Kinds of Scientific Rationalism: The Case for Methodological Liberalism

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Abstract:

The present paper considers the implications of the postulate that the activities of scientists constitute complex phenomena in the sense associated with the methodological writings of the Nobel Prize-winning Austrian economist, methodologist, and political philosopher, F.A. Hayek. Although Hayek wrote extensively on the methodology of sciences that investigate systems of complex phenomena, he never addressed the possibility that science itself is such a system. The application of Hayek's methodology of sciences of complex phenomena to science itself implies some minimal criteria for explanations of scientific rationality. If science is complex in Hayek's sense, then scientific belief may be rational in more than one way. It is argued that a failure to recognize the possibility of multiple kinds of scientific rationality contributes to an error theory of certain unsuccessful accounts of scientific belief in the history of philosophy of science. It is further argued that, where *ecological rationality* is operative, rational belief requires an element of methodological liberty. It is shown that acceptance of the possibility of ecologically-rational scientific outcomes – a view here dubbed *methodological liberalism* – is closely related to Hayek's denial of the possibility of a successful *scientism*, a denial crucial to his arguments against socialism and Keynesian macroeconomics.

> There seems to me to exist a sort of rationalism which, by not recognizing [the] limits of the powers of individual reason, in fact tends to make human reason a less effective instrument than it could be…[T]he best name for this kind of naïve rationalism is rationalist constructivism. It is a view which in the social sphere has…wrought

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> immeasurable harm [.] Rationalism in this sense is the doctrine which assumes that…we have it in our power so to shape our institutions that of all possible sets of results that which we prefer to all others will be realized; and that our reason should never resort to automatic or mechanical devices when conscious consideration of all factors would make preferable an outcome different from that of the spontaneous process.
>
> –F.A. Hayek, "Kinds of Rationalism" ([1964a] 2014, 41–42)

1. Introduction

The present paper considers the implications of the postulate that the activities of scientists (*qua* scientists) constitute *complex phenomena* in the sense associated with the methodological writings of the Nobel Prize-winning Austrian economist,[1] methodologist, and political philosopher, F.A. Hayek. Although Hayek wrote extensively on the methodology of sciences that investigate systems of complex phenomena, he never explicitly addressed the possibility that science itself is such a system.[2] An analysis of science's possible complexity, such as occupies the first two parts of the present paper, leads to a number of interesting conclusions.

[1] Hayek was Austrian both by birth and by economic-scientific predilection. That is, he was both born in Austria (Vienna, to be exact) and a prominent member of the Austrian *School* of economics.

[2] However, a number of modern Hayekians have developed a model of science as an "adaptive classifying system" (see McQuade and Butos (2005), McQuade (2007), McQuade (2010)). The reader interested in such a model is referred to these essays. The present paper is not concerned with the development and defense of a particular model of the phenomena of scientific activity as complex, but with the general implications for methodology of science's possible complexity. These authors explain science on the model of a particular kind of emergent order, namely, Hayek's (1952) model of an organism's "sensory order," while I consider science from the perspective of Hayek's more general theory of complex phenomena. Their analysis entails mine, but not the other way around: the phenomena of scientific activity cannot be like the sensory order without being complex, but may be complex without being like the sensory order.

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First, it is argued that the application of Hayek's methodology of sciences of complex phenomena to science itself implies some minimal criteria for explanations of science, i.e., a meta-methodology.[3] For Hayek, complex phenomena are those with respect to which our knowledge is necessarily deficient: we lack either an adequate theory of the phenomena or data sufficient to generate precise explanations and predictions (these being two sides of the same coin on Hayek's epistemology) of events in the relevant domain. Thus, if science is complex in Hayek's sense, then our ability to explain science is necessarily limited. In particular, scientific beliefs – or, at least, some scientific beliefs – might be rational, yet we would be unable to fully account for this rationality. To adopt a locution of one of Hayek's followers, Vernon Smith (also a Nobel Laureate in Economics), such beliefs would then be rational in an "ecological" sense[4] and no methodology would be tenable that failed to recognize the possibility of ecologically-rational scientific beliefs.

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[3] In this respect, the current essay bears some resemblance to Butos and Koppl (2003) and McQuade (2010). However, the argument of the present essay is sufficiently unique to warrant a distinct treatment. It should also be mentioned that the meta-methodological implications of a model of scientific phenomena as complex in Hayek's sense are not dissimilar to those of other treatments of science as an evolutionary process (such as, e.g., Toulmin (1981), Popper (1984), and Hull (1988)).

[4] Smith's (2008, 2) discussion of the distinction between the *constructivist* and *ecological* varieties of rationality is worth quoting at length:

> *Constructivist rationality*, applied to individuals or organizations, involves the deliberate use of reason to analyze and prescribe actions judged to be better than alternative feasible actions that might be chosen. When applied to institutions, constructivism involves the deliberate design of rule systems to achieve desirable performance…
>
> *Ecological rationality* refers to emergent orders in the form of practices, norms, and evolving institutional rules governing action by individuals that are part of our cultural and biological heritage and are created by human interactions, but not by conscious design.
>
> The two concepts are not inherently in opposition; the issues are emphatically *not* about constructivist *versus* ecological rationality…and in fact the two can and do work together. For example, in evolutionary processes, constructivist cultural innovations can provide variations while ecological fitness processes do the work of selection. [italics in the original]

Like Smith, Hayek ([1964a] 2014, 43) recognized that the kinds of rationality "constantly interact." Butos and Koppl (2003) coin the phrase "processive" rationality to describe the kind of rationality involved in the emergence of complex orders.

Second, that they failed to assume this possibility within *the context of justification*, it is argued, contributes to an explanation of the perceived shortcomings of certain unsuccessful accounts of scientific belief. The history of philosophy of science is, at least in part, the story of the search for an explicit explanation of the scientific method's apparent superior rationality as a mode of belief fixation. Of course, if there are scientific beliefs that are ecologically rational and which, therefore, cannot be fitted into a constructivist framework, then this project was bound to fail. Moreover, those who reacted to the failure of such constructivist philosophies of science by simply denying, against all appearances, the relevant datum of science's superior rationality made the same error of assuming that, if it exists, this rationality must be completely effable. Thus, a Hayekian meta-methodology contributes to an error theory of these failed doctrines, namely, the error of failing to recognize a third possibility other than constructivist rationalism and irrationalism.

Third, it follows that *coercion* cannot be an effective method where scientific beliefs are rational in an ecological sense. And this is so for purely epistemic reasons, without regard to ethical qualms or questions of the incentive structure confronting coerced scientists. If it is not fully understood how to arrive at a rational scientific belief, then, regardless of the coercive techniques adopted, because the coercer doesn't possess the knowledge required to coerce effectively, one cannot be coerced into believing rationally. It further follows that an element of *liberty* is necessary (but, of course, by no means sufficient) for the realization of rational scientific belief in such cases.

Fourth and finally, it is argued that Hayek's well-known arguments against the possibility of an adequate *scientistic* methodology – arguments intimately connected to his criticisms of

both socialism and Keynesian-style aggregate demand management – are closely related to the possibility of ecologically-rational outcomes within the *context of scientific discovery* and, therefore, to the need for a degree of liberty within this context. Scientism, for Hayek, was the view that the social sciences could be (and could only be) *deliberately* reconstructed in order to generate predictions of a degree of specificity and accuracy necessary to facilitate effective socialism and Keynesian demand management. However, Hayek argued, scientistic methodology, not unlike these latter economic-political programs, assumes the sufficiency of constructivism and the impossibility of ecologically-rational social outcomes. But, according to Hayek, the defender of scientism is wrong on both counts. The predictive capacity of the social sciences is constrained by the limits of human cognition with respect to social phenomena. The scientistic program cannot overcome this hurdle. Moreover, if ecologically-rational scientific outcomes are possible, the scientistic prejudice that the conscious application of reason is necessary for the realization of the relevant predictions is misplaced: it is possible that a spontaneously-evolved social science may discover means to predictions of the required degree of specificity without resort to the conscious application of reason. Thus, to deny both tenets of scientism is to recognize both the possibility of ecologically-rational scientific outcomes within the context of discovery and the need for freedom in such cases; it is, in short, to be a methodological liberal. Hayek's implicit arguments for methodological liberalism, which the present paper aims to exposit, are thus intimately connected with his explicit arguments for liberalism in economics and politics.

2. Complex Orders and Their Investigation

2.1. Complex Orders and Complex Social Orders

According to Hayek ([1964b] 2014, 262), complex phenomena are those that consist of a large number of elements the activities of which bear such relations both to each other and to the external environment as to manifest an emergent order that possesses "certain general or abstract features which will recur independently of the particular values of the individual data, so long as the general structure…is preserved."[5] Hayek ([1967a] 2014, 278–279) emphasized the importance of distinguishing between the systems of rules followed by the elements of a particular group and the orders (or "patterns") of activity that may emerge at the group level from the observance of such systems of rules. Given Hayek's ([1967a] 2014, 278) definition of a "rule" as a "statement by which a *regularity* of the conduct of individuals can be described, irrespective of whether such a rule is 'known' to the individuals in any other sense than that they normally act in accordance with it" [italics added], the elements of an order may be of any degree of consciousness. The activities of non-conscious "galaxies, solar systems…and social

[5] However, it should be noted that Hayek's definition of complex phenomena, perhaps strangely, runs in epistemological rather than ontological terms. That is, complex phenomena, according to Hayek ([1964b] 2014, 260–261), are those the *explanation of which requires a model consisting of a large number of variables*.

