

**INTERVIEW WITH
ANDY LO
JANUARY 21, 2014**

SLOAN ORAL HISTORY SERIES

L: Andy Lo

A: Alan White

G: George Roth

A: George mentioned the 150th interview. One of the things you mentioned in that is your desire to complete work toward a theory of human behavior. That was about two and a half years ago when you had that interview. I wonder if you could give us a little update, any progress you feel you've made toward this theory of human behavior.

L: In my view there's been a lot of progress because, from my own perspective, I feel like I have a much deeper understanding for the complete picture of human behavior. Prior to the work I've been doing over the last few years, I think the analytical part of human interactions had been pretty well mapped out by economists—the ability to optimize your resources, strategic behavior in games of strategy, etc. But what was missing was the emotional part of human interaction. We often think that emotion is beyond the ability of mathematics to grasp and to model, but over the course of the last couple years, in doing more reading in the neurosciences and evolutionary biology, and doing some mathematical modeling with my students and collaborators, we've come to the conclusion that you actually can model those aspects that previously were thought to be outside the realm of analytics. At this point, we think we have a pretty good model of individual behavior and how it arises and changes across circumstances and time.

What we're working on now is putting that together in a broader context, to understand how those individuals interact when they're put into social settings. Once we do that—which will take some more time, of course—we will be able to get that complete theory of human behavior across all the various different settings that are relevant.

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A: What is your goal? Do you want to come to a point of predictive behavior given certain circumstances? Where are you headed?

L: The “Holy Grail” for me is to do something that was written about in science fiction decades ago by Isaac Asimov in his *Foundation* trilogy, now I think a sexology, as there have been a number of additional volumes added by other authors.

The idea in the *Foundation* trilogy was to use mathematics to understand the dynamics of human history and civilization as we know it. It’s a rather ambitious goal, but in a way that’s what economics and finance really started out trying to accomplish, and over time have narrowed the focus toward more manageable, more mathematical, quantifiable objectives. I think we can return to the broader ambition of trying to understand how societies change over time, how nations rise and fall, and how certain kinds of financial and economic structures may or may not persist across time and circumstances. That’s the ultimate goal I’ve got in mind.

G: So by developing these kinds of models, we can then understand and do what with that understanding?

L: For example, we can understand how crises, financial or otherwise, emerge, how to best deal with them, and maybe someday how to reduce the likelihood that they’ll occur.

One of the things that’s come out of the research we’ve been doing is the fact that financial crises are unavoidable consequences of the combination of human behavior and free enterprise. If you want to get rid of crises, you can do so by getting rid of one of those two features. But as long as you’re willing to entertain both in the same state and culture, you’re going to be subject to crises periodically. If we understand that, we can design our government policy—our institutions—in a way that either reduces the chances that they occur, or when they do occur, will actually reduce the impact on the individuals that are least able to deal with them. That’s one concrete application.

There are obviously broader applications. For example, in some of the recent work I’ve been doing on financing biomedical innovation, it’s clear that we’re reaching a crisis in our healthcare system. The area I’ve been focusing on in particular is drug development. If there’s a better way for us to reorganize ourselves to deal with the challenges we face in

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healthcare, that would be a major improvement in our civilization. By understanding the dynamics of human behavior in financial markets, the role of incentives, and how we can better organize our institutional structures to make better use of the resources we have, we can actually deal with some of the challenges. I don't believe there's a way we can deal with them otherwise. In other words, I think this is both a necessary and sufficient condition for us to be able to address some of society's biggest challenges. We have to get the financing, the economics, and the incentives right, otherwise I think we're doomed to failure.

A: This is probably going to be a naïve look at this, but we had the area of system dynamics trying in many ways to do some of the same kinds of things. Of course, there was this raging controversy in the school between the economists and those who were trying to do system dynamics.

What is different about what system dynamics was trying to do and what you're trying to do? Aren't you applying a particular discipline to this, and suggesting that just by applying that particular discipline, along with knowledge of human behavior, you'll be able to do things that system dynamics was not able to accomplish?

L: Yes, so this is a fascinating point. I'm glad you brought it up because it highlights an interesting aspect of academia that is also part of the story. You're right, there has been a heated debate between economists and those in system dynamics. I have huge respect for Jay Forrester and John Sterman and others in that field. I think there are a lot of insights that system dynamics brings to these questions that economists either weren't aware of or ignored or dismissed. I think the controversy is very important and a healthy one.

I see this controversy playing out not just between economists and system dynamics researchers, but between economists and psychologists, between economists and neuroscientists, between economists and anthropologists. The debate is very much akin to the age-old parable of the five monks who are blind from birth. They encounter an elephant for the first time. The monk who grabbed the elephant's trunk describes the elephant as being just like a snake. The monk who grabs the elephant's leg feels it and describes the elephant as like a tree.

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Each of the monks, strictly speaking, is correct in his description, but none of them have the complete picture.

What I'm trying to get at is the complete picture. I would argue that economists have one part of that picture, anthropologists have another part, and neuroscientists and psychologists have other parts. It's only by understanding what all of them bring to this discussion that you can actually piece together a complete theory. That's what I'm trying to go after. I think there are tremendous benefits we get from going to these other disciplines.

I spent a fair bit of time--particularly in psychology, neuroscience, and evolutionary biology--trying to understand their perspectives on human behavior and human history. When you bring all of those together, you find a very powerful, and I think much more successful theory that ultimately has greater predictive power.

