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Growth *Is* Good for the Poor

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David Dollar

Aart Kraay

Development Research Group

The World Bank

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Abstract: Income of the poor rises one-for-one with overall growth. This general relationship between income of the bottom fifth of the population and per capita GDP holds in a sample of 80 countries covering four decades. Although there is a fair amount of variation around this general relationship, a number of popular views about the poverty-growth relationship are *not* true. The effect of growth on income of the poor is no different in poor countries than in rich ones. Incomes of the poor do not fall more than proportionately during economic crises. The poverty-growth relationship has not changed in recent years. We also show that policy-induced growth is as good for the poor as it is for the overall economy. Openness to foreign trade benefits the poor to the same extent that it benefits the whole economy. Good rule of law and fiscal discipline are other factors that benefit the poor to the same extent as the whole economy. Avoidance of high inflation in fact is "super-pro-poor": that is, high inflation is more harmful to the income of the poor than to GDP overall. In contrast we find no evidence that formal democratic institutions or public spending on health and education have systematic effects on incomes of the poor. These findings leave plenty of room for further work, because they emphasize the fact that we know very little about what systematically causes changes in the distribution of income.

1818 H Street N.W., Washington, DC, 20433 (ddollar@worldbank.org, akraay@worldbank.org). We are grateful Dennis Tao for excellent research assistance, and to Mattias Lundberg and Shaohua Chen for sharing data on income distribution. The opinions expressed here are the authors' and do not necessarily reflect those of the World Bank, its Executive Directors, or the countries they represent.

While the macroeconomic indicators have often looked good,
real wages in many countries have declined, and wage inequality
has increased both within and between countries.

--Lori Wallach, Leader of the anti-WTO protests in Seattle,
on the impact of globalization

The global economy governed by international financial institutions, the World Trade Organization, and Multinational Corporations proposes structural adjustment for countries in the South in the name of fiscal health. The result is increasing poverty, debt, and unemployment.
-- NGO declaration at the UN Conference on Women

Globalization has dramatically increased inequality between and within nations…
--Jay Mazur
"Labor's New Internationalism," Foreign Affairs

We have to reaffirm unambiguously that open markets are the best engine we know of to lift living standards and build shared prosperity.
--Bill Clinton
Speaking at World Economic Forum

1. Introduction

The world economy has grown well during the 1990s (despite the East Asia crisis), but there is an intense debate going on about the extent to which the poor are benefiting from this growth (see the quotes above). At one end of the spectrum are those – including some of the NGOs that disrupted the WTO meetings in Seattle -- who argue that in general the poor do not benefit from global growth – that all of the benefits accrue to the middle and upper classes. A slightly different view is that the poor may benefit somewhat in absolute terms, but that they benefit proportionally less than the average household, so that inequality within countries is on the rise. Finally, there is the "shared prosperity" view (echoed by Bill Clinton above): for countries that participate in the global economy, there is shared prosperity among countries and among households.

In this paper we investigate the link between income of the poor (defined as the bottom one-fifth of the income distribution) and overall income (per capita GDP). We put together data on income of the poor and mean income for 80 countries covering four decades, giving us 236 episodes in which we can link growth in income of the poor to growth in overall income. We use these data to investigate some of the hypotheses about the growth-poverty nexus:

- What is the general relationship between growth of income of the poor and overall economic growth, and does it differ by level of development, during crises, and/or between time periods?
- Does policy-induced growth, for example, through increased openness to international trade, benefit the poor proportionally – or more or less than proportionally?
- Are there policies that are not necessarily pro-growth but still are important for incomes of the poor?

For the benefit of the non-technical reader we summarize our answers to these questions in the next section. In Section 3 we provide details on the data and our econometric strategy for estimating the relationship between growth of income of the poor and overall income. In that section we also indicate how our work relates to the large literature on income distribution and growth. The detailed results are presented in Section 4. Section 5 concludes with thoughts about further extensions of this work.

2. The Story in Pictures

Income of the poor has a very tight link with overall incomes. The top panel of Figure 1 shows the average income in the poorest fifth of the population plotted against average income for the whole economy (per capita GDP). The graph includes 370 observations covering 125 countries, and multiple observations for a single country are separated by at least five years over time. The slope of this relationship is very close to one, and all of the observations are closely clustered around this regression line. This indicates that as overall income increases, on average incomes of the poor increase by exactly the same amount. For 236 of these observations, we can relate growth of income of the poor over a period of at least five years to overall economic growth, as shown in the bottom panel of Figure 1. Again, the slope of the relationship is slightly larger than one, and although the fit is not quite as tight as before, it is still impressive.[1] There are 108 episodes in which per capita GDP grew at a rate of at least 2% per year: in 102 of these episodes, income of the poor also rose. Thus, it is almost always the case that the income of the poor rises during periods of significant growth. There are a variety of econometric problems with simple estimates of the relationship between incomes of the poor and overall income, which we take up in the following section. Even after addressing these, the basic result that growth in the overall economy is reflected one-for-one in growth in income of the poor turns out to be very robust.[2]

One can use the data in Figure 1 to ask a closely-related question: what fraction of the variation across countries and over time in (growth in) incomes of the poor can be explained by (growth in) overall income? In terms of levels of per capita income, this fraction is very large. The data in the top panel of Figure 1 imply that over 80 percent of the variation in incomes of the poor is due to variation in overall per capita incomes, and only 20 percent is due to differences in income distribution over time and/or across countries. In terms of growth rates, just under half of the growth of incomes of the poor is explained by growth in mean income.[3]

Having seen the importance of growth in overall income for incomes of the poor, we turn to the remaining variation around the general relationship in Figure 1. The main point of this paper is to try to understand systematic patterns in those deviations – that is, what makes growth especially pro-poor or pro-rich? We consider two types of hypotheses. First, we consider hypotheses about the poverty-growth relationship that involve dividing the data points into different groups (poor countries versus rich

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[1] It is useful to clarify that our results refer to the elasticity of average incomes in the bottom quintile with respect to overall average income, which we find to be very close to one. In contrast, it is well-known that the elasticity of the poverty headcount (the share of the population with income below a given fixed poverty line) with respect to average income varies widely across countries and depends among other things on the distribution of income.

[2] This result should not be very surprising in light of the striking absence of any correlation between (changes in) income and (changes in) inequality documented by, among others, Ravallion and Chen (1997) Deininger and Squire (1996), and Easterly (1999a).

[3] The figures in this paragraph are based on the following standard variance decomposition. The logarithm of per capita income of the poor is equal to the logarithm of the share of income accruing to the bottom quintile, plus the logarithm of overall per capita income, plus a constant. Given an observation on per capita income of the poor that is x% above the mean, we would expect that 80% of this deviation is due to higher per capita income, and only 20% due to lower inequality. The figure 80% is the covariance between per capita income and incomes of the poor divided by the variance of incomes of the poor. The calculation for growth rates is analogous.

countries, crisis periods versus normal growth, and the recent period compared to earlier times). Second, we introduce other institutions and policies into the analysis and ask whether these influence the extent to which growth benefits the poor.

A common idea in the development literature is the “Kuznets hypothesis” that inequality tends to increase during the early stages of development and then decrease later on. In our framework, this hypothesis suggests that the coefficient on income of the poor is less than 1.0 at low income stages and more than 1.0 at high income ones. When we split the sample between rich and poor countries, we find a coefficient of 1.06 for the poor countries and 1.07 for the rich. These estimates are shown as two horizontal bars in Figure 2, and can be compared with the basic relationship that holds in the full sample, shown as the bottom bar. These two estimates are statistically indistinguishable from each other, and also from one. In other words, in our large sample of countries and years, there is no apparent tendency for growth to be biased against poor-income households at early stages of development.

Another popular idea is that crises are particularly hard on the poor. Our growth episodes are all at least five years long. Hence, an episode of negative per capita GDP growth in our sample is a period of at least five years in which per capita incomes fell on average: we feel comfortable labeling these as “crisis” periods. If we split the sample between negative growth (crisis) episodes, and positive growth (non-crisis) ones, the estimated relationship between income of the poor and mean income is 1.08 for the crises and 1.09 for the periods of normal growth (Figure 2). We also try dividing the sample into high growth (higher than the median) and low growth (lower than the median) observations. Again, we find that the coefficients are quite similar in magnitude and are statistically indistinguishable from each other and from one. Thus, there is no evidence that crises affect the income of the poor disproportionately. Of course, it could still be the case that the same proportional decline in income has a greater impact on the poor if social safety nets are weak, and so crises may well be harder on the poor. But this is not because their incomes tend to fall more than those of other segments of society.

A third idea is that growth used to benefit the poor, but that the relationship is no longer so robust. We test this by dividing the episodes between the 1960s and 1970s, on the one hand, and the 1980s and 1990s, on the other. We estimate the relationship to be 1.01 in the earlier period, and 1.02 in the later one (Figure 2). Once again, these estimates are not significantly different from each other or from one. Thus, it is not the case that growth has become less pro-poor than it was in the past. In summary, none of the efforts to divide the data points into different groups changes the basic relationship between incomes of the poor and growth.