> [T]here seems to exist a fairly easy and abstract way to measure the degree of complexity of different kinds of abstract patterns. The minimum number of elements of which an instance of the pattern must consist in order to exhibit all the characteristic attributes of the class of patterns in question appears to provide an unambiguous criterion…[W]hen we consider the question from the angle of the minimum number of distinct variables a formula or model must possess in order to reproduce the characteristic patterns of structures of different fields (or to exhibit the general laws which these structures obey), the increasing complexity as we proceed from the inanimate to the ('more highly organized') animate and social phenomena becomes fairly obvious.

Stated another way, our knowledge of complex orders is limited *by definition*. Systems of complex phenomena are just those with respect to which our knowledge is limited and the *degree* of complexity of some phenomena is just a reflection of the extent of this limitation in the particular case.

orders"[6] as well as those of conscious organisms[7] (Hayek [1967a] 2014, 286) cannot be fully reduced to the properties of their elements, but rather emerge from the regular interactions both between various proper subsets of their parts and between the (proper and improper) subsets of their parts and the environment. Social orders are those unique orders the elements of which are conscious organisms.

The rules of conduct the observance of which will, under appropriate circumstances, give rise to an emergent social order may be transmitted either genetically or culturally (Hayek [1967a] 2014, 278). Among order-generating, culturally-transmitted, systems of rules Hayek distinguished between those that are deliberately designed and imposed, and those that evolve in an undesigned process over a (typically long) period of real historical time. Hayek denoted designed orders produced by imposed systems of rules as "organizations" (Hayek [1967a] 2014, 278n) and undesigned orders that emerge from evolved systems of rules as "spontaneous" (Hayek [1967a] 2014, 286).[8] The social orders that we observe in practice are typically products of mixtures of genetically-transmitted and culturally-transmitted – some imposed and some evolved – systems of rules.[9]

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[6] "[S]patial pattern[s] such as will occur in the marching, defence, or hunting of a group of animals or men. The arrow formation of migrating wild geese, the defensive ring of the buffaloes, or the manner in which lionesses drive the prey towards the male for the kill, are simple instances in which it is presumably not an awareness of the overall pattern by the individual but some rules of how to respond to the immediate environment which co-ordinate the actions of the several individuals. More instructive are the abstract and more complex orders based on a division of labor which we find in such insect societies as those of bees, ants, and termites." (Hayek [1967a] 2014, 281)

[7] The mind of a conscious organism (i.e., its "sensory order") is, according to Hayek (1952, 16), "a particular order of a set of [neural] events taking place in some organism and in some manner related to but not identical with, the physical order of events in the environment."

[8] The "chiefly negative (or prohibitory) rules of conduct which make possible the formation of social order are of three kinds…(1) rules that are merely observed in fact but have never been stated in words[;]…(2) rules that, though they have been stated in words, still merely express approximately what has long before been generally observed in action; and (3) rules that have been deliberately introduced and therefore necessarily exist as words set out in sentences" (Hayek [1970] 2014, 343–344).

[9] "[T]he degree to which outcomes are unintended is a continuum" (Schmidtz 2012).

It is not the case that the observance of just any system of rules will give rise to an emergent social order. Whether and to what extent an order emerges from conduct guided by a system of rules depends on the degree to which this system is fitted to the environmental circumstances in which the members of the relevant group act (Hayek [1967a] 2014, 280). A system that includes the rule "murder all newborns" is probably not conducive to the emergence of a social order under any circumstances, while some systems of rules may promote the emergence of an order, say, under peaceful conditions but not under conditions of war, or in a free society but not in a totalitarian dictatorship, or in some technological circumstances and not in others, or in good weather but not in bad, etc. Social *disorder* is thus a consequence of either an absence of rules that guide the activities of individuals or the inappropriateness of prevailing systems of rules of conduct in the particular environment in which members of the relevant group act (Hayek [1967a] 2014, 279–280).

Moreover, social order is a matter of degree, i.e., a question of the extent to which the system of rules governing the activities of the members of a group is adapted to prevailing circumstances, and will change over time with changes in the environment (Hayek [1968] 2014, 308–309). The survival of a group of organisms depends on the orderliness of its society and, thus, requires that individual members adhere to rules of conduct that are fitted to the prevailing environmental circumstances. Given that these latter conditions are in constant (albeit more or less gradual) flux, the persistence of a social order (and, thus, of its members) requires continual adaptation of the system of rules that guide the activities of its members (Hayek [1967a] 2014, 280, 282).

A change ("mutation") in the rules according to which the members of a group act is *progressive* if and only if it contributes to an increase in the group's numbers across time (Hayek [1970] 2014, 345; also [1967a] 2014, 288). This might mean that individuals join the group who were outside of it prior to the mutation or that the mutation leads to more members being born into the group (and surviving) than pass out of the group via death or attrition.

Although Hayek was not specific on this point, the definition of a progressive change in the rules that give rise to a social order can be (and perhaps should be) relativized to the growth of the population of which the group in question may merely be a proper subset. We might hesitate to describe some rule change as progressive if, despite the fact that it increases the numbers of its subscribers, the numbers of the broader population increase more than those of the group over the relevant time span. We can, then, in like fashion, define a *static* rule change as one that merely maintains the group's numbers in a constant ratio with those of the broader population and a *regressive* mutation as one in which the proportion of group members shrinks relative to the population.

The behavior of an individual organism is never fully determined by the (social) rules it follows, but always in conjunction with its own internal impulses and external stimuli.[10] Individual members of the same group will thus at any given moment follow different rules as these latter considerations dictate or "because different rules apply to different individuals according to age, sex, status, or some particular state in which each individual finds itself at the moment…[R]ules of conduct will always act only as a restraint on actions induced by other

[10] The parenthetical "social" is necessary because, if Hayek's (1952) cognitive psychology is sound, then an organism's internal impulses emerge from the rules which, together with external stimuli, determine its sensory order. An organism's behavior would thus be the results of the interaction of external stimuli and systems of rules on multiple levels.

causes" (Hayek [1967a] 2014, 280). Whatever the cause of some particular action of an individual member of a group, that it is part of a system of actions that constitute an order will be indicated by the cessation of the motivating force of the initial cause of the activity (Hayek [1967a] 2014, 280). In other words, whatever craving or discomfort might lead an individual to act in particular circumstances, if she lives in an orderly society and behaves in accordance with the system of rules of conduct from which that social order emerges, her actions and those "of the different individuals will be so coordinated, or mutually adjusted to each other, that the result of their actions will remove" the motivating uneasiness (Hayek [1967a] 2014, 280). Whatever it is that stimulates my desire for prodigious quantities of sweet pastries and whatever reasons inspire the proprietor of my local *patisserie* to sell sugary bits of baked dough, the fact that we live in a social order that emerges from the observance of certain rules (which neither I nor my local *pâtissier* need know explicitly or be able to state explicitly), facilitates the fulfilment of our respective yearnings.

2.2. The Methodology of Sciences of Complex Phenomena

The scientific investigation of complex orders raises special difficulties. The aim of the relevant sciences is the reconstruction of such orders. The social sciences, for example, aim to show how social orders emerge from the "interplay of the rules of conduct of" individual members of the relevant group "with the actions of other individuals and the external circumstances" (Hayek [1967a] 2014, 283). However, our knowledge of complex emergent orders – or, more exactly, our ability to reconstruct them on the basis of knowledge of the properties of their elements, the

interrelations that obtain between the various proper subsets of these elements, and the external connections of these (proper and improper) subsets, not to mention our knowledge of the relevant data, i.e., our knowledge of the values the theoretical variables assume at any particular time and place – is necessarily limited compared to what we can know about systems explicable in terms of models consisting of relatively few variables which, for the purposes of analysis, can be treated as closed with regard to the environment (Hayek [1964b] 2014, 261–262). It is the defining methodological character of such simple phenomena that "it will generally be possible to specify all those aspects of the phenomenon in which we are interested with any degree of precision which we may need for our purposes" (Hayek [1955] 2014, 200), but this is not the case with respect to complex structures that emerge from the interactions of a large number of variables with their environment. Here it is typically the case that either our theory of these interactions is partial and vague, i.e., "more remote from reality—requiring much more additional knowledge before [it] can be applied to particular instances" (Hayek [1956] 1967, 123), or our ability to acquire the data necessary for a more complete explanation of relevant events is deficient (Hayek [1945] 2014, 95–100).

Stated differently, for Hayek, knowledge is of two kinds: theoretical knowledge of general rules (and their internal and external interactions) and empirical knowledge of "particular circumstances of time and place" (Hayek [1945] 2014, 95–96), and because of the multiplicity and structural complexity of the relations obtaining between the former, and because the latter are both equally abundant / complex and often exist "only as the dispersed bits of incomplete and frequently contradictory knowledge which all the separate individuals possess" (Hayek [1945] 2014, 93) – knowledge which will often be held only tacitly by those who act upon it (Hayek

[1963a] 2014, 233–234)[11] – the scientist of complex phenomena cannot construct explanatory models that approach the comprehensiveness of explanations of systems of simpler phenomena.