G: That's a good way to put the debate between exogenous and endogenous modeling characteristics between econometrics and system dynamics—that both could co-exist except the ability to develop models that broad would be very difficult given the technologies that have existed. I'm sure that's changing. I should say I've studied with Jay Forrester, John Sterman, and Peter Senge in particular. One of the things I think Peter in particular has added is the notion that modeling ought not to be done by modelers but by managers, as a way of exposing their mental models and their thinking, as a way of improving their management practice.

L: Right.

G: I think the work you're doing, though, is much more informing of policymakers and decision-makers at the level that influences macroeconomic kinds of characteristics.

L: That's one application. I would argue that at various different levels this kind of perspective can be very informative. For example, let me go back to the point you raised about managers, and the point Peter Senge has been trying to make. I would start by saying that managers are clearly doing modeling in their jobs—the good ones, anyway. When Jack Welch

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instituted his hiring and firing policies at GE, one of the things he was accused of as “Neutron Jack,” was basically wiping out large swaths of people. At GE they had this policy, I’m told. I’ve never experienced it myself first hand, but I’ve read and heard about the fact that every year they would reward the top 10% of the workforce and fire the bottom 10% of the workforce. This is, by the way, not a new practice. In the Roman Empire there was a Roman practice of putting to death the bottom decile of the army. After every battle, if you were in the bottom 10% in terms of how hard you fought, you would be put to death by your own commander. I don’t know if that’s true, but the word “decimation” has that root from Latin. Now that’s a model! In order to carry out such an extreme policy of hiring and firing, you have to have a pretty strong conviction in something. You need a model. I have every belief that Jack Welch, in his years at GE, had an explicit model in his own mind of how he was going to build the firm to where it ultimately reached. But having him articulate that model is a completely different matter.

I view the job of the academic, particularly in a management school, to work with people in industry to articulate those models, and perhaps even to improve upon them. I think it would be the height of arrogance and presumptuousness for an academic to think that he or she could, right after graduate school, having never worked a day in his/her life in the industry, start to make judgments about how to reorganize that industry or that firm, or impose new practices. But over time, as young faculty develop the tools and the perspective and then start interacting with people in industry to understand how they engage in their practices and what kinds of models they may implement, over time you can develop a well-articulated theory.

That’s one of the reasons it’s taken me so long to get to where I am right now. As a graduate student and as an assistant professor, I had very little understanding of how the real world works. Therefore, I had a very narrow set of ideas that I could work on, and they were very technical, because mathematics doesn’t require a lot of infrastructure. Most people have intuition about numbers and relationships early on. But it took me a fairly long period of dealing with practitioners for me to fully appreciate what some of those challenges and industry models are, and from those models, how it mapped into the corpus of economic and financial theories. So that process of integration is where a lot of the value comes from in what we do. It’s not so much trying to develop ideas in a vacuum but interacting with industry to articulate those kinds of models, and then, to improve upon them.

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G: That gets to the heart of what we're interested in talking to you about. The question is: how did you create, find, or develop the environment to let you do that? What's been the role of people here at the Sloan School and the community? I think this really happened for you in coming here.

L: Absolutely.

G: I think that's the essence of what we're trying to capture in these oral history interviews. What is it about the place, the people, the dynamics, and the behavior that helped encourage people to develop the fields they have?

L: That's a wonderful question. I've thought about that many times myself. In my own mind I have a pretty clear understanding of the features of MIT that allowed me to do what I do. There are many things, but I'll try to distill it into a few.

There's one over-arching feature about MIT that you can't help but notice and ultimately be affected by, which is that MIT is a school of engineering. Even the school of science, even the school of management, we are all influenced by the fact that we happen to be at a school of engineering. I've often thought about what the difference is between science and engineering. Why do we have to have a term called "engineering?" I've come to the conclusion that there is a very important difference. The difference is something that I revel in and it's very important to me personally because I didn't realize it until much later on, but I share very much the same kind of ethic that engineers do.

I've spent a lot of time now with people in the basic sciences, as well as in engineering, as well as in my own field. When you speak to engineers, they are every bit as quantitative as their scientific brethren. They are every bit as rigorous in their thinking. They are every bit as dedicated to scientific truth. So what makes an engineer? It dawned on me that the difference is really one of objective. Both are curious, but an engineer actually wants to build stuff. They actually want to change the world in some visible way. For them, understanding is not sufficient. When they understand an idea, that's certainly rewarding, but they want to take

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that understanding and demonstrate its value in some visible manner. I think that ethic, which runs throughout the School, affects the graduate students, the undergraduates, certainly faculty, even the way our classes are organized and the way we relate to each other in conversation. It's infectious, and that's very much why I went into economics—to actually have impact.

I started out in high school and early college as a science major—physics, biology, mathematics. Those were my areas of focus. I chose economics because I thought it was actually more applied, and I could have greater impact. I didn't realize it until much later on in my graduate career that economists, or at least the ones I was exposed to, had this disease which I used to call "physics envy" and now I call "math envy" or "theory envy." They weren't really as focused on having impact in practice. Of course that may be just the economists that I happened to interact with. I now know that the field is much broader and there are many economists that are very much engaged in practical applications, and a number of them have tremendous impact.

At the time, I became disenchanted, and I realized that of all the different fields of economics, the one that was the most applied, the one that had the most applicability was finance. It was really quite by accident that I discovered that. As a first-year graduate student at Harvard in economics, I got quite discouraged about the field. One of my high school friends, who happened to be a senior at MIT, suggested that I take Bob Merton's course. I'd never heard of Merton, never heard of finance, really, but I decided to sit in on 15.415. That course changed my life. It opened my eyes to the fact that you can be just as rigorous and yet have impact. For me, finance was the reason I ultimately chose economics.

