8	2.02
	consumption	841.7	67.1	70.5	-3.4	-0.07
	saving	129.7	139.7	-4.5	144.2	2.33
2004	income	971.4	442.8	387.4	55.4	0.63
	consumption	841.7	345.9	287.7	58.3	0.87
	saving	129.7	96.8	99.7	-2.8	-0.03

Note: Trimmed sample

But (again) these results could just reflect underlying divergence in the growth process.

Impacts with corrections for selection bias

		PS weighted		Kernel matched	
		DD	t-ratio	DD	t-ratio
Trimmed sample					
2000	Income	193.162	2.751	179.657	2.512
	Consumption	-17.662	-0.313	-45.762	-0.751
	Saving	210.84	2.921	225.437	2.757
2004	Income	53.482	0.574	52.741	0.694
	Consumption	58.535	0.786	18.312	0.223
	Saving	-5.037	-0.059	34.448	0.415
Total sample					
2000	Income	213.605	3.287	199.342	3.426
	Consumption	-151.054	-1.180	-189.569	-1.427
	Saving	364.696	3.371	388.953	3.494
2004	Income	-47.159	-0.423	-38.636	-0.304
	Consumption	36.752	0.633	25.893	0.439
	Saving	-83.874	-0.705	-64.486	-0.515

Simple *DD* results are robust to our corrections
for selection on observables

Impacts on income & consumption by source?

- Significant impacts on income in-kind, in both short-term and longer term.
 - Mainly from animal husbandry.
 - Small non-productive animals and new litters of productive animals, to be held over for later consumption or sale.
- Farming and AH account for all the long-run impact.
- No sign of impacts on cash income or expenditure.
- But significant longer-term impacts on consumption in-kind.
 - Possibly consumption of the earlier income in-kind.

Impacts on productivity, assets, prices?

Agricultural productivity?

- Crop-specific farm outputs per unit cultivated area and total farm income per unit area.
- No evidence of impacts on productivity.

Interest payments or loan repayment? No

Taxes/transfers? No

Productive assets and wealth (including housing)?

- Significant livestock gains (cows)
- Otherwise, little sign of impacts in either the disbursement period or the longer-term.

Prices?

- Little impact on prices of agricultural outputs and purchase prices for inputs.

Impacts on schooling, demographics, migration?

