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History, Economics and Society: Dividends of Development, Dividends of Interdisciplinarity

An interview with Mary O'Sullivan

Mary O'Sullivan, Jonathan Marie, Matthieu Montalban and Agnès Labrousse

1 Mary O'Sullivan has been the Director of the Department of History, Economics and Society at the University of Geneva since its creation in 2014. Since 1996, when she graduated with a Ph.D. in Business Economics from Harvard University, Mary O'Sullivan was successively Associate Professor at INSEAD and then at the Wharton School of the University of Pennsylvania and, from 2010, a Professor of Economic History at the University of Geneva.

2 Her research focuses on industrial development and innovation; the organisation, control and financing of firms; and the historical and comparative analysis of capitalist institutions. Advocating an interdisciplinary approach in social sciences, she calls for a more important integration of history in economics. She is the recipient of a number of different awards for her academic research -- she obtained the Harold F. Williamson prize in Business History in 2012 and the Henrietta Larson Award for the best article published in the *Business History Review* in 2014 – and for her teaching.

3 In addition to her numerous publications in academic journals (including "Maximizing shareholder value: a new ideology for corporate governance", with W. Lazonick, *Economy and Society*, vol. 29, 2000; "The Political Economy of Comparative Corporate Governance", *Review of International Political Economy*, vol. 10, 2003; "A Fine Failure: Relationship Lending, Moses Taylor, and the Joliet Iron & Steel Company, 1869–1888", *Business History Review*, vol. 88, 2014; and, most recently, "The Intelligent Woman's Guide to Capitalism", *Enterprise and Society*, winter 2018), Mary O'Sullivan has written three books: *Contests for Corporate Control: Corporate Governance and Economic Performance in the United States and Germany*, Oxford University Press, 2000; *Corporate Governance and Sustainable Prosperity*

(with W. Lazonick), Macmillan, 2002 and *Dividends of Development: Fits and Starts in the History of U.S. Securities Markets, 1865–1919*, Oxford University Press, 2016.

1. Main intellectual influences and analyses

4 **Régulation Review:** You were a student at the University College in Dublin (where you obtained a Bachelor of Commerce), then at Harvard Business School, graduating with an MBA in 1992, and at the Harvard Graduate School of Arts and Sciences where you defended your Ph.D. thesis in Business Economics entitled "Innovation, Industrial Development, and Corporate Governance" in 1996. How did your university education influence your willingness to become a researcher in social sciences? Was it partly contingent?

5 **Mary O'Sullivan:** Yes, there was a degree of contingency involved since I certainly did not embark on an intellectual life. I was born into a milieu in which a university education, even a Bachelor degree, was a novelty. Neither of my parents finished secondary school, and although they were enthusiastic advocates of their children's education, they saw it mainly as a route to our socio-economic advancement. That does not mean that I grew up bereft of intellectual stimulation but, as was common in Irish society, my interests were expressed mainly through literature and history. It would have been difficult for me to conjure up an image of a social scientist at the time, since there were so few role models to whom I could look. Intellectuals in Ireland do not have the kind of public legitimacy that they have in France; indeed, in a recent book published by the Royal Irish Academy, the authors ask whether Irish society is "anti-intellectual". Whatever the answer, the fact that the question can even be asked speaks volumes; certainly, when I was a 17 year old, to say that you aspired to be a social scientist would have provoked bemusement and probably some hilarity.

6 I studied business and economics at university because I was good at mathematics and liked working with numbers. My experience at university was intellectually underwhelming and it reinforced my sense that higher education was about stamping my card to move on to better things. Almost all of my classes took place in large amphitheatres, where I was one of two or three hundred students. We had limited direct contact with the professors and hardly any prospect of being inspired to follow in their footsteps. I did well on my final exams and could have stayed on at university but by then I was impatient to get out of formal education and find something more stimulating to do.

7 That was not so easy in Ireland in the late 1980s with unemployment reaching 17 per cent, and youth unemployment even higher than that, so like many people of my generation I emigrated to London. When I think about the kind of economist I was to become, there is no question that this experience, coming when I was still only twenty years old, was truly formative. I worked for the consulting firm McKinsey & Company, and doing so exposed me to a world in which, on the one hand, economic concepts like profits, investment and capital seemed concrete and consequential and, on the other hand, economic theories and evidence were mobilised and applied with great vigour to promote particular points of view. When McKinsey sent me to the Harvard Business School to do an MBA, the influential and ideological character of economic ideas became clearer still.

8 I arrived in Cambridge, Massachusetts, as Michael Jensen was penning and publicizing some of his most influential work on shareholder value.[1] His course at Harvard Business School was designed as an introduction to price theory for MBA students but no course on price theory ever had the kind of success that Coordination, Control and the Management of Organizations (CCMO) enjoyed. Students flocked in their hundreds to hear Jensen[2] rail against "fat and lazy" U.S. corporations and to advocate the downsizing of their workforces and the distribution of their profits to shareholders. Jensen's weekly class was a lesson in performance, and there was much to admire in his analytical acumen and agility, but sitting in his classroom, surrounded by his acolytes, my stomach turned. I started to take issue with what he preached, arguing every point with him, back and forth, until he snapped one day and told me that if I ever wanted to make my arguments persuasive I should go and get myself a PhD in Economics.

9 Naïve as I was, I followed his advice and trotted across the river to the Harvard Economics department. What a rude awakening that turned out to be! Very few people can say they were truly radicalized by an MBA and a PhD in Economics, but that is what happened to me. No one wanted to talk seriously about capital or profit or enterprise in the Harvard Economics department. Instead, they spoke about the "free market economy" and the "competitive market economy," and, well, you know what they say about a lie. If you repeat it often enough, it becomes politics, and that is when capitalism, and the possibilities that economics offered to understand it, began to intrigue me.

10 **RR:** What are your main intellectual influences? We would be especially interested to know if Stephen Marglin (who was your PhD adviser), and more generally the American radical economics movement, were important to your own intellectual formation.

11 **Mary O'Sullivan:** In the Department of Economics at Harvard, as in other US graduate programmes, we had to complete a year of intensive preparation for a set of general exams in Microeconomics, Macroeconomics and Econometrics. We spent so much time trying to master the technical details of what we were taught that we had no time to think about its economic substance or relevance. I had imagined that I had strong skills in mathematics but I had never taken courses in advanced calculus or matrix algebra and I literally slept with a maths book under my pillow so I could read a chapter or two if I woke up in the middle of the night. From the beginning, some of us were frustrated at the narrowness of the supposedly advanced economics that we were being taught but, in those early months, we had an uneasy sense that our frustration might stem from a failure to fully grasp its technical details.

