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Pro-poor Growth and Pro-growth Poverty Reduction:

What Do they Mean?

What Does the Evidence Mean?

What Can Policymakers Do?

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I. So Much Effort, So Much Ingenuity, So Little Policy Consensus

Many recent papers have explored the links between growth, equality, and poverty reduction. Several ask, Is growth usually or always good for the poor? Is *more* growth usually or always better for the poor than *less* growth? Does growth tend to increase inequality? How does inequality affect the impact of growth on poverty? Such enquiries have produced quite powerful findings, some reviewed below. It has been confirmed beyond reasonable doubt that poverty decline tends, on average, to be faster in times and places of fast, prolonged growth than alongside slow growth, let alone stagnation or decline; and that there is no general or universal tendency for growth, as such, to make income distribution either less or more equal. Beyond this, unfortunately, the aggregate findings are often conflicting, and seldom as useful to policymakers as one might reasonably hope and expect.

This paper first explores *why* the “pro-poor growth” literature, despite being technically sophisticated (indeed, though mostly written carefully and well, in large part too difficult for many economists!), has not proved either very conclusive or very useful to policymakers. Second, it tries to identify research that *does*, or might, address the questions with which policymakers are, or at least should be, concerned.

Section II suggests that the aggregate pro-poor growth debates would be less inconclusive (and would tell policymakers more) if they became less reliant upon:

- (i) Conflicting, sometimes misleading measurements of poverty, growth, and inequality;
- (ii) Different and unreconciled data sources, e.g., household surveys and national accounts;
- (iii) Conflicting evidence on “levels” and “trends” links between growth and poverty;
- (iv) Time-series based on two to three income-distribution data points (often only 1-4 years apart) in each of a few developing countries, plus often doubtful interpolations and extrapolations;
- (v) Numerous growth spells for a single country, India, and a few for People’s Republic of China (PRC)—the two countries together contain over half the world’s poor;

- (vi) Concentration on growth-poverty relationships across the entire sample—often a sample including developed, transitional, and developing countries—rather than on for particular types of country, or of policy régimes;
- (vii) Evidence or reasoning that has not been “filtered” to check whether it is about pro-poor growth (whether, and to what extent, growth reduces poverty); pro-growth poverty reduction (whether, and to what extent, low-end income equalization and/or poverty reduction alters the rate of growth); pervasive policy side effects, of policy aimed at growth/poverty on poverty/growth; and definitional or arithmetical linkages between (a) mean income or consumption, inequality, and poverty or (b) growth, inequality change, and poverty change.

Section III suggests that, by building on available approaches toward a less aggregated, more micro-based and causally structured pro-poor growth literature, economists will be more useful to policymakers. Their main concerns with this literature should be, How does it help us to improve the links among types of policy and hence, in particular types of conditions and/or countries, to choose policy mixes that speed up growth and poverty reduction (both properly defined)? It is probable, though not certain, that this means selecting policies that improve the responsiveness of each goal to changes in the other goal. It is certain that policy selection involves identifying particular paths via which changes in growth rates affect changes in rates of poverty reduction, and vice versa. In low-income areas, where poverty is most heavily concentrated, there is some tentative evidence that a given rate of economic medium-term growth reduces poverty most if focused on (i) increasing staple food production and farm employment, and (ii) providing incentives and enabling devices that reduce fertility, especially among the poor. Conversely, a given rate of poverty reduction via low-end inequality reduction is probably most pro-growth if it focuses on redistributing land and educational assets, and, more generally, if it targets ascribed inequality more than (but not ignoring) achieved inequality.

II. Pro-poor Growth: What Do the Data Tell Us?

A. Measuring Poverty, Economic Growth, and Inequality

Analysts who appear to reach different conclusions about how pro-poor growth is, or the effect of distribution upon this, may do so because they use different indicators of poverty, growth or distribution.

(i) Poverty

- (a) *Incidence/intensity/severity, sharpness.* de Janvry and Sadoulet (JS) [19xx] and many others estimate the impact of growth on poverty incidence only. This does not allow for depth of poverty, nor distribution below the poverty line.
- (b) *Dollar or national poverty lines, or relative poverty?* PPP dollar poverty is used by Ravallion and others. National poverty lines are used in JS, and may make it hard to interpret cross-national regressions where “national poverty incidence” is the dependent variable, if “poverty” refers to very different command over resources in different countries. Relative poverty measures the “lowness” of lower-group income or consumption relative to a national mean or median. The HIID analyses (and Dollar and Kraay [DK]) take off from this definition, and in effect define “poverty” as “low share of income of the worst-off 20 percent in any given country”. The incidence of “HIID-poverty” can never vary (it is always 20 percent), though of course its intensity and severity can change or vary, and it is varying/changing “intensity of HIID-poverty” that, in effect, Timmer-GRW and DK seek to explain. In regressing growth of income in the lowest quintile upon mean growth, the HIID team concentrate entirely on intensity (depth times incidence).
- (c) *Consumption or income?* There are familiar reasons to prefer a consumption-based poverty line, yet most of the “connectedness” literature claims to use income. Dollar and Kraay use the Zhou-Deininger-Squire correction (reported in the Lundberg-Squire 1999 expanded data set).
- (d) *Per person or equivalent adult, or per household:* Per-household income or consumption estimates tell us little or nothing about per-person welfare or poverty. It is

not clear in some data sets, especially for Latin America, whether per-household data are used.

**B. Unreconciled Data Sources: Household Surveys, National Accounts,
Penn; Consumption, GDP**

The work of Ravallion and Datt on India usually estimates mean consumption and its growth, as well as poverty and distribution, from household surveys (the Indian National Sample Survey), and this team often uses similar methods in cross-country comparisons (but adjusting from national to PPP dollar poverty lines). This avoids major problems when household surveys are mixed with other sources. However, what most people want to know is how different rates, paths, or policies or external events affecting national growth—presumably of GDP per person—affect poverty, and how distribution plays a role in this. Therefore a standard procedure in assessing the impact of growth on poverty—and distribution upon that sequence—is to derive estimates of poverty and distribution from household surveys of consumption or disposable income, and growth, usually of GDP, from national accounts. This procedure is used, for instance, in JS. Often (though not in the last citation), Penn World tables are used to convert, into constant purchasing-power-parity command over a global mean consumption bundle, country national-accounts GDP (PPP national accounts) and/or country household-survey measures of poverty (PPP or dollar poverty).¹

However, (a) GDP exceeds personal disposable income which in turn exceeds personal consumption, (b) GDP distribution is not well-defined and personal disposable income distribution is much more unequal than consumption distribution, and (c) typically, survey consumption and income are respectively 15-40 percent and 20-50 percent below national accounts consumption and distribution (at least in developing countries) because the richest seldom take part in household surveys. This does not impede surveys from measuring absolute poverty, but does seriously harm, maybe invalidate, survey measures of distribution, relative poverty, and income or consumption share (or growth) of the poorest quintile, because of unknown but big underestimates of top-quintile income

1. Since PPP estimates exist only for national accounts totals (or per-person means), the latter conversion involves assuming that the purchasing power of a 1985 dollar in India over the poor's consumption bundle, and over the average consumption bundle, have to be increased by the same proportion, to permit comparisons of PPP dollar poverty with (say) Europe. There are of course no PPP estimates of distribution; and even stronger assumptions are implicit in using Ginis that ignore the differences among quantiles in the extent to which their true PPP is misrepresented by official exchange rates.

and consumption. These are probably highly variable over time: e.g., in India the gap between mean consumption estimates from the National Sample Survey and the national accounts increased sharply in the 1990s.

Some studies (e.g., HIID, DK) use PPP income and growth data but do not correct household distributions for PPP differences among quintiles because no conversion data are available. This could make a big difference in inequality measures (presumably reducing them for low-income countries).

