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Sellars in Context: An Analysis of Wilfrid Sellars's Early Works

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Sellars in Context: An Analysis of Wilfrid Sellars's Early Works

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
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DEDICATION

I dedicate this dissertation to the faculty members and fellow graduate students who helped me along the way.

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ABSTRACT

Although Wilfrid Sellars's work holds a prominent place in recent analytic philosophy, little work has been done to situate his early approaches to normativity and the philosophy of language in their proper context. What little work has been done tends to emphasize Sellars's connection to a then dominant logical empiricism at the expense of marginalizing other American philosophical schools. On top of this historical issue, most scholars attempting to explain Sellars's systematic philosophy tend to ignore a developmental picture of Sellars's positions and focus on explicating the systematic character of his thought. My dissertation attempts to correct both of these tendencies by offering a historically situated account of Sellars's early papers that presents his views in relation to logical empiricism, the "Iowa School" of philosophy as embodied in Gustav Bergmann's and Everett Hall's writings, and some aspects of traditional American philosophy. By fleshing out the context of Sellars's early papers, it becomes clear that the "strong" normativist project present in his later essays developed out of his shift from his attempt to fit "traditional" philosophical problems into a formalist approach to language. My thesis acts as a "correction" to the previous interpretive points by presenting a more complex characterization of the contextual influences on Sellars's early papers and creating a foundation for a developmental account of Sellars's later views. I do this by examining evidence from Sellars's unpublished correspondence and works from a myriad of archival sources in conjunction with an analysis of his early publications.

INTRODUCTION

Wilfrid Sellars's work has recently gained prominence in the work of analytic philosophers who are concerned with offering a particular account of the relationship between mind, language, and world. This "particular account" focuses on Sellars's commitment to a normative conception of language and thought that turns on understanding linguistic rules and rule-following as concepts inherently bound to standards of correctness relating to patterns of behavior. Sellars's contemporary influence is not limited to the topic of normativity; his critique of the given, his invocation of "the space of reasons", and his functional role semantics have all been subject to lengthy discussions in the secondary literature. Despite the prominence of Sellars's work in contemporary scholarship, little to no attention has been paid to Sellars in scholarship pertaining to the history of analytic philosophy. In fact, little effort has been made to offer a historically sensitive, developmental account of Sellars's philosophical thought in general.

Due to the fact that the history of analytic philosophy is a relatively new field of study, historians and philosophers alike have generally been mute when it comes to Sellars's place in the canon. Recent scholarship has tended to focus on either the early Neo-Kantian roots of analytic philosophy, debates in England and Germany surrounding the work of Bertrand Russell, Ludwig Wittgenstein, and Gottlob Frege, or on the emergence and development of logical positivism[1]. Although some[2] recent scholarship

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[1] See Uebel 2007 and Hylton 1993.

has focused on the effect the rise of analytic philosophy had on the early American philosophical community, little attention has been paid to the period of philosophy that covers Sellars's philosophical education and early publications. When scholars do address the American philosophical community from about the mid-1930s up until the early 1950s, Sellars's name barely appears. When it does appear it is usually in reference to his seminal work "Empiricism and the Philosophy of Mind" and critiques of the given in epistemology[3]. Sellars's relationship to logical positivism, the rich connection between his philosophy and earlier trends in American philosophy (most notably Sellars's connection to Critical Realism as embodied in his father's place in the movement and to some strains of pragmatism as well) has been largely ignored in the secondary literature.

Most, if not all, attempts at reconstructing Sellars's systematic philosophy as a whole have taken the form of thematically organized collections that make little to no attempt to situate Sellars's work within its historical context or account for shifts in his philosophical commitments.[4] In all fairness to these collections, such historical concerns are not usually the point behind constructing an overview of one philosopher's systematic thought. An accurate portrayal of any philosopher must be able to account for shifts in her thought as well as the influential debates and concerns surrounding her development. To ignore such contextual issues runs the risk of, at best, misconstruing the development of a philosopher and, at worst, misinterpreting a philosopher's views. Historical accounts can help complete and correct the picture provided by thematically organized collections.

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[2] See especially Andrew Jewett's article "Canonizing Dewey: Columbia Naturalism, Logical Empiricism, and the Idea of American Philosophy".
[3] For example, see Kuklick 2001.
[4] It should be noted that this is not necessarily a critique of said texts. The point is simply that such overviews of Sellars's thought do not offer, nor purport to offer, a historical account of his philosophical development. For example, see deVries 2005 and O'Shea 2007.

What little attempt has been made at the periodization of Sellars's work tends to focus on the publication of his essay "Empiricism and the Philosophy of Mind" (hereafter EPM) as the center piece of Sellars's philosophical development.[5] Once this is established, Sellars's career is generally divided into "before" and "after" periods centered on the publication of EPM. This is problematic for a number of reasons - at least two of which are particularly serious. First, such a periodization at least tacitly assumes that there are no significant shifts in Sellars's views prior to the publication of EPM. This is to assume that one could clump the variety of publications from roughly 1947 – 1955 together without running roughshod over any significant conceptual changes in Sellars's philosophical commitments. Such an assumption is problematic, I will argue, precisely because there *are* significant shifts in Sellars's philosophical commitments prior to the publication of EPM.

Second, such a centering of Sellars's philosophical views on EPM also presupposes that the views expressed in EPM are different enough from Sellars's previous positions to represent a significant shift in view. Although there is no denying that EPM is a standout work in Sellars's published writings, a number of his own works (as well as works of his contemporaries and former professors)[6] anticipate large sections of EPM. Certain ideas, especially Sellars's commitment to psychological nominalism, occur earlier than EPM; Sellars's 1954 article "Physical Realism" contains a number of themes and arguments found in EPM two years prior to its publication. The point here is

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[5] The best example of this kind of periodization can be found in the beginning of Rosenberg 2007.
[6] Here I am thinking of the striking similarities between the early sections of EPM and H. A. Prichard's essay "The Sense-Datum Fallacy". Prichard was one of Sellars's instructors at Oxford in the 1930s and in private correspondence Sellars urged his father to re-visit Prichard's critiques of sense data theories. As far as I can tell Prichard's influence on Sellars's epistemology has been largely ignored in the secondary literature.

that there is a story to be told about Sellars's move from philosophy as a formal discipline and his early commitment to ideas found in EPM that has, thus far, been left untold.

In addition to these considerations, one must also be wary of what is essentially presentism or whigish history; the danger of reading contemporary interests back into someone who is now essentially a historical figure. EPM has, at least since the 1980s, become Sellars's most widely discussed piece, but there is no reason to assume that such was the case in the 1950s around the time of its publication. Although Sellars himself considered EPM the anchor of his systematic philosophy, even going so far as to suggest that much of what he had thought and written "since 1956 has been an unpacking and defense of one or another aspect of 'Empiricism and the Philosophy of Mind'", it is not clear that most of Sellars's contemporaries saw matters this way (Sellars 1974, p VIII). Even though EPM was clearly considered an important piece of philosophical work by the mid-1960s, it is not clear that the immediate reaction to Sellars's work was anywhere close to the importance foisted upon it today.

The point of this dissertation is to act as a corrective to the trends in secondary scholarship described above, as well as a supplement for the thematically organized attempts at reconstructing Sellars's philosophical enterprise. My two main claims are that a.) Sellars's early attempts at constructing a "pure pragmatics" should be understood as situating him within a complex relationship between logical positivism and more traditional philosophical outlooks and, b.) That an important shift in philosophical orientation occurs in Sellars's early work; this is a shift from understanding philosophy as a formal discipline to carving out a non-formal path for philosophical contributions to the study of mind, language, and agency. The consequence of the first main claim is that

historical accounts that have characterized Sellars's early efforts as either *internal* critiques of logical positivism or as a simple updating of previous American philosophical trends have oversimplified Sellars's historical context. The consequence of the second main claim is that Sellars's emphasis on the normativity of language should be seen as a shift in his thinking, one that occurred only *after* Sellars shed the formalist skin inherited from Carnap and others.

I argue for both of these claims by constructing a historical narrative concerning the transition from Sellars's earliest publications that define philosophy as a formal discipline, in opposition to what he saw as an overriding commitment to psychologism, to a more traditional definition of philosophy that opened the door for metaphysical speculation – albeit a restrained metaphysics that is underwritten by commitments to both nominalism and scientific realism. I do this by examining Sellars's early published and unpublished works, contemporary debates surrounding his earliest publications, and some of his unpublished correspondence.

The first chapter maps the lay of the land prior to Sellars's emergence on the philosophical scene. I consider a number of American philosophers' positions in regards to the discipline and its relationship to the sciences as compared with the views held by the immigrating logical positivists. I argue that no easy classification of philosophical views, as represented in Alan Richardson's work, helps explain the emergence of logical positivism and the seemingly sudden disappearance of "traditional" American philosophy on U.S. shores. Instead, one should understand the reception history of logical positivism, and thus the context of Sellars's education and early philosophical development, as a variety of differing opinions and views about the relationship between philosophy and the

sciences., One should not read the history of American philosophy as one of disappearance; it is not the case that all "traditional" philosophers vanished once Carnap and others arrived on American shores.

With the historical antecedents fleshed out, chapter two focuses on explicating Sellars's early project of constructing a "pure pragmatics" of language. Although some accounts have attempted to pigeon-hole Sellars's early works as squarely located in the camp of logical empiricism, I argue that such a reading is, at best, excessively narrow and at worst grossly insensitive to the variety of historical currents that shaped Sellars's early views. In order to offer an accurate account of Sellars's earliest works, it is necessary to situate his response to logical positivism, most notably the work of Rudolf Carnap, among the critiques offered by Gustav Bergmann, Everett Hall, and others. I argue that Sellars's conception of language during his "formal" period lacks the strongly normativist element found in his later writings and his critique of the logical positivists' notion of a linguistic rule. Chapter two ends with an account of the reception history of Sellars's pure pragmatics that leans heavily on his unpublished correspondence as well as on the few short discussions that exist of his early works.

Chapter three focuses on what I argue is a substantive shift in Sellars's philosophical orientation that allows for the introduction of a *strongly* normative conception of language. I examine Sellars's early attempts at understanding rule-following, focusing on the analogy he draws between prescriptive terms in moral theory and the "fraught with ought" character of linguistic rules. This examination, I argue, helps clarify Sellars's disagreements with Carnap over linguistic rules and semantics. The chapter ends with a discussion of Sellars's early conception of normativity, in which

I argue that, as of his early papers, an insufficient basis existed to make his pure pragmatics a persuasive position in the late 1940s.

I conclude my dissertation by offering a general picture of Sellars's place in the history of analytic philosophy and some guiding observations concerning further developments of a historical account of Sellars's thought. Specifically, I claim that one should avoid easy comparisons between W.V.O. Quine and Sellars and, instead, focus on Sellars's differences and disagreements with logical positivists in conjunction with his inherited connections to Critical Realism and pragmatism.

Given the description of my project, it should be relatively clear that I am not developing a full,, historically grounded account of Sellars's *entire* systematic philosophy. The point of this project is to situate Sellars's earliest publications amongst those of his contemporaries, while starting to build a road map for a significantly larger developmental account of Sellars's work. Even a comprehensive treatment of Sellars's early essays is outside the scope of this project, as it should, or would, include accounts of a significantly wide variety of antecedent positions and philosophical debates that are only mentioned within this text. In an effort to unify the content of my dissertation, I have focused on Sellars's views on philosophy and language and have largely ignored his early writings on logical modalities, particulars, and various issues in the philosophy of science and ethics.

Chapter 1: American Philosophy and the Rise of Logical Positivism

I. Introduction

Historians of analytic philosophy are born of a recent movement -- one that has focused almost exclusively on the project of explicating either the "fathers" of analytic philosophy (Gottlob Frege, G.E. Moore, or Bertrand Russell), Ludwig Wittgenstein, or a variety of logical positivists and their individual or group projects. But little attention has been paid to two different transitional periods in the history of American philosophy. The first period ranges from the turn of the century up until the 1930s and is embodied by American philosophers' attempts to come to grips with the development of the profession in relation to the sciences and the intellectual community at large. The second period concerns American philosophers' attempts to deal with the influx of logical positivists (from roughly the early 1930s through the mid to late 1940s) and the minimization of "traditional" philosophical categories such as metaphysics and ethics. What is often overlooked in these periods is a commitment to "traditional" philosophical approaches or problems by American realist, idealist, and pragmatist philosophers that seem to clash with the scientistic and revolutionary agenda of most logical positivists.

Research that has dealt with this period tends to offer one of two differing stories. On what I will called the *received account*, various pragmatist and realist schools of thought essentially collapsed during the influx of positivist philosophers from Europe. On the received account there was no significant philosophical debate between various camps in which the positivists came out on top; rather largely social factors (e.g., a lack

of graduate students under pragmatist and realist philosophers) led the way to the demise

of these other schools of thought. Whether this demise was due to more incoming

graduate students that had a background in mathematics or the sciences or due to the

placement of positivist philosophers in prestigious academic positions, the specific reason

for the demise of "traditional" American philosophy is generally left undetermined in the

received account. What is usually agreed upon is that pragmatist and realist philosophers

either were unable to mount much of a defense against positivism, were ignored, or,

assimilated their projects to those of the positivists.[7] The upshot of the received account

is that by the mid to late 1930s the American philosophical community had, more or less,

been conquered by logical positivists and positivistically-inclined philosophers.

A second account, proposed by Alan Richardson, is in direct response to the

received understanding of the American reception of logical positivism. As opposed to

thinking of pragmatism and positivism as rival camps, Richardson argues that logical

empiricism in the 1930s "was received by important pragmatists as scientific philosophy

and, *in that most central regard*, as a kindred project in philosophy" (Richardson 2003, p.

2). Specifically, Richardson thinks of Quine, Rudolf Carnap, and Charles Morris as the

three principle actors in debates between pragmatists and logical positivists, and he

understands Morris and Carnap as holding converging viewpoints on the notion of the a

priori, on the analytic/synthetic distinction, and, most importantly, on a vision of

philosophy as a scientific discipline (Richardson 2003, p. 5).

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[7] As Alan Richardson notes, the received account is part of the accepted mythology and is not generally found in arguments about how one should understand this period of philosophical history. This is not to say that it does not make appearances in the secondary literature. For example, see chapter nine in Cornelis De Waal's *On Pragmatism.*

This is not to say that the points of view of pragmatism and logical positivism were consistent with one another. Richardson acknowledges the gap between most pragmatists' commitment to some form of naturalism and the logical positivists' commitment to a formalism that essentially opposes naturalism. That is, "Logical empiricism, at least in the form associated with Carnap and Reichenbach, is opposed to naturalism, but naturalism is not its most fundamental opponent" (Richardson 2003, p. 5). Richardson argues that such a commitment to naturalism, as generally found in pragmatists and realists, is not the prime target of most logical empiricists in the 1930s. Any vision of philosophy that pictured itself "as something other than a scientific discipline" was the *main* enemy of the logical empiricists (Richardson 2003, p. 5). According to Richardson, both pragmatists and logical empiricists should be thought of as committed to a vision of philosophy as scientific, one should understand the disputes between pragmatists and logical empiricists as *internal* differences located within a shared camp. Thus, differences between Moritz Schlick and C. I. Lewis on verification or disagreements between John Dewey and logical positivists concerning an adequate philosophical analysis of value were internal clashes between group members largely in agreement. One may even go so far, as Richardson does, to suggest that pragmatism in the 1930s should be understood "as a naturalist version of a particular subspecies of scientific philosophy" (Richardson 2003, p. 18).

The interpretive framework of this second account turns on exactly what one means by the phrase "scientific philosophy". Richardson invokes the term to point out "a true commonality of project" between the immigrating logical positivists and American philosophers that is found in "a joint insistence on scientific method in philosophy, an

insistence that led directly to the rejection of traditional metaphysics and traditional epistemology" (Richardson 2003, p. 6). This commitment also lends support to Richardson's claim about why the differences between Morris and Carnap did not give rise to the kind of disputes one finds between Carnap and Quine. In Richardson's account, Carnap and Morris are largely concerned with convincing the general philosophical community of "the scientific status of philosophy itself" as opposed to hashing out conceptual differences (Richardson 2003, p. 6).

Despite the claim of a "commonality of project", Richardson does not sweep discrepancies between both camps under the rug. Disputes concerning the normativity of knowledge, the given, the a priori, formal logic, and the exact relationship between philosophy and political concerns, as well as what is meant by "scientific philosophy," remain in the second account. Where this differs from the received account is in the emphasis on *agreement* between pragmatist and positivist philosophers on a variety of issues from a pragmatic conception of the a priori, the centrality of the analytic/synthetic distinction, and the inseparability of a "scientific attitude" from political responsibility[8] to name only a few (Richardson 2003, p. 11). Where significant differences existed, such as in Lewis's arguments with Schlick over verification or Dewey's emphasis on the importance of a theory of value in philosophy, Richardson turns to the claim that such differences are not "first-order disputes over philosophical theses regarding meaning and

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[8] Although it may seem clear *now* that pragmatists and logical empiricists shared similar commitments to the inseparability of scientific work from political concerns, I think such similarity is not as clear in context (except in the work of Dewey). Given the immigrating logical positivists' interest in keeping a relatively low profile (politically speaking), it is not necessarily clear that such political ideals would have been openly shared. See Reisch 2001 for an in-depth account of this, especially chapter 7 where Reisch offers a counter argument to my previous assertion.

value," but programmatic differences that are overshadowed by the places these philosophers converged (Richardson 2002, p. S39).

Richardson's depiction of the philosophical climate of the 1930s goes a long way in disproving the anachronistic history of philosophy in America present in the received account. There is clearly *something* to the idea that *some* pragmatists, such as Charles Morris, were amenable to an understanding of philosophy as a scientific discipline and, in at least this respect, were closely aligned with *some* of the logical positivists. Richardson provides an excellent account of the philosophical commitments shared by Carnap, Reichenbach, Lewis, Dewey, and Morris. But pointing out *some* of these shared commitments in *some* members of the philosophical community is not enough to justify Richardson's strong reading of the reception history of logical positivism. At least two questions need to be answered in regards to Richardson's account:

1. Did the American philosophical community really think of itself as doing "scientific philosophy" *prior* to the 1930s?

2. Throughout the 1930s and 40s, did the pragmatists, realists, and positivists actually think of each other as philosophical allies working on similar projects?

For Richardson's account to succeed, one must answer in the affirmative for both questions. A negative answer to the first question would indicate that the American philosophical community was not primed for the arrival of something akin to scientific philosophy. Even if one can locate the rhetoric of scientific philosophy in the early American philosophical community, as I believe one can, this does not automatically vindicate Richardson's account of the period. As I will argue in section II, the question is not straightforwardly whether any American philosopher invokes the notion of scientific philosophy but what, exactly, such philosophers meant by the phrase. My contention is

that although the rhetoric of a scientific philosophy did exist, what each American philosopher meant by this is more varied than what one finds in Richardson's account of the period.

Offering a clear, historically grounded answer to the second question may or may not damage Richardson's account, depending on how much weight one puts on the perception by individual philosophers of their own work and the work of their contemporaries. It is clearly somewhat irrelevant if the pragmatists and positivists thought they were involved in differing projects if, in fact, they were not. I will argue that pragmatists, realists, and positivists did offer substantial comments on how they understood the other groups' positions that differ from Richardson's representation of the period. Given all of this, it is reasonable to conclude that while, not *all* individual observations from the time are relevant, a number of them will speak to how the American philosophical community understood the positivists and vice versa. The remaining parts of this chapter will show that the most accurate account that can be offered of this time period tends towards a negative answer for both of these questions.

The account below is developed as a rival reading of the historical context of the reception history of logical positivism. Methodologically speaking, I take issue with Richardson's suggestion that an adherence to the primacy of the use of the scientific method in philosophy, in conjunction with a rejection of traditional approaches to metaphysics and epistemology, are evidence enough to argue that various groups of philosophers were working along the same lines. My concern is that Richardson glosses significant differences between what pragmatist and realist philosophers meant by "science," "exact discourse," and "scientific method," in comparison with their positivist

counterparts. More importantly, it is not at all clear that what it means for an American

philosopher in the 1930s to reject *traditional* approaches to metaphysics is equivalent to

rejecting metaphysics *writ large*. It seems clear that, as will be shown in sections II and

III, a proportionally large number of American philosophers still considered metaphysics

an important philosophical field that was not necessarily amenable to scientific reduction

or elimination.

Richardson seems to conflate a significant difference between the commitments

embodied by his notion of scientific philosophy as a substantive position with the

professionalization of philosophy as a discipline. Surely it is correct to say that the

development and status of the sciences impacted philosophy, both as to the content and

rhetoric of the profession. Richardson does not seem particularly concerned with the

distinction between content and rhetoric; a superficial glance at publications in America

during the teens and twenties initially suggests that the impact of the sciences led to

something akin to scientific philosophy. As I will argue, this is simply not the case; a

number of American philosophers' rhetoric simply does not match the notion of scientific

philosophy explicated by Richardson.

As a consequence of this, both the positivists and Richardson minimize the role

metaphysics have played throughout the history of American philosophy. To support

Richardson's case, in any event, one must read the history this way; it is difficult to see

how the positivists' and pragmatists' projects could be thought of as commensurable

without minimizing this aspect of American philosophy. Despite changes in methodology

and the pragmatists' occasional minimalism[9] towards the metaphysics of the 19th century,

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[9] It should be clear that I am not claiming that in all instances American pragmatists were comfortable with
metaphysics. William James, and even more so John Dewey, were especially hostile to what were seen as

I will argue that this is not a contingent part of American philosophy even into the 20th century. This aspect of American philosophy simply cannot be eliminated from an accurate history. While it is certainly true that part of the history of early 20th century philosophy in America revolves around the discipline's relationship to changes in the sciences, this does not support a claim of broad agreement concerning the need to transform philosophy itself into a science.

In the course of answering the above questions I want to emphasize one overarching point about what kind of story one can tell about 20th century history of philosophy in America. Specifically, I want to emphasize the idea that our history is a messy one; the history of philosophy in America, especially in the 20th century, cannot accurately be categorized under any one banner. Richardson himself acknowledges this point when quoting Ralph Barton Perry in 1930 about the "new eclecticism" seemingly present in the philosophical community at the time (Richardson 2003, p. 4). But, Richardson does not seem to take Perry's observation seriously. If Perry is correct that a "new eclecticism" is indicative of philosophy in America, then why should one think that most, if not all, members of this philosophical community thought of themselves as all practicing a kind of scientific philosophy? The initial pressure of this question can be found in the lack of a coherent program that existed in philosophy in America prior to the 1930s in conjunction with the fact that many philosophers held significantly differing views on the relationship between science and philosophy. Although Richardson seems to admit this point concerning pragmatism, the generalization from pragmatism to all of American philosophy is implicit in his account. At the very least, Richardson is aiming

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the excesses of idealism's metaphysics run wild. Nonetheless, there is a strong metaphysical project in Peirce's philosophy and even some to be found in the later works of James.

for the most read pragmatists of the day, in Dewey and Lewis, in order to solidify his

claims about the nature of pragmatism and American philosophy. Thus, it would seem

that Perry's warning, acknowledged at the beginning of Richardson's account, is ignored

once Richardson begins crafting his account of the period.

II. American Philosophy Prior to Logical Positivism

Arguments over the relationship between science and philosophy, concerning

whether philosophy should follow the sciences in its methods and whether philosophers

should follow the sciences in regards to specialization, were clear concerns for the

American philosophical community prior to the arrival of logical positivism in the early

30s. Although some[10] have traced these concerns as far back as 1860, for current

purposes the debates in the beginning of the 20th century are the most relevant. Though

one could argue that the foundation for 20th century discussions of science and

philosophy was grounded in debates between James, Charles Sanders Peirce, and John

Dewey, such an approach is unhelpful. First, as Bruce Kuklick has noted, focusing on

these three America philosophers makes it almost impossible to explain "subsequent

philosophical analysis" (Kuklick 1977, p. 577). Kuklick's point is that the history of

American philosophy, both before and after pragmatism, is a more complicated story to

write than simply tracing 20th century thought back to these three philosophers. Although

Kuklick does think such an organizational principle is successful if one is explaining the

institutional history of philosophy at Harvard, for example, such an approach to

American thought in general would risk either marginalizing those that do not easily fit

into this mold or placing figures into a mold to which they do not fit. Specifically, such

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[10] For example, see the introduction of Daniel J. Wilson's *Science, Community, and the Transformation of American Philosophy, 1860 – 1930*.

an orientation would place undue emphasis on a narrative surrounding pragmatist philosophers at the expense of ignoring the contributions made by realist and idealist philosophers.

On top of this, there is a danger of making more out of Peirce's influence on early 20th century American philosophy than there was. Although Lewis is often touted as "rediscovering" Peirce's works on logic and epistemology in the early 1920s, by and large the philosophical community of the early 20th century ignored or avoided Peirce's philosophy. By 1924 Lewis observed that "current thought had moved beyond Peirce without much noting him" despite the fact that some dominant philosophical ideals "moved beyond Peirce in the very directions in which he departed from his contemporaries" (Lewis 1924, p. 73-4). This is not to say that Peirce had *no* influence at the turn of the century, but that his explicit influence was greatly reduced in comparison to philosophers that held academic positions.

Prior to the First World War, debates surrounding whether philosophy needed to become more like the sciences – or whether it was a science itself – intensified within the discipline. Part of the reason behind this was the then recent creation of various professional associations within the discipline. Such associations encouraged and fostered public debates over the direction of the discipline and signaled the professionalization of philosophy. It would be fair to suggest that the general consensus edged towards an interest in science and in how philosophy should deal with the sciences' seeming dominance over scholarship. Such generalizations are not helpful in getting an accurate read of the period. As Daniel J. Wilson has noted in his account of the period, "most philosophers were willing to accept the appellation 'scientific' if it meant simply careful,

rigorous inquiry; but that, philosophers often claimed, had long been characteristic of philosophy" and responses to the issue were so varied as to suggest that "each philosopher had his or her own conception of science and its proper relationship to philosophy"(Wilson 1990, p. 121-2). I take Wilson's claim to be consistent with my previously articulated concern that one weakness of Richardson's account is that it over-simplified the reception history of logical positivism in America. Such an observation arises because a.) There was no settled opinion on the relationship between science and philosophy within the philosophical discipline, and b.) It is not clear that an emphasis on "careful, rigorous inquiry" in philosophy was equivalent to the logical empiricists' conception of scientific philosophy.

But comparing the diverse American philosophical community to the somewhat uniform logical positivists raises the question of exactly what the various logical positivists meant by "scientific philosophy". In a typical passage from the early 1930s, one finds Carnap asserting the group's philosophical commitments denoted by the phrase "scientific philosophy":

> But what then, is left over for *philosophy*, if all statements whatever that assert something are of an empirical nature and belong to factual science? What remains is not statements, nor a theory, nor a system, but only a *method*: the method of logical analysis. The forgoing discussion has illustrated the negative application of this method: in that context it serves to eliminate meaningless words, meaningless pseudo-statements. In its positive use it serves to clarify meaningful concepts and propositions, to lay foundations for factual science and for mathematics. The negative application of the method is necessary and important in the present historical situation. But even in its present practice, the positive application is more fertile. We cannot here discuss it in greater detail. It is the indicated task of logical analysis, inquiry into logical foundations, that is meant by '*scientific philosophy*' in contrast to metaphysics. (Carnap 1932, p. 77)

For Carnap, "scientific philosophy" is represented in the logical analysis of language and in the fleshing out of the foundation of the sciences. As opposed to Richardson's suggestion quoted above, in this instance Carnap juxtaposes scientific philosophy against metaphysics and is not simply worried about any opposing conception of philosophy that isn't scientific; Carnap is clearly concerned with counteracting metaphysical approaches to philosophical problems. Thus, in offering a quick definition of "scientific philosophy" Carnap emphasizes both logical analysis and an anti-metaphysical stance as defining commitments of the movement.

In emphasizing logical analysis and a stanchly anti-metaphysical stance, such a framing of "scientific philosophy" squares with Herbert Feigl and Alan Blumberg's "introduction" of logical positivism to the American philosophical community in 1931. Their joint publication stressed the joining of empiricism and logical analysis as the hallmark of the new positivism; moreover, such a commitment is presented as necessarily leading to an unapologetic rejection of metaphysics as a whole. In Blumberg and Feigl's words: "logical positivism goes beyond the Comtean and pragmatic rejection of metaphysics as useless or superfluous and shows that the propositions of metaphysics, in most senses of the term, are, strictly speaking, meaningless" (Blumberg and Feigl 1931, p. 282). Both in letter and spirit, logical positivism, as it was presented to an American audience under the auspice of scientific philosophy, was framed by its rejection of metaphysics as a meaningful area for philosophical investigation.

Part of the problem of constructing any rigid notion of scientific philosophy is that – unless billed in the most general (and thus somewhat vacuous) terms – even very few logical positivists held onto the same ideal of a scientific philosophy throughout their

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career. Carnap scholars, most recently A. W. Carus, have gone a significant distance to work out the periodization of Carnap's differing views from the der logische *Aufbau der Welt* through the Logical *Syntax of Language* and past the semantics phases of Carnap's thought and how his conception of "scientific philosophy" changed over time. Moreover, it is now fairly well documented that logical empiricists as a group did not consist of individuals with uniform conceptions of scientific philosophy; though members shared a commitment to the scientific method and an anti-metaphysical bent, they differed significantly on a variety of philosophical issues. Uebel (2007) offers the clearest examples of disagreement between group members concerning protocol-sentences, but the overriding lesson of such historical treatments is that logical empiricism was far from representative of uniform opinion. Despite occasionally recognizing this fact, most American philosophers tended to attribute various philosophical commitments to logical positivism as a movement.

Within the American philosophical community it is just as difficult to delineate the philosophers who supported turning philosophy into a science from those that did not (and what commitments, exactly, this entailed supporting). Even amongst those American philosophers that were dedicated to making philosophy more scientific, such as Ralph Barton Perry, Arthur O. Lovejoy and others, views varied as to exactly what this meant or how it was supposed to be done. Considering the lack of attention generally paid to Peirce's philosophy in this period, both Perry's construction of the "new realist" group at Harvard and Lovejoy's overt scientism deserve some attention in any account. In Perry's case, the impetus to make philosophy more like science was fleshed out in a collaborative group consisting of philosophers and scientists The founding pressure of the

new realist was not concern over metaphysical speculation *per se* or a need to wave the flag of scientism; the new realists' main opponent was idealism and the dominance it held over American philosophy in the previous century. Perry construed the group's commitment to the existence of independent facts as juxtaposed against "the sway exercised a generation ago over sophisticated minds by the counter-thesis of idealism" (Perry 1918, p. 365).

To this end, the new realists were committed to incorporating "the attitude of science" into philosophy (Perry 1918, p. 365). Specifically, Perry claimed that:

> The realist, then, would seek in behalf of philosophy the same renunciation, the same rigor of procedure, that has been achieved in science. This does not mean that he would reduce philosophy to a natural or physical science. He recognizes that the philosopher has undertaken certain peculiar problems, and that he must apply himself to these, with *whatever method he may find it necessary to employ*. It remains the business of the philosopher to attempt a wide synoptic survey of the world, to raise underlying and ulterior questions, and in particular to examine the cognitive and moral processes. And it is quite true that *for the present no technique at all comparable with that of the exact sciences is to be expected*. But where such technique is attainable as for example in symbolic logic, the realist welcomes it. And for the rest he limits himself to a more modest aspiration. He hopes that philosophers may come like scientists to speak a common language, to formulate common problems and to appeal to a common realm of fact for their solution. Above all he desires to get rid of the philosophical monologue, and of the lyric and impressionistic mode of philosophizing. And in all this he is prompted not by the will to destroy but by the hope that philosophy may become more genuinely useful as a source of enlightenment. (Perry 1918, p. 367-8, emphasis added).

One must be careful not to read too strong an emphasis on science into Perry's position. He is explicit that the scientific method is applicable *only* where it is applicable; That is, Perry thinks applying the scientific method to fields such as formal logic is preferable, but that the application of said methods to all fields of philosophy would not be possible. Notice, this is not a rejection of these other fields as legitimate areas for

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philosophical inquiry but only a comment on different methodological approaches to different problems in philosophy; there is no strict adherence to the scientific methodin Perry's position. For Perry and the new realists, an endorsement of something akin to "scientific philosophy" simply meant the endorsement of the existence of objective facts coupled with the acknowledgment of the value of science. This is not to say, with Carnap, that philosophy's proper role is laying the logical foundations of science. Instead, Perry is committed to a view of philosophy that goes beyond the reach of science (by pulling together a "wide synoptic survey of the world") while staying tethered to the world *via* a commitment to the existence of independent facts.

Lovejoy's views, representing one branch of Critical Realism, lack some of the restraint expressed by Perry and others. Lovejoy's 1916 APA presidential address was explicit about his desires to make philosophy more like the sciences. Although Lovejoy readily admitted that, as of 1916, philosophy was commonly understood as something less than a science, there was no reason to assume that individual practitioners in the present state were good philosophers just because they were good metaphysical poets or preachers (Lovejoy 1917, p. 137). Recognizing two ways that individuals approach philosophy, Lovejoy's address exhibits both a desire to make philosophy into a science and the idea that not all of philosophy is necessarily amenable to being scientific. Lovejoy's main emphasis is to encourage cooperative inquiry and agreement in philosophical discourse, similar to the sciences, but he thought that philosophers could not simply adopt the methods of the sciences. Although Lovejoy is relatively clear when it comes to how this is supposed to occur, he considered certain methods, like statistical analysis in the physical sciences, to not be amenable for incorporation into philosophical

analysis (Lovejoy 1917, p. 141). But even in this stronger expression of a desire to

remake philosophy into a kind of science there is some restraint present in Lovejoy's

position. As compared with the logical positivists' position, Lovejoy didn't think that

philosophers who did not conform to a scientific world view were not practicing

philosophy, but that there were different kinds of methods depending upon the ends of

the philosopher.

Lovejoy's primary "scientific" conception of philosophy focused on the reduction

of error in philosophical reasoning as well as an emphasis on the cooperative nature of

inquiry. Philosophers could not adopt the method of the sciences:

> But we may at least tentatively assume-and it is the only assumption that
> will offer us any escape from our actuarial agnosticism-that the error in
> philosophy, the tendency to which is conclusively proved by the
> disagreements of philosophers, may be due to some ascertainable generic
> cause, always operative, indeed, in philosophizing as hitherto customarily
> carried on, yet not necessarily incapable of elimination by an appropriate
> change of methods. (Lovejoy 1917, p. 141).

Lovejoy's solution for the continuous proliferation of problems in philosophy is

not to strictly adopt the scientific method and focus on the kind of projects advocated by

the logical positivists. Instead, Lovejoy argues for a collective push towards cooperative

inquiry in philosophy. In a somewhat imaginative metaphor, this results in Lovejoy

suggesting that to avoid error philosophers must establish "a sort of metaphysical testing

laboratory, where the materials to be used in the construction of philosophical engines are

first systematically subjected to all conceivable strains and stresses" (Lovejoy 1917, p.

147). Much like the new realists, the guiding idea here is not a commitment to

incorporating the scientific method into philosophy or the banishment of metaphysics (or

traditional philosophical problems) so much as incorporating something like scientific habits.

Embodying, perhaps, Lovejoy's expression that early 20th century American philosophy should be described as "*diverse and swaying*," (Lovejoy 1917, p. 128) the reaction to Lovejoy's views was mixed. As a counter-balance to such views, a number of philosophers still argued for a significant separation of the disciplines. Philosophers such as F. J. E. Woodbridge, Josiah Royce, and Morris Cohen were all sympathetic to incorporating scientific results into philosophical analysis, but stopped significantly short of embracing the kind of scientism found in Lovejoy. Woodbridge argued stridently against the idea that metaphysics and philosophy needed to be more scientific (Wilson 1990, p. 130-1). Royce, though holding a more complicated view, argued that the very notions of science or a scientific community were highly "metaphysical notions" (Wilson 1990, p. 139). As early as 1910 Cohen was grappling with the question of the proper relationship between philosophy and science and offering arguments that were antithetical to what would emerge as the logical positivist's conception of philosophy. Cohen argued *against* the idea that philosophy needed to be more scientific and denied that most philosophical problems could be solved through empirical investigation (Wilson 1990, p. 134). Admittedly, Cohen did think that some branches of philosophy, specifically logic, could be reduced to a kind of science, but this seems to be the exception to the rule for Cohen as well as a common view about logic in the early 20th century (Wilson 1990, p. 135). For philosophy in general, Cohen, much like Perry, understood the aim to be that of pulling together a total world view – one that may use

results from the special sciences *but was certainly not limited to their input* (Cohen 1910, p. 409-10).

Lovejoy's address, and the varied reactions to it, was not the first 20th century instance of American philosophers publically debating the merit of adopting science or scientific inquiry as the model for intellectual investigation, and these concerns manifested themselves as issues surrounding the professionalization of the discipline and not necessarily as substantive philosophical theses. As early as the founding of the APA in 1901 such methodological concerns were clearly at the forefront of public discussions. The initial presidential addresses, given by J. E. Creighton and Alexander Ormond, were both framed by concerns that philosophy had lost its significance in the then current academic climate, especially when compared to the prestige afforded to the natural sciences. Creighton points out that the problem facing philosophers at the turn of the century was that, in the eyes of the public and other academic disciplines, philosophy's "scientific status as a real or independent subject of investigation is tacitly or explicitly denied" (Creighton 1902, p. 232). In the same vein Ormond observes that much doubt existed about the value of philosophy as a whole and that the sciences continued to drive philosophy "from preserve after preserve" until there would be no philosophical territory (Ormond 1903, p.115).

The concern shared by both Creighton and Ormond is that philosophers have failed to define clearly what, exactly, they do and how it relates to other fields. Creighton's approach in discussing the purpose of a philosophical association is to emphasize the importance of cooperative inquiry as modeled, if somewhat vaguely, on scientific associations. Such an idea is presented as compelling because it had historically

been seen to be a successful method for the natural sciences to secure cooperation among scientists and prestige for their respective disciplines (Creighton 1902, p. 221). Creighton understood this to mean that the role of such organizations was motivational as opposed to substantive (Creighton 1902, p. 234). That is, the initial role of the organization was conceived as "indirect, resulting from the effect it produces on its members" in opposition to that of a more proactive governing body (Creighton 1902, p.233). The hope was that, by assembling a specifically philosophical association and encouraging cooperation and original scholarship, philosophy would attain something akin to "scientific effectiveness" in the discipline (Creighton 1902, p.228).

Ormond too urged that philosophers must define their own agenda, but offered a more prescriptive address than Creighton. As Ormond argued, and as would be picked up by other philosophers throughout the early APA meetings, philosophers must come to agree on a specific method and set of doctrines in order to set the discipline on sturdy ground. This was supposed to be accomplished by explicitly defining a point of view, method, and "ultimate criterion" by means of which to test philosophical theories (Ormond 1903, p. 122). Such a plan arose for Ormond because philosophy had lost considerable intellectual ground to the sciences. In fact, Ormond thought that "this very invasion" of philosophy from the sciences would help the profession come to grips with its own methodological and substantive problems (Ormond 1903, p. 115). Although Ormond develops his own substantive views on this matter, no subsequent discussions arose addressing his platform for philosophical reform.

