**The Pragmatics of Scientific Knowledge:**

**Howard Stein’s Reshaping of Logical Empiricism**

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One preoccupation of Howard Stein’s work is the nature of *understanding*, especially the dialectic between knowledge and understanding. He says, for instance, that “understanding without knowledge is blind; knowledge without understanding is impossible” (Stein 2004, p. 135). What does he mean by “understanding”? He makes no attempt to supply a precise or systematic account,[[1]](#footnote-1) though he revisits it again and again from different angles. Indeed, the conception that emerges from these probings seems inimical to *any* sort of definitive formula. This is most explicitly a theme in a 1998 colloquium talk entitled “How Does Physics Bear upon Metaphysics, and Why did Plato Hold that Philosophy Cannot Be Written Down?” Here William Blake, of all people, is quoted to make the point that understanding *evolves* with knowledge: “If others had not not been foolish, we should be so.” And thus: “Reason or the ratio of all we have already known, is not the same that it shall be when we know more.” (Stein 1998, p. 3) More specifically, Stein focusses on the conception of knowledge and understanding expressed — by Plato or someone in his name — in the Seventh Letter, where, as Stein summarizes it,

. . . the whole apparatus of what we might call “object-semantics,” involving both linguistic signs and ordinary things (Plato’s “images”), cannot suffice to determine meaning and truth, without some essential involvement of the *language users* and their *conceptions and beliefs*; and the writer goes on to assert that this determination can occur reliably only in *discussion*, with questioning and answering “free from envy” — and that, indeed, over a long time: a process which, in favorable conditions, can lead to a shining forth of the light of understanding and intelligence (φρονησις and νους). (ibid.)[[2]](#footnote-2)

Later in the talk, when discussing Maxwell’s transformation of the fundamental concepts of Newtonian physics, Stein comes back to this idea. He rejects two popular modern ways to avoid a role for understanding: On the one hand, he sees no point in trying to specify an “ontology” for the theory, i.e. to establish whether Newton’s use “*singled out a certain ‘natural kind’* as the ‘reference’ of his term ‘quantity of motion’, and, if so, *whether that natural kind includes* the electromagnetic momentum.” On the other hand,

I would repudiate in the strongest terms the suggestion that we are here dealing with a mere “convention,” or even an *evasion* performed in order to hide the fact that “the laws of physics *lie*.” I think Plato was quite right: it’s not the nouns and verbs alone that do the job; we have to know how to *think* with and about them. There is here a real conceptual change; but one *sees* — shall I say, *grasps by* νους — that the revised conservation law is as it were the legitimate successor to the original. (ibid., p. 20)

And whatever may be said about a possible “end of physics,” a completed knowledge (however unlikely) would not mean a completed understanding; “there is no reason to believe that the importance of continued reflection *on the position attained in knowledge* will itself terminate.” Stein concludes his talk with a comment on two remarks of Aristotle, first that “it is hard to know whether one knows” (Posterior Analytics I 9) and second that “philosophy begins in wonder, but ends in the contrary state.” (Metaphysics I 2). On which he comments:

Plato never wrote the hinted-at sequel to the *Theaetetus*, *Sophist*, and *Statesman*, to have been called the *Philosopher*. I have long cherished the fantasy, anachronistic though it be, that in that work Socrates, questioning Aristotle, would have led him to admit that it is impossible to know whether one knows, and that if wisdom is the contrary state to wonder, then philosophy never ends. (ibid., p. 25)

But this may seem only to make more puzzling the question I propose to address here: the question how we should place him, how *we* should in turn “understand” Howard Stein’s work, locate him in intellectual history. Stein’s hesitation, shared with Plato, about the degree to which philosophy can be “written down,” makes this question thornier and more difficult, as it seems to put him at odds with the tradition that he himself has repeatedly declared an abiding loyalty to. “I have to confess,” he wrote back in 1972, putting his cards on the table in advance of some critical remarks,

that with respect to the Quinean gospel I am something of a heathen; or, to be more precise, in the face of the new dispensation I am a Jew, and maintain allegiance to the Torah — though acknowledging the need for deuteronomic reforms. . . (Stein 1972, p. 621)

And he has occasionally reiterated this allegiance to suitably modified forms of “the *mitzvoth* of logicism and empiricism” since then, not least in a paper specifically taking Carnap’s side in the debate with Quine (Stein 1992). How can that be? How can the proponent of what cannot be “written down,” the admirer of Plato, who insists on the indissoluble unity of scientific knowledge and philosophical understanding, have any sympathy at all for the strident propagandists who, in some of their best-known pronouncements, proclaimed the end of philosophy and its replacement by formalized *Wissenschaftslogik*? How can it be that — as I will argue — he is best understood as having further developed, indeed reshaped, the logical empiricist tradition?

**1. Carnapian Pragmatics**

To answer this question it will be useful to take a step back, and, first of all, say something about Carnap, especially the later Carnap, who was Stein’s teacher at the University of Chicago in the late 1940s and early 1950s. Stein had been attracted to him from the moment he had read *Foundations of Logic and Mathematics* as an undergraduate, and came to Chicago for graduate work in philosophy partly for that reason.[[3]](#footnote-3) The view of Carnap he developed, based on this first-hand experience, was very different from the logical positivism of philosophical folklore. Stein has given a concise exposition of this view in the paper about Carnap and Quine already referred to (Stein 1992).[[4]](#footnote-4)

He points out here that the scope of *pragmatics* (the study of language in use) — within Carnap’s overall tripartite classification of linguistic theory into syntax, semantics, and pragmatics — is in fact much broader than Carnap himself made it appear. Carnap’s own sketchy characterizations can often make it sound as if pragmatics concerns only “something like the *idiosyncrasies* of use in ordinary language” (ibid., p. 287). Moreover, Carnap’s prejudice (which Stein of course does not share) that “the empirical interpretation of a theory could always be achieved by specifying the *semantics* of the *empirical* part of its language” (ibid., p. 288) led to a further apparent trivialization of pragmatics, as “its role in this fundamental problem of the analysis of ‘empirical content’ would be restricted to the single function of distinguishing, within the language, its ‘empirical part’” (ibid.). In retrospect, Stein says, we can recognize that if we take Carnap’s classification seriously, then pragmatics becomes the scene of conceptual reconstruction; in particular, methodology and epistemology — i.e. most concerns that now fall under the heading of “philosophy of science” more generally and the philosophy of the special sciences — belong to pragmatics (ibid., p. 287).

