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SUBSIDIES AND TRADE BARRIERS
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Subsidies and Trade Barriers

Perspectives on reducing barriers to trade and migration: conditional gains and complex obstacles.

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1. Introduction:

Most people – or most economists at least – will agree that there are enormous potential gains from a better integrated world economy. International wage differentials of 100:1 are not compatible with economic efficiency, let alone equity. Rapidly growing exports have accompanied virtually all successful growth experiences. The analytical arguments for gains (static and dynamic) from trade are well established, and there are many empirical studies supporting the case. But even if we agree with these broad messages, the challenge of quantifying the gains from a change in trade or migration policy is difficult. There is no automatic policy lever that can trigger rapid export expansion, and trade liberalisation does not automatically foster growth. The mechanisms linking trade policy instruments to economic outcomes are highly conditional – under one set of other circumstances a trade policy change might have a transformative impact on economic performance, under another set of circumstances the same policy change might have a minimal effect. And just as the benefits of policy reform are hard to predict, so the obstacles to reform are hard to overcome. There are deep and complex reasons why migration and trade liberalisations are hard to implement.

Can these benefits and costs be quantified? It is certainly worth the effort, but this note argues for caution. Quantification that is too mechanical, that downplays real obstacles to change, that is too speculative (or alternatively offers a spurious degree of precision), may not be a useful approach for promoting economic reform. Asking a series of smaller questions – what reforms are feasible, which are most beneficial, how should they be prioritised – is more insightful than seeking to put a number on a policy change, the outcome of which is conditional on myriad other policy changes.

2. Reducing barriers to trade

The challenge paper covers both international trade and migration and I look first at the arguments to do with trade, and then discuss migration briefly in section 3. The challenge paper (Anderson and Winters 2008) provides a useful summary of the literature in these areas, but I focus in this note on the quantification exercises that it contains. Each of the main elements of quantification are discussed in turn; the

benefits of trade reform; the costs of trade reform; their combination in a present value calculation. The first task is of course to specify the trade policy change being considered. I follow the authors in focusing on the Doha Development Agenda (DDA), although return to discussion of some alternative policy measures in the conclusions (section 4).

2.1 Conditional benefits

The authors' approach involves three main steps. The first is to take a standard computable general equilibrium (CGE) model and use this to produce 'central' estimates of the comparative static effects of the DDA of 0.2% of GDP (p16). The second is to argue that different specifications both of the model and of the breadth of the policy change being investigated can produce results that vary by a factor of 9:1. They therefore keep the 'central' case as the basis for the reported 'lower net gains' and multiply by 5 to provide the basis for their 'high net gains' case. The third step (p17) is to assume that a one-off trade reform also has a long run growth effect, making world economic growth more than 0.5% per annum faster than it otherwise would have been in each year out to 2015, with this growth increment tailing off by 2025 in the low gain case or 2050 in the high gain case.

What numbers come out of this analysis? We can get a sense of the order of magnitudes by comparing the present value (at 6% discount rate) of the income gains from these alternative cases. For the 'lower net gain case' (which has CGE comparative static gains of 0.2% of GDP) the growth effects yield a present value of benefits which is around 25 times larger globally and 40 times larger for developing countries, than is the present value of benefits from the CGE alone. For the high net gains case they are multiplied by a further factor of 5. The CGE modelling effects are therefore just a tiny fraction of the aggregate gains presented.

What is good and what is bad about this process? Let us consider each of the steps in turn.

Starting with a CGE model provides a sound and micro-founded basis for analysis. CGE models allow detailed analysis of sectoral impacts of policy change, but their analysis is, at the simplest level, based on all sectors being perfectly competitive and operating under non-increasing returns to scale. Extended versions of such models contain simple forms of increasing returns to scale, often combined with product differentiation, thereby creating additional sources of gains from trade. In these models a policy change typically produces quite small changes in volumes of output in different sectors and countries, and there are real income gains (losses) where volumes increase in activities operating with price greater (less than) marginal cost due to imperfections such as trade barriers. This in turn yields small real income gains -- the central case DDA gains of 0.2% of world GDP, noted above, with a wide band of variation created by different model specifications.

There is general recognition that CGE models provide a very incomplete picture of the effects of trade. Essentially, they may be good for economies in which there is very little opportunity for productivity improvement, but they provide extremely poor guides to changes in economies (or sectors) where trade can have a 'transformative' and productivity enhancing effect. There is then a need to take into account the growth effects of trade, as the authors attempt to do. As noted above, the authors make assumptions about growth effects which end up yielding benefits which overwhelm the estimates from the CGE modelling; their dynamic effects are at least 25 times larger than those from the CGE model.

