When Can School Inputs Improve Test Scores?

DIME Seminar

Jishnu Das (DECRG)

with

Stefan Dercon, Oxford
James Habyarimana, Georgetown
Pramila Krishnan, Cambridge
Karthik Muralidharan (UC San Diego and NBER)
Venkatesh Sundararaman, World Bank
Motivation

- The relationship between school spending and learning outcomes is of fundamental importance to education policy and has seen hundreds of empirical studies around the world.

- Empirical public finance literature has traditionally paid careful attention to household (HH) responses to public programs.

- But the education production function literature has rarely accounted for HH re-optimization in response to school inputs.

- This is a critical gap for 2 reasons:
  - HH responses to school spending will mediate the extent to which different kinds of education spending translates into learning outcomes.
  - Parameters of education production functions are typically not identified if HH’s respond to changes in school-provided inputs.
This paper

- Use matched school and household panel data to demonstrate
  - Importance of the school-household inputs link

- Argue
  - In absence of household responses, link between school inputs and test-scores identifies production function parameter
  - When household responses are present, link between school inputs and test-scores identifies policy effect
  - The two are not the same
This paper (Specifics)

Specify theoretical model of household optimization over test-scores
- Structures interpretation of results, particularly definition of “price” of test-scores
- Distinguishes between unanticipated and anticipated grants, where household responses are (by definition) eliminated/lower in the first

Study impact of school grants on household inputs and test-scores in 2 settings
- Observational Results from Zambia: Poor, not growing, severe budgetary issues at time of survey (2002-03)
- Experimental Results from Andhra Pradesh: Fast, growing, expanding budget on education at time of survey (2006-07)

Discuss some research and policy implications
Preview of Results

- Use matched school-household data on spending and panel of test-scores to show in *both* settings
- Households substitute away from anticipated school grants, but not from unanticipated
  - Almost 1 for 1
- *Unanticipated* grants raise test-scores in both settings
- *Anticipated* grants do not
Theory

Empirics – Zambia

Empirics – India

Conclusion
Theory (1)

- Households maximize \( U_t = E_t \sum_{t=\tau}^{T} \beta^{t-\tau} [u(TS_t) + v(X_t)] \)
- Subject to \( A_{t+1} = (1 + r)(A_t + y_t - p_t X_t - z_t) \)
- And the production function \( TS_t = F(TS_{t-1}, w_t, z_t, \mu, \eta) \)

- Can define ‘user cost’ or ‘rental price’ of boosting TS for one period only

\[
\pi_t = \frac{1}{F_{z_t}} - \frac{F_{TS_t}}{(1 + r)F_{z_{t+1}}}
\]
Theory (2)

First order conditions for this inter-temporal problem

\[ E_{t-1} \left( \beta \frac{\pi_{t-1}}{\pi_t} \frac{\partial U / \partial TS_t}{\partial U / \partial TS_{t-1}} \right) = 1 \]

`Euler’ equation for this problem.

- Equate marginal utilities from stock of durable good, appropriately priced through user costs

If

- 1. Utility is additively separable in TS and X, CRRA
- 2. Marginal Utility = \( TS_t^{-\rho} \)

Then

\[
\ln \left( \frac{TS_t}{TS_{t-1}} \right) = \frac{1}{\rho} \ln \beta - \frac{1}{\rho} \ln \left( \frac{\pi_{t}}{\pi_{t-1}} \right) - \frac{1}{\rho} \ln(1 + e_t)
\]
Theory (3)

- Effect of Anticipated Inputs *only* through user costs

\[
\frac{d \pi_t}{d w_t} = - \frac{F_{z_t w_t}}{F_{z_t}^2} \geq 0 \text{ if } F_{z_t w_t} \leq 0
\]

- Two effects to distinguish: smoothing + prices
  - Substitutes: Price higher tomorrow (consume more today); consistent with smoothing
  - Complements: Price lower tomorrow (consume less today?); depends on preferences for smoothing

- Unanticipated Inputs: NO household response. Effect of change strictly positive

\[
TS_t = F(TS_{t-1}, w_t, z_t, \mu, \eta)
\]
Theory - Summary

- Impact of increased public spending on schools on education outcomes mediated via HH response
- HH spending may optimally decrease or increase depending on whether the HH and school inputs are substitutes or complements
- We use household data to investigate substitution effect
- We use test score data to investigate impact on educational outcomes
- Spending in data separated into anticipated and unanticipated funding
Theory