I didn't know at the time. I didn't realize even then how special MIT finance was, because apparently Bob Merton was not a typical faculty member that you could find at any business school. What I found fascinating was the fact that Merton had an engineering background before he came to MIT. I couldn't put my finger on what it was that made Bob so special, but sitting in his course, I was mesmerized. I decided pretty quickly that's what I wanted to be. I wanted to do that. MIT seems to have had its impact on Bob, as well as other people in this place. So I think it's the engineering mentality, of taking rigorous theory and putting it into practice and demonstrating value.

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The other aspect of MIT that goes along with it—and maybe this is part and parcel of the Sloan School—is a certain entrepreneurialism, a certain lack of regard for formal disciplinary boundaries, and much more interest in the research itself. I remember as an assistant professor, there was a big push by a number of deans—not just here but at Wharton where I spent four years after I graduated—a push for interdisciplinary studies. We were encouraged, indeed bribed, to have lunch with our colleagues in the management department at Wharton. The finance department was the biggest and probably the most powerful at the time, and may still be. I haven't been there in a while. But the finance department was encouraged to interact with other departments. The dean at the time, Russ Palmer, who is a business man and CEO, thought it would be good to set up free lunches. The Dean would pay for a nice lunch at the faculty club and have different tables where departments would get together. Management and finance, management accounting, accounting and finance, etc., and we would have lunch to find out what the other colleagues in other departments did. One time when I had lunch with some management faculty, at the end of the lunch my finance colleagues and I shook our heads and said, “We still don't know what they do.”

G: And the idea was new ideas or new research directions that were innovative would bubble up from those tables?

L: I guess. It was an abject failure, though. After a few of these events, it stopped because it became almost an inside joke among the faculty, so they discontinued the practice.

G: But it was a bubble-up theory, and I think, reading about your career and what you've done, you have had a bubble-up kind of experience in terms of integrating.

L: The reason it wasn't successful is because it wasn't a bubble-up theory. It was more of a top-down effort. After a few years, and certainly as I got more involved in academic administration after tenure, I realized that interdisciplinary work has to happen organically, otherwise it won't happen at all. But the way that it happens organically I now understand

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completely. Interdisciplinary studies emerge naturally when the object of the study is a practical application as opposed to an academic topic. So if you're trying to solve a problem...

A: Like LFM, for example.

L: Right. If you're focused on a particular challenge—revitalizing the alternative energy industry or dealing with climate change or curing cancer.... cancer is a good example. That's a very concrete problem. It's got some tremendous scientific challenges behind it, but it's not a problem about molecular biology. It's not a problem about bio-informatics. It's not a problem about chemistry. It's not a problem about financing. It's all of those things. It's a problem about curing cancer.

G: And not the categories we invented to put ourselves into in departments in universities.

L: That's right. I'm not saying departments aren't valuable. They are; they serve a very important purpose. But we have to keep in mind that the purpose is not to have a chemistry department just by itself. That's not the end goal. The end goal is not to have a finance group or a marketing group. In the end, the problem is to understand a particular phenomenon and try to come up with useful ideas to deal with those challenges. When you focus on a challenge—as with focusing on a disciplinary problem or narrowly defined question—you automatically have to bring in these other disciplines.

The reason I started getting into the other fields is not because I naturally wanted to get involved in biology or neurosciences. It was because I was trying to understand the stock market, and I couldn't with the tools I had. I was fairly well-tooled for an economist and spent a lot of time in graduate school developing those tools, and applied them to good effect as an assistant professor. Got a number of papers published, got tenure. But after all of that, I still didn't think I had a good theory about how the stock market worked. All I could say was that our existing theories did not work, and that's a pretty unsatisfying result after nine or ten years of study.

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I began trying to deal with the various different violations of our theories. One after the other, there was no way I could fix those theories given the tools I had, so I had to start looking for other tools. I think that's what an engineer does. An engineer has no particular religious conviction about how a problem gets solved. You don't care about *how* to solve the solution, you care about solving it. So if you have to bring in other tools, bring in collaborators, so be it. That's where the interdisciplinary nature comes from. That's one of the reasons why MIT is such an interdisciplinary organization because as an engineering school, it's focused on solving problems, not on publishing papers, not on winning Nobel prizes, although we have plenty of those. But its focus is on solving problems.

A: We've gone back to look at the beginnings of management education at MIT. There was a certain amount of controversy about having a management school or even business studies at MIT. That went on for some time, and at one point they even tried to cancel it a couple of times in the early history. When I came to Sloan some years ago, you still heard comments like, "What are they doing over there talking about organizational behavior when we don't understand the brain?" The work of the Sloan School was seen as second-class work. I'd be interested in your perspective on this. You're working across campus—how do you see Sloan viewed in the eyes of the broader Institute?

L: I'm not sure I can speak for all of MIT, since my interactions have been fairly idiosyncratic. There are certain faculty I interact with quite a lot. I would say that the Sloan School has a mixed reputation. Some of it undeserved, but some of it probably deserved.

First, I think there is some greater respect for the school now than there was 10-20 years ago. I think there are a number of explanations. Partly it's faculty who have reached out and interacted with the Institute—Tom Magnanti, Dick Schmalensee, Ed Roberts. There are a number of faculty that have been in closer touch with the rest of the Institute than others.

