Discussion by Pete Klenow (Stanford University) of:

BIG ANSWERS FOR BIG QUESTIONS: THE PRESUMPTION OF MACRO

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I found Banerjee's paper a pleasure to read, both provocative and compelling. As Banerjee concedes, the most important questions in development are often "macro" ones such as how to boost growth in the fashion of China and India in recent decades. But, Banerjee emphasizes, we don't know very much about how to do this despite lots of research effort. So he suggests we largely abandon macro development research and concentrate on micro development questions such as the effect of randomized interventions in health and education. The following Table summarizes Banerjee's position.

$$\frac{\partial \text{Welfare}}{\partial \text{Research}} = \frac{\partial \text{Welfare}}{\partial \text{Knowledge}} \bullet \frac{\partial \text{Knowledge}}{\partial \text{Research}}$$

Micro	> 0	High	Low
Macro	≈ 0?	Low	High

I wholeheartedly agree with the "micro" row in this Table, so will say no more about it. Instead I will defend the value of doing macro development research. As I consider myself at the micro data end of the macro spectrum, I do this without feeling particularly defensive.

I begin by drawing an analogy to potential micro vs. macro divides in medicine and biology. Consider "micro" researchers in medicine who do carefully controlled clinical trials and lab work. I imagine they might disdain "macro" medicine, which looks at trends in life expectancy, heart disease, cancer, autism, etc. Their skepticism may

extend to epidemiological studies (reduced form regressions) that relate individual outcomes to a variety of other individual observables. Micro researchers may rightly say we learn little about causality and effective treatments from such macro studies. Similarly, one can imagine a conflict between micro-biologists – who study molecular biology, genetics, neuroscience, immunology, embryology, etc. – and macro-biologists who study whole ecologies, i.e., populations and how their traits and numbers evolve and interact.

A common element in macro medicine, macro biology and macro development is that they each motivate and guide a lot of the micro work that is done. In medicine, macro evidence has documented an obesity epidemic in the U.S., and epidemiological evidence has related obesity to heart disease, diabetes, and other ailments. This macro work has motivated a myriad of micro experiments on what sorts of diet, exercise, and drug interventions might best prevent or treat obesity and its complications.

In biology, macro research has documented vanishing diversity of species, and related it to macro factors such as deforestation, pollution, and global climate change. As a result, biologists are studying the consequences of global warming for agriculture and animal populations. Some of these studies take the form of micro experiments, either natural or controlled (unnatural).

Stanford is home to a famous biologist who does both macro and micro work, namely Robert Sapolsky. As he describes in *A Primate's Memoir* (2002), he has spent

many summers in Kenya studying baboon populations. He combines field observations with blood work to document social and individual correlates with stress hormones. He devotes the rest of his research year to lab work. He says his field work is critical for studying normal social interactions, as opposed to contrived lab environments. The field work motivates much of the lab work he does.

Again, macro research tells us the disease, its severity, and its symptoms. Macro work tells us where randomized trials are most urgently needed. In macro development the extent of the disease is the difference in incomes between some African and OECD economies. The symptoms / correlates are low life expectancy, low investment rates in physical and human capital, high fertility rates, political instability, and so on. This macro research has presumably helped motivate the micro experiments on health and education in Africa.

I know my own recent work with Chang-Tai Hsieh on China and India (Hsieh and Klenow, 2007b) has been motivated by macro evidence. China and India are poor and populous but growing fast. Macro development accounting leaves unexplained a nontrivial portion of their initial poverty and subsequent growth. Chang and I use micro data on manufacturing plants to assess the potential contribution of allocative efficiency. This particular hypothesis was also motivated by macro evidence, namely that growth took off in China and India in the wake of a series of policy reforms (privatization, trade and FDI liberalization, etc.) designed to improve allocative efficiency.

