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In Memory of Robert K. Merton

Robert C. Merton and Robert M. Solow

This presentation was given at the 1876th Stated Meeting, held at the House of the Academy on November 12, 2003.

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Robert C. Merton

Robert K. Merton was my father for nearly six decades – and he was my best friend for the last four. In the early years we talked on the phone at least once nearly every day. Later, when email came, he mastered it before I did – it relaxed the constraint in his being a lark and me an owl, so

our interchanges multiplied. We also edited each other's papers, although both relative need and skill ensured that I could never produce anything like the amount of red ink on his papers that he put on mine. I know that, over the years, many of you came to know what I mean about that red ink.

We would sit by the fire in East Hampton in his last ten or fifteen years and talk for hours. When I got that early morning phone call in October of '97 saying, "We have something interesting to tell you," it was RKM (I never called my father RKM but the initials did come in handy in talking about him to others) who got the next phone call from me. At the Nobel banquet in Stockholm there is a center table for the laure-

ates and their significant others, and I chose him as my significant other. In the last ten years he took pleasure in signing his letters "Father of the Economist." I tried to reciprocate using "Son of the Sociologist" – but it didn't start out that way.

When I was a child, my father was surely there and he taught me many important things, not least about the stock market, poker, and magic. Only the magic didn't stick – I didn't have the discipline. As a teenager, I had very little interest in the intellectual or academic life. I was interested in cars. I started building hot rods, if you can believe it, and racing them. It seemed I did almost everything that took me as far away as possible from what he did. As we began to discuss college, I said, "I think the General Motors Technical Institute would be good." That didn't happen, but neither did I apply to Harvard – I wouldn't have gotten in if I had. I went to Columbia Engineering School and majored in engineering mathematics, but my plan was still to be an auto engineer, which was a small move away from becoming a mechanic. I should mention that during my sophomore year at Columbia I took a required course in English, and I wrote what was to become my first published paper, entitled "The 'Motionless Motion' of Swift's Flying Island." When I turned it in to my English instructor, for whom to that point I had done D work, he said, "Well, this has some possibilities for us; maybe we can get it published." Fortunately, I had a father who knew something about such matters. So I asked for his advice and eventually told the instructor, "No, I think it would be just fine if I wrote the paper alone." So, I have a D in English on my transcript, but I also have a paper published in the *Journal of the History of Ideas*.

After a few summers working for the Ford Motor Company, I decided perhaps academia might not be such a bad choice and so, some thirty-five years ago, I went off to Caltech to do a Ph.D. in applied math. After finishing all my courses and passing my qualifiers at Caltech, I was all set to do my thesis when I began to wonder whether water waves in a tank or plasma physics was right for me. Perhaps it was the fact that I used to get up every morning and go down to a local brokerage house for the 9:30 a.m. opening of the New York Stock Exchange – 6:30 a.m. my time. I would trade all kinds of securities – stocks, convertible bonds, warrants, and options – and then go off to class. One day, I came upon a very bad book on mathematical economics; it was so bad that I said to myself, "I can do better." If I had seen a book by Bob Solow or Ken Arrow, I might still be doing fluid mechanics, but instead I

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decided to switch to economics – a move that everyone thought was crazy, except my father who was smiling. You can see where this is heading: I was now in academe and, furthermore, into one of the social sciences.

My research is very mathematical, very quantitative, and seemingly very far away from what my father did. Nevertheless I found myself using many of his ideas. I will list a few examples. There is the concept of *post-factum* interpretation after the observation to provide an explanation, which he paid a lot of attention to and was pretty derisive about. Of course, in economics, *post-factum* interpretations are made daily: read *The Wall Street Journal* today and you can learn exactly why the market did what it did yesterday. *Post-factum* applies even more materially to measuring the performance of money managers, and in that regard it is not a trivial matter.

The next one on the list, the self-fulfilling prophecy, or *SFP* as he called it: Some economists have speculated that the option pricing model that Fischer Black, Myron Scholes, and I derived might not have been descriptive of market pricing before its public release, but if enough people believed in it, maybe prices adjusted so the model became accurate afterwards.