The 1908 APA meeting in Baltimore also marks another occasion where concrete platforms were suggested to improve the profession along scientific lines. Both Karl

Schmidt and Christine Ladd-Franklin presented papers on how to reconstruct philosophy with an eye squarely on the natural sciences as the model for reform. Such an approach, while similar in form to those suggested by Creighton and Ormond in 1902, differs from earlier attempts in that both Schmidt and Ladd-Franklin agreed that philosophy should explicitly model its methods and approach on the sciences. Both philosophers bemoan the current state of the discipline; Ladd-Franklin suggests that a secure, cooperative notion of philosophy is needed "to secure some common, accepted body of doctrine having scientific character" if the discipline is to be rescued from the charge "of being art, or poetry, but not science" (Ladd-Franklin, 1909, p.181). Similar concerns lead Schmidt to propose that:

> Let us then learn this as a lesson from mathematics and science: to choose
> a system as authority with which we can start, which we can criticize,
> measure by certain ideals, change, mend, supplement or finally discard for
> a better, but all with due deliberation and for definite, specific reasons. For
> the promotion of such an end, the meetings and discussions of the
> Philosophical Association seem to offer a suitable opportunity. (Schmidt
> 1909, p. 680).

The common idea expressed by both Schmidt and Ladd-Franklin is that philosophy *as a profession* is in need of reform, a successful version of which has already occurred within the sciences. Suggesting philosophical reform was, and is, not a new idea by this point in philosophy's intellectual history. As will factor into my later argument, it is worth noting that these calls for reform presuppose the defining of philosophy as an academic discipline as opposed to simply a subject of interest.[11]

Although some philosophers were receptive to this notion of reform, the published reactions to these suggestions fail to exhibit the kind of substantive cohesion

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[11] One of the earlier American accounts that explicitly defines philosophy as a professional discipline can be found in Frank Thilly's article "Psychology, Natural Science, and Philosophy" *The Philosophical Review* Vol. 15 (March 1906), p. 130 – 144.

pictured by Schmidt and Ladd-Franklin. Creighton and J. A. Leighton offered responses

to Schmidt and Ladd-Franklin's platforms by arguing that "from the very nature of

philosophy, it ought to be evident that such a platform is neither desirable nor possible of

attainment" (Creighton 1909, p.142). This is not to say that philosophical agreement is

impossible; both Creighton and Leighton stress that a broad sense of agreement is

possible, and already exists, in philosophical disputes. This is not because philosophers

should or even could offer concrete solutions to discrete problems (*ala* the outcomes

found in the natural sciences) but because of the unifying history behind the tradition.

Where all four philosophers do agree is that the discipline as a whole was perceived as

being unscientific in the sense that it did not offer objective certainty in its conclusion

(the perception that philosophy does not "bake bread") and that this was seen externally

as an undermining trait of the profession (Creighton 1909, p. 142). Of course, whether

philosophical conclusions are possible candidates for such objective certainty (or if those

are the kind of answers one should look for in philosophy) was a major dispute between

Creighton/Ormond and Schmidt/Ladd-Franklin.

Even for the large portion of philosophers who were attracted to turning

philosophy into a science, there was nothing in philosophy close to the kind of

agreement, progress, and status that was found in most[12] of the sciences at the time.

Herbert Schneider's report of the annual APA meeting in 1924 reflects the disparity

between both the older and younger generations of philosophers and the sets of problem

they were working on. This is partly the case because the discipline seems to be going the

way of the particular sciences – but only in the sense that research programs seemed

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[12] I say "most" in this case because psychology was also going through something akin to the growing pains philosophy was in this period. Exactly what method should be employed and what counted as psychological concepts opposed to philosophical was still not clear for either discipline.

highly catered to *individual* pursuits (Schneider 1925, p. 44). The kind of cooperative inquiry envisioned by scientifically-inclined philosophers in the early 20th century is nowhere to be found.

Aside from the various realisms and issues surrounding the APA, pragmatists and their descendants also represent an inconsistent match with the immigrating logical positivists. Both the earlier and later Dewey understands science as arising out of, and inherently shaped by, our social values. While being closer to a positivistic outlook on some issues, Dewey's philosophical stance is clearly more socially focused, and explicitly historically grounded, than what one finds in the various explications of logical positivism in America. In addition, Dewey clearly saw these views as set against the positivists' conception of philosophy. In correspondence with James Farrell in 1941 Dewey offers a number of remarks that are particularly telling. In the context of Farrell asking about taking a logic class with Alfred Tarski, Dewey equates the logical positivists' approach to philosophy with the sharpening of tools that one will never use, an inheritance of old metaphysics, and the separation of syntax from semantics to be "one of the silliest notions that has come down the pike (sic)" (Dewey Correspondence).

The students of Dewey and Woodbridge, broadly labeled the Columbia naturalists, could be described, much like Dewey, as developing a scientifically-inclined conception of philosophy. Despite the similarity in rhetoric one might find between the Columbia naturalists and some of the logical positivists, it would be hasty to conclude to that both camps converged on the same commitments. Andrew Jewett's account of the asymmetry between the Columbia naturalist and logical positivists' approach to normativity makes this clear:

The Columbia naturalists employed the term "normative" in a different fashion than did the logical empiricists. The latter group implicitly sorted and labeled value judgments on the basis of their empirical adequacy, describing as "normative" only those judgments they deemed "metaphysical"—i.e. unachievable or ideological, out of step with scientific truth. For their part, the Columbia naturalists labeled all conditional value judgments "normative," and only then distinguished between them on the grounds of empirical adequacy. To put the point another way, the naturalists employed "normative" in pragmatic, behavioral terms, applying the label to any beliefs or ideals, no matter how misguided, that actually functioned as motives to action. They viewed their cultural task as that of replacing empirically unsustainable normative commitments with more reliable normative commitments. By contrast, the logical empiricists imported their own conceptions of truth into the term "normative," restricting it to those beliefs or ideals that should not compel behavior, given the extant empirical findings. They sought to replace normative judgments with thoroughly empirical ones. In short, the logical positivists understood "normative" and "empirical" as opposites, whereas the Columbia naturalists viewed empirical judgments as a subset of the category of normative judgments: some normative judgments were empirically sound, and others were not. (Jewett 2011, p.105).

On Jewett's reading, the Columbia naturalists' use of "normative" was not only different from the logical positivists but also denoted a wider category. For my purposes, the more important observation is that the Columbia naturalists' wider understanding of 'normative' did not necessarily rule-out metaphysical notions from philosophical consideration. This is not to suggest that the empirical adequacy of a given belief played no role for the Columbia naturalists, as it clearly does, but that their conception of a scientific philosophy did not necessarily rule out metaphysical or ethical considerations.

Part of what I have been trying to get at and part of what I think is reflected in Jewett's above observation is the difference in meaning between American philosophers' and logical positivists' uses of terms such as "normative", "scientific", and more importantly "philosophy" or "philosophical". It would be wrong to claim that the rhetorical devices invoked by both early 20th century American philosophers and logical

positivists were not the same. This similarity exists only on a superficial level. When one

digs down into how terms such as "normative" or "scientific" were actually employed by

both groups, it seems clear that different philosophical axes were being ground.

Lewis, as the most influential American philosopher at Harvard between the two

world wars, seemed, unlike Dewey, to avoid the issue of whether scientific developments

necessitated a radical reconceptualization of philosophy – a reconceptualization along

scientific lines. Although his later views on logical positivism are a bit more complex

(and are dealt with more extensively in section III), Lewis was more concerned with

developments in logic and the analysis and interpretation of common experience than he

was on the proper relationship between science and philosophy (Lewis 1929, p. 36). I am

not suggesting, as Wilson does, that Lewis side-stepped the issue completely (Wilson

1990, p. 166). One finds references to the importance of newer methods in logic in his

review of *Principia Mathematica*. Despite certain problems Lewis finds in *Principia*, he

insists that "the chief value of the work is its method" (Lewis 1914, p. 502). Specifically,

Lewis claims that "Whatever one's opinion of logistic or of the particular treatment here

given to mathematics, *one must at least pay his respects to the logical rigor of the method*

and the splendid persistence with which it is maintained. The 'Principia' is to intellect

what the pyramids are to manual labor" (Lewis 1914, p.502). Lewis's concern here is

with metaphilosophical developments about philosophical methodology but his position,

much like most of the American philosophical community, turned on the importance of

rigor or exactness and not the incorporation of the scientific method in philosophy. This,

of course, is not to say that the scientific method plays no role in Lewis's thinking but

that his overriding concern was more with developing a technically rigorous philosophy as opposed to turning philosophy into a science.

In *Mind and the World Order* Lewis is initially preoccupied with understanding various approaches to knowledge and the proper place of metaphysics in relation to science as well as other branches of philosophy. This should not be understood as endorsement of scientific philosophy. In his review of C. D. Broad's *Scientific Thought*, Lewis comes off as relatively dismissive of the scientism he finds in Broad's approach to philosophy. In juxtaposing "speculative" and "critical" philosophy, Lewis thinks that Broad goes a bit too far in offering a quasi-scientific account of our recognition of physical objects. Specifically, Lewis suggests that, if it were not for a methodological mistake, what question Broad should have asked is "What do we mean, *in terms of direct experience*, when we say that a particular physical object exists, is round, at a distance, in uniform motion, etc..?" (Lewis 1925, p. 409). Lewis's own emphasis here is important precisely because it shows his commitment to understanding perception in terms of common experience in contrast to Broad's acquiescence to an explicitly scientific understanding of perception. What one does not find in Lewis's earlier writings is a concrete stance about "scientific philosophy."

Leading up to the 1930s the debate concerning science and philosophy ceased to be a major issue. Perry's band of new realists published a book and a number of articles outlining their positions, but shortly disbanded.[13] Lovejoy, though still convinced of the importance of making philosophy more scientific, turned his attention to the history of ideas. Other critical realists continued to work on epistemological problems inherited

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[13] Most notably "The Program and First Platform of the Six Realists" (Perry, et all) in 1910 and the book *The New Realism: Cooperative Studies in Philosophy* in 1914.

from English philosophers but offered no unified opinion on how philosophy should be done aside from assigning priority to understanding perception. This is not to say that defenders of a kind of scientism won said debates, but that debates over the relationship between science and philosophy seemed to have run their natural course. Even some philosophers, such as J. E. Creighton, who seemed to initially be defenders of a scientific conception of philosophy changed their position in an effort to avoid following the "false analogy" between scientific and philosophical practice (Wilson 1990, p. 159). As has been shown above, opinions were mixed when it came to how philosophers should deal with the newer prominence and respect afforded the sciences, but there is nothing inherent in the American philosophical scene prior to the arrival of logical positivism that would indicate a shared set of principles between the two groups. If this is the case, as I believe it is, there is no reason to think Richardson's account is correct about the stage being set for American pragmatist and realist philosophers to be especially receptive to the logical positivist's program. This is not to say that the interest in scientific progress shared by both camps had nothing to do with the reception history of logical positivism but that Richardson's interpretation of the period simplifies a complex story.

So what is one to make of all of this? An accurate picture of the period prior to the influx of logical positivism does not reflect a philosophical community unanimously practicing something akin to Richardson's conception of scientific philosophy. At the very least, *Richardson's assertion that traditional approaches to metaphysics and epistemology were clearly rejected by the 1930s because pragmatism was something like a naturalist version of scientific philosophy is too strong*. A number of philosophers in the American philosophical community prior to and during the 1930s explicitly did not

think of themselves as rejecting "traditional" approaches to these topics. For those that did, although some were influenced by the progress of the sciences, an equal number of them were influenced by British philosophy and the methods inherent in the new logics – most notably C. I. Lewis. Although Russell clearly gave scientific thinking a prominent role in their discussions of philosophical methodology, there is no reason to conclude that was transferred over in American philosophical doctrines.

Maybe all of this is being too uncharitable to Richardson. There is some truth to the notion that positivists, pragmatists, and realists in America converged on notions of "exact discourse" and "rigorous analysis." Though, certainly, one need not view "scientific philosophy" and "exact discourse" as synonymous terms, one might be tempted to understand a move toward "exact discourse" as a move that coincides with an interest in making philosophy scientific in line with what is suggested by Richardson. At least one problem still remains. Most of the early discussion of philosophy's need to become more scientific in the American philosophical community arose from two forces: concerns over the professionalization of philosophy and a strong interest in Bertrand Russell's logical and epistemological works in the early parts of the 20th century.

What makes the first point problematic for Richardson's account is that it would cast a significantly different light on the scientific rhetoric of American philosophers than one finds in the case of the immigrating logical positivists. The pressures and stresses on American philosophers were, perhaps obviously, significantly different from those of German and Austrian philosophers. As was argued for above, a significant amount of the early American discussion of "scientific philosophy" or "scientific rigor" was driven by concerns over philosophy's place in academia. Lovejoy's presidential address

represents a kind of climax to these debates and one still finds Lovejoy framing his "scientific philosophy" around concerns that philosophy had lost its places in academia as well as the public square. This is not to say that the logical positivists had no concerns *vis-à-vis* the professionalization of philosophy. The point is that even those American philosophers that were in favor of developing a scientific conception of philosophy were responding to different pressures from their logical positivist counterparts.

On the second point, although both pragmatists and positivists came to be influenced by a number of Russell's works, the American philosophical community seems to have adopted Russell's, G. E. Moore's, H. H Price's, and other's emphasis on perceptual analysis and methodology in their move to notions of rigor and clarity as opposed to those of their contemporaries in the various sciences. This is not to say that science plays no role in the influence from England – Russell's early philosophy is heavily influenced by a conception of philosophy that is, in a strong sense, scientific. Russell's influence in this respect actually extends to both camps, as both his writings on logic and epistemology were influential for American philosophers as well as most of the logical positivists, especially Carnap.[14] The American desire to transform philosophy into something that exhibited methodological rigor is more in line with the hazy and ambiguous notion of analysis that one finds in England at the time than the kind of formalism and methodological commitments one finds in Carnap and other positivists. Moreover, it seems clear that the external pressures (*e.g.,* competition with the sciences, pressure from University administrators, etc…) placed on American philosophers

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[14] Concerning Russell's influence on logical positivism, *Our Knowledge of the External World* was hugely influential for, at least, Carnap. See chapter 5 in A. W. Carus's *Carnap and Twentieth-Century Thought: Explication and Enlightenment* for an in-depth description of Carnap's early reading of Russell.

occurred in a different context than those factors that influenced the program of logical positivism.

Part of what makes Wilson's account of the early 20th century appealing is his lack of emphasis on such broad categorizations as scientific philosophy. Instead, one finds Wilson carefully attempting to pick out how individual philosophers, and small groups of allies, attempted to deal with changes in the sciences and academia and how those changes impacted philosophy. This distinction is *crucial* – given that any account would be remiss to suggest that advances in science had no impact on philosophy at the time, the other extreme that I find represented in Richardson's account is just as inaccurate. An accurate account of the period must be able to traces the influences of various causes without conflating the project of turning philosophy into a science with the impact of the sciences on philosophy.

Nonetheless, there is certainly *something* to the idea that philosophy in America was primed for engagement with logical positivism due to its sympathies with a broadly scientific view of philosophy. One line that must be walked carefully here is that between stylistic or methodological similarities and similarities in content; that American philosophers' rhetoric sounds like that of the logical positivists' does not imply that their philosophical positions and commitments were the same. Appearing in conjunction with the continued specialization of philosophy as an academic discipline, the use of technical or scientific terminology in the 1920s and continued pressure for rigorous analysis, one can imagine why, on the surface, it seems as if a "scientific philosophy" was being developed – if not explicitly, at least in spirit. Nonetheless, nothing close to a universally adopted program existed in American philosophy prior to the 1930s. Such an observation

is telling because it points to the unlikely chance that American philosophers adopted a sympathetic stance to logical positivism *because* both schools of thought held the same, or extremely similar, conceptions of philosophy.

III. Allies or Foes?

Even if American philosophers were not practicing something akin to Richardson's sense of scientific philosophy prior to the influx of logical positivism, it may be the case that by the mid to late thirties most had aligned their projects with those of Carnap, Feigl, Reichenbach, and others. Answering the second question posed above, whether American philosophers thought of their work as in the same camp as logical positivists, can help clarify the context of the 1930s. Although a number of the philosophers discussed in section II were still actively publishing in the 30s and beyond, most of the new and critical realists had, more or less, fallen by the wayside. As Victor Harlow notes in his study of the history of American realism:

> Despite the fact that since 1920 a greatly increased number of writers have become more or less interested in some form of realistic thought, the year 1930 carries relatively less material on realism than preceding years, and new points for discussion, as well as interest in them by new writers, are very few. But neither of the original schools have acquired a substantial open following; those who are disposed to accept the fundamental thesis of realism seem to want to do so with their own personal reservations…The final result upon American philosophy of thirty years of emphasis on the realistic thesis, though at present it looks large, cannot yet be appraised, of course, but it seems likely that the primacy in American philosophical literature of realism as an individual school is at an end. (Harlow 1931, p.105).

Though the influence of the new and critical realists never took off, Lewis's philosophical influence was only on the rise at the start of the 1930s. As mentioned previously, Lewis's relationship with logical positivism is a complicated story. At first glance, with his interest in formal logic and verificationism, Lewis looks like the perfect

ally for positivist philosophers in the 1930s. Herbert Feigl and Albert Blumberg's

introductory report on logical positivism in 1931 has C. I. Lewis listed as one of the few

American philosophers whose work exhibited "related tendencies" to those of the

positivists (Blumberg and Feigl 1931, p. 281). Lewis's *Mind and the World Order* is

listed next to P. W. Bridgeman's work in physics and S. K. Langer's philosophical text as

examples of these related tendencies. After his initial visit to the United States in the late

twenties, one finds Feigl reporting back to Carnap that Lewis's work represents an

extremely close alignment with the positivist's project.

Lewis represents an interesting test case for Richardson's account of the period.

Assuming Richardson is correct about the close relationship between pragmatism and

logical positivism in the 1930s, one would expect Lewis, as one of the leading

pragmatists in the 1930s and 40s, to at least be in broad agreement with the positivists'

conception of philosophy. As will become apparent in this section, such

metaphilosophical agreement is nowhere to be found. Though Lewis's views are clearly

not indicative of those of every American philosopher in the 1930s and 40s, he was

certainly a leading voice for pragmatism and the Harvard philosophy department.

Lewis's earliest reactions to logical positivism can be found in an unpublished

manuscript titled "Positivism, Phenomenalism, and Idealism".[15] Although Lewis

discusses numerous points throughout the seventy page manuscript, his discussion of the

positivists' criterion of cognitive meaning and how such a criterion should be related to

metaphysics are the most important parts for my present concerns. Lewis initially

recognizes the similarity between the positivists' criterion for cognitive meaning and the

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[15] This unpublished piece by Lewis is in the archives at Stanford. Since a copy of it is not readily or publically available (as far as I can tell) I will rely on Michael Murphey's interpretation of the text. It should be clear when I am quoting Murphey and when I am quoting Lewis from Murphey's text.

pragmatist conception he traces back to James's philosophy. Lewis draws a distinction

between two forms of the maxim as "a sensible methodological restriction for science"

and as a "metaphysical principle" (Murphey 2005, p. 222). Lewis has no problem with

the physicist invoking the criterion for cognitive meaning in order to exclude a given

hypothesis from physics, but he balks at the notion that one can extend such a criterion to

metaphysics. In wondering what makes a given hypothesis meaningless, Lewis suggests

the following example:

> That there is a super-human and very powerful being, conscious like
> ourselves, who cherishes normal vital wholes, and who acts in conformity
> with this purpose to produce and restore such wholes in ways which do
> not exhibit any *other* detectable uniformity than this uniformity between
> his cherishing and process which he determines by his will. This being is
> not observable to us save through his effects, of which certain distinctively
> vital phenomena are instances. (Murphey 2005, p.222).

> For Lewis, there is nothing philosophically out of bounds in the above example.

Specifically, Lewis claims that "I cannot find anything essentially inconceivable in such a

supposition – anything which is 'bad grammar'" (Murphey 2005, p.222). To rule out such

notions from philosophical consideration would make one "not a good scientist, but a bad

metaphysician," because such a move would rule out a whole host of hypothesizes that

are considered central to philosophical reflection (Murphey 2005, p. 223). Lewis

concludes by settling on a position that is explicitly anti-positivistic in its outlook and

sides with more "traditional" philosophical schools:

> I should, then, like to take my place with the critical realists in a class
> which is to be distinguished from positivism and idealism. And I should
> live to invite the neo- and other kinds of realists to elucidate themselves on
> this point. Is there any reason for the positive assertion that reality is
> limited to what is humanly experiencable as it is humanly experienced?

> The statement that it is seems to me a monstrous fallacy of
> anthropocentrism. And also it insults those interests which are the most

frequent instigation to interest in philosophy—in the question of any other life than that presently verifiable, in any other cosmic powers than those science can observe, in any relation of human ideals to what is beyond what human observation can inductively prove. If philosophy can bake no bread for those who beg some answer to such questions, at least it need throw no stones. (Murphey 2005, p.226).

This manuscript is interesting because it represents Lewis's early engagement with positivism and clearly represents a point of view that does not consist of simple agreement with Carnap and Schlick. Instead, what one finds in Lewis's earliest reflections on logical positivism in general, and the verification theory of meaning in particular, are arguments that place Lewis squarely in opposition to Schlick, Carnap, and others. One of Lewis's concerns can be recast as metaphilosophical; one of the largest problems with the verification theory of meaning is that it represents a narrowing of philosophy that is simply unacceptable. Although Lewis was clearly committed to something like the verificationist theory of meaning, his views in the early 1930s were simply not amenable to a conception of philosophy that would deny any view deemed unscientific.

The initial published disagreement between Lewis and logical positivism appears in his APA presidential address in 1933 and is continued through Schlick's response to Lewis and in Lewis's later articles "Judgments of Value and Judgments of Fact" and "Logical Positivism and Pragmatism". One of the main points of contention between Lewis and logical positivism continues to be their conceptions of the verification theory of meaning and its elimination of metaphysical speculation from philosophy. Much of what Lewis says in his presidential address was derived from the earlier "Positivism, Phenomenalism, and Idealism" manuscript.

Schlick's response is generally conciliatory and he initially attempts to smooth over the differences he finds between pragmatist and positivists. Schlick notes that Lewis objects to the positivist conception of meaning as verification because "its acceptance would impose certain limitations upon 'significant philosophic discussion' which, at some points, would make such discussion altogether impossible and, at other points, restrict it to an intolerable extent" (Schlick 1936, p.343). Schlick's solution to this problem is to expand the notion of "empirical sentences" to include the kind of traditional metaphysical statements Lewis is concerned with.

Things take a particularly odd turn when discussing traditionally construed metaphysical problems such as immortality (defined as the problem of survival after death). Lewis wants to maintain that such questions are *genuine* philosophical questions despite the fact that the answers to such questions are neither empirically verifiable nor fall within the purview of the sciences. Surprisingly, Schlick objects to this point on the grounds that questions of immortality are, indeed, *empirical* questions. Specifically, Schlick claims "We must conclude that immortality, in the sense defined, should not be regarded as a 'metaphysical problem', but is an empirical hypothesis, because it possesses logical verifiability. It could be verified by following the prescription: 'Wait until you die!'" (Schlick 1936, p.356). "Logically verifiable" in this case simply means that a given sentence can be verified "by applying to the sentence the rules of the logical grammar of our language" which are "prescriptions stipulated by acts of definition" (Schlick 1936, p.349). This move seems particularly "odd" because it would seem to allow any sentence to become an empirical hypothesis insofar as one could stipulate a given definition for a linguistic rule.

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Either way, Schlick's attempts at smoothing over the differences between Lewis and logical positivism simply miss the point. Lewis's objections do not turn on Schlick and others maintaining too narrow a conception of the empirical; Lewis is quite clear (and gets clearer in later publications) that he considers such a hypothesis as both meaningful and non-empirical. Although Lewis never really addresses Schlick's point here (he simply repeats his earlier criticisms of logical positivism), it seems clear that Lewis would find it unacceptable because it fundamentally misses the different metaphilosophical commitments operating in the background.

Lewis's later publications do not exhibit the conciliatory spirit found in Schlick's response. More specifically, by the 1940s Lewis's view of positivism seems to have remained relatively unchanged. He still held onto the same point of disagreement with Schlick as is apparent in the 1933-36 exchange. Carnap's transition from the *Aufbau*, constructionist and phenomenalist project into a formalist conception of philosophy actually pushed Lewis farther away from positivism. Aside from Lewis's older objections to the positivistic conception of verification, Lewis thought that Carnap's move into formalism narrowed the focus of logical positivism – a focus which Lewis thought of as being excessively narrow in the first place. Commenting on this switch, Lewis observed that the

> Difference between the logical-positivistic and the pragmatic mode of
> approach to questions of meaning runs very deep, eventually, because this
> attempt to logicize all problems, and to regard them as correctly and
> unambiguously statable only in 'the formal mode'—in terms of syntax of
> language—is connected with the logical-positivistic conception that
> philosophy has no legitimate business except that of logical analysis, and
> that philosophic questions which are characteristically stated in 'the
> material mode,' and which, e.g., concern the relation between experience
> and real objects, are 'pseudo-problems'….A pragmatist must regard such

restrictions to the formal mode as inevitably resulting in failure. (Lewis 1941, p.96-7).

Perhaps more emphatically, Lewis traces the history of pragmatism as one deeply committed to taking metaphysical problems seriously and claims that "it is plain *without discussion* that pragmatist and logical positivists diverge on this point" (Lewis 1941, p. 104). In Lewis's own words:

> Peirce identified himself with a metaphysical position which is a form of pan-psychism or objective idealism. James criticized absolute idealism not as meaningless but as false; and argued not only for realism and pluralism but also for the significance and possible truth of various more speculative metaphysical assertions – as in 'The Will to Believe' and 'The Energies of Men'. If Dewey ordinarily eschews metaphysical questions and exhibits towards certain metaphysical theses—such as those of Platonism—an attitude somewhat approximating to that of the logical positivists, at least he is definitely a realist, and could not plausibly be interpreted as denying significance to all metaphysical issues…It happens that, with the notable exception of James, pragmatists, like logical positivists, have not been much given to discussion of such questions. *But if these are recognized to be significant, then it seems required to remark the existence of a class of statements , commonly labeled 'metaphysical,' which have meaning but are nevertheless not verifiable under human conditions, and do not belong to the field of science*. (Lewis 1941, p.104, 107).

Lewis's statement is particularly revealing if we momentarily leave aside Richardson's definition of scientific philosophy and recall Carnap's position as described at the beginning of section II. There, it will be remembered, Carnap's definition of scientific philosophy turned on the kind of logical analysis that eventually came to fruition in Carnap's formalist project as found in his *Syntax*. *Lewis clearly rejects this conception of philosophy because a number of epistemological and metaphysical issues would unacceptably be rendered "out of bounds" for philosophers*. Lewis thinks that from a historical perspective, pragmatism in general cannot be read as even close to logical positivism when it comes to the issue of metaphysics. Such cross-purposes would

seem to clearly show both that Lewis and the logical positivists (at the very least Carnap post-*Aufbau*) held significantly differing conceptions of philosophy and that Lewis was under no illusions as to this difference.

If such glaring differences did exist, another approach to consider would be to ask why Feigl and others thought of Lewis's philosophy as congenial to their own. Part of the reason would seem to rest on a philosophical misinterpretation and part of it lies on Lewis's general disposition to not argue publically over philosophical matters. Philosophically, it is not difficult for one to give a reading of Lewis's earlier works, especially *Mind and the World Order*, which gives the impression of a philosopher who has no taste for metaphysics. In his effort to critique contemporary theories that have construed the relationship between giveness and the conceptual incorrectly, Lewis offers a critique of two different general positions that underlie a myriad of different theories – one where pure receptivity is considered the way in which one is aware of the world, and one where our knowledge of the external world rests solely on our construction of it (Lewis 1929, p. 39-40). Lewis thinks that philosophers who embrace either position do not differ from him in their "analysis of ordinary experience but rather a difference in the denotation given to the phrase 'true knowledge' *because of a metaphysical theory* which denies ultimate reality to what is cognized by science and common sense" (Lewis 1929, p. 42). Whether Lewis is discussing the intuitionism of Bergson or the new realism of Perry and Holt, the tie that binds these radically different approaches to perception is the common mistake of allowing metaphysics to trump the kind of knowledge one gets from an analysis of science and common sense. If one is inclined to minimize or ignore the role metaphysics plays in philosophy, as Feigl and other positivists were clearly disposed

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to do, Lewis's approach to undercutting other theories of our knowledge of the external world would seem attractive to say the least.

Even this small section of *Mind and the World Order* can go some distance in explaining why one may be tempted to read Lewis as holding a similar view as the logical positivist when it comes to metaphysics. Lewis, both in his early and later works, is still concerned with how metaphysics underlies our philosophical views of "true knowledge" from science and common sense. The majority of his introduction in *Mind and the World Order* is spent explicating his approach to philosophy, specifically how metaphysics fits into the general picture. Although Lewis is rather critical of how some philosophers have used their metaphysical commitments to reject scientific conclusions, this does not lead Lewis to conclude that metaphysics is an unnecessary or problematic field of philosophical inquiry. Instead, one finds Lewis drawing a distinction between metaphysics as responsive to a reflective method versus the kind of knowledge one finds in the sciences (Lewis 1929, p. 8). By "reflective method" in this case Lewis simply means that metaphysical problems "are to be resolved as much by criticism and reflection as by empirical investigation" (Lewis 1929, p.7). Although how, exactly, metaphysical problems are to be resolved empirically is somewhat obscure, Lewis eventually clarifies his position:

> With this explanation, I hope I shall cause no confusion if I say that it is only so far as they are thus critical and reflective that the problems of ontology and cosmology are truly philosophical and that metaphysics as a philosophical discipline is concerned with the nature of the real only so far as that problem is amenable to the reflective method and does not trench upon the field where only scientific investigation can achieve success. (Lewis 1929, p.8).

Lewis argues that the initial principles and fundamental criteria common to all of the sciences and "the general business of life" are metaphysical in nature and the business, properly speaking, of philosophy (Lewis 1929, p. 8). Lewis's distinction about the kinds of issues metaphysics should deal with is not meant to sort philosophy between "serious" and "speculative" endeavors, though; He is quite clear that metaphysics, much like other branches of philosophy, is capable of progress and clarity. In fact, Lewis goes so far as to suggest that metaphysics is capable of progress just in the same sense that formal logic is and, in fact, problems in both fields are of the same general type (Lewis 1929, p. 15). Metaphysical speculation only goes astray when "attempting by sheer force of rational reflection to transcend experience altogether" (Lewis 1929, p.8). Squarely in the pragmatic tradition, Lewis's position on metaphysics in *Mind and the World Order* should be thought of as restrained but not reductive or eliminative. Moreover, as opposed to Carnap's position, Lewis is quite clearly not pitting his approach to philosophy against metaphysics.

Towards the end of his career Lewis turned his attention away from epistemology in an attempt to finish the grand ethical project he had been planning for most of his career. Although he did spend the last years of his life working through these ethical problems(though ultimately such work never saw the light of day in any published form) Lewis still considered the place and importance of metaphysical speculation in philosophy. In correspondence with Victor Lowe six years before his death, Lewis revealed that he had "a number of very unfashionable things" (Murphey 2005, p. 334) to say about metaphysics but, for whatever reason, refused to publish them. Despite Lowe's insistence on the importance of Lewis's metaphysical views, such a publication never

surfaced. Nonetheless, metaphysics or metaphysical speculation (the "content" of the

original wonderment that serves as the starting point for all philosophy) held a constant

presence in Lewis's thought – one that certainly does not seem amenable to the

conceptions of philosophy one finds in Carnap or Schlick (Lewis 1955, p. 233).

Aside from the substantive philosophical differences between the positivists and

Lewis, there are a number of reasons to think that from a professional and personal

standpoint, the positivists were not exactly to Lewis's liking. In recalling his time at

Harvard, Morton White paints a picture of the department that sets Lewis at odds with

Quine and others who were welcoming towards logical positivism. White recalls that

Lewis frequently referred to the positivists as "a bunch of phonies" and, when acting

chair of the Harvard philosophy department, was generally concerned that if White,

Quine, and others had their way, they would fill the Harvard department with positivists

(White 1999, p. 104). This, at least in White's mind, led Lewis to frequently support the

appointment of "devotees of *traditional* philosophy" over those that prescribed to the

logical positivists' conception of philosophy (White 1999, p. 104-5). Such observations

from White would seem quite odd if Lewis were so immersed in a similar conception of

philosophy as the logical positivists as to relegate his concerns over their philosophical

positions to second order disputes.

Marjorie Grene offered similar observations about Lewis's relationship to logical

positivism during her graduate career at Harvard and Chicago. Grene was initially drawn

to studying Lewis's epistemology but for some reason (unfortunately she does not say

why), Lewis suggested Grene go to Chicago to study with Carnap if she wished to pursue

a focus on contemporary epistemology. Although Grene, who, at the time was apparently

unfamiliar with Carnap's works outside of his 1936 lectures at Harvard, ended up at Chicago, she quickly realized Carnap's philosophical project, especially his epistemological commitments, was incompatible with Lewis's approach. Grene recalled that because she was a student of C. I. Lewis it was difficult to see how one could create a purely extensional logic, why one might be committed to a form of phenomenalism, and what exactly this notion of "exact, all-explaining 'scientific method'" was supposed to be (Grene 2002, p. 8-9).

Ultimately, it is fair to say that Lewis was initially attracted to some of the positivists' positions, but quickly began leveling quite clear criticisms against them. Murray G. Murphey's summary of Lewis's attitude provides the best synopsis of this:

> *It is clear that Lewis found Logical Positivism at this time both very attractive and unacceptable.* He saw the verification theory of meaning as very similar to the pragmatic view, found "methodological solipsism" a version of the egocentric predicament, was drawn to Carnap's constitution theory, and to Schlick's interpretation of protocols. At the same time, *he thought the Positivists' view too narrow and could not accept their rejection of the cognitive status of valuation, ethics, and metaphysics.* (Murphey 2005, p. 233).

For my purposes this last part is telling. If Richardson's account is correct concerning the fundamental agreements between positivists and pragmatists concerning conceptions of philosophy, it is difficult to reconcile his view with statements like Murphey's that express fundamental and significant differences between the two groups. Although Lewis found some of the positivists' positions and terminology congenial to his own project, he clearly took issue with their conception of philosophy in general. As Murphey makes clear, "when it became clear that the logical positivists"… "were seeking to extend the reach of science to include all knowledge, Lewis was forced to take up arms to defend values and morality against their scientistic imperialism" (Murphey 2005, p. 406).

While I have not covered every disagreement between Lewis and the various logical positivists, it seems clear that Lewis *qua* leading pragmatist of the 1930s was not working under the same conception of philosophy as Carnap and Schlick. Although there may exist some points of agreement between Lewis and the positivists, such as the phenomenalism found in Carnap's *Aufbau*, such points cannot overshadow the significant disagreement between competing conceptions of philosophy. This difference strikes at the heart of the matter when it comes to Richardson's account of the period. It should be remembered that Richardson's contention is just that leading pragmatists and positivist philosophers converged on a similar, if not the same, conception of philosophy as scientific. No such conception is to be found from Lewis's standpoint. Worse, perhaps, for Richardson's account would be the fact that the difference between Lewis and Schlick explicitly turns on not only the proper conception of philosophy but also over the treatment of metaphysics.

IV. Conclusion

In retrospect, if most philosophers prior to or during the 1930s felt the influence of new developments in the sciences on their disciplines "only a few recalled trying to transform philosophy into a science" (Wilson 1990, p. 182). What is generally true of most, though not all, philosophers in the period leading up to the reception of logical positivism is that they frequently cited an interest in "exact discourse," "rigorous methods," and "the influence of the sciences". Why this might have been the case will be dealt with at the end of the chapter.

Another problem with Richardson's account is that he focuses on the works of Morris and Dewey but seems to ignore other pragmatist philosophers. Worse,

Richardson seems more than willing to generalize his conclusions about the proper interpretive framework to include all pragmatists (if not all American philosophers). This is a glaring error given the vast differences between, say, Dewey and Perry (as well as many other American philosophers). For example, take the issue of the inseparability of scientific progress and political commitments. One might be able to easily make the point that this is a concern that unites some of the logical positivists and Dewey. Such a conclusion does not get us far. The same cannot be said for fellow pragmatists like Lewis. Moreover, there are a number of American philosophers that simply cannot be forced into this characterization (Henry Sheffer at Harvard, for example, was purposely mute on such topics).

If Richardson's description of the philosophical community in the 1930s does not stand up to close scrutiny, what will? Although it makes for a cluttered and complicated historical account, the most accurate understanding of philosophy in the 1930s and 40s is one of confusion. As Richardson himself notes, it is not clear that pragmatism was necessarily the dominant school of thought in the 30s (or even before then). The rise of a quasi-scientific philosophy in Perry and others in the new realist school of thought was certainly a competitor, if not already a successor, to pragmatist thinkers around the time of William James's death in 1910. Even Lewis, whom some have labeled "the last great pragmatist," can be seen as slightly weary in his association with the label.[16] Ultimately, an accurate history of early 20th century American philosophy would be one that resists the urge to classify the first thirty or so years of the century under one banner.

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[16] In correspondence with Morton White one clearly finds Lewis's weariness with the label: "I have always misdoubted any claim I could make to be classed as a pragmatist but I think I have followed out some of Peirce's most basic thoughts." This unpublished correspondence can be found in White 1999.

What Richardson's view fails to account for, and what I am arguing is integral to understanding the development of philosophy from the 1930s through 40s, is that the differences between American philosophers and logical empiricists *were* problems of serious concern – at least for the pragmatists and realists. By the 1930s there was clearly no settled opinion on philosophy's relationship to the sciences. As described above, there were certainly a number of strong views on the issue, but nothing close to agreement on the issue was ever reached. Such an observation might not be damaging to Richardson's account if one could show that most philosophers during the 1930s thought of themselves as working on projects along the same lines as the positivists, but this is certainly not the case either. While Lewis, other American philosophers, and the logical positivists shared some themes, most of those shared themes did not amount to both sides having similar, let alone the same, positions on a given issue. In fact most of the views held by Lewis and others are essentially incompatible with those of Carnap, Feigl, and Schlick.

It may be the case that for philosophers like Charles Morris the influx of logical positivism was a breath of fresh air, but it is simply too large a jump to suggest that Morris's reaction to the arrival of Carnap and others was representative of the majority of the philosophical community at the time. Although it is generally true that many philosophers, at least at Harvard, were turning towards more "technical" fields of inquiry in philosophy, *why* they were making such a turn is a question up for debate. Kuklick has shown that from 1920 to 1930 (in contrast to the previous period from 1907 – 1918) there was a marked increase in technical specialization for philosophy graduate students at Harvard (from 32% to 51%) as well as a marginal increase in specialization in metaphysics (from 28% to 31%), while a significant decrease of interest in specializing in

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moral and religious philosophy (from 40% to 22%) (Kuklick 1977, p. 476). As Kuklick

himself warns, for admittedly different reasons than I do, one must not draw hasty

conclusions from this data.