C. Evidence on “Elasticity of Connection” between Growth and Poverty:

Cross-section and Time-series

Increased availability of internationally comparable data on GDP/capita (Penn World Tables) and the distribution of income (Deininger/Squire) has led to considerable research on what Timmer calls the *elasticity of connection* linking the income of the poor to national mean income per capita. The principal papers are DK and three emanating from HIID: Gugerty and Timmer (1999) or GT, Gallup, Radelet, and Warner (1998) or GRW; and Timmer (1997) or TI. For reasons of data availability, the poor in each of these papers are defined as those in the bottom quintile of the income/consumption distribution as measured through household surveys judged of adequate quality according to the criteria of Deininger and Squire. Similarly, the distinction between GDP/head and mean household income or expenditure is ignored, so that, for instance, bottom quintile mean income is estimated directly from the income/consumption share of the bottom quintile and the PWT estimate of GDP/capita. Note that serious biases could arise from nonrandom variations over time in the distribution, across quintiles, of benefits from nonconsumption GDP.

The starting point for all of the studies is an equation of the following form, or some variant of it:

$$\ln y_{1it} = a + b \ln y_{it} + m_i + d_t D_t + e_{it} \quad (1)$$

Here i and t refer to countries and years, $\ln y_1$ is the natural log of bottom quartile mean income, $\ln y$ is the natural log of national mean income, m is a country fixed effect, The D_t are time dummies; a , b , and the d_t are coefficients to be estimated, and b is the elasticity of connection. If b equals one, then, other things equal, a 1 percent rise in national income per head is causing a 1 percent increase in bottom-quintile income per head. e_{it} is an error term, the assumed properties of which vary among the studies.

Equation (1) is a “levels” equation, a “growth” equation can readily be derived from it by differencing. Viz:

$$ly_{1it} - ly_{1it^*} = b(ly_{it} - ly_{it^*}) + (d_t D_t - d_t^* D_t^*) + (e_{it} - e_{it^*}) \quad (2)$$

(i) Empirical strategy

In using such equations to arrive at an estimate of the elasticity of connection, b , a number of issues present themselves, viz:

(a) Levels equation or growth equation?

Whether estimation of the elasticity of connection should be based on the levels equation or the growth equation is not a straightforward matter, because of a number of considerations:

1. Assuming exogenous regressors and identically independently distributed e_{it} , OLS estimation of the levels equation with country dummies would be preferable to OLS on the growth equation, since while an unbiased estimate of b could be obtained from either equation, the evident serial correlation of the error term in (2) means that OLS estimates will be inefficient in this case.
2. If at the other extreme e_{it} is a random walk, rather than i.i.d, then the growth specification is to be preferred as the differencing produces an i.i.d error term in (2).
3. If the variables in the model are measured with serially independent errors, then differencing is likely to exacerbate the problem: the information contained in the differenced variables may be low.

We find in the literature a varied response to these difficulties. TI chooses the levels equation with country and (sometimes) decadal dummies on the basis of an ingenious example. He considers a country in which y has a constant positive trend and y_1 has a zero trend, so that “the poor are entirely disconnected from the long-run growth process” (TI, p6). Both y and y_1 are supposed to be equally affected by the same random shocks, from weather for instance. It follows that a levels regression will produce an estimate of 0 for b , while a growth regression will produce an estimate of +1. GRW choose in the main to rely on a growth specification, while GT integrate the two. DK use both the levels and growth equations and a “method of moments” estimation technique as further discussed below.

(b) Country coverage

DK use all countries for which data are available and claim that their estimation results are consistent with (1) the same model being appropriate for poor and rich countries, (2) there having been no change in the elasticity of connection during the past four decades. Such considerations need not compel other researchers to aggregate their sample, and the HIID group has preferred to separate out a group of developing countries for some purposes. In particular, what they refer to as the “Timmer sample” contains 27 countries each of which passes his “relevance test”, viz: population “reasonably” large; significantly agricultural sector in terms of both output and employment share; and “reasonably representative of the developing world”.

(c) The irregular periodicity of the data

TI uses all the data available, so that his 181 data points are unevenly spread across countries, with 22 observations for India and only two for seven of the 27 countries. Each of the other papers uses the growth equation for some or all of the analysis. In that context both errors in variables (see above) and the notion that it is longer-term growth processes that are being studied make the use of the year-to-year growth rates that could be calculated for, say, India, seem inappropriate. DK construct nonoverlapping growth spells a minimum of 5 years long; GRW use a “long panel”, allowing just one growth spell per country (the longest available, fitted to all intermediate observations) and a “short panel” in which all adjacent country observations are used to construct growth spells, notwithstanding the drawbacks.

(d) Reverse causation

If there is two-way causation between y_1 and y , OLS with country dummies on equation (1) may produce an inconsistent estimate of b . For example, suppose that (a) e_{it} is positively serially correlated, (b) y depends positively on lagged y_1 . Then a last-period shock to y_1 will both persist into the current period (raising current y_1) because of (a) and raise current y because of (b). The result will be an overestimate of the elasticity of connection. DK’s method of moments estimation method is designed to overcome difficulties of this kind, although this too requires assumptions and, as it happens, it does not allow for the circumstances of the example just given: they assume (p.16) that e_{it} is not serially correlated. The HIID papers do not address this issue.

(ii) Results

The message from DK is that the elasticity of connection is close to +1 for their sample as a whole and for subsamples constructed by separating countries into rich and poor ones (the cutoff does not appear to be specified in DK) and also by separating the 1960-1980 from 1981 onward. Despite the range of robustness checks carried out, questions inevitably remain, as DK acknowledge. For instance, the very sharp rises in inequality together with falls in GDP/capita in the transition countries in the 1990s might perhaps be considered so special to the transition process [see Milanovic, *Economics of Transition*, 1999] as to make it inappropriate to include these episodes in the data set; in any case, the result of their (presumed²) inclusion must have been to push up the estimate of the elasticity of connection.

In the HIID work we find results that are consistent with DK as regards what GT call the “full” sample—one similar to that used by DK. In Table I, the first nine rows reproduce some of their results. Whether the regression is run in growth or level form, whether decadal dummies are included or not, and whether the ‘full’ (comparable to that of DK) or the ‘Timmer’ sample is used, we get an estimated elasticity of connection not too far from +1. Worth noting (compare rows 1 and 2, and rows 3 and 4) is that the insertion of decadal dummies into the growth equations reduces the estimates and the reduction is statistically significant. This raises the possibility that estimates without such dummies are confounding cross-section and time-series information in an unhelpful way. Suppose for example that a given decade saw both (a) generally rapid growth, and (b) a general improvement in income distribution (resulting perhaps from some global trend such as trade liberalization). This would produce a difference in the estimated elasticities of connection like that in rows 1 and 2 of the Table 1, but for most purposes, it would be the lower “cross-section” estimate in row 2 that would be of interest. It should also be noted from equations (1) and (2) above that, if decadal (or more generally time) effects are important, they need to be included in *both* levels and growth regressions to avoid the confounding of cross-section and time-series effects; the HIID research does not do this, but our own regressions on Timmer’s data set, reported below, do. Also of note in rows 1-9 is that

2. DK do not include a list of countries and dates, so we cannot be certain how many observations are included from the transitionals.

the different methods of dealing with periodicity do not, in the growth equations, seem to alter the estimate in a predictable way (compare rows 5-7 with rows 8-10).

TI introduces two refinements in order to go behind the rather uniform picture found in rows 1-9 (and in DK). He asks (a) whether the elasticity of connection differs between high and low inequality countries, using top quintile mean income minus bottom quintile mean income divided by national mean income (call this “relgap”) as his inequality criterion, and (b) whether it makes a difference to disaggregate the growth of output per head into components from agriculture and nonagriculture.

Taking (a) on its own, we find some evidence that more unequal countries have a lower elasticity. We interact $\ln y$ in the levels regression with a dummy for countries for which relgap exceeds +2 on average. The elasticity estimates for high and low inequality countries respectively, row 11, are 0.87 and 0.64 (difference significant at 5 percent); compare the value in row 4 of the table.³ A growth regression with dummies as in equation (2)—we in fact use annual dummies rather than decadal dummies (there seems no reason not to do this and it makes little difference)—gives the same qualitative result, but the interaction term is now not significant (row 12).⁴ As discussed above, we find that the inclusion of time dummies in the growth equation, not done in the HIID research, reduces the estimated elasticities of connection (row 12 less than row 13).

