David Warsh, the former economics columnist for the Boston Globe, is one of only a handful of journalists working today with both a sophisticated grasp of economics and the ability to explain it to outsiders. His new book, Knowledge and the Wealth of Nations*, showcases both these skills, offering a graceful capsule history of the coming of age of modern, mathematically based economics – and, in particular, economist Paul Romer’s formulation of what has been dubbed “new growth theory.”

Here, we excerpt a witty (and accurate) explanation of how academic economics works, along with an introduction to the tale of Romer’s struggles to dig growth theory out of its rut and to convince a skeptical economics profession of the value of his radically different approach to explaining economic growth.

— Peter Passell

“The Meetings,” as its initiates usually call the annual convention of the Allied Social Science Associations, whose agenda is dominated by the American Economic Association, unfold each year like an urban Brigadoon, their location forever shifting among convention cities. On the first weekend after New Year’s Day, economists who are members of the AEA and various hangers-on gather in a big hotel – or in several big hotels – to give talks, learn about the newest ideas, hear the latest controversies ventilated, interview job seekers (or troll for job offers themselves) and just schmooze.

Something like 8,000 people attend; another 12,000 AEA members stay home, content to make their intellectual capital serve for another year. (They can skim the best papers when short versions are published in May in the AEA’s flagship journal, the American Economic Review.) The Meetings are where economics is ceremonially performed by its adherents; they are the capital of a law-governed republic of ideas. And every dozen or so years, as in 1996, the Meetings host unexpected dramas that push the profession beyond a tipping point.

The program listings make a 400-page book. Yet everybody seems to know just what to do – conversations are resumed more or less where they left off the year before. Great intrigues are plotted beforehand, then unfold in public with barely a clue as to the identities of the mugger and the muggee. Rank and privilege are clearly understood; there are very few disruptions, except for the occasional quack. Every year, a few journalists attend for the first time. Everything is open-ended, forward-looking. Talks are intense for two and a half days and then the meeting-goers are gone.

The AEA is, if not the most senior body of those who consider themselves scientific economists, at least the most visible forum in which they convene. Included are the people who win the Nobel Prizes, write the textbooks, coin the vocabulary, staff the President’s Council of Economic Advisors, counsel central bankers and Wall Street – and in the process, frame the understanding with which we discuss the economic issues of the present day. Above all, here are the people who teach the subject to the next generation in colleges and universities.

Indeed, the overwhelming majority of the association’s members are professors. With the exception of four scholars from the Brookings Institution, three ex-economists turned university presidents, and an economist from the Institute for Advanced Study at Princeton, the AEA hasn’t been headed by anyone other than a university professor since it was founded in 1885. Nor has the AEA been headed by anyone who was not a U.S. citizen – the three who were born in Canada included.

There are many ways an economist can make a mark other than as a researcher/teacher: he or (increasingly) she can run a
company, lead a university, make a lot of money, administer a foundation, be a central banker or become a policy maven, a measurement economist, a powerful adviser or even a politician. At least until recently, those who earn their living in financial markets made a sharp distinction between academic economists and markets types, meaning those whose instincts have been honed more by practical experience than by research and teaching. So significant a figure as Paul Volcker disparaged economic book learning, even though (or perhaps because) he received a master’s degree at Harvard in the early 1950s. His successor, Alan Greenspan, completed his New York University PhD 27 years after receiving his master’s, having first served a term as chairman of the President’s Council of Economic Advisors.

The National Association of Business Economists, which meets in the autumn, caters to more practical analysts working mostly for financial and industrial firms. The Academy of Management, for business school professors and consultants, meets every summer. And of course the overwhelming majority of participants in markets – executives, money managers, traders, accountants, lawyers, practitioners of all sorts – are not economists at all.

Some very good economists have gone to work on Wall Street in recent years, and a few remain active in the AEA even though their primary focus has become making money instead of creating a widely shared understanding of it. But the significance of the Meetings is that, at its highest levels, economics is a science practiced and overseen by a professoriate, like astronomy, chemistry and physics. It has been increasingly so ever since Adam Smith, who was, after all, the first economist to be based in a university (the University of Glasgow). This deeply structured community of self-selecting peers is the uppermost stratum of the world of technical economics.

It says “American” on the AEA program. But for the past half-century the Meetings have also served as economists’ de facto world organization. Beneath the umbrella of the Allied Social Science Associations, itself the relic of 19th-century battles among reformers, religious leaders, historians and economists that produced the AEA, there exists a loose hierarchy of professional economic societies – more than 50 of them, of which the AEA is the most important. (The American Historical Association long ago set up housekeeping on its own.) The more specialized societies are organized along both geographical and functional lines – the Western, Eastern, Southern, and Midwestern associations; the Finance Association; the Public Choice Society; the Union for Radical Political Economists and so on. All operate with the informal understanding that players who do well devising explanations that are persuasive to one group will want to present their material in the next higher venue until it is accepted or rejected by all.

