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ESSAYS IN DEVELOPMENT ECONOMICS

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July, 2012
Declaration

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“It ain’t what you don’t know that gets you into trouble. It’s what you know for sure that just ain’t so.”

Mark Twain
Summary

This thesis is motivated by research questions faced by policy makers in developing country governments and donor agencies. In particular, it deals with the appropriate measurement of foreign aid and its possibly pernicious effects in a large cross-section of countries, and explores the microeconomic effects of a distinct macroeconomic policy innovation in Vietnam. While the nature of economic research means the contributions collected here engage with specific questions, those questions interact with a wider body of literature that is relevant to the practice and theory of development policy.

Amidst a vigorous debate about how much aid wealthy donors should be spending, there is little agreement or study about which measure of foreign aid is appropriate for policy analysis and public discourse. The first chapter summarises research evaluating several complementary but distinct measures of foreign aid available to researchers and policy makers based on how much these measures, on average, actually affect the spending patterns of recipient governments.

Using a core sample of more than thirty countries and a variety of econometric models, we find that Country Programmable Aid, rather than competing measures that fully net out reverse factor payments like interest on debt, is the most policy-relevant measure by this standard. This finding is robust to alternative assumptions about the error structure, fixed effects, structural breaks, spatially clustered errors, and extreme events such as natural disasters. One implication of this research is that for the marginal aid flow, aid modalities matter more than aid levels: for recipient governments, how aid is spent matters more than how much is spent.

While the first chapter of the thesis focuses on studying distinct definitions of foreign aid based on maximising the “objective function” of government spending, aid flows have many other relevant first- and second-order effects. The effect of foreign aid on relative prices in recipient countries has emerged as an important research theme in this literature, since relative price distortions plausibly have long-run effects on economic growth. Dutch disease is the increase in a country’s real exchange rate (RER) and contraction in the
production of tradeable goods that follows an inflow of foreign exchange; since foreign aid is effectively a transfer of foreign exchange to developing economies, it might generate Dutch disease effects.

The second chapter of this thesis develops a simple static general equilibrium model of this effect, and examines the question using cross-country data on aid flows and an approximate RER index that corrects for differences in price levels due to the Balassa-Samuelson effect. Since aid is, by definition, not randomly assigned, we investigate if oil prices are a plausibly exogenous instrument. Using recent econometric results on weak instruments and an extended equilibrium model, we are able to define credible sub-samples that unpack the aid-RER link. We find that, on balance, while there are many reasons to critique the level and modes of disbursement of foreign aid, these transfers do not cause appreciations of recipients' real exchange rates.

Foreign aid represents a large transfer of resources between countries, but studying the effect of the aggregate flow on macroeconomic variables of interest cannot capture the microeconomic effects of the policy diffusion that often accompanies these aid flows. The final chapter of this thesis therefore changes the level of analysis to examine the effects of a significant policy shift in Vietnam on low-income rural households.

Access to savings instruments for low-income households is an important area of economics research with clear implications for policies implemented by donors and governments. The issue is typically framed in terms of increasing access, with the implicit assumption that lower-income households want to save but cannot because of market failures in the supply of formal savings products. The third chapter of this thesis tests the assumption by studying the differential effect of an exogenous income shock on the savings behaviour of rural Vietnamese households.

Each additional percent increase in household income is robustly associated with a statistically significant and economically meaningful increase in the probability the average household has a formal savings instrument. A tentative conclusion drawn from the positive and large estimated elasticity is that scarce resources should be refocused on the most financially marginal households.

The contributions collected here do not suggest a single overarching policy innovation or research agenda. Rather, they point to the need for policymakers to rigorously evaluate both the inputs to development policy, in particular the contemporary foreign aid regime, and the outputs: the effects of specific policy innovations on households and individuals.
Acknowledgements

I am very grateful to the Department of Economics of Trinity College, Dublin, which provided generous financial support and gave me an intellectual home where I interacted with an extraordinary group of students, practitioners, and scholars.

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Dr. Michael Wycherley deserves particular praise. He has been an excellent supervisor, keeping his door open to me and taking a personal interest in my research. His critiques of my work have been insightful and necessary, and these papers benefited greatly from his encouragement and careful attention.

While Thomas Carlyle famously called economics the “dismal science” for reasons we will describe here only as very misguided, the experience of writing this thesis would, indeed, have been dismal were it not for Kim Le, Lara Gidvani, and Marc Sellès. They are better friends than I deserve, and have been despite the distances that separated us over the years: between Dublin, Nice, Bombay, London, New York, Montréal, Bangkok, Port Vila, Boston, and Ha Noi. I am, as ever, grateful for their puzzling willingness to put up with me.

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I dedicate this thesis to them, to my Grandparents, Ananta Bijoy and Rani Bhattacharya, and Dennis and Barbara Talbot, and to my Uncle, Purnendu Narayan Roy.
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Chapter 1

*How or How Much? Evaluating Different Measures of Foreign Aid*

1.1 Introduction

National accounts have recorded the transfer of funds under the heading Overseas Development Assistance (ODA) since the 1960s, but estimates of even the total amount of foreign aid spent range from $2.3 trillion US dollars estimated by Easterly (2006) to over $4.9 trillion dollars estimated by Tierney et al. (2012), a huge variation indicative of the opacity affecting aid policy and discourse.

Against this backdrop, two major policy innovations have occurred in the last decade. Firstly, donor countries have committed to increasing how much development assistance they give, in part by tying spending to GDP\(^1\). The International Conference on Financing for Development, in Mexico (United Nations, 2002a), and the World Summit on Sustainable Development, in Johannesburg (United Nations, 2002b), called on donors to allocate 0.7% of national income to foreign aid.

Secondly, a new international consensus has emerged about how that aid should be spent. The *Paris Declaration on Aid Effectiveness* in 2005 and the *Accra Agenda for Action* in 2008 detail responsibilities of donor countries and aid recipients ranging from improved donor coordination and consultation to a commitment to using Environmental Impact Assessments in the design of development projects. The donor community has explicitly agreed to align funding around national development plans to address the

---

\(^1\)Despite the recent decrease in GDP growth caused by the global recession this, for almost all donors, implies higher levels of aid spending
problem of fractured, competing donor activities that bypass recipients’ public finance systems. The Paris Declaration states, for example, “The capacity to plan, manage, implement, and account for results of policies and programmes, is critical for achieving development objectives from analysis and dialogue through implementation, monitoring and evaluation” and commits donors to strengthening recipients’ public financial management capacity and “…rely to the maximum extent possible on transparent partner government budget and accounting mechanisms.”

How we measure foreign aid matters both for evaluating recipients’ progress on development outcomes and monitoring donors’ performance relative to their commitments. If an aid measurement has a very small effect on government spending, then dramatic increases in aid as measured are unlikely to increase government expenditure or enable governments to affect indicators ranging from economic growth to child mortality. Conversely, if an aid measurement has, on average, a very large effect, the opposite is true. If the elasticity of government spending with respect to aid is less than one-for-one, the “aid” flow contains components- possibly substantial shares of the total- that do not affect recipients’ budgets.

This paper tests the different definitions of foreign aid available to researchers and policymakers and ranks them based on their estimated effect on the spending of recipient governments. Specific formulations of foreign aid are empirically shown to have significantly larger marginal effects on recipient government spending, potentially making this subset of available measures more relevant for both discussions or negotiations about aid levels and academic research in which the relationship between aid and outcomes is mediated by government spending.

1.1.1 Government spending and development outcomes

Aid flows represent a large transfer of resources and policy innovations from donors and multilateral agencies to low-income countries, which has inspired an influential academic literature about the relationship between aid and development outcomes ranging from macroeconomic aggregates like economic growth to microeconomic indicators like inoculation rates. The focus of this literature has gradually shifted from whether any effect of aid can be empirically detected to how governments and policies mediate the relationship between aid and outcomes.

In a seminal article, Bourguignon and Sundberg (2007) argue that the aid-outcomes literature implicitly links resource transfers directly to outcomes, but
either ignores the intervening variables of domestic governance and policies or treats them as an unopened “black box.” Hansen and Tarp (2000) provide a history of this literature and focus on what they call the “third wave” of studies that innovate by including many countries and years and interacting measures of aid with measures of policies. Boone (1996), for example, finds that aid does not have an effect on a large set of health and economic welfare indicators, including investment or economic growth. Burnside and Dollar (2000) find that aid increases the growth rate of national income, conditional on good policies, but Easterly et al. (2003) contradict their findings using an updated data set. In contrast, Hansen and Tarp (2000) find that the negative results reported by some papers are compromised by inappropriate modeling choices or questionable sample selection, and that aid is positively and robustly linked to several outcomes and aggregate variables. On balance, the aid-growth relationship may be positive, but is sensitive to model specification, sample size, and underlying data on aid flows, including which measurement of foreign aid is used.

Clemens et al. (2004) provide the current state of the art for the aid-growth literature. They argue that regressions in four- or five-year averages can never capture the long-term effects of most aid flows. Adjusting their measure of aid flows to include only those kinds of aid expected to have a short-term growth impact, they find a significant and positive aid-growth relationship in a cross-section of countries. Their contribution underscores this paper’s motivation: how aid is measured affects a very broad set of findings about aid effectiveness.

1.1.2 Fungibility and diversion

Because the dependent variable of interest is government spending, our research question is naturally related to papers studying how aid is spent by recipient governments. Aid is said to be fungible when it is used for a purpose other than the one intended (McGillivray and Morrissey, 2001), for example when a government receives an additional $1 for health spending but treats this transfer as a general subsidy and passes some (or all) of it into other budget lines.

Feyzioglu et al. (1996) initiated this area of research in the cross-country context, motivating empirical tests with a simple model in which governments maximize a payoff defined over two sets of public goods subject to a budget constraint. Donors and the government have different preferences over these goods, so optimal behaviour by donors is to subsidize the government by an amount equal to the difference between their respective optimal expenditure
levels. When the donor can only allocate money to the government’s overall budget, rather than specifically subsidize the preferred sector, aid becomes fungible: the government’s optimal choice is to allocate the sector-specific aid to other budget lines. In a sample of fourteen countries, the authors find that aid earmarked for the transportation and communication sectors exhibits little or no fungibility, while flows to agriculture, the energy sector, and education are highly fungible.

Pack and Pack (1993) undertake an analogous exercise, also motivating an empirical specification using a simple model of a government that maximizes its payoffs. They use data from the Dominican Republic and find that aid fungibility compromised donor objectives, while in Pack and Pack (1990) they find that aid to Indonesia is not fungible. Similarly, Chatterjee et al. (2007) use a neoclassical model of the government as an infinitely-lived representative agent to derive testable predictions about the degree of fungibility of different kinds of aids flows. In their model, the level of fungibility is determined by the government’s payoff-maximizing response to an exogenous inflow of aid. The authors find that aid for investment is highly fungible, on the order of 90 cents per donor dollar. For aid to social infrastructure, this figure is 0.78 (they do not find evidence of fungibility in other categories).

Lu et al. (2010) find that for aid to the health sector disbursed by the recipient government, the majority of aid flows are not spent as intended. They conclude that “...for every $1 of [development assistance for health] given to government, the ministry of finance reduces the amount of government expenditures allocated to the ministry of health and other government agencies that engage in health spending by about $0.43 to $1.14.” This paper follows Lu et al. in that we do not focus on the mechanism through which aid is made fungible (i.e., through reallocation to other sectors or through graft) but instead focus on measuring the relationship between donor funding for grants, programs and loans and total government expenditure.

In contrast, several studies give us cause to be more optimistic. Using the a term borrowed from the behavioural economics literature to describe aid mostly “sticking” where it is allocated, these papers find a “flypaper effect” of foreign aid: some aid may be re-allocated, but most ends up in the sector the donor intended. In a microeconometric study of a road works project in Vietnam, van de Walle and Mu (2007) find evidence of a low level of aid fungibility: while fewer kilometers of roads were rehabilitated than anticipated in the project plan, more roads were built in the areas targeted. They conclude that this is evidence of a flypaper effect, since aid targeted to public infrastructure “largely stuck to that sector.”
While related to fungibility, diversion is more pernicious because resources are re-directed to private actors rather than other budget lines. Svensson (2000) writes a theoretical model of the connection between foreign aid and domestic rent-seeking to explain this effect, and Alesina and Weder (1999) use cross-country data to show that while corrupt governments do not receive more aid, corruption does not reduce future levels of aid flows. Since the authors use one measure of foreign aid (net ODA, described in detail below), our motivating question still applies: if the outcome of interest is the level of government spending, which measures of aid are appropriate?

One of the most striking examples of a dramatic reduction in aid diversion comes from Reinikka and Svensson (2004) who studied educational aid disbursement in Uganda. Initially, 85 cents of every dollar of central government aid allocated to schools was diverted. After informing community groups about the level and timing of school aid flows, the resulting increase in scrutiny reduced this to around 15 cents per dollar, in line with actual administrative costs.

More generally, Rajkumar and Swaroop (2002) explore the link between government spending and human development outcomes, arguing that corruption and the quality of government bureaucracy are the key intervening variables mediating the relationship between expenditure (including aid-financed expenditure) and development indicators. Their main result confirms our intuition that spending is more effective in countries with better governance. Morrissey (2012) provides the most current cross-country evidence on “aid” and government spending by examining the effect of foreign aid on the fiscal behaviour of recipient governments: while aid does finance government spending (the covariance is large, positive, and robust), fungibility is over-stated and does not make the aid less effective, and aid does not, on average, decrease recipients’ tax effort.

Whether examining the relationship between total aid flows and macroeconomic aggregates or sectoral aid flows and specific development outcomes, fitted coefficients are only identified if foreign aid is measured correctly. We therefore begin by decomposing different statistical definitions of aid flows.

### 1.2 Measuring foreign aid

The largest data set of observations for aid flows is reporting from the OECD’s Development Assistance Committee (DAC), which provides information on aid commitments and disbursements through the Creditor Reporting System.
A commitment is a transfer budgeted by the donor, while a disbursement is a transfer of resources. Since recording a commitment as being disbursed requires additional reporting, the data set of aid commitments has many more observations than the equivalent data set of disbursements: 5,521 country-year observations compared to 3,110, a discrepancy that arises due to a combination of aid promised but not delivered, decreases in funding because projects fail or governments’ capacity to absorb the aid changes, or errors and omissions.

While disbursements are actual transfers, it is not clear they are a “better” measure, since some commitments may be disbursed but not recorded. More generally, Dudley and Montmarquette (1976) argue that commitments are actually more useful because they reflect the intentions/policies of donors. This paper’s goal is to compare the effect of different measures of aid on government spending, so we test both measures.

We innovate by using a relatively new data set, the Project Level Aid Database (PLAID) that supplements the OECD’s CRS data by including additional observations from donor documents, project documents, OECD data not included in the CRS, and aid flows from non-DAC members, including the large aid flows from Arab states that are not DAC members. The overall coverage of project-level aid flows in PLAID is exceptional, with more than 957,000 unique project-country-year observations from 1970 to 2010. (Tierney et al., 2012, give an extensive description of the construction of the data set, and summarise existing research based on it).

The OECD defines a flow to be ODA if “development [is its] main objective, it has a grant element of at least 25% and, if a loan, is offered on concessional terms.” However, their definition of “grant” includes debt forgiveness, for both development lending (ODA) loans and other official flow (OOF) loans that were originally not classified as concessional because they were designated for military or other non-development purposes. (Chauvin and Kraay, 2005, show convincingly that while debt relief theoretically affects countries’ intertemporal budget constraints, it has no observable impact on governments’ social expenditures on health or education). Succinctly, this measure is:

\[
\text{ODA, Commitments/Disbursements} = \begin{align*}
\text{Pure grants} \\
+ & \text{ODA loans extended} \\
+ & \text{OOF loans forgiven} \\
+ & \text{ODA loans forgiven}
\end{align*}
\]
What are called "pure" grants include items that may not be a transfer to the recipient. For example, technical assistance is typically paid to donor agencies or overseas consultants, and capital subscriptions to international organisations like the World Bank.

**Figure 1.1: Unconditional Distribution:**
Commitments/Disbursements and Government Spending

(a) Commitments
(b) Disbursements

1.2.1 **Net ODA**

ODA commitments and disbursements are imperfect measures because they may not represent an actual flow of resources from donors to recipients: if loan forgiveness is a large share of the "aid" flow, we will only observe an effect of aid on government spending, if any, through the change in the government's intertemporal budget constraint and a reduction in current interest payments.

To supplement this "gross" measure, for a subset of donors and recipients the OECD has constructed the net ODA measure that excludes ODA lending and ODA loans forgiven, and nets out the repayments of the principle of outstanding loans.

\[
\text{Net ODA} = \text{ODA, Commitments/Disbursements} - \text{ODA loans forgiven} - \text{OOF loans forgiven} - \text{ODA loans repaid (principle)}
\]

This is a better approximation of "aid" as an transfer of current resources, rather than a decrease in the debt burden.
1.2.2 Net Actual Transfers

Roodman (2004) argues that while the OECD definition of net ODA approaches the idea of net wealth transfers to developing countries, it does not make sense to exclude only payments on the principle of outstanding ODA loans. As he memorably puts it: “When the Government of Ghana sends a check to the government of Japan for $1 million, it hardly matters to citizens in either country whether the check has ‘interest’ or ‘principle’ in the memo field.”

Roodman attempts to correct for this by constructing a “net aid transfers” measure that is essentially net ODA less repayments of both principle and interest:

\[
\text{Net aid transfers} = \frac{\text{ODA, Commitments/Disbursements}}{\text{ODA loans forgiven}} - \frac{\text{ODA loans forgiven}}{\text{OOF loans forgiven}} - \frac{\text{ODA loans repaid (principle)}}{\text{ODA loans repaid (interest)}}
\]

1.2.3 Country Programmable Aid

Recognizing that gross and net ODA measures that were then available were imperfect measures of actual resource transfers, the OECD DAC itself defined CPA in 2007, a new metric that attempts to correct for the so-called “phantom” transfers that are included in other OECD measures of aid flows.

CPA is formulated to capture the share of ODA that recipient governments
can “program,” that is, the share of total aid that developing countries have a meaningful say in allocating. This is distinct from the other components of gross aid, which includes debt forgiveness and repayment, and funding for projects designed and implemented by third parties, such as aid agencies or technical consultancies. Formally, CPA is

\[
\text{Country Programmable Aid} = \frac{\text{ODA, Commitments/Disbursements}}{} - \text{ODA loans forgiven} - \text{OOF loans forgiven} - \text{Humanitarian aid} - \text{Aid to third parties}
\]

This excludes aid flows over which recipients have limited control: humanitarian aid is not programmed into budgets by recipient governments (except in exceptional circumstances), and aid to third parties includes administration costs, aid awareness / research initiatives, funding to international organisations or NGOs, and ODA equity investments. Unlike both net ODA or net aid transfers, CPA does not net out loan repayments on either the principle or interest.

All series are constructed from OECD data, but table 1.1 shows they do not have identical coverage of years or countries, and have distinct summary statistics.
1.3 Empirical framework

Our objective is to evaluate available aid measures based on their effect on government spending. It is important to note that the average level of aid implied by each measure is different: the point of this study is explicitly not to find out how much aid is being given, but to estimate the effect on government spending of a marginal increase in the various measures of aid. The larger the fitted elasticity of aid on spending, the greater the impact of the aid flow, as measured, is on the recipient government’s budget.

We call an observation $aid_{hjit}$ where the $j$ is an the measure (ODA commitments, ODA disbursements, net ODA, net aid transfers, and country programmable aid), $h$ indexes donors, and $t$ is the year. Aid projects are sometimes planned over several years, but it is not possible to clean the OECD data of all cases when the full amount of a multi-year project is recorded in, for example, the project’s first year. We abstract from this and construct for each type of aid measure, country, and year

$$\frac{\sum_{h=1}^{H} aid_{hjit}}{Y_{it}} = a_{jit}$$

Government spending in year $t$ increases in the aid flow and the domestic revenue base $r_{it}$:
\[ g_{it} = \beta \cdot a_{jit} + \gamma \cdot r_{it} + \epsilon_{it} \]

where the error term absorbs other factors that determine government spending. If 100% of the transfer appears in the recipient’s budget, \( \hat{\beta} = 1 \). In many cases, measures of “aid” include components that do not enter the government’s budget, so the relationship will be less than one-for-one: what appears to be a transfer of resources is, for example, spent on services like technical assistance or paying down debt. The total aid flow is composed of a share \( a^G_j \) that passes directly into government spending and a share \( a^D_j \) that is recycled to the donor or other non-government actors, and \( a_{jit} = a^G_{jit} + a^D_{jit} \), so \( a^G \) and \( a^D \) have an inverse relationship. If the orthogonality condition \( \text{cov}(a_{it}, \epsilon_{it}) = 0 \) is satisfied, aid’s effect on government spending is

\[
\beta = \frac{\text{cov}(a^G_j, g)}{\text{var}(g)} + \frac{\text{cov}(a^D_j, g)}{\text{var}(g)}
\]

implying bias of \( \hat{\beta} - \beta = -\frac{\text{cov}(a^D_j, g)}{\text{var}(g)} \). Because we do not observe the share of “aid” that actually enters recipients’ budgets, we infer its size from \( \hat{\beta} \). For example, we expect that paying interest on loans does not increase government spending; since net aid transfers (NAT) excludes loan forgiveness and debt repayments but net ODA does not, a reasonable prior is that \( \hat{\beta}_{\text{NAT}} > \hat{\beta}_{\text{net ODA}} \). We consider an aid measure “relevant” to the extent it covaries with government spending.

### 1.3.1 Regression model

Without expanding the bivariate relationship between aid and government spending, the orthogonality condition does not hold and \( \hat{\beta} \) is estimated with error. An expanded model is:

\[
g_{it} = \alpha_i + \beta \cdot a_{jit} + \gamma' \cdot X_{it} + \eta_t + \epsilon_{it} \tag{1.1}
\]

Many factors that determine both the level of aid flows and government expenditure are fixed over time and captured by the country-level fixed effect \( \alpha_i \). Tavares (2003), for example, finds that colonial origins, and a shared language, religion, or border are strong predictors of bilateral aid levels. It may be that wealthier countries are able to spend more on the public sector, and this wealth can be explained by the country’s colonial origins that also underlie larger aid
receipts, as in the famous contribution from Acemoglu et al. (2001). In these and many other cases, the cross-country variation is fixed over $t$ and absorbed by the fixed effect.

Since DAC country economies are financially integrated, aid varies over a common international business cycle (fiscal expansions in donor countries are accompanied by increased aid expenditure): Minoiu et al. (2010) and others show that aid flows are procyclical with respect to this business cycle. To the extent that global events may determine both developing countries' levels of government spending and aid, we also include annual fixed effects $\gamma_t$.

### 1.3.2 Data

The level of government spending is measured by the ratio of government final consumption expenditure $G_{it}$ to output $Y_{it}$, so we define $g_{it} = \frac{G_{it}}{Y_{it}}$ to remove the price effects in the measurement of the size of government. As described above, we measure the aid flows received according to each measure of total aid by aggregating over the set of donors in each year and dividing out price effects for consistency with $g_{it}$: $a_{jit} = \frac{aid_{jit}}{Y_{it}}$.

Country- and year-fixed effects condition out many variables that determine both the size of government and the level of aid received. We include additional non-redundant information in $X_{it}$. We cannot directly include $r_{it}$ because available data series on central government revenue are short and include many missing observations, so $X_{it}$ includes information that is collinear with the level of government revenue. GDP per capita (in constant US dollars) and the growth rate of GDP per capita are included because both aid and government spending are strong determinants of domestic revenue base. Similarly, population (in log points) is included because more populous countries typically require larger governments to administer them and may also receive more aid. Urbanisation, measured by the share of the total population living in urban areas, affects both the level and kind of aid received and the size of

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2An alternative data source used by several other studies of the effect of foreign aid on government spending (or specific line items in government budgets) is the IMF's Government Financial Statistics (GFS) database. Data from 1972 to 1989 only exist in the historical GFS data series (available on CD-ROM), while data from 1989 are available from the IMF's Government Financial Statistics online database, and reporting standards are inconsistent across the two series.

To determine the appropriate data set, the GFS was used for comparison. When government spending is scaled by national output or converted from local currency units to US dollars and deflated to 2000 constant values, the resulting series includes many implausibly high observations (mostly, but not only, for those countries that experienced rapid inflations). While several other studies of aid flows and government spending rely on IMF data, using them without questionable cutting and trimming of the time series creates significant measurement error.
government, since urban communities that are heavily dependent on public goods require commensurately higher government expenditure.

Rodrik (1996) shows that more open countries have larger governments because globalisation appears to increase income volatility, forcing governments to purchase a more generous safety net. Many developing country governments lack the capacity to collect taxes internally on income or production, and levy taxes on external trade to fund government services. To allow for both the revenue-enhancing and safety net-expanding effects of trade on government spending, we include measures of exports and imports as a share of GDP.

Finally, we include debt service as a share of GDP. Spending more of national output servicing debt (or publicly-guaranteed debt through state-owned corporations) reduces resources available for government final consumption expenditure, and highly indebted poor countries are likely to have distinct macroeconomic policies and relationships with donors.

The original data for these covariates are from World Bank (2008), with transformations as described above, and table 1.2 presents summary statistics for the full sample available, a subset of which (discussed below) is used to empirically compare aid measures by their effect on government spending.

### Table 1.2: Summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>GDP growth, annual pc.</td>
<td>0.039</td>
<td>0.063</td>
<td>-0.51</td>
<td>1.063</td>
<td>7766</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>7.643</td>
<td>1.604</td>
<td>4.057</td>
<td>11.591</td>
<td>7816</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>0.485</td>
<td>0.251</td>
<td>0.02</td>
<td>1</td>
<td>10494</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.413</td>
<td>0.261</td>
<td>0.001</td>
<td>2.173</td>
<td>7248</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>0.356</td>
<td>0.258</td>
<td>0.001</td>
<td>2.344</td>
<td>7248</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>0.047</td>
<td>0.049</td>
<td>0</td>
<td>1.109</td>
<td>4199</td>
</tr>
</tbody>
</table>

1.3.3 Sample selection and baseline results

Our data includes observations of “aid” transfers between donors (as may occur when, for example, Australia’s AusAID co-finances a project with the British Government’s Department for International Development), so we select only countries the World Bank defines as low income, small island states, or lower middle income. In some cases, only a few years of aid transfers are recorded. These are likely to be exceptional, so we limit the sample to those countries with at least 10 years of aid flows. These selection rules reduce our overall sample to around 30 countries.

Another constraint on comparing measures of foreign aid is a mismatch in coverage: table 1.1 shows the shortest time series is for CPA (2000-2010),
while the longest are available from 1960 to the present, with more country-year observations for recent years. To directly compare these measures, we also estimate the preferred model in the sub-sample of $t \geq 2000$, so we have two measures of the fitted elasticity: $\tilde{\beta}$ and $\tilde{\beta} | t \geq 2000$. As constructed, an 1% increase in a measure of foreign aid is associated on average with a $\tilde{\beta}\%$ increase in government spending.

Baseline results include columns with country fixed-effects and heteroscedasticity-robust errors clustered within countries, subtracting the full set of $N$ country-specific intercepts under the assumption $\text{cov}(\alpha_i, \epsilon_i) \neq 0$. In contrast, a random-effects estimation produces estimates for $\tilde{\beta}$ and other coefficients under the assumption that the $\alpha_i$ are normally-distributed with mean zero and orthogonal to the error term. If this orthogonality condition is violated, fixed effects will be consistent while random effects models will not. The Hasman specification test is the standard distance test of the difference between the fitted coefficient matrix of the fixed effects model $\tilde{\beta}_{FE}$ and that for the random effects model $\beta_{RE}$.

Under the assumption that standard errors are clustered at the country level, the test statistic is distributed $\chi^2_{K-1}$ under the null hypothesis of no difference in the coefficient matrices between RE and FE models, with degrees of freedom equal to the number of non-redundant (time-varying) independent variables. The test statistics reported below for each model reject this null, so fixed effects-type models are appropriate and are used throughout.

### Table 1.3: Summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>$\chi^2_{K-1}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitments, pc. GDP</td>
<td>40.02*</td>
</tr>
<tr>
<td>Disbursements, pc. GDP</td>
<td>35.41***</td>
</tr>
<tr>
<td>Net ODA, pc. GDP</td>
<td>64.54***</td>
</tr>
<tr>
<td>Net aid transfers, pc. GDP</td>
<td>73.89***</td>
</tr>
<tr>
<td>CPA, pc. GDP</td>
<td>45.25***</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.10

Since a fixed effects (FE) model is supported, we specify this as the baseline model and estimate the regression problem of equation 1.1 for each measure of foreign aid. Given donors' commitments and a practical interest in the level of foreign aid that passes into government spending, the elasticity of government final consumption expenditure with respect to the aid flow provides a framework in which to rank the available measures of foreign aid; a larger estimated elasticity implies a higher expected effect of aid as measured. Tables 1.13 - 1.16

---

$^3$The test statistic is

$$(\tilde{\beta}_{FE} - \tilde{\beta}_{RE})'\left[\text{var} (\tilde{\beta}_{FE} - \tilde{\beta}_{RE})\right]^{-1}(\tilde{\beta}_{FE} - \tilde{\beta}_{RE})$$
show the results of the baseline regression, and table 1.4 summarises results for our variable of interest.

Aid commitments, net ODA, NAT and CPA all enter the regression significantly; here and throughout this paper, disbursements are only sporadically significant determinants of government expenditure. Net ODA and net aid transfers have a statistically significant effect on government spending of between 0.15 and 0.17, implying a 1% increase in these measures of foreign aid causes on average a 0.16% increase the level of government final consumption expenditure. Despite the fact that it does not explicitly exclude debt repayment and other return aid flows, CPA has the largest marginal effect on governments' spending of around 0.20. The balance of the paper investigates the robustness of this ranking.

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>( \hat{\beta} ), full sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>0.058*</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>0.038</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>0.165***</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>0.151***</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>0.219***</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

1.3.4 Outliers: distribution of \( \hat{\beta} \) and excess influence

In addition to expanding the right-hand side by including \( \alpha_i, \eta_t, \) and \( X_{it} \), we test if the fitted coefficients \( \hat{\beta}_j \) are driven by a subset of countries with exceptionally high / low elasticity (of government final consumption expenditure with respect to aid received) for reasons that are not captured in the added independent variables. We get traction by estimating equation 1.1 and successively excluding each country \( i \ldots N \) to calculate the effect of aid on spending when that country is excluded from the sample. If the resulting distribution of fitted coefficients \( \hat{\beta}_{-ij} \) shows many observations far from the mean, the effect of aid on government spending is sensitive to our choice of sample. For consistency with our baseline results, the regression model is equation 1.1, with year and country fixed effects and heteroskedasticity-robust standard errors clustered at the country level.

Figures 1.6(a) - 1.10(b) show the distribution of estimated elasticities generated by iteratively dropping countries. In a few cases, outliers are far (in a sense we make precise below) from the central moments of the distributions.
Figure 1.5: $\beta$ Distribution, Commitments % GDP

(a) All years
(b) $t \geq 2000$

Figure 1.6: $\beta$ Distribution, Disbursements % GDP

(a) All years
(b) $t \geq 2000$

Figure 1.7: $\beta$ Distribution, Net ODA % GDP

(a) All years
(b) $t \geq 2000$
While graphing the distribution of densities of estimated elasticities with replacement is an intuitive approach, it is useful to formalise our definition of an outlier to avoid ad-hoc trimming of the data. We follow van de Sijpe (2010) by using the estimates of $\hat{\beta}_j$ to calculate a weighting statistic $W_{ij}$ that takes values

$$W_{ij} = \begin{cases} 1 & \text{if } \left| \frac{\hat{\beta}_j - \hat{\beta}_i}{SE_{\hat{\beta}_j}} \right| > \frac{2}{\sqrt{N_j}} \\ 0 & \text{otherwise} \end{cases}$$

where the denominator $SE = \left( \frac{\sigma_j^2 (n-1)g}{(n-k)(g-1)} \right)^{1/2}$ is the standard error scaled by $n$ sample observations in $g$ clusters (countries).

We fit the regression model only for those countries that have $W_{ij} = 0$. The choice of the cutoff value $\frac{2}{\sqrt{N_j}}$ is meaningful because choosing a value less than 2 mechanically creates a larger number of outliers. Our choice of this cutoff is based on Belsley et al. (2004), and van de Sijpe (2010) uses the same cut-off in his work on sectoral aid disbursements and commitments using data from the OECD's DAC database that is the main source for series tested here.
Table 1.5 shows countries identified as outliers, which are not the same for each measure of foreign aid due to differences in construction and coverage of each series. Excluding these outliers only slightly reduces the total sample size.

Table 1.5: Countries with $W_i > \frac{2}{\sqrt{N}}$ by aid measure

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>Countries excluded</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>Burundi, The Gambia, Guinea-Bissau</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>Burundi, The Gambia, Liberia, Sierra Leone, Zambia</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>Guinea-Bissau, Lesotho, Mauritania</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>Guinea-Bissau, Lesotho, Mauritania</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>Liberia, Solomon Islands</td>
</tr>
</tbody>
</table>

Tables 1.17 - 1.21 re-estimate the regression model of equation 1.1 for each measure of foreign aid, excluding countries with excess influence. Table 1.6 compares results for the baseline sample and if outliers are excluded. The estimated effect of aid on government spending changes only slightly, and in some cases dropping outliers increases the statistical significance of the estimate. On balance, outliers do not drive the positive relationship between aid and government spending, and are not responsible for either the size or significance of the estimated elasticity. Country programmable aid dominates alternative measures of foreign aid: these results corroborate those from the full sample.

Table 1.6: $\beta$ by Aid Measure: Fixed Effects, $t \geq 2000$

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>$\beta$, full sample</th>
<th>$\beta$, excluding outliers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>0.058*</td>
<td>0.099***</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>0.038</td>
<td>-0.009</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>0.165***</td>
<td>0.171+</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>0.151***</td>
<td>0.168**</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>0.219***</td>
<td>0.219***</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

1.3.5 Structural breaks

In addition to excluding outlier countries, we can ensure that our ranking is robust to within-country changes that violate OLS identifying assumptions. Specifically, it may be the case that the level of aid undergoes a large structural break, entailing a large, sudden change the amount of foreign aid received. Any contemporaneous change that occurs in country $i$’s government spending causes the least-squares estimator to potentially (mis)attribute variation due to a structural break to changes in aid flows.

Testing for structural breaks has generated a large body of research, including several non-parametric methods that are now popular. Chow (1960) developed
the basic theory of testing for a known structural break at some known date $\tau$. In our model, his test is equivalent to estimating $\hat{\beta}_{tj}$ for sub-samples $t = 1 \ldots \tau$ and $t = \tau + 1 \ldots T$ and testing whether the difference in estimated coefficients is significant. Since we do not have strong priors about break dates for the $N$ countries in our sample, we instead use a version of the non-parametric test developed by Andrews (1993).

For each country $i$ and aid measure $j$ in the core sample we fit the models

$$g_t = \alpha + \lambda_1 \cdot a_t + \delta \sum_{l=0}^{L} \alpha_{t-l} + \epsilon_t \quad | t = 1 \ldots \tau$$
$$g_t = \alpha + \lambda_2 \cdot a_t + \delta \sum_{l=0}^{L} \alpha_{t-l} + \epsilon_t \quad | t = \tau + 1 \ldots T$$

with $L = 4$ for all countries and aid measures for consistency, implying an AR(3) model with $k = 5$ constraints. If the break date $\tau$ is known, then Quandt (1960) shows that an $F$-type test of $H_0 : \hat{\lambda}_1 = \hat{\lambda}_2$ is asymptotically valid and distributed $\chi^2_k$. When $\tau$ is not known a priori, as in our case, the test statistic is the maximum of the vector of $F$-type statistics calculated over all $t$ for aid measure $j$ in country $i$.

The Quandt likelihood ratio (QLR) test therefore finds the largest Chow $F$-test statistic after trimming the first and last 15% of the sample as potential breakpoints. The test statistic does not have a limiting distribution because it is the maximum of $F$-statistics, so critical values are tabulated rather than derived from a probability distribution function. Stock and Watson (2003) calculate these critical values for different combinations of $k$ restrictions (i.e., AR($k-2$) models), significance levels, and amount of trimming of the underlying data. With $k = 5$ and excluding the first and last 15% of the time series, the 1% critical value is 3.36. (We only test for structural breaks in four of the five aid series because because CPA’s range of dates is too short to estimate the model after 30% trimming).