We next turn to the second set of hypotheses concerning the role of various institutions and policies in explaining deviations from this basic relationship between incomes of the poor and growth. A core set of instituitons and policies (notably, macro stability, fiscal discipline, openness to trade, and rule of law) have been identified as pro-growth in the vast empirical growth literature. However, it is possible that these policies have a systematically different impact on income of the poor. For example, the popular idea that ”globalization” increases inequality within countries – as expressed in several of the opening quotes – can be examined by asking whether openness can help explain negative deviations in the relationship between income of the poor and mean income. Alternatively, there may be institutions and policies that have not been established as robust determinants of growth, but are often thought to be good for the poor, notably democracy and social spending. This hypothesis can be considered by asking whether these variables explain positive deviations in the relationship between income of the poor and mean income.

We use Figure 3 to summarize the results of introducing these policies and institutions into the analysis. We decompose the effects of each of these variables on mean incomes of the poor into two components. The first, labeled “growth effect”, shows direct effects of the indicated variable on incomes of the poor that operates through its effect on overall incomes. The second, labeled “distribution effect” captures the indirect effect of that variable on incomes of the poor through its effects on the distribution of income. Openness to international trade raises incomes of the poor by raising overall incomes. The effect on the distribution of income is tiny and not significantly different from zero. The same is true for improved rule of law, which raises overall per capita GDP but does not significantly influence the distribution of income. Reducing government consumption and stabilizing inflation are examples of policies that are “super-pro-poor”. Not only do both of these raise overall incomes, but they appear to have an additional positive effect on the distribution of income, further increasing incomes of the poor. In the case of inflation, this additional distributional effect is statistically significant in most of our specifications, and reflects primarily the reduction of inflation from very high levels. [4] From this we conclude that the basic policy package of private property rights, fiscal discipline, macro stability, and openness to trade increases the income of the poor to the same extent that it increases the income of the other households in society. This is not some process of "trickle-down," which suggests a *sequencing* in which the rich get richer first and eventually benefits trickle down to the poor. The evidence, to the contrary, is that private property rights, stability, and openness directly create a good environment for poor households to increase their production and income.

Finally, we also examine a number of institutions and policies for which the evidence of their growth impacts is less robust, but which may have an impact on the material well-being of the poor. Most notable among these are government social spending, formal democratic institutions, and primary school enrollment rates. The last two of these are also shown in Figure 3, using an index of voice and accountability to measure the strength of democratic institutions. Voice has small, statistically insignificant, and offsetting growth and distribution effects. Primary education has a beneficial effect on growth, but no perceptible effects on income distribution. Public social expenditure shows little effect on either growth or distribution. This reminds us that in many countries public expenditure on social services often is not well-targeted towards the poor.[5]

To summarize, we find that contrary to popular myths, standard pro-growth macroeconomic policies are good for the poor as they raise mean incomes with no significant adverse effect on the distribution of income. In fact, macro stability, proxied

[4] This result is consistent with existing evidence in smaller samples. Agenor (1998) finds an adverse effect of inflation on the poverty rate, using a cross-section of 38 countries. Easterly and Fischer (2000) show that the poor are more likely to rate inflation as a top national concern, using survey data on 31869 households in 38 countries. Ravallion and Datt (1999) find evidence that inflation is a significant determinant of poverty using data for Indian states.

by stabilization from high inflation, increases income of the poor more than mean income as it tends to improve income distribution. Other policies such as good rule of law and openness to trade benefit the poor and the rest of the economy equally. On the other hand, we find no evidence that formal democratic institutions or a large degree of government spending on social services have any effect on income of the poor. Finally, the growth-poverty relationship has not changed over time, does not vary during crises, and is generally the same in rich countries and poor ones. In the remainder of this paper we provide details on how these results are obtained.

We do not want to be misinterpreted as arguing that growth is *all* that is needed to improve the lives of the poor. But we do want to get the message out that growth generally does benefit the poor and that anyone who cares about the poor should favor the growth-enhancing policies of good rule of law, fiscal discipline, and openness to international trade.

3. Empirical Strategy

In this section we outline the empirical strategy that underlies the results overviewed in the previous section. We also relate our approach to the large literature on income inequality and growth.

3.1 Measuring Income and Income of the Poor

We measure mean income as real per capita GDP at purchasing power parity in 1985 international dollars, based on an extended version of the Summers-Heston Penn

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[5] The evidence on the effects of social spending is mixed. Bidani and Ravallion (1997) do find a statistically significant impact of health expenditures on the poor (defined in absolute terms as the share of the population with income below one dollar per day) in a cross-section of 35 developing countries, using a different methodology. Gouyette and Pestiau (1999) find a simple bivariate association between income inequality and social spending in set of 13 OECD economies. In contrast Filmer and Pritchett (1997) find little relationship between public health spending and health outcomes such as infant mortality, raising questions about whether such spending benefits the poor.

World Tables Mark 5.6.[6] In general, this need not be equal to the mean level of household income, due to a variety of reasons ranging from simple measurement error to retained corporate earnings. We nevertheless rely on per capita GDP for two pragmatic reasons. First, for many of the country-year observations for which we have information on income distribution, we do not have corresponding information on mean income from the same source. Second, using per capita GDP helps us to compare our results with the large literature on income distribution and growth that typically follows the same practice. In the absence of evidence of a systematic correlation between the discrepancies between per capita GDP and household income on the one hand, and per capita GDP on the other, we treat these differences as classical measurement error, as discussed further below.

We use two approaches to measuring the income of the poor, defined as the poorest 20% of the population, using an augmented version of the Deininger-Squire (1996) dataset.[7] This dataset reports Gini coefficients for a large number of countries and years, and five points on the Lorenz curve for a subset of these country-year observations. As noted by these and other authors there are substantial difficulties in comparing income distribution data across countries. Countries differ in the concept measured (income versus consumption), the measure of income (gross versus net), the unit of observation (individuals versus households), and the coverage of the survey (national versus subnational). We restrict attention to distribution data based on nationally representative sources identified as high-quality by Deininger and Squire (1996). We adjust the Gini coefficients and Lorenz curves for the remaining differences

[6] We begin with the Summers and Heston Penn World Tables Version 5.6, which reports data on real per capita GDP adjusted for differences in purchasing power parity through 1992 for most of the 156 countries included in that dataset. We use the growth rates of constant price local currency per capita GDP from the World Bank to extend these forward through 1997. For a further set of 29 mostly transition economies not included in the Penn World Tables we have data on constant price GDP in local currency units. For these countries we obtain an estimate of PPP exchange rate from the fitted values of a regression of PPP exchange rates on the logarithm of GDP per capita at PPP. We use these to obtain a benchmark PPP GDP figure for 1990, and then use growth rates of constant price local currency GDP to extend forward and backward from this benchmark. While these extrapolations are necessarily crude, they do not matter much for our results. As discussed below, the statistical identification in the paper is based primarily on within-country changes in incomes and incomes of the poor, which are unaffected by adjustments to the levels of the data.

[7] We use the version of the Deininger and Squire (1996) data set as augmented by Lundberg and Squire (1999). We are grateful to the latter authors for providing the data and for help with comparability adjustments. We add to this data a further 75 observations from the 1999 and draft 2000 World Development Indicators of the World Bank. We are grateful to Shaohua Chen for providing preliminary data from this last source.

using the procedure similar to that of Lundberg and Squire (1999).[8] This results in a set of distribution data that notionally measures the national distribution of household income for all countries and years.

Whenever Lorenz curve data are available we measure mean income in the poorest quintile directly, as the share of income earned by the poorest quintile times mean income, divided by 0.2. For those observations for which we have information on the Gini coefficient but not the Lorenz curve, we estimate mean income in the poorest quintile under the assumption that the distribution of income is lognormal. Given a lognormal distribution of income, it is possible to show that approximately:

(1) $$y^P = -\gamma \cdot G + y$$

where y^P denotes the logarithm of per capita income in the poorest quintile of the population; G denotes the Gini coefficient; y denotes the logarithm of average per capita income in the entire population; and γ=0.036 is a constant.[9] While this lognormal approximation is simple, it works surprisingly well. An OLS regression of this

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[8] Specifically, we regress the logarithm of the Gini coefficient on a full set of country dummies, and five dummies which take the value of one if (i) the survey measures net income; (ii) there is no information whether the survey measures gross or net income; (iii) the survey measures expenditure; (iv) the survey uses individuals rather than households as the unit of observation; and (v) there is no information on whether the survey uses households or individuals as the unit of observation. The results of this regression are shown in Table 1. Under the assumption that the Gini does not systematically change too much within countries over time, the estimated coefficients can be interpreted as the mean percentage difference between the corresponding type of Gini and a Gini based on household gross income. We then adjust all the non-household, non-gross income Ginis by these estimated coefficients. We adjust the income share of the bottom quintile using the same procedure. It is worth stressing that these adjustments are identified using only within-country changes in the concept measured. While this has an obvious appeal, the disadvantage is that there are relatively few within-country changes in the concept measured, and so these adjustments are not very precisely estimated and are very sensitive to the observations included in the regression. Fortunately, our main results do not appear to be very sensitive to the precise adjustments used. See Atkinson and Brandolini (1999) for a detailed discussion of the limitations of these types of adjustments, as well as additional caveats about the use of the Deininger-Squire dataset.