Explanations and predictions are always a matter of "degree," i.e., "they will always state only some and never all the properties of any particular phenomenon to which they refer[,] [and] each property stated will be expressed not as a unique value or magnitude but as a range, however narrow, within which the property will fall" (Hayek [1955] 2014, 201). That is, explanations and predictions prohibit the occurrence of events that fall outside a particular range, and are undermined, if not falsified, by the appearance of events they forbid. This is true even in sciences that deal with comparatively simple phenomena (Hayek [1955] 2014, 201) but, because of our cognitive limitations, our efforts to reconstruct complex orders via theoretical models necessarily represent less of the relevant phenomena than our models of simpler phenomena (Hayek [1955] 2014, 209–212),[12] and thus, the best explanations and predictions that we can manage with respect to the former are necessarily of lesser "degree" than our best explanations of simpler phenomena, i.e., the former prohibit a smaller range of phenomena than the latter.[13]

[11] Hayek emphasized the crucial role in the emergence of social orders of what Polanyi (1966) called "tacit" knowledge and what Ryle (1946) called "knowledge how" as opposed to "knowledge that"—that is, in Hayek's (1952, 19) words, knowledge with regard to which we may not be "explicitly aware," but which we "merely manifest…in the discriminations which we perform."

[12] Again, this is an implication of Hayek's definition of complex phenomena in terms of the complexity of the model required to explain some phenomena, and does not, by itself, imply that there *are* sciences of complex phenomena. The question whether some (or, indeed, any) discipline qualifies as a science of complex phenomena in Hayek's sense is an empirical one. Hayek included among the sciences of complex phenomena theoretical psychology (1952), economics, linguistics ([1967a] 2014, 283), geology, evolutionary biology, and the branches of astrophysics that investigate the formation of stars and galaxies ([1967a] 2014, 286); as well as "cybernetics, the theory of automata or machines, general systems theory, and perhaps also communications theory" ([1955] 2014, 211).

[13] With respect to complex phenomena, our explanations are of "the principle[s] on which a certain mechanism operates" (Hayek [1964b] 2014, 272) and our predictions are of "patterns" in the relevant events rather than of the specific events themselves (Hayek [1964b] 2014, 263–264).

Moreover, any attempt to model complex phenomena as if they were simple is likely to underwhelm our explanatory ambitions (Hayek [1964b] 2014, 263).[14] Hayek's methodological perspective suggests that a possible reason for the failure of a particular explanation is that the relevant phenomena is modeled as simple despite requiring a complex model, i.e., that the particular model (or, more precisely, the assumed meta-model—the model of an appropriate model) is ill-suited to an explanation of the relevant phenomena.

For the social scientist, it may be either that the order itself is more readily accessible to observation than the rules from which it emerges or *vice versa*. That markets (Hayek [1964b] 2014, 269–271), natural languages (Hayek [1963a] 2014, 232), and perhaps science itself exhibit certain regular patterns in the activities of their constituents is probably easier to recognize than the rules the observance of which give rise to these patterns. For Hayek, then, the business of the economist and linguist (and, perhaps, the social scientist of science) is the genesis and testing of hypothesized systems of evolved rules of conduct obedience to which could give rise to the observed order under the relevant environmental circumstances.[15] On the other hand, with respect to primitive societies, "it is often easier to ascertain the rules of individual conduct than to trace from them the resulting overall and often highly abstract order. The individuals will often themselves be able to tell us what they regard as appropriate action in different circumstances, though they may be able to do this only for particular instances but not to articulate the rules in accordance with which they act" (Hayek [1967a] 2014, 281–282). The

[14] "[A] simple theory of phenomena which are in their nature complex [i.e., which can be adequately explained only on the basis of a complex model]…is probably merely of necessity false—at least without a specified *ceteris paribus* assumption, after the full statement of which the theory would no longer be simple" (Hayek [1964b] 2014, 263).
[15] Such a hypothesized system of rules of conduct might be tested "by seeking for yet unobserved consequences which follow from it, and by asking whether all [orders of the kind the system is postulated to explain] which we find can be accounted for" by it (Hayek [1967a] 2014, 287).

anthropologist's task is thus to refine her subjects' statements regarding proper action in various circumstances into statements of general rules of conduct and to reconstruct "the overall order which is produced by actions in accordance with them" so as to discover "the [social] 'functions'[16] which these rules serve" (Hayek [1967a] 2014, 282).

Hayek ([1964a] 2014, [1967b] 2014) associated systems of complex phenomena with a bifurcation in the nature of philosophical rationalism. In particular, he argued that it was appropriate to adopt the traditional Cartesian – "constructivist" – attitude toward the human mind as a source of knowledge only with respect to those phenomena that could be adequately explained in terms of a deliberately reconstructed model. That is, constructivist rationalism is an appropriate attitude only with respect to simple phenomena. But, with respect to complex phenomena, constructivism presupposes more extensive knowledge than the human mind can possess (Hayek [1964a] 2014, 42–43), and, thus, complex orders can at best be only partially reconstructed. Adopting an attitude of Cartesian constructivism with respect to a complex system – i.e., treating it as amenable to a comprehensive reconstruction – is likely to do more harm than good (Hayek [1964a] 2014, 42).[17] Where the human mind is necessarily limited in the knowledge it can possess and is thus not able to fully reconstruct some phenomena, the appropriate attitude is the "anti-rationalism" of David Hume and the Scottish Enlightenment

[16] Social orders "exist because the elements do what is necessary to secure the persistence of that order. The 'final cause' or 'purpose', the adaptation of the parts to the requirements of the whole, becomes a necessary part of the explanation of why structures of the kind exist…A 'teleological' explanation is thus entirely in order so long as it does not imply design by a maker but merely the recognition that the kind of structure would not have perpetuated itself if it did not act in a manner likely to produce certain effects, and that it has evolved through those prevailing at each stage who did" (Hayek [1967a] 2014, 288).

[17] With respect to constructivist philosophy of science McQuade (2010, 45) writes: "the proposal of prescriptive injunctions appeals to those with a constructivist bent, and in this guise they can be positively damaging if imposed (even with the best of intentions) on an adaptive system from the outside[.]"

philosophers, who "pleaded that an effective use of reason required a proper insight into the limits of the effective use of individual reason" (Hayek [1964a] 2014, 41).[18]

To sum up, constructivism is limited by the requirement that everything relevant to a full explanation of some phenomena be known. But, beyond this limit, where rational outcomes require an ecological element, the attitude of constructivist rationalism is, at best, impotent, and, at worst, deleterious. Within the range of ecological rationality, outcomes are such that *if* everything relevant to a full explanation were known or knowable, the phenomena could be recognized as their necessary consequence, but which may not be so recognized precisely because the relevant facts are not all known and, perhaps, are not all knowable.

3. Science as a Complex Social Order

3.1. Properties of a Complex Scientific Order

Social orders are unique in that their elements – conscious organisms or, more exactly, the *minds* of organisms – are, according to Hayek's (1952) theoretical psychology, themselves complex orders "whose chance to persist depends on (or at least is improved by) their being part of the more comprehensive [social] structure" (Hayek [1967a] 2014, 288).

[18] In harmony with the writings of his friend Karl Popper, Hayek described this latter attitude as one of "critical rationalism" ([1964a] 2014, 53). Subsequent authors (in particular, as mentioned above, Vernon Smith (2008)) have described it as an attitude of "ecological" rationalism. The latter locution is adopted here on the grounds that it better expresses what, for Hayek ([1964a] 2014, 43-44), is the crux of the distinction, in particular, that constructivists take society and its institutions to be the products of human reason – which is thus assumed to be unbounded in its capacity for knowledge and control of these institutions – while the attitude of the ecological rationalist takes human reason to be an (at least partially) evolved product of society's development and thus constrained by this development.

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> "We have to deal here with integration *on at least two different levels*, with on the one hand the more comprehensive order assisting the preservation of ordered structures on the lower level, and, on the other, the kind of order which on the lower level determines the regularities of individual conduct assisting the prospect of the survival of the individual only through its effect on the overall order of the society. This means that the individual with a particular structure and behaviour owes its existence *in this form* to a society of a particular structure, because only within such a society has it been advantageous to develop some of its peculiar characteristics, while the order of society in turn is a result of these regularities of conduct which the individuals have developed in society." (Hayek [1967a] 2014, 288; italics added)

If it is ever the case that the activities of scientists form (to some extent or other) a social order – then it is a consequence of individual members of the scientific community conforming to rules of conduct which, in both their mutual interrelations and external connections with the environment, suffice for the emergence of such an order. Following Hayek ([1967a] 2014, 279), if we define the different kinds of elements of a group in terms of the rules that they obey, then we can define scientists as a group of individuals who adhere to a particular system of rules of conduct in their science-related activities. Likewise, we can define, say, natural scientists (or social scientists) as a group of individuals who in their relevant activities follow a particular system of rules of conduct that overlaps the system of rules of conduct from which the broader scientific order, if it exists at a particular time and place, emerges. This same classification procedure can be applied at the level of the members of particular disciplines within the natural

and social sciences, and, again, at the level of the members of particular research traditions within these disciplines and, yet again, to the members of any sub-traditions within these research traditions. Such a conception implies an image of the relations between systems of rules of conduct that are observed on various levels of scientific activity that might look something like this:

Scientists:	All follow:	$A = R_1, R_2, \ldots, R_a$
Natural Scientists:	All follow:	$B = A, R_{NS1}, R_{NS2}, \ldots, R_{NSb}$
Physicists:	All follow:	$C = B, R_{P1}, R_{P2}, \ldots, R_{Pc}$
String Theorists:	All follow:	$D = C, R_{ST1}, R_{ST2}, \ldots, R_{STd}$
Loop Quantum Gravity Theorists:	All follow:	$E = C, R_{LQG1}, R_{LQG2}, \ldots, R_{LQGe}$
Chemists:	All follow:	$F = B, R_{C1}, R_{C2}, \ldots, R_{Cf}$
Organic Chemists:	All follow:	$G = F, R_{OC1}, R_{OC2}, \ldots R_{OCg}$
Inorganic Chemists:	All follow:	$H = F, R_{IC1}, R_{IC2}, \ldots R_{ICh}$

And so on with regard to other natural-scientific disciplines, research traditions, and yet finer-grained distinctions;

Social Scientists:	All follow:	$I = A, R_{SS1}, R_{SS2}, \ldots, R_{SSi}$
Economists:	All follow:	$J = I, R_{E1}, R_{E2}, \ldots, R_{Ej}$
Austrian Economists:	All follow:	$K = J, R_{AE1}, R_{AE2}, \ldots, R_{AEk}$
Hayekian Austrians:	All follow:	$L = K, R_{HA1}, R_{HA2}, \ldots R_{HAl}$

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Misesian Austrians:	All follow:	$M = K, R_{MA1}, R_{MA2}, \ldots R_{MAm}$
Keynesians:	All follow:	$N = J, R_{K1}, R_{K2}, \ldots, R_{Kn}$
Post-Keynesians:	All follow:	$O = N, R_{PK1}, R_{PK2}, \ldots, R_{PKo}$
New Keynesians:	All follow:	$P = N, R_{NK1}, R_{NK2}, \ldots, R_{NKp}$
Sociologists:	All follow:	$Q = I, R_{S1}, R_{S2}, \ldots, R_{Ss}$
Marxist Sociologists:	All follow:	$R = Q, R_{MS1}, R_{MS2}, \ldots R_{MSr}$
Durkheimian Sociologists:	All follow:	$S = Q, R_{DS1}, R_{DS2}, \ldots, R_{DSs}$

And so on with regard to other social-scientific disciplines, research traditions, and yet finer-grained distinctions.

What this table[19] is meant to illustrate is that *if* there is a scientific order that occupies some district of a broader social order, then it requires there to be a system of rules (A) that is followed by some individual members of society, who we call "scientists." Similarly, the table indicates that *if* there is a natural-scientific order within the broader scientific order, then it requires there to be a system of rules that is followed by some of the scientists, who we call "natural scientists." Given that these natural scientists are scientists, then this latter system encompasses the rules that all scientists follow (i.e., A) and then some. Likewise, the table shows that *if* there is a physical-scientific order within the broader natural-scientific order (and, thus, within the still broader scientific order), then there must be a system of rules that is followed by

[19] It should perhaps be emphasized that the table is offered as a heuristic device and not as a collection of postulates. That is, the table shows one way in which the rules (if they exist) that govern various kinds of scientific activity might be related to each other and should not be construed as a commitment to the claim that they in fact both exist and are so related to each other. The table should not be construed so as to commit the author, e.g., to the claim that there is some system of rules which social scientists follow in virtue of which they are scientists just like their compatriots in the natural sciences.

some natural scientists, who we call "physicists." Again, since these physicists are natural scientists and, therefore, scientists, they share certain rules (i.e., B) with all other natural scientists and, thus, with all other scientists (i.e., A). And so on.

Of course, the mere existence of some such system of rules, while necessary for the presence of a social order at the relevant level of scientific activity, does not suffice for the emergence of a social order at that level; this requires both that the various proper subsets of such a system be appropriately related to each other and that the various (proper and improper) subsets of the system be sufficiently adapted to prevailing environmental circumstances. These latter conditions include those of broader social orders in which the system is embedded as well as those of the non-social (i.e., physical) environment. That is, the emergence of a scientific order requires that the relevant system of rules of conduct (A) be adapted not only to physical circumstances, but also to the system of rules of conduct from which the social order emerges—the rules of scientific conduct must adapt both to social and non-social circumstances.[20]

Something similar can be said about any orders that might emerge at lower levels of scientific activity: i.e., among the circumstances to which a system of rules of conduct must be adapted if it is to give rise to, say, an order in the activities of natural scientists are the rules the observance of which might give rise to an order in the activities of the broader community of scientists. Likewise, among the circumstances to which a system of rules of conduct must be adapted if it is to give rise to an order in the activities of physicists are the rules that give rise to an order in the activities of the broader community of natural scientists; and so on down the

[20] "We view science as a complex order comprised of components (individual scientists)…whose behavior is situated and constrained *within an institutional structure of routines, conventions, and organized groupings that are themselves ordinarily and substantially emergent phenomena of the order itself*" (McQuade and Butos 2005, 346; italics added).

various levels of scientific activity to the members of particular disciplines, research traditions, sub-traditions, etc.

This is to say that if the observance of any such system of rules is so internally and externally well-adapted as to give rise to an emergent social order at the relevant level of scientific activity, we will be dealing with integration between orders on at least *three* levels, i.e., between the comprehensive social order that facilitates the persistence of organisms, the mental order of the individual organism that determines its conduct, and an order on at least one intermediate level—i.e., that of the sub-tradition (or research tradition or scientific discipline, etc.). This intermediate order, too, will facilitate the persistence of the organism *in a particular form* (i.e., *qua* string theorist or *qua* physicist, or *qua* natural scientist, etc.), while, at the same time, both contributing its part to the emergence of the broader social order and being dependent upon the rule-guided conduct of certain individuals. The survival of an order at an intermediate level of scientific activity between that of the individual scientist and that of society requires both adequate membership from the lower level of individuals and adaptation both to the physical environment and to any and all systems of rules that exist at higher levels of activity. In short, if the phenomena of scientific activity are complex in Hayek's sense, then the individual scientist owes her existence *in a particular form* to multiple levels of social orders to whose rules she subscribes, because only within such orders has it proven felicitous to develop her unique characteristics, while a social order at any particular level persists only because both individuals act in accordance with the rules from which it emerges and it is sufficiently well-adapted to its environment, including any other systems of rules of conduct in the vicinity.

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The absence of a pattern in the activities at a particular level of the members of a group of scientists – i.e., disorder at the relevant level – is thus a result either of an absence of a system of rules of conduct or the presence of an ill-adapted system of rules at the relevant level of activity. The extent to which the activities at a particular level of the members of a group of scientists are ordered is a matter of the extent of adaptation to conditions in both the social and more narrowly physical environment of the system of rules that guides the conduct of individual scientists at that level.

The definitions of progressive, static, and regressive rule changes discussed in the previous section are – with certain qualifications – readily applicable to the problem of the orderliness of scientific activity. A progressive (full stop) change in the rules of conduct that give rise to an order in the activities of scientists at some level is one that increases the numbers of the group of scientists conforming to the mutated system of rules. Static and regressive mutations are those that, respectively, maintain and decrease the numbers of the group of scientists conforming to the modified system of rules.[21] More expressive notions of progressivity, stasis, and regressivity follow from considering the consequences of a mutation of a system of rules of scientific conduct in the light of changes in the social environment. In particular, we might say that a mutation of the rules of conduct that gives rise to an order in the activities of a group of scientists is progressive (static or regressive) relative to changes in the memberships of groups of

[21] We might improve upon these mere "counting" notions of progressivity, etc., if we could somehow assign weights to the numbers that reflect the relative degrees to which different systems of rules of scientific conduct are accepted by individual scientists. However, the results of such an extended analysis will ultimately depend on the mathematical figure selected to represent these cognitive measurements and, given the current state of methodological knowledge, there's little reason to suspect that any one of the myriad mathematical figures that might represent these weights actually expresses them more accurately than some other figure. In other words, though it might be desirable to ultimately extend these mere "counting" concepts, such an extension could be based, in the current state of knowledge, on nothing more profound than a pretence of knowledge.

scientists at either the same or higher levels of activity. For example, the rules of conduct of a research tradition might change so as to affect the numbers of its members relative to the numbers of members of either other research traditions within the same discipline, the discipline itself, natural science (social science), or all of science.

Relativizing the notion of progressivity in this way permits more nuanced judgments concerning the condition of an order at some level of scientific activity. For example, a research tradition that is progressing *simpliciter* may or may not be progressing relative to the changes in the membership of some other research tradition within the same discipline. That is, a scientific order at some level of activity may be progressive *simpliciter* (if its membership is expanding) but regressive relative to one or more of its rivals (if the memberships of the latter are growing at a faster rate) or *vice versa*. Similarly, there may be contexts in which judgments are required concerning the progressivity of, say, a research tradition relative to the consequences of changes in the rules of conduct at the higher level of the discipline. That is, a research tradition may be progressive (static or regressive) relative to the progressivity (stasis or regressivity) of the discipline to which it belongs. Indeed, yet more complicated judgments are possible given the conception of science as a complex social order: it may be, for example, that a research tradition is progressive *simpliciter*, regressive relative to at least one other tradition in the same discipline, progressive relative to the discipline itself (because membership in the tradition is increasing at a rate faster than that of the discipline), regressive relative to natural / social science (because membership in the tradition is increasing at a rate slower than that of natural / social science), and progressive relative to science itself (because membership in the tradition is increasing at a rate faster than that of the most comprehensive scientific order). Thus, the treatment of science as

a complex social order affords intricate and highly contextualized judgments as to the progressivity of different orders of scientific activity.

