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Qualitative research: does it fit in economics?[1]

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Abstract

This article discusses the role that case studies, built upon open-ended interviews with economic actors, can play in economic research. It argues that such material cannot be treated directly as empirical evidence. Rather it provides a way of building theory, by offering a critical perspective on the standard theoretical assumptions of the discipline and offering alternatives with which to construct theoretical models. It illustrates several alternative ways in which this can be done, drawing on examples from my own research.
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Introduction

The focus in this essay is upon qualitative research methodology as I have used it in my own research as an economist. That methodology has centered around unstructured open-ended interviews with economic actors. The research itself is motivated by a particular policy problem and focused on a particular domain of activity. I began looking at the internal labor markets of large manufacturing firms in order to understand the impact of technology on employment and entered the debate in the 1960s about structural unemployment (Piore, 1968). I turned quickly to the contrast between these jobs and low-wage work in what we came to view as the secondary sector of a dual labor market in order to understand the problems of black workers and the failures of employment and training policy to successfully address them (Piore, 1969; Doeringer and Piore, 1971). I am currently working on the shift in labor market 'regulation' from collective bargaining driven by economic identity to legal regulation driven by political mobilization around social identities such as race, sex and ethnicity (Piore and Safford, 2005), and on the organization of product design and development as a window into new forms of business organization (Lester and Piore, 2004). In between, I have worked on a whole range of other projects, ranging from migration to adjustment to trade.

Despite the variety of subjects, however, the research approach is fairly consistent. It is often described as a case study approach, and in a way it is. However, case studies as practiced in the social sciences tend to be viewed as offering empirical results. I have used my 'case study' findings, however, not as empirical evidence but as inputs into the construction of theory. In principle, I could be building tight mathematical (or symbolic) models conventional in economics – and increasingly in other social sciences – out of the material drawn from the case studies. And several of my students over the years have in fact done so. The results could be tested empirically, but not by replicating the case studies to achieve a larger *n* (a point to which I shall return below). However, I chose to develop that theory in a narrative form instead, reinforcing the 'qualitative' flavor of the research. I conceive this last characteristic as a question of style (a style which I will not try to justify here) rather than of substance.

Thomas Kuhn argues that science has to be understood first as social practice and only afterwards as an intellectual endeavor. A scientific discipline is a social community, and people enter it through a process of initiation and imitation (Kuhn, 1970). What we learn in the university is not scientific theory, and certainly not a theory of how to do science. We are exposed to practices: the practices of our teachers in the classroom and laboratory, and the practices they admire, which we read about in the articles they assign us. The theory of how science should be done is almost never taught. And even the theory that explains the practices and articles to which we are exposed and which gives the discipline some coherence is constructed after the fact. It is not always taught directly, is always incomplete, and is often internally contradictory. Kuhn's view of science is, of course, very much contested (Sardar, 2000). However, it certainly captures how I came to do what I am doing.

I did not choose a methodology of open-ended interviews deliberately or self-consciously. I stumbled into it in my dissertation research in a way that I have described elsewhere (Piore, 1979). It was in part a considered reaction to the limitations and failings I discovered when I tried to apply the more conventional research approach. The courage to react in this way came, as Kuhn suggests, from the example of my thesis advisor, John Dunlop. The work that I found most interesting and original grew out of his practical experience as a labor arbitrator and mediator and from the contrast between the world he encountered through those experiences and the world of economic theory (Dunlop, 1957; Livernash, 1957; Dunlop, 1958). I continued doing it because it was interesting, fun and seemed to yield insights into problems that I considered important to solve, socially and morally. Miraculously, what I was doing attracted enough interest and attention that I got tenure anyway, despite my research approach. It has been only recently, when I reached an age where people could believe – mistakenly – that the cannons of the profession were very different when I was a young researcher that I have felt a need to justify what I was doing back then.

