

# A HAYEKIAN EXPLANATION OF HAYEK'S "EPISTEMIC TURN"

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# **A Hayekian Explanation of Hayek’s “Epistemic Turn”**

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**Abstract:** The present essay investigates F.A. Hayek’s epistemology and his methodology of sciences of complex phenomena for implications relevant to an explanation of Hayek’s own so-called “epistemic turn.” The thesis defended here is that Hayek’s dissatisfaction with his technical economics – in particular, his business cycle project – prompted, in keeping with his evolutionary theory of belief revision, the development of an approach less susceptible to the same disappointment.

**Keywords:** Hayek, epistemic turn, methodology, complex phenomena, explanation of the principle

**JEL Codes:** B2, B25, B3, B31, B4, B41, B53

## **I. INTRODUCTION**

The present paper takes it as a virtual axiom that the epistemological writings of a scholar who has written on the theory of belief formation and revision provide a natural perspective for an analysis of those events in the same scholar’s career that involve the formation and revision of beliefs. Even if such an analysis ultimately shines less light on the relevant events than might be expected *ex ante*, it is surely most charitable to start from an assumption of consistency between events of epistemic significance and the scholar’s own theory of knowledge.

The essay investigates F.A. Hayek’s epistemology and his methodology of sciences of complex phenomena for implications relevant to an explanation of Hayek’s own so-called

“epistemic turn.”<sup>1</sup> It has often been noted – including by the man himself (1964b, 91) – that Hayek’s career as a “very pure and narrow economic theorist” (*Ibid.*) came to a rather abrupt end sometime around the publication of 1941’s *The Pure Theory of Capital*, and that his subsequent career led “into all kinds of questions usually regarded as philosophical” (1964b, 91). However, to my knowledge, no one has previously applied Hayek’s own epistemological arguments to the very complex phenomena of his transition from “technical” economics to philosophy.<sup>2</sup>

The present argument assumes that Hayek’s fallibilist epistemology and the methodology of sciences of complex phenomena that he erected upon its basis are correct in their essentials, and further that the phenomena of an individual scientist’s (philosopher’s, mathematician’s, theologian’s, etc.) inquiries into the problems in which they are interested are complex in Hayek’s sense. If these assumptions are sound, then the evolution of Hayek’s inquiries into the various problems with which he engaged should be explicable in terms of his own epistemology.

No attempt will be made here to defend the correctness of either Hayek’s epistemology or his methodology of sciences of complex phenomena. But, a defense of the premise that the

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<sup>1</sup> The earliest reference to the phrase “epistemic turn” that I’ve been able to uncover in the secondary literature on Hayek appears in Birner (1999).

<sup>2</sup> This paper is dedicated to my friend Gerardo Serra, who first suggested to me the need for and potential fruitfulness of an analysis of Hayek’s work from its own perspective. It was only then that I realized that such was the nature of a project upon which I had, without fully appreciating its significance, already embarked. Gerardo’s suggestion was an enormous aid in the development of the current project. Of course, the standard caveat applies, and neither he nor anyone else is in any way responsible for whatever errors of analysis the present essay leaves uncorrected.

phenomena of scholarly inquiry are complex in Hayek's sense is relatively straightforward. According to Hayek, complex phenomena consist of a comparatively large number of causal elements interrelated (both to each other and to the environment) in a way that gives rise to 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" (Hayek 1964a, 26). Given this definition of complexity, the phenomena of scholarly inquiry are complex if and only if the results of such inquiry, say, the discovery of a new phenomenon or the development of a new theory, or an inquirer's choice between extant theories, emerge from the internal and external interactions of many different elements, and possesses certain properties that are independent of the particular values assumed by the causal elements provided the system of relationships is maintained. If this is right, then the phenomena of, e.g., an individual inquirer's theory choices, emerge from the interaction of, say, systems of methodological and ontological precepts, theoretical structures, the psychology of the relevant inquirer, the professional and social relations of the inquirer's community, and the external environment; and exhibit features that are independent of the particular methodological systems, theoretical structures, etc., involved in any particular theory choice. That is, theory choices will emerge so long as such elements interact with both each other and the environment, regardless of the values these parameters assume in the case of any particular choice.