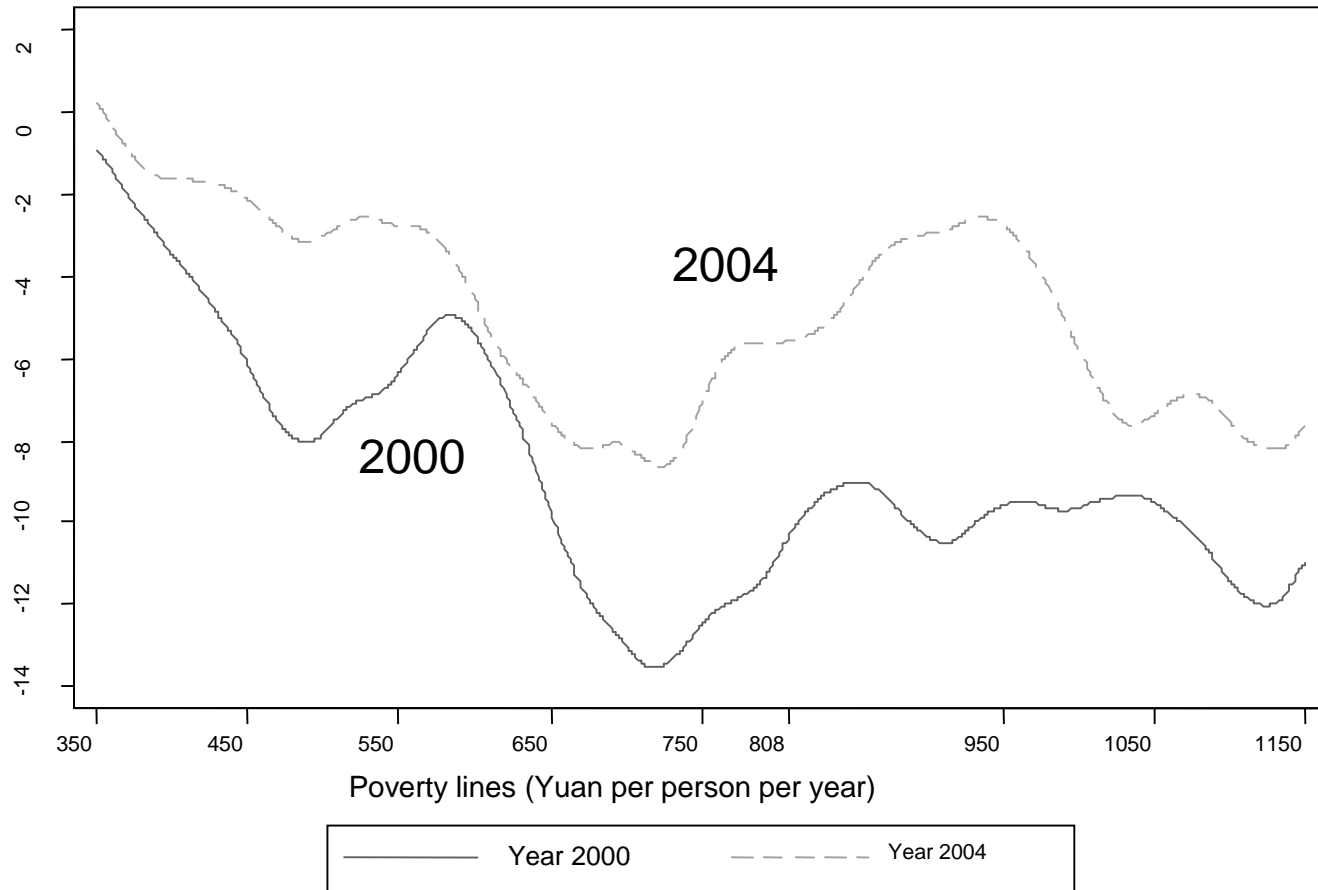
- Significant impacts on school enrolment rates during the disbursement period, with the tuition subsidies
 - PS-weighted DD estimate was 0.074 (with a t-ratio of 2.20), i.e., a 7.4% point increase in the school enrollment rate of children aged 6-11 by the year 2000 is attributed to SWP.
- However, this too had largely vanished by 2004 (when the tuition subsidy was over)
 - the corresponding DD estimate fell to 0.032 ($t=1.00$).
- Small short-term impact on household size (fewer kids), but not sustained in 2004.
- No impact on remittances received or probability of family member migrating out.

Impacts on poverty?

- Poverty impacts in the disbursement period are broadly consistent with impacts on the means.
- Non-negligible but statistically insignificant longer-term impacts on the poverty rate.
- Impacts on income poverty are greater in the disbursement period,
- while the impacts on consumption poverty tend to be greater in the longer time period =>