Like Robert Sapolsky, my Stanford economics colleague Nick Bloom does both macro and micro work. Indian manufacturing firms have lower productivity on average than firms in OECD countries, and Bloom and Van Reenen (2007) documented important differences in managerial practices between Indian and U.S. firms in regards to monitoring (e.g., how well are they tracking whether they are hitting their performance targets?), targets (e.g., are the targets realistic, aimed for, and economically rational?), and incentives (e.g., are people promoted based primarily on tenure or performance?). These macro findings have led Bloom and collaborators (Bloom et al., 2008) to embark on a randomized evaluation of whether management consulting services can improve productivity in Indian firms. Again, macro evidence is usefully motivating micro experimental work.

I have mentioned growth and development accounting several times. Banerjee refers to this macro evidence as containing an unhelpfully wide range of estimates. But I do not think this assessment is accurate. I think a macro consensus exists on many points. To name a few: rising workers per capita accounts for one-fifth of growth in much of East Asia (Young, 1995); physical capital intensity accounts for one-fifth of income differences across countries (Mankiw et al. 1992, Hall and Jones 1999); much of China's growth has come from productivity growth in agriculture and movement of workers out of agriculture (Young 2003, Brandt et al. 2008); and productivity differences between rich and poor nations are concentrated in agriculture and investment sectors (Caselli 2005, Hsieh and Klenow 2007a).

Beyond accounting, macro has contributed a slew of robust correlations that help guide micro experimental work. Correlation need not imply causality, of course, but it certainly does not rule it out. Many correlations are later confirmed to be causal in various ways (lung cancer and smoking, earnings and education, life expectancy and income). Here are a few of the facts emphasized by recent macro development research: developing countries import most of their equipment, and from the U.S., Japan and Germany in particular (Eaton and Kortum, 2001); the local-currency return on reproducible capital is no higher in poor than rich countries (Caselli and Feyrer, 2007); the skill premium increases with liberalization of trade and FDI (Goldberg and Pavcnik, 2007); poor countries exhibit greater dispersion of average products of capital and labor across firms within industries (e.g., Hsieh and Klenow, 2007b). It is up to theorists and micro experimentalists to flesh out the causal mechanisms behind these correlations.

On the issue of causality, there *are* some macro instruments. Climate (rainfall, temperature, windspeed and direction, etc.), geography (distance to the other land masses, ruggedness of terrain, etc.), and accidents (e.g., natural deaths of dictators) have been deployed in the last decade. Another strategy is to interact common shocks with different initial conditions. Acemoglu and Johnson (2007) interacted the epidemiological transition with different initial disease burdens to study the effects of reduced mortality on populations and incomes. And many authors have studied the effects of trade liberalization using the differential effect of a country's WTO entry on tariffs across its industries.

Macro development research consists of much more than cross-country growth regressions, however. Much of the work is not even particularly empirical. Examples include the classic growth theories of Lucas (1988), Romer (1990), Grossman and Helpman (1991), and Aghion and Howitt (1992), as well as the more recent work of Acemoglu (2007), Jones (2008), and others. I will not dwell on this theoretical literature because Banerjee readily (and predictably!) concedes that random evaluations must be combined with theorizing. We will always need general equilibrium models to fill in the gaps between results of randomized trials, and to do welfare analysis.

In the past decade I have organized the NBER Summer Institute Growth Meeting with Chad Jones. The overwhelming majority of papers have been neither empirical nor theoretical, but instead what might be called "quantitative theory" (to use Prescott's phrase). This literature typically identifies exogenous driving forces (such as observable policy distortions documented by the World Bank's Doing Business studies), then calibrates a model to obtain a quantitative sense of how these driving forces might affect aggregates (income, investment, schooling, etc.) across time or countries. Like strictly theoretical work, I would argue such quantitative theorizing is a vital complement to micro empirical research.

To sum up, I agree with everything Banerjee has written about the positive value of micro empirical work in development. But I am less sympathetic with his nihilism toward macro development research. To me, macro research remains enormously valuable alongside more definitive but narrower micro research.

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