Deleuzian Interrogations: A Conversation with Manuel DeLanda and John Protevi

by Manuel DeLanda, John Protevi and Torkild Thanem

Manuel DeLanda, a philosopher and Adjunct Associate Professor in the Graduate School of Architecture, Planning and Preservation at Columbia University, is a famous speaker and leading interrogator into the philosophical thought of Deleuze and Deleuze and Guattari. His work covers a wide range of topics in the natural and social sciences, including issues of war, artificial intelligence and the internet, the evolution of life and self-organization, social ontology and economic organization (see e.g. DeLanda, 1991, 1997, 1998, 2001, 2002b). DeLanda's latest book, *Intensive Science and Virtual Philosophy* (published by Continuum Books, 2002), critically examines Deleuze's ontology of the virtual in relation to ideas and examples from the natural sciences (e.g. dynamical systems theory, group theory and evolutionary biology). He is currently writing a book entitled *Nonlinear Dynamics and Social Complexity*.

John Protevi, a philosopher and Associate Professor of French Studies at Louisiana State University, is a prominent commentator on the philosophical works of Deleuze and Deleuze and Guattari. His most recent book is *Political Physics: Deleuze, Derrida and the Body Politic* (published by Athlone Press, 2001), but Protevi has also done important work on Heidegger, Aristotle and Kant (see e.g. Protevi, 1994, 1998, 2000, 2001a). Most recently, Protevi has directed his attention to geography and social science in the book *Deleuze and Geophilosophy* (co-authored with the geographer Mark Bonta, published by Edinburgh University Press,

(2004).

This conversation explores some of the connections between Deleuzian philosophy, organization theory and work by DeLanda and Protevi, and it springs out of questions initially posed by Torkild Thanem to DeLanda and Protevi as well as questions posed by Protevi to DeLanda. Working through these connections is not completely free of tension. Deleuzian philosophy is a fairly recent arrivant in organization theory. Moreover, both DeLanda and Protevi are outsiders to organization theory, and they both – in their own distinctive ways – critically rethink and reconstruct Deleuzian philosophy.

REALISM

Thanem: Deleuze's writings (and especially his work with Guattari) remain marginalized within philosophy proper. What kinds of questions does Deleuzian thinking enable philosophers to address?

DeLanda: Deleuze's main contribution to philosophy, it seems to me, is to have rescued realism (as an ontological stance) from the oblivion in which it has been for a century or more. In some philosophical circles to say that the world exists independently of our minds is tantamount to a capital crime. Non-realist philosophers (from positivists to phenomenologists) have created a straw man to kick around: the

naive realist, who thinks we have unmediated access to the external world and who holds a correspondence theory of truth. So the key move here was to create a viable alternative form of realism to deprive non-realists of that easy way out. Similarly, when it comes to defend the autonomy of non-human entities (atoms, molecules, cells, species) the crucial manoeuvre is to account for their mind-independent identity without bringing essences into the picture. To take the most obvious example, the real identity of a hydrogen atom is usually treated by realists (like Bhaskar, for example) as founded in the possession of an essence, having one proton in its nucleus, given that if we add another proton it loses its identity and becomes helium. Deleuze's process ontology, however, cannot afford to do that. The identity of any real entity must be accounted for by a process, the process that produced that entity, in this case, the "manufacturing" processes within stars where hydrogen and other atoms are produced. When it comes to social science the idea is the same: families, institutional organizations, cities, nation states are all real entities that are the product of specific historical processes and whatever degree of identity they have it must be accounted for via the processes which created them and those that maintain them.

Protevi: The question of realism is indeed an important one for philosophers to debate. I wonder if Manuel would like to say something about how he sees the relation between realism and materialism, since Deleuze and Guattari tend to use the latter term to describe their work rather than realism?

DeLanda: Well, I cannot imagine a materialist philosophy which is not also realist. On the other hand, someone who believes that god and the devil exist independently of our minds is also a realist but clearly not a materialist. The only problem with the term "materialism" is that not only matter but also energy and physical information are needed to account for self-organizing phenomena and the processes which fabricate physical entities. Also, some forms of materialism may imply reductionism (of the mind to matter, for example) and that is not at all implied by the term "realism".

Protevi: Good. I'd certainly agree that fitting materialism into the contrast of realism and idealism is important. I'd also say that while materialism is often contrasted with idealism, you could also say that the foil for Deleuze and Guattari's materialism is dualism, specifically a spiritualist dualism. So their materialism is a monism (another way of putting this is to say they demand immanence rather than transcendence). Spiritualist dualisms have, because of an impoverished concept of matter as chaotic or passive, too hastily had recourse to a "hylomorphic" schema in which an organized transcendent agent is responsible for all production. The problem is how to account for the ordered and creative nature of bodies and assemblages, for if matter is chaotic, it can't account for order, but if it's passive, it can't account for creativity. Deleuze and Guattari's materialism avoids the forced choice of matter's chaos or spirit's transcendent ordering by calling attention to the self-ordering potentials of matter itself, as outlined in the researches of complexity theory (as Manuel point out above, you have to expand the sense of "matter" to include the energy and information of "material systems"). Deleuze and Guattari can thus account for order and creativity in the world without the heavy ontological price of a dualism or the unacceptable phenomenal price of the denial of creativity as illusory, as in "God's eye view" spiritualist transcendent determinism.

SOCIAL SCIENCE

Thanem: During the past decade Deleuze's philosophy has become increasingly noticed by non-philosophers across the humanities and social sciences, a trend exemplified by this special issue of *Tamara*. As Deleuze commentators who may be seen to inhabit the margins of philosophy, how would you like to comment on the spread of Deleuzian philosophy outside philosophy?

Protevi: I'm all for it! And I'm sure Deleuze and Guattari would be pleased too, given their insistence on the "toolbox" character of their work together. Just on a personal level, working on Deleuze in a French Studies department has freed me up in many ways, and I suspect my experience is not uncommon in this regard. First of all, I'm free of the moribund but still powerful "analytic vs. continental" philosophy split at a couple of levels. In the micropolitics of North American philosophy departments someone working on Deleuze is seen as a "continental" philosopher and so is lumped together with phenomenologists and post-phenomenologists (Heideggerians, Levinasians, Derrideans, etc.) and expected to vote with them on hiring and tenure decisions, curriculum construction, examination questions, and all the daily politics that go on in academic departments. Being free of all that, and hence free to pursue the Deleuze and science connection, I find myself actually having more in common with the "analytic" philosophers in the Philosophy Department of my school. (There is a deeply entrenched suspicion of science on the part of many phenomenologists and post-phenomenologists, which is verbally expressed along the lines of the Heideggerian mantra "science doesn't think", but which I suspect is also tied in with the trauma of the McCarthy era purges in American philosophy departments, as detailed in John McCumber's *Time in the Ditch*.) As Manuel's *Intensive Science and Virtual Philosophy*, which is largely addressed to analytic philosophers, shows, the time has come for us to redraw the map of philosophy along realist vs. anti-realist lines instead of "continental vs. analytic". Christopher Norris has been arguing for this for some time now in fact.

DeLanda: Although I have never done any serious study of the propagation of Deleuzian thought, in my own experience his ideas have had no influence whatsoever outside literary criticism and cultural studies departments. Since these two fields are dominated by non-realists (social constructivists, idealists, post-modern semioticians and so on) it follows that they probably have no real understanding of Deleuze. For many years the only book of his these people read was *Anti-Oedipus*, which happens to be his worst book. At any rate, even those who have read other works deal with him the same way they deal with other French philosophers: they pick up the jargon (yesterday it was "deconstruction" while today they go for "deterritorialization") but there is no real engagement with his thought. It is an embarrassing situation for any real Deleuzian.

