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**Making Aid Smart:**  
Institutional Incentives facing Donor Organizations and their  
Implications for Aid Effectiveness

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## **MAKING AID SMART**

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### **Abstract**

Resources for foreign aid are scarce, and often face skepticism from tax payers with respect to their effectiveness. In order for aid agencies to continue to do good work, they need both to become more effective and to demonstrate their effectiveness. This paper provides seven suggestions on how to make aid “smarter”.

1. We should target aid to countries with good institutional and policy environments.
2. Aid should promote reform, not through conditionality, but through investments in the capacity for self-reform.
3. Aid should finance basic services in the most needy environments, scarred by the deepest poverty.
4. We should target aid to mitigate against price shocks.
5. We should target aid to facilitate post-conflict recovery.
6. We should target aid to prevent conflict.
7. We should use aid as demonstration projects in order to leverage successful programs.

Although there is good empirical and theoretical support for each of these suggestions, reflections on institutional economics imply that people within aid agencies will not have incentives to take up these suggestions. Therefore, the first challenge is to reconfigure the incentives of workers within the aid agencies themselves so as to enable aid to become more effective.



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### **1. Introduction**

Aid is a scarce resource. Tax payers are suspicious that aid is ineffective, and this skepticism has gradually undermined budgets. Aid needs to become demonstrably effective. At present, some of it is and some of it isn't. The challenge is to make aid 'smart' – closing down ineffective uses, and scaling up the most effective.

I am going to suggest three ways in which aid can be made smarter. The first is simply an extension of my past work with David Dollar on aid effectiveness. We make aid smarter by putting it in countries where policies, institutions and other circumstances make it atypically effective and by avoiding countries where it is likely to be ineffective or even counter-productive. The second way in which aid can be made smarter is if it can actually promote reform. I argue that aid can assist reform, but not usually through old-style conditionality. Rather, we have to strengthen the capacity of the society to reform itself. The third way in which aid can be made smarter is if it can be used to finance basic services in the most needy environments. Often these environments are unreformed, and

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The findings, interpretations, and conclusions expressed in this paper are entirely those of the author. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent.

so conventional channels of service delivery are ineffective. I sketch alternative arrangements through which aid might be channeled effectively.

## **2. Aid Effectiveness 101 – a quick revision course**

Most aid is in the form of investment projects. It is very tempting to infer the overall impact of aid from summing the returns on these projects. There are three reasons why this is hopelessly wrong. First, in some form or another, some of these projects would have happened even without the aid. This is the fungibility argument – aid finances the marginal government activity, not what it is ostensibly linked to. Second, projects have externalities that are missed at the level of the individual project but may become apparent in aggregate. For example, there is some evidence that where government ownership of reform is weak, aid increases corruption. We might imagine that if a donor implements a project for which the government has little real interest, public officials devise means to divert the resources, and that the accumulation of these skills of diversion gradually become the corruption problem. Third, the growth process is not, primarily, about investment at all, it is about behavioral change. Aid works best where it accelerates behavioral change. Where it merely adds to the capital stock without changing behavior, its effects are peripheral. Conceptualizing the role of aid as incremental investment and implementing this through projects thus condemns aid to marginality. Hence, projects can look fine and yet overall economic performance can be rotten. The implication is that project-level performance is an inadequate instrument for attaining donor objectives. Yet donor organizations are heavily focused upon raising project-level performance, through design, supervision and evaluation. The project-level focus is not only ineffective, it is immensely costly relative to other means of raising donor effectiveness.

Smartening aid depends upon strategies that fall between the hopelessly small perspective of the project mentality – with which we cannot hope to see the wood for the trees – and the equally hopeless grandiosity of the conditionality approach, in which the donor simply buys the desired policies (an approach I will discuss below).

David Dollar and I have worked on how a reallocation of aid between countries might raise its effectiveness. We estimate that it would be possible approximately to double aid effectiveness in poverty reduction simply by targeting aid on those countries that have the most severe poverty while also having reasonable policies and institutions (Collier and Dollar, 2001, 2002). The Collier and Dollar model of aid, growth and poverty reduction deduces this formally, from a regression analysis, but it is also common sense. In countries with very weak policies, governance and institutions aid soon becomes ineffective – projects have a high failure rate, and aid might actually further undermine governance (Knack, 2001).

Nevertheless, aid flows in large amounts to middle-income countries, and to countries with poor policies. The reasons for this massive misallocation of aid are partly political and partly bureaucratic. Political influence is only to be expected, but astute bureaucrats can do something to contain it. The most important defensive strategy is benchmarking – having a clear allocation rule based on principles of poverty-effectiveness. Political attempts to change allocations from this benchmark must then confront the ugly fact that such reallocation will be poverty-increasing. Discretion starts to look a lot more costly when the political decision has to justify itself against specific evidence as to its cost. Fighting bureaucratic inertia is also difficult. I remember discussing with a Scandinavian ambassador why high country's aid agency continued to send annual evaluation missions of a particular program when it was clearly redundant. His explanation was that staff enjoyed the excitement of leaving a Scandinavian winter. Until recently the Dutch aid program provided finance to over eighty countries. In purely diplomatic terms this might have made sense – whichever country a Dutch minister visited, there was always some aid project to discuss. But in terms of operational effectiveness it was clearly ridiculous. Evelyn Herfkens, the pioneering Dutch aid minister, refocused the program on 17 low-income countries. The poverty-effectiveness of Dutch aid soared as a result.

### **3. Beyond Aid Effectiveness 101 – new results in aid targeting**

The lessons of Aid Effectiveness 101 are indeed being learnt by donors. Allocation practices are changing – aid is getting smarter. Note, our objective is not to get aid allocations set into some rigid formula based on limited information used mechanistically. Rather, we are trying to provide defenses against the overt manipulation of aid allocations for political or commercial objectives. Now I want to turn to some recent results that open up further ways in which aid can be targeted to countries and situations in which it is atypically effective.

### *Targeting aid for shocks*

Many low-income countries are exposed to severe external shocks as their export prices fluctuate on world markets. Potentially, aid could cushion these shocks. Together with Jan Dehn I have studied the effect of 47 large negative export price shocks (Collier and Dehn, 2001). The typical shock was a 40% fall in the price of the countries exports, one year on the next, representing a loss of around 7% of GDP. We studied what this did to the growth of the economy of the next three years. We found that on average the economy went into a recessionary tail-spin, with output contracting cumulatively by around 14%. Thus, each dollar lost through exports triggered a further loss of \$2 of output. More importantly, we found that when aid happened to increase during these periods, it arrested the tail-spin. An extra dollar of aid reduced the contraction in output by \$2. Thus, aid during these periods of severe adverse shock had an extraordinarily high return, for this macroeconomic effect had to be added to its normal return.