[9] If the distribution of income is lognormal, i.e. y~N(μ,σ), and the Gini coefficient on a scale from 0 to 100 is G, the standard deviation of this lognormal distribution is given by $\sigma = \sqrt{2} \cdot \Phi^{-1}\left(\frac{1+G/100}{2}\right)$ (Aitcheson and Brown (1966)). Using the properties of the mean of the truncated lognormal distribution (e.g. Johnston, Kotz and Balakrishnan (1994)) it can be shown that $y^P = y + \ln\left(\frac{\Phi\left(\Phi^{-1}(0.2) - \sigma\right)}{0.2}\right)$. Combining these two results and numerically linearizing the term involving G gives Equation (1) in the text. In the empirics, we rely on the exact, rather than the linearized, estimate of y^P. However, in most of the discussion we use the linearized version for clarity. Quah (1999) provides a number of similar results for the lognormal and other parametric distributions.

approximate measure of mean income in the poorest quintile on the corresponding measure derived from the Lorenz curve yields a slope coefficient of 1.05 and an R-squared of 0.97. In any case, observations based on this approximation constitute less than 15 percent of our sample. In what follows we check the robustness of our results to limiting our sample to observations based on the Lorenz curve alone.

It is worth stressing that the limited data we have at our disposal in a large panel of countries (at most, mean income in the bottom quintile) limits what we can say with any confidence about the very poorest in a country, for example, the bottom 10% or poorer of the income distribution. It is likely that there are interesting changes in the distribution of income within the bottom quintile, within countries and over time. To the extent that these within-quintile distributional changes are important, our measure of mean income in the bottom quintile will only be a noisy indicator of the well-being of the very poorest in a country. We do not have any good data which allow us to assess the importance of this in the large sample of countries and years we consider here. In the absence of such information we can do little more than treat this as an additional source of measurement error in mean incomes of the poor. [10]

A further difficulty with the data on income distribution is that it forms a highly unbalanced and irregularly spaced panel of observations. For some rich countries and a few developing countries a continuous time series of annual observations on income distribution is available for long periods. For most countries only one or a handful of observations are available. Since we are interested in growth over the medium- to long-run we do not want to rely on potentially adjacent annual observations in our estimation. There are two solutions to this problem. The most common is to average available distribution data over pre-specified periods such as decades or quinquennia. Other than convenience, we do not find this approach very compelling. The main difficulty is that it introduces noise into the timing of the distribution data and the other variables we consider. Since one of the most interesting of these, income growth, is very volatile, this mismatch in timing is potentially very serious. In addition, the argument that averaging

[10] A suggestive but hardly conclusive piece of evidence that this measurement error problem may not be too severe can be found in regressing the share of income accruing to the bottom 10% of the income distribution on the share of income in the bottom 20%, using the cross-section of countries reported in the 1999 WDR. This regression gives a slope coefficient of 0.99 and an R-squared of 0.97. This suggests that our results would not be very different if we were to instead define the poor as the bottom 10% of the income distribution.

over time smooths out measurement error in the income distribution data is probably overstated. For reasonably short periods such as quinquennia, there is often only one observation per period for many countries. Moreover, to the extent that measurement error reflects differences in the concepts measured by the survey, as discussed above, these are highly persistent and will not be smoothed by averaging over time.

We therefore prefer to follow Ravallion and Chen (1997) who instead work with an irregularly spaced panel of distribution data using the actual years to which the surveys refer. To avoid relying on adjacent annual observations or on growth over overlapping intervals, we filter the data as follows. For each country we begin with the first available distribution observation. Moving forward in time we then choose the next observation subject to the constraint that at least five years separate observations, until we have exhausted the available data for that country. This results in an unbalanced and irregularly spaced panel of 370 observations on mean income of the poor separated by at least five years within countries, of which 323 are based directly on the Lorenz curve and the remainder are estimated using a lognormal approximation. These data cover a total of 125 countries. In our econometric estimation (discussed in the following subsection) we restrict the sample further to the set of 236 observations covering 80 countries for which at least two spaced observations on mean income of the poor are available, so that we can consider within-country growth in mean incomes of the poor over periods of at least five years. When we consider the effects of additional control variables, the sample is slightly smaller and varies across specifications depending on data availability. The composition of the sample is shown in Table 2.

3.2 Estimation

We estimate variants of the following regression of the logarithm of per capita income of the poor on the logarithm of average per capita income:

(2) $$y_{ct}^{P} = \alpha_0 + \alpha_1 \cdot y_{ct} + \alpha_2 ' X_{ct} + \mu_c + \varepsilon_{ct}$$

where c and t index countries and years, respectively; X_{ct} is a vector of other determinants of mean income of the poor; and $\mu_c + \varepsilon_{ct}$ is a composite error term including unobserved country effects.[11] We have already seen the pooled version of Equation (2) with no control variables X_{ct} in the top panel of Figure 1 above.

We are interested in two key parameters from Equation (2). The first is α_1 which measures the elasticity of income of the poor with respect to mean income. A value of α_1=1 indicates that growth in mean income is translated one-for-one into growth in income of the poor. Estimates greater or less than one indicate that growth more than or less than proportionately benefits the poor. The second parameter of interest is α_2 which measures the impact of other determinants of income of the poor *over and above their impact on mean income*. Many of the variables in X we consider are known to be determinants of high income and/or growth in income across countries. Since mean income is already in the regression, the parameter α_2 measures any impact on incomes of the poor over and above their impact on mean income.

Using Equation (1), we can equivalently write Equation (2) as a regression with the Gini coefficient (or some other measure of income distribution) as the dependent variable, and $(\alpha_1 - 1) \cdot y_{ct}$ on the right-hand side. Finding an estimate of α_1=1 is equivalent to finding that the level of inequality does not vary systematically with the level of income. In this respect our work is closely related to the large literature on the determinants of inequality. Given the striking absence of any correlation between (changes in) income and (changes in) inequality documented by, among others, Ravallion and Chen (1997) and Deininger and Squire (1996), finding an estimate of α_1=1 should not be very surprising. Our contribution to this literature is twofold. First, to our knowledge this is the largest, in terms of country and period coverage, assessment of changes in income and changes in income distribution. Second, after establishing that α_1 is very close to one, we turn our attention to deviations from this relationship and systematically attempt to relate them to other determinants of growth and poverty in this large sample of countries.

[11] It is straightforward to generalize the discussion to include year effects. We do not do so here because in our empirical results we do not find time effects to be significant.

Simple ordinary least squares (OLS) estimation of Equation (2) is likely to result in inconsistent parameter estimates for (at least) three reasons: measurement error, omitted variables, and reverse causation from incomes of the poor to mean income. We discuss each of these in turn.

Measurement Error

As is well-known, classical measurement error in y or X can lead to biases that are difficult to sign except under certain very restrictive assumptions. A more important concern here is that measurement error in mean incomes of the poor may be correlated with measurement error in mean income, which can introduce further biases. A priori this concern is quite reasonable -- after all we are basing our estimates of mean income of the poor and mean income on the same per capita GDP data, which is certainly prone to measurement error. Upon closer inspection, however, this need not concern us greatly, for two reasons. First, as discussed below, we estimate Equation (2) using instruments that can in principle mitigate problems of measurement error. Second, even if we were simply to estimate Equation (2) by OLS, under plausible assumptions measurement error in mean incomes of the poor and mean income “cancel” and OLS is still consistent. We present this argument (which is based on Ravallion and Chen (1997)) in a short appendix.

Omitted Variable Bias

In our empirical work we will be using a fairly parsimonious specification of the determinants of income of the poor in the vector X_{ct}. This raises the possibility that there are omitted variables that affect the income of the poor and are also correlated with either mean income or with the included variables in the vector X_{ct}. Depending on the sign of this correlation our estimates of the impact of these variables on incomes of the poor could be biased up or down. To the extent that our instruments are uncorrelated with these omitted variables, we can mitigate their effect. Moreover, we can test the validity of the assumption that the instruments are uncorrelated with these omitted variables using standard tests of overidentifying restrictions.

