Is the Washington Consensus Dead?
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In the postwar years, most Third World countries turned inward partly in response to what they thought were the disastrous consequences of their 19th century integration into the world economy as the global economy collapsed in the Great Depression. The seeming success of Soviet central planning under Stalin also persuaded the leaders of these newly independent countries to substitute the plan for the market. Planning was all the rage, with the state seeking to control the commanding heights of the economy. Furthermore, the many theorists who created a seemingly "new development economics" provided the intellectual basis for the complex system of *dirigiste* controls on "anything that moved" (as one wit put it) in a market economy.

In the early 1980s, I wrote a small book, *The Poverty of 'Development Economics'* (Lal 1983), which attempted to summarize the logical arguments and empirical evidence against what I called the "*Dirigiste* Dogma," which had done so much damage to the prospects of the Third World's poor. That book, which acquired some notoriety, if not fame, marked the neoclassical resurgence against the *Dirigiste* Dogma as many developing countries began to adopt (at least partly) the classical liberal policy package known as the

Washington Consensus.[1] The reversal of *dirigisme* in the 1980s and early 1990s, particularly in China and India, but also in many other parts of the Third World, led to the surge in per capita incomes and the largest reduction in structural poverty in human history.

The Washington Consensus

The Washington Consensus is the standard classical liberal economic package, consisting of free trade, Gladstonian finance, and stable money. It also requires a good government that promotes Adam Smith's "popular opulence" through promoting natural liberty by establishing laws of justice that guarantee free exchange and peaceful competition. Egalitarianism is rejected as the norm for deriving principles of public policy, because of the contingent fact that there is no universal consensus on what a "just" or "fair" income should be, despite the efforts by moral philosophers to justify their particular prejudices as the dictates of reason. But classical liberals, from Smith to Friedman and Hayek, have also recognized that the state should seek to alleviate absolute poverty through targeted benefits to the indigent and disabled. For various merit goods—health, education, and possibly housing—these involve in-kind transfers through some form of voucher.

Over the past two decades, there has been a backlash against the Washington Consensus. Gerald Meier, a proponent of the new development economics, which draws from models of imperfect information, coordination failures, multiple equilibria, and poverty traps, claims that "with imperfect information and incomplete markets, the economy is constrained Pareto inefficient—that is, a set of taxes and subsidies exists that can make everyone better off" (Meier 2005: 119–20).[2] This echoes a similar claim made by Greenwald and Stiglitz (1986).

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[1]A referee has rightly pointed out that this policy package put together by John Williamson (1989) consists mainly of macroeconomic prescriptions, not all of which are consistent with classical liberal principles. Thus, for example, he argues for managed exchange rates, and though he emphasizes property rights, he has nothing to say about the size of government. In fact, Williamson, who goes some length to distance himself from the views advocated by members of the Mont Pelerin Society, would consider it a canard to call his policy package "classical liberal" (Williamson 2002). Nevertheless, there is enough congruence that the classical liberal policy package can be referred to as the Washington Consensus.

[2]See Lal (2007) for a review of Meier.

Meier (2005: 124) commends Rodrik (1995) for emphasizing "co-ordination failures," and for demonstrating "how the South Korean and Taiwanese governments got interventions right." He claims that this viewpoint, and the Murphy, Shleifer, and Vishny (1989) modeling of the "Big Push," validate the old development economics of Nurkse (1953) and Rosenstein-Rodan (1943). He supports Krugman's (1993) belief that in its earlier incarnation it was not persuasive because its ideas were not formalized in mathematics. But as Stiglitz, (Krugman's discussant), rightly noted: "That we can write down a model of a phenomenon proves almost nothing. It does not make the idea right or wrong, important or unimportant." As regards Rodrik's views about smart *dirigisme* in Korea and Taiwan, Little (1994) convincingly showed that social rates of return to investment in these countries were inversely correlated with the degree of *dirigisme*.