12 The capacity of mainstream economics to disarm potential critics in this way is crucial to its hegemony and it certainly worked its magic on my doctoral class. But then Steve Marglin showed up to teach us. He only had six weeks but he used them well, treating us to a whirlwind tour of Keynes and Marx and Schumpeter and more besides. For me, it was as if someone had turned on a light at the end of a dark tunnel and I was not the only one who experienced Marglin's classes in this way. I remember asking myself why we had spent all that time learning what we had been taught when there was so much really fascinating material to study. In macroeconomics, for example, N. Greg Mankiw transmitted the essence of Keynes by evoking his hairdresser's "sticky prices". Marglin's Keynes seemed much more difficult to reconcile with a mainstream view of the world and made Mankiw's Keynes shrink to trivial dimensions.

13 What Marglin offered was liberating but destabilising too and I do not remember how I managed to knuckle down and prepare for the general exams. Once I surmounted that obstacle, however, I was determined to explore the intellectual richness of economics that Marglin had showed us and that so many of his colleagues seemed determined to conceal. I packed two huge duffel bags full of books, headed to France for the summer, rented a room overlooking the Seine, ate the same cheap meal for dinner every day, and spent my days reading from morning to night. I read all of the economists that Marglin had introduced and many more besides.

14 That summer was the crucial breakthrough for me, revealing the potential intellectual interest in the study of the economy and the wealth of theories and methodologies available for that purpose. It helped me grasp the limits of the education offered by the leading graduate programmes in economics and it made me determined to find my own means to a more rounded and thorough education in the discipline. From that point on, I defined my intellectual ambitions in a way that marked me out as a heterodox economist and I was committed to pursuing them as far as they would take me. I was determined too to take full advantage of the intellectual opportunities that an elite education offered. Harvard University became a kind of treasure trove for me in my discovery of social sciences as well as law and history. It was all the sweeter for the fact that, as a scholarship kid, I received my education there at a bargain price.

15 I still had to get through the rest of the doctoral programme and write a thesis at the end of it but Marglin guided me well in this regard. He had a long-standing interest in themes that I was trying to bring together in my doctoral research. One of them was capital and investment, which Steve had worked on from a mainstream perspective, and later in an increasingly heterodox fashion. The other theme was the organisation of production to which he had made a seminal and controversial contribution with the article, “What Do Bosses Do?”, that contributed greatly to his reputation as a radical economist in the United States. He gave me a lot of useful advice about what I should read but accorded me a good deal of freedom too in terms of what I wrote.

16 Other than Steve Marglin, the American radical economist who influenced me most directly was Bill Lazonick. He had been hired as a Marxist economist in the Harvard Economics Department but he left when it was clear he would not be tenured there. It was Steve who put us in touch since he knew I was interested in bringing theory and history together in a way that had long defined Bill’s research. When I met Bill, he had developed a distinctive approach that combined Marxist and Schumpeterian economics with economic and business history to explore the institutional conditions for economic development. We found fruitful points of contact and embarked on an intellectually stimulating partnership for eight years or so.

17 I found other sources of inspiration among American radical economists, like Mike Best, who had been at U. Mass Amherst for a long time, and wrote a fascinating book at the time called *The New Competition*. I greatly appreciated Jane Humphries too, for her work as a feminist economist, as well as an economic historian, and for the fact that she was one of the few women I met in the sphere of heterodox economists. Alice Amsden deserves a special mention too in this regard for her determination to challenge economic orthodoxy and her unforgettable style in doing so; I still have vivid memories of her wagging her finger at some cowering mainstream economist as she shouted “get the prices wrong”!

18 Notwithstanding the importance of these heterodox economists, however, I did not feel as if I was part of a radical economics movement in the United States. There were other students in the economics department looking for heterodox alternatives and I drew inspiration and support from them but there were limits to what we could offer each other since we all worked on such different topics. Recently, I read an article celebrating the 50th anniversary of the Union for Radical Political Economics (URPE). I have read the work of many of the economists interviewed for that article, as many readers of the *Régulation Review* have too, and I have met a good few of them at one stage or another in my career.[3] However, what struck me most forcefully is how close-knit their group seemed – with photographs of summer camps and barbecues to reinforce the point – and how far that was from my experience as a young economist. I should say that I am not terribly good at 'networking' and I am sure that I could have tried harder in this regard. Still, whatever explains my experience, I look back on my time as a graduate student at Harvard as one of the most intellectually stimulating and socially isolating periods of my life.

19 **RR:** Were you in touch with Régulation theory?

20 **Mary O'Sullivan:** No, not when I was in the United States. What I did come across when I was there is the Social Structures of Accumulation (SSA) approach. As you know, it was developed in the United States in the 1970s and 1980s, by David Gordon, Michael Reich and Richard Edwards, at about the same time that the Régulation theory was emerging in France. And it has important aspects of overlap with Régulation theory, notably in its emphasis on the potentially unstable character of accumulation and growth and the importance of social structures or institutions in controlling this instability. I read a good deal on SSA and then followed the work of some of its founders, with David Gordon being a particular favourite of mine until his untimely death in the mid-1990s.

21 It was only when I moved to France to take up my first academic appointment at INSEAD in Fontainebleau that I came into direct contact with Régulation theory. At the time, I was searching rather desperately for some regular contact with heterodox economists. I could hardly believe my luck when I came across the regular CEPREMAP seminar where I met Michel Aglietta, Robert Boyer, Benjamin Coriat, André Orléan, Pascal Petit and Olivier Weinstein. It soon became clear that there were many points of overlap between the questions of corporate control that Bill Lazonick and I were working on in the late 1990s and early 2000s and the finance-led growth regimes that some of these French economists were studying. Indeed, Bill and I and another colleague, Marie Carpenter, ended up working on a joint project with Michel Aglietta and Antoine Rébérioux as we explored these issues. It was around that time too that I met François Chesnais and Catherine Sauviat, and we went on to enjoy wonderful discussions about economic dynamics for many years thereafter.