For all $F_\tau$ that exceed 3.36, we record a possible break date, and define the most likely structural break date $\tau^*$ as the year of the largest of these. Since we test each aid measure $j$ and country $i$, there is a possible total of $N \times j$ unique break dates. However, not all countries and aid measurements will have an $F$-statistic that exceeds the critical value, so in fact we identify only

$^4$The Quandt test statistic is: $F(\tau) = (T - 2k) \frac{[SSR_{1,\tau} - (SSR_{1,\tau} + SSR_{\tau+1,T})]}{(SSR_{1,\tau} + SSR_{\tau+1,T})}$

$^5$Note that this approach is inefficient; we could pool data from one or more countries if we knew that they had some common break date for an aid measure. However, this requires imposing constraints from narrative or other external evidence on the model. We therefore have chosen parsimony over efficiency
a small set of potential break dates.

Using candidate break-dates, we augment the fixed-effects model of equation 1.1 with the variable $B_t$, defined as

$$B_{ijt} = \begin{cases} 1 & t \geq \tau^* \\ 0 & t < \tau^* \end{cases}$$

and estimate $\hat{\beta}_j$, which is net of the effect of structural breaks, if any. This has the effect of estimating the elasticity in distinct sub-samples. The intuition for conditioning on sub-samples comes from calculating the average values of $a_{jit}$ and $g_{jit}$ for $t \geq \tau^*$ and $t < \tau^*$. For some aid measures in figures 1.10 - 1.13, the implied $\hat{\beta}$ is dramatically different.

**Figure 1.10**: Effect of Structural Breaks at $\tau^*$: Commitments

![Figure 1.10](image)

**Figure 1.11**: Effect of Structural Breaks at $\tau^*$: Disbursements

![Figure 1.11](image)

Since the structural break, if identified, occurs in the $t \geq 2000$ sub-sample for only very few countries, we focus on the regressions over all years available for each measure except CPA. For commitments as a share of GDP, the estimated elasticity is around 0.05 and weakly significant, but the break indicator
Figure 1.12: Effect of Structural Breaks at $\tau^*$: Net ODA

Figure 1.13: Effect of Structural Breaks at $\tau^*$: NAT
has a positive effect on government spending and is highly significant. For commitments, it appears that structural breaks are driving some aspect of the relationship between aid and government spending. This does not undermine our argument that aid measures should be transparently ranked; rather, it implies that additional, third factors may affect this ranking. Disbursements do not enter with any statistical significance, and neither do indicators of possible breaks for this aid measure.

In the case of NAT, the estimated elasticity is large (0.161) and significant at less than 1%. The break indicator for this series is significant at the same level, and the effect of the structural break is, on average, 0.029%. This implies our estimate of NAT, and its effect on government spending, is robust to conditioning on structural breaks. Finally, the effect of net ODA on government spending conditional on structural breaks and all other independent variables is 0.148. Both this point estimate and the break indicator for this series are significant at less than 1%. On balance, conditioning on likely structural breaks does not modify our ranking of measures based on their covariance with government spending. The results are collected below.

Table 1.7: $\hat{\beta}$ by Aid Measure: Conditioned On Structural Breaks,

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>$\hat{\beta}$</th>
<th>Break Indicator</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>0.053*</td>
<td>0.030**</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>0.037</td>
<td>0.013</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>0.148***</td>
<td>0.033***</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>0.161***</td>
<td>0.029***</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

1.3.6 Extreme events

Another possibility is that the observed effect is due to large shocks common to both government spending and aid. At a minimum, the ranking should be robust to conditioning on idiosyncratic natural disasters. We use data from Em-Dat (Université Catholique de Louvain, 2010), a cross-country database of natural disasters by type and total number of people killed and affected compiled from reports by multilateral agencies, non-governmental organisations, insurance firms, research bodies and press reports. These data sources collectively provide imperfect coverage of the scope and scale of natural disasters, but Em-Dat remains the standard source of disaster data.

Based on Em-Dat’s reported number of deaths due to droughts, earthquakes and seismic activity, epidemics, floods, or volcanic activity, we construct two variables: “Deaths (Em-Dat),” the log of (one plus) the number of people
estimated to have been directly affected or killed due to natural disasters for each country-year pair \( i,t \), and “Deaths (Em-Dat), Binary” which is constructed as

\[
\text{Deaths (Em-Dat), Binary}_{it} = \begin{cases} 
1 & \text{if } \text{Deaths (Em-Dat)}_{it} > 0 \\
0 & \text{otherwise}
\end{cases}
\]

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Deaths (Em-Dat), logs</td>
<td>0.915</td>
<td>1.932</td>
<td>0</td>
<td>14.221</td>
</tr>
<tr>
<td>Deaths (Em-Dat), Binary</td>
<td>0.232</td>
<td>0.422</td>
<td>0</td>
<td>1</td>
</tr>
</tbody>
</table>

Using panel regression model from 1.1, we use the data on the number of deaths as a proxy for the intensity of the natural disaster and therefore its possible effect on aid flows. For each measure of foreign aid, tables 1.31 to 1.35 show three regressions estimated by least-squares with fixed effects (calculated by within transformation of the variables).

The first column shows the effect of adding observations of deaths from natural disasters to \( X_{it} \). Since data on natural disasters for developing countries might be measured with significant error that covaries negatively with aid flows, it may obscure the effect of disasters. The second column therefore enters a binary variable in the set of conditioning information that is equal to one when Em-Dat records any deaths due to natural disasters. Finally, we include additional constraints by selecting on subsamples: the third column of each table excludes every country-year pair for which the binary measure of natural disaster deaths is equal to 1, so we only observe the effect of aid on government spending in years in which no natural disaster occurred. The fourth column estimates \( \hat{\beta}_j \) including only those years for which the binary variable is equal to 0, so we observe the effect of aid on government spending in the sub-sample of non-disaster years.

Including the number of deaths from natural disasters in \( X_{it} \) results in only one statistically significant point estimate of the effect of disasters (for CPA), and even in this case the estimated size of the effect is smaller than 0.001, so natural disaster intensity does not greatly affect government spending in our sample. For our purposes, this effectively demonstrates our ranking of aid measures is robust to conditioning on natural disasters. In the case of aid commitments in table 1.31, the point estimate for \( \hat{\beta} \) is 0.058 and weakly statistically significant. Aid commitments in the years that natural disasters occurred (the binary variable is equal to 1) are not significant, while the point-
estimate is large and significant in non-disaster years. This suggests that natural disasters are not driving the underlying relationship.

The same pattern of results holds for all aid measures; in no case does including information about natural disaster intensity contradict the underlying ranking. Conditioning on either the binary variable or the continuous measure of deaths in log points, net ODA has an estimated elasticity of around 0.15 that is highly significant, while Net Aid Transfers' $\hat{\beta}$ is around 0.16. Once again, both net ODA and net aid transfers are strictly dominated by CPA, with a $\hat{\beta}_{CPA} = 0.22$ that is significant at the < 1% level in columns 1 and 2 of table 1.35.

### 1.3.7 Alternative error structure

Throughout the paper, standard errors and thus p-values for model coefficients are calculated with a heteroscedasticity-robust variance-covariance matrix that allows for arbitrary correlation in residuals within countries over time. This may not be robust to correlation across countries. For example, patterns of political unrest or economic development may be common across regions, so both aid flows and government spending are correlated across two or more units in a given year. Similarly, the donor community may focus their efforts on regions that receive significant international media attention, driving a cross-country effect for a subset of countries in the sample.

It is useful to ensure that results are robust to this arbitrary cross-cluster correlation of the error terms. Several formal tests of so-called spatial dependence exist (since the correlation we are concerned about is across units rather than time as in the following section). Of these, the test developed by Pesaran (2004) is commonly used: a rejection of the null hypothesis means that errors are correlated across units, but this is not robust to correlation across time. Driscoll and Kraay (1998) develop an estimator that is robust to both autocorrelation and arbitrary spatial (cross-sectional) dependence. They find in a number of examples that standard errors obtained by applying their transformation to the covariance matrix are larger than those using a fixed effects estimator that is only robust clustering within each cross-sectional unit. We use Hoechle (2006)'s implementation of their estimator.

The results are collected in table 1.9, including the $t \geq 2000$ subsample for comparison with $\hat{\beta}_{CPA}$. The results remain consistent with the existing ranking of aid measures by their effect on government spending. If aid is measured by DAC members' commitments, the implied effect of aid on government spending is around 0.06 and is only weakly statistically significant, so we cannot be confident of even this relatively small level of pass-through. Disbursements
continue to have no significant effect on government spending, while both net aid transfers ($\hat{\beta} = 0.17$) and net ODA ($\hat{\beta} = 0.15$) are significant at less than 1%. Neither measure, including in the $t \geq 2000$ sub-sample, are larger than the fitted elasticity of country programmable aid of around 0.22, with an associated $p$-value of less than 1%. Cumulatively, our ranking of aid measures by their pass-through to government spending is robust to errors that are correlated both across time and across units, so spatial dependence is not driving observed patterns of significance.

Table 1.9: $\hat{\beta}$ by Aid Measure: Driscoll-Kraay Standard Errors,

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>$\hat{\beta}$</th>
<th>$t \geq 2000$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>0.058+</td>
<td>0.099***</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>0.038</td>
<td>-0.009</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>0.165***</td>
<td>0.171***</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>0.151***</td>
<td>0.168***</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>0.219***</td>
<td>0.219***</td>
</tr>
</tbody>
</table>

*** $p<0.01$, ** $p<0.05$, * $p<0.10$, + $p<0.15$

1.4 Dynamic panel methods

If the $\alpha_i$ are uncorrelated with the time-varying covariates, taking a within-transform or first-differencing removes the fixed effect so that least-squares consistently identifies the model's coefficients. FE estimation of the model requires that $E[\epsilon_{it}, \epsilon_{is}] = 0$ for all $t \neq s$, or zero serial temporal correlation in the errors (the off-diagonal elements of the variance-covariance matrix for each country $i$ are zero), which may not be satisfied in practice.

Wooldridge (2002) provides a simple test of this assumption that has been implemented by Drukker (2003). Taking first differences of the regression problem gives

$$\Delta g_{it} = \beta' \Delta (a_{it}) + \gamma' \Delta X_{it} + \Delta \epsilon_{it}$$

(1.2)

Wooldridge shows that if there is no autocorrelation in the residuals and the $\epsilon_{it}$ are identically distributed, then $\text{corr}(\Delta \epsilon_{it}, \Delta \epsilon_{it-1}) = -0.5$. Obtaining the fitted residuals $\hat{\epsilon}_{it}$ from a regression of the first differenced data, we test whether $\rho \neq -0.5$ in the regression $\hat{\Delta} \epsilon_{it} = \rho \hat{\Delta} \epsilon_{it-1} + u_{it}$. If the variance-covariance matrix for this regression allows for clustering at the country level, the test is mechanically robust to heteroscedasticity and clustering. Drukker (2003) provided Monte Carlo evidence to support the test, showing that it performs well with simulated data.

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Table 1.10 presents the test results using the differenced version of the baseline model from the core sample of countries, excluding influential outliers identified in section 1.3.4.

Table 1.10: Wooldridge-Drukker Test of Autocorrelation

<table>
<thead>
<tr>
<th>Variable</th>
<th>F-statistic</th>
<th>$\hat{p}_j$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitments, pc. GDP</td>
<td>28.6***</td>
<td>-0.12</td>
</tr>
<tr>
<td>Disbursements, pc. GDP</td>
<td>33.31***</td>
<td>-0.05</td>
</tr>
<tr>
<td>Net ODA, pc. GDP</td>
<td>31.15***</td>
<td>-0.12</td>
</tr>
<tr>
<td>Net aid transfers, pc. GDP</td>
<td>29.53***</td>
<td>-0.12</td>
</tr>
<tr>
<td>CPA, pc. GDP</td>
<td>20.22***</td>
<td>0.01</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

The test strongly rejects the null hypothesis of no autocorrelation for each measure of foreign aid. Estimated coefficients are still consistent under autocorrelation and estimating standard errors using a robust variance-covariance matrix with clustering at the country level (as we have done) means that inference is valid. However, the presence of autocorrelation in the residual terms means the FE model may be dynamically misspecified. Since the objective is to rank measures of foreign aid by their observed covariance with government spending, the next section tests the robustness of the estimated size and sign of the elasticity by including dynamic terms.

1.4.1 Dynamic regression framework

Since government expenditure is relatively stable over time, excluding a lagged dependent variable from the model of equation 1.1 could (mis)attribute variation in government spending to foreign aid that is just due to the persistence of fiscal policy. Formally,

$$g_{it} = \alpha_t + \phi g_{it-1} + \beta' a_{jt} + \gamma' X_{it} + \eta_t + \epsilon_{it}$$  \hspace{1cm} (1.3)

The within groups transformation no longer mechanically ensures the orthogonality condition is satisfied, since the transformed lagged dependent variable is $\Delta g_{it} = g_{it-1} - (T - 1)^{-1}(\sum_{t=1}^{T-1} g_{it})$ which has non-zero covariance with the transformed error term $\Delta \epsilon_{it} = \epsilon_{it} - (T - 1)^{-1}(\sum_{t=1}^{T-1} \epsilon_{it})$. Similarly, estimating intercept coefficients for each country by OLS is inconsistent because $g_{it-1}$ is positively correlated with the error term through the fixed effect. However, applying the first difference operator to each term as in equation 1.2 removes the fixed effect, resulting in a system of two equations- one in levels, one in differences- on which additional assumptions can be imposed to generate consistent estimates of $\hat{\beta}$ and $\hat{\phi}$. 

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Holtz-Eakin et al. (1988)'s insight is that while $\Delta g_{t-1} = (g_{t-1} - g_{t-2})$ is endogenous with respect to the error $\Delta \epsilon_{it}$, if the errors are not serially correlated, "deeper" lags $g_{t-3} \ldots g_T$ are valid instruments for the differenced equation. Here, we believe that aid and government spending $g_{it}$ are correlated with contemporaneous errors so we instrument them with their lags.$^6$

Using lagged levels of variables may not generate a matrix of instruments that is strongly correlated with the endogenous variables in differences, so Arellano and Bond (1991) and Arellano and Bover (1995) propose supplementing the equation in differences with the equation 1.3 in levels, and using differenced variables as instruments for endogenous regressors in levels. The goal of using a system of equations and an expanded instrument matrix is to avoid the weak instruments problem of low, albeit statistically significant, correlations between instruments and endogenous variables. Roodman (2006) discusses the system GMM estimator and provides the standard pedagogical summary of instrumenting for endogenous variables in a GMM framework.

Windmeijer (2005) uses simulation evidence to demonstrate that standard errors of GMM estimates are biased downwards in finite samples, increasing the risk of type II errors. All standard errors reported here therefore use the Windmeijer correction for the difference between the finite sample and asymptotic variance of the system.

Similarly, low computing cost and the nature of the GMM solution to endogeneity of government spending and aid flows allow researchers to trivially generate very large matrices of instrumental variables: one for each variable, lag distance, and time period. As Roodman (2006) points out, as the number of instruments grows arbitrarily large for panels of even moderate $T$, the instruments cannot extract the exogenous variation from the endogenous variables. Unfortunately, the literature on dynamic panel methods has yet to develop a rigorous answer to this problem, but Roodman proposes a rule of thumb: the number of instruments should be fewer than the total number of groups (excluding outliers with excess weight, this is roughly 30 in our case). We collapse the set of instruments so that only one is generated per endogenous variable and lag length. All of the regressions using the dynamic model report the number of instruments used.

The estimates are accompanied by three test statistics. Firstly, the Hansen "$J$-test" of overidentifying restrictions, with a $H_0$ that the set of instruments are exogenous (the lagged differences do not belong in the levels equation and

---

$^6$With more instruments than parameters to be estimated, this approach folds naturally into a method-of-moments approach, so that we determine a weighting matrix by minimising the quadratic distance of the moment conditions with respect to the observed data.
the lagged levels do not belong in the equation in differences). This Hansen test statistic is robust to heteroscedasticity but loses power as the number of instruments becomes large, providing another reason to collapse the instrument set. Secondly, tests of first- and second-order autocorrelation of the fitted residuals are presented. While we do not observe the error \( \epsilon_{it} \), we should fail to reject the null of at least absence of second-order autocorrelation in the fitted residuals \( \bar{\epsilon}_{it} \) because this is a necessary assumption for the consistency of the system GMM estimator. (If second order autocorrelation were present, we would need to take further differences of equation 1.2).

As expected, including a lagged independent variable in the set of independent information dramatically reduces the estimated effect of aid. Commitments as a share of GDP generate a \( \hat{\beta} \) of 0.022, which is statistically significant at the 5% level. Disbursements, consistent with most of the other specifications presented here, do not vary significantly with government spending, suggesting at a minimum that this series does not proxy well for aid commitments; whether this is because aid that is committed is not actually disbursed (either at all or at the levels recorded), or due to issues with data reporting by the DAC, is not clear. However, the point-estimate for net aid transfers (NAT) is 0.054 and highly statistically significant, and net ODA is also significant at less than 1%, with \( \hat{\beta} = 0.036 \). Finally, and consistent with our ranking results so far, country programmable aid has the highest estimated elasticity, and this point-estimate is significant at comparable levels to that for both net ODA and NAT. While a dynamic specification causes the ranking to converge, CPA still has a larger estimated effect on government spending than other measures, and net ODA, NAT, and CPA strictly dominate commitments or disbursements. Table 1.11 collects the estimates produced individually in tables 1.36 - 1.40.

Table 1.11: \( \hat{\beta} \) by Aid Measure: System GMM

<table>
<thead>
<tr>
<th>Aid measure</th>
<th>( \hat{\beta} )</th>
<th>J-test</th>
<th>AR(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commitment, % GDP</td>
<td>0.022**</td>
<td>0.312</td>
<td>-1.743</td>
</tr>
<tr>
<td>Disbursement, % GDP</td>
<td>0.011</td>
<td>0.835</td>
<td>-1.720</td>
</tr>
<tr>
<td>Net ODA, % GDP</td>
<td>0.036***</td>
<td>0.189</td>
<td>-1.598</td>
</tr>
<tr>
<td>NAT, % GDP</td>
<td>0.054***</td>
<td>0.0574</td>
<td>-1.797</td>
</tr>
<tr>
<td>CPA, % GDP</td>
<td>0.057**</td>
<td>0.610</td>
<td>-0.394</td>
</tr>
</tbody>
</table>

*J-test* report p-value of test statistic

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

\(^7\)We report this heteroscedasticity-robust statistic because a likelihood ratio test of \(-1\ln \psi \sim \chi^2_{N - 1}\) where \( \psi = \prod_{t=1}^{T} \sigma_t / \hat{\sigma} \) rejects the null hypothesis that the distribution of errors is the same across the sample at less than 1% in all series of aid data.
1.5 Discussion and conclusion

Section 1.1.1 presents key contributions from the influential economic literature on the link between foreign aid, government spending, and development outcomes. Conspicuous by its absence from this literature is a cross-country comparison of how the various formulations of foreign aid affect spending by recipient states. In a simple thought experiment, we argued that if researchers evaluate foreign aid based on aid flows that consist mainly of, for example, debt forgiveness, we should not be surprised that aid has little effect on outcomes. Similarly, in policy discourse concerning donors' commitments under the declarations at Paris and Accra, we may be better served by evaluating progress toward those goals using indicators of foreign aid that are observed to actually affect recipients governments' fiscal situations.

This paper takes a simple approach to determining what we should be talking about when we talk about foreign aid, based on a subjective claim that aid is meaningful to recipients to the extent that it affects their expenditure patterns. We test this effect when aid is measured variously by commitments, disbursements, net ODA, net aid transfers, and country programmable aid using a range of dynamic and static models under a variety of alternative specifications and extensions to the set of conditioning information, including clustered standard errors, country and annual fixed effects, Driscoll-Kraay clustered errors, structural breaks and sensitivity to outliers or extreme events.

In every specification and test of robustness, we find that country programmable aid (CPA) has the largest estimated effect on government spending, followed by net ODA or NAT and commitments, while aid disbursements has a very small and often statistically insignificant effect on government expenditure. This is an interesting finding that suggests that CPA is the proper measure of what constitutes foreign aid if we are interested how much aid affects recipient governments' budgets. From the perspective of aid policymakers and public discourse, this measure will be more useful than evaluation based on aid commitments or other measures currently popular in the existing literature.

Our conclusion also has implications for the aid-outcomes research agenda. Most importantly, while CPA does not explicitly net out reverse financial flows like debt repayments, it is observed to have a larger effect on government spending, implying that giving stakeholders meaningful control over aid programming increases the effect of aid on government spending beyond simply increasing aid flows. More generally, studies in which government spending mediates the relationship between aid and outcomes should at a minimum be wary of using measures of foreign aid that, as seen here, have not histori-
cally caused large changes in government spending. Because these measures of aid are significantly but weakly correlated with government spending, they potentially induce a form of the “weak instruments” problem: they covary significantly but weakly with spending and are therefore likely poor predictors of outcomes.

This is preliminary but strong evidence that aid modalities matter more for recipient government spending than aid levels; contemporary policy debates about aid levels cannot- and should not- be disentangled from a transparent understanding of how aid flows are defined, disbursed, and programmed. The robustness of our ranking is surprising, and suggests applied researchers in some cases will benefit by exploiting country programmable aid in preference to other measures, particularly as the length of the time series increases. For the marginal aid flow in the current development cooperation regime, how apparently matters more than how much.
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Disbursements, pc. GDP</td>
<td>0.070</td>
<td>0.083</td>
<td>0.072***</td>
</tr>
<tr>
<td></td>
<td>(0.065)</td>
<td>(0.051)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.121*</td>
<td>-0.096+</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.063)</td>
<td>(0.072)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.047**</td>
<td>0.038*</td>
<td>0.077***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.021)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.038</td>
<td>-0.136+</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.092)</td>
<td>(0.122)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.145</td>
<td>-0.167</td>
<td>0.549**</td>
</tr>
<tr>
<td></td>
<td>(0.267)</td>
<td>(0.267)</td>
<td>(0.218)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.117*</td>
<td>0.104*</td>
<td>0.135**</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.059)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.086</td>
<td>-0.061</td>
<td>-0.181***</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.077)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.057</td>
<td>-0.060</td>
<td>-0.159**</td>
</tr>
<tr>
<td></td>
<td>(0.074)</td>
<td>(0.070)</td>
<td>(0.077)</td>
</tr>
<tr>
<td>Observations</td>
<td>682</td>
<td>682</td>
<td>300</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.117</td>
<td>0.195</td>
<td>0.295</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
<td>36</td>
<td>36</td>
<td>36</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.13

Baseline model

<table>
<thead>
<tr>
<th>VARIABLES</th>
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<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>VARIABLES</td>
<td>$t \geq 2000$</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Commitments, pc. GDP</td>
<td>0.071*</td>
<td>0.066*</td>
<td>0.098***</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.035)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.170**</td>
<td>-0.148**</td>
<td>-0.026</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.064)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.064***</td>
<td>0.061**</td>
<td>0.085**</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.023)</td>
<td>(0.038)</td>
</tr>
<tr>
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<td>-0.051</td>
<td>0.048</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.059)</td>
<td>(0.099)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.125</td>
<td>-0.100</td>
<td>0.410**</td>
</tr>
<tr>
<td></td>
<td>(0.223)</td>
<td>(0.240)</td>
<td>(0.192)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.092+</td>
<td>0.079</td>
<td>0.092*</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.058)</td>
<td>(0.051)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.118</td>
<td>-0.101</td>
<td>-0.171***</td>
</tr>
<tr>
<td></td>
<td>(0.088)</td>
<td>(0.090)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.081</td>
<td>-0.088</td>
<td>-0.179**</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.081)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>Observations</td>
<td>969</td>
<td>969</td>
<td>314</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.110</td>
<td>0.151</td>
<td>0.369</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>Number of countries</td>
<td>36</td>
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<td>36</td>
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</tbody>
</table>

Robust standard errors in parentheses

### ** p<0.01, ** p<0.05, * p<0.10, + p<0.15
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>( t \geq 2000 )</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net aid transfers, pc. GDP</td>
<td>0.125+</td>
<td>0.113</td>
<td>0.186*</td>
</tr>
<tr>
<td></td>
<td>(0.079)</td>
<td>(0.089)</td>
<td>(0.100)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.134*</td>
<td>-0.117*</td>
<td>-0.063</td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.068)</td>
<td>(0.069)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.081***</td>
<td>0.077**</td>
<td>0.100**</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.030)</td>
<td>(0.038)</td>
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<td>Population, logs</td>
<td>-0.016</td>
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<td>-0.023</td>
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<td>(0.038)</td>
<td>(0.065)</td>
<td>(0.109)</td>
</tr>
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<td>-0.134</td>
<td>-0.124</td>
<td>0.331+</td>
</tr>
<tr>
<td></td>
<td>(0.226)</td>
<td>(0.247)</td>
<td>(0.196)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.099**</td>
<td>0.080*</td>
<td>0.059</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.041)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.128</td>
<td>-0.110</td>
<td>-0.111***</td>
</tr>
<tr>
<td></td>
<td>(0.097)</td>
<td>(0.096)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.061</td>
<td>-0.057</td>
<td>-0.074</td>
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<tr>
<td></td>
<td>(0.067)</td>
<td>(0.070)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Observations</td>
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<td>934</td>
<td>291</td>
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<tr>
<td>R-squared</td>
<td>0.126</td>
<td>0.172</td>
<td>0.288</td>
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<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
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<td>Y</td>
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<tr>
<td>Number of countries</td>
<td>33</td>
<td>33</td>
<td>33</td>
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</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.15

Baseline model

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Variables</td>
<td></td>
<td></td>
<td>( t \geq 2000 )</td>
</tr>
<tr>
<td>Net ODA, pc. GDP</td>
<td>0.098</td>
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<td>0.182***</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.157**</td>
<td>-0.141**</td>
<td>-0.046</td>
</tr>
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<td>Real GDP per capita, logs</td>
<td>0.072***</td>
<td>0.070**</td>
<td>0.092**</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.021</td>
<td>-0.048</td>
<td>0.005</td>
</tr>
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<td>Urban population, pc. total</td>
<td>-0.113</td>
<td>-0.085</td>
<td>0.419**</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.101**</td>
<td>0.084**</td>
<td>0.088**</td>
</tr>
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<td>Exports, pc. GDP</td>
<td>-0.122+</td>
<td>-0.105</td>
<td>-0.161***</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
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<td>-0.092</td>
<td>-0.212***</td>
</tr>
<tr>
<td>Observations</td>
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<td>987</td>
<td>321</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.124</td>
<td>0.169</td>
<td>0.354</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
<td>36</td>
<td>36</td>
<td>36</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CPA, pc. GDP</td>
<td>0.267***</td>
<td>0.261***</td>
<td>0.261***</td>
</tr>
<tr>
<td></td>
<td>(0.085)</td>
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<td>Real GDP per capita, logs</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.17

Excluding outliers

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<td>-0.168**</td>
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<td>(0.067)</td>
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<td>Real GDP per capita, logs</td>
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<td>0.073***</td>
<td>0.090**</td>
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<td>(0.021)</td>
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<td>-0.089</td>
<td>-0.168**</td>
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<tr>
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<td>(0.081)</td>
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Robust standard errors in parentheses

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<td>-0.126+</td>
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<td>(0.082)</td>
<td>(0.104)</td>
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<td>0.062+</td>
<td>0.078**</td>
<td>0.080**</td>
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<td>(0.038)</td>
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<td>0.011</td>
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<td>(0.076)</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.19

Excluding outliers

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<td>t$^2$</td>
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<td>(0.073)</td>
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<td>0.079***</td>
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Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.10$, + $p<0.15$
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<td>0.086***</td>
<td>0.100*</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.21

Excluding outliers

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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.22
Ordinary Least Squares
Conditioning on Structural Breaks

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<td>0.053+</td>
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<td>(0.032)</td>
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<td>(0.011)</td>
<td>(0.014)</td>
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<td></td>
<td>(0.065)</td>
<td>(0.067)</td>
<td>(0.058)</td>
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<td>Real GDP per capita, logs</td>
<td>0.061***</td>
<td>0.073***</td>
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<td>(0.020)</td>
<td>(0.040)</td>
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<td>Urban population, pc. total</td>
<td>0.047</td>
<td>0.103</td>
<td>0.398*</td>
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<tr>
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<td>(0.297)</td>
<td>(0.203)</td>
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<td>Imports, pc. GDP</td>
<td>0.079</td>
<td>0.079</td>
<td>0.081+</td>
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<td>(0.059)</td>
<td>(0.061)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.105</td>
<td>-0.095</td>
<td>-0.164***</td>
</tr>
<tr>
<td></td>
<td>(0.086)</td>
<td>(0.091)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.058</td>
<td>-0.084</td>
<td>-0.168**</td>
</tr>
<tr>
<td></td>
<td>(0.074)</td>
<td>(0.081)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Observations</td>
<td>872</td>
<td>872</td>
<td>295</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.110</td>
<td>0.165</td>
<td>0.360</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.23

Ordinary Least Squares
Conditioning on Structural Breaks

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<th>VARIABLES</th>
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</tr>
</thead>
<tbody>
<tr>
<td>Disbursements, pc. GDP</td>
<td>-0.014</td>
<td>0.037</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.139)</td>
<td>(0.086)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Break Indicator</td>
<td>0.015</td>
<td>0.013</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.025)</td>
<td></td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.148*</td>
<td>-0.127+</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
<td>(0.082)</td>
<td>(0.104)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.063+</td>
<td>0.079**</td>
<td>0.080**</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.038)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>0.017</td>
<td>0.028</td>
<td>0.073</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.079)</td>
<td>(0.140)</td>
</tr>
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<td>-0.356</td>
<td>-0.334</td>
<td>0.682***</td>
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<tr>
<td></td>
<td>(0.468)</td>
<td>(0.454)</td>
<td>(0.247)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.133*</td>
<td>0.122</td>
<td>0.173**</td>
</tr>
<tr>
<td></td>
<td>(0.078)</td>
<td>(0.095)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.079</td>
<td>-0.066</td>
<td>-0.217***</td>
</tr>
<tr>
<td></td>
<td>(0.092)</td>
<td>(0.104)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>0.065</td>
<td>0.009</td>
<td>-0.103</td>
</tr>
<tr>
<td></td>
<td>(0.148)</td>
<td>(0.166)</td>
<td>(0.118)</td>
</tr>
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<td>Observations</td>
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<td>559</td>
<td>258</td>
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<tr>
<td>R-squared</td>
<td>0.120</td>
<td>0.180</td>
<td>0.327</td>
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<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
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</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.24

Ordinary Least Squares
Conditioning on Structural Breaks

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<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Net aid transfers, pc. GDP</td>
<td>0.182***</td>
<td>0.161***</td>
<td>0.170+</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.052)</td>
<td>(0.105)</td>
</tr>
<tr>
<td>Break Indicator</td>
<td>0.029***</td>
<td>0.029***</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.085</td>
<td>-0.060</td>
<td>-0.062</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td>(0.069)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.073***</td>
<td>0.080***</td>
<td>0.105**</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.024)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.031</td>
<td>-0.033</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.051)</td>
<td>(0.110)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.005</td>
<td>0.074</td>
<td>0.229</td>
</tr>
<tr>
<td></td>
<td>(0.174)</td>
<td>(0.181)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.083**</td>
<td>0.093***</td>
<td>0.069</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.033)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.191***</td>
<td>-0.185***</td>
<td>-0.093*</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.050)</td>
<td>(0.051)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.123***</td>
<td>-0.141**</td>
<td>-0.075</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.054)</td>
<td>(0.081)</td>
</tr>
<tr>
<td>Observations</td>
<td>823</td>
<td>823</td>
<td>268</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.189</td>
<td>0.263</td>
<td>0.286</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
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Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.25

Ordinary Least Squares
Conditioning on Structural Breaks

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<td>(t ≥ 2000)</td>
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<tr>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Net ODA, pc. GDP</td>
<td>0.159***</td>
<td>0.148***</td>
<td>0.170**</td>
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<tr>
<td></td>
<td>(0.056)</td>
<td>(0.048)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>Break Indicator</td>
<td>0.034***</td>
<td>0.033***</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.008)</td>
<td>(0.011)</td>
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<td>GDP growth, annual pc.</td>
<td>-0.119*</td>
<td>-0.087</td>
<td>-0.046</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.063)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.062***</td>
<td>0.073***</td>
<td>0.098***</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.022)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.038</td>
<td>-0.029</td>
<td>0.046</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.049)</td>
<td>(0.106)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>0.022</td>
<td>0.110</td>
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<tr>
<td></td>
<td>(0.172)</td>
<td>(0.170)</td>
<td>(0.196)</td>
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<tr>
<td>Imports, pc. GDP</td>
<td>0.078**</td>
<td>0.089***</td>
<td>0.100**</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.029)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.165***</td>
<td>-0.162***</td>
<td>-0.162***</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.034)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.170***</td>
<td>-0.190***</td>
<td>-0.210***</td>
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<tr>
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<td>(0.055)</td>
<td>(0.057)</td>
<td>(0.077)</td>
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<td>Observations</td>
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<td>876</td>
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<td>R-squared</td>
<td>0.187</td>
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<td>Country FE</td>
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<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
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</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.26

Driscoll-Kraay Standard Errors

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<td>Commitments, pc. GDP</td>
<td>0.055*</td>
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<td>(0.032)</td>
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<td>(0.020)</td>
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<td>GDP growth, annual pc.</td>
<td>-0.168***</td>
<td>-0.133***</td>
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<td>(0.035)</td>
<td>(0.040)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.062***</td>
<td>0.073***</td>
<td>0.090***</td>
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<tr>
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<td>(0.010)</td>
<td>(0.010)</td>
<td>(0.015)</td>
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<td>0.398**</td>
</tr>
<tr>
<td></td>
<td>(0.100)</td>
<td>(0.113)</td>
<td>(0.124)</td>
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<td>Imports, pc. GDP</td>
<td>0.080*</td>
<td>0.078*</td>
<td>0.081***</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.039)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.105**</td>
<td>-0.093**</td>
<td>-0.164**</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.043)</td>
<td>(0.054)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.065+</td>
<td>-0.089**</td>
<td>-0.168**</td>
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<tr>
<td></td>
<td>(0.044)</td>
<td>(0.043)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>Observations</td>
<td>872</td>
<td>872</td>
<td>295</td>
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<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>Annual FE</td>
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<td>Y</td>
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Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
<table>
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<th>VARIABLES</th>
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<th>(3)</th>
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<td>0.038</td>
<td>-0.009</td>
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<td></td>
<td>(0.072)</td>
<td>(0.070)</td>
<td>(0.031)</td>
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<td>GDP growth, annual pc.</td>
<td>-0.147***</td>
<td>-0.126***</td>
<td>-0.016</td>
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<td>(0.038)</td>
<td>(0.042)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.062**</td>
<td>0.078**</td>
<td>0.080***</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.029)</td>
<td>(0.020)</td>
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<td>0.073+</td>
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<td>(0.056)</td>
<td>(0.058)</td>
<td>(0.042)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.339</td>
<td>-0.319</td>
<td>0.682***</td>
</tr>
<tr>
<td></td>
<td>(0.328)</td>
<td>(0.384)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.134***</td>
<td>0.123***</td>
<td>0.173***</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.041)</td>
<td>(0.046)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.081***</td>
<td>-0.067**</td>
<td>-0.217***</td>
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<tr>
<td></td>
<td>(0.030)</td>
<td>(0.036)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
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<td>0.009</td>
<td>-0.103</td>
</tr>
<tr>
<td></td>
<td>(0.114)</td>
<td>(0.132)</td>
<td>(0.080)</td>
</tr>
<tr>
<td>Observations</td>
<td>559</td>
<td>559</td>
<td>258</td>
</tr>
<tr>
<td>Country FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Number of countries</td>
<td>31</td>
<td>31</td>
<td>31</td>
</tr>
</tbody>
</table>