The elite Econometric Society has a more international flavor than the AEA. It was organized in the 1930s, explicitly along global lines. Half its members reside outside the
United States, it selects presidents from rotating regions, and it has regular meetings on every continent and a world congress every five years. Its membership has two classes. Anyone is free to join, but fellows of the society are nominated by existing fellows. The fellowship itself then votes, and nearly two-thirds of all nominations are defeated on the first try. The result is a self-consciously international society of the crème de la crème of technical economists (580 fellows, 141 of them “inactive,” meaning mostly old, 4,910 members at the end of 2004) that deliberately does not seek the public eye.

Yet even though its intellectual level is pitched much higher, the Econometric Society in fact functions one notch below the AEA in terms of public recognition. For it is the AEA that aims to be the big tent, the forum to which all serious developments eventually must be brought for examination, comparison and collision, the math translated back to verbal formulation before the results are reported to the waiting public. Balkanization among professional specialties is forever a threat. And in recent years the European Economic Association has greatly improved its standing. Even so, there has been no serious challenge to the AEA’s hegemony.

Why does America dominate? Because it is by far the world’s broadest and deepest market for what economics has to offer – everything from cutting-edge research to routine instruction to mechanism design, strategic analysis and prognostication. The financial markets alone account for thousands of jobs for economists. The major banks and Wall Street firms have become founts of serious research. Mainly through the National Science Foundation, the U.S. government spends several hundred million dollars annually training economists and underwriting pure economic research. The opportunity to specialize, to burrow deep, to test wits against others of similar ambition, is greatest in America’s research universities (though its colleges are home to many active scholars, too).

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If you look at the list of AEA presidents in the past few years, a composite picture emerges of all the various ways there are to make a living. Among them:

Arnold Harberger, whose marriage to a Chilean woman foreshadowed the establishment of a strong and durable connection between Chilean technocrats and the University of Chicago – a tie that, by a series of infinitely small ripples, eventually changed the practice of development economics around the world.

Amartya Sen, who rose from a small Bengali village to become master of Trinity College (Cambridge, England) by dint of asking penetrating questions about the nature of wealth and poverty.

Victor Fuchs, who started out studying the economics of the retail fur trade, only to become one of the premier students of health care economics.

Zvi Griliches, a Lithuanian survivor of the Holocaust who zeroed in on the significance of hybrid corn and focused attention on corporate research and development.

William Vickrey, a brilliant Canadian eccen-
tric who had been all but forgotten by the profession until his former students lobbied for his election as president of the association and argued successfully for a Nobel Prize (he died three days after it was announced).

**Thomas Schelling**, the pioneering strategist who made (largely mathematical) game theory serve everyday economics.

**Gerard Debreu**, an austere Frenchman who codified mathematical economics while the Berkeley “free speech” movement unfolded outside his window.

Indeed, go back far enough and you’ll find John Kenneth Galbraith, a literary economist who criticized the profession in a succession of widely read books – and who is said to have won his presidency only after the immediate past president, Milton Friedman, angered the nominating committee (of which he was a member) by asserting that Galbraith “wasn’t an economist at all.”

Moreover, many of those who most profoundly affect economics never go to the convention, at least not until they have reached the point of universal recognition. Like any science, economics reserves its highest honors for those who operate in the realm of original research in such a way as to change professionals’ minds. Thus, a small corps of foreign honorary members of the AEA (the number is limited to 40) provides a way of recognizing the most distinguished thinkers in other lands – 14 from the United Kingdom, six from France, four each from Israel, Germany and Japan, two Indians, and single representatives from Australia, Belgium, Hungary, Spain, Sweden and Switzerland.

Senior figures who are not expected to be called upon to be president, but whose contribution is judged quite remarkable nonetheless, are named distinguished fellows of the association at the rate of two a year – a consolation prize of sorts.

At the very pinnacle of the profession, at least in terms of prestige among outsiders, are the Nobel laureates. The Nobel award for economics is a recent invention. It is reserved for people who have been able to make some dramatic change in the tapestry, the vision of the field as seen by all economists. These thinkers often are not drawn from the same pool as the clubbable leaders of the association. (Academic diplomats make the best presidents.) There are overlaps, of course: Paul Samuelson and Milton Friedman were prizewinners and good presidents. Yet some Nobel laureates are persons who have worked in isolation, interacting intensely with small groups of other specialists but otherwise fairly aloof from civic affairs. There are even those who may not come to the Meetings until they are honored; a few never come at all. What these thinkers have in common is a very deep understanding of rules and ideas that govern debate in what they view as a republic of ideas. They are persons who, one way or another, have changed what it is that economists are able to see.

**A TOUR OF BRIGADOON**

The program of the Meetings is overwhelming, not so much the range of topics – from money and banking to health and environmental economics – but rather the bewildering array of mathematical and econometric tools employed in their discussion. Dynamic programming and matrix algebra. Markov processes and Euler equations. Nonparametric covariance estimation for space-time random fields. Even when economists are talking about familiar topics – self-confidence, say, or identity – their discussion is likely to be couched in terms of hyperbolic discounting and subject to probabilistic interpretation. Hundreds of sessions, dozens of topics, myriad points of view. Sometimes it seems no one
economist could comprehend it all.

