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The Anarchy of Success

By [William Easterly](#)

The Drunkard's Walk: How Randomness Rules Our Lives

by Leonard Mlodinow

Vintage, 252 pp., \$15.00 (paper)

Bad Samaritans: The Myth of Free Trade and the Secret History of Capitalism

by Ha-Joon Chang

Bloomsbury, 276 pp., \$28.95

Among economists, the countries most famous for rapid economic growth are the East Asian "Gang of Four": Hong Kong, South Korea, Singapore, and Taiwan. Between 1960 and 2007, incomes in these countries grew on average by more than 5 percent each year. They have been joined recently by China, whose extraordinary economic development has given it annual per capita growth over the same period of 6.2 percent, second only to Botswana, with 6.5 percent. This means that the average income in China was seventeen times larger in 2007 than in 1960. [11](#)

The success of the East Asian Gang of Four—and now China—has exerted an irresistible lure to researchers of growth. Academic economists who were used to studying whether a politically difficult tax reform might make Americans better off by an amount equivalent to 0.1 percent of US GDP rushed into a field of inquiry that promises to explain how to increase your income seventeen times over. Theoretical breakthroughs in the late 1980s by Paul Romer (now at Stanford) and by Nobel laureate Robert Lucas helped inspire a remarkable effort by economists to find in the empirical data which factors reliably lead to growth. Yet hundreds of research articles later, we wound up at a surprising end point: we don't know.

In 2003, Arnold Harberger, a free-market economist from the University of Chicago, observed that "there aren't too many policies that we can say with certainty...affect growth." A year later, a group of famous economists (including some on the liberal end of the spectrum like Paul Krugman and Joseph Stiglitz) produced something called the Barcelona Development Agenda that announced: "There is no single set of policies that can be guaranteed to ignite sustained growth." And in 2007, the dean of growth research, Nobel laureate Robert Solow, said: "In real life it is very hard to move the permanent growth rate; and when it happens...the source can be a bit mysterious even after the fact."

In view of this acknowledged ignorance, how can there still be so many writers who claim to know how to promote growth? *The Drunkard's Walk* by Leonard Mlodinow offers a crucial insight. Humans are suckers for finding patterns where none really exist, like seeing the shapes of lions and giraffes in the clouds. It wasn't that economists had no explanations of

what causes growth. On the contrary, we had too many. One survey of the field counted no fewer than 145 separate factors that had been found to be associated with growth. But most of these patterns were spurious, because they failed to hold up when other researchers tried to replicate them. Economists can say something useful about economic success, but we have to clear away a lot of false overconfidence before we get to that point.

In *Bad Samaritans*, Ha-Joon Chang is both a critic and a purveyor of such overconfidence. He rightly criticizes those who have made overly strong claims for free trade and orthodox capitalism, but then he turns around and makes equally strong claims for protectionism and what he calls "heterodox" capitalism, which includes such features as government promotion of favored industries, state-owned enterprises, and heavy regulation of foreign direct investment. He seems unaware of both the perils of spurious patterns and the failure of most previous research to establish truly verifiable patterns for explaining growth—making in his own argument many of the common mistakes Mlodinow identifies.

Let's first give Chang credit for his exposé of the dogmatic cheerleaders for free trade as a prerequisite for growth. According to Chang the worst offender in this group is the *New York Times* columnist Thomas Friedman, who in his best-selling book *The Lexus and the Olive Tree* identified a "Golden Straitjacket" of conditions that were allegedly necessary for a country to escape poverty: privatization, small government, free trade, deregulation, etc. Chang correctly ridicules Friedman for statements like "Unfortunately, this Golden Straitjacket is pretty much 'one-size fits all.' ...It is not always pretty or gentle or comfortable. But it's here and it's the only model on the rack this historical season." These statements have no basis in any body of evidence, since we have just seen that economists have had no success in finding a surefire path to success.

Unfortunately, Chang then makes outlandish claims for his own preferred policies. He argues that the success of such countries as the East Asian Gang of Four can be replicated by other countries—he cites Mozambique as a possible example in his introduction—if they embrace "heterodox" policies mixing trade and protectionism. At another point, he suggests that his favored approach would allow a country to increase its income eightfold over a couple of generations.