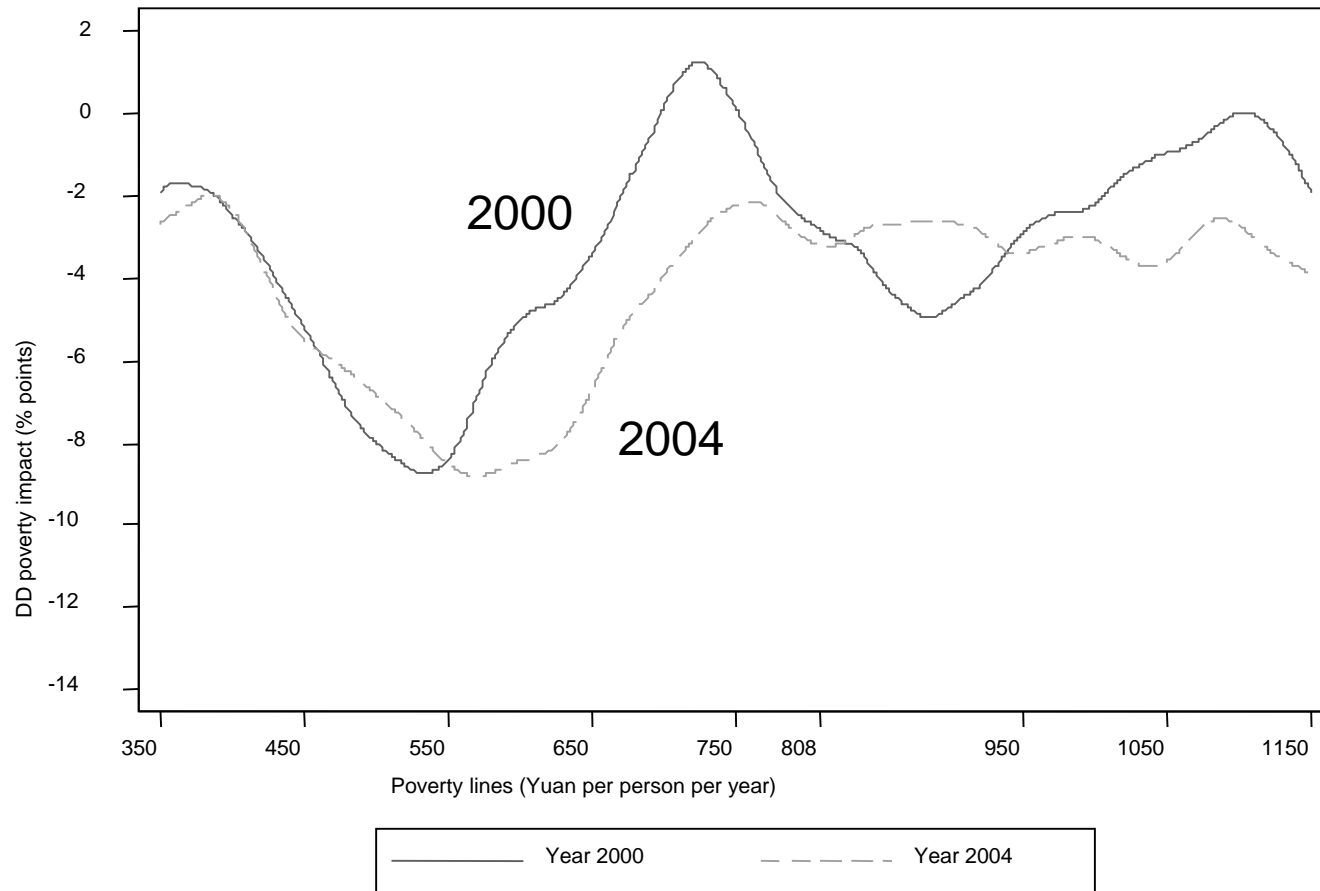
Income poverty

DD impact on headcount index



Consumption poverty

DD impact on headcount index



Heterogeneity 1: “Direct” vs. “indirect” participants

- Direct participants receive loan (60%+10% govt.).
- Simple DD => significantly larger income gains to direct participants, but only in disbursement period.
- Could be time-varying selection bias in choice of loan recipients.
- Score-weighted DD (based on loan recipient model) => no significant difference in impacts between direct and indirect participants
- Confounding omitted variables in selection of loan recipients (e.g., local networks/politics)

Heterogeneity 2: Interaction effects between schooling and income

- Tests for differences in impacts according to the initial income, education and ethnicity.
- When we interacted income with education we found that the longer-term gains were strongest for the relatively well educated amongst the low-income households.
- Also find evidence of significant longer-term impacts on assets and housing for this group.
- But no sign of farm productivity impacts for this group (though some impacts at higher-incomes).