Underneath, however, the social fabric of the Meetings is the very picture of normal science. Large numbers of people are occupied filling in details, elucidating puzzles, solving problems, creating tools, demonstrating applications. To the participants, this is the exciting stuff with which careers are built. To outsiders, much that goes on at the Meetings is boring.

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When big doings are at hand, however, when the system of belief is under stress, you can see people choosing sides—though not necessarily in the public sessions. To discover the institutions through whose workings technical economics changes from year to year, it is necessary to go behind the scenes to the social occasions where much of the convention’s real business is done. The very existence of these is easy to miss, though listings appear in the front and the back of the huge program book. The easiest place to discover them is the exhibition hall. It is here that the real money is made, with the first of several powerful institutions that concern us—textbooks.

Publishing is the operating arm of economics. Many books are on display in the exhibition hall, but the talk centers mainly on “adoptions”—which textbooks are being assigned in colleges, junior colleges and universities in the coming year. At more than $130 a copy (plus as much as another $25 for “sup-
Harvard and the University of Chicago to the Tel Aviv University, Moscow’s Plekhanov Institute of National Economy, and the Delhi School of Economics. How many enroll? Fewer than a quarter of that. How many finish? Not even most – the system churns out plenty of master’s degrees, and ABDs abound (students who have completed all but their dissertation).

The reaction to celebrities is no different at an economics fair than anywhere else.

Around 850 new PhDs have graduated annually from U.S. universities for the last few years, and perhaps as many again in other universities around the world. In contrast, some 15,000 physicians are minted annually in the United States, along with 4,400 dentists, 6,600 PhD engineers, 40,000 lawyers and about 120,000 MBAs.

The professorships and government appointments for which the best young economists eventually will compete are in many ways comparable to (even a bit more rewarding than) the situations of top doctors and lawyers despite a considerable pay gap. A few young economists’ salaries have reached $200,000 in universities and three times as much in some business schools. They can also garner a great deal of outside income – a proprietary software program, a consulting relationship, board memberships, expert witness fees. And, of course, there are always possibilities in the financial markets.

In economics, as in basketball, it is relatively easy to tell who has major-league potential. A walk-on genius turns up occasionally, to be sure. And every year a few prospects apply to economics rather than to some other field at the last moment. But in general most of the top candidates in each entering class have been identified by their colleges from the courses that they take as early as their junior year. Often these are more in mathematics than in economics, for mathematics is the language in which the basic business of economics is conducted, and proficiency in it must be acquired at an early age. So graduate programs compete among themselves to lure these students.

Faculties promise the most personal attention, the best teaching, the greatest support, the quickest and least disruptive paths to senior appointments. The very best students are awarded National Science Foundation grants and scholarships of as much as $50,000 a year to cover tuition and living expenses on the basis of their math Graduate Record Exams and letters of recommendation.

What makes a good student? It takes more than mathematics. Just because acolytes learn the intricacies of formal reasoning doesn’t mean that they necessarily will have anything to say. But, then, neither does background knowledge of specifics seem to have to do with it. Apt pupils come from all walks of life, and one of the best young economists of recent years lived in the former Soviet Union until he was 16. Scientific temperament is a plus (“desire to seek, patience to doubt, fondness to meditate, slowness to assert, readiness to reconsider, carefulness to dispose and set in order,” was how Sir Francis Bacon described it long ago). But the essential gift among those who will have an impact is an aptitude for “thinking economically,” for translating every problem into one that can be addressed by means of the discipline’s standard kit of tools and devising new tools as required.

The first year of graduate school is commonly described as a kind of a boot camp, in
which recruits are drilled in the use of the techniques and tools they must master in order to begin, ever so tentatively, their professional careers. The best schools don’t even bother with textbooks. Instead, there are lectures and assigned readings followed by endless sets of problems to be worked out. Students often compare the process to learning to speak a new language fluently. There are papers to be written, examinations to be taken to establish competence in broad fields and, all the while, the students’ endless conversation among themselves. Two years of courses are enough for students to master the basic tools of economics and the consensus that is the cutting edge of a particular specialty. Then comes a third year of seminars and topics courses in which it is said that students, having received the dogma, must learn to think for themselves. Then a thesis. Five years in all; six, perhaps, four for a few.

When the thesis is nearly complete, the about-to-be minted PhDs enter a job market that resembles a professional sports draft — one in which each year’s entrants are well advertised to potential employers in advance. Every year a few surefire stars join the league along with many journeypersons, the usual collection of utility players and dark horses. Some will go to work in industry or government without ever teaching. Only a handful from each graduating class will eventually enter the research hall of fame. But nearly all will enjoy a full career.

The third institution at the Meetings you are less likely to find on your own. It is the cocktail parties that are given in the evenings by the various university departments for
their alumni and friends. In principle, you are welcome without an invitation, but you may feel a little odd. If the shorthand of the corridors is somewhat telegraphic, the talk at the cocktail parties is even more abbreviated. Much is left out because it is already known. These are, after all, people who have been to the same boot camp. They entered the job market, went forward, changed the dogma – or failed to change it. Surprises occur. The facile kid gets stuck; the quiet one grows graceful. Completely unremembered faces return as peers or, more likely, take up secure positions in the provinces. People come together, briefly relive old glories of the early years, then go on to dinner in quite different combinations.