How does Chang fare on the "patterns in the clouds" test? The first fallacy he commits is what Mlodinow calls mixing up conditional probabilities. Mlodinow explains this by relating the story of a doctor who told him, on the basis of a test result, that there was a 99.9 percent probability that Mlodinow was HIV-positive ("I'm *really* sorry," the doctor said). After initially panicking, Mlodinow figured out that the correct probability that he was HIV-positive based on the positive test result was only about 9 percent; subsequent testing indeed confirmed that he did not have HIV. The doctor had based his erroneous verdict on the fact that people who do have HIV will test HIV-positive 99.9 percent of the time. In other words, the probability *if* HIV-positive, *then* test positive was 99.9 percent. But Mlodinow, unlike the fallacy-prone doctor, knew that the relevant probability for him was the reverse: *if* you test positive, *then* what is the chance you are HIV-positive?

This is a lot different because only about one in 10,000 heterosexual, non-intravenous drug using, white males (Mlodinow's category) is truly HIV-positive. So in a sample of 10,000 in this category, the one HIV-positive man will test positive. However, since one out of every

1,000 HIV-negative men will also falsely test positive (giving the seemingly impressive test reliability statistic of 99.9 percent), there will be ten men out of 10,000 who falsely test positive in addition to the one who is really HIV-positive. So for someone like Mlodinow the probability that if you test positive, then you are HIV-positive is only one out of eleven, or 9 percent.

Chang makes exactly the same mistake as the doctor: he bases most of his argument on the probability that *if* you are a successful East Asian Gang of Four economy, *then* you have East Asian Gang of Four policies. This probability is 100 percent by construction. But for another country trying to assess these policies, what it needs to know is: If I adopt East Asian Gang of Four policies, how likely is it that I will have economic growth comparable to the East Asian Gang of Four? Chang never attempts to compute this radically different probability. To do so, we would have to take into account all countries that have tried protectionism. This group would now include some huge disasters that were very protectionist, like Tanzania from the 1960s through the 1980s, when output per factory worker was steeply declining despite huge state investments in manufacturing. The Morogoro shoe factory, established in Tanzania during this time, was supposed to become a major exporter but never produced more than 4 percent of capacity and then went out of business.

Chang cites examples of state companies like Singapore Airlines, which were supported by subsidies for years before they ever generated profits, to argue that "countries should defy the market and enter difficult and more advanced industries if they want to escape poverty." We all love stories of success after episodes of failure. And indeed the probability that if you are today a success, then you had some past episode of failure on your way to success is quite high. However, the probability we need to know is: Among enterprises that were chronic poor performers, how many of them later became successful? The answer is, surprise: most failures continue to fail! Kenya Railways chronically fails either to provide decent service or to cover its losses despite decades of turn-around attempts. The state-owned Ajaokuta Steel Company in Nigeria went through \$6 billion trying to "defy the market" but has yet to produce a bar of steel. There are also airlines in Angola, Benin, the Democratic Republic of Congo, Equatorial Guinea, Indonesia, the Kyrgyz Republic, Liberia, Sierra Leone, and Swaziland, all of which are so bad that the European Union has banned them from European airspace.

These examples hint at the possibility that getting the wrong conditional probability can be positively harmful. But was Chang at least correct in concluding that most successes were to some degree the result of "protection, subsidies and regulation"? This is one of the more novel parts of his argument: he suggests that the rich North American and European countries followed such protectionist and nationalist strategies before they became advanced economies. Hence, they are being "Bad Samaritans" when they advise developing countries to pursue laissez-faire free trade, thus "kicking away the ladder" that they themselves climbed to succeed. Chang is at least partly convincing: there were more departures from laissez-faire in American and European history than some ideologues would like to admit. But how good is his overall case?

First of all, the shift in development advice in the 1980s toward free trade and free markets had nothing to do with the economic history of rich countries themselves. Their own experience never had much part in development thinking whether in the 1940s, the 1980s, or any other time. Early development economists dismissed the relevance of such history for two

main reasons. First, they aimed to find rapid solutions to world poverty, and the development of Europe and America had been too gradual. Second, in the 1940s, European-American capitalism had been discredited by the Great Depression and US state planning during World War II.