In this article, I examine the basis of these "new" theoretical curiosa questioning the Washington Consensus, as they now form the basis of the advice given by the "new *dirigistes*" to alleviate Third World poverty. I also briefly examine the claim that the Chinese economic "miracle" was created not by the replacement of the plan by the market, but by various forms of *dirigisme*, which has led to a new "Beijing Consensus" to replace the defunct Washington Consensus.

Poverty Traps

I have a tremendous sense of déjà vu at reading this "new" theoretical literature. The "poverty trap" view, which now has wide currency among the young, is just a resurrection of the "vicious circle of poverty" arguments of the 1950s—except now it is attired in sophisticated mathematical garb. Peter Bauer's ([1987] 2009: 173) castigation of this view still stands:

> According to this notion, stagnation and poverty are necessarily self-perpetuating: poor people generally and poor countries or societies in particular are trapped in their poverty, and cannot generate sufficient savings to escape from the trap. This notion became a cornerstone of mainstream development economics. It was the signature tune of the advocates of foreign aid throughout the 1950s. . . . Yet it is in obvious conflict with simple reality. Throughout history, innumerable individuals, families, groups, societies, and countries—both in
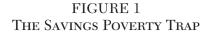
the West and the Third World—have moved from poverty to prosperity without external donations. All developed countries began as underdeveloped. If the notion of the vicious circle were valid, mankind would still be in the Stone Age at best.
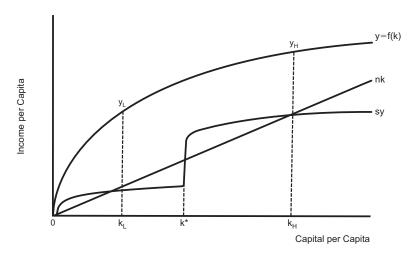
But validating the vicious circle is precisely what the purveyors of poverty traps seek to do.

The theoreticians have fiddled with the standard neoclassical growth model developed by Nobel laureate Robert Solow (1970). He showed that with a given per capita savings rate (as a fixed proportion of per capita income), and given rates of increase in the labor force (directly from population growth and indirectly through labor augmenting technical progress), a poor economy starting off with a low stock of capital per head (and hence low per capita income) will converge to a higher steady state capital per head and income per capita. In the steady state, both income and the capital stock will be growing at the "natural rate of growth" given by the sum of the rate of population growth and labor augmenting technical progress, with per capita income constant at its higher steady state level. But during the "traverse" to this steady state from the initial position, the economy's growth rate of per capita income and capital per head will be faster than this natural rate of growth. The poorer the country, the faster will be its rate of growth of per capita income as it reaches the steady state capital and income per head of the leaders in the world economy.

This convergence in per capita incomes, however, is conditional on the relevant countries having a common institutional and legal framework for economic activity, as Barro and Sala-i-Martin (1991, 1992) have shown. Regions within large economic units like the United States, Japan, and the European Union would meet this condition, and there is evidence of this "conditional convergence" in the growth rates of their regions. Clearly, within this theoretical framework there are no poverty traps.

Now enter the new theoreticians. They find that many countries, particularly in Africa, though poor, do not seem to be growing faster than their richer peers, as mainstream theory predicts. So, they argue the answer must lie in there being "multiple equilibria" where some countries are stuck in a steady state with a low income per capita and a low capital stock per capita, rather than moving smoothly on to the high income per capita steady state of the Solow framework.

FIGURE 1
THE SAVINGS POVERTY TRAP

So how do you theoretically generate these multiple equilibria? Simple, *assume* that instead of the per capita savings rate being a fixed proportion of income per capita, it is a lower share at low levels of income per capita, and then—after some threshold level of income per capita—it suddenly jumps to a higher proportion of income. Savings per capita as a function of per capita income then becomes steeply S-shaped with respect to the capital stock per worker. Savings is low at low levels of capital per worker, increases substantially at an intermediate level, and then levels off.