Protevi: Yes, we'll have to wait and see whether Deleuze is just another in the line of fancy French imports to sweep through the humanities and social sciences. Has his eclipsing of Derrida been because he enables us to ask more interesting and important questions (to use Deleuze and Guattari jargon: to expand our affects and so form more interesting and important "assemblages" or interdisciplinary teams)? Or is it just a function of a new generation needing to show it's more hip, has more outrageous jargon, than the old fuddy-duddies of the previous generation? We

can't forget that Deleuze's major single-authored works, *The Logic of Sense* and *Difference and Repetition*, have only been translated into English since 1990 and 1994 respectively. So there's something of the "hot new thing" aura still clinging to them. It will be interesting to see, then, what the staying power of the Deleuze wave will be.

Thanem: As your own work indicates, there is also movement from (reconstructions of) Deleuzian philosophy to social science. Given your different engagements with social science (particularly Manuel's work on a flat ontology for the social sciences (DeLanda, 2002b) and John's work on geography (Protevi & Bonta, 2004)), what kinds of questions may Deleuze's thinking enable social scientists to ask?

DeLanda: Here as elsewhere I cannot comment on Deleuze himself – only my own reconstruction of his work. Deleuze (and even more so, Guattari) remained a Marxist till the end, while my work is a deliberate attempt to liberate the left from the straightjacket in which Marx's thought has kept it for 150 years. (Needless to say, my book *A Thousand Years* was only the opening salvo. My serious attack on Marx is still to come, but when it does it will be devastating, or so I hope). Keeping that in mind, I believe the main contribution of Deleuze is his neo-realist ontology, and the way in which it can be used to solve the eternal problem of the link between the micro and the macro, of agency and structure. In this ontology all that exists in the actual world is singular individual entities (individual atoms, cells, organisms, persons, organizations, cities and so on) whose main difference from each other is spatio-temporal scale. There are no totalities, such as "society as a whole", but a nested set of singular (unique, historically contingent) beings nested within one another like a Russian Doll. Between one entity and the larger one the relationship is one of parts to whole (not one of membership in a general category). This link is machine-like: lower scale entities form the working parts of a larger scale whole, a whole which emerges (and needs to be continuously maintained) by the interactions between the parts. Thus, interacting persons yield institutional organizations; interacting organizations yield cities; interacting cities organize the space in which nation states emerge and so on. This changes the very way in which the problem of agency and structure is posed, since the term "structure" illegitimately conflates several scales and deprives organizations and cities of causal agency.

Thanem: Given the Deleuzian emphasis on machine-like links between different entities, would you like to say something about what this may mean for the notion of causal agency too and indeed for our understanding of entities such as organizations? In addition to the importance you attribute to the processes by which entities are enabled and maintained, the machinic surely brings a sense of heterogeneity, change and openness to organizations and social relations that makes agency more akin to what you elsewhere – via Spinoza – have referred to as production.

DeLanda: The concept of "causal agency" we inherit from the tradition is deeply non-realist and anthropocentric. This is Hume's concept of a cause as a constant conjunction of events, a conjunction experienced as such by a human, of course. We need to switch to a realist view of causes not as conjunctions but as actual connections in which one event produces another event (e.g. a collision between two billiard balls produces a change of state in the motion of the balls). This immediately suggests a "machinic world", one interconnected by relations of production. In

addition, the current realist definition of "emergent property" (due to Bhaskar) implies this new definition, since a whole is seen as emergent (as opposed to a mere aggregation of parts) if it has causal powers of its own. For Deleuze's solution to the micro-macro problem, as sketched above, the notion of several levels of emergence is crucial.

Protevi: Yes, "emergence" is the biggest question in social science (methodological individualism, structure/agency, Luhmann's differentiation of social structures, and so on). You could say that Deleuze and Guattari bring a political dimension to bear in their encounter with complexity theory so that they thematize the question of emergence above the subject to the level of social (tribal, gang, institutional, urban, State) machines. (Actually, here Manuel's work is indispensable, as Deleuze and Guattari themselves tend to jump straight to the "socius", which would mean the "State" level in analyzing "capitalism".) But they also show the importance of complementing the move above the subject with one moving "below" the subject to a multiplicity of "agents" (a move known in cognitive science as the "society of mind" thesis). Deleuze and Guattari enable us to connect the two moves, above and below the subject. Here the question of emergence as the constraint of lower level components and the concomitant enabling of system level behaviour comes to the fore.

Thanem: Manuel, considering your critique of social constructivism and relativism (DeLanda 1996), how are these questions at odds with these approaches, which have come to influence much social and organizational research in the past couple of decades?

DeLanda: Well, Luckmann and Berger as well as Garfinkel explicitly base their approaches on phenomenology, that is, they operate within a (mostly implicit, hence uncritically accepted) ontology of appearances. They have also taken (like most other social scientists) the so-called "linguistic turn", which in my opinion was the worst possible turn. It follows that the term "construction" is not used in the sense in which Foucault, for example, talks of the construction of soldier bodies through drill and discipline, but to the way our minds "construct" the world of appearances via linguistic categories. There cannot be anything more alien to a materialist like Deleuze than this brand of linguistic idealism.

Thanem: Isn't one problem also that both social constructivism *à la* Berger and Luckmann and ethnomethodology *à la* Garfinkel tend to focus on the construction and maintenance of the dominant social order, thus underestimating the forces of resistance that make things change?

DeLanda: Well, yes, that too. It seems as if sociologists, wanting to have nothing to do with micro-economics, must conceptualize human action in terms of routines and traditional procedures instead of "rational" choice. Now, I agree that when choice is seen as atomized and optimizing (maximizing welfare or utility) it becomes quite hard to take seriously, but we can always move to Herbert Simon's "satisficing rationality" instead. I just do not see why this has to be an alternative: either routines followed by an oversocialized agent or choices made by an undersocialized one. Why not both in different occasions? Say, choices about matching means to ends when the intensity is high (a crisis situation when one must solve a new problem, or a situation where subversive solutions must be invented) and traditional routines when the intensity is low (and one merely reproduces the existing social order). Clearly, both situations coexist in social reality all the time.

Protevi: Yes, I really like the high vs. low intensity situation schema. For me, the place the lack of awareness of emergence above and below the subject creates the most mischief in philosophy is in cognitive science. Why do so many cognitive scientists stop at the brain as the highest emergent level? Why not have this as an intermediate global level, caught up in turn by higher level institutional and social "machinic assemblages", so that we add other levels of constraints (and enablings)? Undoubtedly we need to account for the novelty and unpredictability of individual human subjects (the major concern of cognitive scientists who are still arguing against a positivist conception of science as the construction of universal and exceptionless laws), but we also need to account for social predictability in molar populations. Human beings are rule-followers as well as free agents; in fact many free agents break rules but in so doing form practices that can install new patterns in bodies, patterns that can become rules for others. (The relation of "rule" to "pattern" is what Bourdieu calls "habitus", and what Deleuze and Guattari call de-territorialization accompanied by compensatory re-territorialization.)

So in moving us above and below the subject Deleuze and Guattari do not deny that there is a genuine subject, but they do stress that it is only an intermediate global level of organization. We can thus talk about degrees of freedom of human action. We are more free (1) the greater the constraints the subject level can exercise over "autonomic" sub-systems: (e.g., yogic experimentation with physiological processes); (2) the more one's "subject position" allows one to negotiate social constraints embedded in institutions and free-floating or "peer pressure" systems (e.g., gender and race constraints); and (3) the more money one has (in some places gender and race constraints are being replaced by economic constraints), since then, to complete the system, you can buy somatic training and/or move to places where economic power mitigates race and gender constraints, etc.