There are a number of sociological and philosophical considerations that help

explain the turn towards a specialization in technical fields of philosophy and that do not

commit one to Richardson's account of the period. Leading up to the 1920s, Royce's

seminars on philosophy and science, seminars with a heavy emphasis on metaphysics and

the relationship between science and philosophy, continued to be influential (Wilson

1990, p. 136-7). Lewis, who wrote his dissertation under Royce, was teaching seminars in

epistemology at Harvard in the 1920s and continued to emphasize the importance of

metaphysics – specifically a Roycian conception of community – as a field of

philosophical inquiry (Wilson 1990, p. 167). Even though Lewis's interests lay, partially,

with British approaches to logic and epistemology, he certainly is not a representative of

a whole-sale rejection of traditional approaches to philosophy in general. Given the

continued presence of traditional figures, traditional approaches, and a continued

emphasis on the importance of metaphysics, it is difficult to conclude that the technical

turn represented in Kuklick's statistics necessarily indicates the development of and

agreement with scientific philosophy. Now, it may be the case that those interested in

technical fields were predisposed to be interested in the works of Carnap, Feigl, and

others but this is a different conclusion from the one offered by Richardson.

Three sociological points to take into consideration would be the undergraduate

careers of philosophy graduate students in the 1920s, the emphasis on going abroad for

graduate educations in the 19th century, and the continued professionalization of

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philosophy. Although there seems to be no ready data to support it, an easy assumption would be that a number of philosophers attending graduate school in the 1920s were more likely to have a background in one of the sciences than they might have in the earliest parts of the 20th century. As the most notable example, W. V. O. Quine arrived at Harvard to study with a background in mathematics as opposed to the philosophical canon. The philosophical training of William James, John Dewey, and others differed significantly from that of Quine, Nelson Goodman, and Morton White. It may simply be the case that as the profession changed from the late 19th century to the mid-20th century, with rapid advances in the sciences and formal logic, emphasis on the importance of more technical fields of philosophy became the focus of the profession.

The German University system of the late 19th century was a highly popular destination for American students interested in pursuing advanced degrees. Such facts become more salient in this case when it is realized that a large number of prominent American philosophers pursued their advanced degrees at German institutions. One can count early American philosophers, such as Williams James, in this boat as well the majority of philosophers involved in both the new and critical realist movements. The transition from 19th to 20th century educational concerns represented, on a whole, an "advancing scientism" towards the value and place of academic disciplines that were inherited from German institutions (Veysey 1965, p.173). Although such facts do not fully explain the turn towards more technical studies, they would seem to indicate a predisposition towards such concerns.

This would seem to press the question as to why such an emphasis became prominent in the 20s and 30s. An answer to this can be found in the changing picture of

professional academia, what one might call a *professional* commitment to making philosophy, *qua* academic discipline, more like the sciences, as opposed to a *substantive* commitment to the same idea. In part, the specialization of philosophy resulted from its loss of authority in the 19th century, once the authority that was previously seen to radiate from theological and speculative grounds was eroded by rapid advances in the sciences and their breaking away from being part of natural philosophy into their own autonomous disciplines. For many philosophers in the early 20th century "being scientific" represented the reestablishment of institutional authority for philosophy as a discipline, a largely defensive move in the direction of professionalization that can be seen as split between two components – institutional and substantive (Wilson 1990, p. 124).

On the institutional side, philosophers seemed to have no qualms modeling philosophy after the sciences. All that is reflected in this commitment is largely indicative of philosophy as we see it today: the proliferation of academic journals, advanced degrees, organized associations, and regular meetings (Wilson 1990, p. 124). At the turn of the 20th century such moves could be seen as defensive, given the active interest philosophy as a disciplined held in preventing "further defections from the traditional preserve of moral philosophy by making itself more specialized, more scientific, and more professional" (Wilson 1990, p. 124) and because such a move allowed philosophy as a discipline to survive in an atmosphere that emphasized "the scientific mind" as "the only kind of mind, except the poetic, which commands the respect of scholars" (Eliot 1878, p. 473).

As I have argued throughout his chapter, there was little to no agreement on the substantive issue of how philosophy should become more scientific or if it should

become scientific at all. The difference in undergraduate backgrounds mixed with

external pressure on philosophy as an academic discipline to become more like the

sciences, at least on the institutional side of things, may help explain why the period of

the 1920s showed marked interest in technical fields without having to subscribe to the

idea that most philosophers were practicing something like scientific philosophy. This, I

think obviously, is a somewhat unsatisfying account of why philosophers in the 1920s

turned towards technical interests in philosophy but, while leaving a number of questions

unanswered, this account offers a more accurate and nuanced picture of the status of the

discipline by the early 1930s than Richardson's account of the period.

V. Wilfrid Sellars and American Philosophy

My main conclusion in this chapter is that when attempting to place those

philosophers who were educated during the 30s in their historical context, one cannot

simply focus on what seems to be the dominant conception of philosophy at the time.

For Sellars's American context, there are clearly split allegiances that come from the

distinctly American tradition to be found in realists and, to some degree, pragmatist

philosophers (especially his father RWS and Lewis) and those derived from his readings

and interactions with various logical positivists (especially Carnap and Feigl). At

Harvard, Sellars took classes with Lewis and Perry. Although Sellars was well aware of

new realism and Perry's contribution to the movement, Lewis's philosophy warrants

more attention throughout Sellars's career. Despite the fact that Lewis's work does not

consistently appear throughout Sellars's canon, Sellars explicitly characterized himself as

involved in a "long conversation with Lewis" (Sellars 1973, p. 612); said conversations

also pop up as the background to Sellars's debates with Lewis's main defenders,

Roderick Firth and Roderick Chisholm[17]. Sellars was at Oxford during the period that A. J. Ayer, J. L. Austin, Isaiah Berlin, and others were carefully working through and discussing *Mind and the World Order* and recalled these discussions as the highlight of his time at Oxford (Kuklick 2001, p. 243).

Sellars is explicit about his philosophical debt to previous strands of realism in general and about his debt to his father's version of critical realism in particular (Sellars 1975, online). Although Sellars's most sustained engagement with RWS and critical realism occurs in his 1954 article "Physical Realism", his earliest publications and reflections make it clear that he was aware of the narrative described above. Sellars references the move away from naïve realism in the history of American philosophy as moving through new and critical realism (Sellars 1947a, p. 114). Sellars is also explicit that his "coming to philosophical consciousness" occurred when "the great battles between systems which began the Twentieth Century were drawing to a close, although the lightening and the thunder were still impressive. I cut my teeth on issues dividing Idealists and Realists and, indeed, the various forms of upstart Realism" (Sellars 1979, p.1).

This is not to minimize the impact logical empiricism had on Sellars's thinking. Sellars was clearly interested in logical empiricism as early as 1937 when he took a course offered by Quine that focused on Carnap's *Der logische Aufbau der Welt* and his then recently published *Logical Syntax of Language* (Sellars 1975 online). In addition, Sellars's time at the University of Iowa with Herbert Feigl, his long correspondence with

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[17] The main debate between Sellars and Firth appears in two articles, "Giveness and Explanatory Coherence" and Firth's "Coherence, Certainty, and Epistemic Priority". Aside from some private correspondence, Sellars and Firth continued to debate philosophical issues inspired by Lewis's philosophy into the 80s.

Gustav Bergmann, and his later time with Feigl at University of Minnesota leaves little

doubt that logical positivism plays some role in Sellars's thought. It is the particularly

American context and debates, and how they influenced Sellars's thinking about logical

positivism and language, that I want to emphasize throughout this project.

What I have hopefully done in this chapter is offer an accurate interpretation of

the battle lines that were drawn prior to and during Sellars's emergence on the

philosophical scene. What will continuously loom in the background is this concern over

the development of a specifically philosophical contribution to knowledge that falls

neither into scientism nor metaphysical speculation. Although Sellars does not generally

frame his earliest publications in the 1940s in terms of debates from the beginning of the

century, Sellars is clearly aware of the American dialectic that greeted the influx of

logical positivism in the 1930s. If I am thus far correct about the reception history of

logical positivism, the door is open to discuss the influence of "metaphysically-inclined"

philosophers during and past the rise of logical positivism. Such an opening is important

precisely because, as I will argue, such "traditional" concerns are very much alive in

Sellars's earliest publications. How this influenced Sellars's early views and how much

logical positivism did, or did not, come to dominate his philosophical concerns will be

addressed throughout the rest of this project.

Chapter 2: Pure Pragmatics

I. Introduction

Despite the differences between some American philosophers and the immigrating logical positivists, by the mid to late 1930s, logical positivism was the dominant school of thought in the American philosophical community. Carnap's formalist project, as found initially in his *The Logical Syntax of Language*, offered a particularly mesmerizing program that continued to hold the attention of American philosophers, and even some American scientists, throughout his programmatic shift into formal semantics. This is not to say that traditional American philosophers disappeared – C. I. Lewis's influential *An Analysis of Knowledge and Valuation* was published in 1946 and a number of philosophers continued to argue against various commitments of logical positivism (perhaps most notably Richard McKeon, Warner Wick, and others at the University of Chicago, Columbia, and elsewhere) – but that the positivists' conception of philosophy held sway. It is within this climate, one of a dominant logical positivism and a marginalized "traditional" American philosophy, that Wilfrid Sellars emerged on the philosophical scene.

The publication of *Introduction to Semantics* in 1942 marked Carnap's full transition from the syntax to the semantics phase of his writings on logic and language. Although logical positivists did not have uniform commitments – Neurath's lengthy debates with Carnap over protocol sentences and his stance *against* the formalization of semantics being one example of substantive disagreement – by the

1940s Carnap's logicism had become the popular representation of logical positivism in America.

Sellars held conflicting views on his relationship to logical positivism in general and Carnap in particular. In his early papers and correspondence one finds a version of Sellars that seems relatively eager to ensure that his philosophical projects coincide with those of the positivists. Sellars is clear that he understood the project of "pure pragmatics" to be a "form" of logical empiricism (Sellars 1947a, p. 8) and at one point describes himself as "a rationalistic realist who has *deserted* to the camp of logical empiricism" (Sellars 1947b, p. 28). Although Sellars quickly moved away from these positions – describing himself in 1949 as having *previously* been under the sway of certain aspects of logical empiricism – it is undeniable that they play a major role in his early philosophy.

Such historical considerations have led some, most notably A. W. Carus, to suggest that the proper way to understand Sellars's earliest works is by placing them in the context of the logical positivists' overall philosophical commitments, especially those found in Carnap's understanding of formal semantics. Such a placement need not entail agreement with Carnap and others; clearly, Sellars's philosophy could be understood as a reaction *against* logical positivism. According to Carus, this is not the case; Sellars's critiques of Carnap are "framed as internal" and his stance towards logical positivism is generally one of "agreement and endorsement" (Carus 2004, p. 319).

This move, at least initially, seems plausible. Certainly the terminology used by Carnap is found throughout Sellars's publications and is omnipresent in his earliest papers. In addition, the tenor of Sellars's earliest publications seems to be one of reform

as opposed to revolution. One finds Sellars arguing that pure pragmatics would "make possible, indeed necessitate, a return to the *Aufbau* stage of Logical Empiricism, but with a conception of the *Aufbau* which is as much richer than that of the early thirties" (Sellars 1947a, p. 8). What's up for debate is not whether Sellars's philosophy developed within the context of a dominant logical empiricism – for it certainly did – but how to understand Sellars's relationship to logical empiricism and whether such a relationship exhausts the philosophically relevant facts concerning Sellars's historical placement and early philosophical orientation.

On the other hand, there are a number of facts that ring false concerning this approach. In his earliest papers, Sellars barely mentions Carnap and never explicitly addresses any specific argument to be found in Carnap's philosophy. Although one *could* construct a reading of Sellars's earliest publications that turn on the assumption that Carnap is the focal point of Sellars's philosophy, such an assumption seems out of place. Sellars's most sustained engagement with Carnap's philosophy occurs in the 1950s (mainly in "Inference and Meaning" and "Empiricism and Abstract Entities") over issues that are barely, if at all, present in his earliest essays. In some instances Sellars quite clearly demarcates his views from those attributed to logical positivism. As one example, when discussing his notion of a "reciprocal fix", Sellars claims that such a philosophical commitment is what "distinguishes my position from positivism" (Sellars 1947b, p. 37).

Sellars's recollections of the 1930s and 1940s paint a radically different picture. Discussing his initial brush with Carnap's philosophy, Sellars recalled

> As for the *Logical Syntax of Language*, I reacted, as did many of my contemporaries, with the idea that while a rigorous account of syntax was clearly a *desideratum*, as far as its *philosophical* content was concerned, Carnap was putting the cart before the horse. Surely (or so it seemed to

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me) the syntax of language reflects the structure of the world. And since thought deals directly with the world, *that* is where the action is. Yet a seed was planted. It might have sprouted earlier if the impact of *Syntax* had not been blunted by Carnap's own move into his semantical phase, which seemed to support the above reaction. (Sellars 1975, Online).

Sellars's reflections on his initial impressions of Feigl also seem to support the same reaction; one of a philosopher in the American realist tradition attempting to come to grips with certain aspects of a then dominant logical positivism, though not a member of the movement himself. Sellars's claims that

> At that time, he was (or seemed to be) a paradigm logical positivist. Indeed, it is my understanding that he invented he term. Only later did it become clear that the basic motives of his thought were the exclusive right of sense experience and science to tell us how things are, an anti-Cartesian conviction of mind-body identity, and a deep sympathy with the naturalistic realism so convincingly expounded by his teacher Moritz Schlick before the latter was converted to positivism. (Sellars 1975, online).

Upon reflection

> the relevant fact is that Feigl and I shared a common purpose: to formulate a scientifically oriented, naturalistic realism which would "save the appearances." He was familiar with the general outline of my father's Critical Realism and Evolutionary Naturalism, and when an opening occurred in the University of Iowa Department where he had been teaching since 1931, he suggested that I be invited for an interview. We hit it off immediately, although the seriousness with which I took such ideas as causal necessity, synthetic *a priori* knowledge, intentionality, ethical intuitionism, the problem of universals, etc., etc., must have jarred his empiricist sensibilities. Even when I made it clear that my aim was to map these structures into a naturalistic, even a materialistic, metaphysics, he felt, as many have, that I was going around Robin Hood's barn. (Sellars 1975, online).

One could write this off as wishful thinking from a period significantly removed from the 1940s. As was noted above, Sellars seems to offer conflicting claims concerning his early philosophical orientation, especially concerning his relationship to logical positivism. The overall point is that it is not at all clear whether Sellars was a realist in a

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classic sense of the term (i.e., as being committed to the American versions of new or critical realism), a slightly rebellious positivist (as being committed, in some sense, to Carnap's older, *Aufbau* constructivist project), or a reformer of logical positivism who attempted to offer internal corrections of the movement.

The focus of this chapter is to construct a historically richer understanding of Sellars's early relationship to logical positivism than has hitherto existed. I want to do so by running contrary to Carus's claim that one should understand Sellars's earliest works as largely in agreement with and internal to logical positivism, especially Carnap's version of logical positivism. Instead, I will argue that the narrative running through Sellars's pure pragmatics is framed by Gustav Bergmann and Everett Hall's critiques of Carnap's semantics and their own approaches to semantics and pragmatics. My hope is that a historically correct reading of Sellars's early work can avoid Carus's charge that Sellars radically misinterprets Carnap's philosophy and shed new light on why Sellars was engaged in constructing a pure pragmatics of language. One can avoid Carus's charge by pointing out that Sellars's early project is not driven by an attempt to interpret Carnap's work. Instead, one goal of Sellars's early publications is to correct missteps in the positivists' treatment of language and mind as framed by Bergmann's and Hall's critiques of Carnap. In doing so, my suggestion is that Sellars's "pure pragmatics" is very much a project launched within the conceptual framework of Bergmann's and Hall's approaches to ontology and language. This interpretive framework, in turn, allows for the open discussion of "traditional" philosophy in a way that is not available to one attempting Carnap interpretation and opens the door for Sellars's adoption of some tenets

of Critical Realism and, later, his commitment to a normative conception of language and thought.

This is not to say that Sellars's earliest works made no substantive contact with Carnap's philosophy. One omnipresent aspect of Sellars's later work is the necessity of understanding language as containing an irreducibly normative dimension. This commitment makes its appearance in Sellars's earliest paper in the form of "P-restriction" or "P-lawfulness" in an empirically meaningful language, but it's a relatively open question as to whether Sellars is initially committed to the kind of "strong" normativist project one finds in his later[18] philosophy. This point will be expanded on in later sections, but the idea I am suggesting is that *Sellars's initial presentation of the necessity of a normative dimension of language and rules, and the necessity of material rules of inference, are found in a significantly weaker form than one finds in his later essays*. Specifically, Sellars's initial presentation of the roles that normativity and material rules of inference play are largely consistent with a Carnapian take on semantics. It is only later, in the 1950s, that an explicitly strong understanding of the roles for these concepts to play takes effect.

To steer clear of confusion, it must be pointed out that when I refer to Sellars's "early papers" I am specifically referring to "Pure Pragmatics and Epistemology", "Epistemology and the New Way of Words", and "Realism and the New Way of Words" (hereafter PPE, ENWW, and RNWW). Although a number of works were written and

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[18] In this instance "later" simply means "after Sellars's formal attempts at characterizing language". Although Sellars's project of "pure pragmatics" and his other attempts at characterizing and analyzing language are separated by only a couple of years, there is, I believe, a subtle shift in approach that is, more or less, readily apparent.

published almost immediately after these papers, they move in radically different direction (both in content and form).

II. Carnap's Semantics

Though Carnap did not fully[19] shift into the semantic phase of his philosophy until 1942, the notion of philosophy as an exclusively formal practice began with the publication of *The Logical Syntax of Language*. By marking philosophy as an essentially formal practice Carnap was not contradicting his earlier commitment to a scientific philosophy as described in chapter one. Carnap's early formal conception of philosophy consisted in a dual commitment against a metalinguistic conception of meaning and an emphasis on the analysis of linguistic structure *qua* calculi as opposed to "evolved" natural languages. As found in the *Syntax*:

> Philosophical questions, however, according to the view of philosophers, are supposed to examine such objects as are also investigated by the special sciences from quite a different standpoint, namely, from the purely philosophical one. *As opposed to this, we shall here maintain that all these remaining philosophical questions are logical questions*….The supposed peculiarly philosophical point of view from which the objects of science are to be investigated proves to be illusory, just as, previously, the supposed peculiarly philosophical realm of objects proper to metaphysics disappeared under analysis. Apart from the questions of the individual sciences, only the questions of the logical analysis[20] of science, of its sentences, terms, concepts, theories, etc…are left as genuine scientific questions. *We shall call this complex of questions the logic of science*. (Carnap 1937, p. 279).

On a formalist conception of philosophy the problems and analysis properly offered by philosophers concern only logical questions pertaining to the special sciences which entails that "all philosophical problems which have any meaning belong to syntax"

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[19] The beginning of Carnap's shift into his semantic phase can be found in Carnap 1939.

[20] It's clear that by "logical analysis" Carnap means to cleave the philosophical apart from the empirical. Just below the above passage Carnap asserts that the theory of science, as opposed to the logic of science, would be the broader (though non-philosophical) domain that dealt with empirical, historical, and psychological investigations of scientific activity.

(Carnap 1937, p.280). Such an assertion confines philosophical problems to the formal

mode of language as opposed to the material mode (i.e. the mode that deals with extra-

linguistic objects).

Introduction to Semantics represents a broadening, to some degree, of Carnap's

definition of philosophy but it does not represent a significant change. In the 1940s

Carnap is still committed to an understanding of philosophical problems as properly

located only in the formal mode of speech. As Carus has noted, despite Carnap's

acquiescence to Alfrid Tarksi's semantic conception of truth and rules of designation, his

commitment to a formal understanding of philosophy "survived unscathed for the rest of

Carnap's career" (Carus 2007, p.36). Although Carnap's move into formal semantics

allows for meaning (designation) and truth to play a more robust meta-linguistic role than

one finds in the *Syntax*, Carnap remained committed to the idea that the proper role for

philosophical analysis was offering formal elaborations of scientific notions.

The "broadening" of Carnap's notion of philosophy occurs in his reevaluation of

the claim that all meaningful philosophical problems belong to syntax. Carnap

reformulates this claim by suggesting that

> Thus, *the whole thesis is changed to the following: the task of philosophy
> is semiotical analysis*; the problems of philosophy concern – not the
> ultimate nature of being but – the semiotical structure of the language of
> science, including the theoretical part of everyday language. We may
> distinguish between those problems which deal with the activities of
> gaining and communicating knowledge and the problems of logical
> analysis. Those of the first kind belong to pragmatics, those of the second
> kind to semantics or syntax – to semantics if designata ('meaning') are
> taken into consideration; to syntax, if the analysis is purely formal.
> (Carnap 1942, p. 250).

As suggested above, such a reevaluation does not represent a significant change in

Carnap's view of philosophy but simply encompasses his endorsement of Tarski's

arguments in favor of a semantic conception of truth and a "philosophically respectable" conception of meaning. Carnap remains committed to the idea that "philosophy is the logic of science" and that, insofar as philosophy deals with scientific notions, its proper role is offering a formal analysis of said notions.

In his approach to semiotic, Carnap draws a clear distinction between the syntactic, semantic, and pragmatic dimensions of language;

> If in an investigation explicit reference is made to the speaker, or, to put it in more general terms, to the user of a language, then we assign it to the field of pragmatics. If we abstract from the user of the language and analyze only the expressions and their designata, we are in the field of semantics. And if, finally, we abstract from the designata also and analyze only the relations between the expressions, we are in (logical) syntax. (Carnap 1942, p.9).

One can correspondingly draw a distinction between descriptive and pure notions of syntax and semantics. Descriptive syntax or semantics is the study of the structure of meaning "of some particular historically given language, e.g. French, or of all historically given languages in general" (Carnap 1942, p.11). Descriptive semantics, and syntax, "describe facts" and such studies fall under the domain of the empirical sciences (e.g. psychological, sociological, or ethnological studies of language) (Carnap 1942, p.11). On the other hand, pure syntax and semantics are the construction and analysis of calculi or semantical systems that consist of linguistic rules and their consequences. In discussing pure semantics, Carnap reinforces this description of pure studies of language;

> The construction and analysis of semantical systems is called pure semantics. The rules of a semantical system S constitute, as we shall see, nothing else than a definition of certain semantical concepts with respect to S, e.g. 'designation in S' or 'true in S'. Pure semantics consists of definitions of this kind and their consequences; therefore, in contradistinction to descriptive semantics, it is entirely analytic and without factual content. (Carnap 1942, p.12).

At this stage[21] in his philosophical development, Carnap was committed to a wholly descriptive notion of pragmatics. Despite his previous treatment of syntax and semantics, Carnap offers no corresponding distinction between descriptive and pure pragmatics. Pragmatics is conceived of as an empirical investigation that discovers facts about how language is actually used. In this sense, pragmatics is exhibited in psychological and sociological studies of language use and behavior. Although descriptive syntax and semantics necessarily presuppose pragmatics – "pragmatics is the basis for all linguistics" (Carnap 1942, p.13) – descriptive syntax and semantics need not necessarily take into account pragmatic aspects of language (i.e., descriptive semantics can limit its analysis to expressions and their designata without referencing language users). The situation is completely different with pure syntax and semantics; *these fields are independent of pragmatics*. Here we lay down definitions for certain concepts, usually in the form of rules, and study the analytic consequences of these definitions" (Carnap 1942, p.13).

Despite earlier attempts[22] to give a formal treatment of epistemological concepts, such as verification and confirmation, Carnap relegates such concepts to empirical investigation and as properly the subject matter of pragmatics. Even though one could give an analysis of the formal, syntactical dimension of language that contains such terms, their role in a language is characterized by empirical investigation. Specifically, as Carnap notes, "definitions for them would have to be given within psychology, and more precisely, within the behavioristic theory of language" (Carnap 1936, p.454). Carnap's

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[21] As is discussed in chapter three, Carnap eventually suggests that there is a need for a pure or formal notion of pragmatics. Such admission, as far as I have found, only occurs significantly after *Introduction to Semantics*.
[22] One finds syntactical characterizations of both of these notions in Carnap 1936.

semantic phases sees this widening a bit to include semiotic studies of terms such as "confirmable" that could fall within the domain of pragmatics, syntax, or semantics (though the implication in *Introduction to Semantics* seems to be that such studies would necessarily fall under the descriptive domain of such studies). Thus, one finds Carnap in 1942 still suggesting that concepts such as "confirmed" or "confirmed-to-n-degree" are "pragmatical" concepts (Carnap 1942, p.244-5) and that "concepts like 'believed', 'verified', 'highly confirmed;, etc…belong to pragmatics and require a reference to a person" (Carnap 1942, p.28).

Concerning the notion of "language" under study, Carnap's distinction between "language as calculi" as opposed to evolved, naturally occurring languages is rigid. By "language as calculi" Carnap understands a language as

> a system of conventions or rules…these rules are concerned with elements – the so-called symbols – about the nature and relations of which nothing more is assumed than that they are distributed in various classes…When we maintain that logical syntax treats language as a calculus, we do not mean by that statement that language is nothing more than a calculus. We only mean that syntax is concerned with that part of language which has the attributes of a calculus – that is, it is limited to the formal aspect of language. In addition, any particular language has, apart from that aspect, others which may be investigated by other methods. (Carnap 1937, p.4-5).

On the side of pure semantics, the notion of language under consideration is the interpretation of such calculi; semantical systems. A semantical system is "a system of rules, formulated in a metalanguage and referring to an object language, of such a kind that the rules determine a truth-condition for every sentence of the object language, i.e. a sufficient and necessary condition for its truth" (Carnap 1942, p.22).

One point that is crucial here, and will be one significant difference between Carnap and Sellars, is that Carnap's conception of calculi and semantical systems has

little to do with naturally occurring languages. This is not to say that calculi have *nothing* to do with natural languages. As Carnap notes, "in choosing the rules we are entirely free. Sometimes we may be guided in our choice by a given language, that is, by pragmatical facts. *But this concerns only the motivation of our choice and has no bearing upon the correctness of the results of our analysis of the rules*" (Carnap 1942, p.13). How concepts such as meaning or assertion hook up with action and perception (that is, the pragmatic dimension of language) is not a consideration for the kinds of languages Carnap is concerned with; "in that connection they are the objects of psychological study" (Carnap 1937, p.5).

A calculus consists of formation, transformation, and designation rules. For Carnap, rules are simply shorthand for definitions set down in schematic form. The connection between the two notions are so close that throughout *Introduction to Semantics* Carnap uses the terms "definition" and "rule" interchangeably. A typical passage from Carnap exhibits this tendency: "The rules of a semantical system S constitutes, as we shall see, nothing else than a definition of a certain semantical concept…Pure semantics consists of definitions of this kind and their consequences" (Carnap 1942, p.12). In the context of Sellars's early discussion of linguistic rules, the important point is that the notion of a rule endorsed by Carnap and most logical positivists[23] contains no normative dimension (i.e. no prescriptive components).

Rules of designation are understood as rules that determine the meaning of singular terms, predicates, and sentences of a given language. Rules of designation do not

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[23] It is a vexed issue as to whether all positivists necessarily agreed with this assessment. In Schlick 1936, Schlick seems to describe rules as containing a prescriptive component (claiming that linguistic rules are "prescriptions stipulated by acts of definition"). Nonetheless, I have found very little discussion on this point; it seems unlikely that Sellars was concerned with Schlick's brief discussion of linguistic rules.

make factual assertions about the relationship between expressions and the world. Instead, "the rules merely lay down conventions in the form of a definition of 'designation in S'; this is done by an enumeration of the cases in which the relation of designation is to hold" (Carnap 1942, p.25). At this point, Carnap is relatively quiet concerning whether the designata of expressions are other expressions (or classes of expressions) or objects in the world. Nonetheless, on first glance it seems clear that Carnap means for rules of designation to construct a relationship between expressions in the object language and extra-linguistic objects. Although there is no explicit statement of this thesis, Carnap consistently uses the terms "object" and "property" to discuss the designata of the expressions in question. Thus, when initially setting down rules of designation and truth, Carnap's example is "A sentence pr_i (in_j) is true if and only if the designatum of in_j has the designatum of pr_i (i.e. the *object* designated by in_j has the *property* designated by pr_i)" (Carnap 1942, p.24). Even prior to this Carnap stipulates that "individual signs designate the individuals of the realm in question (objects); they belong to the zero level" (Carnap 1942, p.16-7). A more extensive analysis of Carnap's view on this matter will be given in section III.

Carnap's rules of truth are based on his interpretation of Tarski's disquotational theory of truth. That is, Carnap's "semantical concept of truth" is simply the claim that "true" is used "in such a sense that *to assert that a sentence is true means the same as to assert the sentence itself*; e.g. the two statements 'The sentence 'The moon is round' is true' and 'The moon is round' are merely two different formulations of the same assertion" (Carnap 1942, p. 26). Carnap is not offering a definition of truth but, instead, constructing a standard or norm for the *adequacy* (i.e. "a standard by which we judge

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whether a definition for truth is adequate, i.e. in accordance with our intention") of a definition of truth (Carnap 1942, p.28). As far as an explicit definition of truth, Carnap relies on "the old classic conception that dates back to Aristotle" of correspondence that conforms with "the ordinary use of the word 'true'" (Carnap 1942, p.29).

What one finds in Carnap is a conception of formal semantics that by definition makes no reference to the language user. Studies in pure syntax and semantics necessarily abstract away from any empirical considerations as well as the relation of expressions to extra-linguistic objects; Carnap is quite clear that even rules of designation make no factual assertions. The linguistic rules discussed by Carnap are intended to contain no normative dimension and fail to reach any significance beyond definitions in the form or schema of rules. One can infer from Carnap's statements that, as of 1942, pragmatics, understood as a wholly descriptive dimension of language, was simply an unnecessary or (perhaps) impossible dimension for a *pure* understanding of language. Any scientific notion, such as "confirmed to n-degree", that may arise in a language necessarily fall within the domain of descriptive pragmatics.

III. The Reception History of Carnap's Semantics

The reception history of Carnap's semantics is complicated by the multitude of philosophers that took issue with various parts of Carnap's project. Since Carnap never finished the planned series of volumes on semantics, it has been suggested that Carnap's overall semantic turn remained incomplete. In regards to how Sellars initially encountered Carnap's philosophy in his earliest publications, one need not consider every critique of Carnap's move into semantics. The most salient responses to Carnap's

semantics, or so I will argue, can be found in Everett Hall's and Gustav Bergmann's critiques of Carnap.

The work of both philosophers is integral to understanding Sellars's early essays, because Hall's and Bergmann's critiques of Carnap, their disagreement over how to understand pragmatics, and the proper characterization of epistemological predicates frames Sellars's early publications. Bergmann and Hall also represent what seem to be Sellars's dual commitments to positivism and realism. Sellars himself construed his time at Iowa as a significant period in his own philosophical development. On his move to the philosophy department at the University of Iowa, Sellars recalled:

> Once again, then, I had an exhilarating new beginning. There was so much to do, and so much sense of achievement in doing it, that the tasks of finishing my dissertation, which I scarcely touched, and of publishing, moved into the background. Herbert Feigl moved to Minnesota in 1941, and Gustav Bergmann, who had come to the University as a Research Associate with Kurt Lewin, joined the Department to teach advanced logic and philosophy of science. During his first semester he gave an excellent seminar in logical theory, based on Carnap's *Logical Syntax of Language*. It was attended by the entire Department, which, by now, included Everett Hall, who had joined us as chairman on the retirement of Herbert Martin. Bergmann became a close collaborator with Kenneth Spence, and I began to take behaviorism seriously…Bergmann at this time took a fairly orthodox positivistic position with strong overtones of Carnap and Schlick. He and I argued the whole range of "pseudo-problems." The occasion of most of these discussions was an informal seminar in current philosophical literature which met at Hall's house every week and which everybody religiously attended. The Department was still minute and highly involuted. Ideas of amazing diversity were defended and attacked with passion and intensity. It was not easy to find common ground, yet "for the sake of discussion" we stretched our imaginations. It was, I believe, a unique episode — certainly as far as my own experience is concerned. (Sellars 1975, online).

On top of this, the initial development of Sellars's early publications occurred while he was in Iowa and he continued to correspond with Hall, Bergmann, and other philosophers in and from the Iowa department throughout his career. Moreover, Sellars's

correspondence with Bergmann, Thomas Storer, and others at Iowa and the University of Minnesota indicate that Sellars himself placed his early publications in the context of Bergmann's and Hall's works from the 1940s (as discussed in section V below).

Hall's reading of Carnap construes rules of designation as metalinguistic statements between an expression and the name of an expression. Such a conception of designation lands one in the linguacentric predicament; one "cannot say anything about the relation between language and matter of fact" because all of Carnap's linguistic rules are metalinguistic statements that say nothing about extra-linguistic objects (Hall 1944, p.25). Hall entertains the notion that because of his emphasis on the *formal* character of pure semantics, Carnap's conception of designation could be read as attempting to "reflect a common relation (designation) to be found obtaining between (1) names of expressions and those expressions and (2) expressions and extra-linguistic designata" (Hall 1944, p. 26). Hall denies this, using multiple examples from *Introduction to Semantics* to show how Carnap conceives of his rules as statements in the metalanguage and, thus, not as statements asserting relations between extra-linguistic objects and expressions.

On Hall's reading of Carnap, designation statements like "'Fawn' designates Fawn" is stipulating a relationship between the name of an expression in the metalanguage on the left-hand side of the designates statement and an expression in the object language on the right side. That is, for a given language S "designation is wholly a relation within S, therefore within a language (semantical) system, therefore not a relation of language or linguistic elements to extra-linguistic fact" (Hall 1944, p.27).

If Carnap is saying something about the relationship between extra-linguistic

objects and expressions, the upshot of Hall's critique is that Carnap would be running

roughshod over his distinction between pure and descriptive semantics:

> Pure semantics is concerned with arbitrarily constructed systems. By this
> Carnap seems to mean that for pure semantics 'designates in S' (or 'true in
> S') is wholly a function of rules in S (pp. 12, 24 ff.), and furthermore, that
> rules in S are arbitrary in the sense that they presuppose nothing about
> matter of fact. It therefore seems clear that what any expression in S
> designates or whether any sentence in S is true can be fully decided (in
> pure semantics) simply by a knowledge of S, i.e., without any knowledge
> of matter of fact. Therefore (I conclude), pure semantics cannot say
> anything about the designation or truth of any expression at zero-level, for
> to do so would involve an assertion (however general and abstract) about
> matter of fact as well as about language, and would therefore be
> descriptive. (Hall 1944, p.26-7).

Even in its most general or abstract form, rules of designation and truth that stipulate a

relationship between expressions and extra-linguistic objects would be instituting a

factual relationship between language and world. If this is the case, it would seem that

the rules of designation discussed by Carnap in 1942 would, strictly speaking, belong

under the domain of descriptive, as opposed to formal, semantics.

Hall's solution to the problem of a language formally considered being cut off

from the world is the addition of a class of symbols called empirical ties[24] that represent a

relation between language and world. As Hall described them:

> Roughly, empirical ties are the denotatives: demonstrative pronouns ('this
> ', 'that'), relative adverbs ('here', 'now'), also symbols often not called
> linguistic, such as gestures (pointing), etc. Such an empirical tie as 'this'
> serves to attach language to matter of fact. "What do you mean by 'igloo' ?
> " ' Igloo' designates house." " But what do you mean by 'house' ? " " This."
> This attaching of language to empirical fact, this empirical orientation of
> language, is possible because of a peculiarity of empirical ties. An

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[24] As will be discussed in the next section, Sellars's notion of an "experiential tie" would seem to be the
exact same notion as Hall's "empirical tie" though constructed as a relation within a formal calculus. It
would seem that Sellars borrows the dual role of symbols function as "matters of fact as well as of
language" from Hall's notion of an empirical tie.

empirical tie is both a symbol and (a part of, or in the context of) the designatum of that symbol. Like all symbols, empirical ties are matters of fact as well as of language, but they are peculiar in that their linguistic (symbolic) function is through or by means of their factual occurrence. (Hall 1944, p.36).

Empirical ties occur in historically given languages (as, thus, one would assume in descriptive semantics) and relate expressions to matters of fact. While seemingly avoiding psychologism, such a conception of empirical ties would seem to commit Hall to some form of factualism (i.e., that some *fact, usually a psychological fact about a language user or a language user's mental states,* determines meaning and not a formal analysis of language).

Hall is quite aware of this, but borrows Carnap's distinction between sign-designs and sign-events in order to avoid what he considers the mistake of attributing designata to sign-designs. Specifically, it's Hall's contention that "we must recognize the function of signs as events (in designation of fact by zero-level sign-designs) if we are ever to escape the lingua-centric predicament" (Hall 1944, p. 36). So, the idea here is that only sign-events have differing designata (which is contextually dependent, e.g. the designata of "this book") as sign-designs do not, strictly speaking, designate as they do not occur in natural languages *per se* – only instances (sign-event) of such designs occur in natural languages (Hall 1944, p.36). Of course, we can always represent sign-designs in language with technical devices (e.g. we can, and do, stipulate that "'Igloo' designates Igloo" contains the sign-design Igloo within single quotations).

Ultimately, Hall construes the relation of designation as one between sign-events as some[25] relation between sign-events that do confront matters of fact and those that do not. Specifically,

> If the empirical tie occurs alone (unaccompanied by other signs) then what it designates within its total immediate existential context is wholly ambiguous. But conjoined with other signs, it functions to limit their designata to the case or cases present in its immediate existential context. 'This' or 'look' is wholly ambiguous within the region of its immediate existential context. But 'this book' or 'the colour of this book' is not. "This book" means "The book in the immediate existential context of a particular utterance I (later and by printed marks) name 'this' ". This, however, supposes that other signs (sign-designs) have meaning (are empirically attached) independently of a given occurrence (sign-event) in conjunction with an empirical tie. In order that this be so, however, those other signs (sign-designs) must have been attached (or must now be attachable) to matter of fact through differential existential conjunction (sign-events) with empirical ties. (Hall 1944, p.37).

On Hall's account the correct understanding of rules of designation turn on the occurrence of empirical ties in naturally occurring, as opposed to formally constructed, languages.

Bergmann's reading of Carnap is essentially the same as Hall's.. Two significant differences turn on what the proper construal of the term "formal" is and whether one thinks of Hall's linguacentric predicament as necessarily a bad thing. Bergmann defines "formal" as when "a metalanguage refers only to an object language and not to its reference" (Bergmann 1944, p.241). The upshot of this is that the relationship between an object language and its extra-linguistic referents "cannot occur in a formal metalanguage" (Bergmann 1944, p.241). As Bergmann observes, "extra-linguistic" does not necessarily mean "extra-symbolic"; although one can offer an interpretation of an object language

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[25] I say "some" in this case because the wheels seem to start falling off of Hall's account here. In the block quote after this sentence he seems to initially confuse sign-designs and events (as he seems to be attributing designata to sign-designs, something he explicitly says is a mistake).

that analyzes the relationship between expressions in an object language and their extra-linguistic referents, such an analysis would not be extra-symbolic (Bergmann 1944, p.240). Since pure semantics deals with *formal* relations within and between languages, such an analysis would be stipulating rules of designation between expressions in the metalanguage and the *names* of extra-linguistic referents in the object language.

On Bergmann's reading of Carnap, rules of designation do not stipulate factual relationships between the object language and the world; any formal treatment of a language necessarily stays within the "universe of symbols" (Bergmann 1944, p.240). Any metalanguage that refers to an object language and its extra-linguistic referents ("extra-linguistic" meaning "objects in the world" in this case) would, by definition, be a non-formal account of said relationship. As Bergmann remarks; "I know of no better way to epitomize this situation than to insist *that pure semantics does not deal with the extra-linguistic referents, the designata, in the extrasymbolic sense in which one usually understands these two terms*" (Bergmann 1944, p.248).