The best departments – those of MIT, Harvard, Chicago, Princeton, Berkeley and Stanford – remain on top, year after year. Yet change is forever under way. Departments are like so many major-league sports teams, not just hiring entry-level economists but also making offers (sometimes flurries of offers) to stars in other departments – in effect, free-agent signings. Somehow, the very top of the pole is forever up for grabs. For example, rich Princeton University has made offers to nearly 20 top people around the world. If all were suddenly accepted, Princeton might suddenly be considered the best department in the world. But there are powerful counterforces at work, too. Those who are raided raid back.

There are perhaps a dozen candidates for the next five spots in the pecking order – Yale, Cambridge, Oxford, the London School of Economics, Northwestern University, Boston University, New York University, the University of Pennsylvania, and the University of Michigan among them. Beyond that second tier are many other departments in good universities at which it is possible to do top-quality research. A circular interdependence is the rule: the best universities are the ones that get the best students, while they, in turn, attract the best teacher/researchers. But there are plenty of good students, enough to support well over 100 PhD programs in the United States alone, of which 40 or 50 are considered pretty good.

The final destination on our tour, and in many ways the most important, we will find only if we linger in the corridors after the cocktail parties break up, and meeting-goers stream out into the night to restaurants around the town. Throughout the hotels’ public floors, rooms have been put aside for smaller dinners. These are the editor/referee dinners of the journals, the most nearly invisible and most exclusive of our institutions – here, there is no sneaking in. It is at these dinners where departmental loyalties symbolically are divided, where economics’ ambition to be a true republic of ideas takes over from its more parochial concerns and institutional loyalties.

PUTTING IT IN WRITING

The peer-reviewed journal article is the basic vehicle of a career in economics, just as it is in every other scientific field worthy of the name. Journals, as someone has said, were invented (in the late 17th century) to publish fragments. The idea is to limit claims narrowly to what can be said with absolute certainty; to map them carefully into the tapestry of what is already known; to use citations sparingly to reward prior work and indicate where the new material fits in. Journal articles may be used to announce new discoveries, to comment on or criticize the discoveries of others, to synthesize and seek to build consensus about what is known. The subtlety of the form is very great. Above all, however, each new contribution to the literature must be honest and original. These standards are en-
forced through the tradition of refereeing.

Refereeing in economics (or any science) is not much different from what it is in sports, though the etiquette is more delicate. In economics, the referees don’t wear striped shirts, the job is usually done sitting down, and nobody does it full-time. The main difference is that scientific refereeing is performed anonymously. As science historian John Ziman says, “The mere fact that an author has a PhD – or is even a distinguished professor – does not ensure that he is free from bias, folly, error or even mild insanity.”

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So journal editors depend upon the advice of panels of friendly experts to read submissions and recommend their acceptance or rejection. The manuscript to be reviewed arrives in the referee’s mailbox with no name or affiliation. A recommendation is arrived at and explained and returned, also anonymously. Referees, of course, may instantly recognize the distinctive stamp of a colleague’s work, just as authors often are aware of their referees’ identities. It is in the nature of the virtual communities of experts scattered around the world by which cutting-edge research is performed—“invisible colleges,” Sir Robert Boyle dubbed them 350 years ago—that everybody in a particular subdiscipline knows everyone else. But the arm’s-length facade is maintained. Then the editor decides to accept or reject – and hopes that the published article will not be criticized or, worse, ignored.

It is this mechanism, which ensures that published research meets community standards of plausibility and honesty, that permits economists to utter the most strangely powerful words in all the vocabulary of science, “The profession thinks…” A useful definition has it that “economics is what economists do.” But this is mainly for the benefit of outsiders. What economists are trying to do among themselves is pin things down, to achieve consensus. Following Norman Campbell’s famous formulation of scientific aims, economics is “the study of those judgments [about certain phenomena] concerning which universal agreement can be obtained” – first from referees and eventually from the rest of us.

Notice something interesting, which will turn out to be important: Money has no place in this refereeing system. Yet most top economists spend a significant fraction of their time at the task. Journal editors assemble networks of researchers who have reputations for being the smartest or fairest persons on the leading edges of their fields or, ideally, both. These referees agree to read all new submissions and recommend for or against their publication. Often they suggest changes – or insist on them. Editors judge referees by their turnaround time, the degree of their constructive engagement with authors and their wisdom. The good behavior of referees is reinforced by the fact they, too, are scientists. Their own papers will be submitted to other referees.

Elaborate pains are taken to ensure that success in economics, as in any science, is
independent of wealth, prestige or connections. These efforts are not always successful. A good deal of back-scratching and favor-trading takes place. Still, the peer-selected meritocracy that is professional economics stands in sharp contrast to the social system of commerce and industry, where top-down orders are the rule and making money is the prime directive.