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Net aid transfers, pc. GDP</td>
<td>0.184***</td>
<td>0.165***</td>
<td>0.171**</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
<td>(0.059)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.087**</td>
<td>-0.062</td>
<td>-0.059</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.043)</td>
<td>(0.051)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.072***</td>
<td>0.079***</td>
<td>0.106***</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.016)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.031*</td>
<td>-0.032</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.028)</td>
<td>(0.024)</td>
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<td>-0.008</td>
<td>0.071</td>
<td>0.227*</td>
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<tr>
<td></td>
<td>(0.094)</td>
<td>(0.102)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.083***</td>
<td>0.092***</td>
<td>0.069*</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.016)</td>
<td>(0.043)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.192***</td>
<td>-0.184***</td>
<td>-0.094*</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.021)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.125***</td>
<td>-0.142***</td>
<td>-0.076</td>
</tr>
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Standard errors in parentheses
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Standard errors in parentheses

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Standard errors in parentheses

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<td>(0.004)</td>
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<td>-0.164**</td>
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<td>(0.066)</td>
<td>(0.072)</td>
<td>(0.050)</td>
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<td>0.073***</td>
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Robust standard errors in parentheses

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Table 1.32
Conditioning on Natural Disasters

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<td>Disbursements, pc. GDP</td>
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<td>-0.129+ (0.082)</td>
<td>-0.130+ (0.082)</td>
<td>-0.175* (0.103)</td>
<td>-0.086 (0.097)</td>
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<tr>
<td>Real GDP per capita, logs</td>
<td>0.078** (0.037)</td>
<td>0.077** (0.037)</td>
<td>0.096** (0.047)</td>
<td>0.038* (0.020)</td>
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<td>0.018 (0.082)</td>
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<td>0.073 (0.108)</td>
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<td>0.121 (0.094)</td>
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Robust standard errors in parentheses
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51
Table 1.33
Conditioning on Natural Disasters

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<td>0.078***</td>
<td>0.069***</td>
<td>0.044</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
**Table 1.34**

Conditioning on Natural Disasters

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<th>(4)</th>
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<td>0.150***</td>
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<td>0.071***</td>
<td>0.071***</td>
<td>0.065***</td>
<td>0.044+</td>
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<td>(0.022)</td>
<td>(0.020)</td>
<td>(0.029)</td>
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<td>-0.034</td>
<td>-0.039</td>
<td>-0.022</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.049)</td>
<td>(0.048)</td>
<td>(0.085)</td>
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<tr>
<td>Urban population, pc. total</td>
<td>0.119</td>
<td>0.121</td>
<td>0.064</td>
<td>0.308+</td>
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<td></td>
<td>(0.166)</td>
<td>(0.166)</td>
<td>(0.158)</td>
<td>(0.186)</td>
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<td>Imports, pc. GDP</td>
<td>0.086***</td>
<td>0.084***</td>
<td>0.104***</td>
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<td>(0.030)</td>
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<td>Exports, pc. GDP</td>
<td>-0.159***</td>
<td>-0.158***</td>
<td>-0.181***</td>
<td>-0.132***</td>
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<tr>
<td></td>
<td>(0.034)</td>
<td>(0.034)</td>
<td>(0.045)</td>
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<tr>
<td>Debt service, pc. GDP</td>
<td>-0.190***</td>
<td>-0.190***</td>
<td>-0.173***</td>
<td>-0.144**</td>
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<td>(0.058)</td>
<td>(0.059)</td>
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<tr>
<td>Observations</td>
<td>876</td>
<td>876</td>
<td>614</td>
<td>262</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.263</td>
<td>0.264</td>
<td>0.292</td>
<td>0.476</td>
</tr>
<tr>
<td>Country FE</td>
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<td>Y</td>
<td></td>
</tr>
<tr>
<td>Annual FE</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>N</td>
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<td>Number of countries</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
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<th>(3)</th>
<th>(4)</th>
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<tr>
<td>CPA, pc. GDP</td>
<td>0.212***</td>
<td>0.217***</td>
<td>0.098</td>
<td>0.226***</td>
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<td></td>
<td>(0.061)</td>
<td>(0.061)</td>
<td>(0.088)</td>
<td>(0.073)</td>
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<tr>
<td>Deaths (Em-Dat), logs</td>
<td>-0.001*</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.001)</td>
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<td>Deaths (Em-Dat), Binary</td>
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<tr>
<td>GDP growth, annual pc.</td>
<td>-0.087</td>
<td>-0.086</td>
<td>-0.109</td>
<td>-0.105</td>
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<tr>
<td></td>
<td>(0.078)</td>
<td>(0.080)</td>
<td>(0.079)</td>
<td>(0.072)</td>
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<tr>
<td>Real GDP per capita, logs</td>
<td>0.085**</td>
<td>0.086**</td>
<td>0.066*</td>
<td>0.109***</td>
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<tr>
<td></td>
<td>(0.033)</td>
<td>(0.033)</td>
<td>(0.036)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.000</td>
<td>-0.002</td>
<td>-0.139</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.095)</td>
<td>(0.142)</td>
<td>(0.128)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>0.404*</td>
<td>0.409*</td>
<td>0.790***</td>
<td>-0.240</td>
</tr>
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<td></td>
<td>(0.216)</td>
<td>(0.218)</td>
<td>(0.207)</td>
<td>(0.273)</td>
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<tr>
<td>Imports, pc. GDP</td>
<td>0.128**</td>
<td>0.125**</td>
<td>0.142**</td>
<td>0.064+</td>
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<tr>
<td></td>
<td>(0.053)</td>
<td>(0.053)</td>
<td>(0.062)</td>
<td>(0.041)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.200***</td>
<td>-0.196***</td>
<td>-0.220***</td>
<td>-0.174***</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.048)</td>
<td>(0.066)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>0.027</td>
<td>0.024</td>
<td>-0.279+</td>
<td>0.391*</td>
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<td></td>
<td>(0.137)</td>
<td>(0.138)</td>
<td>(0.182)</td>
<td>(0.214)</td>
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<td>Observations</td>
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<td>149</td>
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<td>R-squared</td>
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<td>0.383</td>
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<td>Country FE</td>
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<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Annual FE</td>
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<td>N</td>
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</tr>
<tr>
<td>Number of countries</td>
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<td>34</td>
<td>32</td>
<td>30</td>
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</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 1.36

System GMM Estimation

<table>
<thead>
<tr>
<th>VARIABLES</th>
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<th>(2)</th>
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</thead>
<tbody>
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<td>GMM</td>
<td></td>
<td></td>
</tr>
<tr>
<td>t ≥ 2000</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Gov't exp., pc. GDP&lt;sub&gt;_t−1&lt;/sub&gt;</th>
<th>0.564***</th>
<th>0.661***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>Commitments, pc. GDP</td>
<td>0.022**</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.076***</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.008***</td>
<td>-0.005*</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.003*</td>
<td>-0.007***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.033***</td>
<td>-0.038***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.078***</td>
<td>0.092***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.036***</td>
<td>-0.065***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.035</td>
<td>-0.073**</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.035)</td>
</tr>
</tbody>
</table>

| Observations                       | 961     | 311     |
| Instruments                        | 50      | 21      |
| J-test (p-value)                   | 0.312   | 0.612   |
| AR(1)                              | -6.592  | -3.928  |
| AR(1) p-value                      | 0       | 0       |
| AR(2)                              | -1.743  | -1.321  |
| AR(2) p-value                      | 0.112   | 0.132   |

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1
Table 1.37

System GMM Estimation

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) GMM</th>
<th>(2) GMM</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gov't exp., pc. GDP$_{t-1}$</td>
<td>0.457*** (0.108)</td>
<td>0.686*** (0.070)</td>
</tr>
<tr>
<td>Disbursements, pc. GDP</td>
<td>0.011 (0.025)</td>
<td>0.017 (0.023)</td>
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<tr>
<td>GDP growth, annual pc.</td>
<td>-0.060*** (0.020)</td>
<td>-0.031 (0.031)</td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>-0.001 (0.002)</td>
<td>-0.004 (0.003)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.007*** (0.002)</td>
<td>-0.006*** (0.002)</td>
</tr>
<tr>
<td>Urban population, pc. total</td>
<td>-0.040*** (0.009)</td>
<td>-0.035*** (0.010)</td>
</tr>
<tr>
<td>Imports, pc. GDP</td>
<td>0.117*** (0.018)</td>
<td>0.085*** (0.019)</td>
</tr>
<tr>
<td>Exports, pc. GDP</td>
<td>-0.060*** (0.011)</td>
<td>-0.063*** (0.015)</td>
</tr>
<tr>
<td>Debt service, pc. GDP</td>
<td>-0.056** (0.024)</td>
<td>-0.065* (0.036)</td>
</tr>
</tbody>
</table>

Observations: 676 297

Instruments: 47 21

J-test (p-value): 0.835 0.669

AR(1): -3.583 -3.998

AR(1) p-value: 0 0

AR(2): -1.720 -1.196

AR(2) p-value: 0.157 0.192

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1
Table 1.38
System GMM Estimation

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) GMM</th>
<th>(2) GMM</th>
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</thead>
<tbody>
<tr>
<td>Gov't exp., pc. GDP&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>0.583***</td>
<td>0.652***</td>
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<tr>
<td>(0.079)</td>
<td>(0.073)</td>
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<td>Net aid transfers, pc. GDP</td>
<td>0.054***</td>
<td>0.037*</td>
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<tr>
<td>(0.014)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>GDP growth, annual pc.</td>
<td>-0.057***</td>
<td>-0.022</td>
</tr>
<tr>
<td>(0.021)</td>
<td>(0.032)</td>
<td></td>
</tr>
<tr>
<td>Real GDP per capita, logs</td>
<td>0.010***</td>
<td>-0.004</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.003)</td>
<td></td>
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<tr>
<td>Population, logs</td>
<td>-0.001</td>
<td>-0.006***</td>
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<td>(0.001)</td>
<td>(0.002)</td>
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<td>Urban population, pc. total</td>
<td>-0.025***</td>
<td>-0.035***</td>
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<tr>
<td>(0.008)</td>
<td>(0.009)</td>
<td></td>
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<td>Imports, pc. GDP</td>
<td>0.074***</td>
<td>0.087***</td>
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<td>(0.011)</td>
<td>(0.022)</td>
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<td>Exports, pc. GDP</td>
<td>-0.024***</td>
<td>-0.050***</td>
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<td>(0.008)</td>
<td>(0.016)</td>
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<tr>
<td>Debt service, pc. GDP</td>
<td>-0.037*</td>
<td>-0.073**</td>
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<td>(0.021)</td>
<td>(0.032)</td>
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<td>Observations</td>
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<td>J-test (p-value)</td>
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<td>0.604</td>
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<tr>
<td>AR(1)</td>
<td>-6.669</td>
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<td>0</td>
<td>0</td>
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<tr>
<td>AR(2)</td>
<td>-1.797</td>
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<td>AR(2) p-value</td>
<td>0.112</td>
<td>0.241</td>
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Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
<table>
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<th>VARIABLES</th>
<th>(1) GMM</th>
<th>(2) GMM</th>
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</thead>
<tbody>
<tr>
<td>Gov't exp., pc. GDP&lt;sub&gt;_t-1&lt;/sub&gt;</td>
<td>0.593*** (0.076)</td>
<td>0.681*** (0.069)</td>
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<td>Net ODA, pc. GDP</td>
<td>0.036*** (0.013)</td>
<td>0.043** (0.018)</td>
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<td>GDP growth, annual pc.</td>
<td>-0.071*** (0.019)</td>
<td>-0.032 (0.030)</td>
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<tr>
<td>Real GDP per capita, logs</td>
<td>0.010*** (0.002)</td>
<td>-0.001 (0.003)</td>
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<td>Population, logs</td>
<td>-0.001 (0.001)</td>
<td>-0.005*** (0.002)</td>
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<td>Urban population, pc. total</td>
<td>-0.030*** (0.008)</td>
<td>-0.035*** (0.009)</td>
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<td>Imports, pc. GDP</td>
<td>0.072*** (0.011)</td>
<td>0.084*** (0.019)</td>
</tr>
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<td>Exports, pc. GDP</td>
<td>-0.031*** (0.008)</td>
<td>-0.057*** (0.014)</td>
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<td>Debt service, pc. GDP</td>
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<td>-0.091*** (0.035)</td>
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<td>AR(1) p-value</td>
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<td>AR(2) p-value</td>
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<td>AR(2) p-value</td>
<td>-1.598</td>
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Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1
Table 1.40

System GMM Estimation

<table>
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<th>VARIABLES</th>
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<td></td>
</tr>
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<td>Gov't exp., pc. GDP&lt;sub&gt;_t−1&lt;/sub&gt;</td>
<td>0.711***</td>
<td>0.711***</td>
</tr>
<tr>
<td>(0.063)</td>
<td>(0.063)</td>
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<tr>
<td>CPA, pc. GDP</td>
<td>0.057**</td>
<td>0.057**</td>
</tr>
<tr>
<td>(0.023)</td>
<td>(0.023)</td>
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<td>-0.044</td>
<td>-0.044</td>
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<tr>
<td>(0.029)</td>
<td>(0.029)</td>
<td></td>
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<tr>
<td>Real GDP per capita, logs</td>
<td>-0.002</td>
<td>-0.002</td>
</tr>
<tr>
<td>(0.003)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.004**</td>
<td>-0.004**</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.002)</td>
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</tr>
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<td>Urban population, pc. total</td>
<td>-0.029***</td>
<td>-0.029***</td>
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<td>(0.009)</td>
<td>(0.009)</td>
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<tr>
<td>Imports, pc. GDP</td>
<td>0.074***</td>
<td>0.074***</td>
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<td>(0.018)</td>
<td>(0.018)</td>
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<td>-0.050***</td>
<td>-0.050***</td>
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<td>(0.014)</td>
<td>(0.014)</td>
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<td>Debt service, pc. GDP</td>
<td>-0.074**</td>
<td>-0.074**</td>
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<td>Observations</td>
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<td>338</td>
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<tr>
<td>Instruments</td>
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<td>21</td>
</tr>
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<td>J-test (p-value)</td>
<td>0.610</td>
<td>0.610</td>
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<td>-5.439</td>
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<tr>
<td>AR(1) p-value</td>
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</tr>
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<td>AR(2) p-value</td>
<td>0.199</td>
<td>0.237</td>
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</table>

Standard errors in parentheses  
*** p<0.01, ** p<0.05, * p<0.1
Chapter 2

Aid Flows and the “Real” Exchange Rate: Does Aid Cause Dutch Disease?

The previous chapter focused on the appropriate measurement of foreign aid flows. The core finding is that how aid is measured significantly affects its estimated effect on government spending, which we regard as an objective function insofar as it matters to both donors and developing country policymakers.

While the absolute value of aid transfers varies depending on the measure used, aid flows by any definition can be sizable shares of the current accounts of many low-income countries. A large and influential literature has developed about the effect of these transfers on outcomes ranging from macroeconomic volatility (Loayza et al., 2007) to civil war (Nunn and Qian, 2012).

One persistent concern is that aid volumes are sufficient to distort relative prices and thereby affect recipients’ real exchange rates, generating pernicious second-order effects. This chapter shifts the focus from which measures of aid are relevant for policymakers (based on the subjective criterion of maximising aid’s marginal impact on government spending) to exploring whether a specific set of price effects potentially caused by those aid flows are, in fact, observed in the available macroeconomic data.

2.1 Introduction

This chapter tells a simple story. Since foreign exchange is equivalent to tradeable goods, inflows of foreign aid to small economies effectively increase the domestic availability of tradeable goods. This relative abundance decreases the price of traded output relative to non-traded output, affecting the real exchange rate and, since the rewards to producing in the non-traded sector
have increased relative to the traded sector, affecting the structure of domestic production. Taken together, these effects are symptoms of the Dutch disease, a term coined by The Economist Magazine (1977) to describe the macroeconomic effects that followed the discovery of natural gas in the North Sea waters off the coast of the province of Groningen in the Netherlands. The resulting inflows of foreign exchange increased domestic demand for non-traded goods and services, causing an appreciation of the real exchange rate and a decrease in the production of tradeable goods.

The literature on the aid-Dutch disease hypothesis is conceptually linked to research on the resource curse initiated by Sachs and Warner (1995): large inflows of capital may not enhance growth. Corden and Neary (1982) wrote an early model of this effect, in which a small open economy “de-industrialises” following a sudden inflow of foreign exchange. More recently, Yano and Nugent (1999) develop a macroeconomic model of the closely-related transfer paradox and show that if financial transfers are spent on traded goods and services, this has a deleterious effect on the recipient economies’ traded sector. Van Wijnbergen (1985) initiated this area of research by integrating the insights of Corden and Neary (1982) to demonstrate that economies are agnostic about the source of foreign exchange inflows: both aid and large resource receipts can have the same effects on the demand for inputs like labour and capital and the resulting production of non-traded relative to traded goods and services.

As Barder (2006) points out in a summary of this phenomenon for policymakers, the term “disease” is a misnomer. After all, the transfer of resources unambiguously raises the welfare of the recipient by relaxing the economy’s budget constraint. Aid’s negative consequences are therefore second order effects: if aid flows drive up the real exchange rate and tilt production towards non-traded goods, under certain conditions the aid recipient may be deprived of the positive learning-by-doing externalities considered a key ingredient of economic growth (Young, 1991, builds an neoclassical model of this dynamic).

Demonstrating these second-order effects requires several assumptions about the aid-recipient economy. For example, as Nkusu (2004) shows, an economy-wide excess supply of factors of production prevents a contraction of the export sector: increased demand just draws in under used factors of production. Similarly, if the elasticity of demand for the export good is low, we are unlikely to

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1These symptoms are closely related to the classical Keynesian transfer paradox. The transfer paradox originated in discussions between Keynes and Ohlin following the Versailles treaty ending the First World War, which stipulated that Germany was required to pay reparations. If the German representative consumer had preferences over both traded and non-traded goods, the payment of tributes in terms of traded goods would increase their relative domestic price, affecting the German terms of trade and real exchange rate. See Lane and Milesi-Ferretti (2000) for a contemporary treatment.
observe a contraction in output due to an increase in price. Exporters of some primary commodities, for example, face a steep demand curve so do not suffer large decreases in export profits for proportional increases in the price of the exported good.

Tressel and Prati (2006) capture the inherent tension between “good” and “bad” effects of aid flows by highlighting the differential effects of aid on productivity in recipient countries. In their model, aid is good for productivity insofar as it increases public expenditure, and bad for productivity because of Dutch disease effects. These countervailing forces determine the ultimate impact of the transfer of tradeables on the recipient economy and therefore on economic growth. (Matsen and Torvik, 2005, show this picture is incomplete if learning-by-doing externalities arise in both traded and non-traded sector).

This literature puts three related research questions in play. Firstly, does aid tilt production towards non-traded goods? By extension, we would like to know if this effect is accompanied by a real exchange rate appreciation: the effects are linked, but we may not observe them simultaneously. Finally, we are concerned with whether aid, through the channels of altering the structure of production in the recipient economy and increasing the real exchange rate, affects economic growth. This paper focuses on a single link in the chain of causation: we investigate if there is a robust, economically meaningful relationship between aid inflows and RER appreciations in recipient countries.

2.2 Related literature

Sy and Tabarraei (2010) provide a summary of the macroeconomic literature about aid flows and real exchange rates. In response to the question of whether aid is systematically associated with real appreciations, their answer is: so far, it depends. If aid does induce a contraction in the traded goods sector and an increase of the real exchange rate, the question becomes: does it matter? If the effects are small, then aid flows’ negative effects might be offset by the increase in income they represent. Rajan and Subramanian (2011) and Rajan and Subramanian (2005) construct a data set of more than 30 countries and use ordinary least squares and instrumental variable techniques to conclude that aid is associated with small RER appreciations and that this is the channel through which aid causes a contraction in the size of recipients’ manufacturing sectors (as in this study, they do not explicitly study the aid-economic growth link). They find that a 1% increase in aid is associated with around a 1% decrease in the growth rate of the recipients’ manufacturing sector. Their paper also delivers preliminary evidence that the channel linking aid inflows
and decreased manufacturing growth is the relative overvaluation of the aid recipient’s exchange rate. Because they exploit within-country, across-industry variation at the ISIC three-digit level, their estimated coefficients are not biased due to omitted (fixed) country-level effects. These results are consistent with Prati et al. (2003), who find that a 100% increase in aid causes around a 4% real exchange rate appreciation.

In an influential article, Rodrik (2007) shows that even relatively small undervaluations of the RER generate economically significant growth effects, suggesting policymakers should consider whether aid flows may cause RER appreciations (in fact, Rodrik specifically argues in favour of competitive undervaluation, since this boosts growth through higher export levels and the learning-by-doing effects of exporting). In contrast, Adam and Bevan (2004) write out and simulate a model of aid inflows and productivity spillovers associated with public-sector expenditure. Calibrated to Ugandan data, they find aid causes some RER appreciation in the short run, but not in the long-run, and that aid can be growth enhancing if we assume sufficiently strong productivity spillovers from public spending.

The difficulty of interpreting evidence from simulation methods is underlined by a similar study undertaken by Arellano et al. (2005) using a calibrated model with a factor transformation curve to mediate the transfer of capital from the non-traded to traded sector. They conclude that under specific assumptions about demand, aid inflows are associated with a smaller traded goods sector.

Elbadawi and Soto (2005) study the link between aid and the real exchange rate in a panel of more than 80 countries and underline the possibility for policymakers to play a positive role. While they show that exogenous aid inflows may be associated with appreciations, the effect of aid on the real exchange rate is strongly dominated by the effects of other fundamentals, particularly other components of the current account and the terms of trade. Relatively painless policy interventions, including reducing taxes on non-traded goods, they argue, could actually drive depreciations of around 30%.

On balance, the question of whether aid causes appreciations of the real exchange rate depends on the size and composition of the sample, the specification of the theoretical or empirical model, and the calibration of the simulation used or the choice of data. A valuable contribution therefore remains to be made if the aid-RER link can be demonstrated or rejected in a large cross-section of countries over a meaningful time period.

This paper innovates in several ways. Firstly, it focuses on aid flows originating with Arab oil exporting countries that are not members of the Development
Assistance Committee (DAC), the coordinating and reporting entity for the majority of aid donors and institutions. Secondly, it explores the strength of oil prices as plausibly exogenous, time-varying instruments for aid flows from Arab donors, and uses results from these instrumental variable regressions to motivate a simple extension to the model, in which the net effect of aid depends on whether aid inflows dominate resource outflows associated with higher oil prices. Finally, we are able to unpack the aid-Dutch disease link, and conclude that aid from Arab donors is not associated with RER appreciations in recipient countries.

2.3 "Real" Exchange Rates and exogenous aid flows

We study an economy with two sectors, producing traded \( T \) and non-traded \( N \) output from a single factor of production sold in competitive factor markets at prices \( w_N, w_T \). Firms produce output according to \( Y_T = G(L_T) \) and \( Y_N = F(L_N) \), where the production technologies \( G(\cdot) \) and \( F(\cdot) \) are convex: \( G'(\cdot) > 0, G''(\cdot) < 0, F'(\cdot) > 0, F''(\cdot) < 0 \).

Firms' objective functions are

\[
\pi_N = P_N Y_N - w_N L_N \\
\pi_T = P_T Y_T - w_T L_T
\]

and differentiating with respect to the inputs \( L_T \) and \( L_N \) delivers the usual zero profit conditions for factor prices in each sector

\[
w_N = P_N F'(L_N) \\
w_T = P_T G'(L_T)
\]

Since factor markets are competitive, \( w_N = w_T \) and we can reorganise the zero-profit conditions to give the first equilibrium condition for equilibrium in factor markets:

\[
\frac{P_N}{P_T} = \frac{G'(L_T)}{F'(L_N)} \tag{2.1}
\]

In addition to buying the factor of production in factor markets, firms in
both sectors sell their outputs of traded and non-traded goods in competitive markets for goods. We use a representative consumer who maximises total utility from the consumption of both kinds of goods:

\[ U(C_T, C_N) = C_N^{\theta}C_T^{1-\theta} \]

subject to the economy-wide resource constraint

\[ Y = P_T Y_T + P_N Y_N + P_T D_T \]

which is the GDP identity with traded and non-traded output sold at their respective prices and the inflow of foreign aid, \( D_T \) priced in terms of tradeable goods. The first order condition \( \frac{\partial U}{\partial C_N} = \frac{\partial U}{\partial C_T} \) gives us the direct demand functions

\[ C_N = \frac{\theta Y}{P_N} \]
\[ C_T = \frac{(1 - \theta)Y}{P_T} \]

Markets for both goods clear, so \( C_N = Y_N \) and \( C_T = Y_T + D_T \). Using \( C_N = \frac{\theta Y}{P_N} \), we can write the condition for equilibrium in the goods market as:

\[ P_N = \frac{\theta Y}{C_N} = \frac{\theta P_N Y_N + P_T Y_T + P_T D_T}{Y_N} \]

Substituting in the production functions and re-organising delivers an expression for relative price levels in terms of production. This is the second equilibrium condition for equilibrium in goods markets:

\[ \frac{P_N}{P_T} = \frac{\theta}{1 - \theta} \left[ \frac{G(L_T) + D_T}{F(L_N)} \right] \] (2.2)

### 2.3.1 General equilibrium

General equilibrium requires conditions 2.1 and 2.2 are satisfied. The intuition is that an inflow of tradeable goods in the form of a foreign aid results in an in equilibrium with higher consumption of traded goods and a lower relative price for traded output. The decrease in the relative price of traded output tilts the
production side of the economy towards the non-traded sector. We can see this concretely by differentiating the condition for equilibrium: \( \frac{\partial (P_N/P_T)}{\partial Y_T} > 0 \) and \( \frac{\partial (P_N/P_T)}{\partial Y_N} < 0 \), so the relative price level increases in the supply of tradeables (non-traded goods become relatively more valuable) and vice versa. An increase in foreign-aid is an increase in the supply of tradeables:

\[
\frac{\partial (P_N/P_T)}{\partial D_T} = \frac{\theta}{(1 - \theta)F(L_N)}
\]

Since an increase in \( D_T \) causes an increase in the relative price of non-traded goods on the production side of the economy, the right-hand side of the factor market equilibrium condition must also increase. Without further assumptions about production technologies and the relationship between \( L_T \) and \( L_N \), this effect is simply an appreciation of the real exchange rate, without necessarily any equilibrium contraction of the traded goods sector. In order for foreign aid flows to generate both an appreciation of the RER and a contraction of the traded goods sector, output in the \( N \) and \( T \) sectors must use a common input in fixed supply.

We normalise the economy’s stock of the common input to unity, so that \( L_T + L_N = 1 \). Applying this normalisation, the condition for equilibrium in factor markets given by equation 2.1 is

\[
\frac{P_N}{P_T} = \frac{G'(1 - L_N)}{F'(L_N)}
\]  

and the condition for equilibrium in goods markets given by equation 2.2 is

\[
\frac{P_N}{P_T} = \left( \frac{\theta}{1 - \theta} \right) \left[ \frac{G(1 - L_N) + D_T}{F(L_N)} \right]
\]  

When output in both sectors uses a common input, an increase in the relative price of non-tradeables due to an increase in \( D_T \) through the goods market equilibrium condition requires an increase in the right-hand side of condition 2.2. When production sets are convex, an increase in \( L_N \) causes a decrease in the marginal product \( F'(L_N) \), and an increase in the marginal product \( G'(1 - L_N) \). Concretely,

\[
\frac{\partial P_N/P_T}{\partial L_N} = \frac{F'(L_N) \cdot -G''(1 - L_N) - G'(1 - L_N)F''(L_N)}{F'(L_N)^2} > 0
\]
Figure 2.1: Increase in equilibrium value of $\frac{P_N}{P_T}$

Figure 2.1 shows the graphical solution: an increase in $D_T$ is a level shift in the set of points consistent with equilibrium in the goods market, resulting in a larger share of the input allocated to non-traded production and a higher equilibrium value of $\left(\frac{P_N}{P_T}\right)$.

The intuition for this model is simple: an increase in the supply of traded goods due to an increase in foreign aid increases their abundance and decreases their relative price, causing an increase in the relative price of non-traded output, $\left(\frac{P_N}{P_T}\right)$. When output uses a common factor, this change in relative prices affects the equilibrium by changing returns to the factor of production in the non-traded and traded sectors; if these factor markets are competitive, this change causes re-allocation of $L$ from the traded to the non-traded sector, allowing factor rents to equalise, and causes a contraction of $Y_T$ relative to $Y_N$.

The combination of these two effects is symptomatic of Dutch disease.

2.4 The “Real” Exchange Rate and relative prices

The real exchange rate is a mapping from relative price levels and the nominal exchange rate to an index. Using * to denote foreign country variables, the real exchange rate between two countries can be written as $\text{RER} = e^{\frac{P}{P^*}}$, where $P, P^*$ are priced in their respective domestic currencies and $e$ is the nominal exchange rate.

To get traction, we use a geometric average across the traded and non-traded sectors with weights $\theta$, so $P \propto P^*_N P^\frac{1-\theta}{T}$ (deriving this from consumption shares $C_T, C_N$ is an exercise that follows from Obstfeld and Rogoff (1996); see ap-
Writing this out and suppressing country-year subscripts, the real exchange rate is given by

\[ RER = e \times \frac{P}{P^*} = e \times \frac{P^N_T}{P^*_N} P^{1-\theta}_T \]

Producers of traded goods are price takers, so the price of traded output is fixed across countries at some price \( P_T \), and trade frictions increase the aid-recipient's price level for traded goods relative to the rest of the world: \( P_T = (1 + \tau)P^*_T \). Substituting and taking logs gives

\[ \ln(RER) = \ln(e) + \theta \ln(P_N/P^*_N) + (1 - \theta) \ln(1 + \tau) \] (2.5)

This paper explores the link between aid flows and the left hand side of equation 2.5. The interpretation of the index is simple: \( \ln(RER) > 0 \) means domestic price levels are appreciated relative to foreign price levels while \( \ln(RER) < 0 \) means domestic prices are depreciated relative to foreign price levels. The RER appreciates due to an increase in nominal exchange rates, the relative price level of non-tradeables, or the level of trade frictions between the aid-recipient and the rest of the world. Our simple static model of price level determination in aid-recipient countries showed how foreign aid receipts influence this "real" exchange rate through relative prices \( \frac{P_N}{P_T} \).

### 2.4.1 Measuring "Real" Exchange Rates

We need a consistent, cross-country measure of real exchange rates to test whether observed levels of aid flows \( D_T \) co-vary meaningfully with price levels.

We construct this with \( XRAT \), which are time series of nominal exchange rates in local currency units (LCU) per US dollar from the Penn World Table (PWT) of Heston et al. (2009). The PWT also provides \( PPP \), the national currency value of GDP in year \( t \) divided by the real value of GDP measured in 1996 US dollars.

The expression for real exchange rates in equation 2.5 is a mapping from trade frictions, nominal exchange rates and relative prices levels to an index. If trade frictions \( \tau \) are fixed across time, they will absorbed by country fixed effects. Ignoring this level shift in \( \ln(RER) \), the real exchange rate for an aid recipient \( i \) in year \( t \) using cross-country data \( XRAT \) and \( PPP \) is:
\[ RER = e^{\frac{\theta P_{N}^{1-\theta}}{P_{N}^{1-\theta} P^{1-\theta}_{T}}} \approx \frac{1}{XRAR} \cdot PPP \]

2.4.2 Balassa-Samuelson correction

Canonical results in macroeconomics show that cross-country measurements of price levels are explained by different levels of productivity, so a naïve regression of \( RER \) on aid flows misattributes variations in the “real” exchange rate due to increases in productivity to changes in aid flows. To remove this Balassa-Samuelson effect in \( \ln(RER) \), we fit the model

\[
\ln(RER_{it}) = a + b \cdot \ln(RGDPC_{it}) + d_{t} + e_{it}
\]

where the right-hand side includes a constant, real GDP per capita, and annual fixed effects. The series RGDPC from the Penn World Table is used for consistency, and an alternate data set is used as a robustness check later in the paper. Approximating movements in the RER that are not explained by economic growth is now, implicitly or explicitly, the standard approach in the literature on aid and growth. For example, see: Rajan and Subramanian (2011) (who include an extensive discussion of this technique), Rodriguez and Rodrik (2000), Johnson et al. (2007), and Rodrik (2007). The pooled ordinary least squares estimate of \( \hat{b} \) is 0.14, for which the associated \( t \)-test rejects \( \hat{b} = 0 \) at the 1% level, indicating a large, statistically significant effect of real income growth on real exchange rates.

We can then use the fitted value \( \overline{RER} \) to adjust this relative price level measure for economic growth

\[
rer_{it} = \ln RER_{it} - \left[ \hat{a} + (\hat{b}) \cdot \ln(RGDPC_{it}) + \hat{d}_{t} \right] \tag{2.6}
\]

The resulting index \( rer_{it} \) is a log-scaled real exchange rate measure that nets out increases in relative price levels due to real income per capita. This metric has a simple interpretation: when it is greater than zero, country \( i \)'s currency is overvalued in real terms relative to the United States, the base of the index.

The unweighted cross-country average of this variable is given by \( N^{-1} \sum_{i=1}^{N} rer_{it} \), and figure 2.2 graphs this average across time:

As expected from the definition of equation 2.6, correcting for the mechanical
relationship between GDP per capita and price levels amounts to a level shift in the RER index, or an (approximately) spread-preserving shift in the mean of the distribution:

Since least squares coefficients are squared mean deviations, this Balassa-Samuelson correction should not greatly affect the relationship between aid flows and the RER index. (The regression model below is also applied to data with an unadjusted RER index as the dependent variable as a robustness test; our results are unaffected).
2.5 Measuring aid flows

For information on aid flows, we use data disseminated by the OECD and collected through the Creditor Reporting System (CRS) which collates self-reported data from the members of the Development Assistance Committee (DAC).2

A transfer is defined as foreign aid if its "primary purpose is economic development, it is offered on concessionary terms (if a loan), and if the flow has a grant element of at least 25%" (OECD Development Assistance Committee, 2011). ODA is therefore statistically distinct from military aid and other government-to-government resource transfers, which are instead recorded as Other Official Flows (OOF) in the CRS and national accounts data.

The CRS records aid transactions in millions of current US dollars as either commitments or disbursements, where the former is the stated / recorded intention of the donor and the latter is a record of the transaction at the time of the transfer. Due to issues of data quality, reporting, and timing, each commitment does not map one-to-one to each disbursement (and vice versa), with the result that the CRS records many more data points for aid commitments than disbursements. To minimise this measurement error and potential bias in regression coefficients, we use only the data on disbursements. While there are several other measures of foreign aid available to researchers, the OECD only collects data on commitments and disbursements for the Arab donor community.

Following the model's notation, an aid flow from donor $j$ to recipient $i$ in year $t$ is $D_{jit}$. To minimise error from price effects and account for the relative scale of the transfer (large transfers to small economies affect the structure of production more than small transfers to large economies), we scale the aid flow by national output

$$d_{it}^{total} = \sum_{j \in \text{all donors}} (D_{jit}/Y_{it})$$

where the denominator is GDP in millions of current US dollars from World Bank (2010). Aggregating $d_{it}^{total}$ across recipients in each year and graphing the resulting series $d_t = N^{-1}\sum_{t=1}^{N} d_{it}^{total}$ shows the secular increase over time in aid as a share of recipients' national output; including the average value of RER

2The DAC Members are: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, the United States and the Commission of the European Communities
across all aid recipients provides visual intuition for the link between aid flows and the RER index formally explored in the following section (figure 2.4).

**Figure 2.4:** Average total aid flows (all recipients) and average RER index

![Graph showing average total aid flows and average RER index over time.](image)

### 2.6 Estimating framework: total aid flows

The estimating framework follows directly from the linearised equation 2.5. The full specification includes country and year fixed effects to remove time invariant features of the data and any shocks common to all countries in a given year (e.g., global business cycle events or changes in the risk-free rate, typically the yield on US treasury bills). With data in levels, our estimating equation is:

$$rer_{it} = \alpha + \beta d_{it} + X'_{it} \gamma + \lambda_i + \delta_t + \epsilon_{it}$$  \hspace{1cm} (2.7)

While panel cointegration tests have low power or are not viable in the presence of structural breaks or gaps in time series data, we may be concerned that autocorrelation in the residuals will affect the estimated coefficients, so we also eliminate fixed effects through first differencing equation 2.7. The analogous first-differenced estimation problem is

$$\Delta rer_{it} = \alpha + \beta \Delta d_{it} + \Delta X'_{it} \gamma + \Delta \epsilon_{it}$$  \hspace{1cm} (2.8)

We are interested in $\hat{\beta}$, the estimated relationship between aid flows and the real exchange rate index constructed in section 2.4. In addition to year and
country fixed effects, we include several sets of covariates that affect price levels. For neatness, consider the matrix of covariates as partitioned according to: \( X_{it}^I \beta = \left[ X_{it}^I, X_{it}^2, X_{it}^3 \right]' \left[ \beta^1, \beta^2, \beta^3 \right] \).