Empirics – Zambia

Empirics – India

Conclusion
The Zambian Education Context

- Historically high enrolments (close to 100%)
- Schools’ teachers are paid centrally, few inputs in kind: schools/parents need to spend on non-teacher inputs
- 2 main sources of school funds for non-teacher inputs
  - BESSIP grant ($600 - 650/school)
    - Fixed per school regardless of enrolment (~45 - 66% of HH spending)
    - Data comes from 2nd year of this program (hence anticipated)
    - Variation comes from per-student value of grant
    - Enrolment is instrumented for by cohort size
  - “Cash budget” grants from district education office
    - Highly idiosyncratic and plausibly arbitrary; depended on the school headmaster (or other staff) being in the district education office at the right time (when money arrived)
    - Large variation among schools
      - Only 25% get any grant of this sort, ~30x variation between schools
Zambia - Data

- Rich data set on 174 schools and about 2700 children (collected in 2002 – 03)
  - Including panel data on student test-scores, and
  - Details on school receipts and spending data, including public expenditure tracking
  - Used for impact on educational outcomes

- Sub-sample of 200 matched children in 35 remote villages
  - With details on household behavior related to schooling (spending)
  - For investigation of HH spending responses
Econometric Specification (HH Spending)

Do Anticipated Grants crowd-out household educational expenditure?

- Test 2: Use “Legislated Funds”

\[
\ln(z_{it}) = \alpha + \beta_2 \ln w_{j(\text{match})}^{\text{anticipated}} + \beta_3 \ln w_{j(\text{match})}^{\text{unanticipated}} + \varepsilon_j + \varepsilon_i
\]

- If substitutes, \( \hat{\beta}_2 < 0 \)
Identification concerns (1)

- No school choice through choice of sample
- $w_{\text{anticipated}}=\$/\text{enrollment}$
- Variation in anticipated funding per capita driven by differences in enrollment.
  - Unobservable village level characteristics could push up enrollment and household spending on education
  - But relatively stable enrollment
- Instrument using size of eligible cohort in catchment of school
  - Note: No correlations between instrument and household characteristics for large number of variables
## Results: Substitution

<table>
<thead>
<tr>
<th>Funding Type</th>
<th>Low Rule Based Grant Schools (N=17)</th>
<th>High Rule Based Grant Schools (N=17)</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Average Per-Child Household Expenditure (Kwacha)</strong></td>
<td>Mean 17882 Standard Deviation [26054] Observations 612</td>
<td>Mean 12022 Standard Deviation [22695] Observations 620</td>
<td>Difference 5860***</td>
</tr>
<tr>
<td><strong>Rule-Based funds (Kwacha)</strong></td>
<td>Mean 5915 Standard Deviation [1733] Observations 17</td>
<td>Mean 12158 Standard Deviation [2893] Observations 17</td>
<td>Difference -6243***</td>
</tr>
<tr>
<td><strong>Total Household and Rule-Based Funding (Kwacha)</strong></td>
<td>Mean 23734 Standard Deviation [25810] Observations 612</td>
<td>Mean 24124 Standard Deviation [23164] Observations 620</td>
<td>Difference -390</td>
</tr>
</tbody>
</table>
Results: Substitution

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rule Based Funds</td>
<td>-0.716**</td>
<td>-0.746***</td>
<td>-0.843***</td>
<td>-1.124***</td>
<td>-1.335***</td>
<td>-0.946**</td>
</tr>
<tr>
<td></td>
<td>[0.285]</td>
<td>[0.256]</td>
<td>[0.252]</td>
<td>[0.266]</td>
<td>[0.392]</td>
<td>[0.460]</td>
</tr>
<tr>
<td>Discretionary Funds</td>
<td>0.0769</td>
<td>0.051</td>
<td>0.0713</td>
<td>0.0661</td>
<td>0.0613</td>
<td>0.0627</td>
</tr>
<tr>
<td></td>
<td>[0.109]</td>
<td>[0.125]</td>
<td>[0.0829]</td>
<td>[0.0910]</td>
<td>[0.0924]</td>
<td>[0.0797]</td>
</tr>
<tr>
<td>Constant</td>
<td>14.69***</td>
<td>15.18***</td>
<td>15.52***</td>
<td>18.42***</td>
<td>20.52***</td>
<td>16.25***</td>
</tr>
<tr>
<td></td>
<td>[2.617]</td>
<td>[2.525]</td>
<td>[2.454]</td>
<td>[2.383]</td>
<td>[3.677]</td>
<td>[3.561]</td>
</tr>
<tr>
<td>Geographic Controls</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Child-level Controls</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Household-level Controls</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Y</td>
</tr>
<tr>
<td>School-Level Controls</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Y</td>
</tr>
<tr>
<td>F-stat of First Stage</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>23.54</td>
</tr>
<tr>
<td>Observations</td>
<td>1,195</td>
<td>1,195</td>
<td>1,116</td>
<td>1,164</td>
<td>1,164</td>
<td>1,085</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.053</td>
<td>0.084</td>
<td>0.239</td>
<td>0.037</td>
<td>0.053</td>
<td>0.238</td>
</tr>
</tbody>
</table>