Despite this high return of aid during negative shocks, we found that on average there had been no tendency of aid to increase during these periods. Donors were simply missing a major opportunity for smart aid. The reason for this was quite simple – donor agencies were in no position to respond quickly to such opportunities. The clearest example of this was the STABEX scheme of the European Union. This had precisely the intention of channeling aid into countries experiencing negative export price shocks. However, the chosen vehicle for aid delivery was projects, and indeed, more specifically, agricultural projects. The lag between entitlement and disbursement was so long that STABEX

tended actually to be pro-cyclical –arriving during periods of high export prices. The only other global mechanism for price cushioning was the IMF’s CCFF. This was virtually never used because it provided only non-concessional lending – such borrowing at a time of falling export prices was correctly seen as too risky by most governments. STABEX was abandoned in 2000, as was the contingent part of CCFF, with the residual component of compensatory financing so seldom used as to be in *de facto* abeyance. Thus, currently the world has no instrument for cushioning shocks. Any such scheme has to be capable of delivering aid within a year or so of the shock, and this precludes project aid.

I know of two bilateral donors that are actively looking at schemes for targeting aid to these situations of extreme negative shocks. At the IFI level, a possibility for IDA to consider would be for loan repayments to be deferred during the onset of such a shock. The advantage of such a response rule is that it does not depend upon the negotiation of new lending but could be triggered automatically by a fall in export prices of a particular magnitude. Sometimes the pay-off to such interventions could be even higher than our estimates suggest. For example, recently the governments of both Ghana and Uganda faced elections that happened to coincide with severe declines in their export prices. Both of these governments were reforming governments by African standards, and so both were in some sense defending a record of reform during a severe economic downturn. The government of Uganda was re-elected, that of Ghana was defeated. As it happens, the new government of Ghana has proved to be equally interested in reform as its predecessor. However, it would clearly have been possible for these shocks to have destabilized reform processes.

### *Targeting aid for post-conflict*

Another way of making aid smart is to target aid better in post-conflict situations. We now know approximately what poverty-efficient flows of aid to post-conflict situations would look like (Collier and Hoeffler, 2002). First, for the first decade of post-conflict, the volume of aid would be approximately double what it would be on the Collier-Dollar formula – post-conflict societies have a high absorptive capacity for aid. Secondly, the



phasing of that aid inflow would be a parabola, rising from modest levels in the first three years to a peak around the sixth year, and gradually reverting to normal levels by the end of the decade. Note that this represents the lower bound of appropriate aid flows into post-conflict countries because it is concerned only with the objective of poverty reduction. Aid should be larger in post-conflict settings because it is atypically effective in the growth process at such times. However, most agencies have additional concerns in post-conflict environments, most obviously that the conflict should not restart. On average, countries face a 50% risk that a conflict will restart within the first five years of peace, hence this is a very real concern and it may be sensible to allocate aid in excess of that which would be appropriate simply on the criterion of poverty reduction.

Actual aid flows post-conflict do not look at all like the pattern that we have suggested would be appropriate. On average, they are too small, and on average they come in too soon and start tapering out just as they should be scaling up. Further, the average conceals huge politically-driven variations – Bosnia, East Timor, and now Afghanistan, being favored, whereas Sierra Leone and various of the other African post-conflict situations were until recently largely neglected. Post-conflict situations are inevitably highly politicized, but for this very reason, poverty-benchmarking is even more important than normal.

#### *Targeting aid for conflict prevention*

So far I have considered the objective of aid to be poverty reduction. For an agency such as the World Bank this is indeed the sole focus. However, bilateral agencies might also legitimately have conflict prevention as an objective. Aid can reduce the risk of conflict by accelerating growth and diversifying the economy away from primary commodity dependence. Hence, potentially, even aid to some middle-income countries might be justified if these countries had, for whatever reason, a high risk of conflict. However, aid allocation would still need to consider policy and institutions. Where these were poor, even though a country had a high risk of conflict, aid would be ineffective in reducing that risk because it would not be able to increase growth.

In fact, conflict risk tends to be concentrated in low-income countries. Taking this in combination with the fact that policy matters just as much for the effectiveness of aid in conflict prevention as in poverty reduction, switching from the objective of poverty-reduction to that of conflict prevention does not introduce such a radical reallocation of aid as might be supposed. Nevertheless, there undoubtedly are some countries with reasonable economic policies and institutions that face atypically high risks of civil conflict. For these countries a bilateral concerned with the maintenance of peace should allocate aid in excess of that warranted purely by the poverty reduction criterion. What is not warranted by the peace objective is to pour large aid flows into high risk poor policy environments.

#### **4. The donor catalytic role**

Aid effectiveness 101, and the extensions I have just discussed, takes policy and institutions as given. This is not a simplification, it reflects the brute fact that donor interventions have not been very effective at changing policies and institutions. However, here I get more ambitious and consider how donors might best influence reform. I start with a quick recapitulation on how not to do it. Then I turn to assisting reform in environments that are already reasonable. Finally, I turn to reform in countries that have proved deeply resistant to it.

##### *Aid as an incentive for reform*

At the other end of the spectrum from project aid is aid-for-reform. Superficially, such aid appears to be 'smart' – it is trying to change the incentives for behavioral change. The reason why this approach to aid is also discredited is that the 'aid-for-reform' approach takes a hopelessly naïve view of the reform process and of the game that is being played.

Even at the most elementary level, aid has two opposing effects a substitution effect and an income effect. With respect to reform these two effects work against each other – the

substitution effect is the direct carrot effect, but the income effect tells us that aid may get the government off the hook of crisis, and so will tend to reduce the appetite for reform. The net effect is in theory ambiguous.

A second problem with this game is that, by making reform the price that must be paid for aid, it creates an incentive for governments to drive up the price. That is, an astute government will exaggerate the costs of reform, pleading difficulties and obstacles so that it 'sells' as little reform as possible for a given amount of aid. Even reform-minded governments have an incentive to do this, but it places them in a poor light with the investor community since they are inadvertently signaling their reluctance to reform.