22 **RR:** We may identify three main themes that you deal with:

- Industrial development and innovation
- The organisation, control and financing of firms
- Historical and comparative analysis of capitalist institutions

23 Could you present your vision of the relations among these domains?

24 **Mary O'Sullivan:** My initial interest in economic analysis was to help me understand the long-term dynamics of capitalist development in my own country. Ireland's economic difficulties were vividly apparent in the 1980s and early 1990s and there was much

discussion about their character and origins. In the Republic of Ireland, as in other postcolonial states, there was debate about whether capitalism itself was at fault or the way it had been imposed. As a result, I had as many questions as answers as I began to think seriously about Ireland's economic challenges.

25 I had begun my PhD in economics in the early 1990s and I used my reading in heterodox economics as well as business, technology and economic history to shape the kind of analysis I wanted to undertake of the Irish economy. A report published in Ireland in 1992, by the Danish sociologist Lars Mjøset, was an important source of inspiration in showing the potential of comparative analysis for my task. His study was unusual in comparing Ireland to European countries other than Britain. Moreover, it relied on a comparative institutional approach, strongly influenced by the national systems of innovation literature, which seemed to me to open up really interesting perspectives on the dynamics of industrial development and innovation.[4]

26 These various influences came together in my earliest academic publication, which was on manufacturing and global competition in Ireland. There I presented a variety of theoretical approaches to the economics of industrial development, grounded in the historical experience of different economies, as the basis for a critical analysis of industrial policy and performance in Ireland. My analysis appeared in a chapter in *The Economy of Ireland*, edited by John O'Hagan, an economist at Trinity College Dublin. He had a more mainstream perspective on Ireland's economic problems than me, and I am sure there was much in my analysis with which he disagreed but, to his credit, he was determined that young Irish economists should have their voices heard. Certainly that publication, which was widely read by undergraduate students in economics all over Ireland, is the only one that has ever earned me a free drink in an Irish pub from someone who was forced to read it for an exam!

27 I continued to work on Ireland and published further research on its industrial policy and performance. But reading Marx and Schumpeter and Penrose and Gerschenkron and Chandler and Rosenberg and Best and Amsden and Nelson and Freeman convinced me that the scope of my research agenda needed to stretch beyond the Irish experience and that explains why I wrote my doctoral thesis on Germany and the United States. From a theoretical perspective too, my thesis marked a transition in my research from a broad concern with the process of industrial development to a more specific focus on the organization and control of firms for understanding it.

28 That said, the way I approached the "theory of the firm" was influenced by my emphasis on the central role that powerful enterprises in shaping the developmental trajectories of the economies in which they were based. And the control or governance of corporations interested me only insofar as it could help me understand the institutions that shaped the decisions they made about allocating resources and returns. Viewed from this vantage point, economic debates on corporate governance in the late 20th century seemed wholly inadequate for dealing with the theoretical complexities and historical realities of the large corporate enterprise.

29 From a theoretical perspective, the Anglo-American debates on corporate governance were confined to shareholder theory, the dominant perspective, and stakeholder theory, its main challenger. Both theories of corporate governance recognised that, in practice, "residual returns" that could not be attributed to the productivity of any individual "factor of production" were generated by business enterprises and persisted for sustained periods of time. What seemed both peculiar and problematic, however, was that

advocates of these theories were happy to pronounce on the distribution of corporate surpluses without any explanation of how they were generated. My critique of this incoherence in leading theories of corporate governance, as well as my efforts to propose an alternative perspective that was grounded in economic research on innovation, formed the basis of my theoretical analysis of the enterprise and its governance.[5] It was closely integrated with a major programme of empirical research, conducted individually and in sustained collaboration with Bill Lazonick, on historical trajectories of corporate control in Britain, the United States, Germany and Japan.

30 The scope of our historical analysis of corporate governance was broad, covering institutions that shaped the role of employees in the corporate economies as well as the relationships of financial interests to corporations. The main finding from this research was that leading theories of corporate governance were a poor guide to understanding the governance institutions that had supported sustained innovation by corporate enterprises over time and across country. From the late 1990s, the growing influence of financial institutions, actors and motivations in shaping the allocation of resources and returns in the corporate economy became an increasingly prominent theme in my research. My joint work with Bill Lazonick on institutional change in the United States culminated in a frontal attack on maximizing shareholder value as a new and destructive ideology for corporate governance. By then, debates about corporate governance had really heated up in Europe so I started to work too on a comparative perspective on these debates to understand how and why the ideology of shareholder value was being mobilised in the corporate sectors of continental European countries.

31 These were lively times, and the stakes went far beyond academia, but eventually I tired of the contemporary debates on corporate governance and I began to seek perspective in a longer view. I became preoccupied with the limitations of existing historical research for addressing important questions about institutions and economic dynamics. In this regard, I was struck by the sharp disconnect between the questions that economists asked about the role of finance in economic development and the answers that one could find in the historical literature on financial systems. Until then, I had been an economist who read widely in history and used it in her research but I had not generated historical evidence myself. Yet, as I encountered one obstacle after another in exploring the historical role of financial systems in economic development, I decided that the only way to address them was to transform myself into a historian as well as an economist.

32 I began by writing an article that identified important analytical questions about the role of financial systems in economic development where historical evidence was especially lacking. Then I conducted a series of historical studies of firms, industries and financial markets to experiment with methodologies and sources I might use to generate the historical evidence I wanted. Finally, I opted for full immersion in historical research, committing myself to a project on the history of US capital markets that culminated in my most recent book, *Dividends of Development: Securities Markets in the History of US Capitalism, 1866-1922*.

33 **RR:** What are the key lessons you've learned from writing this book? More specifically, could you explain this quotation excerpted from the conclusion (p. 360)?[6] "This book [grounds] its explanation of the evolution of US securities markets in the specific, even peculiar, characteristics of the process of US economic development from the Civil War through the First World War. It claims that it was, above all else, the dramatic and

unstable character of the nation's economic development that explains the dynamics of the US securities markets."

34 **Mary O'Sullivan:** I think this quotation illustrates well the way that history and economics have come together in my recent research as well as the potential of bringing them into close contact. Economists have an instinctive commitment to general explanations of economic phenomena. It is no surprise, therefore, that there is a large comparative literature that generalises across time and space in identifying the causal dynamics that purportedly explain the operation and development of securities markets. Historians often take issue with that approach and challenge the arguments it fosters by looking beyond the economic realm to differences in, say, political institutions that shape how economic processes operate.