Examining the raw counts of observations provided in the summary statistics (tables 2.22 and 2.23), it is clear that cross-country data coverage is an additional constraint to estimating equations 2.7 and 2.8. We define the base set of covariates in \( X_{it}^I \) to include logs of population, the first difference of log real GDP per capita, and the level of real GDP per capita.\(^3\)

This base set of covariates is supplemented with the vector \( X_{it}^2 \), which includes the variable \textit{Openness}, measured by the net capital account as a share of GDP because countries that receive larger net inflows of foreign investment exhibit higher equilibrium price levels. Since inflation acts as a useful summary measure of macroeconomic management and affects prices directly, it is also included as \( \text{Inflation}_{it} = \ln \left( 1 + \frac{\text{ CPI}_{it} - \text{ CPI}_{it-1}}{\text{ CPI}_{it}} \right) \).

Because the nominal price of foreign currency may be artificially fixed or otherwise directly affected through exchange rate arrangements (e.g., a dirty float), we also use \textit{Ex. rate classification}, a categorical variable constructed by the IMF measuring the extent of financial openness, including the flexibility of the nominal exchange rate, higher values of which indicate less financial openness.\(^4\)

Finally, \( X_{it}^3 \) includes two covariates drawn from the literature on the geopolitics of foreign aid: a dummy for 
\textit{Egypt} to capture that country’s outlier status as a recipient of significant inflows of (mainly US) foreign aid funding, and a variable for \textit{Interstate Conflict} which is set to one for any year in which an aid recipient was at war. Tables 2.22 and 2.23 provide summary statistics.\(^5\)

\[2.6.1\text{ Results: total aid flows}\]

Table 2.2 provides characteristic results when we estimate the model of equation 2.7 using data on total aid flows in levels using the covariates of vectors \( X_{it}^1 \) and \( X_{it}^2 \) across a core sample of 110 countries (including the additional covariates from \( X_{it}^3 \) reduces the sample size to 69 countries). Conditional on these covariates, total aid flows appear to be \textit{negatively} and statistically significantly

\(^3\)Per capita wealth is included because the Balassa-Samuelson is derived from a pooled, cross-country fitted coefficient; country-level deviations from this relationship may affect the level of aid flows and our measure of relative prices

\(^4\)Reinhart and Rogoff (2002) critique this measure at length, concluding that it is flawed; while their proposed alternative measure probably captures the underlying concept more effectively, data coverage is significantly worse for our core sample of countries

\(^5\)Regressions were also run with several other standard covariates, including measures of democracy. These affected the sample size, but the additional measures did not change the sign or significance of the coefficients of interest
associated with our measure of the real exchange rate, with an estimated $\beta$ between $-0.52$ and $-0.81$. Standard errors throughout are robust to arbitrary heteroscedasticity and autocorrelation in the residuals $\epsilon_{it}$ and are clustered at the country level.

Population, openness and the IMF exchange rate classification all appear to be important determinants of the real exchange rate index, and the $R^2$ increases dramatically with the inclusion of fixed country- and year-effects, suggesting that time-invariant country characteristics and global shocks common across countries are important drivers of the aid-RER relationship.

Table 2.3 supplements these results by successively including covariates, including those from $X_{it}^3$, revealing a consistent and negative relationship between total aid flows (as a share of the recipient’s GDP) and our measure of the RER. While not all covariates are statistically significant, the binary variable for Egypt is positive and significant, indicating that Egypt does, indeed, have a distinct relationship with donors and confirming the importance of including country fixed effects in our regression framework.

If these results are not being driven by autocorrelation, we would expect to see a broadly similar pattern of coefficients when we estimate the first differenced model of equation 2.8 in table 2.4. We continue to observe a negative relationship between total aid flows and the RER index of around $-0.2$, which is not statistically significant at conventional levels. On balance, these regressions suggest a negative relationship between aid and the RER, contradicting the predictions of our simple model. However, these estimates are point-identified only if the right hand side variables of the estimating equations, net of country and year fixed effects, do not covary with the unobserved error term.

2.7 Endogeneity of aid flows

Our failure to demonstrate any consistent positive aid-RER link is symptomatic of the fundamental problem facing applied research on foreign aid: aid flows are not randomly allocated across aid recipients, so aid flows are endogenous with respect to other macroeconomic aggregates of interest (including relative price levels measured by the real exchange rate index used here).

Overcoming this limitation has generated a large and rich literature built on finding and defending plausible instrumental variables capturing exogenous variation in the level of aid. Classic examples are country characteristics such as colonial history, or sharing a common language, border, or religion with
donors, for example in Tavares (2003), as discussed in chapter 1.

While these instruments are innovative and may collectively extract some exogenous features of aid flows, they are fixed across time and therefore collinear with fixed effects. Given that our regression models try to capture the covariation between a measure of relative price levels and aid flows over time, we turn to an alternative identification strategy.

In an innovative contribution, Werker et al. (2009) propose studying aid flows originating from Arab aid donors. While Arab states are significant suppliers of oil, recipients of Arab aid may not be. Since the price of oil is ultimately determined by demand and supply on international commodities exchanges, oil prices can be considered, from the perspective of non-oil exporters, to be exogenous. The OECD CRS contains information on aid flows from three categories of Arab donors: the United Arab Emirates (UAE), Arab agencies, and other Arab donors. Using these data, we construct an analogous measure of Arab aid using this subset of aid flows:

$$d_{it}^{arab} = \sum_{j \in \text{arab donors}} (D_{jit}/Y_{it})$$

The sample is constrained to include only those countries with average oil exports as a share of GDP of less than 1% over the sample period, and excludes any states listed as oil producers by British Petroleum (2010). As before, we focus on consistent aid recipients by including only countries with 15 or more annual positive aid flow observations. We first estimate models analogous to those fitted to data on total aid flows, replacing $d_{it}^{total}$ with the measure $d_{it}^{arab}$.

Table 2.5 confirms the earlier results with total aid flows, showing a negative and statistically significant relationship between Arab aid flows and the RER index when data are measured in levels, with the estimated $\hat{\beta}$ ranging between $-0.16$ and $-0.53$. The sign of this relationship holds when the full set of covariates are successively introduced in table 2.6 and once again the binary variable Egypt is positive and statistically significant.

When estimated in first-differences, there does not appear to be any consistent sign on the estimated relationship between Arab aid and the RER index. Using the covariates in $X'_f$, the estimated coefficient is negative and significant; when we estimate the problem using $X'_f = [X'_f, X''_f]$, the estimated sign is positive but not statistically significant at conventional levels.

The estimated relationship between Arab aid and recipients’ RER index is negative. Estimates from the first-differenced model produce a positive $\hat{\beta}$ conditional an extended set of covariates, but the estimate lacks statistical
significance. These contradictory results for the baseline models motivate us to explore instrumental variable methods.

2.7.1 Instrumental variable results: Arab aid flows

Werker et al. (2009) instrument for Arab aid flows with the price of oil in real US dollars interacted with the share of the recipient that is Muslim. Schematically, this relationship is: $p^\text{oil}_t \times %\text{Muslim} \to \text{Arab aid} \to \text{RER}^6$. Superficially, it appears that Arab aid flows co-move closely with both oil prices and recipients' RER index (represented as an average across all recipients).

Figure 2.5: Average Arab aid flows and oil prices

Figure 2.6: Average Arab aid flows and recipients' RER index

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6 There are several differences: in their paper, aid is measured in real US dollars rather than scaled to the size of the recipient economy, and total aid flows are used in the first stage, rather than aid flows originating only from Arab states.
Formally, the two-stage least squares model is

\[
\begin{align*}
\delta_{it}^{\text{prab}} &= \alpha + \beta (p_{it}^{\text{oil}} \times \%\text{Muslim}_i) + X'_{it} \lambda + \gamma_i + \delta_t + u_{it} \quad (2.9) \\
\text{rer}_{it} &= \alpha + \beta \hat{d}_{it}^{\text{arab}} + X'_{it} \lambda + \gamma_i + \delta_t + \epsilon_{it} \quad (2.10)
\end{align*}
\]

and, as before, we also estimate a first-differenced model of equations 2.9 and 2.10. Table 2.8 presents the second-stage results when the system of equations is estimated by two-stage least squares. Data on oil prices in constant 2008 US dollars are from British Petroleum (2010) and data on the share of the recipient population that is Muslim are from Nath (2007). The results are striking: when the full set of covariates are used, $\hat{\beta} \approx 3$. When the subset of covariates capturing country primitives is used (columns 1 and 2), however, the estimated relationship is negative. Despite an overall $R^2$ between 6% and 12%, these estimates are both internally inconsistent and not statistically significant at the usual levels.

When instruments are correlated with the endogenous variable (here, Arab aid flows), but only weakly, there may be substantial bias in estimated coefficients when problems are estimated by multi-stage least squares: the “weak instruments” problem. Since maximum likelihood methods are relatively robust under weak instruments, we estimate the model of equations 2.9 and 2.10 using limited information maximum likelihood (LIML) in table 2.9, in which the estimated Arab aid-RER relationship shows the same pattern of signs and relative sizes but continues to be statistically insignificant. Finally, for completeness, table 2.10 estimates the system of equations in first differences by both two-stage least squares and LIML, and confirms that using oil prices to instrument for Arab aid does not generate a significant second stage relationship between Arab aid and the RER index.

### 2.7.2 The weak instruments problem

Tables 2.8, 2.9, and 2.10 report two first-stage F-statistics for each regression, the “Cragg-Donald F-statistic” and the “Kleibergen-Paap F-statistic”. The Cragg-Donald F-statistic is a test for weak instruments under the assumption that the errors $\epsilon_{it}$ are independently and individually distributed; for sufficiently “low” values of this statistic, we would be concerned that the interaction ($p_{it}^{\text{oil}} \times \%\text{Muslim}_i$) is a “poor” instrument for Arab aid flows. When we suspect that the errors are clustered at the country-level, the appropriate statistic is based on a test of the rank of the reduced form matrix recently developed by Kleibergen and Paap (2006).
While these first-stage F-statistics are robust under homoscedasticity and heteroscedasticity, respectively, we still require a critical value to which we can compare them. The “rule of thumb” popularised by Staiger and Stock (1994) for estimating relationships by instrumental variables is that the first stage relationship should have a F-statistic above 10. However, Stock and Yogo (2002) show that when the instrument is significantly but weakly correlated with the endogenous variable, least-squares estimates are significantly biased. To augment the conventional cut-off, they tabulate critical values for a range of levels of bias relative to OLS, and different combinations of numbers of endogenous variables and exogenous instruments. In our case of a single endogenous variable (Arab aid, $d_{it}^{arab}$) and a single instrument, the critical value is 16.81 for instrumental variable estimates that are 10% or less biased relative to OLS estimates.

Across all three tables, we see that the Kleibergen-Paap first-stage F-statistics are significantly lower than this critical value, indicating substantial bias relative to OLS and suggesting, despite the unavailability of the Hansen J-test due to a “just identified” system of equations (a single regressor at risk of being endogenous for which we have only one instrument), that the interaction of oil prices and the share of the recipient country that is Muslim is a weak instrument for Arab aid flows.

A simple extension of the model demonstrates why oil prices plausibly violate the exclusion restriction assumed by Werker et al. (2009) that is necessary to identify $\hat{\beta}$: when oil prices simultaneously determine the value of aid received and the value of income exported to pay for the more expensive input, the net effect on aid recipients’ real exchange rates is ambiguous.

2.8 “Real” Exchange Rates and the oil current account

Our failure to find an effect of aid on recipients real exchange rates contradicts a simple model, indicating our underlying assumptions are inappropriate: Arab aid does increase when oil prices go up, but in a pooled sample of aid recipients this effect did not affect the RER. This maybe due to an incomplete characterisation of recipients’ current account dynamics: when oil prices increase, aid flows from Arab donors may increase but the cost of importing oil also increases.

We extend the simple model by studying the short run effect of an increase in a third price, the price of oil, denoted $P_i$. Firms in both sectors uses imported
oil as an input\(^7\), and the short run price elasticity of their factor demand for oil is zero. Then

\[
Y_T = F(L_T, O_T)
\]

\[
Y_N = G(L_N, O_N)
\]

where the overbars denote “fixed in the short run.” As before, profits in both sectors are given by

\[
\pi_{N,T} = P_{N,T}Y_{N,T} - w_{N,T}L_{N,T} - P_lO_{N,T}
\]

and zero-profit first order conditions give

\[
w_N = P_N F'(L_N, O_N)
\]

\[
w_T = P_T G'(L_T, O_T)
\]

If there are no systematic differences in the price of labour between sectors, \(w_N = w_T\). Reorganising,

\[
\frac{P_N}{P_T} = \frac{F'(L_N, O_N)}{G'(L_T, O_T)}
\]

which is the equilibrium condition in factor markets. For equilibrium in the market for output, the representative agent maximises

\[
U(C_M, C_T) = C_N^\theta C_T^{1-\theta}
\]

subject to the modified national income constraint

\[
Y = P_T Y_T + P_N Y_N + P_T D_T - P_T O
\]

while in section 2.3, the current account included only aid inflows \(D_T\), we now include the “oil current account” costs of imports through the term \(P_T O\), and oil imports are priced in the tradeable goods. Arab aid inflows and the value of oil imports are positively related to oil prices through strictly-increasing functions \(h(\cdot)\) and \(l(\cdot)\):

\(^7\)If this oil is produced domestically, we effectively assume it is purchased at international prices, or that the domestic subsidised price is at least linearly related to international prices set by competitive bidding.
\[ D_T = h(P_t) \quad \text{and} \quad \frac{\partial h(P_t)}{\partial P_t} > 0 \]

\[ O = l(\bar{O}_n, \bar{O}_T, P_t) \quad \text{and} \quad \frac{\partial l(\cdot, P_t)}{\partial P_t} > 0 \]

The agent solves the maximisation problem subject to the modified national income budget constraint. Using the conditional demand for goods as in section 2.3, we have

\[ P_N = \frac{\theta(P_T Y_T + P_N Y_N + P_T D_T - P_T O)}{Y_N} \]

which is equivalent to

\[ \frac{P_N}{P_T} = \frac{\theta}{1 - \theta} \left[ \frac{G(L_T, \bar{O}_T) + D_T - O}{F(L_N, O_N)} \right] \]

We are interested in the effect of a change in \( P_i \) on the RER term \( \frac{P_N}{P_T} \):

\[ \frac{\partial (P_N/P_T)}{\partial P_i} = \frac{\theta}{1 - \theta} \left[ \frac{F(\cdot)(G(\cdot) + D_T(P_i) - O^i(P_i)) - (G(\cdot) + D_T - O)F'(\cdot)}{F(\cdot)^2} \right] \]

so the net effect of higher oil prices depends on the non-linear interaction of higher oil prices with factor demands, aid flows, and the oil current account. At the time of changes in the oil price, however, our maintained assumption is that the price elasticity of factor demands for this input is zero, so \( \frac{\partial F(\cdot)}{\partial P_i} = 0 \) and \( \frac{\partial G(\cdot)}{\partial P_i} = 0 \). Therefore the net effect of change in oil prices on recipients’ RER is

\[ \frac{P_N/P_T}{P_i} \leq 0 \iff \left( \frac{\theta}{1 - \theta} \left[ \frac{\partial D_T/\partial P_i - \partial O/\partial P_i}{F(L_N, \bar{O})} \right] \right) \leq 0 \]

This gives us insight into the puzzling econometric findings. The direction of the effect of oil prices on the RER of recipients is determined by whether the elasticity of aid dominates that of the value of oil imports with respect to oil prices. While oil prices are exogenously determined by bidding on commodities exchanges, oil directly affects price levels through the budget constraint. Estimating an empirical model on pooled data is therefore inappropriate, because the net effect of higher oil prices depends on whether, for that recipient, aid inflows \( d_i \) dominate the value of oil imported.
2.8.1 Splitting the sample of Arab aid recipients

The direction of the effect of oil prices on recipients’ RER is determined by whether the elasticity of aid dominates that of oil imports. To take this to the data and observe its implications for the empirical model, we iteratively fit two related reduced form models to each aid recipient $i$:

\[
\begin{align*}
\alpha_i^{\text{arab}} &= \alpha_1 + \eta_i^{\text{arab \ aid}} \times \ln P_{it} + e_{1it} \\
\eta_i^{\text{oil}} &= \alpha_2 + \eta_i^{\text{oil}} \times \ln P_{it} + e_{2it}
\end{align*}
\]

where $d_{it}$ is aid from Arab donors received as a share GDP, $P_{it}$ is the price of oil in 2008 US dollars from British Petroleum (2010) and $v_{it}$ is the value of oil country $i$ imports in year $t$ as a share of GDP (constructed from British Petroleum, 2010, and World Bank, 2010).

The fitted elasticities are

\[
\begin{align*}
\eta_i^{\text{arab \ aid}} &= \frac{\partial \alpha_i^{\text{arab}}}{\partial P_i} \\
\eta_i^{\text{oil}} &= \frac{\partial \eta_i^{\text{oil}}}{\partial P_i}
\end{align*}
\]

and both equations are estimated on data pooled across time within countries. The coefficient $\eta_i^{\text{arab \ aid}}$ is the estimated semi-elasticity of Arab aid flows received by country $i$ and $\eta_i^{\text{oil}}$ is the estimated semi-elasticity of the value of oil imports with respect to the price of oil. Figure 2.7 shows the distribution of these fitted coefficients across countries:

The extension to a simple equilibrium model predicts that Arab aid flows will be positively associated with the RER index in those countries that receive more resources through higher Arab aid flows than are paid through increased import costs of oil. To capture this, we define Type 1 and Type 2 countries:

<table>
<thead>
<tr>
<th>Country</th>
<th>Definition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Type 1</td>
<td>$\eta_i^{\text{arab \ aid}} &gt; \eta_i^{\text{oil}}$</td>
</tr>
<tr>
<td>Type 2</td>
<td>$\eta_i^{\text{arab \ aid}} &lt; \eta_i^{\text{oil}}$</td>
</tr>
</tbody>
</table>

\[8\text{An analogous measure might be the net oil deficit in oil producing countries that also import oil (i.e., have energy demands that exceed domestic energy production). However, our sample is defined to exclude any countries with average oil exports as a share of GDP greater than 1% or any countries defined as oil producers by British Petroleum (2010).} \]
Graphing the values of each fitted coefficient for the sample of Arab aid recipients captures this relationship. The knife-edge condition of equation 2.11 is $\tilde{\eta}_i^{\text{arab aid}} = \tilde{\eta}_i^{\text{oil}}$, which separates our two subsamples: countries in the southeast half of figure 2.8 are Type 2 countries and those in the northwest half are Type 1 countries:

2.9 Estimating framework with Type 1 and Type 2 sub-samples

Extending the model of aid flows and RERs to include imports of oil suggests oil prices may directly determine both Arab aid and recipients’ real exchange rates, thereby violating the exclusion restriction required to use oil
prices as an instrument. We augment the matrix of covariates $X_{it}$ to include oil prices in logs to directly account for the effect of oil prices on aid recipients’ RER indexes. Fitting these models to data from sub-samples tells a simple and consistent story: the Arab aid-RER link is weakly negative and statistically insignificant for those countries that, on average, are characterised by $\hat{\beta}_{it}^{\text{arab aid}} > \hat{\beta}_{it}^{\text{oil}}$, and is strongly negative and statistically significant by those countries characterised by $\hat{\beta}_{it}^{\text{arab aid}} < \hat{\beta}_{it}^{\text{oil}}$.

The underlying structural models remain as in equations 2.7 and 2.8, with selection on sub-samples used to test the prediction that the aid-RER link is fundamentally different in Type 1 relative to Type 2 countries. The baseline results appear in table 2.11, which highlight the main conclusion from this section of the paper.

While the regression across all countries in the sample using the set of covariates in $X_{it}^1$ to maximize sample size produces a negative $\hat{\beta}$ between $-0.66$ (no fixed effects) and $-1.22$ (two-way fixed effects), fitting the model to Type 1 and Type 2 countries shows that this effect is largely driven by countries that, on average, have a higher price elasticity of oil imports than Arab aid flows: Type 1 countries have $\hat{\beta}$ of $-0.434$ (significant at $< 10\%$) and $-0.949$ (significant at $< 1\%$), while Type 2 countries have $\hat{\beta}$ of $-10.821$ and $-9.897$, both of which are statistically significant at $p < 5\%$.

The basic story is clear: when oil prices increase, the value of oil imports increases since oil imports are relatively price inelastic. For some countries, this is offset by increased inflows of Arab aid, which also increases when the price of Arab donors’ main export is high. The net effect of these flows determines the overall movement of the RER index. For those countries that are, on average, net “losers”, Arab aid is associated with a severely depreciated RER. Those countries that are, on average, net “winners” exhibit estimated $\hat{\beta}$s that are much smaller, but still negative. This suggests that RERs in Type 1 countries are appreciated relative to Type 2 countries, but they are not overvalued relative the United States (we re-base the index as a robustness test in the next section).

Extending the set of covariates to include $X_{it}^2$ in table 2.12 does not change this pattern of results. In the full sample, Arab aid flows continue to be negatively associated with recipients’ RER indexes, with a $\hat{\beta} = -0.77$, significant at less than 10%. The estimated coefficient is less than 1 for Type 1 countries and not significant, but less than $-13$ for Type 2 countries and significant at less than 10% without fixed effects and less than 5% when two-way fixed effects are included.
Table 2.13 includes estimates for data in first-differences. The pattern of results remains the same, but with different levels of statistical significance. Across the sample of countries receiving Arab aid, aid flows are associated with either a weakly negative or weakly positive movement of the RER index, depending on set of conditioning variables. When we unpack this effect into Type 1 and Type 2 countries, however, we see that the sign and significance of this effect in first differenced data is the same when data in levels are used. Type 1 countries have a $\hat{\beta}$ associated with $\Delta d_{it}$ of $-0.15$ or $0.20$ with the full set of covariates ($X_{it} \equiv [X_{it}^1, X_{it}^2]$). Type 2 countries have a $\hat{\beta}$ of $-2.23$ or $-7.29$ when the full set of time-varying covariates are included.

2.9.1 Alternative base for rer

Section 2.4 detailed the construction of the “real” exchange rate measure, which expresses price levels relative to the base of the index, the US. While this measure is useful for analysing the sign of the aid-RER relationship, it effectively assumes the US is the principal trading partner for Arab aid recipients.

Formally, we can write the trade-weighted RER index for some aid recipient $i$ across all of its $k = 1 \ldots N$ trading partners as a mapping from bilateral exchange rates $e_{ik}$, trade shares $w_{ik} \in (0,1)$ with $\sum_{k=1}^{N} w_{ik} = 1$, and relative price levels to an index. Equation 2.5 can be expanded to this expression

$$RER_i = \prod_{k \neq i} e_{ik} \times \left( \frac{P_{in}^{\theta} P_{iT}^{1-\theta}}{w_{ik} P_{Nk}^{\theta} P_{Tk}^{1-\theta}} \right)$$

Data limitations inherent in this area of research motivate our use of the PWT data, for which $N = 1$ (the US) and the trade share $w_{ik}$ set to unity. To ensure that the pattern of results on Type 1 and Type 2 countries is robust to a minimally-arbitrary rebasing of the index, we investigate whether aid flows affect the “real” exchange rates of aid recipients when their RERs are measured relative to other aid recipients.

Lacking good cross-country data on trade shares, we approximate this by setting $w_{ik} = 1/(N - 1) \forall i, k$, so that each country’s RER is rebased relative to the average of all other countries in the sample in each year. Then the rebased index $rer_{it}^{rebased}$ reduces to
Graphing the relative distributions of \( rer \) and \( rer_{\text{rebased}} \) in figure 2.9 indicates that the latter has a lower density around the mean and higher variance, suggesting least squares estimates (ratios of covariances) will be different.

**Figure 2.9:** Relative distributions of \( rer_{it} \) and \( rer_{it}^{\text{rebased}} \)

Tables 2.14 and 2.15 show the results of fitting our regression models with \( rer_{it}^{\text{rebased}} \) in levels and first differences, respectively. The results maintain the established pattern. In levels, the pooled sample (across all aid recipients) has an estimated coefficient of between -0.7 and -1.8, both of which are significant at conventional levels. The fitted coefficient is negative but small and not statistically significant, while \( \hat{\beta} \) in Type 2 countries is -34.761 with a minimal set of covariates and -35.125 with an extended set of covariates, both with \( p < 5\% \). Re-basing our measure of the RER for aid recipients strengthens the pattern of results by decreasing the size and increasing the significance of the negative relationship between aid flows and RERs in Type 2 countries that on average experience net outflows when oil prices increase.

For completeness, we also estimate the regression problem with an RER index that is not adjusted for the Balassa-Samuelson correction. Tables 2.16 and 2.17 present the results when \( rer_{it}^{\text{unadjusted}} \) is used in place of \( rer_{it} \) as the dependent variable. As with other robustness checks, the sign, pattern, and statistical significance of results is not overturned.
2.9.2 Alternative measures of financial openness

While the vector $X^2_t$ of conditioning variables includes the capital account as a share of GDP as a proxy for a recipients' financial openness that may directly move price levels and thus $rer^*_t$, there is likely a complex and non-linear relationship the capital account and actual capital openness. Developing de jure and de facto measures of openness has therefore emerged as an important area of research in the literature.

It is useful to investigate whether the pattern of results on Type 1 and Type 2 countries remains when we introduce alternative measures of capital openness. If openness to capital flows other than aid flows is a more relevant determinant of the aid-RER link than country type or level of Arab aid, introducing an accurate measure of openness may cause $\hat{\beta}$ to change size and potentially lose significance.

Chinn and Ito (2005) develop a de jure index of capital account openness for 181 countries over the period 1970 to 2000, with 1,071 country-year observations of the index compared to 1,141 country-observations for $rer^*_t$ in our sample of Arab aid recipients, representing significant but imperfect coverage. This variable, labeled Chinn-Ito Index, ranges between -1.84 and 2.48, with a mean of 1.12. The index is constructed so that higher numbers represent more de jure financial openness.

Tables 2.18 and 2.19 present results for fitting the regression specification to data in levels and first differences. While the Chinn-Ito Index does not appear as significant in any specification, it enters negatively in the pooled sample and Type 1 countries but positively in Type 2 countries, providing cursory evidence that increased financial openness in the latter sub-sample is associated with higher price levels. Since the estimated coefficient is not statistically significant, though, it is not clear that this is a meaningful determinant of the aid-RER link.

The pattern of results on the coefficient of interest, $\hat{\beta}$, remains the same. When data in levels are used, the aid-RER link is weakly negative or positive (depending on covariates) for Type 1 countries but without any statistical significance and with large confidence intervals around these point estimates. However, Type 2 countries exhibit an estimated $\hat{\beta}$ that is strongly negative: $-13.81$ with a base set of covariates and $-13.91$ with an extended set of covariates, and both estimates have $p$ values smaller than 5%.

As in the previous robustness checks, the pattern remains the same when we estimate the problem with data in first differences, and since this index of financial openness is a slow-changing variable, it is not surprising that first
differences of the Chinn-Ito Index are not statistically significant. The aid-RER relationship in Type 1 countries is also not statistically significant but is, once again, weakly negative, with large, negative coefficients reported for Type 2 countries.

2.9.3 Alternative construction of rer

Examining equation 2.5, we see that rer\(_{it}\) is constructed using data from Heston et al. (2009), a data set that provides internally consistent, cross-country coverage of relative prices and exchange rates. However, it is important to make sure that the pattern of results for country sub-samples is robust to using other data sources on GDP per capita. Again using \(PPP_{it}\) from the PWT, we construct an Balassa-Samuelson correction using data on real GDP per capita from United Nations (2010), and also use this measure directly on the right-hand side of the estimating equations 2.7 and 2.8 in place of the analogous measure from the PWT.

The fitted coefficients can be read across Table 2.20 for data in levels and, as before, the significance of the coefficients varies but the pattern of signs and sizes does not: the estimated \(\beta\) in Type 2 countries is \(-11.56\) and \(-12.08\), depending on whether the base or the extended set of conditioning information is used (the latter point estimate is significant at the 10% level). The story for data in first differences is consistent with results presented earlier and in other robustness tests above: in Table 2.21, the estimated coefficient for pooled data is between \(-0.02\) and \(-0.16\), but this negative relationship is largely driven by Type 2 countries, for which the estimated \(\beta\) associated with \(\Delta d_{it}\) ranges between \(-1.54\) to \(-6.75\).

2.10 Conclusion

This paper investigates the link between foreign aid and Dutch disease in a cross-country data set from 1970 to 2000, and innovates in several ways. To get traction, we develop a simple algebraic model linking aid inflows to the twin symptoms of a contraction of the production of tradeables relative to non-tradeables and an increase in the relative price of non-traded output and therefore the “real” exchange rate.

The failure to find evidence for this link in the data on total aid flows, \(d_{it}^{total}\) is due to endogeneity, in which case least-squares estimates of \(\beta\) are inconsistent. Correcting for this endogeneity using plausible instruments suggested by
Werker et al. (2009) with data on aid flows originating from Arab donors, we find that the price of oil, while set exogenously, cannot be excluded from the set of covariates.

A simple extension to the model shows that when oil prices affect both aid levels and price levels, the net effect of oil prices on relative price levels (and so the “real” exchange rate) depends on whether aid inflows dominate the resource outflows caused by higher oil prices. Splitting the sample based on these estimated relative elasticities $\eta_{oil}$ and $\eta_{arab aid}$, a consistent pattern of results emerges across samples that is robust to expanding the set of covariates, defining the real exchange rate index with data from other sources, or rebasing the RER index relative to other aid recipients rather than the US. The relationship between Arab aid flows and the RER index in Type 1 countries is weakly negative or positive but not statistically significant, while in Type 2 countries it is large, negative, and statistically significant.

On balance, there is strong evidence aid flows do not cause relative overvaluations of recipients’ RERs. To the extent that a meaningful relationship between these quantities exists, it is negative, suggesting that while Arab aid may move with the price of oil, aid inflows at observed levels are not sufficient to generate real exchange rate appreciations. On balance, we conclude that while many good reasons remain for economists to research and critique foreign aid levels and modalities, the evidence presented here suggests that “real” exchange rate appreciation symptomatic of Dutch disease is not one of them.
### 2.11 Tables

**Table 2.2**

Panel regression with Total aid flows  
Data in levels

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total aid, pc. of GDP</td>
<td>-0.558***</td>
<td>-0.815***</td>
<td>-0.527*</td>
<td>-0.651**</td>
</tr>
<tr>
<td></td>
<td>(0.195)</td>
<td>(0.177)</td>
<td>(0.272)</td>
<td>(0.254)</td>
</tr>
<tr>
<td>Real GDP per capita</td>
<td>-0.059</td>
<td>-0.214***</td>
<td>0.062</td>
<td>-0.140</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.062)</td>
<td>(0.052)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
<td>-0.143*</td>
<td>-0.111</td>
<td>-0.117</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.105)</td>
<td>(0.102)</td>
<td>(0.128)</td>
<td>(0.127)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.035*</td>
<td>-0.841***</td>
<td>-0.038</td>
<td>-1.037***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.171)</td>
<td>(0.028)</td>
<td>(0.328)</td>
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<tr>
<td>Net capital account, % GDP</td>
<td></td>
<td>0.096</td>
<td></td>
<td>-0.218</td>
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<tr>
<td></td>
<td></td>
<td>(0.157)</td>
<td></td>
<td>(0.251)</td>
</tr>
<tr>
<td>Openness, % GDP</td>
<td>-0.201*</td>
<td>-0.287**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.110)</td>
<td>(0.137)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Inflation, % ch. CPI</td>
<td>-0.032**</td>
<td>-0.025**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
<td></td>
<td></td>
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<tr>
<td>Ex. rate classification</td>
<td>-0.043**</td>
<td>-0.063***</td>
<td></td>
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<tr>
<td></td>
<td>(0.019)</td>
<td>(0.020)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.768+</td>
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<td>0.125</td>
<td>10.202***</td>
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<tr>
<td></td>
<td>(0.486)</td>
<td>(1.869)</td>
<td>(0.485)</td>
<td>(3.555)</td>
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<tr>
<td>Country FE</td>
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<td>y</td>
<td>n</td>
<td>y</td>
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<tr>
<td>Annual FE</td>
<td>n</td>
<td>y</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Number of countries</td>
<td>110</td>
<td>110</td>
<td>71</td>
<td>71</td>
</tr>
<tr>
<td>R-sq. (within)</td>
<td>0.028</td>
<td>0.127</td>
<td>0.126</td>
<td>0.256</td>
</tr>
<tr>
<td>R-sq. (overall)</td>
<td>0.051</td>
<td>0.666</td>
<td>0.0366</td>
<td>0.714</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses  
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20  
Fixed effects coefficients not reported
Table 2.3

Panel regression with Total aid flows
Full set of covariates
Data in levels

<table>
<thead>
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Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Annual fixed effects coefficients not reported
### Table 2.4
Panel regression with Total aid flows
Data in first differences

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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Table 2.5

Panel regression with arab aid flows
Data in levels

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<td>Inflation, % ch. CPI</td>
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Observations 1,141 1,141 584 584
Country FE y y n n
Annual FE y y n n
Number of countries 77 77 51 51
R-sq. (within) 0.028 0.121 0.210 0.316
R-sq. (overall) 0.055 0.708 0.0966 0.777

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
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<td>(0.478)</td>
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<td>(0.490)</td>
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<tr>
<td>Annual FE</td>
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<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
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<td>73</td>
<td>55</td>
<td>77</td>
<td>77</td>
<td>50</td>
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<tr>
<td>R-sq. (within)</td>
<td>0.069</td>
<td>0.0774</td>
<td>0.0643</td>
<td>0.197</td>
<td>0.0695</td>
<td>0.0743</td>
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<tr>
<td>R-sq. (overall)</td>
<td>0.064</td>
<td>0.0576</td>
<td>0.108</td>
<td>0.124</td>
<td>0.0674</td>
<td>0.0663</td>
<td>0.180</td>
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</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Table 2.7
Panel regression with Arab aid flows
Data in first differences

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<th>(4)</th>
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</thead>
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<tr>
<td>ΔArab aid, pc. of GDP (fd.)</td>
<td>-0.254*</td>
<td>-0.226</td>
<td>0.059</td>
<td>0.133</td>
</tr>
<tr>
<td></td>
<td>(0.141)</td>
<td>(0.176)</td>
<td>(0.235)</td>
<td>(0.311)</td>
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<tr>
<td>ΔReal GDP per capita, (fd.)</td>
<td>-0.371***</td>
<td>-0.435***</td>
<td>-0.398**</td>
<td>-0.521**</td>
</tr>
<tr>
<td></td>
<td>(0.085)</td>
<td>(0.087)</td>
<td>(0.196)</td>
<td>(0.207)</td>
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<tr>
<td>ΔGrowth (fd.)</td>
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<td>-0.043</td>
<td>-0.014</td>
<td>0.024</td>
</tr>
<tr>
<td></td>
<td>(0.070)</td>
<td>(0.072)</td>
<td>(0.111)</td>
<td>(0.106)</td>
</tr>
<tr>
<td>ΔPopulation, logs (fd.)</td>
<td>-0.542*</td>
<td>-0.533*</td>
<td>-1.121+</td>
<td>-0.679</td>
</tr>
<tr>
<td></td>
<td>(0.289)</td>
<td>(0.283)</td>
<td>(0.743)</td>
<td>(0.718)</td>
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<td>ΔNet capital account, % GDP (fd.)</td>
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<td>-0.389***</td>
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<td>(0.128)</td>
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<td></td>
</tr>
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<td>ΔOpenness, (fd.)</td>
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<td></td>
<td>-0.568***</td>
<td>-0.619***</td>
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<td>(0.155)</td>
<td>(0.146)</td>
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<tr>
<td>ΔInflation, % ch. CPI (fd.)</td>
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<td>-0.014+</td>
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</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.009)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ΔEx. rate classification (fd.)</td>
<td>-0.027*</td>
<td>-0.034**</td>
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<tr>
<td></td>
<td>(0.014)</td>
<td>(0.014)</td>
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<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.020**</td>
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<td>0.035+</td>
<td>0.044*</td>
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<td>(0.008)</td>
<td>(0.016)</td>
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<td>(0.023)</td>
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<td>909</td>
<td>441</td>
<td>441</td>
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<td>Annual FE</td>
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<td>y</td>
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<td>Number of countries</td>
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<td>77</td>
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<td>51</td>
</tr>
<tr>
<td>R-sq.</td>
<td>0.048</td>
<td>0.10</td>
<td>0.16</td>
<td>0.28</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
### Table 2.8