NATURAL SCIENCE

Thanem: In their last joint work *What Is Philosophy?* Deleuze and Guattari (1994) reject the traditional notion of philosophy as the "mother of all knowledge" yet argue for a close engagement with science that – as you point out above – would be less than popular with scholars working from a phenomenological perspective. This has been an important aspect of Deleuze's earlier work (e.g. 1993, 1994), but it even finds some resonance in *A Thousand Plateaus*. Similarly, you both have pursued an intimate engagement with science – Manuel in *Intensive Science and Virtual Philosophy* and John with respect to natural geography in *Deleuze and Geophilosophy*. Would you care to comment on how you relate to Deleuze and Guattari's (1994) methodological challenge?

DeLanda: Deleuze accepts the objectivity of science but not the legitimizing discourses of scientists. For example, he would not accept the existence of general laws (or of general anything: generalities exist only in our minds) but he would accept, I believe, the topological structure of those laws (e.g. for Newton's laws, a phase space structured by a single singularity or attractor which would explain the "least principles" on which those laws are based). That topological structure complements his ontology of actual individual singularities with virtual universal singularities. I believe that recent developments in nonlinear science (which has discovered just how complex phase spaces can get unlike those of classical

physics) validate his approach.

Protevi: Perhaps I could ask Manuel to expand on what he says at the end of *Intensive Science and Virtual Philosophy* about the way the notion of "science" laid out in *What Is Philosophy?* overlooks the Royal vs. minor science distinction in *A Thousand Plateaus*.

DeLanda: In their last book, they define science in terms of its use of mathematical functions and then define the latter using the characteristics of functions as used in Classical Mechanics. (There's no mention of the operators of Quantum Physics, which use functions as inputs and outputs, nor of the different uses of functions in chemistry or biology). This comes close to saying that the essence of science is its classical physics core. Yet earlier in their work they displayed an awareness of the existence of many scientists who do not fit that mold. Here we may contrast Newton and Hooke, one the grand creator of laws attracted by simplicity (not to mention administrator of the Royal Society), the other the builder of instruments and manipulator of material systems, attracted by complexity (and always with lower prestige in the social world). Philosophers can learn a lot more from the latter type of scientist (embryologists, hydrodynamicists, geologists) rather than from those famous physicists who aimed at taking laws and creating an axiomatic, deductive system based on them. Unfortunately, Deleuze took from Michel Serres a "definition" of minor science which is OK but that does not cover all cases: instead of axioms and theorems "minor scientists" pose new problems; instead of simple solids and gases they look at complex liquids; instead of metric, Euclidean spaces they look at projective, differential or topological spaces and so on. Now, how applicable is this definition to areas other than mathematical physics? Not very much. We need a lot more empirical research on the minor sciences (Organic Chemistry, Fluid Mechanics, Materials Science, etc.) before reaching conclusions here.

Thanem: Scientists keen to protect their own epistemic borders may respond to philosophical and other non-scientific engagements with science by accusing them of stealing concepts from science, of not fully understanding the science behind these concepts, or of reducing complex concepts to superficial metaphors. How serious are these claims?

DeLanda: Well, scientists have a non-realist working philosophy (positivism) which allows them to say, "hey we are not giving a true picture of reality; all we do is to create compact descriptions useful for prediction and control." That's fine with me. Newton had to say something like that since his gravity as "action at a distance" was just not selling among Cartesian scientists for lack of an explicit mechanism. And it was fine he did that because positivist apologetics (without the name, of course) were needed at the time to shield his theory from premature dismissal. But by espousing that form of non-realism scientists have effectively surrendered their right to say what there is in reality. So they are right to complain about misappropriations of their work (or relatively technically ignorant critiques, such as those of Science Studies) but cannot stop realist philosophers from doing the hard ontological work scientists have neglected for so long.

Thanem: Would you like to give some examples of "technically ignorant critiques" in Science Studies (which in general seem to fall short of e.g. Canguilhem's more sophisticated conceptualist philosophy of science)?

DeLanda: Let me qualify that remark. The leaders of that field (Latour, Pickering, Collins, Bloor) are certainly not technically

ignorant. Most of their followers, however, are. But even in the leaders' work, the relatively competent and useful insights are not the ones for which they are famous. They are famous for exaggerated and illegitimate claims like "every laboratory fact is socially constructed". One can, indeed, benefit from the few gold nuggets that are hidden under such purple prose, but in my experience these good insights can be recovered only by assuming the authors are "closet" realists.

Protevi: I suppose whenever you talk about Deleuze and Guattari's relation to science you should mention the "Science Wars" of the mid to late 1990s, even if you run the risk of disinterring a thankfully buried dead horse for more beating. Take Sokal and Bricmont's *Fashionable Nonsense* for example. In general, Sokal and Bricmont's warnings against the sort of chatter that declares a "revolution against Newton" are very well-founded. As Manuel shows in several places, the history of the way linear models have been applied to all natural phenomena, including those we now use nonlinear models to examine, is very intricate and is not at all capturable by the term "revolution". (Not the least of those intricacies is the way in which some natural and social areas of the world have actually been rendered more homogenized and normalized [by genetic modification and by disciplinary practices] and hence more amenable to linear modelling.) In any event, the fact that Sokal and Bricmont are correct here is no reason though to accept their treatment of Deleuze and Guattari, and that for two reasons: they don't uphold the standards for a good polemic, and they polemicize at all! To produce a good polemic you have to reconstruct the context in which the attacked authors make their claims, but this is precisely what Sokal and Bricmont fail to do. Their remarkable chapter on Deleuze and Guattari in *Fashionable Nonsense* largely consists in the presentation of extended quotation juxtaposed with out-of-hand dismissals, which simply *assert* that Deleuze and Guattari's discourse is "utterly meaningless", etc. But even if they had polemicized well, I don't think the genre of polemic is very helpful, for at base, a polemic tells you not to bother to read X. But to see if that is sound advice, you have to then go ahead and read X! (In other words, the genre of polemic is beset by a fundamental "performative contradiction". So go ahead and read Sokal and Bricmont and see if you think I'm doing them justice when I say they don't do Deleuze and Guattari justice!)

DISCIPLINE

Thanem: Obviously, developments in the natural sciences have had a major impact on the social sciences too. Through figures like Comte, Durkheim and the Harvard Pareto Circle the founding of the social sciences as separate from the natural sciences and the construction of organization theory as a distinct academic discipline coincided with a fundamental reliance on natural science methodology and concepts. For example, Mayo's (1933) metaphorical notion that work organizations are like biological organisms draws directly on biomedical research on homeostasis by his Harvard colleague Walter B. Cannon (1932). The social sciences have been less as welcoming of ideas from philosophy and the humanities. Even between the social sciences there is a strong academic division of labour, allocating different disciplines to different problems and different levels of analysis. Mainstream work that stays clear of disciplinary boundaries is still privileged by top academic journals on both sides of the Atlantic. The contrast to philosophy seems

immense. Although Deleuze and Guattari's (1994) particular rethinking of philosophy's relationship to science may remain marginalized within philosophy proper, a close engagement with science, art, literature and the social seems to be a crucial aspect of what it means to do philosophy. Do you agree with this description of philosophy and are there any risks associated with such "unboundedness" – for example, that one may be ridiculed for not doing philosophy?

Protevi: I think here we have to come to grips with the legacy of positivism in the social sciences. Positivism based its "scientific method" on classical physics as a model of scientific rigour and usually attached a reductionist program, so that the soft sciences should strive to attain the rigour of the hard sciences. In that way the truths of society could be reduced to the truths of psychology, biology, chemistry and finally physics, as each step along the way had been "reduced". In such a reduction, we strive to analyse complex systems into components and then claim that adding together the solutions to the equations that account for the behaviour of components will account for the behaviour of the system without remainder. Reductionism consists in a denial of emergence then, so that all "wholes" are mere aggregates. We see this reductionism today in methodological individualism in the social sciences, e.g., rational choice theory.