Cannot reject at mean of sample that households substitute 1 for 1 for school spending
Econometric Specification (Test Scores)

Effect of different funding on gain in learning

\[ TS_t = \alpha_0 + \alpha_1 TS_{t-1} + \alpha_2 \ln w_{it}^{ant} + \alpha_3 \ln w_{it}^{sur} + \alpha_4 \Delta X_t + \varepsilon_{it} \]

PROPOSITION

\[ 0 \leq \alpha_2 < \alpha_3 \]
Identification Concerns (2)

- Potential Placement problem: Funds targeted to schools most likely to improve
  - For instance, funds spent in schools that performed poorly in 2001
- At least on observables, no differences
  - Including baseline test-scores
  - F-stat predicting probability of receipt is 1.58 (p-value 0.17)
Table 4A: The Relative Impacts of Rule-Based Funds and the Receipt of Discretionary Funds on Test-Scores (with a dummy indicator for receipt of discretionary funds)

<table>
<thead>
<tr>
<th></th>
<th>[1]</th>
<th>[2]</th>
<th>[3]</th>
<th>[4]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>English</td>
<td>Mathematics</td>
<td>English</td>
<td>Mathematics</td>
</tr>
<tr>
<td>Any Discretionary Funds Received</td>
<td>0.128**</td>
<td>0.103**</td>
<td>0.0794*</td>
<td>0.0957*</td>
</tr>
<tr>
<td></td>
<td>[0.0583]</td>
<td>[0.0501]</td>
<td>[0.0457]</td>
<td>[0.0481]</td>
</tr>
<tr>
<td>Rule-Based Funds</td>
<td>-0.0272</td>
<td>-0.0184</td>
<td>-0.00416</td>
<td>-0.00445</td>
</tr>
<tr>
<td></td>
<td>[0.0343]</td>
<td>[0.0303]</td>
<td>[0.0216]</td>
<td>[0.0262]</td>
</tr>
<tr>
<td>Constant</td>
<td>0.664**</td>
<td>0.550**</td>
<td>0.467**</td>
<td>0.459*</td>
</tr>
<tr>
<td></td>
<td>[0.288]</td>
<td>[0.259]</td>
<td>[0.187]</td>
<td>[0.235]</td>
</tr>
<tr>
<td>Observations</td>
<td>172</td>
<td>171</td>
<td>172</td>
<td>171</td>
</tr>
</tbody>
</table>
| R-squared         | 0.133       | 0.187       | 0.042       | 0.06        

Treating unanticipated funds as a dummy variable
Regression Results

Unanticipated has impact for English, but not for Mathematics when funding treated as continuous variable.