Interpreting open-ended interviews

The use of open-ended interviews as a research technique depends on the ability to draw out of the interview some material that is interesting and meaningful. It depends, in other words, on the ability to 'read' the interviews, or to use a term which is perhaps more apt (but in this context, as I will shortly suggest, also more ambiguous), to *interpret* the interviews. For me, interpreting interviews has always been at least as much a matter of intuition and instinct as it has been of systematic methodology; one has the feeling of flying by the seat of one's pants. That feeling makes the research process exciting (and scary) relative to the standard theoretical or econometric approaches. Nonetheless, it requires an appreciation for the 'open-ended' interview as a research instrument. For me, this emerged only gradually through practice over time.

Initially, I saw the open-ended interview as preliminary to the interview proper. It was the idle conversation you engaged in with the respondent – the social amenities – before you got down to the 'real' business of posing specific questions. To my surprise, I found these interviews substantively more interesting than the answers I got in response to the questionnaire and occasionally – more than occasionally, I must confess – indulged myself and the respondent by prolonging the interview despite my sense that it was not part of the real research process. But I also discovered rather quickly that many of the same respondents who were easy to engage in the preliminaries did not tolerate the formal questions well, that is, I couldn't seem to get them to answer my questions directly or in the right order. When I followed the formal interview format too closely, they clammed up or provided answers that seemed designed to get me out of the office as quickly as possible. Truth and honesty became very secondary considerations. What worked in interviews was letting the respondents tell their stories. Indeed, I came to believe that this was the only thing that worked consistently. It seemed as though people agreed to be interviewed in the first place only because they had a story to tell, and the formal questions I asked basically became an excuse to let them tell that story. When I tried to forestall the story, I lost the interview.

One can often reconfigure the interview material into a questionnaire format after the fact. However, in principle, the questionnaire should be designed before the interview. One of the advantages of open-ended interviews is that the respondents often answer questions you would not have thought to ask. An elementary textbook in sample survey analysis will tell you that data generated in this way is subject to all sorts of biases because questionnaire results are often very sensitive to the precise wording of a question and the order in which questions are asked. This is not an insurmountable problem in survey research, where the biases are consistent. They are unlikely to be consistent in reconfigured open-ended interviews. However, it is not clear that responses to a formal questionnaire that are driven by the respondent's wish to be rid of the whole thing would be any less biased.

What open-ended interviews do yield, and yield consistently, are stories the respondents tell. The story is the 'observation'. The stories are basically narratives. The question is thus what to do with the stories. Typically, stories are not analyzed as statistical data; stories are 'interpreted'. I have used the stories not as data points but as arguments for particular revisions in theory.

Epiphany, intuition and objectivity

The problem plaguing open-ended interviews as inputs into the reconstruction of theory is that they appear to be so personal and idiosyncratic. They depend on the capacity of the individual researcher to generate surprises, to recognize patterns, and to organize those patterns to form a theory. It is difficult and potentially counterproductive to delegate the task of interviewing to a colleague or a research assistant because one never quite knows in advance what will turn out to be important. It is even difficult to delegate the task of transcribing the interviews because what turns out to be important is not necessarily the direct response to a question but rather the background detail or the apparently random aside that the question provoked in the respondent. The interpretation can depend on a detail far removed from the goals or substance of the interview itself, which the researcher is not even aware of at the moment it presents itself. It emerges through a chain of factoids, which come together, often in an epiphany at some odd moment when the material lies dormant in the back of one's mind.