Hayek's evolutionary account of belief formation implies that such a radical revision of beliefs – in this case, concerning better and worse approaches to the analysis of social phenomena – occurs as a consequence of disappointed expectations formed on the basis of a prior system of beliefs. Hayek's methodology of sciences of complex phenomena bears two important implications for an explanation of his epistemic turn. In the first place, there is the

incongruity, which it seems that Hayek came to recognize, between the complexity of phenomena relevant to an explanation of the business cycle – the project most central to his early career in technical economics – and the explanatory capacities of the tools Hayek applied to these phenomena. In the second place, there is Hayek’s (1964a 1967, 29; also see 1961) argument that one appropriate response to the difficulties of theorizing about complex phenomena is to move to a system of “higher-level generalities” that subsume some of the otherwise inexplicable complexities of the relevant phenomena. In Hayek’s case, this meant moving from the (theoretical) plane of generalities about the phenomena of industrial fluctuations to the (methodological) plane of generalities about the former kind of theoretical generalities. The thesis defended here is that Hayek’s dissatisfaction with his technical economics – in particular, his business cycle project – prompted, in keeping with his evolutionary theory of belief revision, the development of an approach less susceptible to the same disappointment.

Hayek’s theory of belief formation and revision is considered in the first part of the paper. The relevant aspects of the methodology of sciences of complex phenomena are addressed in the second part. The concluding section considers the limits of the present explanation of Hayek’s epistemic turn. In particular, it is emphasized that the argument terminates in what Hayek called an “explanation of the principle” by which such a turn might have occurred, which, though it constitutes a part – namely, the theoretical part – of a full-fledged historical explanation of the relevant events, is not identical with such an explanation.

## II. HAYEK’S FALLIBILIST EPISTEMOLOGY AND HIS EVOLUTIONARY THEORY OF BELIEF FORMATION AND REVISION

Hayek never wrote an explicitly epistemological treatise. However, he did publish a book – *The Sensory Order* – in 1952 on cognitive psychology that concludes with an extended discussion of the book’s epistemological implications. He had already written, over thirty years earlier as a student at the University of Vienna in 1920, a long essay on the nature of consciousness, which both forms the basis of *The Sensory Order* and accords with it in its consequences for epistemology (1920).<sup>3</sup>

Hayek’s cognitive psychology considers the nature of the relationship between the “sensory order” of subjective experience and the order of events described by physical science; the latter, as Hayek conceives it, encompasses the former as a proper subset (Hayek 1952, 16).<sup>4</sup> At the heart of Hayek’s epistemology is his fallibilism, a position he maintained over the course of his career.<sup>5</sup> Fallibilism is the view that humans can at best *believe justifiably*, but never *know*

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<sup>3</sup> The 1920 student paper remains unpublished as of this writing. Many thanks to Bruce Caldwell, General Editor of Hayek’s *Collected Works*, in which the paper will eventually appear, for providing me with a copy of the translation.

<sup>4</sup> See Leslie Marsh’s (2013, 205) comment that “Hayek is through and through a naturalist: a position he has consistently held throughout his career.”

<sup>5</sup> The arguments in the secondary literature to the effect that there are one or more cleavages in Hayek’s epistemological views over time do not withstand close scrutiny. Terence Hutchison (1981) argued that Hayek’s early methodology was closely aligned with the dogmatic, infallibilist, *apriorism* of Ludwig von Mises. However, Scheall (Forthcoming) objects to Hutchison’s argument (*inter alia*) on the grounds that the latter was apparently unaware of Hayek’s 1920 essay on consciousness and its fallibilist implications. More recently, Jeffrey Friedman (2013) has argued that Hayek held multiple mutually-inconsistent epistemologies at

*with certainty*. Of central importance to Hayek's (1920; 1952) epistemology is the notion that anything that can be known is a consequence of either the organism's or its species' confrontations with the environment. As Hayek notes (1920; 1952, 172), this is an especially radical empiricism.<sup>6</sup> Even *a priori* knowledge, such as it is according to Hayek's cognitive

