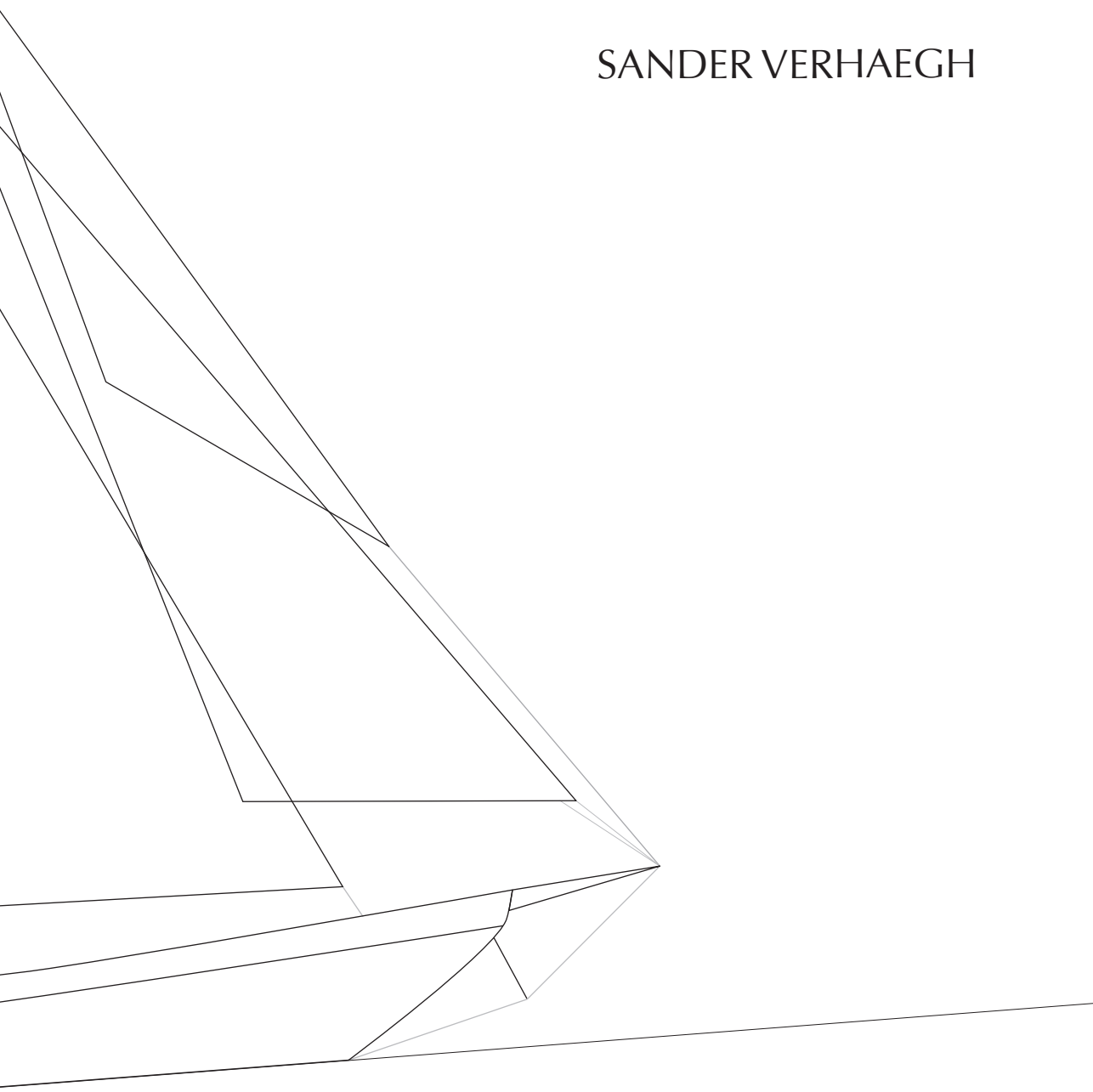
RAFTS, BOATS, AND CRUISE SHIPS

Naturalism and Holism in Quine's Philosophy

SANDER VERHAEGH



RAFTS, BOATS, AND CRUISE SHIPS

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LIST OF ABBREVIATIONS

This section provides a list of abbreviations used in this dissertation to refer to W. V. Quine's (published and unpublished) work. Detailed references can be found in the Bibliography. A complete list of Quine's publications up to 1993 (including reprints and translations) can be found in Yeghiayan (2009). For an overview of papers, books, reprints, and translations after 1993, see Douglas B. Quine's website `http://wvquine.org`. The unpublished papers, letters, lectures, and notebooks listed below (marked with an asterisk) are stored at the Harvard Depository and can be accessed at Houghton Library. The documents' call numbers, box numbers, and item numbers are provided in the Bibliography. A catalogue of much of Quine's unpublished work is provided by Houghton Library at `http://oasis.lib.harvard.edu/oasis/deliver/~hou01800`.

ANM	Animadversion on the Notion of Meaning (1949)
AT*	Afterthoughts (1987)
AWVQ	Autobiography of W.V. Quine (1986)
BTJ*	The Behavioristic Treatment of James' Theory of Perception (1930)
CA	Carnap (1987)
CB	Comment on Berger (1990)
CCE	*Confessions of a Confirmed Extensionalist and Other Essays* (2008)

CD	Contextual Definition (1995)
CGC	A Comment on Grünbaum's Claim (1962)
CH	Comment on Haack (1990)
CK	Comment on Koppelberg (1990)
CLT	Carnap and Logical Truth (1954)
CP	Comment on Parsons (1990)
CPT	Carnap's Positivistic Travail (1984)
CNT	Comments on Neil Tennant's "Carnap and Quine" (1994)
CVO	Carnap's Views on Ontology (1951)
CWH*	Concepts and Working Hypotheses (1931)
DE	Designation and Existence (1939)
EBDQ	Exchange Between Donald Davidson and W. V. Quine Following Davidson's Lecture (1994)
EESW	On Empirically Equivalent Systems of the World (1975)
EJ*	Early Jottings on Philosophy of Language (1937-1944)
EN	Epistemology Naturalized (1969)
ENP*	Epistemology Naturalized; or, the case for psychologism (1968)
EQ	Existence and Quantification (1969)
FLM*	Foundations of a Linguistic Theory of Meaning (1943)

FLPV	*From a Logical Point of View* (1953/1961)
FM	Facts of the Matter (1977)
FME	Five Milestones of Empiricism (1975)
FSS	*From Stimulus to Science* (1995)
GML	The Growth of Mind and Language (1997)
GQW	Nelson Goodman, W. V. Quine and Morton White: A Triangular Correspondence (1947)
GT	Grades of Theoreticity (1970)
HO*	Holism (1987)
ICQ*	In Conversation: Professor W. V. Quine. Interviews by R. Fara (1993)
IKL*	The Immanuel Kant Lectures: Science and Sensibilia (1980)
ILP	The Importance of Logic for Philosophy (1950)
IOH	Identity, Ostension, Hypostasis (1946)
IPOS	In Praise of Observation Sentences (1993)
IQJ*	Interview with Quine for "La Janguardia" (1990)
IV	Immanence and Validity (1991)
IWVQ	Interview with Willard Van Orman Quine. Interview by L. Bergström and D. Føllesdal (1994)
LAOP	A Logistical Approach to the Ontological Problem (1939)

LC	Lectures on Carnap (1934)
LDHP	Lectures on David Hume's Philosophy (1946)
LMAP	Let Me Accentuate the Positive (1990)
LSQ*	Levine Seminar Questions for Quine (1982)
ME*	Mathematical Entities (1950)
ML1	*Methods of Logic*. First edition (1950)
ML4	*Methods of Logic*. Fourth edition (1982)
MMT*	Mathematics as a Mode of Thought (1930)
MSLT	Mr. Strawson on Logical Theory (1953)
MVD	Mind and Verbal Dispositions (1975)
N1*	Nominalism (1937)
N2	Nominalism (1946)
NDL*	The Notre Dame Lectures (1970)
NEN	Notes on Existence and Necessity (1943)
NK	Natural Kinds (1969)
NLOM	Naturalism; Or, Living within One's Means (1995)
NNK	The Nature of Natural Knowledge (1975)
NR*	Notes on Reading (undated)
OAM	On Austin's Method (1969)
OE*	Ontology and Epistemology (1944)
OK*	On the Organization of Knowledge (1927)

OLPL*	Oxford Lecture: Philosophy of Logic (1953)
OM*	Ontology, Metaphysics, etc... (1944-1951)
OME	On Mental Entities (1953)
ON*	Ontology Notes (1944)
ONAS	On the Notion of an Analytic Statement (1946)
OR	Ontological Relativity (1968)
OR67*	Ontological Relativity (1967)
ORE	*Ontological Relativity and other essays* (1969)
ORWN	Ontological Reduction and the World of Numbers (1964)
OW	Otherworldly (1978)
OWTI	On What There Is (1948)
PIP*	A Pragmatic Interpretation of Posivitism (1937)
PL	*Philosophy of Logic* (1970/1986)
PML	The Problem of Meaning in Linguistics (1953)
PPE	The Pragmatists' Place in Empiricism (1975)
PPLT	Philosophical Progress in Language Theory (1970)
PR	Posits and Reality (1955)
PT	*Pursuit of Truth* (1990/1992)
PTE*	The Place of a Theory of Evidence (1952)
PTF	Progress on Two Fronts (1996)

QBC*	The Quine-Bergström Correspondence (1988-1996)
QCC	The Quine-Carnap Correspondence (1932-1970)
QCC*	The Quine-Church Correspondence (1935-1994)
QD	*Quine in Dialogue* (2008)
QGC*	The Quine-Goodman Correspondence (1935-1994)
QHC*	The Quine-Haack Correspondence (1975-1997)
QHWC*	The Quine-Hookway Correspondence (1988)
QKC*	The Quine-Koppelberg Correspondence (1981-1994)
QQN*	Questions for Quine by Stephen Neale (1986)
QSM	Quine Speaks His Mind. An interview by E. Pivcevic (1988)
QU	*Quiddities* (1987)
QWC*	The Quine-Woodger Correspondence (1938-1982)
QWVO	Quine/'kwain/, Willard Van Orman (b. 1908) (1996)
RA	Responses to Articles by Abel Bergström, Davidson, Dreben, Gibson, Hookway, and Prawitz (1994)
RAB	Relativism and Absolutism (1984)
RCP	Reply to Charles Parsons (1986)

RE	Reactions (1995)
RES	Responses to Szubka, Lehrer, Bergström, Gibson, Miscevic, and Orenstein (1999)
RGH	Reply to Geoffrey Hellman (1986)
RHP	Reply to Hilary Putnam (1986)
RHW	Reply to Hao Wang (1986)
RJV	Reply to Jules Vuillemin (1986)
RMW	Reply to Morton White (1986)
ROD	Russell's Ontological Development (1966)
RPR	Reply to Professor Riska's Eight Questions (1992)
RR	*The Roots of Reference* (1973)
RRG	Reply to Roger F. Gibson, Jr. (1986)
RRN	Reply to Robert Nozick (1986)
RWA	Reply to William P. Alston (1986)
RS1	Reply to Stroud (1968)
RS2	Reply to Stroud (1981)
RTE	Responses to Essays by Smart, Orenstein, Lewis and Holdcroft, and Haack (1997)
SBLM	Sellars on Behaviorism, Language and Meaning (1980)
SCN	Steps toward a Constructive Nominalism (1947)
SID	Soft Impeachment Disowned (1980)

SLP	*Selected Logic Papers* (1966/1995)
SLS	The Scope and Language of Science (1954)
SM	Stimulus and Meaning (1965)
SN	Structure and Nature (1992)
SO*	Sign and Object; or, The Semantics of Being (1944)
SSE	Sticks and Stones; or, The Ins and Outs of Existence (1984)
SSS	The Sensory Support of Science (1986)
STCW	On Simple Theories of a Complex World (1960)
STL	*Set Theory and its Logic* (1963/1969)
TC	Truth by Convention (1936)
TCL	Twentieth-Century Logic. An interview by G. Borradori (1994)
TDE	Two Dogmas of Empiricism (1951)
TDR	Two Dogmas in Retrospect (1991)
TH*	Things (1943)
TI	Three Indeterminacies (1990)
TML	*The Time of My Life* (1985)
TO*	A Tentative Ontology (1941)
TR	Truth (1994)
TT	*Theories and Things* (1981)
TTPT	Things and Their Place in Theories (1981)

VD	Vagaries of Definition (1972)
VITD	On the Very Idea of a Third Dogma (1981)
WB	*The Web of Belief* (1970/1978)
WDWD	Where Do We Disagree (1999)
WIB	What I Believe (1984)
WMB*	What it Means to Be (1944)
WO	*Word and Object* (1960)
WP	*The Ways of Paradox and other essays* (1966/1976)
WPB	What Price Bivalence? (1981)
WWI	The Way the World Is (1986)

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Verhaegh, S. (under review-b), Boarding Neurath's Boat: The Early Development of Quine's Naturalism (chapter 4).

Tamminga, A. and Verhaegh, S. (2013). Katz's Revisability Paradox Dissolved. *Australasian Journal of Philosophy*, 91(4):771-784 (section 5.6).

Verhaegh, S. (under review-c), Quine: Philosophy of Logic (section 5.6).

Verhaegh, S. (2013). Quine, Putnam, and the Naturalization of Metaphysics. In Baumgartner, S., Heisenberg, T., and Krebs, S. (eds.). *Metaphysics or Modernity?* (pp. 249-269). University of Bamberg Press, Bamberg (section 6.6).

Verhaegh, S. (under review-d), Rafts and Cruise Ships: Quine's Naturalism Disambiguated (sections 6.7-6.12).

1

INTRODUCTION

During the past few decades, a radical shift has occurred in how philosophers conceive of the relation between science and philosophy. A great number of analytic philosophers have adopted what is commonly called a naturalistic approach, arguing that their inquiries ought to be in some sense continuous with science. Where early analytic philosophers often relied on a sharp distinction between science and philosophy—the former an empirical discipline concerned with fact, the latter an a priori discipline concerned with meaning—philosophers today largely follow Willard Van Orman Quine (1908-2000) in his seminal rejection of this distinction as well as in his reconstruction of their discipline in naturalistic terms, thereby propagating a thorough, scientifically informed, philosophy.

This book offers a historical study of Quine's naturalism. It provides a detailed reconstruction of Quine's development, a novel interpretation of his arguments, and a systematic investigation into the presuppositions underlying his position. As such this dissertation aims to contribute to the rapidly developing historiography of analytic philosophy as well as to a better, historically informed, understanding of what is philosophically at stake in the contemporary naturalistic turn.

1.1 HISTORICAL BACKGROUND

Naturalism, broadly defined as the idea that philosophical inquiry ought to be science-minded in both theory and method, has a rich history. Although it is probably for the first time that so many philosophers identify themselves as naturalists, it

would be a mistake to think of the position itself as new; naturalistic pictures of inquiry, at least in some varieties, are almost as old as science itself.

Naturalistic world views gained prominence especially in the second half of the nineteenth century. In the wake of Darwin and Wallace's work on evolution, the early development of psychology as an independent discipline, and the steady advances in mathematics, physics, and chemistry, the prospects for an exclusively naturalistic perspective on reality became a frequently debated subject among philosophers and scientists. In these often heated discussions, naturalists argued for scientific explanations of supposedly supernatural phenomena, i.e. for a picture of reality in which man, mind, and morality are conceived as part of the natural order.[1]

Across the Atlantic, naturalism came to full bloom from the early 1920s onwards, when a group of pragmatist philosophers, among whom John Dewey, Roy Wood Sellars, George Santayana, and somewhat later, Ernest Nagel, came to develop views they identified as naturalistic.[2] Most of them defended a two-sided naturalism; they argued for the *metaphysical* claim that reality is exhausted by nature as it is studied by the sciences, thus dismissing appeals to supernatural explanation; and they defended the *methodological* claim that scientific method is the only reliable route to knowledge, thereby suggesting that philosophers ought to adopt this method.[3]

1 See Spencer (1862), Lewes (1874), Huxley (1892) and Haeckel (1899). Broadly naturalistic philosophies were also developed by materialists, empiricists, and, to a certain extent, positivists. See Büchner (1855), Mill (1865), and Comte (1830-1842). For some influential anti-naturalist responses from theistic, idealistic, and analytic philosophers, see Balfour (1895), Ward (1896), and Moore (1903).

2 See Sellars (1922), Santayana (1923), Dewey (1925), and Nagel (1954, 1961), as well as the essays collected in Krikorian (1944) and Ryder (1994). For a history of naturalism in the United States before the 1920s, see Larrabee (1944).

3 See Sellars (1924), Randall (1944), and, for a historical discussion, Kim (2003). Today, it is still quite common to distinguish between metaphysical and

Probably the most influential naturalist of this group was John Dewey, the pragmatist who denied there is any distinctively philosophical perspective on reality. Dewey, himself building on the fallibilism and anti-foundationalism of his predecessors Peirce and James, viewed mind as part of the world that is studied by the sciences. For him, the philosopher is someone who "has no private store of knowledge or methods for attaining truth" and must therefore utilize "the best available knowledge of its time and place", such that "its road is the subject-matter of natural existence as science discovers and depicts it" (1925, 408).[4]

During the second half of the twentieth century, naturalistic philosophies rapidly gained support. As a result of this development, naturalism is contemporary philosophy's dominant metaphilosophical ideology.[5] Although it is difficult to isolate the causes of this development, it is safe to say that natural-

methodological varieties of naturalism. See Moser and Yandell (2000, §1), De Caro and Macarthur (2004a, 2-8), and Papineau (2009). Scholars sometimes also distinguish an *epistemological* variant of naturalism which entails that all knowledge is scientific knowledge. See Wagner (1993, 212) and Glock (2003, 27-8).

4 It should be noted that Dewey sometimes defines naturalism in much weaker terms as well. See his (1944, 2) claim that a naturalist is someone "who has respect for the conclusions of natural science". Present-day naturalists too defend a wide range of metaphilosophical positions—some weak, some extremely strong. This wide range of metaphilosophical positions that go under the name 'naturalism' has led many to complain that it is almost impossible to come up with a definition for the view. See van Fraassen (1996, 172), who argues that it is "nigh-impossible" to "identify what naturalism is", but also earlier Seth (1896), Sellars (1927), and Nagel (1954, 3). Maffie (1990) and Haack (1993a), however, have attempted to resolve this situation by offering taxonomies of naturalism.

5 See, for example, Leiter (2004, 3), who speaks about "a naturalistic turn" in philosophy. Leiter's claim is backed up by Bourget and Chalmers' (2014) survey among 931 leading philosophers. In response to the question whether they would describe themselves as naturalists or non-naturalists in metaphilosophy, 49.8% of the respondents answered 'Naturalism', whereas only 25.9% of the respondents chose the non-naturalist option. 24.3% of the res-

ism's contemporary prominence is for a significant part due to the work of Quine, one of the first philosophers to formulate a comprehensive naturalistic worldview that covers almost all major problems in epistemology and metaphysics. Quine's "On What There Is" (1948) has arguably set the agenda for most contemporary metaphysicists, whereas "Epistemology Naturalized" (1969a) paved the way for the in some circles popular reconception of epistemology as 'the science of science'.[6]

Quine is connected to his naturalistic predecessors in that he seems to have borrowed the term 'naturalism' from Dewey. Although Quine, as we shall see, already defended a thoroughly naturalistic philosophy in the 1950s, it is only in his 1968 John Dewey Lectures that he first uses the term 'naturalism' to describe his position; lectures in which he acknowledges Dewey's influence:

> Philosophically I am bound to Dewey by the naturalism that dominated his last three decades. With Dewey I hold that knowledge, mind, and meaning are part of the same world that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy.[7] (OR, 1968c, 26)

pondents were agnostic, insufficiently familiar with the issue, or thought that the question was too unclear to answer.

6 Reception-studies are rare in philosophy. Still Quine's important role in the naturalistic turn seems to be generally recognized. See Leiter (2004, 2), Hacker (2006, 231), Glock (2003, 23-30), and Macarthur (2008, 2). Other oftenmentioned influences (besides Dewey) are Nagel's (1961) *The Structure of Science* and Kuhn's (1962) *The Structure of Scientific Revolutions*. See Rosenberg (1996) and Kitcher (1992, §4). For Quine's influence on contemporary metaphysics and epistemology, see Putnam (2004, 78-9) and Feldman (2012).

7 The above timeline is confirmed by the fact that Quine's seminal "Epistemology Naturalized" (1969a) was initially titled "Stimulus and Meaning" (SM, 1965) and did not yet contain the term 'naturalism', suggesting that Quine had not yet decided to label his philosophy 'naturalistic' in 1965. A further clue is that the first version of "Ontological Relativity" (prepared in March

Quine's naturalism, as I shall argue in the chapters to come, can best be characterized in terms of an immanence-transcendence distinction. Quine rejects the idea of a transcendental extra-scientific perspective and replaces it with a rigorously science-immanent approach, defining naturalism as "the recognition that it is within science itself, and not in some prior philosophy that reality is to be identified and described" (TTPT, 1981d, 21). For Quine philosophy and science are continuous enterprises: we all start our reasoning within our "inherited world theory as a going concern" and try "to improve, clarify, and understand the system from within" (FME, 1975a, 72).[8]

Despite the fact that Quine borrowed the term 'naturalism' from Dewey, it would be a mistake to view his position as a mere continuation of the latter's ideas. Quine had already defended a naturalistic approach in all but name when he was still

1967 and presented at Chicago and Yale in May 1967, does not yet contain the term 'naturalism' either (nor a reference to Dewey) (OR67*, 1967). Finally, when Quine, before 1967, speaks about 'naturalistic arguments' and 'naturalists' in (LDHP, 1946a, 112) and (MSLT, 1953b, 149) respectively, his use of those terms there is distinct from the metaphilosophical view with which he would identify the term from the late sixties onwards.

