

I refereed this paper when it was previously submitted to the EJ. Since then, the paper has been extensively revised. I have deleted below any comments the authors have addressed, and left intact any they have not. I have added clarifying notes on a few points, and have added a few minor points at the end.

1. I have a general concern that what this paper is doing amounts to production function estimation without panel data, and without instruments there is a gigantic literature (e.g. Levinsohn and Petrin, 2003, RESTUD) on how it is hard to estimate TFP. And yet this paper tells us that by assuming Cobb-Douglas, profit maximization, and free movement of factors within provinces, we can solve for equation (5) and estimate it directly. It appears that there is a large gap between what the users of micro-data do and what macroeconomists do when confronted with the same problem. What the authors do is in the spirit of, say Chari, Kehoe, and McGrattan (2007) or Allen (1979). But can we lean so heavily on functional forms and theory to give us simple estimating equations that claim to tell us many things about the world?

If the exercise here really does hinge on some modeling assumptions, I would like to see evidence in their favor.

i. [In the original paper, I worried about the Cobb-Douglas functional form, but this has been addressed]

ii. If the model claims that wages and the return to capital are constant within a province, I would like to see evidence of that from micro-data, at least for some selected countries for which these data are easily available.

iii. Similarly, there are datasets in which you can measure the assumption [page reference removed] that the share of non-agricultural output paid to labor is equal to the share in agriculture.

iv. The assumption [page reference removed] that the components of TFP are log-linear with coefficients of one: is this without loss of generality? Is it testable in micro data where TFP estimation can be done more “correctly” or “completely”? Relatedly, how crucial is the assumption of a coefficient of 1 in equation 14?

v. The paper assumes that land quality is the only component of TFP that varies within a province. It isn’t immediately clear what this means in “the real world”. That one district can’t have better

government than its neighbor? That it can't have older tractors? This too is an assumption that can be tested in micro-data for a handful of countries for which such data exist.

Similarly, the data are a cross-section, not a panel. In a generic sense, this reduces the credibility of the estimation (a paper that can do "within" estimation is generally more credible than one that relies only on "between" estimation). In the specific context of this paper, the outcome variable can only be measured at one point in time, but the main independent variable (effectively log population density) could be measured at any point in history. Are the results different if the HYDE data from 1900 or 1500 are used? If so, do we worry? If not, do we doubt the HYDE data?

There is not much concern about identification here. Fixed effects remove province-level unobservables that correlate with both land quality and population density, but there is little done (e.g. coefficient stability) to show that district-level unobservables are unlikely to explain the results. A simple example: being bisected by a river may improve land quality via water availability while also increasing population because of reduced transportation costs. Of course, this is but one example – there must be hundreds of variables that vary within province and correlate with both the outcome and main independent variables.

2. [In the original paper, I was worried about the relevance of the paper to Malthusian concepts. This has been addressed.]

3. [In the original paper, I was worried if micro data would give similar answers. This has been addressed.]

4. If I take a big step back and think about the paper, the exercise here seems strange. The authors assume that $\ln(Y) = \ln A + b \ln X + (1-b)a \ln K + (1-b)(1-a) \ln L$. Given this equation, I would try to find data on Y, data on X, and an instrument for X. Here, the authors estimate b by regressing $\ln(A)$ on $\ln(L/X)$ and some fixed effects. This follows from the model assumptions, but it seems to be a very roundabout way of doing something straightforward. Should I find it bizarre that a paper estimating the effect of land on output doesn't require data on output?

Minor worries:

- [In the original paper, I worried about districts as the unit of observation. This has been addressed.]
- Why use the HYDE data at all? These are a rather opaque source of mostly back-projected population data that are used for historical periods because of their availability, not because of the transparent construction, reliability, or accuracy. Modern development and growth economists use other sources (especially the GPW) so this is a conspicuously odd choice. When these are used for 1900 and 1950, aren't the HYDE data mostly created using back projection? Regressing log density in 2000 on log density in 1900 gives a too-high-to-believe R-squared. Allusions are made to censuses as the basis of the HYDE data, but it isn't clear to me that's how they are constructed. An appendix describing how these are made would help give credibility, presuming the HYDE data are credible.
- Timing is not clear in this paper. Do we just presume that everything is measured in 2000? The FAO data are somewhat "timeless", the HYDE data appear to be for a specific year, and the lights data appear to be for that year as well. But the year 2000 is a single year... is it just selected for convenience? Why not 2010? 2000 is almost 20 years ago now.
- [In the original submission, I worried about agricultural land per person. This has been addressed.]
- Technically, the model predictions refer to agricultural labor, not to population. Is it fair to treat one as an empirical proxy for the other? Is a sufficient assumption that the share of the rural population engaged in agricultural labor is constant within a province?
- [In the original submission, I wanted the share of labor in agriculture to be validated with IPUMS data. This has been addressed.]
- It seems strange to make nighttime lights a control. That appears to be an outcome of both land quality and population, among other variables. Why is it on the right hand side and not the left hand side? The same comment applies to urbanization.
- Why do the estimations not require data on non-land inputs? Is this for the same reason capital is netted out by the province fixed effect?
- In equation (5), measurement error in population density, which must exist, will systematically bias downwards estimates of (5). Instrumental variables using an alternative measure would be a

standard solution.

- [I have modified this slightly] The specific form of the indirect utility function should be defended, since it isn't clear to me whether it can be derived from any standard utility function.
- Proposition 1c (and generally): should "population" be universally replaced as "the labor force"? These concepts seem to be used interchangeably in the paper and it is not clear if the reader should be misled into believing that the labor force in the model and the population in the data are truly the same.
- [In the initial submission I worried about low quality land. This has been addressed.]
- Should national parks and forest reserves be removed from the data?
- Estimating results separately by region is one way to give us an idea of the variation across the world in beta estimates. Is there any way to plot these on a map? I don't think there's one "right" way to do this, but I have two suggestions. First, estimate a random coefficients model and plot the coefficient estimated for each district. Second, for each district, estimate results keeping districts only within a certain distance of it, and plot that number as the estimate on the map. Researcher-imposed sample splits cannot be the only valid way to examine heterogeneity in the data, nor can it be the best way to depict it visually.
- [In the initial submission I worried about factor shares. This has been addressed.]
- There doesn't seem to be much consideration of prices or cash crops in this paper. If my output is not consumed locally, would that change the model and invalidate the estimating equations? If my government subsidizes or taxes cash crops, is that an omitted variable that we can simply ignore in the main regressions? In section 3.6, it seems bizarre to drop districts below the 25th percentile in staple crop production. This just drops small districts, whether or not they are dependent on cash crops. This dropping should be based on the importance of cash crops (or their relative productivity) within the district, e.g. 25% of this district's output is staple crops, or e.g. the most high productivity crop in this district is a staple crop. Also: how are cash crops defined?
- [I have modified this slightly] The section that replicates Acemoglu should be cut. It is somewhat unrelated to the rest of the paper, builds on a paper that has been heavily criticized, involves arbitrary cuts at the median, seems to forget that the main point of the Acemoglu paper was a zero effect on GDP.

New Points:

- The authors have made it clear that they are estimating an aggregate production function that need not be approximated by any individual or farm-level production functions that can recovered from micro data. That is sensible, though they should comment on the issues involved in aggregating production functions. The Cambridge capital controversy may be relevant here.
- On page 23 there is a space missing after the beta.