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different times in his career. However, in order to accept Friedman's thesis that there is an inconsistency between the fallibilist-"interpretivist" epistemology of *The Sensory Order* (1952) and the (allegedly) infallibilist-"non-interpretivist" epistemology of "The Use of Knowledge in Society" (1945), one has to believe not only that Hayek changed his epistemological views between 1945 and 1952, but that he was almost quite literally *schizophrenic* in his ever-dithering epistemological attitudes. The fallibilism of *The Sensory Order* is also apparent in the 1920 essay upon which (as Friedman acknowledges) it was explicitly based. So, either (as implied by Friedman's argument) Hayek was a fallibilist in 1920, an infallibilist in 1945, and a fallibilist again in 1952, or – as seems more likely – when Hayek wrote of "knowledge" in the 1945 essay, he meant the word in its fallibilist sense. If this is right, then the inconsistency Friedman alleges disappears. In any case, either Hutchison or Friedman (or both) must be wrong: Hutchison argues that Hayek became a thoroughgoing fallibilist with the publication of "Economics and Knowledge" in 1937 and remained one for the balance of his career; Friedman's argument implies that Hayek was a fallibilist over the entire course of his career with the exception of a brief infallibilist interlude in 1945.

<sup>6</sup> Hayek was profoundly influenced by Ernst Mach's (1885 and 1905) subjectivist-phenomenological psychology. Indeed, Hayek's (1920) early theory of consciousness was conceived as an immanent criticism of the anti-metaphysical Mach's failure to eradicate untestable precepts like that of a "pure core of sensation" (i.e., sense data) from his theory of

psychology, is the result of encounters with the environment. Some of this knowledge is acquired phylogenetically<sup>7</sup> rather than ontogenetically, but, in the absence of any such confrontation,

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sensations. Hayek's cognitive psychology goes without anything like a concept of objective phenomena: the organism's phenomenal experiences are a function of interactions between its own mind and the environment (including the *milieu intérieur*), and depend upon how the former classifies stimuli incoming from the latter. In this sense, Hayek's epistemology is less anti-positivistic than *über*-positivistic: Mach wasn't wrong to try to rid his psychology of metaphysics; he only failed to complete the process. Relatedly, it's important to read the relevant parts of Hayek's various criticisms of the logical positivists of his own era as an accusation of failing the anti-metaphysical project rather than a rejection of it. For example, see (1952, 191n): "In our sense [the 'physical language' of logical positivists Otto Neurath and Rudolf Carnap], since it refers to the phenomenal or sensory qualities of the objects, is not 'physical' at all. Their use of the term rather implies a metaphysical belief in the ultimate 'reality' and constancy of the phenomenal world for which there is little justification."

<sup>7</sup> Given the emphasis that Hayek placed on evolutionary themes in his later writings, it is well to note the central role that Hayek assigns at an early date to inherited linkages. There may be, in any attempt to explain consciousness, a heuristic benefit in treating the central nervous system as, originally, a "blank slate," so that all simultaneous stimuli can be thought of as having an equal opportunity to create connections among the relevant ganglion cells: "(h)ere a network encompassing the linkages between all the elements of the nervous system could take shape purely on the basis of individual experience...No living organism, however, can ever experience such a starting point for its individual development, because the evolution of its progenitors has already shaped its brain in such a way that certain pathways have been smoothed in advance or



“mind” – as Hayek conceives it – is well and truly a *tabula rasa*. The organism’s initial encounter with the environment establishes mental (neuronal) linkages between stimuli that are subsequently reactivated upon recurrence of the relevant stimuli. These linkages establish the mental categories that thereupon determine the content of the organism’s sensory experience. They are thus *a priori* to the organism’s sensory experiences, but not to its pre-sensory, linkage-creating, confrontations with the environment.<sup>8,9</sup> For Hayek (1952, 165), even “the apparatus by means of which we learn about the external world [i.e., an organism’s “mind”] is itself the product of a kind of experience.”

Moreover, there is no aspect of an organism’s knowledge that is immune from revision as a consequence of predictive failure. Even the principles that regulate the classificatory apparatus that is an organism’s mind, which constitute *a priori* knowledge on Hayek’s system, are liable to change, albeit more gradually than the elements of the sensory knowledge they serve to ground (*Ibid.*, 166-169). According to Hayek, the classificatory system constituted by these linkages is a “partial and imperfect” replica of the “relations existing between the corresponding physical

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perhaps even from the very start guaranteed the transmission of impulses between certain elements...heritable pathways and hence uptakes are always present from the very beginning” (Hayek 1920).