If the government 'sells' reform as the price to be paid for aid, it is also clear who 'owns' the reforms – the donors. I recall in the early 1990s an African president, exasperated by donor demands for democratization, who retorted that if the donors didn't shut up he would reverse the economic reforms. That this could be meant – and understood – as a threat, implied that in that country it was quite clear who indeed owned the reforms, namely the donors. In such an environment of low government ownership it is easy for governments to reverse reforms. The aid-for-reform game provides them with every incentive to do so – namely, they can sell the same reform all over again. During a fifteen year period the government of Kenya sold the same agricultural reform to the World Bank five times, reversing it each time.

Further, most real-world reforms have fairly low observability – their operation depends upon behavior at the local level, or within the bowels of various ministries, over a long period. Presidents have learnt over time to install finance ministers who talk the talk that donors wish to hear, and write the documents that donors wish to read. Presidents can sign these documents without actually implementing effective change.

Donors are extremely badly structured to play hardball with such presidents. Their staffing has radically less continuity than the typical president. Worse, their staff have strong incentives to keep lending, both to prevent default and to maintain the flow of

project work which underpins their jobs. Conditions are seldom clearly breached because implementation is hard to monitor and if they are breached there are many mitigating circumstances. Finally, donors do not agree among each other, and can relatively easily be divided by an astute president. For whatever reason, conditionality has not worked – there has been no link from aid flows to the pace of reform (Dollar and Svensson, 2000). The World Bank's recent study, *Aid and Reform in Africa*, which looked at ten countries in detail, concluded that the only circumstances in which conditionality had been helpful was for a brief period when a government was already fully embarked upon a reform program and committed to it.

Aid provided to governments with very poor policies as an inducement for them to change those policies is not smart. In playing this game the donor community wasted large sums of money. There is even evidence that in conditions of low government ownership of reform, aid significantly increases corruption (Knack, 2001). Hence, the aid-for-reform game not only wastes resources that could otherwise be used to reduce poverty, it might actually be intensifying the problem it is attempting to address.

### *Demonstration projects*

In countries where policies and institutions are reasonable, aid is effective. Historically, most of the aid flows in these conditions have been through projects. The traditional reason why agencies have done projects has been closely related to fiduciary responsibility – projects were what aid paid for. We all know that in reality projects do not provide fiduciary responsibility due to fungibility. Donor projects are substitutes for government projects and so release government resources for other uses. Aid therefore really finances the marginal activities that governments choose to do with the resources released by the presence of aid projects. I now want to suggest a different rationale for projects, namely their role in promoting change through demonstration effects.

Take a major government activity such as education. Education budgets in developing countries total around \$200bn annually. Suppose that by a major effort, donors

collectively provide \$5bn for education. This may raise total spending to \$205bn, but more likely, it will raise it to around \$201bn – around a fifth of the incremental resources being devoted to education budgets. The other \$4bn then get spent on whatever else are priorities at the margin. However, even if the full \$5bn is spent on education, it constitutes a very small percentage increase in total educational spending. The improvement in educational outcomes will be modest if the \$5bn achieves only the same productivity as average educational spending. The challenge is to use the aid as a catalyst for change. A useful way of conceptualizing this is to think of donor projects as demonstration projects.

If the rationale for donors to do projects is that they are demonstrations, then the design of projects needs to be very different from past practice. First, the project needs to embody something that is innovative, at least in the country or region in which it is being located. This implies that far greater resources need to be devoted to project preparation to ensure that it is indeed a vehicle for something worth learning. Second, if the purpose of the project is to demonstrate that it is worth copying, then convincing evaluation must be built into the project from the design stage. Evaluation is not there in order to check up on the project, it is a critical part of the project. If outcomes cannot be demonstrated, the project cannot function as a demonstration project. Third, the purpose of the project is to convince a target group of decision takers to change their current practices. Again, at the design stage, the project will need to identify this target group and develop a communications strategy whereby, if the project is successful, the target group will learn about it in a convincing way. When a demonstration project comes to be evaluated, what should be evaluated is not whether the project ‘worked’ in the sense of functioning according to its direct purpose, but rather whether government practices changed as a result of the project. Only through demonstration projects can a few billion dollars of educational lending hope to transform educational outcomes that are the result of how \$200bn of resources are utilized.

*Reform in poor policy environments*

I now want to return to the problems posed by those low-income countries that have very poor policies and institutions. On my analysis they are not appropriate for large aid programs. In these environments projects have high failure rates and so even the role of demonstration projects is limited. Evidently, what these countries need is policy and institutional change, but conditionality, I have argued, is not the means by which such change will be achieved. I now pose the question whether there is anything that the donor can do to effect change. I suggest a two-stage process by which donor agencies can hope to accelerate the pace of policy and institutional change. The first stage is to identify a limited agenda of reform. The second stage is to strengthen those domestic actors best able to promote this agenda.

In environments where virtually everything is wrong, analyzes are typically comprehensive condemnations of present policies and institutions. Or worse, whatever specialist team visits the country will find considerable need for improvement in its own area of specialization. Such analyzes are unhelpful for the practical business of change since change is largely incremental. Evidently, these societies have an unusually low capacity to implement change and so, despite their atypical need for change, the change agenda should be kept modest and highly focused. What are the two or three 'zero generation' reforms? I suggest that they should meet two distinct sets of criteria, economic and political. The economic criteria are the easier to satisfy. Because so many policies are wrong, many reforms would yield improvements. Obviously, this is subject to the caveat of 'second best' theory – sometimes, putting one policy right in isolation can have perverse effects. Second best considerations will imply that some sequences of reform are superior to others. However, it is highly unlikely that there is a unique sequence. Economic considerations will therefore leave a wide choice as to which reforms should be prioritized. The other set of criteria are socio-political. Reforms should be chosen that are feasible in socio-political terms and that, once completed, sufficiently reinforce the constituencies for reform, that other reforms become feasible, thus opening up the next generation of reform. What characteristics should be sought in reforms that are socio-politically feasible? One criterion is surely that they should have a swift and substantial pay-off to those groups most critical for further reform. For example, a group

that is likely to be significant in the reform process is small and medium enterprises. Deregulation of such enterprises might have rapid effects and so strengthen their sense of being able to improve their policy environment through collective action.