G: What field was your sister studying?

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L: She was in biology. She did a UROP with David Baltimore. That's another example of how extraordinary a place like this is. I don't think David had won the Nobel Prize at the time, but he was certainly well on his way, and everybody knew he was on the short list for such an honor. Yet, he had no issues with taking on undergraduates and working with them. It was a phenomenal experience.

G: My reason for asking was: did you have a view of the Sloan School back from those days?

L: I had never heard of the Sloan School in those days. I didn't even know there was a Sloan School, and if I did, I'm sure I would have had no interest in it whatsoever. But there are a number of faculty over the years that have created the reputation we now enjoy: Franco Modigliani, Bob Merton, Stew Meyers in my area; Jay Forrester, Bob McKersie, Glen Urban, John Little, Tom Magnanti. As I got a little broader in my focus, and as I ventured out of finance into the other areas, I developed a much deeper appreciation for just how extraordinary Sloan is. I think the Institute has developed an appreciation of that as it has matured in its understanding of what really constitutes business and management.

I think there's a second theme, which is that there's a broader appreciation now in the rest of the Institute that business is actually a pretty important part of the world view that they have. And that they need to develop more expertise in how to relate to the business world, whether it's because of challenges in manufacturing, aerospace, semiconductors, or other industries. All these different industries play an important role in the educational ecosystem that we're a part of.

A: At the conclusion of your 150th interview, the interviewer asked if you wanted to comment on something that hadn't been asked, and you mentioned social responsibility. With all the comments that have been made about the lack of social responsibility in the business world and the financial industry, and with what Sloan is trying to do in education with the master of finance program, etc., how do you feel we're doing in this area? What are we doing to create conditions in which there may be more social responsibility than has occurred in the past?

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L: I've been very pleased with the progress that the finance group and the Sloan School have made in that direction. Let me talk a bit more broadly about social responsibility, because I think that as an educational institution, MIT has a responsibility.

I know that some faculty, once they hear the term "social responsibility," they immediately become skeptical and tune out because that's for somebody else. We can't teach ethics. By the time kids come to MIT it's already too late, it's already baked in. There are all sorts of ideas that would lead people to dismiss the whole area of social responsibility. But I look at it more broadly. I think that as an academic organization, we all have certain responsibilities. As a faculty member, you have a responsibility to show up for class, to prepare for class, to talk with students, to advise students, to engage in research. There are a number of responsibilities we all have when we take on a position. As a tenured faculty member, you have extra responsibilities. I don't know that we have ever been told this, but we should have been.

I think in the finance industry, which I know best, one of the premier institutions I've had the privilege of interacting with over the years is Goldman Sachs. At Goldman Sachs there's a very competitive environment and the objective is to make partner. When you make partner at Goldman Sachs, there is a partners' introduction, an off-site meeting where it's both a celebration and an orientation. The orientation I'm told is one where you are there with other partners that have just been promoted and you are told what's expected of you. What's different? You are now a partner, what does it mean? What are your responsibilities, what are your privileges? And there are many of both. But they have an orientation where they take you through all those features. We don't do that for tenured faculty, and I've often wondered why. I could have used that orientation. I would have liked one where some of the more senior and accomplished faculty, like Franco Modigliani or Bob Merton, would have said to us, "You now have guaranteed lifetime employment. That's an incredible privilege that very few people in society have. You should understand what that means, that privilege. Along with that privilege comes responsibility, and the responsibility is you've now demonstrated that you're able to do first-rate research. With tenure, you need to kick it up a notch. Now you need to change the world because you've been given something that less than one one-hundredth of one percent of the population has. Do something with it."

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I think that that's understood by example. Who are our examples? Paul Samuelson. Bob Merton. Franco Modigliani. John Little. Ed Roberts. These are extraordinarily accomplished people who have made huge contributions, not just to their field, not just to the Sloan School, but to society. That's the kind of responsibility I'm talking about first and foremost. It's the responsibility to actually make a difference. On top of that, within each of our respective fields, we have to think deeply about what aspects of our field are relevant for society, and do what we can to see that it is realized.

Not everybody is going to focus on all of these issues. We obviously have different interests, different talents, different skills, but I think we all have some obligation to think more broadly. It's not an obligation that comes due every year. It's not an obligation that you have to be thinking about and that should weigh on you. But at some point in your career, you start thinking about things. For me, it happened over the years as I saw other finance faculty have a really important impact on what's going on. For me, the financial crisis crystallized all of this because it made very clear that if you don't focus on some of these responsibilities and you let the field develop without these considerations, you can have some pretty bad, unintended consequences.

So we have tried, over the last few years, to make a difference by focusing on aspects of finance that are related to government policy and consumer finance, and try to understand how things went so far wrong over the course of the last decade, and what can be done to make them better. I think every discipline, whether it's finance or accounting or organizational studies, will have the same kind of soul searching, and ought to go through that process periodically. It often takes a major event, like a crisis, to issue that wakeup call.

A: Could you go a little further in terms of how you have impact on students? Understanding the crisis is a key part of it, understanding the things that went into it. But what can we do proactively?

L: You're right that the starting point is understanding. You can't manage what you don't measure. So you have to first start by measuring the underlying drivers of crises. To do that, you need a framework. So that's the beginning. But beyond that, I think it's also developing

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a sense of how to have impact. If you identify the issues that need to be addressed, there's still the problem of how you effect change. Obviously, those in organizational studies have spent a huge amount of time thinking about that. But we in finance haven't. We just assume that if you write down the equation and it's the right equation, it will get implemented somehow, magically. This gets back to human behavior. There's very little appreciation in finance of the dynamics of human interaction.

