

**Stability and Flexibility in the Economy:  
Reason and Interpretation in Economic Behavior**

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This paper represents one man's attempt to situate the Conventionalist school within the larger enterprise of contemporary economics as a scientific discipline. It is secondarily an effort to understand its relationship to my own somewhat idiosyncratic research program which I see as closely linked to the Conventionalist endeavor. It is written against the backdrop of the background paper prepared for this Colloquium, but the argument was largely conceived and developed before I saw that paper. It is divided into four sections as follows: The first attempts to place the Conventionalist endeavor in the framework of the broader research agenda in the discipline of economics. It argues that the Conventionalist school is distinguished by its effort to address the concerns of economics by drawing upon sociology and anthropology. The second section identifies particular problems with this approach. The third discusses various efforts to address these problems and compares them to the approach that has grown out of recent work I have been conducting with colleagues on product innovation in the Industrial Performance Center at MIT. The last section concludes.

## **Part I**

I see the Conventionalist endeavor, in a way which has been greatly influenced by Mary Douglas, as a part of the broader attempt of the economics discipline to understand the construction and maintenance of economic order. An economy, as we all recognize, consists of a series of interdependent parts. The central economic problem is that of how to get these parts to work together in reasonable harmony. This is the problem of coordination. The Colloquium's background paper uses the term coordination to define the domain of the Conventionalist school. My problem with using the term in this way is that coordination basically defines economics itself. I would use the term order instead because the concerns which motivate research in economics today are both more specific and much broader than coordination. They derive from the presumption of neoclassical economic theory that coordination can be produced spontaneously by individuals interacting with each other in a market. A good deal of modern economic research, however, has gone on to undermine this presumption; particularly influential in this regard was Arrow's impossibility theorem in the early postwar period and Mancur

Olsen's work on collective action; the same lesson has been taught more recently, but more forcefully, by the extensive development of game theory (see, North, 1990, p. 15-16). A spontaneous order, it now appears, should, if it can be created at all, be extremely unstable. And yet in point of fact, the economy does not degenerate into chaos and anarchy, at least not with anything like the frequency that the theoretical difficulty of maintaining order would lead us to expect. A major research project in the discipline (one might say the major research project) has become the search for how such an order might be achieved. The term "institutions" has come to be employed in economics as a shorthand for those structures which permit order to emerge and be sustained. But to say this is to displace the problem from order itself to the question of where the institutions which are critical to it come from. This has moved economics into the realms of politics, sociology, and cognitive psychology. And in each of these realms it has tried to recast the problem in terms of simulated markets.

There are several subsidiary points about which there is not much disagreement in the discipline. One is that order is not a foregone conclusion: There are enough examples where the economy degenerates into a type of chaos or anarchy—in labor unrest, for example, in business panics, or in certain periods of crisis or depression—to say that order is not a trivial problem. But, still, given the theoretical difficulties of constituting such order that have been uncovered by contemporary research, the fact that seems most important to explain is the robustness of order and not its fragility. Second, once one makes order the central research question, stability is not the only characteristic which one would like to be able to explain. However stable the order that is achieved, that order appears to be flexible, and sufficiently open and dynamic to permit economic growth to occur regularly. The maintenance of order is also consistent with substantial changes over time in the "texture" of the economy, as evinced in the products which populate our daily lives and our expectations about such basic characteristics of the society as social mobility, income, and employment. Such changes could happen through periods of chaos, anarchy and rebuilding, but it does not seem to always happen in that way or necessarily happen in that way. And even when periods of anarchy do occur, there is considerable continuity in the "texture" of the economy. Hence, the characteristics of the institutions which create the economic order are actually threefold: stability, flexibility,

and continuity. That then is the problem the discipline is trying to resolve: *The constitution of an economic order through institutions, an order which exhibits the properties of stability, flexibility and continuity.*