8 Because the immanence-transcendence distinction will play such an important role in the chapters to come, let me note in advance that Quine uses the distinction in three distinct senses; two "mundane" (WDWD, 1999b, 164n7) and one "august" (RA, 1994d, 230). In *Philosophy of Logic*, Quine introduces an immanence-transcendence distinction for grammatical categories. A category is immanent when it is defined for a particular language, e.g. the class of *der*-words in German grammar, and transcendent when it is defined for languages generally (PL, 1970c, 19-22). Another 'mundane' use of the immanence-transcendence dichotomy can be found in (IV, 1991a, 242), where Quine distinguishes between metatheories which are and metatheories which are not expressed in the language of the object theory. In describing his ideas about the relation between science and philosophy, however, Quine is not referring to these mundane distinctions as they are "irrelevant to issues of naturalism" (WDWD, 1999b, 164n7). In what follows, therefore, I will limit my examination to Quine's 'august' immanence-transcendence distinction.

largely unfamiliar with Dewey's work,[9] and when he *did* start to study the early pragmatists in the late 1960s, he largely disagreed with their instrumentalist reading of inquiry, i.e. with their picture of science "as a conceptual shorthand for organizing observations". In particular, Quine questioned whether their instrumentalism is consistent with the naturalists' view that we always talk "within our going system when we attribute truth" and "cannot talk otherwise" (PPE, 1975e, 33-4).[10]

Something similar can be said about the naturalistic turn in general. Despite the fact that naturalism has a rich history, ranging from nineteenth-century views about the implications of the theory of evolution to the development of pragmatic naturalism in early twentieth-century American philosophy, it would be a mistake to view the naturalistic revolution in the second half of the twentieth century as a mere continuation of the developments sketched above.

To get a historically more accurate picture of Quine's position as well as the naturalistic turn in general, both should be discussed against the background of developments within the analytic tradition in philosophy. For not only did Quine develop his philosophy largely in response to Russell and Carnap, the naturalistic turn too is generally viewed as a revolution that took place within analytic philosophy.[11]

Since its inception in the late nineteenth century, analytic philosophers have evinced a deep respect for the achievements of

9 With a possible exception of C. I. Lewis, Quine seems to have been little influenced by the early pragmatists in general. See Koskinen and Pihlström (2006), R. Sinclair (2012, 336), and Godfrey-Smith (2014). For Quine's own account of his relation to the early pragmatists, see (PPE, 1975e), (CP, 1990d, 292), (RPR, 1992a, 213), and (TCL, 1994f, 60-1).

10 Quine's critique of the pragmatists on this score is discussed in more detail in section 4.5.

11 See Kitcher (1992), Kim (2003, 84), and Leiter (2004, 1-8). A notable exception is Hacker (2006, 231), who *does* see naturalism as a "pragmatist tradition". This, no doubt, has to do with his unusual definition of analytic philosophy, as shall become clear below.

both the empirical and the formal sciences.[12] Yet during the first half of the twentieth century, most analytic philosophers combined this respect for science with a firm anti-psychologism, i.e. with the idea that facts about psychology are irrelevant to questions of logical truth, justification, and meaning. Analytic philosophers, though generally applauding the developments in experimental psychology, believed that the field was categorically irrelevant to their inquiries into the question as to how we *ought* to reason.[13]

Mostly due to the influence of the early Wittgenstein, analytic philosophy's early anti-psychologism evolved into a general distinction between science and philosophy, the former an empirical discipline concerned with fact, the latter an a priori discipline concerned with meaning.[14] Many logical positivists and ordinary language philosophers eagerly adopted the distinction, dismissing the relevance of science for their inquiries.[15] In fact, the idea that there is a sharp distinction between science and philosophy, was so pervasive among early analytic

12 Some even argue that analytic philosophy can be defined in terms of its respect for science. See Rorty (1982, 220), Wang (1986, 75), and Quinton (1995, 30) as quoted in Glock (2008b, 160).

13 Most famous in this respect is Frege's attack on Mill's (1865, 359) claim that logic is a branch of psychology. Frege held that the "psychological is to be sharply separated from the logical" since logical truths are true regardless of whether we judge them to be true (Frege, 1884, 17). In a similar fashion, the early Moore and Russell attacked idealism for its implicit reliance on psychological notions and developed a metaphysics in which there is "no overt concern at all with the nature of thought or the mind or experience" as these notions were "looked on as psychological, and for this reason of no interest to philosophy" (Hylton, 1990, 108). For a wide-ranging study of the history of the debate on psychologism, see Kusch (1995).

14 See Ricketts (1985), Coffa (1991), and Hacker (1996).

15 For logical positivists the distinction was one between science and the "logic of science" (Carnap, 1934, §72), for ordinary language philosophers the distinction was one between "talk[ing] sense with concepts" and "talk[ing] sense about them" (Ryle, 1949, 7). In what follows, I focus on Carnap's version of the distinction.

philosophers that Hacker (1996) has claimed that this distinction defines the tradition.[16]

It is the background of this widespread distinction between science and philosophy that explains the revolutionary character of the naturalistic turn. And it was Quine, in his seminal "Two Dogmas of Empiricism" (1951b), who contributed most to the decline of this distinction. In arguing against two central dogmas of logical positivism—(1) the idea that we can strictly distinguish between analytic and synthetic statements, and (2) the idea that every synthetic statement can be tested in isolation—Quine undermined the distinction between questions of fact and questions of meaning on which the science-philosophy dichotomy was largely based. Against (1), Quine argued that there is no clear empirically acceptable definition of the analytic-synthetic distinction and against (2), Quine argued that individual statements can only be tested in conjunction with background theory, thereby advocating a thoroughly holistic picture of inquiry.

Although Quine is most famous for his rejection of (1), it is his holistic argument against (2) that is most significant from a historical perspective. After all, Quine's argument against (1) is solely concerned with the impossibility of finding an empirically acceptable explication of the distinction, whereas his argument against (2) shows that even if such a definition were to be found, it would be epistemically useless since a holistic picture of inquiry precisely shows that any analytic truth will be revisable in the light of adverse experience, and hence will be epistemologically indistinguishable from synthetic truths.[17]

16 Indeed, Hacker draws the radical conclusion that Quine's ideas "contributed [...] to the decline of analytic philosophy" (1996, xi) and that "[c]ontemporary philosophers who follow Quine have, in this sense, abandoned analytic philosophy" (1997, 10).

17 This is something Quine himself would later stress as well. See (RGH, 1986d, 207). See also (QSM, 1988, 27): "Once we appreciate holism [...] the notion of analyticity ceases to be vital to epistemology".

Quine's holism, i.e. his idea that individual hypotheses cannot be tested in isolation, is a relatively innocent observation about the logic of theory testing, first proposed by Pierre Duhem.[18] According to Quine, it is simply an empirical fact that we can never test a single hypothesis, a fact firmly supported by scientific practice. Still the idea is controversial, especially in discussions about Quine's philosophy. The reason is that Quine is often read as arguing for an extremely radical version of holism, suggesting that the scope of holism should be extended to "the whole of science" (TDE, 1951b, 41), and that confirmation and meaning are constituted holistically as well, ideas which are widely dismissed by philosophers of science and philosophers of language.[19]

Although, as shall become clear in the chapters to come, the relation between holism and naturalism is more complex than is often presupposed, it is evident that "Two Dogmas", in dismissing the picture of science and philosophy that had dominated analytic philosophy for a large part of its history, has contributed significantly to both Quine's naturalistic development and the naturalistic turn which took place in the following decades. For Quine himself became "more consciously and explicitly naturalistic" in the years immediately following "Two Dogmas" and contemporary naturalists also hail the paper for

18 See Duhem (1914, 187): "the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed".

19 See Dummett (1973), Fodor and Lepore (1992), Maddy (1992), Sober (1999), and Achinstein (2001). In chapter 5, I discuss several varieties of holism that have been attributed to Quine and argue that once one appreciates the naturalistic views that underlie his holism, his views on the matter turn out to be more straightforward than is often suggested.

its influence on their ideas about the relation between science and philosophy.[20]

1.2 NATURALISM AND HOLISM

The history of the naturalistic turn as sketched above suggests that there is a direct connection between Quine's holism and naturalism. After all, Quine's holistic picture of inquiry played an essential role in defusing the science-philosophy distinction that dominated early analytic philosophy. This connection between holism and naturalism is further substantiated by the essential role they both play in the Neurathian boat metaphor that Quine so often uses to illustrate his ideas.[21] On the one hand, the metaphor illustrates that we cannot test any hypothesis in isolation; that in plugging the leaks in one part of the ship we will always require some other part of the boat as a foothold. On the other hand, the metaphor emphasizes that there cannot be a transcendental extra-scientific perspective, that it is impossible to dock the boat and examine or repair its foundations while firmly standing on the shore.

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20 See Kitcher (1992, §4), Haack (1993a, 171), and Rosenberg (1996, 2). My emphasis on "Two Dogmas" as a strong influence on the contemporary naturalistic turn is of course not to deny the influence of Quine's more explicitly naturalistic work, in particular *Word and Object* (1960b) and "Epistemology Naturalized" (1969a). With respect to the last work mentioned, it is a little noted fact that Quine originally titled his paper "Epistemology naturalized; or, the case for psychologism" (ENP*, 1968a), something which provides further support for the claim that his ideas ought to be viewed in the light of the developments in early analytic philosophy as outlined above.

21 See Neurath (1932, 92): "We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components". Quine uses the metaphor in (IOH, 1950a, 79), (OME, 1952a, 223, 225), (PR, 1955, 253), (WO, 1960b, 3, 124, 210), (NK, 1969b, 126-7), and (FME, 1975a, 72). For Quine's relation to Neurath, see Koppelberg (1987, 1990) and (CK, 1990c, 212).

Despite this intrinsic connection between holism and naturalism in Quine's philosophy, however, neither the historical background nor the philosophical ramifications of this relation has been carefully examined in the literature. What is the exact relation between holism and naturalism in Quine's philosophy? How did Quine develop these positions? And what role does holism play in the contemporary naturalistic turn?

This dissertation aims to develop answers to these questions. I argue that the early development of Quine's naturalism goes hand in hand with his gradual adoption of an ever broadening holism, that his arguments against traditional conceptions of philosophy crucially depend on holistic presuppositions, and that the close relation between holism and naturalism in Quine's philosophy is responsible for some of the main problems contemporary naturalists identify in his work.[22]

1.3 READING QUINE IN CONTEXT

Only thirty years ago analytic philosophy's self-image was still predominantly ahistorical. Analytic philosophers thought of themselves primarily as seekers of truth, not of historical understanding. That is, they were largely committed to the idea of an eternal, theoretically neutral framework for philosophical inquiry—unaffected by context, free of presupposition.[23]

22 The problems I am referring to are the problems which have led many present-day naturalists to develop what they call 'pluralistic', 'liberal', or 'open-minded' naturalism. See Putnam (1981), Strawson (1983), Haack (1993a), Stroud (1996), Almeder (1998), and Maddy (1997, 2007) as well as the essays collected in De Caro and Macarthur (2004b, 2010).

23 See the introduction to Sluga's (1980, 2) book on Frege: "From its very beginning, the [analytic] tradition has been oriented toward an abstracted, formal account of language and meaning, and not toward the comprehending of concrete historical processes"; as well as the preface to Hylton's (1990, vii) book on the early Russell: "Analytic philosophy has largely rejected historical modes of understanding. [...] [A]nalytic philosophy seems to think of

To be sure, analytic philosophers did not neglect their intellectual predecessors: many interesting analytic studies have been written about a wide range of historical figures. Still in engaging with their philosophical forerunners, they primarily treated them as contemporaries, ignoring conceptual and contextual differences. Analytic philosophers, in other words, have been prone to rationally reconstruct their predecessors' views in contemporary terms, such that their theories could be evaluated in terms of "philosophical truth and falsehood".[24]

Analytic philosophy's lack of concern with the historical mode of understanding is not surprising however. For there seems to be a direct connection between these views on historiography and the anti-psychologism discussed in section 1.1 above. Since virtually all analytic philosophers were interested only in logical, not in psychological relations among propositions in their ordinary work, it should not be very surprising that they were also inclined to dismiss the relevance of both psychological and historical contexts in writing about their predecessors.[25]

Today the situation is entirely different. In the late eighties and early nineties, analytic philosophy witnessed what some have called a 'historical turn'. History of analytic philosophy is now widely viewed as an important field of study, comprising a relatively large community of researchers, as is evinced

itself as taking place within a single timeless moment". For a somewhat more distanced assessment of analytic philosophy's ahistoricism, see Peijnenburg (2000b), Glock (2008a), and Reck (2013a).

24 The phrase is Russell's. In the introduction to his book on Leibniz (1900, xx), Russell argues that "[p]hilosophical truth and falsehood [...] rather than historical fact, are what primarily demand our attention". Other often mentioned examples of ahistoricist works in the history of analytic philosophy are Strawson's (1966) reconstruction of Kant, Bennett's (1971) work on Locke, Berkeley, and Hume, and Kripke's (1982) interpretation of the later Wittgenstein. See, for example, Watson (1993) and Beaney (2013a, 59).

25 This link between anti-psychologism and ahistoricism is explicitly made in Beaney (2013a, 37).

by the ever increasing stream of new monographs, papers and collections.[26]

Whether there is a connection between the historical and the naturalistic turn is a matter of speculation and makes up an interesting subject for a different study.[27] Quine, in any case, did *not* combine his naturalism with a strong historical sensitivity. Although Quine includes history in his notion of 'science' (RTE, 1997a, 255), thereby suggesting that philosophy is also continuous with history, his own historical work has been criticized.[28]

As time passes, so do the frontiers of history. Where early historians of analytic philosophy were mainly interested in the nineteenth and the first half of the twentieth century,[29] today there is a growing historical interest also in philosophers who, like Quine, were largely active after the Second World War.[30] In Quine's case, in particular, such a historical approach is much greatly needed. For there is a general consensus among contem-

26 For an overview of some of the work, see Floyd (2009) as well as the essays collected in Floyd and Shieh (2001), Reck (2013b), and Beaney (2013b).

27 On the one hand, it seems that in rejecting the sharp distinction between the logical and the psychological, naturalists create philosophical space for a more historically informed philosophy. Yet on the other hand, the more philosophers come to think of their inquiries as scientific in the sense of 'natural science', the more they will be inclined to dismiss the relevance of the work of their predecessors. For some diverging opinions on the issue, see Taylor (1984), Rorty (1984), Hylton (1990, 2-7), and, more recently, Williamson (2014, §3).

28 See, for example, Friedman (1987, 1992) and Richardson (1990, 1992, 1998), who strongly dismiss Quine's interpretation of Carnap's (1928a) *Aufbau*. Notorious, in this respect, is Quine's quip that there are two sorts of people interested in philosophy: "those interested in philosophy and those interested in the history of philosophy". See MacIntyre (1984, 39-40).

29 Indeed, Floyd, in her overview, speaks about "the history of *early* analytic philosophy" as a field of study (2009, 157, my emphasis).

30 See Hylton (2001, 2002), Decock (2002c, 2004), Mancosu (2005, 2008), Ben-Menahem (2005, 2006), Isaac (2005, 2011), Ebbs (2011a, 2014), Frost-Arnold (2011, 2013), R. Sinclair (2012), Murphey (2012), and Lugg (2012). With respect to naturalism in general, little historical work has been done, as has also been noted, in a somewhat different context, by Richardson (1997).

porary Quine-scholars that, despite the large body of work on his philosophy, his work has been little understood.[31] Not only is Quine's work difficult to interpret because of its comprehensive character,[32] subtle shifts in his philosophy throughout his seventy-year long (!) academic career also require that one reads him in *historical* context.

It is precisely this that I aim to do in the present study. In examining the relation between naturalism and holism in Quine's philosophy, I focus both on how his views systematically hang together, and on how his choices, theories, and arguments are influenced by conceptual and historical contingencies. In doing so, I hope to have found a balance between historical and systematic concerns and thereby to contribute to the developing field of Quine studies as well as the historical turn in analytic philosophy in general.

1.4 PLAN

This dissertation is structured as follows. The first part offers a novel interpretation of Quine's arguments for naturalizing epistemology (chapter 2) and metaphysics (chapter 3) and it provides a historical reconstruction of his early development (chapter 4). Together, these chapters show how Quine's early naturalism crucially relies on his holism. The second part turns to the interplay between naturalism and holism in later stages of Quine's career. It shows how Quine softened his tone by slightly reconfiguring his views about logic and science, it explicates the holistic picture of inquiry underlying Quine's naturalism (chapter 5), and it shows that Quine can maintain his

31 See Kemp (2006, ix), Gregory (2008, 1), and Becker (2013, ix).

32 See Peijnenburg (2000a) and Hylton (2007, 1): "Many commentators have not sufficiently appreciated the extent to which his views hang together to form a coherent whole [...] anyone who approaches Quine's work primarily interested in one [topic] [...] is likely to miss the larger Quinean picture".

views in the face of the sustained criticisms from contempary naturalists who are sceptical about his holism (chapter 6).

In chapter 2, I start out with the *locus classicus* of Quine's naturalism in epistemology, his paper "Epistemology Naturalized" (1969a). On the basis of the argument in this paper, Quine's rejection of traditional epistemology (or 'first philosophy') is often claimed to be based on an argument from despair. According to this standard conception, Quine rejects first philosophy because all attempts to reconstruct our scientific theories in terms of sense experience have failed. I show that this picture is historically inaccurate and that Quine's argument against traditional epistemology is considerably stronger than this received view suggests. For Quine the first philosopher's quest for foundations is inherently incoherent; the very idea of a self-sufficient sense datum language it presupposes is without sense; there is no science-independent perspective from which to validate science. I argue that Quine's stronger argument relies on his holism, and that a great deal of the confusion surrounding Quine's argument is prompted by certain phrases in "Epistemology Naturalized". Scrutinizing Quine's work both before and after the latter paper provides a better key to understanding his naturalistic views about the epistemological relation between theory and evidence.

After this focus on Quine's argument against traditional epistemology, chapter 3 turns to Quine's position vis-à-vis traditional metaphysics. *Prima facie*, Quine's attitude toward metaphysics seems to differ from his attitude toward epistemology. For it is often claimed that Quine *saves* rather than dismisses metaphysics in arguing that ontological questions are "on a par with questions of natural science" (CVO, 1951a, 211). Where Carnap rejects metaphysical existence claims as meaningless, Quine is taken to restore their intelligibility by dismantling the former's internal-external distinction. In the chapter I argue that this popular view is incorrect and that Quine, like Car-

nap, rejects traditional metaphysics. I argue that a historically more accurate perspective on the Carnap-Quine debate should distinguish between two separate internal-external distinctions, only one of which is dismissed by Quine. In support of my interpretation I show that Quine, from the earliest stages of his career, defends a view about metaphysics that is in many respects similar to Carnap's and that the later Quine, in theorizing about the nature of both truth and reference, appeals to an internal-external distinction himself; a distinction moreover which shows that Quine's arguments against traditional epistemology and metaphysics are cut from the same cloth.

After this reconstruction of Quine's rejection of transcendental perspectives in epistemology and metaphysics, chapter 4 deals with the question of how Quine *developed* his naturalism. For even though Quine has always been a science-minded philosopher, he did not adopt a fully naturalistic perspective until the early 1950s. In this chapter I reconstruct the genesis of Quine's ideas on the relation between science and philosophy by examining his development in the first decades of his career. After identifying three commitments underlying his naturalism—viz. empiricism, holism, and realism—I trace the sources of these commitments to three distinct phases in Quine's early development, showing how his early empiricism gradually evolved into the naturalistic position that was to have such an tremendous impact on post-war analytic philosophy. In particular I show how Quine's adoption of a wide-scoped holism in the late 1940s was crucial to his development, thereby providing further evidence for the strong relation between Quinean naturalism and holism.

The first part of this dissertation, in short, shows how Quine's holism plays a crucial role in the early development of his naturalism as well as in his arguments for this position. In chapter 5, I zoom in on the question of how we are to understand Quine's holism. A great variety of holisms have been ascribed to Quine

in the literature, all of them to some extent controversial. In the chapter, however, I argue (1) that at the core of Quine's holism is a relatively innocent observation about the logic of theory testing and (2) that even Quine's ideas about the scope of holism are not as radical as they often appear. Furthermore, I reconstruct some developments in Quine's position in later stages of his career, showing how he slightly changed his views about the breadth of holism, the analytic-synthetic distinction, and the nature of logical truth and inference.

In chapter 6, finally, I discuss and evaluate two arguments which aim to show that there exists a fundamental tension between Quine's holism and his naturalism. First, I discuss Penelope Maddy's argument that Quine's naturalism is *too weak*. A true naturalist, Maddy argues, should take scientific practices at face value, not evaluate them in terms of their contribution to science as a whole. Against Maddy, I argue that Quine *can* accommodate what scientists are doing, thereby maintaining a naturalistic perspective, without giving up on his holism. Secondly, I discuss Susan Haack's argument that Quine's naturalism is *too strong*. According to Haack, Quine unconsciously vacillates between two notion of 'science', something which pushes him into the direction of an implausibly strong scientism. Against Haack, I argue that Quine's naturalism is more moderate than it might first appear and should not be interpreted as scientistic.

This study, in sum, is concerned with a historical and systematic investigation of naturalism as it was developed by Quine, the perspective on the relation between science and philosophy that has played such an important role in the contemporary naturalistic turn. In the conclusion I take up the question as to how my findings shed light on the issues that have been introduced in this first chapter: the interplay between holism and naturalism in Quine's philosophy and the presuppositions underlying the contemporary naturalistic turn.

Part I

IMMANENT AND TRANSCENDENT

2

QUINE'S ARGUMENT FROM DESPAIR

Summary: Quine's naturalism admits of both a positive and a negative characterization. Positively, Quine defines naturalism as the "recognition that it is within science [...] that reality is to be identified and described" (TTPT, 1981d, 21). Negatively, naturalism can be defined as the rejection of *first philosophy*. In this chapter, I offer a historical reconstruction of Quine's argument against first philosophy, an argument which is routinely perceived as an argument from despair. According to this standard conception, Quine rejects first philosophy because all attempts to reconstruct our scientific theories in terms of sense experience have failed. I show that this picture is historically inaccurate and that Quine's argument against first philosophy is considerably stronger and subtler than this received view suggests. For Quine, the first philosopher's quest for transcendental foundations is inherently incoherent; the very idea of a self-sufficient sense datum language it presupposes is without sense. [1]

2.1 INTRODUCTION

According to Quine, naturalism can be characterized negatively as the abandonment of "the goal of a first philosophy" prior to science (FME, 1975a, 72). Where traditional epistemology aspired to contain science by attempting to "construct it somehow from sense data", the naturalist rather sees epistemology as "contained in natural science" (EN, 1969a, 83). But what ex-

1 This chapter is an extended and slightly adapted version of the paper "Quine's Argument from Despair" (Verhaegh, 2014) that appeared in the *British Journal for the History of Philosophy* (volume 22, issue 1, pp. 150-173).

actly are Quine's reasons for rejecting first philosophy? Why, in other words, does Quine believe that we are bound to evaluate our epistemic practices from within, that we are "busy sailor[s] adrift on Neurath's boat" (FME, 1975a, 72)? In the present chapter, I examine Quine's ideas about first philosophy and reconstruct his argument for dismissing the project.