We brought Debbie Lucas from Northwestern, via the Congressional Budget Office, to Sloan. She's going to be heading up the Center for Finance and Policy (CFP). This is a new initiative we are launching, along with Bob Merton, myself and Andrei Kirilenko, who is the former chief economist of the CFTC. The CFP will be focused not on advocating for any kind of policy, but rather on research that informs policy. We're going to focus on applied finance research that speaks to a variety of policy issues. So it's going to be analysis, it's not going to be advocacy. We're not going to be lobbying. But our hope is that we can be the trusted third party that provides the information necessary for policymakers to make a difference.

Then we're taking a lot of these ideas and bringing them into the classroom so our students—not just MBA students but our MFIN students, our EMBA students, the Sloan Fellows—they're all exposed to these ideas. Ultimately, they're going to go forth and implement a number of them. We've had examples recently of students who would never have taken a job in government. They would go to Wall Street and have very successful careers in investment banking. A number of them are now interviewing and accepting jobs at the Fed, Treasury, and SEC, with the understanding that they're not going to be earning a lot of money but they're going to get a lot of valuable experience. They're going to make a difference, and in three or four years they can take that experience and move to the private sector. But starting out in government, or maybe going back to government service over a period of time, could actually give them something what they couldn't get with Wall Street, which is impact. I think that's the most important difference, is demonstrating to our students that they can actually make a difference and that they *need* to make a difference if they want a better financial system.

We had the financial crisis not because we had too much financial wisdom going on in the industry, but because we had too little. Policymakers, consumers, investment bankers, they didn't understand the consequences of their actions because they didn't have that broader

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perspective of how all of the pieces fit together. We hope our students will have a deeper appreciation of those pieces and be able to make that difference.

A: They may not have a broader understanding, but there's also the argument that the finance material is too complicated. I know that the goal here is to educate people so they understand these complications, but there's a certain amount of worry that the system is just so complicated that it inherently can somehow or another bring destruction.

L: I think that's a real issue, not just with finance, but with society as a whole.

G: You mentioned in the 150th that technology exceeds our ability to understand the consequences of using it.

L: Right. This gets to the responsibility that MIT has as an institution. I believe technology is at the core of why *Homo sapiens* has flourished the way it has. The example I like to cite in my class is that in 1900 this planet had 1.5 billion humans on it. Today we're at about 7 billion. In 100 years—which on an evolutionary time scale is nothing, it's the blink of an eye—we've actually increased by more than four-fold. That's an extraordinary accomplishment for any species. How did we do that? The answer is we did it with all sorts of technology—medical technology, agricultural technology, manufacturing technology, and yes, financial technology.

The problem is that technology is a two-edged sword. It has some very significant unintended consequences, and we're dealing with them right now. If you were in Beijing over the last year or so, you know what kind of technology I'm talking about or what kind of problems. How do we deal with them? In the end, we deal with them with more advanced technology. The answer is not to try to put the genie back in the bottle, that's just not going to happen. We're not going to tell China to undo its industrialization and simply stop burning fossil fuels. That's not going to happen. So we need better technology. In the same way that in the US, when we had massive air pollution in the 1960s and '70s, we dealt with that by developing better technologies—catalytic converters, filters, and other kinds of fuels. We need to develop better technologies.

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This is why MIT has a unique responsibility in society. Technology is our last name. We are at the core, at the forefront, of leading technologies. Shouldn't we also, at the same time, be thinking in parallel about the unintended consequences of those technologies? What that can do to society, and what we need to do in order to prepare for that? There's hope that as we develop those technologies, we can also think much more carefully about the consequences without having to wait for a real-time experiment to tell us that we really need to develop version 1.1 instead of leaving things as they are. I think you're right. We are in a situation where there are lots of unintended consequences, and we need to be much more thoughtful about them, complexity being one of them. The way we deal with complexity is to make things even more complex, so that they're simplified.

A case in point is the iPhone. The iPhone is an incredibly complex device. It's so complex that it's actually easy for us to use. If all of us were taught how to program in Assembler, we wouldn't need these really nice touchscreens and icons. We could simply program things directly. But humans don't do that, it's too hard. So in order for us to make it easy, we've got to do much more complex programming in the background. We need version 2.0 in financial systems, which may be even more complex, to address the nature of the complexity, to make it simple for humans to use them responsibly.

I think anybody who really understands the true nature of technology appreciates the fact that we can't afford to be Luddites and say, "Nope, not going to use it." We need to be smarter and work harder and say, "How can we do better? How can we make it idiot-proof?" That's the hard part.

One of the projects I'm working on now, in contrast to artificial intelligence, is a project called "artificial stupidity." I want to understand how it is that humans are so bad at making financial decisions. I want to model the mistakes people make. If I can understand how they make those mistakes, I can then understand how to help them avoid those mistakes.

G: You said it well in that interview. With one mouse click you can change your retirement future...

L: It's scary.

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A: We've got to get back to your area of human behavior.

L: Right. It all comes back to that: The fact that we've designed incredibly powerful tools, but we haven't redesigned human behavior to the point where we can always use them responsibly and effectively. What we need is to make those tools idiot-proof. We're not there yet in big finance. We have a ways to go.

A: I wish this interview included Ed Schein. The two of you would be quite interesting. I don't know how often you had discussions about what you were trying to do with Ed Schein.

