No bricks without straw:
a critique of Ravi Kanbur's 'Modest Proposal for Introducing Development Outcomes in IDA Allocation Procedures'

by

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Introduction:

I submit this short note in response to the paper by Ravi Kanbur (2005) circulated in advance of the meeting at Columbia University on April 5th, 2007, organised by the Initiative for Policy Dialogue at Columbia University on the CPIA and Aid Allocation. I agree with Ravi Kanbur that the two key factors in aid allocation are 'aid productivity' or performance and "valuation of outcomes" or "need". That, however, is where the agreement stops. In particular, I am dismayed by his wholesale dismissal of existing methodologies for prioritising and allocating aid. Although the use of final outcome indicators in the IDA aid allocation formula is not undesirable in principle, the motivation offered by Ravi Kanbur in his specific proposal is unconvincing at best. If it leads to less emphasis being placed on governance indicators, the net result for the quality of aid allocation could be disastrous.

I. The conventional aid effectiveness model

Adopting Kanbur's notation, let D^i be a measure of the final development outcome for country i (for simplicity take it to be a scalar), W^i the donor's valuation of the development outcome of country i, A^i aid given to country I, Y^i a vector of structural variables that the government cannot control and that are independent of the volume of aid, X^i a vector of policy variables under the control of the government. We should think of it not only as policy actions, but governance rules and institutions that can be changed, given time, by the government. Following Ravi, let

$$W^i = w(D^i), \ w' > 0, \ w'' < 0 \tag{1}$$

and

$$\begin{aligned} D^i &= d^i(A^i;...) \\ &= \alpha + \beta Y^i + \theta X^i + \gamma A^i + \eta X^i A^i + \varepsilon^i \end{aligned} \tag{2}$$

where ε^i is a classical stochastic error term which is independent of all the other variables on the RHS of (2). The interaction term between aid and government policy is the simplest way to motivate the kind of governance-type intermediate indicators used in the CPIA, since

$$\frac{\partial D^i}{\partial A^i} = \theta + \eta X^i \tag{3}$$

II. The first disagreement with Kanbur: conditionality matters

My first disagreement with Ravi is when he says that

".... (2) sees no role for the aid flows themselves to influence policy, in other words, it sees no role for conditionality in changing government policy. This is surely right, because if the experience of two decades has taught us anything, it is that the development assistance tail cannot wag the domestic political economy dog. Rather, we should take the policies as emerging out of the domestic political economy, and take them as givens in the aid allocation decision".

I find this statement extraordinary. Equation (2) has nothing to say about the presence or absence of any relationship between aid and policy, but is consistent with any view on this relationship. I can easily append a set of relationships mapping government policy into aid (and vice-versa). In the simple case where government policy is a scalar, this yields:

$$\begin{aligned} X^i &= x^i(A^i;...) \\ &= k^i + \phi A^i + v^i \end{aligned} \tag{4}$$

Doing proper political economy for the recipient country government means recognising the existence of (4). Therefore, instead of (3) we have

$$\frac{\partial D^i}{\partial A^i} = \theta + \eta X^i + \eta \phi A^i \tag{5}$$

The fact that Burnside and Dollar (2000) found no relationship between aid and policies in a jointly estimated equation system may well be due to the fact that there is, in the data, a third missing relation between policies and aid, reflecting the

political economy of the donors; Under the CPIA framework, aid was indeed dependent on policies, so

$$A^i = a^i(X^i;...)$$
$$= l^i + \varphi X^i + \zeta^i \tag{6}$$

Here k^i and l^i are structural variables independent of policy and aid and v^i and ζ^i are classical regression disturbances. The joint endogeneity of aid and policy means that the Burnside and Dollar estimation methods are inconsistent. Indeed, the effect of aid on policy, captured by ϕ may not even be identified.[1]

