perspective paper

CHRONIC DISEASE

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Perspectives paper on "Chronic disease prevention and control" by Jha et al

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The Challenge Paper by Jha et al "Chronic disease prevention and control" provides a comprehensive perspective on a range of relevant health and economic issues around chronic diseases in developing countries. In particular, the authors propose the following five interventions, due to their highly favourable Benefit-Cost ratios: 1) tobacco taxation, 2) acute management of heart attacks with low-cost drugs, 3) Heart disease, strokes: salt reduction, 4) Hepatitis B immunization, and 5) secondary prevention of heart attacks and strokes with 3-4 drugs in a "generic risk pill".

In my review I focus on three principal issues that may affect either the basic rationale for intervention and/or the size of the benefit-cost ratios.

(1) Value of a statistical life year

Jha et al use the VSL concept to value the benefits of their proposed interventions. Given the persistent lack of VSL estimates in developing countries, this does require strong assumptions. The authors acknowledge and take account of the uncertainty around what could be the true VSL in low income countries by using a very wide range of VSL estimates. In the VSL estimates used for the calculation of the economic burden of chronic disease (section 1.3.3), the highest VSL estimate (and hence the overall cost estimates) is 6.3 times the value of the lowest VSL estimate, while in the VSL estimates used in the benefit valuation of the intervention this difference is five-fold.

The authors' strategy to foresee such wide intervals is to be recommended. Yet the question arises whether it is possible to narrow down the VSL estimates to what might be the most likely "true" value? If that was possible, the overall BC ratios of the recommended interventions might be easier to compare to BC ratios in the other policy areas covered by the Copenhagen Consensus.

A common way of "transferring" VSL estimates from countries that have VSL estimates to those (typically low income ones) that do not is by using the income elasticities of VSL coming from meta-analytical studies. This is also the approach used by Jha et al for part of their estimates, at least when calculating the economic burden of chronic disease. In particular, they use the parameter estimates (associated with GDP per capita and life expectancy) from the landmark review and meta-analysis of VSL studies by Viscusi/Aldy (2003), who had included about 50 studies from 10 countries, all but one of which from high income countries. While there exists no perfect way of transferring VSL figures, this is arguably a less ad hoc approach than the rule of thumb employed by the Commission on Macroeconomics & Health (CMH), i.e. that of using a value that is equal to 1-3 times the value of the

country's per capita income. Hence it should in principle deliver more reliable VSL estimates, if and only if the assumed income elasticity is the correct one.

Jha et al do not give the VSL estimate they obtain from applying this procedure but since the income elasticity in the Viscusi/Aldy study ranges from 0.46-0.60 - meaning that as per capita national income grows by one percent, the VSL will grow by about half that percentage - one can roughly estimate the VSL for an average low income country by applying the simplified[1] formula:

$$VSL_b = VSL_a \times (per\_capita\_income_b / per\_capita\_income_a)^{elasticity}$$

Here *a* denotes the country for which a VSL estimate does exist (eg the US) and *b* stands for the country for which a VSL is to be estimated.

Using a VSL for the US of USD 6.3 million (which is the VSL that Robinson (2008) suggests for the US in 2007), the 2007 value of national GNI per capita of $46,900 (according to World Bank Development Indicators (2012)), an income elasticity of 0.5, and an average per capita GNI of a low income country in the same year ($395,-), this gives a VSL for the low income country of $578,166. To compare this figure to the estimates of the CMH approach (which are given in terms of the value of a statistical life *year*[VSLY]), we need to transform this VSL figure into the corresponding figure for a life year. Again, using simplifying assumptions this results in a VSLY of $27,074 for the low income country - and $295,012 for the US.

Comparing these figures to the national per capita GNI gives some idea of its dimension: for a low income country, the VSLY is 68 times bigger than its GNI per capita, while for the US the difference is six-fold. It may be unrealistic to assume that the average individual in low income countries would indeed be willing to dedicate such a large amount for a relatively limited mortality risk reduction, not least due to the need to fund basic necessities (Hammitt & Robinson 2011).

Obviously, the relatively large VSLY in the low income country (as a share of its GNI per capita) is driven by the low income elasticity found in the Viscusi/Aldy study. The lower (higher) the income elasticity, the higher (lower) will be the resulting estimated VSL in a low income country, derived from that elasticity.

While the debate is far from settled, there may reason to believe that the income elasticity of VSL is closer to one or maybe even higher. Table 1 gives a (non-exhaustive) overview of comparable meta-analytical studies that have come up with estimates of VSL income elasticities.

Table 1: Other meta-analysis findings on income elasticity of VSL

Study	Income elasticity of VSL
Mrozek and Taylor (2002)	0.46–0.49
Liu et al. (1997)	0.53
De Blaeij et al. (2003)	0.5
Miller (2000)	0.85–1.00
Bellavance et al (2009)	0.84–1.08
Bowland and Beghin (2001)	1.7–2.3

[1] The key critical assumption is that elasticity is constant over the income levels of concern.

