MODALITIES AND COUNTERFACTUALS IN HISTORY AND THE SOCIAL SCIENCES: SOME PRELIMINARY REFLECTIONS

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1. *Introduction*

Ronald Reagan was an early front-runner for the part of Rick Blaine in "Casablanca". If he, instead of Humphrey Bogart, had gotten the part, he could have ended his first inaugural address with a line from his most famous movie: "I think this is the beginning of a beautiful friendship". And if Paulette Goddard had beaten out Vivien Leigh in the tight race for the lead in "Gone With the Wind", she would – one presumes – be more famous today as Scarlett O'Hara than as one of Charlie Chaplin's wives. Not only the popular press revels in might-have-been stories (Sabulis, 1989); historians indulge in them as well. If Henry IV (who was murdered in 1610), rather than Louis XVI, had been the king of France at the time of the French Revolution, François Furet speculates, there would have been no religious conflict. Rudolf Augstein, for one, disapproves vigorously. To argue in terms of "had" and "would" is sheer frivolity ("*Windbeutelei*"), he thinks (Augstein, 1989).

There is more involved in this disagreement than French caprice. Counterfactual statements have always made most historians feel uneasy. After all, aren't counterfactuals untestable in principle?[1] One would like to be able to proceed without resorting to them, not unlike Laplace who felt he didn't have to invoke God in order to understand the universe ("*je n'avais pas besoin de cette hypothèse*"). But then, *can* the historian dispense with counterfactual thinking altogether? Elster (1978, 1983) rightly urges us to distinguish what (most) historians profess from what they are actually doing. Even the most painstaking of historians, when ranking the various causes of a given event according to their perceived importance or singling out the 'major' cause(s) of an event, relies at least implicitly on counterfactual thought experiments, since he has to ponder over

questions like, "What would have happened in the absence of cause X?", etc. The difference between, say, the political historian who, like Augstein, intends to 'stick to the facts' and the economic historian who reconstructs whole counterfactual economies (e.g. Fogel, 1964) *in practice* turns out to be a difference in degree rather than in kind.

It is not just that Elster argues that counterfactual arguments are of paramount importance in history and the social sciences. He goes on to consider the essential use of counterfactual reasoning as a criterion to demarcate (causal) explanation in social science from its equivalent in natural science, where, he believes, one doesn't find "thought experiments on this gigantic scale" (Elster in Callebaut, 1990).[2]

Ascertaining this *empirical* claim of Elster's is not our purpose here, but several philosophical remarks are in order (section 2). They will provide us with the necessary background to delineate, in sections 3 (on the relation between the 'new' problem of contingency and the 'old' problem of social order) and 4 (preview of the papers in this issue), the quite limited number of themes at stake in this issue from the already immense cluster of logical, epistemological, ontological and methodological problems and issues related to modality and counterfactual reasoning in (social) science and history.

2. *Four points concerning counterfactuals and explanation in social natural science*

First, Elster's distinction between natural and social science in terms of the different roles counterfactuals play in the explanations in these respective domains is *not* correlated with the quite fashionable idea that what distinguishes natural from social science are different modes of explanation, say causal explanation vs. non-causal explanation in terms of meaning, respectively (as claimed in Blom and Nijhuis' paper in this issue). To be sure, Elster actually claims that it makes sense to assign different modes of explanation to different realms of science; roughly, causal, functional and intentional explanation would be typical of physics, (evolutionary) biology and social science, respectively. There *is*, then, a mode of explanation – intentional explanation – that is unique to the social sciences on Elster's view; but he goes on to argue that rather than ruling out causal explanations, intentional explanations *supplement* "sub-intentional" and "supra-intentional" causal explanations in social science (Elster, 1983).