35 What my analysis in *Dividends of Development* shows is that the economic processes that shape the operation and evolution of securities markets may also be quite particular to a specific time and place. The book's central claim is that the development of US securities markets was determined, above all else, by the country's dramatic, but volatile, pattern of economic development, which was transmitted to the US securities markets through its impact on the demand for, and supply of, corporate securities. The crucial implication of this finding is that the history of the U.S. securities markets from the Civil War through the First World War is a quite particular, even peculiar, story that we are unlikely to find when we look at the development of securities markets in other places and times.

2. Development of financialisation and varieties of capitalism

36 **RR:** You worked a great deal on corporate governance systems and their economic implications. Along with Bill Lazonick, you were one of the very first to underline the problems of "downsizing and distribute" related to shareholder value.

37 **Mary O'Sullivan:** Yes, Bill and I articulated our "downsize and distribute" critique of shareholder value in an article, "Maximizing shareholder value: a new ideology for corporate governance", published in *Economy and Society* in 2000. It was a succinct and polemical expression of the critique of shareholder value that we had been working on. And I think it echoed with many people because they were looking for some way of grappling with, and confronting, the seemingly inexorable financial pressures on employment and investment in the corporate economy.

38 If you look at the incidence of "shareholder value" in the published material covered by Google Books, you see a really sharp increase between the mid-1990s and the early 2000s, with most of the writing dominated by shareholder value advocates. At the time, there were hardly any critical voices attacking the economic ideology that shareholders are the "principals" for whom corporations should be run. The few exceptions there were – Margaret Blair and Lynn Stout deserve special mention in this regard – were trying to stretch mainstream economic analysis to generate an alternative stakeholder perspective on corporate governance. Such approaches, in analysing corporate governance as a structure of individual incentives, seemed to me to overlook the problem, as a reviewer of Blair's 1996 book put it, of having "a large dog in your home and it chews the furniture, bites the children, and defecates on the living room carpet".[7] If Bill and I had something original to say about corporate governance, I think it was because we acknowledged the

reality of the large dog and took it as our challenge to explain why it was biting the children and defecating on the carpet. In our account, the incentives of the people in charge of the dog mattered a great deal but the character and implications of these incentives were profoundly influenced by the fact that the dog itself was so large and powerful. In more straightforward terms, the incentives that corporate executives faced to buy into, or reject, shareholder value were shaped by the enormous resources and returns at the disposal of the powerful economic organisations they ran.

39 **RR:** Critiques of shareholder value in the corporate sector now feature as one aspect of many analyses of financialisation. However, there is still an ongoing debate in this literature about the causal mechanisms involved. On the one hand, there are those who consider that shareholder value and the restructuring of large firms are the consequences of the pressure of "institutional shareholders" (such as mutual funds, pension funds, banks...) on corporate managers ("finance dominates industry"). On the other hand, there are scholars who contest the claim that corporate managers are subordinated to these institutional shareholders and argue that managers continue to exercise strong control over corporations and have been opportunistic in exploiting financial markets for their own purposes, notably to increase their compensation. In your article for *Socio-Economic Review* in 2007 on the transformation of the French financial system, you seem to belong to this second position. Could you elaborate on this?

40 **Mary O'Sullivan:** Yes, I would characterise myself as an advocate of the second position. Indeed, I articulated that view early on in my research on corporate governance, notably in the analysis of the historical evolution of US corporate governance that I presented in my book, *Contests for Corporate Control*, and in the "Maximising Shareholder Value" article with Bill Lazonick. And, as you note, it is a central claim in my *Socio-Economic Review* article on "Acting out Institutional Change". In all of this research, I argue that senior corporate executives exercise considerable discretion in shaping the destinies of the companies they run and, even in an era of shareholder value, that continues to be the case. Certainly, structural changes in the financial system have created new pressures and opportunities for executives in the corporate sector. But if we want to understand the impact of these changes on the behaviour of corporations, we must analyse managers' changing motivations and decisions since they are the ones who "act out" institutional change.

41 Having made my own position clear, I think the question you pose is a central one in the literature on financialisation and one reason why there is ongoing disagreement about it is that there is so little evidence available to closely "observe" the interactions between industry and finance. One exception to the rule is a book by sociologist, Robert Freeland, which offers a fascinating window into the tensions between corporate managers and large stockowners at General Motors between 1924 and 1970. Based on archival sources, Freeland analyses the evolving relationship between the senior management team at General Motors and the Du Pont family. What his account shows is that senior GM executives made sustained and successful efforts to ensure they had discretion in running the corporation and that outside shareholders, even powerful ones who had seats on the board, simply could not stay abreast of what was happening, no matter how hard they tried. That finding accords with what I have seen elsewhere in the annals of business history, not just for the United States but for other countries too, but rarely with the richness of evidence that we find in Freeland's account. Certainly we need more of this kind of research, or similarly creative research based on different empirical

methodologies, to shed light on the character and evolution of the relationship between financiers and executives.

42 **RR:** You have contributed too to the debate on change in comparative systems of corporate governance. Your main concern in this regard was with characterising the complex political and economic dynamics involved in the changing interactions between the financial and productive systems in different countries. And you suggested there were some limits to the existing debate between convergence and varieties of capitalism for understanding these changes.

43 **Mary O'Sullivan:** It is generous of you to refer to my "contribution" to this debate because I would be hard pressed to put my finger on what it might have been. When I wrote an article on "The Political Economy of Comparative Corporate Governance" in the *Review of International Political Economy* in 2003, I was struggling to define a research agenda on an issue that seemed of central importance to these debates. A striking development in the financial systems of rich economies – albeit far from the only one – was the changing role of securities markets. Yet, there was limited analysis in the literature on comparative systems of corporate governance of the role that securities markets played in different economies and what there was depended on highly stylised characterisations. Advocates of a greater role for securities markets portrayed them as well-oiled machines that encourage savings and facilitate investment, while critics characterised them as vehicles for speculation and financial instability but these portrayals were hardly ever based on systematic empirical studies of securities markets. It seemed to me that historical evidence could be mobilised to shed more light on the operation and dynamics of securities markets so I embarked on the research trajectory that culminated in my recent book. Therefore, my "contribution", if we can even speak of one, was to the development of my own research as much as anything else! Having brought that research to fruition, I think it offers potentially interesting insights for the comparative analysis of capitalism but I think I need a bit more distance from the book before I can articulate clearly what they might be.