<table>
<thead>
<tr>
<th></th>
<th>[1]</th>
<th>[2]</th>
<th>[3]</th>
<th>[4]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent Variable:</td>
<td>School-level Test Score Gains (Normalized)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amount of Discretion</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ary Funds Received</td>
<td>0.0700**</td>
<td>0.0598**</td>
<td>0.0193</td>
<td>0.0287</td>
</tr>
<tr>
<td></td>
<td>[0.0331]</td>
<td>[0.0274]</td>
<td>[0.0236]</td>
<td>[0.0240]</td>
</tr>
<tr>
<td>Amount of Discretion</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ary Funds Received</td>
<td>-0.00488*</td>
<td>-0.00422*</td>
<td>-0.000524</td>
<td>-0.00122</td>
</tr>
<tr>
<td>Squared</td>
<td>[0.00274]</td>
<td>[0.00231]</td>
<td>[0.00185]</td>
<td>[0.00192]</td>
</tr>
<tr>
<td>Rule-Based Funds</td>
<td>-0.025</td>
<td>-0.0159</td>
<td>-0.00603</td>
<td>-0.00617</td>
</tr>
<tr>
<td></td>
<td>[0.0348]</td>
<td>[0.0314]</td>
<td>[0.0215]</td>
<td>[0.0261]</td>
</tr>
<tr>
<td>Constant</td>
<td>0.544*</td>
<td>0.439</td>
<td>0.461**</td>
<td>0.438*</td>
</tr>
<tr>
<td></td>
<td>[0.313]</td>
<td>[0.287]</td>
<td>[0.204]</td>
<td>[0.248]</td>
</tr>
<tr>
<td>F-Test (H0: Impact</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>of Discretionary</td>
<td>4.27</td>
<td>2.87</td>
<td>0.99</td>
<td>0.98</td>
</tr>
<tr>
<td>Funding is not equal</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>to impact of rule-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>based funding at</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>mean levels of</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>discretionary funding</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>172</td>
<td>171</td>
<td>172</td>
<td>171</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.139</td>
<td>0.192</td>
<td>0.047</td>
<td>0.065</td>
</tr>
</tbody>
</table>
Reviewing Concerns

- Variation in ‘anticipated’ funding per student driven entirely by variation in enrolment
  - Cannot fully rule out that size of catchment correlated with demand for test-scores, although no correlations with observed characteristics

- If discretionary grants were targeted based on potential for improvement, this would bias our estimates upward
  - We find no correlation between observable school characteristics and receipt of discretionary funding, but can’t rule out targeting

- Finally, cannot confirm that HH spending does not respond to unanticipated funding (HH sample is small - and only 4 out of 34 schools report any such funding)

- We therefore run a randomized field experiment in a completely different setting (India)
Theory

Empirics – Zambia

Empirics – India

Conclusion
Andhra Pradesh Randomized Evaluation Study (AP RESt)

- A large randomized evaluation of the impact of providing a block grant to schools to spend on inputs used directly by students (as opposed to teacher or infrastructure spending)

- Carried out over 2 years

- In the first year, the grant was a complete surprise to the schools in the study (no reason to expect it at all)

- In the second year, the grant was continued, and was largely anticipated
Location of Study

- Indian State of Andhra Pradesh (AP)
  - 5th most populous state of India
  - Population of 80 Million
  - 23 Districts (2-4 Million each)

- Close to All-India averages on many measures of human development

<table>
<thead>
<tr>
<th>Measure</th>
<th>India</th>
<th>AP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gross Enrollment (6-11) (%)</td>
<td>95.9</td>
<td>95.3</td>
</tr>
<tr>
<td>Literacy (%)</td>
<td>64.8</td>
<td>60.5</td>
</tr>
<tr>
<td>Teacher Absence (%)</td>
<td>25.2</td>
<td>25.3</td>
</tr>
<tr>
<td>Infant Mortality (per 1000)</td>
<td>63</td>
<td>62</td>
</tr>
</tbody>
</table>
Andhra Pradesh (AP) Context

- Typical rural school is quite small
  - 80-100 students across grades 1-5

- Teacher salaries account for over 90% of non-capital education expenditure in AP

- Very little funds (~5% of spending on salaries) left for non-teacher and non-capital spending

- Suggests that marginal returns to non-teacher spending may be quite high
  - Pritchett and Filmer (1999)
The School Block Grant Experiment

• Block grant program details
  • Administered by the Azim Premji Foundation (APF) – completely independent of the government and therefore no “substitution bias” with other govt. spending
  • Grant amount was Rs. 125 ($3)/child
  • Guidelines that money had to be spent on inputs *directly used by children*
  • Baseline tests in June-July 05 (school year from mid June to mid April)
  • Program *randomly assigned* to 100 schools out of a *representative sample* of 200 schools in rural AP

• After random assignment, project staff from APF personally went to program schools and communicated the details of the program:
  • Schools were given 2-3 weeks to make a list of items they would like to procure
  • Head teachers and APF staff jointly procured items and delivered it to the school
  • Schools never saw any cash but had freedom to choose materials to procure
Summary of Experimental Design

• Conduct baseline tests (June/July 05)

• Stratified random allocation of 100 schools to treatment and control (2 schools in each sub-district to each group) (August 05)

• Schools make list and items are jointly procured (by September 05)

• Conduct end of year tests (March/April 06)

• Household survey conducted at end of each school year to collect education expenditure data

• Repeat in the school year 2006 – 07. Materials procured around July/August, but expectation of continuation from the start of the school year in June
Data