Second, it may be asked – for instance, by someone disa-greeing with Elster's insistence that functional explanations have little role to play in social science[3] – if and how counterfactual reasoning and functional explanation can go together. When functions are considered to be necessary conditions, the preva-lence of functional equivalents can be invoked to argue against the soundness of functional explanation. Explanation of why, say, a social group displays a certain behavior *as opposed to another behavior, which would be equally adaptive (etc.)*,[4] cannot be given in terms of the functions of those behaviors (cf. Horan, 1989, concerning functional explanations in sociobiology). A functional explanation cannot decide between functional equiva-lents. The way to solve this problem would seem to consist in supplementing the functional explanation with explanations of historical – and additionally, in the case of biology, of develop-mental and physiological – constraints (Hull, 1989).[5]

This conclusion leads directly to our third point – the tricky issue of possible ontological differences between the domains of natural and social science, respectively. Here, Herbert Simon's notion of an *artificial system* seems particularly instructive. In *the Sciences of the Artificial*, Simon has observed that

> "... certain phenomena are 'artificial' in a very specific sense: They are as they are only because of a system's being molded, by goals or purposes, to the environment in which it lives. If natural phenomena have an air of 'neces-sity' about them in their subservience to natural law, artificial phenomena have an air of 'contingency' in their malleability by environment." (Simon, 1970, p. ix)

It is the very contingency of artifical phenomena that "has always created doubts as to whether they fall properly within the compass of science" (p. x). Simon and others have argued that criticism directed at the teleological character of artificial systems as such (and the consequent difficulty of disentangling prescription from description) is misguided; teleological func-tional explanations can be reconstructed as non-teleogical *evolu-tionary* explanations (see in particular Wimsatt, 1972, who was inspired by W. Ross Ashby and Donald T. Campbell). The genuine problem, according to Simon, is "to show how empirical proposi-tions can be made at all about systems that, given different circumstances, might be quite other than they are" (Simon, 1970, p. 10). His own strategy has been to invoke constraints – cf. Hull above – imposed by the *bounded rationality* of social man, i.e. to show how "the empirical content of the phenomena, the necessi-

ties that rise above the contingencies, stems from the inabilities
of the behavioral system to adapt perfectly to its environment –
from the limits or rationality, as I have called them" (ibid.).
Simon, who started out in administration science, emphasizes that
contingency is not just the mark of the social:

> "(...) I thought I began to see in the problem of artificiality
> an explanation of the difficulty that has been experienced
> in filling engineering and other professions with empirical
> and theoretical substance distinct from the substance of
> their supporting sciences. Engineering, medicine, business,
> and painting are concerned not with the necessary but with
> the contingent – not with how things are but with how they
> might be – in short, with design. The possibility of creating
> a science or sciences of design is exactly as great as the
> possibility of creating any science of the artificial. The two
> possibilities stand or fall together." (p. xi)

Simon, of course, omits one important discipline in his enumera-
tion here (this is corrected in Simon, 1983): evolutionary
theory, which explains the products of evolution in terms of
design by a "blind watchmaker" – natural selection (Dawkins,
1986).[6]

Finally, although Elster doesn't refer to them, his down-
grading of the role of counterfactual reasoning in *natural* sci-
ence sits well with the 'atheistic' stance of philosophers such as
Quine and van Fraassen regarding modalities and counter-
factuals.[7] Best known is this respect is, of course, Quine's
rejection of modal logic because of its tie-up with metaphysical
essentialism. (For very accessible accounts of Quine's philosophy
we refer the reader to Gochet [1986] and Gibson [1988]. For a
critical survey of modal logics, see Bradley and Swartz [1979]
and Loux [1979], which focus on the logical and the philosophical
problems respectively. For two alternative vindications – in the
light of Quine's challenge – of objectively based physical neces-
sity, see in particular Fisk [1973] and Apostel [1974].) Quine's
epistemological holism, which is intimately connected with his
naturalistic and behavioristic conception of language, is an
"outright denial" (Gibson) of the idea that to each synthetic
statement "there is associated *a unique range of possible sen-
sory events* such that the occurrence of any of them would add
to the likelihood of truth of the statement, and that there is
associated also another unique range of possible sensory events
whose occurence would detract from that likelihood" (Quine,
quoted in Gibson, 1988, p. 14; italics ours). To our knowledge,

Harré (1986) is the most sophisticated attempt to date to deal with Quine's challenge, in terms of a distinction between three distinct scientific enterprises – the science of objects of common experience (e.g., biological taxonomy as based on actually observed phenotypic characters), the science of objects of possible experience (e.g. social science as understood here), and the science that purports to describe objects beyond all possible experience (in particular, advanced physics).