Endogeneity

As noted by a variety of authors, most notably Lundberg and Squire (1999), inequality, income and growth may be jointly determined and should therefore be considered as a system. This highlights the possibility that there may be reverse causation from incomes of the poor to mean income, through a variety of channels advanced in the literature on why inequality might be bad (or good) for incomes and growth. We formalize this potential difficulty as follows. Suppose that mean income depends on its lagged value, on lagged incomes of the poor, as well as other variables, in the following growth regression:

$$(3) \qquad y_{ct} = \beta_0 + \beta_1 \cdot y_{c,t-k(c,t)} + \beta_2 \cdot y^P_{c,t-k(c,t)} + \beta_3 ' Z_{ct} + \eta_c + \gamma_t + v_{ct}$$

where Z_{ct} is a vector of determinants of growth which may or may not include some of the variables in X_{ct} in Equation (2). Equation (3) differs from a standard growth regression only in that we consider growth in mean incomes over irregularly spaced intervals corresponding to our irregularly spaced data on income distribution. In particular, subtracting $y_{c,t-k(c,t)}$ from both sides of Equation (3), we have growth in country c between years t-k(c,t) and t as the dependent variable, and initial income at the beginning of the period, initial income of the poor at the beginning of the period, and a vector of other growth determinants as explanatory variables. Substituting in Equation (1) gives the more familiar (from the growth literature) formulation with initial inequality as one of the explanatory variables for growth in the subsequent period.

It is clear from Equation (3) that as long as β_2 is not equal to zero, OLS estimates of Equation (2) are inconsistent. For example, high realizations of μ_c which result in higher incomes of the poor relative to mean income in Equation (2) will also raise (lower) mean incomes in Equation (3), depending on whether β_2 is greater than (less than) zero. This could induce an upwards (downwards) bias into estimates of the elasticity of incomes of the poor with respect to mean incomes in Equation (2). How important this problem is a priori depends greatly on how robust is the existing evidence on inequality as a determinant of growth. From our reading of this literature, we are somewhat skeptical of the robustness of these results. On the one hand, Perotti (1996) and Barro (1999) find evidence of a negative effect of income inequality on growth, the latter only after interacting with the level of income. On the other hand, Forbes (2000) and Li and Zou (1998) both find positive effects of income inequality on growth. Finally, Bannerjee and Duflo (1999) modestly, and perhaps most appropriately, conclude that there is at best weak evidence of a U-shaped correlation between income inequality and growth and that very little can be said about causation in either direction. Nevertheless, we do not want to discard outright the possibility of reverse causation from mean incomes of the poor to mean income, and so we treat mean income as endogenous when estimating Equation (2). Again, this points to the need for suitable instruments for per capita income in Equation (2).

A final issue in estimating Equation (2) is whether we want to identify our effects of interest using the cross-country or the time-series variation in the data on incomes of the poor, mean incomes, and policies. An immediate reaction to the presence of unobserved country-specific effects μ_c in Equation (2) is to estimate it in first differences.[12] The difficulty with this option is that it forces us to identify our effects of interest using the more limited time-series variation in incomes and income distribution.[13] This raises the possibility that the signal-to-noise ratio in the within-country variation in the data is too unfavorable to allow us to estimate our parameters of interest with any precision. In contrast, the advantage of estimating Equation (2) in levels is that we can exploit the large cross-country variation in incomes, income distribution, and policies to identify our effects of interest. The disadvantage of this approach is that the problem of omitted variables is more severe in the cross-section, since in the differenced estimation we have at least managed to dispose of any time-invariant country-specific sources of heterogeneity.

Our solution to this dilemma is to implement a system estimator that combines information in both the levels and changes of the data.[14] In particular, we first difference Equation (2) to obtain growth in income of the poor in country c over the period from t-

[12] Alternatively one could enter fixed effects, but this requires the much stronger assumption that the error terms are uncorrelated with the right-hand side variables at all leads and lags.
[13] Li, Squire, and Zou (1998) document the much greater variability of income distribution across countries compared to within countries. In our sample of irregularly-spaced observations, the standard deviation of the Gini coefficient pooling all observations in levels is 9.4. In contrast the standard deviation of changes in the Gini coefficient is 4.7 (an average annual change of 0.67 times an average number of years over which the change is calculated of 7).
[14] This type of estimator has been proposed in a dynamic panel context by Arellano and Bover (1995) and evaluated by Blundell and Bond (1998).

k(c,t) to t as a function of growth in mean income over the same period, and changes in the other X variables:

$$\text{(2')} \qquad y_{ct}^{P} - y_{c,t-k(c,t)}^{P} = \alpha_1 \cdot \left(y_{ct} - y_{c,t-k(c,t)}\right) + \alpha_2 ' \left(X_{ct} - X_{c,t-k(c,t)}\right) + \left(\varepsilon_{ct} - \varepsilon_{c,t-k(c,t)}\right)$$

We then estimate Equation (2) and Equation (2') as a system, imposing the restriction that the coefficients in the levels and differenced equation are equal. We address the three problems of measurement error, omitted variables, and endogeneity by using appropriate lags as instruments. In particular, in Equation (2) we instrument for mean income using growth in mean income over the five years prior to time t. This preceding growth in mean income is by construction correlated with contemporaneous mean income, provided that β_1 is less than one in Equation (3). Given the vast body of evidence on conditional convergence, this assumption seems reasonable a priori. We can test the strength of this correlation by examining the corresponding first-stage regressions. Differencing Equation (3) it is straightforward to see that past growth is also uncorrelated with the error term in Equation (2), provided that ε_{ct} is not correlated over time. In Equation (2') we instrument for growth in mean income using the level of mean income at the beginning of the period, and growth in the five years preceding t-k(c,t). Both of these are by construction correlated with growth in mean income over the period from t-k(c,t) to t. Moreover it is straightforward to verify that they are uncorrelated with the error tem using the same arguments as before.

In the version of Equation (2) without control variables, these instruments provide us with three moment conditions with which to identify two parameters, α_0 and α_1. We combine these moment conditions in a standard GMM estimation procedure to obtain estimates of these parameters. In addition, we adjust the standard errors to allow for heteroskedasticity in the error terms as well as the first-order autocorrelation introduced into the error terms in Equation (2') by differencing. Since the model is overidentified we can test the validity of our assumptions that the instruments are uncorrelated with the error terms using tests of overidentifying restrictions.

When we introduce additional X variables into Equation (2) we also need to take a stand on whether or not to instrument for these as well. On a priori grounds, difficulties with measurement error and omitted variables provide as compelling a reason to instrument for these variables as for income. Regarding reverse causation the case is less clear. It seems implausible that many of the macro variables we consider respond endogenously to relative incomes of the poor. In what follows we choose not to instrument for the X variables. This is in part for the pragmatic reason that this further limits our sample size. More importantly, we take some comfort from the fact that tests of overidentifying restrictions pass in the specifications where we instrument for income only, providing indirect evidence that the X variables are not correlated with the error terms. In any case, we find qualitatively quite similar results in the smaller samples where we instrument, and so these results are not reported for brevity.

Before proceding to the results, it is useful to contrast our empirical strategy with other approaches to understanding sources of growth in incomes of the poor. One obvious candidate is to estimate Equations (2) and (3) simultaneously. This is roughly the approach taken by Lundberg and Squire (1999), who estimate reduced-form equations for growth and inequality that could be derived from a system like Equations (2) and (3).[15] We prefer the single-equation approach simply because of the possibility that misspecification in the growth equation may taint inferences regarding coefficients of interest in the equation for incomes of the poor. Given the prodigous econometric difficulties with this equation alone described above, we are reluctant to introduce another potential source of difficulty into our main equation of interest.

A second approach to understanding determinants of income of the poor that has gained some popularity recently is to estimate separate growth regressions for each quintile of the income distribution (e.g. Anderson and Knack (1999), Lundberg and Squire (1999)). Equation (3) is a standard growth regression for mean income. We noted that income of the poor is linked to mean income and the Gini coefficient through Equation (1). If we substitute from (1) into (3), we can derive the following growth regression for the income of the poorest quintile:

[15] The caveat "roughly" applies for a two reasons. First, they express income distribution as a function of growth in average incomes. In contrast, substituting Equation (1) into Equation (2) we have income distribution as a function of the level of average income. Second, they do not allow initial income in their panel growth regression to vary by period, so that their growth equation is not a true dynamic panel like Equation (3).