### Three Research Approaches to This Problem

There are three distinct research approaches to this problem: the *standard* (I borrow this term from the background paper), the *psychological* (or behavioral), and the *sociological*, a fourth if one includes Marxism (where, however, the analytical apparatus leads one to expect both the stability and the chaos which these other approaches are trying to explain). The standard approach to the problem continues to focus on the interaction of rational, autonomous individuals. It tries to characterize the conditions under which these individuals might coalesce into a stable order or create a set of institutions that would bring about and sustain such an order, adding to the standard model various complexities in terms of information, transaction costs, uncertainty and risk, etc. This is the dominant program in the discipline. Some people who pursue it seem to believe that it will eventually show that a spontaneous order is indeed possible (i.e., that you do not actually need to invoke the black box of institutions after all). Others believe that by exploring the limits (and the strengths) of this approach, we will narrow down the characteristics of the “institutions” which are required, that, while they may not exactly open the black box, they can significantly narrow the scope of what is in it.

This has to be the dominant research program in economics because it operates within the canons of the discipline; it is, in other words, what economists are trained to do, and what they are good at. And because we are all trying to chip away at the same black box (to mix a metaphor), we have to be interested in that program and the results which spring from it, even if we do not actually participate in it (and do not believe that in the end it will prove to be a very direct route to understanding).

The other two programs try to fill the black box more directly. The psychological approach starts with the idea that the critical elements of human behavior, i.e., critical to understanding the emergence of economic order, are wired into the mind of the actors biologically. In other words, that rationality is ultimately a biological characteristic of

human beings, and ultimately to be explained by the evolution of the human species and the process of natural selection (hence an affinity to socio-biology as in, for example, Becker, 1976, chapter 10). But the wiring that has evolved may be a little different – even very different – from that which is assumed in rational choice theory. And one should look at ways to alter those assumptions and explore the impact that various alternatives have upon behavior. For this purpose, the critical question is where the alternative assumptions come from. A lot of these assumptions of late have come from experimental economics (Camerer and Thaler, 2003; Fehr and Gächter, 2000). The attraction of this approach is that it preserves the research program of looking at the interaction of autonomous individuals. Hence, it tends to look very much like the standard approach and to utilize the skills and techniques which that discipline teaches and has perfected.

The third approach is sociological or anthropological. It starts from the idea that human beings live naturally and invariably in communities and that these communities create larger structures of meaning and understanding. It is in terms of such structures that we, as individuals, understand the world in which we live, and our place within that world; community is the context in which we understand ourselves as individuals and the meaning of our lives. The structures of these communities provide the order and stability of economic life, just as they do for social life more broadly.

The Conventionalist school is, I believe, distinguished by its commitment to this third approach. It is the only school of economics that I know of which occupies this territory. There are indeed very few individual economists not directly committed to an identifiable school of thought who share this commitment -- I count myself among them (although as I read the background paper I began to see myself more and more as a Conventionalist). The most prominent independent economist who takes this approach is probably George Akerloff, but I am not sure his own commitment is as self-conscious as that of the people gathered here at this Colloquium (and he tends to draw upon particular sociological “effects” without seeing them as part of a broader structure of related ideas or insights into human behavior). Economic sociology, it could be argued, has a commitment of this kind, but because it is a branch of sociology and not economics, its practitioners are not motivated by the preoccupation with the problem of economic order

and they are much less attentive to the other attempts to address that problem, through behavioral economics or through the deeper exploration of standard economic theory. But while in this sense distinct from economic sociology, this third approach borrows from sociology in the way in which the psychological approach borrows from psychology and social psychology. In so doing, it is confronted with the basic limits of sociology as a source of theories of the economy: Sociology yields an understanding of social structures that is too rigid. It provides an explanation of order, of why the economy does not degenerate into chaos and anarchy, but at the expense of the three other characteristics of the economic order which we would like to capture: its flexibility, its capacity for dynamic growth, and its continuity. I read the bulk of the background paper as a response to that challenge.