Panel regression with arab aid flows  
Data in levels, estimated by TSLS  
Instrument is Oil price x %Muslim

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arab aid, pc. of GDP</td>
<td>-0.362</td>
<td>-0.515</td>
<td>3.071</td>
<td>3.274</td>
</tr>
<tr>
<td></td>
<td>(2.165)</td>
<td>(2.373)</td>
<td>(3.230)</td>
<td>(3.472)</td>
</tr>
<tr>
<td>Real GDP per capita</td>
<td>-0.086**</td>
<td>-0.083*</td>
<td>-0.012</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.042)</td>
<td>(0.061)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
<td>-0.216</td>
<td>-0.257</td>
<td>-0.517**</td>
<td>-0.570**</td>
</tr>
<tr>
<td></td>
<td>(0.242)</td>
<td>(0.233)</td>
<td>(0.264)</td>
<td>(0.259)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.045**</td>
<td>-0.046**</td>
<td>-0.060***</td>
<td>-0.061***</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.019)</td>
<td>(0.023)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>Net capital account, % GDP</td>
<td>-0.004</td>
<td>-0.348</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.036)</td>
<td>(1.046)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Openness, % GDP</td>
<td>-0.286*</td>
<td>-0.294*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.161)</td>
<td>(0.166)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Inflation, % ch. CPI</td>
<td>0.023</td>
<td>0.025</td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.028)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ex. rate classification</td>
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<td>-0.057**</td>
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<tr>
<td></td>
<td>(0.026)</td>
<td>(0.028)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.006**</td>
<td>0.916**</td>
<td>0.797+</td>
<td>0.802+</td>
</tr>
<tr>
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<td>(0.425)</td>
<td>(0.424)</td>
<td>(0.519)</td>
<td>(0.543)</td>
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<td>Observations</td>
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<td>1,141</td>
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<td>584</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.058</td>
<td>0.075</td>
<td>0.092</td>
<td>0.122</td>
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<tr>
<td>Annual FE</td>
<td>n</td>
<td>y</td>
<td>n</td>
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<td>Cragg-Donald F-statistic</td>
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<td>242</td>
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<td>Kleibergen-Paap F-statistic</td>
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<td>10.7</td>
<td>3.96</td>
<td>3.75</td>
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</table>

Robust standard errors in parentheses  
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20  
Fixed effects coefficients not reported
Table 2.9
Panel regression with arab aid flows
Data in levels, estimated by LIML
Instrument is Oil price x %Muslim

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<th>VARIABLES</th>
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<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arab aid, pc. of GDP</td>
<td>-0.362</td>
<td>-0.515</td>
<td>3.071</td>
<td>3.274</td>
</tr>
<tr>
<td></td>
<td>(2.165)</td>
<td>(2.373)</td>
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<td>(3.472)</td>
</tr>
<tr>
<td>Real GDP per capita</td>
<td>-0.086**</td>
<td>-0.083*</td>
<td>-0.012</td>
<td>-0.011</td>
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<td></td>
<td>(0.044)</td>
<td>(0.042)</td>
<td>(0.061)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
<td>-0.216</td>
<td>-0.257</td>
<td>-0.517**</td>
<td>-0.570**</td>
</tr>
<tr>
<td></td>
<td>(0.242)</td>
<td>(0.233)</td>
<td>(0.264)</td>
<td>(0.259)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.045**</td>
<td>-0.046**</td>
<td>-0.060***</td>
<td>-0.061***</td>
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<tr>
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<td>(0.019)</td>
<td>(0.019)</td>
<td>(0.023)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>Net capital account, % GDP</td>
<td>-0.004</td>
<td>-0.348</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.036)</td>
<td>(1.046)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Openness, % GDP</td>
<td>-0.286*</td>
<td>-0.294*</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(0.161)</td>
<td>(0.166)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Inflation, % ch. CPI</td>
<td>0.023</td>
<td>0.025</td>
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<tr>
<td></td>
<td>(0.026)</td>
<td>(0.028)</td>
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<tr>
<td>Ex. rate classification</td>
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<td>-0.057**</td>
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<td></td>
<td>(0.026)</td>
<td>(0.028)</td>
<td></td>
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</tr>
<tr>
<td>Constant</td>
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<td>0.916**</td>
<td>0.797+</td>
<td>0.802+</td>
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<td>(0.424)</td>
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<td>1,141</td>
<td>584</td>
<td>584</td>
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<tr>
<td>R-squared</td>
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<td>0.075</td>
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<tr>
<td>Annual FE</td>
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<td>y</td>
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<td>Number of countries</td>
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<tr>
<td>Cragg-Donald F-statistic</td>
<td>273</td>
<td>242</td>
<td>99.7</td>
<td>88.6</td>
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<tr>
<td>Kleibergen-Paap F-statistic</td>
<td>10.7</td>
<td>10.7</td>
<td>3.96</td>
<td>3.75</td>
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Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20

96
Table 2.10
Panel regression with arab aid flows
Data in first differences
Instrument is Oil price x %Muslim

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<th>(4)</th>
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<td>LIML</td>
<td>2SLS</td>
<td>LIML</td>
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<td>-2.602</td>
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<td></td>
<td>(2.508)</td>
<td>(2.508)</td>
<td>(1.843)</td>
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<td><strong>ΔReal GDP per capita, (fd.)</strong></td>
<td>-0.499</td>
<td>-0.499</td>
<td>-0.397**</td>
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<td>(0.270)</td>
<td>(0.108)</td>
<td>(0.282)</td>
</tr>
<tr>
<td><strong>ΔPopulation, logs (fd.)</strong></td>
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<td>0.057</td>
<td>-1.294+</td>
<td>5.517+</td>
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<tr>
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<td>(1.543)</td>
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<td>-0.218</td>
<td>-0.218</td>
<td>-0.218</td>
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<td>(1.023)</td>
<td>(0.263)</td>
<td>(1.023)</td>
</tr>
<tr>
<td><strong>ΔOpenness, (fd.)</strong></td>
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<td>-0.545***</td>
<td>-0.868**</td>
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<tr>
<td></td>
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<td>(0.363)</td>
<td>(0.152)</td>
<td>(0.363)</td>
</tr>
<tr>
<td><strong>ΔInflation, % ch. CPI (fd.)</strong></td>
<td>-0.014*</td>
<td>-0.015</td>
<td>-0.014*</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.014)</td>
<td>(0.008)</td>
<td>(0.014)</td>
</tr>
<tr>
<td><strong>ΔEx. rate classification (fd.)</strong></td>
<td>-0.027**</td>
<td>0.012</td>
<td>-0.027**</td>
<td>0.012</td>
</tr>
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<td>(0.027)</td>
</tr>
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<td>-0.053</td>
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<td>-0.181+</td>
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<td>(0.061)</td>
<td>(0.061)</td>
<td>(0.025)</td>
<td>(0.134)</td>
</tr>
</tbody>
</table>

Observations                      | 909 | 909 | 441 | 441 |
R-squared                          | -0.008 | -0.008 | 0.144 | -0.078 |
Number of countries                | 77 | 77 | 51 | 51 |
Cragg-Donald F-statistic           | 24.4 | 24.4 | 11.8 | 11.8 |
Kleibergen-Paap F-statistic       | 3.25 | 3.25 | 0.77 | 0.77 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Variables in first differences
<table>
<thead>
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<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>Arabic aid, pc. of GDP</td>
<td>-0.659**</td>
<td>-1.219***</td>
<td>-0.434*</td>
<td>-0.949***</td>
<td>-10.821*</td>
<td>-9.897**</td>
</tr>
<tr>
<td></td>
<td>(0.307)</td>
<td>(0.413)</td>
<td>(0.239)</td>
<td>(0.366)</td>
<td>(5.563)</td>
<td>(4.941)</td>
</tr>
<tr>
<td>Real GDP per capita</td>
<td>-0.096*</td>
<td>-0.179**</td>
<td>-0.077</td>
<td>-0.226**</td>
<td>-0.109</td>
<td>-0.074</td>
</tr>
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<td></td>
<td>(0.053)</td>
<td>(0.077)</td>
<td>(0.072)</td>
<td>(0.112)</td>
<td>(0.067)</td>
<td>(0.104)</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
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<td>-0.004</td>
<td>0.028</td>
<td>-0.771**</td>
<td>-0.774**</td>
</tr>
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<td></td>
<td>(0.246)</td>
<td>(0.247)</td>
<td>(0.280)</td>
<td>(0.268)</td>
<td>(0.346)</td>
<td>(0.383)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.067***</td>
<td>-0.964***</td>
<td>-0.065**</td>
<td>-0.899***</td>
<td>-0.077**</td>
<td>-1.163*</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.313)</td>
<td>(0.026)</td>
<td>(0.317)</td>
<td>(0.034)</td>
<td>(0.667)</td>
</tr>
<tr>
<td>Constant</td>
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<td>8.768***</td>
<td>1.099*</td>
<td>8.473***</td>
<td>1.154*</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.471)</td>
<td>(2.885)</td>
<td>(0.651)</td>
<td>(3.178)</td>
<td>(0.604)</td>
<td>(0.000)</td>
</tr>
</tbody>
</table>

| Observations                  | 1,141   | 1,141   | 732     | 732     | 409     | 409     |
| Country FE                    | n       | y       | n       | y       | n       | y       |
| Annual FE                     | n       | y       | n       | y       | n       | y       |
| Number of countries           | 77      | 77      | 44      | 44      | 33      | 33      |
| R-sq. (within)                | 0.030   | 0.121   | 0.00918 | 0.124   | 0.106   | 0.219   |
| R-sq. (overall)               | 0.056   | 0.708   | 0.0776  | 0.740   | 0.0878  | 0.703   |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20

Fixed effects coefficients not reported
Table 2.12
Panel regression with arab aid flows
Disaggregated by country type

<table>
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<tr>
<th>VARIABLES</th>
<th>(1) All</th>
<th>(2) All</th>
<th>(3) Type 1</th>
<th>(4) Type 1</th>
<th>(5) Type 2</th>
<th>(6) Type 2</th>
</tr>
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<tbody>
<tr>
<td>Arab aid, pc. of GDP</td>
<td>-0.184</td>
<td>-0.779*</td>
<td>-0.276</td>
<td>-0.051</td>
<td>-13.012*</td>
<td>-13.868**</td>
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<tr>
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<td>(0.243)</td>
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<td>(0.255)</td>
<td>(0.373)</td>
<td>(7.200)</td>
<td>(6.848)</td>
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<td>Oil price</td>
<td>0.006</td>
<td>-0.028</td>
<td>0.018</td>
<td>-0.117</td>
<td>-0.004</td>
<td>0.133</td>
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<td>(0.032)</td>
<td>(0.101)</td>
<td>(0.031)</td>
<td>(0.163)</td>
<td>(0.049)</td>
<td>(0.123)</td>
</tr>
<tr>
<td>Real GDP per capita</td>
<td>0.038</td>
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<td>0.053</td>
<td>0.101</td>
<td>-0.014</td>
<td>-0.439*</td>
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<td>(0.133)</td>
<td>(0.051)</td>
<td>(0.121)</td>
<td>(0.102)</td>
<td>(0.252)</td>
</tr>
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<td>Growth in real GDP per cap.</td>
<td>-0.088</td>
<td>-0.069</td>
<td>0.101</td>
<td>0.075</td>
<td>-0.211</td>
<td>-0.097</td>
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<td>(0.134)</td>
<td>(0.138)</td>
<td>(0.191)</td>
<td>(0.191)</td>
<td>(0.222)</td>
<td>(0.238)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.077***</td>
<td>-0.928**</td>
<td>-0.126***</td>
<td>0.376</td>
<td>-0.076*</td>
<td>-2.041***</td>
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<td>(0.028)</td>
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<td>(0.027)</td>
<td>(0.392)</td>
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<td>(0.680)</td>
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<td>Net capital account, % GDP</td>
<td>-0.033</td>
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<td>-0.716</td>
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<td>1.833</td>
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<td>(0.499)</td>
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<td>-0.492***</td>
<td>-0.495</td>
<td>-0.817***</td>
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<td>Inflation, % ch. CPI</td>
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<td>(0.031)</td>
<td>(0.021)</td>
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<td>-0.077***</td>
<td>-0.069***</td>
<td>-0.068***</td>
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<td>0.959**</td>
<td>0.000</td>
<td>1.242</td>
<td>23.061***</td>
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<td>(0.400)</td>
<td>(0.000)</td>
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<td>y</td>
<td>n</td>
<td>y</td>
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<td>27</td>
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<td>R-sq. (within)</td>
<td>0.21</td>
<td>0.316</td>
<td>0.227</td>
<td>0.340</td>
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<td>R-sq. (overall)</td>
<td>0.095</td>
<td>0.777</td>
<td>0.164</td>
<td>0.753</td>
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</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
Table 2.13
Panel regression with Arab aid flows
Data in first differences
Disaggregated by country type

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<tr>
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<th>(5)</th>
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<td>Type 2</td>
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</table>

<table>
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<tr>
<th>VAR</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
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<tbody>
<tr>
<td>Arab aid, pc. of GDP (fd.)</td>
<td>-0.176+</td>
<td>0.048</td>
<td>-0.148</td>
<td>0.204</td>
<td>-2.227</td>
<td>-7.286+</td>
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<td>(0.135)</td>
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<td>(5.529)</td>
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<td>Oil price (fd.)</td>
<td>-0.046***</td>
<td>0.009</td>
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<td>(0.021)</td>
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<td>(0.025)</td>
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<tr>
<td>Real GDP per capita, (fd.)</td>
<td>-0.376***</td>
<td>-0.401**</td>
<td>-0.331***</td>
<td>-0.269</td>
<td>-0.514***</td>
<td>-0.574*</td>
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<td>(0.083)</td>
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<td>(0.194)</td>
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<td>Growth (fd.)</td>
<td>-0.074</td>
<td>-0.011</td>
<td>-0.133+</td>
<td>-0.099</td>
<td>0.076</td>
<td>0.095</td>
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<td>(0.069)</td>
<td>(0.109)</td>
<td>(0.084)</td>
<td>(0.132)</td>
<td>(0.130)</td>
<td>(0.163)</td>
</tr>
<tr>
<td>Population, logs (fd.)</td>
<td>-0.541*</td>
<td>-1.127+</td>
<td>-0.402+</td>
<td>-0.354</td>
<td>-1.370+</td>
<td>-2.219*</td>
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<td>(0.291)</td>
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<td>(0.278)</td>
<td>(0.548)</td>
<td>(0.933)</td>
<td>(1.332)</td>
</tr>
<tr>
<td>Net capital account, % GDP (fd.)</td>
<td>-0.295**</td>
<td>-0.304***</td>
<td>-2.071+</td>
<td>-0.099</td>
<td>0.076</td>
<td>0.095</td>
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<td>(0.130)</td>
<td>(0.163)</td>
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<td>Openness, (fd.)</td>
<td>-0.576***</td>
<td>-0.622***</td>
<td>-0.481*</td>
<td>-0.014*</td>
<td>-0.015*</td>
<td>-0.010</td>
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<td>(0.266)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.015)</td>
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<td>lnflation, % ch. CPI (fd.)</td>
<td>-0.027***</td>
<td>-0.044***</td>
<td>-0.013</td>
<td>-0.027***</td>
<td>-0.044***</td>
<td>-0.013</td>
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<td>(0.014)</td>
<td>(0.015)</td>
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<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.020)</td>
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<tr>
<td>Ex. rate classification (fd.)</td>
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<td>0.036+</td>
<td>0.016**</td>
<td>0.018</td>
<td>0.043+</td>
<td>0.060+</td>
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<tr>
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<td>(0.008)</td>
<td>(0.022)</td>
<td>(0.008)</td>
<td>(0.018)</td>
<td>(0.028)</td>
<td>(0.042)</td>
</tr>
<tr>
<td>Constant</td>
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<td>0.036+</td>
<td>0.016**</td>
<td>0.018</td>
<td>0.043+</td>
<td>0.060+</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.022)</td>
<td>(0.008)</td>
<td>(0.018)</td>
<td>(0.028)</td>
<td>(0.042)</td>
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<td>604</td>
<td>219</td>
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<td>222</td>
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<td>24</td>
<td>33</td>
<td>27</td>
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<tr>
<td>R-sq.</td>
<td>0.057</td>
<td>0.16</td>
<td>0.069</td>
<td>0.22</td>
<td>0.044</td>
<td>0.14</td>
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</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
## Table 2.14

Panel regression with Adjusted RER Index
Disaggregated by country type

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<th>(2) All</th>
<th>(3) Type 1</th>
<th>(4) Type 1</th>
<th>(5) Type 2</th>
<th>(6) Type 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arab aid, pc. of GDP</td>
<td>-0.696+</td>
<td>-1.763**</td>
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<td>(0.472 )</td>
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<td>(14.295)</td>
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<td>Oil price</td>
<td>0.028</td>
<td>-0.262</td>
<td>0.062</td>
<td>-0.438</td>
<td>-0.001</td>
<td>0.061</td>
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<td>(0.070 )</td>
<td>(0.274 )</td>
<td>(0.075 )</td>
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<td>(0.105 )</td>
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<td>Real GDP per capita</td>
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<td>-0.167</td>
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<td>(0.457 )</td>
<td>(0.457 )</td>
<td>(0.420 )</td>
<td>(0.454 )</td>
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<td>Population, logs</td>
<td>-0.186***</td>
<td>-1.554</td>
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<td>-3.536**</td>
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<td>Net capital account, % GDP</td>
<td>-0.172</td>
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<td>6.872</td>
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<td>(1.233 )</td>
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<td>(1.088 )</td>
<td>(1.196 )</td>
<td>(7.085 )</td>
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<td>Openness, % GDP</td>
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<td>-1.051*</td>
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<td>(0.064 )</td>
<td>(0.059 )</td>
<td>(0.061 )</td>
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<td>(1.239 )</td>
<td>(11.526 )</td>
<td>(1.014 )</td>
<td>(14.859 )</td>
<td>(2.133 )</td>
<td>(18.433 )</td>
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</table>

Observations                      | 584    | 584    | 286       | 286       | 298        | 298        |
Country FE                        | n      | y      | n         | y         | n          | y          |
Annual FE                         | n      | y      | n         | y         | n          | y          |
Number of countries               | 51     | 51     | 24        | 24        | 27         | 27         |
R-sq. (within)                    | 0.21   | 0.309  | 0.245     | 0.348     | 0.238      | 0.467      |
R-sq. (overall)                   | 0.094  | 0.775  | 0.133     | 0.732     | 0.279      | 0.845      |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
### Table 2.15
Panel regression with Adjusted RER Index
Disaggregated by country type
Data in First Differences

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<th>(2) All</th>
<th>(3) Type 1</th>
<th>(4) Type 1</th>
<th>(5) Type 2</th>
<th>(6) Type 2</th>
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</thead>
<tbody>
<tr>
<td>△Arab aid, pc. of GDP (fd.)</td>
<td>0.276</td>
<td>0.385</td>
<td>0.639+</td>
<td>0.442</td>
<td>-18.556+</td>
<td>-16.937+</td>
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<td>(0.363)</td>
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<td>(0.659)</td>
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<td>△Oil price (fd.)</td>
<td>0.007</td>
<td>0.568</td>
<td>-0.009</td>
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<td>-0.014</td>
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<td>(1.077)</td>
<td>(0.067)</td>
<td>(1.445)</td>
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<td>△Real GDP per capita, (fd.)</td>
<td>-0.979**</td>
<td>-1.299***</td>
<td>-0.735+</td>
<td>-1.212**</td>
<td>-1.333*</td>
<td>-1.664**</td>
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<tr>
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<td>(0.435)</td>
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<td>(0.517)</td>
<td>(0.551)</td>
<td>(0.755)</td>
<td>(0.801)</td>
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<tr>
<td>△Growth (fd.)</td>
<td>0.068</td>
<td>0.174</td>
<td>-0.088</td>
<td>0.170</td>
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<td>(0.245)</td>
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<td>△Population, logs (fd.)</td>
<td>-2.048</td>
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<td>-2.915</td>
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<td>(2.079)</td>
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<td>△Net capital account, % GDP (fd.)</td>
<td>-1.072***</td>
<td>-1.336***</td>
<td>-1.119***</td>
<td>-1.588***</td>
<td>-5.616+</td>
<td>-3.883</td>
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<td>(0.318)</td>
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<td>(0.345)</td>
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<td>(4.578)</td>
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<td>△Openness, (fd.)</td>
<td>-1.351***</td>
<td>-1.443***</td>
<td>-1.500***</td>
<td>-1.423***</td>
<td>-1.049+</td>
<td>-1.133*</td>
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<td>(0.369)</td>
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<td>(0.399)</td>
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<td>△Inflation, % ch. CPI (fd.)</td>
<td>-0.035*</td>
<td>-0.035*</td>
<td>-0.039*</td>
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<td>-0.023</td>
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<td>(0.037)</td>
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<td>△Ex. rate classification (fd.)</td>
<td>-0.054*</td>
<td>-0.069**</td>
<td>-0.099***</td>
<td>-0.098**</td>
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<td>(0.051)</td>
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<td>R-sq. (within)</td>
<td>0.14</td>
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<td>R-sq. (overall)</td>
<td>0.15</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20

Fixed effects coefficients not reported
Table 2.16
Panel regression with RER Index not adjusted for Balassa-Samuelson effect
Disaggregated by country type

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<td>Arab aid, pc. of GDP</td>
<td>0.240</td>
<td>-0.779*</td>
<td>0.111</td>
<td>-0.051</td>
<td>-15.828**</td>
<td>-13.868**</td>
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<td>(0.297)</td>
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<td>(6.848)</td>
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<td>Oil price</td>
<td>0.196***</td>
<td>-0.127</td>
<td>0.214***</td>
<td>-0.217</td>
<td>0.169***</td>
<td>0.033</td>
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<td>(0.031)</td>
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<td>(0.033)</td>
<td>(0.163)</td>
<td>(0.047)</td>
<td>(0.123)</td>
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<tr>
<td>Real GDP per capita</td>
<td>0.091+</td>
<td>0.082</td>
<td>0.152**</td>
<td>0.236*</td>
<td>-0.014</td>
<td>-0.304</td>
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<td>(0.061)</td>
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<td>(0.060)</td>
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<td>Growth in real GDP per cap.</td>
<td>0.060</td>
<td>-0.069</td>
<td>0.237</td>
<td>0.075</td>
<td>-0.062</td>
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<td>(0.144)</td>
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<td>(0.200)</td>
<td>(0.191)</td>
<td>(0.229)</td>
<td>(0.238)</td>
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<td>Population, logs</td>
<td>-0.146***</td>
<td>-0.928**</td>
<td>-0.206***</td>
<td>0.376</td>
<td>-0.134***</td>
<td>-2.041***</td>
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<td>(0.034)</td>
<td>(0.460)</td>
<td>(0.043)</td>
<td>(0.392)</td>
<td>(0.044)</td>
<td>(0.680)</td>
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<td>Net capital account, % GDP</td>
<td>-0.822</td>
<td>-0.162</td>
<td>-0.948</td>
<td>-0.716</td>
<td>1.876</td>
<td>1.833</td>
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<td>(0.664)</td>
<td>(0.632)</td>
<td>(0.612)</td>
<td>(0.482)</td>
<td>(2.996)</td>
<td>(3.180)</td>
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<td>Openness, % GDP</td>
<td>-0.528**</td>
<td>-0.514***</td>
<td>-0.367**</td>
<td>-0.492***</td>
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<td>-0.817***</td>
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<td>(0.216)</td>
<td>(0.252)</td>
<td>(0.149)</td>
<td>(0.171)</td>
<td>(0.315)</td>
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<td>Inflation, % ch. CPI</td>
<td>0.010</td>
<td>-0.002</td>
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<td>(0.021)</td>
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<td>Ex. rate classification</td>
<td>-0.089***</td>
<td>-0.077***</td>
<td>-0.082***</td>
<td>-0.068***</td>
<td>-0.102***</td>
<td>-0.129***</td>
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<td>(0.024)</td>
<td>(0.024)</td>
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<td>Constant</td>
<td>-0.551</td>
<td>7.643</td>
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<td>-6.657</td>
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<td>21.232***</td>
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<td>(0.513)</td>
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<td>(0.804)</td>
<td>(7.853)</td>
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Observations 584 584 286 286 298 298
Country FE n y n y n y
Annual FE n y n y n y
Number of countries 51 51 24 24 27 27
R-sq. (within) 0.46 0.582 0.483 0.661 0.476 0.669
R-sq. (overall) 0.22 0.826 0.341 0.850 0.372 0.868

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
Table 2.17

Panel regression with RER Index not adjusted for Balassa-Samuelson effect
Disaggregated by country type
Data in First Differences

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<td>All</td>
<td>Type 1</td>
<td>Type 1</td>
<td>Type 2</td>
<td>Type 2</td>
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<td>△Arab aid, pc. of GDP (fd.)</td>
<td>0.057</td>
<td>0.133</td>
<td>0.239</td>
<td>0.149</td>
<td>-9.036*</td>
<td>-6.130+</td>
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<tr>
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<td>(0.226)</td>
<td>(0.311)</td>
<td>(0.281)</td>
<td>(0.422)</td>
<td>(4.865)</td>
<td>(4.735)</td>
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<td>△Oil price (fd.)</td>
<td>0.040**</td>
<td>0.354</td>
<td>0.035+</td>
<td>0.137</td>
<td>0.026</td>
<td>0.608+</td>
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<td>(0.017)</td>
<td>(0.309)</td>
<td>(0.024)</td>
<td>(0.426)</td>
<td>(0.027)</td>
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<td>△Real GDP per capita, (fd.)</td>
<td>-0.147</td>
<td>-0.386*</td>
<td>0.028</td>
<td>-0.337+</td>
<td>-0.391</td>
<td>-0.560+</td>
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<td>(0.210)</td>
<td>(0.207)</td>
<td>(0.271)</td>
<td>(0.238)</td>
<td>(0.334)</td>
<td>(0.360)</td>
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<tr>
<td>△Growth (fd.)</td>
<td>-0.114</td>
<td>0.024</td>
<td>-0.267+</td>
<td>0.024</td>
<td>0.077</td>
<td>0.141</td>
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<td>(0.122)</td>
<td>(0.106)</td>
<td>(0.162)</td>
<td>(0.134)</td>
<td>(0.163)</td>
<td>(0.163)</td>
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<tr>
<td>△Population, logs (fd.)</td>
<td>-0.645</td>
<td>-0.679</td>
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<td>-0.034</td>
<td>-1.702</td>
<td>-1.741+</td>
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<td>(0.775)</td>
<td>(0.718)</td>
<td>(0.611)</td>
<td>(0.734)</td>
<td>(1.427)</td>
<td>(1.309)</td>
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<td>△Net capital account, % GDP (fd.)</td>
<td>-0.316**</td>
<td>-0.380***</td>
<td>-0.305**</td>
<td>-0.472***</td>
<td>-3.242**</td>
<td>-1.475</td>
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<td>(0.154)</td>
<td>(0.128)</td>
<td>(0.130)</td>
<td>(0.124)</td>
<td>(1.443)</td>
<td>(1.637)</td>
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<td>△Openness, (fd.)</td>
<td>-0.565***</td>
<td>-0.619***</td>
<td>-0.661***</td>
<td>-0.581***</td>
<td>-0.386+</td>
<td>-0.512**</td>
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<td>(0.157)</td>
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<td>(0.172)</td>
<td>(0.182)</td>
<td>(0.298)</td>
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<tr>
<td>△Inflation, % ch. CPI (fd.)</td>
<td>-0.011+</td>
<td>-0.014+</td>
<td>-0.013+</td>
<td>-0.011</td>
<td>-0.002</td>
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<td>(0.008)</td>
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<td>(0.010)</td>
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<tr>
<td>△Ex. rate classification (fd.)</td>
<td>-0.028**</td>
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<td>-0.047***</td>
<td>-0.042**</td>
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<td>(0.018)</td>
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<td>(0.200)</td>
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Observations | 441 | 441 | 219 | 219 | 222 | 222 |
Annual FE | n | y | n | y | n | n |
Number of countries | 51 | 51 | 24 | 24 | 27 | 27 |
R-sq. (within) | 0.11 | 0.35 | 0.19 | 0.50 | 0.10 | 0.32 |
R-sq. (overall) | 0.12 | 0.34 | 0.20 | 0.48 | 0.11 | 0.32 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
**Table 2.18**

Panel regression with arab aid flows with *de jure* financial openness

Disaggregated by country type

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<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<td>Arab aid, pc. of GDP</td>
<td>-0.164</td>
<td>-0.785*</td>
<td>-0.262</td>
<td>0.027</td>
<td>-13.811**</td>
<td>-13.909**</td>
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<td>(0.261)</td>
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<td>(6.834)</td>
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<tr>
<td>Oil price</td>
<td>0.003</td>
<td>-0.039</td>
<td>0.007</td>
<td>-0.132</td>
<td>-0.005</td>
<td>0.135</td>
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<td>(0.028)</td>
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<tr>
<td>Real GDP per capita</td>
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<td>0.098</td>
<td>-0.046</td>
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<td>(0.055)</td>
<td>(0.119)</td>
<td>(0.090)</td>
<td>(0.262)</td>
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<td>Growth in real GDP per cap.</td>
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<td>-0.058</td>
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<td>(0.253)</td>
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<td>Population, logs</td>
<td>-0.079***</td>
<td>-0.892*</td>
<td>-0.119***</td>
<td>0.499</td>
<td>-0.076*</td>
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<td>(0.030)</td>
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<td>(0.390)</td>
<td>(0.043)</td>
<td>(0.670)</td>
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<td>Net capital account, % GDP</td>
<td>-0.207</td>
<td>-0.285</td>
<td>-0.531</td>
<td>-0.967***</td>
<td>3.602</td>
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<td>(0.444)</td>
<td>(0.477)</td>
<td>(3.428)</td>
<td>(3.271)</td>
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<tr>
<td>Openness, % GDP</td>
<td>-0.409*</td>
<td>-0.474*</td>
<td>-0.247**</td>
<td>-0.400**</td>
<td>-0.523</td>
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<td>Inflation, % ch. CPI</td>
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<td>0.002</td>
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<td>-0.005</td>
<td>0.022</td>
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<td>(0.015)</td>
<td>(0.018)</td>
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<td>Ex. rate classification</td>
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<td>-0.069***</td>
<td>-0.070***</td>
<td>-0.084***</td>
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<td>(0.022)</td>
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<td>(0.038)</td>
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<td>22.806***</td>
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Observations 564 564 271 271 293 293
Country FE n y n y n y
Annual FE n y n y n y
Number of countries 51 51 24 24 27 27
R-sq. (within) 0.21 0.309 0.208 0.340 0.251 0.510
R-sq. (overall) 0.10 0.779 0.152 0.769 0.308 0.848

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
### Table 2.19

Panel regression with Arab aid flows
Data in first differences with *de jure* financial openness
Disaggregated by country type

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<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta$Arab aid, pc. of GDP (fd.)</td>
<td>$-0.136$</td>
<td>$0.041$</td>
<td>$-0.101$</td>
<td>$0.203$</td>
<td>$-2.005$</td>
<td>$9.188^+$</td>
</tr>
<tr>
<td></td>
<td>$(0.132)$</td>
<td>$(0.243)$</td>
<td>$(0.126)$</td>
<td>$(0.287)$</td>
<td>$(3.047)$</td>
<td>$(5.730)$</td>
</tr>
<tr>
<td>$\Delta$Chinn-Ito Index</td>
<td>$-0.010$</td>
<td>$0.003$</td>
<td>$-0.020$</td>
<td>$-0.001$</td>
<td>$0.003$</td>
<td>$-0.006$</td>
</tr>
<tr>
<td></td>
<td>$(0.015)$</td>
<td>$(0.018)$</td>
<td>$(0.018)$</td>
<td>$(0.029)$</td>
<td>$(0.025)$</td>
<td>$(0.029)$</td>
</tr>
<tr>
<td>$\Delta$Oil price (fd.)</td>
<td>$-0.045^{***}$</td>
<td>$0.003$</td>
<td>$-0.057^{***}$</td>
<td>$-0.016$</td>
<td>$-0.018$</td>
<td>$0.006$</td>
</tr>
<tr>
<td></td>
<td>$(0.014)$</td>
<td>$(0.014)$</td>
<td>$(0.013)$</td>
<td>$(0.018)$</td>
<td>$(0.030)$</td>
<td>$(0.024)$</td>
</tr>
<tr>
<td>$\Delta$Real GDP per capita, (fd.)</td>
<td>$-0.415^{***}$</td>
<td>$-0.424^{***}$</td>
<td>$-0.366^{***}$</td>
<td>$-0.261$</td>
<td>$-0.586^{**}$</td>
<td>$-0.643^*$</td>
</tr>
<tr>
<td></td>
<td>$(0.089)$</td>
<td>$(0.205)$</td>
<td>$(0.099)$</td>
<td>$(0.245)$</td>
<td>$(0.249)$</td>
<td>$(0.345)$</td>
</tr>
<tr>
<td>$\Delta$Growth (fd.)</td>
<td>$-0.051$</td>
<td>$-0.002$</td>
<td>$-0.127^+$</td>
<td>$-0.103$</td>
<td>$0.168^+$</td>
<td>$0.132$</td>
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<tr>
<td></td>
<td>$(0.075)$</td>
<td>$(0.111)$</td>
<td>$(0.090)$</td>
<td>$(0.140)$</td>
<td>$(0.130)$</td>
<td>$(0.168)$</td>
</tr>
<tr>
<td>$\Delta$Population, logs (fd.)</td>
<td>$-0.465^*$</td>
<td>$-0.863$</td>
<td>$-0.281$</td>
<td>$0.026$</td>
<td>$-1.621^*$</td>
<td>$-2.143^+$</td>
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<tr>
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<td>$(0.279)$</td>
<td>$(0.684)$</td>
<td>$(0.245)$</td>
<td>$(0.400)$</td>
<td>$(0.977)$</td>
<td>$(1.308)$</td>
</tr>
<tr>
<td>$\Delta$Net capital account, % GDP (fd.)</td>
<td>$-0.341^{**}$</td>
<td>$-0.348^{***}$</td>
<td>$-2.182^+$</td>
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</tr>
<tr>
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<td>$(0.091)$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta$Openness, (fd.)</td>
<td>$-0.523^{***}$</td>
<td>$-0.535^{***}$</td>
<td>$-0.465^*$</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>$(0.155)$</td>
<td>$(0.181)$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta$Inflation, % ch. CPI (fd.)</td>
<td>$-0.013^*$</td>
<td>$-0.015^*$</td>
<td>$-0.009$</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>$(0.008)$</td>
<td>$(0.009)$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta$Ex. rate classification (fd.)</td>
<td>$-0.027^{**}$</td>
<td>$-0.044^{***}$</td>
<td>$0.015$</td>
<td></td>
<td></td>
<td></td>
</tr>
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<td>$(0.016)$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>$0.018^{**}$</td>
<td>$0.029^+$</td>
<td>$0.013^{**}$</td>
<td>$0.006$</td>
<td>$0.047^+$</td>
<td>$0.060^+$</td>
</tr>
<tr>
<td></td>
<td>$(0.008)$</td>
<td>$(0.022)$</td>
<td>$(0.007)$</td>
<td>$(0.015)$</td>
<td>$(0.030)$</td>
<td>$(0.042)$</td>
</tr>
<tr>
<td>Observations</td>
<td>$846$</td>
<td>$425$</td>
<td>$559$</td>
<td>$207$</td>
<td>$287$</td>
<td>$218$</td>
</tr>
<tr>
<td>Number of countries</td>
<td>$77$</td>
<td>$51$</td>
<td>$44$</td>
<td>$24$</td>
<td>$33$</td>
<td>$27$</td>
</tr>
<tr>
<td>R-sq.</td>
<td>$0.055$</td>
<td>$0.14$</td>
<td>$0.075$</td>
<td>$0.19$</td>
<td>$0.039$</td>
<td>$0.15$</td>
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</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Table 2.20
Panel regression with Arab aid flows with UN data
Disaggregated by country type