An example of such a chain are the clues to the origins of the Italian industrial districts in Central Italy that Chuck Sabel and I visited and which ultimately led us to develop the argument of *The Second Industrial Divide* (Piore and Sabel, 1984). Our trip to Italy was motivated by a completely different research project: Undocumented immigration to the United States and our inability to find an underground labor market which the extra-legal status of the immigrants had led us to expect. We had been looking in New York and thought perhaps we were missing it there because we did not know what to look for. Italy was notorious for its underground market so we went there to find out what such a market would look like once it emerged. We expected to find a set of markers that would signal its development in advance, if it had not had time to develop. We were surprised by what we found. The first surprise was that many of the supposedly underground, retrograde firms were, in fact, open, above-board, and technologically dynamic, and that even the underground firms (of which there were many) seemed to be moving in this direction. This would, however, never have led to a theory of the end of mass production had Sabel and I not already been engaged in a debate with each other about the division of labor, a debate which we conceived of as completely separate and independent from the immigrant project that had brought us to Italy in the first place.

An important factor in the emergence of these dynamic, small firms was a complex intergenerational effect. The founding generations of these firms were skilled craftsmen with extensive practical knowledge but no formal education. They had acquired their skills in large companies and had been laid off in one of the several waves of Italian labor militancy that, as the aristocracy of the working class, they tended to lead; they had founded their own companies with the large severance payments that their employers were obliged by law to pay. This generation of older workers had transferred their practical knowledge to their children, who worked with them in the family business after school and during vacations. However, the children – unlike their parents – also had a formal education, which provided technical knowledge and exposure to the wider world and its markets. The children had planned to take that education and move with it into large firms and government bureaucracies in what they (and we) thought of as the modern sector. However, the economic and social rigidities of Italy in the 1970s, the rigidities that their parents' militancy had created, manifested itself in very high youth unemployment. These upwardly mobile, educated children were unable to find work when they left school and were forced back into their parents' firms. It was these kids who created what we called 'flexible specialization,' combining advanced technology to which they gained access through their formal education with their practical knowledge in traditional industries to cater to niches for specialized products in world markets. The clue to all this were the old men, who took us on tours of the family factory when their children were too busy managing the enterprise to do this themselves. When you visit a manufacturing plant – whether it is a family shop making high-fashion wedding dresses or a 2000-worker factory assembling jet engines – there is always a factory tour (the factory tour is part of the ritual of this kind of research). You would never think to write down in your notes who gave you the tour or where the tour guide stood in the management hierarchy or what role he or she had played in the history of the enterprise. You invariably have to make conversation with your guide, but you do not think of the conversation as an 'interview'. A formal interview with our factory tour guide in Italy would not have captured the pride of the father in his son's accomplishments, because these were carried by the tone of his voice and the look in his eyes as much as by the substance of what he was saying. And yet that pride, remembered months later in an idle moment, was the clue to the role of intergenerational transition in the emergence of the Italian industrial districts.

What can one do to stimulate epiphanies of this kind? Does it all depend on luck and personal intuition? One sure way of broadening the interpretative process is to work in teams. It is difficult to delegate the interviews. However, they can be shared by having a colleague or a research assistant present during your interview, hearing the same things you hear, 'seeing' the same gestures, the hesitations and fumbling which cannot be captured in the transcript or on tape. It is no accident that *The Second Industrial Divide* was a collaborative endeavor. The team works to best advantage when its members discuss what they have seen and can bring different perspectives to the situation because they come from different backgrounds.

This approach is actually captured in our case studies of product design (Lester and Piore, 2004). It is one of the ways in which designers work. Each year, for example, Levi-Strauss sends a team of its designers, accompanied by people from the textile houses that provide its materials, and the laundries to which it subcontracts its finishing operations, to Europe to 'look'. They spend their days walking the streets, watching what people wear, shopping in stores, and listening to people talk to each other about the clothes on the rack. Then they come back to the hotel at night and sit around comparing notes, arguing with each other about what they have seen and what it implies about the possible directions in which fashion might evolve and how Levi's might lead it.

Interpretation through theory and theory through interpretation

Existing theory can play a role similar to that of the design team. It sits in the back of your mind as you ruminate about the interview material. When the theory is strong and demanding, it is as if a team of your colleagues were there beside you arguing about what the interviews mean. It is like being engaged in a continual debate with the rest of the profession about what you are finding and what it means.