## **Part II**

### Boltanski and Thevenot

The basic character of that response is to multiply the systems of moral judgment and evaluation in a society's repertoire, what the background paper calls a plurality of systems. These were present in one of the very early works of the Conventionalist school, Boltanski and Thevenot's Les économies de la grandeur. But an important characteristic of that work was that the actual number of such systems (or *cites*, to use their terminology) was quite limited. Thus it seemed to suggest that the economy had a capacity for adjustment by shifting among these alternative systems without introducing a kind of complex plasticity along an infinite set of dimensions. I found the systems of moral judgment that Boltanski and Thevenot identified plausible in the light of my own research in different work settings; and in fact they provided a way of organizing a variety of observations that were otherwise confusing and contradictory. But the postulate (or assertion) of these multiple systems had two problems. First, Boltanski and Thevenot did not say where these different systems came from. They themselves abstracted them from a reading of classic texts. But they did not explain how those texts were chosen or whether the systems, which they identified, constituted a closed set. The second problem with this approach was that Boltanski and Thevenot did not explain how the economy (or society) might move from one such system, or set of systems, to another.

And indeed, because Boltanski in other contexts focused on the way in which these systems of moral judgments clashed in, for example, the denunciations in the letters to the editor published in newspapers, the way in which the plurality of systems operated in practice seemed to reinforce the very rigidity from which the sociological approach needed to escape.

It should be said – parenthetically at least – that the rigidity created by the inability to move from one system of moral judgment to another did capture a critical characteristic of the period in which Boltanski and Thevenot were writing, namely the labor market flexibility debate and the attachment to the workforce of a set of employment protections which had developed in the era of mass production and seemed to clash with the variation and uncertainty of demand in an increasingly global economy and with the requirements of the productive systems of the new economy. The way in which employees and employers talked about the rules governing employment security, moreover, was eminently consistent with the language of denunciation upon which Boltanski was focused. The Conventionalist school did not get credit for explaining this rigidity, however, partly because its members never focused on this problem, but more basically because the rigidity could also be explained quite easily by an insider/outsider model that was consistent with standard economic theory, without resort to sociology at all.

In my own research, I have recently become increasingly interested in the relationship between personal identity and economic behavior. This relationship seems critical in understanding career trajectories in the new economy. It is also a key factor in understanding the shift which seems to have occurred, at least in the United States, in the locus of political mobilization, a shift from political mobilization around classic economic identities associated with economic class, craft, profession and industry to social identities associated with sex, race, ethnicity, physical handicap, sexual orientation, age, and the like. And the link between economic organization and systems of personal evaluation which lie at the heart of Boltanski and Thevenot's typology seems like a very promising way of understanding what is happening.

But to return to the main argument, the basic problems with Boltanski and Thevenot lie in their failure to explain where the basic *cite* comes from in the first place and how the economy, or a sub-sector within it, might move from one to the other.

### Boltanski and Chiapello

Part of this problem has been addressed, at least indirectly, by Luc Boltanski with Eva Chiapello in Le Nouvel Esprit du Capitalisme, a book that focuses on the emergence of a new system of evaluation, a new *cite*. Their book does not explicitly talk about the problem of where the new system comes from and how it emerged – from an analytical perspective I find that disappointing and unfortunate – but it is about the emergence of a new system. And from the material it presents, one can at least infer an idea of where the new system comes from. Boltanski and Chiapello also do not address the problem of how the economy moves from one system to another. It appears from the evidence internal to the book that the new system emerges in new industries, i.e., in sites which socially at least are green fields. Since there is no suggestion anywhere that the economy has only one such system of evaluation, this is a seemingly satisfactory resolution to the logical problem of how space emerges in the economy as a new alternative. But it leaves unaddressed the problem posed by the debate about labor market flexibility which is – to put it crudely – the need, or at least the pressure within the economy, to move from one system to another in older industries and the difficulty of doing so.

## **Part III**

### Beyond Boltanski et al.

The background paper for the conference, and, it would appear, the bulk of the current Conventionalist research program, focuses not on these meta-systems of moral judgment and evaluation, but on lower level, local processes through which rules are created and evolve over time, and hence on precisely this problem of change and adjustment. Here the term plurality refers to a much larger number of alternatives: The term plurality is easily understood as an infinite variety of alternatives. The big question then becomes how does one think about and understand these lower level, local



processes. And what is the relationship between the conventions which emerge at this level and the meta-systems of Boltanski et al.