And this in turn becomes more significant in the light of Stein’s observation that the later Carnap’s central project of *explication* is in critical respects quite different from the earlier reconstructive projects in the Enlightenment tradition, including the Vienna Circle program of rational reconstruction. In that earlier program, better (more precise) concepts were to replace vaguer and less useful concepts, *once for all*. Only two languages were envisioned, the precise language of science and the vague language of everyday life, of which vestiges remain in many parts of actual science, even in the hard sciences. Rational reconstruction was seen as piecemeal replacement of the one language by the other, the gradual extension of more precise scientific concepts into the territory formerly occupied by vague, informal concepts.[[5]](#footnote-5)

What Stein points out (ibid., pp. 291-2) is that under the post-*Syntax* auspices of Carnapian tolerant pluralism, this process is no longer a one-way street; it becomes a *dialectical* interchange of mutual adjustment between the languages employed to articulate theory and to mediate practice. The question about the relation between explicandum and explicatum is not itself *internal*,[[6]](#footnote-6) at least not within the explicatum language:

The explicatum, as an exactly characterized concept, belongs to some formalized discourse — some “framework.” The explicandum. . . belongs ipso facto to a mode of discourse outside that framework. Therefore *any* question about the relation of the explicatum to the explicandum is an “external” question; this holds, in particular, of the question whether an explication is adequate — that is, whether the explicatum does in some appropriate sense fully represent, within the framework, the function (let us say) “presystematically” by the explicandum. (ibid., p. 280)

Thus explication itself — the identification and specification of appropriate explicanda, the invention of new explications for them, and the decision among different explications for a given explicandum — falls within the realm of pragmatics, and indeed becomes perhaps its central component. This already became clear to Stein as a beginning graduate student in the late 1940s.[[7]](#footnote-7) He concluded, accordingly, that for those who took their cue from logical empiricism, specifically from Carnap, pragmatics was “where the action is.” Pragmatics had become the sector of the overall conception of philosophy (or its replacement) entertained by logical empiricism and allied movements to which a great deal of the philosophical weight had shifted. The question of the hour was how to go about developing or creating this new component of what philosophy would henceforth become.

Carnap’s own preferences tended, of course, to the formal. And yet he was not entirely happy with those of his students who followed this path without undertaking the hard propaedeutic labor of clarification — of identifying the explicandum in the first place and provisionally suggesting those of its uses most worth preserving in its subsequent explication. Omitting this essential task of orientation, he thought, could vitiate the result. When his student R.M. Martin,[[8]](#footnote-8) for instance, took some first steps toward a technical apparatus for pragmatics (Martin 1959), Carnap wrote to Evert Beth, the editor of the series to which the book had been submitted, recommending it for publication but affirming that if he himself had been doing such a thing, he would have started by clearing the ground with more preliminary clarifications, as agreement had not yet been reached even there:

Since probably at the present time various authors would choose different concepts and different methods, it might at first be advisable to write a less technical treatise explaining the concepts, defining them informally in such a way as to indicate clearly how they would be defined in a formalised metalanguage without necessarily giving actually their formalised definition, studying alternative explications of the various concepts, etc. (What I have in mind here is roughly analogous to Russell’s preparatory discussions in his *Principles of Mathematics* before writing *Principia* or my preliminary discussions in *Meaning and Necessity* in preparation for a not yet written treatise of a semantics of extensions and intensions in either an extensional or an intensional formalised metalanguage.) (ASP 1958a)

From our vantage point of half a century later, such cautious skepticism looks prescient. The subsequent investigations of concepts such as “belief” — which Carnap himself (1955, p. 250) held to be a basic concept of pragmatics — has revealed that they contain fundamental ambiguities that would need to be decided one way or another before a formalization of any sort could even be contemplated.[[9]](#footnote-9)

So Stein is right to point out that Carnap’s conception of pragmatics can be regarded as having a substantially broader scope than Carnap himself explicitly acknowledged in his scant writings on the subject.[[10]](#footnote-10) Pragmatics in Carnap’s sense, at its most general, was to step into the conceptual location of what has traditionally been the “philosophy of philosophy,” the perennial question of what kind of thing philosophy, or the enterprise of understanding more broadly, can or should be. It is the piecemeal development of a meta-framework in which to investigate the “many problems concerning conceptual frameworks” which, Carnap wrote in reply to a critic, “seem to me to belong to the most important problems of philosophy.” He adds that he is thinking here “both of theoretical investigations and of practical deliberations and decisions with respect to an acceptance or a change of frameworks, especially of the most general frameworks containing categorial concepts which are fundamental for the representation of all knowledge” (Carnap 1963, p. 862).[[11]](#footnote-11)

**2. Stein’s Post-Carnapian Conception of Pragmatics**

Stein’s approach, from the outset, was quite different from Martin’s, or even from Carnap’s own more cautious and clarificatory one. Rather than seeking to identify (and then clarify, let alone explicate) the fundamental concepts of pragmatics from some a priori or “philosophical” — epistemological or common-sense — point of view, he sought out exemplary cases of real-life explication, turning points in the history of knowledge, where alternatives had been available and choices had been made, as springboards for a consideration of what might be said about explication more generally (i.e. of what pragmatics might focus on). In his doctoral dissertation, he proposes “a reflective examination of particular problems instead of a frontal approach to first principles.”[[12]](#footnote-12) The goal was above all to focus attention on these particular episodes, these exemplary instances of explication, as a way of achieving some clarity concerning what we are even talking about. But whether this can lead to iterative clarification and eventual explication is left open. Certainly this has not been achieved so far, and Stein sympathizes, as we saw, with the view regarding what Plato calls “dialectic” that we encountered above in the Seventh Letter — that it is not reducible to λογοι or formulas. Stein sees in the notion of “inquiry” discussed by Galileo in the opening passages of the *Two New Sciences* a kind of dialectic in this sense, and points out that unlike Descartes, Galileo shared with Plato “the conviction that inquiry as such is not a profitable subject for positive doctrinal exposition (‘Discourse on Method’, ‘Rules for the Conduct of the Understanding’), but has to be learned and taught through its practice. . .” (Stein 1974, p. 397).[[13]](#footnote-13)

But although attention was focussed on particular episodes in the history of science, the primary goal was not to contribute to empirical knowledge about the past. The historical episodes were to be used as data for a specific purpose. The conceptual development of physics, for instance, was to be observed not from the viewpoint of the historian seeking to describe or explain the appearance of certain human social and behavioral patterns at certain times, but from the viewpoint of the *physicist* seeking to understand *her own* basic concepts by studying their provenance and genealogy, or more generally their rationale. *Reconstruction* of the historical sequence of explications is required — reconstruction from the viewpoint of *our own* present-day science, since *that* is what we are trying to understand and see in a larger perspective. The precise location, within its social, institutional, and intellectual context, of each step in the explicative sequence leading to the present can assist this effort but is not its goal.