However, we should not over-estimate the ability of people to notice and correctly attribute small changes in their circumstances. For example, in Uganda during the 1990s per capita growth was so rapid that the incidence of absolute poverty declined by around 40%, with the distribution of income virtually unchanged. Objectively, poverty reduction in Uganda during the 1990s is about as good as we are ever likely to experience in Africa. Yet a 'participatory opinion survey' conducted in 1999 concluded that in all communities 'the poor were getting poorer'. Partly, the results tell of the severe limitations of this sort of focus group approach – it is easily hijacked by researcher bias. However, to the extent that this tells us anything beyond that radical social scientists will find what they want to find, it is that people find it difficult to recognize changes in their economic circumstances. Similar results have been found from surveys in Russia – even where people's objective economic circumstances have improved very substantially, they are inclined to describe their situation as having deteriorated. Underlying this apparent paradox are subtle issues of costly information. People learn about how local or national circumstances are changing not just from their own experience but from talking with other people. If everyone else is saying that things have got worse, then an individual might reasonably let this override the information from their own direct experience. Economists have studied such opinion formation under the theory of 'information cascades' (Bikhchandani, et al., 1998). The dismal conclusion of the theory of information cascades is that on many issues of public policy, if opinion formation starts off with the wrong belief, these wrong beliefs will simply be reinforced. Everyone will be voicing the same, wrong opinion, but this voicing will be treated as evidence by each individual, since they will assume that others know something that they do not know. These properties of information cascades make policy reform more difficult. A population does not start with neutral views as to the efficacy of different policies. The society inherits a set of policies and a discourse which provides some rationale for these policies. Thus, departures from these policies will be expected, under the inherited

discourse, to have adverse effects: if it were otherwise, the policy would already have been changed. The really pernicious aspect of information cascades is that they are able to defy individual experience. Thus, once a reform has been adopted, if the prevailing discourse has not changed, then even if the reform is objectively successful, it is likely to be judged to have been a failure. This came very close to happening in Uganda during the 1990s. The market liberalization which got underway in the early 1990s was in the face of the prevailing local discourse – reinforced by the prevailing discourse of the local donor community. Liberalization was expected by its many critics to lead to widening inequality, the rich getting richer at the expense of the poor. This is precisely what the focus group evidence found to be popular assessment of the reforms even after they had delivered spectacular poverty reduction. The focus group evidence, misleading as it was, came dangerously close to derailing the reform process in Uganda. What saved the day was the evidence from serious, large-scale, quantitative household surveys, undertaken every year during the 1990s. These showed the massive and widespread reduction in poverty and so enabled the reformers effectively to critique their critics. There is an important general lesson here – as part of the reform effort systems must be put in place which will provide accurate and timely feedback to the pertinent population as to what have been the consequences of the reforms. Data systems in the poorly performing low-income economies are usually awful. They cannot be comprehensively rectified in a realistic time frame and so data improvements must be coordinated with the identified narrow reform agenda. If the objective is to achieve three reforms, each of which is claimed to have a particular consequence for particular groups, these consequences become the outcomes which should be measured. The measurement needs to be quantitative – recall that we are definitely not seeking opinions. Surveys which simply feed opinions back to the population are actually exacerbating the problems of the information cascade. The results need to be fed back to the population in a timely and accessible fashion. This is a demanding task for a donor agency because it requires coordination between three distinct types of activity – policy advice, research, and public relations. The policy advice group has the responsibility for choosing the target reforms. The research group has the responsibility for monitoring the outcomes. The public relations group has the responsibility for feeding the results back. In practice these three



functions are almost never coordinated. Uganda during the 1990s is the only case I know of in which, more by luck than judgment, the three activities actually got coordinated.

Of course, if the reforms fail to deliver in objective terms, then the reform process will be severely damaged. This is why the zero generation reforms must be very carefully chosen so as to be relatively safe bets for a rapid pay-off. However, it also tells us that the reform process needs an irreducible amount of luck. If the economy is hit by adverse shocks shortly after a reform then, even though it is objectively true that things would have been even worse without the reforms, the population is unlikely to give the reformers the benefit of the doubt. Consider, for example, three big African reform efforts: Nigeria in 1986, Zambia in 1991, and Uganda in 1992. The Nigerian government launched reform as the oil price crashed. With the fall in the oil price living standards unavoidably fell – oil accounted for nearly half of national income. The popular discourse, which had in any case opposed liberalization, misinterpreted this fall in living standards as the results of ‘structural adjustment’. One of the reasons why reform in Nigeria is now so difficult is that the population thinks that it learnt from this experience that liberalization lowers living standards. The late 1980s in Nigeria were actually the only period of fast growth that Nigeria has experienced in the past thirty years, but this growth in output coincided with a decline in income due to the deterioration in the terms of trade. Nigeria had bad luck in its timing of reform. After years of mismanagement, Zambians elected a new government in 1991 which embarked on reform. At the time this was the most hopeful development on the continent. The Zambian government made the mistake of choosing the wrong sequence of reform – opening the capital account too soon – but I think what really killed the reform process was that the country was promptly hit by the worst drought in Southern Africa for half a century. Zambia was unlucky with the timing of reform. By contrast, in Uganda the reforms were promptly followed by a short but substantial coffee boom. The short term effects of reform were exaggerated by this boom. Uganda was lucky with its timing of reform.

So far I have sketched the first stage in promoting reform in those countries that have been mired in poor policies for a long time. The first stage has involved choosing a

limited agenda of 'zero generation' reforms, and putting in place data systems that can monitor the effects that the reforms claim to have should they actually take place. I now turn to the second stage which is the active promotion of this reform agenda.

The scope for donor influence on change is limited. As I have discussed above, I do not believe that aid-for-reform is a viable strategy, at least in these cases where poor policies are deeply entrenched. We should recognize that change is a domestic socio-political process. Interest groups shape policies and institutions in ways that they believe will serve those interests. Donors can intervene through two routes: by changing the beliefs of such groups about what policies and institutions best serve their interests, and by marginal changes in the balance of influence between groups. I take these in turn.

As I have discussed, information cascades lock populations into false beliefs about the effects of policies. Above, I was concerned that these beliefs should not contaminate the assessment of reforms once they had been adopted. Now I am concerned to shake these beliefs *ex ante*. I will introduce the problem by a simple and practical example. In both Nigeria and Uganda the largest value domestic currency note was worth less than one dollar. This inflicted real costs upon transactions – retailers had to buy note counting machines, and it was costly for the government – the cost of printing currency was very high. This state of affairs persisted because the small business community believed that printing larger denomination notes would be inflationary and so was strongly opposed to it. The origins of this false belief are quite evident – for years governments had indeed been printing money in an inflationary fashion and the visible indication of this money printing had been the introduction of higher denomination notes. I chose this example because it is a case where the belief is quite precise, and incontrovertibly wrong, and where it has quite clear costs. How can such a belief be broken? In Uganda the government went ahead regardless of opinion and introduced a new note with a value of around ten dollars. A local think tank did a survey of the opinions of small traders after the note had been introduced. The think tank publicized the results of their survey – unsurprisingly the results showed that traders opposed the new notes because they would be inflationary. This was precisely the wrong sort of think tank activity. In Nigeria an

economist with a local think tank published an analysis in a Nigerian business magazine showing that, controlling for the money supply, there was no link between the denominations of currency available, and inflation. Little-by-little, this sort of think tank activity may change opinion on this issue.