<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>Arab aid, pc. of GDP</td>
<td>0.083</td>
<td>-0.689</td>
<td>-0.152</td>
<td>0.072</td>
<td>-11.556</td>
<td>-12.081*</td>
</tr>
<tr>
<td></td>
<td>(0.255)</td>
<td>(0.453)</td>
<td>(0.286)</td>
<td>(0.402)</td>
<td>(7.184)</td>
<td>(6.888)</td>
</tr>
<tr>
<td>Oil price</td>
<td>-0.014</td>
<td>-0.025</td>
<td>-0.001</td>
<td>-0.120</td>
<td>-0.032</td>
<td>0.105</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.105)</td>
<td>(0.029)</td>
<td>(0.177)</td>
<td>(0.048)</td>
<td>(0.125)</td>
</tr>
<tr>
<td>Real GDP per capita (UN)</td>
<td>0.061</td>
<td>0.034</td>
<td>0.076</td>
<td>0.161</td>
<td>-0.011</td>
<td>-0.262</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.122)</td>
<td>(0.050)</td>
<td>(0.134)</td>
<td>(0.064)</td>
<td>(0.210)</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
<td>-0.088</td>
<td>-0.064</td>
<td>0.105</td>
<td>0.104</td>
<td>-0.214</td>
<td>-0.151</td>
</tr>
<tr>
<td></td>
<td>(0.147)</td>
<td>(0.143)</td>
<td>(0.210)</td>
<td>(0.196)</td>
<td>(0.243)</td>
<td>(0.238)</td>
</tr>
<tr>
<td>Population, logs</td>
<td>-0.059**</td>
<td>-0.878**</td>
<td>-0.086***</td>
<td>0.390</td>
<td>-0.067</td>
<td>-1.870***</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.440)</td>
<td>(0.028)</td>
<td>(0.344)</td>
<td>(0.049)</td>
<td>(0.641)</td>
</tr>
<tr>
<td>Net capital account, % GDP</td>
<td>-0.105</td>
<td>0.028</td>
<td>-0.348</td>
<td>-0.553</td>
<td>2.749</td>
<td>2.271</td>
</tr>
<tr>
<td></td>
<td>(0.468)</td>
<td>(0.564)</td>
<td>(0.421)</td>
<td>(0.417)</td>
<td>(3.348)</td>
<td>(3.488)</td>
</tr>
<tr>
<td>Openness, % GDP</td>
<td>-0.454**</td>
<td>-0.528**</td>
<td>-0.378**</td>
<td>-0.537***</td>
<td>-0.504</td>
<td>-0.742*</td>
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<tr>
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<td>(0.208)</td>
<td>(0.255)</td>
<td>(0.149)</td>
<td>(0.181)</td>
<td>(0.311)</td>
<td>(0.395)</td>
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<td>Inflation, % ch. CPI</td>
<td>-0.007</td>
<td>-0.002</td>
<td>-0.021</td>
<td>-0.009</td>
<td>0.010</td>
<td>0.014</td>
</tr>
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<td>(0.014)</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.020)</td>
<td>(0.030)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Ex. rate classification</td>
<td>-0.073***</td>
<td>-0.081***</td>
<td>-0.075***</td>
<td>-0.074***</td>
<td>-0.075**</td>
<td>-0.130***</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.029)</td>
<td>(0.030)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.610+</td>
<td>8.563*</td>
<td>0.697*</td>
<td>0.000</td>
<td>1.281*</td>
<td>19.627***</td>
</tr>
<tr>
<td></td>
<td>(0.417)</td>
<td>(4.600)</td>
<td>(0.420)</td>
<td>(0.000)</td>
<td>(0.765)</td>
<td>(6.966)</td>
</tr>
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<td>Observations</td>
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<td>558</td>
<td>276</td>
<td>276</td>
<td>282</td>
<td>282</td>
</tr>
<tr>
<td>Country FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Annual FE</td>
<td>n</td>
<td>y</td>
<td>n</td>
<td>y</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Number of countries</td>
<td>47</td>
<td>47</td>
<td>23</td>
<td>23</td>
<td>24</td>
<td>24</td>
</tr>
<tr>
<td>R-sq. (within)</td>
<td>0.610</td>
<td>0.315</td>
<td>0.226</td>
<td>0.350</td>
<td>0.229</td>
<td>0.498</td>
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<tr>
<td>R-sq. (overall)</td>
<td>0.12</td>
<td>0.737</td>
<td>0.130</td>
<td>0.713</td>
<td>0.268</td>
<td>0.817</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
Fixed effects coefficients not reported
Table 2.21
Panel regression with arab aid flows
Data in first differences with UN data
Disaggregated by country type

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<tbody>
<tr>
<td></td>
<td>All</td>
<td>All</td>
<td>Type 1</td>
<td>Type 1</td>
<td>Type 2</td>
<td>Type 2</td>
</tr>
<tr>
<td>△Arab aid, pc. of GDP (fd.)</td>
<td>-0.155</td>
<td>-0.015</td>
<td>-0.141</td>
<td>0.100</td>
<td>-1.535</td>
<td>-6.748</td>
</tr>
<tr>
<td></td>
<td>(0.126)</td>
<td>(0.259)</td>
<td>(0.119)</td>
<td>(0.294)</td>
<td>(2.909)</td>
<td>(5.608)</td>
</tr>
<tr>
<td>△Oil price (fd.)</td>
<td>-0.051***</td>
<td>0.006</td>
<td>-0.060***</td>
<td>-0.025</td>
<td>-0.035</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.014)</td>
<td>(0.021)</td>
<td>(0.030)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>△Real GDP per capita, (fd.)</td>
<td>-0.288***</td>
<td>-0.200</td>
<td>-0.164+</td>
<td>0.170</td>
<td>-0.604**</td>
<td>-0.592**</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
<td>(0.179)</td>
<td>(0.123)</td>
<td>(0.155)</td>
<td>(0.250)</td>
<td>(0.298)</td>
</tr>
<tr>
<td>△Growth (fd.)</td>
<td>-0.165**</td>
<td>-0.120+</td>
<td>-0.236***</td>
<td>-0.243**</td>
<td>-0.034</td>
<td>-0.041</td>
</tr>
<tr>
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<td>(0.068)</td>
<td>(0.080)</td>
<td>(0.091)</td>
<td>(0.097)</td>
<td>(0.100)</td>
<td>(0.125)</td>
</tr>
<tr>
<td>△Population, logs (fd.)</td>
<td>-0.413+</td>
<td>-0.888</td>
<td>-0.362+</td>
<td>0.062</td>
<td>-1.256+</td>
<td>-1.904*</td>
</tr>
<tr>
<td></td>
<td>(0.286)</td>
<td>(0.724)</td>
<td>(0.276)</td>
<td>(0.525)</td>
<td>(0.865)</td>
<td>(1.131)</td>
</tr>
<tr>
<td>△Net capital account, % GDP (fd.)</td>
<td>-0.293***</td>
<td>-0.285***</td>
<td>-2.270*</td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>(0.111)</td>
<td>(0.092)</td>
<td></td>
<td></td>
<td>(1.345)</td>
<td></td>
</tr>
<tr>
<td>△Openness, (fd.)</td>
<td>-0.579***</td>
<td>-0.600***</td>
<td>-0.513*</td>
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<tr>
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<td>(0.159)</td>
<td>(0.176)</td>
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<td></td>
<td>(0.282)</td>
<td></td>
</tr>
<tr>
<td>△Inflation, % ch. CPI (fd.)</td>
<td>-0.013*</td>
<td>-0.011+</td>
<td>-0.011</td>
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<tr>
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<td>(0.008)</td>
<td>(0.008)</td>
<td></td>
<td></td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>△Ex. rate classification (fd.)</td>
<td>-0.025+</td>
<td>-0.048***</td>
<td>-0.015</td>
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<td></td>
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<tr>
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<td>(0.016)</td>
<td>(0.018)</td>
<td></td>
<td></td>
<td>(0.019)</td>
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</tr>
<tr>
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<td>0.027+</td>
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<td>0.043*</td>
<td>0.054+</td>
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<td>(0.021)</td>
<td>(0.008)</td>
<td>(0.017)</td>
<td>(0.026)</td>
<td>(0.034)</td>
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</table>

Observations   857  424  571  212  286  212  
Number of countries  70  47  40  23  30  24  
R-sq.   0.050  0.15  0.058  0.20  0.060  0.17  
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.20
2.A Summary statistics

Table 2.22: Summary statistics: dependent variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>rer_{it}</td>
<td>0.009</td>
<td>0.476</td>
<td>-2.032</td>
<td>2.449</td>
<td>4005</td>
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<tr>
<td>rer_{unadjusted}</td>
<td>-0.797</td>
<td>0.518</td>
<td>-3.052</td>
<td>1.614</td>
<td>4005</td>
</tr>
<tr>
<td>rer_{base} \text{d, all aid recipients}</td>
<td>-0.075</td>
<td>0.86</td>
<td>-4.599</td>
<td>5.026</td>
<td>2834</td>
</tr>
<tr>
<td>rer_{base} \text{d, Arab aid recipients}</td>
<td>-0.135</td>
<td>0.91</td>
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<td>4.009</td>
<td>1141</td>
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<tr>
<td>rer_{f} \text{, Arab aid recipients}</td>
<td>0.016</td>
<td>0.445</td>
<td>-1.987</td>
<td>2.535</td>
<td>3501</td>
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</table>

Table 2.23: Summary statistics: independent variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Real GDP per capita</td>
<td>8.259</td>
<td>1.053</td>
<td>5.032</td>
<td>11.624</td>
<td>4005</td>
</tr>
<tr>
<td>Growth in real GDP per cap.</td>
<td>0.013</td>
<td>0.085</td>
<td>-1.051</td>
<td>0.838</td>
<td>3949</td>
</tr>
<tr>
<td>Population, logs</td>
<td>8.178</td>
<td>2.02</td>
<td>3.666</td>
<td>14.054</td>
<td>4337</td>
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<tr>
<td>Net capital account, % GDP</td>
<td>0.009</td>
<td>0.045</td>
<td>-0.18</td>
<td>1.35</td>
<td>2640</td>
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<tr>
<td>Openness, % GDP</td>
<td>0.768</td>
<td>0.455</td>
<td>0.053</td>
<td>3.754</td>
<td>3435</td>
</tr>
<tr>
<td>Chinn-Ito Index</td>
<td>-0.354</td>
<td>1.373</td>
<td>-1.844</td>
<td>2.478</td>
<td>3285</td>
</tr>
<tr>
<td>Average tariffs, % of GDP</td>
<td>12.646</td>
<td>6.724</td>
<td>0</td>
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</tr>
<tr>
<td>Inflation, % ch. CPI</td>
<td>2.342</td>
<td>1.228</td>
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</tr>
<tr>
<td>Interstate conflict</td>
<td>0.011</td>
<td>0.105</td>
<td>0</td>
<td>1</td>
<td>4430</td>
</tr>
</tbody>
</table>
2.B Deriving the price index

The consumption-based price index $P$ is the minimum expenditure $Y = P_T C_T + P_NC_N + P_TD_T$ such that $C = \omega(C_N, C_T) = 1$ given prices $P_N, P_T$, where $\omega(\cdot)$ is a linear-homogenous function. We claim the price index $P$ satisfies this definition:

$$P = P_N^\theta P_T^{1-\theta} \iff \frac{P}{P_T} = \left(\frac{P_N}{P_T}\right)^\theta$$

To start, note that the demands that maximize consumption $C$ given spending $Y$ are

$$C_N = \frac{\theta Y}{P_N} \quad C_T = \frac{(1-\theta)Y}{P_T}$$

The representative agent maximises the function $U(C_N, C_T) = C_N^\theta C_T^{1-\theta}$, so that the highest value of $U(\cdot)$ is given by

$$\left(\frac{\theta Y}{P_N}\right)\theta \left[\frac{(1-\theta)Y}{P_T}\right]^{1-\theta}$$

Because $P$ is the minimum expenditure such that $C = 1$,

$$1 = \left(\frac{\theta Y}{P_N}\right)\theta \left[\frac{(1-\theta)Y}{P_T}\right]^{1-\theta}$$

then

$$P = \left(\frac{\theta}{P_N}\right)^{-\theta} \left[\frac{1-\theta}{P_T}\right]^{\theta-1}$$

and

$$\frac{P}{P_T} = \left(\frac{P_N}{P_T}\right)^{\theta} \left(\frac{1-\theta}{\theta}\right)^\theta \frac{1}{1-\theta}$$

which is proportional to the price index $(P_N/P_T)^\theta$ as claimed, with constant proportionality.
Chapter 3

Household Income and the Demand for Formal Saving: Is Access or Poverty the Binding Constraint?

Chapters 1 and 2 explored the effect of different formulations of foreign aid on government spending and the effect of aggregate aid flows on relative prices in recipient countries. These questions have clear relevance at the policy level, but are concerned with the effect of policies on states, rather than what Sen (1988) famously argued must be the primary units of (development) economic analysis: the household and the individual.

In this chapter, we turn our attention to the effect on rural Vietnamese households of a discrete national policy innovation. In doing so, we are not explicitly studying the effect of foreign aid on macroeconomic aggregates but instead exploring the relevant and measurable microeconomic effects of a specific change in macroeconomic policy.

3.1 Motivation

Because households use savings to smooth consumption after negative income shocks or to invest in or expand a business, increasing access to formal savings products has become an important element of contemporary development policy, particularly through changing banking legislation, creating new pro-poor banking institutions, and investing in financial literacy campaigns. The motivating assumption is that households want to save using formal savings technologies but cannot because such products are either not available or are too expensive. Surprisingly, this assumption has not been widely examined in
the development economics literature.

Investigating to what extent poverty or access is the binding constraint on ownership of formal savings products is a timely research question. While the first-wave of microfinance research focused on access to credit, pro-poor financial innovation is now broadening in scope to include savings technologies. Some institutions have been successful in rolling out savings products: as early as 1996, Bank Rakyat Indonesia had over 16 million deposit savings accounts. But a survey of microfinance institutions analysed by Reille and Kneiding (2009) suggests this was exceptional, since all institutions surveyed offered a credit product of some kind but less than a third offered any kind of savings account.

As Morduch (2000) points out, without local currency-denominated assets (deposits), microfinance institutions are exposed to exchange rate movements and reliant on the availability of credit or donor funds. Robinson (1995) argues that deposit accounts are good for banks because they provide funds for new loans, and Wright et al. (1997) summarise evidence demonstrating welfare gains accruing to low-income households from having savings accounts that are collateral for further lending, buffers against consumption volatility, or as a source of low-cost capital for investments or to meet urgent consumption needs. Because of benefits to both institutions and households, the donor community is encouraging the move towards savings products: the Bill & Melinda Gates Foundation recently earmarked over 20 million dollars for microfinance institutions to increase the supply of small-scale savings products (The Economist Magazine, 2010).

This paper uses an exogenous, positive shock to household income in Vietnam to study to what extent availability of savings products is the relevant constraint. If poor households exposed to an increase in income are able to buy formal savings products, it suggests the relevant constraint is poverty, not access; if increases in income are not associated with a higher likelihood of saving formally, then increasing access remains at least a necessary condition.

### 3.1.1 Related literature

Browning and Lusardi (1996) provide a useful taxonomy of economic models of why households save out of current income, for example as a precautionary buffer against negative income shocks. When household income is low and volatile, precautionary savings are welfare enhancing: if consumption equals income, households cannot store assets against future shortfalls and are totally exposed to bad income realisations.
While there is broad consensus on this point, disagreement remains about the long-run impact of negative income shocks. At one extreme are papers like Dercon and Hoddinott (2003), who study the effect of weather shocks on households in Zimbabwe and find that because families cannot smooth consumption across states of nature, even a modest bad shock has long-run consequences by pushing the household into a poverty-trap of permanently lower consumption. Gertler and Gruber (2002) construct a health index using Indonesian panel data and show that non-medical consumption declines significantly when the head of a household gets sick. Other researchers, for example Jalan and Ravallion (1999), argue that temporary shocks do not create these long-term poverty traps, but at least agree there is a large difference in the speed at which poorer households converge back to their pre-shock levels of consumption relative to wealthier households or those that can self-insure with savings products.

In addition to income-smoothing, saving creates a store of liquid assets that can be used for investment. Robinson and Dupas (2009) focus on the effects of access to savings on entrepreneurship using on a field experiment in Western Kenya in which they offer a randomly-selected group of women market vendors access to a savings product with no fixed withdrawal costs. Relative to the control group, the treatment group with access to lower fixed-cost savings products had higher incomes six months after the intervention with an implied return on capital of more than 5%. The authors conclude that these vendors want to store their savings safely and use them for investment, and suggest that high fixed costs remain the key barrier to access.

Indeed, savings accounts’ best attribute might simply be that they are a safe way to store income. In a randomised trial, Karlan and Zinman (2007) varied the interest rate offered to households that open a savings account. Variations of more than 1% in a sample of over 7,000 offers did not significantly affect the probability a household would accept the offer, providing preliminary evidence that returns are not driving demand for savings products. This impression is corroborated in Bangladeshi data by Wright and Muteesassira (2001) who emphasize the benefits of savings accounts by presenting surprising statistics on the costliness of informal and semi-formal savings technologies. In their data, the average accrued costs from saving using formal sector accounts was 15% of the amount saved; the same statistic for informal sector accounts was 25%, indicating that low-income households are willing to bear high costs to save at all.

The difference between models of savings behaviour based on consumption smoothing relative to life-cycle considerations is the planning horizon. As Modigliani (1986) emphasizes, a life-cycle model is appropriate when we con-
sider households that are finitely-lived but smooth consumption across the entirety of that horizon. In one of the few papers to explicitly contrast and compare the life-cycle and consumption-smoothing motivations for saving, Deaton (1997) explores survey data from developing countries and concludes that smoothing across shocks better fits observed household behaviour.

Collins (2009) summarizes data on household portfolios across many countries, and finds that poor households actually spend relatively large shares of income on “luxuries” like alcohol, tobacco, consumer durables like radios or televisions, and services including funeral rites or wedding celebrations. Banerjee and Duflo (2007) corroborate these findings, focusing on financial behaviour of families living around or below the World Bank’s poverty line of $2 US dollars per day. Their data show that poor households spend large shares of incomes on goods that we expect to have a very high income elasticity of demand. Despite this, they find a strikingly low uptake of savings products among households in survey data from 13 developing countries: 79% of the households in Cote d’Ivoire had a savings account, but this figure is under 14% for other countries in their sample, and less than 1% of poor households in Panama and Peru are banked (uptake of informal savings products like rotating savings and credit associations or self-help groups is also low).

Regardless of theoretical motivation, whether households do save depends on if they can save: that is, household behaviour is constrained by the availability of excess income, and the earnings of poor, rural households are typically low, volatile, and required for current consumption. Whether or not poor households can save is therefore ultimately an empirical question about their consumption, income, and portfolio allocation decisions.

### 3.1.2 An exogenous shock to rice prices in Vietnam

It is difficult to identify the effect of income on savings status: wealthier households are more likely have savings accounts, and having a savings account increases wealth. Researchers must locate a source of exogenous variation in income to overcome this endogeneity. Paxson (1992), for example, use variation in household income due to rainfall to study the effect of exogenous income shocks on household portfolios in Thai panel data. She concludes that households save more out of transitory positive shocks than they do out of permanent income. This paper is conceptually similar, but we do not rely on weather for the variation driving identification.

Beginning in 1986, the Communist Party of Vietnam implemented a series of reforms collectively known as *Doi Moi* to move the country from a collectivist
command economy towards a socialist-oriented market economy, including several measures during the early 1990’s that specifically targeted agricultural production.

Firstly, the Vietnamese government made major changes to external trade policy. The rice export quota increased from less than a million metric tons in 1992 to 4.5 million by 1998, which caused market rice prices to converge to the (higher) world price per kilo. Benjamin and Brandt (2002) argue that the quota did not lead to gray market arbitrage, and Minot and Goletti (2000) find that the export quota was enforced and binding until at least 1995.

Secondly, decree No. 140/TTg of the Vietnamese Government was implemented in March, 1997, and lifted internal restrictions on transport and the trade in rice within Vietnam. Benjamin and Brandt (2002) calculate that before these reforms the 1995 export value of rice was US$269 per metric tonne, while households producing rice in the Mekong Delta earned US$205 per metric tonne, implying a significant 25% tax rate.

Thirdly, Benjamin and Brandt (2002) show that the market for fertiliser was extensively liberalised during this period, causing large real decreases in the price of popular fertilisers. Cheaper fertiliser increases the marginal profitability of inputs allocated to rice farming even if producers do not change their mix of inputs. Niimi et al. (2003) calculate that over 1993-8, the quota on urea, the precursor for chemical fertiliser production, varied between 1.3 and 1.85 million tons, associated with a 19% decrease in the real price of fertiliser. These policy changes reduced the price of a key input to rice production and increased the returns to selling rice.

Finally, in 1994 the law governing the exchange and sale of land (including agricultural land) was reformed. Do and Iyer (2003) summarise the shift in land policies that began at the start of Vietnam’s Doi Moi era: in 1988, agricultural land was de-collectivised, and land use rights were assigned to rural households. In 1993, a formal market for land exchange was created by making the land use rights tradeable through inheritance or transfer by sale or lease. They argue that the reform effectively transferred property from the state to households, and with ownership came incentives for longer-term investments and increased production effort from lower risk of expropriation. Their results show an increase in the share of land allocated to longer-term crops, and it is reasonable to believe that the same incentive effects drove at least part of the increase in Vietnamese rice production over this period.

Some of these interventions increased the supply of rice, others effectively increased the demand. The cumulative effect of this series of national-level
reforms in the 1990's was a significant increase in rice prices, representing an exogenous increase in the incomes of rural rice-producing households. This particular source of exogenous variation in household income has been studied and exploited in other widely-cited papers, most famously by Edmonds and Pavcnik (2005), who use the exogenous positive shock to rice prices to study the effect of changes in household income on the supply of child labour. They find that Vietnamese rice-producing households were less likely to supply child labour, and that the decrease in this probability is statistically robust and economically significant. This paper uses the same exogenous shock to income to determine the size of the effect of income on the probability a household owns a formal savings product.

3.2 A simple model of the decision to own a savings instrument

The smallest administrative unit in Vietnam is the commune, and we observe the saving status of some household \( i \) in commune \( j \) at time period \( t \). The status variable \( S_{ijt} \) is a binary outcome such that

\[
S_{ijt} = \begin{cases} 
1 & \text{ household owns savings instrument,} \\
0 & \text{ otherwise} 
\end{cases}
\]

We would like to be explicit about the link between \( S_{ijt} \) and household characteristics, particularly income. Suppressing subscripts for neatness, \( v \) denotes the value to the household of a formal savings instrument (like a deposit checking account). The household invests in savings instruments if this value exceeds a hurdle level of costs we aggregate and call \( f \). (We can think of \( f \) as collecting costs associated with owning and using the instrument, such as search, transportation, and fixed costs, and the present discounted value of transaction costs). The household owns a savings instrument if its value exceeds these aggregated costs. Since the outcome is binary, the household’s decision rule is

\[
\Pr(S_{ijt} = 1) \Leftrightarrow (v - f > 0)
\]

\(^{1}\text{Note that for our purposes the source of the exogenous variation in rice prices does not matter, since we are only using the change in rice prices to identify the effect of income on the probability the household has a particular type of savings product.}\)
3.2.1 The value of savings instruments

The household can invest income in risky assets like land, crops, or livestock or put money into a risk-free asset in the form of a savings instrument. To get traction, the household has a CARA-type utility function $u(\cdot)$ and access to both a savings instrument certain gross return $R$ and a risky asset that pays an uncertain return $\tilde{R}$, with known mean $E[\tilde{R}] = \tilde{R}$ and variance $E[(\tilde{R} - \tilde{R})^2] = \sigma^2_{\tilde{R}}$. There may be some premium to holding the risky asset: $\tilde{R} - R > 0$.

The household observes an exogenous positive increase in income $y$ and chooses a share $x \in (0, 1)$ to invest to the risky asset, implying $(1 - x)$ is allocated to the risk-free savings instrument. Because we cannot estimate $x$ from the available survey data around the time of the liberalisation in rice prices, we do not model the choice of this portfolio weight and leave it as a free parameter. The value of the household’s portfolio is

$$\tilde{P} = y(x(\tilde{R} - R) + R)$$

with an average return $E(\tilde{P}) = y(x(\tilde{R} - R) + R)$. For any choice of $x > 0$, this portfolio return is risky because of exposure to the risky asset. The household will pay some amount $c$ to remove this risk and have the expected utility of the portfolio with certainty, so the value of $(c \mid x < 1)$ is given by solving

$$u(\tilde{P} - c) = E[u(\tilde{P})]$$

Taking Taylor expansions around $\tilde{P}$ delivers

$$u(\tilde{P}) - u'(\tilde{P})(\tilde{P} - c - \tilde{P}) \approx u(\tilde{P}) + u'(\tilde{P})E(\tilde{P} - \tilde{P}) + u''(\tilde{P})E(\tilde{P} - \tilde{P})^2$$

and using $E(\tilde{P} - \tilde{P})^2 = y^2x^2\sigma^2_{\tilde{R}}$ gives

$$(c \mid x < 1) \approx -\frac{1}{2} \frac{u''(\tilde{P})(yx)^2\sigma^2_{\tilde{R}}}{u'(\tilde{P})} \tag{3.1}$$

If the household did not have access to the savings instrument, it would have $x = 1$ and a portfolio $(y\tilde{R})$. We solve for the equivalent value $(c \mid x = 1)$ by setting up the analogous problem $u(y\tilde{R} - c \mid x = 1) = E[u(y\tilde{R})]$ and taking
Taylor approximations\(^2\) around the expected value\(^3\) of the portfolio, \((y\bar{R})\).

The value of removing portfolio risk to this non-saving household is therefore

\[
(c \mid x = 1) \approx -\frac{1}{2} \frac{u''(y\bar{R})y^2\sigma_R^2}{u'(y\bar{R})}
\]

### 3.2.2 The household’s decision rule

Conceptually, the value of a savings instrument is that it reduces risk, but if risk and return are positively related, this reduction in risk affects expected returns. Formally, a household invests in the savings instrument if the expected utility of doing so satisfies

\[
\mathbb{E}u(\bar{P}) > \mathbb{E}u(y\bar{R})
\]

which is approximately equal to

\[
u[\bar{P} - (c \mid x < 1)] > u[y\bar{R} - (c \mid x = 1)]
\]

The household has CARA preferences, \(u(\cdot)' > 0\) and \(u''(\cdot) < 0\), so for this expression to be positive requires

\[
\bar{P} - (c \mid x < 1) > y\bar{R} - (c \mid x = 1)
\]

We define the value of moving from not owning the savings instrument to doing so as the difference in these terms:

\[
v = (c \mid x = 1) - (c \mid x < 1) - (y\bar{R} - \bar{P})
\]

Reorganising and substituting values for \((c \mid x = 1)\), \((c \mid x < 1)\), and expected portfolio values gives

\(^2\)This is the same as choosing \(x = 1\) in expression 3.1

\(^3\)\(\mathbb{E}(\bar{P} \mid x = 1) = y(1 \cdot (\bar{R} - R) + R)\)
Using $\gamma$ to denote the Arrow-Pratt coefficient of absolute risk aversion, our expression for the value of moving from informal to formal savings is

$$v = \frac{1}{2} u''(yR) y^2 \sigma^2_R + \frac{1}{2} u''(\bar{P})(yx)^2 \sigma^2_R + (\bar{P} - y\bar{R})$$

$$v = \frac{y^2 \gamma \sigma^2_R}{2} (1 - x^2) + (\bar{P} - y\bar{R})$$

(3.2)

### 3.2.3 $v$ when portfolio returns are similar

If the gross returns from investing in risky assets and owning a savings account are roughly the same, $\bar{R} \approx R$. Under this constraint, the value of the savings instrument approximately reduces to

$$v = \frac{y^2 \gamma \sigma^2_R}{2} (1 - x^2)$$

(3.3)

The probability a household owns a savings instrument increases in the difference between this term and the fixed costs $f$,

$$\Pr(S_{ijt} = 1) \Leftrightarrow \frac{y^2 \gamma \sigma^2_R}{2} (1 - x^2) - f > 0$$

Differentiating with respect to right-hand side variables shows

$$\frac{\partial \Pr (S = 1)}{\partial y, \partial \sigma^2_R, \partial \gamma} > 0$$

so households that experience larger positive income shocks, are more risk averse, or face riskier returns are more likely to own savings instruments, while

$$\frac{\partial \Pr (S = 1)}{\partial x, \partial f} < 0$$

so the probability that a household invests in a savings instrument decreases in $x$ and the fixed costs $f$.  

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The value to the non-saving household of owning savings instruments declines exponentially in the households’ choice of $x$, but the income shock $y$, level of risk aversion $\gamma$, and variance of risky returns $\sigma^2$ are level shifts in $v$:

**Figure 3.1:** $v$ for different choices of $x$ and values of $y$

![Figure 3.1](image)

For any given value of the parameters, higher fixed costs $f$ reduce the probability the household owns a savings account:

**Figure 3.2:** $v$ for different choices of $x$ and fixed costs $f$

![Figure 3.2](image)

This is intuitive. Households that choose to hold a larger share of wealth in risky assets would get less benefit from low-risk savings accounts. However, for all choices of $x$, households value formal savings more highly if they experience larger increases in wealth, face riskier returns, and / or are more risk averse. (Extending the decision rule to the case of CRRA preferences is simple and delivers the same qualitative predictions; see appendix 3.A).

### 3.2.4 $v$ when portfolio returns are different

Imposing the constraint of approximately equal expected returns is a strong assumption: livestock and crops should have higher expected returns than formal savings products available to rural households. We relax this assumption and assume $\bar{P} \neq y\bar{R}$. The value of investing in a formal savings product is now
\[ v = \frac{y^2 \gamma \sigma_R^2}{2}(1 - x^2) - y(\bar{R} - \bar{R})(1 - x) \]  

(3.4)

If the rates of return are not approximately equal in expectation, the premium to owning riskier assets like land or livestock reduces the benefit of moving wealth \( y \) into savings, creating a countervailing effect of higher choices of \( x \): if the household’s choice of \( x \) is small, the value of owning a savings instrument increases in the portfolio weight, but if the household prefers to hold a large share of income in the risky asset (\( x \) is large), the value decreases. In the extreme case of \( x = 1 \) the household only owns the risky asset and the result converges to the simple case:

\[
(v \mid x = 1) = 0
\]
\[
(v \mid x = 0) = \frac{y^2 \gamma \sigma_R^2}{2} - y(\bar{R} - \bar{R})
\]

The second term shows the intuitive result that the value of the savings instrument is the same as the case of similar portfolios but now reduced by the value of the risk premium. Differentiating equation 3.4 with respect to the portfolio weight shows

\[
\frac{\partial v}{\partial x} = -\gamma y^2 \sigma_R^2 x + y(\bar{R} - \bar{R})
\]

so \( \frac{\partial v}{\partial x} = 0 \) at \( x = \frac{(\bar{R} - \bar{R})}{\gamma y^2 \sigma_R^2} \). The value of owning a savings instrument is an asymmetric parabola in \( x \). This does not contradict our prediction for the effect of income on the probability of owning a savings instrument because increases in the exogenous income shock \( y \) are level shifts in this function: \( (v \mid y + \epsilon) \) remains everywhere larger than \( (v \mid y) \) for all values of \( x \), as in figure 3.3.

In the case that the return from holding risky assets is different than holding risk-free savings, \( \frac{\partial v}{\partial x} \) changes sign over the interval of \( x \). We see from figure 3.3 that the value of the savings instrument increases in \( x \in (0, \frac{(\bar{R} - \bar{R})}{\gamma y^2 \sigma_R^2}) \) and decreases in higher values of \( x \). The intuition is that for sufficiently high choices of \( x \), the value of the risk premium dominates the household’s higher expected utility from a lower risk profile of returns.

Whether \( \bar{R} \approx R \) or \( \bar{R} \not\approx R \), our testable prediction is the same: increases in income drive up the value of owning a savings instrument regardless of the household’s choice of \( x \), the share of income \( y \) allocated to risky relative to
risk-free assets.

Figure 3.3: $v$ for different choices of $x$ when $\bar{R} \neq R$

\[ x = 0 \quad \frac{(\bar{R}-R)}{\gamma(y+\epsilon)} \quad \frac{(\bar{R}-R)}{\gamma y^2 R} \quad x = 1 \]

3.3 Empirical evidence

Since $v$ increases in the savings shock $y$, households that experienced higher incomes due to the exogenous increase in the price per kilo of rice are more likely to own savings instruments. We cannot take the decision rule directly to the data because we do not observe key parameters, including risk aversion.

We focus on the main result that for any level of the risk premium, risk aversion, or variance of asset returns, the probability that a household owns a savings instrument increases in the size of the income shock. Since higher rice prices are a windfall to rice producing households, we use within-commune, within-year, across household variation in the exposure to rice prices to identify the effect of income on savings status.

3.3.1 Data

The series of policy changes that caused an increase in the market value of rice began around 1993. We exploit household-level survey data before and after this policy shift from the Vietnamese Living Standards Survey (VLSS) implemented by the Government of Vietnam’s Government Statistics Office (GSO) with support from the World Bank. These surveys include household and commune-level questionnaires, and include observations of market prices for a range of food and non-food commodities, including three varieties of rice. A subset of households in each cross-section was surveyed in both rounds, delivering a large-$N$, $T = 2$ panel. (The data files for the 1992/3 and 1997/8 surveys are obtained from the GSO).
Vietnam is administratively organised at the national, province, and commune levels, and our unit of analysis is the household facing rice prices determined at the commune level. We limit our sample to households in the 120 rural communes in the 1992/3 survey round. Thus defined, our universe of data is the 4,305 rural panel households formed by combining data from the two survey rounds, and our regression framework is applied to 1,972 panel households drawn from this sample

\[ \text{see appendix 3.C for details}. \]

### 3.3.2 Savings status, rice prices, and rice production

Section 14.C of the 1992/3 round and 1997/8 round questionnaire ask the respondent (typically the head of the household) whether the household owns different kinds of savings instruments.

We set \( S = 1 \) if the household has at least one of

<table>
<thead>
<tr>
<th>Savings instrument</th>
<th>Response</th>
</tr>
</thead>
<tbody>
<tr>
<td>Savings book at State bank?</td>
<td>1</td>
</tr>
<tr>
<td>Savings book at other type of bank?</td>
<td>2</td>
</tr>
<tr>
<td>Savings book at credit cooperative?</td>
<td>3</td>
</tr>
<tr>
<td>Government Bonds?</td>
<td>4</td>
</tr>
</tbody>
</table>

and \( S = 0 \) otherwise (missing observations for this question do not enter the estimating equation). This distinguishes saving-type households from those that store assets in cash, livestock, or other asset classes. Frequency statistics suggest that there was a significant change in the sample between the two rounds:

<table>
<thead>
<tr>
<th></th>
<th>1992/3</th>
<th>1997/8</th>
</tr>
</thead>
<tbody>
<tr>
<td>( S_{ij} = 1 )</td>
<td>349</td>
<td>974</td>
</tr>
<tr>
<td>( S_{ij} = 0 )</td>
<td>1,729</td>
<td>2,944</td>
</tr>
</tbody>
</table>

Some households earned unexpected windfalls when rice prices rose due to trade liberalisation, and use the combination of commune-level rice prices and household-level rice production to measure the household-level exposure to this positive income shock.

Since household \( i \) only receives a windfall from an increase in market prices to the extent that it produces rice, we use section 9.B to define three measures of rice production measured in log-points: total rice harvested in kilos

\[ \text{The sample is a subset of all households due to missing observations of independent variables} \]
(\(\ln \text{rice}_{\text{harvested}}\)), total rice cultivated (\(\ln \text{rice}_{\text{cultivated}}\)), and total rice sold or bartered (\(\ln \text{rice}_{\text{sold}}\))^5. These measures are imperfectly collinear, since households that produce rice also consume it, and cultivating rice does not automatically imply that a household also sells rice.