Prima facie, Quine's argument against first philosophy seems to be pretty straightforward: we ought to abandon traditional epistemology because, historically, all attempts to ground our beliefs have failed. In "Epistemology Naturalized" (1969a), Quine divides traditional epistemology into a doctrinal and a conceptual program and argues that neither project can be carried out satisfactorily. On the doctrinal side, Hume's problem of induction prevents us from deducing our beliefs about the world from basic observation statements. On the conceptual side, Quine criticizes the epistemologist's attempts to translate our theoretical concepts in sensory terms. In particular, he criticizes Carnap's project of rational reconstruction, arguing that it fails to "offer any key to translating the sentences of science into terms of observation, logic, and set theory" (EN, 1969a, 77). As an alternative to these projects, Quine proposes his naturalized epistemology, the study of how theory and evidence are *actually* related:

> If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect. (ibid., 78)

Where the traditional epistemologist rejects such a naturalism as circular, Quine believes that he is free to use scientific knowledge in his inquiries: "scruples against circularity have little

point once we have stopped dreaming of deducing science from observations" (ibid., 76).

Let me call this the *standard conception* of Quine's argument against first philosophy. In the standard conception, we are justified in adopting a naturalized epistemology only after we have established that all attempts to reduce our knowledge to sense experience have failed. Quine's argument, in other words, is construed as a conditional argument: we can legitimately take on a naturalized epistemology only when we have demonstrated that we ought to "stop dreaming of deducing science from sense data" (EN, 1969a, 84) and that we ought to "despair of being able to define theoretical terms generally in terms of phenomena" (FME, 1975a, 72). The Quinean naturalist is not a 'busy sailor' from birth, but "someone who later elects to enlist, perhaps in reaction to some deep disappointment" (Maddy, 2007, 85). Quine's argument, in short, is pictured as an *argument from despair*.[2]

The standard conception is widespread among both Quine scholars and critics. In "The Key to Interpreting Quine", for example, Roger F. Gibson summarizes Quine's arguments against the doctrinal and the conceptual program and concludes that "[t]he thesis that there is no first philosophy is a comment on the failure of traditional epistemologists to find a foundation outside of science upon which science [...] can be justified" (1992, 17). Similarly, P. M. S. Hacker claims that "[t]he failure of the Carnapian enterprise seemed to Quine to warrant the nat-

2 This apposite phrase is David Shatz's: "Quine arrived at [his] proposal by route of an argument we might term the argument from despair. The traditional project of validating common sense and scientific beliefs in the face of skeptical challenge has been, and is doomed to be, a failure; therefore, the project is best dropped" (1994, 117). According to Shatz, the alternative to an argument from despair is a dialectical naturalism, which aims to "confront the problem of skepticism and of circularity head on". Shatz believes that Quine in some places "provides a partial defense of dialectical naturalism" (ibid., 120).

uralization of epistemology" (2006, 236), and Penelope Maddy argues that the Quinean naturalist seems to be "driven to her position by 'despair' at the failure of any or all attempts to 'ground' science" (2007, 85).[3]

Still there seems to be something odd about the standard conception. For one thing, Quine's argument from despair only occurs in "Epistemology Naturalized" (1969a) and in "Five Milestones of Empiricism" (1975a). The argument is conspicuously absent in Quine's work before and after these two papers, even when he discusses the distinction between traditional and naturalized epistemology. This gap is particularly apparent in *From Stimulus to Science* (1995b). In the first chapter of this book, Quine gives an extended summary of the traditional quest for certainty, starting with sceptical worries about our knowledge of the external world and ending with Carnap's project of rational reconstruction. Yet in the second chapter, which deals with his naturalism, Quine nowhere uses the traditional epistemologists' failure as an argument for adopting a naturalistic perspective. Rather, he reflects on the "phenomenalistic orientation" of the traditional project, i.e. about "[t]he idea of a self-sufficient sensory language as a foundation for science" (FSS, 1995b, 15).

Second, if the argument from despair were all he had to offer, Quine would not have made a particularly strong case for the naturalization of epistemology. For as many epistemologists have objected, it is one thing to dismiss the traditional quest for absolute foundations, it is quite another thing to reject the search for justification *tout court* and to claim that "[e]pistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science" (EN, 1969a, 82).[4] Quine only shows that we cannot *completely* ground our beliefs on sense experience, an argument that is too weak to con-

3 See also, for example, Roth (1999, §2), Kertész (2002, §3), and Fogelin (2004, 19-27).

4 The *loci classici* of this argument are Putnam (1982) and Kim (1988).

vince any first philosopher who shares the former's scepticism about the Cartesian dream. In response to Quine's despair, traditional epistemologists could easily adopt a "*moderate* first philosophy, which eschews certainty but which allows for the independence (of epistemology from science) sought by the traditionalist" (Siegel, 1995, 53).

In this chapter, I argue that the standard conception is mistaken. I show that Quine's argument against the first philosopher is considerably stronger than the standard conception suggests. In works both before and after "Epistemology Naturalized", Quine does not abandon traditional epistemology out of despair but because the project is demonstrably flawed from the beginning. According to Quine, it is a mistake to believe that one can develop a self-sufficient sensory-language, independent of our best scientific theories of the world. The first philosopher does not fail because he aims at Cartesian certainty, but because he presupposes that he can adopt some science-independent perspective. I argue, in short, that "Epistemology Naturalized", when considered in isolation from the rest of his work, misrepresents the strength of Quine's position.[5]

What I offer, then, is a historical reconstruction of Quine's actual argument against first philosophy, focusing on his work both before and after "Epistemology Naturalized". This chapter is structured as follows. I start by outlining the standard conception and examining Quine's argument from despair (section 2.2), after which I introduce his stronger argument (section 2.3) and show how he uses it to dismiss both the traditional epistemologist (section 2.4) and the sceptic (section 2.5). Next, I analyze Quine's views about the theory-evidence relation, which underlie his argument, and show that his position is more nuanced than it might initially seem to be (sections 2.6-

5 Note that this chapter focuses exclusively on Quine's negative claim, i.e. on his argument against traditional epistemology. Quine's positive characterization of naturalism will be the subject of the sections 4.2-4.5.

2.7). I end this chapter with an analysis of how we might better read "Epistemology Naturalized" in the light of these findings (section 2.8).[6]

2.2 FROM CERTAINTY TO STRAIGHT PSYCHOLOGY

Although we seem to know a great many things about ourselves and the world around us, we can never be absolutely sure that our beliefs are true. Even our best scientific theories, history has taught us, might turn out to be false or to rest on misguided assumptions. According to Quine, traditional epistemology starts from a deep dissatisfaction with this situation: "the theory of knowledge has its origin in doubt, in scepticism. Doubt is what prompts us to try to develop a theory of knowledge" (NNK, 1975d, 257).[7] In order to restore confidence in both our everyday convictions and our scientific theories, traditional epistemologists seek to ground our beliefs upon something more secure. Quine often refers to this project as 'the Cartesian dream',

6 A great deal of the confusion on the part of the standard conception seems to be triggered by certain phrases in "Epistemology Naturalized". I am not the first to point at the somewhat problematic relation between this paper and the rest of Quine's work. See Putnam (1982, 244) and Johnsen (2005). Johnsen concludes that Quine himself is to blame for this confusion: "the fault lies not in the universal incompetence of the essay's readers, but rather in a disastrous failure of its author's outsized gifts as an expositor of his own views" (2005, 79). For the purposes of this chapter, it should be noted that Johnsen focuses almost exclusively on Quine's positive claim that epistemology becomes a chapter of psychology; a claim that wrongly implies that Quine comes to reject normative epistemological questions (see section 6.11). I take it that my reading of Quine in this chapter is compatible with Johnsen's interpretation and see my main claim as complementary: not only does "Epistemology Naturalized" fail to accurately delineate Quine's positive views, as Johnsen claims, it also misrepresents Quine's arguments against traditional epistemology.

7 See also (LDHP, 1946a, 50-51) and (FSS, 1995b, 1).

the dream of an indubitable foundation for our beliefs about ourselves and our surroundings.[8]

In analyzing the epistemologists' quest for certainty, Quine has focused almost exclusively on *empiricist* attempts to ground our knowledge.[9] In his discussion of empiricist epistemology, Quine distinguishes two projects, one doctrinal and one conceptual. The doctrinal project is concerned with truth and aims at deriving our beliefs about the world, especially our well-established scientific theories, from basic observation statements. The conceptual project, on the other hand, is concerned with meaning and aims at translating our scientific concepts in sensory terms. The two projects are connected: if one succeeds in defining all scientific concepts in sensory terms, then one's scientific beliefs and one's basic observation statements will be couched in the same sensory language, an accomplishment that will enable one to examine whether the former can be derived from the latter (EN, 1969a, 69-71).

According to Quine, the classical empiricists failed in both respects. On the conceptual side of epistemology, Locke, Berkeley, and Hume were unable to indicate how our complex ideas about the world can be constructed out of indubitable simple ones; defining even the very notion of an enduring physical body turned out to be problematic. Still their problems were worse on the doctrinal side. For Quine, it was Hume who showed that it is impossible to establish a deductive relation between theory and evidence even if both are couched in the

8 See (PT, 1990g, 19). In referring to the quest for certainty as 'the Cartesian dream', Quine is referring to Descartes' tremendous influence in putting the search for foundations on the philosophical agenda. Quine is aware, however, that Descartes was not the first to attempt to ground our knowledge about the world. He recognizes that "the quest for certainty goes back to Plato, and nobody knows how far beyond" (LDHP, 1946a, 52).

9 An exception is (LDHP, 1946a, 54-9), where he explicitly discusses and rejects *rationalism*. Most notably, Quine questions whether the rationalist can be certain that her innate ideas are true, even if they seem self-evident.

same sensory language; neither general statements nor singular statements about the future can be deduced from any finite set of sensory evidence (EN, 1969a, 71-2).

Quine is convinced that there is no progress to be made with respect to the doctrinal project: "The Humean predicament is the human predicament" (ibid., 72). Although the value of inductive reasoning in science can hardly be overestimated, the traditional epistemologist simply has to admit that we are never strictly entitled to rely on induction (RA, 1994d, 231-3). Still there was progress to be made with respect to the conceptual project. Quine argues that some major advances in the eighteenth and nineteenth century breathed new life into the empiricists' program.

One of these developments was Jeremy Bentham's work on contextual definition, or what he called *paraphrasis*. The classical empiricists had developed what Quine has called a "term-by-term empiricism" (TDE, 1951b, 42).[10] Their goal was to define

10 Strictly speaking, classical empiricism is better characterized as an 'idea-by-idea' empiricism, since its aim is to define complex *ideas* in terms of simple ones. Quine has often credited John Horne Tooke, a contemporary of Hume, for shifting the empiricists' attention from ideas to words: "Tooke appreciated that the *idea* idea itself measures up poorly to empiricist standards" (FME, 1975a, 68). See also (TDE, 1951b, 38-9), (FM, 1977, 271-2), and (FSS, 1995b, 6). According to Quine, the "idea idea" was a leftover from Cartesian rationalism. Where rationalists stressed that we should ground our beliefs upon clear and distinct ideas, the classical empiricists rejected innate ideas and replaced them with the impressions we obtain through the use of our senses. Still even though they disagreed with the rationalists on the *source* of our ideas, the empiricists maintained the view that our beliefs about the world should be grounded in *ideas* that are clear and distinct (LDHP, 1946a, 63). So although the classical empiricists took a big step in repudiating innate ideas, bringing the process of belief acquisition more out into the open, they failed to see that the empiricist project would gain much in clarity if it were phrased as the program of defining the *words* we use in sensory *terms*; for "[i]f there is sense to be made of the compounding of ideas, clearer sense can be made of the compounding of language. Words, unlike ideas, are out where we can see what we are doing" (FSS, 1995b, 6).

our complete vocabulary in sensory terms. Bentham showed, however, that terms can also be defined contextually; one can define a term simply by showing how all sentences containing the term can be paraphrased into sentences without it. In general, one can contextually define a word *W* on the basis of some accepted defining vocabulary *V*, by explaining how to paraphrase every sentence *S* in which *W* occurs, into a new sentence that contains only words of *V* and *S* other than *W* (VD, 1972, 55).[11]

According to Quine, Bentham's method proved to be greatly beneficial for the empiricists' conceptual studies. Instead of defining our complete theoretical vocabulary in sensory terms, the empiricist could now also choose to explain away some terms as fictions:

> Hume's [...] desperate measure of identifying bodies with impressions ceased to be the only conceivable way of making sense of talk of bodies, even granted that impressions were the only reality. One could undertake to explain the talk of bodies in terms of talk of impressions by translating one's whole sentences about bodies into whole sentences of impressions, without equating the bodies themselves to anything at all. (EN, 1969a, 72)

Next to Bentham's method of paraphrasis, Quine lists the development of set theory in the late nineteenth century as an advancement that led to substantial progress on the conceptual side of the empiricists' quest for certainty. Taking our sense impressions as the fundamental objects of a set theoretic structure, the empiricist "is suddenly rich: he has not just his impressions

11 See also (CD, 1995a). As an example, consider the mathematical expression $\frac{\sin(\pi)}{\cos(\pi)}$. This expression may be abbreviated as $\tan(\pi)$ without defining 'tan' directly in terms of 'sin' and 'cos'. It is enough to relate definiendum and definiens contextually such that for all x, $\tan(x)$ equals $\frac{\sin(x)}{\cos(x)}$ (TC, 1936, 78).

to play with, but sets of them, and sets of sets, and so on up" (ibid., 73); he has access to an infinite universe of sets containing all possible combinations of impressions.

According to Quine, Russell was the first the see the epistemological potential of these logical and mathematical advancements.[12] Still Quine credits Carnap as the philosopher who actually attempted to carry out the project by using these formal tools to construct our beliefs about the world out of primary sense experiences. According to Quine, Carnap's *Der Logische Aufbau der Welt* (1928a) constituted "a masterful construction" of the external world from the data of sensation "using the sophisticated devices of mathematical logic" (CA, 1987b, 144).[13] Carnap opens the *Aufbau* with the claim that he attempts to es-

12 See (ROD, 1966a, 83-4) and (EN, 1969a, 73-4) for Quine's reading of Russell's *Our Knowledge of the External World* (1914). It should be noted, however, that Quine's reading is not uncontroversial. According to Pincock (2007), for example, Russell should not be interpreted as reviving the empiricist project.

13 Here too it should be noted that this is *Quine's* interpretation of the *Aufbau*. Friedman (1987, 1992), Tennant (1994), and Richardson (1990, 1992, 1998), among others, have criticized Quine's reading and developed an alternative interpretation in which Carnap's choice to work from a phenomenalist basis in the *Aufbau* was merely arbitrary. In this reading, Carnap's intention "is not so much to give a traditional empiricist justification for our knowledge of the external world as to exhibit what Carnap calls the "neutral basis" common to *all* epistemological views—whether empiricist, transcendental idealist, realist, or subjective idealist" (Friedman, 2007, 5). In a reply to Tennant, Quine recognizes that there are some passages in the *Aufbau* that support such a neutralist reading, but he maintains that the *Aufbau* was initially supposed to be part of the phenomenalist project:

> I have a further hypothesis [. . .] to account for Carnap's profession of neutrality between a phenomenalistic basis and a physicalistic one. I picture Carnap as having been a single-minded phenomenalist when he devised the constructions that went into the *Aufbau*. When the book was about ready for printing, I picture Neurath pressing the claims of physicalism. I then picture Carnap writing and inserting those paragraphs of disavowal by way of reconciling the book with his changing views. Significantly, he took the physicalistic line in his subsequent

tablish a *constructional system*, "a step-by-step derivation or 'construction' of all concepts from certain fundamental concepts" (Carnap, 1928a, §1). The concept he chooses as the foundation of his construction is what he calls an *elementary experience*, an individual's totality of experiences at a given moment in time involving all sense modalities.[14] Next to a fundamental concept, Carnap also introduces the dyadic predicate Rs. This relation Rs holds between two elementary experiences x and y whenever the subject recognizes x and y as partially similar (ibid., §78). Using only these very basic elements,[15] Carnap manages to define "a wide array of important additional sensory concepts which, but his constructions, one would not have dreamed were definable on so a slender basis" (TDE, 1951b, 39). Most importantly, Carnap succeeds in constructing the five sense modalities as well as the basic sense qualities that are taken for granted in the classical empiricists' epistemological framework.

After having constructed these basic sensory concepts, Carnap attempts to step outside the subjective arena of experience into the intersubjective world. As a first step, Carnap wants to assign the sense qualities in our visual field—i.e. the colors in our two-dimensional visual space—to points in the three-dimensional physical space order, a manoeuvre that Carnap be-

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> writings, and refused permission to translate the *Aufbau* for more than thirty years. (CNT, 1994a, 216)

See also (FSS, 1995b, 13-4). Whether or not this reading is correct; in what follows I mainly limit my discussion to Carnap's project as it is conceived by Quine.

14 Note that this description of an elementary experience only makes sense from the point of view of the finished construction; individuals and sense modalities are not presupposed at the start of Carnap's project.

15 In fact, Carnap even succeeds in defining elementary experiences in terms of Rs, such that his construction makes only essential use of logic, mathematics, and one dyadic predicate. Moreover, in the sections 153-5, Carnap attempts to eliminate Rs as well by introducing the notion of *foundedness* into his logic and by defining Rs in terms of it. Scholars have been critical of Carnap's success on this score, however. See Friedman (1987, 532-3).

lieves to be "one of the most important steps in the constructional system" (Carnap, 1928a, §124). The idea, as Quine notes, is to translate sentences of the form 'Quality q is at point-instant x;y;z;t' in terms of the fundamental notions that Carnap allows in his constructional scheme (TDE, 1951b, 40).

As he himself has recognized in the preface to the second edition of the *Aufbau*, however, Carnap did not succeed in constructing the intersubjective 'is at' connective from any of the subjective lower level concepts:

> One of the most important changes [in the second edition] is the realization that the reduction of higher level concepts to lower level ones cannot always take the form of explicit definitions [...] Actually, without clearly realizing it, I already went beyond the limits of explicit definitions in the construction of the physical world. For example, for the correlation of colors with space-time points, only general principles, but no clear operating rules were given. (1928a, viii)

Instead of providing a full translation of our color-assignments, Carnap was only able to provide a list of desiderata that any assignment of colors to space-time points should satisfy "as far as possible", while being aware that they can never be "precisely satisfied" (ibid., §126).

It is important to see why Carnap's reduction broke down at this point. Carnap's desiderata for assigning colors to world points only prescribe a complete assignment, not a point-by-point allocation. The reason for this is that one needs to distinguish between genuine information from the outside world and subjective color experiences such as hallucinations and disturbances of the eye (ibid.). The problem for Carnap is that one can only judge whether some experience is hallucinated when one examines whether it fits in one's total allocation of visual

experiences over time. One cannot judge whether a single experience is hallucinated on the basis of that very experience alone; "the assignment of sense qualities to public place-times has to be kept open to revision in the light of later experience, and so cannot be reduced to definition" (ROD, 1966a, 85). In consequence, one cannot assign one color to a particular space-time point without considering its place holistically in the total color-to-world allocation. Carnap's construction broke down, in other words, because he failed to take into account the holistic nature of the theory-evidence relation.[16]

In response to his failure to develop a satisfactory criterion of empirical significance, Carnap radically altered his views after the *Aufbau*. In "Testability and Meaning" (1936; 1937), Carnap gave up on the idea that theoretical sentences should be strictly translatable into the observation language if they are to be empirically significant. Instead, he introduced a liberal form of reduction that allows theoretical sentences to be correlated with lower level sensory sentences in a way short of translatability. Rather than demanding strict reductions such that theoretical sentences are eliminated in favor of observation sentences, Carnap now also admitted *reduction sentences* that define new theoretical terms only relative to specified experimental conditions.[17]

In "Epistemology Naturalized", Quine argues that Carnap's adjustments were fatal for traditional epistemology. For in dis-

16 See also (CPT, 1984a, 125-6): "A typical single sentence of a theory has no distinctive empirical content of its own; it can be singled out for testing, but only by agreeing meanwhile to hold other sentences of the cluster immune [...] in the *Aufbau* the very mechanism of [this] Duhem effect is strikingly and imaginatively depicted".

17 Carnap (1936, §8) defines reduction sentences as follows. Let Q_3 be a theoretical predicate, let Q_1 and Q_4 describe experimental conditions which have to obtain in order to find out whether or not a space-time point b has the property Q_3, and let Q_2 and Q_5 describe possible results of the experiments. Then Q_3 can be introduced as a new predicate in one's language by means of the following pair of sentences R_1 and R_2:

pensing with reduction by elimination, "the empiricist is conceding that the empirical meanings of typical statements about the external world are inaccessible and ineffable" (EN, 1969a, 78-9). That is, in allowing more liberal forms of reduction, Carnap acknowledged that he would never be able to completely specify the empirical meanings of isolated theoretical sentences. "Epistemology Naturalized", in other words, seems to construe Carnap's concession as a natural endpoint for traditional epistemology. Where Hume had already demonstrated that we cannot hope to fulfil the doctrinal project, Carnap's *Aufbau* showed that the conceptual project is too demanding as well. Quine argues that to "relax the demand for definition, and settle for a kind of reduction that does not eliminate, is to renounce the last remaining advantage that [...] rational reconstruction [had] over straight psychology; namely, the advantage of translational reduction" (ibid., 78). We ought to "stop dreaming of

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(R_1) $\quad Q_1 \rightarrow (Q_2 \rightarrow Q_3)$

(R_2) $\quad Q_4 \rightarrow (Q_5 \rightarrow \neg Q_3)$

Definitions of this form are *partial* definitions because the meaning of Q_3 is only specified relative to a set of experimental conditions Q_1 and Q_4.

Shortly after "Testability and Meaning" (1936; 1937) Carnap recognized that even these partial definitions are not yet liberal enough; sentences containing highly theoretical concepts like 'absolute temperature' and 'ψ function' resist an operationalist interpretation. Again the problem was the holistic character of the theory-evidence relation. In the laboratory, a negative test result does not necessarily imply that a certain disposition is not present (the scientist can always maintain her belief that a disposition is present by revising one of her auxiliary hypotheses). Similarly, a positive test does not imply that the disposition *is* present. Reduction sentences, however, do not allow for these possibilities as they are intended to state necessary conditions for the application of a term. See Hempel (1952, 32) and Carnap (1956, 68). In response, Carnap proposed an even more liberal criterion of empirical significance, one which recognizes that "[t]he definition of meaningfulness must be relative to a theory T, because the same term may be meaningful with respect to one theory but meaningless with respect to another" (ibid., 48). Yet even this definition did not fully implement the lessons of holism as Quine shows (CPT, 1984a, 125).

deducing science from sense data" (ibid., 84) and we ought to "despair of being able to define theoretical terms generally in terms of phenomena" (FME, 1975a, 72). Hence, we are better off studying the actual relation between theory and evidence.[18]

2.3 TWO STRATEGIES

The argument outlined above is mainly concerned with the empiricists' ideas about the *relation between* theory and evidence, with their attempts to *connect* our scientific beliefs with our primary sense experiences. Quine argues that we are unable to ground our beliefs on sense experience and that we cannot translate our theoretical vocabulary in observational terms. Schematically, the problem is that we have (A) our primary sense-experiences, and (B) our best scientific theories, but that we do not seem to be able to relate (A) and (B) in an epistemologically satisfying way. The holistic character of the theory-evidence relation prevents us from establishing an epistemologically satisfying connection between the two because a typical single (B)-sentence "has no distinctive empirical content of its own" (CPT, 1984a, 125).