The use of theory to stimulate the interpretation of interviews should be possible in any social science discipline, but it seems to me, the hostility of my colleagues notwithstanding, that it is both easier and more important to do in economics. It is easier and more important because it plays off two characteristics of economics as a discipline. First, economics is highly structured. Second, the discipline has a strong normative disposition. Economics is structured in the sense that it operates from a very tight body of theory and an equally tight, and theoretically grounded, set of empirical techniques. Economics is normative in the sense that it seeks to evaluate economic arrangements and prescribe improvements. The high theory is structured around the notion of pareto optimality, which defines normative criteria in a very precise way. Applied economic research is directed at the solution of a set of specific, and in the end, well-specified social problems. The theory itself is built around the idea of rational individuals pursuing their self-interest in a competitive market, where they interact indirectly with each other through price signals. The theory seeks to produce as its outcome a stable equilibrium; normative judgments are derived by comparing alternative equilibria.

The vulnerability of economics is that it is addressing problems in the world. When the solutions it proposes do not seem effective, the theoretical apparatus is challenged. However, that apparatus is so tightly woven that it is very difficult to respond to that challenge in a systematic way.

One could question any one of the assumptions upon which the basic model is built, but there is no guide as to what alternative assumptions to put in its place. In addition, when one actually tries to think through the relationship between the necessarily simplified and abstract theory and the 'real world' in which the problems that theory addresses arise, there are so many assumptions to reconsider that even if one knew how to select alternatives, it is hard to know which ones to reconsider.

In this sense, what my 'case study' methodology has amounted to is using the material from open-ended interviews to identify the assumptions of conventional theory that seemed to be wrong and the alternative assumptions to replace them. The research 'worked' because it was problem-oriented; the problems were real and important, people were looking for solutions to them, and the prescriptions derived from conventional theory were not working. I have to say that it 'worked' for a second reason as well: Because it drew upon the actors themselves and their actual motivation and behavior (or what they reported their motivation and behavior to be), the actors recognized themselves in the theories I was constructing and thus 'certified' my 'results'. Whether a theory needs to be built around 'realistic' assumptions, whether the actors should be able to recognize themselves in a theory, are much debated methodological issues (Lester, 1946; Machlup, 1946; Friedman, 1953). I have no special wisdom to offer on this score. Personally, I have always felt more comfortable with theories of this kind, and certainly more comfortable with this kind of theory than with theories that actors themselves reject as a characterization of their behavior. But this is probably because I tend to judge theory (especially theories that we use to make policy) as a story or narrative; people who have an aesthetic which gives primary weight to logical coherence and consistency – a criteria which, incidentally, I also think is important – tend not to care about the storyline in this sense. However, whatever its methodological validity, the fact that the actors certify the theory gives it enormous legitimacy in the face of an overtly hostile profession. My work on low-income labor markets has benefited especially from this. The dual labor market hypothesis suggested that workers and employers in the secondary sector behaved differently from those in the primary sector. Although this hypothesis violated the strong presumption in economics that there is a unified theory of behavior, workers and employers recognized themselves in the distinction. My work on migration, in which the conventional 'assumption' regarding economic man was limited to first-generation migrants, was intuitively plausible to government officials working in migrant communities and to employers who hired these migrants, as well as to the migrants themselves.