Both of these reasons caused economists from the 1940s through the 1970s to recommend the kind of inward-looking state planning that Chang likes. What later shifted the thinking toward trade and markets, ironically for Chang, was the unexpected rapid growth of the East Asian Gang of Four. Chang and those he criticizes both agree: "just be like Korea." They just don't agree on what it entails to "be like Korea."

This is what Mlodinow calls "confirmation bias." When the evidence is mixed, we tend to select the parts of the evidence that confirm what we already believe. Pro-traders such as Anne Krueger, the chief economist of the World Bank in the 1980s, noted that Korea's spectacular growth was based on heavy participation in global trade, and especially on the great success of Korean exports. Chang and other protectionists, in contrast, attribute Korea's performance to significant tariffs or quotas on *imports*. Similarly, Chang chooses the Gang of Four as success stories to imitate, because they have had the highest per capita growth in the world since 1960—except for one other country, Botswana. Yet he nowhere discusses this other case of remarkably high growth. Is free-trade, free-market, democratic Botswana left out because it goes against his confirmation bias? Worried readers may want to know.

Chang shows other signs of confirmation bias. His main piece of evidence for the superiority of heterodox policies, which he repeats over and over throughout the book, is that developing countries grew during the "heterodox" period of the 1960s and 1970s "on average, at double the rate" they have since the 1980s, when "neo liberal" free trade policies became orthodoxy. The big question is, what year should we pick as the breaking point between the protectionist era and the free-trade era? It is easy to manipulate breakpoints to confirm your beliefs.

Chang picks 1980 as the big turning point, allowing him to calculate a 3 percent average per capita growth in developing countries between 1960 and 1979, with 1.7 percent average growth in the same countries from 1980 to 2002.^[2] But Chang elsewhere suggests that the change in policy occurred around 1983. The developing countries, he writes, were "first pushed by the IMF and the World Bank" to liberalize trade "in the aftermath of the Third World debt crisis of 1982." But if we take 1983 as the breaking point, the change in growth rates is less dramatic: 2.6 percent between 1960 and 1982 as compared to 1.8 percent between 1983 and 2002. (There was a big recession in 1980–1982, so it's a little suspicious that Chang includes this bad time in the free-market period even though he says the policy change wasn't until 1983.)

Another classic way to check before-and-after claims is to find some new data. We now have data up through 2008. If we include this most recent data *and* use Chang's own policy breaking point of 1983, there is virtually no change: growth in developing countries was 2.6 percent between 1960 and 1982 and 2.7 percent between 1983 and 2008. Chang's key piece of evidence goes up in smoke once we correct for confirmation bias.

What about earlier American and European economic history? Although this history has not been influential for development economists, it is obviously relevant. Unfortunately, however,

Chang has once again picked the episodes that confirm the story he wants to tell. He shows that US trade policy has swung back and forth between protectionism and free trade, but he does not mention that the average per capita growth rate in the US, regardless of the policy direction, has been remarkably stable for two centuries. Nor does he note that rich countries have often protected agriculture rather than industry.^[3]

In contrast to their protectionist policies at home, Chang suggests that Europe and the United States often imposed free trade on developing countries during the colonial era. But the concept of "free trade imperialism" was contradicted decades ago by the economic historian D.C.M. Platt.^[4] Yes, the mother country wanted raw materials from colonies and other sources of imports, and discouraged industry on some occasions, but Platt showed that there is no general historical pattern of "free trade imperialism." More importantly, Chang overlooks the greatest barrier to free trade in poor countries—transportation costs. With long distances separating them from Europe and America, and even more drastically primitive and scarce transport infrastructure of their own, the poor countries had plenty of "protection" against cheap imports into the mid-twentieth century.

Meanwhile, there was burgeoning trade among European countries by rivers, canals, railroads, and short coastal voyages. The eastern ports of the United States and the western European ports were connected by good ocean routes, and in turn were connected to vast interior markets by good infrastructure. During this long period in which non-European countries were falling further and further behind, it would be hard to argue that the rest of the world was de facto more "free trade" than Europe and America. If we broaden the question to consider which regions had institutions like property law and commercial banks to support "free markets," the consensus is that the rest of the world was in fact less "free market" than Europe and America. So while Chang shows that the history of industrialization in the now-rich countries was far more complicated than an embrace of laissez-faire, he misses how those countries leaped ahead over a very long period when they were overall more free-trade and more free-market than the rest of the world.