As David Harvey's *Explanation in Geography* (the work of social science methodology with which I'm most familiar) is very good at pointing out, vast problems are created for social sciences when they adopt a positivist model. First of all, time is irrelevant in classical physics (the "reversibility of time's arrow" thesis), and there are in principle no unrepeatable events (replication of results is an essential part of physics), but the irreversibility of time and the uniqueness of events are of the essence of historical phenomena. Furthermore, making the prediction of human behaviour the goal of your social science pushes you to develop disciplinary methods of intervention and control. Discipline tries to make social reality conform to rational choice models by normalizing humans, that is, turning them into "individuals" whose behaviours can be classified relative to the norm of a population of other such individuals. Classical and neo-classical economics make assumptions of these goals of disciplinary practice, which thereby enables them to model economies as equilibrium systems. The result is an elegant model whose inability to predict reality is often blamed on some recalcitrant feature of reality: the model says markets should behave in such and such a fashion; real markets do not behave in this fashion; therefore there must be some government distortion of the real market preventing it from behaving in the way it should; therefore we must remove such distortion to "allow" – that is, to make – the system behave the way the model says it should. The key point is that neoliberal governments (in today's world, under the pressure of the IMF) try to bring such "rational economic" behaviour about by actively producing the social situations the model assumes: normalization of behaviour by making people behave in individual self-interest (due to lack of social interaction/social security). The problem comes when people write about such economics as if they were only a matter of assumptions and models rather than prods for concerted efforts to produce a social reality conforming to the model's assumptions. (Chuck Dyke's *The Evolutionary Dynamics of Complex Systems* contains an eloquent statement of this position of the relation of discipline and

rationality.)
Now how does philosophy fit into this picture? First, philosophy has often taken the "meta" level as its domain. In this way it steps back from first-level scientific work and tries to articulate the conceptual presuppositions of the sciences and their connection with social practice (NB: this need not – and should not – be an anti-realist "sociology of science" standpoint, as Manuel will no doubt be keen to point out). By doing so, philosophers can construct a vocabulary that can help scientists in different disciplines talk with each other (I think this is what the "philosophy of mind" tries to do with regard to cognitive science).
DeLanda: Though I agree that early twentieth century social science had physics and biology as its model, I would argue that the latter part of the century (starting with Cultural Anthropology) has been much more influenced by the humanities, particularly Hermeneutics and Lit Crit. Clearly, as much damage as the organism metaphor caused in sociology (favouring integration instead of conflict, and giving rise to ahistorical functionalisms) that is nothing relative to the damage that linguistic idealism has caused. At any rate, the boundaries between disciplines, formed as much by principled distinctions as by academic battles over turf, are mostly meaningless. It follows that a philosopher cannot take those artificial limits into account, and that it should push multidisciplinary approaches to the limit. If that makes it lose its own identity as a field, so much the worse for those silly boundaries.

METAPHOR

Thanem: Indeed, and what you say about social science in the late twentieth century seems to hold for certain European strands of organization theory too. But partly as a result of its admiration for natural science and its somewhat lesser fascination with the humanities, organization theory abounds in metaphors, often uncritically and superficially introduced to foster "new" thinking about organizations. Deleuze and Guattari are important in this context, as they problematize the ostensible difference between ordinary words and metaphors. How can this attitude be explained, and what are your takes on metaphor and analogy?
Protevi: I think what they're after is the following: metaphor has traditionally been defined as the transfer of the meaning of a sign from an old object (the "literal meaning") to a new object (the "figurative meaning"). The new object is then "seen in a new light" or some such formulation of an epistemic gain on the part of an observing subject. But Deleuze and Guattari are not interested in such linguistic effects, for two reasons. First, as Manuel often points out, they are trying to demonstrate that the same "abstract machine" lies behind different actual systems. I wonder if he would like to expand on this point.
DeLanda: Deleuze created his ontology partly as a response to Foucault's analysis of the "classical episteme", which had four major axes: similarity, identity, analogy and contradiction. Since all classical thought is bounded by those four categories (if we are to believe Foucault) it follows that any approach that wants to be non-classical must avoid them as foundational categories. But that does not mean they cannot be included as derivative notions: if identity is explained by a process (as I suggested above) it's quite acceptable. So is similarity if it too is accounted for by contingencies of process (e.g. the similarity of members of a species due to selection pressures). The same point applies to metaphors. Within linguistic idealism they become foundational (Lacan, e.g., claims

that all meaning is metaphorical) but detached from that ontology they are useful devices. They just cannot be expected to do the heavy theoretical work that needs to be done, which necessitates deeper topological isomorphisms, e.g. the sharing among processes of the universal singularities I mentioned before. Take for example the distinction between "markets" and "hierarchies" as done in Organizational Theory by people like Oliver Williamson. A Deleuzian treatment (a de-marxified one) would treat these two as special cases of decentralized networks and centralized structures. Or even more radically, as specific actualizations of two more abstract cases, rhizomes and strata, which also have biological and geological actualizations. What would make all the actualizations comparable not metaphorically would be that the processes which produce them share a deep topological isomorphism, the same virtual singularities guiding the processes.

Protevi: The second thing to say about Deleuze and Guattari and metaphor is that for them meaning is not the relation of signs to one another but the probability that an environmental change can function as the trigger of a material process in a given body politic by pushing that body to one of its thresholds of self-organization. (Such a triggering event is what they call the "order-word" in human language; or more generally a "sign", as when a difference in the food gradient of a medium will prompt the change in structure of the slime-mould amoeba, or when a temperature difference will prompt the move from conduction to convection in a heated liquid. Such a view requires us to change our notion of "perception" as well; all this echoes work done in the cybernetics period, as detailed by many contemporary authors; I can recommend Jean-Pierre Dupuy's *The Mechanization of the Mind* for an introduction to the relation of cybernetics to the history of cognitive science.) Thus to change the meaning of a sign would entail changing the "political physiology" of that body: installing a new set of patterns and thresholds so that new triggering relations with signs are set up. This sort of body work can take a long time and requires cautious corporeal experimentation in a safe social context. From this perspective, the idea that a reading that shuffles signs about, that assigns new signs to old objects without any body work, would count as a political intervention, is silly academic self-flattery. Even more delusional would be the idea that the spontaneous production of such counter-hegemonic readings by consumers of cultural products constitutes effective popular "resistance". As Thomas Frank (whose bitter critique of cultural studies in *One Market Under God* is necessary reading) would put it, why should we make such a big deal about what is just a glorified snickering behind the boss's back? The point is to construct new bodies politic, and that requires work in political physiology, not in counter-hegemonic reading.

THE VIRTUAL

Thanem: The Bergsonian concept of the virtual and the complex relationship between the virtual and the actual is a central theme in Deleuze's philosophical enterprise, which you both have dealt with in your own works (see Protevi 2001; De Landa 2002a). To readers unfamiliar with Deleuze, the virtual may at first seem to take one away from a concrete, political physiology. At the same time, a large number of organization theorists, management researchers and other contemporary writers in the social sciences and humanities conflate the virtual with issues of globalization, networks and recent

developments in information technology (e.g. Hedberg et al., 1997; Czerniawska & Potter, 2001; Robins & Webster, 2002). Few – if any – of these seem to appreciate the Deleuzian and Bergsonian concepts of the virtual. How do Bergson's and Deleuze's concepts of the virtual differ from this literature and what are your takes on it? Alternatively, what do you see as the main implications of the Deleuzian concept of the virtual on empirical problems and phenomena?