Then there was RKM's concept of manifest and latent functions, well cited in my early papers but even more so in my later work applying functional analysis to better understand financial innovation and institutional change. Consider a speculative market, say the stock market. Its manifest function is to permit transactions that allow firms to raise funds by issuing shares that investors can buy and trade amongst each other. So if you were never going to do any transactions in the market, why then would you have any interest in it? One answer is that the market gives you information. Managers who might never issue equity in their firms nevertheless need to look to the market for information to help them make effective decisions about the firm's investments – thus providing information is an important latent function of speculative markets.

Still another one of RKM's formulations, the concept of unanticipated and unintended consequences of social actions, came into my work when I was analyzing the implications of the U.S. government providing guarantees for corporate pensions. In the last decade I've focused on developing the so-called functional perspective in finance to examine the evolving financial system. This perspective uses financial functions instead of institutions as "anchors," or framing elements of the system – thus freeing us to think about the dynamics of change in economic institutions as an endogenous process. You can imagine the discussions I had with my father about that. So I find myself very, very close to precisely where I was trying not to be at the start, more than four decades ago – and darn happy about it!

Now, apart from our endless talks, my father and I did actually work together: in 1981 we had a draft of a theory including a mathematical model on problem choice and the functions and dysfunctions of priority in the reward system in science. Although subsequently both of us became very involved in our other separate research projects, we continued to work together to expand on the ideas and potential applications. In fact, in 1989, our joint paper was going to be the lead article in *Rationality and Society*, the journal that James Coleman had just started, but my father did what he had done more than once before. As it was about to go to press he said, "No, it's not quite good enough." Fifteen years later, it's still not good enough, but I'm going to try to make it right. In the fall of 2002, my father published his last book, *The Travels and Adventures of Serendipity* (coauthored with Elinor Barber), which actually originated in the 1950s. So, in terms of his holding back and delaying publication, our joint project is nowhere close to the record.

In sum, try as I could to do otherwise, I ended up not at all far from my father. He was indeed a role model in the deepest sense of that term. He was so aware and so active and so intellectually productive up to the end of his life – and that's a heck of a role model to have going forward.

I am really delighted that this meeting is taking place here. This was a very important institution in my father's life, and I had the great good fortune to join the Academy and be a part of it while he was too. Indeed, we had several such shared connections, including membership in the National Academy, and we were the only father and son ever to hold honorary degrees from the University of Chicago.

I cannot end my remarks without noting another collaboration that never occurred – the one in which the two of us coauthor a piece with Robert Merton Solow. My father said to me, "We simply must do a joint piece with Bob." And I immediately reacted, "That's great . . . let's do it! I surely wouldn't mind being sandwiched between the two of you." He elaborated, "Of course, there can be only one way to order the authors – Robert Merton Solow and Robert Merton Duo."

Robert M. Solow

I am not a sociologist, as you know, nor have I ever taught at Columbia University. So far as I can recall, in fact, I have never been formally or institutionally associated with Bob Merton, senior. There is, however, one thread of connection between Robert K. Merton and Robert Merton Solow that goes back more than sixty years – to January 11, 1942, to be precise. (There is someone here this evening who goes back even further: Ruth Smullin, the wife of my MIT colleague Louis Smullin, was Merton's tutee as a Radcliffe student in 1938.)

If you look at the bibliography in the 1970 reprint of Merton's 1938 classic *Science, Technology and Society in Seventeenth-Century England*, you will find a reference to a paper by one Robert Solow entitled, of course, "Merton's *Science, Technology and Society in Seventeenth-Century England*." The paper is dated January 11, 1942. On January 11, 1942, I was a sophomore at Harvard College, a little less than seventeen and a half years old. I had been taking Talcott Parsons's course on the sociology of science, and had turned in my term paper on time. Parsons gave it an A and sent it on to Merton. With his usual grace, Bob Merton read it, entered it in his bibliography, and found it unnecessary to comment on it. I live in hope that the paper is irretrievably lost or destroyed. I am afraid I can imagine what it was like. I didn't get to know Merton personally until much later.