<sup>8</sup> See Scheall (Forthcoming) on Hayek’s fallibilist conception of *a priori* knowledge.

<sup>9</sup> For Hayek, this is merely to say that observation is theory-laden. Of course, even the order of linkages that provides the theoretical framework of the organism’s sensory experiences is a consequence of confrontations with the physical order. However, according to Hayek, the organism is not “observing” or “sensing” at the time of these linkage-establishing confrontations, but having a kind of experience that is a prerequisite for sensory experience.

stimuli” (*Ibid.*, 145). In these respects, the mental “model” of the environment that the organism builds up over time is “distorted” and will “often be proven to be false, that is, give rise to expectations which will not be borne out by events” (*Ibid.*; also see *Ibid.*, 108-109). To put the point another way, that some aspect of the organism’s knowledge must be *a priori* relative to the contents of sensory experience does not mean that this “apparatus of classification” also “govern[s] the order of the events in the physical world” (*Ibid.*, 168). The sensory order is not fixed, but is “continuously affected by the addition of new linkages, so that the attributes of consciousness keep changing even in a developed consciousness” (Hayek 1920). That is, the organism’s knowledge changes whenever the order of linkages that grounds its sensory knowledge is modified in virtue of the establishment of new (or the modification of existing) linkages. Thus, what an organism knows at a particular time can only be known tentatively and is subject to revision in virtue of sensory order-altering encounters with the environment. There is nothing the organism knows with certainty: fallibilism is an implication of Hayek’s cognitive psychology.

When expectations based on the existing mental order are disappointed in virtue of some confrontation with the physical order, then, if the chances for the organism’s persistence are to be maintained or improved, something(s) within the former order must be revised. That is, on Hayek’s conception, belief revision is an aspect of the evolutionary problem of facilitating the organism’s (and, by extension, its descendants’) persistence and flourishing in the environment. Experience compels the rearrangement of the sensory order so as to make it more resilient to relevant circumstances. It is, of course, the failure of expectations based on the existing mental order that necessitates its rearrangement.

If this is right, then Hayek's epistemic turn is a consequence of disappointed expectations based on his pre-turn beliefs concerning the effectiveness of the modes of economic analysis he had accepted to that point in his career. These beliefs led to expectations that were not borne out by subsequent encounters with the environment. In order to improve the odds of Hayek's survival in the environment – construed (very) broadly so as to include his social and professional contacts – something(s) within Hayek's mental order had to change. In the next section, I look to Hayek's methodology of sciences of complex phenomena for an account of these disappointed expectations and the substance of his subsequent belief revision.

Before proceeding, it is important to mention two caveats. Both are further implications of Hayek's epistemology. In the first place, there is the consequence that the process of belief revision impedes in large part on beliefs that are held only "tacitly." Much of the process of reordering the organism's mental order effects changes to beliefs of which the organism is not "explicitly aware," but that it "merely manifests...in the discriminations which [it] performs" (Hayek 1952, 19). In particular, the classificatory system built up from pre-sensory linkages, as it is a precondition of consciousness, is not itself accessible to consciousness. To the extent that it was the tacitly-held elements of his mental order that were modified in the light of failed expectations, not even Hayek could have discursively explained his epistemic turn.

In the second place, Hayek's cognitive psychology implies that explanations are always circumscribed relative to the events they aim to explain. The classificatory system of linkages that is built up in virtue of the organism's encounters with the environment constitutes – subject to all of the aforementioned qualifications – a "map" of the history of the organism's encounters with the environment. An explanation is, for Hayek, a mental "model" of the relevant parts of the environment to be explained that is composed of elements of the organism's mapping of the

environment. As the former is always tentative, incomplete, error-prone, and largely only tacitly-known, so too are the models (explanations) drawn therefrom. Of course, given that I've assumed the correctness of Hayek's epistemology, this caveat applies no less to the explanation offered here.

