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The Cambridge tendency

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HIGHLIGHT:

Where is economics going? If the best young economists in the world are any guide, a good answer might be: back to Cambridge, Massachusetts

IN MACROECONOMICS, the pre-eminence of America's Cambridge was for many years the pre-eminence of a single institution; the Massachusetts Institute of Technology. MIT has been the professional home of Messrs Paul Samuelson, Robert Solow and Franco Modigliani, three of the five postwar giants of the discipline -- all of them legendary teachers as well as original thinkers who gave the field its modern shape. (The other two are Mr James Tobin at Yale and Mr Milton Friedman at Chicago.) They moved the spiritual home of twentieth-century economists from Britain to America -- but above all from Cambridge, England, to Cambridge, Massachusetts.

Economics in America's Cambridge reached its peak of influence in the 1960s; in the 1970s it fell quiet; now it is flourishing again. This article glances at the work of eight of the best young economists in the world. It finds that six of them work in Cambridge (and that the other two have connections there). To see the revival in context, start with a little history.

For the past 40 years MIT has stood for a certain way of doing economics. One strand in its approach had to do with method. By earlier English standards, the work going on in America in the 1950s and 1960s involved little chat, and a lot of maths. Yet these new tools were wielded undogmatically, to say what needed to be said more crisply. By the later standards of Chicago, say, or Minnesota (of which more later), this avowed pragmatism made Cambridge seem technically informal, even sloppy.

The other strand in the Cambridge approach was, for want of a better terms, ideological, MIT has always been passionately interested in economic policy. Such an interest takes two ideas for granted. First, that markets left to their own devices sometimes get things wrong -- otherwise there would be no job for policy to do. Second, that policy can sometimes have fairly predictable effects on the economy -- otherwise it would be pointless to ask what its job might be. Keynes never had doubts on either score. Neither did MIT.

The dominance of MIT, and of its way of doing economics, did not go unchallenged. The Keynesian consensus collapsed in the 1970s. On one side, it was undermined by the failure of traditional Keynesian policies. The rising unemployment of the 1970s did not respond to attempts to "reflate". The result of such attempts was stagflation: high inflation and high unemployment.

Keynesian thinking also faced a new attack at the theoretical level -- one that seemed to explain why reflation no longer worked. Professor Friedman had been criticising Keynesian policies from his monetarist perspective since the 1950s, without anybody paying much attention. But in a paper in 1968 he added a new ingredient that had nothing to do with monetarism. He argued that the Phillips curve -- the idea that there is a trade-off between inflation and unemployment -- was unstable in the short run and non-existent in the long run. Governments could not swap inflation for jobs. If they tried, they would get higher inflation with no durable fall in unemployment. The 1970s proved this right.

Mr Friedman's Chicago began to ride high. Its broader agenda -- which included monetarist strictures against the use of fiscal policy, and a free-market liberal distaste of government intervention of any sort -- became the new conventional wisdom.

Younger economists then took up the running. Theorists at Chicago, Carnegie-Mellon, Rochester and Minnesota founded the so-called New Classical school, partly on the presumption that markets left to themselves work well. An important element in their thinking concerned the role of expectations. This too followed on from Mr Friedman. The idea of an exploitable trade-off between inflation and unemployment was wrong, on his view, because the inflationary effects of expansionary policy would be anticipated: workers would demand wage rises to keep their expected real pay constant, thus preventing any increase in the demand for labour.

Mr Robert Lucas of Chicago and Mr Thomas Sargent of Stanford's Hoover Institution (formerly of Minnesota) are still the leaders of the New Classical group. In the 1970s they developed their enormously influential theory of rational expectations. This theory, often ridiculed by people who do not understand it, says something simple and plausible -- namely, that people learn from their mistakes. Yet once that observation is taken seriously, orthodox prescriptions of economic policy fall like skittles. Many of them, it turns out, rely for their effectiveness on the government's ability to fool people and keep on fooling them.

All this added up to a rejection of the economic role of government. But the challenge to the Cambridge tradition went further. The New Classical approach insists that economics has to account for the decisions of firms and people in ways that are consistent with the idea of "optimising behaviour". In everybody else's work New Classical devotees detect ad hoc assumptions about why firms and people do what they do -- assumptions that do not sit comfortably with the micro-foundations of economic theory. To avoid this trap, New Classical theorists have to make their work highly abstract. Models are exhaustively spelt out, and great ingenuity has to go into manipulating them.