Keeping this first point in mind, Bergmann offers a reading of Carnap's semantics that places rules of designation within Hall's linguacentric predicament. The difference that emerges between Hall and Bergmann is that the latter does not consider such a strongly formalist reading of Carnap as a bad thing. Instead, Bergmann argues that the point of pure semantics "is to determine how much about the interpretational relationship can be learned from a *formal* study of and within the hierarchy" of language, metalanguage, and meta-metalanguage (Bergmann 1944, p.245).

Carnap responded to both philosophers in one article as he suggests that Hall and Bergmann are essentially making the same mistake. Carnap claims that he has always

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been committed to the view that pure semantics deals with the extra-linguistic objects of

expressions in the object language;

> I have especially emphasized its distinction from syntax (Morris'
> "syntactics"); while the latter discipline deals only with relations among
> expressions in a language and thus with an entirely intra-linguistic subject-
> matter, *it belongs to the essential characteristics of semantics that it refers
> not only to language but also to extra-linguistic matter*. The book does not
> only state and emphasize this characteristic in abstract terms, but also
> illustrates it by a great number of examples of semantical rules. (Carnap
> 1945, p.148).

Carnap chalks up Hall's confusion between expressions and names of expressions as a

confusion of use and mention. Specifically, Carnap argues that

> The one fundamental mistake made by Hall is just the neglect of this
> distinction between using an expression and referring to it. Consider the
> semantical rule: " 'a ' designates Chicago ". The rule says that the relation
> of designation holds between two entities, which are specified in the rule.
> What are these two entities? We find in the rule at the first argument-place
> (i.e., at the place preceding the relation-word 'designates ') the name of a
> sign. (In the above formulation this name is built with the help of
> quotation marks; in my book, I use instead certain German letters which
> are introduced as technical names of signs; this difference is inessential, as
> mentioned before). Therefore, what is referred to in the first place is the
> sign whose name occurs, viz., the first letter of the Roman lower case
> alphabet. At the second argument place (i.e., following the word '
> designates ') we find the word ' Chicago '. Therefore, what is referred to in
> the second place-and this is the decisive point-is the entity whose name
> occurs at the second place; this entity is not the word 'Chicago ' but the
> physical thing Chicago. (Carnap 1945, p.152).

Carnap tries to sidestep most of the issues raised by Bergmann and Hall by

explicitly denying that pure semantics is a formal study of language. Specifically, Carnap

remarks that "I should prefer to say that pure semantics is the study of object languages,

not of metalanguages, and further, that it is not a formal study but a study of

interpretation" (Carnap 1945, p.154). Although Carnap's assertion here is

straightforward, it does not clarify. If pure semantics is the study of object languages,

then it would seem as if Carnap is placing rules of designation in the object language. This clearly won't do; rules of designation "speak about" relations in the object language and are, thus, ostensibly one level higher. If this is the case, on Bergmann and Hall's reading of Carnap, rules of designation cannot be a relationship between language and world if it is formulated in the metalanguage.

Tthis response does not seem to get at the deeper worry present in Hall and Bergmann's critiques. This concern does not arise just as a misreading of Carnap but as a general problem with even his clarified approach to semantics. When attempting to address this specific issue, there are two relevant claims in *Introduction to Semantics* that should give pause. First, Carnap claims that "the rules of designation do not make factual assertions as to what are the designata of certain signs. *There are no factual assertions in pure semantics*" (Carnap 1942, p.25). Second, in a later discussion of rules of designation Carnap seems to reinforce the idea that such rules do not make reference to extra-linguistic objects:

> With respect to some of the types to which the relation of designation is here applied, the puzzling question is sometimes raised, what exactly is the kind of designata of the expressions of the one type or the other? Thus it is e.g. discussed whether the designatum of a thing-name (e.g. 'Chicago') is a corresponding thing or its unit-class (e.g. whether it is Chicago or {Chicago}). Further, the question is discussed whether the designatum of a predicate of first degree is a property or a class. In both cases it is said as an argument in favor of the second answer that a designatum should always be a class. (Carnap 1942, p.52-3).

After defining adequacy as a standard for possible rules of designation that keeps the creation of new rules in "agreement with our intention for the use of the term" Carnap settles on a position in favor of objects over unit-classes (Carnap 1942, p.53). At this point Carnap asserts that "On the basis of an *adequate* relation of designation, the

question of the designatum of an object name is to be answered in favor of the object as against its unit class" (Carnap 1942, p.54). So, as Carnap argued in his response to Bergmann and Hall, his conception of pure semantics does stipulate a relationship between expressions or names and extra-linguistic objects. If pure semantics makes no factual assertions, how is it that rules of designation stipulate relationships between names and the world? Would not such a relationship be a fact concerning how expressions are related to the world? As I will argue in the next section, this is one of the tensions that motivates Sellars's construction of a pure pragmatics.

Despite their general convergence on the same reading of Carnap's semantics, Hall and Bergmann held significantly differing conceptions of pragmatics and epistemological predicates. Hall's conception of pragmatics roughly coincides with what one finds in Carnap's conception of pragmatics. Hall never offers an explicit definition of pragmatics but claims that "a sentence's pragmatic properties are those which it exhibits by virtue of its occurrence – as stated, believed, entertained by someone" (Hall 1947, p.124). Hall thinks that the proper domain for understanding epistemological predicates is only partly located in pragmatics. One should relegate concepts like "confirmed" or "verified by" to semantics because "to define degree of verification as degree of psychological conviction on the part of the reasonable man would be circular, since 'reasonable' is itself (at least tacitly) defined in terms of appropriate recognition of relevant degrees of verification" (Hall 1947, p.124). For Hall, the fundamental point is that "verification has to do with the truth of the sentence, with a sentence in its meaning or reference, and not with its occurrence" (Hall 1947, p.124).

Bergmann's exact conception of pragmatics is somewhat obscure[26] but it is clear

that his notion of a pragmatical metalanguage, and pragmatics in general, differed from

Carnap's or Hall's understanding of the terms. On Bergmann's initial account

> Pragmatics is defined as the study of the relationships between a language,
> its interpretation and its users. A pragmatical metalanguage P contains
> therefore the names of the expressions of the object language, the names
> of their referents and the semantical predicates; briefly, it contains
> semantics in the same indirect sense in which semantics can be said to
> contain syntax. But P contains moreover another set of predicates which
> correspond to the verbs for the activities of the Self, such as 'knowing',
> 'believing', 'doubting', 'verifying', and so forth. Naturally, P is a very rich
> descriptive language. What would be the result of its formalization? Any
> such formalization, it seems safe to predict, will be an axiomatization in
> the proper sense of the term, that is, the verbs of consciousness would turn
> out to be descriptive predicates and many significant statements in which
> they occur, as for instance, 'A believes that p after he has verified p' would
> turn out to be synthetic. I want to make it as clear as possible that I do not
> intend here to make any specific suggestions as to the structure of a
> correct and complete axiomatization, a geometry of consciousness. It will
> be noticed, for instance, that the cognitive verbs have not been described
> as primitive or undefined predicates, since the actual achievement of a
> behaviouristic [sic] psychology of the higher processes would probably
> amount to their definition in terms of descriptive relationships which lie
> "below" the level of consciousness. *Generally it can be said that a
> materially significant formalization of pragmatics is the axiomatics of an
> objective or, as one also says, behaviouristic psychology of the higher
> processes. Thus the matter is in the hands of the scientists and no
> immediate results, such as in the case of semantics, are to be expected.*
> (Bergmann 1944, p.255-6).

On Bergmann's initial account of pragmatics, one could construct a formal analysis of

pragmatics as opposed to Carnap's or Hall's wholly descriptive understanding of

pragmatics. This notion of pragmatics is not an inherently *philosophical* notion;

Bergmann stresses that such an axiomatization of the verbs of consciousness would be

the job of the scientists and *not* the philosopher.

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[26] "Obscure" in this case might possibly mean "legendary". In difference instances Virgil Hinshaw, Sellars, Hall, and Storer remark about the perplexing character of Bergmann's pragmatics.

Despite the admission of a formal dimension of pragmatics, Bergmann's position would seem to indicate that pragmatics is still a scientific field. Bergmann's position on pragmatics was received as such; Hall commented that Bergmann (and other's) notion of pragmatics was limited to understanding epistemological predicates such as "verified by" to "their occurrence-aspect" (Hall 1947, p.125); that is, Bergmann's conception of pragmatics was limited to empirical accounts of language in use. Bergmann balks at the idea that his "notion of pragmatics is essentially that of Morris and Carnap" (Bergmann 1947a, p.272). Bergmann responds by drawing a distinction between psychological and philosophical pragmatics. While "psychological pragmatics is a branch of natural science", philosophical pragmatics concerns the reconstruction of the metalanguage, L, scientists use to formulate the results of their psychological investigations into the object language, O, of their subjects (Bergmann 1947a, p.272-3). When discussing languages, Bergmann claims that "philosophical pragmatics is the study of their structure and of their role in the total pattern I have proposed as a reconstruction of L" (Bergmann 1947a, p.273). Despite initial appearances Bergmann does conceive of a role for a philosophical account of pragmatics that is not, strictly speaking, empirical[27].

Virgil Hinshaw (a graduate student at Iowa during Sellars's time there and a reviewer of his early publications) also held a notion of pragmatics that is both heavily indebted to psychological studies of language yet open to formalization. Hinshaw's initial account of pragmatics opens the door for a formal treatment of pragmatical

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[27] Bergmann isn't exactly clear about how this necessary diverges from psychological pragmatics. The main idea seems to be that philosophical pragmatics would be a formal reconstruction of the scientist's own language as opposed to the language the scientist investigates. So, by "formal" in this case Bergmann seems to mean "metalinguistic". As is discussed in section IV, Sellars occasionally holds the same understanding of "formal".

notions. Aside from the fact that a pragmatical metalanguage would contain concepts of

truth and designation which correspond to those developed in pure semantics

> in addition, pragmatics contains another set of predicates which
> correspond to the verbs for the activities of the self, such as 'knowing,'
> 'believing,' 'verifying,' and the like. The formalization of pragmatics
> would, then, consist in an axiomatization centered around these
> descriptive predicates of consciousness. Accordingly, an essential part of
> the analysis of the traditional mind-body problem would find its locus in
> the development of a philosophical pragmatics thus conceived. *It might
> also be noticed that the execution of this schema is to a considerable
> extent the program of behavioristic or objective psychology yet to be
> undertaken*. (Hinshaw 1944, p.87).

Hinshaw's conception of pragmatics is not wholly formal. As was also exhibited in

Bergmann's early account of pragmatics, Hinshaw is committed to the idea that the

formalization of pragmatics is a project properly carried out by the psychologist as

opposed to the philosopher.

Though sharing little common ground, most philosophers at Iowa had little

problem with "giving up" certain concepts or notions to sociological and psychological

studies. Aside from Carnap's above described position concerning confirmation,

Hinshaw's conception of pragmatics was heavily indebted to sociological and

psychological approaches to language. In demarcating various aspects of epistemology,

when considering pragmatics Hinshaw argues that

> the pragmatical part of a language, which treats of the *empirical*
> application of the pure logic and semantics. Pragmatics includes within its
> scope all language of the sciences. Here we must distinguish the terms
> 'verified' and 'verifiable', which, with their correlatives, 'falsified' and
> 'falsifiable', can be applied only within a pragmatical system, that is,
> within a symbolism that includes, in addition to a logic and semantics,
> such verbs as 'know,' 'believe,' 'verify,' and the like...Of the three branches
> of language analysis, *it is pragmatics, I suggest, which comprises the
> legitimate scope of the sociology of knowledge*. (Hinshaw 1948a, p.6-7).

Hinshaw's conception of pragmatics broadly lines up with Carnap's treatment of "verifiable" or "confirmable" though, as discussed above, Hinshaw (like Bergmann) does leave the door open for a formal conception of pragmatics though such formalization is construed as the job of psychologists as opposed to philosophers. Moreover, it is indicative of the *general* attitude of the time at Iowa that pragmatics was a strongly empirical approach to analyzing philosophical and scientific notions.

Aside from Carnap's, Hall's, and Bergmann's approaches to pragmatics and the relationship between philosophical and psychological accounts of epistemological predicates, the general academic climate between philosophy and psychology at Iowa should be kept in mind. Although, as far as my research has determined, Sellars is mute on this topic, there was discussion at the University of Iowa about the relationship between the two departments. Hall reported that both departments cross-listed a number of graduate and undergraduate classes as required for both philosophy and psychology majors, co-chaired dissertations, and co-authored papers (Hall 1944, p. 80). This "redintegration of psychology and philosophy at Iowa" was seen as a mutual recognition of the value of each discipline to the other. Hall is quick to point out that there is a distinct role for philosophers to play when it comes to the mind but that it could not be constructing theories without reference to the experimental findings of psychology (Hall 1944, p. 81).

Sellars's period at the University of Minnesota is more of the same story. In fact, a number of volumes published by Feigl and Sellars either took up psychological issues from within a philosophical framework or published papers by psychologists themselves (most notably B. F. Skinner). Granted, all of this does not entail anything like a

collapsing of the disciplines; it was generally thought that philosophical analysis had a

distinct role to play. Nonetheless, there was clearly a general agreement that the

philosophical discipline would have less and less to contribute on the content side of

issues concerning the mind and world (Hall 1944, p. 81). Such a climate would seem, in

part, to help explain Sellars's concerns from an institutional standpoint.

The general problem that can be construed out of these debates centers on two

issues that are at the focal points of Sellars's early essays. First, if Hall and Bergmann

are right concerning Carnap's understanding of rules of designation, then it would seem

that he is either committed to a form of naïve realism[28] or his semantics is completely cut

off from the world. Second, Hall's and Bergmann's debate concerning the proper

understanding of verification in relation to pragmatics doubly represents a rejection of

Carnap's endorsement of descriptive pragmatics. The larger concern, as is found in

Sellars's early publications, turns on whether pure pragmatics can be construed as a

philosophical account of language.

As I will show in section IV, despite the fact that Sellars's approach to pragmatics

was influenced by Hall's and Bergmann's differing approaches to pragmatics, this need

not indicate agreement. As will be shown, Sellars is clearly interested in a developing a

conception of pure pragmatics that seems to differ significantly from Hall's conception

and, at least, differs in important ways from Bergmann's as well. Sellars's approach to

these, and other, problems represents a third way that should be understood as more than

simply another response to Carnap's shift into semantics. While Hall's and Bergmann's

critiques of Carnap represent an attempt to reformulate Carnap's approach to semantics,

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[28] Otto Neurath levels this charge against Carnap's semantics and it is implicit in Bergmann's treatment of
Carnap. This issue will be discussed more explicitly when dealing with Sellars's understanding of rules of
designation in the following section.

Sellars's attempt to save philosophy from psychologism and an implicit commitment to naïve realism will take the form of the construction of a pure treatment of pragmatics.

IV. The Project of Pure Pragmatics

Sellars's construction of a pure pragmatics of language should be framed as addressing the dual problems listed in section III. Sellars is interested in constructing a purely formal treatment (in a sense to be determined) of epistemological predicates, such as "designates" and "verified", that would avoid the kind of factualism and psychologism present at Iowa and beyond. The point of placing this project in the context of the work of Bergmann and Hall, as opposed to Carnap, is that one can show how Sellars's project attempted to both borrow from Bergman's and Hall's approaches to semantics and pragmatics while simultaneously overcoming their individual commitments to psychologism. The resolution of the lingua-centric predicament without a commitment to naïve realism or the minimizing of a role for philosophical analysis is the ultimate goal of pure pragmatics. Prior to offering Sellars's "final" solution to these problems, one must grasp the initial orientation and machinery of Sellars's project.

Sellars's initial publications begin by defining philosophy as "the formal theory of languages" (Sellars 1947a, p. 4) or "the pure theory of empirically meaningful languages" (Sellars 1947b, p. 28). Sellars takes this to be an emphatic statement that the "defining characteristic of *philosophical* concepts is that they are formal concepts relating to the formation and transformation rules of symbolic structures called languages" (Sellars 1947a, p.4). Sellars's initial definition of philosophy is fairly restrictive; it seems that the philosophical dimension of any concept would turn on the analysis of its structural properties alone. Thus, one finds Sellars claiming that

Philosophy is pure formalism; pure theory of language. The recommendation of formalisms for their utility is not philosophy. Hume's scepticism was a consequence of his mistake in supposing that the *philosophical* questions he asked *in the study* were sweeping questions of fact, and that therefore *outside the study* he took an unquestioning attitude towards factual propositions questioned in the study. The truth of the matter, and I speak in the tradition of Hume, is very opposite. There are no factual statements that become philosophical in the study (though there are non-factual statements which are philosophical outside the study); and *in philosophy*, scepticism is a self-contradictory position. (Sellars 1947a, p.25).

This definition of philosophy results from a concern to demarcate properly philosophical treatments of a given concept from scientific treatments. Sellars argues that analytic philosophy has generally been marked by a rejection of psychologism – that is "the mistake of identifying philosophical categories with those of psychology" – and that developments within modern logic have been, at least initially, rightly heralded as an adequate formal treatment of philosophical concepts (Sellars 1947a, p.4). Analytic philosophy's movement from the syntax phase of logical empiricism to a focus on formal semantics has failed, in Sellars's view, to properly demarcate philosophical concepts from psychological or empirical treatments of said concepts. As Sellars describes the problem:

Of what assistance to the analytic philosopher has been the development of this new branch of linguistic analysis? It would be encouraging if we could say that as a result of this new development, philosophers of the analytic school are agreed in attacking psychologistic and, in general, factualistic accounts of *the additional concepts which make their appearance in the rules of pure semantics. …* Unfortunately, not only have analytic philosophers not made proper use of the new tools available to them, not only have they not pressed on to new victories in the battle against psychologism and factualism; ground has actually been lost! ... As a result, factualism and psychologism are flourishing in analytic philosophy, and by no means on the fringes only. (Sellars 1947a, p.5).

On Sellars's reading of the contemporary philosophical scene, despite the fact that the syntax phase of logical positivism had offered a formalist way out of psychologistic and factualistic accounts of epistemological concepts, such a move had been abandoned by the late 1940s. Analytic philosophy in general[29] was largely guilty of giving into psychologism or factualism.

The philosophical treatments of the terms "'true', 'false', and 'designates'" are still infected with psychologism or factualism, because philosophers have failed to offer a *formal* treatment (in a sense left undefined) of "'verifiable', 'confirmable', 'verified', 'confirmed', and 'meaningful'" (Sellars 1947a, p. 5 – 6). When such terms are considered by philosophers, Sellars suggests they are generally assigned to pragmatics. This is to assign concepts to a pragmatics that is located within the realm of the empirical sciences and, thus, the analysis of terms such as "verifiable" is treated as an empirical, as opposed to philosophical, problems.

The constructive point of these early essays is ostensibly to "rescue" pragmatics from the psychologistic treatment it had thus far received. Sellars proposes to do so by showing that properly epistemological predicates, such as 'verifiable', can be shown to play "a purely formal role" in a calculus (Sellars 1947b, p. 29). That is, a properly philosophical treatment of such concepts will recognize that, much like syntactical and semantical concepts previously, concepts such as "'verifiable' belong in rules definitive of a type of object calculus"; these would be meta-linguistic rules that help define a new dimension of calculus structure that "alone entitles them to be called *languages* in an

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[29] It's particularly interesting that Sellars indicts analytic philosophy in general as opposed to singling out logical positivism. Amongst many other points I think this lends creditability to my claim that Sellars was not, strictly speaking, only worried about Carnap and other positivists. Such a broadening of his target would allow Sellars to sight Hall and other philosophers in his crosshairs.

genuinely epistemological sense of the term" (Sellars 1947a, p. 6). For Sellars, a

"genuinely epistemological" language would be a language that is "capable of being

'about' a world in which it is used" (Sellars 1947a, p. 10). This successful

supplementation of semantics with pure pragmatics

> would serve two functions: (1) It must make possible the development of a
> system of concepts in terms of which all genuinely philosophical questions
> can be given an adequate formulation. (2) It must lead to the clarification
> of the very distinction between philosophical and empirical concepts, so
> that we can understand what it means to say that questions involving
> philosophical concepts are answered on *a priori* rather than empirical
> grounds. (Sellars 1947b, p.28-9).

What Sellars is interested in doing here is developing a general "logical skeleton" for

philosophical problems that would separate philosophical, that is formal, treatments of

concepts from psychological or factual treatments.

The new dimension of calculus structure referred to by Sellars presupposes a set

of formation and transformation rules for a given calculus but attempts to supplement

these rules with "conformation rules" that restrict,[30] for each non-relational predicate in a

calculus, the possible relational predicates that participate in a sentence with the same

individual constant that is "conjoined in a sentence with the non-relational predicate in

question" (Sellars 1947a, p. 11). Sellars is relatively ambivalent on the point of whether a

"pure pragmatics" would necessarily eclipse "proper" semantical treatments of the same

concepts. Sellars is at least initially open to the idea that one could conceivably expand a

notion of semantics (and one assumes here that Sellars is gesturing towards Carnap's

notion of semantics) to include the considerations of pure pragmatics (Sellars 1947a, p. 6

- 7). Although Sellars's does not frequently mention Carnap by name, it *initially* appears

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[30] Sellars uses the term "conformation rules" interchangeably with a notion of the "P-restrictions" on a
language, where the later term simply means a set of conformation rules.

as though he has Carnap in mind when discussing previous syntactical and semantical treatments of languages.

Sellars's conception of psychologism is broader than the conventional notion of psychologism found throughout the early stages of the analytic tradition. For example, Carnap defines psychologism as the mistake of thinking "that logic is a science concerning thinking, that is, either concerning the actual operation of thinking or the rules according to which thinking should proceed" (Carnap 1935, p. 34). This narrower conception of psychologism concerns mistaking the aims of *logic* for those of psychology. As described above, Sellars clearly embraces a broader notion of psychologism as abandoning properly philosophical concepts or categories to psychological investigation.

Sellars is quite aware of the difference between traditional notions of psychologism (of the kind found in Husserl, Frege, and Carnap) and his own. Specifically, Sellars claims that

> under the broader heading of *psychologism* as the confusion of epistemology with psychology, we can distinguish two sub-forms according as epistemology or empirical psychology predominate in the confusion. If the former, epistemological content appears in the guise of psychological acts and objects *sui generis* (*Wesenschau*, universals as apprehendible objects, intentional acts, intentional objects, etc.) These are ranged alongside the facts of empirical psychology, which persists in the confusion. This first sub-form can be called epistemologism (Plato, Aristotle, Kant). On the other hand, if empirical psychology dominates, we have *psychologism* in the narrower sense attacked by Husserl (who was himself guilty of epistemologism). Here the epistemological (which has less survival power) tends to be reduced to a descriptive study *of how we think*. Epistemologism has the virtue of preserving philosophical content, though at the expense of constructing a fictitious psychology. Psychologism in the narrower sense lacks merit as philosophy, although the philosopher and psychologist can join hands in approving its avoidance of pseudo-psychology. (Sellars 1948a, p.52).

Sellars's notion of psychologism here clearly represents something closer to the traditional notion of psychologism. As I have argued previously and will argue in section IVa, the broader conception of psychologism (as opposed to epistemologism and narrow definitions of psychologism) found throughout Sellars's papers is his main concern.[31]

Although one could cash out Sellars's project as a corrective to Carnap's conception of pragmatics and pure semantics, this would be too myopic a reading. As indicated above, although the dominant conceptions of pragmatics seemed to have been limited to the descriptive notion of pragmatics there was certainly a notion of formal pragmatics in the air at Iowa. In the hands of Bergmann and Hinshaw, such formalization is presented as the job of *psychology* as opposed to philosophy. *It is this picture of pure pragmatics, as opposed to the absence of such a concept in Carnap's philosophy, which Sellars is reacting against*. One can make sense of one of Sellars's sub-forms of psychologism; he is explicitly concerned with not only accounts of pragmatics that are limited to descriptive investigations (as is the case with Carnap's treatment of pragmatics), but more so with Bergmann's and Hinshaw's abandonment of formal pragmatics to the domain of natural science.

If one assumes that Carnap is Sellars's target in these early papers, it would certainly seem to be a particularly horrific misunderstanding of Carnap's overall project to think that the construction of a *pure* pragmatics was necessary. This would be to badly

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[31] One critique of Sellars's early efforts is that he employs a conception of psychologism that is out of step with the common understanding of the term as found in Carnap and others (Carus 2004, p. 320). If one were to assume that Carnap was the main target of Sellars's early papers, his conception of psychologism would seem quite odd. As Carus points out, Carnap and, more generally, the analytic tradition from at least Husserl or Frege forward held a narrower definition of psychologism than one finds in Sellars (Carus 2004, p. 322). Sellars is clearly working with a broader conception of psychologism than this but not, as Carus suggests, one that is wholly out of step with the analytic tradition. To argue that Sellars's notion is somehow wrong because it does not line up with Carnap's understanding of psychologism is to oversimplify the context in which Sellars's philosophy developed.

misconstrue the sense in which Carnap takes descriptive semantics and descriptive syntax to presuppose pragmatics. *I want to reject this assumption and suggest that the motivation behind Sellars's pure pragmatics is born not from his understanding of Carnap's philosophy but is caught up within a debate between Bergmann and Hall over the distinction between psychological and philosophical pragmatics and Sellars's lengthy relationship with Bergmann.* It is the flaw in Carnap's conception of rules of designation, as characterized by Hall and Bergmann, that helps shape Sellars's approach to pure pragmatics. If I am correct, then there is a way to read early Sellars that avoids interpreting his project as simply an idiosyncratic, and mistaken, reading of Carnap. Now, it may be the case that Sellars's understanding of pure pragmatics arose in the context of a general misinterpretation of Carnap and I believe this is the case. Such a suggestion does not require that we place Sellars's earliest publications wholly within the context of logical empiricism, or acquiesce to the charge that Sellars was offering internal critiques of logical positivism.[32]

Bergmann's and Hall's philosophy is integral to understanding Sellars's early papers because it offers an understanding of pragmatics, psychologism, language, and Carnap's philosophy that underwrites Sellars's early project. This is not to say that Sellars is merely repeating Bergmann's and Hall's critiques of Carnap - there are *substantial* disagreements between all three philosophers. Sellars and Bergmann's relationship reaches back to the beginning of Sellars's career[33], and the correspondence between the two philosophers in the 1940s suggests more than a simple awareness of

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[32] This is the reading of Sellars suggested in Carus 2004.
[33] The earliest reference I can find to Sellars in Bergmann's writing dates to 1942 in "An Empiricist Schema of the Psychophysical Problem". Within the article Bergmann references discussions with Sellars that started as early as 1939/1940.

Bergmann's work. As will be discussed at the end of this chapter, Sellars's attempt to construct a pure pragmatics was even recognized by some[34] as a project indebted, in some sense, to Bergmann's own conception of philosophical pragmatics.

At first glance one could object that Bergmann himself was a logical positivist and, thus, even if Sellars's early papers are more heavily indebted to Bergmann than Carnap, the context of said papers should be understood as framed by issues surrounding logical positivism. I do not want to be misunderstood as denying that logical positivism had anything to do with Sellars's early publications. My emphasis on Bergmann's and Hall's role in Sellars's early work should serve as a reminder of the multiple currents of thought that influenced Sellars. On top of this, it is important to keep in mind that Bergmann developed a *metaphysical* understanding of logical positivism – one that put him at significant odds with Carnap and other Vienna Circle members – and he went to great lengths to distinguish his philosophical commitments from those of Carnap. Specifically, Bergmann's conception of logical positivism involved the "reconstruction of classical metaphysical questions and answers so as to resolve the traditional problems – instead of merely dismissing them as meaningless" (Hochberg 2001, p. 4). This approach to philosophy, as discussed above, should be seen as quite sympathetic with Sellars's own approach.

It is not simply that one *could* characterize Bergmann this way. Although such an interpretation might be interesting in and of itself it would have little relevance to understanding Sellars's overall position. It is clear that Sellars did understand Bergmann this way:

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[34] This suggestion can be found in Hinshaw 1948b.

Gustav, - there is no doubt but that you have had much influence on my thinking, since I returned from the Navy. As I have often told you I regard you as one of the most important of contemporary philosophers, particularly so since you are one of that rare group – a positivist who has not been spoiled for genuine philosophy. The influence you have exercised has been via two articles and two alone, Pos. Meta of Consc. and PSSP, for these are the only two I studied while working out my argument, and I studied them only when working out my criticisms of your pragmatics in my article on consciousness. As you say, the problem is one for the historian. I am myself not up to the job, beyond this general account. Perhaps when we get together, we can put our finger on the details where we see eye to eye. THAT OUR GENERAL AIMS ARE THE SAME THERE IS NO DOUBT, particularly since your note rejecting (though I don't quite understand how) psychologism in your pragmatics. (Sellars Correspondence, Aug. 1947).[35]

From this (and other) letters there is no indication that Sellars's understood Bergmann's philosophy as simply an instance of logical positivism. On the contrary, Sellars quite clearly thought of Bergmann as a logical positivist who was able to avoid simply rejecting traditional philosophical problems (which falls in line with Hochberg's suggestion quoted above).

Sellars's approach to pure pragmatics should be framed within the context of Bergmann's and Hall's approaches to pragmatics, rules of designation, epistemological predicates, and the notion of an empirically meaningful language. Sellars does show initial signs of discomfort with Bergmann's conception of pragmatics but seems to fall in line with Bergmann's rebuke of Hall;

Last summer, after writing a virulent attack on your conception of a pragmatic meta-language (the paper is still sitting on Moore's desk) in which I showed to my own satisfaction that it was nothing but axiomatized behavioristics, I returned to the task of revising my Realism and the New Way of Words. In the process of doing so, it occurred to me that the predicates 'verified' and 'confirmed' point to a metalinguistic structure from which semantics must be regarded as a bleeding slice. That such a type of meta-language must not be confused with empirical psychology

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[35] The last part of this quote gets a bit incoherent; it's unclear (at least to me) what Sellars is referring to. This paragraph is a bit out of place in the letter because it more or less comes out of nowhere.

(behavioristic or other wise) was a primary conviction which is undoubtedly due to my rationalistic background. Thus, though I choose to call such meta-linguistic structures 'pragmatic' I draw a fundamental distinction between my 'Pure Pragmatics' and what I (perhaps mistakenly) regarded as your tidied version of pragmatics a la Morris, Carnap, et al. When I told Hall of my conclusion, and tried to convince him that 'verified' and 'confirmed' belonged in a non-psychologistic meta-language, he was not moved. For reasons which I gather that you will appreciate, I was disturbed at his article in Φ Sci. (Sellars Correspondence, Aug. 1947).

Nonetheless, Bergmann's conception of pragmatics offers a negative conception of psychologism that is in line with Sellars's use of the term. Bergmann's distinction between philosophical and psychological pragmatics is one of the first offers of an alternative conception of pragmatics that steers clear of psychologism in Sellars's sense of the term. Bergmann is clear that epistemological predicates like "verified" belong in a "pragmatic metalanguage" that is demarcated from a psychological conception of pragmatics (Bergmann 1947a, p. 273)[36]. Bergmann's later conception of pragmatics is one that attempted to demarcate philosophical from scientific analysis. From such a standpoint, psychologism *would* be a confusing of the categories of philosophy and psychology. Such a conception of psychologism, perhaps obviously, is what Sellars explicitly repeats in these early papers.

That Sellars's pure pragmatics was shaped by Bergmann's project; this need not imply sameness of meaning or agreement. Sellars places significantly more weight on the notion of "formal" considerations than Bergmann. In fact, Bergmann's pragmatic metalanguage serves the general purpose of containing verbs such as "verifying" and "knowing" that are not, apparently, contained within semantics due to their references to

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[36] Although references will be made to some of Bergmann's works that are published at the same time as Sellars's early papers, this is mainly due to their clarity. Bergmann makes the same points in earlier essays, specifically in Bergmann 1944 and 1945.

a language user (Bergmann 1944, p. 255) but makes no real *initial* attempt to demarcate

this treatment of pragmatics from overtly psychologistic treatments. As noted above,

when Bergmann does draw the explicit distinction between philosophical and

psychological pragmatics in 1947, the demarcation does not turn on formal *qua* structural

notions. Instead, the important move for Bergmann is the assignment of epistemological

predicates to a philosophical metalanguage and not a psychologistic metalanguage.

On the other hand, it is easy to draw a connection between Sellars's understanding

of psychologism and Bergmann's and Hinshaw's abandoning of formal pragmatics to

psychology. The conception of formal pragmatics prior to Sellars's early publications

was one that clearly abandoned the task of the formalization of pragmatics to psychology.

As indicated in Sellars's correspondence above, he initially read Bergmann as doing just

that. Even where an axiomatized version of pragmatics predated Sellars's own attempt at

formalization it was assigned as the job of the scientists as opposed to the philosopher.

And this seems to be exactly what Sellars has in mind when he charges analytic

philosophy with confusing philosophical for psychological categories.

Sellars's pragmatics should also be understood in opposition to Carnap's wholly

descriptive notion of pragmatics, but Sellars's pragmatics is, as has been argued

previously, not based solely on a critique of Carnap. Bergmann is explicit that his

definition of pragmatics is not that of Carnap and Morris and there is no reason to think

that Sellars is beholden to their definition either. However, understanding a formal, pure,

or philosophical conception of pragmatics as turning on the structural (i.e. formal) study

of the hierarchy of languages and the role they play in relation to an object language

connects Bergmann's and Sellars's conceptions of pragmatics in a way that is nonexistent

between Sellars and Carnap. As indicated by the correspondence above, Sellars was not only concerned with Hall's understanding of pragmatics but lumped it together with wholly psychological accounts of language use.

It is fair to say that Sellars's broad notion of psychologism is a result of concerns that outstrip traditional notions of psychologism. It is true that part of the reason for these concerns turns on the wholly descriptivist account of pragmatics that existed in Hall, Carnap and Morris. Sellars's wider notion of psychologism can also be explained by the explicit abandonment of axiomatized versions of pragmatics to the sciences. Sellars may have initially appropriated the notion of a pragmatic metalanguage from Bergmann's work, but he clearly did not intend for it to fall within the domain of the natural sciences. Considerations of exactly how Sellars understands *pure* pragmatics turn on his understanding of what it means to offer a *formal* treatment of language.

V. Types, Tokens, and *Formal* Requirements

Sellars inherits the type/token distinction from the resources of formal semantics but employs the distinction in a more robust role than one finds in formal semantics. Sellars will eventually argue that said distinction is essential to understanding not just pragmatics but also concepts such as "truth", "designation", "verification", and others, because each concept presupposes the ability to meta-linguistically talk about designata in a "story" as being tokens of sentences in another story (Sellars 1948a, p. 67). In other words, the ability to speak formally (in a sense to be determined) of a linguistic instance presupposes the ability to talk of language as type and to distinguish between the two. "Token" is a predicate in the meta-language and is "used properly" when it is said that the state of affairs or expressions designated by one expression in a language is the token

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of another expression in the language (Sellars 1948a, p. 61). If we say "p" is a token of "q" then we're picking out "q" as the type of expression "p" is an instance of. The significance of this distinction is that any predicates that apply to the expression type also apply to instances (tokens) of said type (Sellars 1947b, p. 35)[37]. In Sellars's words, "we have here a grammar in accordance with which *metalinguistic predicates can be associated with certain expressions* belonging on the 'right hand side of designation sentences'" (Sellars 1948a, p.61).

This distinction plays a significantly larger role in Sellars's pragmatics than it has traditionally played in semantics for two reasons. First, Sellars thinks that the assignment of certain predicates to the metalanguage is what allows one to offer a *formal* treatment of concepts such as "designates". The type/token distinction is what allows Sellars to classify sentences as either "verified" or "confirmed" sentences that exhibit a unity over and above the usual atomicity of simple sentences by stipulating formal relationships that obtain between types of expressions. In addition, the type/token distinction is what allows Sellars to offer a critique of incomplete, empirical language schemas from the standpoint of a language proper (in a sense to be determined in section VII). This second point is crucial to grasping Sellars's account of pure pragmatics, as it allows him to avoid confusing the "formal harmony" between type and token and the factual relationship between utterances and norms (Sellars 1948a, p. 67 – 8).

This, in turn, is what sets the stage for the development of a functional role semantics – an account of meaning that points to the role a given expression plays in a language (in relation to the norms of the language) as a way to avoid the relational

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[37] I'm more than willing to admit that Sellars's claim here is wrong. Nonetheless, in the spirit of charity it would seem that Sellars could be read as making the claim that the structural properties exhibited by the restricted combinations of expression types would necessarily be exhibited in their concrete occurrences.

account of meaning that was dominant at the time. The roots of this idea, found full-blown in "Meaning as Functional Classification", can be found Sellars's insistence that "the predicates of a language are differentiated from one another in terms of the formal roles they play in a language" (Sellars 1947b, p.34). Although this conception of semantics becomes an important notion in Sellars's later philosophy, as will be argued in section IV below, only a *hint* of this position exists in Sellars's earliest papers.

There is an inconsistency in Sellars's account of the type/token distinction concerning what, exactly, a given type designates. In RNWW Sellars says that the designation relation exists between an expression and a *state of affairs* of another expression (Sellars 1948a, p. 61), whereas in PPE the relationship is only between *expressions* (Sellars 1947a, p. 9). Assuming Sellars genuinely wants to avoid factualism in any form, the preferred formulation should be the latter one. As will be explained later in this section, Sellars does want to stipulate some relationship between types, tokens, and the world. Additionally, this terminological inconsistency could be read as a subtle hint to the debates that Sellars's early papers were motivated by. Specifically, Hall's and Bergmann's critique of Carnap's *Introduction to Formal Semantics* centered on how to construe the relationship of designation in formal semantics. This debate, in turn, depends on one's understanding of a "formal" treatment of designation and semantics. Sellars's own understanding of what counts as a "formal" treatment of an empirically meaningful language is crucial to understanding his earliest papers, because all three of them define philosophy, as explicated previously, as the formal treatment of languages.

One charge against Sellars's early attempts at constructing a pure pragmatics is that he horribly misconstrues Carnap's use of "formal" and inconsistently applies his own

definition of the term (Carus 2004, p. 320). There is some truth to the second part of this charge; Sellars himself admits that his use of "formal" in earlier papers "has been confused and misleading" (Sellars 1950, p. 175) and attempts to reconcile this ambiguity in later papers. Unfortunately, the answer to this problem seems to be that there is no solid basis for Sellars's use of "formal". He switches back and forth between possibly meaning "metalinguistic", "nonfactual", "logical", "contained within the universe of symbols", and a whole host of other possibilities that are a bit more difficult to pin down. Especially in PPE, Sellars is quick to discuss the various "formal requirements" of an empirically meaningful language without clarifying exactly what that means.

There are many examples of this in Sellars's early essays. When initially discussing the lack of attention paid to epistemological predicates Sellars claims that "a formal, or metalinguistic, analysis" of these concepts does not yet exist (Sellars 1947a, p.6).. In this instance Sellars clearly means "metalinguistic" when using the term "formal". In fact, this seems to be Sellars's main (yet not only) meaning for the term "formal". Again, one finds Sellars using "formal" as a stand in for metalinguistic: "This concept of an experiential tie is, consequently, a purely formal one. It is the *philosophical* concept which has been sought mistakenly in the psychological object language" (Sellars 1947a, p.10).