Figure 1 is a simple diagrammatic explication of the standard Solow growth model and the multiple equilibria grafted on to it by the purveyors of a savings "poverty trap."[3] The standard neo-classical production function (Oy) shows output (real income) per capita (y 5 Y/L) as a function of capital per capita (k 5 K/L). The ray nk shows the investment required to maintain any given capital-labor ratio constant, when the sum of the rate of population growth (p) and labor-augmenting technical progress (t) is equal to n (n 5 p 1 t). The savings function (sy) is a constant at a low

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[3]I am sorry to say that much of this higher nonsense has been perpetrated by my mathematical economics colleagues at UCLA. See for instance the genesis of this genre in Azariadis (1996) and Azariadis and Drazen (1990).

rate until capital per capita is k_L, and then jumps to a constant higher rate when the capital per capita is $k*$. There will then be two stable steady state capital-labor ratios. If the country starts off with capital below k_L, it will reach the low steady state capital-labor ratio k_L and stay there. It is in a poverty trap as income per capita cannot increase. If, however the economy starts with capital per capita greater than $k*$, it will end up in the higher steady state with capital of k_H, and the higher steady state income per capita of y_H. For a country stuck in the poverty trap at k_L, a large gift of capital from foreign aid would push it toward the higher capital-labor ratio k_H, and the higher steady state income y_H. If the threshold capital-labor ratio $k*$ at which the savings function jumps is to the left of k_L, then even if the country starts off with a low per capita capital stock it will converge to the high steady state capital-labor ratio k_H, and income per head of y_H.

So what is the evidence on the shape of the savings function for poor African countries and thence the likelihood that they are caught in the low steady state per capita income and capital stock "poverty trap"? Aart Kray and Claudio Raddatz (2007: 316) have examined the evidence and find that "the actual cross-country relationship between savings rates and capital per capita observed in the data is far from meeting these conditions. If anything, savings rates seem to be increasing at low levels of capital per worker, flat at intermediate levels and increasing again at high levels."[4] Once again, we have a theoretical curiosum. But, as in the past, it is being used by academics to urge the "great and good" in the West to support a massive surge of foreign aid to Save Africa.[5]

The Big Push, "New" Trade Theory, and Industrial Policy

In the 1950s, development economics had made much of concepts like the "Big Push" and "backward and forward linkages," which were based on the importance of increasing returns and pecuniary external economies. Those concepts have been formalized and

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[4]Kraay and Raddatz (2007) also show how the other sources of purported theoretical poverty traps (e.g., low levels of technology at low levels of development or a low level of subsistence consumption) have no empirical support.

[5]See Sachs (2005) and the report authored by Nicholas Stern for the Commission for Africa (2005). Easterly (2002) had argued for aid to break poverty traps in Africa, but clearly changed his mind in Easterly (2009).

constitute the basis of a new *dirigiste* literature reviving the case for industrial policy (IP). They are all linked to the age-old debate about free trade and protectionism. The modern theory of trade and welfare has provided a theoretical answer to all these puzzles.[6]

Most of the arguments for protectionism, going back to those of Hamilton and List in the 19th century, are based on some distortion in the working of the *domestic* price mechanism, so that market prices do not reflect true opportunity costs (i.e., social values), and that a tariff can be used to provide a welfare-improving outcome. This argument had led to the *Dirigiste* Dogma that both free trade and laissez-faire are harmful. The theory of trade and welfare developed in the 1950s and 1960s disconnected the case for free trade from that for laissez-faire. It showed that the best way to deal with a distortion in the domestic price mechanism was to deal with it at its source, by some suitable domestic tax and subsidy instruments (hence departing from laissez-faire). But free trade should still be maintained. Tariffs and quantitative restrictions were often the worst possible instruments to deal with these domestic distortions and could lead to a loss of welfare (see Corden 1997).