The key point is that as serious social scientists, we have to allow for the possibility of an effect of (the prospect of) aid on policy. In other words, we have to recognise that conditionality may matter, even if it has been used badly or ineffectively in the past. If governments care about aid and recognise that different policies may produce different volumes of aid, then policy will depend on aid and not allowing for this relationship would lead to suboptimal outcomes from the perspective of the donors. Whatever the experience of 40 years of conditionality has taught me, it

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[1] Equations (4) and (6) imply the reduced form:

$$X^i = (1-\phi\varphi)^{-1}k^i + (1-\phi\varphi)^{-1}\phi l^i + (1-\phi\varphi)^{-1}(\phi\zeta^i + v^i)$$
$$A^i = (1-\phi\varphi)^{-1}\varphi k^i + (1-\phi\varphi)^{-1}l^i + (1-\phi\varphi)^{-1}(\zeta^i + \varphi v^i)$$

We therefore can, at most, if we have observations on both k^i and l^i and if k^i and l^i are not perfectly correlated (roughly speaking, are not the same variable, so we have exclusion restrictions), end up with consistent estimates of the following reduced-form coefficients: $(1-\phi\varphi)^{-1}$, $(1-\phi\varphi)^{-1}\phi$, $(1-\phi\varphi)^{-1}\varphi$

$$\Omega = (1-\phi\varphi)^{-2}\begin{pmatrix} \phi^2\sigma_{\zeta^i}^2 + \sigma_{v^i}^2 & \phi\sigma_{\zeta^i}^2 + \varphi\sigma_{v^i}^2 \\ \phi\sigma_{\zeta^i}^2 + \varphi\sigma_{v^i}^2 & \sigma_{\zeta^i}^2 + \varphi^2\sigma_{v^i}^2 \end{pmatrix}$$

Unless either ϕ or φ equals zero, there is no useful information in the variance-covariance matrix of the reduced-form disturbances. If k^i and l^i are the same variable, the only informative reduced-form coefficients we can estimate are $(1-\phi\varphi)^{-1}(1+\phi)$ and $(1-\phi\varphi)^{-1}(1+\varphi)$, so all we can identify is

$$\frac{1+\phi}{1+\varphi}.$$

is not that we should treat endogenous behaviour as exogenous. Incentives matter in the economics and political economy I practice.

Proposition 1. Conditionality ('the rules and procedures according to which a donor transfers resources to a recipient, (Kanbur 2005, p. 3)) can be expected to influence the behaviour of the recipient. Not recognising these potential effects and not allowing for them to the best of our ability is bad economics and will lead to bad aid allocation.

On a related point, Ravi's statement that *"the development assistance tail cannot wag the domestic political economy dog"* assumes that there is such a thing as domestic political economy. There is not. There is political economy: domestic and international pressures and constraints jointly influence the behaviour of the recipient country government and of the other key domestic stakeholders in the aid game. But at least Ravi does not drag out the deservedly much-maligned concept of 'country ownership', and he deserves credit for that (see Buiter.

III. The second disagreement with Kanbur: do we need a 'common development model'?

Ravi correctly points out that the CPIA approach can be fully justified only if equation (3) is the same for all potential recipient countries. If instead we had

$$\frac{\partial D^i}{\partial A^i} = \theta^i + \eta^i X^i \qquad (7)$$

or, recognising the political economy channel:

$$\frac{\partial D^i}{\partial A^i} = \theta^i + \eta^i X^i + \eta^i \phi^i A^i \qquad (8)$$

aid allocation would not have much to go on, unless we somehow managed to come up with estimates of the country-specific parameters in (7) or (8). What can I say? Life if tough and social science is tougher. If every country is unique, any cross-country comparative work is useless and we can close most applied social science research units and stop the teaching of panel data methods to macroeconomists. I

believe that Ravi's position is far too nihilistic as regards what can be learnt about aid effectiveness in a country from the experience of other countries and from the past history of the country itself. In any case, there is no alternative. If we are unable to beg, steal or borrow the country-specific parameter values (in general this includes different functional forms and different lists of policy instruments as well), economists and other social scientists have no useful contribution to make to the aid effectiveness debate. I don't believe that. There is information out there that is useful – the result of decades of research.