Still criticizing the epistemologist's ideas about the relation between theory and evidence is not the only way to challenge the traditional project. There remains a second option. Instead of showing that all attempts to base our scientific beliefs on some science-independent foundation have failed, one can also

18 Or, as he put it already in his Oxford Lectures of 1953: "To start from scratch [and] obtain science by pure incontrovertible reason—that's out. The rationalist dream. To start with a *tabula rasa* and fill it in with pure experience by [definition and] logic—that's out. The phenomenalist dream. But something of the original ill-formulated motivation of epistemology does remain valid. The [twofold] root of scientific knowledge: 1) the stimulation of our end organs by rays and molecules [...] 2) our inner apparatus [...] for weaving theories. All this is subject matter for science" (OLPL*, 1953c, my transcription).

attempt to criticize the very idea of a science-independent foundation itself. That is, instead of challenging the *nature* of the relation between (A) and (B), one can also call into question the *epistemological value* of connecting (B) with (A) in the first place. One could, for example, dismiss the traditionalist's ideas about the epistemological status of (A) and argue that sense experience does not constitute a truly science-independent foundation to begin with.

In "Epistemology Naturalized", Quine does not discuss this second option.[19] That is, he does not question the idea of a self-sufficient sensory language presupposed in the epistemologist's attempts to reduce science to sense experience. Quine only argues that *once* we have adopted a naturalized epistemology, we can substitute our talk about sense data with talk about its scientific analogue, the physical stimulation of our sensory receptors:

> one *effect* of seeing epistemology in a psychological setting is that it resolves a stubborn old enigma of epistemological priority. [...] In the old epistemological context [...] we were out to justify our knowledge of the external world by rational reconstruction, and that demands awareness. Awareness ceased to be demanded when we gave up trying to justify our knowledge of the external world by rational reconstruction. What to count as observation now can be settled in terms of the stimulation of sensory receptors. (EN, 1969a, 84, my emphasis)

19 At least, Quine does not discuss this second option when it concerns the empiricist program of reducing science to sense data. Quine *does* use the second strategy when he dismisses the logicist program of reducing mathematics to logic and set theory. Quine argues that the logicists failed because their foundations were not truly mathematics-independent. According to Quine, set theory is itself a branch of mathematics, and so the logicists failed to do "what the epistemologist would like of it", i.e. revealing the ground of mathematical knowledge (EN, 1969a, 70).

In the remainder of this chapter, I argue that both before and after "Epistemology Naturalized", Quine argues exactly the other way around. Quine does not give up on sense data because of his naturalism. Rather, he naturalizes epistemology because of his doubts about the idea of "a self-sufficient and infallible lore of sense data" (NLOM, 1995c, 462). That is, Quine's doubts about "epistemological priority" are not a consequence of his naturalism, they are the very reason he adopts a naturalized epistemology in the first place. Both before and after "Epistemology Naturalized", in short, Quine *does* use the second strategy; he criticizes the traditional project because he believes that attempts to connect (A) and (B) are futile from an epistemological perspective.

2.4 SELF-SUFFICIENT SENSORY LANGUAGES

A first argument for my reading of Quine's rejection of first philosophy is based on his ideas about the sensory basis of science. From the very beginning of his philosophical career, Quine has thought about the relative benefits of phenomenalistic ontologies. Already in "On What There Is" (1948), for example, Quine posed the question of whether we should adopt a "phenomenalistic" or a "physicalistic conceptual scheme". His position was a pragmatic one: we want an ontology that is as simple as possible, but both conceptual schemes are simple in their own respects.[20] A phenomenalistic ontology posits only subjective events of sensation, whereas a physicalistic scheme can be said to offer conceptual simplicity (OWTI, 1948, 17).

20 Note that in talking about 'conceptual schemes' here, Quine is not invoking a distinction between conceptual schemes and languages; for Quine they are one and the same. See (VITD, 1981a, 41): "Where I have spoken of a conceptual scheme I could have spoken of a language".

Despite this pragmatic attitude,[21] however, Quine was aware that we cannot reduce our complete vocabulary to sensory terms, i.e. that the idea of a complete rational reconstruction is an idle dream: "there is no likelihood that each sentence about physical objects can actually be translated [...] into the phenomenalistic language" (ibid., 18). A few years later, in "Two Dogmas Empiricism", Quine explained why such a strict reduction is impossible: "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (TDE, 1951b, 41).

As a result, the main ingredients of "Epistemology Naturalized" were already in place in the early 1950s: Quine was already familiar with the possibility of adopting a purely physicalistic conceptual scheme and he had already shown that the traditional epistemologists' attempts at reduction were fruitless. Still Quine had not yet adopted a naturalized epistemology at this point. He still believed that there might be epistemological reasons for adopting a phenomenalistic conceptual scheme:

> From among the various conceptual schemes best suited to [...] various pursuits, one–the phenomenalistic–claims epistemological priority. Viewed from within the phenomenalistic conceptual scheme, the ontologies of physical objects and mathematical objects are myths. The quality of myth, however, is relative; relative, in this case, to the epistemological point of view. (OWTI, 1948, 19)

This situation had not changed in "Two Dogmas", where Quine continued to talk about "sense data" in describing the evidential boundaries of his newly developed holistic empiricism (TDE, 1951b, 44).[22]

21 As we shall see in the sections 4.7 and 4.11, it is not completely correct to think of Quine as defending a pragmatic position on this issue. For our present purposes, however, it will do.

22 See also Quine's introduction to the first edition of *Methods of Logic* where he claims that "[t]he seeing of a green patch, and the simultaneous utterance

Between "Two Dogmas" (1951b) and Word and Object (1960b) however, Quine *did* switch exclusively to a physicalistic conceptual scheme. Looking back on this period, Quine has referred to the ten years between these two works as the decade in which he became "more consciously and explicitly naturalistic"; as the period in which he "stiffened up his flabby reference to 'experience' by turning to our physical interface with the external world: the physical impacts of rays and molecules upon our sensory surfaces" (TDR, 1991b, 398). That is, in the decade following "Two Dogmas", Quine adopted a physicalistic conceptual scheme and started to talk exclusively about the stimulation of sensory receptors.[23]

So why did Quine give up on phenomenalism? Did Quine give up on sense datum languages out of despair? Did he, in other words, come to regard the traditional perspective as hopeless because we cannot reduce science to sense experience? No, he did not. What changed is that he became convinced that the very idea of a sense datum language is not *epistemologically prior to* but *dependent on* our best scientific theories of the world; that "[s]ense data are posits too" (PR, 1955, 252). Quine came to believe that the traditional project was flawed from the beginning; in appealing to a phenomenalistic language as a starting point for her inquiries, the epistemologist already presupposes a good deal of science:

> Talk of subjective sense qualities comes mainly as a derivative idiom [...] Impressed with the fact that we know ex-

'Green patch now', constitute the sort of composite event which, in its rare occurrences, gladdens the heart of the epistemologist" (ML1, 1950c, xi). See Murphey (2012, 89). Interestingly, Quine has deleted this in the fourth edition of *Methods of Logic*. There he only talks about "the utterance of a statement on the occasion of a stimulation to which that string of words has become associated" (ML4, 1982b, 1). I thank Thomas Ricketts for this suggestion.

23 A more detailed historical account of the evolution of Quine's naturalism in the first decades of his career will be provided in chapter 4. In this section, I focus on Quine's reasons for abandoning phenomenalism.

> ternal things only mediately through our senses, philosophers from Berkeley onward have undertaken to strip away the physicalistic conjectures and bare the sense data. Yet even as we try to recapture the data, in all their innocence of interpretation, we find ourselves depending upon sidelong glances into natural science. (WO, 1960b, §1)

Traditional epistemology builds on the idea that sense data are independent of our basic theories of the world. This is why a reduction of our beliefs to sense data would constitute a major epistemological achievement. Quine, however, became convinced that this presupposition is incorrect.[24]

As an example of the dependence relation between science and sense data, Quine discusses the idea that our elementary experiences are two-dimensional, an idea that Carnap also presupposed in the *Aufbau* when he wanted to assign the sense qualities in our two-dimensional visual field to points in the three-dimensional physical space order. According to Quine, however, the idea that our elementary visual experiences are two-dimensional is itself based on rudimentary science:

> The old epistemologists may have thought that their atomistic attitude toward sense data was grounded in introspection, but it was not. It was grounded in their knowledge of the physical world. Berkeley was bent on deriving depth from two-dimensional data for no other reason than the physical fact that the surface of the eye is two-dimensional.[25] (RR, 1973, 2)

24 It should be noted that this is not Quine's only argument against sense data. See (WO, 1960b, §§48-9) and, for an extensive list, Gibson (1982, 157-9). Given the purposes of this chapter, I will here focus on Quine's argument against the epistemic priority of sense data.

25 See also (WO, 1960b, 2): "We may hold, with Berkeley, that the momentary data of vision consist of colors disposed in a spatial manifold of two dimen-

Not only the empiricists' ideas about the two-dimensional basis of their construction depend on scientific knowledge, but even the very empiricism that underlies their attempts to construct science from sense data depends on their scientific picture of the world. According to Quine, the empiricists' claim that all knowledge is empirical can only itself rely on empirical knowledge:

> The champions of atomic sense data were seeking the unscientific raw materials from which natural science is made, but in so doing they were being guided, all unawares, by an old discovery that was the work of natural science itself [...] It is the discovery that all our information about the external world reaches us through the impact of external forces on our sensory surfaces [...] This is a scientific finding, open, as usual, to reconsideration in the light of new evidence.[26] (SSS, 1986l, 328)

As a result, the supposedly science-independent sense data, the so called neutral basis for a purely epistemological foundation

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sions; but we come to this conclusion by reasoning from the bidimensionality of the ocular surface, or by noting the illusion which can be engendered by two-dimensional artefacts such as paintings and mirrors, or, more abstractly, simply by noting that the interception of light in space must necessarily take place along a surface"; and (NNK, 1975d, 258): "the accepted basis of the construction, the two-dimensional visual field, was itself dictated by the science of the external world [...] The light that informs us of the external world impinges on the two-dimensional surface of the eye, and it was Berkeley's awareness of this that set his problem".

26 See also (OME, 1952a, 225): "The crucial insight of empiricism is that any evidence for science has its end points in the senses. This insight remains valid, but it is an insight which comes after physics, physiology, and psychology, not before"; and (WO, 1960b, 2): "The motivating insight, viz. that we can know external things only through impacts at our nerve endings, is itself based on our general knowledge of the ways of physical objects—illuminated desks, reflected light, activated retinas. Small wonder that the quest for sense data should be guided by the same sort of knowledge that prompts it".

for science, are theoretical posits as much as the physical objects that the traditional epistemologist attempts to construct from them. For Quine, the only epistemological difference between the two is that our physicalistic conceptual scheme is what *actually* ties our experiences together: "The memories that link our past experiences with present ones and induce our expectations are themselves mostly memories not of sensory intake but of essentially scientific posits, namely things and events in the physical world" (FSS, 1995b, 15). We construct sense data only after we have acquired an object-based conceptual scheme. This is why painters have to be trained to reproduce their three-dimensional view of the world into a two-dimensional picture (GT, 1970a, 1).

The standard conception presupposes that traditional epistemology fails because we ought to despair of deducing science fully from sense data. The present reflections show, however, that Quine's rejection of traditional epistemology beyond "Epistemology Naturalized" is guided by the second strategy distinguished above. For Quine, the epistemologists' quest for foundations was misguided from the beginning; there is no prior sense datum language, no transcendental science-independent perspective from which to validate science.[27]

2.5 QUINE'S RESPONSE TO THE SCEPTIC

Quine's rejection of first philosophy thus seems to be guided by an argument against transcendence, not by despair. Quine

27 See also, (WO, 1960b, 3): "[t]here is every reason to inquire into the sensory or stimulatory background of ordinary talk of physical things. The mistake comes only in seeking an implicit sub-basement of conceptualization, or of language. Conceptualization on any considerable scale is inseparable from language, and our ordinary language of physical things is about as basic as language gets [...] If we improve our understanding of ordinary talk of physical things, it will not be by reducing that talk to a more familiar idiom; there is none".

is not primarily worried about the epistemologists' ability to reconstruct science from sense data, but with their claim that sense data might provide a science-independent neutral foundation for science. This interpretation is confirmed by Quine's response to the sceptic, which, as I will show in this section, relies on the same type of reasoning.

Recall that, for Quine, traditional epistemology starts from a deep dissatisfaction with the problem of error, with "worries about our knowledge of the external world" (FSS, 1995b, 1). Now, if the standard conception were correct, and if Quine's argument against traditional epistemology were indeed an argument from despair, his naturalism would constitute a surrender to the sceptic. For in despairing of reconstructing science from sense data, Quine would be despairing of the epistemologist's attempt to provide our beliefs with a proper foundation. In waking up from his Cartesian dream, in other words, Quine would be forced to admit that the sceptic was right all along; we simply ought to despair of providing our beliefs with the kind of justification the sceptic demands.

In reality, however, Quine does not admit that the sceptic has been right from the beginning. Instead of despairing of being able to answer the sceptic, he makes a move similar to the one discussed above: he argues that the sceptic *too* presupposes a good deal of science in her inquiries. Where the traditional epistemologist inadvertently relied on scientific knowledge in her talk about sense data, the sceptic cannot question science without presupposing science:

> Doubt prompts the theory of knowledge, yes; but knowledge, also, was what prompted the doubt. Scepticism is an offshoot of science. The basis for scepticism is the awareness of illusion, the discovery that we must not always believe our eyes. Scepticism battens on mirages, on seemingly bent sticks in water, on rainbows, after-images, double images, dreams. But in what sense are these illu-

> sions? In the sense that they seem to be material objects which they in fact are not. Illusions are illusions only relative to a prior acceptance of genuine bodies with which to contrast them.[28] (NNK, 1975d, 258)

Sceptical questions are thus questions internal to science. According to Quine, it is science itself that shows that our evidence for science is meager; the sceptic needs to presuppose at least some theory in order to question it. The sceptic too is misguided when she believes that she can coherently doubt the reality of our beliefs from some self-sufficient science-independent perspective. Her terms too are only intelligible within a more inclusive theory of the world: "the term 'reality', the term 'real', is a scientific term on a par with 'table', 'chair', 'electron', 'neutrino', 'class', [...] all these are part of our scientific apparatus, our terminology, so that the only sense I can make of scepticism is that somehow our theory is wrong" (EBDQ, 1994b, 252).[29]

The question of how theory relates to evidence is an open question, but it is a question internal to science, it is an *immanent* challenge. We cannot step outside our conceptual scheme and question that scheme all at once. As a *transcendental* challenge scepticism simply makes no sense: "There is no such cosmic exile" (WO, 1960b, 275), no self-sufficient vantage point from which to question science.[30]

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28 See also (RR, 1973, 2-3): "The skeptics cited familiar illusions to show the fallibility of the senses; but this concept of illusion itself rested on natural science, since the quality of illusion consisted simply in deviation from external scientific reality".

29 This perspective on sceptical challenges Quine also developed in the ten years between "Two Dogmas" and *Word and Object*. See (SLS, 1954b, 229).

30 Are not sceptical challenges just as problematic when we recognize that they are "of a piece with the scientific endeavor" (RS2, 1981b, 475)? Barry Stroud certainly seems to think so. According to Stroud, Quine is "committed at least to the coherence of [the traditional sceptical question] by his very conception of knowledge" (Stroud, 1981, 468). Quine, like the traditional epistemologist, distinguishes between our objective input from the world and

2.6 TAKING HOLISM SERIOUSLY

Let me sum up what we have established thus far. Quine's rejection of first philosophy, both before and after "Epistemology Naturalized", is not based on despair, but on his rejection of *transcendence*, his dismissal of the idea of a science-independent perspective. According to Quine, "[t]here is no external vantage point, no first philosophy" (NK, 1969b, 127). Both the sceptic and the traditional epistemologist presuppose an Archimedean point in their inquiries. The sceptic presupposes that she can challenge science from some science-independent perspective, while the epistemologist presupposes that she can answer this challenge by reducing our theories to some science-independent sensory language.

Now, what underlies these traditional presuppositions is an absolute distinction between theory and evidence. Both the sceptic and the epistemologist rely on a strict theory-evidence dichotomy in their inquiries. The sceptic questions our scientific theories because she believes that our evidence for these theories is too meager. Yet, her doubts only constitute a transcendental challenge when that very evidence does not itself depend on

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our beliefs about the world as a result of that input. According to Stroud, any such "bipartite view of knowledge leaves open the general possibility that the objective world is different from the way we take it to be" and that, in consequence, we can never know "that that possibility does not obtain" (ibid.). More metaphorically, Stroud argues that the naturalist, the busy sailor adrift on Neurath's ship, can never dismiss the possibility "of sawing all around that meagre portion of the ship that represents our sensory data, and setting the rest of it adrift" (Stroud, 1984, 234).

Stroud's critique would be valid if the standard conception were correct, if Quine had dismissed traditional epistemology out of despair. Yet, Quine's view precisely implies that we *cannot* strictly distinguish between our input from the world and our beliefs about the world as a result of that input. Sure, Quine has a bipartite view of knowledge, but his bipartite picture is one internal to science. His ideas about input and output are immanent ideas. We simply cannot maintain our sense data as a self-sufficient raft while setting the rest of our ship of knowledge adrift. See (RS2, 1981b, 474-5).

those theories. Similarly, the traditional epistemologist's project of reconstructing science from sense data only constitutes a truly foundational project when these sense data themselves are not intruded by our best scientific theories.

As a result, in both cases Quine's rejection of transcendence seems to boil down to a rejection of an absolute theory-evidence distinction. Indeed, Quine has argued that, in some qualified way, "observation is inseparable from theory" (PTF, 1996a, 477), that we cannot draw a clean distinction between an observation's evidential value and the influence of intrusive information.[31] According to Quine, even a very basic one-term observation sentence like 'Red', which might be taken to report a sense datum, is to some extent susceptible to intrusive information. After all, one can imagine extreme cases in which we "may be persuaded, by collateral information about odd lighting and juxtaposition, that something is really red that did not seem so or vice versa" (WO, 1960b, 41). This shows that even an innocent observation sentence like 'Red' is never completely theory-free. It is therefore not surprising that Quine proposes that it would make more sense to speak about "degrees of theoreticity", with sentences like 'Red' at one extreme and highly theoretic observation sentences like 'There was copper in it' at the other (PTF, 1996a, 477).[32]

Ironically, what underlies Quine's rejection of a strict theory-evidence dichotomy is his holism. Thus far, I have presented Quine's holism as a thesis that affects the relation between theory and evidence. Our theories are said to be significant only

31 The nature of the qualification will the subject of section 2.7. In this section I discuss Quine's rejection of the strict theory-evidence distinction as if it is unqualified.

32 See also (RES, 1999a, 263): "This vision of science is a step from Karl Popper toward Thomas Kuhn. The observation categoricals that are the checkpoints of a theory are built of observation sentences that are themselves irreducibly theoretic to various degrees, so an apparent counter instance of such a categorical is strong evidence against the theory but not necessarily lethal".

in clusters because a single theoretical statement "has no distinctive empirical content of its own" (CPT, 1984a, 125-6). Let us call this *narrow-scoped holism*. As we have seen, this holism is strong enough to explain why we cannot translate our theoretical concepts into observation terms. Yet, this type of holism is of a narrow scope because it applies only to the theoretical sentences of a theory. Nothing is said about the way in which the content of observation sentences themselves are constituted. As it now turns out, however, the scope of Quine's holism is considerably broader. After all, if 'observation is inseparable from theory', holism affects our observation sentences as well. The content of a one-term sentence like 'Red', too, partly depends on the contribution it makes to our theory as a whole, a thesis that we might call *wide-scoped holism*.[33]

Quine's position, then, might be summarized as follows. At the highest level of generality, Quine's rejection of first philosophy is a rejection of transcendence, a rejection justified by his wide-scoped holism. There is no external vantage point because our statements will only make sense within our theory of the world. Quine's dismissal of a strict theory-evidence distinction, and hence his dismissal of both the sceptic's and the traditional epistemologist's presuppositions, is an application of his ideas about transcendence and hence a consequence of his wide-scoped holism. The problem with Quine's argument from despair is that is too weak. It grants the traditional epistemologist and the sceptic their strict theory-evidence distinction and argues on the basis of the weaker narrow-scoped holism that we cannot derive the one from the other.

33 As we shall see in chapter 4, this is not the only sense in which Quine's holism is wide-scoped. In chapter 5, the exact nature of Quine's wide-scoped holism will be the subject of discussion.

2.7 THEORY VS. EVIDENCE

At this point one might start to wonder whether Quine is not dismissing too much in rejecting first philosophy and embracing a wide-scoped holism. Is not Quine closing off our connection with the world in claiming that observation is inseparable from theory, in rejecting an absolute theory-evidence distinction? It might seem that if we cannot take our evidence to be theory-free, we are somehow trapped within our system of beliefs. It might seem, in other words, that the cure is worse than the ailment, that Quine's views about evidence should be cause for a much greater despair than the initial argument that we cannot reconstruct science from sense data.

Such a conclusion would be misguided however. Quine's ideas about the relation between theory and evidence are more nuanced then they might initially seem to be. In this section, I argue that, to some extent, Quine does allow theory-free observation sentences; that to some extent, he does allow a strict theory-evidence dichotomy. I argue that Quine only claims that theory-free observation sentences are not available for the traditional epistemologist's purposes.

The key to understanding Quine's more nuanced ideas about the relation between theory and evidence is his distinction between *holophrastic* and *analytic* observation sentences. Consider the very basic observation sentence 'Fluffy cat', for example, and suppose that a subject utters the sentence in the presence of a fluffy cat. If this subject is a competent speaker of English, her sentence will contain meaningful parts; it will be an analytic observation sentence.[34] The speaker knows what is meant

34 The notion of 'analyticity' here should not be confused with the notion of analyticity that plays an important role in Quine's rejection of the analytic-synthetic distinction. In order to avoid such confusion, Quine also sometimes speaks about 'taking an observation sentence piecemeal'. See (IPOS, 1993b, 412).

by the component term 'cat' and she uses the word to refer to a particular physical object. Furthermore, the speaker will be disposed to assent to an alternative observation sentence if that sentence describes the situation equally well. If the speaker were to learn that a cat can also be referred to as a 'felis catus', for example, she would immediately be able to form the sentence 'Fluffy felis catus' and see that the sentence is also true in her situation. Moreover, the speaker will be prepared to withdraw her assent to the observation sentence when she discovers that the catlike object is not really a cat after all.