The evidence reviewed in the preceding paragraph is certainly not strong as regards the proposition that in unequal countries the poor are disconnected from the growth process. It is true that the estimated coefficients all suggest this rather than the reverse, but only in the levels regression do we find a statistically significant result—and then only by employing a dummy for the unequal countries rather than allowing inequality to influence the elasticity of connection linearly. However, stronger results are obtained when the GDP data are disaggregated into agricultural and nonagricultural components. We present here results only on levels regressions.

Using the fact that $d\ln y/d\ln y_{ag}$ is equal to the share of agricultural output in GDP, s_{ag} (here y_{ag} is output per person in agriculture) equation (1) can be generalized to:

$$\ln y_{it} = a + b_{ag}(s_{ag} \cdot \ln y_{ag,it}) + b_{nag}(s_{nag} \cdot \ln y_{nag,it}) + m_i + d_t D_t + e_{it} \quad (3)$$

3. Interacting relgap itself, rather than a dummy for it, produces a negative coefficient on the interaction term, but this is significant only at 8 percent.

4. These results are in line with GT with one exception. In one case they find that the elasticity of connection is estimated to *increase* with inequality in the Timmer sample; we are unable to reproduce this result.

where the “nag” subscript refers to nonagriculture, and—in the absence of distributional effects—one would expect b_{ag} and b_{nag} to equal +1. Levels regressions on (3), with and without decadal dummies, are presented in rows 14 and 15 of the table. These results ought in principle to be the same as those in TI, but differ in the numerical detail, possibly because Timmer appears to use a different weighting scheme in constructing the bracketed regressors in (3). In any case, the results are that in relatively equal countries, the elasticities of connection to both agriculture and non-agriculture are insignificantly different from +1, but in unequal countries the picture is quite different, the elasticity falling to around one half for non-agriculture and to practically zero for agriculture.

(iii) Summary

Are the results of DK and those from the HIID researchers in direct conflict? The answer is no, since DK have not looked for effects on the elasticity of connection arising from either inequality levels or agriculture and non-agriculture separately. Can the striking results from the HIID research be believed? The proposition that growth in agriculture has almost no effect on the income of the poor in very unequal countries seems counter to our detailed knowledge about the location of poor people and the determinants of their wellbeing (sec. 3e below). It therefore seems appropriate to consider very closely at the robustness of the results in rows 14 and 15 and the results of investigation of this will form a part of the final version of this paper. Two lines of investigation seem to suggest themselves. First, and most simply, it should be easy to discover the extent to which the results depend on one or two countries; it may be that the choice of +2 for the critical value of ‘relgap’ has been critical to the results. Second, the fact that the numbers of observations by country differ so much is worrying. In principle, if the within-country error terms turned out to be autocorrelated in some cases, then there could be statistical grounds for downweighting the observations in those cases (this corresponds to the intuition that multiple, closely-spaced observations from one country may, in some sense, contain less information per observation than sparse and separated observations for another).

Table 1

Row	Source	Sample	Levels (L) or growth(G)	Time dummies	Split by inequality category?	Other regressor	EC _{CO}	EC _{AG}	EC _{NAG}
1	GT	Full	L	No	No		0.979 (0.026)	–	–
2	GT	Full	L	Decadal	No		0.934 (0.042)	–	–
3	GT	Timmer	L	No	No		0.951 (0.042)	–	–
4	TI,EL	Timmer	L	Decadal	No		0.788 (0.070)		
5	GT	Full; max time span.	G	No	No		0.96 (0.183)	–	–
6	GT	Full; max no. of intervals	G	No	No		1.10 (0.095)	–	–
7	GT	Full; intervals at least 5 yrs	G	No	No		1.11 (0.096)	–	–
8	GT	Timmer:max time span	G	No	No		1.2 (0.289)	–	–
9	GT	Timmer: max no. of intervals	G	No	No		1.16 (0.169)	–	–
10	GT	Timmer: intervals at least 5 years	G	No	No		1.18 (0.172)	–	–
11	EL	Timmer: max no. of intervals	L	Decadal	Yes	Dummy for unequal countries: -0.23(.09)	0.87 (0.08)		
12	EL	Timmer: max no. of intervals	G	Annual	Yes	Dummy for unequal countries: -0.71(.48)	1.19 (0.27)		
13	EL	Timmer: max no. of intervals	G	None	Yes	Dummy for unequal countries: -0.42(.35)	1.23 (0.20)		
14	EL	Timmer	L	None	Yes	Ag unequal co. dummy: -1.11(.28) Nonag unequal co. dummy: -0.57(.16)		1.17 (0.18)	1.07 (0.11)
15	EL	Timmer	L	Decadal	Yes	Ag unequal co. dummy: -1.22(.27) Nonag unequal co. dummy: -0.64(.16)		1.26 (0.18)	1.02 (0.10)

**D. Growth-poverty Relationships Overall,
or for Particular Types of Country or Policy Régime?**

DK claim, on the basis of their data, that there is no apparent decline in the capacity of growth to benefit the poor. They show this for the set of observed growth spells taken together, but not necessarily for any specific case. For example, the poverty impact of growth (a) in India was much less in 1992-1997 than in 1975-1990 (there was no growth or poverty reduction in 1990-1992, or indeed 1960-1975 at PPP). The poverty impact of growth (b) in the PRC was enormous in 1977-1984 alongside land reform *and* substantial price liberalization *and* very fast agricultural growth based on technical progress. It was much smaller in 1984-1993 and 1996-1998 despite fast growth; and absurd, incredible, in 1993-1996 (when allegedly 140 million Chinese transcended the PPP dollar poverty line!)—everyone knows these numbers are strange [cf. A.R. Khan 19xx] though there are conflicting views about what is wrong with them. Since the PRC and India contain over half the world's absolute poor, only in a special sense can it can be correct to claim no decline in the poverty impact of growth.

Further, is the claim of no decline in [(percent change in mean income of poorest quintile) / (percent change in mean income)] related to the fact that a large number of “special” observations, viz. almost all the “growth spells” for transitional economies post-1989, show big negative growth, big rise in inequality, and hence very big fall in poorest quintile's mean income?

**III. What Sorts of Growth, Poverty Reduction,
and inequality Reduction Promote Each Other?**

We have reviewed papers relating growth, poverty reduction, and income and consumption distribution via national cross sections, both of levels and of time series. This final section:

- (i) Summarizes aggregate evidence on how growth affects income or consumption (IC) distribution, and, thus and otherwise, poverty.
- (ii) Reviews how *initial* IC distribution affects growth (and the impact of such growth on poverty); whether it is asset or IC distribution that is at work; and impact of *redistribution* on growth and, hence and otherwise, on poverty.
- (iii) Presents an “Economics 101” view of what *types* of growth might be relatively *bad* for poverty reduction in developing countries.

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- (iv) Asks if the effect of inequality and redistribution on growth and poverty reduction might depend on whether inequality is very high, “normal”, or very low; and ascribed or achieved.
 - (v) Explores, in view of the above sections, the differences they reveal between developing and developed countries (and among developing countries), and possible cumulative causation— what country characteristics, policy types, or events might stimulate sequences of faster growth, moderate or reduced inequality, and faster poverty reduction. We suggest three “pro-poor growth” policy emphases: on food staples production, especially via faster, employment-intensive technical progress; on decentralized land redistribution; and on fertility reduction.

