

**Response to:
"Growth May be Good for the Poor, But Decline is Disastrous"
by Richard Ashley**

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Ashley (2003) criticizes the Dollar and Kraay (2002) regression of log incomes of the poor on log average incomes. The paper claims that the irregularly-spaced panel used by DK causes three problems that invalidate DK's use of the standard system-GMM dynamic panel estimator. These are discussed as Claims 1-3 below. Ashley also uses a subset of the DK dataset and makes a fourth claim, that the growth increases inequality when growth is negative.

Claim 1: Ashley claims that the DK first-stage regressions are misspecified because of the unbalanced panel. The argument is as follows (this is my rendition of the argument, which is much clearer than what actually appears in the paper). Suppose log per capita income follows and AR(1):

$$y_t = \rho \cdot y_{t-1} + \varepsilon_t$$

Iterating forward and rearranging we have:

$$y_t - y_{t-k} = (\rho^k - 1) \cdot y_{t-k} + \sum_{s=1}^k \rho^s \cdot \varepsilon_{t-s}$$

DK use y_{t-k} as an instrument for $y_t - y_{t-k}$. While this is a standard internal instrument, the critique in the paper is that when k is different for different observations, it does not make sense in the first-stage regression to impose the same slope coefficient for observations with differing lag length k , because, as is clear from the above, the coefficient will vary with the lag length.

While this point is narrowly correct, there are four problems with how it is exposed in the paper:

- a) It is very unclearly presented. Ashley makes a vague statement that the slope coefficients in the first-stage regressions "do not have well-defined values". This is not the case at all, as the above example shows the values are in fact very well-defined, they just happen to be a nonlinear function of the lag length and some underlying parameter.
- b) While this parameter heterogeneity is present, it is not clear how big a deal it is. Suppose that $\rho=0.99$ which is probably not bad for t indexing annual data on real per capita GDP. According to the Ashley's own Table 1 which is intended to emphasize the unbalancedness of the panel, 272 of the 286 observations in DK have a lag length between $k=5$ and $k=15$ (which by the way doesn't sound so bad). The value of the

coefficient on initial income will therefore vary between -.05 and -.14. It's not clear this range of variation is such a big deal.

c) If parameter heterogeneity in the first-stage regression really is a big deal, then a better solution would be to incorporate it, i.e. there is nothing to prevent doing a nonlinear GMM estimator which incorporates the nonlinear relationship between the underlying parameter ρ and the coefficient on lagged income. This seems more attractive than just arbitrarily dropping most of the sample as Ashley does in the next section.

d) Ashley apparently goes on to ignore his own critique in the next section of the paper. There he takes a cross-section of spells of changes in incomes of the poor and average incomes, where the final year of the spell is the latest available. This cross-sectional dataset also has spells of varying length, and he goes ahead and performs TSLS estimation. While it is not explicit I assume he is using the same internal instruments that DK does -- and so has exactly the same problem he highlighted earlier.

Claim 2: Ashley claims that the Newey-West correction used by DK is inappropriate in an unbalanced panel. This claim is very confusing and appears to reflect a basic misunderstanding of what DK do. The issue is the following. DK assume that the time-varying part of the error term in the levels equation is not serially correlated. This is not an unreasonable assumption given that the error term in levels also includes a fixed effect which picks up lots of persistence (more on this below). But of course, in the differenced equation the error term will have first-order serial dependence due to the differencing operation. This is well-understood in the dynamic panel literature, and a standard diagnostic test is to verify that the differenced errors exhibit first-order, but not second-order serial correlation. DK in fact perform this diagnostic check throughout and the differenced error terms never exhibit more than first-order serial correlation. DK also adjust their standard errors using the Newey-West procedure with a bandwidth set equal to one for the differenced equation, to admit precisely this possibility of first-order serial correlation in the differenced errors.

The paper seems to be confused about the time horizon, and seems to think that by setting the bandwidth equal to one DK allow for serial dependence in the errors only from one year to the next. But this is not what DK do. They allow for serial dependence in differences, *from one irregularly-spaced spell to the next*, consistent with the assumption that the time-varying part of the error term is uncorrelated, *from one irregularly spaced observation to the next..* So this concern is a non-issue.

Claim 3: Ashley claims that DK is misspecified because there is no lagged dependent variable and one is required to soak up persistence in the error term. He moreover claims that it is not feasible to include a lagged dependent variable given the irregular spacing of the panel. It is not clear this is a concern, for two reasons:

a) As explained above, the issue here is whether the time-varying part of the error term exhibits serial correlation or not. Ashley could easily have gone to the DK data and tested for this to show whether this is a real concern. But he does not. And moreover, as

noted above, one test for serial correlation in the time-varying part of the error term is whether the differenced errors have second- or higher-order serial correlation. DK perform this test and reject the presence of such correlation.

b) Ashley also claims that it is infeasible to add a lagged dependent variable is clearly incorrect. The reason should be clear from the discussion of Claim 1 above. All that happens with an irregularly-spaced panel is that the coefficient on the lagged dependent variable becomes a nonlinear function of the lag length and some underlying parameters. This is in principle not a big deal and can be addressed with a nonlinear specification.

Claim 4: Finally, the second part of the paper jumps to a completely different claim: that the coefficient on growth in average incomes is different when growth is negative. There are again several problems here:

a) Ashley arbitrarily discards most of the DK dataset by focusing only on a cross-section of spells where the terminal observation is the latest available for each country. So, one possibility is that this different result is an artifact of this subsample, we have no way of knowing.

b) Ashley looks only at the differenced estimator and not the system estimator that DK do, it could be that this different result is driven by the within- variation alone -- again we don't know.

c) As mentioned above the paper is internally inconsistent in that it criticizes DK for using an unbalanced panel, but it does so itself here as well, since the length of the spells is different. In particular the concerns about the DK first-stage regression persist here as well, as noted in the discussion of Claim 1. Moreover, Ashley does not reveal explicitly the fact that his own panel here might be just as irregular as the DK dataset. Ashley notes in passing that 86 of the 89 spell observations he uses have an initial year falling after 1987. Given that most of the terminal years fall in the second half of the 1990s, it seems likely that there is still quite a bit of variation in the length of the spells.

References

Ashley, Richard (2003). "Growth May Be Good for the Poor, But Decline Is Disastrous". Manuscript, Virginia Tech (http://ashleymac.econ.vt.edu/working_papers/E2003_3.pdf).

Dollar, David and Aart Kraay (2002a). "Growth is Good for the Poor". *Journal of Economic Growth*. 7(3):195-225.