What do we know about the growth effects of trade? While this is not the place to undertake a full review of this hotly contested area of research, certain facts stand out. The first is that there is a strong association between trade and growth. For example, Hausmann, Pritchett and Rodrik (2005) identify 80 growth accelerations, and find that the export to GDP ratio is around 10 percentage points higher at the beginning of an acceleration than previously, and 15 percentage points higher 8 years into the acceleration. Jones and Olken (2007) identify 30 'up-breaks' in the growth of 125 countries over 40 years. Up-breaks are strongly associated with increased trade, and during an average up-break the share of exports in GDP increases by 12.2 percentage

points. Patillo and Gupta (2005) study 34 sub-Saharan African growth accelerations and find that export growth around 5-14% points higher during acceleration than otherwise.

Second, the causal relationships underlying this association are not well understood. I doubt that any researcher would make a claim like 'successful completion of the Doha round will cause my country to have a growth and trade acceleration'. It takes complex sets of reforms to bring about sustained growth, and to attribute an acceleration of growth of income or trade to a single policy lever is not correct. Even in the simple world of economic theory, we know that there are threshold effects involved in growing export sectors. Success depends on raising productivity, and there are increasing returns such that small initial differences between countries can translate into large differences in outcomes.

In view of this, how should we assess the authors' approach of assuming that growth is 0.5% higher in all countries for some number of years? I think we can say with some confidence that in countries lacking supporting policy measures the growth effect will be smaller. In countries that are enabled to grow new export sectors (such as labour intensive manufacturing) the growth effects may be many times larger. The point is that when circumstances are correct the growth effects of trade liberalisation can be hugely larger than those used by the authors, and when the economic environment is not supporting the effects can be absent. This conventional wisdom is perhaps summarised by World Bank (2005); '.. trade protection is not good for economic growth …. trade openness by itself is not sufficient for growth…'.

Is 0.5% extra growth for the next several decades a good average across these cases? I don't think that the paper provides evidence for this claim, this raising serious doubts about the usefulness of the numbers presented. Furthermore, since outcomes are conditional on other policy measures, it is not clear what other policies this averaging should be conditional on. Trade reform coupled with 'optimal' domestic circumstances and reform may have a transformative effect, bringing about Asian

growth rates, but such domestic policies and circumstances are not directly controllable.

The difficulty of modelling the dynamic and transformative effects of trade together with the conditional nature of the effect of trade policy, make for a challenging research agenda for trade researchers and for those, such as the Copenhagen consensus, who seek to quantify the relationship between policy actions and outcomes. This is not a call to abandon attempts to quantify the effects of trade reform. But it suggests that it may be better to break investigation up into detailed analyses of what we think actually happens, rather than undertake such highly conditional aggregate calculations.

2.3 Complex costs

The authors attribute two main sources of cost to removing trade barriers. One is the cost of international negotiation, and the other the adjustment costs faced by factors of production that become redundant in one sector and need to relocate to another. The authors admit to the difficulty of this task, and work with estimates based on the adjustment costs of sectoral trade reforms. They place these at 15% (high case) or 5% (low case) of the comparative static gains, lasting for just 8 years.

In my view the political economy of reform raises a wide range of issues that go beyond the simple costs of negotiation and adjustment. To see why, it is worth recalling one of the most basic (and the most widely cited) models of the political economy of reform, due to Fernandez and Rodrik (1991). Following reform, one sector of the economy will expand and its workers will gain. The other sector contracts, and some of its workers will lose (they will find it hard to be re-employed) while others will gain (they get re-employed in the expanding sector). Before the reform, workers in the contracting sector do not know whether they will be re-employed or not, so their expectation (knowing the probabilities of re-employment) is a loss. It is then easy to find an example in which reform is blocked (eg with majority voting, the contracting sector employs more workers than the expanding one), even though there would be aggregate gains from the reform and, *ex post*, a majority of

workers would gain. Can the losers be offered compensation, so that their opposition to reform would be bought off? In this example there are enough resources for compensation to be paid and a Pareto improvement to be made, but there is also a fundamental problem. Once the trade reform has happened and some of the workers have been re-employed (and the uncertainty about who gets re-employed has been resolved), those remaining in the contracting sector may now be a minority of the population. It will not be in the interest of the majority to pay whatever compensation was previously offered. Whereas the *ex ante* median voter was in the contracting sector, *ex post* the median voter is in the sector that expands. In short, the offer of compensation was incredible – and workers, knowing this, oppose the original reform. Society has no credible way of committing to compensate the losers.