Community-based selection greatly reduced overall impact

- The “poor but schooled” group had slightly higher SWP participation rate (61% vs. 59%); but still many were not covered.
 - If SWP had saturated this group, with no gains to others, then the long-run impact on mean income would have been four times higher (150 Yuan p.c. vs. 40 Yuan).
- =>The community-selection process put too little weight on the educated poor and so failed to assure maximum impact from the SWP.

Impacts on migration and remittances

- No impacts on average remittances and out-migration probabilities.
- However, significant positive impacts when we stratified by initial income and education
 - the gains in out-migration and remittances were significant for those who were initially above median income,
 - and were larger for those with more schooling (amongst those with above-median income).

Rapid appraisal? No impacts on perceived satisfaction with life

- Subjective assessments of whether living standards had improved since the project began are not significantly different to those found for the non-SWP villages.
- Adding the corrections for differences in baseline characteristics between the treatment and comparison villages does not change this finding,
- although the impacts on household and village living standards are now significant at roughly the 10% level.

Impacts on self-assessed satisfaction with life

	Single difference on total sample			PS weighted and trimmed sample		
	Mean in project villages	Diff. (project- comparison)	t-ratio	Mean in project villages	Diff. (project- comparison)	t-ratio
Overall standard of living of h'hold	0.357	0.001	0.018	0.343	0.108	1.635
Income	0.328	-0.005	-0.094	0.324	0.026	0.326
Food	0.377	0.017	0.334	0.356	0.073	1.050
Clothing	0.363	0.028	0.55	0.345	0.094	1.364
Housing	0.313	-0.045	-0.952	0.292	0.006	0.089
Asset accumulation	0.173	-0.009	-0.228	0.151	-0.079	-0.974
Agriculture skill	0.101	-0.026	-0.935	0.087	0.009	0.285
Credit availability	0.190	0.011	0.251	0.190	0.035	0.589
Affordability of primary/mid. school	0.22	0.007	0.163	0.209	0.067	1.265
Health	0.302	-0.035	-0.71	0.285	0.092	1.550
School infrastructure	0.392	-0.053	-0.928	0.382	0.039	0.497
Health infrastructure	0.240	-0.059	-1.217	0.219	0.006	0.090
Knowledge of village affairs	0.170	-0.010	-0.227	0.161	0.048	0.992
Participation in decision-making	0.174	-0.017	-0.413	0.157	0.026	0.536
Democracy	0.232	0.015	0.321	0.216	0.075	1.353
Overall village standard of living	0.345	0.034	0.626	0.325	0.116	1.761
Service to village by county govt.	0.200	0.017	0.369	0.157	0.009	0.133

Recall bias in rapid appraisal method

- Regressions of the perceived change in the household's standard of living on the change in log consumption per person, with controls for respondents' gender and age.
- Subjective recall over such a long period is evidently not reflecting well the changes in living standards as measured by consumption.
- The recall data put too high a weight on the current level of living and are affected by many idiosyncratic factors not accountable to consumption.

=>

By not adequately reflecting baseline outcomes, long recall has a hard time identifying impact.

Regressions for perceived change in overall standard of living

	(1)		(2)		Difference	
	SWP villages		Non-SWP villages			
	Coefficient	t-ratio	coefficient	t-ratio	(1)-(2)	t-ratio
Intercept	1.971	1.248	2.217	0.903	-0.246	-0.085
Change of log consumption 2005-1996 (β_0)	0.360	2.511	0.277	1.501	0.083	0.358
Log consumption in 1996 (β_1)	0.328	1.668	0.733	2.903	-0.405	-1.269
Gender of respondent	0.200	0.850	0.339	1.520	-0.138	-0.429
age of respondent	0.082	2.089	-0.009	-0.183	0.091	1.422
age ²	-0.001	-1.62	0.000	-0.080	-0.001	-0.887
R ²	0.037		0.047			
Test $H_0 : \beta_0 = \beta_1$ (p-value)	0.8712		0.0827			