Indeed, refereeing is perhaps the single most effective way to demonstrate good citizenship and to assess character. Journal editors are selected from those who serve most successfully as referees. So are the officers of professional associations, such as the AEA. Only a few referees prove to be downright dishonest. They may press submitters for inappropriate citations of their own work, or even engage in “front-running” – trying to incorporate the freshest and most penetrating insights they glean from the work they have been asked to review into articles of their own.

But even the best referees can censor ideas that deserve to be heard. In fact, the most common failing is the tendency to attach too much weight to received opinion. Most pathbreaking contributions – not many, most – are rejected several times before they finally find a home.

That’s why there is not just one journal of economics but many, with overlapping interests. The “big four” mainstream journals publishing work of the broadest interest are the Journal of Political Economy, the Quarterly Journal of Economics, the American Economic Review and Econometrica. The Economic Journal, the flagship of economics in the United Kingdom for more than a century, will always have a sentimental place in the firmament, too, but it is now regarded as being well off the chase.

A second echelon exists, and a third and a fourth. Startups, such as the Journal of Economic Theory, occupy an important place in the production of the scientific literature; they can light up the sky for a time by taking the winning side in doctrinal disputes, eventually forcing others to imitate and conform. The sense of hierarchy, however – among departments, among journals – is always present.

**SETTING THE STAGE**

Now let us visit a particular set of Meetings – the convention of the American Economic Association held in San Francisco in January 1996. Our interest here is how a particular paper published in 1990, “Endogenous Technological Change,” precipitated some changes, first in the language of economics and thereafter in the wider world. For the purposes of our story, the San Francisco meetings were unusually interesting. It was there that the new ideas were presented to a general audience for the first time.

Those who attend each year’s Meetings are taking part in an intricately choreographed ritual that, for all its imperfections, serves to survey, summarize and put the stamp of social recognition on some portion of what is deemed to be the best formal work done by members of the profession in the year or three years or five before. I say three or five because there is something “rolling” about the procedure; there can be no attempt to have

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everything on the program every year. And besides, three to five years is about the time it takes to get a new contribution thought through, tested and ready to show.

I say formal because the papers presented at the Meetings are only a certain part of technical economics. The hardest work, real economics, is done elsewhere, alone or in intense collaborations. The work presented at the Meetings almost always has a long history as part of an individual’s research agenda.

The contributions that are reported may have begun in a moment of inspiration. But before they can make their way to the Meetings, models must be carefully spelled out, evidence assembled and drafts written before initial submissions can be posted to the community as “working papers” by first-draft publishers such as the National Bureau of Economic Research and the Social Science
The Milken Institute Review

The incoming president confronts a blank agenda that looks like a Yahtzee scorecard.

The incoming president confronts a blank agenda that looks like a Yahtzee scorecard. Only when a skein of work has achieved a certain degree of recognition as being significant in relation to all the rest of the stream of published research is it considered eligible for recognition at the Meetings. Then the wait for an invitation begins.

The assignment of the president-elect is to organize Meetings that will showcase the most interesting work across all fields. No one person can possibly have well-informed opinions about the 18 or so sub-disciplines into which economics is divided by the AEA. So for that purpose the new president appoints a program committee. Its members organize a majority of the invited sessions, while the president personally chooses three or four panels. The program of the Meetings only looks passive and impersonal. Inevitably, the program bears a strong element of personality.

The 1996 San Francisco meetings, for example, were organized by a Stanford University economist named Anne Krueger. Like every president, she had her story. Krueger had succeeded greatly in a world that until recently had been populated almost entirely by men. As adviser to the government of Turkey, she had been a contributor to the renaissance of market liberalism in the 1970s at a time when relatively heavy regulation of many aspects of economic life had been taken for granted by most economists. There were many other significant contributors to what in the early 1980s came to be known as the new political economy. They included many distinguished names, and to enumerate even the best known among them is to risk sounding like Homer’s Catalog of Ships.

By the early 1990s, the mood of perestroika was universal, and it was time to begin handing out honors. It was appropriate that someone from the new political economy community should get the nod. In 1996, it was Krueger.

The incoming president confronts a blank agenda that looks like a Yahtzee scorecard. There are a certain numbers of regular functions to be arranged. In San Francisco in 1996, a luncheon would be given for John Nash, winner of the Nobel Prize the year before. The mathematical genius had been confined to the sidelines for 30 years by schizophrenia, but he grinned sheepishly in the San Francisco limelight. Another luncheon featured Stephen Ross of Yale University, the generalizer of the capital asset pricing model. A scholar had to be invited to give the Meetings’ most important address (the Ely Lecture). Krueger chose Martin Feldstein, who used the occasion to call for the privatization of the U.S. Social Security system – striking evidence of the sea change in expert opinion that was taking place.

The outgoing president had to be intro-
duced; the health economist Victor Fuchs gave a jaunty address. A distinguished fellow of the association was to be named and presented. Walter Oi of the University of Rochester almost perfectly exemplified the charm of economics as a discipline in which pure thought may be as valuable as a computer or a scientific instrument. For despite being blind, the Chicago-trained Oi had made contributions to several fields over three decades, culminating in service to the commission that helped create the all-volunteer U.S. Army.