(4) $$y^P{}_{ct} = \beta_0 + (\beta_1 + \beta_2) \cdot y^P_{c,t-k(c,t)} + \beta_3{}' Z_{ct} - \gamma \cdot (G_{c,t} - \beta_1 \cdot G_{c,t-k(c,t)}) + \eta_c + \gamma_t + v_{ct}$$

The important point here is that income inequality needs to be included in such a growth regression for the poor in order for it to be consistent with Equation (1) – an identity – and Equation (3) – a standard growth regression. This is true even if income of the poor (or income inequality) is not one of the "determinants" of growth in average income in Equation (3). Existing papers we have seen estimate Equation (4) omitting the terms involving the change in the Gini coefficient, and then test whether the coefficients on the other determinants of growth in Z_{ct} are different across quintiles. Comparing Equation (4) excluding the terms involving the Gini coefficient and Equation (3), it is clear that the coefficients on the growth determinants will be different if the change in the Gini coefficient is a linear combination of the level of policies. Thus, this strategy of estimating Equation (4) without the change in Gini is an indirect test of whether the change in the Gini coefficient is a linear combination of policies. We have two problems with this approach. First, estimating a misspecified version of Equation (4) seems an inefficient way to test the hypothesis that changes in Ginis are linearly related to levels of policies, when that hypothesis could be explored directly. Second, the hypothesis that changes in Ginis are a function of the level of policies seems a priori unlikely to us. If there is a one-time opening up to foreign trade, would we expect this to lead to a one-time change in inequality or to place the economy on a path where the Gini changes period after period? Policies are relatively stable in rich countries, and we do not observe clear trends in Gini coefficients, so it seems unlikely to us that this is the right model. In our model we link the level of inequality to the level of policies and changes in equality to changes in policy. If one wants to explore the other model, estimating growth regressions for the poor is an inefficient way to do that.

4. Results

4.1 Growth is Good for the Poor

We start with our basic specification in which we regress the log of per capita income of the poor on the log of average per capita income, without other controls (Equation (2) with α_2=0) The results of this basic specification are presented in detail in Table 3. The five columns in the top panel provide alternative estimates of Equation (2), in turn using information in the levels of the data, the differences of the data, and finally our preferred system estimator which combines the two. The first two columns show the results from estimating Equation (2) in levels, pooling all of the country-year observations, using OLS and 2SLS respectively. OLS gives a point estimate of the elasticity of income of the poor with respect to mean income of 1.06, which is (just) significantly greater than 1. As discussed in the previous section there are reasons to doubt the simple OLS results. When we instrument for mean income using growth in mean income over the five preceding years as an instrument, the estimated elasticity falls somewhat to 0.96. However, this elasticity is much less precisely estimated, with a standard error that jumps by a factor of 10 relative to the OLS estimates. This primarily reflects the fact that lagged growth (in this simple specification) is not a particularly strong instrument for the level of income. This is documented in the first column of the bottom panel, which shows the first-stage regression of the log-level of per capita GDP on a constant and lagged growth. Lagged growth is not very significant, although it significance does vary somewhat across subsamples and generally performs better than in the case shown here.

The results which use information in the differences of the data look much more promising. The third and fourth columns in the top panel of Table 3 show the results of OLS and 2SLS estimation of the differenced Equation (2'). We obtain a point estimate of the elasticity of income of the poor with respect to mean income of 1.02 using OLS, and a slightly larger elasticity of 1.06 when we instrument using lagged levels and growth rates of mean income. The estimated standard errors are somewhat larger than in the the OLS levels estimation, and so we cannot reject the null hypothesis that the elasticity is equal to one in either the OLS or 2SLS results. The much greater precision of the 2SLS differenced estimation in part reflects the much better performance of our instruments in this case. The second column of the bottom panel reports the corresponding first-stage regression, in which both instruments are highly significant and an F-test of joint insignificance strongly rejects the null. This gives us some confidence that the relatively similar OLS and 2SLS differenced results are not an artifact of weak instruments. Moreover, the differenced equation is overidentified. When we test the validity of the overidentifying restrictions we do not reject the null of a well-specified model for the differenced equation alone at conventional significance levels.

In the last column of Table 3 we combine the information in the levels and differences in the system GMM estimator, using the same instruments as in the single-equation estimates reported earlier. The system estimator delivers a point estimate of the elasticity of 1.05, which is not significantly different from 1. Since the system estimator is based on minimizing a precision-weighted sum of the moment conditions from the levels and differenced data, it is not surprising that the estimates are quite similar to the (relatively precisely estimated) differenced estimator. This general pattern of more precise estimation in differences persists in most of the variants on this basic regression that we discuss in the rest of the paper. Despite this, we prefer to rely on the system estimator since it is the only way to obtain (admittedly imprecise) estimates of the effects of some of our regressors which vary only across countries and not over time. Finally, it is worth noting that since our system estimator is overidentified, we can test and do not reject the null that the instruments are valid, in the sense of being uncorrelated with the corresponding error terms in Equations (2) and (2').

We then proceed to a series of robustness checks. First, we add regional dummies to the levels equation, and find that dummies for Latin America, Sub-Saharan Africa, and the Middle East and North Africa are significant. Their inclusion results in a point estimate of the elasticity of 1.01 with a standard error of 0.10, as shown in the second row of Table 4. We keep the regional dummies in all subsequent regressions. The other robustness checks involve dropping all of the observations in which we use the lognormal approximation to estimate the income of the poor and dropping the observations in which there is a shift from an expenditure-based survey to an income-based one (or vice versa). In both of these specifications the point estimates remain close to and insignificantly different from 1.0.

To investigate a number of hypotheses about how the growth-poverty relationship might vary in different situations, we introduce a variety of interaction terms into the basic regression, as discussed in more detail in Section 2. We find that the elasticity of incomes of the poor with respect to mean income does not differ between low-income observations and high-income ones; between high growth observations and low-growth ones; between negative growth observations and positive growth ones; and between 1960s-70s, on the one hand, and the 1980s-90s, on the other. Not only are the estimates not statistically different, but the point estimates in each of these comparisons are nearly identical to eachother and also to one.

4.2 Good Macro Policies are Good For the Poor

In general, the relationship between growth of income of the poor and overall economic growth is one-to-one. That finding suggests that policies that are good for growth will be equally good for the poor. However, it is possible that growth from different sources has differential impact on the poor. In this section we take a number of the policies that have been identified as pro-growth in the empirical growth literature and investigate whether they have differential impact on income of the poor. The four policy indicators that we focus on are inflation, which Fischer (1993) finds to be bad for growth; government consumption, which Easterly and Rebelo (1993) also find to be bad for growth; exports and imports relative to GDP, which Frankel and Romer (1999) find to be good for growth; and a measure of the strength of property rights or rule of law The particular measure is from Kaufmann, Kraay, and Zoido-Lobatón (1999). The importance of property rights for growth has been established by, among others, Knack and Keefer (1995).

First, we take the basic regression from Table 3 and add these policy indicators one at a time (Table 5). It is important to emphasize that mean income is included in each of these regressions, so that the effect of policies working through overall growth is captured there. The coefficient on the policy indicator captures any differential impact that the policy has on the income of the poor. So, for example with openness: the hypothesis that openness is less good for the poor suggests that the coefficient in this regression will be negative. It is in fact positive, though not statistically different from zero. The same is true for the measure of rule of law. In the case of inflation and government spending, both enter negatively, although not significantly. The point estimates indicate that both higher inflation and higher government have adverse effects on incomes of the poor over and above their effects on mean income. If we put all of the policy variables in together, the pernicious effect of inflation on the incomes of the poor becomes statistically significant as well.

4.3 Globalization Is Good for the Poor

One possibly surprising result in Table 5 is the lack of any evidence of a significant negative impact of openness to international trade on incomes of the poor. In addition to the often vociferous popular claims to the contrary, recent empirical work by Lundberg and Squire (1999) finds some evidence that openness lowers income growth in the bottom quintile. We therefore investigate the robustness of our basic finding in more detail. Following Frankel and Romer (1999), we use exports plus imports relative to GDP as a measure of openness. Their careful work establishes that this measure of openness can be treated as an exogenous variable in growth analysis and that it has an important effect on growth. We take it as a measure of the extent to which the real economy is open as a result of country characteristics (such as size and location) and of policy. We repeat our result on trade openness and income of the poor in the first column of Table 6: there is a positive and insignificant coefficient on trade openness. The Frankel and Romer result combined with ours indicates that trade openness is good for the poor: it increases mean income and the poor benefit one-for-one.

One possible reason for the difference between the Lundberg and Squire (1999) results and ours is that they use the openness indicator developed by Sachs and Warner (1995). Rodriguez and Rodrik (1999) argue that this indicator is not capturing trade openness per se, but rather “serves as a proxy for a wide range of policy and institutional differences” (p .16). Nevertheless, it is widely used in the literature, and Lundberg and Squire (1999) find that it is related to growing inequality. It is useful to establish whether the different result that we find comes from the different openness measure. If we replace exports plus imports relative to GDP with the Sachs-Warner measure, the sign of the coefficient does shift to negative but it is not statistically different from zero, as shown in the second column of Table 6. It remains negative and becomes even less significant if we include the other policy variables from Table 5 into the regression (third column). Another possible source of discrepancy is that Lundberg and Squire (1999) restrict attention to a sample that is roughly half as large as ours (119 observations covering 38 countries) owing to the limited availability of some of their preferred control variables, notably land inequality. In order to verify that the choice of countries is not driving the results, we reestimate our regression in the same set of 38 countries they consider. The results are virtually identical with those in our larger sample. Finally, we include the control variables in this limited sample and again find no effect of the Sachs-Warner measure. There are several other differences between the Lundberg and Squire (1999) approach and ours which may account for the differences in our findings.[16] Nevertheless, based on our results we do not find the argument that trade openness is bad for the poor to be very compelling. If one accepts – as we do – the Frankel and Romer (1999) results on the large positive effects of openness on per capita incomes, then trade openness is good for income of the poor, and good to the same extent as it is for the average household.