44 **RR:** Related to the question of shareholder value and corporate governance, you developed a theory of how corporate governance can enhance innovation. Indeed, the basis of your critique of economic ideologies of shareholder value was that they did not take adequate account of the social conditions required to support sustained innovation. However, American capitalism seems revitalised by the rise of GAFA, Uber and the gig economy, even in a context that is still financialised. How do you account for this development? Is it because these companies operate based on different systems of corporate governance?

45 **Mary O'Sullivan:** These days, I usually try to deflect such questions by saying that if it happened after 1930 I have nothing to say. That is not entirely tongue-in-cheek since I tend to retreat into a kind of intellectual cave when I work intensively on a topic, as I did on the history of US capital markets over the last 10 years. Still, even if I admit I had to look up "GAFA", I have thought a bit about what the emergence of these players implies about the character of US capitalism. Indeed, it is hard for me to resist that temptation since I've worked on earlier manifestations of the so-called Silicon Valley phenomenon in the United States.

46 I dealt with it quite extensively in my earliest research on U.S. corporate governance. And, yes, as the framing of your question suggests, I do think it makes sense to think of these companies as operating according to a system of corporate governance that was

distinct from the one that influenced the corporate allocation of resources and returns in mass-production industries. As I emphasised in *Contests for Corporate Control*, it is impossible to understand the emergence of a post-war electronics complex organised around Silicon Valley without analysing the distinctive roles played by the federal government and a particular constellation of financial institutions in the decades after World War 2.

47 Today, the corporations that are seen as exemplifying the Silicon Valley phenomenon no longer represent an electronic complex and the institutions that shape their governance have changed a great deal too. Certainly, their relationship to the US government has been altered but it is striking too how much the role of financial institutions in their development has changed too. The availability of funding from the federal government after World War 2, along with venture capital that was often allocated based on government contracts as a kind of collateral, meant that startups could invest before they generated profits and even revenues. There were limits to how long they could do that, however, with most startups expected to turn a profit, or be close to doing so, before their shares were offered to the public on the stock market. As a result, more than 80 per cent of the companies that completed initial public offerings in the United States between 1980 and 1989 were profitable. The situation has changed radically since then, with only 40 per cent of companies completing IPOs between 2001 and 2006 being profitable, and a mere 29 per cent between 2011 and 2016.[8] Moreover, even after companies go public, they may remain unprofitable, even as they continue to make massive investments; the example of Uber is only the most extreme example of a more general phenomenon.

48 We have moved, therefore, from a world in which the U.S. federal government injected massive subsidies into the country’s startup economy to one in which Wall Street is now doing much the same and on an unprecedented scale. We might well ask why this change has occurred and wonder whether there is some massive speculative bubble in the making. I have a hunch that there is something else going on, something more sinister perhaps, with some Wall Street financiers systematically pushing for the destruction of existing business models, in the hope of making a massive killing if they can reshape entire industries. That is just a hypothesis, of course, but one that suggests that there is plenty of research to be done to figure out what is going on there.

49 **RR:** How would you assess the account given by post-Schumpeterian analyses of the innovation process within firms and industries, rejuvenated in the 1980's by Nelson & Winter's well-known book “An Evolutionary Theory of Economic Change”? Did it inspire you? In which ways does your approach depart from the one put forward by these approaches?

50 **Mary O’Sullivan:** Yes, certainly it inspired me, especially when I was starting out in economics. I took a serious interest in technological change, initially by reading Marx and then moving on to Schumpeter, so I was excited to come across living people who were developing these traditions. In terms of post-Schumpeterian scholars, I initially came across their research through the national systems of innovation work that Richard Nelson developed in collaboration with Chris Freeman and Bengt-Åke Lundvall. These scholars, and many others working along similar lines, helped me a great deal as I grappled with the conceptual and historical complexity of the economics of innovation. Of course, there were limits to what the national systems of innovation research offered; some critics have pointed to its neglect of the role of the State but for me a significant

flaw was its neglect of finance, a surprising oversight given Schumpeter's overriding concern with the relationship between finance and innovation.

51 I discovered the book by Nelson and Winter as I dug into the economics literature on the theory of the firm. It seemed to me then, as it still does now, that this book made a really important contribution in marking out a challenging alternative to the contractual approaches to the firm that were coming into vogue. Crucially, the concept of routines evoked the possibility of heritability in firms, a phenomenon that had proven elusive for many economists, and it gave a major impetus to theoretical and empirical analysis of innovation. Its direct influence on my own perspective on the microeconomics of innovation was more limited. The evolutionary theory of the firm, and much of the conceptual work inspired by it, abstracted too much from the role of power or control in shaping the character of the innovation process as well as from the organisation – the division and coordination of work – that generated it. In this regard, the ideas of Edith Penrose proved an important antidote for me, both her theory of the growth of the firm and her scepticism about economists' use of biological analogies. Nevertheless, it was not necessary to agree on these issues to have stimulating discussions with evolutionary economists and to learn a great deal from their empirical research.

3. Some reflections on the state of social sciences in general and economics in particular

52 **RR:** What are your main methodological and analytical principles? Which kind of materials do you use in your work?

53 **Mary O'Sullivan:** Debates in economics have served as a crucial source of inspiration for my research over the last twenty-five years. There is a statement – usually attributed to Jacob Viner, the Canadian-born economist – that economics is what economists do. There are many reasons to admire Viner but his definition of economics, ironic as it may have been, is not one of them. It seems to me that what economists do is a matter of indifference unless it tells us something significant about economic patterns and dynamics.

54 As a student of economics, that meant I got into bad company without even trying since I gravitated towards economists who grappled with phenomena that were of clear importance to the way the economy operated. Such bad company included a few clever fellows like Smith and Ricardo and Marx and Veblen and Schumpeter and Keynes, as well as highly intelligent women like Luxemburg and Robinson and Penrose. Perhaps more surprisingly, I've spent plenty of time reading economists like John Bates Clark, notorious among heterodox economists as the founder of neoclassical economics in the US, but well acquainted with the dynamics of US economic development, despite his embrace of a theoretical framework that is notorious for excluding them.

55 A crucial aspect of my methodological approach, therefore, is reading and re-reading debates in economic thought. Indeed, if you have an interest, as I do, in investment and capital and profit, it seems difficult to imagine that you could begin to see the limits to our understanding of these economic phenomena without understanding the convoluted and contentious paths we have taken to get here. Yet, the economic debates of the past are far from my only source of inspiration with recent discussions about capital's role and rewards, stimulated by the publication of Thomas Piketty's *Capital in the Twenty First*

Century, serving as an important stimulus for some of my recent work. In general, I read plenty of contemporary economics, with a fair dose of mainstream economics thrown in, to help me develop my research.