There is no mystery in the fact that he was unappeasably attracted to all kinds of ideas, especially ideas about social institutions, but really about anything. What is more mysterious is that somehow ideas were attracted to him, as if he were some kind of intellectual flypaper. If we took any six of us here and put us in a room with Bob Merton and released an idea somewhere near the chandelier, the odds are two to one that it would flutter down and come to rest on Merton. You realize what that means: the random idea had a two-thirds probability of finding its way to Merton and a probability of

just one-eighteenth of coming to rest on one of us. And we are intelligent, idea-friendly people or we wouldn't be here. He must have emitted some pheromone-like come-on.

It struck me that the obviously right way for me to pay tribute to my friend – and everybody's mentor – is to stick to ideas. So I reread one of Bob Merton's most celebrated papers, and I propose to describe it and discuss it now. The paper I have in mind is "On Sociological Theories of the Middle Range." I chose it partly for selfish reasons: I think of myself as an economic theorist of the middle range, so I am curious about parallels and differences between the disciplines.

Merton tells us that this essay expands some comments he had made on a paper read by Talcott Parsons at the 1947 meeting of the American Sociological Society. Merton's essay can be read as a sort of declaration of independence from his teacher and friend, because Parsons was then the leading advocate and exemplar of the tendency in social theory that Merton was opposing. He says that serious work in sociology ranges from single, isolated empirical studies of particular situations with little or no potential for generalization or even for interconnection, all the way to grand, all-inclusive systems of sociological theory. He thinks that the best prospects for progress in sociology lie in theories of the middle range: "theories that lie between the minor but necessary working hypotheses that evolve in abundance during day-to-day research and the all-inclusive systematic efforts to develop a unified theory that will explain all the observed uniformities of social behavior, social organization and social change." And later: "Middle-range theories consist of limited sets of assumptions from which specific hypotheses are logically derived and confirmed by empirical investigation . . . These theories do not remain separate but are consolidated into wider networks of theory."

Merton offers, as sociological examples of the middle range, the theory of reference groups, the theory of social mobility, of role conflict, and of the formation of social norms; and he thinks of these as analogous to such classical instances as a theory of prices, a germ theory of disease, or a kinetic theory of gases.

A difficult question arises about the relation of middle-range theories to total systems of sociological theory, like those associated with such names as – I have to quote this – "Marx and Spencer, Simmel, Sorokin and Parsons." (It was certainly not lost on Merton when he wrote down that passage that neither Karl Marx nor Herbert Spencer had even a microscopic sense

of humor.) It is clear that Merton does not think it is fruitful to start by deducing middle-range theories from such comprehensive theoretical systems. In the first place, all those grand theories in sociology are more like "theoretical orientations" than they are like tight, rigorous theories. So it may not be possible to get from some grand theory to a particular middle-range theory. Merton points out explicitly that a successful middle-range theory may be logically compatible with more than one grand theory. I don't remember that he says so explicitly, but it seems to me that, in sociology, one grand theory may be compatible with more than

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one middle-range theory aimed at the same group of observable facts. But it is pretty clearly Merton's view that if a successful – that means empirically successful – middle-range theory is incompatible with some overarching system, so much the worse for that grand system.

It is very important for him that middle-range theories have the potential to interconnect with one another and form networks of related hypotheses that can give rise to broader generalizations; but the examples of this that he mentions, without developing them, seem to involve fairly small increments in generality. It is not clear to me whether Merton harbored the thought that very general social theory could be built from the bottom up by this process of accretion. But he made no bones about where he thought the opportunities for productive work in sociology and social theory were to be found – and my experience in economics leads me to believe that he was right.

Not every sociologist agreed with him. The paper gives some examples of polarized arguments pro and con the middle range. I am not knowledgeable enough about what went on before and after to try for a balanced interpretation. But I will say that Merton's discussion of the controversy is distinguished by sanity,

and that comes as no surprise. He cannot have had a monopoly on sanity, but he had a healthy market share; it was clearly his natural temperament.