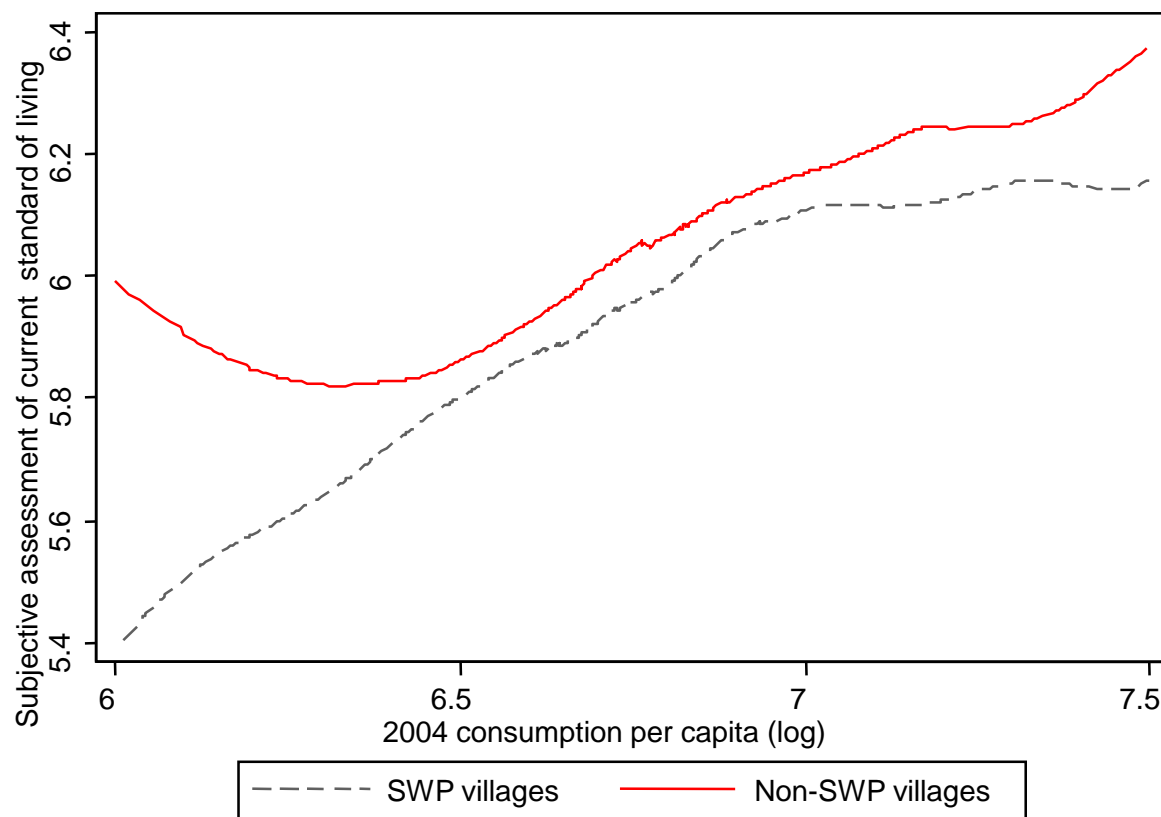
$$R_i = \alpha^T + \beta_1^T \Delta Y_i + \beta_0^T Y_{i0} + \gamma^T X_i + \varepsilon_i^T \text{ for } i \in T$$

$$R_i = \alpha^C + \beta_1^C \Delta Y_i + \beta_0^C Y_{i0} + \gamma^C X_i + \varepsilon_i^C \text{ for } i \in C$$

Impacts on non-income dimensions of self-assessed welfare? **No**

- Subjective module also included questions about perceptions of current welfare and living conditions.
- Test: regressing each subjective measure on log consumption per capita in 2004/05, a dummy variable for SWP villages and the interaction effect between these two variables.
- Almost no sign that the relationship is any different.
Figure=>
- Only exception: road quality.
 - SWP enhanced perceived road quality for better-off households?
 - We are more inclined to the view that this one significant result in 30 tests is purely by chance.