More to the point, the problem of how one goes about revising theory is central to research within the discipline of economics, whatever one thinks of my own particular solution to it. The most systematic approach to this problem in the discipline at the moment is the newly emergent field of experimental economics, which derives both the conventional assumptions it questions and the alternatives it puts in their place from controlled (and one might argue contrived) psychological experiments. The broader field of behavioral economics seems to be defined by a general willingness to consider alternative behavioral assumptions. Another approach has been to focus on a particular set of assumptions and to introduce apparently *ad hoc* alternatives (*ad hoc* in the sense that they have no empirical content) in their place. An example of this second approach is the focus on the assumption of perfect information by the group of economists awarded the Noble Prize in 2002 (Akerlof, 2002; Stiglitz, 2002). Joe Stiglitz traces his preoccupation with information to experiences in Kenya in his early career that are a somewhat less systematic version of my own case studies. However, one suspects that his preoccupation is also due to the analytical tractability of this problem in the profession as a whole. That tractability derives, I believe, from the fact that econometrics, the empirical branch of economics, is essentially a theory of rational inference from incomplete information. Neither behavioral economics nor, Stiglitz aside, the approach focusing on a particular set of theoretical assumptions are motivated by policy concerns (although of course they have implications for policy). The innovations in economic theory that grew out of the great depressions – particularly Keynesian economics – are counter-examples; it was the policy problem that created both the motivation and the space within the discipline for an alternative theory to emerge. However, the particular assumptions on which the new theories focused, and the alternative assumptions around which they were built, are not so obvious. Still another approach – the one which is generally offered in textbook science – is the conflict between theory and empirical results. However, the empirical branch of economics does not lend itself to this role. I take it as an empirical fact that it does not; why it does not is a much more profound question. It has always seemed to me that the reason it does not stems from an interaction of two factors. On the one hand is the strength of our attachment to economic theory. On the other is the empirical theory, which is extremely complex and sophisticated relative to the techniques that are actually used in practice to analyze data. As a result, the empirical analyses always seem inadequate. When theory and empirics conflict, it has proven easier to question the empirics than to question the theory.

The use of case studies for the construction of theory need not be limited to economics. My own research has been less a reaction to theory in the strict sense, and more a reaction to the surprise I experienced when listening to what the actors were telling me. And when I tried to identify the source of the expectations that led to that surprise, I found it to be the story about the world which economic theory seems to tell. Hence, I ended up trying to trace down systematically the 'surprise' that violated my expectation, the part of the story that created the expectations and the way in which those expectations were embedded in the more formal and parsimonious version of economic theory. Any discipline creates a series of expectations; ultimately those expectations derive from theory. Hence the 'methodology' of looking for the surprise in the interviews, tracing its source in theory, and then trying to identify how the theory might be amended to incorporate the surprise is as applicable to social science in general as it is to economics.

However, while the methodology is general, it does raise particular problems in economics. Among the social sciences, the discipline of economics is unique in conveying the sense of a system of *interactive* elements. Outcomes are not generally the result of the actions of any single individual but instead reflect the interactions *among* individuals. Thus, abstracting the behavior of individual actors from interviews is only the first step in 'modeling' the process at issue.

Minimal and maximal approaches

The revision of theory is an especially acute problem in economics, but it is an issue in any scientific discipline. Using interview material to revise theory poses the same problem raised by using empirical data, that is, whether to challenge the theory by parsing out the material among a set of theoretical categories or using the narrative directly as the 'observation'. And it leads to a distinction between what I will call a minimalist approach and a more radical approach to this kind of research. To make this point, I need to briefly discuss the structure of conventional economic theory.

The theory has two components: a theory of individual behavior and a theory of how, given their behavior, individuals interact and cohere to form a larger economic system. I could illustrate my point using either of these components, but will focus on the theory of individual behavior. Behavior, in that theory, is understood as a series of discrete acts. Each act is self-conscious and deliberate, the outcome of a specific decision. The decision is *instrumental*; the decision-maker is presumed to make a sharp distinction between means, ends, and a causal model connecting the former to the latter. Decisions are *rational* in the sense that the decision-maker organizes the means so as to maximize the ends, given his or her understanding of the underlying causal relationships.

The minimalist approach to the use of open-ended interviews would take each step in the decision-making process as a potential point of entry into the revision of the theory. It tries to parse out the material collected in narrative form over the standard set of theoretical categories. Thus, one might infer from these interviews the means available to the actors, the ends, and/or the causal models used to solve the problems. In this sense, it is the theoretical analogue of the approach that parses out the answers over a formal questionnaire and uses them to generate data for empirical research.