Quine's target, in the above quote from his "Two Dogmas of Empiricism" (originally written in 1951), was modern empiricism. But the most authoritative among contemporary empiricists, the "constructive empiricist" Bas van Fraassen, although he does not shun the use of powerful logical tools, including modal logics, when studying the *pragmatics* of science,[8] is as close to atheism with respect to modality as Quine himself when he insists that "[t]he locus of possibility is the model, not a reality behind the phenomena" (van Fraassen, 1980, p. 202). Again, the realist disagrees. According to him, modality is not just important for *pragmatic* reasons[9] but ultimately because of its close connection with *causality*. "One way of understanding at least some aspects of causality", he maintains, "is to regard the modal structure of scientific models as representing a causal structure in real systems" (Giere, 1988, p. 99). The "modal realist" Giere takes it that, "in some cases at least, a causal counterpart of the modal structure of a scientific model may exist in nature" (ibid.).

After these considerations about counterfactuals and explanation in general, we now turn to the more specific issue of contingency assumptions in social science.

3. Contingency and the problem of social order

Social reality is traditionally conceived as the *negation of the contingency* that somehow inheres naturally in human action, i.e. as an order overcoming the "double contingency" (Parsons) which threatens every interaction, as Ego and Alter cannot look inside each others' heads. What if the problem of order (Hobbes), were *not* the basic problem of social science? Imagine, as a thought experiment, that not "double contingency" but the question how contingency is maintained and reproduced in everyday social life would be constitutive. Suppose contingency to be crucial in the following sense: that to conceive or experience something as "social" is to conceive it as existing in a merely possible, non–necessary way. Social reality would then be intrinsically contingent. Thus perceived, social actions, communi-

cations, institutions, social structures, and what have you, are actually contingent realizations of possibilities. What conse- quences does this radical supposition have? How would this Gestalt switch affect social science and history? Devastating it seems indeed on all levels traditionally deemed important by the philosophy of science.

To begin with, one faces all the problems (re)activated by the possible words interpretation of modal discourse. Recall, for example, Quine's complaint about the possible fat man in the doorway trying to shove away the possible bald man in the doorway:

> "Are they the same possible man, or two possible men? How do we decide? How many possible men are there in that doorway? Are there more possible thin ones than fat ones? How many of them are alike? Or would their being alike make them one? Are no two possible things alike? Is this the same as saying that it is impossible for two things to be alike?" (Quine, 1980, p. 4)

What Quine ludiciously calls to attention is, of course, the problem of identity which accompanies all talk of "possible being", i.e. ontological or *de re* modality. To be a thing, we normally assume, is to be a "something", is to have characteristic qualities and features distinguishing it from other things. Diffe- rences and identities seem naturally implied by the concept of 'being an entity' itself. But what if a thing is what it is just accidentally, if its characteristic qualities and features could have been otherwise? What is characteristic about a quality which is not necessary for a thing to remain the same? What counts for the identity of an entity if not a set of necessary qualities? And how are we to identify entities if a certain constancy of properties is not guaranteed? For example, how are we to identify an action as a political action if political actions do not exhibit certain specific features? 'Possible being' seems to definitely block the operationalization of our concepts and theo- ries. All methodological devices, ingenious as they may be, would seem to become obsolete in a world in which things are what they are in a purely contingent way.

Trying to avoid these nasty consequences, one might argue that this interpretation of 'possible being' focusses on properties exclusively.[10] Another reading of 'possible being' could be sug- gested, according to which this phrase refers to contingency of existence, to the idea that nothing in our social reality is necessary 'out there'. However, this solution does not turn out

much better. How could we explain the recurrence of action patterns or the endurance of social institutions if there were no coercion whatsoever to do the things we do, if functional exigencies do not enforce some constraints in the organization of our social life, etc.? The whole idea of social structure, of an enduring order in the web of social relations rests on the assumption of enduring stability which in some way or another is recognized as an essential, non-contingent feature of social reality (cf. Simon above). We are well acquainted with the idea that our world is an ordered world, a world which can only endure as long as its elements keep their places, remain different from each other. Not everything is possible. Some lasting constraints and structures must exist if we are to have an experienceable and knowable reality at all. Of course this holds also for our social reality. We have the *intuition* that there exists something like a social order, that societal life exhibits constraints not easily surpassed.