The broader lesson here is that most of these countries are extremely short of local analytic capacity allied to effective media communications. In these environments there are policies which persist not because they are massively to the advantage of vested interests, but because false beliefs are never challenged. Hence, one thing that donors can do to promote the reform agenda is to strengthen domestic capacity to analyze the proposed reforms. Depending upon the reforms, this may mean building university-based think tanks, or the analytic capacity of small business organizations or trades unions. Currently by far the best organized reform movement in Africa is the MDC in Zimbabwe. This is based on the trades union movement which, during the mid-1990s, developed a very effective in-house capacity for policy analysis. There are already major efforts to support African think tanks and upgrade university-based economists, most notably through the African Capacity Building Foundation and the AERC. However, these generic approaches need to be supplemented with support that is targeted to specific countries and specific reforms. Otherwise, the generic initiatives tend to help most those countries that are already relatively strong. For example, the AERC was recently unable to find a single Angolan economist able to participate in its research programs.

In addition to strengthening local analytic capacity, development agencies can target their own knowledge transfers more broadly on society rather than narrowly on the ministry of finance. This would require a somewhat different style of product, more accessible, and more based on examples from other countries rather than from first principles. However, such direct knowledge transfer has to be approached carefully. In many countries overt advocacy by IFIs or foreign agencies can be counter-productive. In the Nigerian reform effort of 1986 World Bank staff appeared on local television as advocates of new policies. This is very likely to be inappropriate. However, training local journalists in economic issues is appropriate.

I now turn from the strategy of puncturing false beliefs to the strategy of marginally changing the balance of influence between groups in society. In many societies some interests have very little influence relative to their latent support in society. The classic illustration of a group that is large but weak is that of smallholders. Being large, diffuse and poor, the group cannot overcome its collective action problem. Robert Bates pioneered the analysis of public policy in developing countries are reflecting the severely unbalanced power of different interest groups. However, the representation of interests is a public good the production of which is communication-intensive. Both of these features lead to the under-supply of representation. The standard problem of the supply of a public good is compounded by the fact that communication costs in poor countries are high. Thus, interests are usually far less represented than in developed countries.

This problem on the part of interest groups is compounded by the typical style of government. Colonial governments did not need to be responsive to interests and were not organized to be so. Post-colonial governments, with rare exceptions, continued this style of top-down management. Governments were astonishingly disconnected even from relatively well-organized pressure groups such as the urban business community. If government is unresponsive then this lowers the return to lobbying and so further reduces the organized representation of private interests.

The donor community can legitimately and effectively intervene to assist the representation of interests. A simple method is to cover the costs of twinning a local interest group with an equivalent organization in a developed country, or in a developing country where it has already proved effective.

A second method is to lower the costs of information to the group. For example, we can show that the delivery of basic services is better where there are more local-language newspapers. Newspapers are an obvious mechanism for scrutiny and complaint. A project which set up or reinforced local newspapers might have a more substantial impact on health outcomes than a project to finance health clinics. A now-famous experiment in Uganda provided information to local communities through posters and radio, about

financial releases from the Ministry of Finance intended to reach schools. Prior to the experiment only around 20% of the money released from the Ministry actually reached the schools. Once local communities were informed, the proportion leapt to 90%. This information project had a far greater impact on education than any number of school-building projects.

## **5. Providing basic services in hostile environments**

In Sections 2 and 3 I focused on aid in policy and institutional environments that made it atypically effective. In Section 4 I discussed how aid might be used to change the policy and institutional environment. I started from the position that our previous efforts to promote change had basically failed. This indicates partly that we were doing the wrong things, but also that change is difficult. We should not get carried away by high expectations of our ability to achieve major change, especially in the countries that are most in need of it.

I now turn to the thorny issue of what, if anything, can we do in the meantime, in those countries where policies and institutions are so poor that aid projects are likely to be ineffective and where our efforts at reform are likely to take a long time to pay-off.

Many of the countries in which policies and institutions are very poor have dreadful social outcomes. The question is not whether anything should be done to remedy these outcomes, but rather whether anything can be done. It is unlikely that conventional channels for aid delivery in these conditions will be effective for all the reasons discussed above. If health and education outcomes are to be significantly improved prior to the achievement of more widespread reform, innovative delivery mechanisms will need to be used.

I will now sketch what an organization would look like that would be capable of delivering improved health and education outcomes in a country where the government was either unable or unwilling to undertake the task itself, and where reform of the

government was not currently feasible. I will suggest six features that such an organization would need to have.

First, it would need to be autonomous from government with high standards of accountability to donors. Without such direct accountability, donors would not be able to fulfill their fiduciary responsibilities. Second, it would need to have an incentive system for staff which motivated them to achieve the objectives of the donor, precisely specified in terms of health and education outcomes. This would require not just a payment-by-results system, but a sufficiently reliable way of measuring results independently of the management of the agency. Thirdly, the organization would be a 'wholesaler' through which donors would channel their funds to 'retail' providers of services. Donors are not in a position to deal directly with retail providers on a sufficiently large scale and so such a wholesale-retail structure is necessary. There should be multiple types of retail provider, such as private providers, NGOs, religious organizations and even local government, but all should be properly monitored for cost-effectiveness. One of the functions of the wholesale organization would be to compare costs so as to gradually increase the cost-effectiveness of provision. A fourth feature, is that the organization would be contracting with retail providers for a flow of services rather than pump-priming a project. For example, a contract might be to provide so many student-days of primary school enrollment, but it would not be a contract to build a school. The organization would thus fund recurrent and capital costs as appropriate for services delivery, rather than simply capital costs. A fifth feature would be that the government should itself channel funds through the organization, ideally equivalent to its prior levels of spending on the services to be provided by the authority. Obviously, the rationale for this is to provide some defense against fungibility. Otherwise, the government can simply reduce its own provision. In effect, the authority would take over existing government provision. Finally, the organization would gradually come to serve as a channel for all donor funding to the country for basic health care and primary education, or whatever it was providing.