An infant who has just learned 'Fluffy cat' as one of her first sentences, on the other hand, will use the sentence in a completely different holophrastic way. She will not see the sentence as composed of distinguishable meaningful parts. Rather, her sentence will just be an unstructured whole, a random cry 'Fluffycat' that she is conditioned to utter or assent to in appropriate circumstances:

> Observation sentences contain words that refer to objects when used in mature discourse, but the infant first acquires such a sentence only as a seamless whole, conditioned—like the signal cry of the ape—to an appropriate range of global neural intakes. (NLOM, 1995c, 464-5)

The infant does not use the sentence to refer to a particular object. Rather, she is trained to utter the complete sentence as an unstructured whole in relevant situations. As a result, even if she were to be conditioned to utter the sentence 'felis catus' in these circumstances as well, she would not automatically be able to form the sentence 'Fluffy felis catus'. Furthermore, the infant at this stage will also not be able to withdraw her assent to any observation sentence in the light of new information: "Second thoughts are not yet relevant; they become so only at a later stage, when scientific theory has begun to interrelate observation sentences and generate conflict" (PTF, 1996a, 476).

Now, according to Quine, in the latter holophrastic sense observation sentences *are* theory-free, i.e. independent of intrusive information. As soon as a speaker has learned to use her observation sentences analytically, however, a complicated process that proceeds by "short leaps" (NNK, 1975d, 267), these sentences will be subject to Quine's wide-scoped holism. That is, these sentences too will be significant only in clusters:

> [Holophrastic observation sentences] are associated as wholes to appropriate ranges of stimulation, by conditioning. Component words are there merely as component syllables, theory-free. But these words recur in theoretical contexts in the fullness of time. It is precisely this sharing of words, by observation sentences and theoretical sentences, that provides logical connections between the two kinds of sentences and makes observation relevant to scientific theory. (PT, 1990g, 7)

As a result, Quine's distinction between holophrastic and analytic observation sentences perfectly illustrates the scope of his holism. As soon as a speaker starts to master his language beyond the stage of conditioned "animal cries" (IPOS, 1993b, 412), holism sets in. The word 'cat' starts to occur in more and more observations sentences and the infant gradually acquires the ability to form new sentences all by herself; a point in her development at which she already relies on a substantive object-based theory of the world. In consequence, as soon as we have acquired the bare essentials of our language and the capacity to refer, the content of our observation sentences too will depend on the contribution they make to our theory as a whole.[35]

35 Another way to express the same point is to say that holophrastic observation sentences are momentary, whereas records of those sentences are stored in the web as theoretical standing sentences. This is at least how Quine sometimes expresses the point in responses to questions. See (LSQ*, 1982a) and (QQN*, 1986b).

The distinction between holophrastic and analytic observation sentences therefore explains why Quine believes it to be impossible to develop a phenomenalistic language that is truly independent of science. For whenever the epistemologist starts her inquiry with sentences like 'Red patch now', she will only have provided a theory-free science-independent foundation if she uses these sentences holophrastically, if she considers them as unstructured wholes. Yet, holophrastic observation sentences will not be of any use for the traditional epistemologist's purposes, precisely because they are radically unstructured and theory-free. The ability of an infant to utter the one-term sentence 'Red' in the holophrastic sense, whenever confronted with a red experience, does not amount to anything more than her ability to cry whenever she has hurt herself. She cannot yet refer to the color or use the term in an epistemologically interesting sense: "We can credit the child at this point with being able to *discriminate* red, to *recognize* red. [. . .] But to say that [she] refers to the color would be to impute our ontology to him" (RR, 1973, 81-3). Using sentences like 'Red patch now' in the holophrastic sense as a foundation for science, even if this were possible for the epistemologist who has already mastered the English language, would therefore be fruitless. Without the ability to refer and the ability to utter truth-valued sentences, the traditional epistemologist's project will never get off the ground. It is only in the analytic sense that observation sentences can be linked to scientific theory. Yet, in the analytic sense the component terms of observation sentences cannot be separated from the theoretical system in which they take part.

As a result, even though Quine's argument against traditional epistemology rests on his wide-scoped holism, his rejection of a strict theory-evidence distinction is no cause for despair; our observation sentences are argued to be ultimately grounded in theory-free responses to sensory stimulation. According to Quine, we can maintain that "observation sentences stay on in

their old definition and their role as [...] the checkpoints of science", even if we have to acknowledge that "observation is inseparable from theory" (PTF, 1996a, 477) when we are using our observation sentences analytically.

2.8 REINTERPRETING "EPISTEMOLOGY NATURALIZED"

In this chapter, I have reconstructed Quine's reasons for dismissing traditional epistemology. I have challenged the claim that his argument is one from despair. Quine does not reject traditional epistemology because we cannot reduce our science to sense data. Rather, Quine dismisses the project as flawed from the beginning; it is impossible to develop a self-sufficient sensory language, independent of our best scientific theories of the world. The search for a transcendental perspective, independent of science, is a mistake. Quine's argument is supported by his wide-scoped holism, the thesis that the content of both our theoretical and our observation statements, considered analytically, depends on the contribution they make to our theory as a whole. As a result, observation is to a large extent inseparable from theory, and we are all bound to start our inquiries from within; even sceptical questions are immanent.

Let me, in conclusion, examine how we might interpret "Epistemology Naturalized" in the light of these findings; that is, examine how we might make better sense of Quine's argument in the paper. I believe that the paper can be better understood if we keep in mind the distinction between immanent and transcendental inquiry. As we have seen in section 2.5, there are two ways in which one might interpret sceptical challenges. In the transcendental reading, the sceptic is seen as questioning science from some science-independent external vantage point, while in the immanent reading scepticism is a challenge from within. Quine dismisses the transcendental challenge as inco-

herent but admits that sceptical scenarios are live possibilities when construed immanently.

Now, since the epistemologist's project of reducing science to sense data is supposed to provide an answer to the sceptic's challenge, it admits two interpretations as well. On the one hand, one can interpret rational reconstruction as an attempt to fulfill the Cartesian dream, to ground our knowledge in some science-independent sensory language. In this transcendental reading, rational reconstruction is a project within first philosophy. As we have seen, Quine dismisses this project as incoherent because he rejects the idea of a self-sufficient sensory language.

Yet, one can also interpret 'rational reconstruction' as a project *internal* to science. In this reading, the project does not presuppose an external vantage point. One can just posit a phenomenalistic language, acknowledge that this language is not self-sufficient, and examine whether we can simplify our theory of the world by reducing our scientific talk to this language. In this reading, the project need not be dismissed because it presupposes an implicit sub-basement of conceptualization. Rather, it fails because we ought to despair of ever being able to successfully define the empirical content of a single theoretical statement in isolation.

In this chapter, I have limited my discussion to the transcendental interpretation and argued that Quine's argument against this type of rational reconstruction is not an argument from despair. Yet, the careful reader of Quine *after* "Epistemology Naturalized" will notice that Quine has never limited himself to either one of these two interpretations. Consider, for example, the following passages:

> Various epistemologists, from Descartes to Carnap, [...] sought a foundation for natural science in mental entities, the flux of raw sense data. It was as if we might first fashion a self-sufficient and infallible lore of sense data, innocent of reference to physical things, and then

> build a theory of the external world somehow on that finished foundation. The naturalistic epistemologist dismisses this dream of a prior sense-datum language.[36] (NLOM, 1995c, 462)

> My attitude toward the project of a rational reconstruction of the world from sense data is [...] naturalistic. I do not regard the project as incoherent, though its motivation in some cases is confused. I see it as a project of positing a realm of entities intimately related to the stimulation of the sensory surfaces, and then [...] to construct a language adequate to natural science. It is an attractive idea, for it would bring scientific discourse into a much more explicit and systematic relation to its observational checkpoint. My only reservation is that I am convinced, regretfully, that it cannot be done.[37] (TTPT, 1981d, 23)

Although Quine is talking about the same project in both passages, viz. reconstructing science out of sense data, the former constitutes a transcendental and the latter constitutes an immanent reading of the project. For whereas the former talks about 'seeking a foundation for science', the latter talks about 'positing' sense data and about bringing 'scientific discourse into a much more explicit and systematic relation to its observational checkpoint'. It is because of this reason that Quine uses a distinct argument in each case. In the former he rejects the idea of a self-sufficient sense datum language and in the latter he is convinced, regretfully, that the project cannot be fulfilled.

Now let me turn to "Epistemology Naturalized". Quine's goal in the paper is to convince the reader that we should abandon "creative reconstruction" and that we should examine how the construction of scientific theories "really proceeds". In

36 See also (SSS, 1986l, 327-8).

37 See also (PTF, 1996a, 477).

order to establish this, Quine argues that there are no advantages of rational reconstruction over 'straight psychology".[38] Now, when one reads Quine's paper with the above distinction between immanent and transcendental reconstruction in the back of one's mind, one finds that Quine is almost exclusively concerned with dismissing the advantages of rational reconstruction in its immanent reading. Quine spends almost no time on rejecting the Cartesian quest for a foundation of knowledge. He uses only a few words to argue that, with respect to the doctrinal side of epistemology, we are no farther along today than where Hume left us (EN, 1969a, 72). The implication here is that since the doctrinal project fails, the transcendental quest for foundations can be abandoned, both on its conceptual and its doctrinal side. It is at this point, that Quine could have inserted his argument against self-sufficient sensory languages; but he did not, probably because he presupposed that the reader already accepted the hopelessness of the project. The bulk of Quine's argument is concerned with dismissing the advantages of Carnap's project (ibid., 72-80), a project that he interprets immanently as he emphasizes that Carnap already saw the "Cartesian quest for certainty [...] as a lost cause" (ibid., 74).[39]

Quine does not interpret Carnap as a first philosopher aiming to validate our scientific theories. Rather, he believes that the

38 As noted in chapter 1 (fn. 20), Quine originally titled his paper "Epistemology naturalized; or, the case for psychologism". In this 1968-version of the paper, Quine's famous passage "Why not settle for psychology? Such a surrender of the epistemological burden to psychology is a move that was disallowed in earlier times as circular reasoning" reads as follows: "Why not settle for psychology? Such a surrender of the epistemological burden to psychology is a move that was denounced by Lotze, Frege, and others in the nineteenth century under the disparaging name of psychologism, partly for fear of circular reasoning" (ENP*, 1968a).

39 See also (FSS, 1995b, 13): " Carnap's motivation was not [the] traditional quest for certainty. Rather, his goal was just a systematic integration [...] of our scientific concepts of mind and nature".

advantage of Carnap's project, if it were to succeed, is that it would "elicit and clarify the sensory evidence for science" (EN, 1969a, 74); a project that is immanent, as it will only legitimize the concepts of science "to whatever degree the concepts of set theory, logic, and observation are themselves legitimate" (ibid., 76). Given this immanent interpretation of Carnap's project, it is no surprise that he uses his argument from despair to dismiss it.

My suggestion, therefore, is that one should not read "Epistemology Naturalized" as an argument against traditional epistemology in its transcendental interpretation, even though some passages invite such a reading.[40] Quine and Carnap (and many other epistemologists for that matter) had already rejected this type of first philosophy elsewhere. Rather, Quine was concerned with the type of "creative reconstruction" that continued to be an essential element of Carnap's epistemology. Quine's aim was not to show that this type of inquiry is naturalistically unacceptable, he only attempted to establish that this project, regretfully, could not be fulfilled, that "[w]e must despair of any such reduction" (EN, 1969a, 77).

40 One of the most confusing elements of "Epistemology Naturalized" is that Quine uses the term 'epistemology' to denote both the Cartesian quest for certainty and the relatively innocent attempt to examine the relation between theory and evidence. In this respect, I agree with Johnsen (2005) that "Epistemology Naturalized" fails to expose Quine's views as clearly as possible. See footnote 6.

3

INTERNAL AND EXTERNAL QUESTIONS

Summary: Quine's negative characterization of naturalism as the rejection of first philosophy invites the question of why exactly he thinks that first philosophy ought to be dismissed. Having reconstructed Quine's argument against traditional epistemology in chapter 2, in this chapter I turn to Quine's position vis-à-vis traditional metaphysics. *Prima facie*, Quine's attitude toward metaphysics seems to differ from his attitude toward epistemology. For it is often claimed that Quine *saves* rather than dismisses metaphysics in arguing that ontological questions are "on a par with questions of natural science" (CVO, 1951a, 211). Where Carnap rejects metaphysical existence claims as meaningless, Quine is taken to restore their intelligibility by dismantling the former's internal-external distinction. In the present chapter, I argue that this popular view is incorrect and that Quine, like Carnap, rejects traditional metaphysics. I argue that a historically more accurate perspective on the Carnap-Quine debate should distinguish between two distinct internal-external distinctions, only one of which is dismissed by Quine. In support of my interpretation, I show that Quine, from the earliest stages of his career, defends a view on metaphysics that is in many respects similar to Carnap's and that the later Quine, in theorizing about the nature of both truth and reference, appeals to an internal-external distinction himself; a distinction moreover which shows that Quine's arguments against traditional epistemology and metaphysics are cut from the same cloth.[1]

1 This chapter is an adapted version of the paper "Electrons, Elephants, and Empty Sets: Quine on Metaphysics and the Internal-External Distinction" (un-

3.1 INTRODUCTION

Most metaphysicists agree that we should not rest content with our ordinary ascriptions of existence. Although in everyday life and in the sciences we may freely talk about elephants, electrons and empty sets, as philosophers we must investigate whether these objects *really* exist. Carnap, as is well known, has argued that such philosophical questions of existence are devoid of cognitive content. In his seminal "Empiricism, Semantics, and Ontology" (1950), he argues that the metaphysicist's questions are meaningless when they are conceived as theoretical questions admitting of a truth-valued answer. For Carnap argues, existence claims only make sense internal to a linguistic framework and we cannot ask whether an entity is 'real' in an (external) framework-independent way; 'reality' itself is a concept internal to a framework and as such "cannot be meaningfully applied to the [framework] itself" (ibid., 207). Instead, Carnap proposes to reinterpret metaphysical questions as practical questions about which linguistic framework the philosopher ought to adopt.

Quine believes that Carnap's strict distinction between internal and external questions cannot be maintained. According to Quine, no question is either purely theoretical or purely practical; just like one's decision to adapt a hypothesis in the light of new experiential data, one's decision to adopt a certain framework will be informed by both theoretical knowledge and pragmatic criteria. The question whether or not to accept a certain entity as 'real' therefore *is* a meaningful question that can be answered by ordinary scientific means. In Quine's naturalistic perspective on metaphysics, in other words, ontological questions are "on a par with questions of natural science" (CVO, 1951a, 211)

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der review-b), presented at the VAF 2014 conference in Groningen as well as at the workshop "The MetaMetaphysical Club" in Rotterdam.

This little stick-figure summary of the Carnap-Quine debate suggests that Quine breathed new life into the metaphysical project that was deemed meaningless by Carnap and his fellow positivists. For where Carnap rejects philosophical existence claims as meaningless, Quine seems to restore their intelligibility by dismantling the former's internal-external distinction. Indeed, this seems to be Quine's own perspective on his debate with Carnap:

> I think the positivists were mistaken when they despaired of existence [. . .] and accordingly tried to draw up boundaries that would exclude such sentences as meaningless. *Existence statements in this philosophical vein do admit of evidence*, in the sense that we can have reasons, and essentially scientific reasons, for including numbers or classes or the like in the range of values of our variables. (EQ, 1968b, 97, my emphasis)

When quantifying over abstract objects or elementary particles is indispensable for the formulation of our best scientific theories, Quine argues, we are to countenance these objects and particles as real. As a result, Carnap and his positivistic comrades were simply "wrong if and when they concluded that the world is not really composed of atoms or whatever" (SN, 1992b, 405).

The picture that Quine revived the legitimacy of philosophical existence claims is often defended in the literature. In his introduction to the history of analytic philosophy, for example, Avrum Stroll argues that Quine blurs "the boundary between speculative metaphysics and science, thus giving a kind of credibility to metaphysics that Carnap would never have countenanced" (2000, 200). Similarly, Nicholas Joll claims that "Quine saves metaphysics from positivism" (2010) and Stephen Yablo argues that Quine's view, unlike Carnap's, provides us with a

way to attach "believable truth values to philosophical existence claims" (1998, 259).[2]

Yet, there is something puzzling about this picture. For it does not sit well with the fact that Quine, on many occasions, *does* seem to argue against the intelligibility of metaphysical existence claims. Quine has argued, for example, that he is "[n]o champion of traditional metaphysics" (CVO, 1951a, 204) and that the question "what reality is *really* like [...] is self-stultifying" (SN, 1992b, 405). On a few occasions, Quine even argues that the "[p]ositivists were right in branding such metaphysics as meaningless" (ibid.).[3] Even more surprising from the above sketched perspective is that Quine often appeals to the very same argument Carnap gives in "Empiricism, Semantics, and Ontology", viz. the argument that the notion of 'reality' cannot be given any sense outside the system of which it is an element. We simply "cannot significantly question the reality of the external world", Quine argues, "for to do so is simply to dissociate the [term] 'reality' [...] from the very applications which originally did most to invest [this term] with whatever intelligibility [it] may have for us" (SLS, 1954b, 229).

In sum, Quine's ideas about the intelligibility of metaphysical existence claims seem inconsistent. On the one hand, Quine dissolves the Carnapian distinction between internal and external questions, thereby dismantling the latter's "special strictures against philosophical questions of existence" (EQ, 1968b, 96). On

2 See also Murphey (2012, 14). Price (2007, 380) aptly summarizes the above perspective on the Carnap-Quine debate by claiming that Quine is traditionally regarded as "the savior of a more robust metaphysics" by driving "a stake through the heart of "Empiricism, Semantics, and Ontology", [...] thus [dispatching] the last incarnation of the Viennese menace". See also Eklund (2013, 229), who suggests that the above perspective dominates contemporary debates in metaontology as well.

3 See also, for example, (SSS, 1986l, 337): "if some scientifically undigested terms of metaphysics [...] were admitted into science along with all their pertinent doctrine [...] [i]t would be an abandonment of the scientists' quest for economy and of the empiricists' standard of meaningfulness".

the other hand, Quine speaks about traditional metaphysics as being sinful (SN, 1992b, 406) and incoherent (TTPT, 1981d, 21). Since both perspectives are present in Quine's early and later writings, the hypothesis that he changed his mind provides no plausible solution to the puzzle.

In the present chapter, I argue that Quine's ideas about metaphysics *are* consistent by showing how he can both reject Carnap's internal-external distinction and dismiss traditional metaphysics. After introducing Carnap's position in "Empiricism, Semantics, and Ontology" as well as Quine's arguments against Carnap's position (section 3.2), I argue that the standard interpretation of Quine's views is incorrect because it rests on an equivocation between two different internal-external distinctions, one adopted and one dismissed by Quine (section 3.3).[4] To substantiate this conclusion, I shall defend two claims. I show, first, that the early Quine is largely in agreement with Carnap when it comes to the status of metaphysical existence claims (section 3.4) and, second, that the later Quine, in his disquotational theories of reference (section 3.5) and truth (section 3.6), makes use of something like an internal-external distinction himself. Finally, I argue that this latter internal-external distinction is strongly related to the distinction which underlies Quine's rejection of traditional epistemology (chapter 2), and that as a result, Quine's arguments against traditional epistemology and metaphysics are more closely aligned than is often suggested (section 3.7).

4 In my dismissal of the received view in section 3.3 and part of section 3.4, I build on the work of Alspector-Kelly (2001) and Price (2007, 2009), who were the first to suggest that the standard conception of the Carnap-Quine debate is mistaken. Although my interpretation of Quine's views and arguments differs somewhat from the readings offered by Price and Alspector-Kelly, my conclusions in these sections are to a large extent compatible with theirs.

3.2 INTERNAL AND EXTERNAL EXISTENCE CLAIMS

Carnap's problems with philosophical existence claims are deep-rooted. Already in his pre-Vienna period, he considered metaphysical disputes to be "sterile and useless". In his "Intellectual Autobiography", he describes the origin of these anti-metaphysical sentiments as follows:

> When I compared [metaphysical] argumentation with investigations and discussion in empirical science or in the logical analysis of language, I was often struck by the vagueness of the concepts used and by the inconclusive nature of the arguments. I was depressed by disputations in which the opponents talked at cross purposes; there seemed hardly any chance of mutual understanding, let alone of agreement, because there was not even a common criterion for deciding the controversy. (Carnap, 1963a, 44)

Influenced by the early Wittgenstein, Carnap developed the view that metaphysical theses are without cognitive content, arguing that they are pseudo-sentences because they "cannot in principle be supported by an experience" (1928b, 328). Where metaphysicists will usually agree about whether or not a certain entity is real in an everyday empirical sense, they rely on a "nonempirical (metaphysical) concept of reality" when they are involved in a philosophical dispute (ibid., 340).

Although Carnap has never felt any qualms about these early arguments against metaphysics,[5] he later felt the need to return to the subject and explain his position once more. Carnap wrote "Empiricism, Semantics, and Ontology" in response to critical empiricists who objected that he referred to abstract objects without having shown that they "actually exist" and ar-

5 See Carnap's (1950, 215) and (1963b, 870).

gued that these empiricists "[neglect] the fundamental distinction" between ordinary and philosophical ascriptions of existence (1963a, 65-6).[6]

It is in "Empiricism, Semantics, and Ontology" that Carnap differentiates between internal and external questions in order to capture this distinction. Ordinary questions of existence with respect to a certain entity x, Carnap argues, should be viewed as questions *internal* to a linguistic framework containing the rules for our use of the concept 'x'. Philosophical questions of existence, on the other hand, are to be viewed as *external* questions about the reality of xs prior to the adoption of the framework. These latter questions are the questions that Carnap's fellow empiricists had in mind when they wondered whether Carnap is justified in using a framework that quantifies over abstract objects without having shown that they actually exist. According to Carnap, however, such external questions are meaningless when they are considered to be theoretical and admitting of a truth-valued answer. For, and here his argument is similar to the one developed in his early work, the very concept of 'reality' appealed to in metaphysical questions of existence cannot be given a meaningful interpretation:

> The concept of reality occurring in [...] internal questions is an empirical, scientific, non-metaphysical concept. To recognize something as a real thing or event means to succeed in incorporating it into the system of things [...] according to the rules of the framework. From these questions we must distinguish the external question of the reality of the thing [...] itself. In contrast to the former questions, this question is raised neither

6 See Alspector-Kelly (2001, 2002) for a more detailed discussion of Carnap's motives in "Empiricism, Semantics, and Ontology". Since the present chapter deals with Quine's ideas about metaphysics, I will mostly limit myself to a discussion of Quine's interpretation of Carnap, acknowledging that this reading might not do justice to the historical Carnap.