L: Not at all. I've interacted with him on occasion in committees and so on, and I've heard a lot about his work. I have never had the privilege of talking with him about this.

A: Would you say something about the concept of "too big to fail" in the context of what you've been saying?

L: Sure. That's another issue that I think lends itself to the kind of interdisciplinary research I've been doing. I have a very different take on "too big to fail" than my economics colleagues. Part of it is informed by some of the perspective I gained from talking to evolutionary biologists; some of it is just my own analysis of how to optimize corporate growth.

If we look at financial services firms (or any firm for that matter), we have to start by asking the question, "Are there advantages to size? Does size matter?" The biologists have asked this question for many generations now, and they've come up with some pretty compelling answers. In the world of biology, size absolutely matters. It's interesting to notice the change in size over time and across different environments. At one point we had dinosaurs that ruled the earth. They were huge creatures. We don't have that today. Why? Why is there a difference? It's because of the nature of the environment. For certain environments, sheer size provides you with significant competitive advantages. But in other environments, in other climates, where you have

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different resources, size can actually be a disadvantage. The biologists have very elegant mathematical models to describe the optimal size for a creature given the competition, given the resources, given the climate. All of these considerations come into play in determining size.

That logic applies to everything, including companies. There are certain companies that are advantaged and very large-size; there are other companies that are disadvantaged at large size. So the question to be asked is: “Are there any reasons for a given company to be a given size?”

Let’s turn back to the financial industry. What is finance about? At the heart of it, finance is not just about moving money around, it’s about taking risk. Risk is central to financial intermediation. “Nothing ventured, nothing gained.” “You don’t get something for nothing.” “You’ve got to risk in order to make money.” All of these adages come from the fact that finance is about making wagers and being right and wrong. If your business is about taking risk, there’s a natural consequence, which is you want to reduce that risk, you want to control the risk. One of the best ways of controlling risk is to have a large portfolio of independent bets. If you’re trying to reduce the volatility of your corporate earnings, you reduce volatility by having lots of different kinds of earnings that are not perfectly correlated. The more you have, and the less correlated they are, the more likely it is that you will survive the ups and downs of the business cycle. That suggests there is strong economic incentive to become too big to fail.

I haven’t even mentioned the notion of economies of scale. That is, when you’re larger, you can negotiate better pricing from your counter-parties. We already knew that. I’m talking about something distinct from that, which is economies of diversification. As corporate CEOs, the leaders of these financial services firms have every incentive to make their company too big to fail, even without government guarantees waiting in the wings. If there were no government at all, they would still want to grow as large as possible, be as diverse as possible, and be as complex as possible. You would think they should stick to their knitting. But if you stick to your knitting, you don’t get diversification. You’re just increasing the size of the firm.

G: You get standardization, which is what drives a lot of large organizations. They get to a size and don’t have the oversight so they try to standardize, which is the antithesis of diversification.

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L: Right. And there are economic incentives that force us down that path. I think the first point is to recognize that there are incentives to be too big to fail. If we want to prevent it, we have to somehow change those incentives, or at least recognize that they exist and somehow address them using other means.

The second point is: too big to fail is not necessarily inevitable if you provide proper pricing. If there is an implicit guarantee, make it explicit and then price it, charge for it. If we know for a fact ahead of time that a large company that employs lots of constituents of politicians will go under, and you know that it's inevitable that those companies are going to be bailed out, when you get to a certain size, you ought to be forced to buy that kind of insurance. Because you're going to get it anyway, so you may as well pay for it and the taxpayer will be made whole. We can certainly do that.

General Motors is a good case in point. GM had nothing to do with finance as far as I can tell. They had nothing to do with the mortgage crisis, and they had nothing to do with anything. They produced cars that people didn't want to buy. They lost money and GM was going to go under. Somehow, as part of the financial crisis, we wrote a check to GM for \$40 billion. In fact, we gave \$80 billion to the auto industry as a whole. What did that have to do with the financial crisis? Why are we handing out \$80 billion? Because we care about the auto industry, because it employs hundreds of thousands of American workers. That's a very important aspect of political dynamics that nobody ever anticipated. But it's there. If it's there, and if GM was too big to fail, maybe they should have been paying some kind of insurance premium the way that FDIC insurance is paid by the banks ahead of time.

Too big to fail isn't necessarily a bad thing. We did a very good thing in saving the auto industry and making it more competitive and reshaping it. Hopefully, it will be more competitive going forward. But make no mistake—that cost the taxpayers a significant amount of money that could have been spent doing other things, like curing cancer. In order to make those proper tradeoffs, financial markets, when they're allowed to work, actually work well. But then we have to make it work for the entire system, not just for part of it. We can't simply exclude a part of the system and say, "Oh, you don't have to pay for that. You get that for free." Whether it's banks or the auto industry or the manufacturing industry, we need to properly price

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everything so we get a sense of what is going to be the best use of our dollars. It's when we don't use the price system that we get into trouble because then there are non-economic motives for making decisions. Who knows whether or not that's driven by a small, elite group of people that has nobody's interests except their own at heart, or whether it's driven by public servants who do care about the public interest. We can't tell.

G: I was thinking of the automobile industry example. The externalities. It's one thing to have paid \$40 billion to keep GM going. Could we have assessed what the cost to the government would have been to provide the social services that would be necessary for the workforce and then dissolution there? It was perhaps not putting that temporary financial fix in place. I don't know if anyone has ever analyzed that, but clearly there was not a strong incentive to do that.