Similarly, there is evidence of planting by diktat, in which case the Commune’s People’s Committee of the Communist Party designates a specific area of land for rice cultivation through so-called “land use rights”, increasing the area of rice cultivated and the amount of grain harvested, but not the amount of rice the household sells to the market. We deal with this and related concerns through robustness tests later in the paper.

Table 3.2: Rice production measures, summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean (Std. Dev.)</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>(\ln \text{rice}_{\text{harvested}})</td>
<td>7.34 (0.98)</td>
<td>1.61</td>
<td>10.97</td>
</tr>
<tr>
<td>(\ln \text{rice}_{\text{cultivated}})</td>
<td>8.5 (0.88)</td>
<td>4.61</td>
<td>12.3</td>
</tr>
<tr>
<td>(\ln \text{rice}_{\text{sold}})</td>
<td>6.49 (1.37)</td>
<td>2.2</td>
<td>10.93</td>
</tr>
</tbody>
</table>

In addition to household surveys, the VLSS enumerators recorded three distinct market prices for a range of food and non-food products, ranging from agricultural staples to medication. Relevant for our study are the market prices of paddy rice, glutinous rice, and ordinary rice. Ordinary rice can be boiled and consumed, paddy rice has been harvested but not husked, and glutinous rice is a short-grained variety of rice used in some Vietnamese cuisine that is produced using the same inputs as ordinary rice.

We abstract from the differences between types of rice crop by defining \(\text{price}_{ji}^{\text{nominal}}\) as the (natural log of the) average of the prices of all three varieties across all market observations within each commune; this variable is in log thousands of nominal Vietnamese Dong per kilo.

3.3.3 Covariates

Several household characteristics that may be relevant for ownership of savings instruments are available from the survey data and exhibit within commune, across-household variation, and are included in the estimating equation in vectors we label \(B_{ijt}, W_{ijt},\) and \(H_{ijt}\).

\(B_{ijt}\) includes biographical information about the head of the household: his or her age, level of education, and gender. Several measures of age are available in the survey data; the measure used here is from question 1.A of the 1992/3

^5 All the log values of the rice production variables were defined as \(\ln(1+\cdot)\) to avoid left-censoring the rice production data
and 1997/8 surveys, which is age imputed by the survey enumerator based on the respondent’s year of birth. Education is measured using a categorical variable measuring the highest terminal level of education the household head achieved, ranging from “primary school” (1) to “postgraduate” (6). This question was modified in the second survey round to include higher levels of academic achievement; all levels above postgraduate are top coded to that level for comparability across rounds.

A consistent empirical regularity in the access to finance literature is that wealthier households are more likely to own savings instruments, though we cannot assume that the positive correlation between savings instruments and wealth represents a causal link. Estimating wealth for low income households from survey data is a significant and longstanding area of development economics research, so we proxy for wealth using observable household characteristics. Section 12.C of the 1992/3 and 1997/8 questionnaire lists binary responses for whether respondents’ households include numerous different kinds of assets, ranging from electric irons to televisions. The measure Assets, raw index sums these values, and is higher for households with “more” assets. Despite failing to account for quality, price or depreciation, this measure performs well, and is statistically related to savings status in most specifications. In addition, we include the measure Durable goods index measured in log points constructed by the World Bank / GSO to approximate household wealth from observable durable goods. The two measures are distinct, but substantially related with a covariance of 0.8 across the whole sample.

Household quality and related characteristics included in vector $H_{ijt}$ provide additional indicators of household wealth that are observable but not captured by the durable goods or a raw asset count. Section 6.A allows the survey...
enumerator to record the quality of the dwelling. This is a categorical variable ranging from 1 ("A city house surrounded by a garden") to 7 ("Temporary house or other"). Finally, we also include a binary measure recorded by the enumerator for whether the household was a "farm" household, and the total size of the household (measured by the total number of full time residents, including children).

Table 3.10 presents least-squares estimates of the relationship between information in the vectors of conditioning information and households' savings status. The results are generally consistent: the age of the household head and the asset count index are statistically significant and positively correlated with the probability the household owns a savings instrument.

Examining the final column of table 3.10, we see that these relationships remain statistically significant at the 1% to 5% level even when commune and survey round fixed effects remove variation that is fixed within communes and across households within each survey round. Throughout the paper, we report only heteroscedasticity-robust standard errors clustered at the commune level.

### Table 3.3: Conditioning variables, summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age, head of HH</td>
<td>46.269</td>
<td>13.514</td>
<td>17</td>
<td>94</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>2.517</td>
<td>1.392</td>
<td>0</td>
<td>6</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>1.202</td>
<td>0.401</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>8.811</td>
<td>4.803</td>
<td>1</td>
<td>34</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>6.244</td>
<td>1.335</td>
<td>0.533</td>
<td>10.836</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>1.835</td>
<td>0.371</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>4.359</td>
<td>2.483</td>
<td>1</td>
<td>7</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>1.748</td>
<td>0.434</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>HH size</td>
<td>4.983</td>
<td>1.927</td>
<td>1</td>
<td>16</td>
</tr>
</tbody>
</table>

### 3.4 Econometric framework

The value of owning a formal savings instrument increases in $u$, so we write the value of a savings instrument to the household as a continuous but unobserved index $S_{ijt}^*$ determined by household, commune, and survey-round level observations through a mapping we write compactly as $S_{ijt}^* = \mathbf{x}_{ijt} \zeta + \epsilon_{ijt}$. The normally-distributed error term is orthogonal to $\mathbf{x}$ and may be arbitrarily correlated across households within each commune (all reported standard errors are heteroscedasticity-robust and clustered at the commune level). The binary outcome constructed from VLSS data is related to this underlying index through
where the normalisation to zero is arbitrary if $x$ contains an intercept. This formulation links the simple household decision rule over $v$ to the observed binary outcome:

$$P(v = \{S_{ijt}\} = \Pr(x'\zeta + \epsilon_{ijt}) = \Pr(\epsilon_{ijt} > -x'\zeta) = F(x'\zeta)$$

By explicitly writing out the terms of the compact form $x'\zeta$ using information about prices, rice production, and household characteristics discussed above that affect households’ decisions about owning formal savings instruments, and assuming $F(\cdot)$ is linear in its arguments, we derive the linear probability model

$$S_{ijt} = \alpha + \beta_1 \cdot \ln price_{jt}^{\text{nominal}} + \beta_2 \cdot \ln rice_{ijt}$$
$$+ \beta_3 \cdot (\ln price_{jt}^{\text{nominal}} \times \ln rice_{ijt})$$
$$+ \gamma_1 \cdot B_{ijt} + \gamma_2 \cdot W_{ijt} + \gamma_3 \cdot H_{ijt}$$
$$+ \lambda_j + \theta_t + \epsilon_{ijt}$$

where $\theta_t$ and $\lambda_j$ are commune and survey round fixed effects (1992/3 is the excluded period). With both fixed effects included, $\beta_{1,2,3}$ measures the effect of rice prices, rice production, and their interaction on the probability that a rural household owns a formal savings instrument, net of any variation that is constant across all households in either year or fixed within each commune across both survey rounds.

Table 3.11 presents the paper’s main result by supplementing the vectors of covariates with the row vector of information about household-level exposure to rice prices: the average price of rice in the commune, the amount of rice in log kilos the household sold or bartered, and the interaction of commune-level rice prices and household-level rice sales. Commune and survey round fixed effects $\lambda_j$ and $\theta_t$ are entered in columns 2 and 3; the third column is the fully constrained model of equation 3.5 and is our preferred specification.

All three variables measuring exposure to rice prices are statistically significant.
at between the 1% and 5% level across all three specifications: without fixed effects, with only commune fixed effects, and with both commune and survey round fixed effects. Price and the quantity of rice sold increase the probability the household owns a formal savings instrument, while their interaction is negatively related to savings status\(^6\).

Because the marginal effect of rice prices on savings status is a function of the interaction term, we cannot immediately interpret least squares estimates \(\hat{\beta}_{1,2,3}\). Since rice prices at the commune level are exogenous with respect to each household \(i\), we evaluate the effect of rice prices on savings status by taking the first derivative of the regression model:

\[
\frac{\partial S}{\partial \text{price}^{\text{nominal}}} = 0.667 - 0.064 \times \text{rice}^{\text{sold}}.
\]

Table 3.4 evaluates this derivative for fitted values of the coefficients at interesting values of \(\text{rice}^{\text{sold}}\).

<table>
<thead>
<tr>
<th>ln (\text{rice}^{\text{sold}})</th>
<th>Sample value</th>
<th>(\partial \text{Pr}(S = 1)/\partial \text{price}^{\text{nominal}})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>6.49</td>
<td>.25%</td>
</tr>
<tr>
<td>Mean + 1 st. dev.</td>
<td>7.86</td>
<td>.16%</td>
</tr>
<tr>
<td>Mean - 1 st. dev.</td>
<td>5.12</td>
<td>.34%</td>
</tr>
</tbody>
</table>

At the average level of household rice sales, an 1% increase in the price of rice is associated with a 0.25% increase in the probability the household owns a savings instrument. This indicates a strongly positive and statistically significant relationship between income and savings status.

Evaluating the expression in a range of one standard deviation about the sample mean generates a range of 0.16% to 0.34% for the estimated elasticity of savings status with respect to a 1% increase in market rice prices. For small rural households, the marginal effect of increased income through rice sales on savings behaviour appears to be large and both economically and statistically significant.

### 3.5 Alternative explanations

While the estimated effect of rice prices on savings status is statistically significant at conservative levels, the sizes of the estimated coefficients are biased upwards if included right-hand side variables covary positively with variables that do not appear in the model of equation 3.5. This section tests the robustness of the observed coefficients using a variety of measures.

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\(^6\)\(\hat{\beta}_3\) is the fitted second derivative. The pattern of signs on the fitted coefficients means the probability of owning a formal instrument increases in the rice prices and sales, but at a decreasing rate.
3.5.1 Inflation and real rice prices

Despite extensive policy and macroeconomic reforms during the 1990’s, Vietnam continued to record relatively high inflation, including in rural areas covered by the VLSS surveys. To the extent that higher rice prices reflect differentially high inflation in those communes where the uptake of savings instruments was differentially high, the estimated elasticity is biased upward. One plausible narrative is that households in communes with higher economic growth were both more likely to transition into formal savings technologies and experienced higher nominal rice prices.

The monthly price changes are calculated by the GSO from the Vietnamese CPI, which calculates a weighted basket of food and non-food items and is available for each province for the periods covered by the 1992/3 and 1997/8 VLSS, using 1995 weights for the consumption basket.

To ensure the robustness of our result, we exploit the fact that the 1992/3 and 1997/8 price questionnaires record the month in which enumerators observed market prices in each commune. Combining information on the timing of observations with deflators calculated by the GSO from a monthly consumer price index and included in the survey data (January 1998 = 1), we can estimate the real price of rice for any observation month \( m \) as: \( \text{price}^{\text{real}}_{jt} = \text{price}^{\text{nominal}}_{jmt} / \text{deflator}_{jmt} \). As in Edmonds and Pavcnik (2005) and Benjamin and Brandt (2002), we deflate local prices using the national deflator to make sure that the choice of deflator is not driving the result (local level deflators may covary with some omitted factor that simultaneously affected both households’ saving status and local rice prices).

For some communes in the 1992/3 survey data, the month of the price observation was not recorded. In these cases, prices were deflated using the January 1992 deflator, creating the largest possible ratio between real and nominal values since inflation was positive in all months. This conservative treatment of missing data ensures that data coding is not driving the result by effectively increasing the 1992/3 prices, reducing the relative change in real prices within a commune between the survey rounds, and biasing us against finding a positive relationship between rice prices and savings status.

The sample means for real rice prices are 0.964 and 1.186 for the first and second VLSS rounds. Since prices are measured in log points, this implies \( \Delta \text{price}^{\text{real}} = 1.186 - 0.964 = 22\% \), in-line with the 25% effective average export tax on rice due to the quota on rice exports reported by Benjamin and Brandt (2002).

Comparing relative densities for the distribution of real rice prices across com-
munes within each survey round shows the smaller increase in the mean (log) rice price in each year relative to nominal rice prices in figure 3.5.

Table 3.12 shows the estimated effect of real rice prices on savings status, when rice production is measured by total kilos of rice sold or bartered. When $\theta_t$ and $\lambda_j$ are included for the fully constrained model in column 3, results are completely consistent with those for nominal rice prices: high rice prices and larger levels of rice sales are statistically significant and positively associated with savings status.

Evaluating the marginal effect of real rice prices on savings status shows this relationship is also economically meaningful: at the average level of kilos of rice sold or bartered, a 1% increase in rice prices is associated with an average 0.23% increase in the probability the household has a formal savings instrument.

<table>
<thead>
<tr>
<th>$\ln \text{rice}^{sold}$</th>
<th>Sample value</th>
<th>$\partial \Pr(S = 1)/\partial \ln \text{price}^{real}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>6.49</td>
<td>.23%</td>
</tr>
<tr>
<td>Mean + 1 st. dev.</td>
<td>7.86</td>
<td>.06%</td>
</tr>
<tr>
<td>Mean - 1 st. dev.</td>
<td>5.12</td>
<td>.39%</td>
</tr>
</tbody>
</table>

### 3.5.2 Endogenous labour supply

An increase in real rice prices increases the marginal revenue product of agricultural labour. In equilibrium, the change in factor rewards should increase the level of household labour or other scarce inputs supplied to rice farming (we will refer to this set of plausible endogenous responses collectively as a "labour supply response"). Households with a larger labour supply response may also
be households that are most likely to invest in formal savings instruments, so the estimated $\beta$'s are biased upward, capturing the positive covariance between the propensity to own formal savings instruments and propensity to increase labour supply to higher-return activities.

To ensure that our estimated coefficients are robust, we fix household rice sales at the level recorded during the first survey round, so that $\beta_3$ estimates the effect of real rice prices on savings status holding constant the level of rice sold or bartered.

Table 3.13 confirms that an endogenous labour response is not driving the observed relationship between the income shock and household saving status, with estimated coefficients displaying the same pattern of sign and significance. The marginal effect of a doubling of rice prices is now associated with an average 16% increase in the probability the average household owns a savings instrument, which is roughly consistent with the 23% elasticity estimated when rice production in both survey rounds is used (table 3.11).

<table>
<thead>
<tr>
<th>$\ln r_{\text{sold}}^{1992/3}$</th>
<th>Sample value</th>
<th>$\partial \Pr(S = 1)/\partial \ln price^{\text{real}}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>6.26</td>
<td>.16%</td>
</tr>
<tr>
<td>Mean + 1 st. dev.</td>
<td>7.6</td>
<td>.01%</td>
</tr>
<tr>
<td>Mean - 1 st. dev.</td>
<td>4.92</td>
<td>.31%</td>
</tr>
</tbody>
</table>

### 3.5.3 Alternative rice production measures and cultivation by diktat

National food security has been a major focus of the Communist Party of Vietnam since the founding of the modern Vietnamese state. At the household-level, this policy manifested itself in instructions from Commune-level Communist Party officials to dedicate specific areas of land under cultivation to rice farming. Markussen et al. (2011) show that these "land-use" rights reduced crop diversification, artificially inflating cultivation of specific crops.

While forced cultivation increased the square meters of rice cultivated and number of kilos harvested, the policy would not directly determine the level of rice sold or bartered by households. More generally, since market rice prices only affect the decision rule (equation 3.2) through rice sold or traded, other measures of rice production should not directly affect savings status.

Fitting the regression model in equation 3.5 to the data using alternative measures of household rice production confirms these results. Tables 3.14 and 3.15 show results from fitting the model with $rice^{\text{harvested}}$ and $rice^{\text{cultivated}}$ and
nominal prices, and tables 3.16 and 3.17 show results from fitting the model with $price^{real}$. The alternate measures of rice production are not statistically significantly associated with savings status in any of these four cases.

It is initially surprising that $t$-tests do not reject the null hypothesis of no effect of rice harvested / cultivated on savings status, since these measures of rice production are mechanically correlated for the obvious reason that households that sell more rice must cultivate and harvest more rice: the correlation between rice harvested (measured in kg) and rice sold is 0.82, and between rice cultivated (measured in $m^2$) and rice sold is 0.76.

Table 3.18 constrains the regression sample to include only those households with non-zero and non-missing observations of rice sales (those households for which $rice^{sold} \geq 0$). Columns 2 and 4 of this table confirm that the alterative measures of household rice production are statistically significant predictors of saving status in the subset of households that recorded non-missing levels of rice sold or bartered: cultivating and harvesting rice only predicts household savings status in those households that sell rice to local markets.

Cumulatively, it is clear that cultivating and harvesting rice does not measure exposure to rice prices, since these measures are not statistically significant at even large levels ($p \leq 0.15$). However, since these measures are mechanically positively correlated with rice sales, they are statistically significantly related to household savings for the subset of households that bring rice to market. The channel through which higher rice prices affect savings status is therefore rice sales, consistent with the comparative statics results derived from our simple decision rule.

### 3.5.4 Household-level fixed effects

Linearising the expression for the value of a savings instrument from equation 3.2 shows

$$\ln v = 2 \ln(y) + \ln(\gamma) + \ln(\sigma_{R}^{2}) + \ln(P - yR) + \ln(1 - x^2) - 2$$

Since parameters such as risk aversion ($\gamma$) and the household’s preferred holding of risk assets ($x$) cannot be observed in the VLSS data, any variation in these terms that is not explained by other right-hand side variables such as household assets enters the error term $\epsilon$. In the preferred specification that includes both commune and survey round fixed effects $\lambda_j$ and $\theta_t$, the error term will not include any variation that is fixed across VLSS surveys within
each commune or common to all communes in each survey round.

The estimated coefficients of interest $\hat{\beta}_{1,2,3}$ will therefore be biased in the direction of the covariance between the household-level individual effects that are not explicitly included in the coefficient vector $x$. While we cannot observe these variables and include them in the linear regression model, a minimal test of robustness is to ensure that the pattern of estimated signs and significance does not change when commune-level fixed effects $\lambda_j$ are replaced with household-level fixed effects denoted by $\phi_i$:

$$
S_{ijt} = \alpha + \beta_1 \cdot \ln price_{jt}^{nominal} + \beta_2 \cdot \ln rice_{ijt}
+ \beta_3 \cdot \ln (price_{jt}^{nominal} \times \ln rice_{ijt})
+ \gamma_1 \cdot B_{ijt} + \gamma_2 \cdot W_{ijt} + \gamma_3 \cdot H_{ijt}
+ \phi_i + \theta_t + \epsilon_{ijt}
$$

Table 3.19 shows the effect of removing variation that is fixed within households across survey rounds with real rice prices and household exposure to rice prices measured by the level of rice sold. Since $T = 2$, including $\phi_i$ is exactly equivalent to subtracting the average value of $rice^{sold}$ from each household. (Using nominal prices in table 3.20 increases the estimated effect of rice prices on savings status).

Table 3.7: Marginal effects at interesting values of $rice^{sold}$

<table>
<thead>
<tr>
<th>$\ln rice^{sold}$</th>
<th>Sample value</th>
<th>$\partial Pr(S = 1)/\partial \ln price^{real}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>6.49</td>
<td>.23%</td>
</tr>
<tr>
<td>Mean + 1 st. dev.</td>
<td>7.86</td>
<td>-.01%</td>
</tr>
<tr>
<td>Mean - 1 st. dev.</td>
<td>5.12</td>
<td>.47%</td>
</tr>
</tbody>
</table>

For the average household, a 1% increase in the price of rice is associated with around a 0.23% increase in the probability the household has a formal savings instrument of some kind. Examining the estimated elasticities in table 3.7, the linear prediction $\hat{S}$ will be less than zero for sufficiently large choices of $rice^{sold}$. However, fitted estimates outside the (0,1) interval contradict the data and arise from our choice of regression framework, so we test the problem in a non-linear framework to make sure the linear model is not driving our core result.
3.5.5 Non-linear econometric framework

The major limitation of estimating the regression problem 3.5 by least-squares is that the estimated coefficients do not constrain $x_{ijt} \zeta$ to be on the unit interval, mechanically contradicting the data since $S_{ijt}$ is binary. Because the error term $\epsilon_{ijt}$ is normally distributed by assumption, we choose $F(\cdot)$ to be the standard normal cumulative distribution function and estimate a probit model by maximising the associated log-likelihood function.

Table 3.21 shows results from fitting the probit model to the VLSS household data. Estimated coefficients are statistically significant and have the same signs as coefficients estimated by the linear probability model. The magnitudes of these coefficients are different, since the underlying model is now non-linear. We are still interested in the marginal effect of a 1% increase in the real price of rice on the probability the household has a formal savings instrument, $\Pr(S = 1 | \mathbf{x})$. (Using nominal prices instead of deflated real rice prices in table 3.22 results in larger fitted coefficients, since the nominal price differential between survey rounds is larger than the real price differential).

Because $\Phi(\cdot)$ maps from household characteristics and coefficients to the outcome, the marginal effect of a change in rice prices is no longer a linear function of the fitted coefficients (so the fitted coefficients tell us the sign but not the magnitude of the marginal effect). Differentiating with respect to rice prices shows that

$$\frac{\partial \Pr(S = 1)}{\partial \ln \text{price}_{\text{real}}} = \phi(x'_{ijt}\zeta)(\beta_1 + \beta_3 \times \ln \text{rice}_{ijt}^{\text{old}})$$

so the marginal effect can be calculated as either the marginal effect for the average household in the sample (the marginal effect at the mean) or as the average of the of the partial effects (APE) calculated for each household $i$. In the linear probability model, the APE and marginal effect at the mean (MEM) are identical. The maintained hypothesis is that the effect of rice prices on the average household in the sample was to increase probability of owning a formal savings product. We observe the MEM by setting the arguments of this expression to their average values calculated for households in the estimation sample.

\[^7\text{In the linear case, } E[\beta_1 + \beta_3 \times \ln \text{rice}_{ijt}^{\text{old}}] = \beta_1 + \beta_3 \times E[\ln \text{rice}_{ijt}^{\text{old}}] \]
\[ \frac{\partial \Pr(S = 1)}{\partial \ln \text{price}^{\text{real}}} = \phi(\mathbf{x}' \hat{\boldsymbol{\xi}})(\hat{\beta}_1 + \hat{\beta}_3 \times \ln \text{rice}^{\text{sold}}) \]

and other interesting values of rice sold for comparability with results from the linear probability model presented above. For real rice prices, the effect of a 1% increase in market prices is a 0.27% increase in the probability that the average household has a formal savings instrument, very close to fitted estimates from a linear model.

<table>
<thead>
<tr>
<th>( \ln \text{rice}^{\text{sold}} )</th>
<th>Sample value</th>
<th>( \frac{\partial \Pr(S = 1)}{\partial \ln \text{rice}^{\text{sold}}} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>6.69</td>
<td>.27%</td>
</tr>
<tr>
<td>Mean + 1 st. dev.</td>
<td>8.02</td>
<td>.02%</td>
</tr>
<tr>
<td>Mean - 1 st. dev.</td>
<td>5.36</td>
<td>.53%</td>
</tr>
</tbody>
</table>

While estimating a MEM is appropriate given our motivating research question, there is some discussion in the literature about which marginal effect is preferred. In some cases the MEM does not have a useful interpretation because setting independent variables to their means creates a logically inconsistent regression. (Bartus, 2005, gives the example of a regression in which the binary outcome is whether the respondent is retired, but the average age in the sample is lower than the minimum retirement age; the MEM would then measure the effect of being below the retirement age on being retired). We therefore estimate the APE (see appendix 3.B) for completeness. The two elasticities are very similar in magnitude: the average of the partial effects of a 1% increase in real prices is 0.29%. The closeness of the MEM and APE show that household-level heterogeneity does not contradict our main result in a non-linear estimation framework.

### 3.6 Banking in rural Vietnam

McCarty (2001) provides one of the few formal, large-scale studies of the penetration of state banks, NGOs, private banks, and microfinance institutions into rural areas. The study concludes that there has been remarkably little extension of savings services to rural areas, noting that the minimum deposit levels are a constraint on access to savings products: they cite figures from 2001 of 50,000 Vietnamese Dong at the Government-run Vietnamese Bank for Agriculture and Rural Development (VBARD) and around 100,000 Dong for commercial banks.
While VBARD’s mandate is ostensibly to provide banking and savings services to the financially marginal households that are the focus of this paper, the authors find that the VBARD deposits are “overwhelmingly urban with only a small proportion coming from rural households. Therefore, so long as other institutions follow the [lead of] VBARD, there remains a dire shortage of well-designed instruments for mobilizing public savings.”

The study extracts compelling evidence of constraints on access to savings products from a novel survey of microfinance institutions and schemes that were operating in Vietnam in 2000\(^8\). Of a sample of 84 schemes, less than half (34) reported collecting savings in any form from households, of which only two schemes reported savings levels at 50% and 24 reporting savings equivalent to less than 20% of loans.

While this evidence is from a cross-section studied after the shock to rice prices we study using the VLSS surveys, it suggests that the market for savings products for rural households was underdeveloped, and that changes in the level of savings products provided, if there were any, were relatively small during the years bracketed by our household-level observations.

### 3.6.1 The 1997/8 cross-section

Since questions to generate proxies for financial access are only included in the 1997/8 survey round, we focus on the cross-section of households in the second survey round to construct a minimal measure of access to formal savings services. This implies estimating an expanded version of the regression problem 3.5 using all rural, panel households with non-missing vectors of observations for information in x.

Using \(d_{ijt}\) for some measure of the supply of formal savings products gives

\[
S_{ij1997/8} = x_{ij1997/8} \zeta + \phi \cdot d_{ij1997/8} + \epsilon_{ij1997/8} \quad (3.6)
\]

where \(x\) excludes survey-round and commune fixed effects \(\theta_t\) and \(\lambda_j\) because we are working with the 1997/8 cross-section and our measure of financial access, \(d_{ij1997/8}\), is fixed within each commune.

A crude but useful measure of access to savings products for rural households is the average distance to a financial institution that offers savings products, a measure we expect to be correlated (albeit imperfectly) with the provision

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\(^8\)Their survey, implemented in 2001, was backward looking, so these data are from 2000
of access to and information about formal savings. Question 1 of section 5 of the 1997/8 community questionnaire asks the respondent to list up to four financial institutions and question 5.2 asks whether that financial institution offers options for credit (1), savings (2), or both (3). Limiting respondents to four institutions effectively top-codes the data, but frequency statistics show this only affects a minority of communes.

Table 3.9: Top-coding of Number of Financial Inst.s

<table>
<thead>
<tr>
<th>No. of Financial Inst.s</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>(total)</td>
<td>No. of communes</td>
</tr>
<tr>
<td>1</td>
<td>55</td>
</tr>
<tr>
<td>2</td>
<td>74</td>
</tr>
<tr>
<td>3</td>
<td>11</td>
</tr>
<tr>
<td>4</td>
<td>9</td>
</tr>
</tbody>
</table>

Question 5.6 asks the distance in km of each financial institution from the People's Committee of the commune or ward at which the survey is being administered. For those institutions that recorded a response of (2) or (3) to question 5.2, we calculate $d_{ij}$ as the average km in question 5.6. The measure $d_{ij}$ is therefore the average distance for households $i$ in commune $j$ to a financial institution that offers formal savings products.

Table 3.23 includes this measure of financial access with conditioning information but excludes measures of exposure to the change in (real) rice prices. Using all the households available in the 1997/8 cross-section, the measure of financial access enters negatively but is not at all significant when other measures that affect the decision to own a savings instrument (in vectors $B$, $H$, and $W$) are entered.

If the estimated $\hat{\beta}$'s associated with our measures of household-level exposure to rice prices retain their sign and significance when we estimate 3.6, it provides minimal evidence that the observed relationship is not being driven by unobserved access to finance variables. Table 3.24 shows the results of estimating the model with nominal prices. Column 1 includes only the measure of distance to financial institutions and with a $\hat{\phi}$ of $-0.005$ that is significant at below the 1% level, indicating that financial institutions that are further away from the household decrease the probability that households $i$ in commune $j$ own a formal savings product.\(^9\)

\(^9\)Unfortunately, the set of households for which we can estimate fitted $\hat{\beta}$'s using the panel of rural households includes only very few for which we also have observations of $d_{ij}$. If this were not the case, one possibility would have been to estimate purely cross-sectional coefficients using regression model 3.6 and compare these to coefficients from an expanded version of the model 3.5 that includes a measure for $d_{ijt}$, with $d_{i1997/8}$ defined by the second cross-section and $d_{ij1992/3}$ set to zero in all communes, effectively assuming the entire change in the supply of savings products occurred during the rapid increase in rice prices, providing
However, when the full set of covariates is included in column 2, this measure loses any statistical significance (the number of households in the cross-section is the same, so selection on observable characteristics is not driving the loss of statistical significance). The estimated coefficients associated with rice production, rice prices and their interaction retain their signs and are statistically significant at various levels, all below 10%. Estimating the problem with real rice prices (the consumer price index constructed by Vietnam’s General Statistics Office is monthly) in table 3.25 provides similar results.

Cumulatively, this shows that physical distance to financial institutions is not a useful predictor of savings status when other measures of household and household head characteristics are included. To the extent that physical distance is a useful proxy for financial access, this provides a minimal test of the robustness of the estimated elasticities obtained from linear and non-linear estimates of the main regression problem of equation 3.5.

While it is possible that the observed positive effect of rice prices on household-level ownership of at least one formal savings product is due to the above-average increase in the supply of these products in communes where rice prices rose more than average, cross-sectional results and narrative evidence suggest the size of this confounding effect, if present, is small.

### 3.7 Discussion and future research

We set out to develop a simple decision rule to explain household investment in formal savings instruments that are safer and more useful stores of resources than physical assets or informal savings technologies. Broadly speaking, the agenda set by microfinance specialists has been to increase access to savings products by increasing their supply.

Using the exogenous increase in rice prices experienced by rural Vietnamese households roughly between 1992 and 1998 through a set of policy reforms at the national level, the paper estimates the effect of exposure to rice prices on household ownership of at least one “formal” savings product. The results are encouraging: at the margin, a 1% increase in rice prices is associated with around a 0.20% increase in the probability the household has at least one formal savings product. In a non-linear probit model with real rice prices, this average marginal effect is 0.27%.

The approximate size, significance, and direction of this elasticity is robust to a “maximal” robustness test. Unfortunately, the small number of panel households for which we have observations of $d_{ij}$ precludes this possibility.
concerns about an endogenous increase in labour supplied to rice cultivation, mismeasurement of prices due to inflation, and household-level characteristics that are fixed across both survey rounds. The effect does not appear when measures of rice production other than rice sold or bartered are used, suggesting that it is exposure to market prices that affects households’ choices of savings technologies.

These estimated effects may be confounded if the right-hand side variables exclude a measure of access to financial institutions, and these institutions supplied more savings products to areas where rice prices increased above the national average (net of commune and survey-round fixed effects). A minimal robustness test using the full cross-section of observations from the 1997/8 survey round shows that average distance to financial institutions that provide savings products is negatively associated with savings status, but this measure is not statistically significant when other household-level observations are included. This provides some evidence that access is not driving the results from our panel data set from which the paper’s take-away result is derived.

Unfortunately, we cannot forcefully conclude that poverty alleviation is the best marginal use of scarce donor or government resources. Firstly, examining equation 3.2, we see that the value of formal savings in this framework also increases in the variance of returns. As Vietnam liberalised trade, rice prices may have both risen and become more volatile. To the extent that these variables co-moved, the estimated effect of rice prices on savings behaviour may be biased upwards if the price volatility was differentially higher in those communes with differentially larger increases in rice prices, net of fixed effects that absorb any variation fixed over time or common across all communes in either survey round. Unfortunately, commune-level data on rice prices (specifically, a time-series of data to construct volatility measures) is not available. Disentangling the income and volatility effects, if any, remains an interesting area for future research on self-insurance by rural households.

More generally, we cannot compare the estimated $\hat{\beta}$'s with those from a comparison study in which rural households are exogenously (for example, randomly) allocated a formal savings product and their subsequent income or wealth compared to a control group: households selling rice to markets may simply be different in ways not captured by our econometric model. Finally, the magnitude of the coefficients is meaningful but less than one: in a linear probability model and using real rice prices, a doubling of market prices causes a 23% increase in the probability that $S_i = 1$.

On balance, this paper shows that higher incomes are causally associated with selection into the market for formal savings products: the rising tide of eco-
nomic growth causes many households to invest in better and safer access to their liquid assets. McCarty (2001) notes that “The poor, according to some scheme designs and workshop participants, need to be ‘taught how to save’. This is probably a misunderstanding relating to relative perceptions of risk and uncertainty, based on different historical experiences and information sets. Building confidence in savings institutions may also require marketing and related efforts. Offering safe and accessible savings options may therefore only be a first step.”