DeLanda: While the word "virtual" is typically associated with some kind of "virtual reality" (whether digital simulations, the internet, or some other computer related phenomenon) for Deleuze the virtual is a "real virtuality", a non-actual dimension of the real world. Now, adding extra dimensions to the world is clearly burdensome for the realist since defending a mind-independent world is hard enough without adding extra stuff. But the burden is relieved once we realize that virtual singularities allow us to dispense with "eternal laws" in physics and elsewhere. We add a dimension but eliminate another, hence it's not inflationary. As far as the relation to Bergson goes I can only add a few notes. The key to rigorously thinking about the virtual is to conceive of it as a space with very different properties than actual space. While the latter is "metric" (it depends crucially on notions like "rigid length" or "fixed area") the former shares the properties of projective, differential and topological geometries. The work of Riemann on "differential manifolds" is key here, as these form the basis of phase space and of the Deleuzian notion of "multiplicity". Deleuze takes this from Bergson's thoughts on Riemann. Similarly, if the virtual is not going to be an eternal reservoir of "topological essences" (e.g. attractors) it will also need its own form of non-metric temporality, one in which the notion of a "stretch of present time" (the time equivalent of a fixed length) is not meaningful either. Bergson's ideas about time and memory come close to being such a theory of non-metric time (or a proposal for such a theory) and they also greatly influenced Deleuze.

Protevi: Let me ask Manuel if he would like to clarify for us the way in which he says nudging a material system from equilibrium to non-equilibrium (what he calls the "intensive") will provide a "window on the virtual".

DeLanda: Imagine a physical system on its way to equilibrium, the state with the maximum disorder. At that point in time the final state can literally be "seen" deterministically attracting the dynamics of the system. The final state, not having been reached yet, is not actual but virtual. But thermodynamicists, until very recently, studied such systems only once the equilibrium had been actualized, that is, when it had lost its virtual status. This approach effectively concealed the virtual. From the 1960's on, people like Prigogine began to study systems which are not allowed to actualize that equilibrium (they are constrained to remain far from equilibrium), and that has revealed not only the existence of the "old" steady-state attractor, but of several others (periodic, chaotic). Given that even without human intervention parts of the world are at equilibrium while others (e.g. the atmosphere-hydrosphere system) are far from it, we can say that some areas of the world reveal the morphogenetic potential of matter (the potential which universal singularities are supposed to explain) while others hide it from view, in a kind of "objective illusion".

Thanem: Following attacks by radical (often Marxist or poststructuralist) organization theory on mainstream (often Parsonian) systems theory in organizational research,

"reformed" systems theorists (e.g. Brown & Eisenhardt, 1997; Anderson, 1999) have for the past couple of decades looked to the sciences of chaos and complexity (e.g. Prigogine) for new inspiration. But despite proposed affinities between Deleuzian philosophy (esp. the concept of the virtual) and (e.g. Prigogine's work in) complexity theory (you have both made this point), they have not as of yet shown any interest in Deleuze. Equally, during the past few years the growing interest in Deleuze (and Guattari) among radical organization theorists (perhaps with the exception of Chia, 1998) is rarely accompanied by attempts to engage with complexity theory – perhaps because their aversion to computer simulation (however nonlinear) seems to be stronger than ever? Appreciating that you are outsiders to organization theory, why do you think mainstream and radical organization theorists have adopted one and ignored the other? Or, what does Deleuze provide that Prigogine does not?

Protevi: I'm not sure why the connection between Deleuze and Guattari and complexity theory, which Manuel and Brian Massumi have been making since the early 1990s, has not been followed up on in organization theory (and unfortunately, not too much in philosophy either). I suspect those with scientific backgrounds might be put off by the sheer exuberance of Deleuze and Guattari's writing (this has nothing at all to do with a "postmodernist playfulness" or what have you, which aims at signifier effects), as well as by their Marxist orientation (more on that later). On the other hand, for those with the typical "continental" philosophical background (phenomenology, post-phenomenology, or God help us "postmodernism"), the science connection is probably anathema, either because of anti-realist commitments or because they just don't want to take the effort to come to grips with the science. (The difficulty in becoming familiar with the basic principles of complexity theory, it seems to me, is grossly exaggerated. I'm not saying it would be easy to gain the level of mastery Manuel demonstrates in *Intensive Science and Virtual Philosophy*, for instance, but we are fortunate that many gifted scientists – I'm thinking here of Ilya Prigogine, Stuart Kauffman, Brian Goodwin, Francisco Varela, Gerald Edelman, Antonio Damasio, and Joseph LeDoux in particular – balance their original front-line research with book-length non-technical treatments. [When I say "non-technical" I mean a conceptual rather than mathematical treatment – or at least one which provides a conceptual explanation of the mathematics involved.])

DeLanda: I think the main obstacle to engaging with Deleuze directly is the style. He writes as if he deliberately wanted to be misunderstood, or at least that's the way it impacts someone who, like me, is trained mostly in Anglo-American analytical philosophy. (I suppose if one is used to struggling with Continental authors one may get a different impression.) He changes terminology in every book (so that the virtual dimension becomes a "plane of consistency" in one, a "body without organs" in another, a "machinic phylum" in another and so on) and never ever gives explicit definitions (or hides them well). I suppose that was an attempt on his part of preventing a given terminology to solidify too soon, to keep things fluid and heterogenous. Fine. But I cannot deal with that and hardly expect complexity theorists to put up with it either. Hence the incentive to write *Intensive Science* as a way to explain to complexity theorists the advantages for their own field of a Deleuzian approach. By the way, I wonder who you mean when you say "radical organization theorists", given that what

passes for radicality today (e.g. the neo-institutionalist school in sociology) is so deeply affected by linguistic idealism that it is really conservative, not radical.

Thanem: It would be great to see *Intensive Science* being picked up by social scientists with a leaning towards complexity theory *or* Deleuzian philosophy. Also, I agree with you on the inflation of "radicality", but it doesn't generally seem that the radical organization theory of people like Gibson Burrell, Heather Höpfl and Martin Parker would be easily grouped with neo-institutionalism. Having said that, much of what is labelled radical organization theory pursues a linguistic idealism that (inspired by Derrida, Lyotard or Baudrillard) is akin to what one might find in literary theory and cultural studies. Intentionally or unintentionally, these postmodernist pursuits often reproduce a relativism with at least conservative implications. In contrast, the recent interest in Deleuze appears to move radical organizational theorizing in more materialist – and radical – directions, and style seems less of an obstacle now than five years ago. (Indeed, some figures in complexity theory – particularly Maturana & Varela and Luhmann – are not exactly known for stylistic clarity!) Could you elaborate on the advantages a Deleuzian approach may have for complexity theory?

DeLanda: It really boils down to getting the right ontology for the entities postulated within nonlinear dynamics and other fields. What, for example, are "attractors"? Are they eternal archetypes? And if so, how are they different from Platonic essences? (Notice that this question is relevant only to a realist; since many of the scientists involved in nonlinear fields are positivists they do not really have to care about it). Many analytical philosophers of science (non-realists like Van Fraasen, and realists like Ronald Giere) are today turning to an ontological analysis of the crucial notion of "phase space". But here Deleuze is ahead of them, with a more accurate breakdown of the contents of that space (including the vector field and not only the solution trajectories) thanks to the fact that this tradition in mathematics is of French origin (our current treatment of phase space derives from the work of Poincaré) and Deleuze had direct access to these insights back in the 1960's (e.g. the work of Charles Lautmann). But again, his style has led these important insights to get lost elsewhere in the academy.