### III. THE IMPLICATIONS OF HAYEK'S METHODOLOGY OF SCIENCES OF COMPLEX PHENOMENA FOR HIS "EPISTEMIC TURN"

Hayek's methodology of sciences of complex phenomena bears two important implications for an explanation of his epistemic turn. In the first place, there is the notion that, given our cognitive limitations, it becomes progressively more difficult to develop satisfactory explanations as the phenomena under investigation grow increasingly complex (Hayek 1964a). Any attempt to theorize about complex phenomena using analytical tools which, though they may be appropriate for the study of simpler phenomena, are inappropriate for the investigation of more complex processes is unlikely to succeed. In other words, Hayek's methodology as applied to the complex social phenomena of scientific theorizing predicts that we should expect to observe patterns of failure where scientists apply to analyses of complex phenomena tools appropriate only for the analysis of simpler phenomena.

In 1945's "The Use of Knowledge in Society," Hayek (1945, 80) explicitly bifurcated his epistemology. He argued from that point forward that there are two varieties of knowledge: "[A] little reflection will show that there is...a body of very important but unorganized knowledge which cannot possibly be called scientific in the sense of knowledge of general rules: the knowledge of the particular circumstances of time and place." This distinction between

theoretical knowledge (of “general rules”) and empirical knowledge (of “particular circumstances”) is essential to Hayek’s methodology of sciences of complex phenomena.

The possibility of a “full” explanation according to Hayek’s methodological writings, requires that the scientist possess both kinds of knowledge to a sufficient extent: “[s]uch prediction will be possible if we can ascertain...all the circumstances which influence those events. We need for this both a theory which tells us on what circumstances the events in question will depend, and information on the particular circumstances which may influence the event in which we are interested” (Hayek 1961).<sup>10</sup> That is, a full explanation requires both an adequate theory and knowledge of the particular values that the parameters of the theory take at the time relevant to explanation. However, in the sciences that investigate more complex phenomena, it is comparatively difficult to uncover all of the relevant theoretical parameters (not to mention the internal interrelations between the proper subsets of these variables and the external relations between the subsets of parameters and the environment) and it is more difficult to discover the respective data.<sup>11</sup> If there is a mismatch here – if, for example, the complexity of some phenomena outruns our capacity for (either theoretical or empirical) knowledge of the phenomena – then resulting explanations will simply fail to reflect the unaccounted aspects of the phenomena.

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<sup>10</sup> An explanation “can never explain everything to be observed on a particular set of events” (Hayek 1952, 182). Explanatory “fullness” is context-sensitive.

<sup>11</sup> For more on these difficulties – i.e., the “theory problem” and the “data problem” – and their implications for the “predictive degree” of theories that suffer from them, see Scheall (Manuscript B).

There is reason to believe that Hayek eventually recognized such an incongruity to be a problem for his business cycle project. Generally speaking, Hayek came to think of all economic phenomena as complex in the relevant sense (1964a, 34-36). But, more specifically, as Hayek seems to acknowledge in the introductory sections of *The Pure Theory of Capital* (1941), despite its tremendous complexity, the theory was not nearly elaborate enough to express the intricacy of the phenomena under investigation. In order to understand this point, it is important to review some of the difficulties Hayek encountered in the development of the business cycle project that was so very central to his early career as an economic theorist.

Hayek's earliest writings in technical economics<sup>12</sup> aimed to clarify the foundations of the theoretical framework upon which he subsequently built the trade cycle theory expounded in the companion pieces *Monetary Theory and the Trade Cycle* (1933b; originally published in German in 1929) and *Prices and Production* (1931). It was the development of an appropriate concept of equilibrium and, later, a theory of capital adequate to the problem of industrial fluctuations that would prove most intractable in this regard.

Hayek (1928) was aware that Walras' static general equilibrium framework was an imperfect tool upon which to base a theory of the cycle in a dynamic, money- and capital-using economy. Nonetheless, when he came to consider the methodology of cycle theories in *Monetary Theory*, he argued that the goal of unifying an explanation of the cycle with the then-accepted corpus of economic theory required the Walrasian framework (Hayek 1933b, 18-19). The uniqueness of Hayek's early theory lies in the fact that, with the introduction of assumptions concerning money and the activities of bankers in the creation of credit, cyclical fluctuations can be generated out of the otherwise perfectly-adjusting equilibrium framework.