56 Yet, even if past and present debates in economics are a fount of inspiration for my work, most of what I do is grounded in systematic empirical research and, increasingly over the last fifteen years, in historical research. The empirical methodologies I employ vary from one project to another, not least since I shift around from one unit of analysis to another, and I rely on a varying mix of qualitative and quantitative evidence depending on the question I am trying to address. The sources I exploit are varied too, ranging from archives of stock exchanges, banks and industrial enterprises and personal papers of financiers and politicians to data on the prices and volumes of traded securities or on patterns in the financing of corporate investment.

57 I think the biggest methodological challenge I confront is in finding principles to guide the integration of the theoretical and historical aspects of my research since there is a real tension here and I still struggle to manage it. Scholars with strong theoretical interests have difficulties in incorporating historical and comparative detail for fear of undermining the conceptual clarity they seek. Historians, on the other hand, are interested in the historically situated meaning of human activity and are hesitant to incorporate too much theoretical abstraction and simplification. Working at the interstices of economics and history, I confront this tension all of the time and dealing with it is not simply a matter of communicating with audiences with different frames of reference. Really engaging with distinct ways of understanding a specific historical pattern or development means thinking about it in those various ways yourself. How to do that effectively, without getting mired in confusion, strikes me as an art and, although I have gotten better at it through trial and error, I still struggle with the tension between economic and historical analyses.

58 **RR:** History is at the very heart of your approach. Do you think that mainstream economists usually neglect economic history? Geoffrey Hodgson, in his seminal book "How Economics Forgot History"[9], shows that there were strong traditions in institutional economics – like the German historical school and the original American institutionalists – that were deeply historical but were swept off the landscape of economics. Is the neglect of history deep-seated among contemporary heterodox economists too? Finally, what are the consequences of such a disregard on theories?

59 **Mary O'Sullivan:** Yes, I think that book is my favourite one from Hodgson. And I certainly agree with him that, in the development of the discipline of economics, there has been a strong tendency to cast aside historical research in favour of generalising, indeed universalising, statements about economic behaviour, organisation and institutions. That said, I think the book would have benefitted from more sustained attention to the "the problem of historical specificity in social science", which Hodgson takes as the sub-title of his book. He presents a number of different schools of economic thought that acknowledged this problem, and tried to confront it in their economic research, and he offers some criticism of how they went about their task. Still, I would have liked to hear more about where economists have gone wrong in the past in this regard.

60 In dealing with the American institutionalists, for example, Hodgson is tough on Veblen but he is less categorical when it comes to other institutionalists. That is unfortunate since these economists were really influential in their day, as Hodgson notes, so it seems

especially important to understand why their emphasis on historical research did not endure. One of the most prominent of these economists was Wesley Clair Mitchell, whose papers I have been working on recently. Mitchell was a professor of economics at Columbia University, a founding faculty member of the New School for Social Research and, perhaps, most significantly, the director of research at the newly-created National Bureau of Economic Research from 1920 until 1945. He was an ardent exponent of the importance of historical research and his prominent position in the economics discipline should have allowed him to transmit his historical commitments to a new generation of economists. Understanding why that did not happen does turn to some extent on developments beyond Mitchell's influence – some of which Hodgson highlights – but it reflected too the limits of Mitchell's own efforts to incorporate historical specificity into economic theory.

61 Mitchell was primarily interested in expounding a theory of business cycles. What is striking as you follow the evolution of his work is the extent to which the increasingly complex historical analysis of cycles that he built up seemed to overtake his capacity to make theoretical claims, even historically-situated claims, about them. That made Mitchell, and the economists he inspired, vulnerable to the charge levelled by Tjalling Koopmans that they were engaged in "Measurement without Theory". The criticism was exaggerated – Mitchell was deeply interested in theoretical analysis – but what he had to say about it was increasingly submerged by the weight of all of the historical data he brought to bear on it.

62 To my mind, Hodgson does not devote enough attention to the challenge involved in making economics and history speak meaningfully to each other or, relatedly, to the fact that so few economists have been interested in trying. Marx is a crucial exception, of course, but even if there are some Marxists who have followed his lead in this regard, there are not that many of them. Schumpeter is an exception too even if the overwhelming complexity of his two volumes of *Business Cycles* makes it difficult to argue they should serve as models for future efforts. There are other exceptions too – and Hodgson's book is a rich source on many of them – but it is true that economists who make theory and history interact in creative tension with each other are thin on the ground.

63 So when we think about why history is not more prominent in theoretical analysis in economics – and I do believe, as your question suggests, that this is as true of heterodox schools as it is of mainstream economics – it may be worth acknowledging just how difficult it is to change that. As I noted above, I struggle with the problem of integrating theory and history in my work and I often deal with it by pulling away from theoretical questions and steering closer to historical analysis, or *vice versa*, which is not quite the same as resolving the tension. Therefore, when we hear calls to bring more history into economics, as we have heard recently, they immediately beg the question of how to go about that task.

64 The field of economic history, understood in a narrow sense as a subfield of economics, brings history in as a type of applied economics. From this perspective, historical evidence is interesting insofar as it offers more variation in the empirical data at economists' disposal but of limited value for shaping theoretical debates in economics. Now and then some "big" debate breaks out, which briefly pushes economic historians into the limelight, holding out the promise of some greater impact on the economics discipline. But those moments fade fast and the long-term patterns are pretty clear,

pointing to a limited role for history in the leading economics journals no matter how optimistically one interprets the evidence.[10]

65 Would heterodox economics look any more historical if you did a similar analysis? I would not bet on it although I would be interested to see a careful study. I should say that I do not think it is important to use historical analysis for addressing every economic question we are interested in. But there are specific questions that tend to be of interest to heterodox economists – some of them being questions that really distinguish them from their mainstream counterparts – where historical research could help to strengthen theoretical discussion.

66 A good example is the issue you raised earlier about the relationship between finance and industry, which is as controversial a feature of a much older literature on *Finanzkapital* as it is of the contemporary discussion of financialisation. One important difference, however, is that there is a great deal of historical material available for the early 20th century that could allow us to get a better grip on the character and dynamics of finance capital in ways we could never hope to achieve for the present. Yet, at least to my knowledge, most heterodox economists who invoke the concept of finance capital display little interest in using historical research for this purpose.