Without entering into the details of the differences between the studies, it is of note that at least three of the six studies have produced higher income elasticity estimates, and only one has produced a lower estimate.

The possibility that income elasticity may be closer to or even above one is further strengthened by evidence from longitudinal studies: e.g. Hammitt, Liu, and Liu (2000) estimate the relationship between wages and job-related risks for each year from 1982 to 1997 in Taiwan in times of fast economic growth, and they find income elasticities ranging from 2.0 to 3.0. Costa and Kahn (2004) consider changes in the wage-risk relationship between 1940 and 1980 in the U.S., finding elasticity estimates in the area of 1.5 to 1.7.

We may thus tentatively assume that the Viscusi/Aldy income elasticity could be too low, and we might therefore be able to exclude some of the higher ranges of the VSL estimates. This applies to both the VSL estimates in the economic burden section and in the Benefit-Cost estimates, where the upper boundary VSLY estimate ($5,000) corresponded to a more than 11 times higher figure than national per capita GNP in low income countries.

A more realistic assumption might be that of an income elasticity closer to 1 or beyond, though of course there is no way of being certain about this. By way of illustration, what would be the effect of, say, an income elasticity of 1 or even 1.2 on the VSLY estimates in low income countries? This would produce a VSLY of $2,485 for a unitary elasticity and VSLY of $956 for an income elasticity of 1.2, corresponding to, respectively, 6.3 times and 2.6 times the GNI per capita. Hence, if income elasticity was about 1.2 or higher, then even the lower boundary of the VSLY used by Jha et al would be too high an estimate.

A further issue relates to the within-country distribution of VSL - a consideration not explicitly taken into account in the paper. If VSL varies with income across countries, it will also vary with personal incomes within countries. If it is the case that the beneficiaries of the selected interventions are the less wealthy members of society, who likely have a lower willingness to pay to (marginally) reduce mortality risks, and hence lower VSLs, then this would tend to reduce the expected benefit of the intervention, compared to a scenario in which the population on average would benefit.[2] The section on “Chronic disease s and poverty” (2.1) appears to suggest that chronic diseases may well be to a large extent a problem of the poor within developing countries, and that hence the benefits might well occur primarily among the lower socioeconomic groups. The truth is, however, probably more nuanced than this, not least because we are dealing with various and very different chronic disease conditions. For instance, it is fairly uncontroversial to assume that tobacco consumption is disproportionately a problem of the poor within low income countries (as opposed to the better off in those countries). Taken together with the observation that smokers in lower socioeconomic groups are more responsive to tobacco price increases, one could expect that the health benefits from the proposed tobacco taxation are primarily incurred by the poor within countries, and hence that the VSL might need to be corrected downwards.

[2] This assumption could be seen as implicit in the use of an average VSL for the country as a whole.

By contrast, the distribution of other risk factors, eg obesity, is decidedly different from smoking, in that the shift of the obesity burden from the rich to the poor (within countries) has not yet progressed to the extent we see in the case of smoking - certainly not among men at least (Monteiro et al 2004; Dinsa et al 2012). If this is an indication of the socioeconomic distribution of heart attacks and stroke, this would suggest that the parts of the population that stand to benefit from the recommended drug treatment and prevention are either of an average or of a higher level of economic wealth. As a result, using an average VSL for one country may underestimate the benefit of the intervention in this case.

Some discussion of the potential variation in the likely beneficiaries by socioeconomic status across the different interventions may have been worthwhile, even though there is preciously little hard evidence on the distributional effects of the interventions, especially so in low income countries.

(2) The (non-)consideration of the economic burden estimates in the Benefit-Cost analysis

While part of the above comments suggest that the higher boundaries of the VSL estimates (and hence of the Benefit estimates associated with the interventions) may indeed be too large, there are also reasons to think that the benefit provided by Jha et al could be underestimates. This is because while the authors have discussed at some length the existing economic costs associated with chronic diseases (esp cost of illness [COI] and value of lost output), when Jha et al turned to estimating the benefits of the interventions, none of the previous "economic burden" estimates are factored into their benefit-cost ratios. The authors present extensive data on the costs of chronic diseases, using the COI approach, and based on the impact of health on growth. To the extent that the authors are confident about these findings, one might have expected them to reflect at least part of those in the BC-estimates.