The bulk of social science, as it has been handed down to us, is not well-equiped to deal with contingency. Time and again, the systematic search for relations and relational order has led to a treatment of the contingent aspects of social life as a 'negative' or 'residual' problem. Of course, to depart radically from the opposite direction – to posit contingency as the *alpha* and *omega* of social science – is to embark on an artificial and rather sterile project. The true problem is how to develop models and methods which can integrate contingency, understood as a positive factor of social reality, with notions of social order.

4. *Preview*

In the first paper, "Why Do Social Scientists Tend to See the World as Over-ordered?", Raymond Boudon sets out to explain the persistent tendency of social scientists to ascribe more regularity to societal phenomena than is warranted. He insists on going beyond Popper's well-known diagnosis ("science is essentially incompatible with the very notion of *unconditional* laws") in pointing to the ubiquity of *conditional* lawlike statements in social-scientific discourse. Boudon identifies the *a priori* aspects of any theory or model as a major source of overorderliness. These *a prioris* may induce the social scientist to consider as genuine consequences of his *explicit* theory or model statements that are actually only derivable from its *implicit* part. Boudon illustrates this very convincingly in the case of the neo-Marxist theory of semi-feudal relationships between landowners and

tenants. In addition to *a prioris* of the "Kantian" variety – those appearing "naturally" in *all* human think processes –, Boudon also analyzes the biases in theory building associated with "Kuhnian" *a prioris* that are due to the circumstance of belonging to a specific scientific culture. For instance, sociologists working in the positivistic tradition, which downgrades the subjectivity and motivations of the actors, are less likely to recognize (objective) contingency than their colleagues of an actionist persuasion; yet recognizing contingency "is as crucial to see the world as it is – i.e., to be scientific – as to recognize determinism".

Tannelie Blom and Ton Nijhuis ("Contingency, Meaning and History") aim to outline the foundation of an alternative theory of historical explanation, one which departs from extant approaches in that it takes seriously the contingent character of the historical process. To accomplish this, they offer an account of meaning which allows them to reconstruct the deep structure of historical narratives as well as to reinterpret traditional criteria for the adequacy of narratives such as explanatory power, scope, originality, and plausibility. They also suggest a new criterion: conditionality.

Steven Rappaport ("The Modal View of Economic Models") compares the modal view of economic models to the "lawlike generalization" and the structuralist views of economic models in order to show the superiority of the former. On the modal view, a model consists of a first set of definitions and/or non–definitional assumptions, and a second set of *hypothetical* objects whose behavior is described by the first and its deductive consequences. On Rappaport's interpretation of the modal view, (only) theoretical models are about hypothetical objects; they include (*un*conditionally) *true* statements about them. On the other hand, only applied models contain true or false statements about the *real* world. Contrary to what some of its critics maintain, this view is not committed, then, to the *existence* of hypothetical or possible entities of any kind. On the lawlike generalization view, a model is a deductively organized system of interrelated lawlike (true or false) general statements about *real world* objects. As a consequence, the distinction between theoretical and applied models collapses here, which does not fit well the fact that economics includes nondescriptive models. The structuralist or semantic view of theories associates a model with the definition of a predicate P. A *realization* for a model is an item which is denoted by this predicate. The model's set of *intended applications* comprises the real world items X which the proponents of the model want to be realizations of the model. "X

is a P" is an *empirical* hypothesis. Rappaport argues that whatever advantages have been claimed for the structuralist view, such as making sense of the "empirical immunity" of economic theory or understanding theoretical model building in economics (modeling involving perfect rationality, perfect information and perfect competition), are advantages of the rational reconstructions on the modal view as well. In addition, he claims, the latter is a much better description of the practice of economists than either of its two rivals.

In "Scientific Explanation, Necessity and Contingency", Erik Weber uses certain results from contemporary theories of scientific explanation to argue against the widely accepted view according to which science is a system of natural or social laws expressing relations of physical or social necessity between events. Neither on Hempel's nor on Salmon's account to explanation is the *contingency-necessity* opposition essential. Weber shows that the general image of science derivable from Hempel's influential DN–IS account can be stated in terms of deontic and, in a subordinate role, epistemic modalities; it is that of a system of 'imperatives of expectation'. On Hempel's '*epistemic*' account of explanation, there is thus no immediate link between explanation and physical necessity. Neither is there on the major alternative to Hempel's view of explanation, Salmon's *causal* account, which Weber takes to be superior to the former in various respects. For here, the relation of *attainability* is crucial; and the 'paraconsistency' of this relation makes it incompatible with the necessity/contingency image of science. According to Weber, this should lead us to abandon – *pace* the realist Salmon – the 'realistic' general image of science in favor of a more instrumentalistic one. With respect to the social sciences, Weber joins Boudon in urging that the determinism that is traditionally associated with historical-materialist and functional social science explanations be given up.