Often the price of such rigour has been irrelevance. In making their theories tractable, yet consistent with their standards of professional decency, the New Classicals are often embarrassed by untidy facts about the real world. One of those facts is unemployment. Many New Classical economists argue that there is no such thing as involuntary unemployment (ie, that the unemployed freely choose to be out of work, given present wage rates and unemployment benefits). It is hard to say much else if one accepts the tenets of New Classicism -- that prices are flexible, that markets clear, and that people and firms act in their own best interests.

For a time it seemed that the New Classical school would come to dominate economics. The abstruse virtuosity of its practitioners appealed to students who enjoy difficulty for its own sake -- ie, the best ones. Moving with the fashion, the journals became even less readable for those without training in maths. Economics seemed about to get so clever that it could get along by itself for the next few decades, with little to learn from the world and nothing to teach it.

Eight of the best

Happily, it has not worked out that way. The world's best young economists have absorbed the lessons of the New Classical approach, recognising it for the breakthrough that it was. But, for many of them at least, technical brilliance is no longer to be pursued for its own sake.

Our eight representatives of the best of the new -- Paul Krugman and Jean Tirole of MIT; Jeffrey Sachs, Lawrence Summers, Gregory Mankiw and Alberto Alesina of Harvard; Andrei Shleifer of Chicago; and Sanford Grossman of Princeton (moving soon to Pennsylvania's Wharton School of Finance) -- are all at home with highly abstract economics. Even the least experienced have won their technical spurs. So they are now free to follow a less "rigorous", more pragmatic sort of research, where they think that this will be more fruitful -- and without having to face the charge that they are too dim to do the other sort.

But a bigger difference between the best of this youngish generation of academic economists and their New Classical forebears is that the newcomers -- regardless of the many differences in their style of work, field of research, and political sentiments -- are all concerned with market failure of one sort or another. And where markets fall there might sometimes be a role for policy.

That is why the Cambridge connection is more than a matter of mere geography -- though the geography is impressive. MIT is now having to share its prestige with Harvard, which has four of the eight. After many years of improving its economics faculty inch by inch, Harvard has lately taken several large strides. In Messrs Sachs and Summers it has two of the most respected economists in the profession, and (despite their relative youth) two of the most influential outside it. Recently it has polished its credentials by recruiting Mr Robert Barro, a New Classical star, from Rochester.

Moreover, thanks to Mr Martin Feldstein (himself a Harvard professor), Cambridge has become home to the National Bureau of Economic Research (NBER). This is a privately financed research group which recruits economists for spells of full- and part-time work on its programmes. The bureau has more than 200 associates and fellows engaged -- as it insists they must be -- in empirical analysis of the American economy. All but one of the men on our list (the exception is Mr Tirole) are on its books.

With MIT, a newly revived Harvard, and an increasingly energetic NBER all nestling side by side on the banks of the Charles River, it is beginning to look odd for any empirically curious student of economics to want to be anywhere else.

Our group of eight needs to be split into two. Messrs Grossman, Krugman, Sachs and Summers are still only in their mid-30s, but each already has a full career's worth of achievement to his name. Messrs Alesina, Mankiw, Shleifer and Tirole are, on the whole, a bit younger. They are attracting a lot of attention inside the profession, though not yet much outside it. But they are engaged in work which is likely to make them leading lights over the coming years. What pointers in the interest of this octet are there to the future course of economics?

Sandy Grossman would stand a good chance of topping a poll of economists asked: age aside, who is the cleverest of you all? Last year he won the intensely desirable Clark medal, awarded every two years to "the American economist under 40 who is adjudged to have made a significant contribution to economic thought and knowledge". Mr Grossman's main interest is the economics of information. His work is in the rational-expectations tradition, but by refining this sort of economics he has greatly altered the way economists think about it.

It used to be assumed, for instance, that stockmarkets were "informationally efficient". This means that, at any given moment, share prices take account of all the information there is to be known about the underlying shares. This does not imply that everybody knows everything. There will be informed traders and uninformed ones. Informed traders will bid prices up or down to accord with the value of the shares; uninformed traders can ask for no better information than the current price.