> by the man in the street nor by scientists, but only by philosophers. Realists give an affirmative answer, subjective idealists a negative one, and the controversy goes on for centuries without ever being solved. And it cannot be solved because it is framed in a wrong way. To be real in the scientific sense means to be an element of the system; hence this concept cannot be meaningfully applied to the system itself. (Carnap, 1950, 207)

Concepts, according to Carnap, only make sense in as far as the rules for their use are specified within a suitable linguistic framework. As a result, the concept of 'reality' itself will only make sense within a linguistic framework and hence philosophers fail "in giving to the external question and to the possible answers any cognitive content" (Carnap, 1950, 209).[7]

Because external questions fail to be meaningful when interpreted as theoretical, Carnap proposes that we should view them as practical questions, as matters "of practical decision concerning the structure of our language" (ibid., 207). Rather than asking whether or not a certain entity *x really* exists, we should ask whether or not it is useful to adopt one or another *x*-related framework, a question that will be guided by pragmatic criteria. According to Carnap,

> the introduction of [a] framework is legitimate in any case. Whether or not this introduction is advisable for certain purposes is a practical question of language engineering, to be decided on the basis of convenience, fruitfulness, simplicity, and the like. (1963a, 66)

7 Of course, Carnap's argument with respect to the concept of 'reality' applies equally to philosophical notions that serve the same function. In their metaphysical inquiries, Carnap argues, philosophers might also talk about "subsistence" or the "ontological status" of an entity. These alternative philosophical notions, however, are also without cognitive content because philosophers have failed to explain their use "in terms of the common scientific language" (Carnap, 1950, 209).

As a result, although Carnap considers external questions to be devoid of cognitive content, such questions can still be given "a meaning by reinterpreting them or, more exactly, by replacing them with the practical questions concerning the choice of certain language forms" (1963b, 869). Carnap's internal-external distinction, in short, becomes a distinction between the theoretical and the practical when external questions are reinterpreted as questions about whether or not to adopt a certain framework.

Quine rejects Carnap's distinction between the internal and the external. In fact, he develops two arguments against the distinction: one in which he reduces it to the analytic-synthetic divide, and one in which he argues that both internal and external statements are partly theoretical and partly practical in nature.[8] Quine's first argument is largely negative. He argues

8 Quine develops these arguments in "Two Dogmas of Empiricism" (1951b, 45-6), "On Carnap's Views on Ontology" (1951a), and "Carnap and Logical Truth" (1954a, 132). In the second essay, Quine also provides a third argument against Carnap's distinction by reducing it to a dichotomy between category and subclass questions. According to Quine, external questions are concerned with the existence of entities expressed by a category word (e.g. 'Are there *things*?' or 'Are there *numbers*?'), whereas internal questions are concerned with the existence of subclasses of them (e.g. 'Are there *rabbits*?' or 'Are there *prime numbers between 10 and 20*?'). He then argues that the latter distinction is trivial, because "there is no evident standard of what to count as a category" (EQ, 1968b, 92). As several scholars have noted, however, Quine's argument here misses the mark because the distinction between internal and external questions of existence cannot be based on the category-subclass distinction; category as well as subclass questions can be asked in both an internal and an external vein. In later work, Quine is somewhat more careful in his reading of Carnap when he describes the latter's ideas about category words (*Allwörter*) and the related category-subclass distinction as "an *early* doctrine of Carnap" (ibid., 91, my emphasis), explicitly referring only to the latter's *The Logical Syntax of Language* (1934, §76), not to "Empiricism, Semantics, and Ontology".

For a different interpretation of Quine's third argument, see Price (2009, §§4-5), where Quine's dismissal of the category-subclass distinction is read as an argument against Carnap's idea that language can be divided into frameworks, not as an argument against the internal-external distinction. Given (1) Quine's remark that the category-subclass distinction "underlies Carnap's

that "a double standard for ontological questions and scientific hypotheses" requires "an absolute distinction between the analytic and the synthetic", a distinction which he famously argues to be untenable (TDE, 1951b, 43-4). In Quine's interpretation, the semantic rules of a linguistic framework are analytic because they are laid down by convention, whereas internal statements are either analytic or synthetic depending on the nature of the framework in question.[9] As a result, if Carnap is correct that "statements commonly thought of as ontological are proper matters of contention only in the form of linguistic proposals", then these philosophical existence claims can only be distinguished from internal statements by appealing to their analytic character (CVO, 1951a, 210).[10] Quine, however, rejects the analytic-synthetic distinction because it lacks a clear behavioristic explication.

Quine's second argument is more positive, and is based on his constructive "empiricism without the dogmas" (TDE, 1951b, §6). If, as Quine maintains, science is a unified structure whose statements face experience only in clusters such that no statement is in principle immune to revision, then all statements that are relevant to science, including Carnap's linguistic proposals, will be guided by both theoretical and practical concerns. Just like the decision to adapt a hypothesis in the light of new expe-

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distinction of the external and internal" (CVO, 1951a, 297) and (2) the fact that an internal-external distinction can also be drawn for a single undivided language, however, Price's interpretation seems to me unjustified. In any case, on Price's interpretation too "Quine's criticism of Carnap [...] is inconclusive" (2009, 335).

9 E.g. internal statements in the thing-language are synthetic, whereas internal statements in the number-language are analytic. See Carnap (1950, 208-9).

10 Internal statements can be analytic too of course. Quine, however, claims that he does not see why Carnap "should care about this" (ibid., 210). In any case, Carnap himself also seems to have appreciated the close relation between the internal-external distinction on the one hand, and the analytic-synthetic distinction on the other. See (1950, 215n5).

riential data, one's decision to adopt a certain framework will be informed by theoretical as well as practical considerations:

> Within natural science there is a continuum of gradations, from the statements which report observations to those which reflect basic features say of quantum theory [...] The view which I end up with [...] is that statements of ontology [...] form a continuation of this continuum, a continuation which is perhaps yet more remote from observation [...] The differences here are in my view differences only in degree and not in kind. Science is a unified structure, and in principle it is the structure as a whole, and not its component statements one by one, that experience confirms or shows to be imperfect. Carnap maintains that ontological questions [...] are questions not of fact but of choosing a convenient conceptual scheme or framework for science; and with this I agree only if the same be conceded for every scientific hypothesis.[11] (CVO, 1951a, 211)

Quine, in sum, dissolves Carnap's internal-external distinction; first by showing that it relies on the untenable analytic-synthetic divide, and second by arguing that a more realistic model of theory change construes all revisions as guided by both theoretical and practical considerations. In arguing that the difference between scientific and ontological claims is only gradual, Quine therefore seems to blur the boundary between metaphysics and science, a boundary that Carnap had propagated in order to dismiss metaphysics as meaningless.

11 See also (ME*, 1950b, 15): "we find ourselves saying, with Carnap: choose your ontology as proves convenient. But I think Carnap is wrong in supposing that our choice here is different in principle from, and freer than, our choice of a physical theory in the light of sense experience".

3.3 TWO DISTINCTIONS

The question whether or not Quine's arguments effectively undermine Carnap's internal-external distinction has been a matter of some controversy.[12] In what follows, however, I limit my discussion to a more specific, yet not entirely unrelated issue, viz. the question what type of internal-external distinction Quine aimed to undermine. I argue that we ought to differentiate between two types of internal-external distinctions and that Quine's arguments apply to only one of them. In the sections to come, I reconstruct Quine's position about the second distinction and argue that his views on this matter are remarkably similar to Carnap's.

As we have seen in the previous section, Carnap differentiates three types of questions of existence depending on whether we are concerned with the metaphysicist's theoretical perspective or with Carnap's practical reinterpretation:

(I) *Internal questions* about the reality of *x*s, asked after the adoption of an *x*-related framework.

(TE) *Theoretical external questions* about the reality of *x*s, asked before the adoption of an *x*-related framework.

(PE) *Practical external questions* about whether or not it is advisable to adopt an *x*-related framework.

From Carnap's perspective these three types of questions are completely different: TE-questions are theoretical but without cognitive content, whereas PE-questions have exactly the opposite characteristics; they are meaningful questions of a practical

12 See Haack (1976, §3), Bird (1995), and Glock (2002, §5) for a critical evaluation of Quine's arguments. Yablo (1998, §§5-7) and Gallois (1998, §2), on the other hand, yield a more positive verdict.

nature. I-questions, finally, are both theoretical and meaningful.[13]

Although it is generally recognized that Carnap distinguishes between three types of questions, TE and PE-questions are often conflated under the general heading 'external questions' in discussions about the Carnap-Quine debate. In itself, combining these two types of external questions is relatively innocent. Yet some have failed to realize that Carnap's threefold distinction implies that we cannot speak about '*the* internal-external distinction' in general. That is, they have ignored the fact that "Empiricism, Semantics, and Ontology" contains two such distinctions, depending on what type of external question one is talking about:

(I/TE) A distinction between meaningful internal questions and theoretical external questions without cognitive content.

(I/PE) A distinction between internal questions of a theoretical and external questions of a practical nature.

I/TE distinguishes between the meaningful and the meaningless, whereas I/PE differentiates between questions of a theoretical and questions of a practical nature.

Now, consider the question whether Quine was attacking I/TE or I/PE. If one believes that Quine, in criticizing the internal-external distinction, aimed to revive the meaningfulness of metaphysical existence claims, one clearly presupposes that Quine was attacking I/TE.[14] For if Quine had really aimed to

13 See also Eklund (2013, 237): "Carnap is actually drawing a *tripartite* distinction: between questions internal to a framework, questions about which framework we should choose to employ, and the pseudo-questions—the supposed theoretical external questions".

14 See, for example Haack (1976, §3.1) and Bird (1995, §2), where Quine's arguments are evaluated in terms of their effectiveness in undermining I/TE.

breathe new life into the metaphysical project that was deemed meaningless by Carnap, he would have tried to show that the distinction between internal and theoretical external questions should not be viewed as a distinction between the meaningful and the meaningless. He would have tried to show, in other words, that Carnap's TE-questions can be given "a clear cognitive interpretation" or can be given "a formulation [...] in terms of the common scientific language" (Carnap, 1950, 209).

Yet Quine does not seem to be concerned with anything like this at all. Rather, as we shall see, there are many reasons for thinking that he was concerned with undermining not I/TE but I/PE.[15] Quine's arguments against Carnap's distinction provide the first reason. Recall that Quine first reduces the internal-external distinction to the analytic-synthetic divide and then argues that both scientific hypotheses and Carnap's linguistic proposals are guided by theoretical as well as practical considerations. Now, if Quine were really aiming to undermine I/TE, then neither of these arguments would have made sense. Since Carnap rejects TE-questions as meaningless, they are neither analytic nor synthetic; an argument against the analytic-synthetic distinction therefore has no relevance if one aims to undermine I/TE. A similar conclusion can be drawn about Quine's second argument. Quine's claim that the distinction between the theoretical and the practical is a matter of degree, not kind, is not relevant had he targeted I/TE, since Carnap regards both I-questions and TE-questions as theoretical.[16]

15 Eklund (2013) correctly suggests that Quine attacks I/PE, although he does not argue for this claim.

16 See also Price (2009, 326): "Quine's claim is that there are no purely internal issues, in Carnap's sense. No issue is ever entirely insulated from pragmatic concerns about the possible effects of revisions of the framework itself [...] Quine's move certainly does not restore the non-pragmatic external perspective required by metaphysics. In effect, the traditional metaphysician wants to be able to say, 'I agree it is useful to say this, but is it true?' Carnap rules out this question, and Quine does not rule it back in".

If, on the other hand, we interpret Quine as arguing against Carnap's I/PE-distinction, his arguments begin to make sense. For the lack of a sharp distinction between the analytic and the synthetic seriously undermines Carnap's attempt to draw a distinction between "the acceptance of a language structure and the acceptance of an assertion formulated in the language" (1950, 215). Similarly, if both scientific hypotheses and linguistic proposals are guided by theoretical as well as practical considerations, Carnap cannot uphold his claim that the two can be distinguished because I-questions are theoretical and PE-questions are practical. Quine, in this interpretation, both shows that an I/PE-distinction cannot be maintained and develops a positive theory in which the distinction between the theoretical and the practical is a matter of degree.

A second reason for thinking that Quine was concerned with undermining I/PE instead of I/TE is the way in which he describes Carnap's external questions. In his critical papers on Carnap's distinction, Quine consistently refers to those questions as 'linguistic proposals'. In "Two Dogmas", for instance, he argues that Carnap sees ontological questions as concerned with "choosing a convenient language form, a convenient conceptual scheme or framework for science" (TDE, 1951b, 45).[17] Describing external questions in this way only makes sense if Quine has in mind Carnap's PE-questions. TE-questions, after all, are certainly not linguistic proposals, they are theoretical questions concerning the reality of a certain class of entities.

Finally, my interpretation of Quine's aims in rejecting the internal-external distinction is supported by the background of the debate between Carnap and Quine. I have already noted that Carnap wrote "Empiricism, Semantics, and Ontology" in

17 See also Quine's (ML1, 1950c, 208), (CVO, 1951a, 210) and (CLT, 1954a, 132), where Carnapian external questions are described as a "linguistic convention devoid of ontological commitment", as a "linguistic proposal" and as "a matter [...] of linguistic decision".

order to respond to critical fellow empiricists who had objected that he referred to abstract objects without having shown that they "actually exist". Now, as it turns it out, Quine was one of those critics.[18] In the late 1930s, Quine developed his criterion of ontological commitment, according to which we are committed to an entity "if and only if we regard the range of our variables as including such an entity" (LAOP, 1939a, 199).[19] Carnap, who claimed to accept Quine's criterion (1950, 214n3), however still maintained that his talk about abstract entities should be seen as "a practical decision like the choice of an instrument" (1947, §10). From Quine's perspective, therefore, Carnap was dodging his ontological commitments. That is, although Carnap accepted his "standard for judging whether a given theory accepts given alleged entities" (CVO, 1951a, 205), he still did not acknowledge that he was committed to abstract objects because he viewed his "acceptance of such objects [as] a linguistic convention distinct somehow from serious views about reality (WO, 1960b, 275).[20]

If we take this background into consideration, it becomes clear that when Quine attacked Carnap's internal-external distinction in the early 1950s, he was not concerned with the lat-

18 In his "Intellectual Autobiography", Carnap lists Quine as one of the philosophers who rejected his way of speaking as "a 'hypostatization' of entities" (1963b, 65).

19 For a history of Quine's criterion, see Decock (2004).

20 Quine's discontent with Carnap's position can be traced back at least to 1937, when he, in a lecture on nominalism, suggests that although Carnap succeeds in avoiding metaphysical questions by rejecting them as meaningless, he does not "provide for reduction of all statements to statements ultimately about tangible things, matters of fact", and thereby fails to show how we can keep "our feet on the ground—avoiding empty theorizing" (N1*, 1937b). See Mancosu (2008, 28-9). See also Alspector-Kelly (2001, §3): "As Quine understands it, Carnap endorsed Quine's criterion of ontological commitment [...] Nonetheless, Carnap did not take himself to be committed to abstract entities, and so did not take himself to be a Platonist, despite the fact that he quantified over abstract objects. Nor did he have any plan to show that such quantification can be avoided".

ter's claim that the traditional metaphysicist's questions are devoid of cognitive content, i.e. with the claim that TE-questions are meaningless. Rather, his job was to argue that there is no proper distinction between the ontological commitments internal to a framework, and the linguistic conventions upon which our framework choices are based, i.e. the distinction between I-questions on the one hand and PE-questions on the other. For given Quine's belief that Carnap dodged his ontological commitments by suggesting that one should regard his use of the framework of abstract objects as a mere linguistic convention, it was sufficient for Quine to argue that such an I/PE-distinction cannot be maintained. Indeed, when Quine first learned about the internal-external distinction in a 1949-letter from Carnap, he scribbled on the back of this letter: "When are rules really adopted? Ever? Then what application of your theory to what I am concerned with (language now)? [...] Say frameworkhood is a matter of degree, & reconciliation ensues" (QCC, 1932-1970, 417). Whether or not this is consistent with Carnap's intentions, therefore, Quine from the very beginning interpreted Carnap's distinction as one between questions internal to a framework and questions regarding the choice of the framework itself.

In sum, Quine was not out to attack the I/TE-distinction, but was concerned with undermining Carnap's I/PE-distinction. Quine did not aim to restore the legitimacy of metaphysics, but rather to criticize the Carnapian view that "statements commonly thought of as ontological are proper matters of contention only in the form of linguistic proposals" (CVO, 1951a, 210).[21] I have argued that my interpretation is supported firstly by the nature of Quine's arguments against the internal-external dis-

21 Given this misunderstanding, it is not surprising that some scholars have concluded that "Quine's criticisms leave Carnap's central points untouched" (Bird, 1995, 41). For if Carnap's "central point" was "the contrast between internal and external *theoretical questions*" (ibid., 59)—a claim that might be doubted given the background of the Carnap-Quine debate—Quine was simply not concerned with criticizing that distinction.

tinction, secondly by the way in which Quine describes Carnap's external questions, and thirdly by the background of the Carnap-Quine debate.

3.4 QUINE ON METAPHYSICAL EXISTENCE CLAIMS

If I am right in claiming that Quine aims to undermine I/PE instead of I/TE in his critical papers on Carnap's internal-external distinction, then a question that remains to be answered is what *is* Quine's position on I/TE, i.e. on the distinction between ordinary and metaphysical existence claims. After all, the claim that Quine aimed to criticize Carnap's I/PE-distinction does not imply anything about Quine's views about the tenability of the I/TE-distinction. In the remainder of this chapter, I address this latter question. I argue that Quine's position on I/TE is remarkably similar to Carnap's and that Quine, in theorizing about the nature of both truth and reference, is committed to something like an I/TE-distinction himself.[22]

Let me start by considering Quine's views on TE-questions. From the very beginning of his philosophical career, Quine has been sceptical about metaphysical existence claims. In one of his early (1938) letters to Carnap, for example, he already characterizes "metaphysical expressions" as "devoid of denotation, truth, and falsehood" (QCC, 1932-1970, 247-8). This attitude did not change when he developed his criterion of ontological commitment. For Quine has always made clear that his criterion is concerned with questions of existence "from the point of view of a given language" (ibid., 388), or as he phrases it in "On What There Is":

22 I use the qualifications "similar" and "something like" here because, as we shall see, Quine does not, like Carnap, appeal to a strict criterion of significance in dismissing traditional metaphysics.

> We look to bound variables in connection with ontology not in order to know what there is, but in order to know what a given remark or doctrine, ours or someone else's, *says* there is. (OWTI, 1948, 15)

The traditional metaphysicist's question of existence, in other words, falls outside the scope of his theory of ontological commitment. Questions about what a theory *says* there is, after all, are I-questions and not TE-questions.[23] But where Carnap has always advertised his dismissal of TE-questions, Quine, in those early stages of his career, limited himself to brief remarks in his letters to Carnap[24] and personal notes.[25] Quine's first *published* remarks concerning his views about TE-questions, as we shall see shortly, are from the 1950s.

Quine's early reservations about rejecting metaphysical questions of existence are explained by the fact that he took himself to be explicating the elements of traditional metaphysics that *are* legitimate. Quine believed that his use of the concept "ontology" in his theory of ontological commitment had been "nuclear to its usage all along" (CVO, 1951a, 204). So although Quine, like Carnap, proposed to *reinterpret* the traditional metaphysicist's questions, he did not, like Carnap, explicitly distance himself from the concepts used by those traditional metaphysicists: "meaningless words", he claimed, "are precisely the words which I feel freest to specify meanings for" (ibid., 203).

This difference in approach is illustrated by the way in which Carnap and Quine dealt with the question of nominalism. Where Carnap rejected the issue of nominalism "as meaning-

23 To be more precise, questions about what a theory says there is are partly I and partly PE, according to Quine, because he believes no statement to be purely theoretical or purely practical.

24 See also, for example, Quine's letter to Carnap from May 1, 1947: "most metaphysical statements simply mean nothing to me" (QCC, 1932-1970, 410).

25 As we shall see in section 4.7, Quine in the early 1940s already held that there simply is no transcendental perspective for the metaphysician.

less because metaphysical", Quine believed that "the problem of universals [...] *can* be given, an important meaning" (N2, 1946b, 9, my emphasis):

> As a thesis in the philosophy of science, nominalism can be formulated thus: it is possible to set up a nominalistic language in which all of natural science can be expressed. The nominalist, so interpreted, claims that a language adequate to all scientific purposes can be framed in such a way that its variables admit only concrete objects, individuals, as values. (DE, 1939b, 708)

Carnap agreed that Quine's reinterpretation of nominalism "is a meaningful problem" but doubted whether it is "advisable to transfer to this new problem in logic or semantics the label 'nominalism'", because the concept stems from the "old metaphysical problem" (1947, 43). Again, Carnap and Quine agreed about which types of questions are legitimate, but disagreed about whether or not those questions ought to phrased using the traditional metaphysicist's concepts. Carnap believed it to be safer to introduce new concepts, whereas Quine wanted to emphasize that he was explicating those elements of the traditional metaphysicist's question that *are* signficant.[26]

So although their positions seem to differ greatly at surface level, Carnap's and Quine's views on TE-questions were actually remarkably similar in the 1930s and 1940s. Both dismissed

26 See also, for example, Quine's letter to Carnap from January 5, 1943, in which Quine talks about there being a "kernel of technical meaning in the old controversy about reality or irreality of universals" (QCC, 1932-1970, 295). Somewhat related, in 1947 Goodman and Quine published their joint paper "Steps toward a Constructive Nominalism" (1947). When Goodman proposed to dub the joint position he and Quine defended 'particularism', Quine argued in a letter (June 12, 1947) that they should stick with 'nominalism' because it is "a shame to disavow a noble tradition when we are squarely in line with it" (QGC*, 1935-1994). See also Mancosu (2008, 42).

traditional metaphysics but accepted Quine's theory of ontological commitment as well as his reinterpretation of nominalism. They differed only on whether Quine's theories were to be viewed as faithful explications of the traditional metaphysicist's questions. Carnap, as we have seen, proposed to "replace" them with "practical questions concerning the choice of certain language forms" (1963a, 869), whereas Quine believed that there is no I/PE-distinction and hence proposed to replace them by questions about "the ontological commitments of a given doctrine or body of theory" (CVO, 1951a, 203). That is, Carnap and Quine agreed that TE-questions ought to be rejected but differed about whether to *reinterpret* those questions as either I-questions (like Quine)[27] or PE-questions (like Carnap). Quine's reticence to be explicit about his views on TE-questions has probably contributed to the misunderstanding that he was aiming to restore the intelligibility of metaphysical existence claims.