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<sup>12</sup> Some, but not all, of these essays have been translated and anthologized in Hayek (1984)

However, in the 1933 essay “Price Expectations, Monetary Disturbances, and Malinvestments,” in which he responds to Gunnar Myrdal’s (1933) criticism that entrepreneurial expectations play no role in the theory of *Prices and Production*, Hayek (1933a) reverses course and argues against this view that the superimposition of monetary assumptions upon the skeleton of Walrasian equilibrium suffices to generate an adequate explanation of the cycle. This latter method is “to press the problems into the strait-jacket of a scheme which does not really help to solve them” (*Ibid.*, 136). Instead, what is needed is “a development of our fundamental theoretical apparatus which will enable us to explain dynamic phenomena...I am now more inclined to say that general theory itself ought to be developed so as to enable us to use it directly in the explanation of particular industrial fluctuations” (*Ibid.*, 137-138). An adequate account of the cycle required a reconceptualization of the equilibrium framework itself.

Hayek (1937) subsequently developed a unique concept of economic equilibrium according to which equilibrium exists to the extent that the relevant beliefs of individual market participants are both intersubjectively consistent and accurate with respect to external conditions. He employs this framework throughout *The Pure Theory of Capital*, but “repeatedly apologizes for doing so. Although he clearly considers the new definition to be an advance over those found in earlier models, he also suggests that equilibrium analysis in general is, at best, *preparatory* to a more advanced causal analysis of economic phenomena” (Caldwell 2004, 224; italics in the original; also see Chapter Two of Hayek 1941, 31-51). That is, Hayek judged his epistemic concept of equilibrium, more complex though it was than the traditional treatment, to be still too simple for an adequate causal explanation of the complex phenomena of the cycle.

With respect to capital theory, *Prices and Production* places Böhm-Bawerk’s concept of the “average period of production” – a measure of the temporal duration of the capital structure –

at the heart of an explanation of industrial fluctuations. It was this element of Hayek's early cycle theory that was to receive perhaps the harshest criticism from both Hayek and his peers in the years immediately following the book's publication. As Hayek came to recognize, an average period of production can be non-circularly defined only under severely restricted assumptions. When these conditions are relaxed, the definition of the average production period becomes circular in that it both depends upon and is a determinant of the interest rate (Hayek 1936, 497-498; White 2007, xxii).

The theory of fluctuations offered in *Prices and Production* was intended to be – and, given the circumstances of its rushed preparation, could only be – a mere sketch of an elaborated explanation of the cycle.<sup>13</sup> But, as it became clear that the simplifications of the latter book, especially with regard to the temporal element embodied in the period of production concept, “evaded so many essential problems that the attempt to replace it by a more adequate treatment...raised a host of new questions which had never been really considered and to which answers had to be found[,]” Hayek was unable to proceed immediately to a more detailed account of the cycle. He came to believe that the capital theory upon which the analysis of *Prices and Production* was founded was too simple: “I can see in the simplified form in which I had to use it in my former book it may be more misleading than helpful” (Hayek 1939, 7; quoted in White 2007, xxii). The consequences of the simplifications of *Prices and Production*, especially with regard to capital, could not be ignored (Hayek 1941, 4).

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<sup>13</sup> On the circumstances of the hurried preparation of Hayek's invited LSE lectures in 1931 (ultimately published later in the same year as *Prices and Production*), see Hayek (1931, 191-196) and Caldwell (2004, 171-173).



The theoretical gaps in *Prices and Production* include a theory of the bust or depression phase of the cycle, or as Hayek calls it, a theory of the “economics of decline” (1932, 137), and a theory of what it means to maintain capital intact over time.<sup>14</sup> Hayek attempted to settle this latter question on a number of occasions (1935, 1936, 1941). Indeed, he dedicated the better part of the 1930s to reconstructing Böhm-Bawerk’s theory of capital so as to make it a more appropriate basis for an explanation of the cycle.