I want to illustrate this discussion of the interconnection issue by an anecdote. Bob Merton was not a participant, but I dearly wish he had been. To begin with, in the course of the middle-range essay he mentions that Durkheim's *Suicide* is his nominee for the Academy Award for middle-range theory, with Weber's *The Protestant Ethic and the Spirit of Capitalism* maybe a close second. (The Academy Award locution is mine, not his. Like many of you, I have read both books, but missed the movie versions.)

Now the story continues. Back in 1957 – 1958, by chance, Talcott Parsons, David Landes, and I were all fellows at the Center for Advanced Study in the Behavioral Sciences in Palo Alto. One afternoon, Landes was giving the weekly seminar. He had (serendipitously) come upon a treasure trove of personal letters written by members of some entrepreneurial textile-manufacturing families in the cities of Roubaix and Turcoing in northern France. The letters included hortatory messages from fathers and uncles to sons and nephews. These people were all pious, unquestioning Catholics. But the messages they were transmitting and the advice they were giving sounded exactly as if they had been lifted from Weber's Calvinists.

So what is then to be made of the Protestant ethic and the spirit of capitalism? Could Weber's middle-range theory have been wrong? It was in Bob Merton's mind that one of the great advantages of middle-range theories is that they could be wrong and you could show them to be wrong. Talcott Parsons, who sort of owned Max Weber in those days, would not allow the possibility of error. Weber hadn't actually meant Protestantism, but maybe just some appropriate religious orientation, or even some ethically sanctioned canons of behavior that could in principle originate outside of formal religion altogether.

What would Merton have said had he been there? I think he might have said: "OK, so it's not exactly Protestantism, and Weber erred. But there is a research project here. Where, across time and space, have there arisen similar ethical injunctions to achieve demonstrated goodness by worldly success? And where have they been broadly accepted? And what do those instances have in common? Are there other well-documented middle-range theories that bear obliquely on this sort of thing?" This would, of course, have been put elegantly, and with a reminder that to proceed in that direc-

tion would be to stand on the shoulders of Max Weber rather than to sit at his feet.

The spirit of Merton's stern but reasonable essay went down so smoothly with me that I naturally began to think about its relevance for economics. It seems to me that there are two characteristics of economics as a discipline that matter in this respect. The first is that the very objects that economic theorists think about are very narrowly defined: economic theory is about explaining and understanding prices and quantities of goods and services. If what you are saying is not at least indirectly about prices and quantities, then you are not talking about economics. I don't want to ask whether this restriction is somehow inevitable, or whether there could be a recognizable economics that was about something else. The fact is that prices and quantities are what essentially all economics is about.

This fact – historical or logical or accidental – creates two contrasts with sociology. The first is that it limits drastically one's freedom to create grand theories in economics. Grand theories often differ from other ones because they change the subject, they introduce wholly new, or apparently new, concepts. If it all has to come down to the price of orange juice in the end, you can try to go at the problem differently, but not too differently.

The other contrast is that prices and quantities are numerical. There is no getting away from the fact that economics has to be quantitative. I don't think that the use of mathematics is all-important, though it certainly matters for the day-to-day business of working economists. It is fundamentally the more-or-lessness that focuses economists on the middle range.

Merton had thought about this too. He speaks of the virtue of "bring[ing] out into the open the array of assumptions, concepts, and basic propositions employed in a sociological analysis. They thus reduce the inadvertent tendency to hide the hard core of analysis behind a veil of random, though possibly illuminating comments and thoughts. . . . [S]ociology still has few formulae – that is, highly abbreviated symbolic expressions of relationships between sociological variables. Consequently, sociological interpretations tend to be discursive. The logic of procedure, the key concepts, and the relationships between them often become lost in an avalanche of words." He suggests that one of the important functions of mathematical symbols is to provide for the simultaneous inspection of all terms entering an analysis.

By the time I knew him, his breadth of knowledge and depth of penetration were lightly covered over by good nature. But no one after five minutes would have mistaken him for a harmless old codger.