Stein can therefore afford to bracket the methodological quarrels exercising the history of science profession, such as the distinction (and relation) between internal and external histories of science, now often swept under the rug. In discussing past episodes of knowledge-building as an aid to to understanding our present knowledge, we will naturally use a framework in which the full extent of our present knowledge can be articulated.[[14]](#footnote-14) This kind of conceptual history might thus be called, in a nod to the traditional vocabulary, “*strictly* internal” history of science. And while the case has been made that internal and external history of science, in their received polemical senses, cannot actually be precisely distinguished,[[15]](#footnote-15) this consideration does not affect the feasibility of *strictly* internal history, which is well-defined; it is simply the history of scientific concepts and theories — as well as their interplay with empirical testing and observation on the one hand, and with broad “philosophical” policies and values on the other — articulated in the language(s) employed by present knowledge (including both ordinary language and the technical tools available to the more developed branches of the sciences).[[16]](#footnote-16) It is thus a part of (descriptive) pragmatics, and only incidentally of empirical knowledge; it is judged by its usefulness in illuminating the character of the enterprise, not by its adequacy to the entire range of possibly recoverable empirical facts that may have some bearing on it.[[17]](#footnote-17)

Past scientific discourse is interpreted somewhat differently, then, by empirical and “strictly internal” historians of science. The strictly internal historian will not restrict herself to interpretations within the past scientist’s own documentably available conceptual vocabulary, but will also consider interpretations that permit the past scientist to have been trying to articulate ideas for which there *was as yet no* available vocabulary. Thus Stein uses the vocabulary of modern four-dimensional affine geometry (with additional structures to characterize “temporal distance” and “spatial distance”), for instance, to pinpoint the precise degree to which Newton understood (and could not quite have understood) that he needed absolute time but not absolute space for a fully consistent dynamics.[[18]](#footnote-18) Another example is Newton’s rather obscure pronouncement on the nature of spatial extension in the untitled fragment beginning with the words “De gravitatione. . .,” in which he rejects the view that it is either a “substance,” an “accident” (attribute), or a “nonentity,” and asserts rather that it “has its own way of existing, which fits neither substances nor accidents” (Newton 2004, pp. 21ff.). This passage has been much puzzled over, and generally been interpreted in theological terms.[[19]](#footnote-19) Stein, however, reviewing it in conjunction with other Newtonian texts, concludes that it

. . . can be taken, in rather modern terms, as saying that space is a *structure*, or “relational system,” which can be conceived of independently of anything else; its constituents are individuated just *by their relations to one another, as elements of this relational system*. But the system, or its constitutive elements, nonetheless can *and must* “affect,” in the appropriate way, all things that exist: all existing things have spatial and temporal relations to one another by virtue of their having, each one of them, the appropriate kind of relation to the parts of space and of duration. . . (Stein 2002, p. 272)[[20]](#footnote-20)

For all these differences between Stein’s “strictly internal” history of science, as I have called it, and the empirical history of science as it is now programmatically pursued,[[21]](#footnote-21) Stein’s approach to what Carnap called pragmatics also differs, as we have seen, from Carnap’s own in many respects. However, there are important underlying continuities. Stein’s pragmatically-motivated strictly internal history of science may be seen as a response to, and correction of, precisely those elements in Carnap’s approach to pragmatics that turned out not to work very well or, at least, have — so far — not in fact shown themselves amenable to further development: (a) the a priori choice of key pragmatic concepts, in abstraction from any actual episodes of scientific development; (b) the assumption that pragmatics can be formalized, without considering whether the actual explicanda, as evident in the historical development of particular scientific disciplines, lend themselves to such treatment (e.g. whether they contain an element of knowledge-how as well as knowledge-that); and (c) the concomitant overreliance on logic to specify not only theoretical frameworks but also their experimental and practical application.

**3. The Middle Way**

“Understanding” also has an important role as a placeholder, or explicandum, in Stein’s Carnapian conception of the *provenance* of our knowledge. One can think of any such conception of provenance as occupying a point along a continuum between two extremes, which one might call “constructivism” at one end, and “naturalism” at the other. At the one extreme our knowledge is imposed by us on nature, at the other extreme it is imposed by nature on us. I like to think of Helmholtz as paradigmatically searching for a middle way between these extremes,[[22]](#footnote-22) and of Carnap as continuing that effort. Stein continues that same tradition, seeking out the *dialectical interplay* between our constructions and nature’s impositions. Think of Kuhn at the constructivist end, and Quine or perhaps Mark Wilson at the naturalist end. What is the relation between explicandum and explicatum at the two ends? At the constructivist end (in Kuhn’s version, at least), successive explicata are incommensurable, not just with each other, but with the original explicandum as well. They are separate concepts, articulated in distinct and not fully intertranslatable languages. At the naturalist end, successive explicata are all run together — with the explicandum as well — into a *single* concept; explication (“regimentation”) simply extends the original concept’s range of application. So at both ends, there is no *choice* among concepts. At the constructivist end, they are imposed by the culture they are embedded in; at the naturalist end, they are imposed by nature, and our constructions, insofar they are in accordance with nature, must ultimately be equivalent (i.e. must ultimately *not* be constructions, in the sense intended here).

The Carnapian middle way grants to the constructivist that there is a language barrier to overcome; explicandum and explicatum (as well as successive explicata) are different concepts residing in different languages. But it is also granted to the naturalist, on the other hand, that there is an important *connection* between successive members, and often among all members, of an explicative genealogy. Stein sees the continuity among an explicandum and its successive explications as residing in a structure, a “framework,” a “form.” What is it, he asks, that “*de facto*, in the history of physics to date, has tended most to *persist as stable* (and, it appears, reliable) in what we think we know about the world”? It is certain “forms,” he says, or certain aspects of the forms “that are characterized by what Carnap called ‘frameworks’”, i.e. it is what has survived translation over the generations:

In fact, if we ask, say, of the physics of the end of the seventeenth century, what of all it told us about the world we can still regard as ‘true’ or as having proved itself ‘real’, the answer is. . . striking: Not Newton’s hard particles, not Leibniz’s material continuum, not Huygens’s ether — indeed, hardly anything to which most philosophers would accord ‘ontological’ status. . . And of fundamental processes: no impacts of atoms, no pressures of continuous media, no immediate and instantaneous actions at a distance — indeed, no instants at all! And yet, although Huygens’s ether has gone, the ‘form’ of the propagation of light to which he contributed a first crude sketch is still discernible in the theory of electromagnetic waves, and through that theory — again transformed — in the quantum theory of electrodynamic and optical processes. (Stein 1994, p. 652)[[23]](#footnote-23)