Protevi: Once you get past their style (and yes, it is less daunting for someone like me who came up through the ranks reading Heidegger and Derrida, but the ontological shift, from post-phenomenology to materialism, is wrenching!), there are indeed lots of reasons why the Deleuze and complexity theory connection is so interesting. The key for me is the notion of the "consistency" or "assemblage" (a flexible, open system, what Manuel calls a "meshwork"). Traditional systems theory, as well as its cousin cybernetics, was fixated on the notion of homeostasis, which measured a system's ability, via negative feedback loops, to return to a set point after environmental shocks. The key point here is "stability": how much of a shock can the system withstand and still return to "normal"? An open system, on the other hand, possesses "resilience": the ability to form new patterns and thresholds, either as the result of an environmental shock or as the result of endogenous "evolutionary drift", to use the term of Varela. What's great about Deleuze and Guattari is that they give us a wide-ranging and nuanced ontology with which to think about the difference between such systems. And this ontology seems to resonate with the latest science. Stuart Kauffman's latest work in *Investigations*, in which he talks about the expansion of biospheres into "the adjacent

possible" seems to me to fit right into the DeleuzoGuattarian notion of an open, expanding, creative, multiverse.

Thanem: Why is Deleuze and Guattari's "nuanced ontology" so important to scientific research? In *What Is Philosophy?* they explicitly say scientists don't need philosophers in order to reflect on their practice.

Protevi: Manuel should certainly feel free to expand on this point, but I'd say that the working out of basic ontological concepts can provide a series of relays among scientists in different disciplines. ("Oh, so that's the sort of singularity you guys find in modelling your systems!") This in turn can help scientists form interdisciplinary teams to investigate complex areas of the world, which, after all, aren't really interested in our scientific division of labour! There's an overlap of form and content here too. If the interesting problems of the world occur in assemblages, consistencies, meshworks, etc., then it seems reasonable we would need interdisciplinary teams, which are themselves consistencies or meshworks, to study them. At the end of *Intensive Science and Virtual Philosophy*, Manuel cites Ian Hacking on the "intensive" epistemology of the lab assemblage. Here we would see a sociology of science that needn't be an anti-realist "social constructivism". In fact you could say that Manuel's phrase "intensive science" refers not only to a science that studies the intensive or non-equilibrium parts of the world, but also a science that itself operates intensively, that is, in consistent, heterogeneity-preserving, interdisciplinary, assemblages.

ECONOMIC ORGANIZATION

Thanem: Whereas Deleuze's thinking of the virtual and Deleuze and Guattari's work on becoming makes an extraordinary effort to understand heterogeneity, creativity and openness, it seems from your own work (I guess Manuel's in particular) that Deleuze and Guattari's largely Marxist discussion of capitalist economic organization in *Anti-Oedipus* and *A Thousand Plateaus* may be critiqued for being relatively inattentive to these themes. This is interesting, as issues of economic organization constitute the very focus of the (particularly North-American) anti-Marxist mainstream of organization theory. Manuel, in your work you have not just critiqued Deleuze and Guattari for giving a highly selective reading of Ferdinand Braudel's monumental economic history (see DeLanda, 1997). *Inter alia*, your understanding of the history of capitalism complements Braudel (1973, 1982, 1984) with two central figures in organization theory: Simon's (1945, 1969) notion of bounded rationality and Williamson's (1995) discussion of transaction cost economics. What are Simon's and Williamson's most important contributions to the understanding of capitalist economic organization, and how is this reconcilable with new left politics (which you also seem to be arguing for)? (Has anything changed since *A Thousand Years of Nonlinear History*?) But like John, I'm also interested in what you find to be the most serious problem with Deleuze and Guattari's analysis of capitalist economic organization, so I therefore pass over to him first.

Protevi: Manuel, *Intensive Science and Virtual Philosophy* accepts Deleuze's use of axiomatics to analyze major or Royal Science. Yet you are critical of Deleuze and Guattari's use of axiomatics as a way to conceptualize capitalism (e.g. DeLanda, 1997, p. 331, n. 7), which you see as an example of a top-down positing of a whole. I certainly would agree with you that far too much Marxist work has been simplistic, historical determinist, reductive, totalizing, functionalist, top-down, etc., but I wonder if

you aren't being too harsh with Deleuze and Guattari's attempts to define a theory of capitalism that avoids each of these dangers? They certainly adopt a notion of "machinic surplus value", moving beyond a simple labour theory of value (machines as "congealed muscular energy", as you put it in *A Thousand Years* (1997, 79)). Don't they also consistently deny any historical determinism of stages of development by emphasizing the contingency of capitalist formations, as well as conduct a sustained polemic against reductive base-superstructure models of society? Don't their constant reminders that the line of flight is primary prevent any totalizing accounts?

DeLanda: I agree that if I had to choose among all the Marxist accounts of economic history I would probably pick theirs. It does have all the advantages you mention. (Even Braudel quotes Deleuze and Guattari on the idea that "capitalism could have emerged anywhere" not just the West.) Yet, I believe they would have benefited greatly from a better reading of Braudel. They seemed to have read only volume one of his history of capitalism and not the other two volumes, which are really the most radical part. This is clear when in *A Thousand Plateaus* in one page they quote Braudel's stress on the role of cities and yet in the very next page they go on to define capitalism as a "market economy", an idea which Braudel attacks as historically false. So I wonder what would have happened to their theory had they understood the last point: that there is no such thing as "the market" in general and no such thing as a "logic of exchange" in general (doesn't the idea of a capitalist axiomatic depend on the idea of a logic of exchange?). Once we separate oligopolies from the market (they are strategic not primarily exchangist entities) and identify capitalism with oligopolies (as Braudel does) we can still use some of Deleuze and Guattari's ideas since markets (as well as anti-markets) have always caused "lines of flight" to pass among societies, particularly closed societies (it's in the marketplace that we meet outsiders; that foreign objects and ideas enter a city; that heterogeneity is injected, etc.).

Protevi: I wonder if Deleuze and Guattari ignore the Braudelian distinction because, like Marx, they think the important element to be examined in capitalism is production rather than exchange?

DeLanda: Well, no, not really. I agree that the dichotomy "market/antimarket" does give that impression, hence I probably won't use it again. But the same distinction applies to production: it's the difference between economies of scale and economies of agglomeration. That is, between oligopolies using managed prices, routinized labour, hierarchical structure, vertical integration etc. and networks of small producers using market prices, skilled labour, decentralized structure and functional complementarities. You must remember the study that compares Silicon Valley and Route 128 as production systems (mentioned in *A Thousand Years*) or what I have written about Emilia-Romagna. Braudel (and Jane Jacobs following in his steps) places a great emphasis on this distinction (though he does not use the terms) and views it as applying across history for at least a millennium (hence economies of agglomeration would not be a late stage of capitalism as some Marxists have tried to argue using the term "flexible specialization" or the ridiculous one of "post-Fordism" but an alternative to economies of scale.

Protevi: So for you, it's the type of productive organization that counts, not just productivity as such. After all, production is the key ontological concept in *Anti-Oedipus*

(the whole world, nature and humans together, is composed of interlocking series of connected machines that produce materials that are fed into other machines).

DeLanda: This is correct. I myself add to this when I attack the Humean notion of causality (as perceived constant conjunction) and define it as a real connection in which one event produces another event. And more generally, when I stress that to get rid of essences one must always give the intensive process of production which yields any individual entity (atoms, organisms or commodities). Intensive thinking in general is about production.

Protevi: From this productivist perspective (which I think is amenable to a nonlinear dynamics analysis of the material and energy flows that keep the open production systems far-from-equilibrium), the key issue is the productive conjunction of capital and labour (here machinic surplus value vitiates a pure labour theory of value), whether or not the products of that labour flow into markets or anti-markets. And the key to coercing labour into exploitative production processes is to threaten the production of labour power with interruption of the flows that sustain it.