67 **RR:** Many mainstream economists state that economics is more and more interdisciplinary and pluralist. Do you think it is really the case? Is this internal diversity enough for a lively debate on economic topics and to promote a true dialogue with other social sciences, instead of economic imperialism?

68 **Mary O'Sullivan:** Yes, I have heard and read such statements. In a recent one in *Le Monde* [11], French economist, Jean Tirole, seems to have been so inspired by his Swedish banking prize that he has declared the end of *homo economicus* and called for "une plus grande transversalité avec les sciences sociales". What one makes of these statements depends on what you think mainstream economists mean by "transdisciplinarity" or "pluridisciplinarity" or "interdisciplinarity". The looseness in the use of such terms – that some of them crop up in the same article though they would seem to suggest quite different relationships across or among the social sciences – already suggests that such statements do not always stem from profound reflection about the way that economics needs to reorient itself.

69 These statements seem to be stimulated, at least in part, by an interest in undermining any effort by heterodox economists to step into the breach created by recent criticisms levelled at mainstream economics. It is clear too that the whole behavioural turn in economics has captured the imagination of mainstream economists like Tirole who seem to see the future of economics in incorporating empathy and errors into economic models. There is plenty of that work going on already and there is surely more to come but I do not see this kind of research as having the kind of potential that truly interdisciplinary research could promise. What economists pushing these developments seem to have in mind is stretching their models of human behaviour to incorporate new elements into preference maps or individual choice behaviour. If we want to know where such effort might lead, I think that post-Tirolean industrial organisation is a good place to look and it is not a pretty picture.

70 What we find is a kind of voluntarism in which a sufficiently-determined economist can build a model evoking any kind of industrial behaviour she wants. It seems impossible to aggregate from such models and subjecting them to any kind of empirical analysis is

wracked with all kinds of problems.[12] Along the way, we generate a kind of modelling promiscuity in which all kinds of weird and wacky models are possible but, for me, that is not the same as pluralism. There are still some basic rules, although they often remain unspoken, that mean that no matter how many models mainstream economists build, they never seem to suggest new answers to the "big" questions. And while modellers in industrial organisation were fiddling around with a kaleidoscope of strategic interactions among competitors, they ceded the high ground to Chicago school economists who took over anti-trust policy by pushing pretty old-fashioned price theory to new limits.

71 Mainstream economists often respond to attacks by claiming that the simplified understanding of the world that appears in economics textbooks does not capture the sophisticated ways in which mainstream economists work out their ideas in the leading academic journals. That statement is both true and entirely disingenuous. If "sophistication" means modelling complexity and tortuous empirical strategies for identifying causation, then the top journals are full of it. But when it comes to saying anything of general import about antitrust policy or income distribution, mainstream economists have to fall back on the hackneyed ways of "explaining" that are prominent in their textbooks since it is so hard to draw any broad conclusions from their more sophisticated work. Moreover, when someone uses interdisciplinary research to challenge these explanations, mainstream economists line up against him. We see a clear illustration in the mainstream reaction to Thomas Piketty's research: when Piketty suggested, based on his extensive historical research, that the elasticity of substitution between labour and capital might not be exactly equal to one, he induced a kind of Cobb-Douglas-induced apoplexy from his fellow economists.

72 To conclude on a more positive note, I think that if mainstream economists are sufficiently mollified, and even humbled, by the crisis to be serious about doing interdisciplinary research, there are steps they could take to make their efforts meaningful. First, they should work only with specialists from other disciplines who they acknowledge to be smarter than themselves. Second, they should design research to challenge and, if necessary, change what is taught in intermediate microeconomics and macroeconomics textbooks. And, finally, they should try to publish their results in journals where they need to earn a scientific imprimatur from specialists outside of economics. The cynic in me suggests that if mainstream economists were to follow these steps, their interdisciplinary research might shrink to a minuscule output but that may well be an optimum.

73 **RR:** In the aftermath of the last financial crisis, a wide-ranging debate has grown on the relevance of economics and on the marginalization of heterodox economics both in research and teaching. In France, the French Association of Political Economy, founded in 2009[13], raised the issue of the institutional domination of the mainstream on political economists, of the ingrained partiality of the ranking of journals and the ensuing extinction of various heterodox perspectives. Did you follow these debates? What do you think about it?

74 **Mary O'Sullivan:** I followed them closely. Indeed, the efforts of the AFEP were inspiring for us at the University of Geneva since we were engaged in a similar battle but on a much more modest scale. When I joined the university, the economic history department had just been merged into a larger department of economic sciences. A year or so later, the mainstream economists in the department announced that they wanted to merge with

the university's management department to create a new faculty of economics and management, separate from the faculty of social sciences.

75 Having struggled for years to get out of business schools, the irony was nearly too much for me! More importantly, the stakes of such a move for economic history seemed very high. We fought an extended and bitter battle to keep economic history in the faculty of social sciences but what really drew fire was when we asked the university's rectorat for positions for economists, open to the social sciences, in our faculty. We won that battle to create a new department of History, Economics and Society and we shall see where it will take us but it has borne fruit already in our gaining Bruno Amable as a colleague and launching a new Master's programme on the Political Economy of Capitalism.

76 **RR:** During your career, you experienced a broad variety of disciplinary as well as institutional and organizational contexts, from business economics to economic history, from Ireland to the US, from France to Switzerland, from universities to business schools. How would you characterize the distinguishing features of these contexts? What lessons, if any, could be drawn from your experience to define a favourable context for interdisciplinarity?

77 **Mary O'Sullivan:** When I came out of graduate school, I had a job interview at one of the leading radical economics department in the United States (that narrows down the possibilities!). I thought it was where I wanted to be until I met the economists who were deliberating on my candidacy. They seemed much more preoccupied with what my advisor was doing at Harvard than with what I might do in their department and more concerned with the closeness of my allegiance to him than with what I was working on. The fact that they were all men – every single professor of economics who I met that day – only made the experience seem more hostile. Still, it made it easier for me to accept that I would not be hired by a heterodox economics department, as I had hoped, but by the business schools that seemed much keener to have me.

