



Introduction

European Management Review (2006) 3, 131–141. doi:10.1057/palgrave.emr.1500066

The full-length articles in this issue are about corporations and governance. They are also about the ideological debates inside of countries and business communities regarding what is best for whom. Since ‘whom’ is rarely society at large but rather particular interests, politics are consequential and cannot be ignored. Management journals, and business schools, often shy away from the influence of politics for many reasons. It is probably time to shift the light and focus on the actors, their interests, and their strategies of mobilization.

The article by Martin Hoepner and Gregory Jackson on the hostile takeover of Mannesmann studies the changes in German institutions that have opened the door to corporate control by shareholders. They dismiss the many claims that Germany has not substantially undergone a transformation. There was no revolution, but clearly institutions have evolved incrementally. The collective impact of these many small changes is to tip the complementary mix of institutions towards a more market-oriented system of control. As their own review of the paucity of hostile takeovers suggests, the effects of these changes have not resulted in the same market dynamics as seen in the UK or the US. Yet, the re-orientation toward markets and shareholders are all the same palpable.

A careful reading of the text suggests a second story. Throughout the text, Hoepner and Jackson note that the behaviours of Mannesmann and Vodafone were subject to intense public scrutiny. The most fascinating part of the story is the opposition of unions and work councils to the hostile takeover, because it threatened to upset the restructuring plan that represented, in their view, the best economic value. In other words, labour objected to the means but accepted the ends that shareholders and economic value should count heavily in determining whether to fight or accept the hostile bid. Thus, when Deutsche Bank’s chief Esser announced a victory for shareholders at the conclusion of the takeover, unions and work councils were relatively passive. This passivity in the fact of an acquisition is not surprising in one major respect: the institutions of work councils and union representation are largely untouched by the institutional changes. German institutions are changing, except in the critical areas where power endures.

The article by Henrik Glimstedt, William Lazonick and Hao Xie is a study of another complementarity between the owners of knowledge, their access to labour markets, and hence their power to bargain for stock options. Thus, the Swedish company Ericsson felt compelled to succumb to issuing options to top employees in their acquired American companies. The puzzling element is why did Ericsson then import this policy into their Swedish operations. Did they think it would improve their performance? Were they afraid of losing Swedish engineers to the new Swedish internet companies?

No matter the explanation, granting stock options appears to be badly explained by these rationales. The statistical analysis does not attribute a very impressive role to the rationale of attracting and retaining employees. Lacking any evidence that granting stock options were doing much good, Ericsson largely dropped this policy while expanding a program to increase stock ownership among employees. Glimstedt, Lazonick and Xie conclude that Ericsson imported a bad practice from the US, but out of this negative experience, the company learned and created a stock incentive policy that represented an improvement over past practices.

The final full-length article is by Anna Staffsudd, who poses a very intriguing question: why have foreign nationals succeeded in penetrating Swedish boards more than women despite the popular claims of the equity of the Swedish model? She presents a methodical statistical investigation that confirms the highly conservative properties of social structure to perpetuate itself. In short, increasing the number of women is difficult because there are so few women on boards that can generate social opportunities and pressures for additional women to join.

Staffsudd’s analysis sheds light on the Hoepner and Jackson claim that small changes can eventually lead to institutional changes. However, in Staffsudd’s study, the changes are still too small to matter. If we permit the guess that there is a critical tipping point in the simple percentage of women on boards required to change the historical situation, Sweden has yet to cross this point. On the other hand, foreign nationals are increasing in percentage, presumably because pressures for their presence are driven by social forces outside the Swedish system. This analysis is a sobering analysis of why change is difficult to achieve, even when there is popular support.

The two remaining contributions to this issue consist of a note and a ‘project report’. The note is a contribution by Muir Macpherson on ‘palace wars’ in economics and the diffusion of privatization. There is a popular belief, often shared by the most sophisticated scholars, that globalization and Americanization are the same phenomenon. Of the many subtle problems with this view, the most imposing one is that America is, like all countries, heterogeneous and is dynamically changing. While MacDonalds might be a favored example of this identity of globalization and Americanization, it is easy to point to the spreading of French cuisine in high culture. It is not obvious if the export of mass cuisine is more influential than the importation of haute cuisine. How you feel about this comes down to whether you believe political power over economic policy is exercised by mass opinion or by elite control.