Researchers, especially those coming from a microeconometrics/panel data estimation tradition where unobserved heterogeneity, sample selection bias and path dependence have been recognised as key issues for decades, are trying to address the issue of 'uniqueness' through country effects (fixed and/or random) and similar approaches, without abandoning the hope that something can be learned from other countries' experience (and indeed also from the past experience of the country itself)..

IV. The third disagreement with Ravi: realised past outcome changes as a measure of future aid productivity

What will almost surely fail to produce even a faint approximation to the true model (7) or (8) is Ravi's proposal for a 'final outcome indicator'. What he proposes is in fact:

$$\frac{\partial D_t^i}{\partial A_t^i} = \frac{\Delta D_{t-j}^i}{A_{t-j}^i} \quad j \geq 0 \tag{9}$$

where ΔD^i is the (average) change in the performance indicator over some historical past period.

It is possible that there exists a universe in which (9) would be a reasonable approximation to (7) or (8), but if there is one, I have never encountered it. It isn't

even 'post hoc ergo propter hoc'. Let's quote Ravi: *"A country that has very low levels of girls' enrolment in primary schools should get more aid on grounds of need. But a country that is showing rapid improvement of girls' enrolment from this low level, relative to the aid it is receiving, should get even more."* The first sentence makes sense if there is evidence that more aid does indeed boost girls' enrolment. So Ravi must know something like equation (7) or (8) for the country in question. However, one could complete the second sentence of this quote, with equal lack of justification, with the words *'…, should therefore get less aid, because it does very well without aid and therefore obviously does not need aid.'*

Ravi's complete lack of confidence in cross-sectional evidence contrasts starkly with his astounding faith is the most naïve form of time-series evidence: the past actual change in some performance indicator during the benchmark period, relative to the volume of aid allocated (to a country, or just to the improvement of that indicator in he country in question?), is an indicator of the present or future marginal product of aid for that indicator. The aid could have been looted, diverted or wasted, that is, not even spent on any activity likely to boost the indicator, and the improvement in the indicator could have been produced by domestic or foreign factors that have nothing to do with the aid dispensed during the benchmark period, but never mind…

Ravi appears to believe that by suitably normalising the rate of improvement of some outcome variables over some period by the total aid flow over this period, he actually takes a structural ('behavioural') partial derivative of future improvement of the outcome variables with respect to a current increment in aid. What earthly reason could there be for this belief? A country that gets no aid in the reference period but

records any progress at all would show an infinite productivity of aid, even if it might not be able to use aid effectively..

As responsible social scientists we cannot possibly recommend that aid be allocated on the basis of Ravi's proposed criterion. I summarise this argument as Proposition 3:

Proposition 3: Ravi Kanbur's proposed measure of performance - the rate of improvement of some outcome variable(s) over a given period of time, suitably normalised by the total aid flows over this period – makes no sense and should be firmly rejected..

V. Conclusion

There is no royal road to the determination and measurement of aid effectiveness. Current approaches, including the CPIA are imperfect and in need of improvement. There is no alternative to learning systematically about aid effectiveness, using all available information – cross-section, time-series and panel data. Recognising the uniqueness of each country does not mean that a common framework cannot be developed. The final outcome indicator proposed by Ravi Kanbur is without merit.

Would I support adding the past change in some performance indicators as a further criterion in CPIA-type exercises. Not without much further study of both the incremental predictive content of this indicator for future aid effectiveness and a more robust understanding of the causal mechanism involved. Even if there is statistical persistence in the within-country behaviour of some indicator, and even if this persistence survives the Granger-causality filter and turns out to be incremental predictive content, this by itself tells us nothing about aid effectiveness as regards this indicator. Unless Ravi Kanbur can yield the kinds of models and data that he rejects to show and convince me that past output indicators really are a good guide to future

aid productivity, I would not feel comfortable with adding such a final outcome indicator to the existing bundle of indicators.

References

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