• Data on HH spending on education (of the child in the school studied) collected at 3 points in time: Y0 (pre-program), Y1 (after first year), Y2 (after second year)
  • Collected retrospectively to ensure inclusion of all spending over the course of the school year

• Data on learning outcomes collected by independent tests that were also conducted at 3 points in time: Y0 (June – July, 05); Y1 (March – April, 06), and Y2 (March – April, 07)
### Sample Balance (Baseline Characteristics)

#### Table 6: Sample Balance Across Treatments

<table>
<thead>
<tr>
<th></th>
<th>[1]</th>
<th>[2]</th>
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</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Control</td>
<td>Block Grant</td>
<td>P-value</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(H0: Diff = 0)</td>
</tr>
<tr>
<td><strong>School-level Variables</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Enrollment (Grades 1-5)</td>
<td>113.2</td>
<td>104.2</td>
<td>0.39</td>
</tr>
<tr>
<td>Total Test-takers (Grades 2-5)</td>
<td>64.9</td>
<td>62.3</td>
<td>0.64</td>
</tr>
<tr>
<td>Number of Teachers</td>
<td>3.07</td>
<td>3.03</td>
<td>0.84</td>
</tr>
<tr>
<td>Pupil-Teacher Ratio</td>
<td>39.5</td>
<td>34.6</td>
<td>0.17</td>
</tr>
<tr>
<td>Infrastructure Index (0-6)</td>
<td>3.19</td>
<td>3.40</td>
<td>0.37</td>
</tr>
<tr>
<td>Proximity to Facilities Index (8-24)</td>
<td>14.55</td>
<td>14.66</td>
<td>0.84</td>
</tr>
<tr>
<td><strong>Baseline Test Performance</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Math (Raw %)</td>
<td>18.4</td>
<td>16.6</td>
<td>0.12</td>
</tr>
<tr>
<td>Telugu (Raw %)</td>
<td>35.0</td>
<td>33.7</td>
<td>0.42</td>
</tr>
</tbody>
</table>
# Spending of Block Grant

## Table 7: Average Spending of Block Grant

<table>
<thead>
<tr>
<th>Items</th>
<th>Year 1</th>
<th>Year 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Rs.</td>
<td>%</td>
</tr>
<tr>
<td>Textbooks</td>
<td>110</td>
<td>1.1</td>
</tr>
<tr>
<td>Practice books</td>
<td>1782</td>
<td>17.7</td>
</tr>
<tr>
<td>Classroom materials</td>
<td>2501</td>
<td>24.9</td>
</tr>
<tr>
<td>Child Stationary</td>
<td>4076</td>
<td>40.5</td>
</tr>
<tr>
<td>Child Durable Materials</td>
<td>864</td>
<td>8.6</td>
</tr>
<tr>
<td>Sports Goods + Others</td>
<td>723</td>
<td>7.2</td>
</tr>
<tr>
<td>Average Total Expenditure per Block Grant School</td>
<td>10057</td>
<td>100</td>
</tr>
</tbody>
</table>
Household Spending Response

The main specification is:

\[
\log(HH_{ijkl}) = \beta_0 \cdot Y_0 + \beta_1 \cdot Y_1 + \beta_2 \cdot Y_2 + \beta_3 \cdot BG \cdot Y_0 + \beta_4 \cdot BG \cdot Y_1 + \beta_5 \cdot BG \cdot Y_2 + \varepsilon_{ijk}
\]

The terms of interest are:

- \(\beta_3\) which tests the validity of the randomization
- \(\beta_4\) which captures the HH response to an unanticipated grant
- \(\beta_5\) which captures the HH response to an anticipated grant

Can also restrict the analysis to just the panel (where the same HH is observed in all 3 periods)
Household Spending Response (Logs)

Table 8: Household Expenditure on Education of Children in Block Grant Schools (relative to comparison schools) over time

<table>
<thead>
<tr>
<th>Log of Household Expenditure</th>
<th>BG * Year 0</th>
<th>BG * Year 1</th>
<th>BG * Year 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.099</td>
<td>-0.043</td>
<td>-0.25</td>
</tr>
<tr>
<td>[0.157]</td>
<td>[0.028]</td>
<td>[0.040]***</td>
<td></td>
</tr>
</tbody>
</table>