Impacts on self-rated welfare? **No**



Impacts on credit availability?

Yes, for direct participants

- No sign that overall access to credit improved more in SWP villages.
- However, access to credit improved for direct participants
 - significantly higher responses on both current credit availability and the improvement over last 10 years.
 - though attenuated by our corrections for selection bias

Spillover effects?

Bias due to mobility and/or trade? No

- Mobility effects? Unlikely due to land laws/customs.
- Trade effects? Possible, but we find no signs of impacts on cash income, in either short term or long term.
- Yet most inter-village trade transactions are in cash.
- Nor do we find impacts on prices, in either short-term or long-term.

Bias due to displacement effects? Yes

- Quantitatively large displacement effects for some non-SWP activities.
 - The effects are particularly strong for farming, animal husbandry, forestry, student tuition subsidies, new school construction, and infrastructure projects.
- For a number of items the mean in SWP villages is < half non-SWP villages, implying that 40%+ of the non-SWP spending allocation to SWP villages was cut, and re-allocated to non-SWP villages.
- Such large displacement effects would imply that the benefits of the SWP are likely to have spilled over to our comparison villages, leading us to under-estimate the impacts of SWP.

Testing for displacement of non-SWP development projects in SWP villages

	Mean in SWP villages	Mean in non-SWP villages	Difference	t-ratio	PS weighted diff.	t-ratio
Farming projects	0.79	2.11	-1.32	-2.45	-1.68	-2.09
Animal husbandry	1.51	3.03	-1.52	-2.08	-2.21	-1.98
Forestry projects	0.54	1.34	-0.79	-2.50	-1.50	-1.84
Terracing	0.12	0.65	-0.53	-2.08	-0.94	-1.58
Drinking water	0.31	0.90	-0.59	-3.04	-0.86	-2.58
Irrigation	0.24	0.60	-0.36	-1.80	-0.30	-1.42
Electricity	0.28	0.58	-0.30	-2.21	-0.49	-2.01
Roads	0.19	0.39	-0.20	-1.89	-0.24	-1.39
Student subsidies.	0.82	2.35	-1.53	-3.03	-1.74	-2.79
New schools	0.35	0.79	-0.44	-2.10	-0.55	-1.96
Teacher training	0.07	0.37	-0.30	-1.87	-0.39	-2.25
Health insurance	0.16	0.31	-0.14	-2.05	-0.12	-1.26
New clinic	0.06	0.24	-0.18	-3.84	-0.12	-1.85
Doctor training	0.07	0.26	-0.18	-1.62	-0.18	-1.56
Total no. projects	6.07	14.81	-8.73	-3.25	-11.72	-2.45
Total no. households	415.38	1026.10	-610.71	-1.74	-399.02	-2.22

Note: Based on counts of the number of new non-SWP projects of each type that started between 1996 and 2001 (inclusive). So this is the change in the number of non-SWP projects during the period. Interpretable as DD.

How much bias in mean income impact?

- Let

- $DD^* = DD + SPILL$ be true impact, where $SPILL$ = income gain to the comparison group due to the spillover effect.
- G_{SW} (G_{NSW}) = local government's investment per capita SWP (non-SWP) villages; $G = wG_{SW} + (1 - w)G_{NSW}$
- r_{SW} (r_{NSW}) = corresponding rates of return
- $A = w.A_{SW}$ is external aid per capita

- Assume displacement within poor counties and G is same in the absence of the SWP.

$$\Rightarrow \frac{DD}{DD^*} = 1 - \delta \frac{r_{NSW}G}{r_{SW}A} \text{ where } \delta = \frac{w(1-k)}{1-w(1-k)}; k = G_{SW} / G_{NSW}$$