The progressivity, stasis, and regressivity of mutations in systems of rules of conduct are inherently dynamic notions, but we can derive from them synchronic concepts concerning the condition of an order at a particular time. That is, we can define the *dominant* order on a particular level of scientific activity at a given time as the one with the largest membership at that time. *Subordinate* orders are then just orders on the same level of activity with smaller memberships. These synchronic concepts can then be combined with the dynamic notions of progressivity, stasis, and regressivity, and deployed in yet more complex judgments of the condition of an order on a particular level of scientific activity. A research tradition, say, might then be evaluated for the degree of its dominance and the extent of its progressivity relative to either other research traditions, its discipline, natural science / social science, or science itself.

3.2. A Hayekian Social Science of Science

If there is ever an order to some degree or other in the activities of scientists, then there is an object for a Hayekian social science of science that would aim to determine the extent of such orderliness and provide explanatory principles of the mechanism by which it emerges from the interaction between the observation of a system of rules of conduct by a group of individuals and the prevailing circumstances of the social and physical environment. Beyond this, the Hayekian social scientist of science might seek an explanation of the extent of *dis*orderliness at some level of activity in either the absence or environmental incompatibility of a particular system of rules

of conduct. Such a scientist might further aim to distinguish the environmental circumstances most conducive to the emergence of scientific order in general (i.e., more or less regardless of the rules observed) from those conditions in which the emergence of an order would require the observance of a particular system of specific rules of conduct. Similarly, the Hayekian social scientist of science might aim to identify the principles of operation of systems of rules of conduct which, under the environmental conditions then prevailing, would give rise to an emergent scientific order. In short, a Hayekian social science of science would involve the development of models of hypothesized environments consisting of physical and social circumstances, and their marriage with postulated systems of rules of conduct, and the drawing out of implications therefrom, which would then be tested empirically via comparison with actual circumstances.

Naturally, given the complexity of the phenomena, the Hayekian scientist of scientific order would be in an unenviable epistemic position relative to the one she would occupy if scientific phenomena were simpler. Her ability to reconstruct an observed scientific order would depend on her limited knowledge of the relevant rules, the interrelations between these rules, and their relations to environmental circumstances. However, the scientific order itself may be more obvious to the observer than the rules from which it emerges. These rules may be largely tacit to the scientific actor and, thus, mostly inaccessible to the Hayekian social scientist of science. The capacity of the latter to explain particular events within or manifestations of this order would depend on her ability to know the relevant data, i.e., the circumstances to which these rules are applied at particular places and times. The limits of human knowledge with respect to complex scientific phenomena require the scientist of scientific order to aim lower in her explanatory

ambitions; thus, her limitation to explanations of the principle of the mechanism by which scientific orders might emerge in appropriate circumstances. Like all explanations, these will prohibit the occurrence of events that fall outside a particular range of phenomena to which they refer (and will be undermined, if not falsified, by the appearance of events they forbid). But, because of our epistemic limitations, our best explanations and predictions with respect to scientific orders will necessarily be of lesser "degree" than our best explanations of simpler phenomena.

Given the limitations of the social scientist of science with respect to the relevant phenomena, a constructivist approach to complex scientific orders, i.e., an attempt to model the complex phenomena of a scientific order as if they were simple, and, thus, fully and explicitly explainable, will eventually and inevitably come up short as an account of rational scientific belief. All of this means that, if scientific rationality includes an ecological component, the attitude of the constructivist rationalist is inappropriate in methodological investigations. More exactly, since constructivist and ecological rationality operate together, pure constructivism, unleavened by an appreciation for the possibility of ecological scientific rationality, will ultimately lead the methodologist astray.

4. Twentieth Century Explanations of Science from the Perspective of a Hayekian Meta-Methodology

The possibility that some scientific beliefs are rational only in an ecological sense contributes to an error theory of the shortcomings of previous attempts to explain scientific rationality. That

science *is* more rational than other methods was typically treated as a datum by earlier empirically-inclined philosophers of science. However, these same philosophers typically assumed that the rules of scientific conduct that account for this superior rationality could all be consciously discovered and discursively stated. In other words, it was generally assumed that scientific beliefs are rational only in a constructivist sense.[22]

Constructivism was part and parcel of the philosophies of certain members of the Vienna Circle—the authors of the *Wissenschaftliche Weltauffassung* pamphlet, in particular, come to mind.[23, 24] One would be hard pressed to find a more representative statement of constructivist rationalism, as Hayek and others have conceived it, than the statement that "[t]he Vienna Circle

[22] The goal of the offending constructivist philosophies of science was to "articulate an objective procedure or set of criteria by which such candidate knowledge [propositions generated by individual scientists] could be justified as being acceptable…without regard to the context in which they were produced" (McQuade 2010, 26).

[23] Written in 1929, the "pamphlet is the product of teamwork; [Otto] Neurath did the writing, [Hans] Hahn and [Rudolf] Carnap edited the text with him; other members of the Circle were asked for their comments and contributions" (Carnap, Hahn, and Neurath 1929). However, not all members of the Circle were on board with the "manifesto," which "alienated [Kurt] Gödel…to the point that he came to the meetings less and less frequently" (Menger 1994, 210). Indeed, Moritz Schlick himself, "to whom the manifesto was dedicated, was less than satisfied with the result. This was first of all because he was not taken by the conception of the circle as a 'movement' of any sort, favouring a more modest and more narrowly scientific approach…But it was also because he was distressed by the political tone of the piece, and more specifically by those portions which suggested some sort of alignment of logical positivism with socialism and with the movement for workers' education in Vienna at the time" (Smith 1994, 9–10).

Relations between members of the Austrian School of economics and the Vienna Circle of Logical Positivism were, to say the least, complicated. On the one hand, the mathematician Karl Menger (son of Carl Menger, founder of the Austrian School) and the Husserlian philosopher Felix Kaufmann participated in, and maintained close, in some cases, lifelong, friendships with members of both *kreise*. On the other hand, there was no love lost between Ludwig von Mises, the arch-liberal political economist around whom much of post-WWI Viennese economics centered, and Otto Neurath, the arch-Marxist "social science expert" of the Vienna Circle. Mises and Neurath had been co-participants in Eugen Böhm-Bawerk's famous economics seminar before the war, as well as disputants in the German-Language Socialist Calculation Debate after the war (for more on the relationship between Mises and Neurath, see Chapter Five of Caldwell 2004). Indeed, Barry Smith (1990, 220) speculates that Hayek himself may have "seriously considered joining the Vienna circle," but was dissuaded (one might imagine, under the influence of his mentor, Mises) by Neurath's "naïve, not to say absurd, economic views."

[24] For a discussion of several further representatives of constructivism in the sciences and philosophy – namely, the American economist Wesley Clair Mitchell, the Canadian scientist G. Brock Chisholm, The German-American philosopher of science Hans Reichenbach, and Hayek's friend and great rival, the British economist John Maynard Keynes – see footnotes 4 and 23 of Hayek ([1970] 2014). Hayek would have undoubtedly agreed with Weimer's (1980, 178-179) assessment of Descartes, Rousseau, Comte, Marx, Lenin, Freud, Bertrand Russell, John B. Watson, and B.F. Skinner as devout constructivists.

believes that…it fulfils a demand of the day: we have to fashion intellectual tools for everyday life, for the daily life of the scholar but also for the daily life of all those who in some way join in working at the conscious re-shaping of life. The vitality that shows itself in the efforts for a rational transformation of the social and economic order, permeates the movement for a scientific world-conception too" (Carnap, Hahn, and Neurath 1929); or, again, the notion that "[t]he scientific world-conception knows *no unsolvable riddle*" (Carnap, Hahn, and Neurath 1929; italics in the original); or, finally, that "[w]e witness the spirit of the scientific world-conception penetrating in growing measure the forms of personal and public life, in education, upbringing, architecture, and the shaping of economic and social life according to rational principles. *The scientific world-conception serves life, and life receives it*" (Carnap, Hahn, and Neurath 1929; italics in the original).

The slogan that scientific justification means *verification via logical analysis* – "reduction to the simplest statements about the empirically given" (Carnap, Hahn, and Neurath 1929) – or, for that matter, any appeal to a "naïve" falsificationism (Lakatos 1968–1969, 152–162) that takes a single countervailing observation as sufficient to both conclusively falsify a universal statement and warrant its rejection – implies an assumption of constructivist rationality.[25] That is, these views imply that scientific rationality requires nothing more than the mechanical application of some known (or, at least, knowable) rule to explanations of a particular body of evidence. Indeed, implicit in such accounts is the notion that a (the) system of rational scientific beliefs could (at least in principle and perhaps in practice) be reproduced in a

[25] Weimer (1980, 166) argues – convincingly, to my mind – that even more "sophisticated" versions of Popper's critical rationalism are constructivist: "[t]he Popperian notion of critical rationalism holds that conduct is rational to the extent it follows clearly specified general rules *that can be known to and acted upon by practitioners*."

laboratory environment.[26] All that would be required would be to enforce adherence to the relevant rule(s) among human test subjects who had been provided with certain bits of evidence and various competing explanations.