I term such organizations 'Independent Service Authorities' analogously to Independent Revenue Authorities. I think that the only reason that the latter exist whereas the former do not yet exist is that both the international community and the worst governments have agreed on prioritizing increased revenue over effective expenditure, albeit for different reasons. The international community prioritizes revenue because incremental revenue reduces inflation and the need for aid, as long as the government does not spend the revenue. Governments prioritize revenue because, for given levels of inflation and aid, they can increase spending on the things they most want – which are usually not improved health and education.

Nevertheless, ISAs are demanding and in most low-income countries they are inappropriately interventionist. For the typical low-income country, the donor task is simply to work with the government. However, some governments are not realistic partners and for countries with such governments a different approach is required. Among these governments, some would not agree to the creation of ISAs. I suspect that some would. During the 1980s the international community attempted a 'grand bargain' of aid in return for reform. This was the wrong bargain because it was not incentive-compatible. I think that over the next decade there is scope for a different 'grand bargain' of aid in return for improved health and education. Policy reform is not an incentive-compatible goal of aid for all the reasons I discussed above. Governments have too much scope to frustrate its objectives, and often they have strong incentives to do so. By contrast, through the vehicle of ISAs, donors do have the opportunity to make the goal of better social outcomes incentive-compatible. Donors would retain sufficient control of the detailed incentive structure for staff that reasonable performance could be secured. Admittedly, such an organization would work as an enclave. But that is the hard lesson we need to learn. In the worst environments systemic change is difficult and largely outside the control of donors. In the absence of systemic change in such environments we either leave populations to suffer, or we deliver basic services through enclaves. It may be that over time, an effective enclave has a demonstration effect that promotes systemic change. But that would take us into the realm of hope rather than experience.

## **6. Conclusion: what does it mean for donor agencies?**

One of the basic questions that institutional economics teaches us to ask is that if something new is such a good idea, why is it not being done already.

The organization must be presumed to be in an equilibrium, so that there are forces preventing change.

Above I have suggested quite a substantial agenda of currently neglected opportunities for aid agencies. If I am correct in thinking that aid could be made much smarter than it currently is, then in some sense, hopefully not too disrespectful, aid agencies are like the poor governments I have been considering – they are stuck with the wrong policies.

Potentially, we can use the same framework to think through what to do about this. We can use the Aid Effectiveness 101 message –take institutional competence and policies as given, and simply reallocate to those aid organizations that are most cost-effective in reducing poverty away from those that are least effective. I am happy to say that among the aid agencies, IDA has about the best record of cost-effective poverty reduction, much superior to the average bilateral donor. So, a clear implication of Aid Effectiveness 101, applied to inter-agency resource allocation, is transfer aid budgets towards IDA (with some honorable exceptions).

Then we turn to the messier business of effecting change within an agency. The analog of the demonstration project within an agency is, I suppose, piloting. This is the realistic approach to change – it is less threatening, and can gradually win people over. However, the same strictures apply as to a demonstration project – effective and convincing evaluation is the essence of a pilot project.

I will close by running through the changes I have suggested to see where the likely impediments to change within aid organizations may be. First, I suggested that – following the principles of Aid Effectiveness 101 – aid should be reallocated to those countries with high poverty and reasonable policies. This sounds to be so sensible that,



since it is not done, there must be very powerful and obvious forces against it. I have already mentioned political and commercial interests, but these are not internal to aid agencies. I suspect that within aid agencies a more important pressure is that for continuity. If an agency has staff specialized on water projects in the middle-east, or on education projects in central America, those staff will be lobbies for the continuation of such projects.

Second, I suggested targeting aid to countries hit by sudden large drops in their export prices. The bureaucratic impediment here is that this would require speed. At present, the only part of aid agencies that is designed for speed is that which provides emergency relief. Yet the response to economic shocks is a use not for relief aid but for development aid. Projects cannot be the vehicles for such swift-disbursing aid. To have a capacity for rapid response, aid agencies would need either to raid that part of their budget dedicated to emergency relief, or would need to radically change disbursement practices for some of their development assistance. Each option will meet bureaucratic resistance.

Third, I suggested targeting aid to post-conflict countries. This seems to me to be the most feasible of all the reforms I have suggested. The real challenge is overcoming the political pressures on allocation, rather than bureaucratic challenges within the aid organization. However, the parabola that aid volumes should ideally describe, may be administratively difficult. Nobody likes to see a program wound down.

Fourth, I suggested that aid should be targeted to those countries with reasonable economic policies and institutions, but which for other reasons faced an atypically high risk of civil conflict. I think that this poses considerable organizational difficulties because it cuts across diplomatic and developmental portfolios. The diplomatic perspective will be to provide aid for all those countries at high risk, regardless of their policies, while the developmental perspective will be to ignore conflict risk altogether because the agency does not have the appropriate information with which to assess it. Trying to blend these different perspectives is not organizationally easy.

Fifth, I suggested that project aid should be reshaped so as to be about delivering demonstration effects rather than investment per se. Organizationally, this is immensely difficult. It places huge extra tasks on aid evaluation, and it creates a premium on knowledge rather than on routine. It is analogous to switching from volume to customized production. The skill mix would need to change and this is one of the most difficult things to achieve in any organization.

Sixth, I suggested that aid agencies should attempt to promote change in difficult environments. Projects that work to improve information, or to build the capacity of groups that might be strategic in change, do not involve large amounts of money. They are basically knowledge strategies which require relatively little finance. However, they do require some money, and they would probably be intensive in staff time. This poses considerable incentive problems for an aid agency. It pushes the agency into lower disbursements per staff member, and on crude measures this will show up as a reduction in productivity. Further, knowledge to promote reform is intrinsically more difficult to measure than conventional projects. For a knowledge-for-reform strategy to be organizationally feasible it must be capable of evaluation. This in turn requires new approaches to aid evaluation. I suggest that a useful approach is to break up a strategy into its components according to the principles of a 'logical framework'. Activities are inputs into expected outcomes, conditional upon specified assumptions. These are in turn inputs into other outcomes. By this means it should be possible to assess a program of activities designed ultimately to promote a reform agenda, in terms of clearly measurable indicators. Once this is done, it becomes possible to reward staff for these activities in the same way as they are currently rewarded for investment projects.