In later work, however, Quine *did* become more explicit about the difference between his views on ontology and the questions asked by traditional metaphysicists. He came to accept that Carnap was right in claiming that philosophers who treat questions of existence "as a serious philosophical problem [...] do not have in mind the internal question" (1950, 209). That is, he explicitly recognized that there are two ways to understand existence claims: an ordinary and a philosophical one. The traditional metaphysicist, Quine argued, is not interested in questions of ontological commitment but rather wants to "inquire into the *absolute* correctness of a conceptual scheme" (IOH, 1950a, 79) wants to know "what reality is *really* like" (SN, 1992b, 405), or, in Kantian terms, "whether or in how far our science measures up to the *Ding an Sich*" (TTPT, 1981d, 22). In response to these questions, Quine now explicitly argues against traditional metaphysics. According to Quine, any inquiry into the absolute correctness of a conceptual scheme is "meaningless"

27 But see footnote 23.

(IOH, 1950a, 79) and any question about what reality is really like is "self-stultifying" (SN, 1992b, 405) and senseless (TTPT, 1981d, 22).[28]

Yet, not only did Quine become more explicit about his position with respect to TE-questions, he also started to develop an argument against them. And just as his views on TE-questions are similar to Carnap's, his argument against those questions is in Carnapian spirit as well. As we have seen, Carnap's argument against TE-questions relies on the idea that the very concept of 'reality', which plays an important role in the metaphysicist's question about whether a certain object *really* exists, cannot be meaningfully applied outside the framework of which it is an element. If we now replace Carnap's talk about frameworks with Quine's holistic picture of science as a "man-made fabric which impinges on experience only along

28 Despite Quine's use of the word "meaningless" in (IOH, 1950a, 79), there is ample evidence that he did not, like Carnap, subscribe to a strict, philosophically potent criterion of significance. See for example (PPLT, 1970b, 7) and (FSS, 1995b, 48). I thank Peter Hylton for pressing this point. In fact, when we look at his personal notes, we see that Quine, as early as 1937, writes

> Meaninglessness must be abandoned as meaningless—at least insofar as it might be used against metaphysics. Even supposing we would make sense ultimately of an operational criterion, this would rule out all the non-intuitionistic part of math[ematics] also. But we keep latter, because *useful* algorithmically for science. We discard metaphysics because useless for science. If part of met[aphysics] became useful for science, we might use it on same grounds as non-constructive math[ematics] (April 2, 1937). (PIP*, 1937a, my transcription)

This idea that metaphysics is not meaningless but merely useless is something Quine held on to until the end of his career. See, for example, (NDL*, 1970d, 12-3, my transcription), where Quine argues that although metaphysical questions are "technically meaningful", they are "pragmatically empty and need not arise"; and (HO*, 1987c, my transcription), where Quine quotes Laplace's "Je n'ai pas besoin de cette hypothèse", and argues that metaphysical statements are "just danglers, not contributing to joint implications of observation".

the edges" (TDE, 1951b, 42), we get a very similar argument. In the early 1950s, Quine starts to argue that key philosophical concepts like 'reality' cannot be divorced from their everyday scientific applications. When the traditional metaphysicist asks us what reality is *really* like, Quine argues, she "dissociate[s] the [term] 'reality' [...] from the very applications which originally did most to invest those terms with whatever intelligibility they may have for us" (SLS, 1954b, 229).[29] According to Quine, "[t]here is no deeper sense of 'reality' than the sense in which it is the business of science itself, [...] to seek the essence of reality" (QWVO, 1996b, 348). When the traditional metaphysicist asks us about the true nature of reality, in other words, she presupposes that we can separate the term 'reality' from its ordinary scientific use. According to Quine, however, this cannot be done; to give up on our ordinary interpretation of our key philosophical terms is to give up on the very intelligibility of these concepts.

Quine's argument here depends on his holism, in this context the thesis that "the understanding of a term [cannot] be segregated from collateral information regarding the object" (CGC, 1962, 132).[30] Holism implies that we cannot completely bracket our theory of the world when we utter a sentence containing a term that is also used in our formulation of the theory. For our very understanding of the sentence then depends on our understanding of the theory. Applied to the traditional metaphysicist's question, this entails that we cannot ask about the true nature of reality while setting aside our scientific theory of the world:

29 The argument first occurs in Quine's "On Mental Entities", where he speaks about "the ordinary usage of the word 'real'" (OME, 1952a, 225). See also section 4.11.

30 Quine's holism here is closely related to the wide-scoped holism we have already encountered in section 2.6, and which plays an important role in his rejection of traditional epistemology. In chapter 5, we will take a closer look at Quine's holism.

> The question about what there is, what objects there are, is for me a question to be answered within our total empirical, scientific system of the world, and not outside it [...] We say in so many words, in the name of science, that there are sticks, stones, electrons, and classes [...] The sentences thus uttered are part of a network that enjoys good logical relations with observation sentences that are sustained by sensory stimulation. We cannot ask better than that. (SSE, 1984c, 323)

According to Quine, then, the very idea of a TE-question is useless because it purports to make use of notions that are without clear content. The traditional metaphysicist presupposes that we can make sense of our key philosophical notions independently of the conceptual scheme we presuppose in scientific inquiry. For Quine, however, asking what reality is *really* like independently of our scientific system of the world is "like asking how long the Nile really is, apart from parochial matters of miles or meters" (SN, 1992b, 405). Just like our notion of 'length' makes little sense apart from related notions like 'mile' and 'meter' and some standards of measurement, our notion of 'reality' is useless when one purports to use it in a way that transcends our theory of the world and scientific standards. Hence, we are bound to rely on our scientific concepts in interpreting the traditional metaphysicist's question: "all ascription of reality must come [...] from within one's theory of the world; it is incoherent otherwise" (TTPT, 1981d, 21).

3.5 RABBITS AND COMPLEMENT-RABBITS

Let me sum up what we have established thus far. I have argued that we ought to distinguish between two types of internal-external distinctions: I/PE and I/TE. Quine should not be viewed as aiming to attack I/TE, thereby breathing new life

into the metaphysical project that was deemed meaningless by Carnap, because he was solely concerned with undermining I/PE. In fact, Quine's perspective on TE-questions, even in the early stages of his career, is remarkably similar to Carnap's. Although Carnap and Quine disagree about how to *reinterpret* TE-questions, they both reject these questions and use similar arguments to show why they ought to be dismissed.

In the light of these conclusions, one would expect Quine to endorse Carnap's I/TE-distinction. That is, one would expect him to recognize that Carnap was right in distinguishing between legitimate and illegitimate questions of existence. Yet Quine never did; he never explicitly adopted the view that there is a sense in which Carnap's internal-external distinction is legitimate.[31] Quine's reticence on this issue is probably related to his rejection of Carnap's strict criterion of significance.[32]

31 A possible exception is "Structure and Nature", where Quine comes very close to admitting that Carnap's internal-external distinction is to some extent correct. Having rejected the question what "reality is *really* like" as self-stultifying, Quine argues that "[t]he positivists were right in branding such metaphysics as meaningless". Yet, this passage is immediately followed by the claim that the "positivists were wrong if and when they concluded that the world is not really composed of atoms or whatever" (SN, 1992b, 405). Note that Quine here first uses the term 'really' in an external sense and then switches to an internal reading in claiming that the world really is composed of atoms, as is indicated by his italicizing 'really' only in the first sentence. Unfortunately, however, Quine fails to recognize that Carnap would have agreed that the world really is composed of atoms when 'really' is interpreted in an internal sense.

32 See footnote 28. That is, even though Quine like Carnap dismisses traditional metaphysics, he speaks about "a blurring of the boundary" here as well (CPT, 1984a, 127-8). See also Quine's (CA, 1987b, 144) and (QU, 1987d, 18-21, 27-9). Since Quine's argument against TE-questions is holistic and because he only admits an empirical notion of meaning, as we shall see in chapter 5, there will also be intermediate cases in which a question has only very minimal empirical content. Quine proposes to "accept those questions as meaningful" in "interest of overall simplicity" because they "can be formulated in the same vocabulary and the same idioms that are also useful, in other combinations, elsewhere in science". Despite this blurring of Carnap's strict

Yet even though Quine does not explicitly admit it, he *is* committed to something like an I/TE-distinction. In this section and the next, I show that the later Quine implicitly makes of use of some such distinction in his disquotational theories, in which he differentiates between 'immanent' and 'transcendent' notions of truth and reference. In short, I argue that in Quine's later work, the Carnapian I/TE-distinction reappears as a distinction between the immanent and the transcendent, the distinction that also plays an important role in Quine's dismissal of traditional epistemology.

Let me start with Quine's views on reference. As is well known, Quine argues that there is no fact of the matter as to what our terms refer to. Reference is inscrutable, Quine argues, because for any given ontology T, one can mechanically construct an empirically equivalent ontology T′ by applying to T a proxy function which maps all objects and predicates of T onto, for example, their spatio-temporal complements; because both the objects and the predicates of T are reinterpreted, a T-sentence like 'the rabbit is sitting on the grass' is guaranteed to be compatible with the same data as the T′-sentence 'the complement-rabbit is complement-sitting on the complement-grass' (GML, 1997b, 189).

Prima facie, Quine's conclusions about reference seem to support the metaphysicist's claim that there are two legitimate senses of existence. For although we are perfectly happy to talk about rabbits in everyday life, there is the further conclusion that rabbits need not *really* exist, because our talk about rabbits can easily be reconstrued as being about complement-rabbits,

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distinction, however, he is still not willing to grant metaphysics a similar intermediate position: "There are further reaches of discourse [...] for which not even these claims to a scientific status can be made. One thinks here of bad metaphysics" (WWI, 1986m, 169). Presumably the reason is precisely that metaphysics *cannot* 'be formulated in the same vocabulary' as science, because the metaphysicist appeals to concepts that are divorced from their everyday scientific applications.

rabbit-stages, or rabbit-qualities, without doing any violence to our evidence:

> there is [...] a curious difference between commonsense existence statements and philosophical ones that needs to be played up [...] For let us reflect that a theory might accommodate all rabbit data and yet admit as values of its variables no rabbits or other bodies but only qualities, times, and places. The adherents of that theory, or *immaterialists*, would have a sentence which, as a whole, had the same stimulus meaning as our sentence "There is a rabbit in the yard"; yet in the quantificational sense of the words they would have to deny that there is a rabbit in the yard or anywhere else. Here, then, prima facie, are two senses of existence of rabbits, a common sense and a philosophical sense. (EQ, 1968b, 98)

Although on the level of reference, there is no fact of the matter as to what our terms refer to, on the level of existence Quine "grants" the metaphysicist the claim "that there are for him two senses of existence" (ibid., 99). As a result, in his early work on ontological relativity, Quine seems not particularly bothered by the metaphysical claims invoked by his thesis.

In later work, however, Quine's attitude changes. He argues that we *cannot* draw any metaphysical conclusion from his inscrutability thesis because the very idea of ontological relativity itself only makes sense from within our theory of the world. The metaphysicist's claim cannot have the status of what Quine calls a *transcendental* conclusion, i.e. a conclusion somehow divorced from our everyday conceptual scheme. Rather, the metaphysicist's conclusion will always be *immanent* because it depends on the very conceptual scheme that is presupposed in developing the thesis that reference is inscrutable:

> The semantical considerations that seemed to undermine [my unswerving belief in external things] were concerned

> not with assessing reality but with analyzing method and evidence. [...] Those considerations showed that I could indeed turn my back on external things [...] and ride the proxy functions to something strange and different without doing violence to any evidence. [...] To recognize this is not to repudiate the ontology in terms of which the recognition took place.[33] (TTPT, 1981d, 21)

So where the early Quine seems to accept the metaphysicist's conclusion that there are two legitimate senses of existence, he later maintains that the metaphysicist cannot use the inscrutability of reference to justify his conclusion that rabbits are, in some *philosophical* sense, not real. For this very conclusion is still an immanent conclusion, presupposing the ontology in terms of which Quine developed his thesis. The metaphysicist, in other words, mistakenly treats Quine's conclusion as a *transcendental* thesis, presupposing "that we can stand aloof and recognize all the alternative ontologies as true" (ibid.). She forgets that Quine's thesis about the evidential relation between our neural input and our ontologies depends on the very conceptual scheme in which the relation is described. What appears to be a transcendental conclusion, in other words, is in fact a conclusion immanent to our epistemology.[34] The very idea of a transcendental conclusion simply does not make much sense:

33 See also (RAB, 1984b, 295): "The truth of physical theory and the reality of microphysical particles, gross, bodies, numbers, sets, are not impugned by what I have said of proxy functions [...] Those remarks had to do not with what there is and what is true about the world, but only with the evidence for what there is and what is true about the world. I was showing that scientific discourse radically unlike our own, structurally and ontologically, could claim equal evidence and that we are free to switch. Still we can treat of the world and its objects only within some scientific idiom".

34 This is not to say, of course, that the inscrutability of reference becomes an epistemological thesis in the sense that we can never *know* whether someone is referring to rabbits or rabbit-complements. See Friedman (1975), Gibson (1986), (RRG, 1986j), Peijnenburg and Hünneman (2001) and Glock (2003, 209-10). Quine's point remains ontological: there is nothing to know.

> Transcendental argument, or what purports to be first philosophy, tends generally to take on this status of immanent epistemology insofar as I succeed in making sense of it. What evaporates is the transcendental question of the reality of the external world. (ibid., 22)

Although Quine here uses a different terminology, one concerned with immanent and transcendental questions, his distinction here roughly matches Carnap's conclusions on I/TE: only internal claims about our ontology are legitimate, any attempt to transcend our conceptual scheme and claim something about what *really* exists, any attempt 'to stand aloof', simply does not make much sense; we should not "revert to the sin of transcendental metaphysics" (SN, 1992b, 406). Quine, in sum, presupposes something like an I/TE-distinction in rejecting metaphysical interpretations of his inscrutability of reference.[35]

3.6 THE IDEAL OF PURE REASON

Quine's commitment to something like I/TE via his immanence-transcendence distinction is even more explicit in his struggle with the concept of truth. Quine, as is well known, defends a deflationary theory of truth according to which our truth predicate is nothing more than "a device of disquotation" (PL, 1970c, 12). According to Quine, to claim that a sentence like 'Snow is white' is true, is simply to claim that snow is white: "To ascribe truth to the sentence is to ascribe whiteness to snow [...] As-

35 George (2000, 14) has aptly termed Quine's argument a *linguacentristic* one: "Quine has insisted that nonsense awaits if one fails to recognize that one must work from within, that one cannot leap outside language and all systems of belief to evaluate these as from a distance". In George's terminology, my interpretation of the Carnap-Quine debate entails that both Carnap and Quine, in arguing against the I/TE-distinction, depend on linguacentrism, or as we may might call it in Carnap's case: 'frameworkcentrism'.

cription of truth just cancels the quotation marks" (PT, 1990g, 80).

In many of his latest books and papers, Quine (FSS 1995b; NLOM 1995c; RE 1995d; WDWD 1999b) ponders the question whether his disquotational theory of truth is compatible with the way in which the concept of truth is used in science, and therefore, the question whether this theory of truth is acceptable from a naturalistic perspective. Quine's musings in these papers are prompted by his observation that scientists seem to presuppose a transcendental notion of truth. That is, they seem to presuppose that truth lies beyond their theories and that their claims are true or false once and for all, regardless of whether their theories *say* they are true. Few, if any, scientists, for example, would accept that our planet started orbiting the Sun only a few hundred years ago when Copernicus' hypothesis became generally accepted. Rather, they say that Copernicus discovered something about our solar system that has been true all along:

> usage dictates that when in the course of scientific progress some former tenet comes to be superseded and denied, we do not say that it used to be true but became false. The usage is rather that we thought it was true but it never was. (WDWD, 1999b, 164)

In talking about falsified theories as having been false all along, scientists seem to presuppose a notion of truth that does not fulfil Quine's disquotational scheme: even when we believed Earth to be the center of the universe, the statement 'the Sun orbits Earth' failed to be true. Like the metaphysicist, scientists presuppose that our best scientific theories might be *really* false, even if we believe them to be true. In talking about our scientific theories, in other words, we all seem to be unregenerate realists;

we all seem to believe that the question whether or not our theories are *really* true *is* a legitimate question.[36]

At points, Quine seems convinced by the above argument, thereby coming close to acknowledging that transcendental questions, and hence TE-questions, are intelligible. In a response to Davidson (1995), for example, he concludes that truth "looms as a haven that we keep steering for and correcting to", that it is "an ideal of pure reason, in Kant's phrase", and that although truth is immanent in most respects, it is transcendent in this one (RE, 1995d, 242). Similarly, in "Naturalism; Or, Living within One's Means" Quine claims that "[w]e have scientists pursuing truth, not decreeing it" and that truth "stands forth as an ideal of pure reason [...] and transcendent indeed" (NLOM, 1995c, 472).[37]

Despite appearances to the contrary, however, Quine does not believe the above 'quirk of usage' (RA, 1994d, 230) to be incompatible with his rejection of the transcendental. Quine's commitment to the immanence-transcendence distinction is strong enough for him to seek another way out.[38] The conclusion that a recently falsified hypothesis has been false all along, Quine argues, does not require a transcendental notion of truth. Accord-

36 A related phenomenon which Quine often discusses in his later papers, is the fact that whenever we contemplate hypotheses on which science will never take a stand, we still believe these hypotheses to be either true or false. We will probably never find out, for instance, whether there were an even number of grass blades in Boston Common at the inception of 1901. Still we believe that the hypothesis that there were an even number of grass leaves is either true or false. We presuppose that all assertoric sentences are truth-valued, regardless of whether we will ever be able to establish these truth-values, and hence, we seem to presuppose the idea of a transcendental truth. See (WDWD, 1999b, 165).

37 The tension here is also noted by Glock (2003, 132).

38 See, for example, his response to Burton Dreben: "the immanent is that which makes sense within naturalism, *in mediis rebus*, and the transcendent is not. Accordingly, truth better be immanent for me [...] too" (RA, 1994d, 230); and a letter to Bergström (February 24, 1995), where Quine playfully notes that "truth is eminently immanent" (QBC*, 1988-1996, my transcription).

ing to Quine, a disquotational theory of truth might explain this fact of usage adequately, as long as we are willing to accept that this usage reflects a *theoretical* choice, a choice guided by criteria internal to science. The unregenerate realism "integral to the semantics of the predicate 'true"' (FSS, 1995b, 67), in other words, is just a reflection of a theoretical choice, a choice guided by the same standards, for example, as the choice to adopt a heliocentric world view. Just as the astronomer's belief that Earth orbits the Sun is a consequence of her theory about the Solar System, her belief that Earth *has always* orbited the Sun is a consequence of her theory that the workings of the world around us do not depend on our beliefs about that world.[39] The claim that our best scientific theories might be false is itself an immanent thesis, and hence does not conflict with Quine's rejection of the transcendental. Our belief that the world might be different from what we believe it to be is deeply entrenched, but it is not a belief somehow divorced from our scientific conceptual scheme. The metaphysicist's claim that our theories might not describe the world as it is *in itself*, on the other hand, *is* a transcendental claim and ought to be rejected.[40]

39 Cf. Hylton (2007, 277-8).

40 In a similar way, Quine solves the 'grass blades problem' mentioned in footnote 36. According to Quine, we do not need to presuppose the idea of a transcendental truth in order to account for the fact that we believe all assertoric sentences to be truth-valued, even if we will never be able to establish the truth-value of some sentences. For we only need to presuppose the law of the excluded middle to derive from disquotationalism the thesis that all assertoric sentences are either true or false. That is, if we add the law of the excluded middle to a disquotationalist account of truth, it automatically follows that assertoric sentences are truth-valued, even if science will never take a stand on some of them:

> let 'p' stand for a sentence to the effect that there were an even number of blades of grass in Boston Common at the inception of 1901. By excluded middle, p or not p; so, by disquotation, 'p' is true or 'Not p' is true. (WDWD, 1999b, 165)

As a result, Quine deals with transcendental notions of truth in the same way he dealt with the supposedly metaphysical consequences of his theory of reference. Just as the conclusion that reference is inscrutable, the thesis that the world might be different from what we believe it to be depends on our very theories about that world. In both cases, Quine argues that his theses are immanent to our conceptual scheme, and as such his argument against TE-questions remains valid: we cannot make sense of key philosophical concepts independently of our scientific system of the world. Just like our concept of reality, "[t]he concept of truth belongs to the conceptual apparatus of science on a par with the concepts of existence, matter, body, gravitation, number, neutrino, and chipmunk" (WDWD, 1999b, 165). In both cases, in other words, Quine remains committed to a distinction between immanent and transcendent notions of truth and reference.

3.7 CONCLUSION

Quine has struggled with the status of metaphysical existence claims throughout his career. After introducing his theory of ontological commitment in the late 1930s, he was reluctant to reject TE-questions because he took himself to be explicating the elements of traditional metaphysics that *are* legitimate. In later work, Quine wrestled with the supposedly metaphysical implications of his disquotational theories of truth and reference. In a sense, therefore, Quine's rejection of transcendental perspectives in metaphysics is somewhat less stable than his rejection of the transcendental in epistemology.

The law of the excluded middle, in turn, is not adopted for metaphysical reasons. Rather, it is adopted "for the simplicity of theory it affords" (WPB, 1981e, 32). That is, the decision whether or not to adopt the law of the excluded middle is just a revisable theoretical decision as well, not a transcendental metaphysical one.

Still in the end, Quine has always maintained that TE-questions ought to be dismissed. Although Quine at first only explicitly rejected TE-questions in his notes and letters to Carnap, he later dismissed those questions in published writings as well and developed an argument that is thoroughly Carnapian in spirit. Also in his theories of truth and reference, Quine resolved his issues by explicitly rejecting metaphysical claims, this time phrasing the I/TE-distinction as one between immanent and transcendent claims. Quine's arguments against traditional epistemology and metaphysics, in consequence, are cut from the same cloth. In both cases, Quine's shows how traditional philosophers presuppose the viability of a transcendental perspective and in both cases he appeals to his holism to show that such a perspective cannot be had.