A. Summary: Impact of Growth on Distribution, and Hence and Otherwise on Poverty

Did anyone ever believe that stagnation of mean income was *in general* better for the poor than growth, or slow growth than fast? That view is refuted by international cross sections on changes in distribution and poverty during growth spells, initially analyzed by Ravallion, and followed up with assorted methodological improvements by Dollar and Kraay, Lundberg and Squire, de Janvry and Sadoulet (for Latin America), Gallup et al., and Timmer. Such evidence is inconclusive regarding the *size or timing* of effects of growth on poverty for reasons discussed in section II notably the problems of inference from cross sections of time series data; the large proportion of growth spells evidence from one country, India, where distribution changed very slowly; the low proportion from some very populous countries, notably the PRC; the strong influence in some series of transitional economies when growth was sharply negative and distribution becoming sharply less equal; and the treatment of (benefit from) GDP and its distribution as identical, by assumption, with personal disposable income. Nevertheless, the above evidence strongly suggests that typically more growth means less absolute poverty (national elasticities of incidence to growth vary up to more than 3, but seldom below 1 and hardly ever negative [WDR 2000-2001, 54]), with no *general* tendency for a changing ratio between the income of the poor and that of the nonpoor.

However, the general assertion that “growth is good for the poor” is perhaps not the most interesting way to interpret this finding. As pointed out by Oxfam [2000], “research carried out at the Institute of Development Studies in Sussex using data from 143 growth episodes found that the in-

come share of the poorest 20 percent fell in 69 cases.” This is perfectly consistent with Dollar and Kraay (2000) and with the proposition that the *ratio* of the poorest quintile's mean income to national mean income does not change systematically as a result of growth: if that ratio falls in 69 of 143 cases, obviously it rises in 74 cases. What is interesting is the following. If we find a group of cases where growth leads to substantial rises in the ratio, and another group where it leads to substantial falls, can we find features (a) normally held in common by the latter group, but (b) normally absent in the former group? What, in short, are the characteristics (or variable values) of cases of growth that is *more* pro-poor, vis-a-vis cases of growth that is *less* pro-poor? If the characteristics or values are mainly or solely the results of different policies, there is a strong case for switching policies toward those of the group where growth is more pro-poor, unless cases (countries and periods) in that group also show significantly slower growth.

This task cannot be undertaken here. The issues in section II need to be clarified first. Above all, what are “cases”? Do some countries show growth that is much more pro-poor in some growth spells, and much less so in others? All we can do here is to suggest, in section III-E below, some sorts of policy for which theory or evidence suggests that they would make growth more pro-poor, either in all developing countries or in a definable subset of them.

A further, converse policy issue is, Has the tendency toward rising within-country inequality in 1985-2000 (sec. **zz**) been due to particular growth-seeking policies? Certainly since the late 1980s some *large* countries with fast growth in inequality (Bangladesh, PRC, Russia, United States) have liberalized to some extent, in part to accelerate economic growth. The example of Russia, with negative growth and worsening distribution, does not suggest that *positive* growth would worsen distribution. Within the developing world, however, countries containing large proportions of the world's poor—Bangladesh, PRC, India to start with—have liberalized considerably between 1988 and 1995, accelerated growth, and worsened inequality. If one were to count *persons* rather than countries, and especially if the exercise were confined to the developing world, we suspect that a very large proportion would reside in countries where faster growth in the 1990s had gone alongside worse income distribution. The very slow recorded falls in poverty in India during the accelerated growth of 1992-1997, and in the PRC, despite rapid growth, since 1985 (apart from the surely very questionable data for 1993-1996), do not in any way refute the findings of Dollar and Kraay, Sachs,

Gallup et al., Ravallion and others. Faster growth is normally better for the poor than slower growth, and is not systematically offset by any change in distribution. But huge exceptions—and the possibility of clusters of countries where growth is much better for distribution, or much worse—mean that these findings are the beginning, not the end, of the inquiry. Residuals matter. In particular, growth may be more pro-poor in more initially equal places and times. That tendency could increase over time, or decrease, according to whether low-end inequalities diverge or converge. We return to this issue below.

B. Summary of Evidence: Impact of Distribution on Growth and Poverty

So far we have concentrated on pro-poor growth. However, the prospects for pro-poor growth and pro-growth poverty reduction are intertwined. If the sign of “effect of growth on poverty” is the same as the sign of “effect of poverty on growth”, the mutual effects cumulate and strengthen one another, e.g., a small amount of pro-poor growth will later be amplified because the poverty decline then ignites further growth. If the signs are different, each effect is damped. So “whether (or how much) growth causes poverty reduction (or low-end redistribution)” depends, after the earliest stages, on “whether poverty reduction (or low-end redistribution) causes extra growth”. Moreover, data analysis can seldom, if ever, firmly assert that the evidence establishes, say, pro-poor growth and not **pro-growth poverty reduction**. Tests of direction of causation are often reported, but are hard to interpret, given the shortness of most growth spells and the likelihood that a country's Gini in one year, or even one quinquennium, is a good predictor of its Gini in the next.⁵

There are three ways in which lower, or falling, Ginis can improve the rate of transformation of growth into poverty reduction. Reducing the per-person income or consumption Gini⁶ normally reduces *static poverty*, given mean income. It may also tend to improve the *impact of a given growth rate upon the poor*, for arithmetical or political economy reasons.⁷ But the effect of greater equality

5. This likelihood may be even stronger for rankings; if 1980-2000 revealed a *global* tendency to rising within-country Ginis (Cornia and Kiiski 2001, Kanbur and Lustig 1999), it may affect all countries (although Heckscher-Ohlin and the fate of the transitionals suggests big exceptions).

6. Shown by the prevalence of lognormal distributions of income and consumption (Ravallion and Datt on India and Brazil; Dollar and Kraay more generally) normally to accompany raising the income share of the poor.

7. It is plausible that the rich are both more powerful to seize a larger share of any growth, and willing to make more sacrifices and to incur more costs to do so, if inequality is greater than if it is smaller initially.

on poverty depends also—and over time increasingly—on *what greater income equality does to the growth rate*.

(i) Impact of Income or Consumption Distribution on Growth

Until the 1970s most economists argued that inequality was conducive to faster growth. Classical economics, and Kaldor [19xx], saw this as happening via higher savings rates. Kuznets suggested that urbanization, being a proxy for a shift from agricultural to industrial production, implied increasing inequality as a “cause” of early development: assuming rural-agricultural and urban-industrial sectors internally equal with the urban sector much smaller, as people urbanize this traces out a pattern of rising inequality alongside early economic growth.⁸

Both arguments rest to some extent on a mental image of a zero-saving, nonaccumulating, stagnant but equal (hunter-gatherer?) society, from which modern growth and development emerge via inequality, which is associated with urbanization and savings. That model ignored the quite high savings rates (partly due to seasonality and harvest fluctuation) associated with even very poor settled farmers. Indeed, evidence for systematically rising inequality during, let alone causing, early economic growth was always in short supply. In 19xx Kanbur showed that evidence did not show that most countries experienced a time series Kuznets curve, especially in the alleged segment during which rising inequality accompanies and pushes early growth.

On the other hand, theory and evidence began to emerge that inequality might cause *slower* growth, at least in developing countries. The original evidence [Persson and Tabellini 19xx, Rodrik and Alesina 19xx] was supported by a political economy explanation: very unequal distributions produced pressure on governments from “median voters” to redistribute, leading to high tax disincentives and distortions that slowed growth. Clarke (1997) strengthened the evidence by controlling for more possible causes of growth, but refuted the explanation, showing that the inequality-to-growth link was no weaker (indeed somewhat stronger) in nondemocracies, where the political economy pressures from median voters were presumably less. It says much about current intellectual fashions that the extreme inequality is supposed to do its damage to growth, not of itself, but because it cre-

8. (1) The rise is sharper if, as is usually the case, intra-urban distribution is more unequal than intra-rural. (2) If these distributions are identical, the rise is reversed when urban shares of population and income pass a critical point, generating the inverse-U “Kuznets curve”.

ates pressures toward inept redistributive measures! But the above result makes sense of simpler grounds. Very high IC Ginis are likely to have a nonincentive, ascribed component, not helpful to growth (section D-i below). They are likelier than low IC Ginis to reflect exclusion of substantial groups—women, ethnic or linguistic minorities, the remote—or, simply, discontinuous income thresholds below which the poor are unable to borrow, move from work to school, etc. All these things are much likelier to prevail in poorer countries: low mean IC **and** high inequality pushes significant proportions of people below an initial mean income permitting even adequate nutrition (Payne and Lipton 1992) and greatly reducing willingness to take risks⁹ and thus to escape poverty. Hence extreme inequality in low-income countries appears a plausible cause of reduced competitiveness, wasted human resources, and slow growth. **But the arguments on both sides are a priori; more empirical work is needed.**

Barro (2000) has produced the most information-rich and robust analysis of the effects of inequality on growth to date. He confirms a clear negative impact for countries with mean income below \$1985PPP2100, with a 1-SD reduction in the Gini associated with a 0.5 percent rise in subsequent annual growth of GDP per person. He identifies a further effect of high Ginis in raising fertility—in turn known to cause subsequent slower growth (and less equal distribution) in developing countries, more so where income is lower or fertility higher (Barro 2000, Kelley and Schmidt 1994, Eastwood and Lipton 1999¹⁰). Barro finds no impact of IC inequality on distribution over the entire range of countries and periods—and a favorable impact in developed countries—but in poor countries the negative impact seems clear, not very small, and robust to the inclusion of many other variables believed to account for economic growth.