My own views on these questions have developed in the context of the research at the Industrial Performance Center at MIT conducted in collaboration with Professor Richard Lester. For empirical content, the research draws heavily upon a series of case studies on product design and development. More recently, the IPC has been engaged in a series of case studies of local innovation systems, with special interest in the role of the university in these geographically-based industrial districts. At one time, I thought this was leading me in a very different direction from the Conventionalist school, although Olivier Favereau always assured me that I misunderstood what they were arguing. And reading the background paper, I became convinced that he is right. Our differences are in large part semantic. Semantic differences can have substantive implications, and in this case, they may be of some importance in the broader context of the various attempts to address the problem of economic order in which I have been trying here to place the Conventionalist research program. My quarrel revolves around the terms bounded rationality and now interpretative rationality, which seem to me to confound two modes of behavior that I have come to view as sharply distinct.

This view reflects the sharp distinction that our research at the IPC has led us to make between two modes of behavior which we call analysis and interpretation. Analysis is essentially a mode of problem solving: The design that emerges through analysis is the result of deliberate, self-conscious decision-making. The nature of analysis is captured by models of rational choice. Interpretation, on the other hand, is better understood as a process ongoing in time. It is like a conversation, and the manager operates more like a hostess at a cocktail party than as an analytical decision-maker, promoting a series of conversations among the guests. These conversations, i.e., the interpretive process, generate a framework for action that is like a language. And the process through which that framework emerges is like the process through which a language emerges and evolves over time. The existence of this language-like framework is a precondition for analysis. It is as if the material out of which the analytical problem is formulated is drawn out of the interpretative process, and the interpretive process

continues in the background, even as business is conducted in terms of a series of analytical decisions.

I would emphasize two aspects of this characterization, especially in the light of the problem of economic order, which I have been arguing lies at the heart of the broader research problem of economics of which the Conventionalist school is an important part. Those aspects are the sharp distinction between analysis and interpretation, and the emphasis on language in our understanding of the second of these modes of behavior.

We have used language, and the way in which it arises and evolves over time, as a metaphor or analogy for the interpretative mode of behavior. Language has certain features that makes one think it is a particularly appropriate approach. First, language is preeminently social. It is something that arises in a social setting, and hence is a product of a kind of interaction among people which is fundamentally different from that envisaged by standard economics. It thus points toward a way of thinking about the world that distinguishes the social research program from both the behavior research program and the research program associated with traditional rational choice. Second, language has the basic properties of the system we have been evoking institutions to explain, i.e., stability, flexibility, and continuity. Third, the properties of language have been extensively studied and are the subject of a theoretical and empirical literature, in psychology, cognitive science, linguistics, brain science and the like. This should make it possible to ground a social approach to institutions in a biological theory that can be compared to the theories arising out of behavior economics and socio-biology. It suggests that the relationship between institutions (or conventions) and language may be more than metaphorical; that institutions may actually be like language, or, more probably, that we can precisely distinguish institutions (or conventions) from language.

The language analogy and the contrast to rational choice suggests that what we have here are two radically different modes of behavior, that the one cannot be reduced to another, and that were one to pursue the behavioral research program, one would find they draw upon very different spheres of the human brain. It is basically for that reason that I reject the term interpretative rationality. And it leads me to believe that the phenomena which I once, like many of my Conventionalist colleagues, thought of as a

reflection of bounded rationality are actually of two very different kinds, one of which might reflect the limits of rationality but the other reflects not rationality but language.

However one characterizes them, it is clear that in practice analysis and interpretation require the manager to play very different roles. In analysis, the manager is sometimes a decision-maker, essentially an engineer or technocrat, working out the optimal solution to the problems he or she encounters, sometimes a negotiator trying to mediate and resolve conflicts between the goals of the different factions within the enterprise. In the interpretative process, however, the manager operates, as we have suggested, like a hostess at a cocktail party, inviting the guests, introducing them to each other, initiating conversations among them and insuring that the conversations continue to engage those involved. But the difference goes beyond the roles managers play. In analysis, behavior is the product of a series of discrete choices, each of which is the subject of a deliberate, self-conscious decision. The parameters of those decisions are abstracted from time and place. Analysis produces acts, or actions (the peculiarity of behavior as a series of discrete acts is a characteristic of standard theory which is seldom noted). In interpretation, behavior is a process ongoing in time. Analysis strives for clarity; interpretation is about ambiguity. To the extent that one can think of analysis in terms of language, the applicable analogy is to strip down incomplete languages where messages are always clear and unambiguous; linguists call such languages pidgins. (The dictionary definition of a pidgin is in fact a traders language; and this suggests that the way in which they solve the problem of conventions and standards is very different from that of a real, or complete, language.)