Finally, I suggested that aid agencies have a role in basic service delivery in the most difficult environments. That role is not the glamorous role of direct contact with retail providers – the direct involvement with local communities. It is one stage upstream, in the construction and supervision of an effective wholesaling organization. This is less glamorous but offers a more realistic prospect of improvement of basic health and education outcomes on a substantial scale. The design of an effective ISA requires skills

in incentive-compatible contracting. In essence, an ISA would be a web of such contracts. It would be delivering a flow of services rather than administering a flow of projects. Again, this requires distinct skills.

So, the good news is that I have given you seven ways in which aid could be made smarter. The bad news is that, on the basic premises of institutional economics, you won't want to do any of them. However, I think that all aid agencies are now facing a crisis of confidence. Either we become demonstrably more effective, or we perish.

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## David A. Crocker on Paul Collier's "Making Aid Smart"

The topic of what makes aid more effective is enormously important. I am a social-political philosopher, and my students often say, "Enough about goals, ends, and basic objectives! Let's find out about making aid more effective." Paul Collier's paper is an important contribution to that investigation.

Although aid effectiveness is important, it is good to recall how difficult it is to separate means from ends. Therefore, though Collier's paper focuses on effective means, he does occasionally make use of a conception of appropriate ends, as when he discusses what counts as success and what aid ought to be doing. In fact, poverty reduction, conflict resolution, peace, and democratization are mentioned at several points in the paper as worthy and important goals. I want to underscore the importance of keeping the question "Effective for what?" before us at all times.

Collier is quite right to warn us against two defective approaches. The first is a narrow focus on aid projects, whose criteria of evaluation often ignore long term effects, behavioral change, fungibility, and sustainability. I would also stress here, regarding behavioral change, that we keep the *human development* part of development always in front of us: what we are finally interested in is people—individuals and groups—having a better opportunity to lead decent lives. I would like to push our speaker at this point for leaving the notion of poverty reduction unclear, despite accomplishing much else in a short paper. There has been much recent work by the World Bank and various development scholars on what we should count as poverty. Should we, for example, focus on income poverty, or should we additionally (or instead) focus on other kinds of poverty—the deprivation of health, longevity, security, political participation, and the linkages amongst these? Clarifying our notion of poverty and its multiple dimensions allows us to determine more easily how effective we are in alleviating it.

Collier also rejects an approach to institutional reform that attaches strings or conditions, although he might say more about whether some strings are less indefensible than others. One of his arguments against conditionality is that it undermines "ownership." I return to this idea presently.

Collier's paper does a good job in giving us a disaggregated view of the aid enterprise. Rather than just talking about aid as such, he disaggregates three very different types of countries:

(1) The first type of country, Costa Rica might be an example, is poor but has an effective policy environment. (2) A second type, for example, Honduras, despite currently having poor policy environments has the potential for institutional reform, (3) A third type of country, perhaps Guatemala, is both terribly poor and has a hostile and unpromising policy environment. In this third type aid not only fails to do any good, but may become

part of the problem if it is hijacked by a ruling elite. My own work, increasingly in the area of transitional justice, focuses on how a new democracy—fragile and incomplete—responds to prior violations of human rights. This question, like the question of aid, depends on disaggregating types of nation states. For reckoning appropriately with past wrongs depends significantly on what the transition is from, and what the transition is to. It is one thing to make a transition to Mandela’s South Africa and another thing for a post-conflict country to have a military man like Pinochet calling the shots behind the democratic scene. Unless we disaggregate different sorts of countries, we have an insufficiently focused approach.

With respect to type 1 countries, I endorse Collier’s notion of targeting aid based on poverty *benchmarks*. His suggestion here is that if a policy maker or bureaucrat wants to target aid away from those countries with the most severe poverty despite good institutional environments, then the burden of proof regarding the aid allocation should lie with that policy maker or bureaucrat. This suggestion seems exactly right to me: finally and importantly, donors should aim for poverty reduction, and those donors who have alternative goals (many of which are worthy) should also have the burden of proof to demonstrate the worthiness of those goals.

Other goals, like conflict resolution, however, call for a very different portfolio of interventions, many of which are outside the typical aid portfolio. For example, truth commissions, trial and punishment, and reparation are often useful in reducing current conflict, or preventing its reemergence. In fact, these tools may be more than just backward-looking tools, but can also serve as forward-looking tools to enable a people polarized and suffering from recent conflicts to deal with the past and move forward. In this regard, I fully support Collier’s recommendation that aid be sequenced over a 10 year period to post-conflict but promising countries and that aid seek a role in conflict reduction.

With respect to type 2 countries, Collier aptly doesn’t give up on aid for these countries with ineffective institutions. Rather he shifts the topic from aid allocation in a narrow sense to institutional reform – so that someday aid may be used well. This move is important because we know that there are a lot of deprived people who need assistance but who are living in environments that would be poor candidates for reform via foreign aid. This institutional focus means, however, that Collier must say more about the ideal of ownership. What is ownership? What does it mean? And why is it important? How can it be institutionalized? An underlying theme in Collier’s paper is that development capacity should be “indigenized” so that people can help themselves rather than being seen as, in Sen’s phrase, “passive recipients of the benefits of cunning development programs.” (*Development as Freedom*, p. 11). Governments and civil society groups, including their hybrids, offer venues in which a country (or groups within a country) may be involved in making decisions that affect their own outcomes. There are, of course, many ways in which this can be done. My own interest here is in a kind of ownership

that is a kind of deliberative democracy. This deliberative participation could occur in a parliament, but often in a repressive government—like Guatemala—this is not possible. Civil society and non-governmental organizations, however, may offer an appropriate venue. By deliberative democracy, I mean, that kind of interchange between fellow citizens—and sometimes outsiders—about the most effective means of arriving at the most basic ends that these citizens set for ourselves. One claim to test out empirically is whether the ownership in the sense of deliberative democracy reduces the corruption that vitiates so much development aid.

Collier also recommends that development donors in type 2 countries lower their expectations and emphasize (a) demonstration projects – instead of wholesale institutional change – from which lessons can be learned, and (b) qualified domestic actors. One part of qualification is the ability to think critically in the face of “information cascades.” Local think tanks and universities have a particular responsibility to promote, what Jonathan Glover calls, “a culture of criticism,” which, for example, can puncture the myth that growing relative poverty entails increased absolute poverty. A good example of such an institution is Honduras’s think tank and advocacy group the Citizens Forum (*Foro Ciudadano*). If sometimes a tension exists between domestic “ownership” and demythologizing (“these are *our* beliefs”), that tension can best be eased by ongoing and vigorous public discussion. Aid may promote some civil society groups in contrast to other such groups and a government captured by special interests.