One difficulty with this, however, is that if everybody believes that shares are correctly priced, there is no incentive to look for new information. But if nobody looks for new information, that there can be no such thing as an informationally efficient market: the idea is self-contradictory. The answer to this puzzle -- which is called the Grossman-Stiglitz Paradox -- is to look more carefully at the conditions under which rational people will incur costs to get information.

Prices are influenced not just by the actions of informed traders, but also by economic shocks coming from beyond the market, and by the willingness of traders to accept the financial risk that the possibility of such shocks creates. This uncertainty means that trading takes place behind a "screen of noise". Mr Grossman devised "noisy rational-expectations" models which do indeed provide rewards for the discovery of new information. This gave birth to a new and still-expanding literature.

The same goes for other aspects of his work. This includes research (much of it in collaboration with Mr Oliver Hart, an Englishman at, yes, MIT) on corporate takeovers, contract theory and the theory of incentives. Across all these areas, Mr Grossman is exploring the consequences of imbalances in information (which are a sort of market imperfection) in a world where people behave rationally.

Paul Krugman is also much concerned with market failures, though in different contexts. His work on exchange rates, debt and trade has made him one of the world's top international economists.

His research on exchange rates began with oft-cited articles on currency volatility and on the mechanics of balance-of-payments crises. More recently he has been trying to fill a gap in the literature where the pure theory of exchange-rate target-zones ought to be. His work suggests, reassuringly, that target zones should stabilise exchange rates even within their bands -- target-zones should do more, in other words, than merely cut off the extremes of volatility. These articles use results drawn from the theory of options-pricing in financial markets -- an innovation that is likely to yield more in due course.

In his work on third-world debt, Mr Krugman found, at last, a valid use for the Laffer curve. Recall that the original version said a government will collect nothing in taxes if its tax rate is zero (obviously) and nothing if its tax rate is 100% (because nobody would go out to work). Somewhere in between, therefore, is a tax rate that maximises revenues. Mr Krugman's debt-relief Laffer curve" starts with the observation that a burdensome stock of debt is like a tax on good economy policy. The bigger the debt, the bigger the share of any increase in export revenues the country will have to pay to foreigners. This debt-tax might rise to the point where it is not in the country's interests to adopt sound policies -- especially if that would be painful.

The debt-relief Laffer curve pursues the analogy, putting debt where the tax rate used to be, and expected debt repayment (which depends on today's economic policies) in place of expected revenues. An interesting conclusion follows: for countries on the downward-sloping part of the curve, banks can increase their receipts by granting debt relief.

Mr Krugman's most notable work to date has been in the theory of international trade. This is truly path-breaking stuff. He is asking, to what extent does the presence of imperfect competition invalidate the case (hitherto thought to be rock-solid) for free trade? He considers different sorts of imperfection. One arises from the fact that some industries are dominated by a small number of large firms. This creates the possibility of "strategic interaction" between them: they will struggle to grab the excess profits earned in the industry (excess by comparison with the points of full competition). This creates, in turn, a chance that subsidies might raise one country's income at the expense of another.

Using methods from game theory, Mr Krugman clarifies these interactions and opportunities. In the process the pure theory of comparative advantage takes a heavy knock. Yet, after weighing the advantages and disadvantages of government intervention, Mr Krugman concludes that wise voters will serve their interests best if they shackle their governments to good old-fashioned free trade.

Harvard's sons

The other top-rank international economist on our list, Jeffrey Sachs, came to attention ten years ago with a series of papers

on the rise of unemployment in the 1970s. He stressed the importance of imperfections in labour markets and the links between different national economics, and was the first to look carefully at the connection in Europe between labour-market institutions and real-wage rigidity. With Mr Michael Bruno (now the governor of Israel's central bank) he wrote "The Economics of Worldwide Stagflation", which has become essential reading on the subject.

He is well known outside academia mainly for his work on international debt. He advised the Bolivian government on its recent economic-stabilisation programme -- the only one of Latin America's many such programmes to succeed -- and in its dealings with banks and the IMF. From full-blown hyperinflation (ie, five-digit annual inflation rates) Bolivia has returned to economic stability. It is now being rewarded (too modestly, in Mr Sachs's view with extra assistance from industrial-country governments. This adjustment was abrupt and painful: the public-sector payroll had to be slashed, though unemployment was already high. Mr Sachs agrees with Mr Krugman on the need to reward the countries that undertake such efforts; and that means debt relief.