All this has led to a situation in economics that would have amused Bob Merton. I think it has to do with the focusing role of prices and quantities. Only one overarching system, or paradigm, survives: it is what is uninformatively and vaguely called "neoclassical economics." As an inclusive theory it has serious problems of its own. Generally, however, even important subversive movements seem rather to modify it but not to replace it, although some of its characteristic conclusions have to be given up. I will mention two outstanding examples: Keynesian economics gives up on the assumption that prices and wages move flexibly and quickly to equate supply and demand for labor and goods; this is a big deal, but leaves the basic paradigm still quite recognizable. Secondly, so-called behavioral economics wants to give up, in some circumstances, the clearly over-simple assumption that economic decision making is governed exclusively by greed and rationality. It is not so clear what to put in its place, however. Recent research on the role of reciprocity in economic behavior would have fascinated Merton; he would also have noticed that this is middle-range theory of the best kind. In any case, the findings of behavioral economics would change some of the implications of the paradigm, but not fundamentally its basic outline.

Is this resiliency a good thing? Too much resiliency eases over into flabbiness. I didn't like it much when Parsons tried to defend Weber against Landes's observations. But that was a middle-range theory, not a "paradigm." A middle-range theory is supposed to live or die by its empirical bite. Neoclassical economics is, in Merton's phrase, a "theoretical orientation." This is another case where I can imagine my half of a conversation with Merton, but I am not so sure about his.

All that is interesting but not amusing. What would have brought a smile to Bob Merton's

face is the fact that defenders of the pure, unadulterated neoclassical system have found themselves forced by the threat of Keynesian subversion to reduce the all-inclusive system to a single middle-range sort of model and then try applying this to the data of everyday macro-economic life, where it has, on the whole, had no real success. Merton, thou shouldst be with us at this hour.

I hope I have said enough to show you that intellectual engagement with Bob Merton was always an exhilarating experience for a social scientist, even for an economist. He was right about the useful scope for social theory, and he had an unerring eye for intellectual puffery and pomposity. Sadly for me, I only got to know him well in his later years; and this leaves me uncertain about what he was like in his forties and fifties, at Columbia and, with Paul Lazarsfeld, at the Bureau of Applied Social Research.

I had intended to leave aside the celebrated *On the Shoulders of Giants* and the soon-to-be-celebrated *Travels and Adventures of Serendipity*. After all, they are just books about the origins of particular words and phrases and how they acquire and change their meanings as they are transmitted from person to person and place to place and time to time. But I decided at the last minute that I couldn't just forget them: those books say too much about the Merton I knew.

They are, of course, a lesson in how to wear learning gracefully. Maybe it is a lesson we don't need, because so few of us have that much learning to wear. They also demonstrate a much more complex quality. They tell their story. Then they think about the social relations of the people who populate and carry the story, mostly scientists and other intellectuals. Then they reflect on the act of doing the sociology of science and the sociology of knowledge. And then somehow they take an unbemused but tolerant view of the whole act of writing the book. There may be further layers that I am not clever enough to detect. One does first think of Tristram Shandy. But Sterne is artless compared with Merton. No, that's not fair: Merton knew about Tristram Shandy, stood on his shoulders.

What I wonder about in particular is how much of the later Merton persona was already visible in those earlier years. By the time I knew him, his breadth of knowledge and depth of penetration were lightly covered over by good nature. But no one after five minutes would have mistaken him for a harmless old codger. He was in fact the embodiment in the intellectual sphere of Muhammad Ali's description of his

own style: Float like a butterfly, sting like a bee. I believe firmly that anything worth doing is worth doing light-heartedly; and I could point to Bob Merton as an adequate demonstration.

I said that I had never been formally associated with him. That is true. But I did overlap with his wife Harriet for some years on the board of the Center for Advanced Study in the Behavioral Sciences. Sharing breakfasts with Bob and Harriet in the hotel dining room before those meetings taught me what I wanted to be when I grew up. I don't even care that I never made

it. He used to say that he would sometime like to write a joint paper with his son Bob and with me. He wasn't sure about the topic, but it should be signed Robert Merton Duo and Robert Merton Solow. Two thirds of the authorship of that unwritten paper are here, but the best third is missing. ■

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