The haute cuisine in Macpherson’s study is economics and the practitioners are not chefs but economists. He shows that US economics was not a homogeneous

community, but was split by a palace war between Chicago economists and the rest of the profession. Via their graduates, this war spilled over into the rest of the world. The outcome was a diffusion of the diffusers that then made it more likely that a country would adopt a pro-market reform, such as privatization. Indeed, as Chicago came to win the palace war, the impact of whether a PhD was from Chicago or from MIT faded away, leaving only a relationship between US trained economists and privatization as the operating causality.

One wonders if one had the data, whether differential impacts of French trained and British trained graduates could be found on the political and economic evolution of former colonies. Perhaps, tracing elite ideas more than the legendary hamburgers would show many threads in a rich fabric of globalization. Without making the effort to measure the trade of European and American sociological ideas in the last 30 years, it is hard to know *a priori* who would be the net debtor. Surely, in economics, Lord Keynes might have a different view on the trade balance between the UK and the US, though he might feign satisfaction in not having to witness a reversal in this trade balance through a triumphant Chicago.

The project report included in this issue is from the Institute of International Business and is written by Henrik Glimstedt and the Co-Director of the Institute, Udo Zander. These reports are motivated by the observation that in Europe and in many other geographies, scholars often work in coordinated teams where the appropriate artefact of research is not the article, but the project itself. IIB is part of the Stockholm School of Economics and is closely associated with the largest multinational corporations in Sweden.

The danger of such proximity is all too obvious. However, if IIB suffers some loss of objectivity, it gains substantially from two sources. The first is that the Institute has had a deep cooperation with multinational corporations that has permitted access to data and field interviews on a sustained and long-term basis. The second source of gains is the curious paradox of Sweden being a small country with many multinational corporations. Thus, the multinational corporations are a highly salient component of the Swedish debate. In many respects, the Swedish multinationals were innovators, because quickly they could not staff their overseas operations simply by transferring Swedes overseas. As they became reliant on foreign managers and workers and as they acquired considerable assets overseas, they pioneered new forms of organization, including systems by which to identify new practices and transfer them across borders.

Outside of Scandinavia, the only other major centre of research in Europe on multinationals that has a major impact is at Reading in the United Kingdom. Much of the research at Reading was focused on the economics of foreign direct investment, along with a vital interest in business history. With this exception – and that of the major MBA business schools, other institutions in European countries have not developed this impressive record of field and statistical research focused on multinational corporations.

It has always surprised me that in national centres of research, the knowledge of the organization of international firms and how they operate across subsidiaries has been

relatively impoverished. No doubt, this is what the European Commission means by centres of excellence, in which certain universities and schools in particular countries are leaders in specialized areas of research. Let us hope that European academics recognize these different traditions and specializations and yet engage in open and vigorous trade of their ideas and research! In looking at the articles that come to the Review and the national-specificity of their terms and methods, I propose that more can be done.

In September, I had the honor to give an informal speech of welcome to the new Ph.D. students at IESE in Barcelona. While I have given a talk on a few occasions to graduating Ph.D. students, I had not given a welcome speech before that was scheduled for over an hour. Such a talk, I was advised, should be inspirational and personal, as these students were commencing a program of several years of study. Many had worked before and this re-entry into academic study is sometimes quite jarring. At the same time, I did not want it be simply personal, for I wanted to signal by substance that we are lucky to be able to read, study, research, teach, and engage in the world.

In thinking about my time as a Ph.D. student, I realized how hard I worked. I chose to study at the Sloan School at MIT, which was surprising as I had up to then an aversion to business and business schools and I had not taken up in my college classes what I was told I was destined to do, namely more technical studies. Somehow, I decided that business schools had the right combination of intellectual rigor and worldly engagement – my instincts were right here, and that MIT would balance my previous education in social sciences and humanities. This second motivation was also correct, though I had no idea that entering into a program that was populated by students with engineering, economic, and math/science undergraduate degrees was to set such a high bar.