The distinction between analysis and interpretation is very clear in the product development cases we studied. The product that eventually emerged was a concrete object. Its use may be problematic, but in its concreteness there is no ambiguity about the characteristics of the product itself. Those characteristics, moreover, are, in product development, the subject of analysis, and the characteristics that actually emerge are the outgrowth of a series of deliberate decisions that can be cast as rational choices, without doing fundamental violence to the intent, or even to the actual practice, of the actors. But the alternatives that are considered in those decisions – the dimensions of the product that the actors consider, the different designs which they choose among, the parameters of the

decision-making process – all of these are abstracted from an ongoing process of interpretation which precedes analytical decision-making and continues in the background even as a particular design is being developed and a concrete product ultimately produced. In new product development, moreover, the ambiguity around which interpretation centers is absolutely critical; where there is no ambiguity, there is no profit to be made. Standard products are commodities.

Because interpretation is a process, it has no product and is not observable in the way that analysis is observable. What we “observe” is only a point in time. But one can capture the process by lining up the successive generations of the same product in a sequence, as several of the cellular telephone companies that we visited did in their headquarters’ lobbies. The interpretative process then emerges in relation to a series of analytically designed products in the same way that a motion picture emerges out of a film composed of a series of still pictures. The difference between analysis and interpretation is essentially a matter of perception. But our argument is not that the difference is artificial, as this insight might imply, only that the two cannot be simultaneously perceived. Interpretation and analysis are like the wave and particle theories of light. The analogy is, we believe, a strong one in the sense that waves are like interpretation, continuous, whereas the discrete acts into which analysis divides behavior are like particles.

I have not tried to work out this relationship in detail for the moral and evaluative systems which are associated with the *cites* in Boltanski and Thevenot, but I imagine that the relationship between formal rules and standards associated with the *cite* and the lower level, more local processes on which the background paper focuses are very similar to that of product development. This would imply that the nature of “conventions” at the two levels are very different: at the lower level—interpretation; at the higher level—analysis. I think this is consistent with the role of denunciation, which one finds primarily in letters to the editor – but seldom in daily life. Similarly, the production systems that are the focus of Salais and Storper (and also The Second Industrial Divide) have a similar relationship to production practice. I see this relationship as underlying the emergence of the new *cite* which Boltanski and Chiapello identify (although I am admittedly not a disinterested reader).

One can also work out the relationship between the two processes in relationship to identity (still another area in which my own work parallels that of the Conventionalists). Personal identity is a complex phenomenon which arises and evolves continually through interactions in a social environment, but the identities around which political and social mobilization take place are abstracted from this ongoing process and become the fulcrum for analytically motivated action.

#### **Part IV: Conclusions**

It seems appropriate by way of conclusion to return to the problem of coordination, upon which the Conventionalists, along with the discipline as a whole, are focused. As just suggested, I see that the distinction between analysis and interpretation could be said to be already present in the two types of pluralities associated with the Conventionalist school: The formal rules and the abstract moral principles of the *cites* seem to be applied analytically; the local conventions upon which the background paper focus are interpretative, they are like a language and emerge and evolve through conversational interaction. In a sense, these are also two different modes, not only of behavior but also of social coordination. But the flexibility of the economy suggests that there must be a third mode of coordination, among the components of the economy which are governed by different *cites*. Boltanski's study of denunciation, with the potential for moral outrage in economic activity, suggests that this kind of coordination might not be easy to achieve. It is among such rigid and potentially antagonistic unities that one might hope that the arms-length, relatively autonomous relationships of a market operate. How is one to understand then the relationship between the market (economic coordination, *stricto sensu*) and social coordination? This difference between the two might be understood in terms of language, in this case the difference between a pidgin and a true language, with a real grammar.

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