The only argument for trade intervention that survives in this modern theory is John Stuart Mill's "optimum tariff" argument. If a country has monopoly or monopsony power in foreign trade, it can derive a welfare gain by imposing an optimal tariff if it has monopsony power in its imports, or an optimal export tax if it has monopoly power in its exports. The reason why trade intervention is required is because in this case the distortion is in *foreign* trade. But, given the danger of retaliation, this argument has not had much practical relevance except in the case of oil exporters in the OPEC cartel.

Moreover, once the political process and the ubiquitous phenomenon of rent seeking is taken into account, even the recommendation of using domestic tax subsidies to correct a domestic distortion collapses (see Krueger 1974). If producers know the government is in the subsidy business, they are as likely to lobby for a subsidy claiming a domestic distortion in the workings of the price mechanism, where none in fact exists. If successful, they stand to gain producer rents. They will be willing to spend resources (up to the limit of these rents) on seeking subsidies, which will represent a net welfare loss for the economy.

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[6]This literature was surveyed and extended for the general reader in Lal (2006).

In practice, it is virtually impossible to determine whether a domestic distortion exists. Uncertainty over the size of the distortion and the requisite subsidy encourages rent seeking. This implies that the tax-subsidy solution for dealing with domestic distortions will not lead to a welfare improvement. Hence, the best policy may be to leave well enough alone—that is, to follow a laissez-faire approach. So even within the framework of the theory of domestic distortions in the working of the domestic price mechanism, because of these rent-seeking considerations, the wheel does seem to have come full circle: free trade and laissez-faire, as the 19th-century classical liberals saw so clearly, do hang together (Lal 2003).

While there seems to be general acceptance of this view as far as direct interventions in foreign trade in the form of tariffs and quantitative restrictions are concerned, there is a growing band of "new development economists" praised by Meier (2005), who are arguing for government intervention in the form of industrial policy to take account of various externalities. The successful industrialization of Japan, South Korea, and Taiwan is claimed to be due to their use of industrial policy that internalized these dynamic Marshallian externalities. The basis for this "new" *dirigiste* program is supposed to be the "new" trade theory that has emerged by incorporating various aspects from industrial organization theory, such as imperfect competition and strategic trade policy. In fact, Baldwin (1992) has shown that much of this new trade theory is merely a variant of the old terms-of-trade argument, which is valid only if there is no retaliation. If there is a trade war, the country initiating it may well be the loser. Hence, though theoretically correct it is not of much practical relevance.

The bases for *dirigiste* industrial policies are variants of the infant industry argument based on the presumed existence of what are termed *pecuniary* externalities. These are to be distinguished from the *technological* externalities adduced by environmentalists for government intervention. The latter are like the smoke from a factory which damages a nearby laundry. Government action may be needed to make the factory internalize these external costs it imposes on the laundry. These externalities exist as the relevant costs and benefits are not mediated through the price mechanism. By contrast, pecuniary externalities reflect the interdependencies mediated through the price mechanism. For example, if a new whisky producer opens a distillery, increasing the supply of whisky and reducing its price, the profits of existing whisky distilleries decline but the welfare of whisky

drinkers increases. This result is purportedly a pecuniary diseconomy for whisky producers. But as Buchannan and Stubblebine (1962) and Viner (1931) showed, these pecuniary externalities are not Pareto-relevant, and no attempt should be made to offset them. For the loss of the producers is less or equal to the gain of the consumers. Similarly, if there is a cost-reducing innovation by a producer that reduces the price and increases output at the expense of other producers, it is readily shown that the consumer gains (in terms of consumer surplus) offset any loss of rents of the now inefficient producers. These changes in a dynamic economy are mediated through the price mechanism and improve the allocation of resources; no government action is required to deal with them.