78 When I accepted a position at INSEAD in Fontainebleau, some of the senior faculty there encouraged me to start a new course on critical perspectives on corporate governance. I taught what I did research on, and I did not pull any punches with the students, so we had extremely lively and stimulating debates about shareholder value. But not everyone was happy with this development, as I discovered halfway through the course, when I received the gift of a book entitled *Karl Marx: Racist* by Nathaniel Weyl from the "finance department". The gift was clearly intended to intimidate me but a member of the visiting faculty – a wonderful fellow called Henry Mintzberg – happened to be in my office when it arrived and he hit the roof. He grabbed the book, marched to the office of the dean – himself a finance professor -- and demanded that he make his colleagues issue a formal apology to me. They did so but only grudgingly and not before suggesting that I was partly to blame for inciting dangerous talk in my classroom.

79 What I learned about business schools in the years that followed confirmed these initial impressions. Certainly, these institutions offered an unexpected intellectual freedom to do the research I wanted to do, a freedom that I could not have found in a mainstream economics department and perhaps not even in a heterodox one. For all of that, there were limits to what could be done at business schools without running into obstacles. Sometimes the opposition was more juvenile than intimidating but I saw uglier sides to it too, especially as I advanced further in my career and the stakes became higher. And, finally, and perhaps most draining for me in the long term, was the fact that you could never forget you were at a business school. I felt no particular enmity towards my

students – after all, I had once sat where they were sitting – but their interests and horizons seemed farther and farther from mine and it eventually seemed like a burden to get up in the morning if I had to teach them.

80 Finding a more congenial intellectual home for myself was not that easy. Eventually, it was my becoming more and more of a historian that helped me make the move but it seemed to take an eternity to get there. That move initially brought me to a department that allowed me to cultivate and develop my historical interests. More recently, as we have extended our group to include economists as well as historians, we are broadening our horizons a bit more. It is hard to say if it will work out the way we hope but there is no question that it does seem to be worth the considerable effort involved in trying.

81 **RR: What does it mean to be a woman in fields that are often dominated by men? Do you see differences among national contexts and disciplines? How is it evolving?**

82 **Mary O'Sullivan:** Well, to answer your first question, it means being a woman in fields that are often dominated by men! Having grown up in a country that was a patriarchy and religious orthodoxy in the 1970s and 1980s, I had more preparation for what I found in economics than many women. In Ireland I survived being on the wrong side of these hierarchies by becoming combative and irreverent. And I have honed those traits still further in navigating a career in economics. Sometimes I think that there may be an advantage in being female and heterodox in economics since you are never quite sure where the anger or disdain you generate is directed. But I can tell you that I have confronted some pretty extraordinary displays of hostility and scorn that seemed just a tad disproportionate in light of the alleged, albeit unspoken, offence that I gave.

83 Is something evolving? Maybe. It is true that I have spent more than 25 years in male-dominated fields and, until this year, no one had ever asked me the question you posed. And now I have been asked that question or something similar on five or six occasions. Unfortunately, that does not mean that I have a good answer to give you. The difficulty here stems not from any lack of possible answers I might give but rather in figuring out which of them might help other people to think about the issues you are getting at. What I know, of course, is largely what I have seen happen to me and to women around me. There is much in that experience that I could rail against by drawing on the considerable inventory of war stories that most women in male-dominated fields have at their disposal. Stories of this kind can be helpful, not just in offering a kind of catharsis to those who tell them but also in illustrating the character and depth of the problems that many women encounter. Yet, the specific details that often come together in vivid illustrations of these problems make it difficult to draw larger conclusions. To put it bluntly, I am a combative woman, with a taste for provocation, so when I encounter a furious backlash from a male economist, it is hard to know if it reflects a widespread misogyny in economics or simply the fact that I can be so annoying. What I would say, however, is that I have observed certain attitudes and behaviour towards women in the economics profession that are hard to explain away in terms of the specific characteristics and behaviour of the women involved.

84 One issue is the sexualised way in which women economists are treated and discussed. The recent scandal, unleashed by Alice Wu's paper on the portrayal of female economists on the US job market, has increased awareness of gender stereotyping in economics.[14] However, the casting of female economists as objects of sexual interest is not simply one of men – or, to be more precise, certain men – paying undue attention to women's various body parts at the expense of their intellectual capacities. It extends much farther and

deeper than that, with professional judgements about women economists being based not only on their appeal as sexual objects but also on the way they indulge their own desire for sexual gratification.

85 Perhaps the best way to capture what I mean is to refer to an anecdote that Geoff Harcourt recounted about Joan Robinson in an interview. He described a boozy dinner he had attended and then went on to claim "that this dinner, too much to eat and drink, was the basic reason why, for the only time in my life, I slept with Joan Robinson".[15] I remember nearly gagging on my coffee when I read that quote, thinking to myself that surely Harcourt would not stoop so low. But then he continued, describing how he and Robinson were sitting on a couch, listening to Kenneth Arrow talking about uncertainty and the welfare economics of medical care, until they fell asleep together as Arrow presented his paper. I do not know how much Harcourt thought about telling that story but in its delivery and content it strikes me as an ironic and evocative statement of a real truth. There is a curiosity about what women economists do in their sexual lives, often with a judgement about what it means for their intellectual capacities, that would be deemed an embarrassing prurience were it expressed about the sex lives of Samuelson or Solow. To see that, you only need to look at a recent biography of Joan Robinson where you will learn a great deal more about her love affair with Richard Kahn and a great deal less about her economic ideas than you might want to know.[16]

86 A second issue I would highlight is a type of social pressure that is exerted on woman in economics in order to be taken "seriously". I have received such advice repeatedly over my career, often from well-meaning colleagues and even from other women, and I have heard similar advice being dispensed to other female economists. The specific forms of such advice vary, depending on the person to whom it is addressed. I have been told to be less aggressive/angry/hostile/caustic/sarcastic/sardonic/irreverent/insolent, presumably to be more pleasing to my (mostly male) audiences, and I have heard other women being told to be more assertive/combative/forthright/straightforward, presumably to less pleasing to their (mostly male) audiences. We are clearly talking here about some kind of ideal balance that is extremely hard for women to get right but more striking than what women are being told is the fact that they are being told at all. I have seen male colleagues make presentations that break every single rule of effective communication but never have I seen one of them being taken aside by a senior male colleague for advice on "style". The good news is that I have learned to be more self-conscious about the impact I have on my audience and more prepared for the reactions I tend to provoke. Still, being an observant woman and more than a little self-critical of my own quirks and foibles, I could have done with less pressure to change what I did and a bit more reinforcement of what made me good at it.

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