The current debate about globalization also involves openness of economies to capital flows. The East Asia crisis has focused attention on the potential for international capital flows to contribute to exchange rate and banking crises that have sharp ramifications for the real economy. It is therefore reasonable to ask whether increased openness to financial flows has any adverse effects on incomes of the poor. We measure this using an indicator from the IMF that denotes presence of capital controls. Existing evidence in the literature has not shown much effect from financial openness to growth.[17] Therefore, the only way it should affect incomes of the poor is through its effects on income distribution. We find that capital controls have a small negative effect on income of the poor, but one that is not significantly different from zero. Thus, we do not find any evidence that capital account liberalization is anti-poor.

[16] They use quinquennial average data and a parametric correction for within-country error persistence that is different from our non-parametric approach. In addition, their strongest findings on the adverse effects of openness are based on growth regressions by quintiles of the income distribution, which we have argued above are an indirect test of the effects of policies on incomes of the poor.

[17] See for example Grilli and Milesi-Ferretti (1995) and Kraay (1998).

4.4 Other Poverty Policies?

Next we add to the basic regression three variables that are commonly thought to influence the extent to which the poor share in economic growth: democracy, government social spending as a share of total public spending, and the primary school enrollment rate. Of these three, only primary schooling is generally found to be a determinant of growth in per capita incomes, while the other two exhibit little robust association with overall growth. However, these policies may be especially important for the poor. Consider for example primary enrollment rates. Most of the countries in the sample are developing countries in which deviations from complete primary school enrollments are most likely to reflect the low enrollment among the poorest in society. This in turn may be an important factor influencing the extent to which the poor participate in growth.

We add these variables one at a time, with and without the macro policies included (Table 7). The coefficients on the macro policies do not change significantly and are not reported again. The democracy index has a positive coefficient, reflecting a positive distributional impact of this variable. However, the coefficient is not statistically significantly different from zero, especially when the macro policies are included. Social spending has a negative coefficient, but again one that is not significant. Since the tests of overidentifying restrictions perform poorly in this specification we interpret the results with some caution. Nevertheless, this result should come as no surprise: the variable is the share of government spending devoted to health and education. These social expenditures often benefit the middle class and the rich primarily. The share of public spending devoted to the social sectors is in no way an indicator of whether government policy and spending is particularly pro-poor. In these regressions the primary school enrollment rate is also not significant. Thus, none of these variables can help explain whether or not growth is particularly pro-poor.

4.5 Growth and Distribution Effects

In drawing inferences about the impact of policies on the income of the poor, we have referred to results from the larger growth literature that we view as robust. Our data set is not ideal for estimating growth regressions because it is irregularly spaced and because country coverage is limited by availability of distribution data. Nevertheless, it is useful to show that the main results from the growth literature are present in the data we consider. Estimates of the effects of policies on growth in this data set can then be combined with our estimated effects on distribution (all of which are small) to provide estimates of the impact of different policies on income of the poor.

For convenience, we repeat our equation for income of the poor, including all of the policies, in the first column of Table 8. (We omit government social spending because this policy variable did not enter significantly in our earlier results and also sharply reduced the sample size.) In the second column we report the growth regression that we estimate with the system estimator in this data set. In particular, we estimate Equation (3), under the assumption that the explanatory variables for growth in Z_{ct} coincide with the explanatory variables for incomes of the poor in Equation (2). This regression is broadly consistent with the literature: higher growth is associated with good rule of law, more trade, lower government consumption, and the absence of high inflation. This regression also reflects the reality that the good policies tend to go together, so that if we put them all into a growth regression the individual significance is modest. We have a fair amount of confidence that the overall package promotes growth, and a weak ability to estimate the impact of individual policies. As in other work, neither democracy nor primary school enrollment has much relationship with growth.

We then use these coefficients to estimate the long-term effect of a one standard deviation change in each policy variable. The thought experiment here is to permanently raise each of the policy variables by one standard deviation and then calculate the long-term effect on mean income. We can then use the coefficients in the first column to calculate the change in long-term income of the poor that arises indirectly through growth and directly through the impact of the policy change on the distribution of income. In particular, from Equation (2) we can write the change in mean incomes of the poor induced by a change in one of the policy variables as:

$$(5) \qquad \frac{\partial y_{ct}^{P}}{\partial X_{ct}} = \frac{\partial y_{ct}}{\partial X_{ct}} + \left((\alpha_1 - 1) \cdot \frac{\partial y_{ct}}{\partial X_{ct}} + \alpha_2 \right)$$

The first term captures the effect on incomes of the poor of a change in one of the determinants of growth, holding constant the distribution of income. We refer to this as the "growth effect" of policies. The second term captures the effects of a change in one of the determinants of growth on incomes of the poor through changes in the distribution of income. This consists of two pieces: the difference between the estimated income elasticity and one, and the direct effects of policies on incomes of the poor in Equation (2). Finally, iterating forward Equation (3) it is easy to see that the long-run effect of X_{ct} on the level of mean incomes is given by $\frac{\partial y_{ct}}{\partial X_{ct}} = \frac{\beta_1}{1-\beta_1} \cdot \beta_2$. Since in a conventional growth regression policies are assumed to affect the *growth rate* of income, a permanent improvement in one of these policies has a large effect on the *level* of income.

The results of this decomposition are shown in the last three columns of Table 8. The third column indicates the magnitude of a one standard deviation increase in each of the variables, based on the pooled sample of observations. The remaining columns report the growth and distribution effects of these one-standard deviation increases. The results are also summarized graphically in Figure 3. The main story here is that the growth effects are large and the distribution effects are small. A one standard deviation improvement in rule of law, government consumption (i.e., less consumption), and inflation (i.e. lower inflation) in each case leads to about a 50% increase in income of the poor in the long run. The point estimate for more trade openness is at the low end of existing results in the literature: about a 10% increase in income from a one standard deviation increase in openness. This should therefore be viewed as a rather conservative estimate of the benefit of openness on incomes of the poor. It should also be emphasized that although the point estimates of the growth effects of these policies are not that precisely estimated in this specification, they are broadly consistent with results in the empirical growth literature. We therefore have confidence in their rough order of magnitude and also in the significance of the overall package.

In contrast, the effects of these policies that operate through their effects on changes in the distribution of income are much smaller in magnitude. We have already seen that, with the exception of inflation in some specifications, the distributional effects of these policies are statistically insignificantly different from zero. Interestingly, both the direct and indirect effects of the democracy and primary enrollment on incomes of the poor are small in magnitude and are not significantly different from zero.

4. Conclusions

It should come as no surprise that the general relationship between growth of income of the poor and growth of mean income is one-to-one. Furthermore, other work has established that there is no generalized Kuznets curve (growth benefiting the poor less than one-for-one in the early stage of development, and more than one-for-one later).

What is new here is showing that a number of popular ideas about the poverty-growth nexus are *not* supported by empirical evidence. In particular,

- The poverty-growth relationship is *not* different in negative growth (crisis) episodes and normal growth periods;
- The poverty impact of growth has *not* declined in recent decades;
- Growth spurred by open trade or other macro policies (good rule of law, low government consumption, macro stability) benefits the poor as much as it does the typical household; and
- Growth of income of the poor is not influenced by formal democratic institutions.

This does not imply that growth is *all* that is needed to improve the lives of the poor. Rather, these findings leave plenty of room for further work, because they emphasize the fact that we know very little about what systematically causes changes in the distribution of income. What we do learn is that growth generally does benefit the poor and that anyone who cares about the poor should favor the growth-enhancing policies of good rule of law, fiscal discipline, and openness to international trade.