Each successive explication of a concept that has undergone such a series generally progresses in the direction of greater generality, embeddedness in a framework that makes clearer and more transparent its relations to a larger and more comprehensive system of other concepts.[[24]](#footnote-24) Indeed, the striving for such generalization often motivates these successive reformulations. Stein cites the example of Newtonian gravitational theory:

A whole series of classical investigators, including Lagrange, Gauss, Hamilton, and Jacobi, found alternative ways of formulating the dynamical law of a Newtonian system. These formulations are not all equivalent; rather, they all generalize a certain common domain, and generalize it in different directions. And the generalizations have very important physical significance; for example, the Maxwellian electromagnetic field is not representable as a Newtonian system, but it is representable as a Lagrangian or Hamiltonian one. But of course, Lagrange and Hamilton were consciously building upon and transforming Newton’s principles. The result is a transformation of the concept of a ‘natural power’ or ‘force of nature’: such a force is now to be given, not by a law of motive force characterizing action-reaction pairs, but (for instance) by a Hamiltonian function. (ibid., p. 651)

In what sense is Newtonian mechanics still ‘the same’ theory when it is translated into the language of Lagrangeans or of Hamiltonians? Only by virtue of the relation that holds between an explicandum and its successive explicata.

That words will not do the job on their own Stein illustrates with an example from Hilary Putnam, who — back in the good old days when he was a realist of sorts — used the notion of the ether, in contrast with that of atoms, as an example of a scientific term that failed to “refer.” There is no such thing as the ether, he said, whereas there are atoms. Larry Laudan, on the other hand, used this same example to *attack* realism, on the grounds that it showed the lack of connection between “instrumental success” and “real reference.” “For my part,” retorts Stein, “I throw up my hands at this:”

Why should we say that the old term “ether” failed to “refer”? — and that the old term “atom” did “refer”? Why, that is, except for the superficial reason that the word “atom” is still used in textbooks, the word “ether” not?. . . in brief: our own physics teaches us that there is *nothing* that has all the properties posited by nineteenth-century physicists for the ether *or* for atoms; but that, on the other hand, in *both* instances, rather important parts of the nineteenth-century theories are correct. . . The two cases [“ether” and “atom”] are, in my view, so similar, that the radical distinction made between them by the referential realists confirms in me the antecedent suspicion that this concern for reference — and associated with it, another Quinean motif, the concern for what is called the “ontology” of theories — is a distraction from what really matters. (Stein 1989, pp. 57-8)

But what sort of question is this, anyway? Why should it matter whether we distinguish explicanda from explicata, like Kuhn, or run them together, like Quine? Who cares where we draw boundaries among concepts? Well, I submit that the two extremes fundamentally misrepresent the enterprise. They both regard it simply as a blind, impersonal *process*, omitting to connect it with the human aspiration to know and understand — the very aspiration expressed so eloquently near the beginning of our philosophical tradition in Plato’s cave story. At the one extreme, this process is viewed as a socio-cultural one, of competition among different subcultures within a scientific discipline, but with no room for conceptual mediation among them in which the priority accorded to knowledge and understanding could find expression. At the other extreme, the process is seen as a gradual, step-by-step adjustment of human conceptual systems to a fixed external reality. So again, pragmatic meta-conceptual discourse has no role to play. Room for such discourse can only be found *between* the two extremes, if we allow that proposed new concepts and languages are indeed radically different from their naturally-evolved predecessors, *and* that these new concepts and languages are nonetheless recognizeable — if only by νους! — as legitimate successors to their explicanda.

This is not by any means to deny that there *is* an element of blind process to the development of knowledge. That process (in all its dimensions — sociocultural, political, biological, adaptive) can be, and is, the object of empirical study, resulting in empirical history of science. But that process is *also* an object of normative consideration, where the question is not “what *is* that process, left to itself?” but “what should we, in our present situation, *make* of that process, given that we are engaged in it, and have choices?” Conceptual engineering, like any other form of engineering (or indeed any form of creative endeavor), can apply only to a context where there are choices and scope for active intervention. And conceptual engineering — pure pragmatics — needs descriptive pragmatics as a relevant context for such creative generation of explicata and the guidance of choices among them.

But does, after all, the distinction between an explicandum and its explicata reflect a “real” distinction, a genuine distinction in “reality”? This is something many naturalists have been concerned to deny.[[25]](#footnote-25) And they are right. The distinction is *not* a “real” one — it expresses a *decision* to leave room (in the pragmatic context) for the aspiration expressed in Plato’s cave story. Except that where Plato saw precise reasoning as a gateway from the flickering appearances of everyday life to a genuine “reality” in the realm of the forms, Carnap and Stein (in the tradition of Helmholtz) see the realm of forms as *created by us* (by our sensory processors, to begin with, and on that basis by our intellects) to both serve our practical needs *and* our aspirations of understanding the world we live in. But the process of creation Stein conceives as a long, sustained, cumulative one; there is no single act of creation. The realm of forms is not just a little nomadic tent camp that every succeeding generation tears down and rebuilds from scratch on new ground, as Kuhn would have it. It is an enterprise with very significant underlying continuities, a building that succeeding generations contribute to and refashion, but which remains recognizably the same structure over the generations, even if its original form is no longer discernible in a later one at first glance, and the continuity reveals itself only to close historical study.

A Quinean naturalist might acknowledge the desirability of the Platonic aspiration, expressed in the distinction between explicandum and explicatum, but might still demur, on the grounds that the proposed distinction simply won’t wash — it flies in the face of the facts. There just is no clear line to be drawn, she might argue. And in any case, Carnapian explicative deliberation according to Stein[[26]](#footnote-26) is, as we have seen, *external* to the language of the explicatum, and it is a fundamental tenet of Quinean naturalism that there can be no such thing as an external question. There are only *internal* questions; there is no stepping outside our “conceptual scheme” to some “transcendental” level.[[27]](#footnote-27) There is no Archimedean point outside our discourse from which we can appraise or remodel it, in this view, no drydock in which we can rebuild our boat from scratch on a new plan.

And from this perspective, the Quinean-Wilsonian naturalist might ask, “Wherein precisely does the supposed connection between an explicandum and its successive explications actually consist, then?” Stein acknowledges, as we have seen, that *de facto* no one has found a way to spell this connection out; the ability to recognize it remains, to some degree, a form of knowledge-how that can be conveyed among members of our species only by a kind of apprenticeship, under conditions of a certain receptivity. The connection can only be discerned, in the end, by a kind of skill or craft based on a (perhaps fortuitously) evolved human faculty (νους) that must however be specially tuned or trained in the course of long and arduous *Lehr- und Wanderjahre*. This does not, in itself, make the connection a psychological or sociological one. On the contrary, part of what makes this connection so mysteriously fascinating is precisely that it (or some essential component of it) evidently lies in a relation among the successive explications themselves. This statement has a Platonic ring, certainly, and a Kuhnian constructivist might object, on the other side, that it is nothing in the end but an expression of unreflective realism. But actually it does *not* imply that the successive concepts “refer to” any *thing* (let alone everything) in common — though of course they are “successive” partly by virtue of explaining many of the same data.[[28]](#footnote-28) And it seems better to acknowledge that there *is* some connection among successive explications than to turn a blind eye to the plainly obvious.