In game theory, common knowledge assumptions are standard. For a group of individuals to have common knowledge that *p* means that everybody knows that *p* is true, that everybody knows it, etc. In normal form games, players are endowed with common knowledge of the rules and of their respective preferences and beliefs about exogenous uncertainty as well as about the other players' choices and beliefs. Are common knowledge assumptions also needed in extensive form games, where the succession of choices available to each player, the information each player has when it is his turn to move, and eventual payoffs to each player are indicated? In the paper which concludes this issue, "Counterfactuals and Backward Induction",

Cristina Bicchieri considers finite, extensive form non–cooperative games of perfect information, in which the (unique) classical equilibrium solution is obtained by backward induction. The *theory* of the game here consists in (i) assumptions about the players' rationality and their beliefs about each other's rationality, (ii) a specification of the structure of the game and (iii) of the players' strategies and payoffs, and (iv) the hypothesis that (ii) and (iii) are common knowledge. Bicchieri argues that common knowledge of the theory makes the theory inconsistent. She goes on to show that a rich enough theory of the game – one including a model of belief revision in various hypothetical situations – can contain both an assumption of rationality and a common knowledge assumption.

NOTES

1. A realist philosopher of science like Salmon, who wants to make a case for the indispensability of counterfactuals in science, invokes the experimental approach as "a direct way of dealing with (...) counterfactual assertations" in natural science: "The result of the experiment establishes some counterfactual statements as true and others as false under well–specified conditions." (Salmon, 1984, pp. 149–50). In the social realm, the possibilities for experimentation are, of course, severely limited in comparison (but see Cook and Campbell, 1979); whereas in history, only thought experiments – with uncontrollable outcomes – would seem possible.

2. Another overriding criterion he takes to be "social hysteresis", i.e. the pragmatic necessity for social scientists to invoke the quite distant past to explain the present.

3. At least one influential philosopher of science has recently argued that there is room for functional explanations at the sub–biological, viz. the physico–chemical level (Rosenberg, 1985).

4. As Elster (1983) and others have observed, functionalists in social science cannot rely on one single equivalent of the *maximand* of their colleagues in biology: fitness.

5. Thus, Elster's two major criteria to demarcate social science from natural science – heavy reliance on counterfactual reasoning, essential historicity – turn out to be more intimately related than he seems aware.

6. A prerequisite for the possibility of *nomological* evolutionary explanations of social behavior is the development of a *comparative* social theory, viz. "the deduction of principles

that define the evolution of social life in intelligent, culture-transmitting species wherever they might occur" (Wilson, 1980, p. 62); cf. Rosenberg (1980).

7.   Elster could have invoked these authors in turn to update his own Humean-agnostic analysis of causality.

8.   Cf. van Fraassen, 1980, p. 201f. ("Modality without Metaphysics"). As Hooker (1987, pp. 164-5) nicely puts it, van Fraassen's feat is "to deflect many of the sharpest criticisms from realists by simply incorporating them into his own position"; thus he is able "to cut the boundary of empiricist doctrine more finely, closer to the core yet consistently defensible while consigning all of the new leftovers to non-cognitive pragmatics".

9.   Cf., on this score, Anthony Flew: "The very idea of counterfactual conditionality is (...) by a logical necessary connection linked with those of practical necessity and practical impossibility. It is, surely, obvious that and why this is an idea which is easy for us to grasp? We are, after all, agents familiar as such with the ever-present possibility and power of doing other than we do do. Suppose we were instead creatures who were not agents possessed of personal powers and who were, therefore, never confronted with actual alternative possibilities? How could we then become seized of the concept of counterfactual conditionality; or, indeed, of any other concept whatsoever? (Does not the possibility of any correct verbal usage itself presuppose the possibility of the incorrect?)" (Flew in Flew and Vesey, 1987, p. 135).

10. Cf. Kripke's critique in *Identity and Necessity* (Kripke, 1980) of a purely qualitative interpretation of possible worlds.

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