Of course, it was widely acknowledged by the middle of the third quarter of the last century, largely as a result of the work of W.V.O. Quine ([1951] 1961) and Thomas S. Kuhn (1962), that actual scientific practice is not so simple. Quine (as Duhem ([1914] 1954) had previously done) argued that, strictly speaking, *no* statement is falsifiable in isolation. Thus, the rule to accept only falsifiable, but not-yet-falsified, statements could be neither a necessary nor a sufficient criterion of rational scientific belief. Our imaginary test subjects would sooner or later believe *ir*rationally were they to unwaveringly apply only naïve falsificationism: they would reject hypotheses which, in the actual course of the history of science, have for one (presumably good) reason or another been retained in the face of apparent falsification, or they would retain hypotheses that have in fact been rejected for reasons other than falsification. For his part, Kuhn emphasized the social nature of scientific practice: scientific beliefs, such as they are, cannot merely fall out of the mechanical application of logic to empirical evidence, but result from processes that necessarily involve psychological and social considerations. There is no mechanical decision algorithm that might be provided to our imaginary test subjects that would either necessitate or suffice for their convergence upon a system of rational scientific beliefs.

Yet, for all the credit due to Quine and Kuhn[27] for deflating this crude constructivism, certain of their followers responded by committing what, from the perspective of a Hayekian

[26] The goal of the defenders of the so-called "Received View" (Suppe 1977) in philosophy of science was "to reduce [scientific] success to a set of logical, even algorithmic rules for grinding out truth claims, sometimes known as Scientific Method" (Leonard 2002, 142).

meta-methodology, can only be counted as the same error as the one made by the constructivist empiricists. Rather than recognizing the possibility of rational, but not-fully-effable, scientific beliefs, some in the sociology of scientific knowledge (SSK) tradition (especially Latour and Woolgar ([1979] 1986) and, perhaps to a lesser extent, Barnes and Bloor (1982)) simply rejected the relevant datum: science, they argued, despite all appearances, is not in fact more rational than other methods of belief fixation. Notice, however, that this leap from the inadequacy of the aforementioned empiricist philosophies of science to irrationalism is merely constructivism *redux*: either the rules of theory choice that constitute science's superior rationality are fully describable, or there are no such rules and this apparent rationality is an illusion; science is little better than witchcraft, religious dogma, or an appeal to political authority as an epistemic method. The third possibility that the scientific method is in fact epistemically superior to these alternatives (or, more exactly, that systems of rules of scientific conduct contribute to the emergence of order *under appropriate environmental conditions*), but that this epistemic superiority is at best only partially amenable to deliberate reconstruction, was never seriously considered by those writers who leapt from arguments like those of Quine and Kuhn to the conclusion of epistemic relativism.[28] For constructivists – of either an empiricist or a relativistic bent – it is all or nothing when it comes to science's rationality.

A Hayekian meta-methodology that conceives of the justification of scientific beliefs as complex phenomena in the relevant sense implies some minimal criteria for explanations of scientific rationality. In the first place, against constructivist empiricist philosophers, a Hayekian meta-methodology implies that science's superior rationality cannot be taken for granted. To

[27] And other, similarly-minded thinkers such as Stephen Toulmin (1953, 1961), Michael Polanyi (1958, 1966), Norwood Russell Hanson (1958), and Paul Feyerabend (1965))
[28] Related to this, see Laudan (1990)

accept the possibility of ecologically-rational scientific beliefs is to recognize that scientific beliefs can be *more or less* rational—i.e., it is to recognize both that scientists can adhere more or less to an otherwise order-generating system of rules and that such a system can be more or less well-adapted to prevailing conditions; and, therefore, that various contexts of justification can be more or less ordered. The matter of science's epistemic superiority – its degree of orderliness vis-à-vis other methods of belief fixation – is an empirical question that will likely be answered differently for unique research traditions, disciplines, etc. at various places and times.

Moreover, the ecological perspective of a Hayekian meta-methodology does not assume that, if and when adherence to some system of rules manifests a degree of order in the beliefs of scientists, the relevant rules will all be accessible to cognition. If the phenomena of scientific-belief justification are complex, then the rules the observance of which give rise to the emergence of a scientific order cannot all be explicitly stated.

Finally, if the phenomena of scientific practice are indeed complex in Hayek's sense, then scientific orders emerge from the interaction of rules that concern scientists' confrontations with *both* the physical environment and society. That is, an explanation of science that runs exclusively in terms of either empirical or social considerations *but not both* is necessarily deficient on a Hayekian meta-methodology. The trick is to show how scientific orders emerge from the interrelations between rules of scientific conduct, confrontations between theories and empirical evidence, and scientists' own social interactions.

In short, a Hayekian meta-methodology predicts the failure of any methodology that aims to reconstruct scientific rationality on the basis of either a simple or static set of rules, and the failure of any explanation of science that denies or ignores either the empirical or social aspects

of the process from which scientific orders emerge. Constructivist empiricist philosophers of science erred in adopting a meta-model predicated on the comprehensive intelligibility of the rules that constitute science's epistemic superiority, and which was, for this reason, unable to adequately encompass the complexity of the context of justification. But, constructivist relativists, made the same blunder in leaping from the failure of the empiricist constructivist's meta-model to the conclusion that there are no rules of scientific justification, and that science is *merely* a matter of psychological and social considerations.[29]

5. Liberalism in Politics, Economics, and Methodology as Consequences of Complexity

It follows from the foregoing considerations that, where a scientific order emerges despite limited knowledge of the circumstances that lead to its emergence, i.e., where scientific rationality is to some degree ecological, *coercion* can be neither necessary nor sufficient as a scientific method. That coercion is unnecessary under such circumstances is an implication of the very possibility of the spontaneous emergence of a scientific order. That coercion cannot suffice under such conditions follows from the absence of the knowledge required to make it effective, i.e., where the knowledge required to deliberately realize a scientific order is not available, no amount of force – however brutal the coercive measures employed – will suffice for the manifestation of an order.

In his political philosophy, Hayek ([1960] 2011, 57) defined *liberty* as "that condition…in which coercion of some by others is reduced as much as is possible in society."

[29] McQuade (2010, 41–48) discusses various postpositivist explanations of science which, in his view, better conform to a Hayekian conception of science as an "adaptive classifying system."

That is, individuals are free to the extent to which their actions are uncoerced by others. Thus, there is a clear connection between Hayek's political philosophy and the implications for scientific order of his writings on complex phenomena.[30] To say that scientific rationality cannot be coerced where it is ecological is to say that the realization of scientific order under such circumstances requires a degree of methodological liberty.

Moreover, the concept of emergent scientific orders contributes to an explanation of the *context of scientific discovery*[31] that accords well with Hayek's argument(s) against the methodological variant of constructivist rationalism – a position he dubbed "scientism"[32] – arguments that figure centrally, if not always explicitly, in his well-known criticisms of both socialism and Keynesian-style aggregate demand management. Indeed, Hayek is probably best known as one of the twentieth century's most vehement critics of these programs. What is perhaps less appreciated is that Hayek's criticisms and, thus, his related defenses of liberalism in politics and economics, are largely epistemological / methodological in nature.

In the wake of the Great Depression, during the interwar years and after, a wide-ranging consensus arose to the effect that government planning of the economy was crucial for the future

[30] Also see Hayek ([1962] 2014): "[w]e have…learnt that there exist political systems which make very difficult even such intellectual honesty as is a basic condition for all genuine science. It is certainly possible to preserve intellectual honesty in the most difficult conditions. But we are not all heroes, and if we value science[,] we must also advocate a social order which does not make such intellectual honesty too difficult. There seems to me to exist in this respect a close connection between the ideals of science and the ideals of personal liberty."

[31] On the similarities between market competition and scientific method as "discovery procedures," see Hayek ([1968] 2014, 305). Like competitive methods, "[t]he advantages of accepted scientific procedures can never be proved scientifically, but only demonstrated by the common experience that, on the whole, they are better adapted to delivering the goods than alternative approaches." The analogy is drawn yet closer, if one accepts Hayek's ([1946] 2014, 115) argument that "[c]ompetition is essentially a process of the formation of opinion: by spreading information, it creates…unity and coherence…It creates the views people have about what is best and cheapest, and it is because of it that people know at least as much about possibilities and opportunities as they in fact do. It is thus a process which involves a continuous change in the data and whose significance must therefore be completely missed by any theory which treats these data as constant."

[32] Although he is often credited with originating the term, according to his reminiscences ([1967] 1992, 173-4), Hayek borrowed the phrase from Otto Neurath, who occasionally used "scientism" to describe his own view.

maintenance and flourishing of western civilization. From the dogmatic Marxism of Neurath, the "social science expert" of the Vienna Circle, and the gradualist socialism of Fabians like Beatrice and Sidney Webb, Hayek's employers at the London School of Economics, to the "Tory Socialists,"[33] who acquiesced to the postwar consensus for the British welfare state, there was widespread agreement across the political spectrum in favor of some form of central planning. However, it was also generally acknowledged that the social sciences, as then constituted, were inadequate to the onerous requirements of successful centralized economic planning. Social control of the sort required of such planning could only be predicated on highly specified and reliably accurate predictions of social phenomena. The predictive and explanatory deficiencies of the social sciences in these respects were too obvious to ignore. The issue for advocates of economic planning then became the means by which social-scientific predictions might be made to meet the required standards of specificity and accuracy. Reform of the social sciences, along the lines of the successful techniques of the "hard" sciences, was suggested as a means to the end of precisified predictions of social phenomena, itself a means to the further end of effective economic administration of either a socialist or Keynesian variety.