This experience left me with the metaphor that graduate programs of study accept tennis players who have one strong arm and then who face a choice. They can strengthen that arm or they can also develop the other arm. I prefer to see both arms developed and this advice is what I give to my graduate students. However, for some students, strengthening the second arm is not only difficult; it is also not seemingly rewarded in their fields in the short run. There is surely a classic trade-off between variety and specialization. Over the long duration, though, I hold to the proposition that two arms allow for more sustained research careers.

At MIT, the most exciting field at that time was finance, which was centred on Fischer Black, Robert Merton, and Stew Myers. There was a strong affiliated economics program. It made sense, therefore, to migrate toward these fields. There was after all almost no organizational theory or strategy at Sloan during this time, though there were excellent faculty, such as John van Maanen, in organizational behaviour. The action was in finance, my friends were also drawn to this area, and hence so was I. It was a good decision to move intellectually to where the ideas and tools were in the process of innovation.

At Sloan and economics, there were great researchers, many of whom won or would win the Nobel Prize, and



many of them were fabulous teachers. Black in particular taught an amazing course consisting of a hundred questions, of which a few would be discussed each day. We would try to answer them, he would often stand there with his arm held in mid air supported by the other hand cupping his elbow, and occasionally writing down something in a notebook, which seemed to have little correlation with the intelligence of the comment just offered by a student. One of the questions was to state the conditions under which the Capital Asset Pricing Model would be accurate. Similarly, he explored when the Black-Scholes option pricing formula would not hold. After my reading the magisterial article by MacKenzie and Millo (2003) on Black and option pricing, I am sure that Black would have endorsed the results of that study, which investigated how mathematical rules change trading and thereby open markets to unexpected volatility when these rules do not seem to work any longer. He puzzled over the possibility that option trading increased the likelihood of a meltdown of markets, such as the 1987 crash that was linked to volatility of option prices. In retrospect, his equally impressive colleague, Robert Merton, who was also a fine teacher, seemed less concerned by these 'irregularities'; he later was deeply implicated in another melt-down caused by the firm Long Term Capital Markets in which he was a senior partner. Despite being the son of the renowned sociologist, Merton appeared to be less of a two-armed tennis player.

We were very fortunate to be students at MIT during this time of innovation, but it is significant to note that many colleagues did not finish their degree even though they were very talented. Other surprises came later. One of the brightest and most generous of the Ph.D. students shocked us all by taking a job 'across the river' at Harvard Business School, for MIT defined itself as the school of science and research compared to the worldly and case-oriented HBS. Not long after, Merton and our Ph.D. director Al Silk left Sloan for HBS. Fischer Black, who gave the appearance of working 24 hours because he kept his door closed, his window blinds down, and his office light always lit, took a job at Goldman Sachs. Soon, many of our classmates and friends left their jobs at Berkeley, Wharton, and Columbia to join Wall Street and to bring science to financial markets. More than one was also implicated in financial market meltdowns, and after this time they did not seem to come back to 'tweak' the model and to patch the problem. One student, self-described as the brightest, became an itinerant secular monk, and another, who was the most brazen, clearly broke ethical boundaries in promoting a book for popular consumption. And a few of us stayed happily in academics.

The experience of my cohort shows that a Ph.D. is a valuable education, whether it is pursued in academics or outside. I am pleased that many of my peers went on to very successful business and public service careers. A doctoral degree from a business school should surely have a worldly recognition and value.

I met recently with my MIT roommate – we lived in the downstairs flat of a house owned by another brilliant Ph.D. student Richard Homonoff – Nalin Kulatilaka, who has remained one of those friends that seems only possible to make when you are young, working as hard as you can, and

oblivious to almost everything. After MIT, we wrote a few papers together, which were in many ways as much an excuse to get together as to do work. In any event, Nalin observed how much we still love what we do. We were also somewhat surprised that few of our cohort were still doing academic research. We wondered why.