So, what are the pecuniary externalities proponents of industrial policy say require government action? The major one provides the argument for the Big Push as developed by Rosenstein-Rodan. Suppose a new shoe factory is set up in a developing country. Not much of the demand resulting from the expenditures on shoe making will be spent on shoes, making the factory unprofitable. If, however, a large number of factories making various consumer goods are set up simultaneously, then Say's law—that supply creates its own demand—will operate, and the resultant industrial complex will be viable. This idea can be called that of "demand complementarity." But as Little (1982: 38) rightly noted: "The demand complementarity argument seems to require a closed economy." For if the economy is open, the producer of shoes could always export his shoes if there was insufficient domestic demand, and make a profit as long as the factory was viable at world prices.

Murphy, Shleifer, and Vishny (1989) recognize this, and in their two-good model, the source of presumed market failure lies instead in increasing returns (economies of scale) and imperfect competition in a high productivity modern good, as compared with a traditional constant returns to scale low productivity good produced under perfect competition.[7] This leads to two equilibria in

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[7]But they continue the muddle created by Scitovsky's (1954) definition of pecuniary externalities, which were not those as defined by Buchanan and Stubblebine (1962) and Viner (1931), but which concerned increasing returns and imperfections of markets (particularly those for futures). Thus Murphy, Shleifer, and Vishny (1989: 1004) write: "In all the models described in this paper, the source of multiplicity of equilibria is pecuniary externalities generated by imperfect competition with large fixed costs."

the open economy: a good one, where, as a result of government intervention the economy specializes completely in producing the modern good; and a bad one, where (without the intervention) the economy specializes completely in producing the traditional good. This model of pecuniary externalities is more correctly called one of increasing returns and infant industry protection. It has been used to justify selective government intervention in industrial policy (see Pack and Westphal 1986).

As regards the infant industry argument, Baldwin (1969) demonstrated that a domestic tax-subsidy intervention is preferable to interventions in foreign trade. For any intervention to be justified, a necessary condition is that the inputs per unit of output decrease more rapidly in that industry both relative to foreign competitors and to other domestic industries. But even that condition is not sufficient. In addition, it is necessary that the discounted net present value of the losses incurred during the high-cost phase are recouped during the post-infancy phase, to earn at least the social rate of return to investment in the economy.

So what is the evidence on industrial policy based on the infant industry argument? Ann Harrison and Andres Rodriguez-Clare (2009; hereafter HRC) have surveyed the numerous empirical studies addressing that question.

First are the few industry studies that have attempted a correctly formulated empirical answer. These found that "protection may lead to higher growth but result in net welfare losses. . . . These case studies suggest that designing policies that increase overall welfare is very difficult" (HRC 2009: 32).

Second are cross-industry studies. From these, HRC (2009: 34) conclude:

> There is no evidence to suggest that intervention for IP [Industrial Policy] reasons in trade even exists. If intervention were motivated by IP reasons, we would expect the pattern of protection to be skewed towards activities where positive externalities or market failures are largest. Instead existing evidence suggests that protection is motivated by optimal tariff considerations, for revenue generation, or to protect special interests.

On the evidence for Marshallian externalities (i.e., the benefits of geographical concentration) they find that "agglomeration may be

necessary but not sufficient for increased productivity. . . . To put it crudely, subsidizing the software sector may not generate a Silicon Valley in a developing country" (HRC 2009: 35–36).

Third, from the cross-country studies, HRC (2009: 38) find that "in the late 20th century, in contrast to the last century when industrial countries protected emerging industries, it appears that trade barriers are often designed to protect 'sunset' industries rather than to encourage 'sunrise' industries."

Finally, on the voluminous literature, mainly econometric, on the relationship between trade openness and growth, HRC (2009: 3) conclude that "there was no significant relationship in the second half of the twentieth century between average protection levels and growth." However, there was "a positive association between trade volume and growth." This suggests that "any successful IP strategy must ultimately increase the share of international trade in GDP." Finally, "the fact that so many countries have been unsuccessful in offsetting the anti-trade bias of their interventions may explain why so many have failed to succeed at IP."