However, Hayek was far from satisfied with the results of this endeavor. The preface to *The Pure Theory of Capital* is little more than an apology for the inadequacies of the theory *despite its massive complexity*. In particular, Hayek perceived the flaws of *The Pure Theory* to lie “in the fact that...it leaves some problems of real importance unsolved” (1941, 5). Though “[i]t would undoubtedly be highly desirable...that this should be done once and for all...I can only plead that I have grappled honestly and patiently with what even now appears to me to be by far

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<sup>14</sup> As argued in Scheall (Manuscript A), perhaps the most important lacuna in Hayek’s early business cycle theory, at least from the perspective of philosophy of science, is its lack of implications for the temporal spans that separate the causes posited from their purported effects. In the absence of these temporal parameters, the theory offers no guidance as to which phenomenal patterns qualify as instances of economic-cyclical processes. In other words, the absence of temporal considerations makes the theory untestable. Of course, Hayek was well aware of the fact that economists cannot (and should not try) to say sooths of a temporally-indexed variety, so this is not intended as a criticism; however, as argued in *Ibid.*, this non-testability, together with a similar non-testability of the theory of Hayek’s great rival, Keynes, has profound consequences for the prospects of settling disagreements between defenders of the respective theories.

the most difficult part of economic theory, and that the present book with all its shortcomings is the outcome of work over a period so prolonged that I doubt whether further effort on my part would be repaid by the results” (*Ibid.*). Indeed, the limited discussion of the trade cycle such as it appears in the fourth part of *The Pure Theory* remains “condensed and sketchy” (*Ibid.*), despite the fact that an elaboration of an improved theory of the cycle was the original motivation for writing the book! For all intents and purposes, from that point forward, Hayek-the-technical economist was dead—long-lived was Hayek-the-philosopher.

For Hayek, in the last analysis, theorizing about the dynamics of the cycle meant either constructing an overly simplified theory which, given the complexity of the phenomena, would be “probably of necessity false” (Hayek 1964a, 28) or developing a highly elaborate theory – like the one he constructed in *The Pure Theory of Capital* – “so damned complicated it’s almost impossible to follow it” (Hayek 1994, 141). In an effort to defend the original cycle theory of *Prices and Production* from the criticisms directed at its capital-theoretic core, Hayek expounded a theory that was ultimately too complicated to serve as the basis of an extended explanation of cyclical phenomena and yet, at the same time, was too simple an explanation of the phenomena of capital.

Stated plainly, it seems that Hayek’s early business cycle project failed to bear the fruit he expected of it (at least in part) because, while still failing to express the complexity of the phenomena, the theoretical tools he developed started to outrun his cognitive abilities. Hayek had taken these tools as far as he could – which may have been as far as they could have been taken by anyone – but not far enough to complete the capital theory project, much less the elaborated theory of the cycle. Of course, given Hayek’s methodology, failure is what we should expect when tools appropriate only for the analysis of simpler phenomena are applied to more complex

phenomena. That is, Hayek's later methodology of sciences of complex phenomena implies an explanation of the failure of his earlier business cycle project and, to the extent this failure was a prerequisite for his epistemic turn, contributes to an explanation of the latter.

The second element of Hayek's methodology that is relevant to an explanation of his epistemic turn concerns his argument that moving to a system of "higher-level theories" that subsume some of the otherwise inexplicable complexities of the relevant phenomena is an appropriate response to the difficulties of theorizing about complex phenomena:

"Though we may never know as much about certain complex phenomena as we can know about simple phenomena, we may partly pierce the boundary by deliberately cultivating a technique which aims at more limited objectives—the explanation not of individual events but merely of the appearance of certain patterns or orders. Whether we call these mere explanations of the principle or mere pattern predictions or higher-level theories does not matter. Once we explicitly recognize that the understanding of the general mechanism which produces patterns of a certain kind is not merely a tool for specific predictions but important in its own right, and that it may provide important guides to action (or sometimes indications of the desirability of no action), we may indeed find that this limited knowledge is most valuable" (1964a, 40).

Hayek (1961) considered "those systems of equations in which mathematical economists describe the conditions of market equilibrium" to epitomize such a higher-level theory. But, of course, the methodology of sciences of complex phenomena, which takes as its elements the relations between theories and *their* elements is of a yet higher order than the theories it

encompasses.<sup>15</sup> In Hayek’s case, the move to a higher-level theory involved a shift from the (theoretical) plane of generalizations about economic phenomena to the (methodological) plane of generalizations about the former kind of theoretical generalizations.