A different approach is to take narrative itself as the observation. This is – at least in my understanding – what statistical theory would suggest. What might that mean? It could mean that the narrative itself becomes a functional part of the working of the system. For example, I have recently been studying identity groups based on race, sex, ethnicity, and so on, within the engineering profession. Many of these groups meet regularly to 'network' but also to hear a speaker, generally a member of the identity community, talk about his or her career. The speaker's talk is invariably presented in narrative form. These narratives, one can argue, create models or pathways through the labor market for members of the group in an economy where careers are no longer based on well-defined professions or the lines of progression in bureaucratic organizations. Thus, they come directly to influence behavior in the economy. Treating the narrative as an observation in this way is clearly different from breaking the narrative into a series of components, which are then abstracted from the narrative context itself. It actually contravenes a component of the aesthetic of economic theory, which I have not talked about – the notion that the variation among individuals is smooth and continuous and not lumpy and discontinuous. However, one is still interpreting the narrative material in terms of the basic categories of instrumental decision-making.

A second approach to treating the narrative as the unit of observation is to analyze it in terms of its characteristics as a narrative. There is a literary tradition about interpreting narratives, with an enormous theoretical literature which seems potentially helpful here (Lieblich *et al*., 1998). I cannot claim to have mastered this literature. Indeed, it is so vast that I have not even tried. I have, however, read around in it. And although I still have the hope that the key article is just over the horizon, I have not found this literature very helpful. The problem is that it focuses on a set of abstract characteristics like the structure of the plot or the use of time, which do not map in any obvious way to the structure of economic theory.

The focus on the narrative itself as the unit of observation leads to a still more radical departure from the conventional framework (and, incidentally, one in which the literary tradition of narrative analysis could come to play a role): The narrative may be taken as a marker of a pattern of cognition and behavior totally different from that hypothesized in economics and rational choice behavioral models more broadly. Here, the key assumption of the economic view of behavior is not that it is instrumental or rational, but that it consists of a series of discrete acts, each of which is deliberate and hence motivated. An alternative is to think of behavior as ongoing in time, moving in a particular direction or toward a particular object, but deflected (or redirected) by situations the actor encounters along the way. Because narrative links together action in time and highlights the kind of encounters that redirect action, it reflects the way in which the actor thinks about behavior of this kind. Their understanding of others is an 'interpretation' of such narratives, and their own behavior is conceived in terms of a similar narrative in which they imagine themselves to be acting. In the hands of the German philosopher Martin Heidegger and the hermeneutic theory which develops this idea of behavior, the key is not just the narrative but the 'meanings' that are ascribed to it (Dreyfus, 1991). That meaning is in turn developed through interaction between people in a process that resembles conversation and in the way in which language evolves through conversation. This complicates the open-ended interview because it suggests that the observation in the interview is not actually the narrative itself, but the *interpretation* of the narrative. Moreover, because the act of interpretation is conversation-like, the interviewer becomes implicated in the process as an interlocutor with the respondent in the interpretative process.

Because this is so far from the conventional model, it is hard to see exactly what its implications are for economic analysis. At the Industrial Performance Center at MIT, we have been addressing the problems of industrial design and product development in a series of case studies in terms of this view of behavior. We are trying to understand economic processes through a dual perspective that uses both the conventional approach of behavior as rational decision-making and the alternative, hermeneutic approach; and we are using the material of open-ended interviews as yet another window into economic activity.