Unfortunately, the sample size is too small to come to any conclusion other than the following opinion. Two-armed tennis players have the advantage in recognizing and engaging in a wide variety of subjects. While at MIT, my adviser Richard Robinson told me it was expected that I should have an overseas experience and recommended working in a factory in Algeria. I found my own solution by responding to a poster in MIT's main corridor that said 'study in central and eastern Europe'. I applied for the IREX grant, was invited to an interview on a very snowy day in December where I was interviewed by Laura Tyson – who had also been an IREX recipient for Yugoslavia, and tried every way to get back to my exams through one of those famed North-east blizzards. This grant provided me language training in Rotenburg ab der Tauber, and at Middlebury College (which has a fabulous language summer school), a six-month stay in East Germany at the Humboldt Universitaet, and a 4-month internship at the Rand Corporation in Santa Monica. Along the way, I met a woman whom I eventually married, and this connection led me to finish my last graduate year at the Institute for International Business at the Stockholm School of Economics. There, much to the delight of the senior faculty, I taught several classes in 6 weeks: international finance, a mini-course on corporate finance (since the finance in those days was 'suspect' and not part of the required curriculum), and a strange class called 'strategy', which I had never taken as a student and which required case teaching; I had never been taught by a case – in fact, I had never taught before. It was cold, almost always dark, I made a thousand teaching mistakes, half of which I never made again. I did manage to finish a thesis too.

While my thesis did not reflect much of a two-armed tennis player – I over-shot the optimal diversity by a few 'arms', the MIT training left me with the desire to know more and to be open to variety.

These personal observations constitute the bulk of my support for the theory of the two-armed tennis player (plus some tolerance for serendipity) as an explanation for sustained academic careers.

Unfortunately for this theory, it has a more eloquent rival that is tersely expressed in a marvellous and short book, *Old Masters and Young Geniuses*, authored by an economist David Galenson. He posits that there are two types of artists: conceptual innovators who 'find' and experimental innovators who 'seek'. From this classification, he offers the proposition that conceptual innovators, such as Picasso, earn success early in their lives through breakthrough ideas. Experimental innovators proceed by trial and error, with success coming later in life. Often, like Cezanne, these artists struggle for perfection until their final years.

Galenson supports this proposition by examining auction prices and art books. For example, he finds that Picasso's early art, especially his cubism, is far more valued than this later work. By contrast, the prices are dramatically higher for Cezanne's late work, even that done in his 60s.

Since it is costly to print plates in art books, Galenson decided that this would be a good measure of value placed on a painting; thus he collected data on which paintings were included in art history books. The pattern is similar: Picasso's paintings completed when he was in his 20s or early 30s are far more represented than this later work, whereas it is Cezanne's late work that is included. Galenson notes also that X-rays of canvases shows how much Cezanne reworked his paintings; Picasso completed rapidly a canvas and rarely reworked a picture.

The book broadens its scope to argue that artists such as Michelangelo, Rembrandt, Cézanne, Jackson Pollock, Virginia Woolf, Robert Frost, and Alfred Hitchcock were experimental old masters. Vermeer, van Gogh, Picasso, Herman Melville, James Joyce, Sylvia Plath, and Orson Welles were conceptual young geniuses.

It is unclear why this pattern should be found. A clear hypothesis is that conceptual abilities peak at a younger age. If you happened to have been too distracted while young, or you failed at being conceptual, you still have a chance to be experimental. Unfortunately, since the arrow of time points in one direction, the other evolution is not possible. Or it could be that conceptual breakthroughs are more media worthy. A young genius is thus easily distracted into a life filled with non-artistic events, whereas the experimentalist drudges in the studio, waiting for the world to recognize a mature master.

This type of reasoning is frequently heard in mathematics, and consequently echoed in economics, that mathematical breakthroughs are 'discovered' only by young scholars. Although Galenson did not collect data on economists, I requested Cecile Maciulis, the librarian at INSEAD, to assemble data on ISI citations to the most cited work by 21 Nobel Prize winners in Economics.¹ I chose them on the basis of my knowledge regarding the mathematical content of their work. The citation count is not perfect, as we include books that contain articles published earlier. Yet, even in these cases, the books are published fairly soon after the articles. The full tables are attached as an appendix.

Overall, the Galenson thesis holds up. Let's take just four of the Nobel Prize winners. On the conceptual side, we have John Nash and Robert Merton. At the age of 23, Nash published the article from which resulted the most important equilibrium concept in game theory – 'Nash equilibrium'. The movie 'A Brilliant Mind' shows Nash interminably suffering to come up with a thesis idea, but this suffering must have been of short duration. Similarly, Merton published his paper on a theory of rational option pricing at the age of 29. Samuelson and Merton were engaged in a competition with Myron Scholes and Black to 'discover' a closed-form equation for option pricing. This competition replicated very closely the story of the discovery of the 'double helix' theory of the structure of DNA by two young scientists a few decades earlier.