Thus, the scientism that Hayek argued against was the view that the social sciences could be, and could only be, *deliberately* reconstructed to deliver predictions of the specificity and accuracy required of these economic-political programs. Hayek's argument(s) against scientism aimed to show that importation of the methods of the physical sciences is no way to improve predictions of social phenomena and, moreover, that there is no known means for the deliberate

[33] See the proposed chapter outline of an early version of Hayek's unfinished *Abuse and Decline of Reason* project reproduced in Caldwell (2010, 5)

realization of the relevant goal.[34] The *discovery* of a system of rules of scientific conduct capable of predicting social phenomena with the degree of specificity required for the relevant economic-political programs – assuming some such system is possible – cannot be deliberately brought about, but requires an element of spontaneity. That is, Hayek's conclusion against scientism constitutes an *ignoramus*, but not an *ignorabimus*. From the conclusion that we don't know how to deliberately realize the goal of scientism with respect to social-scientific predictions, it does not follow that this goal will remain forever unrealized. We might, as it were, *learn* or *develop* new methods in the course of other pursuits – or circumstances outside our epistemic purview might otherwise be sufficiently fortuitous – that the goal is eventually realized despite our present ignorance. The key point is that, if it is to be realized, it will come about as a result of a process that we cannot foresee or understand *a priori*: we cannot plan our way to the required

[34] Hayek's understanding of scientism and, thus, the reasons he offered for its inadequacy, changed over time. In fact, he offered two distinct explanations for the inefficacy of attempts to import the methods of the physical sciences into the social disciplines. According to Hayek's early methodological writings (see especially "Scientism and the Study of Society" [1952] 2010), there is a meaningful difference between social and physical phenomena that makes the methods appropriate to explaining the latter inappropriate for explanations of the former. However, Hayek eventually came to the view, perhaps under the influence of Karl Popper, that this sharp distinction between the social and physical sciences was untenable. According to Hayek's later methodological writings (see especially [1955] 2014 and [1964b] 2014), there is only one scientific method, and it looks more like the causal-genetic approach he had explicated in the "Scientism" essay (see Caldwell 2014, 14) as the spontaneously evolved method of the social sciences than the constructivist suggestions of the defenders of scientism. To the extent this change meant that an appeal to the methods of scientism for the social sciences could not be justified on the grounds of their unique successes in the physical sciences, Hayek's early argument against scientism was buttressed; but, by the same token, since this change in his conception of the sciences also meant that there's nothing unique about the social sciences, Hayek's argument against scientism required some considerable modification. If the techniques of the social disciplines are the same as those of the physical disciplines, why are the predictions of the former so much less satisfactory and practically useful than those of the latter?

Hayek's answer, foreshadowed in places in "Scientism and the Study of Society," and emphasized in his later methodological writings, was that the predictive limitations of certain disciplines, especially the social sciences, are to be attributed not to fundamentally different or deficient methods, but to the comparatively higher degree of complexity of the phenomena they investigate. The physical sciences are successful compared to the social disciplines not because their methods are, in some sense, "better" or more worthy of imitation, but because they deal with phenomena that can be explained on the basis of models that are comparatively simple: where the scientific method is applied to phenomena that can be explained with a few variables relatable in terms of experimentally-discoverable laws, it is possible to predict with the degree of specificity and accuracy that we find in the paradigmatic physical sciences. However, where the same method is applied to complex phenomena, it is only possible to explain the "principle" from which the phenomena emerge and predict "patterns" in the phenomena. For more on Hayek's arguments against scientism, see Caldwell (2010, 35–38).

predictions.[35] Thus, Hayek denied both tenets of scientistic methodology: not all scientific goals can be brought about deliberately and some scientific goals, if they can be realized at all, require a non-deliberative component.

This is to say that Hayek's argument is directed against scientism as a means to the realization of social-scientific predictions of the required degree of specificity and not against scientism as an end in itself, or as a means toward some other end. The conclusion is not that we don't know how to effect scientism in the social sciences – i.e., the conclusion is not that we don't know how to act like natural scientists with respect to social phenomena – but that such pretense is no means to the end of improving social-scientific predictions. The social sciences can *try* scientism, and may eventually discover other ends for which it is an appropriate means, but we should not expect these ends to include that of improving social-scientific predictions to the extent required of effective political administration of the economy. This latter point implies a further possibility, namely, that *both the means and ends* of science can be rational in an ecological sense. New methods might develop that allow existing, but previously unachievable, goals to be realized, or new scientific ends might develop that are realizable with extant methods that were, however, inadequate for any previous goal.

Again, it follows from these considerations that, where ecological processes are operative, successful science – the aptness of scientific methods for scientific objectives – cannot be realized via coercion. And this is so, moreover, for purely epistemic reasons, without regard to ethical qualms or questions concerning the incentive structure confronting coerced scientists.

[35] See Hayek ([1963b] 2014), "[p]erhaps someday we shall get something like a general science of man which will provide us with both the required conceptual framework and the appropriate technique which then as specialists in economics or any other social science can employ as mechanically and unthinkingly as the natural scientists can follow the scientific method he has been brought up to use."

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If the knowledge required to make effective some plan for the realization of a particular scientific goal has not been discovered, coercion (intimidation, force, duress, strong-arm tactics, etc.) can be neither necessary nor sufficient for the discovery of the requisite knowledge. No amount of violence can make social scientists generate predictions of the degree of specificity required to facilitate effective political administration of the economy.

And this is to say that some degree of liberty is required in both the pursuit of ends and the choice of methods in science. This is *methodological liberalism*. It is essentially identical with the denial, on the grounds of the possibility of ecologically-rational scientific outcomes, of the adequacy of constructivist methodology and, thus, of the possibility of coercion as an effective scientific method.[36] Successful science requires that methods be well-adapted to goals (and *vice versa*). The methodological liberal insists that this fit between scientific ends and the means for their realization is at least sometimes, to some degree, the process of an evolutionary, rather than a designed (or design*able*), process. The methodological liberal denies both aspects of the scientistic thesis. That is, according to the methodological liberal, it is not the case that

[36] It should be noted that the methodological liberal is not a methodological anarchist. The latter shares with the former the denial of both tenets of scientism, but the anarchist, in effect, goes much further in denying the possibilities for a constructivist understanding of some aspects of scientific activity. The methodological liberal, though she is skeptical of the possibility of a comprehensive account of scientific explanation, need not be so nihilistic as the methodological anarchist. The methodological liberal does not subscribe to Feyerabend's ([1975] 2010, 12) dictum that the only rule of scientific practice is "anything goes." This is because, as scientific ends are (at least partially spontaneously) realized, i.e., as new scientific orders emerge, strong forces in the form of rules, customs, and traditions will be set up that serve to maintain the existing order. The observance of the rules that inform the activities of the members of some scientific order may be required to secure the persistence of either the order itself or of the scientist within the order. In other words, there will usually exist good (but perhaps only tacitly-known) reasons for scientists within established scientific communities to accept the problems and adopt the methodology associated with their respective community. But, this is not – indeed, cannot – be the case in new areas of inquiry, where the main problem is the discovery of the rules, if any, that will secure an order and its persistence in the relevant environmental circumstances. As Weimer (1980, 163) points out, Feyerabend ([1975] 2010) implicitly accepted "the usual dichotomization between 'science is rational only if my methodology is accepted' and 'if not, science is irrational and therefore anything goes'[.]" Feyerabend, like the constructivist philosophers of science he attacked and the irrationalists he inspired, failed to recognize the possibility of ecologically-rational scientific outcomes. The "anything goes" dictum is, in fact, disguised scientism.

science can always be deliberately changed to serve whatever goals we might adopt. Nor must science be *designed* in order to serve our chosen ends. Both the ends and means – the discoveries and methods – of successful science can, under appropriate circumstances, result from spontaneous processes. If, at any given time, science (or, more exactly, one of its many disciplines, research traditions, or sub-traditions) is successful, it is in virtue of a bi-directional fit between relevant goals and methods that may well be, at least to some degree, unintended. To insist on the possibility of this result in scientific inquiry – to insist that there are circumstances in which scientific success cannot be planned and controlled, and, thus, cannot be coerced – is to be a methodological liberal.

There is another, related route from Hayek's writings on political philosophy to the conclusion of methodological liberalism. Hayek often argued that the best justification for liberty in the economic and political domains is the absence of knowledge concerning the circumstances necessary for the deliberate maintenance, much less the improvement, of society, i.e., that "the case for individual freedom rests largely on the recognition of the inevitable and universal ignorance of all of us concerning a great many of the factors on which the achievements of our ends and welfare depend" (Hayek [1960] 2011, 80). Hayek took it to be empirically obvious that the deliberate reconstruction of the multitudinous considerations "on which the achievements of our ends and welfare depend" requires knowledge that extends well beyond the limits of human cognition and, thus, that such schemes ought to be avoided in the political and economic realms. That is, according to Hayek, there is no evidence that supports belief in the knowledge and abilities of those who would aspire to deliberately reconstruct or administer society so as to

increase achievement of human ends and welfare beyond that which manifests in a liberal society, and so, a preference for liberty should be the default attitude.