- The bias is small if either:

- the project is small ($w=0$); or
- the displacement is small ($k=1$) or
- rate of return to displaced activities is small ($r_{NSW} = 0$)

Small bias due to spillover effects

- Possibly somewhat lower rates of return to the government's poor area programs, but for the present purpose it is probably reasonable to assume that $r_{SW}=r_{NSW}$
- One quarter of villages in the poor countries participate in SWP, so let $w=0.25$.
- Based on the test results we can take $k=1/3$ to be a reasonable lower-bound.
- The level of investment per capita under the non-SWP projects is about half that under SWP.

Then $DD^*/DD=1.10.$, i.e., 10% bias.

Upper bound at full displacement: $DD^*/DD=1.20$

What have we learnt?

What have we learnt?

- An evaluation that focused solely on the income gains during the disbursement period would clearly give a very deceptive picture of this project's true impacts.
 - Large and significant impact on mean household income in the participating villages during the project's disbursement period.
 - Much smaller impact on consumption during that period; the project's short-term income gains were largely saved.
- Four years after disbursements ended, both project and non-project villages had seen sizeable economic gains, with only modest net gain to mean income in the project villages.
- Non-negligible impacts on poverty in the longer-term, with 5-10 percentage point reductions in the incidence of poverty attributed to the project.

Why the high savings rate?

- When interpreted in terms of the simplest **Permanent Income Hypothesis** (PIH), our results imply that participants felt that a large share of the income gains was likely to be transient.
- Under simple PIH, the consumption impact of the project identifies the impact on permanent income.

Program impacts under the PIH

Decompose income gain into permanent and

transient components: $G_{it}^Y = G_{it}^{YP} + G_{it}^{YT}$

$$(C_{it}|D_i = 1) = Y_{it}^{*P} + G_{it}^{YP} + v_{it}$$

$$(C_{it}|D_i = 0) = Y_{it}^{*P} + v_{it}$$

Then: $G_{it}^{YP} = G_{it}^C + C_{it}^* + \varepsilon_{it}^C - Y_{it}^{*P} - v_{it} = G_{it}^C$

since $(C_{it}|D_i = 0) = Y_{it}^{*P} + v_{it} = C_{it}^* + \varepsilon_{it}^C$.

Thus the consumption gain from the program identifies the permanent income gain.

Does the PIH explain the results?

- 2004 consumption gain actually exceeds the increment to permanent income due to (transient) SWP income gains for plausible rates of interest.
 - More plausible for kernel-matched DD
 - However, can't reject the null that consumption gain equals permanent income gain
- 2000 consumption gains reflect high precautionary savings given uncertainty about income gains during disbursement.

Responses of local political economy

- Positive **spillover effects** to the comparison villages through the displacement of other development spending
 - development support from local resources switched to the villages represented by our comparison group.
- Such interference with the controls suggests that all the classic impact evaluation methods will systematically **underestimate** the long-term impacts.
- In this case, these spillover effects are only imparting a small bias to our impact estimates, under plausible assumptions on the relevant parameters.
- But they may be a bigger concern in other settings.

Rapid appraisal methods

- ...suggest rather little longer-term impacts of the project on living standards, including various “non-economic” dimensions
 - public service quality, local democracy and participation in, and knowledge of, village affairs.
- However, this method is vulnerable to a recall error bias
 - respondents’ perceptions of how their living conditions have changed give too high a weight to current circumstances,
 - and so do not properly reflect actual changes since the baseline.

The most plausible interpretation?

- SWP entailed a large but uncertain transient income gain during the disbursement period.
- A large share was saved, given the uncertainty.
- Modest consumption gains were spread over time.
- The better educated amongst the poor had an advantage.
- This group was under covered by the community-based selection process, which greatly reduced impact.

But this was clearly not the “big push”/“virtuous cycle” growth stimulus that would yield the substantial, lasting, reduction in poverty that had been hoped for.