On the other hand, Sellars could simply mean "not in the object language" when invoking the term "formal". There are instances where Sellars emphatically denies that formal terms belong in the meta-language; it could be the case that such terms reside in the meta-meta-language. If this is the case, then it is clear that Sellars cannot be read as

simply meaning "meta-linguistic" when using the term "formal". When discussing the distinction between a language schema and a language proper, Sellars argues that

> If 'language-schema' and 'language proper' are formal predicates, it is clear that they do not belong in L. Do they belong to the meta-language which constitutes L? No, for in view of what we said at the time of introducing these terms, it is clear that if they concern formal features of language, it is the rules relating to language-schemata and languages proper that are being compared. We are thus forced to the conclusion that if these predicates are formal predicates, they belong in a language two levels above their ostensible subject-matter. (Sellars 1947a, p.20).

For every instance in which one can find Sellars using "formal" *qua* metalinguistic one can find another in which it seems to mean something else entirely. Sellars's frequently invokes the phrase "formal science" (Sellars 1947a, p.12) but this clearly cannot mean "metalinguistic science". In other instances, Sellars seems to take "formal" as meaning "analytic". In discussing the logical character of meaning statements in "applied" languages Sellars asserts that "there corresponds to it a meta-language which contains (formally) true meaning statements about the expressions of the language" (Sellars 1948a, p.55). There is some plausibility to these readings, as Sellars also invokes the notion of a "formal contradiction" (Sellars 1947a, p.20).

In other instances Sellars's use of the term is simply circular. He frequently defines formal as philosophical and philosophical as formal. This, as was discussed in the previous section, is not particularly surprising, given that Sellars defined philosophy as pure formalism. In discussing confirmation, Sellars argues that "pragmatic concepts (e.g., *degree of confirmation*) can receive formal, that is, philosophical, treatment" (Sellars 1947a, p.15). In this case it seems as if Sellars, more along the lines of Carnap, is simply juxtaposing formal or philosophical with empirical. "Formal" in this instance (*i.e.,* not empirical) does not have to mean "meta-linguistic", it could simply mean "non-factual".

A significant amount of argumentative weight rides on Sellars's conception of "formal", because the term does a substantial amount of work in his early publications. Despite the surface similarity with Carnap's use of "formal", to suggest that Sellars misconstrues Carnap's understanding of the term presupposes that Carnap's use of "formal" was the only game in town. Although such a suggestion is *categorically* false, it is not quite clear that there existed any philosophically relevant differences in understandings of "formal". I say "philosophically relevant" because Bergmann, for example, uses "formal" in inconsistent ways (much like, as we saw, Sellars does) but it is not clear that there is anything philosophically *interesting* about his (or Sellars's) inconsistency; they are simply different notions of "formal" from the one found in Carnap's philosophy. What is interesting about these differing notions is how they lead to, or depend upon, differing conceptions of semantics and pragmatics because of each philosopher's understanding of designation.

In various essays Bergmann invokes "formal" when intending to mean "symbolic" (Bergmann 1944, p. 240) or "uninterpreted" (Bergmann 1944, 239). This first definition of "formal" leads Bergmann to make the claim that an object language is only being considered formally, or we could only call a metalanguage formal, if the metalanguage deals with expressions of the object language but *not* the references of those expressions (Bergmann 1944, p. 241). For Bergmann, as long as the philosophical treatment of language does not leave the "universe of symbols" then it is considered formal. Although this was not always the case, Bergmann's use of 'formal' in this instance is similar to some of Sellars's readings of "formal" as simply meaning "metalinguistic" (Sellars 1947a, p. 6).

This does not *exactly* line-up with Carnap's understanding of the term. To offer a formal treatment of language, as juxtaposed with "descriptive" or "empirical", is to only make reference to the "'form of expressions', i.e. to the kinds of signs occurring in an expression and the order in which they occur" (Carnap 1942, p. 10). Both Carnap's and Bergmann's definition of 'formal' exclude reference to the designata of expressions but Bergmann's use tends to be a bit wider than Carnap's construal of the term. For example, Bergmann has no problem discussing the designata of a given expression in a formal treatment of a language insofar as said discussion does not leave the "universe of symbols". That is, for Bergmann any analysis of language can constitute a "formal" analysis insofar as it is restricted from making claims about extra-linguistic objects. On Bergmann's understanding of a "formal" analysis one can mention designata but it would only be the naming or labeling of designata that would be mention, not the designata themselves. On the other hand, Carnap's conception would *seem* to restrict the formal analysis of expressions to their rules of formation and transformation, leaving aside the "arbitrary" reference to designata[38].

Hall's formulation of this argument is of particular interest because of his subsequent attempt to reunite a language, formally considered, with the world and because Carnap (on more than one occasion) suggested that Bergmann's approach to his philosophy was "mostly Chinese" (Hochberg 2001, p. V). In trying to "correct" Carnap's approach to pure semantics so that one could still formally discuss the extra-linguistic reference of an empirically meaningful language, Hall argues that any historically

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[38] For Carnap, such definitions are "arbitrary" or "freely invented" in the sense that there is no fact of that matter that constrains the rules of designation for a given semantical system. One could, as a question of motivation, correlate rules of designation with a historical language but, formally speaking, the one has nothing to do with the other.

considered language contains what he conceives of as a class of symbols that function as "empirical ties" to the world, as explained in section III (Hall 1944a, p. 35)[39]. Although not presented as a "formal requirement" Hall clearly considers this as a precondition for any language to be about the world.

Such a concept is interesting in relation to Sellars's concerns over formal requirements because of Hall's reliance on the type/token distinction (or, in Hall's explication of Carnap, the distinction between sign-design/sign-event) to cash out his notion of an empirical tie. There is some philosophically relevant symmetry between Hall's suggestion that a language needs an *empirical* tie to be empirically meaningful and Sellars's suggestion that a language needs an "*experiential* tie" to be empirically meaningful (Sellars 1947a, p. 10 - 11). Hall's idea that such empirical ties would need to function as both matters of fact (in linguistic episodes) and as a facet of language (Hall 1944a, p. 36) seems to be mirrored in Sellars's claims that experiential ties, in conjunction with the notion of a token, allow for a truly philosophical (i.e., not a disguised form of psychologism) notion of verification (Sellars 1947a, p. 10). In fact, Sellars even refers to the notion of an experiential tie as the "empirical tie" of a language (Sellars 1947a, p. 11). The difference between the two notions is that Sellars's notion of an experiential or empirical tie is presented as a *formal* requirement of pure *pragmatics* as opposed to a presupposition of formal *semantics* (as in the case of Hall). As was seen earlier in Sellars's correspondence with Bergmann, Sellars construed the placement of such concepts in pragmatics, as opposed to semantics, as a significant point (though, unfortunately, in later passages Sellars seems ambivalent on this point). Nonetheless, the

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[39] The genesis of worrying about an "empirically meaningful language" as opposed to a constructed calculus will be discussed in section V.

two notions hold a similar enough function to, and I think safely, claim that the constructive side of Hall's critique of Carnap provides part of the framework for Sellars's notion of an experiential tie[40].

The arguments between Bergmann, Hall, and Carnap are particularly influential on Sellars's approach to pragmatics and designation. Sellars understands the type/token distinction as presupposing designation and having to do with the relationship between a descriptive language and language as norm or type (Sellars 1948a, p. 52). More importantly, as a consequence of this position Sellars should be read as agreeing with Bergmann and Hall that "designates" is not only a metalinguistic predicate but one that has *nothing* to do with the relationship between language and world (Sellars 1948a, p. 52 – 3). As described above, this is *exactly* the position one finds in both Bergmann's and Hall's critique of Carnap's semantics some years prior to Sellars's early papers. It leads Hall and Sellars to the project of cashing out how an empirically meaningful language can be about the world and Bergmann's additional challenge that the intentionality of language is an illusion. Of course, Sellars's arguments about the intentionality of language should be framed in opposition to this commitment of Bergmann's, but this will be discussed in section VIII.

Even with the appropriate background considerations now in place, it is difficult to discern Sellars's actual understanding of the term "formal", because he makes no reference to the source of his stipulations (seemingly mentioning at random that a certain concept is a formal requirement or restriction on an empirically meaningful language). One interpretive strategy, following Carus, would be to assume that Sellars uses "formal"

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[40] Given Sellars's indifference between referring to an experiential or empirical tie of a given language, one might safely assume that Sellars's is simply adopting Hall's notion in a slightly modified form.

to stand in for "metalinguistic" (Carus 2005, p.) and one could argue that he generally ascribes to Bergmann's understanding of the term. This approach has the advantage of some textually accurate instances (Sellars 1947a, p. 6) but the advantage of uniformity comes at the price of ignoring the numerous contexts in which Sellars clearly does not mean "metalinguistic' in his use of "formal". Sellars opposes the formal to the empirical (Sellars 1948a, p. 49 & 51), syntactic (Sellars 1948a, p. 55), and (though I think arguably) to the normative as opposed to the factual (Sellars 1948a, p. 61)[41]. In all fairness, Carus recognizes that Sellars plays a bit fast and loose with his use of the word "formal", but I want to suggest that settling on an understanding of the term as something like "metalinguistic elucidation" does not do justice to the complexity of Sellars's early essays (Carus 2005, p. 322).

Another approach, following Jeff Sicha, would be to admit that there exists a variety of meanings behind Sellars's use of the term "formal," but that Sellars intends it to pick out a given notion as belonging in the realm of "practical discourse" (Sicha 2005, p. xxi). I think this badly misconstrues Sellars's indebtedness to the debates over Carnap's semantics and how to understand formal approaches to language. Sicha is quite right that one should not ignore the variety of uses of "formal" in Sellars's early essays but wrong to minimize the roles described above. Sicha's general interpretive strategy is to adopt an understanding of "formal" as "practical" because of Sellars's strong normativist conception of language in his later essays (Sicha 2005, p. xix). This is an interpretive strategy that should be resisted; there is no reason to think that Sellars's

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[41] This is not to mention the numerous instances of "formal" in PPE that have no clear meaning in context. Specifically, Sellars's numerous claims that such and such a concept counts as a "formal requirement or restriction" rings hollow. How one might charitably interpret this is dealt with at the end of this section.

conception of "formal" or of "rules" in the 1950s that pick out inherently normative

notions should be read *back* into his early papers.

Although apparently unaware of this fact at the time, Sellars is quite aware later

that his use of "formal" in his earliest publications is wildly inconsistent (Sellars 1950, p.

175); but his attempt to clarify his understanding of 'formal' is less than helpful. In a

footnote in his article "Quotation Marks, Sentences, and Propositions", Sellars

acknowledges this inconsistency and redefines "formal" as "the phenomenology of

linguistic functions" (Sellars 1950, p. 175). By "phenomenology" in this context Sellars

means "the systematic exhibition of the rules of a language by the use of that same

language for this purpose. The phenomenology of language is the exhibitory use of

syntactical and semantical meta-languages, meta-meta-languages, etc…" (Sellars 1950, p.

175). This attempt at clarification has the advantage of presenting an explicit definition of

"formal" with the disadvantage of being ambiguous. Sellars's "phenomenology of

linguistic functions" insofar as it is the "exhibitory use" of language seems, almost by

definition, descriptive as opposed to formal. And this cannot be what Sellars means, since

it would defeat the entire purpose of offering a *formal*, as opposed to factual, account of

language. I suppose what Sellars could have meant by "phenomenology" was the

occurrence of formal meta and meta-meta-languages but this would not help us get any

closer to a definition of "formal".

Even if Sellars's early project fails to clarify in what sense any of his assertions or

restrictions of an empirically meaningful language are "formal," it should be a bit clearer

as to what Sellars was attempting to do. Leaving aside the technical missteps, it seems

that the most charitable interpretation of Sellars's formal analysis would place it in a

quasi-Kantian vein as offering arguments for the conditions for the possibility of a given language being empirically meaningful – a broadly transcendental argument dressed up in logical form[42]. Of course, given the dominance of logical empiricism in the 1940s, such an argument, if presented as a straightforwardly transcendental argument, would have been significantly out of step. Although Sellars, on my reading, is offering arguments that are largely antithetical to a broadly understood logical empiricist picture of philosophy, he should be read within the context of logical empiricism.

Carus is wrong to paint a relatively simplistic picture of this context – one that myopically places Carnap at the center. Instead, one should see the largest determiner of Sellars's earliest philosophy as problems and commitments inherited from traditional philosophy combined with the relatively complex reception history of Carnap's work. It's telling that one of Sellars's early unpublished attempts at dealing with the relationship between language and world framed the issue of the adequacy of a language (that reappears in RNWW) as *not* being an issue of logic but one of epistemology "and (*if you permit*) metaphysics" (Sellars 1946, p. 4, emphasis added). Carus is quite right to insist that many have minimized the role logical empiricism plays in Sellars's early philosophical development but wrong to think that said role is so straightforward. Even attempting to cash out what Sellars means by "formal" in relation to logical empiricism and other positions, as I think has been shown above, cannot be encapsulated in a straightforward comparison to Carnap's work.

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[42] This is more of a slight stab in the dark (though I think a *correct* stab in the dark) than a full blown theory. Between his attempted thesis on Kant, studies under Marvin Farber, and various hints in his early papers to Kant, there is clearly some merit in comparing parts of Sellars's early papers to a broadly Kantian approach to philosophy. Given the evidence that Sellars's attempt at a formal analysis falls a bit short of the goal, this would seem the most charitable or coherent approach to Sellars's early methodological commitments.

Terminological inconsistencies aside, Sellars's understanding of designation can now be initially explained. Sellars draws a distinction between "language as a descriptive category" and "language as an epistemological category for which the relation of type to token is not that of empirical class to member" (Sellars 1948a, p.52). The distinction Sellars is trying to draw is between language as empirical fact and language as a formal construction. This being the case, Sellars offers his initial analysis of "designates":

> 'meaning' or, as it prefers, 'designation' is a term in a language about languages in the second sense. Its primary employment is in connection with expressions as norms, and consequently cannot concern a direct relation of language expressions to objects of acquaintance (even essences). It is only symbol-events that could enter into such a psychological transaction. If this is the case, it is hard to see what kind of factual relation 'designates' could be. *The New Nominalism takes 'means' or 'designates' to be a purely formal term, that is to say, a term which as little stands for a feature of the world as 'implies' or 'and'.* It has nothing to do with psychological acts, intuition, or, indeed, with experience of any kind. It refers to no psychological act, intuition, or transaction of any sort. (Sellars 1948a, p.52-3).

There are a couple of different things going on here. Sellars's treatment of designation statements as formal clearly turns on an understanding of formal *qua* meta-linguistic in this instance. Much like Bergmann, in this case Sellars is stipulating, contra Carnap, that rules of designation only concern the relationship between meta-language and object language. This is to deny that meaning or designation statements stipulate a relationship between the object language and extra-linguistic objects or states of affairs.

If Sellars's account ended here, his placement of designation in the meta-language and his stipulation that such statements do not concern a one-to-one correspondence between expressions and the world would essentially be a re-iteration of Hall's linguacentric predicament; though it would be, like Bergmann's position, an endorsement of such a predicament. Sellars's account of pragmatics does want to determine how a

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language formally relates to world; Sellars is not interested in placing his conception of language within Hall's linguacentric predicament or Bergmann's universe of symbols *per se*. Sellars's final account of designates turns on his conception of a world, story, and world-story and his defining formal predicate "co-ex".

VI. Rules

In *The Logical Syntax of Language*, Carnap lays out a brief discussion of the role of extralogical rules of inference or P-rules. Carnap takes these to be the "so-called universal laws of nature i.e. universal sentences of physics" but, in principle, one could extend the material transformation rules of a language to also include concrete instances of observation sentences[43] (Carnap 1937, p. 180). In the *Syntax* (and beyond) Carnap understands the existence of P-rules for a language as "a matter of convention and hence, at most, a question of expedience" and they hold a small place of consideration for Carnap outside of these brief passages in the *Syntax* (Carnap 1937, p. 180). Carnap and other logical positivists understood rules as definitions that are explicitly stated but contain no prescriptive component (Carnap 1942, p. 24- 5). Carnap's semantics contains numerous kinds of rules (rules of formation, rules of transformation, rules of designation, and rules of truth) but says little about extralogical rules of inference.

Against this backdrop, Sellars's emphasis on the necessity of material rules of inference looms large in his early papers, but exactly how this works is, much like the other defining concepts of pure pragmatics, a rather vexed story. Sellars discusses various P-rules and P-restrictions on a language in PPE and they play a central role in ENWW

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[43] I'm not exactly sure why Carnap suggests that observation sentences are possible additions to rules of transformation. Nonetheless, on p. 180 one finds Carnap asserting that among transformation rules one could include the "universal sentences of physics" as well as "concrete sentences – such as empiricial observation-sentences."

and RNWW. In PPE Sellars defines P-restrictions on a given language as occurring in both negative and positive phases:

> The negative element in the concept of a P-lawful system might be called a *principle of confirmation*…A still more suggestive title might be 'the principle of the internality of primitive relational predicates' for the conformation rules of a calculus, the expressions of which can qualify for pragmatic predicates, *specify for each non-relational predicate in the calculus, the relational predicates which can participate in sentences with one and the same individual constant which is conjoined in a sentence with the non-relational predicate in question*.(Sellars 1947a, p. 11).

The negative phase of a P-lawful system is certain P-restrictions on a calculus that restrict the combinations of predicates, individual constants, and atomic sentences in a given language. Specifically, Sellars describes said rules in their negative phase as "a certain type of restriction on the predicates of the calculus in which such a system can be formulated" (Sellars 1947a, p.11). When Sellars invokes "conformation rules" he is usually referring to the "negative phase" of the concept.[44] In Sellars's mind the historical parallel of the two phases would be Leibniz's principle of compossibility and principle of plentitude (Sellars 1947a, p. 11).

The positive phase of P-rules is offered in tentative form only. Sellars describes the positive phase of a P-lawful system as where

> A set of sentences in a calculus with a given set of P-restrictions (conformation rules) and for which there are an identity of indiscernibles, will be called a text, if (1) every individual constant appearing in the set appears in both a non-relational and at least one relational sentence of the set; and (2) no sub-set of the set fails to contain at least one individual constant which appears jointly in a relational sentence with at least one individual constant not appearing in the sub-set. (Sellars 1947a, p.12).

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[44] The "positive phase" would seem to encompass his definitions of a text, complete text, fix, reciprocal fix, and story (Sellars 1947a, p. 12). On an interesting note, Sellars's reference to a system exhibiting "P-lawfulness" necessarily points back to the *rules* of the language and, in the positive phase of P-lawfulness, such rules are presented as *definitions*. This should be seen as a bit more evidence for my point below that Sellars's early papers exhibit no uneasiness with Carnap's conception of a rule.

It isn't exactly clear why Sellars splits P-restrictions into two phases. The point of the "positive phase" would seem to be the stipulation of the unity of sentences that appear in a world-story over and above their arbitrary combination into complex sentences. By stipulating that the individual constants of a set participate in at least one relation predicate, Sellars seems to be setting up the P-restrictions of a language to "gear in" with his notion of confirmed, as opposed to verified, sentences as discussed below. So, in this case, the positive phase of P-restrictions represents the overall unity provided by conformation rules that does not exist in the Carnapian picture of atomic sentences and individual constants.

Within the basic machinery of Carnap's logic, and formal logic in general, there are no restrictions on the combinatorial properties of atomic sentences or individual constants. In an uninterpreted or interpreted calculus, any individual constant can be combined with any predicate provided that the rules for formation are not violated. The negative and positive phase of Sellars's P-restrictions represent a supplementation of formation rules (hence conformation rules). Thus, one finds Sellars claiming that

> In order for a world-story to contain sentences which are *confirmed* but not *verified*, the atomic sentences which constitute the story must have a unity over and above that of satisfying the syntactic requirements (formation rules) of the language. The status of being confirmed but not verified requires a criterion of togetherness in one sentence structure; *conformation rules* as well as *formation rules*. (Sellars 1948a, p.57).

With these restrictions in place, Sellars is able to provide a formal counterpart to the idea that an empirically meaningful language exhibits a unity (a connection between sentences that confront their designata and those that do not) over and above arbitrarily combined atomic sentences. This occurs, in part, because of Sellars's commitment to analyzing

empirically meaningful languages as opposed to formal calculi. How this commitment shapes Sellars's construal of conformation rules will be dealt with in section VIII.

The significance of conformation rules that restrict the possible combination of predicates (…or atomic sentences, or individual constants; Sellars is somewhat inconsistent on this point)[45] is that such rules stipulate the formal roles such predicates play in a language (Sellars 1947b, p. 34). By restricting the combination of atomic sentences and individual constants with predicates, Sellars is restricting what kinds of sentences can be verified or confirmed. Thus, Sellars claims that since 'designates' is a purely formal concept

> The conclusion at which we are arriving is that from the standpoint of epistemological analysis, the predicates of a language are differentiated from one another in terms of the formal roles they play in a language. Using the term 'syntax' in a broader sense than is current, we could say 'different syntax, different predicate; same syntax, same predicate'. We shall prefer to say that *predicates are differentiated only by the conformation rules which specify their combining properties*. The concept of combining properties of predicates (and it must be remembered that in this paper we are only concerned with primitive predicates) concerns the relation of predicates to individual constants in the following way. It involves (1) *the concept of a 'skeletal relational predicate (there may be more than one, provided they are syntactically related) which signifies the fundamental type of order in which the individuals to which the language can refer must stand*; and (2) the concept of restrictions on the non-relational predicates which can be associated with individual constants where the restrictions are a function of (a) the predicates, (b) the (skeletal) relational sentences in which these individual constants are making an appearance. *We have here a coherence theory of meaning characterized in purely syntactical terms*. (Sellars 1948a, p. 60-1).

The restrictions on predicates are related to individual constants in that they introduce the concept of a "skeletal relational predicate" that specifies the order in which individuals constants in a language must stand. Despite the appearance of conformation rules as extralogical rules of formation, they are never given a *material* interpretation; Sellars

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[45] For example, see PPE (page 11- 12) and then ENWW (page 32).

only exhibits such rules in syntactical form despite their occurrence as material

restrictions on a language. In the quotation above Sellars is quite clear that conformation

rules are what differentiate predicates and that "the predicates of a language are

differentiated from one another in terms of the *formal* roles they play in a language"

(Sellars 1948a, p.60). Sellars's initial understanding of conformation rules would seem

to be syntactic (as Sellars mentions above, one could use the slogan "'different syntax,

different predicate; same syntax, same predicate'"), though syntactic in a significantly

broader sense[46] than the one used by Carnap. Unfortunately, *Sellars never clarifies what

is meant by a "broader" understanding of syntax.*

For Sellars, material rules of inference play a significantly larger role than one

finds in Carnap's philosophy. By the 1950s Sellars is quite clear, especially in "Inference

and Meaning", that material rules of inference are required for an accurate

characterization of an empirically meaningful language. It is in these articles, *after*

Sellars's earliest papers, that one finds Sellars's explicit disagreements with Carnap's

work. Perhaps more importantly, in his earliest papers, there is no sign of Sellars's later

anxiety with Carnap's understanding of a rule. *Conformation rules are presented

alongside formation and transformation rules as an additional requirement of pure

pragmatics but not as a revision to the conception of a rule as found in formal semantics.*

In fact, Sellars says absolutely *nothing* about possible problems with understanding

formation and transformation rules on Carnap's schema. Given that Sellars construes

pure pragmatics as a supplementation of semantics, pure pragmatics necessarily

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[46] It may be that between 1947 (the initial appearance of conformation rules) and 1948 (the source of the quoted passage above) Sellars simply changed his mind. Although I am not advancing a substantive hypothesis at this point, it is worth noting that after Sellars 1947a he never mentions conformation rules as P-rules again.

presupposes the kind of formation and transformation rules found in pure semantics. The kind of arguments one finds against this understanding of rules, while heavily present in Sellars's later works, is nonexistent in his early papers.

On top of there being no inherently normative conception of a rule, there seems to be little confrontation between Sellars's conformation rules and formal semantics. Conformation rules are intended as an addition or supplementation to the formation and transformation rules of formal semantics. It's only in the context of pure pragmatics that such rules are presented as essential; "The concept is not a decisive one for pure semantics, for while semantic rules can define calculi which involve P-lawfulness, *a calculus defined by a set of semantic rules need not have this characteristic*. Pure semantics is indifferent to the presence or the absence of P-lawfulness in object-language structures" (Sellars 1947a, p. 9). Why does Sellars characterize pure semantics as defining a calculus that need not, and does not, recognize P-rules as essential (Sellars 1947a, p.9)? His conception of meaning as *requiring* conformation rules against a background of critiques centered on the psychologistic mistakes of formal semantics would seem to entail that Sellars cannot afford to be indifferent on this matter.

I think there are two possible ways of reconciling these points with Sellars's commitment to conformation rules. First, one could claim that although it is possible to construct a calculus that is indifferent to such rules, to do so would be to cut it off from any pretention to characterizing a language in the full-bodied sense. So, on this reading pure semantics does not deal with what Sellars calls 'empirically meaningful languages' but only abstract calculi. This reading would hold the distinct advantage of actually lining up with Carnap's contention that pure semantics only deals with semantical

systems. This move is not a live option for Sellars. He initially admits that the calculi constructed in pure semantics are close to what one would appropriately call languages (Sellars 1947a, p. 5). Moreover, Sellars's initial claims turn on the idea that pure semantics, as it stood in the 1940s, was inadequate to characterize the metalinguistic predicates that differentiated it from pure syntax.

If Sellars is to remain consistent, he would need to claim that a correct understanding of semantics would *require* conformation rules. Given that Sellars spends a large amount of time arguing against the conception of designation or meaning found in analytic philosophy at the time, and given that conformation rules are presented as the "skeleton" of a (coherence) theory of meaning, it is difficult to see what conception of semantics could survive without said rules. And, in a different passage from the one quoted above, Sellars seems to have this idea in mind. By claiming that "it may be preferable to extend the term 'semantics'" when considering these rules as opposed to developing a separate domain of linguistic study, Sellars seems committed to the idea that pure semantics, once supplemented with conformation rules, could offer a philosophically respectable account of meaning (Sellars 1947a, p.5). If Sellars is read this way, then it seems as if his position is a bit more consistent than on first glance.

Sellars's conception of a rule in pure pragmatics, once set against the backdrop of Carnap's philosophy, would seem to indicate that Sellars initially had no problem with the logical positivists' conception of a rule. Bergmann and Hall have little to say on this matter; their critiques of Carnap take no issue with his conception of a rule. One must look to Sellars's later publications, especially "Inference and Meaning", to note a shift in Sellars's view. This will be the addressed in chapter three.

VII. Worlds, Possible Worlds, and Formal Semantics

One issue that differentiates Bergmann's, Hall's, and Sellars's approaches to semantics and pragmatics from Carnap's is their commitment to analyzing empirically meaningful languages as opposed to uninterpreted calculi or semantical systems. For Sellars, this means that his formal reconstruction of an empirically meaningful language turns on the notion that

> *the minimum formal requirement which a formal system must fill in order to be a candidate for the position of empirically meaningful language is that it be capable of being 'about' a world in which it is used*. This statement should be kept in mind as the key to the argument which follows, for its aim can be summarized as *the attempt to give a formal reconstruction of the common sense notion that an empirically meaningful language is one that is about the world in which it is used*. (Sellars 1947a, p.10).

This differs radically from Carnap's project (in both its syntax and semantic phases); as was discussed previously in section II, Carnap is not concerned with language as applied (in Sellars's terminology) or with the connection between calculi and naturally occurring languages. Although what is demanded by an analysis of an empirically meaningful language clearly differs for Bergmann, Hall, and Sellars, the commonality is that all three philosophers have embraced *linguistic* reconstruction in a very different sense than one finds in Carnap's philosophy.

For Sellars, a formal reconstruction of empirically meaningful languages commits one to dealing with the fact that empirically meaningful languages "speak" about the world. In turn, Sellars introduces the notions of a "world' and a "world-story' to partially encompass such an idea. In his earliest paper Sellars never gives an explicit definition of a "story" or "world story". At best, one finds an attempt to implicitly define the concept through the subcomponents of a text and a complete text. This, to say the least, is neither

clear nor helpful. What, exactly, a world-story is, is left relatively ambiguous, because

Sellars never explicitly defines a text or complete text in PPE. Sellars defines a text as a

set of sentences in a given language where, presupposing certain formation rules and P-

restrictions of the language, "(1) every individual constant appearing in the set appears in

both a non-relational and at least one relational sentence of the set; and (2) no sub-set of

the set fails to contain at least one individual constant which appears jointly in a

relational sentence with at least one individual constant not appearing in the sub-set"

(Sellars 1947a, p. 12). The point of such a concept is to hold certain restrictions,

apparently in addition to P-restrictions, on the analysis of an empirically meaningful

language, as to what individual constants are combined in relational predicates with other

individual constants. Sellars's notions of a text, complete text, fix, and reciprocal fix all

turn on having certain sentences and individual constants that exhibit a connection with

each other – a restricted sense of connection that is based on his antecedent, and

extremely problematic, notion of the "formal requirements" of a language. This is a

byproduct of Sellars's idea that there is a "rational connection", in a sense to be

determined, between verified sentences that experientially confront their designata and

merely confirmed sentences that do not (Sellars 1947b, p. 32).

From this, in PPE, we get a definition of a story as a complete text with a

reciprocal fix (the idea that all the texts formulable in a calculus which includes such a

complete text are synonymous with it) which contains the verified sentences and the

experiential tie attached to said sentences (Sellars 1947a, p. 12 – 13). "Experiential tie",

as discussed in section III, would seem to be a complementary concept to Hall's notion of

an empirical tie. Sellars's notion of experiential tie would fulfill the same role as Hall's

notion of an empirical tie; that of a class of symbols that links the tokening of sentences with an experience (Sellars 1947a, p.10-11). The significant difference between Hall and Sellars would seem to be that Hall locates empirical ties in the object language (as they are the link between expressions in the object language and extra-linguistic objects); Sellars places experiential ties in the metalanguage[47]. Specifically, Sellars claims that "the concept of an experiential tie is, consequently, a purely formal one. It is the *philosophical* concept which has been sought mistakenly in the psychological object language" (Sellars 1947a, p.10). It is not a stretch of imagination to think Sellars has Hall in mind when making this remark.

This "technical" treatment does not clarify the concept of a world or a world-story. One finds a clearer exposition of a "world" and "world-story" in ENWW and RNWW. Sellars defines a world as the "collective term for the designata of the world-story" (Sellars 1947b, p. 38) and a world-story as the "body of logically simple (atomic) sentences which constitute the story of the universe" for a language user (Sellars 1947b, p.30). Pragmatically speaking, the concept of world-story is what allows the tokening of sentences to be "about" states of affairs within experience. For this to work within the context of *pure* pragmatics, Sellars would need to tack on an additional requirement that stipulates how this would occur for a language user. Sellars's introduction of an "irreflexive, symmetrical, and transitive two-place predicate" that stands for "the common sense expression of 'is-present-to-consciousness-along-with'" (Sellars 1947a,

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[47] Sellars places experiential ties in the metalanguage if one reads his stipulation of experiential ties as "formal" requirements to simply mean metalinguistic. If not, I'm not exactly sure what Sellars has in mind here. In any case, what he could mean is that experiential ties, so identified, exist in the object language but only in a formal treatment (leaving "formal" undefined in this case) of the object language. This would still seem to be a point that differentiates Hall's and Sellars's treatments of empirical or experiential ties.

p.10) is a relatively baffling *formal* requirement of an empirically meaningful language but fulfills the required role mentioned above.[48]

The introduction of the predicate "coex" is how Sellars links the notion of a world and world-story with a language user; it is how Sellars is able to offer a formal *pragmatics* of language. That is, in worrying about the world-story that constitutes what a language is about, Sellars is concerned to construct a world-story *qua* world-story of a language user in particular. In Sellars's words:

> We have the notion that for the world-story to which Jones is committed
> to be the story of his world, Jones' immediate sense experience must
> include tokens of sentences which constitute the sense biography of Jones.
> In other words, Jones' immediate experience must include items which are
> tokens of sentences which designate the content of Jones' immediate
> experience. Tersely put, *tokens of (Jones) sense-biographical sentences
> must be co-experienced with the sense data which these sentences mean or
> designate*. (Sellars 1948a, p.54).

The point of introducing the predicate "coex" is to link a language user's experience with statements in the pragmatic metalanguage. This, as will be shown, is what allows Sellars to avoid the kind of naïve realism present in Carnap's rules of designation while staying out of Hall's linguacentric predicament.

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[48] As Carus points out, such a requirement would seem to be missing any specifically *formal* feature and, instead, would seem to invoke the kind of psychological concepts of thought that both Carnap and Sellars wish to avoid (Carus 2004, p. 322). It's not exactly clear that Sellars's conception of "co-ex" runs afoul of his conception of psychologism. Sellars is careful to point out that the common sense expression "is-present-to-consciousness-along-with" should be thought of as the *model* for the formal requirement on any calculus to be an empirically meaningful language. What Sellars means by this is that although the *model* for the predicate "co-ex" is the factual language statement of "x being co-experienced with y", as used in pure pragmatics, the predicate only amounts to a formal ("formal", here, as simply meaning "nonfactual") stipulation of a empirically meaningful language. To confuse the model of "co-ex" with the factual statement would be to confuse the *formal* harmony of a language proper with the *factual* instances of the language schema we employ in everyday communication (Sellars 1948a, p. 67 – 8). Although, as argued above, Sellars's invoking of "formal" or "formal requirements" is, at best, confused, it is not clear that he is violating his own stipulations against psychologism when it comes to suggesting that an empirically meaningful language would be formally required to contain the predicate "co-ex".

Sellars's linking of statements to immediate experience does not entail that all sentences in a world-story necessarily confront their designata. In finally offering the formal analysis of epistemological predicates, Sellars draws a sharp distinction between verified (and verifiable) sentences and confirmed (and confirmable) sentences. A verified sentence is a sentence whose terms "confront" their designata in experience. (1948a, p.56). In other words, "a verified sentence is a sentence a token of which is co-experienced with its designatum" (Sellars 1948a, p.56-7). This is not to say that all sentences will be co-experienced with their designata; thus a world-story would contain sentences that are confirmed but not verified (Sellars 1948a, p.57).

As noted in the previous section, it is conformation rules (in addition to formation rules) that allow Sellars to claim a unity between confronting and non-confronting sentences. By restricting the combination of individual constants and relational and non-relational predicates, Sellars is able to "link" verified and confirmed sentences without suggesting that all sentences of a language confront their designata. It is the conformation rules that restrict the possible combinations of individual constants and predicates and provide the "togetherness in one sentence structure" of a language (Sellars 1948a, p.57).

This formal relationship between verified and confirmed sentences is one that exhibits the kind of uniformity one finds in laws of nature; Sellars makes the same allusions to "laws of nature" in ENWW. There, as in RNWW, such a world-story could exhibit the uniformity of the laws of nature, because any language containing said story would be partially constituted by conformation rules that underwrite said "lawfulness" (Sellars 1947b, p. 32). A world-story, or more so "a world", would be the flesh that fills out the bones of the "skeletal relations" of conformation rules and can help make sense of

Sellars's claim that these skeletal relations are "relations of matter of fact" (Sellars 1948a, p. 60).

There is an open question of factualism between Hall and Sellars that is worth broaching, because Hall's position seems to clarify Sellars's notion of a world-story. The general point of insisting on a world-story for Sellars is to assert that an empirically meaningful language, as opposed to a simple calculus, would require not just sets of sentences that confront their designata but also sentences that are only verified indirectly (confirmed sentences) and that said sentences exhibit a rational connection with those that are directly verified. Within the framework of formal semantics, Hall's treatment of the predicate "verified" is one that is relatively skeptical, as is Sellars's, that only sentences directly verified could have meaning in a story (Hall 1947. P. 127). In rejecting the consequence relation to fulfill this role, Hall argues that any notion of verification would need to embody various assumptions about the *factual* relation of sentences; specifically that if some state of affairs, p, obtains then this *grants some reason to suppose* that another state of affairs, q, obtains as well (Hall 1947, p. 129). In short, Hall's solution is to stipulate that a requirement on any empirically meaningful language would be that there must be some accounting for this connection beyond syntax; there must be a way to account for the common sense notion that some verified sentences entail sentences that do not confront their designata.

If this is all Hall stipulates of an empirically meaningful language, then it would seem that his treatment of "verified" is exactly the kind of factualist treatment that Sellars is trying to avoid. By "factual assumption" Hall means "factual" in a very narrow sense – that any notion of verification must contain a factual assumption about the world that

would posit this connection. 'Factual' in this sense simply denotes categorical (Hall 1947, p. 129) and such a sense of 'factual' does not seem to run against Sellars's concern with factualism. For Hall, this assumption is exhibited in what is generally thought of as the uniformity of the laws of nature and he thinks it equally applies to realist and positivist notions of verified sentences. The difference between the two schools of thought is that most realists would also require an empirically meaningful language to contain a complex principle indicating the relationship between perception and thing sentences much like Sellars's notion of the two-place predicate co-ex (Hall 1947, p. 129 -130).

Sellars's notion of *a* world story, opposed to *the* world story, can be seen as a clarification of Hall's general idea of the factual assumption (in Hall's narrow sense of 'factual') that any account of an empirically meaningful language must make. In Sellars's view, one could imagine that Hall gets it right when characterizing this connection between verified sentences and the sentences they entail as a *requirement* on an empirically meaningful language but wrong when it comes to asserting that they indicate *the* world. Sellars claims that "if we introduce the term 'world' as a collective term for the designata of a world-story, then it is a purely formal truth that every story in every empirical language designates *a* world' (Sellars 1948a, p.65).

Charitably speaking, the role of a world and world-story in Sellars's early philosophy seems to be three-fold:

1. To express the idea that any empirically meaningful language must *presuppose* an indeterminate set of atomic sentences (that express or stand for states of affairs); such a presupposition is part of what allows one to keep in mind the idea that languages are about the world in which they are used (avoiding Hall's

linguacentric predicament). Without this, as in Hall's arguments, no accurate notion of verification or confirmation is possible.

2. To express the idea that from the standpoint of a language proper (or from a "formal" standpoint, in this instance contrasted with considerations from language-behavior) the designation of a given language schema is that of *a possible world* but not *the* world. Formally speaking, any language can designate as many possible worlds as needed, but there is only one actual world (Sellars's minimal thesis of Realism).

3. To assert the formal requirement (or factual assumption in Hall's terminology) that an empirically meaningful language must furnish assumptions about the world (more appropriately *a* world) which it is about.

With this interpretation in place we can revisit Sellars's conception of designation.[50] The idea here would seem to be that the state of affairs offered by a world-story (contained in the verification base of a language) would include tokens of "items" in the world story that confront or are "co-experienced with" their designata. Requiring that a language contains a world-story in this sense is what allows Sellars to claim that "a language is the sort of thing about the world in which it is used" (Sellars 1947a, p.10).

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[50] As a historical note, it should be pointed out that Sellars's discussion of "a world-story", "history", and "text" seems to rely heavily on antecedent discussions of these terms as found in Bergmann, Max Black, and others. More important, most of these antecedent notions arise not, as it would seem, directly from a close reading of Leibniz so much as a close reading of Russell's *The Philosophy of Leibniz*, especially the fifth chapter on the identity of indiscernibles, possibility, and compossibility. There were a number of publications in the 1940s and early 1950s that centered on this chapter and Sellars's early mention of "Leibnizitian terms" seems explicitly indebted to this (Sellars 1947a, p. 11). I am not suggesting that Sellars never read Leibniz; it could be the case that his heavy involvement in the history of philosophy program at Iowa committed a chunk of his time to closely working through Leibniz's text. In the same vein, Sellars himself asserts his indebtedness to Leibniz's philosophy and not Russell's take on Leibniz (Sellars 1948b, p. 91).