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Appendix: Measurement Error

In this appendix we show that under plausible assumptions, measurement error in mean income and income of the poor need not lead to inconsistent OLS estimates of α_1. The key step in the argument is drawn from Ravallion and Chen (1997). Suppressing country and period subscripts, let u_1 denote classical measurement error in y. Since mean income of the poor in logs is the sum of mean income in logs and a measure of distribution, let u_1+u_2 denote classical measurement error in y^P, where u_2 is measurement error in the distribution of income. We assume $E[u_1]=0$, $E[u_2]=0$, $V[u_1]=\sigma_1^2$, and $V[u_2]=\sigma_1^2$. In the absence of other right-hand side variables in Equation (2), it is straightforward to show that the OLS estimate of α_1 converges to

$$\text{(A1)} \qquad \text{p}\lim \hat{\alpha}_1 = \alpha_1 \cdot (1-\phi) + \left(1 + \frac{COV[u_1, u_2]}{V[u_1]}\right) \cdot \phi$$

where $\phi = \frac{V[u_2]}{V[y+u_2]}$ is the ratio of the variance of measurement error in mean income to the variance of observed mean income. If measurement errors in mean income and income distribution are uncorrelated, then Equation (A1) simplifies to:

$$\text{(A2)} \qquad \text{p}\lim \hat{\alpha}_1 = \alpha_1 + \phi \cdot (1-\alpha_1)$$

Under the null hypothesis that if $\alpha_1=1$, OLS is consistent. However, OLS estimates are biased downwards towards one if $\alpha_1>1$, and upwards towards one if $\alpha_1<1$. Are these biases empirically important? If they were we should expect to find that our instrumented estimates of α_1 tend to be further away from one than the uninstrumented ones. In most specifications we do not find this, despite the fact that we have quite strong instruments in most specifications. This leads us to believe that this type of measurement error is not responsible for our results.

One possible explanation for this is the following. Suppose, as do Ravallion and Chen (1997), that an x% mismeasurement of mean income leads to the same mismeasurement of income distribution as if true income were x% higher. This allows us to write

(A3) $$u_2 = (\alpha_1 - 1) \cdot u_1$$

so that $\frac{COV[u_1, u_2]}{V[u_1]} = (\alpha_1 - 1)$. Substituting into Equation (A1) we see that OLS estimates of E quation (2) will be consistent if measurement error is of this plausible form. This may explain the absence of any major differences between our instrumented and uninstrumented estimates of α_1.

Table 1: Adjustments to Gini Coefficients and Income Shares

	Gini Coefficient		Income Share of Bottom Quintile	
	Coefficient	Std Err	Coefficient	Std Err
Expenditure	-0.041	0.024	0.045	0.043
Net Income	-0.087	0.024	0.016	0.049
Unknown Income	0.044	0.019	-0.088	0.039
Household Unit	0.069	0.014	-0.047	0.029
Unknown Unit	0.033	0.018	0.005	0.033

Notes: This table presents the results of a fixed-effects regression of the logarithm of indicated variable on dummy variables which take the value of one if the underlying survey measures expenditure, net income, or no information is available; and if the survey uses the household as the unit of observation or the unit of observation is unknown. The estimated coefficients can be interpreted as the percentage difference in the indicated measure of distribution that can be attributed to deviations from the benchmark of an expenditure based gross income survey in which individuals are the unit of observation. The gini coefficients and quintile shares are adjusted by the estimated coefficients. Country fixed effects are not reported. The regressions cover 814 and 725 pooled country-year observations, respectively.

Table 2: Data Sources

	Source	Number of Observations		
		Total	Spaced Sample	Changes
1	Deininger and Squire High Quality Sample	693	311	200
2	Lundberg and Squire (1999) Additions	68	44	23
3	World Development Report (1999)	19	9	6
4	World Development Report (2000)	56	28	16

Notes: This table shows the four sources of data on income distribution on which we rely to construct estimates of mean incomes of the poor. Total refers to the total number of annual observations. Spaced sample refers to observations separated by at least five years from eachother within countries. Changes refers to the source of the final year for each pair of observations for which it is possible to construct a five-year change within countries in incomes of the poor.

Table 3: Basic Specification

Estimates of Growth Elasticity

	Levels OLS	Levels IV	Differences OLS	Differences IV	System
Intercept	-1.728	-0.858			-1.613
	(0.231)	(3.303)			(0.851)
Slope	1.060	0.956	1.019	1.059	1.046
	(0.027)	(0.392)	(0.071)	(0.107)	(0.102)
P-OID				0.280	0.518

First-Stage Regressions for System

	Dependent Variable: ln(Income)	Dependent Variable: Growth
Intercept	8.359	
	(0.070)	
Lagged Growth	0.458	
	(0.313)	
Lagged Income		0.010
		(0.002)
Twice Lagged Growth		0.288
		(0.106)
F-Stat	1.179	7.126
P-Zero Slopes	0.240	0.008

Notes: The top panel reports the results of estimating Equation (2) (columns 1 and 2), Equation (2') (columns 3 and 4), and the system estimator combining the two (column 5). OLS and IV refer to ordinary least squares and instrumental variables estimation of Equations (2) and (2'). The bottom panel reports the corresponding first-stage regressions for IV estimation of Equations (2) and (2'). The row labelled P-OID reports the P-value associated with the test of overidentifying restrictions. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Table 4: Variants on the Basic Specification

(Dependent Variable is ln(Per Capita Income of the Poor))

	Estimate	Standard Error	# Obs	P-Value for Ho: a1=1
Basic	1.046	0.102	236	0.65
Regional Dummies	1.009	0.095	236	0.92
No Lognormal Approximations	0.964	0.098	208	0.71
No Change in Income/Expenditure Gini	1.031	0.097	218	0.75
High Income	1.056	0.088	121	0.52
Low Income	1.065	0.104	115	0.53
High Growth	1.03	0.08	119	0.71
Low Growth	1.036	0.08	117	0.65
Positive Growth	1.079	0.076	204	0.30
Negative Growth	1.089	0.077	32	0.25
1960s and 1970s	1.01	0.084	70	0.91
1980s and 1990s	1.017	0.079	166	0.83

Notes: This table reports the results of estimating Equations (2) and (2') as a system, with no control variables, and with the indicated subsamples. None of these sample splits result in statistically significant estimates of the elasticity of income of the poor with respect to mean income. The last column reports the p-value associated with a test of the null hypothesis that the elasticity of income of the poor with respect to mean income is equal to one in the indicated subsample. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Table 5: Pro-Growth Policies and the Poor

(Dependent Variable is ln(Per Capita Income of the Poor))

	(1)	(2)	(3)	(4)	(5)
ln(Per Capita Income)	1.055	1.063	1.021	1.042	1.104
	(0.102)	(0.088)	(.080)	(.092)	(.094)
(Exports+Imports)/GDP	0.004				-0.004
	(0.055)				(.013)
ln(1+Inflation)		-0.134			-0.021
		(0.089)			(.012)
Government Consumption/GDP			0.0001		0.0005
			(.0001)		(.001)
Rule of Law				0.005	-0.041
				(.067)	(.065)
# of Observations	213	232	214	235	210
P-OID	0.166	0.384	0.208	0.271	0.499

Notes: This table reports the results of adding the indicated control variables to the system estimator of Equations (2) and (2'). The row labelled P-OID reports the P-value associated with the test of overidentifying restrictions. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Table 6: Openness and the Poor

(Dependent Variable is ln(Per Capita Income of the Poor))

	Basic	S-W Index	S-W Index with Controls	L-S Sample	L-S Sample with Controls	Capital Account Restrictions
ln(Per Capita Income)	1.055	1.078	1.155	1.127	0.903	1.021
	(0.102)	(.064)	(0.096)	(0.122)	(0.125)	(0.115)
(Exports+Imports)/GDP	0.004					
	(0.055)					
Sachs-Warner Index		-0.071	-0.058	-0.061	0.029	
		(0.047)	(0.045)	(0.054)	0.035	
Capital Account Restrictions						-0.013
						(0.065)
# of Observations	213	214	199	122	101	189
P-OID	0.166	0.326	0.649	0.154	0.041	0.073

Notes: This table reports the results of adding the indicated control variables to the system estimator of Equations (2) and (2'). The row labelled P-OID reports the P-value associated with the test of overidentifying restrictions. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Table 7: Pro-Poor Policies and the Poor

(Dependent Variable is ln(Per Capita Income of the Poor))

	Without Growth Policies			With Growth Policies		
ln(Per Capita Income)	1.006	1.138	1.126	1.101	1.225	1.201
	(0.094)	0.103	(0.097)	(.092)	(0.121)	(0.096)
Voice	0.058			0.069		
	(0.059)			(0.062)		
Social Spending/Total Spending		-1.027			-0.509	
		(0.539)			(0.630)	
Primary Enrollment			-0.091			-0.07
			0.131			(0.133)
# of Observations	233	105	205	208	103	193
P-OID	0.373	0.067	0.18	0.593	0.033	0.67

Notes: This table reports the results of adding the indicated control variables to the system estimator of Equations (2) and (2'). The first three columns report results including only regional dummies. The last three columns include the policy variables in Table 5. The row labelled P-OID reports the P-value associated with the test of overidentifying restrictions. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Table 8: Growth and Distribution Effects of Policies

(Dependent Variable is ln(Per Capita Income of the Poor))

	Dependent Variable is: ln(Income of the Poor)	ln(Per Capita Income)	Standard Deviation	Growth Effect	Distribution Effect
ln(Per Capita Income)	1.17				
	(0.095)				
Lagged ln(Per Capita Income)		0.892			
		(0.077)			
(Exports+Imports)/GDP	-0.02	0.021	0.459	0.080	0.004
	(0.051)	(0.021)			
ln(1+Inflation)	-0.109	-0.184	0.249	-0.378	-0.091
	(0.078)	(0.089)			
Government Consumption/GDP	0.001	-0.01	5.154	-0.426	-0.067
	(0.005)	(0.003)			
Rule of Law	-0.119	0.075	0.874	0.541	-0.012
	(0.062)	(0.046)			
Primary Enrollment	-0.0006	0.0004	11.872	0.039	0.000
	(0.001)	(0.001)			
Voice	0.049	-0.009	0.826	-0.061	0.030
	(0.060)	(0.028)			

Notes: The first column reports the results of adding the indicated control variables to the system estimator of Equations (2) and (2'). The second column reports the results of applying the same system estimator to the growth regression in Equation (3). The remaining columns show the growth and distribution effects on incomes of the poor of a one-standard deviation increase in each of the explanatory variables, as discussed in the text. Standard errors are corrected for heteroskedasticity and for the first-order autocorrelation induced by first differencing using a standard Newey-West procedure.