Murphy, Shleifer, and Vishny (1989: 1005) note that their model supporting a Big Push also calls for "coordinated investment across sectors lead[ing] to the expansion of markets for all industrial goods and can thus be self-sustaining even when no firm can break even when investing alone." This so-called coordination of investment plans is of course nothing else but the planning syndrome—that is, the search for a centrally determined investment plan that takes into account not merely current but all future changes in the demand and supply of a myriad of goods. It is well known that no market economy can attain the inter-temporal nirvana promised by the Arrow-Debreu utopian theoretical construct. But neither can the planners, as pointed out by Hayek and Mises in the interwar debate about the efficiency of Soviet-type central planning (Lal 1983). The collapse of this system is a conclusive empirical confirmation of the validity of the Austrian insight that, in the real world, imperfect markets are superior to imperfect planning.

So, what of the purported success of industrial policy in East Asia, which is credited with the rapid industrialization of Japan, Korea, and Taiwan? Noland and Pack (2003) have surveyed the numerous studies that have tried to answer that question. Their conclusions are particularly noteworthy because they start with the assumption that IP can work.

On Japan, the pioneer of industrial policy, Noland and Pack (2003: 36–37) find that "the empirical estimates . . . do not reveal robust evidence that selective intervention was welfare or growth enhancing in the period after post-War reconstruction." That performance "could be due to the inability of policymakers to identify market failures and design appropriate interventions." However, the more plausible conclusion is that "political considerations may have been central to this outcome" because "most resource flows went to large, politically influential 'backward' sectors." This conclusion conforms to the traditional theory of trade and welfare, which incorporates "political economy" considerations (Lal 2003).

In Korea, industrial policy in the mid-1970s was used for a heavy and chemical industry (HCI) drive to steer industrial output toward more capital- and technology-intensive sectors. The results were disappointing. Thus, the studies by Kim and Yoo find the policy was unsuccessful. Kim (1990: 42) found HCI "had the predictable result of generating excess capacity in favored sectors while starving non-favored sectors for resources, as well as contributing to inflation and the accumulation of foreign debt." Yoo (1990: 43) finds "that in macroeconomic terms the Korean economy would have been better without the HCI policy." His analysis of rates of return for HCI, light industry, and all manufacturing mirror Little's (1994) finding that social rates of return in Korea were inversely correlated with the degree of *dirigisme*.

On the role of industrial policy in promoting total factor productivity in Korea, as required by the infant industry argument, Lee (1996) found that for the period 1963–83, protectionist measures slowed the growth rates of labor productivity and TFP, and tax-subsidy policies had no impact on sectoral productivity growth. He concluded that 'the Korean data present evidence that less intervention in trade is linked to higher productivity growth" (Lee 1996: 392).

On the argument for inter-industry linkages and the potential welfare enhancing coordination of investments by government, Noland and Pack (2003: 46) note that "while government intervention might have reduced some interpersonal transactions costs, many of the potential externalities were presumably dealt with by Coasian agreements among the [chaebol] firms."

Finally, Noland and Pack (2003: 7–8) find no evidence to support the claim of Rodrik (1995) that Korean industrial policy was successful not by engineering an export but an investment boom.

On Taiwan, Noland and Pack (2003: 56) conclude (citing the survey of evidence by Smith 2000) that the empirical studies "fail to find links between the [industrial policy] interventions and sectoral TFP growth or trade performance in the 1980s." Policymakers appear to have been motivated more by "political economy considerations, such as sectoral employment, the presence of large firms, or the degree of sectoral concentration, than by dynamic comparative advantage."[8]

Rosenstein-Rodan (1961) also had a second string in his bow in arguing for a Big Push, which was later taken up by Murphy, Shleifer, and Vishny (1989). His argument concerned the indivisibilities in the provision of non-tradable social overheads like power and transport needed by all industrial producers. This was of course the policy recommended to Africa in the 1960s. Many donors financed various public sector infrastructure projects in anticipation of future demand, and with little provision for their efficient maintenance. Africa is littered with the rusting remains of many of these grandiose monuments of the last Big Push there, of which the Tan-Zam railway remains emblematic, particularly as the Chinese now seem to be donating large sums of money and expertise to develop African infrastructure as part of their sweeteners to acquire natural resource mines and wells. Whether these will suffer the same fate as the Tan-Zam railway remains to be seen.