Given all of this, Sellars seems able to reconnect language and world without giving into naïve realism or being trapped in Hall's linguacentric predicament. In other words, the upshot of Sellars argument is that "to characterize certain items in a world as true, verified, meaningful, etc., is to talk in a pragmatic meta-language about designata of sentences in a story being tokens of other sentences in the story" (Sellars 1948a, p.67). That is, Sellars's attempt to link tokens of sentences in the metalanguage that confront their designata with those that do not allows him to develop a coherence theory of meaning that manages to avoid the predicament described in section III.

VIII. Languages, Language Proper, and Language Schema

Worlds, world-stories, and conformation rules all appear in Sellars's early essays in an attempt to analyze empirically meaningful languages in a way that "alone entitles them to be called *languages* in a genuinely epistemological sense of the term" (Sellars 1947a, p.6). Such a concern is not reflected in Carnap's or other logical positivists' approach to the formalization of language. As explained in section II, Carnap's pure semantics has little connection to historically given languages and his project is not motivated by epistemological concerns *per se*. What did frame Sellars's understanding of a language was the "Iowa School" approach to languages as exhibited in the philosophy of Bergmann and Hall. Both philosophers' influence on Sellars paved the way for his conception of an empirically meaningful language as well as the distinction between a language proper and a language schema.

There is precedent in both Bergmann's and Hall's philosophy to treat the languages in question as empirically meaningful and naturally occurring languages as

opposed to constructed abstract calculi. Bergmann's earliest publications on a positivist conception of languages turn on understanding pure syntax and semantics as "possible model formalizations of the empirical language" (Bergmann 1942, p.76). For Bergmann, this means that a language up for linguistic analysis is not simply marks on paper but "the total pattern linguistic events exhibited" (Bergmann 1947b, p. 58). That is, the formalization of language is only genuinely an epistemological, as opposed to a methodological, exercise when one is concerned with empirically meaningful languages. When discussing "formal" pragmatics Bergmann is quick to point that what one would be formalizing is the kind of language Sellars has in mind when worrying about an empirically meaningful language (Bergmann 1944, p. 256 – 7). Although Sellars does not formulate his views in this manner, it seems clear that both philosophers are united in the idea that a formal analysis or reconstruction of language understands "language" as empirically meaningful.

Bergmann introduces the ideas of both a language schema and an ideal matrix; ideas that are both present in Sellars's early papers.[51] Bergmann suggests that an "at least partial schemata of the empirical language" are created once one moves away from languages *qua* mathematical devices (Bergmann 1944, p. 239). Bergmann defines "empirical language" as "one of those informal, universal, interpreted languages, say English, which we all understand and speak about the world in" (Bergmann 1944, p. 239). Such a notion both captures, in part, Sellars's notion of a language schema (as discussed below) and the move away from an understanding of "language" that hooks up directly with Carnap's conception of languages as uninterpreted or interpreted calculi.

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[51] It's helpful to keep in mind Thomas Storer's 1950 remarks concerning Bergmann and Sellars's similar philosophical approaches. It's particularly difficult to piece together whether both Bergmann and Sellars mean the same thing when using the same term. Neither philosopher clearly defines his terminology.

An "ideal matrix" would be a complex hierarchy of languages that could capture the abstract pattern of linguistic events (Bergmann 1945, p.256). The point of such constructions would be to show how certain problems are simply due to the fact that we speak a "radically and irredeemably" inadequate language schema (again, Sellars echoes Bergmann) as opposed to an ideal matrix of languages (Bergmann 1945, p. 209). Part of such hierarchies being "ideal" is that they are differentiated from the actual schemas we speak, much like Sellars's distinction between a language proper and a language schema (again, to be discussed below).

Hall argues that when considering meaningful languages in semantics, one cannot simply constrain the analysis of languages to mathematical calculi. Specifically, Hall argues that when worrying about languages that are meaningful, one is dealing with factual languages. This, as commented in section III, does not seem to violate Sellars's concern over factualism. Hall explicitly denies that calling such languages "factual" is dealing with actual occurrences in the world. The idea is that in worrying about languages that are not mere calculi, one must worry about "something not arbitrarily invented, not a mere fiction" (Hall 1947, p. 130). In terms of pure semantics, Hall argues that

> Whether one accepts the view that it literally embraces the relation
> between symbols and their referents or only pictures or reflects this
> relation, in either case it is factual. Not in the sense of saying just what
> actual things (referents or designata) are concretely in the world, but in the
> sense of dealing with something not arbitrarily invented, something not a
> mere fiction. The occurrence of particular cases of designation or
> reference is a fact in the narrower sense The nature of designation,
> however, is also included in the world, is a fact for analysis. Now it might
> be said that this is true for a semantics interpreted as talking about
> 'designation,' etc., in the ordinary meaning of such terms, but that pure
> semantics is an un-interpreted calculus, a mere pattern of marks. To this I
> can only reply dogmatically that, if it be true, then pure semantics is not a

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branch of semiotics, not a part of a linguistic study (or construction, if you please); it is an arbitrary game whose rules happen to produce a pattern similar to that of a semantics interpreting 'designates' in accordance with common usage. (Hall 1947, p.130-1).

Hall's commitment here is to the idea that if one is talking about languages where meaning or designation occur, one cannot, *contra* Carnap, be talking about pure semantical systems (Hall 1947, p. 130 – 1).

Despite the precedent found in Bergmann's and Hall's approaches to language, there are certainly significant divergences between their approaches and Sellars's own. Most blatant would be Bergmann's contention that the intentionality of empirically meaningful languages "*is neither a natural, nor philosophical, nor a linguistic relation, but a linguistic illusion*" (Bergmann 1945, p. 209). This is somewhat difficult to square away, but Bergmann seems to mean that in an idealized language (a language proper) there would be no aboutness relation despite the fact that such notions are deeply imbedded in our natural languages (Bergmann 1945, p. 209). The idea is that differing levels of the formal reconstruction of an empirically meaningful language are not, strictly speaking, about anything. Meaning or aboutness only occurs "casually and contextually" in natural languages (Bergmann 1945, p.217)[52]. Thus, Bergmann suggests that "formally considered" the different levels of language simply do not communicate (Bergmann 1945, p. 219).

Despite the fact that Sellars's project hangs on cashing out the idea that empirically meaningful languages are ones that are about the world, there are passages that seem sympathetic to Bergmann's point. Sellars's and Bergmann's accounts converge on the notions of meta and meta-meta-languages; in Sellars's case the idea that, formally

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[52] This is a particularly vexing part of Bergmann's philosophy. It becomes especially tortuous because Bergmann later claims (in the 1950s) that aboutness is a central tenet of his philosophy.

speaking, the observation that a metalanguage is about an object language would need to be constituted one level above its subject matter (hence a meta-meta-language). Formally considered, such languages are autonomous (or "atomic" in Bergmann's terminology) and are only about another language "in the factual-descriptive sense that the manipulations of the former are glimpsed in its manipulations" (Sellars 1947a, p. 21). Such a move is reminiscent of Bergmann's Wittgensteinian contention that in a final analysis of an ideal matrix of a hierarchy of languages (as laid against concrete linguistic events) such patterns would *show* themselves (Bergmann 1945, p. 201).

Sellars defines a language schema in relation to what he calls a language proper. The notion of a "language proper" is introduced as a Wittgensteinian device to clarify our incomplete language schemas, though Sellars's use of the device contains strong similarities with Bergmann's introduction of an idealized language.[53] A "language proper", or the language of an omniscient being in RNWW, is a calculus where the variables (for both individuals and predicates) are defined by an explicit list of constants that would replaces the variables (Sellars 1947a, p. 18). An omniscient user of a language proper would be able to do away, so to speak, with universals and replace any variable with an explicitly defined list of constants strung together by disjunction or conjunction. A "language schema" is a calculus that contains the same variables yet has no access to such an explicitly defined list of constants (Sellars 1947a, p. 18). Since a language schema does not have access to an explicitly defined list of constants, general terms in a language schema obtain their content from "keeping an eye" on a "miniature language proper" as the model for general statements (Sellars 1947a, p. 18). Sellars's

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[53] Both Bergmann and Sellars seem to be taking cues from Wittgenstein's introduction of an idealized language in his *Tractatus Logico-Philosophicus*. Historically speaking, this is the genesis of the idea, but it's not clear that there is a lot of philosophical import resting on this fact.

idea here is that abbreviated lists, so to speak, such as "x, y, z, etc…" gain content in reference to a language proper; language schemas function *as though* they were properly construed, full-blown languages despite lacking the resources of a language proper (Sellars 1948a, p. 48).

Although, at least initially, the point of a language proper is to evaluate or criticize a language schema, the relationship between the two is not this straightforward. It initially seems to be the case that "language proper" and "language schema" are metalinguistic terms that are used to both classify and criticize a given object language. Sellars construes this view as a trap, one that would necessarily lead back into Platonism and psychologism. Instead, Sellars argues that the distinction is a *factual* one that is associated with language in a psycho-sociological sense (Sellars 1947a, p. 20). To be blunt, it's not quite clear what Sellars thinks he is doing (or what he's *actually* doing) when invoking the distinction between language schema and language proper in this instance. By insisting that the distinction is a *factual* one, Sellars seems to run roughshod over his claims that no one speaks a language proper.

Sellars's early discussion of languages as committed to a normative conception of language, at least a normative conception of languages *qua* actual instances of languages, comes out in its clearest formulation in RNWW. Here, Sellars characterizes certain concepts ("true" and "valid" being his examples) of pure pragmatics as normative as opposed to factual (Sellars 1948a, p. 61). But Sellars characterizes this sense of normativity as "*tentative*" at best in a way that is never clarified (Sellars 1948a. p. 61). Returning to the type/token distinction, Sellars is also willing to characterize formal

languages as normative as opposed to descriptive but, again, only tentatively (Sellars 1948a, p. 52). What Sellars seems to have in mind here is the use of a formally constructed language proper (what he calls the constitutive sense of language in PPE as opposed to the descriptive) as a norm or standard compared against tokens of empirical languages. The model of a language proper acts as a norm for the language schema we speak in the sense that it can be used to reveal philosophically misleading formulations of language schemas. Given Sellars's comment that such a distinction is *factual*, it is difficult to see how this would all work out. Going out on an increasingly frail limb, it would seem that Sellars would need to understand the distinction as a formal one in order to make sense of his previous comments; otherwise it is difficult to see in what sense language is normative.

This could clarify a number of positions in Sellars's early papers, most importantly making contact with Bergmann's and Hall's critique of designation. To make sense of Sellars's description of these concepts as formal is to understand their role as "expressions as norms". So, reaching back to section V, only empirical instances (symbol-events or tokens) of expressions can ever confront their designata, but the classification of "meaning" or "designation" must necessarily occur in the metalanguage about expressions in the object language. Such metalinguistic concepts could be construed as normative in the sense that they serve as formal symbol-designs or types in a language proper that are tokened in language schemas; such a formally "complete" language would be the model for our incomplete language schema.

What the central question will become when reconciling Sellars's pure pragmatics with his later essays is whether this sense of "normative" survives his shift away from a

formal analysis. In some sense it does; part of the analogy between evaluative and epistemic language turns on seeing the former as a model for the latter. Fleshing out in exactly what sense this occurs will be taken up in chapter three.

IX. The Results of Pure Pragmatics: Expectations and Reception

With all of Sellars's machinery in place, we can now determine how his project fits within the narrative found in Bergmann's and Hall's critiques of Carnap on rules of designation. As discussed in section VII, with the introduction of the predicate "coex", Sellars is clearly constructing a formal treatment of an empirically meaningful language that is not cut off from the world. Nonetheless, Sellars is strictly offering a formal (in some sense of the term) treatment of this relation; Hall's proposed solution of abandoning designation or meaning to descriptive semantics is clearly the antithesis of Sellars's project.

On the other hand, Sellars wants to avoid the kind of naïve realism found in Carnap's and other's insistence on an understanding of designation and truth that seems committed to a one-to-one correspondence between language and world. "Naïve realism" is defined by Sellars as the idea of "language as mirroring the world by a one-to-one correspondence of designations with individuals" (Sellars 1947b, p.30). Sellars's rejection of naïve realism amounts to the idea that the relationship between the left and right hand sides of designation rules "is not to be explained in terms of a direct comparison of language with the world (naïve realism)" (Sellars 1948a, p.55). Sellars manages to construct a picture of language that avoids naïve realism because of his notions of a world-story and the predicate "co-ex".

In Sellars's words:

> The final abandonment of Naïve Realism comes with the realization that the right-hand side of designation sentences together with the predicate 'designates' and the semi-quotes on the left-hand side are all alike formal devices belonging to the grammar of epistemological predicates; that is to say, their function is the purely formal one of hooking up with the rules relating to the assignment of such predicates as 'verified sentence of L,' 'true sentence of L,' 'meaningful predicate of L' (see below) and many derivative epistemological predicates that would have to be introduced in a complete discussion. (Sellars 1948a, p.64).

Ideally, what Sellars has done in this instance is give a purely formal analysis of meaning or designation that is able to avoid that kind of psychologistic or factualistic pitfalls inherent in Carnap's, Bergmann's, and Hall's approaches to semantics and pragmatics.

Sellars's emphasis on *a* world as opposed to *the* world an empirically meaningful language designates allows him to clarify how pure pragmatics can offer a non-factual treatment of designata without giving into the kind of factualism Sellars is trying to avoid:

> *The pure theory of empirical languages as formally defined systems which are about the worlds in which they are used, has no place for THE world; but only for the world designated by the story which is the meaning base of a language*…An understanding of the completely non-factual character of epistemological statements rests on the insight that not even the predicates 'verified' and 'confirmed' have an intrinsic tie with any single world, with 'the REAL world.' They are purely formal predicates and no properly constructed world-story stands in a privileged position with respect to them. (Sellars 1948a, p.65).

The idea here is that the "skeleton" Sellars is providing does not offer factual claims about the relationship between a world designated by a set of sentences and the world. Though Sellars is relatively mute on the issue, one would assume that such claims would be the job of descriptive semantics or pragmatics and not formal treatments of language.

Sellars is able to offer a characterization of epistemological predicates that avoids psychologism and factualism, a characterization that can account for the rational

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connection between confirmed and verified sentences (sentences which confront their designata), and a wholly formal treatment of an empirically meaningful language. If my argument is sound up to this point, it would seem that Sellars's program fails for a number of reasons previously discussed. The terminological inconsistencies, technical mistakes, and general ambiguity of Sellars's project leave it somewhat difficult to see how it even gets off the ground. Without specific references to the secondary literature, it is not a stretch of the imagination to suggest that this is the general opinion of most of the philosophical community; at least the portion of the community that has bothered to glance at Sellars's earliest papers. Such an assumption is somewhat out of step with the reactions of Sellars's contemporaries. It's not that everyone understood Sellars's papers – far from it – but their reasons for being baffled by it were not because of the philosophical ideals that they contained. Instead, the main failure of Sellars's early works seems to be a result of the way they were expressed.

Though little philosophically relevant correspondence survives between Sellars and Carnap, two letters do survive that paint a more sympathetic reaction from Carnap than one would expect. Carnap's general reaction to both ENWW and "Sentences and Propositions"[54] is, more or less, confusion about the formal apparatus Sellars employs. He is particularly confused by Sellars's use of the word "atomic" and his definition of 'text' and 'complete text' (Carnap Correspondence, May 1947). Concerning the latter point, Carnap thought that Sellars's inability to clearly define "text" and "complete text" actually made a number of his arguments impenetrable. Given this, the overall point is that Carnap's complaints centered more on Sellars's attempt to handle the technical machinery of formal semantics than the content of his claims.

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[54] This was an early draft of "Quotation Marks, Sentences, and Propositions".

What is primarily important in Carnap's initial reaction to Sellars's early papers is what he does *not* say. Carnap does not object to the content of ENWW and, in fact, thinks that there are a number of particularly interesting ideas, especially the idea of sense data confronting sentence tokens (Carnap Correspondence May 1947). He does not object to the suggestion of a formalization of pragmatics or to Sellars's overall project. And, I think most important, he does not object to the sense in which Sellars uses "language".[55] Instead, the only actual reaction we have from Carnap is one limited to his serious confusion over Sellars's attempted formalism,

Nagel's short review of ENWW and RNWW is perhaps the most informative concerning the general reaction to Sellars's project. Nagel admits to having a difficult time understanding both the general intent and the specific arguments of the essays but manages to summarize the main points (Nagel 1948, p. 223). Nagel takes no explicit issue with the notion of developing a pure pragmatics but argues that Sellars's essays do nothing to clarify the notion and are wrought with technical and terminological confusions. Moreover, Nagel insists that the defining predicate of pure pragmatics, 'co-ex', seems to be an arbitrary stipulation on an abstract calculus; Sellars provides no reason to assume that factual or psychological considerations should or could simply be ignored simply because a certain predicate would be required of a given calculus (Nagel 1948, p. 223). Specifically, Nagel claims that

> It is also puzzling how, if the predicate "coex" is specified only with
> respect to some of its formal properties, either it or the definitions based

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[55] Carus makes much of this point, claiming that Sellars fails to understand in what sense Carnap understands a formal versus natural language. It would be quite odd to think that Carnap never touched on this point when being asked for explicit commentary on the papers in questions. Of course, it's quite possible that Carnap did have a problem with Sellars's use of "language" and simply did not mention it (or the letters have been lost, destroyed, etc…). The point I want to drive home is that Carnap gives no indication of finding Sellars's to be up to Carnap interpretation or involved in something totally foreign to his conception of philosophy.

on it are any more relevant for clarifying the issues of epistemology than is any other arbitrarily constructed abstract calculus. On the other hand, if a meaning is associated with "coex" which does make its use clearly relevant for handling philosophic problems, it is by no means obvious that psychological and other factual considerations can be swept aside. Moreover, in the absence of explicit reasons for the assumption that the verified sentences must entail the remaining true statements of a language, both the assumption and the problem to which it gives rise in Dr. Sellars's hands appear as entirely arbitrary and gratuitous. (Nagel 1948, p.223).

Although Nagel does not discuss the distinction Sellars draws between a language schema and a language proper in his comment concerning 'co-ex' appearing in an abstract calculus, he generally gets Sellars's arguments correct. What seems in line with most reactions to Sellars's early papers are the ideas that they are extremely hard to follow (to the point of its being impossible to understand most of the content) and that Sellars does not define a number of formal notions throughout the papers.

One of Nagel's damning charges is that the "rational connection" that *must* exist between verified and confirmed sentences is never argued for by Sellars but merely assumed as a necessary component of an empirically meaningful language (Nagel 1948, p. 223). While this may be true, it should be remembered that a number of Sellars's contemporaries, most notably Hall, did offer arguments for why this must be the case. As discussed above, Hall's own arguments turn on what requirements a characterization of an empirically meaningful language would need. One could offer a comment such that Sellars does not offer *his own argument* for the necessity of this connection, but it was not out of step to suggest such a thing. The point is that, in context, there is no reason to assume that Sellars's insistence on this connection would be utterly foreign to all of his readers despite Nagel's insistence that this is a weak point of Sellars's papers.

Nagel's initial observation that if Sellars's "coex" predicate is defined only along formal properties it would seem to be irrelevant to epistemology, is particularly damaging. Given that one point of Sellars's project is to properly classify epistemological predicates, Nagel's observation is damaging because it seemingly robs Sellars's project of its philosophical relevance. If this is the case, Sellars's pure pragmatics stands on no better footing than any other arbitrarily constructed calculus.

The reception of Sellars's early papers as plagued with technical errors is also reinforced in the correspondence between Bergmann and other philosophers as well. Bergmann apparently turned over replying to Sellars's early papers to Thomas Storer (a former student of Bergmann) but both philosophers seem to share the same appraisal of Sellars's publications. Confirming this suggestion, Storer beings his letter by expressing general confusion over PPE:

> I may say at the beginning that I am pleading for enlightenment. There are many parts that I just can't understand and, while it may be naïveté, I have the feeling that if I can't understand it, who the heck can, since we speak, essentially, the same lingo, and have picked up much of this from the same source. (Storer Correspondence, Jan. 1948).

As was seen in Nagel's review of the same paper, the initial reaction to Sellars's project is largely one of confusion. Storer's remark that both he and Sellars "picked up much of this from the same source" would seem to indicate, again, that Sellars's project is heavily indebted to Bergmann's philosophy.

The content of these letters from 1948 point out numerous technical errors with PPE, specifically Sellars's use of the type/token distinction and his defining predicate "co-ex" (Storer Correspondence, Jan. 1948). The general idea is that Sellars repeatedly confuses sentence names, sentences, and variables throughout the paper, rendering the

type/token distinction somewhat meaningless. The charge is leveled against Sellars that "co-ex" is fundamentally incoherent, since Sellars's formulation of it in PPE would allow for his formal calculus to prove meaningless expressions (Storer Correspondence, Jan. 1948). As Storer puts it:

> You discuss, by the way, the irreflexive, symmetrical, and transitive character of 'coex' on page 187. I have registered my mis-understanding of this before. If you wish it to possess all three characteristics, it seems to me that you cannot say that it is irreflexive, but that both: (x coex x) and ~ (x coex x) are meaningless. Thus you exclude the expression (x coex x) on the grounds that it is meaningless, rather than it is false. This leads to the peculiarity, however, that your logic is capable of proving meaningless expressions. (Storer Correspondence, Jan. 1948).

The long technical analysis of Sellars's various mistakes in PPE ends with Sellars's correspondent suggesting that he honestly understands little of the article (Storer Correspondence, Jan. 1948).

What makes this exchange most revealing is the follow up letter between Storer and Bergmann. Here Storer suggests that both Bergmann and himself are unified in their opinion that Sellars fails to understand his own use of formalisms (Bergman Correspondence, Jan. 1947). The most damning comment seems to be that Sellars should "try to learn something about logic, or else forget to write this type of thing" (Bergmann Correspondence, Jan 1947.). In a letter to Bergmann in 1949, May Brodbeck suggests that Bergmann's harsh critique of Sellars (from a series of letters that do not seem to exist at this point) was quite justified. Not only that, both Brodbeck and Bergmann find it somewhat amazing that Ryle would publish such a paper (Bergmann Correspondence, Jan. 1949).

A passage in Bergmann's letter to Sellars in January 1949 best sums up his attitude towards Sellars's early papers:

> Having read your paper, I feel like Fabrice del Dongo, the hero of the Charter-heuse who, as you know, was never quite certain whether he had taken part in the battle of Waterloo. All he can say is that he spent a day riding horses, losing one, finding another, that he jumped a ditch, saw the smoke line of artillery fire, caught a glimpse of a little man in a tri-cornered hat galloping past. But how can he tell that this was the battle of Waterloo? In fact, he isn't even sure that he got the German and…similarly, what have I read? (Bergmann Correspondence, Jan 1949).

It seems fair to say that although Bergmann could be classified as a supporter of Sellars's early career, he was not impressed with his attempts at technical philosophy. What few parts of Sellars's essays Bergmann did understand, he seemed to dismiss because of the technical errors in Sellars's account (note the discussion of "co-ex", the defining concept of pure pragmatics, above).

Virgil Hinshaw's short review represents the only unqualified positive reaction to Sellars's early papers, calling them "a substantial contribution to philosophy in general and semiotic in particular", but it lacks in any substantive comment on the work and simply summarizes the major point of ENWW (Hinshaw 1948, p. 57). This is not particularly surprising, given that Hinshaw was one of Sellars's earliest students. What is particularly interesting about Hinshaw's review is that he links Sellars's project in PPE and ENWW[56] with Bergmann's attempts at constructing a pure pragmatics in "Pure Semantics, Sentences, and Propositions" and related essays (Hinshaw 1948, p. 57).[57] Such recognition should bolster the idea that one of Sellars's motivations in these early papers is Bergmann's approach to pragmatics. In the same vein, Herbert Schneider's editorial comments on early rejected versions of ENWW are encouraging towards

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[56] Thomas Storer makes the same connection in his 1950 article "On Communication". Given that Storer was a student of both philosophers (Bergmann was the chair of his dissertation committee) this is not surprising.
[57] Hinshaw does not say what related essays he has in mind, but one imagines he meant Bergmann's article "A Positivistic Metaphysics of Consciousness". This is most likely the case as Bergmann presents the later essays as a sequel to "Pure Semantics, Sentences, and Propositions".

Sellars, but no specific details are given about what Schneider found encouraging aside from Sellars's focus on pragmatics and a non-psychologistic notion of meaning (Schneider Correspondence, Feb. 1947).

As has been remarked, the general reception of Sellars's earliest efforts is simply one of confusion. It is an interesting sense of confusion, both historically and philosophically, because it does not arise, despite the commonly held assumption, from the idea that what Sellars was trying to articulate was *radically* out of step with his contemporaries. A second idea, closely related to the first, is that Sellars's early works were not understood because most philosophers did not grasp the *content* of these essays[58]. As to the first idea, I think it is rather clear from the preceding sections that this simply was not the case; Sellars's early works squarely fit within the narrative found between Bergmann and Hall. Sellars's arguments were very much at home amongst the dissenting opinions of Bergmann, Hall, and others. Although Sellars's specific project of pure pragmatics is different from Hall's and Bergmann's attempt to correct pure semantics, there is no evidence that it was, or would have been, received as an utterly foreign concept.

The second idea is also unfounded but, again, not for the commonly asserted reasons. As is, I think, evident from the correspondence discussed above, the hurdle for understanding Sellars's early papers was not the philosophical ideals behind them but the technical errors and ambiguous terminology within them. Bergmann's and Nagel's responses are the most telling in this sense because their predominant complaints are

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[58] Both of these ideas are often echoed but rarely, if ever, published. The reason for this seems to be that almost no one publishes on Sellars's earliest essays. One does find general comments, for example in Pedro Amaral's preface to Sellars's early papers, to the effect that Sellars was out of step with his contemporaries but this is more of an introductory remark than a developed thesis.

based on not being able to understand the bulk of the material *because* of technical problems. Sellars's early papers had a problematic reception not because his ideas were out of step with the norm but because of technical flaws in the presentation of the ideas.

One of the most interesting remarks contained within Sellars's correspondence with Bergmann turns on Sellars's own lack of confidence concerning his formal project of pure pragmatics. By 1948 Sellars remarks that he is getting "cold feet" about the use of technical devices in his papers (Sellars Correspondence, Dec. 1948). Sellars realizes that over the last three years (which encompassed the publication of PPE, ENWW, and RNWW) he had made enough technical blunders to run the risk of making a fool of himself in publications (Sellars Dec. 1948). Unfortunately, Bergmann confirms Sellars's concern about his use of formalism in his early paper. As far as reception history goes, it is interesting to see that Sellars's own concerns with the technical aspects of his early papers seem to line up with the general reception of said paper.

Sellars's correspondence with Bergmann can go some way in explaining why Sellars quickly drops an overtly formal approach to his understanding of language. Given the reception of these papers combined with Sellars's lack of confidence in his own formal competence, it is not surprising that a formal approach to the philosophy of language does not appear in subsequent essays. In 1949, "Language, Rules, and Behavior" is published and contains some of the themes found in Sellars's earliest works (the notion of an empirical or experiential tie for one) and it borrows some terms from Bergmann's work in the 40s (e.g. juxtaposing one's own view against a "casuist" position). The approach found in the 1949 article does not contain the kind of formal approach one finds in in these early papers, but it does contain a strong argument for the

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normativity of language that is based on an analogy with moral language or reasoning. This last theme, combined with Sellars's explicit disagreements with Carnap, will be the focus of the next chapter.

X. Conclusion: After Pure Pragmatics

Having offered what I hope is a more robust context for Sellars's early work than has previously existed, I want to focus on three connected and lasting disagreements between Sellars, Carnap, and anyone endorsing a broadly positivistic conception of language. Specifically, Sellars's conceptions of rules (both formal and material), language, and norms all make appearances in Sellars's early and later essays. That being said, the proper context to flesh out Sellars's explicit disagreements with Carnap should be found in his slightly later essays and not within his articles dealing with pure pragmatics. As noted previously, Sellars shows no signs of being uncomfortable with Carnap's definition of a rule in his early works; Sellars's notion of conformation rules appears as an *addition*[59] to the resource of pure semantics and *not* a correction. By the time "Inference and Meaning" is published in 1953 this is clearly not the case.

Though the language of norms and the normative does appear in Sellars's early essays, they do not seem to represent the inherently "strong" sense of the normativity of language one finds in Sellars's later works.[60] The parallels that Sellars draws with evaluative and moral language are not found in his formal attempts at dealing with pure pragmatics, and only RNWW places any strong emphasis on language as normative.

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[59] I say "addition" in this case because Sellars quite clearly presupposes the "usual" rules of formation and transformation. Although one could interpret conformation rules as a corrective to Carnap's overall characterization of a language, it would be difficult to read this as a rejection of Carnap's conception of a rule. There is nothing in early Sellars, or so I have argued above, that suggests he initially holds a radically different conception of rules than Carnap.

[60] Of course, by "later works" I do not mean exceedingly later. Sellars's introduction of what I have called the "strong' normativist project starts in 1949.

Although language *qua* normative does appear in RNWW, how strong a notion of the normativity of language this commits Sellars to, or how this is supposed to work in a coherent fashion, is left unclear..

All that being said, there is a sense in which early Sellars has the last laugh when it comes to pragmatics. By 1955 Carnap was willing to admit that, as a whole, pragmatics was woefully underdeveloped and that any promising philosophical line of inquiry into descriptive pragmatics would need a framework of pure pragmatics to rest upon (Carnap 1963, p. 861). Of course, this needn't imply agreement with Sellars's conception of pure pragmatics (though Carnap does cite Sellars as an example of work in pure pragmatics) but it seems unlikely that Carnap was downright hostile to Sellars's overall project. As noted in the correspondence above, Carnap's initial concerns with Sellars's project are not ones that are generally dismissive of the venture or the distinctions that Sellars was drawing but concern technical errors in Sellars's formulation.

It is quite difficult to discern what, if anything, Sellars's early papers accomplish. As has been noted, the reception of his early work was clearly one of puzzlement at best, even by Sellars himself. The numerous technical errors and terminological confusions make it difficult to patch together a coherent argument from PPE, ENWW, or RNWW. One does get a picture of what commitments, or lack thereof, are exhibited in Sellars's early works. One certainly finds a commitment to a broadly rationalist revision to empiricism and a realist understanding of ontological and epistemological issues. I contend that the normative conception of language that exists in these early papers is not as strong as the one finds in Sellars's papers outside the project of pure pragmatics (starting with "Language, Rules, and Behavior"). Such a distinction is telling because

Sellars's later emphasis on his disagreement with Carnap's conception of rules and the necessity of standards of correctness are by-products of Sellars's later thought and not, so I suggest, contained in his early papers. The next chapter will focus on isolating Sellars's exact relationship to Carnap on these issues as well as constructing an account of how Sellars moves from a weak conception of the normativity of language to a significantly stronger one and whether that move is justified.

Chapter 3: From Formalism to Psychology

I. Introduction

Despite Sellars's early attempt to define philosophy as a formal discipline and subsequently construct a pure pragmatics of language, by his 1949 publication of "Language, Rules, and Behavior" (hereafter LRB) most pretense of defining philosophy along strictly formal lines vanished. Sellars's exploration of standards or norms of "rule-regulated behavior" is approached "from the standpoint of what might be called a philosophically oriented behavioristic psychology" (Sellars 1949, p. 117). Sellars announces at the beginning of LRB:

> More specifically, our frame of reference will be the psychology of rule-regulated behavior, or rather, since such a science as yet scarcely exists, it will be such anticipations of a psychology of the so-called higher processes as can be precipitated from common sense by the reagents synthesized by the naturalistic revolution in psychology. (Sellars 1949, p. 117).

Such an announcement should initially give pause, given Sellars's then recent commitment to philosophy as a *formal* practice situated squarely against psychological treatments of language and the mind. One would think that a philosopher committed to the idea that linguistic analysis had suffered from an invasion by psychologically minded philosophers would avoid such a frame of reference. Even Sellars's later commentary on rules of language and rule-regulated behavior is presented as yielding "new insight into the psychology of language" (Sellars 1954, p. 321). This seeming shift in philosophical orientation gives rises to at least three distinct questions:

1. What caused Sellars to shift from a definition of philosophy as a formal practice to a practice that uses psychology as a frame of reference?

2. Does Sellars's new approach avoid giving in to the kind of psychologism he argued against in 1947-8?

3. How does Sellars's shift in philosophical orientation change his conception of a language and a linguistic rule?

As to the first question, Sellars's switch to a more psychologically friendly approach to understanding language and behavior would seem to be precipitated by at least two factors: the failure of Sellars's approach to pure pragmatics and the changing philosophical conversation that shifted away from formalism and towards a behavioristically informed philosophy of language. As described in the previous chapter, the general reception history of Sellars's pure pragmatics was poor and Sellars himself was well aware of that fact. In the subsequent literature one finds little reference (positive or otherwise) to Sellars's early formal project outside of one or two passing references by Herbert Feigl, May Brodbeck, and Rudolf Carnap.[61]

The lack of attention paid to Sellars's early publications is not the only reason behind his shift from understanding philosophy as a formal discipline juxtaposed against psychologism to one that is more "psychologically friendly". An overtly formalist approach to understanding language, as grounded in Carnap's *Syntax*, had fallen significantly out of favor by the late 1940s. Charles Morris's *Signs, Language, and Behavior* (hereafter SLB) was published in 1946 and marked a shift in conversations over

[61] For example, See Feigl's article "Existential Hypothesis" and its sequel "Logical Reconstruction, Realism, and Pure Semiotic" as well as May Brodbeck's "Coherence Theory Reconsidered".

the study of language. Morris's project in SLB is to construct semiotics as a scientific study of linguistic behavior. The text "is directed both to scientists and to those concerned with the basic issues of contemporary individual and social life" and attempts to offer a *a wide survey* of scientific approaches to language (Morris 1946, p. 3). In this context, Morris demarcates scientific from philosophical (and other) accounts of language behavior by his "conviction that a science of signs can be most profitably developed on a biological basis and specifically within the framework of the science of behavior" (Morris 1946, p. 2). Semiotics is presented as a scientific, behavioristically grounded approach to language; one that starts with primitive terms used to describe animal behavior that are built upon eventually to offer more complex definitions of terms that capture the wide variety of human linguistic behavior.

Morris is somewhat vague as to the role of philosophy in relation to a scientific study of behavior. What is clear is that Morris does not consider philosophy as part of a scientific study of behavior and he leaves the door open for "metaphysical discourse" to play a role in speculations about language (Morris 1946, p. 132). Despite his substantive relationship to logical empiricism, Morris invokes a somewhat more traditional definition of philosophy as the critical determination and organization of "our basic beliefs" (Morris 1946, p. 235). That is, philosophy is "a comprehensive systematic organization of basic beliefs" and that these "basic beliefs" confront the philosopher "in his culture by statements of fact, by appraisals of value, and by prescriptions for conduct" (Morris 1946, p. 235). Though based upon content derived from the sciences and the common sense world, philosophy, in its determination and organization of this content, necessarily includes "*more* than scientific statements" and extends beyond strictly scientific picture

of the world (Morris 1946, p. 133). The commitment to understanding philosophy as including "more" than scientific statements sets Morris apart from his logical positivist contemporaries and signals his rejection of the empiricist meaning criterion as the test for the relevance or meaningfulness of philosophical statements (Morris 1946, p. 133 - 4) Unfortunately, Morris is somewhat vague as to what this "more" is, aside from a passing reference to "the totality of statements which a given thinker believes to be true" (Morris 1946, p. 133).

The publication of SLB ignited a firestorm of secondary literature[62] concerning the role of a behavioristically based science of language in relation to philosophy. Although reactions varied as to the correctness and promise of Morris's work, a useful approach to the secondary literature can evenly divide response between philosophers that found internal problems with Morris's specific brand of behaviorism and those that found external issues that led to the rejection of Morris's project from a philosophical perspective. Max Black's review of SLB is indicative of the internal approach and he argues that Morris's behavioristic definitions are inadequate as pieces of behavioral psychology. This is not to reject Morris's approach as a whole but to deny that his contribution is particularly satisfying. Black claims "that a simple vocabulary of 'stimulus,' 'response,' and cognate 'behavioristic' terms" is "excessively narrow for fruitful application to human language" (Black 1947, p. 259). Black's issue with Morris's approach to language is not concerned with external questions relating to the usefulness of behavioral psychology to philosophy but with internal problems surrounding specific behavioral definitions and their application to complex human behavior.

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[62] Storer 1948 contains a fairly exhaustive list of the many of reviews and essays surrounding the publication of SLB.

While Black's approach is to take issue with Morris's primitive and more complex definitions of behavioral terms, Storer's critique of Morris's project focuses on the relationship between behavioral psychology and philosophy and should be seen as indicative of the external approach. Storer argues that even a "completely developed behavioristic" theory of language would only amount to a "rather minor clarification in the methodology of psychology" (Storer 1948, p. 316). This is not to say that psychology contributes nothing to philosophy but that a specifically *philosophical* contribution to the study of language falls outside a science of signs. Philosophical contributions are distinct from the "formal or factual sciences" in that they explicitly focus on epistemology or epistemological considerations (Storer 1948, p. 319). By invoking "epistemology" in this context Storer is arguing that philosophy is "a precursive investigation, a preliminary inquiry, that anticipates the current level of scientific discovery and common sense opinion" which amounts to an early Carnapian commitment[63] to understanding the job of philosophy as the "construction (or reconstruction) of reality" (Storer 1948, p. 319).

Given his definition of philosophy, Storer's critique of SLB turns on the idea that Morris's work should properly be construed by "its relation to a rather specialized section of behavior psychology" and that such investigations necessarily presupposes philosophical analysis (Storer 1948, p. 318 – 9). Despite Storer's negative reaction to SLB and his antecedent commitment to philosophy as epistemology, this does not imply that he thinks linguistic analysis plays no role in philosophy. Storer claims that the

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[63] Storer specifically invokes his commitment to logical positivism in his description of philosophy. His inherited version of positivism from Gustav Bergmann clearly places his positive understanding of the movement somewhere between Carnap's *Aufbau* and *Syntax*.

relationship between the world, language, and perceivers[64] must ultimately be understood "only by considering how the linguistic elements fit with the non-linguistic elements into the pattern of the 'common-sense world'" (Storer 1948, p. 326). By "common-sense world" Storer means "a world where there are organisms behaving according to a standard pattern" (Storer 1948, p. 326). As a reaction to Morris's scientifically based study of language, Storer's conception of philosophy and linguistic analysis manages to carve a path for uniquely philosophical contributions to the study of language without opening the door for "metaphysical discourse". Although Storer's conception of philosophy is not Sellars's, there is an important link between his conception of the proper role of linguistic analysis and Sellars's more psychologically inclined approach to the philosophy of language. This connection will be taken up at the end of this section.

Given the then current debate over the relationship between psychology and philosophy, one finds Sellars endorsing, in a sense to be determined, a position somewhere between those of Storer, Morris, and Black that should be situated between the internal and external approaches sketched above. Sellars frames his approach to "rule-following" as an attempt to construct a place for a philosophical analysis between approaches classified as "rationalistic apriorism" and "descriptivism" (Sellars 1949, p. 117). Sellars's approach to explaining the relationship between behavior and standards or norms is to find a place between positions that assert the existence of a "domain of non-empirical qualities and a corresponding apparatus of acts and intuitions" and a position that claims all such concepts are either empirical (and thus fall under the domain of the sciences) or are pseudo-concepts or problems (Sellars 1949, p. 117-118). More

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[64] Storer is vague on how to classify this third element. He wants to avoid language that posits the existence of a self (an inherited concern from Gustav Bergmann). He simply uses the term 'I' but 'perceiver' seemed just as, if not more, neutral in this case.

specifically, descriptivism in this case is defined as the claim "that all meaningful concepts and problems belong to the empirical or descriptive sciences, including the sciences of human behavior" (Sellars 1949, p. 117).