The Cezanne of the sample is clearly Douglas North. His most cited books all date from the later part of his career, with the most celebrated one being *Institutions, Institutional Change and Economic Performance*, which he wrote at the age of 70. A more surprising 'old master' is George Stigler, who published his most cited work at the age of 60, with all of his more cited articles published after the age of

40. Neither North nor Stigler were mathematical – relative to Nash and Merton, though one might argue that Stigler was clearly conceptual.

Surely, the equivalence of math and conceptual breaks down in the case of Ronald Coase. He published his famed theory of the firm at the age of 27, and he is most famous in management schools for this 'discovery'. However, as one knows from his contributions to a book published in his honor (edited by Oliver Williamson and Sidney Winter), this paper was very much based on reflections from field data. It is not surprising that Coase's other articles – amazingly well cited by comparison to the other more mathematical entries in our sample – date later in his life.

In all, it would seem as only 7 to 9 (depending on how the age split) among this highly non-random sample of Nobel Prize winners in economics are experimentalists. After discussing this book, we took a poll of the 20 to 30 people in the room to ask them if they were 'conceptual' or 'experimental'. About 30% said conceptual, most said experimental, a few said both, and a few looked bewildered. I thought bewildered was a perfectly acceptable response of Ph.D. students during their first week of studies.

This thesis is no doubt better than my two-armed theory. It might be possible that experimentalists are two-armed players, and conceptual geniuses more 'one armed'. Unfortunately, Galenson's theory has the very important property that it is measurable, a quality that I often stress in advising authors who submit articles to the EMR. So I will follow my own advice and withdraw my theory before the stronger argument, with far better measurement, offered by Galenson.

Galenson's book presents an inspirational research theory of creativity, for it says that creativity will vary over the life cycle but it is always present. It may vary by individual type and ability, but there are possibilities of sustained careers and sustained justifications for life-long commitments to academic research, not only as a mentor but also as a contributor. For some reason, that has more meaning as one proceeds in career, but I suspect that Galenson's thesis provides also inspiration for young doctoral students. Now let's just hope that it is true.

Bruce Kogut

*Insead, Strategy Department, Boulevard de Constance,
Fontainebleau, France*

Note

1 Cecile also included citation counts according to Google Scholar, but these references are clearly biased by the recency of the internet and I leave them out.

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- MacKenzie, Donald and Yuval Millo, 2003, "Constructing a Market, Performing Theory: The historical sociology of a financial derivatives exchange". *American Journal of Sociology*, 109: 107–145.
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Appendix

The Most Cited Papers and Books by Some Nobel Prize Winners in Economics and their Age at the Time of their Publication

George A Akerlof – PN 2001**b1940****ISI Ranking**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|--|-------------|--|---------------------------|
| 1511 | The Market for “Lemons”: Quality Uncertainty and the Market Mechanism | 1970 | <i>The Quarterly Journal of Economics</i> , Vol. 84, p: 488 | 30 |
| 352 | Labor Contracts as Partial Gift Exchange | 1980 | <i>The Quarterly Journal of Economics</i> , Vol. 97, p: 543 | 40 |
| 215 | A near-rational model of the business-cycle, with wage and price inertia | 1985 | <i>The Quarterly Journal of Economics</i> , Vol. 100, p: 823 | 45 |
| 183 | A theory of social custom, of which unemployment may be one consequence | 1980 | <i>The Quarterly Journal of Economics</i> , Vol. 94, p: 749 | 40 |
| 173 | Efficiency Wage Models of the Labor Market | 1986 book | Cambridge University Press | 46 |
| 118 | Gift exchange and efficiency-wage theory – 4 views | 1984 | <i>American Economic Review</i> , Vol. 74, p: 79 | 44 |