In his more theoretical work on debt and development, Mr Sachs has been trying to sort out why some countries got into trouble in the early 1980s while others did not. Unless you are content to stop at saying "they borrowed too much", this turns out to be complicated. However, one recurring theme is the abject failure of fiscal policy. In many countries this was called as much by an inability to collect taxes as by reckless overspending. This work links up with his newer research on the comparative fiscal performance of rich-country governments. Here, an early result is that multi-party coalitions seem to be worse at fiscal policy than single-party governments. All this comes under the heading of "governability and economic policy".

Larry Summers would give Mr Grossman a run for his money in the cleverness poll. His genetic endowment is perfect. Mr Paul Samuelson is an uncle on his father's side, and Mr Kenneth Arrow (another Nobel laureate) on his mother's; both parents, naturally, are economists. He has ranged far and wide in his research. And although he has not made any field quite his own (as Mr Grossman has with information and rational expectations, Mr Krugman with strategic trade theory, or Mr Sachs with debt and fiscal policy), he has a remarkable knack of contributing something vital to every subject he lights upon. Releases from his recent pre-occupation with America's presidential election (he was one of Mr Michael Dukakis's advisers), he is getting back to research.

One of his main interests is labour markets. Starting from the fact which New Classical economists find so uncomfortable -- namely, that there is evidently such a thing as involuntary unemployment -- he asks why this should be. One answer might be that employers willingly (and rationally) pay more than the market-clearing wage to create a sort of rationing of jobs; this allows the firm to pick better workers. Moreover, a high-wage policy creates loyalty, and encourages workers to co-operate over demarcation, flexible hours, and so on. If such "efficiency wages" drive up average pay, the result might be higher overall unemployment than would otherwise happen.

A separate line of research is concerned with the persistence of unemployment. Here, Mr Summers helped to stimulate the current interest in "hysteria" models. These explain how a seemingly temporary rise in unemployment can become a longer-term rise if, for instance, firms regard spells of unemployment as a sign of job-hunter's fitness for future work. Once again, apparently national behaviour (ie, firms extract useful information from the fact that somebody is unemployed) becomes a cause of market failure.

Mr Summers has also done valuable work on the efficiency of financial markets. A recent paper showed that the conventional statistical tests have little power to decide whether financial markets are efficient or not; another shows that markets might be inefficient because, under some circumstances, arbitrage (ie, buying cheap and selling dear) cannot do its job. And a third major area of research is savings and the taxation of capital. This work is trying to say, for instance: how far taxes and inflation distort savings; whether bequests matter more than savings in the accumulation of wealth; and why people make the bequests they do (rational self-interest has more to do with it than you might guess).

The next stars

Unsurprisingly, the work of the second four overlaps, in varying degrees, with the work of the first. The range of Messrs Grossman, Krugman, Sachs and Summers is so wide that it could hardly be otherwise. In some cases the links extend to joint authorship of research papers. Plenty of opportunity for collaboration is one of the benefits of having a lot of talent in one place. Evidently, though, this need not preclude originality.

Albert Alesina has a foot in Harvard's School of Government as well as in its economics faculty. Aptly so. Partly in alliance with Mr Sachs, he is looking at the economic effects of the electoral cycle. A theme of a lot of recent economics has been the importance of "shocks": in the rational-expectations tradition, only unanticipated events have much effect. One of the biggest recurring shocks is a change of government. (Even if economic agents reckoned there was an 80% chance that Mr Bush would win the last election, his victory was still a "shock", because the 20% margin of doubt suddenly disappeared.) Despite this, econometric studies almost never allow for changes of government; they assume that changes in policy (eg, tax rates, interest rates) capture everything that matters. This is clearly wrong. Mr Alesina is opening up an extremely fruitful area for research. Like all

breakthroughs, one immediately asks why such work was not done years ago.

Gregory Mankiw (pronounced manqueue) has worked mainly on interest rates. Part of his research looks at an old puzzle: what determines the gaps between the interest rates on debts of differing maturities? The logic of efficient markets implies that this "term structure" reflects investors' beliefs about future rates. Mr Mankiw's investigations show that this cannot be right: yield gaps seem to embody strikingly poor interest-rate forecasts.