The biggest draw in San Francisco turned out to be a 54 year-old economist named McCloskey, a conservative former University of Chicago professor, president of the Economic History Association and member of the AEA executive committee who during the autumn had undertaken a change of gender. Donald had become Deirdre. (Many of these events spilled over into public view; McCloskey’s absorbing book about her metamorphosis was published in 1999 as Crossing.)

Another drama was unfolding behind the scenes: Graciela Chichilnisky was suing a colleague for allegedly having misappropriated her ideas. Chichilnisky still had some of the glamour that got her in Vogue magazine in the mid-1970s, when she was the well-publicized young Argentine genius with twin PhDs in math and economics from Berkeley. The halls were abuzz with gossip about her lawsuit. Within a matter of weeks Chichilnisky’s charges would be embarrassingly withdrawn, amid indications that she had fabricated her evidence.

The most startling event of the meetings was the ambush of David Card. Card had been awarded the John Bates Clark Medal, given every two years to the American economist who is judged by a panel of his peers to have made the greatest contribution by the age of 40. Card was handsome, rich and winsome. But he had coauthored a controversial book about experiences with the minimum-wage law that had angered the more conservative of his fellow labor economists. In a session just before the award, a group of Chica-goans on the dais jumped him with scathing critiques of his statistical methods. So on what otherwise would have been one of the happiest afternoons of his life, Card was rendered momentarily speechless. Nothing further was said at the time, but the incident would be long remembered.

With so many interesting people engaged in such various goings-on, it was easy to overlook the events at the San Francisco meetings that in time would have the greatest effect on the discipline. The more visible of these were to be found in a session devoted to recent developments among economists concerned with economic growth – its theory and its history.

Like most of the other discussions, Session 53 of the Meetings was held in a windowless room. The crowd of about 200 that gathered at 2:30 p.m. on Friday, January 5, 1996, in Plaza Ballroom B of the San Francisco Hilton was big enough to fill the long, narrow room and pack the back with standees – but not so large as to spill into the hall outside. “New Growth Theory and Economic History: Match or Mismatch?” met on schedule with a couple of dozen journalists in attendance; they knew that something was up. There was also the usual quota of young kids just starting out.

As we have seen, there are many perspectives from which to view a session. This one brought together two groups that usually don’t have much to say to each other: economic historians and theorists of economic growth. There was a picturesque quality to the occasion, for a dozen years earlier at the 1984 Meetings in Dallas a similar session had
been organized by the historians to consider
the “total darkness” that was said to exist at
the intersection of theory and history.

In 1984 the theorists were defensive. The
historians argued persuasively that theorists
had selected only those problems that could
be answered easily, giving rise to an elegant
but sterile formalism that had left a vast array
of disturbing considerations said to lie just
“outside” of economics, exogenous to its
models.

But the 1984 session had taken place on
the eve of a revolution. Barely a year later, the
University of Chicago professor Robert Lucas
reminded his colleagues of a riddle known
loosely as “the problem of increasing returns”
and, in a famous lecture, placed it at the cen-
ter of the policy questions of the 1980s. A
great debate had ensued. Most of the macro-
economists of the rising generation had taken
part. Factions had raised competing (and
confusing) banners: Neo-Schumpeterian Eco-
nomics, The New Economics of Imperfect
Competition, The New Economics of Techni-
cal Change, The Increasing Returns Revolu-
tion, New Growth Theory or, most simply
and obscurely, Endogenous Growth.

Whatever else, this particular intersection
was no longer dark in 1996. For a decade the
corner where theory meets history had been
illuminated by battlefield flares and muzzle
flashes. Yet the San Francisco session was
hardly one of emotion recollected in tran-
quility.

The historians and new growth theorists
talked at cross-purposes. Joel Mokyr, author
of The Lever of Riches: Technological Creativity
and Economic Progress, chaired the session.
He was the foremost of the new generation
of economic historians working on the problem
of technical change. He had a lively interest in
identifying the most important element in
growth, and he had organized a panel to per-
mit listeners to assess some of the new work.

From London, the historian Nicholas
Crafts sent a paper called “The First Industri-
al Revolution: A Guided Tour for Growth Economists.” Crafts was the proprietor of
what is known among economic historians as
the Crafts-Harley view of England’s industri-
al revolution: to wit, that there was no indus-
trial revolution, that the “take-off” into sus-
tained growth of the 18th century was an illu-
sion, and that the “years of miracles” (in a fa-
mous phrase) should be seen as a matter of
misleading index numbers.

Crafts’ paper cast a cold eye upon the re-
cent new growth theory, which is to say, the
work associated with Paul Romer. His tone
was gingerly skeptical of the new claims – re-
luctant even to confront them directly. The
new emphasis on market size seems to be
misplaced, he said; the evidence from history
was “less than compelling.” If there had been
a theoretical revolution, it was hard to tell it
from the historian’s response to new growth
theory.

CARVING ECONOMICS AT ITS JOINTS

Now it was Paul Romer’s turn. He was the one
the journalists were there to see. His reputa-
tion was as among the most mathematical of
the new generation. He was also seen as the
foremost young standard-bearer of the new
style of University of Chicago argumentation,
with its high-tech rigmarole: infinite-horizon
planning models, dynamic programming,
competitive equilibria reduced to maximiza-
tion problems for a single representative con-
sumer, rational expectations and all that.