**4. Logical Empiricism after Stein**

Logical empiricism is widely considered dead.[[29]](#footnote-29) In Howard Stein, though, we find a form of it that has survived well into the twenty-first century. How should we describe the form of it that he represents? Is it a “liberalized” logical empiricism, in some sense? Certainly Stein does not eschew or despise “metaphysics” as unconditionally as Carnap did. But the metaphysics he condones is not metaphysics in the sense of Descartes or Hegel.[[30]](#footnote-30) It is metaphysics in the sense of Helmholtz, and in the sense of Carnap’s “external” questions, practical questions about the choice among categorial frameworks, as we saw in the passage quoted above. It is a matter of artifice rather than knowledge. On this issue, there is no difference between Carnap and Stein that makes a difference.

Nor is there much of a difference about empiricism. True, Stein’s empiricism is shorn of the ambition of articulating a unique characterization of “knowledge” — but that was also true of Carnap, after the *Logical Syntax*. What about Carnap’s continuing efforts to define “empirical content” precisely? Stein certainly regards this particular project as having failed, but still thinks the *question* of characterizing empirical content is important, even if we have no clue how to go about answering it at the moment. And he certainly thinks that it can be addressed within the broader architectonic of Carnap’s approach without following Carnap’s *particular* route of attempting to arrive at an explicit definition of “empirical content.” In fact he follows a very Carnapian line even when rejecting Carnap’s particular route: he attaches more weight to the structures or “forms” that survive in our appraisal of what is genuinely empirical knowledge over the generations, rather than the *entities* to which those theories are taken at various times to “refer.”

Regarding *logicism*, there is a parallel reservation. Stein does not think that logic has the sort of universal applicability Carnap attributed to it. He regards it as misapplied (as just noted) to the problem of empirical content. More generally, he considers the notion of “correspondence rules” abjectly oversimplified, and thinks that applied and experimental physics are simply not reducible to *frameworks* in the way Carnap sought to achieve for physics as a whole — or at least not in the simple way that the early Carnap suggested with his “phenomenal-theoretical dictionary” as the “second volume” of a three-volume compendium of all physics, with volume 1 being the axioms and volume 3 being the evidence.[[31]](#footnote-31) Here, too, though, this could be regarded as a difference in *descriptive* pragmatics that the actual, historical Carnap could have accommodated, just as he in fact accommodated Kuhn’s portrayal of scientific revolutions with some enthusiasm. In fact Stein’s descriptive pragmatics is far closer in spirit to Carnap’s own than was Kuhn’s, and would presumably have been met, had Carnap had the opportunity of comparing them, with an even higher degree of enthusiasm.

Where *is* the difference, then? I think the really important difference is that Stein made explicit, and considered the consequences, of the *dialectical* nature of Carnap’s later thought, the mutual feedback between the theoretical and the practical realms. And this *does* have a number of important implications:

First of all, it leads Stein to a very different approach to pragmatics from Carnap’s own. Carnap had thought that the central concepts of pragmatics could be identified a priori, as it were, or that they could be chosen — like the concept of “belief,” for instance — on the basis of a casual survey of the situation of a language user in relation to concepts. In Stein’s approach they arise immanently from the examination of actual episodes of explication, in which we see what problems were actually being addressed by those who laid the foundations for the conceptual frameworks we use today. No attempt is made to regiment what appears here into a fixed set of *Grundbegriffe*. The complexity and context-sensitivity of these problem-solving episodes leads Stein, like Plato or Galileo, to doubt at least provisionally whether it is possible to codify explicit rules for anything like Plato’s “dialectic” or Galileo’s “inquiry”.

This can be seen, from a certain angle, as just a difference of emphasis. Carnap’s distinction between semantics and pragmatics, and especially his distinction between pure and descriptive pragmatics, certainly leaves *room* for a distinction between knowledge and understanding. But in the few places where he addresses this question directly, he certainly seems unwilling to give a serious role to knowledge-how in relation to knowledge-that. In *Foundations of Logic and Mathematics*, for instance, Carnap admits that understanding has a component of knowledge-how, but the upshot of this passage (Carnap 1939, pp. 67-9) is more to *trivialize* knowledge-how, and to downplay the importance of understanding more generally, than to acknowledge the interdependence of knowledge and understanding as Stein does (e.g. in the quotation at the beginning of this paper). Stein can be seen as simply upgrading the relative role of understanding, giving it a higher value (and paying more attention to it), without actually changing much about the *cognitive* appraisal of the situation. It is admittedly hard not to see this as quite a fundamental difference of view.

This difference is discernable less in explicit doctrinal differences than in stylistic terms, in Stein’s exploratory, tentative, and dialogical approach to his themes, rather than the flat and systematic expository style favored by Carnap. Here again Stein follows Plato and Galileo in seeing genuine insight as arising from the interplay of contrary views and juxtaposed perspectives — from a three-dimensional view of the subject — rather than from explicit doctrinal exposition.

But this amounts to a development and re-shaping, not to a fundamental change in direction. Logical empiricism has been seen as a successor to the Enlightenment; it began as a direct continuation. By pushing certain Enlightenment themes to an extreme, with the latest logical tools, logical empiricism was ideally positioned to expose the critical weaknesses of the Enlightenment, and it began to fix them, especially in Carnap’s shift first to the syntax view and then to his later dialectical one. Stein has taken this repair process one step further and, by his example, set a new agenda for philosophical history of science.

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**Literature Cited**

Audi, R, ed. 1999 *The Cambridge Dictionary of Philosophy*, 2nd edition (Cambridge: Cambridge University Press).

Bird, G. 2006 *The Revolutionary Kant: A Commentary on the* Critique of Pure Reason (LaSalle, IL: Open Court).

Carnap, R. 1923 “Über die Aufgabe der Physik und die Anwendung des Grundsatzes der Einfachstheit” *Kant-Studien* 28, pp. 90-107.

Carnap, R. 1939 *Foundations of Logic and Mathematics* (Chicago: University of Chicago Press).

Carnap, R. 1946 *Meaning and Necessity: A Study in Semantics and Modal Logic* (Chicago: University of Chicago Press).

Carnap, R. 1950 *Logical Foundations of Probability* (Chicago: University of Chicago Press).