There is also more detailed empirical evidence on countries that did try a Big Push in the past. Four were included in the Lal-Myint (1996) study—Ghana, Madagascar, Brazil, and Mexico. The results invariably were disappointing if not disastrous (as in Ghana and Madagascar). To promote the pursuit of such bad policies just because some theorists have now been able to clothe the theoretical curiosa of the older discredited development economics in some algebra is not only puerile but wicked—given the high costs that the poor people thus being experimented upon suffer.

So much for the efficacy of industrial policy being touted by the "new" development economics, based again on theoretical curiosa. If Third World governments choose to follow this path charted by the

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[8]A conclusion also supported by Little (1994) and Thorbecke and Wan (1999).

theorists commended by Meier, it will be as disastrous for the prospects of the world's poor as was the old development economics of their aged peers.

A Beijing Consensus?

Joshua Cooper Ramo, an analyst at the Foreign Policy Centre in London, has coined the term "Beijing Consensus" in opposition to the Washington Consensus that he seeks to bury (Ramo 2004). He rightly claims that China's 30-year growth spurt has been based on an eclectic mixture of institutional forms. He holds that state capitalism has been the dominant form and offers an attractive alternative for developing countries compared to the classical liberal model.

Yasheng Huang, an economist at MIT, has disputed the notion that China's growth has been uniform—first in a book *Capitalism with Chinese Characteristics* (2008) and most recently in a paper "Rethinking the Beijing Consensus" (2011). He distinguishes between two periods of Chinese growth: the more liberal entrepreneurial capitalist model followed in the 1980s and the more statist model since the 1990s. Huang (2011: 18) argues that many of the Western students of China have been misled in not distinguishing these two periods, as they have taken the first period in which growth was based on town and village enterprises (TVEs) as being led by these collective firms. This, he shows persuasively, is due to a statistical misunderstanding. The term used by the official Chinese statisticians includes "both TVEs controlled by townships and villages and TVEs controlled by private entrepreneurs." Disaggregating these data by ownership type, Huang found that "private TVEs absolutely dominated the total pool of TVEs."

This statistical misunderstanding has led many Western economists to describe the Chinese growth miracle as being based on a unique form of collective enterprise, when clearly (at least in the early reform period) it was not. As Huang (2011: 16) notes, the new *dirigistes* provide formal models that purport "to explain the performance of TVEs as an efficient substitute in a weak [institutional] environment." From their perspective, the rise of the TVEs challenges standard economic claims that "private ownership rights motivate entrepreneurs to invest and take risks"—more theoretical curiosa based on empirical sand.

Huang's second contribution to the debate on the two rival consensuses is to distinguish between per capita GDP growth and that of personal income (the income accruing to Chinese households) per capita. There was a large divergence between the two in the 1990s: real per capita income (output) grew at 8.1 percent between 1989 and 2002, while personal income per capita grew by only 5.4 percent a year. Given the ongoing financial repression, the difference between the two was appropriated by the state. Despite similar GDP growth rates in the two periods, Huang (2011: 5) observes that "Chinese personal income grew fastest during the 1980s and then slowed dramatically during the 1990s, whereas GDP growth remained unaffected." He emphasizes that those "trends correlate closely with the different economic policies." China followed the more market-oriented development in the first period and the more statist one in the second.