But what about the failure of economic research to establish what determines growth, cited at the beginning of this article? Was it because of these same fallacies? Most growth researchers did not reverse conditional probabilities; instead of focusing only on what policies the success stories followed, as Chang does, we compared the average policies in the failures to those in the successes. This knocked out the case for protectionism and industrial policy, because these were just as common among failures as among successes. But it also knocked out the opposite case for free trade and free markets, because they likewise failed to show a reliable association with per capita growth over the short to medium run.

Unfortunately, we had too many answers to how to raise growth. The 145 separate "answers" arose because of a slightly more sophisticated version of "confirmation bias." When you study the effect of a particular policy on growth, you also have to control for some of the other factors affecting it, for which there are practically endless possibilities. There are also many possible ways to measure the policy you are studying. All of these possibilities suggest that you can keep trying lots of different statistical exercises until you get the result that conforms to your prior beliefs.

To the credit of economists, at least we have ways of catching confirmation bias such as adding new data like I did with Chang's before/after exercise above. But economists, including Chang and almost everyone else, have also been suffering from a third common fallacy, what Mlodinow (following Nobel laureate Daniel Kahneman) calls the Law of Small Numbers. This is a sarcastic reference to the Law of Large Numbers, in which you can have a high degree of confidence in the average value of a sample if the sample includes a very large number of observations. The Law of Small Numbers is when you stop way short of having "enough" observations and show high confidence anyway. The Law of Small Numbers is our tendency to judge performance by too small a slice of experience.

Early development economists made this error when they dismissed Euro-American capitalism on the basis of the Great Depression and the good performance of Soviet growth during the same period. These turned out to be aberrations in the longer run of economic history, in which capitalism has decisively outperformed central planning. Similarly, Chang at one point suggests as evidence that free trade isn't working the fact that Mexico had only 1.8 percent per capita growth from 1994 to 2002—the period following the enactment of the North American Free Trade Agreement. But economic growth is so volatile across and within countries that it takes a while to decide what policies are having a positive effect on growth.

This suggests the possibility that if you get a result associating high growth with a particular country as characteristic in one period, it is likely to vanish in the following period. Chang himself shows this amusingly in the spurious correlations that have often been drawn between culture and growth. Being Protestant was obviously the best thing for growth, Max Weber argued; just look at England and America. But then Catholic Ireland, Portugal, and Spain started growing faster than the Protestant countries! Well, at least Max Weber was right (looking back at the nineteenth century) that Confucianism was bad for growth. But now Confucian East Asia is growing faster than anyone else. So let's celebrate "Japanese values" after remarkably high Japanese growth between 1950 and 1990; but no, Japan has mostly stagnated since 1990, so now Japanese values are not so attractive. The lesson is that as soon as you have come up with a persuasive "explanation" for growth, you will be surprised by its failure and the emergence of a new one.^[5]

Why is growth so unstable? Among many other reasons, countries suddenly discover "big hits" in export markets and then get a burst of growth exploiting that particular breakthrough. Ken-yans captured the European market in cut flowers, Fijians broke through in the US market for women's cotton suits, and Egyptians seized the Italian market for bathroom ceramics. What heterodox or orthodox recipe would have led you to tell the Egyptians: "just sell toilets to Italians"? "Randomness" in economic success is not random in the sense of somebody flipping a coin, but rather in one's inability to predict who will be in the right place at the right time to sell the right thing to the right people. This is why the Law of Small Numbers is damaging—you jump to erroneous conclusions too quickly.

One way to escape from the Law of Small Numbers is to seek to explain levels of per capita income already attained today rather than rates of growth. The level of income you have reached today frees you from small numbers because it reflects the outcome of your entire previous growth experience. So let's ask, who are the richest and the poorest countries now, and what is the difference between them? I argued above that the now-rich countries leaped ahead during a long period in which they were more free-trade and free-market (although far

from laissez-faire) than the rest of the world. Economists therefore have a much higher degree of consensus on this question—both evidence and intuition suggest that such things as education, private property, contract enforcement, and freedom from government expropriation contribute to economic development.