In a suitably broad sense, Sellars's notions of psychologism and descriptivism line up fairly well. It should be remembered that Sellars's earlier conception of psychologism is broader than traditional notions of psychologism construed as the confusion of laws of thought for laws of logic.[65] Instead, Sellars offers a broader notion of psychologism as the "mistake of identifying philosophical categories with those of psychology" (Sellars 1947a, p. 4). Although descriptivism is clearly the broader notion of identifying *any* meaningful category with those of the sciences, Sellars's earlier notion of psychologism is indicative of the same concern. The upshot is that Sellars's concern, as expressed by both concepts, is the abandonment of any specifically philosophical contribution to the understanding of the mind and language in favor of a wholly scientific approach.

It is clear that Sellars wants to avoid this kind of descriptivism. In the context of "fixing" pragmatism, Sellars states at the beginning of LRB that "Now it will be my contention in this paper that a sound pragmatism must reject descriptivism in all areas of philosophy, and that it can do so without giving one jot or tittle of comfort to what has so aptly been called the new Failure of Nerve" (Sellars 1949, p. 119). Sellars's passing reference to Sydney Hook's article "The New Failure of Nerve" is somewhat telling in regards to his overall philosophical orientation. Hook's concern is to champion a broadly liberal picture of society that embraces a scientific worldview juxtaposed against

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[65] Since writing chapter 2 I have run across passages in Edmund Husserl's *Logical Investigations* that are remarkably similar to Sellars's early definition of psychologism. Considering that Sellars both read widely in the history of philosophy and studied under Marvin Farber it would not be surprising if Husserl, and not a misreading of Carnap or Frege as Carus suggests, is actually the source for this broader conception of psychologism.

approaches that represent a "flight from responsibility" (Hook 1943, p. 73). Amongst

many other positions, Hook counts "the refurbishing of theological and metaphysical

dogmas" as one of the approaches that signals a societies' sliding back into a reliance on

something akin to supernaturalism (Hook 1943, p. 74). In the context of LRB, Sellars's

mentioning of Hook's article signals that he is interested in carving a path for

philosophical contributions that avoids the extremes of both scientism and metaphysical

excess.

Concerning rules and rule-regulated behavior, Sellars explicitly argues that "to

urge that these are questions for the empirical psychologist to answer, and that we must

wait upon his convenience, is to leave the field of cognitive and moral psychology to the

rationalist" (Sellars 1949, p. 124). As opposed to his earlier essays, Sellars is working

with an undefined notion of what would now count as a uniquely philosophical

contribution to the study of language. Although Sellars never offers an explicit definition

of philosophy in LRB, there is one passage that provides a hint as to what he had in mind.

While warning empiricists away from giving in to the rationalistic tendency of hijacking

common sense intuitions about the mind, Sellars asserts that:

> Common sense also contains cues which, when combined with the
> achievements to date of empirical psychology, can be developed into the
> outlines of an adequate psychology of rational behavior, and to do this is
> an urgent task for the embattled empiricist. In thus reconstructing common
> sense psychology, the empiricist will find that the outcome shows more
> structural kinship with the pseudo-psychologies of the rationalist than with
> much that passes today for psychology among empiricists. (Sellars 1949,
> p. 124).

What Sellars seems to have in mind here is that a genuinely *philosophical* contribution to

the study of language would be found in a construction or reconstruction of the common

sense picture of rational behavior with developments in empirical psychology.

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The problem in regards to Sellars's methodological shift is whether it violates his earlier opposition to the invasion of philosophy by psychologism. At first glance, his approach in LRB seems to relinquish this stark opposition to psychologism by frequently invoking psychology as the field for the study of linguistic behavior. Statements such as "I suspect that *my trouble with the concept of a rule is in large part due to my ignorance of the psychology of the higher processes*" certainly makes it sound as if Sellars gave into the kind of psychologism he argued against in his earlier publications (Sellars 1949, p. 122). If this was where the story ended then, indeed, Sellars would be running afoul of his previous opposition to psychologism. As pointed out above, Sellars clearly wants to avoid the kind of descriptivism that would abandon discussion of linguistic behavior to psychology or any other branch of the empirical sciences.

What seems to save Sellars's account from contradicting his earlier opposition to psychologism would be the idea that he is concerned with the development of "the outlines of an adequate psychology of *rational* behavior" (Sellars 1949, p. 124). That is, *the uniquely philosophical contribution to the study of language turns on a reconstruction of common sense conceptions of standards or norms that are used to "justify a piece of conduct"* (Sellars 1949, p. 120). One can find evidence for this interpretation in Sellars's move to suggest that "the distinction between tied behavior and free, rule-regulated symbol activity, whatever they may have in common, is a *fact of experience*" (Sellars 1949, p. 123). It is the reconstruction of these facts of experience that constitute the proper domain of philosophical analysis. Although this indicates that Sellars's conception of philosophy shifted from his earlier commitment, there would

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seem to be no clear slide into the kind of psychologism that Sellars argued against in his 1947-8 publications.

Given these considerations it seems clear that Sellars was not lockstep in line with Morris's project of developing semiotics as a science of linguistic behavior. It is not as if Sellars was inherently hostile towards behavioristic models of language use; in fact, Sellars should be seen as siding with Morris (*contra* Storer's position) concerning his emphasis on the importance of "behavioristics" to philosophy. Prior to Sellars's later discussions of behaviorism and his distinction between methodological and substantive models of behaviorism, one finds Sellars endorsing a behavioristic model that is strikingly similar to Morris's project in SLB. In Sellars's own words:

> Not that I should deny for one moment that animal learning theory
> provides the key to all psychological phenomena. On the contrary I am
> convinced that this is the case. And not that I should deny that the laws of
> animal learning (if we had them) would explain even the mathematician's
> behavior in developing alternative postulate sets for n-dimensional
> geometries. I am even prepared to endorse this promissory note. (Sellars
> 1949, p. 123).

Sellars and Morris converge on a scientific, behavioristically grounded study of language that endorses the possibility of explaining human linguistic behavior on the model of animal learning. Where the two philosophers differ significantly is on the contribution of philosophical analysis; Morris is more than willing to allow, if somewhat vaguely, metaphysical speculation, whereas Sellars (remembering his passing reference to Hook's article) is somewhat more hesitant to embrace said speculation. In any event, the main difference between both approaches to the study of language turns on differing conceptions of philosophy but *not* differing conceptions of the value of behaviorism to

philosophy. Sellars shows no hesitancy towards embracing the possibility of a *fully developed* behaviorism capturing the totality of human linguistic behavior.

Somewhat oddly, Sellars's reflections on this period do not square with his published remarks. Commenting on his time at Iowa, Sellars claims that "The idea that something like S-R-reinforcement learning theory could provide a bridge between white rat behavior and characteristically human behavior was a tempting one, but I could see no way of cashing it out in the philosophy of mind. In particular, I could not see how to relate it to the intentionality which I continued to think of as the essential trait of the mental" (Sellars 1975, online). While this quote is indicative of Sellars's attitude towards the importance of philosophical contributions in the philosophy of mind and language, it would seem that he is misremembering his published remarks on behaviorism. The promise of a fully developed behaviorism as represented by Morris in SLB would seem to be a temptation that Sellars, in fact, embraced despite his later reflections on this period.

As to Black's critique of Morris's behavioral definitions, here too Sellars seems quite comfortable with Morris's approach to the issue as found in SLB. Specifically, Sellars claims that "much of what (among philosophers) passes for tough-minded psychology is an over-simplified extension to the higher processes of the *dog-fingersnap-sit-up-sugar* schema of tied responses to environmental stimuli. Not that I should deny for one moment that animal learning theory provides the key to all psychological phenomena" (Sellars 1949, p. 123). Sellars's issue is with what would constitute a philosophical contribution to the study of language and not, *ala* Black's critique of

Morris, an issue with the behavioral definitions of animal learning as applied to human linguistic behavior.

Sellars's connection to Storer's conception of behavior and language should be apparent. It is worth noting that Storer ranks as the only early graduate student mentioned in Sellars's "Autobiographical Reflections"; Sellars remarks: "We soon had some first-rate graduate students. Among the earliest and best was Thomas Storer, whose death at an early age was a genuine loss to philosophy" (Sellars 1975, online). On top of that fact that Storer was one of Sellars's and Bergmann's students and that Sellars turned to Storer for reviews of his own work, there is a similarity in approach to understanding linguistic behavior from a philosophical perspective. When attempting to define a positive role for philosophical analysis in relation to linguistic behavior, Storer and Sellars both claim that the intersection between rules, behavior, and the world are a proper subject for philosophical, as opposed to psychological, investigation. While Sellars discusses the "meshing of rule-regulated symbol activity" with environmentally tied behavior (Sellars 1949, p. 125), Storer emphasizes analyzing the how the human linguistic and non-linguistic elements fit together with standard patterns of behavior. While it is not necessarily the case that both philosophers have the same idea in mind, it seems clear that Sellars is tacitly invoking Storer's discussion of Morris's work. Both philosophers are in agreement that *the place for genuine philosophical contribution in the study of language rests on examining how language relates to norms of behavior*. How, exactly, Sellars fleshes this out will be dealt with in section II.

On the other hand, there are significant differences between Storer's approach and Sellars's own. Storer seems to think that Morris's semiotic would have little to no

relevance for philosophical analysis. As the passages previously quoted from LRB show, Sellars is quite confident that a behavioristically oriented psychology, combined with a reconstruction of "common sense" views of rational behavior, is relevant to philosophy.

Even if Sellars's "new" conception of philosophy manages to avoid falling prey to the kind of psychologism his earlier conception tried to avoid, much remains to be fleshed out concerning what concepts survive this transition and what roles they play in his overall philosophical system in the early 1950s. Sellars's "new" notion of what could count as a properly philosophical problem is left ambiguous at best. Clearly his commitment to conformation rules and a coherence theory of meaning survives the transition and plays a prominent role in his post-pure pragmatics writings. Both concepts appear in LRB (Sellars 1949, p. 124 – 126) and are continually invoked, in one guise or another, throughout Sellars's career. However, Sellars's discussion of rules in LRB seems drastically different from his discussions of rules in his earliest publications. And it is here, spurred on by the shift in philosophical orientation, that the entering wedge for a normative conception of rules and language is to be found.

II. Rules

Sellars famously describes linguistic objects as "fraught with ought" (Sellars 1962, p. 212) and insisted on a conception of language and rules that stressed the necessity of the normative. Such insistence does not find its expression in Sellars's earliest discussions on pure pragmatics but, as I have previously suggested, comes to the forefront of Sellars's thinking in his shift *away* from a formalist orientation. Thus, one finds Sellars claiming in LRB that "A rule, on the other hand, finds its expression either in what are classified as non-declarative grammatical forms, *or else in declarative*

sentences with certain special terms such as 'correct,' 'proper,' 'right,' etc… that serve

to distinguish them from generalizations" (Sellars 1949, p. 123). This formulation of

linguistic rules would come to dominate Sellars's philosophy for the rest of his career and

signals his adherence to a strongly normative conception of language. In context, such a

conception represents a radical break from the then dominant conceptions of linguistic

rules and language inspired, in part, by the emergence of logical positivism.

Sellars sets his discussion of rules and rule-following against the background of two

related positions: The then generally accepted definition of a linguistic rule and the

emotive theory in ethics; both of them are broadly aligned with logical positivism[66]. The

logical positivists' conception of a linguistic rule, as grounded in Carnap's *Syntax* and

Introduction to Formal Semantics, served to encompass a variety of rules posited by

Carnap and others, including formation, transformation, and designation rules of a

language. Specifically, Carnap claims that "the rules of a semantical system S constitute,

as we shall see, nothing else than a definition of certain semantical concepts with respect

to S, e.g. 'designation in S' or 'true in S'. Pure Semantics consists of definitions of this

kind and their consequences" (Carnap 1942, p. 12). Linguistic rules are definitions and

their consequences and have nothing like the prescriptive terms argued for by Sellars.

Again, in terms of pure semantics Carnap remarks, "Here we lay down definitions of

certain concepts, usually in the form of rules, and study the analytic consequences of

these definitions. In choosing the rules we are entirely free" (Carnap 1942, p. 13).

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[66] I realize that talking about logical positivism in a general sense is somewhat of a historical *faux pas* at this stage of the secondary literature surrounding the movement. Nonetheless, at this stage I am simply following Sellars's framing of the issues in LRB. The later sections of this chapter should provide a more concrete target in Carnap. That being said, the general views above should not be attributed to each and every individuals described as a logical positivist at one point or another.

Emotivism as an ethical position or theory arose out of the logical positivist's critique of normative ethics and traditional attempts to analyze normative ethical terms into either factual terms or non-reducible, non-empirical terms. Emotivists argued that the proper analysis of such normative terms as "ought" or "right" added nothing over and above the factual content of any given statement. Ayer's formulation of the emotivist position in *Language, Truth, and Logic* offers the logical positivists' canonical take on normative terms:

> We begin by admitting that the fundamental ethical concepts are unanalyzable, inasmuch as there is no criterion by which one can test the validity of the judgments in which they occur…We say that the reason why they are unanalyzable is that they are mere pseudo-concepts. The presence of an ethical symbol in a proposition adds nothing to its factual content. Thus if I say to someone, 'You acted wrongly in stealing that money,' I am not stating anything more than if I had simply said, 'You stole that money.' In adding that this action is wrong I am not making any further statement about it. I am simply evincing my moral disapproval of it. (Ayer 1936, p. 107).

An emotivist reading of normative ethical terms such as "ought" or "right" is committed to the idea that such terms simply *express* moral approval or disapproval and fulfill no "special" role, in Sellars's terminology, that would need to be accounted for by either a reductive or non-reductive account of such terms.

Sellars's account of language in LRB is set against both of these positions in an effort to clarify the then contemporary understanding of rules and rule-regulated behavior. This is not to say that Sellars is arguing for a linguistic or ethical position closer to that of rationalism or intuitionism. As was argued for in section I, Sellars is clear that his approach in LRB is to carve out a specific understanding of rules and rule-regulated behavior that would avoid the excesses of either admitting a realm of non-empirical qualities or abandoning the entire project to the special sciences. And again, as noted

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above, it is the carving of a path between these two positions that allows for Sellars to construct a strongly normative picture of linguistic rules.

It is not altogether clear that Sellars was always committed to a strongly normative conception of rules prior to the shift in philosophical orientation described above. One could argue that there are at least two possible locations for a normative conception of language or rules to reside in Sellars's formalist essays: in either his conception of conformation rules as *restrictions* on a calculus or somewhere in the relationship between a language schema and a language proper. Sellars's later emphasis on the "normative flavor" of rules (Sellars 1953, p. 230) is absent in his discussion of conformation rules. Nonetheless, one could make the argument that such rules placing a *restriction* on a given calculus would be enough to constitute a *weak* sense of normativity – that is, such restrictions could count as norms of a given formal language but not as norms peppered with "'ought', or 'ought not'" (Sellars 1953, p. 230). This suggestion would agree with the periodization of Sellars's work present in my account of his early papers but deny that Sellars's formal papers contained a strictly non-normative conception of rules.

I think this interpretation works only if one reads too much into conformation rules as "restrictions" on a calculus and ignores then contemporary moves similar to Sellars's introduction of conformation rules. One finds Storer introducing similar restrictions on a language as a norm of said language without including anything like the "normative flavor" of such a requirement. In discussing the relationship between semiotic and philosophy Storer introduces two possible restrictions on a language: a norm of science and his somewhat idiosyncratic reading of the positivist meaning criterion. Storer's first restriction is "Do not introduce into language predicates unless these predicates refer to

qualities that have been experienced or can be defined in terms of such predicates"

(Storer 1948, p. 325). This norm, much like Sellars's conformation rules, restricts the

introduction of predicates into a language as well as their possible combinations. In his

own reading of the positivist meaning criterion, Storer insists that that it "is a meta-

linguistic formation rule *restricting* the possibilities of combination for a language"

(Storer 1948, p. 326). Despite the presence of the term "restrictions" in Storer's

description of norms for a given language or calculus, there is no sense in which Storer is

conveying an inherently normative conception of rules *that goes above and beyond the*

positivist understanding of rules as shorthand for explicit definitions. In other words,

Storer's conception of formation and transformation rules of a given language does not

deviate from what one finds in Carnap's texts; there is no prescriptive component to

Storer's conception of a rule.

It is not even clear that Sellars, at least in LRB, thinks that *all* linguistic rules

would need to be understood as normative. Though some of his later publications will

focus on the idea that Carnap's conception of a rule is incomplete because it does not

account for the normative dimension of rules, it is not the case that one finds such a

strong emphasis on this dimension of language in Sellars's LRB remarks. Sellars's later

critique of Carnap will turn on the idea that his notion of assertion and of transformation

rules are devoid of the normative terms that are essential to a rule that functions as a rule.

At least in parts of LRB Sellars seems to accept a more traditional notion of formation

and transformation rules, suggesting that "a language must have conformation rules as

well as *the familiar rules of formation and analytic inference*" (Sellars 1949, p. 131).

Another way to look at this might be to reflect on the role of rules in Sellars's earliest publications. As explained in chapter two, Sellars accepts the Carnapian notion of formation and transformation rules for a given calculus or semantical system. Conformation rules (the defining rules of pure pragmatics) are presented as presupposing the usual formation and transformation rules of pure semantics. *Conformation rules are introduced are supplementing the already existing rules of formation and transformation.* Given that Sellars explicitly introduces conformation rules in this fashion, it is difficult to see how one could read him as holding a significantly different notion of a rule.

Although Sellars will later (in IM) insist that a rule is necessarily always a rule for doing something, I would argue that this sense of a rule, while certainly correct, is not present in Sellars's earliest papers. Even though Sellars will take Carnap to task for the absence of reference to actions in his rule schema, this concern does not appear in Sellars's earliest publications. If Sellars was willing to accept "the usual" rules of formation and analytic inference I would suggest that there is nothing in "the usual" conception of these rules that makes reference to an action[67].

Granted, in PPE Sellars initially introduces P-restrictions on a calculus as "a certain type of restriction on the predicates of the calculus in which such a system can be *formulated*" (1947a, p.11). Clearly, formulating a calculus is an action and if conformation rules as restrictions on a calculus, then rules are rules for guiding behavior (specifically the behavior of symbol manipulation). This should be read as a meta-comment on the rules themselves; there is nothing inherently different from Carnap's

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[67] Granted, one could read a reference to actions into Carnap's notion of a formation or transformation rule. He clearly does not intend that to be the case. For example, take formation rules; Carnap presents them as the definition of "Sentence in S". It's difficult to see why such a definition would need to make reference to some action. Though one *could* construction formation rules as prescriptions for actions, I would argue that Carnap clearly did not think of them this way.

conception of a rule to be found in Sellars's notion of conformation rules (either in their negative or positive phase). Although they are presented as restrictions on a given calculus, I have tried to show through Storer's work that such restriction need not be read as guides for actions.

Given Sellars's strict adherence to offering a *formal* analysis it is unclear as to whether rules could be rules for guiding behavior. Cannot one read Sellars's discussion of P-restrictions as a necessary dimension for characterizing empirically meaningful languages and not for enforcing behavior of one kind of another? Now, it may be the case that lurking in the background of Sellars's conformation rules is uneasiness with the abstract notion of a rule as found in pure semantics; I am not sure that I want to necessarily deny that interpretation. It may be the case that by 1947 Sellars was already uncomfortable with thinking about rules as simply definitions and their analytic consequences. When I suggest that Sellars's earliest papers exhibited a *deflated* sense of normativity I have three things in mind; 1) that the introduction of conformation rules as a necessary component for characterizing an empirically meaningful language did not represent a rejection of Carnap's conception of a rule, 2) even if, on closer inspection, all notions of rules as *restrictions* are rules for prohibiting actions or behavior, it is not clear that Sellars can reconcile that fact with his reliance on a formal analysis of language that is necessarily distinct (on his own model) from symbol behavior, and 3) there was no reference to the "prescriptive dimension" of rules one finds in the later Sellars.

The point is that there was a *deflated* sense of "norm" in use that did not imply anything like Sellars's later introduction of the normative flavor of rules that necessarily includes something over and above definitions and their analytic consequences. One

could argue, and I think perhaps correctly, that this deflated sense of norm or normative is present in Sellars's earliest papers (much like they are present in Storer's work) but that such a conception of the norms of language falls significantly short of Sellars's later insistence on a *strong* sense of the normativity of rules. *In other words, what seems to be missing from Sellars's early papers is a conception of formation and transformation rules that are normatively binding in the way he presents linguistic rules in the 1950s* (as will be discussed below). To read more than this into Sellars's use of conformation rules as restrictions on a calculus smacks of presentism at its best.

As was discussed in the previous chapter, the second option is not really a live one for Sellars. One could reconstruct Sellars's earlier position along normativist lines such that a language proper would act as a standard or norm for symbol behavior occurring in the very human language schemas that represent language in actual use. Sellars is quite clear that this is *not* what he has in mind when drawing a distinction between a language proper and language schema for two different reasons. First, Sellars claims that although it would possibly be clarifying to claim "that as language *schema*, its nature is to be clarified and criticized with reference to the standard provided by a language proper" this would be misleading (Sellars 1947a, p. 19). One can never confront a language schema with a language proper but, instead, simply with another language schema. Given this stipulation, it would seem difficult to construe how a language proper could hold normative force over schematic language use.

Since Sellars constructs the distinction between a language proper and a language schema as a factual/psychological distinction as opposed to a formal one, it must be understood as relating "to the psychology of formal manipulations, and can no more be

formulated within formal science itself than can the concept of a *mistake*" (Sellars 1948a, p. 76 – 77). *If one cannot formulate the concept of a mistake within formal sciences, how could one construct a notion of correctness or incorrectness within a philosophical language*? Given Sellars's barring of the formulation of a mistake and the distinction between language proper and language schema within formal sciences, and given that this occurs during the period in which Sellars asserts "philosophy is pure formalism; pure theory of *language*" (Sellars 1947a, p. 25), the intersection of language proper and language schema cannot be the source for the normative force of linguistic rules in Sellars's early publications.

Sellars initially constructs his normative account of linguistic rules from an analogy with moral reasoning. Specifically, Sellars is concerned with how the subsuming of a piece of conduct under a moral law is distinguished from a law of nature "by the use of the word 'ought' in its categorical sense" (Sellars 1949, p. 120). Sellars wants to distance his views from those of the emotivist, arguing that "I find the emotive theory of moral obligation as unacceptable as would be an emotive theory of logical necessity, or (*pace* Hume) an emotive theory of physical necessity" (Sellars 1949, p. 120). This is not to deny the insight that normative terms play an expressive role, but what they express are "an *observance of a rule*" as opposed to the psychological states of an agent (Sellars 1949, p. 120). Sellars's motivation for this move is, in part, something akin to incorporating "Kant's assimilation of the ethical 'ought' to the logical and physical 'musts,'"…"because I have increasingly been led to assimilate the logical and physical 'musts' to the ethical 'ought'" (Sellars 1949, p. 120).

One way Sellars adopts Kant's idea of the structural similarity between ethical and logical terms is by exploiting his distinction between actions that conform to duty and actions that occur because of a duty. In Kant's *Groundwork for the Metaphysics of Moral* one finds the relevant passage Sellars has in mind in Kant's discussion of the moral obligation to preserve one's life:

> On the other hand, to preserve one's life is a duty, and besides everyone has an immediate inclination to do so. But on this account the often anxious care that most people take of it still has no inner worth and their maxim has no moral content. They look after their lives *in conformity with duty* but not *from duty*. (Kant 1785, p. 11).

Kant's ethical insight is to draw a distinction between actions which merely conform to one's obligation and actions that done because of one's obligations. Though this is not the only point at which Sellars exploits various facets of Kant's take on moral reasoning, the above distinction is the vehicle Sellars uses to separate human from animal behavior.

The Kantian distinction between actions that *conform* to a rule and actions that occur *because* of a rule is what allows Sellars to draw a clear distinction between animal behavior that conforms to rule-following behavior and high-level human behavior that should more properly be classified as rule-following. The idea is that a description of animal behavior as rule-following is, strictly speaking, misleading; although an animal's actions may conform to a generalization or a rule there is no reason to think that such any animal actions occur *because* of a rule.   Such instances of animal behavior would fail to participate in the idea that "a rule isn't functioning as a rule unless it is in some sense internal to action" (Sellars 1949, p. 122). On the behaviorist model of animal learning to which Sellars subscribes, there is nothing particularly "internal" about the generalization or rule used to categorize the animal's actions. As Sellars remarks: "Thus, if I train an

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animal to sit up when I snap my fingers, the animal's behavior conforms to the generalization 'This animal sits up when my fingers snap,' but we should scarcely say that the animal acts on the rule of sitting up when I snap my fingers" (Sellars 1949, p. 122). Instead of thinking of such pieces of behavior as rule-following, Sellars suggests that they are essentially "learned habits of response" that are "inextricably tied to the environment in a way in which much characteristically human behavior is not" despite the fact that such behavior can be characterized by generalizations that share the same logical form as genuine rules (Sellars 1949, p. 122).

The consequence of this move, the move to disqualify animal behavior as a candidate for rule-following considerations, leads Sellars to claim that "the type of activity which is rule-regulated is of a higher level than that which is produced by simply animal learning procedures" (Sellars 1949, p. 122). At this stage in Sellars's argument, what distinguishes human, rule-regulated behavior from animal behavior is that characteristically human actions "may well be characterized as *free*" (Sellars 1949, p. 122). That is, human rule-regulated actions are not inextricably tied to environmental cues, and are thus not simply learned responses to stimuli in the way animal behavior is inextricably tied to environment cues. This is not to suggest that rule-regulated human activity is necessarily severed from animal learning procedures; Sellars claims that "Certainly, we learn habits of response to our environment in a way which is essentially identical with that in which the dog learns to sit up when I snap my fingers. And certainly these learned habits of response – though modifiable by rule-regulated symbol activity – remains the basic tie between all the complex rule-regulated symbol behavior which is the human mind in action" (Sellars 1949, p. 122). Despite our basis in learned habits of responses, what

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makes human behavior uniquely classified as rule-following as opposed to simply

conforming to a generalization is that it is not constituted by habits alone. In Sellars's

terms, "To say that man is a rational animal, is to say that man is a creature not of *habits*,

but of *rules*" (Sellars 1949, p. 123).

It's important to see that this kind of analogy would simply not be available if

expressed in formalist garb. Looking back to Sellars's attempts at constructing a pure

pragmatics of language, it is difficult to see how the analogy between subsuming a

pattern of behavior under a moral law and subsuming a pattern of behavior under a

linguistic rule would be permissible in pure pragmatics. This is not to say that no

philosopher, prior to Sellars's work, attempted to fit prescriptive terms into formalist

garb. Perhaps tellingly, Sellars's former student Thomas Storer made an attempt to do

just this in his article "The Logic of Value Imperatives" one year prior to Sellars's first

publication. Storer's concerns with formalizing imperatives are not Sellars's concerns; by

the time LRB was published Sellars seems to have ceased considering such problems in

the formal mode of speech.

After framing the issue through the lens of behaviorism combined with insights from

Kantian moral theory, Sellars gives a lengthy "argument"[68] for what constitutes rules.

Sellars's normative conception of rules turns on a number of different points:

> A rule, existing in its proper element, has the logical form of a
> generalization. Yet a rule is not *merely* a generalization which is
> formulated in the language of intra-organic process. Such a generalization
> would find its overt expression in a declarative sentence. A rule, on the
> other hand, finds its expression either in what are classified as non-
> declarative grammatical forms, or else in declarative sentences with

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[68] It is suspect to call much of the early parts of LRB "arguments". Sellars seemed quite aware of this fact, commenting "If what I have just said appears to be rhetoric and not philosophy, I can only plead that it ought to be psychology" (Sellars 1949, p. 123). Most of LRB reads as a list of claims, sometimes squared against other philosophical positions, but not necessarily arguments.

certain special terms such as 'correct,' 'proper,' 'right,' etc., serving to distinguish them from generalizations. What do these special features in the formulation of rules indicate? They give expression to the fact that a rule is an embodied generalization, which to speak loosely but suggestively, tends to make itself true. Better, it tends to inhibit the occurrence of such events as would falsify it – if it weren't already false, that is, for the generalizations which lie at the core of rules are rarely if ever true, and unless they *could* (logical or physical possibility) be false, they could scarcely function as rules. (Sellars 1949, p. 123).

Initially, what sets rules apart from mere generalizations is the inclusion of "special" terms such as 'ought', where "special" here is indicating that they play a role over and above strictly descriptive statements or terms. Specifically, such normative terms indicate the permissible or impermissible behavior in regards to the generalization the rule is based on. As emotivists claim, this does not necessarily entail that they contain anything over and above the factual content of a sentence – they could simply being expressing the approval or disapproval of the utterer.

Sellars clearly thinks normative terms express *more* than the factual content of declarative sentences. The normative force of rules comes from the fact that terms such as "ought" express rules that act to restrict actions surrounding "embodied generalizations" – that is, generalizations necessarily internal to action – and that such terms tend to make the generalizations they are based on true or inhibit events that would contradict said generalization. Sellars's example of this is the moral rule "'One ought to tell the truth'" which he asserts is based on the generalization "'People always say what they believe'" (Sellars 1949, p. 124). The idea is that even though the generalization is false, the normative force of the rule "One ought to tell the truth" is derived from the fact that such a rule acts to constrain or guide behavior in the interest of making the generalization true or prohibiting it from being falsified.

This portrayal of rules manages to avoid the emotivist's account of normative terms as essentially expressive terms by offering an alternate explanation of the function of such terms. Sellars does not provide a critique of emotivist theories of moral terms over and above suggesting that they do not capture the "phenomenology of moral thought and experience" (Sellars 1949, p. 120). Although no explicit critique of Carnap's definition of a rule is to be found in LRB, his normative account clearly assigns significantly more weight to the "rulishness" of rules than Carnap's understanding of the concept. On Sellars's reading, the main disagreement between himself and emotivist accounts of normative terms does not concern whether such terms are necessarily expressive but turns on exactly what they express. Sellars's account differs in that such terms indicate the observance of a rule that functions to prohibit or enforce the generalization that such rules are built upon.

In the context of a then still domain logical positivism, one might wonder why Carnap or the emotivist would feel any pressure to respond to Sellars's notion of a rule and rule-regulated behavior. In fact, barring the inclusion of arguments from Sellars's later publications, it seems quite clear that what is missing from LRB is a critique or argument to the effect that the logical positivist notion of linguistic rules or normative terms should be considered wrong for specific reasons. What Sellars relies on in LRB to do the arguing against logical positivism is the intuition that there is, in fact, an inherently normative dimension to moral rules that could or should be carried over into deliberations about linguistic rules and rule-following behavior. Especially from the perspective of the emotivist, there is no reason to accept the analogy between the place of normativity in

moral rules and the place of normativity in linguistic rules. The only way such an analogy even gets off the ground is if one accepts the normativity of moral rules in the first place.

So, as a specific critique of logical positivism, Sellars's position in LRB falls a bit short of the mark. It is not that Sellars does not develop, to some degree, his own account of rules that emphasizes the normative dimension of language (he does), but that he offers no reason to suspect that emotivism is wrong or that Carnap's conception of linguistic rules is somehow off target. I am not suggesting that such ideas are not implicit in Sellars's alternate account of rules and rule-regulated behavior or that arguments to this effect do not appear in Sellars's later publications. My only point is that, as far as LRB goes, Sellars's initial account of the normativity of rules should be read as relatively weak in comparison to competing positions of the time because Sellars offers no concrete reasons to suspect that such positions are wrong.

Although LRB represents Sellars's initial attempt to construct a rival conception of rules, he falls somewhat short of offering a full-blown critique of Carnap's conception of the various linguistic rules found in *Introduction to Formal Semantics* or his other texts. Certainly, if one buys into Sellars's conception of rules there is a significant gap between logical positivism and a normative conception of rules. In this early piece Sellars does not offer an explicit critique of Carnap's conception of a rule. His project here "is to stimulate those philosophers who are always talking about rules – usually rules of language – to explain more fully what they have in mind" and to develop his own account of how to properly characterize rule-regulated behavior(Sellars 1949, p. 124). Although an implicit critique of the logical positivists' conception of a rule may be contained within LRB, Sellars's explicit criticism of Carnap's conception of a rule appears in his

later publications. Ultimately, what LRB represents is Sellars's shift away from a formalist model of philosophy and his initial foray into the construction of a normative account of rules and rule-regulated behavior that is incomplete.

III. Sellars's Critique of Carnap

None of this is to say that Sellars's early papers have no bearing on Carnap's philosophy or that there is nothing useful in understanding them in relation to logical empiricism – the story is significantly more complicated than that. As noted above, the arguments found in LRB are situated against emotivist and intuitionist treatments of normative terms; Sellars's project in LRB is to offer a rival understanding of rules and rule-regulated behavior and not to raise numerous problems with the positivists' conception of linguistic rules. Sellars's 1953 article "Inference and Meaning" (hereafter IM) marks his first explicit critique of Carnap's conception of languages and linguistic rules. This later critique of Carnap sees Sellars's normative conception of rules expanded significantly to include formation and transformation rules of a language – a move, as I argued in section II, that seems to be missing in Sellars's earlier publications. Sellars's critique of Carnap focuses on the latter's understanding of the transformation rules of a language. Specifically, Sellars's critique centers on two related concerns: A.) the lack of normative force in Carnap's conception of transformation rules and B.) the lack of reference to actions in Carnap's conception of rules as definitions.

Sellars's starting point is to take issue with Carnap's characterization of transformation rules as "direct consequence in S". Specifically, Sellars claims that "by telling us that transformation rules can be formulated as definitions of 'direct consequence in S', Carnap gives the impression that *the force of a rule* to the effect that

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expressions of kind A can be 'transformed' into expressions of kind B relates solely to the existence of a structural relationship between these two kinds of expressions" (Sellars 1953, p. 230). On Sellars's reading of Carnap this would turn the normative force of transformation rules into a purely syntactical feature of a language that constitutes a structural relationship between the definiendum and definiens used to ground out a given transformation rule.

Again, as is the case with his arguments in LRB, Sellars's rival conception of a linguistic rule (and his critique of Carnap) turns on an understanding of normative terms that asserts the existence of "surplus meaning over and above" descriptive terms and leans heavily on an analogy between the function of linguistic and moral rules (Sellars 1953, p. 230). Sellars's example in IM is the moral rule "'Happiness ought to be maximized'" which he defines on a Carnapian schema as "'X is morally right = $_{Df}$ X maximizes human happiness'" (Sellars 1953, p. 230). Sellars argues that in the context of moral reasoning the wrongness of the above definition as a ground for the moral rule "Happiness ought to be maximized" is self-evident. On the face of the matter, one would deny that the left and right side of the definitional schema function as synonyms for each other and are, thus, not simply interchangeable. Even if the two terms had the same descriptive meaning, the definiendum "has a surplus meaning over and above these which can be indicated by the word 'ought'" (Sellars 1953, p. 230). That is, "X is morally right" should be read as containing a prescriptive conceptual component to the effect that it entails "One ought to do X".

If Carnap's understanding of a rule *qua* definitional schema "is, with any plausibility, to do the work of a rule" then it must account for the normative dimensions

of rules that are captured by such terms as "ought" or "ought not" (Sellars 1953, p. 230).

Sellars suggests that, as it stands, Carnap's construction of transformation rules as the

definition of the term "direct consequence" shares more of a structural kinship with

relational terms such as "next to" or "between" as opposed to more properly construed

definitions that would need to contain some term indicative of the "rulish force" of rules.

Without the inclusion of normative terms into Carnap's definitional schema for

transformation rules, he will be unable to capture the normative force of such rules. In

short, Sellars's first critique of Carnap shows that with only his definitional schema in

place, Carnap's conception of linguistic rules lacks the normative force necessary to

make them function as rules in a legitimate sense.

Sellars's second closely related critique of Carnap turns on the idea that "a rule is

always a rule for *doing* something" (Sellars 1953, p. 230). Specifically, "it is the

performance of this action (in specified circumstances) which is enjoined by the rule, and

which carries the flavor of ought" (Sellars 1953, p. 230-1). Sellars's concern here harks

back to his initial points in LRB that for a rule to function as a rule (and not merely a

generalization) it must be internal to action and that rule-regulated behavior is behavior

that occurs because of a rule and not merely in conformity with a rule. The significant

difference between this observation in LRB and what one finds in IM is that Sellars

directs this point specifically *against* Carnap's conception of a rule. Sellars admits that

one could read Carnap's definition of a rule as capturing the "action component" he

mentions by its use of "deriving" in transformation rules (such that one would be deriving

the right hand side of the definitional schema from the left). Such a reading would be "a

snare and a delusion" (Sellars 1953, p. 231). As Sellars remarks:

It is as though one offered the following 'definition' as a formulation of a basic rule governing the activities of policemen: 'X is arrestable $=_{Df}$ X has broken a law'. It is obvious that such a definition would be a mistake not only because the definiendum 'arrestable' has, as we saw, a normative force not shared by the definiens, but also because it designates an act, the act of arresting, which is not designated by the definiens. (Sellars 1953, p. 231).

Although Carnap's broader conception of transformation rules as permitting or restricting an *action* (namely deriving) would seem to solve one challenge raised by Sellars, this does not solve the overall problem. The problem is that both sides of the definitional schema are simply not interchangeable; as Sellars remarks, even on the broader notion discussed above, the normative and action-based dimensions of "arrestable" fail to be transferred over to the definiens. Even if such schemata are able to capture the action-based quality of rules (at least in the definiendum) they, at best, only "specify the circumstances in which a person is arrestable" as opposed to capturing a general rule for arresting someone (Sellars 1953, p. 231).

Perhaps oddly, Sellars's critique of Carnap here is similar to the move one finds Socrates making against Euthyphro in Plato's *Euthyphro*. Euthyphro's initial definition of piety failed precisely because he was, at best, specifying instances of piety or the circumstances for pious actions but not giving a definition of piety. Sellars's critique of Carnap echoes the same concern; Carnap's definitional schema simply fails to give an accurate definition of what counts as a linguistic rule. The upshot, in both Socrates's and Sellars's cases, is that Euthyphro and Carnap both mistakenly assume that they are giving a definition of the terms in question (piety in the case of the former, rules in the case of the latter). As it stands, the definitional schema that is at the heart of Carnap's definition of transformation rules, at best, specifies "the type of circumstances in which a certain

kind of action ought to be done; it cannot specify *that* it ought to be done" (Sellars 1953, p. 230).

On first approach, Sellars seems to badly misconstrue Carnap's overall conception of a language. In critiquing Carnap's conception of a rule, Sellars characterizes Carnap's position by claiming that:

> Indeed, he writes on occasion (e.g. p. 4) as though a language, formally considered, were identical with its syntactical rules, from which it would follow that the transformation rules of a language would be at least a part, and might – in the light of the passage we have quoted on the power of transformation rules to specify the syntactical structure of a language – be identical with the language.…Let me hasten to add that the identification of a calculus, or game or language, with its rules, though strictly a mistake, can be regarded as a paradoxical way of stating an important truth; and *I have dwelt on this matter only because Carnap's statement is symptomatic of a carelessness with the term 'rule' which pervades his otherwise admirably incisive and patiently meticulous argument*. (Sellars 1953, p. 229).