This is just one argument of many that can be made about the political economy of reform, but it illustrates clearly that simply saying compensation is cheap, fails to capture the obstacles to reform. Without suitable commitment mechanisms government may simply find it impossible to implement reform. Any government seeking to implement a reform package will come under pressure from lobby groups, in particular those who see the reform as undermining their influence or their well-being at future stages of the reform process. Political capital has to be expended in a reform process, and we should be asking, what is the opportunity cost of this political capital? If quantification is the imperative, then the authors should be looking at the shadow price of reform and of political capital, not simply assuming that the nominal cost is equal to the full opportunity cost. In short, there are quite deep reasons why reforms have not already been undertaken, and to simply ignore these is not very helpful.

2.4 Present value calculations.

Combining costs and benefits, the authors find that the effect of the DDA are to cause income in 2098 to be around 10% greater (in the high case) than it otherwise would have been. The associated net present values range from $50 trillion (low case, 6% discount rate) to $424 trillion (high case, 3% discount rate), ie from somewhat larger

than current annual world GDP to approximately ten times current GDP. Global benefit cost ratios range from 269:1 to 1121:1.

Two brief observations are worth making. One is a reminder that gains grow with GDP, assumed to be growing at least 3.5% pa. Discounting at 3% therefore places us in the looking glass world where gains would be unbounded but for the assumed fixed time horizon, and where postponing reform (and shifting the time horizon accordingly) raises the present value of net benefits. Second, the counterfactual to the policy change being considered is that the DDA, or something like it, will not happen in the next 100 years. Put at its simplest, this says that we are evaluating DDA now or never. I do not find this a very helpful thought experiment. It would be better to allow for a probability that trade reform will occur at some date, perhaps by working with a higher discount rate.

Both these observations point to the fact that it would be much better to express net gains as an annual flow, telling us what we are foregoing because a trade reform has not yet happened, than it is to compute a present value and associated benefit-cost ratio.

3. International migration

The migration experiment is probably on more secure ground in its estimate of economic benefits, if not of costs. The experiment is well defined, and draws on a recent World Bank Study (World Bank 2006). 14.2 million additional workers, plus their families, move from developing to high income countries over a given time period. The gains are estimated at an annual flow of $674 billion, or $48,000 per worker. This is a comparative static gain, essentially coming from the difference in the marginal value product of workers before and after migration. Some fuller discussion of assumptions underlying this figure would have been helpful (what is assumed about cost of living differences, skill levels, training incentives, future convergence or divergence of international wage differences and so on). The estimate nevertheless provides a good reflection of the massive inefficiency burden imposed on the world economy by immobility of labour.

The cost side is vulnerable to criticisms along the same lines as those made above for the authors' handling of trade liberalisation. The costs on which the authors focus are simply those of administration, travel, and some labour market adjustment. Once again, this underestimates – or ignores – the social and political economy costs of migration. These may be difficult to quantify, and may be politically sensitive. But to ignore them is surely wrong.

4. Feasible reform alternatives; a targeted challenge.

I am sceptical about the value of the exercise undertaken in this paper – while at the same time thinking that expanded trade would bring very large gains for many countries, and also believing in the value of quantification.

Advocacy carries several dangers – such as the 'lets use the largest numbers that seem plausible' syndrome. The fact is that the actual gains from economic growth associated with exporting which have occurred in some countries would have seemed, ex ante, to be implausibly large. But, as we have argued, they are conditional gains and have only been achieved after surmounting complex obstacles.

It would be more insightful – and perhaps also more influential – to focus the challenge on particular sets of actions that would enable countries to participate more effectively in the world trading system. A liberal world trading environment is one aspect of this, but so too are measures such as aid for trade, domestic reform agendas, or policies that enable trade reform to move forwards. Regional integration is also important. It is dismissed far too quickly in the present paper, largely on the basis of calculations from CGE models which, according to the authors' own method, are a guide to less than 5% of the gains from trade liberalisation. The same is true of the treatment of trade preferences, for developing countries, where prospects for transformative export growth and associated productivity improvements and changing comparative advantage are simply ignored.

In conclusion, study of a well-defined problem – for example, how to formulate policy and quantify the gains of diversifying Africa's exports – would be a more targeted and useful challenge.

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