

The paper estimates the elasticity of agricultural output with respect to land. It finds that the elasticity in temperate regions is significantly higher than that in tropical regions. As the effect of shocks in population or technology on living standards is increasing in this elasticity, the findings have implications to our understanding of the differences in economic development between temperate and tropical regions. The paper presents some evidence that supports this prediction and are consistent with the estimated differences in elasticities across regions of the world.

I believe this is an interesting and important paper that is using a clever approach to estimate the elasticity of agricultural output with respect to land, without simply using factor shares. Using factor shares requires strong assumption, in particular that factors are paid their marginal cost and information on land shares are available and distinct from other infrastructure attached to the land.

However, I have several concerns regarding the estimation method presented in section 2, and the estimation in section 3:

You write before equation (2) "In each district those wages and returns are determined by [equation 2]" But (2) seems to me like an identity. Are you claiming that the share of labor and the share of capital determine wages? Doesn't it go in the other direction?

Moreover, don't you assume in (2) that the shares of factors are identical across all districts within a province? Isn't this justified by the combination of assuming that the elasticities are identical across districts and that factors are paid their marginal product? If so, the claim you do not rely on prices equal marginal products is wrong. If not, how can you justify the claim that all districts share the same factor ratio? And if firms do not employ according to the marginal product, how do markets clear?

All this leads me to conclude that, unless I'm missing something, you should provide an explanation for how wages are determined, if not according to marginal products, and explain the optimization of firms and market clearing. But if I'm missing something, just clarify it.

You use the FAO data for measures of productivity. But, as far as I understand, FAO provides estimates of expected output per unit of land. Are these estimates based on identical quantities of labor and capital per unit of land within all regions of the world? If yes, then you have a perfect estimate of productivity. But I'm not sure that's the case. If FAO provides estimates for potential output based on some "required" levels of factors that can perform the tasks of plowing, sowing, harvesting, maintaining irrigation systems, storage of grain, and so on, the quantities of factors could vary with productivity. In the extreme case that factors are proportional to output then FAO doesn't provide any relevant data on productivity. In the more reasonable case, the data is somewhere in between the two options of identical factors in all regions and factors proportional to output. That is, factors increase with output, but not proportionally. Some of the required labor, one could assume, isn't affected by land productivity (perhaps when plowing and sowing), but for others tasks (such as harvesting and storing), one can reasonably assume that the required labor is increasing with land productivity. In that case, I'm concerned, there will be some systematic bias in the estimates. This potential bias should be addressed.

My major concern is that there could be systematic differences in the FAO predicted output and required factors ratio between temperate zones and tropical zones. The suitable grain or other crop perhaps isn't the same in temperate and tropical regions, and different crops could require different capital labor or capital land ratios. Moreover, it could be the case, that the most suitable crop (maximum potential calories per unit of land) according to FAO isn't the crop that is used in that area.

I guess that these differences between temperate and tropical zones could bias the estimates and might show a difference in elasticity when such a difference doesn't exist, but perhaps one can still claim that the data and methodology support the authors claims. These concerns should be addressed.

Minor comments:

1. The subscript A, denoting factors employed in agriculture, seems redundant, and is used inconsistently.
2. It seems (5 – 6) should be $\ln(L/X)$ instead of $\ln L/X$.
3. Footnote 8 repeats a statement in the main text on page 5.
4. You show in an extension to the model that as the elasticity gets higher, the economy gets more sensitive to population and technological change. This seems rather obvious and doesn't really require a model, in particular since, as you mention, this is a standard result in the literature. Perhaps you can consider moving the formal analysis to an appendix and just keep the intuitive explanation, which is straightforward.