9. Bowles [19xx] fears that “potentially offsetting efficiency losses may result from egalitarian asset transfers where, as will generally be the case, they result in a transfer of control over productive risk taking from less to more risk averse agents”, so that “an important productivity-enhancing aspect of high levels of wealth inequality is that assets are controlled by agents who are close to risk neutral, and who thus choose a more nearly socially optimal level of risk.” This seems to be much too gloomy. Risk aversion presumably differs little among the richest 5 percent of persons, but rises steeply as income falls below the poverty line. Costs of transition apart, shifting income or assets from the top 5 percent to the poorest 15-30 percent would therefore increase willingness to take risks in the population as a whole (e.g., riskier, but normally more valuable crop mixes would be planted).

10. “Cross-national regressions indicate that higher fertility increases poverty both by retarding economic growth and by skewing distribution against the poor. Our median country in 1980 had dollar-a-day poverty incidence of 18.9 percent; had it reduced its fertility by 4 per 1,000 throughout the 1980s (the sample median fall) ... incidence would have been reduced to 13.9 percent, growth and distribution effects being roughly equally responsible for this reduction.”

(ii) Impact of Income or Consumption Distribution on Conversion of Growth into Poverty Reduction

If poverty is defined as **absolute dollar poverty incidence**, data for 65 developing countries in the 1980s and 1990s suggest a big negative effect on conversion of growth into poverty reduction. Growth of 1 percent in mean per-person consumption with an initial Gini of 0.2 brought a 2.9 percent fall in dollar poverty incidence; with a Gini of 0.4, a 2.1 percent fall; and with a Gini of 0.6, a 1.2 percent fall (*World Development Report 2000-2001*, 54, drawing on Chen and Ravallion 2000 and Ravallion 1997). As for the effect of initial inequality on growth of **mean income of the poorest quintile relative to the overall mean**, the above review suggests a negative impact from initial inequality (measured as the IC gap between richest and poorest quintiles as a proportion of the mean) in retarding the impact of growth on relative poverty also.

This last finding, since income Ginis and inter-quintile inequality tend to move very closely together,¹¹ seems to contradict the view that income Ginis are sluggish: if they did not change at all, then the bottom quintile would always have the same share of IC (suggested as the expected and average finding in Dollar and Kraay [2000] but with many country growth spells diverging in each direction). Birdsall (1999) suggests that political economy endogenizes inequality, so the Gini tends in normal times to be sluggish because it reflects a power structure, and both it and the income Gini normally tend to perpetuate themselves. This cannot be taken too far, however. First, “endogenous inequality” might mean, not stagnant Ginis, but a tendency for Ginis to diverge, with strong and powerful “masses” tending to reduce already low Ginis, and for strong and powerful “classes” to raise already high ones.¹² Second, policy regimes or exogenous “climates” can change sharply, and inequality with them. There is growing evidence that, after a long period of sluggish IC Ginis (Bruno et al. 1996, Deininger and Squire 1996), many within-country IC Ginis rose sharply from the mid-1980s worldwide (Kanbur and Lustig 1999, Cornia and Kiiski 2001, Lundberg and Squire 1999), especially in a substantial number of transitional (Milanovic 19xx), East Asian (Ahuja et al. 1997) and devel-

11. For example, because per-person income and consumption are distributed close to lognormal (Dollar and Kraay 2000).

12. Ravallion (19zz) has argued that Ginis converged among countries after about 1980. Perhaps there are cycles of rising and falling Ginis, as the self-reinforcing process of “classes and masses” reaches limits and generates incentives to employ, or politically mobilize, very poor workers, or frustrated talent denied high rewards.

oped (Luxemburg Income Project) countries. This would imply that growth gets less poverty-reducing over time, in countries with rising Ginis.

What does all this mean for the effects, in developing countries, of reduced initial IC inequality on poverty? First is the static effect: the poor get more of initial income. Second, there are the two Barro effects: lower Ginis in low-income countries accelerate growth, and also curb fertility that further accelerates growth. Finally, there is another way in which, via fertility, lower Ginis in developing countries help the poor through a virtuous circle of more growth and more poverty reduction. Fertility reduction raises the income of the poor, not only via growth, but to almost the same extent through itself further reducing inequality (Eastwood and Lipton 1999).

(iii) Income or Consumption Distribution or Asset Distribution?

However, several analysts (e.g., Lundberg and Squire 1999) present evidence that asset inequality is slowing growth,¹³ not income inequality, which if asset inequality is controlled for has no effect on growth. However, this is a hard statement to read, since income inequality is logically *identical* to the upshot of inequalities in: assets, returns to assets, labor power, labor rewards, and household ratios between dependants and workers or asset-holders. It is plausible to see asset inequality as proxy for “ascribed” inequality, and thus *capturing the part of* income inequality that is dysfunctional for growth, rather than as *distinct from* income inequality.

Land asset inequality has been associated with slower growth (Deininger and Olinto, Tyler and el-Ghoneimy). It is also not rare to find a “Latin American variable” (dummy) retarding growth, or worsening income distribution, when controlling for other influences, and this may be a proxy for a high land Gini; for example, the influence of lower fertility on subsequent poverty (via growth and distribution) in developing countries is less in Latin America, but the influence is reduced almost as powerfully (and highly significantly) by a high land Gini, if that is used as an explanator instead of the Latin American dummy. Land inequality may depress growth in labor-surplus economies because it concentrates the scarce factor, land, in large farms with high labor-related transactions costs and hence somewhat lower net value added per hectare. There are no scale economies in agriculture, and in developing countries maybe the reverse (Binswanger, Deininger, and Feder 1995; Lipton

13. We have seen no analysis of whether asset, rather than income, inequality harms the conversion of growth into poverty reduction, but this proposition meets the same problem as outlined here.

1993]). If benefits from rural growth depend heavily on access to farmland, then its inequality is likely to be important for the transmission of growth into poverty reduction, especially for the higher-order alpha measures of poverty: poverty, and especially severe poverty, is disproportionately rural (IFAD 2001).

Birdsall and Londono (19xx) and Birdsall, Sabot et al. (19xx) have provided evidence that *educational* access inequality and resulting inequalities in human capital retard growth within and among countries. Certainly, great inequality in educational access tends to mean that many people (the poor, especially girls) do not complete primary schooling that normally has a higher social rate of return than tertiary schooling (Psacharopoulos 19xx), which dominates educational budgets and whose benefits are distributed very unequally.

However, concentrated management of industrial and other “heavy” assets—which may feature economies of scale or agglomeration—may well be good for growth. It is likely to be correlated with unequal ownership of such assets, though they are not the same. Development normally accompanies growing urbanization and industrialization of assets, i.e., shifts assets from sectors where transaction costs bring scale diseconomies to sectors where they bring scale economies.¹⁴ Unless development also brings a growing divorce between asset ownership and asset management, one would then expect asset inequality, even though a “cause” of slower growth in developing countries, to be less so in developed ones, or even the reverse, following Barro's (2000) finding on income inequality.