Carnap, R. 1955 “On Some Concepts of Pragmatics” *Philosophical Studies* 6, pp. 89-91, reprinted in *Meaning and Necessity*, 2nd ed. (Chicago: University of Chicago Press 1956), pp. 249-51.

Carnap, R. 1963 “The Philosopher Replies” in P. Schilpp, ed. *The Philosophy of Rudolf Carnap* (LaSalle, IL: Open Court), pp. 859-1013.

Carus, A.W. 2007 *Carnap and Twentieth-Century Thought: Explication as Enlightenment* (Cambridge: Cambridge University Press).

Carus, A.W. (forthcoming) “History and the Future of Logical Empiricism” in E. Reck, ed. *The Historical Turn in Analytic Philosophy* (Basingstoke: Palgrave Macmillan).

Chang, H. 2004 *Inventing Temperature: Measurement and Scientific Progress* (Oxford: Oxford University Press).

De Regt, H.W. and D. Dieks 2005 “A Contextual Approach to Scientific Understanding” *Synthese* 144, pp. 137-70.

Donald, M. 2001 *A Mind So Rare: The Evolution of Human Consciousness* (New York: Norton).

Gadamer, H.-G. 1965 *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik,* 2nd edition (Tübingen: Mohr-Siebeck).

Gendlin, E.T. 1995 “Crossing and Dipping: Some Terms for Approaching the Interface between Natural Understanding and Logical Formulation” *Minds and Machines* 5, pp. 547-60.

Hesse, M. 1970 “Hermeticism and Historiography: An Apology for the Internal History of Science” in R.H. Stuewer, ed. *Historical and Philosophical Perspectives of Science* [Minnesote Studies in the Philosophy of Science, vol. V] (Minneapolis: University of Minnesota Press), pp. 134-60.

Humphreys, P. “Analytic and Synthetic Understanding” in J.H. Fetzer, ed. *Science, Explanation, and Rationality: The Philosophy of Carl G. Hempel* (Oxford: Oxford University Press), pp. 267-286.

Janiak, A. 2008 *Newton as Philosopher* (Cambridge: Cambridge University Press).

Jeffrey, R. 1994 “Carnap’s Voluntarism” in D. Prawitz, B. Skyrms, and D. Westerståhl, eds. *Logic, Methodology, and Philosophy of Science IX* (Amsterdam: Elsevier), pp. 847-66.

Kosso, P. 2007 “Scientific Understanding” *Foundations of Science* 12, pp. 173-88.

Kripke, S. 1979 “A Puzzle about Belief” in A. Margalit, ed. *Meaning and Use* (Dordrecht: Reidel).

Martin, R. 1959 *Toward a Systematic Pragmatics* (Amsterdam: North-Holland).

Newton, I. 2004 *Philosophical Writings*, ed. A. Janiak (Cambridge: Cambridge University Press).

Quine, W.V.O. 1951 “Two Dogmas of Empiricism” reprinted in *From a Logical Point of View* (New York: Harper 1953), pp. 20-46.

Quine, W.V.O 1962 *Word and Object* (Cambridge, Mass.: MIT Press).

Richardson, A. 2007 “ ‘That Sort of Everyday Image of Logical Positivism’: Thomas Kuhn and the Decline of Logical Empiricist Philosophy of Science” in A. Richardson and T. Uebel, eds. *The Cambridge Companion to Logical Empiricism* (Cambridge: Cambridge University Press), pp. 346-69.

Schurz, G. and K. Lambert 1994 “Outline of a Theory of Scientific Understanding” *Synthese* 101, pp. 65-120.

Shanker, S.G. 1995 “The Nature of Insight” *Minds and Machines* 5, pp. 561-81.

Shapin, S. 1992 “Discipline and Bounding: The History and Sociology of Science as Seen through the Internalism/Externalism Debate” *History of Science* 30, pp. 333-69.

Smith, G.E. 2010 “Revisiting Accepted Science: The Indispensability of the History of Science” *The Monist* [this issue].

Stein, H. 1958 *An Examination of Some Aspects of Natural Science* (Ph.D. Dissertation, Department of Philosophy, University of Chicago).

Stein, H. 1967 “Newtonian Space-Time” *The Texas Quarterly* 10, pp. 174-200.

Stein, H. 1972 “Graves on the Philosophy of Physics” *Journal of Philosophy* 69, pp. 621-34.

Stein, H. 1974 “Maurice Clavelin on Galileo’s Natural Philosophy” *British Journal for the Philosophy of Science* 25, pp. 375-97.

Stein, H. 1977 “On Space-Time and Ontology: Extract from a Letter to Adolf Grünbaum” in J. Earman, C. Glymour, J. Stachel, eds. *Foundations of Space-Time Theories* [Minnesota Studies in the Philosophy of Science, vol. 8] (Minneapolis: University of Minnesota Press), pp. 374-402.

Stein, H. 1988 “*Logos*, Logic, and *Logistiké*: Some Philosophical Remarks on the Nineteenth-Century Transformation of Mathematics,” in W. Asprey and P. Kitcher, eds. *History and Philosophy of Modern Mathematics* [Minnesota Studies in the Philosophy of Science, vol. 11] (Minneapolis: University of Minnesota Press), pp. 238-59.

Stein, H. 1989 “Yes, but. . .: Some Skeptical Reflections on Realism and Anti-realism” *Dialectica* 43, pp. 47-65.

Stein, H. 1990 “Eudoxos and Dedekind: On the Ancient Greek Theory of Ratios and Its Relation to Modern Mathematics” *Synthese* 84, pp. 163-211.

Stein, H. 1992 “Was Carnap Entirely Wrong, After All?” *Synthese* 93, pp. 275-95.

Stein, H. 1994 “Some Reflections on the Structure of Our Knowledge in Physics” in D. Prawitz, B. Skyrms, and D. Westerståhl, eds. *Logic, Methodology, and Philosophy of Science IX* (Amsterdam: Elsevier), pp. 633-55.

Stein, H. 1998 “How Does Physics Bear upon Metaphysics; and Why Did Plato Hold that Philosophy Cannot be Written Down?” Colloquium Talk at the University of Chicago (unpublished).

Stein, H. 2002 “Newton’s Metaphysics” in I.B. Cohen and G.E. Smith, eds. *The Cambridge Companion to Newton* (Cambridge: Cambridge University Press), pp. 256-307.

Stein, H. 2004 “The Enterprise of Understanding and the Enterprise of Knowledge,” *Synthese* 140, pp. 135-176.

Wilson, M. 2006 *Wandering Significance: An Essay in Conceptual Behavior* (Oxford: Oxford University Press).

Zabell, S.L. 2005 *Symmetry and its Discontents: Essays on the History of Inductive Philosophy* (Cambridge: Cambridge University Press).