DeLanda: Well, but the same point applies here: the conjunction of capital and labour can take place in different forms (scale, agglomeration) and it is clear that only the economic power of the former allows the kind of threat of withdrawal you are talking about: only if a firm is very capital intensive (large machines, large start-up costs functioning as barriers to entry) and if the process is based on routinization (the less skills a worker brings the less bargaining power he/she will have when it comes to set wages) can this form of coercion work. I am not saying that power relations are absent from networks of small producers but there the ability of workers to bargain for a fair wage (particularly if unions exist) is much greater and the permeability of the division between classes is greater too (if a typical firm has less than a hundred employees and it is not capital intensive, it's much easier for a motivated, creative worker to start his/her own business). The point is that all of this is obscured (if not made invisible) by the blanket concept of "capitalism".

As to theories of value: we need to go beyond the very notion of surplus value. (It's not enough to simply add the "machinic" type to escape the labour theory). Why just adding machines to "abstract labour" (read, routinized labour)? Why not also fossil fuels, starting with coal? And what of knowledge, skills and organizational procedures? And then, the main defect of labour theory here is to include supply factors and not demand factors, but the latter also matter, and so marginalist approaches to this side of the equation must be added. (Over the objections of Marxists who would rather die than include bourgeois marginalism in a theory of value.)

Protevi: For you, what explains Deleuze and Guattari's tenacious loyalty to what they must have thought was a suitably modified form of Marxist analysis? Is their calling what they do a form of Marxism simply the result of their social position as part of the non-Communist French left? In other words, a way of thumbing their noses both at neo-liberals and at party loyalists?

DeLanda: Well, frankly, I think Marxism is Deleuze and Guattari's little Oedipus, the small piece of territory they must keep to come back at night after a wild day of deterritorializing. Who could blame them for needing a resting place, a familiar place with all the reassurances of the Marxist tradition (and its powerful iconography of martyrs and revolutionaries)? The question is whether we need that same resting place

(clearly we need one, but should it be the same? Shouldn't each of us have a different one so that collectively we can eliminate them?).

I believe that the main task for today's left is to create a new political economy (the resources are all there: Weber, Veblen and the old institutionalists, Galbraith, Braudel, some of the new institutionalists, like Douglass North; redefinitions of the market, like those of Hayek/Simon etc.) based as you acknowledged before, on a non-equilibrium view of the matter. But how can we do this if we continue to believe that Marxists got it right, that it is just a matter of tinkering with the basic ideas? At any rate, concepts like "mode of production" do not fit a flat ontology of individuals as far as I can tell. But then, this is the part of my reconstruction of Deleuze that I am the least sure he would accept: in *Difference and Repetition* he cheerfully talks about the "virtual multiplicity of society" (using Marx as his example, of course) a term I would never use (since my ontology explicitly rejects totalities like "society as a whole").

Thanem: John, what is your view on Deleuze and Guattari's understanding of capitalist economic organization?

Protevi: On the one hand, as Manuel has demonstrated, the principles of Deleuze and Guattari are that you can't just posit abstract entities, but must show their concrete coming-to-be via material processes, their "morphogenesis." On the other hand, Deleuze and Guattari use terms like "capitalism" which seem to be abstract entities that have not yet received a demonstration of their morphogenesis. Now it might be that this discrepancy is simply a result of Marx being Deleuze and Guattari's "own little Oedipus." Or it might be that they think Marx himself has provided the morphogenetic account (or at least the principles thereof), historically in the writings on "primitive accumulation" (Chapters 26-33 of Volume I of *Capital*) and systematically in all of *Capital*. So to resolve the tension we need someone to try to demonstrate that Deleuze and Guattari's use of Marxist concepts: (1) respects their own principles; and (2) is such that those concepts are still recognizable as "Marxist". I don't know if I or anyone else will ever be able to provide such a demonstration. So in the meantime, I think Manuel's right that we have to go with Deleuze and Guattari's principles, and demand a "bottom-up" morphogenetic account before we accept any entities into our social ontology. That means suspending our use of Marxist categories and reformulating political economy. And I also accept Manuel's suggestion that the difference in productive organization between economies of scale (centralized management and routinized, deskilled labour) vs. economies of agglomeration (networks of skilled labour) should be a fundamental category (along with differences at the city and regional levels).

There is a turn of the screw here though, concerning not organizational differences, but ownership (that is, worker co-operatives vs. absentee-owner firms). Let's grant that the ethical impulse behind Marx's denunciation of private property (not of course personal property, but ownership of the products of someone else's labour in exchange for a wage), needs to be bracketed when it is put in terms of an *a priori* demonstration that such alienated labour betrays the essence of human nature *qua Gattungswesen* ("species being", or co-operative production). Nonetheless, I suspect Deleuze and Guattari would have a pretty strong predilection for worker co-operatives over absentee-owner firms, although they would also say we need to investigate the life-affirming or life-denying aspects of particular concrete assemblages, and ask

whether in fact this or that worker co-operative (whether a centralized economy of scale operation or a networked economy of agglomeration operation) produces better and larger sets of affects than this or that absentee-owner firm. And I think Deleuze and Guattari would also have to admit that it's not at all clear that worker co-operatives will *always* be superior: it might be that the fear of bankruptcy might paralyse the initiative of some worker co-operatives, and so on.

This is not an entirely satisfying way of posing the question however. To perform a good evaluation of the life-affirming or life-denying affects of worker co-operatives, we need to have lots of examples of them to study, but they aren't very prevalent. How do we account for their scarcity? In what Deleuze and Guattari call "population thinking" (what Manuel calls the investigation of an "institutional ecology") you have to specify the variation-generating mechanisms and the selection pressures that accounts for the distribution of traits in a population, in this case, the ownership patterns in firms. In terms of variation, we might surmise that the very availability of credit, based on predictions of return on investment (based perhaps on past performance of worker co-operatives, but which might also be influenced by sheer fear of the unknown or simple class prejudice on the part of bankers), constrains the variation of ownership forms and makes the generation of worker co-operatives difficult. The price of the credit, the interest a worker co-operative would have to pay, would in turn form part of the selection pressures, as would lots of other factors (can worker co-operatives bargain with absentee-owner firms for materials, or would they be subject to boycotts, price-fixing, etc.?). In other words, the accent has to be on the "political" in "political economy"!

Thanem: Would you care to comment on Hardt and Negri's understanding of contemporary capitalism in *Empire*?

Protevi: *Empire* is an interesting book that covers ground that needs to be covered by anyone who wants to understand major trends in our current situation. Beyond the obvious international relations questions about the relations of the USA and the UN in global "policing" operations such as Afghanistan and Iraq, we have their political economy analyses. Now do Hardt and Negri pose these questions in a properly bottom-up way? No, not really, although I think they are perhaps not as far away from a bottom-up approach as it might appear. For instance, despite many pronouncements about our new "postmodern" era, I think they are less committed to an "epochal" reading (where everything has changed) than to one in which it is merely the "leading sector" (today, informatics, affective labour, etc.) that has changed and in so doing sends changes through the rest of the economy. Although they don't use these terms, the "leading sector" form of analysis can be translated into one in which we examine the change in selection pressures in the institutional ecology.

They also have interesting things to say about outsourcing and flexible labour forces, about the "pink collar" ghetto in the service industry, about the growing importance of FIRE and global cities that direct production networks. Again, although they use pretty traditional Marxist terminology (though with Negri's characteristic insistence that is the struggles of the workers that prompt the shifts in capitalist organization) in describing these phenomena, not all of it is beyond recuperation by a bottom-up account. In particular, I think you can translate into bottom-up terms what they say about "formal" and "real" subsumption

(the increasing commodification of previously non-commodified social relations – the first process by colonialism, the second by technological and social methods: "surrogate motherhood" for instance) by making it cross the "structural adjustment" policies of the IMF begun in the mid-1980s (the lever here is national debts incurred in the immediate post-colonial period). Here we might see the outlines of an "consistent" global system that reaches from workers' bodies through firms to states, international organization (the G7), and quasi-state actors (the IMF) and back down again, demonstrating the sort of interlevel causality – top-down constraint and bottom-up emergence – we look for in demonstrating systematicity. (We would need the big caveat that this analysis should not be in terms of a blanket notion of "capitalism", but should take into account the difference between economies of scale and economies of agglomeration, with the former being the beneficiary of the IMF policies, even when they are dispersed in networks of subcontracted sweatshops.) The key is the way structural adjustment policies push states to change their bio-power policies: cutting public assistance programs will increase the threat of malnutrition, disease, and/or "social deprivation" (shunning of homeless people). We might then ask what are the disciplinary effects of these policies in creating an atomized and normalized, urbanized and de-skilled, work force in a country?