These possible, plausible, but highly contestable causal links from IC or asset equalization, to faster growth in low-income countries, but slower growth in high-income ones, are **not** universal determined paths, removing policy choices from governments. Even in a highly developed country, there will be scope for public sector actions, or withdrawals from action, that are specially helpful to the poorest in ways that reduce exclusion, promote access and competition, and accelerate growth. Even in a low-income country, where *many* government actions (or withdrawals) improving IC-distribution will thereby later accelerate growth, *some* are liable to retard it.

14. Further, because of rising capital/labor ratios in agriculture, development means that reduced capital transaction costs through large farms becomes more important, and reduced labor transaction costs through small farms becomes less important. This similarly may weaken or reverse the negative impact of inequality on growth.

Crucial in this regard is the distinction between ascribed and achieved inequality (section D-i). One way in which ascribed inequality slows (and distorts) growth is by artificially “rationing” market access, not by capacity to get good returns from what is bought but by status, and by purchasing power acquired not through the market value or amount of one's effort but through status. Extreme ascribed asset inequality, and the extreme income inequality it helps to create, characterize land-based developing economies, and harm growth by denying large parts of human resources a chance to develop themselves—making both them and the educational system cost-ineffective, as well as inadequately competitive. A high land Gini appears disproportionately important in predicting a high IC Gini even where agriculture has dwindled to only 10-15 percent of GDP (Carter 2001). This recalls Barrington Moore's (19xx) use of historical methods to argue that, where societies are based on severe and unreformed land inequalities, that impedes the development of modern political systems based on equality before the law and open justice. If indeed, as argued elsewhere by Barro (19xx), such matters (and the associated forms of property rights) are crucial for growth, then the freezing of anti-poor structures of ascribed asset inequality may be a long-term inhibitor of growth, as well as of poverty reduction given growth.

(iv) Redistribution and the Growth–poverty–reduction Sequence

Lustig and Kanbur (19xx) suggest that sharp rises in national IC Ginis in the 1980s and 1990s, together with new evidence that this may harm growth in developing countries, have placed redistribution “back on the agenda”. We fully agree, but the agenda item is slightly different from the question of whether a smaller IC or asset distribution is better than a bigger one, for growth or even for “welfare” or “capabilities”, in general; or even in a particular case. In short, there are transition/transaction costs of redistribution. There may also be benefits, e.g., if the poor learn through distribution to use their capacities more effectively. These costs and benefits of the shift from old to new distribution are taxes, or “subsidies”, to the growth process—over and above the different effects on it of the new distribution, compared to the old one.

The extent to which redistribution is disruptive, consensual (or consensus-forming), well targeted, and inexpensive in administrators (and not rent-creating for them¹⁵) is relevant. Can Ginis be

15. This is not easy. However, ascribed inequalities both embody and create rents; and, apart from rent amounts, *rent-seeking* and its costs (Krueger 19xx) may be cut by reductions in inequality.

cut consensually? Perhaps, if the rich are convinced this will increase their own welfare faster than alternatives, e.g., if they gain more from faster growth than they lose from redistribution. This links up to the types of inequality issue. If land reform and fertility-reducing changes induce growth as well as redistribution, even the rich and powerful might get absolutely better off. Would they “buy” a loss in relative position to attain that? Clearly it depends on the scale of changes. Suppose that sharply reducing some very growth-constricting form of ascribed inequality raises the rate of growth of mean income from 1 to 5 percent, but for the poor from 0.8 to 8 percent, and for the rich from 1.1 to 4 percent. The rich might accept that, especially if (a) they dislike seeing poverty around them; (b) they are scared of its risks for them, from infection to revolution; or (c) they hanker after “insurance-type redistribution” against the risk that they may later go from riches to rags. A probably “political-economy-compatible” form of redistribution might involve subsidized insurance that raised security for all; it has more impact on the poor, who are more subject to and damaged by adverse events (Pritchett 19xx).

C. Economics 101 View of When Growth Might be Bad for the Poor

First, there may be “immiserising growth”. Some countries are “large” relative to particular export markets. If a country has a market share for an export above its price elasticity of demand (as with Sri Lankan tea in the 1960s), country volume growth of that export is revenue-decreasing. Since there are production costs at the margin, immiserising growth happens even with some excess of product elasticity over market share. Value-added in such a product may be a big share of GNP (e.g., cloves in Zanzibar in the 1970s), so that equiproportionate growth in the volume of *all* value-added could well be mean-welfare-reducing. (Such “growth” ought not to count as growth on a PPP-measure, but would you bet on it?) With no change in distribution, that would obviously increase poverty on any of the alpha measures.

Second, growth can be bad at, or even negative for, poverty reduction if growth shifts *demand* away from products made by immobile poor workers (the coarse-cloth effect).

Third, on the *supply* side, growth can be poverty-unfriendly if due to technical progress that is either Hicks labor-saving (the tractor effect)¹⁶ or relatively slow in sectors in which the immobile poor are concentrated (handloom-weavers effect).

Fourth, whether due to demand or to supply factors, growth may shift resources away from making poor people's products, so their relative prices rise unless the economy is fully open with zero transport costs (the millets-to-milk effect).

These are all really micro-effects; all are stated above in unduly partial-equilibrium form; and all have a sort of "converse" if read as "conditions under which poverty increases [reductions] might not be bad [good] for growth". In addition there are familiar possible *macro*-effects, some of which stem from poverty effects of types of policy used to obtain faster growth.

D. Effect of Different Degrees and Types of Inequality

Analysis of cross sections of national time periods to establish causal links between Ginis and subsequent or prior growth or poverty has suggested some important, largely negative, findings. However, such analysis is unlikely to capture the complex and situation-specific effects of distribution on growth and poverty. Also, when better data are available, even international cross-section analysis needs to test the plausible hypotheses that:

- (i) Very high and very low inequality are both likely to harm growth and poverty reduction;
- (ii) While some forms of *achieved* inequality reflect incentives (rather than producers' surpluses) conducive to growth and thus poverty reduction, *ascribed* inequality—based on (say) inheritance, ethnic or gender discrimination, or urban bias—is a 'tax

16. It is a lump-of-labor fallacy that labor displaced through Hicks-labor-saving technical progress must become unemployed. But if technical progress (or investment) increases GDP more slowly than it decreases labor use per unit of GDP *at given factor prices* (i.e., than it shifts the aggregate demand curve for labor), then full-employment labor income falls. If the poor depend disproportionately on labor income, this reduces their gains from growth, perhaps below zero. If there are fixprice elements in labor markets (e.g., due to legislation, search costs, or trade unions) then some people become unemployed as capital is substituted for labor (incidentally, reducing equilibrium GDP). In completely flexible labor and capital markets (and on further strong perfect-market assumptions, which is why Keynes rightly dismissed this as a *general* theory) all the costs are borne as wage rate falls but full employment is maintained. In either case labor (the poor) lose. They might lose more from redistribution from labor to capital than they gain from economic growth. The greater the extent to which the poor own capital, the less this risk. At the extreme, if capital is equally distributed, income shifts from capital to labor have *no* effect on poverty. This is one reason why equality increases connectedness.

on incentives', a barrier to efficient markets, and hence enemy to both growth and poverty reduction.

(i) How Much or How Little Initial Inequality is Good or Bad for the Poor?

Barro (2000) seems to show that initial IC equality does not affect growth, controlling for other variables, in regressions over the whole range of country values. However, there may be an effect over defined ranges of the variables only. Barro himself shows a favorable impact for low-income countries and an unfavorable impact for high-income countries. But consider first extremes of *income inequality*. If everyone has almost the same IC—irrespective of effort, risk-taking, saving, or market demand for skills—incentives are stifled and growth is likely to be very slow. If 5 percent of people enjoy 95 percent of IC, then, especially if the mean is low, most people are likely to be excluded from risk-taking (including some forms of migration), entrepreneurship, and saving, including the diversion of time from earning to learning; this too is likely to make for very slow growth.