(3) The last part of Collier’s paper considers the role of aid in very hostile environments with bleak prospects in the short and middle term. Again he reminds us that there are still human beings who are suffering in these countries, and this happens because of incredibly bad governments and weak economies. How do we understand the role of aid in this context. Here Collier introduces the notion of an “independent service authority.” What does he mean? A group set up to be a wholesale facilitator of groups that promote basic capabilities, such as good nutrition or basic education. What are the weaknesses and the promise? Here, I think of my recent work in Yugoslavia on the role of the Soros/Open Society Foundation in setting up (rather than selecting from among) local information gathering and advocacy groups. There is, of course, danger with these groups; namely, they are often identified with foreigners and accused of selling out. There is also the opposite danger—these groups sometimes simply capitulate to the extant power structures and do not serve as an independent voice. Collier’s notion of an enclave (should we say “engaged” enclave?) seems to offer an promising method of avoiding these dangers.

I would like to close with a final comment on the role of ethical commitment in achieving such morally urgent goals as poverty reduction. Ethical commitment can serve as part of the approach to reduce the dangers of corruption and promote the well-being of aid “recipients.” This ethical commitment can be exercised inter alia by an appropriate choice of colleagues. It also can serve as a kind of internal moral incentive that

supplements and corrects the incentive of self-interest donor and “recipient” alike. Collier’s perceptive paper is suffused with such commitments. He should express them more explicitly in his important search for ways to make aid more effective.



## Peter Murrell on Paul Collier's "Making Aid Smart"

In this paper, Paul Collier has used New Institutional Economics (NIE) in a persuasive and challenging manner to point out particular ways in which we can make aid smarter. As I was reading and thinking about this paper, the thing that struck me most as a New Institutional Economist was the extent to which the paper focuses on the recipient side of the equation. That is, the paper addresses the productivity of recipient countries' use of foreign aid, how hostile governments often misuse foreign aid—directing it away from basic services for their own ends—and should therefore be handled carefully, and how conditionality can lead recipient countries to lack ownership of reforms. While the paper does a good job in analyzing all these components of the recipient side of the foreign aid equation, it focuses less on the supply side of the equation—the donors. And, here is where I see possibilities for the paper to be strengthened.

For example, the paper does not address why projects and conditionality continue despite their well-known deficiencies; neither does it address why donors continue to direct aid to countries with weak policy environments; finally, it does not address donors' capacities to implement the range of policy suggestions laid out in this paper. The very last paragraph of Collier's paper does hint at these lacunae, saying "the good news is that I have given you seven ways in which aid could be made smarter. The bad news is that, on the basic premises of institutional economics, you won't want to do any of them." This warning, though, is more an afterthought than a central part of the paper's argument.

There may be good reasons why donors will not follow through on these suggestions. I explain three of these reasons, and their relationships with NIE, below.

- 1) The paper does not take sufficiently seriously the difficulties of imperfect information.
- 2) The paper does not examine in sufficient detail the complex forces that maintain institutional equilibria, even when those institutions fail to yield certain desiderata.
- 3) The paper fails to account for what Professor Williamson calls the "remediableness criterion", which measures inefficiency or institutional failure against feasible alternatives that can be implemented.

First, imperfect information:

The paper claims that it will be possible to double aid effectiveness by targeting the poorest countries, especially those with good policies. I agree entirely with Collier's claim on targeting the poorest countries, but I have some reservations about how we can feasibly target countries with good policies. There is no indication in the paper as to how we might go about determining, *ex ante*, the quality of the policy environment in a particular country. In fact, the paper admits that making this determination is particularly difficult: "most real world reforms have fairly low observability—their operation depends on behavior at the local level, or at the bowels of ministries over a long period." Thus, it is difficult to judge determinately, at any given time, the reasonableness of a country's policies. In some sense, the analysis of the paper sidesteps this issue by looking at a within-sample analysis of a set of countries, which are by nature backward-looking rather than prospective. Finally, it looks at *levels* of policy. However, if by looking only at levels of policy, it misses many of the subtleties that determine the effectiveness of aid.

To target aid at countries with good policies, we need to be able to make prospective judgments about what policy will be like in the upcoming years. In fact, one of the most important uses of aid is to help countries that have recently changed policies implement reforms in the new policy regime. This gives a hint as to why conditionality has become so important—for conditionality links aid to forward-looking steps about how policies should develop in the future.

In sum, I am skeptical about the ability of donor agencies to implement a non-arbitrary, forward-looking determination of the quality of the policy environment that would prove useful for targeting aid to those countries.

To illustrate, in 1991, we would not have provided aid to any of the transition countries, using then current policy environments as the basis of our allocations. We directed aid then only on the basis of what these countries were willing to promise—that is, on a conditionality basis. Furthermore, no one would have aided China in 1979 using this criterion.

Now let's take up the second point: institutional inertia with respect to conditionality in projects.

According to the paper, conditionality is hopelessly grandiose and projects have such limited perspectives. Yet both of these aspects of donor agencies have been around quite a while. Why? I would like to suggest that the organization of aid agencies themselves is largely responsible for these characteristics of aid delivery. By breaking up aid outputs into chunks that can be monitored, advertised, and evaluated, aid agencies can exert control over their investments, to varying degrees. Thus, we should expect conditionality and projects to continue until someone devises another method of controlling the investment of resources to aid. Obviously, given the record of conditionality, we would like a better technology of control. But the people within aid agencies themselves, who are responsible for running those agencies, must demonstrate to their own governments in some objective way the fruits of their labors. Thus, it is unlikely that this will change in the near future.

Finally, I will briefly address the third point: the remediableness criterion.

We know how projects work and how conditionality works. And we know, to various degrees of certainty, those circumstances in which these vehicles are feasible. While Collier's paper provides a set of methods that give us a good idea of how to improve the effectiveness of aid, until we place these methods in the context of a bureaucracy that makes up rules for delivering aid, these suggestions do not fully meet Williamson's remediableness criterion. They fail to do so because they suggest goals for the delivery of aid without suggesting any feasible way to implement those goals.

Collier's paper, "Making aid smarter" provides good suggestions on the recipient side of the equation, but once we think about the entire context of donor aid and bring in the supply side of the equation, some of these suggestions will prove infeasible. This observation is more a call to research than a blanket criticism of his paper, which makes

an important contribution to the literature. We now need to devise ways to incorporate Collier's insights into a body, like USAID, that can implement these suggestions. This will require institutional change in bureaucracies as much as in the recipient countries.