These findings just confirm the Western consensus around the basic concept of a state shaped by representative democracy, safeguarding individual rights and supplying crucial infrastructure such as transport, while rewarding entrepreneurship and technological creativity. Such common-sense ideas have stood the test of time over the very long run, both in their acceptance by the population in most economically successful societies (compared to their absence and rejection in unsuccessful economies) and in their pragmatic consequences for prosperity (as showed by the comparison to the poverty of states that lack most of the above conditions). Development is too complex to fit these ideas 100 percent of the time, of course—there are authoritarian exceptions like Singapore. China is not yet an exception because its income level is still less than one tenth that of the US. We can conjecture that China's rapid change in income has followed positive changes in individual economic (and even some political) rights.

This does not imply the extreme free-market orthodoxy imputed by Chang to his opponents. Prosperity is not a Thomas Friedman straitjacket, which comes only in one color, Golden. It is more like—apologies for stretching this metaphor too far—a Soho retailer that lets many different customers pick and choose which among fairly similar items of clothing will make them look their best.

But getting it right in the long run still leaves huge dissatisfaction, because most of us want to know how to reach a good outcome much faster. It's not good enough to gradually develop the same way the rich countries did, when so much poverty and misery exist in the third world. The profession of "development expert" exists expressly to address this problem. But despite sixty years' worth of development research, the third world has not come any closer to catching up to the first world—it has grown at the same per-capita rate as the first world.

However, poverty as defined by an *absolute* poverty line has declined. The average growth in developing countries from 1960 to 2008 was 2.7 percent per year in living standards, increasing income per person by 3.5 times over that period. There was only low and erratic growth in these same countries before 1960 (which is how they wound up being classified as "third world" as it was defined at about that time). The 2.7 percent growth per year means the last half-century has featured the greatest escape from absolute poverty in human history. Absolute poverty is defined as the percentage of the world's population living on a dollar a day or less (adjusted for purchasing power), which declined from 32 percent in 1960 to 10 percent today. The third world is approaching an absolute income level that will see the end of extreme poverty, even if it is not catching up in relative terms to the still-growing first world. Rates of child mortality have plunged over the same period, and democracy and individual rights reached a greater proportion of humanity than ever before.

Few of these gains appear to have anything to do with particular expert recommendations on development strategies. Despite Chang's air of desperation about the experts getting it right (something shared by some of Chang's free-market opponents), in the end, third-world growth seems to have been fairly expert-proof. Perhaps prosperity is not after all designed from

above; perhaps it emerges from below, from the independent actions of many individuals who figure out their own paths.

In the end, Korea didn't need experts like Ha-Joon Chang as much it needed entrepreneurs like Ju-Yung Chung. Chung was the son of North Korean peasant farmers, who had to leave school at fourteen to support his family. He had failed at successive jobs as a railway construction laborer, a dockhand, a bookkeeper, and a deliveryman for a rice shop in Seoul. At age twenty-two, he took over the rice shop, but it failed. He started A-Do Service Garage to do auto repair, which also failed. At age thirty-one in 1946 in Seoul, Chung again started an auto service, which finally became his first successful business. That auto service grew and diversified over the years. It is now known as Hyundai.

Notes

^[1]With the caveat that Chinese economic statistics have attracted a lot of doubters, for both the period of central planning before 1978 (when economic growth does not have the same meaning for local well-being) and the more free-market period since.

^[2]Chang doesn't give his end point for this particular calculation, but he apparently stopped somewhere around 2002, which is the end date that allowed me to reproduce his number, and was likely the most recent data available to him at the time of researching the book (and he does give this end point for other calculations in the book).

^[3]Chang could have learned this from a review of his previous book, *Kicking Away the Ladder: Development Strategy in Historical Perspective* (London: Anthem Press, 2002), in EH.net by Douglas Irwin, April 2004.

^[4]See, for example, D.C.M. Platt, "The Imperialism of Free Trade: Some Reservations," *The Economic History Review*, New Series, Vol. 21, No. 2 (August 1968), pp. 296–306.

^[5]Dambisa Moyo's otherwise sound new book, *Dead Aid: Why Aid Is Not Working and How There Is a Better Way for Africa* (Farrar, Straus and Giroux, 2009), is marred by a similar overconfidence about policy and growth, in the opposite direction from Chang. To support her argument for pro-market, pro-globalization policies in Africa, she cites the last few years of high African growth, which again is way too small a slice of experience to conclude much of anything.