Zabell, S.L. 2007 “Carnap on Probability and Induction” in M. Friedman and R. Creath, eds. *The Cambridge Companion to Carnap* (Cambridge: Cambridge University Press), pp. 273-294.

1. As is attempted, in very different ways, by e.g. Gadamer (1960), Schurz and Lambert (1994), Shanker (1995), Gendlin (1995), Humphrey (2000), Donald (2001), De Regt and Dieks (2005), Kosso (2007). [↑](#footnote-ref-1)
2. Stein adds, in parentheses, the further point that “. . . the writer remarks that names have no fixed connection to objects, and therefore by their use alone obscurity cannot be avoided; nor can it be so by λογοι, since these are made up of nouns and verbs. I believe that Plato would have been unimpressed by the causal theory of reference and the postulate of rigid designators; I wish we had the Socratic dialogue on this subject.” (ibid.) [↑](#footnote-ref-2)
3. Personal communication. The other luminary who attracted Stein to Chicago was Richard McKeon, who in contrast to Carnap would prove a disappointment. [↑](#footnote-ref-3)
4. More recently, Carus (2007) sets this view out more systematically, and seeks to re-interpret the more notorious phases of Carnap’s career (up to 1935) in the light of Stein’s understanding of the later Carnap. Stein’s view, though unusual when first articulated, is consistent with that of others who had studied with, or been associated with Carnap, especially Richard Jeffrey (1994). [↑](#footnote-ref-4)
5. This is in fact just what Quine, after first rejecting any such thing (1951, p. 25), later endorses as “explication” (1960, pp. 258-266); see further discussion in Carus (2007), pp. 265-6. [↑](#footnote-ref-5)
6. In the sense of Carnap’s (1950) “Empiricism, Semantics, and Ontology,” where “internal” questions are those asked with respect to a spelled-out language framework, while “external” questions are framework-free. Thus the question “are there infinite numbers?” is an internal question if asked e.g. with respect to Zermelo-Frankel set theory (in which case the answer is affirmative). Without the specification of a framework, Carnap says, the question has no meaning. It can be re-interpreted, though, he allows, as a pragmatic question about which language framework to adopt, or about whether it is most useful to adopt a framework that contains infinite numbers. [↑](#footnote-ref-6)
7. Personal communication; Stein remembers saying something along these lines to, or writing something about it in a paper for, Carnap, who strongly approved. It was Stein’s impression that while Carnap had perhaps considered this before, it was not consisently present to his mind. [↑](#footnote-ref-7)
8. Martin had been part of a small reading group at Carnap’s Hyde Park home, in which Stein had also participated. Other participants included Ruth Barcan (later Marcus) and Raymond Smullyan. Carnap later said, in a recommendation letter, that of those who had attended these meetings, Stein had been the “most intelligent”. [↑](#footnote-ref-8)
9. Carnap would not, of course, have been the least surprised about the contradictions arising from the the “folk” concept

   of belief, as famously diagnosed by Kripke (1979). In the discussion since then of this and similar analyses, not much of