Is it fanciful to imagine that we might one day derive a growth-optimal or a poverty-reduction optimal range of values for the Gini (or the Timmer measure, the gap between richest-quintile and poorest-quintile mean income as a proportion of the national mean)? Measurement will probably always be too weak, in different ways and to different extents, for that. Also types of inequality are too different among countries and times. Yet it is clear that, in almost any imaginable country, a 0.9 Gini (or a 1000 Timmer ratio) is too high, and 0.1 (or 1.5) is too low.

(ii) Achieved and Ascribed Inequality, Growth, and Poverty Reduction

Types of inequality, and hence of redistribution if we assume not-too-large transaction/transition costs, differ in impacts both on growth and on elasticity of poverty to growth. In particular, inequality may be achieved, for example through effort or market performance (Bill Gates inequality), or ascribed, by laws or customs that assign income or assets on account of, for example, inheritance, status, gender, or ethnic group (Nineteenth Earl inequality).

Not all achieved inequality is "Bill Gates inequality", useful for growth. Some achieve top-end income by force or fraud. Some achieve it legitimately but in substantial part as producer's surplus (e.g., people who would exert 99 percent of their effort and skill, even if paid only 65 percent of their time-rate); or as market reward for skills or efforts that retard growth (e.g., cigarette advertisers); or do not clearly enhance it (witch-doctors, professional chess and poker players, pop and classical

musicians, economists). Call this, in honor of rock and sports stars, Joe/Jill Headbutter inequality. However, almost certainly, some societies have overtaxed “Bill Gates inequality”, and in general earned incomes and hence effort and achievement, and thereby retarded growth enough to raise long-run poverty. That this is in principle possible is a Rawlsian argument against *some sorts of* redistribution. Further, if achieved inequality is so curtailed that economic activity provides few incentives, those highly motivated to enrich themselves are, by definition, likelier to use other means, including crime, war, and politics in its public-choice definition (Bierce 1903?): the use of public resources for private advantage. This shift from Gatesian to the illegal end of Headbutter inequality further harms growth, and may well redistribute it against the poor.

Nineteenth Earl inequality, however, is normally bad for poverty reduction. Statically, like Gatesian inequality but without its offsetting stimulus to economic activity, it reduces income available for the poor. Dynamically, high-ascribed incomes are a deadweight tax on GDP, reducing the amount available as incentive income. Further, those ascribed, or given prior or stronger claims on, assets—education, land, a high-prestige job, ownership of a parent's business—on account of inheritance, ethnic group, or gender, exclude others, perhaps better able to generate private and social returns from those assets. This harms efficiency and growth directly, and also by weakening and segmenting competitive pressures.

The only growth defence of inherited inequality—only one type of ascribed inequality, though maybe that with most effect on distributional outcomes—is that it provides savings incentives, especially to the old (bequest motive), which may advance growth. This is relevant only if (i) growth is national-finance-constrained; (ii) the positive effect of bequest motives, on the savings and the work (or education-seeking) ethic of potential legators, outweighs their negative effect on those of potential legatees. Perhaps enforcement (national or global) of high, progressive taxes on bequests and gifts inter vivos—say 95 percent+ over \$5 million cumulative—would induce a huge “capitalist” growth surge.¹⁷ Virtually eliminating large inherited claims, even by democratic consent, may well be judged oppressive, inconsistent with “the constitution of liberty” or even with human (and animal)

17. Given this policy, people can differ about the extent to which the revenue should be used (i) to reduce public debt, (ii) to cut taxes, (iii) to raise public spending (not necessarily state-enlarging, e.g., the revenue could be hypothecated for a substantial and identical “entrepreneurship-education grant” to each citizen at the age of 18).

nature-genetic programming to pass on more than genes to children. But, in sharp contrast to extreme compression of achieved inequality, it would probably be good for growth.

It may be objected that policies to cut inequality—even ascribed—reduce security of property rights. Some indicators of arbitrary abrogation of property rights are correlated (not always very robustly) with slower subsequent growth (Sachs 19xx, Barro 19yy). But inheritance tax law and its enforcement have differed hugely, even among liberal and democratic countries and over time; has anybody shown any effects on growth? It is not clear that a democratically legitimized decision to shift a given tax burden toward high, progressive inheritance taxation is an abrogation of property rights, let alone an arbitrary one. Nor has anyone shown that property taxes are worse for efficiency or growth than others. Adam Smith (1776) argued that they were better, admittedly in the course of a plea for “light” taxation overall. There are less provocative measures encouraging property redistribution by private actions, and discouraging enormous private inheritances, such as the Rignano plan (to levy Italian estate duty on recipients proportionately to the size of the bequest) [ref] or progressive land tax. This, by encouraging land redistribution consensually, would also probably move land-holdings nearer optimal size, accelerating farm growth in land-scarce, labor-plentiful areas.

There is a problem, though, about identifying “desirable achieved” inequality, and placing low tax disincentives on it because it helps growth, while taxing “undesirable acquired” inequality heavily because it is a deadweight upon growth. The problem is that even pro-growth achieved inequality *now* readily leads to its self-protection by political system manipulation, and, thus and via inheritance, to dysfunctional ascriptive inequality *later*. This brings “social exclusion” of the poor later (Bowles 19xx, emphasising race and gender effects), but it goes far beyond that. How can a society build in limits to such processes, while providing sufficient stimuli to work, risk-taking, discovery, enterprise? That is perhaps the central problem of pro-poor growth.

There is a link between the two plausible hypotheses that (i) the extremes of both equality and inequality are liable to harm growth, and (ii) ascribed inequality is normally anti-growth, but (some types of not-too-extreme) achieved inequality are normally pro-growth. It is plausible that really extreme inequality can long persist only in societies where it is largely ascribed, enforced, and associated with inherited land, rather than achieved through market performance; humans are not all that different. If extreme inequality is mainly ascribed, and moderate inequality mainly achieved, this

forms another reason to expect moderate equality to help growth but extreme inequality to harm it. That leaves open what is extreme and what is moderate, and what are our tradeoffs between growth, poverty reduction, libertarian considerations including the right to bequeathe, and (with positive inequality aversion [Atkinson 19xx]) inequality reduction as an end in itself.

E. Types of Policy Favoring Growth, Equality, and Poverty Reduction

It is plausible that for countries as a whole, though emphatically not for countries in particular circumstances with particular policy types, etc., income equality and income growth are orthogonal: more equality (growth) does not, in and of itself, mean either more growth (equality) or less. However, there may be a general tendency for lower IC, and especially lower asset, Ginis *in developing countries* to bring somewhat faster growth.

Regressions get us only so far, even if we allow for the fact that specific sorts and degrees of inequality make growth more pro-poor, or poverty reduction more growth-friendly. The circumstances of poor countries differ. Can one identify such policies without descending to casual-empirical adhocery? That this may, up to a point, be feasible is suggested by combining the guidelines in section C above with some facts about the world's 1.2 billion dollar-poor.

They spend some 70 percent of their income on food, and about half their income on food staples. Also, they earn over two thirds of their income in cash and kind from growing such staples, or working for those who do; over three quarters of the dollar-poor are mainly engaged in agriculture (sometimes urban agriculture), and many of the rest in rural nonfarm activities whose growth depends mainly on growing demand from farm-based producers and (especially) consumers (IFAD 2001). Policies tending to increase the poor's command over food via more labor income and less food cost will be main approach. Although much trade liberalization has taken place, poor growers of food staples are often "protected" by these products' high unit transport costs and low value/weight ratios, so increasing local food production does cut food prices and make food access more reliable. Rapidly rising labor supply and dependency burden, however, restrain growth of the poor's employment income and command over food.

This suggests that making growth more pro-poor in early development equals mainly (i) achieving labor-intensive growth in food staples production, especially by appropriate technical progress; (ii) enhancing this impact of cheaper food and higher demand for rural labor by stimulating

smaller-scale farms, e.g., via land distribution; (iii) increasing incentives to reduce fertility (mainly lower child mortality, more and better female education and work options, and probably family planning information) and spreading them to the poor.