I have argued elsewhere that Hayek’s methodology implies an explanation of episodes of disequilibrium that includes (the substance, but not the methodology, of) his earlier theory of the cycle as a special case.<sup>16</sup> This move (which was not necessarily a conscious one on Hayek’s part—recall the role of tacit knowledge in belief revision) to the methodological plane lead to an explanation of industrial fluctuations that allowed Hayek to rise above the very intricate details that so heavily weighed upon his early account. The methodological explanation attributes episodes of disequilibrium to action under a “pretence” that the complexities of economic phenomena are cognitively tractable. More to the point, Hayek’s (1975) argument is that action – usually, political action – intended to maintain (or restore) a state of economic equilibrium, or of “full employment,” which is founded on belief in a simpler-than-required theory of the economy, is likely to interfere with the equilibrating tendency of the price system and, thus, lead away from rather than toward equilibrium. Whatever the possible demerits of such an explanation of economic fluctuations, it successfully abstracts from many of the seemingly intractable intricacies that undermined Hayek’s technical economics.

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<sup>15</sup> “The term ‘higher level regularities’ which I have used to describe the content of such statements about the general character of an order is meant to indicate that it does not refer to relations between particular elements of such an order, but only to relations between relations, or even relations between relations between relations between the elements” (Hayek 1961)

<sup>16</sup> See Scheall (Manuscript C)

Thus, the methodology of sciences of complex phenomena both explains and can be read as a reaction to the failure of the business cycle project. One aspect of the relevant belief revision was the development of a methodological perspective which, among other virtues, explained the failure of Hayek's technical economics and was less vulnerable to the various shortcomings of the latter.

#### IV. CONCLUDING REMARKS

The picture that emerges from the current analysis is that of an inquirer who, disappointed with the results of the mode of theorizing he had theretofore employed, shifted to a mode that both explained why his former theory was bound to disappoint and avoided similar problems. All of this is perfectly in keeping with Hayek's evolutionary account of the process of an organism's belief revision in the face of disappointed expectation.

It is important to emphasize that the current argument does not terminate in a full-fledged historical explanation of Hayek's epistemic turn, but rather in (what Hayek called) an *explanation of the principle* "on which a certain mechanism operates" ([1964] 1967, 37). The relevant mechanism is the one by which Hayek "turned" from technical economics to philosophy; more generally, it is the mechanism by which such "turns" might occur in any scholar's career. Stated another way, the present explanation is a *theory* (which neither aspires to nor attains comprehensiveness) of the circumstances that might lead to such transitions in a scholar's beliefs over time. The principle posited to explain such turns emerges from the interrelations between three sub-principles: the principle of belief revision in the face of disappointed expectations that emerges from Hayek's cognitive psychology; the theory of the consequences of a mismatch between the complexity of a phenomena and our cognitive

capacities; and the principle that such a mismatch can be (at least in part) mitigated by moving to a higher-plane of analysis.

In keeping with Hayek’s methodology of sciences of complex phenomena, the distance in logical space that separates this explanation of the principle from a historical explanation must be filled in with the relevant empirical data (and, given the non-exhaustiveness of the aforementioned principles – i.e., given that the current explanation instantiates the “theory problem”<sup>17</sup> – additional theoretical considerations that I’ve failed to uncover). It is the business of the historian of ideas – rather than that of a *theoretician* of the history of ideas (the guise I’ve adopted here) – to discover and populate with empirical details a theoretical model like the one offered in the current paper. I’ve traversed a bit of the distance to a historical explanation by adducing evidence that Hayek was indeed disappointed that his early theory failed to reflect the complexity of the relevant phenomena and that Hayek’s methodology refers to higher-order generalities than does his early cycle theory. Nonetheless, the “data problem” remains: this evidence could surely be strengthened and more, and unique, evidence could be adduced. However, if Hayek’s methodology is correct, a comprehensive enunciation of the relevant data is beyond our cognitive abilities—all explanations are circumscribed relative to the events they aim to explain.

What is especially interesting about the argument of the current paper is that, if it is sound, Hayek both experienced *and* offered materials relevant to a theoretical explanation of such a “turn”. I believe the principles offered in the present paper fit Hayek’s career well or, more exactly, that the facts of Hayek’s career do not undermine the posited explanation of the principle. It would be a further test of the theory to consider in the light of the present

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<sup>17</sup> See Scheall (Manuscript B)

explanation the careers of other scholars (e.g., Hayek's distant cousin, Ludwig Wittgenstein) whose beliefs are alleged to have "turned" over time.

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