BOOK REVIEWS


Development economics has seen its waves of enthusiasm in recent years, and I mean the word in its religious sense: a joyous, apostolic campaign to bring the heathen unfortunates now making policy in poor countries into the blessed peace of the neoclassical synthesis. In fact, the attack is being waged on several fronts. On one of these, the quaintly-robed apostles errant of human capital combat heresies postulating the existence of educated unemployment; on another, the scholastics of social cost–benefit analysis debate subtleties of discount rate and shadow price of foreign exchange while investment rates stagnate. The Great Crusade against import substitution and trade distortions is still with us, battering at the high tariff walls of its chosen Jerusalem. And now a new sort of monetary Calvinism is being wrought by Messrs. McKinnon and Shaw, stressing the need for financial redemption before salvation can be attained.

Will the majority of the developing countries be better off, when all these generous counsels to Get Their Prices Right have been buried under the rapidly accumulating sands of forgetfulness? Common sense suggests that great laissez-faire growth spurts are unlikely, but even so, serious economists should still evaluate the missionary messages on their own merits. An evaluation of the financial doctrine is what we attempt here, admittedly from the obscurantist point of view of the 'structuralism' which McKinnon and Shaw hope to expunge. One can't let the Christians win all the doctrinal battles, without at least putting up some token resistance.

To the eye of the infidel, both books attempt to impart the same message and will largely be discussed together. No doubt the authors are more aware of differences and nuances in their arguments than I, but let's leave these aside to keep things simple. Basically, their logic takes the following form:

(i) The price level (and its rate of change) is controlled by interaction of the supply of and demand for money. Supply of nominal balances is in the hands of the government, while demand for real balances depends on income and the 'deposit rate', the difference between the average interest one receives for holding money and the expected rate of inflation.

(ii) At least in developing economies which are not highly monetized, the level of income depends positively on the amount of real balances. However, decreasing returns ultimately set in here, for a financial intermediation industry is also costly in terms of clerks, fixed capital embodied in computers, and skilled labor.

(iii) At least when the real deposit rate increases a lot (say from minus ten percent to plus ten percent), savings rates rise. They also rise in response to higher income levels.

(iv) Finally, investment also responds positively to higher real balances, which lubricate the friction points making capital formation such a laborious task in the developing world. In almost antediluvian terminology, the absorptive capacity constraint is loosened by Enancc.

As the authors point out, assumptions (i) and (ii) pretty much underlie received post-Keynesian monetary theory—leaving aside all Clower–Leijonhufvud second thoughts as irrelevant. If one ignores the costliness of money creation, then it has a positive 'income effect' which persists until such point as money's marginal product falls to zero. On the other hand, an increase in real money has a negative 'substitution effect', reducing the savings flow into capital formation. (Keynes, who knew the mother tongue better than his successors, called excessive money savings 'hoarding'.) The solution to this dilemma lies in fixing a high deposit rate to call forth enough demand for money to drive its marginal product to zero, and then
making good the savings deficit with frictionless fiscal policy. Shaw and McKinnon at the very least deserve sainthood for insisting that all this is completely irrelevant to the developing world. (I desist from the extension northward.) Their assumptions (iii) and (iv) may reflect wishful thinking, but they at least add true grit to the ectoplasm of neoclassical theory.

But do these assumptions have anything to do with developing countries? It is intuitively clear that (i)–(iv) together are an argument for increases in real balance and interest rates (the latter from artificially low levels maintained by credit rationing on the lending side and diverse forced savings schemes for deposits). Higher real balances and interest rates result from increases in the deposit rate. These in turn lead to more and more efficient investment, since a higher lending rate enforces rationality in project selection, and besides the absorptive capacity constraint is relaxed. And everything is conveniently limited from above by increasing costs of intermediation. To summarize, all you have to do to reach a higher steady state growth path is jack up the deposit rate and at the same time inject with sufficient agility the newly demanded real money into the economy. To repeat, is this scenario at all plausible in a representative developing country?

There are at least two ways to answer this question. One is a priori criticism of the basic assumptions (i)–(iv); the second is institutional analysis of the countries where the financial reformation has allegedly run its beneficient course. Although limited by space, let us attempt both types of criticism. Begin with a discussion of the assumptions:

(i) Does the rate of inflation depend on what goes on in the money market, in the politically relevant medium run? A structuralist (ECLA variety or otherwise) will automatically answer, ‘No!’ Clearly, if you press the monetary brakes long and hard, you can induce a depression deep enough to decelerate prices, but the transition is drawn out and exquisitely painful. (It took two years to deflate in Brazil under a tough government – a point we return to below.) Over a reasonable time period, and for depths of recession within the political range of most governments, prices are expectation and cost-push exogenous, even if the authorities control the money supply. In many cases they of course have no such powers, Shaw wisely points out. The export crop growers, the government employees, the state corporations – all want to be paid off, if necessary by the printing press. A strong enough government can control its money, and bash away with restrictive monetary policy long enough to convince its constituency that inflation has been reduced, but one must admit that this is almost an ideal case. The authors attempt to bring the Ideal down to the Real by asserting that (as McKinnon puts it) ‘The real rate return on holding money ... can be made to rise quickly in the course of deflation by a credible fall in price movements expected in the future, coupled with a suitable and delicately adjusted rise in nominal rates of interest ... paid to depositors.’ (The quote is from page 86 of McKinnon’s book. The ellipses delete only algebraic symbols, and the italics are mine.) In other words, you can simultaneously deflate and maintain the demand for real balances at a high enough level to avoid recession, if only you convince a sceptical public that inflation is over and moreover delicately float up the interest rate. Can you be so convincing? Can you subtly manage deposit rates in an environment where oligopolistic bankers make their profits from the spread between inflation and the nominal rate (another Shaw point)? Regrettably, one must recognize that such nimbleness is beyond most of us.