Kenneth J Arrow PN 1972**b1921****ISI ranking**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|---|---------------------------|
| 1680 | Social Choice and Individual Values | 1960 book | Yale University Press | 39 |
| 1093 | Social Choice and Individual Values | 1951 book | Wiley, New York | 30 |
| 887 | The economic-implications of learning by doing | 1962 | <i>Review of Economic Studies</i> , Vol. 29, p: 155 | 41 |
| 846 | Uncertainty and the welfare economics of medical-care | 1963 | <i>American Economic Review</i> , Vol. 53, p: 941 | 42 |
| 851 | General Competitive Analysis | 1971 book | Holden-Day, San Francisco | 50 |
| 720 | Essays in the theory of risk-bearing | 1971 book | North-Holland, Amsterdam | 50 |

Gary S Becker PN 1992**b1930****ISI ranking**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|---|---------------------------|
| 1469 | A theory of the allocation of time | 1965 | <i>Economic Journal</i> , Vol. 75, p: 493 | 35 |
| 1367 | Treatise on the Family | 1981 | Harvard University Press | 51 |
| 1358 | Crime and punishment - economic approach | 1968 | <i>Journal of Political Economy</i> Vol. 76, p: 169 | 38 |
| 1040 | Human capital | 1964 | Chicago: University of Chicago Press | 34 |
| 887 | A theory of competition among pressure groups for political influence | 1983 | <i>Quarterly Journal of Economics</i> Vol. 98 no. 3, p: 371 | 53 |
| 661 | Economic Approach to Human Behavior | 1976 | U of C press | 46 |

**Ronald H Coase PN 1991**
ISI ranking**b1991**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---------------------------------------|-------------|---|---------------------------|
| 2356 | The problem of social cost | 1960 | <i>Journal of law & economics</i> , Vol. 3, p: 1 | 50 |
| 2158 | The nature of the Firm | 1937 | <i>Economica</i> Vol. 4, p: 386 | 27 |
| 224 | Durability and monopoly | 1972 | <i>Journal of law & economics</i> , Vol. 15, p: 143 | 62 |
| 210 | The firm, the market, and the law | 1988 | University of Chicago Press | 78 |
| 188 | The federal communications commission | 1959 | <i>Journal of law & economics</i> Vol. 2, p: 1 | 49 |
| 129 | The Lighthouse in Economics | 1974 | <i>Journal of law & economics</i> Vol. 17, p: 357 | 64 |

Gerard Debreu PN 1983
ISI ranking**b1921**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|--|---------------------------|
| 1258 | Theory of value an axiomatic analysis of economic equilibrium | 1959 | Yale University Press, New Haven | 38 |
| 279 | The Coefficient of Resource Utilization | 1951 | <i>Econometrica</i> Vol. 19, p: 273 | 30 |
| 251 | A limit-theorem on the core of an economy | 1963 | <i>International economic review</i> Vol. 4, p: 235 | 42 |
| 201 | Economies with a finite set of equilibria | 1970 | <i>Econometrica</i> Vol. 38, p: 387 | 49 |
| 178 | A social equilibrium existence theorem | 1952 | <i>Proceedings of the National Academy of Sciences of the United States of America</i> , Vol. 38, p: 886 | 31 |

Milton Friedman PN 1976
ISI ranking**b1912**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|--|-------------|---|---------------------------|
| 1211 | A theory of the consumption function | 1957 book | Princeton University Press, Princeton | 45 |
| 1200 | Capitalism and freedom | 1962 book | University of Chicago Press | 50 |
| 1000 | Essays in Positive Economics | 1953 book | University of Chicago Press | 41 |
| 824 | The Role of Monetary Policy | 1968 | <i>American Economic Review</i> , Vol. 58, p: 1 | 56 |
| 785 | Monetary History of the United States, 1867–1960 | 1963 book | Princeton University Press | 51 |

Daniel Kahneman PN 2002
ISI Ranking**b1934**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|---|---------------------------|
| 3758 | Judgment under Uncertainty: Heuristics and Biases | 1974 | <i>Science</i> Vol. 185. no. 4157, pp: 1124–1131 | 40 |
| 3816 | Prospect Theory – An Analysis of Decision under Risk | 1979 | <i>Econometrica</i> , Vol. 47, p: 263 | 45 |
| 2660 | Attention and Effort | 1973 book | Prentice-Hall | 39 |
| 2253 | Judgment under Uncertainty | 1982 book | Cambridge University Press | 48 |
| 2044 | The framing of decisions and the psychology of choice | 1981 | <i>Science</i> Jan 30; 211(4481): 453–458 | 47 |
| 1588 | Availability: A heuristic for judging frequency and probability | 1973 | <i>Cognitive Psychology</i> , Vol. 5, pp: 207–232 | 39 |