The difference in GDP growth and in personal incomes has provided the financing of a massive increase in public investment—not least on infrastructure in the stimulus during the recent global economic crisis—as well as the massive buildup of foreign exchange reserves. Yao Yang (2010) of Peking University has recently agued this this investment has very low rates of return. China's accumulated foreign exchange reserves of more than $3 trillion, held in large part in U.S. and eurozone public debt instruments, are beginning to look more like the unprofitable deployment of Japanese surpluses by its private sector in the 1980s. Those funds were used to go on a shopping binge from Hollywood movie studios to impressionist paintings, which had to be sold at a loss with the subsequent economic retrenchment when the Japanese stock market and property bubble collapsed in the late 1980s. Today, with the ongoing sovereign debt crisis in the eurozone and an incipient one in the United States, China's massive holdings of U.S. and European government debt are beginning to look like the subprime mortgages held by U.S. banks before the Great Recession, with dire consequences for the returns on this deployment of the savings of the Chinese people by the state.

Finally, China's large monetary easing during the global financial crisis has led to a bubble in the property market, again echoing pre-bust Japan. So, is the latest adherent to the "Asian" model likely to meet the same fate as its parent? I do not think so. For China, unlike Japan in the 1980s, is still in the catch-up phase of economic growth, and with savings rates remaining high until the

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"demographic dividend" ends in 2025, with an elastic labor supply in relatively free labor markets, and abundant industrialization opportunities for expansion beyond the coast, China should be able to maintain high growth rates in the next decade.

The dangers are longer-term, once the catch-up phase ends. Huang (2008, 2011) has argued that the state-led capitalist model China has followed since the 1990s is inferior to the Indian model (which is closer to the Washington Consensus). This inferiority is due to the stunted size of the Chinese indigenous private sector and the continuing reliance on financial repression. Huang's most alarming finding is that there has been a marked decline in total factor productivity in China since the late 1990s. India, he argues, has progressed further in financial liberalization, and its private market–led development model since 1991 has fostered indigenous entrepreneurship, whereas China is still dominated by highly profitable state monopolies. This has meant that India has generated growth rates close to China's in the 2000s with much lower investment. On the presumed failure of the Indian state to provide infrastructure to match China's, Huang notes that, in 1980, China started with an infrastructure disadvantage compared with India, and yet had spectacular growth rates. So like India today, "FDI and infrastructural investment played a minor role in China's initial economic takeoff" (Huang 2008: 268). They have followed rather than led growth. He concludes that India has a better chance of maintaining high, sustainable growth rates than China. So much for the Beijing Consensus replacing the Washington Consensus.

Conclusion

My conclusions can be brief. The "new" development economics, which it is claimed has overturned the classical liberal policy package identified as the "Washington Consensus" is just the old "development economics" refurbished with mathematical bells and whistles. Its "poverty traps" are just the old "vicious circles of poverty." The various arguments for industrial policy are just the old arguments for protection based on the "Big Push" depending on the irrelevant pecuniary externalities or Marshallian externalities of agglomeration and increasing returns. As with the old "development economics," the empirical evidence particularly for Japan, Korea, and Taiwan does not support these theoretical curiosa.

Similarly, the purported success of the statist Beijing Consensus in generating China's post-liberalization growth is based on a misunderstanding of Chinese statistics. The TVEs in the first period of Chinese growth in the 1980s were *private* not state enterprises. Hence, that period was characterized by policies close to the Washington Consensus. In the 1990s a more statist model was followed, with the favored state enterprises, financed by financial repression and a big squeeze on domestic consumption, implying smaller increases in welfare compared with the 1980s. The declining productivity of the investment surges since the 1990s has led to a marked decline in total factor productivity growth since the late1990s. By contrast, India's post-1991 liberalization based largely on the Washington Consensus has generated close to Chinese growth rates of domestic consumption, at lower costs in terms of savings and investment, and has a better chance of maintaining high sustainable growth rates than the current statist Chinese model.

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