(ii) Whether or not increased real balances shift up the production function is a somewhat metaphysical question, but a guardedly positive response is probably appropriate. In the authors’ star countries – South Korea, Taiwan, Brazil, Mexico and (to a lesser extent) Indonesia – there is some correlation of increases in real balances with increases in the growth rate. This naturally proves nothing, since much else was going on in these countries at the same time, but the evidence cannot be discarded out of hand. However, the really important question is not about how productive real balances are, but about how much their expansion and subsequent management costs. Open market operations, for example, presuppose the existence of a large and willingly held pool of short-term government debt, on which appropriately high interest must be paid. Regardless of debate about long-term burdens, government bonds in the present channel large interest payments to people who can afford to buy them – the wealthy receive a subsidy at least commensurate with their generosity in maintaining the backing for the money supply. Needless to say, these transfers have a high shadow price in terms of the welfare of the poor, who for many years may not be laved by the trickle down from finance-led growth.
(iii) The savings effect of higher interest rates (and higher incomes) is at the heart of the McKinnon-Shaw analysis, and deserves careful scrutiny. Space again limits us to a few brief observations, beginning with a question about what kinds of savings are supposed to increase in response to higher interest rates. In many, if not most, developing countries the government may be responsible for half or even more of savings, so that the size of the savings gap does not depend so much on financial markets as on fiscal policy (a point accepted by the authors—but only at the end of their books). And even if financial savings by persons increase as inflation falls and interest rates rise, there is no guarantee that the increment will be greater than the volume of savings previously collected through the inflation tax. Moreover, in capital markets which persist in being fragmented even after a liberalization, increased personal savings are as likely to go into cars and housing as into more socially useful investments. The savings for the latter must come from persons at the top of the earnings distribution, and from owners of capital. It’s fine if these groups respond to government favors with higher savings rates, but once again an unfavorable near term distributional impact will ensue. A government supported by the rich and espousing capitalism may make financial liberalization work, but the fact is that few governments in the Third World are of the appropriate type.

(iv) The increase in absorptive capacity for investment which supposedly accompanies better finance would be even harder to verify empirically than the real money income effect postulated in (i). The discussion in the texts on this point is wholly a priori, resting finally on faith in the efficiency of deep capital markets in searching out new projects. The authors’ deep conviction lends force to their arguments, but for at least two reasons doubts persist. On the one hand, there are so many obstacles to capital formation in developing countries—ranging from incompetent postal systems to building contractors whose cupidity is not diminished by the absence of a Wall Street in which to invest its proceeds. A wave of the financial wand may exorcise some of these demons, but innumerable others remain. Just being assured of the option to borrow at twenty-five per cent is not enough to make investment an easy task. And on the other hand, who is to guarantee that investors will actually make use of additional savings, newly available but at historically high costs? They may not want to borrow, disliking high real rates and lacking the government’s vision of the financial Valhalla to come. After a time, they may not be able to borrow effectively, if the economy actually manages a boom and it becomes impossible to import technically non-competitive capital goods when exports aren’t buoyant. The policy-maker needs great skills to steer between these twin perils of Kaleckian oversaving and Chenierian import strangulation.

Summarizing, we may credit Shaw and McKinnon for recognizing that brewing the financial medicines requires real effort, and for pointing out that there are some cases where the financial therapy may be expected to have enough beneficial effects to justify its cost. How can we characterize these cases more precisely?

In the first place, it is clear that absorptive capacity limitations were absent in the authors’ selection of successful countries. Taiwan and South Korea both enjoyed many years of foreign aid at fabulous rates and direct contact with American know-how before they took off. Before that, they had seen Japanese tutes, and in any case inherited a long, achievement-oriented cultural tradition. Many of the same comments apply to Mexico (although the form of American involvement was different), and the much-maligned import substitution strategy had given Brazil a large and diversified industrial park long before that country’s financial wizards appeared on the scene. Of course, these observations are not enough to justify a denial that the rapid spread of capital markets in these countries was useful (or perhaps inevitable) at a certain stage in their development, but they certainly can support an assertion that the ground was very well prepared for the seed to sprout.

Second, there’s no use ignoring the fact that the financial reformations occurred under governments at least as authoritarian as, and far more firmly entrenched than the average in developing countries. Moreover, financial growth was correlated with clear deterioration of the income distribution in Brazil and Mexico, and real wages were and are still very low in the Asian success countries. How many governments are both willing and able to go through a year or two of tightened belts, rebellious urban workers whose real wages are clearly lagging the inflation, escalating interest rates, etc., etc.—all in the name of better capital markets? Not many, I suspect.

For these reasons we must finally deny that financial liberalization is as powerful a medicine
as Shaw and McKinnon at time assert. That's perfectly reasonable, of course – they have their point to make and want to present it as forcefully as possible. A critical reader must take a more restrained view, for some of the reasons I have tried to indicate here.

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