Robert C Merton PN 1997
ISI ranking

b1944

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|---|---------------------------|
| 1179 | Theory of rational option pricing | 1973 | <i>Bell Journal of Economics</i> , Vol. 4, p: 141 | 29 |
| 719 | Intertemporal capital asset pricing model | 1973 | <i>Econometrica</i> vol. 41, p: 867 | 29 |
| 719 | Optimum consumption and portfolio rules in a continuous-time model | 1971 | <i>Journal of Economic Theory</i> , Vol. 3, p: 373 | 27 |
| 379 | Lifetime portfolio selection under uncertainty - continuous-time case | 1969 | <i>Review of Economics and Statistics</i> , Vol. 51, p: 247 | 25 |
| 363 | Option pricing when underlying stock returns are discontinuous | 1976 | <i>Journal of Financial Economics</i> , Vol. 3, p: 125 | 32 |

Franco Modigliani PN 1985
ISI ranking

b1918

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|--|-------------|---|---------------------------|
| 1151 | The cost of capital, corporation finance and the theory of investment | 1958 | <i>American Economic Review</i> , Vol. 48 p: 261 | 40 |
| 394 | Corporate-income taxes and the cost of capital - a correction | 1963 | <i>American Economic Review</i> , Vol. 53 p: 433 | 45 |
| 277 | Utility analysis and the consumption function: an interpretation of cross-section data | 1954 | In Kurihara, K.K (ed.): <i>Post-Keynesian Economics</i> | 36 |
| 130 | New developments on the oligopoly front | 1958 | <i>Journal of Political Economy</i> , Vol. 66, p: 215 | 40 |
| 162 | Innovations in interest rate policy | 1966 | <i>American Economic Review</i> , Vol.56, p: 178 | 48 |
| 118 | Inflation, rational expectations and term structure of interest rates | 1973 | <i>Economica</i> , Vol. 40, p: 12 | 55 |

John F Nash, Jr. PN 1994
ISI ranking

b1928

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
|--------------------|---|-------------|---|---------------------------|
| 1110 | The Bargaining Problem | 1950 | <i>Econometrica</i> Vol. 18(2), pp: 155-162 | 22 |
| 749 | Non-cooperative games | 1951 | <i>Annals of Mathematics</i> Vol. 54, p: 286 | 23 |
| 487 | Two-Person Cooperative Games | 1953 | <i>Econometrica</i> Vol. 21(1), pp: 128-140 | 25 |
| 465 | Equilibrium points in n-person games | 1950 | <i>Proceedings of the National Academy of Sciences of the United States of America</i> , Vol. 36, p: 48 | 22 |
| 260 | The Imbedding Problem for Riemannian Manifolds | 1956 | <i>The Annals of Mathematics</i> , Vol. 63, no. 1, p: 20 | 28 |
| 247 | Continuity of solutions of parabolic and elliptic equations | 1958 | <i>American Journal of Mathematics</i> Vol. 80, p: 931 | 30 |

**Douglass C North PN 1993**
ISI ranking**b1920**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
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Paul A Samuelson PN 1970
ISI ranking**b1915**

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Amartya Sen PN 1998
ISI ranking**b1933**

| <i>Times cited</i> | <i>Cited work</i> | <i>Year</i> | <i>Bibliographic information</i> | <i>Age at publication</i> |
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Thomas C Schelling – PN 2005
ISI ranking

b1921

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Myron S Scholes PN 1997
ISI ranking

b1941

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ISI ranking

b1916

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Vernon L Smith PN 2002
ISI ranking

b1927

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Robert M Solow PN 1987
ISI ranking

b1924

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George J Stigler PN 1982
ISI ranking

b1911

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James Tobin PN 1981
ISI ranking

b1918

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ISI ranking

b1899

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