Yet more than any other, it was Romer who
had put the new ideas about growth and
knowledge and market power on the table.
His 1983 PhD dissertation was characterized
by formidably difficult mathematics. The
1986 paper he carved out of it, “Increasing
Returns and Long-Run Growth,” remained difficult and austere. The 1990 version, “Endogenous Technological Change,” contained simpler math, thanks in large part to Robert Lucas, as well as a striking new formulation of the problem by Romer. That new formulation amounted to a bombshell, at least in the tight community of growth theorists, insofar as it placed him in opposition not only to his teacher Lucas but also to the entire Chicago tradition of taking perfect competition as its most fundamental assumption.

Romer had been insistent all along about the power of mathematical methods. They state problems more clearly and solve them with greater clarity and persuasiveness than any other method, he asserted – including field trips to factories or sifting through great quantities of data. He described the process. The evidence of the senses is where the theorist must begin, moving then to verbal description, to theorizing and up to formal math in a steadily ascending arc of ever greater generality – and then back down again from high abstraction to verbal formulation and the evidence of the real world.

This last step checks the math, he said. It is the only part of the process for which we needn’t take the theorist’s word (though, of course, by now many other theorists had become involved as well). But then, Romer also had often warned that logic and evidence “have a power that transcends the wishes, beliefs and preferences of the people who use them. When you start up the trajectory towards abstraction, you don’t know exactly where you will come back down.” He himself had found his politics altered by what he discovered in his equations – from acolyte of Milton Friedman in his freshman year in college to, as a professor, advocate of some new forms of government intervention.

For a decade the corner where theory meets history had been illuminated by battlefield flares and muzzle flashes. Yet the San Francisco session was hardly one of emotion recollected in tranquility.

Six years had passed since Romer first presented his most important paper and the issues had been thoroughly examined by the little community of growth theorists. Now in San Francisco he was trying to close the loop, to describe his model in simple terms, to compare it to alternative explanations, to turn its key equations back into English. His title: “Why Indeed in America? Theory, History and the Origins of Modern Economic Growth.”

He began with a defense of formal methods. Every time a new piece of mathematical formalism is introduced, he said, the same objections are raised. Some people complain “these equations are so simplistic and the world is so complicated.” Others claim that equations “don’t tell us anything new.” For example, he said, economists at least since Adam Smith have understood that total output depends on the quantity of physical capital and labor effort expended.

When, in the 1950s, a group of economists at MIT expressed this relationship mathematically as an “aggregate” production function, a group of British economists kicked up a
terrific row. Nevertheless, the idea of a production function – a mathematical statement of the relationship between inputs and outputs – had quickly become accepted around the world as an invaluable shortcut for measuring productivity.

When, a few years later, economists at the University of Chicago introduced new mathematical expressions aimed at capturing individuals’ accumulation of experience and education, it was the MIT economists (and others) who objected. Yet “human capital” as a gauge of personal productive capacities had become a standard tool. Now in the 1990s, Romer said, economists were arguing that knowledge was a key output of the economy and that its production is systematically related to the resources committed to its pursuit.

That the growth of knowledge depends on the number and quality of scientists producing new knowledge and on its prior stock was not exactly a shocking proposition. Yet expressed in formal mathematics as a differential equation, the new theory once again was encountering the usual objections. And this time, the resistance was coming from both Chicago and MIT. (Cambridge, England had long since been left behind.)

Such objections miss the point, said Romer. It is true that the introduction of mathematical language often seems to cause a neglect of important issues. Modelers focus on the issues that are easy to formalize and defer the more difficult issues for a later day, even though they may be recognized as being quite important. History suggests that as the math accumulates, its scope expands and the important questions do not go away. “The sensible approach is not to shut down the development of formal theory, but to tolerate a division of labor in which natural language and formal theorizing continue in parallel. Specialists in each camp can address those issues in which they have a comparative advantage and periodically compare notes.”

The appearance of new variables usually reflected an improved understanding of the economy as a whole. A good theory sizes up the overall system, then identifies certain points at which it can be broken down into a natural collection of subsystems that interact in a meaningful way.

To illustrate, Romer borrowed the familiar example of the steam engine. A famously bad explanation offered by an engineer in the mid-19th century ascribed its motion to a “force locomotif” (in a similar way Aristotle had attributed the fall of stones to their “inner nature”). A more satisfying explanation said Romer, divided the steam engine into component parts – firebox, boiler, steam governor and so on – in order to account for the working of the locomotive overall. “What theories do for us is take all the complicated information we have about the world and organize it into this kind of a hierarchical structure,” he said. By removing ambiguity and stressing
logical consistency, math makes clarity easier to achieve.

Next, Romer described the controversy in present-day economics. The “old” growth theory associated with Robert Solow explains growth through the interaction of two kinds of factors, he said. There are conventional economic inputs. And there is “exogenous”
technology, improving at a steady rate outside the system. At the next level of decomposition, conventional economic inputs are divided into physical capital, labor and human capital, but technology is still considered a force apart.