Figure 1: Growth and the Poor

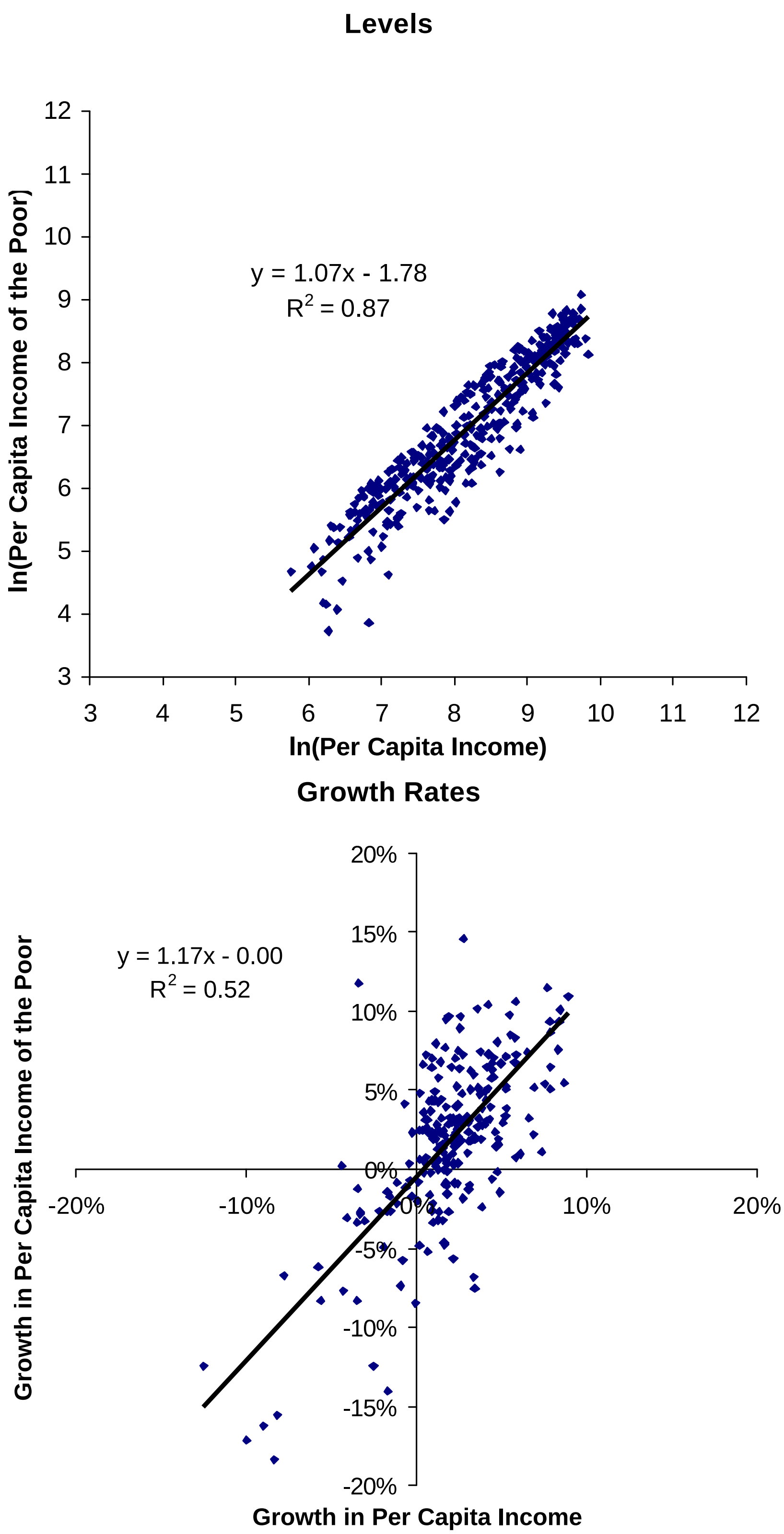

Figure 2: Variants on Basic Regression

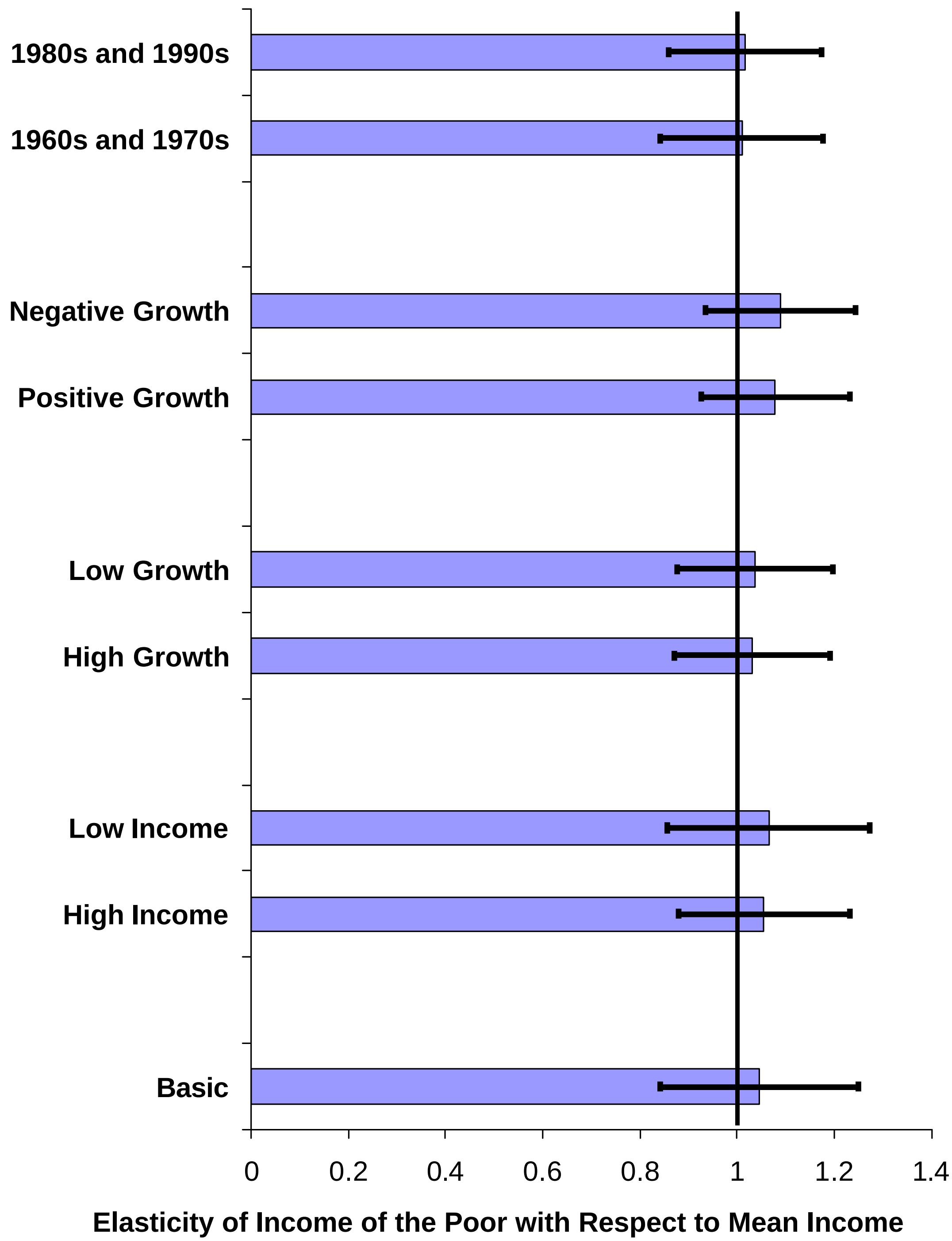

Figure 3: Growth and Distribution Effects of Policies