Scarce donor and government resources should be effectively targeted. Since increasing incomes appears to increase uptake of savings products, effort should be focused on those marginal households that have been largely excluded from the increase in rural incomes during the 1990’s and, due to some combination of poverty, remoteness, information constraints, and habit persistence, are the least likely to invest in formal savings products.
### Table 3.10

Household characteristics and saving status

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<thead>
<tr>
<th>VARIABLES</th>
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<th>(2)</th>
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<th>(4)</th>
<th>(5)</th>
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<td>0.001**</td>
<td>0.002***</td>
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<td>(0.004)</td>
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<td>(0.015)</td>
<td>(0.015)</td>
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</tr>
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<td>-0.010</td>
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<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.017)</td>
<td></td>
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<td></td>
</tr>
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<td>4,266</td>
</tr>
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<td>0.040</td>
<td>0.221</td>
<td>0.224</td>
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<td>n</td>
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<td>Survey round FE</td>
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</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Table 3.11
Household exposure to change in nominal rice prices
Measure of production is rice sold/bartered
Dependent variable is saving status

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \ln p_{\text{rice}^{\text{nominal}}} )</td>
<td>0.863***</td>
<td>0.594**</td>
<td>0.667**</td>
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<tr>
<td></td>
<td>(0.192)</td>
<td>(0.231)</td>
<td>(0.285)</td>
</tr>
<tr>
<td>( \ln p_{\text{rice}^{\text{sold}}} )</td>
<td>0.046**</td>
<td>0.070***</td>
<td>0.073***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.022)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>( \ln p_{\text{rice}^{\text{nominal}}} \times \ln p_{\text{rice}^{\text{sold}}} )</td>
<td>-0.076***</td>
<td>-0.061**</td>
<td>-0.064**</td>
</tr>
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<td>(0.022)</td>
<td>(0.024)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Age, head of HH</td>
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<td>0.002**</td>
<td>0.002**</td>
</tr>
<tr>
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<td>(0.001)</td>
<td>(0.001)</td>
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<tr>
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<td>-0.003</td>
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<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
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<td>-0.007</td>
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<tr>
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<td>(0.028)</td>
<td>(0.027)</td>
<td>(0.027)</td>
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<tr>
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<td>0.017***</td>
</tr>
<tr>
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<td>(0.005)</td>
<td>(0.005)</td>
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<tr>
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</tr>
<tr>
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<td>(0.014)</td>
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<td>(0.016)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
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<td>0.000</td>
</tr>
<tr>
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<td>(0.026)</td>
<td>(0.026)</td>
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<tr>
<td>Dwelling quality, index</td>
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<td>-0.016</td>
<td>-0.003</td>
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<td>(0.013)</td>
<td>(0.014)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
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<td>0.005</td>
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<td>(0.030)</td>
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<td>HH size</td>
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<td>-0.010**</td>
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<tr>
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<td>(0.006)</td>
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<td>(0.005)</td>
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<td>(0.190)</td>
<td>(0.223)</td>
<td>(0.264)</td>
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Observations | 1,972 | 1,972 | 1,972 |
R-squared | 0.081 | 0.256 | 0.257 |
Commune FE | n | y | y |
Survey round FE | n | n | y |
Communes | 115 | 115 | 115 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.12

Household exposure to change in rice prices
Measure of production is rice sold/bartered
Dependent variable is saving status

<table>
<thead>
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<th>VARIABLES</th>
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<th>(3)</th>
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<td>In $price_{real}$</td>
<td>1.132***</td>
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<td>0.998**</td>
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<td>(0.385)</td>
<td>(0.414)</td>
<td>(0.416)</td>
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<td>In $rice_{sold}$</td>
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<td>0.139***</td>
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<td>(0.052)</td>
<td>(0.052)</td>
</tr>
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<td>In $price_{real} \times \ln rice_{sold}$</td>
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<td>-0.119**</td>
<td>-0.119**</td>
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<td>(0.049)</td>
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<td>(0.008)</td>
</tr>
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<td>Sex, head of HH, 2 = Female</td>
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<td>(0.028)</td>
<td>(0.027)</td>
<td>(0.027)</td>
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<tr>
<td>Assets, raw index</td>
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<td>0.017***</td>
<td>0.016***</td>
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<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
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<tr>
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<td>-0.013</td>
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<td>(0.014)</td>
<td>(0.016)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
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<td>(0.026)</td>
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<tr>
<td>Dwelling quality, index</td>
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<td>-0.002</td>
<td>-0.007</td>
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<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.017)</td>
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<td>HH Type, 2 = Farm</td>
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Observations: 1,972, 1,972, 1,972
R-squared: 0.074, 0.256, 0.256
Commune FE: n, y, y
Survey round FE: n, n, y
Communes: 115, 115, 115

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.13
Household exposure to change in rice prices
Measure of production is rice sold/bartered in 1992/3
Dependent variable is saving status

<table>
<thead>
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<th>VARIABLES</th>
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<th>(2)</th>
<th>(3)</th>
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</thead>
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<td>ln ( price_{real} )</td>
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<td>1.361***</td>
<td>1.261***</td>
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<tr>
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<td>(0.362)</td>
<td>(0.349)</td>
<td>(0.342)</td>
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<tr>
<td>ln ( rice_{sold}^{1992/3} )</td>
<td>0.080*</td>
<td>0.188***</td>
<td>0.179***</td>
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<td>(0.043)</td>
<td>(0.044)</td>
<td>(0.044)</td>
</tr>
<tr>
<td>ln ( price_{real} \times \ln rice_{sold}^{1992/3} )</td>
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<td>-0.177***</td>
<td>-0.168***</td>
</tr>
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<td>(0.047)</td>
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<td>(0.043)</td>
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<td>0.002**</td>
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<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Education, head of HH</td>
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<td>-0.006</td>
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<td>(0.006)</td>
<td>(0.006)</td>
</tr>
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<td>-0.022</td>
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<tr>
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<td>(0.033)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.017***</td>
<td>0.012**</td>
<td>0.012**</td>
</tr>
<tr>
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<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
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<td>-0.018</td>
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<td>(0.016)</td>
<td>(0.016)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
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<td>0.016</td>
<td>0.018</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.028)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>0.018***</td>
<td>0.026***</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.008)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>-0.008</td>
<td>-0.001</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.035)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.006</td>
<td>-0.007</td>
<td>-0.006</td>
</tr>
<tr>
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<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.679**</td>
<td>-0.993**</td>
<td>-0.857**</td>
</tr>
<tr>
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<td>(0.339)</td>
<td>(0.383)</td>
<td>(0.380)</td>
</tr>
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<td>1,103</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.164</td>
<td>0.375</td>
<td>0.377</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
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Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.14
Household exposure to change in nominal rice prices
Measure of production is rice harvested
Dependent variable is saving status

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
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<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(\ln \text{price}^{\text{nominal}})</td>
<td>0.541**</td>
<td>0.197</td>
<td>0.132</td>
</tr>
<tr>
<td></td>
<td>(0.240)</td>
<td>(0.239)</td>
<td>(0.253)</td>
</tr>
<tr>
<td>(\ln \text{rice}^{\text{harvested}})</td>
<td>0.007</td>
<td>0.008</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.027)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>(\ln \text{price}^{\text{nominal}} \times \ln \text{rice}^{\text{harvested}})</td>
<td>-0.021</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.029)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.001**</td>
<td>0.002***</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.007</td>
<td>-0.007+</td>
<td>-0.008+</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.000</td>
<td>-0.010</td>
<td>-0.010</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.015***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.030**</td>
<td>-0.018+</td>
<td>-0.019+</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.023</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.019)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.038***</td>
<td>-0.019*</td>
<td>-0.032**</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.002</td>
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<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.023)</td>
<td>(0.023)</td>
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<tr>
<td>HH size</td>
<td>-0.004</td>
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<td>-0.006*</td>
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<tr>
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<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Constant</td>
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<td>-0.234</td>
<td>-0.173</td>
</tr>
<tr>
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<td>(0.230)</td>
<td>(0.210)</td>
<td>(0.219)</td>
</tr>
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<td>Observations</td>
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<td>3,402</td>
<td>3,402</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.059</td>
<td>0.230</td>
<td>0.231</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>120</td>
<td>120</td>
<td>120</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln price^{nominal}$</td>
<td>0.790***</td>
<td>0.447*</td>
<td>0.377</td>
</tr>
<tr>
<td></td>
<td>(0.258)</td>
<td>(0.254)</td>
<td>(0.270)</td>
</tr>
<tr>
<td>$\ln r_{ice^{cultivated}}$</td>
<td>0.008</td>
<td>0.031</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.028)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>$\ln price^{nominal} \times \ln r_{ice^{cultivated}}$</td>
<td>-0.052*</td>
<td>-0.030</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.028)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.002**</td>
<td>0.002***</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.005</td>
<td>-0.007</td>
<td>-0.007+</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.006</td>
<td>-0.013</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.016***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.026**</td>
<td>-0.018+</td>
<td>-0.018+</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.023</td>
<td>-0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.019)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.033***</td>
<td>-0.017+</td>
<td>-0.029*</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
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<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
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<td>(0.023)</td>
<td>(0.023)</td>
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<td>HH size</td>
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<td>-0.005+</td>
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<td>(0.003)</td>
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<tr>
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<td>-0.433*</td>
<td>-0.369+</td>
</tr>
<tr>
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<td>(0.262)</td>
<td>(0.243)</td>
<td>(0.252)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,421</td>
<td>3,421</td>
<td>3,421</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.065</td>
<td>0.231</td>
<td>0.231</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>120</td>
<td>120</td>
<td>120</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

Fixed effects coefficients not reported
Table 3.16
Household exposure to change in nominal rice prices
Measure of production is rice harvested
Dependent variable is saving status

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln $\text{price}^{\text{nominal}}$</td>
<td>0.541**</td>
<td>0.197</td>
<td>0.132</td>
</tr>
<tr>
<td>ln $\text{rice}^{\text{harvested}}$</td>
<td>0.007</td>
<td>0.008</td>
<td>0.006</td>
</tr>
<tr>
<td>ln $\text{price}^{\text{nominal}} \times \ln \text{rice}^{\text{harvested}}$</td>
<td>-0.021</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.001**</td>
<td>0.002***</td>
<td>0.002***</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.007</td>
<td>-0.007+</td>
<td>-0.008+</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.000</td>
<td>-0.010</td>
<td>-0.010</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.015***</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.030**</td>
<td>-0.018+</td>
<td>-0.019+</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.023</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.038***</td>
<td>-0.019*</td>
<td>-0.032**</td>
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<tr>
<td>HH Type, 2 = Farm</td>
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<td>-0.002</td>
<td>-0.002</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.004</td>
<td>-0.006*</td>
<td>-0.006*</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.042</td>
<td>-0.234</td>
<td>-0.173</td>
</tr>
</tbody>
</table>

Observations 3,402 3,402 3,402
R-squared 0.059 0.230 0.231
Commune FE n y y
Survey round FE n n y
Communes 120 120 120

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.17
Household exposure to change in nominal rice prices
Measure of production is rice cultivated
Dependent variable is saving status

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln $price_{\text{nominal}}$</td>
<td>0.790***</td>
<td>0.447*</td>
<td>0.377</td>
</tr>
<tr>
<td></td>
<td>(0.258)</td>
<td>(0.254)</td>
<td>(0.270)</td>
</tr>
<tr>
<td>ln $rice_{\text{cultivated}}$</td>
<td>0.008</td>
<td>0.031</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.028)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>ln $price_{\text{nominal}} \times \ln rice_{\text{cultivated}}$</td>
<td>-0.052*</td>
<td>-0.030</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.028)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.002**</td>
<td>0.002***</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.005</td>
<td>-0.007</td>
<td>-0.007+</td>
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<tr>
<td></td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.006</td>
<td>-0.013</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.016***</td>
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<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.026**</td>
<td>-0.018+</td>
<td>-0.018+</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.023</td>
<td>-0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.019)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.033***</td>
<td>-0.017+</td>
<td>-0.029*</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.012</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.023)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.000</td>
<td>-0.005+</td>
<td>-0.005+</td>
</tr>
<tr>
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<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
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<td>-0.433*</td>
<td>-0.369+</td>
</tr>
<tr>
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<td>(0.262)</td>
<td>(0.243)</td>
<td>(0.252)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,421</td>
<td>3,421</td>
<td>3,421</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.065</td>
<td>0.231</td>
<td>0.231</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>120</td>
<td>120</td>
<td>120</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
### Table 3.18

Effect of $rice^{harvested}$ and $rice^{cultivated}$ in rice-selling households

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Full sample</th>
<th>Sub-sample: $rice^{sold} ≥ 0$</th>
<th>Full sample</th>
<th>Sub-sample: $rice^{sold} ≥ 0$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$ln , price^{real}$</td>
<td>0.130</td>
<td>1.179+</td>
<td>0.679</td>
<td>1.957**</td>
</tr>
<tr>
<td></td>
<td>(0.476)</td>
<td>(0.725)</td>
<td>(0.493)</td>
<td>(0.980)</td>
</tr>
<tr>
<td>$ln , rice^{harvested}$</td>
<td>0.007</td>
<td>0.134+</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.086)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$ln , price^{real} \times ln , rice^{harvested}$</td>
<td>0.001</td>
<td>-0.121+</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.061)</td>
<td>(0.082)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$ln , rice^{cultivated}$</td>
<td></td>
<td></td>
<td>0.071</td>
<td>0.203*</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.057)</td>
<td>(0.105)</td>
</tr>
<tr>
<td>$ln , price^{real} \times ln , rice^{cultivated}$</td>
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<td></td>
<td>-0.063</td>
<td>-0.193*</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.054)</td>
<td>(0.098)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.002***</td>
<td>0.002**</td>
<td>0.002***</td>
<td>0.002**</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>-0.008+</td>
<td>-0.004</td>
<td>-0.007+</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.008)</td>
<td>(0.005)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.010</td>
<td>-0.009</td>
<td>-0.013</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.027)</td>
<td>(0.018)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.016***</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.017***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.019+</td>
<td>-0.013</td>
<td>-0.018+</td>
<td>-0.012</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.016)</td>
<td>(0.012)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>0.000</td>
<td>-0.001</td>
<td>-0.000</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.026)</td>
<td>(0.019)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.032**</td>
<td>-0.009</td>
<td>-0.029*</td>
<td>-0.006</td>
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<td>(0.016)</td>
<td>(0.017)</td>
<td>(0.016)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>-0.002</td>
<td>0.006</td>
<td>0.002</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.030)</td>
<td>(0.023)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.006*</td>
<td>-0.009*</td>
<td>-0.005+</td>
<td>-0.008*</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.003)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.228</td>
<td>-1.344*</td>
<td>-0.787+</td>
<td>-2.120**</td>
</tr>
<tr>
<td></td>
<td>(0.502)</td>
<td>(0.782)</td>
<td>(0.529)</td>
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</tr>
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<td>Observations</td>
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<td>3,421</td>
<td>1,972</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.231</td>
<td>0.253</td>
<td>0.231</td>
<td>0.255</td>
</tr>
<tr>
<td>Commune FE</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>y</td>
<td>y</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>116</td>
<td>111</td>
<td>116</td>
<td>111</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

Fixed effects coefficients not reported
Table 3.19

Household exposure to change in real rice prices
Measure of production is rice sold
with household-level fixed effects

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln $price^{real}$</td>
<td>1.209***</td>
<td>1.464**</td>
<td>1.354**</td>
</tr>
<tr>
<td></td>
<td>(0.388)</td>
<td>(0.668)</td>
<td>(0.659)</td>
</tr>
<tr>
<td>ln $rice^{sold}$</td>
<td>0.111**</td>
<td>0.218**</td>
<td>0.206**</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.084)</td>
<td>(0.082)</td>
</tr>
<tr>
<td>ln $price^{real}$ $\times$ ln $rice^{sold}$</td>
<td>-0.130***</td>
<td>-0.192**</td>
<td>-0.180**</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.079)</td>
<td>(0.077)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.001</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.010</td>
<td>-0.016</td>
<td>-0.020</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.015)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.016</td>
<td>0.170</td>
<td>0.180</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.165)</td>
<td>(0.167)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.020***</td>
<td>0.016**</td>
<td>0.015**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.035**</td>
<td>-0.039</td>
<td>-0.045+</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.030)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.029</td>
<td>-0.042</td>
<td>-0.040</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.057)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.002</td>
<td>0.011</td>
<td>-0.026</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.009)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.016</td>
<td>-0.011</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.058)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.008+</td>
<td>-0.006</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.014)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.830**</td>
<td>-1.486*</td>
<td>-1.287+</td>
</tr>
<tr>
<td></td>
<td>(0.413)</td>
<td>(0.832)</td>
<td>(0.830)</td>
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<tr>
<td>Observations</td>
<td>1,972</td>
<td>1,972</td>
<td>1,972</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.083</td>
<td>0.083</td>
<td>0.086</td>
</tr>
<tr>
<td>Household FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>115</td>
<td>115</td>
<td>115</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.20

Household exposure to change in nominal rice prices
Measure of production is rice sold
with household-level fixed effects

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln \text{price}^{\text{nominal}}$</td>
<td>0.890***</td>
<td>0.890**</td>
<td>0.852**</td>
</tr>
<tr>
<td></td>
<td>(0.198)</td>
<td>(0.358)</td>
<td>(0.413)</td>
</tr>
<tr>
<td>$\ln \text{rice}^{\text{sold}}$</td>
<td>0.052**</td>
<td>0.106***</td>
<td>0.104***</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.034)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>$\ln \text{price}^{\text{nominal}} \times \ln \text{rice}^{\text{sold}}$</td>
<td>-0.080***</td>
<td>-0.102***</td>
<td>-0.099***</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.033)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.001</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.008</td>
<td>-0.017</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.014)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.011</td>
<td>0.189</td>
<td>0.192</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.166)</td>
<td>(0.166)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.018***</td>
<td>0.016**</td>
<td>0.016**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.036**</td>
<td>-0.049*</td>
<td>-0.051*</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.029)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.027</td>
<td>-0.041</td>
<td>-0.041</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.058)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.030**</td>
<td>-0.005</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.018)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.014</td>
<td>-0.022</td>
<td>-0.024</td>
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<tr>
<td></td>
<td>(0.027)</td>
<td>(0.058)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.007</td>
<td>-0.007</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.014)</td>
<td>(0.014)</td>
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<td>Constant</td>
<td>-0.281</td>
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<td>-0.539</td>
</tr>
<tr>
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<td>(0.203)</td>
<td>(0.457)</td>
<td>(0.519)</td>
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<tr>
<td>Observations</td>
<td>1,972</td>
<td>1,972</td>
<td>1,972</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.090</td>
<td>0.092</td>
<td>0.092</td>
</tr>
<tr>
<td>Household FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>115</td>
<td>115</td>
<td>115</td>
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</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

Fixed effects coefficients not reported
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln \text{price}^{\text{real}}$</td>
<td>3.678**</td>
<td>5.552**</td>
<td>5.615**</td>
</tr>
<tr>
<td></td>
<td>(1.501)</td>
<td>(2.334)</td>
<td>(2.421)</td>
</tr>
<tr>
<td>$\ln \text{rice}^{\text{sold}}$</td>
<td>0.274</td>
<td>0.858***</td>
<td>0.863***</td>
</tr>
<tr>
<td></td>
<td>(0.212)</td>
<td>(0.322)</td>
<td>(0.326)</td>
</tr>
<tr>
<td>$\ln \text{price}^{\text{real}} \times \ln \text{rice}^{\text{sold}}$</td>
<td>-0.355*</td>
<td>-0.689**</td>
<td>-0.693**</td>
</tr>
<tr>
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<td>(0.197)</td>
<td>(0.286)</td>
<td>(0.289)</td>
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<td>Age, head of HH</td>
<td>0.004</td>
<td>0.010**</td>
<td>0.010**</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.044+</td>
<td>-0.023</td>
<td>-0.022</td>
</tr>
<tr>
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<td>(0.029)</td>
<td>(0.037)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.047</td>
<td>-0.016</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.098)</td>
<td>(0.119)</td>
<td>(0.119)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.070***</td>
<td>0.078***</td>
<td>0.079***</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.021)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.139***</td>
<td>-0.071</td>
<td>-0.070</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.063)</td>
<td>(0.064)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.108</td>
<td>-0.014</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td>(0.106)</td>
<td>(0.112)</td>
<td>(0.112)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.007</td>
<td>-0.028</td>
<td>-0.012</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.041)</td>
<td>(0.078)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.058</td>
<td>0.012</td>
<td>0.011</td>
</tr>
<tr>
<td></td>
<td>(0.096)</td>
<td>(0.126)</td>
<td>(0.126)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.032+</td>
<td>-0.062***</td>
<td>-0.062***</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.024)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Constant</td>
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<td>-6.540**</td>
</tr>
<tr>
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<td>(1.661)</td>
<td>(2.596)</td>
<td>(2.693)</td>
</tr>
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<td>1,669</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15

Fixed effects coefficients not reported
Table 3.22

Effect of nominal rice prices on savings status
Probit model

<table>
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<th>VARIABLES</th>
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<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln $price_{\text{nominal}}$</td>
<td>3.038***</td>
<td>2.800**</td>
<td>3.363**</td>
</tr>
<tr>
<td></td>
<td>(0.804)</td>
<td>(1.094)</td>
<td>(1.536)</td>
</tr>
<tr>
<td>ln $rice_{\text{sold}}$</td>
<td>0.152+</td>
<td>0.429***</td>
<td>0.449***</td>
</tr>
<tr>
<td></td>
<td>(0.097)</td>
<td>(0.134)</td>
<td>(0.139)</td>
</tr>
<tr>
<td>ln $price_{\text{nominal}}$ $\times$ ln $rice_{\text{sold}}$</td>
<td>-0.253***</td>
<td>-0.328***</td>
<td>-0.345***</td>
</tr>
<tr>
<td></td>
<td>(0.096)</td>
<td>(0.126)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.004</td>
<td>0.010**</td>
<td>0.010**</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.036</td>
<td>-0.022</td>
<td>-0.019</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.037)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.031</td>
<td>-0.015</td>
<td>-0.021</td>
</tr>
<tr>
<td></td>
<td>(0.099)</td>
<td>(0.120)</td>
<td>(0.121)</td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.067***</td>
<td>0.077***</td>
<td>0.080***</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.022)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.145***</td>
<td>-0.075</td>
<td>-0.072</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.063)</td>
<td>(0.064)</td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.098</td>
<td>-0.017</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
<td>(0.111)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.116**</td>
<td>-0.065</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.072)</td>
<td>(0.078)</td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.053</td>
<td>0.011</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.127)</td>
<td>(0.126)</td>
</tr>
<tr>
<td>HH size</td>
<td>-0.029</td>
<td>-0.061**</td>
<td>-0.063***</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.024)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.390***</td>
<td>-3.066***</td>
<td>-3.423**</td>
</tr>
<tr>
<td></td>
<td>(0.859)</td>
<td>(1.143)</td>
<td>(1.357)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,972</td>
<td>1,669</td>
<td>1,669</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>y</td>
<td>y</td>
</tr>
<tr>
<td>Survey round FE</td>
<td>n</td>
<td>n</td>
<td>y</td>
</tr>
<tr>
<td>Communes</td>
<td>111</td>
<td>81</td>
<td>81</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.23

Household characteristics and saving status in the 1997/8 cross-section

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Avd. dist. to FI</td>
<td>-0.002 (0.002)</td>
<td>-0.002 (0.002)</td>
<td>-0.002 (0.002)</td>
<td>-0.001 (0.002)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.004*** (0.001)</td>
<td>0.003*** (0.001)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.057*** (0.012)</td>
<td>0.042*** (0.011)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.009 (0.019)</td>
<td>-0.006 (0.019)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.012*** (0.004)</td>
<td>0.008* (0.004)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.011 (0.015)</td>
<td>-0.013 (0.015)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>0.003 (0.020)</td>
<td>-0.010 (0.020)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td></td>
<td>-0.078*** (0.014)</td>
<td>-0.056*** (0.014)</td>
<td></td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td></td>
<td>0.042* (0.024)</td>
<td>0.060*** (0.023)</td>
<td></td>
</tr>
<tr>
<td>HH size</td>
<td></td>
<td>-0.005 (0.004)</td>
<td>-0.006 (0.005)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.085 (0.066)</td>
<td>0.197** (0.082)</td>
<td>0.674*** (0.109)</td>
<td>0.279** (0.138)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,823</td>
<td>3,823</td>
<td>3,823</td>
<td>3,823</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.027</td>
<td>0.017</td>
<td>0.028</td>
<td>0.049</td>
</tr>
<tr>
<td>Commune FE</td>
<td>n</td>
<td>n</td>
<td>n</td>
<td>n</td>
</tr>
<tr>
<td>Communes</td>
<td>149</td>
<td>149</td>
<td>149</td>
<td>149</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.24
Savings status and banking access measures
1997/8 cross section and nominal rice prices

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln price_{nominal}$</td>
<td>1.170**</td>
<td>1.365***</td>
</tr>
<tr>
<td></td>
<td>(0.562)</td>
<td>(0.485)</td>
</tr>
<tr>
<td>$\ln rice_{sold}$</td>
<td>0.091</td>
<td>0.141*</td>
</tr>
<tr>
<td></td>
<td>(0.100)</td>
<td>(0.082)</td>
</tr>
<tr>
<td>$\ln price_{nominal} \times \ln rice_{sold}$</td>
<td>-0.113</td>
<td>-0.156**</td>
</tr>
<tr>
<td></td>
<td>(0.085)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>Avd. dist. to FI</td>
<td>-0.005**</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.002**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td></td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.073***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.020</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td></td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.018***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.029</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td></td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td></td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.042+</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td></td>
</tr>
<tr>
<td>HH size</td>
<td>-0.003</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.834</td>
<td>-1.247**</td>
</tr>
<tr>
<td></td>
<td>(0.667)</td>
<td>(0.583)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,594</td>
<td>1,594</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.049</td>
<td>0.100</td>
</tr>
<tr>
<td>Communes</td>
<td>126</td>
<td>126</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
Table 3.25
Savings status and banking access measures
1997/8 cross section and real prices

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln ( price^{real} )</td>
<td>0.984+</td>
<td>1.188**</td>
</tr>
<tr>
<td></td>
<td>(0.603)</td>
<td>(0.531)</td>
</tr>
<tr>
<td>ln ( rice^{sold} )</td>
<td>0.063</td>
<td>0.113</td>
</tr>
<tr>
<td></td>
<td>(0.105)</td>
<td>(0.088)</td>
</tr>
<tr>
<td>ln ( price^{real} \times ln rice^{sold} )</td>
<td>-0.093</td>
<td>-0.138*</td>
</tr>
<tr>
<td></td>
<td>(0.092)</td>
<td>(0.077)</td>
</tr>
<tr>
<td>Avd. dist. to FI</td>
<td>-0.005**</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Age, head of HH</td>
<td>0.003**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td></td>
</tr>
<tr>
<td>Education, head of HH</td>
<td>0.073***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>Sex, head of HH, 2 = Female</td>
<td>-0.019</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td></td>
</tr>
<tr>
<td>Assets, raw index</td>
<td>0.018***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>Durable goods index, logs</td>
<td>-0.029+</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>Worked outside HH in last 7 days</td>
<td>-0.023</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td></td>
</tr>
<tr>
<td>Dwelling quality, index</td>
<td>-0.021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td></td>
</tr>
<tr>
<td>HH Type, 2 = Farm</td>
<td>0.038+</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td></td>
</tr>
<tr>
<td>HH size</td>
<td>-0.003</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.567</td>
<td>-0.977+</td>
</tr>
<tr>
<td></td>
<td>(0.696)</td>
<td>(0.623)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,594</td>
<td>1,594</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.043</td>
<td>0.095</td>
</tr>
<tr>
<td>Communes</td>
<td>126</td>
<td>126</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10, + p<0.15
Fixed effects coefficients not reported
3.A \( \psi \) under CRRA preferences

The coefficient of relative risk aversion is denoted \( \psi \) and defined for an arbitrary portfolio \( z \) as \( \psi = \frac{u'(z)}{u(z)} \). Using the definitions derived earlier, we write the value of removing portfolio risk for the household without a savings instrument as

\[
(c \mid x = 1) \approx -\frac{1}{2} \frac{y^2 \sigma^2_R}{u'(y \cdot \hat{R})} \times \frac{y\hat{R}}{y\hat{R}}
\]

where the last term allows us to write this as a function of \( \psi \):

\[
(c \mid x = 1) = \frac{\psi \sigma^2_R y^3 \hat{R}}{2}
\]

Similarly, the value of eliminating portfolio risk if the household has access to the risk-free savings instrument and can choose \( x < 1 \) is

\[
(c \mid x < 1) \approx -\frac{1}{2} \frac{y^2 \sigma^2_R}{u'(\hat{P})} \times \frac{\hat{P}}{\hat{P}}
\]

where \( \hat{P} = y(x(\hat{R} - R) + R) \), so this can be written as:

\[
(c \mid x < 1) = \frac{\psi \sigma^2_R y^3 x^2(x(\hat{R} - R) + R)}{2}
\]

Using the definition \( v = (c \mid x = 1) - (c \mid x < 1) \),

\[
v = \frac{\psi \sigma^2_R y^3}{2} (\hat{R} - x^3(\hat{R} - R) - Rx^2)
\]

we can see that the value to a household with CRRA preferences of moving from not owning a risk-free savings instrument to owning one will be maximized at \( (c \mid x = 1) \), and minimized at \( (c \mid x = 0) = 0 \). We study the intermediate cases by observing the direction of the change in this value for small changes in \( x \) by differentiating with respect to the portfolio weight. Since \( y > 0 \), the term \( \frac{\psi \sigma^2_R y^3}{2} \) is positive. Then the direction of change is given by

\[
= \frac{\psi \sigma^2_R y^3}{2} (-3x^2(\hat{R} - R) - 2xR) < 0 \ \forall x > 0
\]

As with CARA preferences, the value \( v \) is maximised when the household’s preferred amount of income \( y \) to hold in the risky asset is zero, and is zero when the household wants to hold all its income risk assets.
For the intermediate cases $x \in [0, 1]$, the value to a household without a savings instrument of owning one is strictly decreasing in $x$. Figure 3.6 shows the effect on $v$ of varying income by a small amount $\epsilon$ for the set of choices $x \in (0, 1)$.

### 3.B Average Partial Effects

While the marginal effect at the mean evaluated the fitted elasticity at the mean values of all the households in the sample, the average partial effect averages the elasticity fitted to each household. In a linear model the two are the same, but non in a non-linear case such as Probit: $E[\phi(\cdot)] \neq \phi(E[\cdot])$.

Formally, the APE of our model is

$$
E \left[ \frac{\partial \Pr(S_{ijt}=1)}{\partial \ln \text{price}_{ijt}^{\text{real}}} \right] =
E \left[ \phi(x'_{ijt} \hat{\xi}) (\hat{\beta}_1 + \hat{\beta}_3 \times \ln \text{rice}_{ijt}^{\text{gold}}) \right]
$$

We observe a distribution of effects because the function produces a value for each $i$ in the sample. The APE is the central moment of this distribution. Figure 3.7 shows the densities of this distribution for the VLSS data used here.

![Figure 3.7: Average Partial Effects: Probit model, real rice prices](image-url)
Since the mass greater than zero dominates that less than zero, the average partial effect is positive. The mean of this distribution is 0.29%, so a doubling of household income causes a 29% increase in the probability the household owns a formal savings instrument.

3.C  Data set construction

The variables in $x$ are from the 1992/3 and 1997/8 rounds of the Vietnam Living Standards Survey (VLSS). Since the format, structure, and specific questions change across the survey rounds, data for each variable are taken from different sections of the survey data in each round and merged on the common household-level identification variable hid92.

Table 3.26: Additional covariates & data locations, 1992/3 survey round

<table>
<thead>
<tr>
<th>Question</th>
<th>Description</th>
<th>Page no.</th>
<th>Data set .dta</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.A.2,5,6</td>
<td>Identity and age of HH head</td>
<td>7</td>
<td>SCR004</td>
</tr>
<tr>
<td>2.8</td>
<td>Education of HH head</td>
<td>13</td>
<td>SCR008</td>
</tr>
<tr>
<td>4.A.2,3</td>
<td>Work outside HH</td>
<td>19</td>
<td>SCR012</td>
</tr>
<tr>
<td>6.A.1</td>
<td>Dwelling type</td>
<td>36</td>
<td>SCR028</td>
</tr>
<tr>
<td>12.C.1</td>
<td>Household assets</td>
<td>98</td>
<td>SCR107</td>
</tr>
<tr>
<td>14.C.1</td>
<td>Savings technologies</td>
<td>105</td>
<td>SCR115</td>
</tr>
</tbody>
</table>

Note: Question 2.8 from the 1992/3 data is recoded so that any entries $\geq 6$ are set to 6, for compatibility with 1997/8 data.

Table 3.27: Additional covariates & data locations, 1996/7 survey round

<table>
<thead>
<tr>
<th>Question</th>
<th>Description</th>
<th>Excel file no.</th>
<th>Data set .dta</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.A.2,3,5,6</td>
<td>Identity and age of HH head</td>
<td>Sec01</td>
<td>SCR01A2</td>
</tr>
<tr>
<td>1.A.4</td>
<td>Education of HH head</td>
<td>Sec01</td>
<td>SCR02A</td>
</tr>
<tr>
<td>4.A.2,2</td>
<td>Work outside HH</td>
<td>Sec04</td>
<td>SCR04A</td>
</tr>
<tr>
<td>6.A.1</td>
<td>Dwelling type</td>
<td>Sec06</td>
<td>SCR06A</td>
</tr>
<tr>
<td>9.B1.4,6</td>
<td>Rice production measures</td>
<td>Sec09</td>
<td>SCR09B1</td>
</tr>
<tr>
<td>12.C.1</td>
<td>Household assets</td>
<td>Sec12</td>
<td>SCR12C</td>
</tr>
<tr>
<td>14.C.1</td>
<td>Savings technologies</td>
<td>Sec14</td>
<td>SCR14C</td>
</tr>
</tbody>
</table>

The World Bank and Vietnam's GSO have prepared two household-level datasets for 1997/8 and 1992/3 data: HHEXP98N.DTA and HHEXP92N.DTA, which include hhtype and hhsize, a binary variable for farm / non-farm households and the total number of full-time household members, respectively.

A common household identifier is generated in the 1998 data set SCR01B using:

```
gen hid92=s0bq02*100+s0bq021
```

which generates 1992 household codes from the 1998 household identifier. The resulting file contains both househol and hid92, allowing data from both survey rounds to be combined (hid92 identifies panel household). All missing
observations are dropped, as these are households in the 1998 survey round that are not in the 1992 survey round (i.e., they do not form part of the panel).

drop if hid92 == .

Using generate hid92 = hholdno to create a matching variable in the 1992/3 data, the data sets are combined using

merge 1:1 hid92 using <filename>

where the 1992 data is the master data set in memory and the 1998 data is the using data set.

This generates a “wide” format data set consisting of unique households, with each variable “stub” attached to a two-digit identifier for the relevant survey round, for example agegroup92 and agegroup98. For time series work, the wide format data set is reshaped to a long format version using

reshape long <stubs>, i(hid92 househol hholdno) j(round)

resulting in a data set with observations uniquely identified by household number and survey round. To declare the long format data as a two-period time series, we use
tsset hid92 round, generating a strongly balanced survey panel data set consisting of 4,302 households. The stubs are the variable names.

To create unique commune-level codes for each household, we use the 1998 commune numbering scheme by issuing

gen village_98_coding=int(hhousehol/100)
gen commune_98_coding=int((village_98_coding+1)/2)

Several of the variables are coded as text (string) format, and need to be re-coded to be used in our empirical framework: agegroup, farm, urban92 urban98. These can be recoded into binary or categorical variables using egen varname_numeric = group(varname).

3.D Construction of key variables

Rice production:

The natural experiment affects households' budget constraints through an exogenous change in the price of rice. The quantity of rice supplied is measured by two variables drawn from section 9, part B1 in file SCR056 of the 1992/3 survey data and SCR09B1 1996/7 survey data. The two sections are essentially identical, so we can derive consistent measures of rice production.

Question 4 from section 9 of the survey asks how much rice was harvested during the previous 12 months, question 3 asks what the area in m² under cultivation was, and question 6 asks how much rice was sold or bartered during the last 12 months for each of: paddy, ordinary, and specialty rice. We

\footnote{In the 1992/3 data this is translated from the Vietnamese version of the questionnaire as “glutinous rice”}
aggregate each production measure across all three types of rice for each household, for the three measures of rice production used here: $r_{\text{harvested}}$ in kg, $r_{\text{cultivated}}$ in m², and $r_{\text{sold}}$ in kg.

**Savings technologies:**

In 1997 questionnaire, data on savings accounts are in section 14c, recorded data set SCR14C.DTA. The possible answer values for question 14C1 are from 1 - 15. Each value is a different savings technology, a subset of five of which are used to define whether a household has access to a formal savings technology. $S_i = 1$ for household $i$ if it has at least one of:

<table>
<thead>
<tr>
<th>Savings technology</th>
<th>Response</th>
</tr>
</thead>
<tbody>
<tr>
<td>Savings book at State bank</td>
<td>1</td>
</tr>
<tr>
<td>Savings book at other type of bank?</td>
<td>2</td>
</tr>
<tr>
<td>Savings book at credit cooperative?</td>
<td>3</td>
</tr>
<tr>
<td>Government Bonds?</td>
<td>4</td>
</tr>
</tbody>
</table>

For the 1992 questionnaire, a scanned copy of the questionnaire is provided instead of questions disaggregated into Excel sheets. Section 14, C, question 1 is the same, and the categories are the same. Page 105 of the survey questionnaire details the question and the mapping from this question and the categories is one-to-one with respect to 1997. The responses to 14C in the 1992 data are in SCR115.DTA.

**Rice prices:**

In both the 1992/3 and 1997/8 survey rounds, an additional questionnaire was administered at the commune level that collects price data in thousands of nominal Dong for a range of food, non-food, medical, and agricultural products. Section 1 of the price survey for both rounds records up to three nominal market prices per kilo for each of

<table>
<thead>
<tr>
<th>Crop types</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ordinary paddy</td>
</tr>
<tr>
<td>Ordinary rice</td>
</tr>
<tr>
<td>Glutinous rice</td>
</tr>
</tbody>
</table>

where each price should have been recorded from an independent market (i.e., three distinct markets within the commune, not three prices within the same market). Where this was not possible, one or two prices are recorded. In addition to the nominal price, the month in which the enumerator made the price observation was also recorded, allowing us to construct real (deflated) values for rice prices.

While the 1992/3 survey disaggregates the rice production measures detailed above by each type of rice, the second survey round only does this for ordinary rice and glutinous. For comparability across the surveys, we measure nominal prices in thousands of Dong per kilo as the average of the three prices of paddy, ordinary, and glutinous rice.  

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11Paddy, rice, and glutinous rice are not perfect substitutes. Paddy rice consists of unprocessed rice kernels without their protective hull removed, which must be milled into ordinary...
To ensure prices are comparable across the two survey rounds, we deflate the nominal rice prices with the consumer price index \(mcpi\) which is constructed with January 1998 = 1, and each month of the survey in either survey round is set relative to this value.

**Savings services**

The paper measures access to savings using the proxy of average distance in Kilometers to the nearest financial institution that offers a formal savings product. Question 5.1 of the 1997/8 community-level questionnaire asks the respondent to list four institutions that offer financial services to the commune’s residents and question 5.2 asks whether that financial institution offers options for credit (1), savings (2), or both (3).

We limit the data set to only those institutions for which 5.2 is \(\geq 2\), or only those institutions that offer some form of savings products, by issuing `keep if s05q2 >= 2` and then calculate the average number of kilometers to these institutions from the commune headquarters of the People’s Committee using `bysort commune_98: egen avg.distance = mean(s05q6)`. This delivers a measure of the average distance from the commune to institutions that offer formal savings products using data from `CMT05.dta` of the 1997/8 survey data set.
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