L: That's just it. I think there are so many questions that we should have asked, and we could have answered in making a better decision. Not just with GM, but with AIG, with all of the banks. We wrote a \$120 billion check to AIG.

G: Right, and who got it from AIG is another issue, where it flowed back down, how that money was used.

L: This is the large-scale question that, as a society, we have to address. The US is in a privileged position because, first of all, we can afford it. We wrote a huge number of checks during the financial crisis, and it's astonishing that we did. But to me, it's also astonishing that we can afford to do it. That tells you something about this country. It's a great country. But it also suggests that with that kind of money we could do a lot of different things.

A: Along that line, you talked about a huge investment to cure cancer. How do you go about determining that kind of investment? I've seen up to \$100 billion. What difference would it make? Years ago I went to Eli Lilly research labs. They had just gotten some new high-speed computers. We asked them what they were going to do with them, and they said, "Well,

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we've been using these wooden blocks. Now we're really going to be able to..." They started using the new, high-speed computers and we asked them what kind of progress they were making. They said, "We're making some progress, but the problem is that the bugs are faster than we are." We throw more dollars, more technology, etc.

So my question is: how do you come to a comfortable statement that a huge investment of that order could have that kind of impact?

L: It's really out of reading the current literature on where we are in the fight against cancer that gives me optimism that we can actually make a difference. The way I got interested in this was quite by accident. It was family and friends who were afflicted with cancer that got me to start thinking along these lines. When you read the literature, both cancer research as well as some of the more applied medical literature, you're struck by the fact that over the course of just the last decade, we've developed enormous new insights into the underlying mechanisms of cancer.

You're right that the bugs change faster than they have in the past. But the difference is now we know that. Not only do we know that, we actually now know how they change. We can track it, measure it, take it apart. Part of this is due to the human genome project. We now have sequenced the human genome so we know how to read our DNA. We know how cancers alter or mutate in ways that make them grow. We're not there yet in terms of understanding how to turn it off, but we're getting close—which may be 10-20 years. It may not be in my lifetime, but I think it's going to be in my children's and certainly in their children's lifetime that we're going to be able to understand the issues.

We're getting smarter, and that's part of a larger theme I've been focusing on in terms of intergenerational collective intelligence. The fact is we've spent, over the last 30 years, \$300 billion on cancer, both at the NCI and in industry collectively. That number shocks some people, and it's led some critics to say, "You know the war on cancer that President Nixon declared in 1971? We've lost. Not only have we lost the war, but we have no hope of winning the war." That's a very defeatist attitude. That argument goes on to say, "We've spent \$300 billion. What's another \$10-20 billion going to do?" I find that almost laughable, that kind of logic. It cost NASA about \$100 billion to go to the moon. That's in the end, if you add up all the

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costs, what it took to get a man on the moon. Suppose we had asked the same question at \$90 billion. “We’ve spent \$90 billion trying to go to the moon. But what’s the point of putting another \$10 billion in?” If it costs \$100 billion, that’s what it costs.

In the case of cancer, we’re actually pretty close, but we’re not there yet. If you build 90% of the bridge, do you give up and say, “I’ve already spent enough money. I can’t afford to finish the bridge.” We need to finish the bridge. It may take another \$30-40, even \$100 billion. In the end, if it costs \$400 billion to cure cancer, in my view, that’s a bargain based on where the science is today, and the fact that we’ve actually invested in this for 40 years. The National Cancer Act of 1971 gave us 40 years of concentrated effort to understand the disease. And the human genome project, another \$3 billion on top of that, added enormous understanding for how to deal with the genetic basis for cancer. We have examples of successes. Certain kinds of leukemia are now cured. We’ve now divided breast cancer into like 15 different diseases, each of which can be tackled, except for some of the really nasty ones. We understand what we need to do in order to deal with them. So our understanding has greatly increased. That’s what gives me confidence that we’re almost there. But by almost there, I don’t mean another few hundred thousand. We need another \$30-50-100 billion to get us there. And we shouldn’t give up now. This is the worst time to pull money back from this space, and yet that’s what we’re doing. We’re doing that because of the complexity. Instead of shying away from it, we ought to embrace it and say, “Wow! We are so close. It’s time to redouble our efforts to get there.”

This is not based on my own expertise, of course. I’m not an oncologist, I’m not a molecular biologist. But this is one of the wonderful things about being at MIT. I have a very skewed view of the world. We’ve got the Koch Institute, the Whitehead Institute, and the Broad Institute, just within walking distance. I’ve interacted with people at every one of those institutions, and they’re just amazing people. I have confidence that if you give these amazing people enough runway, enough resources, and let them at it, they’re already pretty highly motivated, so they will do the right thing. But you’ve got to get those resources. So as an economist, that’s what I can do, that’s my small part in trying to get the ecosystem working.

A: Could you say a little about regulation in the financial industry and the state of regulation? Where do you think we should be going with regulation?

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L: There again, I have a different view about regulation that's informed by my overarching theme of human behavior. I think we've forgotten that regulators are human, too. So the regulations we designed are relatively static, command-and-control kinds of rules. They're static, meaning they proscribe particular kinds of behavior and put limits on other kinds of behavior. Then it's up to the regulators to implement a number of these policies, not taking into account the fact that the regulators are also using human judgment, which can be faulty at times.