From my early work on internal labor markets, for example, I gained the insight that workers saw their wage rate as an end in itself and not as a means either to efficient resource allocation in the enterprise or to higher levels of consumption (as is presumed in conventional theory). However, I also discovered that workers understood causal processes in production very differently from the way an engineer or a manager understood those processes, even though everybody in the shop used the same vocabulary. Another example is the use of the equivalent of open-ended interviews with corporate management to argue that the firm maximizes growth rather than profits (Marris, 1968; Galbraith, 1972) or that managers are not rational (but only boundedly rational).

Pursuing this approach to interpreting interviews, one can make a number of additional points. I will make three here. First, in most narratives the actors' behavior can be explained by a combination of several analytical models. Respondents also include in the narratives events that they do not understand analytically; they use the mere proximity of events in space and time as a substitute for an analytical model (Bruner, 1990). In thinking through the interview material, the goal should be to separate out these different elements, which is not easy. Second, one is ultimately looking for analytical models because that is what we, as social scientists, use to think about social problems. Thus, the narratives contain several different kinds of information. First, they offer us analytical models of the behavior of actors themselves (Piore, 1995). Although Friedman (1953) would dispute it, I believe – as already noted – that these models are in and of themselves important and hence that a plausible theory should be able to account for them. These models are of tremendous forensic value in the policy-making process, since the actors are attracted by arguments in which they recognize themselves (which is not to deny the forensic importance of models which present actors not as they are, but as they would *like* to be). Third, the actors' own models of their behavior are clues to the way the larger social system behaves. That behavior cannot, of course, be inferred directly. However, since actors operate within that larger social system, one can ask what social system would be consistent with the actors' own models of their behavior. What would the social system have to look like for it to allow actors to hold and believe in the models they carry around in their heads?

To my mind, it is on this last point that social science, but particularly economics, has been most deficient. The deficiency lies in the failure to give sufficient importance to the distinction between information and the framework in which the information is processed and understood. There is not even a standard vocabulary for making this distinction, although sometimes it seems to be carried by the distinction between information and knowledge. In econometrics, it is the distinction between data (observations) and a structural model. The key question is: What alternative models are used to analyze the same data and from where do they come? The supposition is that at the very least the models that the actors use are consistent with their experiences.

A final point: In interpreting interviews, I do not think sufficient attention is ever given to the possibility that the world is really chaotic; it doesn't fit anybody's models, not those of the social scientist and not those available to the actors. Sometimes the actors themselves recognize this, as when they link together events that do not actually have a causal relationship, using proximity in time and space as a kind of pseudo-causality. The great movement toward decentralization of power in large enterprises in the 1980s is a case in point. We tended to see this as a deliberate effort to adapt to a newly unstable and uncertain environment in which local knowledge had achieved much greater importance than it had had in the past. I still believe that the movement was largely defined by this. Nonetheless, it is hard to distinguish what one might call principled decentralization from a kind of *de facto* decentralization that occurs when the center loses confidence in its understanding of the situation and simply leaves the decisions to be made by default at lower levels of the hierarchy.

Conclusion

To end on this note is – to state the obvious – to have moved a very long way, both methodologically and theoretically, from the core of economics as a discipline. If a discipline is defined, as Kuhn would suggest, by practice, the methodology described above moves beyond the boundary of economics itself into the realm of anthropology and the territory of hermeneutics, which has claims as a discipline in and of itself. It seems important, therefore, to recall that we arrived at this point only after considering a number of ways in which qualitative research and open-ended interviews can be absorbed within the core of the discipline. These imply treating the interview material as something different from empirical data points, for, given the cannons of the empirical methodology of economics, they will never qualify as legitimated facts. Rather, by treating interview material as inputs into the revision of theory, they become essentially equivalent to the products of the laboratory experiments conducted within the emergent subfield of behavior economics.