Initially, Sellars seems to be correctly parsing the difference between formally considered and natural occurring languages in the above quote, but his critique of Carnap's conception of language generally turns on Sellars's failure to respect this difference. In discussing the role for material rules of inference in a language, Sellars asserts that:

> To be sure, Carnap, in the above passage, is not discussing the syntax of natural languages, but rather the construction by logicians of artificial languages. *Yet he is clearly conceiving of these artificial languages as candidates for adoption by language users*. And presumably, an artificially construed calculus with an appropriate syntactical structure becomes a natural language by virtue of (1) the adoption of its syntactical rules by a language speaking community; (2) the association of certain of its descriptive terms with sensory cues. (Sellars 1953, p. 224).

So, on Sellars's reading of Carnap one mistake that is being made is the identification of a language with its rules. There are two related claims here: one about the way rules function in a language (broadly construed) and one about Carnap's

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conception of formal calculi. What allows Sellars to make claims about Carnap's

conception of a language as a whole is his running together of this distinction between

formal calculi and natural languages. It is Sellars's commitment to the idea that a logician

(or, more accurately, Carnap) working on a formal calculus is necessarily open to it as a

possible candidate for adoption by a given linguistic community that allows him to

critique Carnap's conception of language *writ large*.

Although initially correctly drawing the distinction between language *qua*

calculus and language *qua* natural language, Sellars runs these concerns together when

identifying a language with its rules. This move badly misconstrues Carnap's distinction;

Carnap is quite clear that there is a *significant* difference between the rules of a calculus

and possible rules of a natural language. In the beginning of the *Syntax*, Carnap quite

clearly asserts his commitment to differing approaches for artificial and natural

languages: "In consequence of the unsystematic and logically imperfect structure of the

natural word-languages (such as German or Latin), the statement of their formal rules of

formation and transformation would be so complicated that it would hardly be feasible in

practice" (Carnap 1937, p.2).

If Sellars's conflation of the distinction between natural and formally considered

languages is a genuinely bad misreading of Carnap, it is a slightly understandable one.

Later passages in Carnap's *Syntax* (generally sections 45 – 47, which Sellars quotes

heavily from in IM) may give a reader the impression that Carnap himself tends to

conflate this distinction. In discussing general considerations of formation and

transformation rules Carnap seems to broaden his scope of what counts as a language:

In this section we shall attempt to construct *a syntax for languages in*

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general, that is to say, a system of definitions of syntactical terms which are so comprehensive as to be applicable to any language whatsoever. [We have, it is true, had chiefly in mind as examples languages similar in their principle features to the usual symbolic languages, and, in many cases, the choice of the definitions has been influenced by this fact. Nevertheless, the terms defined are also applicable to languages of quite different kinds]. (Carnap 1937, p. 167.).

Such passages would seem to open the door for Sellars's reading of Carnap as considering artificial languages as candidates for adoption by language users. Only a page later, Carnap quickly closes this door by defining the *logical* character of natural languages as necessarily presupposing extra-syntactical and extra-linguistic factors (Carnap 1937, p. 168). This *completely* closes the door on Sellars's reading of Carnap; in discussing formation and transformation rules Carnap is quite clear that "In what follows, we shall deal *only with languages which* contain *no expressions dependent upon extra-linguistic factors*" (Carnap 1937, p. 168). And, I think, Carnap clearly conceives of semantical systems as "languages" in this sense. For Carnap, the role of formation and transformation rules will always depend upon pragmatical considerations that greatly limited in their relationship to formal calculi. Specifically, such rules of natural language could only concern "the *motivation* of our choice and has no bearing upon the correctness of our results of the analysis of the rules" of a formal language (Carnap 1942, p. 13). The overall point is that the formation and transformation rules of naturally occurring and artificially constructed languages (and what they involve) are *radically* different in Carnap's philosophy.

Although IM represents Sellars's first explicit clash with Carnap's definitional schema for rules, his later[69] publication "Empiricism and Abstract Entities" (hereafter

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[69] There is an open question as to exactly when this paper was written. Jeff Sicha suggests that it was in the early 1950s while A. W. Carus claims that it was significantly closer to the publication of EPM in 1956.

EAE) expands this critique to include Carnap's overall take on syntax, semantics, and meaning. The bulk of Sellars's essay focuses on Carnap's later works, especially "Empiricism, Semantics, and Ontology", and is concerned with the relationship between meaning or designation statements and abstract entities. Although such issues are not directly relevant to Sellars's account of rules and rule-regulated behavior *per se*, EAE does touch on and expand the critique of Carnap implicitly found in LRB and explicitly located in IM. More importantly, although Carnap never responded to Sellars's arguments in IM he did respond to similar points made in EAE.

After a discussion of Carnap's then more recent publications and W.V.O. Quine's entry into debates over ontology, Sellars frames his critique of Carnap in EAE in terms of Carnap's distinction between pure and descriptive semantics. Specifically, Sellars is concerned with how Carnap draws this distinction in *Introduction to Semantics*. Sellars's analysis leans heavily on two passages cobbled[70] together from Carnap's work:

> By descriptive semantics we mean the description and analysis of the semantical features either of some particular historically given language, e.g. French, or of all historically given languages in general…Thus, descriptive semantics describes facts; it is an empirical science. On the other hand, we may setup a system of semantical rules, whether in close connection with a historically given language or freely invented; we call this a semantical system. The construction and analysis of a semantical system is called *pure semantics*. (Carnap 1942, p. 11 – 12).

Aside from this initial gloss on the distinction, Carnap later elaborates on the relationship between descriptive syntax, semantics and pragmatics:

> Sometimes the question is discussed whether semantics and syntax are dependent upon pragmatics or not. The answer is that in one sense they are

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Unfortunately, neither author offers any evidence for either of these claims. The Archives of Scientific Philosophy at the University of Pittsburgh does list the initial typescripts and drafts of EAE as occurring in 1954.

[70] Although I have already elaborated on this point in chapter two it bears repeating given how often Sellars references these specific passages from the beginning parts of *Introduction to Semantics*.

but in another they are not. Descriptive semantics and syntax are indeed based on pragmatics...Only after finding by observation the pragmatical fact that [Eskimos] have the habit of using the word 'igloo' when they intend to refer to a house are we in a position to make the semantical statement "'igloo' means (designates) house" and the syntactical statement "'igloo' is a predicate." In this way all knowledge in the field of descriptive semantics and descriptive syntax is based upon previous knowledge in pragmatics. *Linguistics*…is the descriptive, empirical part of semiotics….hence it consists of pragmatics, descriptive semantics and descriptive syntax. But these three parts are not on the same level; *pragmatics is the basis of all linguistics*. However, this does not meant that, within linguistics, we must always explicitly refer to the users of the language in question. Once the semantical and syntactical features of a language have been found by way of pragmatics, we may turn out attention away from the users and restrict it to those semantical and syntactical features. Thus, e.g. the two statements mentioned before no longer contain explicit pragmatical references. In this way, descriptive semantics and syntax are, strictly speaking, parts of pragmatics. (Carnap 1942, p. 12 – 13).

The starting point for Sellars's critique is to ask two related question: 1) exactly what is meant by "empirical" in this instance of "empirical science"? and 2) What does Carnap mean by "descriptive" when discussing descriptive semantics? As to the first question, Sellars argues that "empirical" in this case is rather ambiguous. One could mean that a statement in descriptive semantics "is empirical in the broad sense if it is properly supported by reasons of an empirical, and, ultimately, of an observational character" (Sellars 1963, p. 264). On the other hand, one could mean that a statement in descriptive semantics "is empirical in the narrow sense, if it is empirical in the broad sense and, apart from logical terms in a suitably narrow sense, contains no concepts which could not, in principle, be constructed out of descriptive primitives" (Sellars 1963, p. 264).

In the quoted passages from Carnap above, it is not clear whether descriptive semantics is empirical in Sellars's broad or narrow sense of empirical. In fact, as Sellars

suggests, the notion of a descriptive statement is not unproblematic itself. Although

Carnap never explicitly defines what he means by "descriptive" aside from equating it

with empirical, Sellars's initial suggestion is that "a descriptive term is one which, in its

basic use, properly replaces one of the variables in the dialogue schema: 'What brought it

about that x is Φ? The fact that y is Ψ' where what is requested is a causal explanation"

(Sellars 1963a, p. 265). What Sellars wants to do here is to suggest that, in an

unproblematic fashion, one could admit that such notions of descriptive statements and

descriptive semantics could be empirical in the broad sense without being empirical in

the narrow sense.

Such considerations are important for Sellars's overall conception of semantics

and his critique of Carnap, because they are essentially the entering wedge for Sellars's

claims about the necessary aspects of the prescriptive components of a language – even a

wholly descriptive account of a language. After quoting Carnap's second elaboration of

how descriptive semantics and syntax presupposes pragmatics, Sellars argues:

> Now, if one takes the pragmatical study of an historical language (L) to
> eventuate in statements which are empirical in the narrow sense – if, for
> example, one takes it to be the behavioristic socio-psychology of language
> habits in a certain community – then no process of 'abstraction' will result
> in semantical or syntactical statements about L, or even in pragmatical
> statements about L…The point is most obvious in the case of syntactical
> statements about L, *for even if these latter are not prescriptive statements,
> they involve prescriptive concepts*. (Sellars 1963a, p. 266 – 7).

Sellars's remarks here are somewhat puzzling. This critique anticipates Sellars's

positive argument for a functionalist account of meaning that includes reference to

language users when determining the given role a word plays in a linguistic community.

Carnap's notion that descriptive semantics can, though need not, "abstract away" from

pragmatics is relatively innocuous. The hypothetical linguist would have already

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established that, in Carnap's own words, "*people have the habit of using the word*"; that

is, the role of such utterances *within a linguistic community* would have already been

empirically established by observation of the habits of said linguistic community.

Sellars's objection that no such study could formulate syntactical statements such as

"'Red' is an observation predicate of L" without reference to pragmatical facts is just

confusing. Such facts, including the reference to language users, would have already been

established prior to "abstracting away" from pragmatics.

Sellars's remarks here run deeper than initial appearances. Although it seems as if

he has slightly misconstrued Carnap's characterization of the sense in which descriptive

semantics and syntax can be "abstracted away" from pragmatics, his concern harks back

to points from LRB. Specifically, Sellars is implicitly invoking (though not arguing for)

the idea that any linguistic rule would contain a normative dimension akin to saying "x

may (or may not) be asserted in circumstance c". That is, *Sellars is invoking his*

previously argued for normative conception of rules in order to deny Carnap's claim that

descriptive semantics is wholly empirical in the narrow sense. As Sellars's explains in

EAE:

> The point at which I am driving is that the fact that empirical evidence is
> relevant to the statements of descriptive semantics no more entails that
> characteristically semantical concepts are *descriptive*, than the fact that
> empirical evidence is relevant to the statements of descriptive syntax
> entails that characteristically syntactical concepts are descriptive, or the
> fact that empirical evidence is relevant to the statements of comparative
> ethics entails that characteristically ethical concepts are descriptive.
> (Sellars 1963a, p. 267).

Given this rejection of a tidy distinction between descriptive and pure semantics,

Sellars thinks that the "burden of Carnap's account" that separates empirical from other

treatments of language "rests on a comparison of descriptive semantics with physical

geometry" (Sellars 1963, p. 276). Sellars invokes Carnap's frequently used analogy

between semantics and geometry as found in *Introduction to Semantics*: "Both in

semantics and in syntax the relation between the pure and the descriptive field is perfectly

analogous to pure or mathematical geometry, which is a part of mathematics and hence

analytic, and physical geometry, which is a part of physics and hence empirical" (Carnap

1942, p. 12). The point of Carnap's analogy, one that occurs as early as his *Syntax*, is

that, just as in mathematics and physics, one should evenly divide between empirical and

wholly formal approaches to the study of a language (or a myriad of other subjects).

Fleshing out the consequences of Carnap's analogy between semantics and

geometry, Sellars is quick to point out one untenable conclusion:

> Consequently, this account of the relation between pure and descriptive
> semantics presupposes that semantical expressions in actual usage are
> definable in terms of sign designs and non-linguistic entities…Nowhere,
> however, does Carnap give an *independent* defense of the idea that
> semantical expressions in ordinary usage are thus definable (or
> explicable). (Sellars 1963a, p. 277).

Sellars's claim here is that Carnap's understanding of concepts in pure semantics

implicitly commits him to the idea that one could define descriptive terms simply by

invoking the schema definitions found in rules of designation. This, though not for the

first time, badly misconstrues Carnap's project. This issue will be addressed further in

section IV.

These considerations add up to a problem with Carnap's distinction between pure

and descriptive semantics that amounts to Sellars's general complaint about Carnap: "his

treatment of the relation between pure and descriptive semantics is too much perfunctory.

It leaves important and relevant things unsaid, and what he does say is, by its over-

simplification misleading where it is not down-right mistaken" (Sellars 1963a, p. 276).

Sellars's overall point in EAE, in regards to themes previously discussed, is that Carnap is too quick to assume that one can cash out descriptive semantics as a wholly empirical discipline. This objection, although clearly influenced by Sellars's earlier conception of the role of prescriptive terms in linguistic rules, actually harks back to Sellars's earliest objections against psychologism in the study of language. As noted in section II, such objections do not disappear in Sellars's shift away from philosophy as formalism (re-appearing in LRB as concerns over descriptivism).

Here, as in IM, one runs into the same problem where Sellars continues to conflate Carnap's careful distinction between formal calculi and natural languages. From the outset of EAE, Sellars explicitly claims that he is "concerned with the light thrown on descriptive semantical statements *in actual usage* by Carnap's studies in pure semantics" (Sellars 1963a, p. 255). This earlier methodological claim by Sellars ends up, more so than one finds in IM, being one of his main critiques of Carnap:

> The emphasis of Carnap's studies in semantics is on the formal manipulation of semantical words as defined expressions in pure semantical systems. He deals in much too cavalier a fashion with semantical words as they function in the assertions of descriptive semantics, that is to say, with semantical words functioning as such. The latter, however, is the essential concern of a *philosophical* semantics. For it, *the primary value of formally elaborated semantical systems lies in their contribution to the analysis of semantical concepts in actual usage. Now Carnap is, of course, aware that a pure semantical theory is a semantical theory only if it relates its vocabulary to semantical expressions in actual usage*. (Sellars 1963a, p. 276).

Again, as in IM, while correctly initially characterizing the point of Carnap's pure semantics Sellars's reading falls off the rails when he insists that the importance of such studies turns on their relation to language *in actual use*. As was covered in chapter two

and section three of this chapter, this is simply a strong misreading of Carnap's

philosophy.

It is relatively clear that as a general reading of Carnap, this line of thinking is

simply wrong. A sympathetic interpreter of Sellars's arguments could claim that the issue

is not Carnap interpretation *per se*, but concerns a proper treatment of empirical (in

Sellars's broad sense) studies of historical languages and their relation to formal studies.

As an argument about *this* issue, Sellars's claims could hold weight but such an issue

runs slightly far afield of the current topic. The interpretive problem here is that in

instances like the quoted passage above, Sellars is not making general arguments about

descriptive and pure semantics; Sellars's concern is an accurate reading of Carnap's

philosophy and what he sees as the consequences of Carnap's positions. As such,

Sellars's starting point misses the mark.

Nonetheless, when including considerations from Sellars's previous discussion of

linguistic rules and rule-regulated behavior in LRB and IM it seems clear that he *is*

making an overarching point concerning the place of a *philosophical* contribution in the

study of language. As opposed to Sellars's early commitment to philosophy as formalism,

Sellars is now offering arguments that necessarily ignore the binary distinction raised by

Carnap between a given study being wholly empirical or wholly formal. On Sellars's

understanding of the issue, descriptive semantics can – nay *must* – contain prescriptive

components in the form of linguistic rules as well as descriptive, i.e. empirical in the

broad sense, components. In fact, as shown in section I, Sellars is committed to the idea

that a uniquely *philosophical* contribution to the study of language is located at the point

of meshing the prescriptive and descriptive aspects of language use without giving into scientism or "the new failure of nerve".

EAE represents a rather bulky entry into Sellars's philosophical canon. The majority of the essay is concerned with issues surrounding ontology, language, a fleshing out of psychological nominalism beyond his position in LRB, and functional role semantics that are a bit too broad to be dealt with here. Nonetheless, the core of Sellars's main complaint against Carnap's conception of a rule is expanded from what one finds in IM to encompass a more general problem with Carnap's distinction between pure and descriptive semantics. Given that EAE and Carnap's reply represent the main published dialogue one finds between the two philosophers; EAE represents one more important development in Sellars's rejection of Carnap's philosophical commitments.

IV. Carnap's Reply

Though Carnap occasionally references some of Sellars's works in footnotes throughout his publications in the late 1940s and early 1950s, the most substantive clash between the two philosophers occurred in the *Library of Living Philosophers* volume on Carnap. Carnap's short reply to Sellars touches on a myriad of issues addressed in EAE but specifically covers Sellars's insistence on the occurrence of prescriptive terms in linguistic rules. Not surprisingly, Carnap was not particularly receptive to Sellars's suggestions. The issue, as far as Carnap understood it, was that Sellars's insistence on the inclusion of prescriptive terms in linguistic rules turned on a simple misunderstanding:

> Sellars's belief that my descriptive syntax and descriptive semantics
> contained prescriptive conceptual components is perhaps due to the fact
> that I used the word 'rule' both in syntax and in semantics. Perhaps he
> understood this term in its everyday sense, i.e., as referring to prescriptive
> rules, prescriptions, prohibitions, or permissions. However, I use the word
> 'rule' in this field only in order to conform to the customary usage in

logic…It seems to me that in the development of modern logic it has become even more evident that logic, and likewise syntactical and semantical analyses of language, are purely theoretical; the use of the terms like 'rules', 'permitted operations', and 'prohibited operations' is here, just as in algebra, merely a psychologically useful way of speaking which should not be understood literally. (Carnap 1963, p. 923 – 4).

It should be clear from the quote above that Carnap and Sellars are simply talking past each other. Carnap's response to Sellars's critique in EAE is to suggest that Sellars has simply misunderstood the sense in which Carnap (and others) have been using the term "rule". This hardly represents a fair response to Sellars's arguments and would seem to be indicative of the fact that Carnap did not fully understand Sellars's emphasis on the prescriptive component behind descriptive statements. Even in LRB, and especially in IM, Sellars's argument is not simply that Carnap has not correctly characterized rules but that his conception of a rule (despite its origin) cannot fulfill the syntactic and semantic roles it was designed for.

Moreover, Sellars's critiques of Carnap do not turn on a literal understanding of a rule. Sellars's rival conception of linguistic rules invokes normative terms as ineliminable presuppositions of descriptive terms. Part of his argument in EAE turns on the idea, initially raised in LRB, that for a linguistic rule to be a rule in a legitimate sense it must contain a normative dimension. Although Sellars is less explicit on this exact point in EAE, one finds the same point being raised against Carnap's conception of descriptive semantics as a wholly empirical endeavor. It is not that Sellars has misunderstood Carnap's project as covertly including prescriptive components. Instead, what Sellars is doing in EAE is offering objections to Carnap's conception of pure and descriptive semantics based on his own rival account of a language and linguistic rules.

But what of Sellars's broader objection concerning Carnap's distinction between pure and descriptive semantics? Carnap is quite clear that Sellars has, overall, misconstrued his position here as well. In constructing a distinction between formal and natural languages and their corresponding treatments in pure and descriptive semantics, Carnap is quite clear that the construction of rules of designation would be quite different in each case. In descriptive semantics, which necessarily presupposes pragmatics, one would analyze the sentence "(In German) the word '*blau'* designates blue" as based on the pragmatic statement "In the German language community, the German word '*blau'* is mostly used as designating Blue" (Carnap 1963, p. 925). In the context of descriptive semantics, the rules of designation assert that "a certain factual (but not causal) relation holds between the word design '*blau'* in German and the property Blue" that is based on the psychological concept of designation found in pragmatics (Carnap 1963, p. 925).

Alternately, the rules of designation found in pure semantics are construed in a completely different manner and, as has been remarked previously, have no real substantive connection with the rules found in pragmatics aside from possibly playing a motivational or external role. In the kind of constructed language (in this example the language G) dealt with in pure semantics the rules of designation would be "defined by an enumeration of pairs, each pair consisting of a predicate in G and a property" (Carnap 1963, p. 925). Carnap agrees with Sellars that there is a "radical difference" between the rules of designation in pure and descriptive semantics and "the two terms would have at best the same extension" but does not see this difference as a problem (Carnap 1963, p. 926). Again, as noted in section III, Sellars's insistence on what he thinks should be a close relationship between designates in pure and descriptive semantics turns on his

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confused reading of Carnap; a reading that insists on a closer relationship between formal and natural languages than one finds in Carnap's own writings.

Despite Carnap's insistence on natural language only playing a possible motivational role for formal calculi, he does think that there can, though need not, be a connection between concepts formally construed and those occurring in natural languages. One could consider the corresponding descriptive concept for the formally considered rules of designation to be any designation relation in a natural language that has the same truth-conditions as the formally considered notion (Carnap 1963, p. 926-7). So, in other words, one could "connect" the enumerated pair "'blau', Blue" defined via a rule of designation to the descriptive statement "In German '*blau*' designates blue" insofar as the latter statement holds in all cases in which the former statement (once the pair is set in a rule of designation) does. On Carnap's understanding of the distinction between pure and descriptive semantics, this would not be to claim that they are the same concept; the rules of designation in pure semantics represent "the logical connections among various facts involving the relation of designation in any language" while the descriptive concept necessarily turns on pragmatic and psychological considerations (Carnap 1963, p. 927).

Ultimately, Carnap's reply to Sellars amounts to the idea that Sellars has simply run together his careful distinction between pure and descriptive semantics. Carnap's reply ends with his conviction that the analogy between divisions within semantics and geometry holds:

> In pure semantics we cannot give an analysis of the concept of designation
> in its ordinary sense because for this purpose psychological concepts are
> required. The situation is analogous to the situation between pure
> geometry and physical geometry (where pure geometry is understood as

represented, not by an uninterpreted axiom system, but rather by a purely logical theory concerning a certain structure). In pure geometry, we cannot analyze the physico-spatial concepts, because concepts of physics or of the observation language would be needed for this purpose; but pure geometry can mirror the logical connections holding between physico-geometrical concepts or propositions. (Carnap 1963, p. 927).

This is a particularly interesting response for two reasons: First, Carnap seems to simply ignore Sellars's point that the analogy between pure and descriptive semantics and pure and physical geometry does not hold. Second, Carnap's discussion of the designation relation in connection to pure and descriptive semantics would seem to run into one of the problems that Sellars raised in IM.

As discussed above, the force of Sellars's arguments against the analogy between semantics and geometry is that if the analogy were to hold, Carnap's conception of semantics would necessarily entail the idea that terms in ordinary usage could be defined solely by sign designs and non-linguistic entities. But, as with some of Sellars's other moves, this seems to miss the mark by misconstruing Carnap's understanding of the relationship between pure semantics and ordinary usage. In this case, Sellars's denial of the analogy falls short because, as Carnap explicitly mentions, "*In pure semantics we cannot give an analysis of the concept of designation in its ordinary sense because for this purpose psychological concepts are required*" (Carnap 1963, p. 927). What Sellars seems to be suggesting is that Carnap's analogy between semantics and geometry would force him to be committed to the idea that a formal calculus could explicitly or implicitly define terms in their ordinary sense . As is evident from the passages above, this is simply not so.

Second, Sellars explicitly took issue with Carnap's definitional schema for rules in general in IM. Specifically, one of Sellars's arguments turned on the idea that defining

designation rules *via* simple enumeration runs into the Euthyphro-ish problem that it seems as if all Carnap's definitional schema accomplishes is giving examples of instances of rules and does not define rules of designation (or formation and transformation rules) themselves. Carnap's discussion of designation in pure and descriptive semantics would seem to do nothing to diffuse this objection.

One may wonder whether Carnap really has to pay much attention to this critique, given his explicit disavowal that pure semantics deals with meaning or designation *in their ordinary use*. If this is the case, then what I have classified as Sellars's first critique seems to fall on deaf ears. Nonetheless, Sellars's second objection (rooted in his arguments from IM) would seem to be the more powerful of the two. The issue of how pure semantics relates to ordinary linguistic usage does not undermine Sellars's objection that Carnap's conception of a rule is able to, at best, suggest the circumstance in which a given word is used but does not actually define the rule itself.

Ultimately, it may be possible that Carnap simply misses some, or all, of Sellars's objections to his conception of semantics. What is more likely the case is that Carnap was simply unfamiliar with Sellars's earlier publications (especially LRB and IM)[71] and, thus, unfamiliar with his rival conception of rules and his explicit argument against Carnap's conception of a rule. Although, as described above, Sellars does make some reference to his conception of a rule, there is nothing close to an explanation of his conception of a

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[71] Although I have found passing reference in footnotes by Carnap to RNWW, "The Identity of Linguistic Expressions and the Paradox of Analysis", and "Acquaintance and Description Again", there seems to be no evidence (as far as I can tell) that Carnap read either LRB or IM. Of course, it could be the case that Carnap read both and simply did not recognize some of the ideas from both of these essays lurking in the background of EAE. Nonetheless, given Carnap's seeming confusion over Sellars's insistence on the inclusion of prescriptive terms in semantics it seems unlikely Carnap read either essay.

rule in EAE. In defense of Carnap, his response to Sellars's objections may seem

somewhat superficial given Carnap's lack of familiarity with Sellars's own position.[72]

V. The Road to a Strongly Normative Conception of Language

On top of these issues, perhaps one of the more telling aspects of this exchange

between Carnap and Sellars is one of Sellars's concluding remarks at the end of EAE. In

an attempt to summarize his overall position concerning Carnap's philosophy, Sellars

remarks that "My thesis, so far as it concerns him, amounts rather to the wish that he had

devoted more of his time and energies to bringing out *the full philosophical significance*

of his syntactical and semantical studies, and less to the technical elaboration of lemmas

and corollaries" (Sellars 1963a, p. 282). Although Sellars had not defined philosophy as

"pure formalism" since 1948, this remark in the mid-1950s signals just how much

Sellars's philosophical orientation had shifted.

If Sellars's positive account of rules and rule-regulated behavior is grounded in

the initial observations found in LRB, his full-blown account (prior to his post-EPM

publications) is located in "Some Reflections on Language Games" (hereafter SLRG). It

is in SLRG that Sellars lays down a full account of rules, rule-regulated behavior, the

meshing of language and perception, and addresses the myriad of problems surrounding

all of these issues. Sellars's developmental path up to SLRG should now seem quite

clear; the initial shift from a formalist understanding of philosophy to the

sociological/psychological framework of rules and rule-regulated behavior is what allows

Sellars to construct a normative conception of language. Sellars's conception of rules, his

critique of Carnap's conception of a rule, and his break with the formalist conception of

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[72] Of course, there are also historical or sociological reasons to consider; by the time Sellars would have sent his article to Carnap for a response, Carnap's focus was not on formal issues from the early 1940s. Carnap had long since moved on to worrying about inductive logic, probability, and writing text books.

philosophy is what paved the way for his position in SLRG. It is these specific

philosophical moves and changes in Sellars's early positions that allow for SLRG in the

first place. In this concluding section, I am not particularly concerned to characterize

SLRG or Sellars's later writings on language. Instead, my point will be to make explicit

how Sellars moved from the formalism of pure pragmatics to a normative conception of

rules placed in the sociological/psychological framework that lead up to his full-blown

account of rules in SLRG.

Although this normative conception of rules develops from mere claims in LRB

to a full-blown account of the role of normative terms in IM, I have previously suggested

that the positive arguments in LRB and, to a lesser extent, IM are essentially incomplete.

In rough fashion, one can construct the development of Sellars's strongly normative

conception of language in three stages: Sellars's initial break with formalism, the

development of a rival conception of rules, and his full-blown critique of Carnap's

conception of rules (all three stages roughly corresponding to sections I – III above).

Sellars's first philosophical period should be encapsulated by his three earliest

publications. His overriding commitment to defining philosophy as a formal pursuit

marks a different approach from his earlier unpublished work and the rest of his

philosophical canon. Sellars's general conception of a rule during this period seems

relatively orthodox; the major difference in Sellars's views as opposed to those of Carnap

is his insistence on the need for conformation rules. Although Sellars does offer some

seemingly vague critiques of the then contemporary approaches to semantics, his overall

position is not strikingly similar to his later philosophy. LRB represents a significant

shift from this point. Although Sellars does not offer specific critiques of logical

positivism or Carnap's conception of a rule in 1949, he does offer a rival account of what properly constitutes a linguistic rule. It is only in IM and later essays that Sellars explicitly takes issue with Carnap's conception of a rule and offers arguments against such notions.

The narrative, as I have constructed it, runs from philosophers offering strictly formal contributions to the study of language, to a more psychologically-inclined approach that allows Sellars to address the notion of a linguistic rule as a fact of experience. Of course, I am not claiming that this is a linear progression. Sellars, in perhaps typical Sellarsian fashion, attempted to develop a number of these ideas at the same time. Thus, one finds various versions of psychological nominalism crop up in RNWW, LRB, EAE, and other essays prior to its more known formulation in "Empiricism and the Philosophy of Mind". The same claim can be made, in one form or another, about Sellars's conception of functional role semantics, a coherentist approach to meaning, and his critiques of the given and abstract entities. It is not as if these were the only topics Sellars puzzled over and published essays on during this period of time. However, that being said, I have focused on Sellars's conception of a rule and its development in this chapter as an example of how his views developed in a both positive and negative fashion. If I am correct thus far, his early account of rules shows little to no sign of the later normative commitments one finds in Sellars's philosophy, but there is a relatively clear story of Sellars's progression from a more orthodox conception of rules to a strongly normative conception as found in SRLG.

Chapter 4: Conclusion

Where does one ultimately situate Sellars's early work in the history of analytic philosophy? Was he part of the end of logical positivism? Was he, perhaps like Quine, a somewhat rebellious positivist who rejected a number of commitments held by logical positivists but remained somewhat faithful to its overall project? Were his initial philosophical writings an attempt to reconcile his father's critical realism with a more modern day scientific philosophy? It seems, in an appropriately Sellarsian vein, that a positive answers to all of these questions would contain a grain of truth. As I have hopefully shown in the preceding chapters, Sellars's story is a significantly complicated one; it's a story that contains a number of philosophical shifts and changes that have thus far been glanced over in the philosophical literature.

Sellars's earliest period, as marked by the three essays constituting his focus on pure pragmatics, should be characterized as an uneasy alliance with logical positivism. There is some truth to the idea that Sellars was interested in saving Carnap's formalist project from what he saw as its misguided commitment to naïve realism and psychologism. Although a number of points in these essays are directed against logical positivism (in a suitably broad sense), Sellars quite clearly sees value in their overall stance and intends to align himself with the movement in his published remarks. A reading of Sellars's early papers that emphasizes this point could construe his initial philosophical orientation as generally sympathetic with logical positivism and interpret his critical remarks as internal critiques of the movement. Such a reading would

seemingly gain support by placing Sellars's early works in the context of Bergmann's critiques of Carnap; Bergmann was clearly concerned with *reforming* logical positivism as opposed to abandoning the project as a whole.

Such a reading, as I argued in chapter two, is a mistake. Although one could read Sellars's early work as framed by his indebtedness to Carnap's philosophy, such a reading would omit Sellars's commitment to traditional philosophical notions and problems – specifically a commitment to taking seriously metaphysical or ontological problems. Sellars's autobiographical reflections support this claim but one can also see a concern with traditional philosophical problems scattered through his early works and correspondence. Sellars's early commitment to a formalist conception of philosophy does not signal his allegiance to logical positivism; the substance of his early arguments clearly shows this not to be the case. Instead, one should read the early Sellars as focusing on traditional metaphysical and epistemological issues that he attempted to present in formalist garb. As the reception history of Sellars's early work shows, this attempt at formalism did not succeed in either revising or rejecting logical positivism or in laying the foundations for a normative conception of language.

One may wonder why so many of Sellars's early contemporaries simply did not seem to notice or address the stream of traditional philosophical thought running through his early publications. I think the reason is three-fold. First, as has hopefully become apparent throughout this project, Sellars's early publications are not exactly clear. Although he invokes a number of canonical philosophers and tends to lean on traditional frameworks of inherited problems, this fact in and of itself does nothing to set Sellars apart from his contemporaries. There was nothing irregular in mentioning the more

classical or traditional framework of a problem to situate contemporary issues within the philosophical canon. Although some logical positivists, such as Rudolf Carnap, were reluctant to do so, a number of his philosophical allies were more than willing to use this somewhat rhetorical device to their advantage (Herbert Feigl being, I think, one of the best examples).

Second, by the time of Sellars's first publication in 1947, "traditional" American philosophy of the new and critical realist stripe (and one might even argue pragmatism in all thirteen flavors) had ceased to be competing schools of thought. In the introduction to *Naturalism and Ontology* Sellars seems to confirm this observation when he provides a brief description of his philosophical upbringing:

> When I was coming to philosophical consciousness, the great battles
> between the systems which began the twentieth century were drawing to a
> close, although the lightening and the thunder were still impressive. I cut
> my teeth on issues dividing Idealist and Realist and, indeed, the various
> competing forms of upstart Realism. (Sellars 1979, p. 1).

Although the concerns and commitments of earlier American philosophical schools were in the background of Sellars's thought, the framework such issues were housed in had been long since abandoned by the time Sellars's first publications arrived on the scene. When such framing does appear in the majority of publications in the late 1940s, they are just that – rhetorical framing devices to situate, in a non-historical sense of "situate", a given contemporary problem.

The third consideration is the sociological fact that Sellars tended to align himself with logical positivists and those generally sympathetic with logical positivism. His close association with Gustav Bergmann and Herbert Feigl, as well as his public and private expression of indebtedness towards the philosophy of Carnap, Hempel, and

Reichenbach paints a picture of someone fairly construed (at least at first glance) as in bed with logical positivism. It may be the case that the *substance* of Sellars's early pure pragmatics and his later shift towards a social/psychological account of language use is necessarily antithetical to Carnap's formalist project. In both his explicit remarks and the terminology he adopted, Sellars certainly seems as if he is committed to the project of logical positivism.

By 1949 such a positivistically inclined reading of Sellars would need to disappear relatively quickly. Although Sellars's earliest papers do not offer explicit critiques of Carnap or other logical positivists, by the publication of LRB Sellars has begun to construct what would eventually become his strongly normative conception of language. As was argued for in chapter three, Sellars's conception of philosophy had shifted significantly from the formalist model he embraced in his earliest publications. Aside from bringing substantive changes to his philosophical commitments, this shift also represents the development of an explicit critique of Carnap's philosophy – one that begins in LRB and moves through EAE and beyond.

As was mentioned in the introduction, this historical account of Sellars's early work does not purport to be a complete account of Sellars's philosophical development. Such a project is simply too large an undertaking for a dissertation (and quite possibly too large an undertaking for one book). I think the preceding chapters have offered something akin to a "road map" of how to approach Sellars's early works in relation to the rest of his philosophical canon. Anyone interested in offering a developmental account of Sellars's philosophical progression will need to take into consideration the various philosophical

schools (both alive and dead at the time) in the background of his thought and philosophical education as well as the myriad of contemporary debates he was embroiled.

Although Sellars was a contemporary philosopher, his philosophical education was grounded in debates that held sway in the early 20th century American philosophical community. This point cannot be overlooked; Sellars's indebtedness to new and critical realism, as well as various strains of pragmatism (as found in Charles Pierce, John Dewey, and C. I. Lewis), served to frame his early encounters with philosophy and continued to influence his philosophy throughout his career.

Despite how recent of a philosopher Sellars was, I want to suggest that the historical dimension to his early works is a necessary component for a developmental picture of Sellars's views. The fact that Sellars worked and published on issues that are still very much alive tends to obscure the fact that his philosophical education really is rooted in debates that have long since been forgotten. Perhaps obviously, the issues at stake in debates between idealists, pragmatists, and the new and critical realists have been largely ignored in the history of 20th century philosophy. As indicated by the quote above, understanding how these issues were framed by earlier American philosophers could help situate Sellars's early philosophical commitments.

In this vein, institutional histories – especially those surrounding the University of Iowa and Minnesota – could play a large role in fleshing out Sellars's work prior to the publication of "Empiricism and the Philosophy of Mind". This is especially the case with the University of Iowa; Sellars clearly thought of his time there as formative and described his arrival at Iowa as an "exhilarating new beginning" (Sellars 1975, online). Looking at Sellars's early publications, this is not surprising. The combination of an

emphasis on the history of philosophy, logical positivism, and the development of behaviorism throughout the psychology department at Iowa highlights the major themes Sellars was dealing with in his early publications.

One last thing to keep in mind is Sellars's commitment to the analogy between the function of normative terms in moral arguments and the function of a prescriptive component presupposed by an analysis of descriptive terms. This analogy plays an important role in Sellars's overall approach to understanding language and thought. As was argued in chapter three, Sellars's strongly normativist conception of language turns on the intuition that moral arguments or claims have a prescriptive component over and above the descriptive dimension of claims. Despite the significant role this analogy plays in constructing a normative conception of language, Sellars's early understanding of moral and practical reasoning has been left relatively untouched in the secondary literature.

There are, I believe, three future areas of historical research for this issue: the published work and Sellars's correspondence surrounding his co-edited anthology with John Hospers, *Readings in Ethical Theory*, Sellars's early published and unpublished writing on various themes in ethics, and Sellars's later unpublished correspondence with Jeff Sicha on the proper analysis of moral and practical reasoning. All three areas are essentially undeveloped in the secondary literature surrounding Sellars and of the three, Sellars's unpublished correspondence with Jeff Sicha would seem to represent the most promising area for research. On top of this, placing Sellars's early remarks on ethics in the context of what he saw as the dominant interlocutors of his time in emotivists and

intuitionist philosophers would help clarify the motivation behind Sellars's early ethical commitments.

Sellars's canon represents a rich area for future research for both the historically inclined philosopher and the intellectual historian. What I have hopefully provided in this dissertation is the beginning of a thoroughly historical reading of Sellars's systematic philosophy. If my arguments leading up to my conclusion are sound, it would seem that any approach to understanding Sellars's philosophy situated within its proper context will need to account for a number of historical trends and traditions that have hitherto been ignored when addressing Sellars's philosophy. Such a task is quite large; hopefully I have made the job of approaching Sellars's philosophy from a historical and developmental perspective marginally easier.

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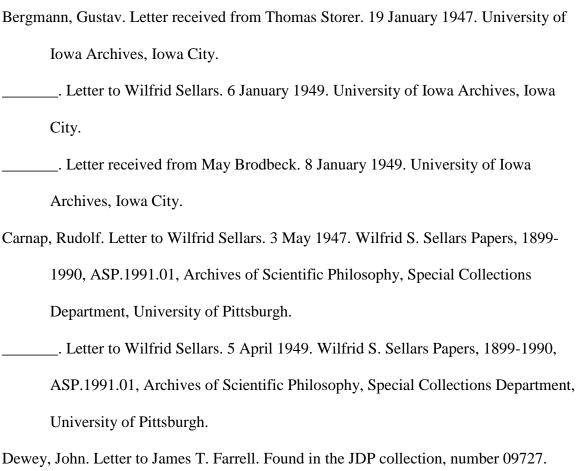
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