A good case in point is the derivatives market, which figured so prominently in the financial crisis, particularly credit default swaps. In 1999, Brooksley Born, who was, at the time, the head of the CFTC, raised an issue about the credit default swaps market, saying that it was growing very fast, very big, and it really needed to be reined in. She was unilaterally shouted down by Larry Summers, Bob Rubin, and Alan Greenspan in no uncertain terms. It was brutal how they dealt with her. So nothing happened to rein in the credit default swaps market in 1999.

Now it's easy to say that Brooksley Born is a hero and these other three men were villains. But the story is a bit more complicated. If you look at the data back in 1999, the credit default swaps market was actually pretty small. But from 1999 to 2006, which was the peak of the housing market, credit default swaps grew by a factor of 20. They just exploded in popularity. In 2006, the market was way too big. So in 1999, Summers, Rubin, and Greenspan weren't actually wrong, but Brooksley Born wasn't wrong either. She was just too early. She sounded the alarm probably three years too soon, so nobody paid attention to her. But rather than keeping track of that issue, monitoring it, and then reacting to it along the way, people just forgot about it because we were all making too much money. Housing markets were going up. We had the Internet bubble bursting, and the stock market took off after that. The only thing people learned from the Internet bubble was to buy on tips. Everybody was doing well. The economy was growing, the housing market was doing well, consumer spending was up, inflation was reasonably low, monetary policy looked like it was working beautifully. All of the things you would like an economy to be doing were happening. So the regulators, the policymakers, the politicians, everybody looked brilliant.

Someone once said it is very easy to confuse your IQ with your W-2. We all looked terribly smart during those years! As a result, human behavior being what it is, we all

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became complacent. We became fat and lazy. The regulators as well. And by the time we realized it, it was too late.

Regulation has to recognize these patterns of feast and famine. They occur, and when they do occur, it's going to be very hard to effect policy changes, just as Brooksley Born encountered. If Brooksley Born tried in 2003 or 2004 to do what she did in 1999, she would have been shouted down by even more people because things were going even better then.

It's not enough to have regulators like Brooksley Born. If we had only Brooksley Borns out there as regulators, they still wouldn't be able to move markets and institutions when everything is going well. We need to design a more robust system that adapts to these changing market conditions. In other words, we need version 2.0 in regulation. We need regulations like the iPhone, which is so easy that it's idiot-proof. And we haven't even begun to understand how to go about doing that.

So that's really complex, but I believe that it's part and parcel of the same kinds of problems we're working on in other areas. System dynamics is a good way of thinking about it. Regulation is part of the system, and you need to incorporate that part of the system when you're designing the entire system and make it robust. We haven't done that yet.

A: So we're an accident waiting to happen.

L: We are. It's quite likely that within the next 2-3 years, we're going to see another accident for that reason because we haven't designed robust regulation. We need to have another crisis. I think it was Rahm Emmanuel who said: "Crisis is a terrible thing to waste." I think we wasted it. We have made some progress, but it's going to be two steps forward, one step back. We're going to have another crisis. Certainly Europe is a crisis waiting to happen. China is on the verge of a significant event. ICBC engaged in a loan that ultimately went sour. It's not a very large amount of money but they're being asked to make restitution.

A: It's coming back down.

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L: That's right, and that could have a domino effect. So there are a number of things that could trigger the next crisis. If we do, it will be very disruptive. But the silver lining will be that maybe then we will develop the moral imperative to make genuine reforms that can actually be effective. We'll take the time to try to understand what reforms are going to be the most effective. We're not there yet, and I think we're another accident or two away from developing the resolve to do that. But we'll get there.

A: We should ask you if there's anything you'd like to say.

L: The one thing we haven't talked as much about is the students at MIT. I think that's an important element. There's a very different attitude among the students here than at other schools. I haven't been to all that many schools, but I was a grad student at Harvard, undergraduate at Yale. I taught at Wharton, and I've got friends at all the other major universities.

There is something very special about MIT students. Because MIT is such a strong engineering school, we tend to attract those students. But the students I've had the privilege of working with here—and it really is a privilege—I view a number of them more as colleagues than as students. They bring to the school an incredible amount of curiosity and a hunger to make a difference. What they don't have are blinders to what is possible. That's incredibly refreshing. As academics get older, it's very easy to get set in your ways because by definition, if you have been around here for 20-30 years, you must have reached a certain level of success and accomplishment, and it's very easy to become lulled into a false sense of complacency about your own worth. As a result, you develop very strong views—I would almost say “religious convictions”—about how the world ought to be. Our students come in with a fresh perspective, but they also come with a healthy skepticism of the status quo, and I think that's a very important part of the culture.

These students have a degree of self confidence that is both necessary and sufficient for making real progress. Working with the students has given me tremendous insights that I don't think I would have gotten on my own, and certainly would not have allowed me to engage in as many different activities. The students provide an enormous amount of intellectual

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leverage for faculty members. And they benefit as well in being able to interact with faculty as much as they do. I've talked with colleagues at other universities—they don't have the same experience of being able to get so much out of their students. I've published papers with undergraduates here, never mind the PhD students. The undergraduates are phenomenal resources as well. So this is a pretty special place.

For me, it's really like a kid in a candy factory, not a candy store. There are so many things to be done, so many interesting questions to ask. And we have the resources to be able to do that. So it's a really special privilege for me. Every time I walk into a classroom, I feel grateful for being part of that, because there are so many people who would love to be in my shoes. To be able to walk into an MIT classroom and have 100 students listening to me talk about my ideas, I think that's pretty extraordinary.

A: Really appreciate it.

L: Thank you! It's an honor to be part of this.

END OF INTERVIEW