An analogous argument for methodological liberty would seem to be defensible on the basis of the considerations of the present paper. The deliberate reconstruction of the circumstances that lead to scientific success requires knowledge that extends beyond the limits of human cognition. If, when, and to what extent the activities of scientists constitute an order, the rules adherence to which give rise to this order are not all accessible to the methodologist (or "social scientist of science"). At best, the methodologist can partially reconstruct this order, but – as its emergence depends on circumstances, e.g., with respect to physical and social conditions in the relevant environment, which the methodologist cannot possibly know in advance of scientific inquiry – only after the realization of success. In other words, the methodologist never possesses the advance knowledge required to ensure the success of some untried scientific inquiry, but can, at best, pronounce upon what has worked in the past in various circumstances. There is no reason to believe in the methodologist's ability to deliberately administer scientific activity so as to improve the prospects for scientific success beyond that which manifests in an environment in which the activities of scientists are not coerced, and so, a preference for methodological liberty should be the default attitude. This principle applies not only to the most comprehensive scientific order, if there is one, but also to each of any orders that might obtain at the level of disciplines, research traditions, etc.

There is no *a priori* reason to suspect that science is a one-size-fits-all endeavor and that a system of rules that has proven successful in one domain can be transferred without cost (or with benefit) to another in which it has yet to be tried or the results of its application observed.

That is, even after the evidence has been collected concerning the rules that have proven successful in one domain, the methodologist's knowledge remains in another sense limited: she is not in a position to say with a high degree of confidence why such a system has worked where and when it has. To assert, then, the transferability of one system of rules from a field in which it has proven successful to either a new environment or a novel field of inquiry is epistemic hubris. Similarly, if the methodologist is not in a position to declare in advance which *existing* system of rules is most appropriate for some proposed scientific inquiry, less so is she able to conjure all of the possibilities for *new* systems of rules. Though the methodologist may be able to point to failed applications of scientific methods to particular objectives in the history of science, there can be no *a priori* grounds for the methodologist to prohibit the pursuit of any system of rules scientists might devise. Again, absent evidence that the methodologist can deliberately improve upon the workings of spontaneous scientific practice, a preference for liberty should be the default attitude. Stated another way, in the words of Charles Sanders Peirce, whom Hayek was fond of quoting: "[u]pon this first, and in one sense this sole, rule of reason, that in order to learn you must desire to learn, and in so desiring not be satisfied with what you already incline to think, there follows one corollary which itself deserves to be inscribed upon every wall of the city of philosophy: Do not block the way of inquiry."[37, 38]

[37] Hayek (1967) adopts this quote, Peirce's "first rule of reason," as the epigram of the *Politics* section of *Studies in Philosophy, Politics, and Economics*. This would seem to be a further indication of the extent to which, to Hayek's way of thinking, epistemological / methodological issues were deeply connected with the problems of politics.

[38] There is another Hayekian route via which the necessity of the methodologist's limited knowledge might be established. According to Hayek (1952, 188), no classificatory system can adequately explain itself because any such system must be more complex than that which it classifies: "[a]n apparatus capable of building within itself models of different constellations of elements must be more complex…than any particular constellation of such elements of which it can form a model." For Hayek (1952, 5), examples of classificatory systems include the minds of organisms (which classify stimuli confronted in an organism's interaction with the physical environment) and science (which re-classifies the organism's interactions with the physical environment so as to generate a more reliable model). This means that, if it is to be capable of comprehensively classifying the phenomena of the

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In closing this section and the main text of the present paper, I'd like to quote an extended passage from the pen of economist James Buchanan, Hayek's friend, sometime colleague,[39] and fellow Nobel laureate and defender of classical political liberalism. The passage appears not in any of Buchanan's explicitly methodological (or even theoretical) writings, but in a most unusual place, namely, a letter dated October 17, 1960 addressed to Kermit Gordon (at the time, a Director in Economic Development and Administration at the Ford Foundation from which Buchanan and his then-colleagues at the Thomas Jefferson Center for Studies in Political Economy at the University of Virginia were seeking a grant). Buchanan was at pains to express the methodological uniqueness of the Jefferson Center vis-à-vis the mainstream of the economics discipline.[40] "There seem to me to be two essential ways of approaching the study of problems of political, social [and, thus, by the lights of the present essay, scientific], and economic organization," Buchanan wrote,

> "The first way is that of setting up independently certain criteria or goals for achievement and to examine existing and potential institutions in the light of their performance or expected performance in meeting these criteria. This approach, for purposes of exposition

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environment, science must be capable of producing at least as many classifications as there are kinds of phenomena in that environment: a complete scientific explanation of the physical environment requires that science be at least as complex as the environment. But, since methodology classifies science, this means that a comprehensive descriptive methodology must be more complex than science and *a fortiori* more complex than the physical environment. (And, again, meta-methodology must be yet more complex than methodology, and so on.) But, since the scientist's knowledge of the complex phenomena of nature and / or society is limited, the methodologist's knowledge of science is more limited (and the meta-methodologist's knowledge of methodology is yet more limited, etc.)

[39] Regarding Hayek and Buchanan's personal and professional relationship, see Buchanan (1992). Hayek and Buchanan were together for a semester at the University of Virginia in 1961, briefly at UCLA (Buchanan in the economics department, Hayek a visiting professor in the philosophy department) in 1969–1970, and again as visiting scholars at Stanford's Hoover Institution in the late-1970s.

[40] Many thanks to David Levy and Sandra Peart for permission to quote from their (draft) manuscript "'Almost Wholly Negative': The Ford Foundation's Appraisal of the Virginia School," which includes the Buchanan-Gordon letter in question.

> here, may be called the 'social welfare function' or 'social engineering' approach[41]…The second approach is that which deliberately avoids the independent establishment of criteria for social organization…and instead examines the behavior of private individuals as they engage in the continuing search for institutional arrangements upon which they can reach substantial consensus or agreement. *It follows from this difference in approach itself that 'individual liberty', in the sense of individual participation in the choices of appropriate constraints on human action, will tend to assume a necessary, and hence more prominent, role in the second than in the first*." (Buchanan 1960; italics added)

Later in the same letter, Buchanan acknowledges, as has been stressed in the present essay, that the two approaches often exist side-by-side. The issue is a matter of emphasis: "Both approaches to social problems are, of course, appropriate, and in many cases, they tend to coincide and merge one with the other. The same tools of analysis will be employed in many occasions. The difference in emphasis is, however, important, even if subtle in any specific context" (Buchanan 1960).

5 Concluding Remarks

The present essay has addressed the implications of the postulate that the activities of scientists constitute complex phenomena in Hayek's sense. Three interrelated theses have been defended, namely, that 1) Hayek's methodology of sciences of complex phenomena as applied to science

[41] Hayek, of course, would have called this constructivism and, when applied to social science, scientism.

itself implies a meta-methodology, i.e., a set of minimal criteria for explanations of scientific rationality; 2) this meta-methodology provides an error theory of certain failed explanations of scientific rationality; and 3) methodological liberalism – the recognition of both the possibility of ecologically-rational scientific outcomes and, in these cases, of the impossibility of coercion as an effective scientific method – is an implication of the treatment of science as complex phenomena and is closely related to his arguments against scientism, which were themselves fundamental to his arguments against socialism and Keynesian countercyclical policies.

I will close by suggesting a possible avenue for future thought and discussion that considerations of brevity do not permit me to take up in earnest in the present essay. Namely, the question whether recognition of the possibility of ecological scientific rationality doesn't imply constructivism at the meta-methodological level. To postulate the complexity of the phenomena of scientific activity is (to some extent or other) to attempt to reconstruct the rules of methodology and since, by the argument's own lights, such attempts will always be inadequate if methodological phenomena are complex, the postulate may well cannibalize itself. On the other hand, perhaps the potentially self-undermining nature of the postulate is more virtuous than vicious in the sense that it is yet another reminder of our "inevitable ignorance" and of the limits of knowledge across several related domains, i.e., science, methodology, and meta-methodology. The postulate essentially asserts that no matter how elaborate our explanations, there will always remain an unexplained (and, perhaps, unexplainable) residue concerning practice in these fields. If so, then the possibility of ecological scientific rationality explains (the principle of) its own limitations. Indeed, in the last analysis, the postulate may be nothing more and nothing less than

a reminder that constructivism is always an inappropriate attitude to adopt toward complex phenomena.

We should not assume that we possess, or can acquire, all of the knowledge necessary to explain some phenomena to whatever degree we might like, until experience has shown, with respect to these phenomena, that we *do* in fact possess all of the knowledge necessary to explain the phenomena to the desired extent. The phenomena that satisfy this condition are those we call "simple" and it is only with respect to these phenomena that constructivist rationalism is an appropriate attitude. Outside of this realm, where either our knowledge has yet to be tested or has in fact shown itself inadequate to the desired explanations, the assumption that the deliberate application of reason is both necessary and sufficient for the realization of our ambitions is an unfounded article of faith, and the constructivist attitude that ignores the possibility of ecologically-rational outcomes is bumptious.

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