Another possibility is that the term structure compensates investors for differences in risk -- ie, long rates are high when long-term bonds are particularly risky. But Mr Mankiw finds this to be wrong too. There is no evident relationship between the term structure and risk. This suggests that investing in long-term instruments when long yields are high will fairly reliably deliver higher returns than investing in a series of short-term instruments -- and it does. Efficient markets are supposed to deny investors such straightforward chances to make money.

Andrei Shleifer is one of the three foreigners on the list -- and undoubtedly the best Russian neoclassical economist around. He has worked with Mr Summers on "noise-traders" in financial markets. A lot follows from assuming that there are two sorts of trader -- informed and uninformed. For a start, standard arguments (mostly due to Mr Friedman) about efficiency start to wobble. And they wobble in surprising ways. Mr Shleifer has shown, for example, that if noise-traders (ie, the uninformed) are in the market, adding informed people can destabilise it.

Separately, he has taken up some of the questions that Mr Grossman raised about takeovers. For example, what sort of stake in the business gives managers the best incentive to raise profits? (The answer, paradoxically, is not the bigger the better; the evidence suggests that once managers get too rich they seem to lose interest in profit and start to think about other things.) A third area of work has returned to some old ideas about development: to what extent does the fact of imperfect competition make it sensible for developing countries to try (as the conventional wisdom now tells them not to) for a "big push" toward industrialisation? All this is turning heads in admiration.

Arguably, Jean Tirole should stand with the first group rather than the second. (He is a Frenchman, by the way; according to one slightly exaggerated assessment, France's six best economists are now at Harvard or MIT.) Mr Tirole is already a leader of modern microeconomics -- which offers far less scope than the macro-branch of the subject for becoming well known outside academia. His new textbook on industrial organisation marks the restoration of that field to its proper place at the heart of microeconomics. Here, too, there was an earlier Harvard tradition of research, based on empirical observation combined with rather woolly theorising, that began to give way in the 1970s to far more rigorous modelling. Drawing on game theory and other strange techniques, the new approach began to make sense of strategic behaviour that had seemed theoretically unmanageable.

The point is this: in an imperfectly competitive world, the actions of one firm affect others, and this interaction will be taken into account by all sides in deciding how to behave. Even at its cleverest, traditional microeconomics never did justice to this elementary fact of life. Mr Tirole is in the vanguard of the new wave; in time it will raise microeconomics to a new level of relevance.

Good omens

The elements common to all this new research are clear enough. First, economics has lost patience with the paradigm of perfect competition. Analysing departures from that ideal is always going to be technically difficult, but economics cannot hope to say much about an imperfectly competitive world unless it tries to do so. This means taking seriously the idea that markets often fail to clear smoothly, or at all -- but then striving to account for this in ways that do not make implausible assumptions about why firms and people behave as they do. In every case, a vital question is: how do economic agents process the information they have to hand in forming their beliefs about the future?

This points to what will be discarded, and what will be retained, from the New Classical approach. Out will go the presumption that markets clear smoothly, that agents are always in an equilibrium which is to their liking. In will stay the idea that agents are not stupid, that they learn from their mistakes and draw intelligent inferences about the future from what is happening around them. For contributing this insight, and forcing the whole profession to deal with it, the rational-expectations revolution of Messrs Lucas and Sargent deserves loud applause.

Unfortunately, the second main finding is that economics is getting no easier. The modern stress on technical prowess is not fading, especially in the way the subject is taught. A recent survey of American graduate students asked what mattered most for success in economics: 57% said that "excellence in mathematics" was "very important"; only 3% said the same of "having a thorough knowledge of the economy", while no less than 58% thought that was "unimportant".

If that is depressing, take heart at these eight economists, and many of their peers. Their technical expertise is of the highest, but they would all be judged by what they can find out about the world. Their research, despite the maths, speaks of intense

curiosity about how real economies work. For a time it seemed that such passion might take second place to difficulty for the sake of difficulty. That would be the death of economics. Instead, economics is looking outwards again.

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GRAPHIC:

Illustration 1, no caption; Illustration 2, Sanford Grossman; Illustration 3, Paul Krugman; Illustration 4, Jeffrey Sachs; Illustration 5, Lawrence Summers; Illustration 6, Alberto Alesina; Illustration 7, Gregory Mankiw; Illustration 8, no caption; Illustration 9, Andrei Shleifer; Illustration 10, Jean Tirole

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