   anything like what Carnap called *explication* has been in evidence; attention has focussed more on the natural-language folk concepts themselves rather than on any attempted systematic replacement of them (e.g. in the social and cognitive sciences) by better or more precise concepts. [↑](#footnote-ref-9)
10. One important exception to this is the final chapter of Carnap’s (1963) replies to critics. [↑](#footnote-ref-10)
11. Stein particularly likes to cite this passage. [↑](#footnote-ref-11)
12. The “relative modesty” of this route, Stein continues, “may be compared to that of the method of Locke, who proposed to examine the character of the human understanding by observation of the ‘facts’ of cognition (‘historical plain method’), and whose program of doubt he contrasts, by implication, with that of Descartes: the latter consists in refusing to believe — indeed, hyperbolically, in denying — any proposition not fully established up to certainty in the light of reason; the former in critical reservation, in the submission of all cognitions to scrutiny in order to ascertain their origin and thereby the degree of evidence which they can claim. . .” (Stein 1958, pp. 7-8) In defense of this more modest approach Stein also remarks that “although a program based on this idea can hardly be expected to achieve the resolution of such ultimate questions [as those raised by the “philosophy of philosophy”], it may at least hope to provide useful material for such an ambitious attempt, and even a kind of dim provisional light, if the questions on the borderline of science and philosophy are so treated as to elucidate in some measure the nature of the achievements and prospects of science, and the role of the philosophical discussion itself in examining them.” (ibid., p. 7) [↑](#footnote-ref-12)
13. While Stein himself shares this conviction, at least as a starting point, he does not reject the *possibility* of progressive formalization, as a long-term strategic program. Carnap’s inductive logic, for instance, though defective in certain basic respects (see Zabell 2005, 2007), is regarded by Stein as a very interesting and worthwhile attempt. [↑](#footnote-ref-13)
14. “Naturally” in the sense that in our practical lives (the realm of pragmatics), our default preference is to use the best and most advanced knowledge available about the issue we are addressing, whether it is the effects of drugs, the stability of helicopters, or the construction of buildings in earthquake zones. Carnap (e.g. 1946, p. 43) —characteristically — compared the construction of languages to the design of airplanes. [↑](#footnote-ref-14)
15. E.g. Hesse (1970). It is hard, though, to disagree with Shapin’s (1992) retrospective assessment, doubting “that our discipline ever conducted an adequately informed and systematic debate over what [the internal/external distinction] was actually about, still less over the respective merits of each theory.” (p. 345). [↑](#footnote-ref-15)
16. To some this enterprise will appear “Whiggish to a nicety,” as Stein’s work was described by one referee of a grant application. But such responses are based on a confusion of two different enterprises. This is almost exactly the confusion deprecated, in a somewhat different idiom, by Hasok Chang (2004, pp. 247-8), who distinguishes what he calls the “complementary mode of HPS” (here called descriptive pragmatics) from the “social history of science.” The former, he says, “is not meant to be an incomplete sort of history that ignores the social dimension; it is ultimately a different kind of enterprise altogether from the social history of science.” (ibid., p. 247) For more on this distinction see also Carus “History and the Future of Logical Empiricism” [forthcoming]. [↑](#footnote-ref-16)
17. Its use as descriptive pragmatics takes for granted, of course, that it is available as empirical knowledge in the first place. Some would argue that, in effect, the nature of this empirical knowledge is in some sense *fundamentally* holistic, so that it is *impossible* to extract — or *ab*stract — the “internal” aspects needed by pragmatics from the densely interwoven tangle of historical knowledge as it presents itself to us, representable only by, say, “thick description.” But if it is possible for the historian, situated in a modern society, to enter hermeneutically into a past mentality or form of life, then it is also possible, if enough data are available, to constrain past usages of any give symbol up to quite a high level of precision, i.e. to find out what they meant to their users, at least within sufficient tolerances to make comparisons possible with later (up to and including present) concepts. [↑](#footnote-ref-17)
18. “. . . although he is clear that dynamics does not provide any way to distinguish motion from rest, Newton does not seem to have conceived the philosophical possibility that *that distinction cannot be made at all*; that is to say, that the spatio-temporal framework of events does not intrinsically possess the struc­ture of the Cartesian product SxT, but a weaker structure. One easily understands why Newton should not have conceived this possibility; even Poincaré, at the end of the nineteenth century, could express the view that if rotation is real then motion must be real, and if acceleration is real then velocity must be real. But the more abstract point of view that mathematics has now made available allows us to see, today, that these considerations are specious, and that the true structure of the space-time of Newtonian dynamics, with its Galilean invariance, is the one I have already described, in which there is an absolute time but no absolute space — that is to say, a natural mapping upon T but none upon S. The point that is really crucial for kinematics is that within this structure *there is no absolute or intrinsic notion of velocity, but there is an absolute or intrinsic notion of velocity-difference*—and *therefore* of rotation and of acceleration.” (Stein 1967, pp. 182-3) [↑](#footnote-ref-18)
19. One of the more extensive recent treatments is that of Janiak (2008, Ch. 5, pp. 130-162), which considers Stein’s interpretation — oddly, in the light of the passage quoted here — to be a “radically empirical” view of Newton. Janiak prefers a Newton who entertained both a “mundane metaphysics” (for which Stein’s interpretation is roughly on the mark) and, distinguishable from it, a more traditional and backward-looking “divine metaphysics.” Regardless of the merits of these different interpretations, they provide examples of the willingness of strictly internal historians to consider interpretations that go beyond the documentable contemporary conceptual repertoire (Stein) and the tendency of more empirically-oriented historians to rely on documentably available vocabularies as constraints on interpretation (Janiak). [↑](#footnote-ref-19)
20. This interpretation, spelled out in detail in 2002, first appeared in Stein (1977, pp. 395-7). [↑](#footnote-ref-20)
21. Though less often practiced; see Carus “History and the Future of Logical Empiricism,” sec. 4. [↑](#footnote-ref-21)
22. In his mind, they were represented in his own time by German idealism on the one side and positivism on the other. Helmholtz liked to regard Kant as a predecessor in this regard, i.e. as having also tried to find a middle way between similar extremes (Leibnizian rationalism and Humean empiricism, in that case) — all Kant was missing for success, he thought, was Helmholtz’s own empirical research into the constructive nature of human perception. [↑](#footnote-ref-22)
23. Another example of such deep continuity is explored in Stein’s’ paper on Eudoxos and Dedekind (1990), in connection with which it is worth remarking, as Wilfried Sieg urges me to do, on a whole dimension of Stein’s thought — the mathematical dimension — that had to be left aside in this paper, but is of central importance as the source of the “forms” Stein discerns. As he paraphrases an aspect of Riemann’s famous habilitation lecture: “Geometry is not an empirical science, or a part of physics; it is a part of mathematics. *The role of a mathematical theory is to explore conceptual possibilities* — to open up the scientific *logos* in general, in the interest of science in general. One might say, in the language of C.S. Peirce, that mathematics is to serve, according to Riemann, . . . the interest of “abduction” — of providing the means of *formulating* hypotheses or theories for the empirical sciences.” (Stein 1988, p. 252) The influence of Riemann and especially Dedekind on Stein’s conception of mathematics in this role are evident throughout the paper from which this quotation is taken. [↑](#footnote-ref-23)
24. This of course is why the extensions of the successive explications cannot be identical, but merely ‘overlapping’. An attempt to make this precise would have to capture the sense in which previous observations regarded as belonging to the extension of a previous explication are also considered to belong (along with new ones) to the extension of a new explication. In his contribution to the present issue of the *Monist*, George Smith (2010) has in my view succeeded in providing a framework for such a conception of explicative genealogies. [↑](#footnote-ref-24)
25. As noted above; Quine, for instance, regards “regimentation” as simply extending a term’s range of application; Wilson (2006) points out how this fragments its reference into a loose collage of diachronically successive and synchronically superposed maps of the “same terrain” useful in different contexts, but still prefers to regard this collection as belonging to the “same concept” and also resists a distinction between theoretical and applied science. [↑](#footnote-ref-25)
26. Recall the quotation above, p. 4, where Stein says that “*any* question about the relation of the explicatum to the explicandum is an ‘external’ question; this holds, in particular, of the question whether an explication is adequate.” [↑](#footnote-ref-26)
27. Graham Bird suggests, in his new commentary on the first critique, that an illuminating way to translate Kant’s distinction between “transcendental” and “empirical” into a more recent idiom is Carnap’s distinction between “external” and “internal” questions (Bird 2006, pp. 92-6), on which see footnote 6 above. [↑](#footnote-ref-27)
28. See footnote 24 above. Carnap’s hope, in principle, was to be able to express this relation in terms of the bodies of evidence accounted for by successive theories, as measured by their inductive probabilities (e.g. Carnap 1950), but he was never able to bring this program close to realization, though successor programs of Bayesian theory appraisal are still pursued (Zabell 2005). George Smith (2010) has now framed this relation of successor theories with respect to their shared bodies of evidence in somewhat different terms, broadly compatible with Carnap’s program (Carus (forthcoming)), but without attempting to express this relation quantitatively. [↑](#footnote-ref-28)
29. There is a widespread consensus that it never recovered from the twin blows of Quine’s “Two Dogmas” and Kuhn’s *Structure of Scientific Revolutions*. Even among those who are sympathetic, it is usually conceded that “in the twenty-first century, no one is a logical empiricist” (Richardson 2007, p. 346). [↑](#footnote-ref-29)
30. Or in the sense of the *Cambridge Encyclopedia of Philosophy*: “**metaphysics**, most generally, the philosophical investigation of the nature, constitution, and structure of reality. It is broader in scope than science, e.g. physics and even cosmology (the science of the nature, structure, and origin of the universe as a whole), since one of its traditional concerns is the existence of non-physical entities, e.g. God. It is also more fundamental, since it investigates questions science does not address but the answers to which it presupposes. . .” (Audi 1999, p. 563) [↑](#footnote-ref-30)
31. Stein (1994, pp. 637-8) cites this example from early Carnap (1923) as paradigmatic of such oversimplification. If he could bring himself to overlook the sometimes almost aggressive untidiness of presentation, Stein would probably be very sympathetic to Mark Wilson’s (2006) actual portrayal of the cognitive status of applied science (as opposed to the above-noted naturalist and realist interpretations Wilson gives this portrayal). [↑](#footnote-ref-31)