So far so good, said Romer, “because technology does differ from all other inputs” in that it can be employed by any number of persons at the same time. For reasons of convenience, however, old growth theory maps the dichotomy between technology and conventional economic inputs directly on the traditional distinction between universally available “public” goods and entirely private goods. For the purposes of the analysis, technology is viewed as being essentially a public good, freely available to anyone who wants some. The government provides it through universities. It is to be thought of as being like ham radio, available to anyone who wants to tune in – in one famous analogy, like “manna from heaven.”

In old growth theory, no further distinctions can be made. To speak of “intellectual property” in its terms would be as striking a contradiction as if one were to seek to describe a “private public good.” Untangling this paradox is what Romer’s work is all about.

New growth theory divides the world along different lines – into “instructions” and “materials,” Romer said. In the short interval between his talk in San Francisco and its appearance in print a few months later, his vocabulary changed slightly: “instructions” became “ideas” and “materials” became “things.” Materials can be thought of as goods with mass or energy (electricity, for instance), Romer explained. Instructions are goods that can be stored as a bit string of computer code: software, content, databases, that sort of thing.

The catchy distinction between atoms and bits was being made elsewhere, by others, in speeches and magazine articles instead of mathematical papers.

When we make dinner, he said, the materials we use are “our pots and pans (our capital), our human capital (our brains) and our raw materials (the ingredients). The recipes stored as text are our instructions.”

On this day, Romer offered a more homely illustration. When we make dinner, he said, the materials we use are “our pots and pans (our capital), our human capital (our brains) and our raw materials (the ingredients). … The recipes stored as text are our instructions.” The key distinction, he said, is no longer between public and private, but between “rival” and “nonrival” goods – that is, between items that can be consumed by one person at a time and those that can be employed simultaneously by any number. And in many industries, the most important assets are more like recipes or instruction sets than anything else – computer software, pharmaceuticals, musical recordings.

In new growth theory, said Romer, technological change is no longer the “force locomotive” of economics. “With materials and instructions, you can give a simple answer that shows how economic growth works. Humans use nonrival instructions together with rival goods (like pots, pans and machine tools) to
transform other rival goods, rearranging them into new configurations that are more valuable than the old ones. We rearrange steel rod into ball bearings or sheet steel into grinding machines that make ball bearings.”

Often people can exclude others for a time from using a particular set of potentially non-rival instructions. They can keep it secret, or have a patent — a possibility that is excluded by old growth theory’s public-good approach. The hope of appropriating some part of a stream of earnings from an invention is enough to animate a constant search for new ideas. The ultimate impossibility of excluding others from copying and improving new ideas is enough to guarantee a steady stream of growth.

These ideas, as old as Adam Smith, have been neglected, Romer averred, yet they are absolutely central to economics. Since an idea can be copied and used over and over, its value increases in proportion to the quantity of the rival materials it can be used to transform: the larger the market, the greater the payoff to a new idea. More widgets can be sold in a big city than in a small town, more in a big country than in a small one.

Indeed, that more than any other is the reason the United States long ago surpassed Great Britain in income growth. “Scale effect should no longer be treated the way a growth accountant such as [Edward] Denison did, as a kind of afterthought that had something to do with plant size,” Romer concluded. “They should be treated the way Adam Smith did, as one of the fundamental aspects of our economic world that must be addressed right from the start.”

The audience stirs uncomfortably. People have not heard these ideas before. Ordinarily the authority to whom readers are exhorted to return in order to regain their bearings is John Maynard Keynes, not Adam Smith.

The next speaker was Martin Weitzman of Harvard University. Romer is cool, fortyish, laid-back, California; Weitzman is hot, fiftyish, excited, Lower East Side. If Romer is difficult to grasp all at once, Weitzman is all but unfathomable. He was one of the first to anticipate how general the discussion of increasing returns was becoming when, in 1982, he invoked increasing returns to explain why the unemployed couldn’t simply go into business for themselves. Now he was trying to mediate between the old growth theory and the new: his topic is hybridization as a suitable metaphor for the growth of knowledge. Economic growth will continue as long as there are different strains of knowledge to cross.

He had given this talk before, at the summer meeting of the economic fluctuations group at the National Bureau of Economic Research. In fact, because of the continuing stream of this sort of work, the project had been renamed “fluctuations and growth.” But Weitzman’s mathematics was unfamiliar — combinatorics instead of programming — and the talk of evolutionary biology left the bright young people cold. The economist was met by uncomprehending stares. It did not dim his enthusiasm. “Holy smokes, Marty,” said his discussant Robert Solow afterwards. “You were like Savonarola in there.” Weitzman replied, “It’s like a revolution!”

But the young people were not similarly struck. Who in the world is Savonarola? There was no ambush, no counterattack, no controversy, no debate. The meeting ended on a flat note. The economists streamed out of the hotel, out of San Francisco, to fly into a snowstorm, to convene again the following year in New Orleans. The reporters did not race for the phones. The new ideas had not yet found the proper path to widespread understanding. The natural resistance to them was great. The stakes were high.