In a very different sense, however, to arrive at the borders of the discipline, and cross into the domain of other social sciences, is actually quite consistent with the current evolution of economics, and thus with the paradigm as it is currently evolving. This is true in two senses. First, economics has become an imperial science, pursuing its approach to human behavior as a general theory applicable to understanding all human endeavor (Becker, 1976). Such a research program seems to demand that one look, as well, for the limits of that approach and the possible relevance of the approaches of other disciplines for problems, which have been the principal concern of economics itself. Second, economics has in a way already responded to this implication with an increasing interest in social psychology. One could take these developments as a license to move in the direction of anthropology and hermeneutics as well. However, in the process of this expansion, economics is bound to adapt to these new terrains, as new facts often stimulate new methods and theories.

Notes

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References

Akerlof, George A., 2002, "Behavioral macroeconomics and macroeconomic behavior". *The American Economic Review*, **92**: 411–433.

Becker, Gary S., 1976, *The Economic Approach to Human Behavior*. Chicago: University of Chicago Press.

Bruner, Jerome Seymour, 1990, *Acts of Meaning*. Cambridge, MA: Harvard University Press.

Doeringer, Peter and Michael J. Piore, 1971, *Internal Labor Markets and Manpower Analysis*. Lexington, MA: Lexington Books.

Dreyfus, Hubert L., 1991, *Being-in-the-World: A Commentary on Heidegger's Being and Time, Division I*. Cambridge, MA: MIT Press.

Dunlop, John T., 1957, "Wage contours". In G.W. Taylor and F.C. Pierson (eds). *New Concepts of Wage Determination*. New York: McGraw-Hill, pp: 127–139.

Dunlop, John T., 1958, *Industrial Relations Systems*. New York: Holt.

Friedman, Milton, 1953, *Essays in Positive Economics*. Chicago: University of Chicago Press.

Galbraith, John Kenneth, 1972, *The New Industrial State*. New York: New American Library.

Kuhn, Thomas, 1970, *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Lester, Richard K., 1946, "Shortcomings of marginal analysis for wage employment problems". *American Economic Review*, **36**: 63–82.

Lester, Richard K. and Michael Piore, 2004, *Innovation: The Missing Dimension*. Cambridge, MA: Harvard University Press.

Lieblich, Amia, Rivka Tuval-Mashiach and Tamar Zilber, 1998, *Narrative Research: Reading, Analysis, and Interpretation*. Thousand Oaks, CA: Sage Publications.

Livernash, Robert E., 1957, "Job clusters". In G.W. Taylor and F.C. Pierson (eds) *New Concepts of Wage Determination*. New York: McGraw-Hill, pp: 140–160.

Machlup, Fritz, 1946, "Marginal analysis and empirical research". *American Economic Review*, **36**: 519–554.

Marris, Robin L., 1968, *The Economic Theory of 'Managerial' Capitalism*. New York: Basic Books.

Piore, Michael J., 1968, "The impact of the labor market on the design and selection of productive techniques within the manufacturing plant". *Quarterly Journal of Economics*, **82**: 602–620.

Piore, Michael J., 1969, "On the job training in a dual labor market". In Arnold Weber, Frank Cassell and Woodrow Ginsburg (eds). *Public-Private Manpower Policies*. Madison, WI: Industrial Relations Research Association, pp: 101–132.

Piore, Michael J., 1979, "Qualitative research techniques in economics". *Administrative Science Quarterly*, **24**: 560–569.

Piore, Michael J., 1995, *Beyond Individualism*. Cambridge: Harvard University Press.

Piore, Michael and Charles Sabel, 1984, *The Second Industrial Divide: Possibilities For Prosperity*. New York: Basic Books.

Piore, Michael and Sean Safford, 2005, *Changing Regimes of Work Place Governance, Shifting Axes of Social Mobilization, and the Challenge to Industrial Relations Theory*. Mimeo: MIT.

Sardar, Ziauddin, 2000, *Thomas Kuhn and The Science Wars*. Duxford, Cambridge, UK, New York: Icon Books, Totem Books.

Stiglitz, Joseph, 2002, "Information and the change in the paradigm in economics". *American Economics Review*, **93**: 460–501.