Observations: 34645
R-squared: 0.96
P-value (BG * year 1 = BG * Year 2): 0.000

Evaluated at mean of HH expenditure, this implies an elasticity of substitution of -0.8 (cannot reject equal to 1)
Impact of School Grants on Test Scores

The main specification is:

\[ \Delta T_{ijkm}(Y_n) = \alpha + \gamma \cdot T_{ijkm}(Y_0) + \delta \cdot BG + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \]

\[ i = \text{Child}, \; j = \text{Grade}, \; k = \text{School}, \; m = \text{Mandal(Sub-District)} \]
# Impact of School Grants on Test Scores

## Table 9: Impact of Block Grant on Student Test Scores

**Dependent Variable = Gain in Normalized Test Scores**

<table>
<thead>
<tr>
<th>Mathematics</th>
<th>Language (Telugu)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>First-year Gain (Unanticipated Grant)</strong></td>
<td><strong>First-year Gain (Unanticipated Grant)</strong></td>
</tr>
<tr>
<td><strong>Second-year Gain (Anticipated Grant)</strong></td>
<td><strong>Second-year Gain (Anticipated Grant)</strong></td>
</tr>
<tr>
<td><strong>Two-year Gain</strong></td>
<td><strong>Two-year Gain</strong></td>
</tr>
<tr>
<td>[1]</td>
<td>[4]</td>
</tr>
<tr>
<td>[2]</td>
<td>[5]</td>
</tr>
<tr>
<td>[3]</td>
<td>[6]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Block Grant School</th>
<th>Mathematics</th>
<th>Language (Telugu)</th>
</tr>
</thead>
<tbody>
<tr>
<td>School</td>
<td>First-year Gain</td>
<td>Second-year Gain</td>
</tr>
<tr>
<td></td>
<td>[0.091]</td>
<td>[-0.008]</td>
</tr>
<tr>
<td></td>
<td>[0.042]**</td>
<td>[0.049]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Observations</th>
<th>13778</th>
<th>12844</th>
<th>9891</th>
<th>13926</th>
<th>12878</th>
<th>9981</th>
</tr>
</thead>
<tbody>
<tr>
<td>R-squared</td>
<td>0.293</td>
<td>0.302</td>
<td>0.325</td>
<td>0.254</td>
<td>0.206</td>
<td>0.238</td>
</tr>
</tbody>
</table>

**Notes:**

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.

2. Estimates of two-year gains do not include the cohort in grade 1 in the second year (since they only exposure to one year of the program)

* significant at 10%; ** significant at 5%; *** significant at 1%
Heterogeneity of Program Impact

• The main result is that there is NO heterogeneity of program impact by several covariates – including HH affluence

• Mean HH spending in the control group is Rs. 450/year, while the grant amount is Rs. 125/child

• Only 12% of HH in control schools spent less than Rs. 125/year

• Suggests that even poor HH were spending enough on education that the grant was less than their desired level of spending and that the grant did not push them beyond this level
Robustness

- Were different types of grants spent on items with different extents of substitutability (say teachers vs. non-teachers)?

- Other budgetary offsets?

- ‘True’ surprises vs. ‘Fully’ anticipated funds?
**Summary**

- We develop a general model of optimal HH responses to changes in schooling inputs and test it in two completely different developing country settings:
  - Zambia – with cross-sectional data
  - India – with experimental data

- We find in both settings that HH spending and school spending on non-teacher, non-infrastructure inputs are substitutes:
  - HH spending responds much less to unanticipated changes in spending than to anticipated changes

- The differential responses of HH spending to the types of grants lead to different effects on test scores:
  - Student test scores improve when the grants are unanticipated (and when there is an increase in “net” spending); In India, we also see an effect on student attendance in school
  - But anticipated grants have no effect on test scores
  - High levels of “pass through” even in low-income settings
Implications for Research and Policy

We have shown that the theoretical distinction between estimating “production function” parameters and “policy response” parameters is empirically important (in multiple locations).

In addition to identification and specification, other explanations for the vast variation in empirical estimates of EPF’s could be variation in
- Unanticipated versus Anticipated Inputs (and one time versus permanent changes)
- The extent of substitutability/complementarity between school and household inputs
- Time horizon of evaluations

Education research and policy cannot ignore HH responses, and our results may help explain why “spending” does not seem to “matter”

Implications for interpretation of experimental evaluations in general?

Policy implication is NOT that inputs should be ‘surprises’!
- Recognize centrality of HH in decisions regarding human capital accumulation
- Focus on inputs that are less substitutable or have more public goods characteristics