That said, personally I would have some concerns about part of the economic burden results presented in section 1.3. In particular the value of lost output approach rests on WHO's EPIC model, which in turn rests on the assumption that there is a harmful effect of health on economic growth, and in particular a harmful effect of chronic disease mortality on growth. These two critical assumptions might deserve some discussion in light of the influential Acemoglu/Johnson (2007) finding that improvements in life expectancy mainly trigger faster population growth, but have a negative causal effect on income per capita, at least over an adjustment period (which can last several decades). They regress income growth on the increase in life expectancy between 1940 and 1980, and instrument for the growth in life expectancy by exploiting the wave of health innovations that occurred as of the 1950s and affected all countries worldwide: they use the pre-intervention distribution of mortality from 15 diseases and the dates of global interventions to construct a country-varying instrument for life expectancy.

These findings appear to have spurred an interest among economists to re-consider the relationship between health and growth. Some recent studies have indeed found opposing results to Acemoglu & Johnson, see e.g. Aghion et al (2011) and Cervellati & Sunde (2011).[3] Most of the existing studies have, however, not paid specific attention to the effect of growth on chronic diseases. It may well be that the effect of chronic diseases on growth is different from that of infectious diseases or

[3] See also Lorentzen et al (2008) for a careful study finding growth enhancing effects of adult mortality reductions.

malnutrition. After all, they tend to affect very different segments of the population - i.e. chiefly elderly people in the case of chronic diseases. Depending on the demographic profile of the population, if the reduction of chronic diseases as a result of the proposed interventions occurs only among those already near or beyond retirement age, then it is conceivable that the economic growth impact will be minimal and may even be negative. On the other hand, reductions in chronic disease risk may have less of a population-augmenting effect compared to infectious disease risk reductions. (The population enhancing effect of health improvements was the main driver of the negative growth effects of life expectancy improvements in the Acemoglu & Johnson study.)

To the best of our knowledge only one study has looked specifically at the growth impact of chronic diseases (Suhrcke & Urban 2010), finding that reductions in cardiovascular disease mortality in adults can significantly reduce subsequent per capita income growth. However, this finding applied only to the high income countries, not to the low and middle income group. (This may however also be explained by data deficits in these countries.)

The mixed evidence on the macroeconomic growth impact is in contrast to the fairly reliable impact of health (including chronic disease) on individual level economic outcomes, such as labour market earnings, labour market participation etc (Suhrcke et al 2006). Jha et al have however not considered the latter aspect. The microeconomic effect might have been a useful additional input into the Benefit-Cost analysis, even though there are no consensus estimates on the magnitude of such effects.

On balance, and given the substantial variation and challenges in the current economic cost evidence - and those challenges might be larger than Jha et al have acknowledged - it is probably understandable that Jha et al decided not to include the COI and value of lost output estimates into their benefit-cost estimates.

(3) Economic rationales for public policy investment

In the context of tobacco taxation, there is some discussion of what the market failures might be that could justify public policy intervention in this particular area. Jha et al mention two information imperfections (about health risks and about the consequences of addictive properties of smoking) and one on externalities. While the latter market failure could justify tobacco taxation as a direct response, tobacco taxation would not - from an economic perspective - be the first response to the presence of information imperfections. The first recommendation, if those were the main market failures, would be to run information campaigns[4].

Further economic rationales could be brought to bear in the context of tobacco, i.e. the existence of less than perfectly rational decisions made by individuals trading off the long term "pleasure" of consumption now with the adverse health effects a (too long) time in the future - the idea of time-inconsistent preferences. If this was an empirically proven point, then there might be a rationale for the state to help the individuals make "better" decisions - better in the sense of "in people's long term self-interest" (which is seen in conflict with the immediate self-interest). It is, however,

[4] The evidence for information campaigns to be effective in changing health behaviours in general and in tobacco in particular is very poor.

important to bear in mind that the welfare economic implications of behavioural economics are complex, and public policy intervention cannot always unambiguously be inferred (Sugden 2009).

There is no discussion in the paper on what the market failures might be underlying the other interventions. The rationale for salt regulation may not be too dissimilar from tobacco, though arguably the addictive properties of the latter are far greater than of the former. Consumers have little choice in the amount of sodium they consume every day, as most of it is already inside the food products purchased and/or consumed. It may also be difficult to understand how much sodium is in a given food. The latter may represent an imperfect information the direct response to which would information campaigns (though this would likely be an ineffective response). Excessive salt consumption may also exert external costs if the treatment costs for heart diseases et al that result are being co-financed by others in society (if there is collective insurance financing).

The recommended intervention no. 2 - acute management of heart attacks with low cost drugs - is estimated to provided a BC ratio of 25:1 - which is a very high return on investment, which makes one wonder, why the individual does not pay him-/herself to fund an intervention that is low cost and entails such huge benefits. It could be a problem of poverty - i.e. the low cost of the drug are not low enough for them to be truly affordable. Or the cash needed to pay is not available to the individual and capital markets do not function enough to allow for borrowing. In any case, some discussion of the potential market failure and how it relates or not to the recommended intervention would be of use.

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