DeLanda: Though I agree with much in what you just said, every time you hit on a traditional Marxist cliche I shiver. Take for example the phrase "ownership of the products of someone else's labour in exchange for a wage". Does not that very thought imply that, as Marx would want it, "wage labour" is a form of surplus extraction? But who ever demonstrated that is true? Certainly, the workers at coal mines in England who unionized early on did not believe that. They thought there was such a thing as a "fair wage" and they just needed to have the bargaining power needed to reach this fair outcome in negotiations. Or take the expression "the increasing commodification of previously non-commodified social relations". What is that supposed to mean outside a Marxist analysis of the commodity form? An analysis which, at least in volume one of *Capital*, is completely *a priori* and moralistic. (I find the expression "commodification" worse than useless.) And finally, yes Marx was aiming for a morphogenetic account of the entities he posits. The question is: is the "negation of the negation" (or morphogenesis through synthesis of opposites) a valid scheme? Does it not go directly against Deleuze's call for a positive treatment of difference in all morphogenesis (e.g. intensive differences)?

Protevi: Yes, it is hard to bracket all the Marxist concepts with which I've thought for so long! But while I'm willing to bracket concepts like "commodity", I would like to stick up for the primitive accumulation chapters. Although they are conducted in terms of "capitalism" and thereby overlook the scale vs. agglomeration distinction, they don't use any Hegelian synthesis at all, but are concrete historical investigations of enclosures, vagrancy and poor legislation, colonialism, slavery, national debt, and so on. (Okay, I'll admit it, the phrase "negation of negation" does appear in the peroration at the end of Chapter 32, but it's just window dressing and could be excised without any loss to the historical demonstration, which is in principle suited to the sort of intensive or differential treatment you rightfully point out as a Deleuzian requirement. For instance, Robin Blackburn uses the ideas Marx develops in

the primitive accumulation chapters to show that the critics of the "Williams thesis" about the role of slavery profits in the take-off phase of English industrialization precisely ignore the catalytic [intensive, differential] effects of such profits making credit easier [1997, p. 532].) From that perspective, my worry about marginalist accounts of the demand for skills as determining fair wages is that they might tempt some people to overlook the historical genesis of the de-skilled and landless worker. Now this worry doesn't apply to you, Manuel, as I know you talk about the deterritorialization of workers in *A Thousand Years of Nonlinear History* in your account of growing economies of scale in English agriculture. Nonetheless, I think it's always important to emphasize that while the unionizing miners you mention might have been trying to make the best of a bad situation (and yes, of course, to understand their bargaining we have to replace the abstract rational agent with a situated Simonian agent using satisficing rationality), we also have to account for how they got into that bad situation in the first place. So I guess I'd say that if any part of *Capital* is going to survive the bottom-up reconstruction we're demanding, it would be the primitive accumulation chapters.

Thanem: Deleuze and Guattari's (1984, 1988) discussion of Marx, capitalism and fascism includes some examination of bureaucratic organization. In *A Thousand Years of Nonlinear History*, Manuel touches upon issues of formal organization and control, taking issue among other things with Drucker's (1967) uncritical attitude to Taylorism. In *Empire* Hardt and Negri (2000, pp. 152-153) discuss the transformation of capitalism in terms of the emergence of the postmodern organization, corporate culture and diversity management. Is this an indication that organizational theorizing – beyond Simon's and Williamson's perspectives – has a role to play for philosophers concerned with the social? And are there reasons to expect that further engagement by philosophers will take place?

DeLanda: As far as I can tell, on the question of Taylorism, the most important insight which goes beyond economics is due to people like Michel Foucault. The basic idea is that several of the key elements of mass production are not of bourgeois origin but of military origin. It was in French armouries in the mid eighteenth century and later in American armouries and arsenals, that a system of industrial management and discipline was created in order to produce weapons with interchangeable parts (see Roe Smith, 1985). The degree of uniformity needed to create true spare parts needed to be imposed on artisans via routinization of the labour process and constant monitoring from above. As Foucault says, discipline increases the powers of the body in economic terms of utility but decreases them in political terms of obedience. How are we to change this oppressive system if we are not even aware of its origins? (Remember that Lenin welcomed Taylorism into the Soviet Union as a "good thing" from capitalism, which shows how uncritical Marxists have always been in this respect. We had to wait one hundred and ten years for one of them, Harry Braverman, to perform the first critique, and even this one is marred by false problems such as "do white collar workers produce surplus value?") As long as we call this system "Fordism", are we not concealing its real sources? Marxists at this point like to mention Adam's Smith's pin factory as example of a civilian use of discipline prior to rifle manufacturing, but for every example they use I can find an earlier military one, such as the Venetian arsenal which by the fifteenth century was already

the largest military-industrial complex of its time.

Protevi: Just a few additional points. As Manuel himself pointed out in his *War* book (1991, p. 63), Virilio's concept of "military proletarianization" shows that it's impossible to draw clean lines around the use of force, coercion, duress, persuasion, etc. by military, police, and "social welfare" (either governmental, private, or "faith-based") organizations to herd people into disciplinary institutions. The situation would be even more complex considering the disciplinary situation in the Caribbean sugar plantations Sidney Mintz examines in *Sweetness and Power*, for there you would have to factor in the intricate assemblages in the Atlantic slave trade, which combined State and private enterprise factors in varying proportions. We would also have to investigate the way patriarchal systems have provided the means to "pre-discipline" girls in family settings well before they become workers in sweatshops.

THE FUTURE

Thanem: Are these concerns reflected in your current research or will they be in your future research? Indeed, what projects are you currently working on and what research plans do you have for the foreseeable future? And what forthcoming appearances and publications by Manuel DeLanda and John Protevi respectively should philosophers and non-philosophers watch out for?

Protevi: My next work, after the geophilosophy book, will be *The Act of Killing: An Essay in Political Physiology*, a study of the warrior vs. soldier figures in Western culture, from Achilles vs. Agamemnon through Patton vs. Eisenhower, Schwarzkopf vs. Powell, and so on. I'll be trying to articulate the work in the neurophysiology of emotion by Antonio Damasio (*Descartes' Error*; *The Feeling of What Happens*) and Joseph LeDoux (*The Emotional Brain*; *The Synaptic Self*) with various analyzes of military corporeal technology, as in the work of David Grossman (*On Killing*) and William McNeill (*Keeping Together in Time*). I've put an essay on the Columbine High School massacre that sets out what I think are the principles of "political physiology" at www.artsci.lsu.edu/fai/Faculty/Professors/Protevi.

DeLanda: The issues raised above are not really part of my current research. I am currently writing a book on social ontology titled *Nonlinear Dynamics and Social Complexity* where many of the issues we have discussed here will be treated in depth. It should be finished by the end of the year at which point I must pick up the fight I have been waging against Science Studies (Latour, Bloor, Pickering) and work on a book about history and philosophy of science.

Thanem: This sounds exciting, and I think both of you should expect to meet an attentive audience in organization theory, given its recent concern with issues of embodiment and (perhaps ironically) given that its study of technology has involved considerable – if not particularly critical – engagement with Actor-Network Theory and Science Studies.

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