What is the evidence? First, given overall growth of GDP or mean consumption, a *high rate of growth in agriculture* is associated with subsequent poverty reduction, though there is controversy about whether this is also true where agricultural income or land is initially very unequal. Second, *land redistribution* increases the income share of the poor, certainly statically (directly), and also probably by increasing subsequent growth of agricultural value-added, as well as the share of small-holders and laborers in such growth. Third, *declining fertility* causes both faster growth of per-person GDP and consumption, and their more equal distribution, in both cases reducing poverty; where there may also be a “virtuous circle” in which greater equality, and some forms of economic growth, in turn cause fertility decline.

Some general points should be made about all these three effects in the context of pro-poor growth and pro-growth poverty reduction. First, there is some evidence that all three effects are stronger in poorer countries. Second, there are numerous and widespread examples of fertility decline, rapid agricultural growth, and (despite prevailing skepticism) land redistribution (IFAD 2001, 77-82], in developing countries in the past 50 years; in many cases there is, at least, plausible evidence at national level that such events induced subsequent growth, redistribution, and/or poverty reduction. Third, all three events are often due to policies (sometimes deliberate, sometimes not), but sometimes to “events”, including *past* growth or redistribution, creating the prospect of positive or negative feedback between growth that is pro- (or anti-) poor, and redistribution that is pro- (or anti-) growth. Fourth, there may be long, and among countries quite variable, lags between policy change, outcomes for land distribution or agricultural growth or fertility, and especially impact of such outcomes on growth, distribution and poverty.

(i) Agricultural Growth

It has been shown that agricultural growth has been more pro-poor than industrial growth in India (Datt and Ravallion 1998), in Indonesia (Thorbecke and Jung 1996), and in cross-national data sets for all (Timmer 1997) and small and medium (Bourginon and Morrisson 1998) developing countries. However, Timmer (19xx) finds that in the less-equal subsample of developing countries

agricultural growth does less than nonagricultural growth—indeed, as we have reformulated his results nothing at all—to enhance average income in the poorest quintile. This is supported by de Janvry and Sadoulet (19zz), whose work on 11 (highly unequal in income and especially land) Latin American countries suggests that agricultural growth did little or nothing to reduce absolute poverty.

These exceptions need more analysis. It is credible that agricultural growth is less pro-poor where income, and especially land, are more unequal than where they are less so. But it is difficult to believe that in those countries agricultural growth does not, as in the other analyses cited, reduce poverty more than nonagricultural growth. Agriculture still produces much food, including food staples, in Latin American (and other unequal) developing countries, and is still much more labor-intensive than industry (and less skill-intensive than most services, especially the fast-growing ones), though these agricultural characteristics are less marked than in developing countries with less unequal agricultures. These pro-poor differences in sectoral characteristics apply to marginal (growth) production, as well as to average (initial) production. So while it is easy to see mechanisms making agricultural growth less poverty-reducing in unequal developing countries *than elsewhere*, it is hard to see mechanisms making it less poverty-reducing *than domestic nonagricultural growth*; so hard, given the Economics 101 assumptions earlier, as to cause serious doubt.

The evidence of the Green Revolution is that technical progress is a very substantial cause of agricultural growth, and has been very pro-poor (Kerr and Kohlavalli 1999; Lipton with Longhurst 1989), indicating national and international measures to revive it and spread it to neglected crops and areas (IFAD 2001). Improved price incentives to agriculture obviously have a part to play in this. However, we need to raise questions about a policy sequence that might, to some, seem to advance pro-poor growth, and to support the case for pervasive liberalization, so obviously as to seem a “no-brainer”: liberalize, remove anti-agricultural bias, and thus ensure pro-poor growth (a good, crisp exposition is Krueger, Valdes, and Schiff 1996?). The argument is as follows. Bias against farm production and the rural sector involves massive resource transfer *out* of the sector whose growth is likeliest to avoid the above problems. Agriculture in most developing areas:

- (i) produces most of the poor's main consumable, food staples;
- (ii) employs (or self-employs) most of the poor;

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- (iii) pays them in kind, or with access to local products (the poor often being self-employed smallholders or local farmworkers with high marginal propensity to consume food) and is thus “product self-absorbing”, avoiding the demand-side problems above;
 - (iv) and has the economy's highest average, and more important marginal, labor/capital ratios, (and hence employment per unit of the scarce resource, capital).

Moreover, the great bulk of anti-agricultural *price* bias in most developing, especially countries is due to trade and foreign-exchange manipulation (Krueger et al. 19xx). So liberalize, remove that bias, and growth is at once accelerated and made more pro-poor!

It is this argument, together with the quite strong evidence that trade liberalization does speed economic growth (plus the residual Heckscher-Ohlin hope that, despite some evidence to date, freer trade also stimulates labor-intensive specialization overall), that has led many, including “left liberals”, to favor trade liberalization so strongly (e.g., Sen in Dreze and Sen 199x). While the argument and evidence have some force, the reasoning is in fact quite complex and often doubtful. There are many caveats *not* objections to agricultural liberalization, but to the expectation of high food-price and employment response and hence poverty impact. We have space only to list the caveats here:

- (i) Most anti-farm/rural bias is not price bias.
- (ii) Given the internal polity, exogenously reformed cuts in price bias against rural producers bring correspondingly increased nonprice bias, e.g., in education or roads allocation, which, being less susceptible to price response, may harm the poor more.
- (iii) Agricultural liberalization lags other sectors in poor countries and even more in rich ones, which therefore overproduce farm products, signalling artificially low values to farmers and governments in developing countries.
- (iv) Agricultural growth, and subsequent poverty reduction, have historically resulted more from changing incentives by technical progress (raising output/input transformation rates) than by price reforms (raising output/input price ratios).

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- (v) The nonfarm poor lose if the farm price rise comes through higher marketed food prices, not from cutting parastatal profits/inefficiencies; in many unequal countries even the rural poor are mostly net food buyers.

Removing farm price repression has helped to stimulate farm employment and production and to reduce distortions in their composition. But pro-poor agricultural growth has depended more on appropriate technical progress—research, discovery, and innovation—and on the rural infrastructures that made adoption attractive.

(ii) Land Redistribution

“The English are still imbued with that doctrine, which is at least debatable, that great properties are necessary for the improvement of agriculture, and they seem still convinced that extreme inequality of wealth is the natural order of things” (Alexis de Tocqueville *Journey to England* [1833,72], cited in Bowles 2000).

Even if the rich are “fully compensated” for land in a redistributive land reform, and even if the poor gainers (with an intermediate market-rate loan) pay the cost of such compensation eventually, the poor gain more than the rich lose (and there is a GNP gain), since the poor use that land more labor intensively and with higher net value added per hectare. Thornton (and JS Mill) made the case in the 19th century. Modern micro-level evidence confirms it (Binswanger et al.1995, Lipton 1993). The presence of scale diseconomies in farming where labor transaction-cost reductions central to efficiency (i.e., in labor-surplus economies) accounts for the negative impact of land inequality on subsequent growth (Tyler and el Ghoneimy 1992, Deininger and Olinto 1999).

(iii) Fertility Reduction

Fertility reduction helps the poor both via faster growth and via more equality. The effect is strongest where initial inequality is most, poverty most pervasive, and initial mean income least (Eastwood and Lipton JDS 1999). There may be a virtuous circle in which equality causes fertility reduction too (Barro 2000).

This happens at two levels. Nationally, lower labor supply—both by workers, and induced to support dependants—pulls up labor income (wage-rates and/or employment); and lower growth in population demand presses down the real price of food, assuming either imperfectly open economies or significant transport costs. At household level, lower dependency ratios increase the extent

to which given employment income and food price permit adequate household nutrition. The former effect depends on national fertility decline, but the latter depends on the spread, to the poor, of incentives to cut fertility, and hence provides a further channel through which pro-poor policies (here especially to spread child mortality reduction and female education) improve future growth, since lower fertility contributes significantly to economic growth, albeit with a 10-15-year time lag that may not be captured in Barro's formulation (Kelley and Schmidt 199x).