A further consequence of my analysis in the present chapter is that the inconsistency in Quine's views on metaphysics, as described in the introduction, is merely apparent. Metaphysical existence claims ought to be rejected if they are interpreted as external or transcendental. They only make sense when they are reinterpreted as internal or immanent claims. Carnap and Quine are in perfect agreement on this issue. They only disagree (1) about whether transcendental claims ought to be rejected as *meaningless* and (2) about whether there is room for an alternative distinction, one between ordinary questions of existence and questions about which language to adopt in our inquiries. In sum, although Quine is sceptical about differentiating between questions of fact and questions of meaning, thereby dissolving Carnap's I/PE-distinction, Quine's perspective on the I/TE-distinction, and hence on the question whether there are *really* such things as elephants, electrons and empty sets, is closer to Carnap's than has often been presupposed.

4

BOARDING NEURATH'S BOAT

Summary: Having reconstructed Quine's rejection of transcendental perspectives in epistemology (chapter 2) and metaphysics (chapter 3), in this chapter I turn to the question as to how Quine developed his position. For even though Quine has always been a science-minded philosopher, he did not adopt a fully naturalistic perspective until the early 1950s. In this chapter, I reconstruct the genesis of Quine's ideas by examining his development in the first decades of his career. After identifying three commitments underlying his naturalism—viz. empiricism, holism, and realism—I trace the sources of these commitments to three distinct phases in Quine's early development, showing how his early empiricism gradually evolved into the naturalistic position that would have such an enormous impact on analytic philosophy. In particular, I show how Quine's adoption of a wide-scoped holism was crucial to his development, thereby providing further evidence for the strong relation between Quinean naturalism and holism, a relation which already played an important role in the chapters 2 and 3.[1]

4.1 INTRODUCTION

Many excellent papers have been written about the interpretation of Quine's naturalism, its scope, and its far-reaching consequences for epistemology, metaphysics, and the philosophy of

1 This chapter is an adapted version of the paper "Boarding Neurath's Boat: The Early Development of Quine's Naturalism" (under review-a), presented at the University of Zürich. I thank Peter Hylton, Thomas Ricketts, and Hanjo Glock for their valuable suggestions and comments.

mind.[2] Little attention has been paid to the genesis of Quine's ideas on the subject however. Although historians in recent years have contributed significantly to an understanding of the development of Quine's views on the analytic-synthetic distinction,[3] not much work has been devoted to the steps Quine took in developing his naturalism.[4]

Given that Quine did not endorse a naturalistic perspective until the early 1950s, this fact seems particularly surprising. In the early stages of his career, Quine never used the term 'naturalism' to describe his perspective on the relation between science and philosophy.[5] Indeed, as we have already seen in section 2.4, Quine himself has also noted that he became "more consciously and explicitly naturalistic" only in the 1950s; that is, "in the ten years between "Two Dogmas" and *Word and Object*" (1991b, 398).

The question that arises, therefore, is how exactly Quine arrived at the naturalistic position that would have such a tremendous impact on post-war analytic philosophy. In this chapter, I make a first stab at answering this question by reconstructing the steps that Quine took in developing his perspective. Building on Quine's early writings as well as on the existing literature about his early views in other domains, I argue that although some features of Quine's naturalism were already present in the early 1930s, the wide-scoped holism that led him to

2 For an outstanding overview, see the papers collected in Føllesdal (2001) or, more recently, Weir (2014) and Hylton (2014).

3 See, for example, Creath (1987, 1990), Isaacson (1992), Hylton (2001), Mancosu (2005), Ben-Menahem (2005, 2006), Isaac (2005, 2011), Ebbs (2011a), Frost-Arnold (2011, 2013), and Lugg (2012).

4 A possible exception is Murphey's *The Development of Quine's Philosophy* (2012), which briefly deals with one element of the evolution of Quine's naturalism, viz. the way in which his physicalistic explication of observation sentences contributed to his rejection of a phenomenalist epistemology. As we shall see, however, I disagree with Murphey on some fundamental points.

5 Quine first uses the term 'naturalism' in his 1968 John Dewey Lectures "Ontological Relativity". See section 1.1.

reject the analytic-synthetic distinction in "Two Dogmas" also played a crucial role in his development of the idea that there is no distinct first-philosophical perspective, thereby providing further evidence for the strong relation between Quinean naturalism and holism.

This chapter is structured as follows. After identifying three commitments underlying Quine's naturalism, viz. empiricism, holism, and realism (sections 4.2-4.5), I trace the sources of these presuppositions to three distinct phases within the first decades of Quine's career. First, I show that Quine has been attracted to a behavioristic version of empiricism from the earliest stages of his philosophical development (section 4.6). Next I argue that although traces of holism were already present during his graduate years, it took quite some time before he started using this idea in answering the question of how an empiricist can provide a satisfying account of logical and mathematical knowledge (section 4.8). Finally, I argue that when Quine first combined his empiricism and holism in the late 1940s, he only gradually started to grasp the radically naturalistic consequences of his position, a process that culminated in the early 1950s, when he first adopted his unregenerate realism and endorsed the view that epistemology is a science, not a distinctively philosophical project (sections 4.9-4.11).

4.2 QUINEAN NATURALISM DECOMPOSED

In order to reconstruct the development of Quine's naturalism, we first need an account of what his position essentially involves. A starting point is provided by our reconstruction of Quine's position in epistemology and metaphysics in the previous chapters, which suggests that at the most general level, Quinean naturalism consists of two elements: the principled rejection of *transcendental* perspectives on reality, and the adop-

tion of a perspective *immanent* to our scientific conceptual scheme.[6]

(NT) *No Transcendence*: the rejection of any detached science-independent perspective on reality.

(SI) *Scientific Immanence*: the *prima facie* acceptance of our inherited best scientific theories and methods.

Although we have primarily focused on NT in the preceding chapters, NT and SI together deliver some of the characteristic naturalistic theses we have encountered. Epistemologically, NT entails that we ought to abandon the "the Cartesian dream of a foundation for scientific certainty firmer than scientific method itself" (PT, 1990g, 19) and SI implies that our scientific theories do not require "any justification beyond observation and the hypothetico-deductive method" (TTPT, 1981d, 21). Metaphysically, NT shows that the transcendental question of "what reality is *really* like [...] is self-stultifying" (SN, 1992b, 405), whereas SI implies that ontological questions are "on a par with questions of natural science" (CVO, 1951a, 211).

Although NT and SI seem to be complementary, they are logically independent. A sceptic, for example, could accept NT and deny SI; she might insist that we are not justified in accepting our best theories about the world from either a philosophical or a scientific perspective. Conversely, many present-day non-naturalists will presumably accept some version of SI, granting that philosophers should at least start out presupposing that our best scientific theories and methods are largely correct, yet deny that there is no distinct philosophical perspective from which those theories and methods might be evaluated.

NT and SI, therefore, provide us with a first indication of what Quine's naturalism involves. If we are to reconstruct the

6 See in particular the sections 2.6 and 3.5-3.6 as well as the definitions Quine provides in (FME, 1975a, 72) and (TTPT, 1981d, 21).

way in which Quine developed his position, however, we need something more. For NT and SI are not just philosophical dogmas unsupported by any further arguments; if they were, Quine would be vulnerable to the objection that his naturalism itself is a transcendental extra-scientific thesis.[7] In the next three sections, I identify three commitments underlying Quine's naturalism as specified above; empiricism, holism, and realism, thereby tying together some elements we have already found to play an important role in the chapters 2 and 3. In the sections 4.6-4.11, then, I reconstruct the development of Quine's naturalism by tracing these commitments back to their origins in his work.

4.3 COMMITMENT 1: EMPIRICISM

The first commitment underlying Quine's naturalism as defined by NT and SI is pretty straightforward. If anything, Quine's position presupposes the radical empiricist thesis that all our information about the world ultimately comes from sense experience. Indeed, in "Five Milestones of Empiricism" (1975a), Quine presents naturalism as a distinct stage in the development of empiricist philosophy. At several points in the past two centuries, Quine argues, empiricism has taken a turn for the better; and the (for now) final milestone of empiricism is naturalism.[8] Since Quine thus pictures naturalism as a distinctively

7 For the objection that naturalism is self-refuting because it is itself not supported by our best scientific theories, see Almeder (1998, 64), Moser and Yandell (2000, 10), and Macarthur (2008, 10). For a response, see Verhaegh and van der Kolk (forthcoming).

8 Quine's rational reconstruction of the historical development of empiricism is partly outlined in section 2.2. Quine also connects naturalism with empiricism in (PT, 1990g, 19): "The most notable norm of naturalized epistemology actually coincides with that of traditional epistemology. It is simply the watchword of empiricism: *nihil in mente quod non prius in sensu*".

empiricist position, I take empiricism to be the first commitment underlying his position.[9]

Empiricism bears on Quine's naturalism in two distinct ways. First, it supports NT because it rules out many purported extra-scientific sources of knowledge. Traditionally, many philosophers aimed to ground our scientific theories in an indubitable a priori foundation. According to Quine, however, the empiricist can dismiss these attempts as illegitimate; there is no reason to believe that the rationalist's self-evident propositions are actually true.[10] Empiricism thus supports NT because it simply dismisses any distinctively philosophical question about the a priori foundations of science. Secondly, empiricism supports SI in providing us with an explanation of *why* we should accept our best scientific theories of the world. If one agrees with Quine that science is our best attempt to systematically account for our sensory input, then a commitment to empiricism implies that one should at least start out one's inquiries presupposing that our best scientific theories and methods are largely correct.

Empiricism, however, is not just a philosophical dogma; it is itself supported by our best scientific theories: "it is a finding of natural science itself, however fallible, that our information about the world comes only through impacts on our sensory receptors" (PT, 1990g, 19). Empiricism, for Quine, is simply our best scientific theory about our sources of knowl-

9 This is not to say that empiricism is a necessary condition for naturalism. Quine's three commitments, as will become clear, are themselves empirical and revisable theses. To say that any change in these commitments would amount to giving up on naturalism would be unnecessarily essentialistic. Rather, the three commitments might be better viewed as theses which contribute to the plausibility of naturalism as defined by NT and SI above.

10 See "Lectures on David Hume's Philosophy" (1946a, 54-9). After reconstructing Descartes' account of self-evidence in mathematics and philosophy, Quine asks: "Why should the self-evidence of mathematical axioms be a guarantee of their *truth*, rather than merely a compulsion to belief—possibly mistaken belief—on our part? And similarly for any other self-evident truth".

edge, as is exemplified by the fact that he believes it to be at least possible that scientists would one day discover that there are other sources of knowledge as well.[11] Of course, the justificatory structure here is somewhat circular: the respect for science that is embodied in Quine's naturalism is supported by empiricism, whereas empiricism itself, in turn, is a finding of science. It is characteristic of Quine's naturalism, however, that he has no qualms about such circularity; since there is no extra-scientific perspective, we cannot but presuppose science in justifying our *prima facie* acceptance of science.[12]

For the purposes of this chapter, it should be noted that Quine's empiricism is relatively strict. For Quine, concepts concerning mind and language are empirically acceptable only when we are able to provide them with behavioristic definitions.[13] The intuitive clarity of notions like 'meaning' and 'synonymy', for example, does not suffice to allow their use in our best scientific theories of the world; if we cannot explicate these concepts unambiguously in terms of behavioral dispositions, we should simply do without them. As will become clear in the sections to come, Quine's struggle with the concept of 'analyticity' in the 1930s and 1940s is partly due to the fact that

11 "There is no telepathy, clairvoyance, revelation, or extrasensory perception. This is a scientific finding, open, as usual, to reconsideration in the light of new evidence" (SSS, 1986l, 328).

12 Hylton even calls this way of reasoning the most characteristic feature of Quine's naturalism: "how do we know that the methods and techniques of natural science are our best source of knowledge about the world? Quine's predecessors within the analytic tradition [...] might at this point start [...] invoking philosophical ideas which Quine would not accept as playing this absolutely fundamental role. [...] Quine, by contrast, insists that the naturalistic claim [...] too must be based on natural science. (If this is circular, he simply accepts the circularity.) This is the revolutionary step—naturalism self-applied, as it were" (2014, 150).

13 Indeed, behaviorism is so important to Quine, that in his (PPE, 1975e, 37) he ponders the question of adding it to his list of empiricist milestones. He refrains from doing so, however, because he sees behaviorism "as integral to naturalism".

he hoped to provide it with a definition in terms of behavioral dispositions, a definition he *was* able to present in *The Roots of Reference* (1973).

4.4 COMMITMENT 2: HOLISM

In "Five Milestones of Empiricism", Quine does not only present his naturalism as a distinct stage in the history of empiricism, he also gives us a glimpse of what else he believes to be the main commitments underlying his position. After defining naturalism in the way mentioned above, he distinguishes 'two sources' of naturalism, the first of which is his holism:

> Naturalism has two sources, both negative. One of them is despair of being able to define theoretical terms generally in terms of phenomena, even by contextual definition. A holistic or system-centered attitude should suffice to induce this despair. (FME, 1975a, 72)

Holism, as we have seen, is the thesis that typical theoretical sentences have no distinctive empirical content of their own; only clusters of theory are inclusive enough to imply observable consequences. Whenever we are confronted with an observation contradicting our best scientific theories, "we are free to choose what statements to revise and what ones to hold fast" in restoring consistency between theory and evidence (EESW, 1975c, 230). Like empiricism, holism is a thesis that is itself supported by empirical findings; Quine believes it to be an empirical fact about scientific practice that scientists have many options to restore a theory's consistency with observation in the light of adverse experience.[14]

So why does Quine believe that 'a holistic attitude' supports naturalism? What he seems to have in mind in the above passage is the following: once we realize, on the basis of holistic

14 See chapter 5 for a more detailed discussion of Quine's holism.

considerations, that we cannot translate our theoretical terms in epistemologically more basic sensory concepts, we ought to acknowledge that the Cartesian dream of providing an absolute science-independent foundation for our scientific beliefs ought to be given up. If Quine's ideas about the holistic relation between theory and evidence are correct, in other words, the classical empiricist project of "deducing science from sense data" (EN, 1969a, 84) simply cannot be carried out.

As we have seen in chapter 2, however, Quine's argument is stronger than this; not only does Quine believe that we ought to "despair of being able to define theoretical terms generally in terms of phenomena" (FME, 1975a, 72), he also argues that this project is flawed from the beginning since the sense data the classical empiricists appealed to do not constitute a truly science-independent foundation to start with. Quine, we have seen, argues that "[s]ense data are posits too" (PR, 1955, 252), such that our ideas about sense experience themselves depend on prior scientific theorizing. Even if it *were* possible to translate our theoretical concepts in terms of sense data, such a reduction would not constitute a truly science-*independent* foundation for science.

This stronger argument too relies on Quine's holism; our basic observation sentences—considered analytically (see section 2.7)—too will be significant only in virtue of the contribution they make to our scientific theory as a whole. As a result, the first half of our definition of Quine's naturalism—NT above—is based largely on his holism; Quine rejects a detached extra-scientific perspective on reality because his holistic picture of inquiry leads him to the conclusion that such a perspective simply cannot be had: "Conceptualization on any considerable scale is inseparable from language [...] If we improve our understanding of ordinary talk of physical things, it will not be by reducing that talk to a more familiar idiom; there is none" (WO, 1960b, 3). Next to his rejection of an a priori science-

independent perspective on the basis of his empiricism (see p. 96 above), therefore, Quine also dismisses the possibility of an a posteriori science-independent perspective; sense data are simply not science-independent; our ideas about them themselves depend on scientific theory.

4.5 COMMITMENT 3: REALISM

Let us turn to the third commitment underlying naturalism that Quine identifies in "Five Milestones of Empiricism":

> The other negative source of naturalism is unregenerate realism, the robust state of mind of the natural scientist who has never felt any qualms beyond the negotiable uncertainties internal to science. (FME, 1975a, 72).

Why does Quine cite realism as a source of naturalism? It is my contention that the answer can be found in "The Pragmatists' Place in Empiricism", the conference-paper on which "Five Milestones" is based. In this paper, Quine compares his naturalism with the instrumentalist pragmatism of James, Schiller, and Dewey. According to Quine, these pragmatists "viewed science as a conceptual shorthand for organizing observations" (PPE, 1975e, 33), such that we cannot ascribe reality to our scientific posits and theories. Now, given his ideas about underdetermination, the view that there exist alternative conceptual schemes that would equally fit our observational evidence, Quine seems *prima facie* committed to something like instrumentalism as well. After all, his underdetermination thesis seems to imply that "the systematic structure of scientific theory [...] is invented rather than discovered, because it is not uniquely determined by the data" (ibid.). Quine, however, believes that he is not committed to such a view, precisely because of the unregenerate realism that underlies his naturalism:

> For naturalistic philosophers such as I [...] physical objects are real, right down to the most hypothetical of particles, though this recognition of them is subject, like all science to correction. I can hold this ontological line of naive and unregenerate realism, and at the same time I can hail man as largely the author rather than discoverer of truth. I can hold both lines because scientific truth about physical objects is still the *truth*, for all man's authorship [...] We are always talking within our going system when we attribute truth; we cannot talk otherwise. (PPE, 1975e, 33)

Realism, in other words, is a crucial component of Quine's naturalism; without it his position would lapse into instrumentalism. After all, instrumentalism is a variant of empiricism too, a variant moreover which is perfectly compatible with some weak varieties of holism.

A question that remains to be answered is how Quine justifies his realism. We have seen that both empiricism and holism, the first two sources of naturalism, are theses which are themselves supported by science. Have we here finally found a philosophical presupposition underlying Quine's naturalism? I believe not. Quine justifies his realism by appealing to his holism. As the last sentence of the above quote shows, Quine believes that we cannot but think about our scientific theories as true; 'we are always talking within our going system when we attribute truth; we cannot talk otherwise'. As we have seen in chapter 3, this claim should be taken quite literally: according to Quine, key philosophical concepts are without content when they are divorced from their everyday scientific applications. When the instrumentalist pragmatist accepts science but regards it "as literally false on ontological points" (PPE, 1975e, 35), she presupposes a science-independent notion of 'truth'. Similarly, when the traditional metaphysicist asks us about the true nature of reality, she presupposes that we can separate the term 'reality' from its ordinary scientific use. According to Quine, however,

this cannot be done because these very notions are elements of the conceptual scheme they are supposed to transcend; they cannot be separated from their everyday applications.[15]

The third source of Quine's naturalism, his realism, therefore, is supported by his holism.[16] According to Quine, we cannot ask about the 'reality' and 'truth' of our scientific posits and theories in a distinctively philosophical way without stripping those concepts of their intelligibility. Quine's realism therefore contributes significantly to the justification of both NT and SI. For it provides him with both an additional reason as to why there is no detached science-independent perspective and an extra argument for why we are bound to accept our best scientific theories and methods.

4.6 EARLY EMPIRICISM AND BOLD BEHAVIORISM

Now we have examined what commitments underlie Quine's naturalism as defined by NT and SI—viz. empiricism, holism, and realism—we are able to reconstruct the way in which he developed his position. In the remainder of this chapter, I piece together the evolution of Quine's naturalism by examining the origins of his commitments one by one, showing that although some features of Quine's naturalism were already present in the early 1930s, the wide-scoped holism that led him to reject the analytic-synthetic distinction in "Two Dogmas" also played a crucial role in the development of his naturalism.

Let me start with Quine's empiricism, the first source of naturalism distinguished above. Determining the roots of Quine's

15 Quine makes the connection between naturalism, instrumentalism and the anti-transcendentalist argument explicitly in a response to Hookway: "Hookway finds "Two Dogmas" instrumentalist. I think this is fair, and that it applies to my later work as well. But realism peeps through at the checkpoints, and takes over altogether when we adopt a sternly naturalistic stance and recognize 'real' as itself a term within our scientific theory" (RA, 1994d, 233).

16 See also the sections 3.4-3.6.

empiricism is not a complicated affair. From the very beginning of his career, Quine has been a determined empiricist; nowhere does he question its plausibility or even take seriously alternative positions. In fact, on the few occasions where he looks back on his intellectual development, Quine suggests that he was even committed to a strictly *behaviorist* variant of empiricism from the very start. Reflecting on his dismissal of some intensional notions in Whitehead and Russell's *Principia Mathematica* during his final year at Oberlin College, for instance, Quine notes:

> The distrust of mentalistic semantics that found expression in "Two Dogmas" is thus detectable as far back as my senior year in college. Even earlier I had taken kindly to John B. Watson's *Psychology from the Standpoint of a Behaviorist*, which Raymond Stetson had assigned to us in his psychology class. Nor do I recall that it shocked any preconceptions. It chimed in with my predilections.[17] (TDR, 1991b, 390)

Exemplary of Quine's early empiricist commitment are his ideas about the relation between empiricism and pragmatism. *Prima facie*, pragmatist philosophers have played an important role in Quine's early development: William James' *Pragmatism* was one of the first philosophical books he read (AWVQ, 1986a, 6), one of his teachers during his graduate studies at Harvard was C. I. Lewis, and the young Quine was present when John Dewey gave the first of the Williams James Lectures in 1931 (TML, 1985, 345). Still, Quine has never really understood what it means to be a pragmatist except if one classifies it as part of a global empiricist movement:

17 See also (TML, 1985, 59) and (AWVQ, 1986a, 7). Quine's early sympathy with behaviorism also shows itself in his student papers from the late 1920s and early 1930s. See, for example, (MMT*, 1930a, 9) and (BTJ*, 1930b). An excellent account of Quine's development at Oberlin as well his graduate years at Harvard is provided by Isaac (2005, §§2-3).

> It is hard to say what constitutes pragmatism. If one considers it a branch of the empiricist tradition then yes, it is very important to me [...] But I don't think that the influence on me was distinctively American; it was rather one of international empiricism.[18] (TCL, 1994f, 60-1)

Further evidence for Quine's early commitment to a strictly behaviorist variant of empiricism is his approach to analyticity in the 1930s and 1940s. Although Quine gave up on the analytic-synthetic distinction only in the late 1940s, as we shall see in section 4.8, he was already seeking a behavioristically acceptable notion of analyticity in the early stages of his career. Even in his 1934 "Lectures on Carnap", for instance—lectures he would later describe as "abjectly sequacious" (TDR, 1991b, 391)[19]—Quine proposes that we render

> only such sentences analytic as we shall be most reluctant to revise when the demand arises for revision in one quarter or another. These include all the truths of logic and mathematics; we plan to stick to these in any case, and to make any revisions elsewhere. (LC, 1934, 63)

Where analyticity has always served an epistemic function for Carnap, Quine here interprets the concept in strictly psychological terms; we call the truths of logic and mathematics analytic because it is a psychological fact that we will not give

18 See also (RPR, 1992a, 213): "I do feel philosophically akin to Dewey and C. I. Lewis [...] My hesitation over the classification of pragmatist [is] only my uncertainty over what distinguishes a pragmatist from any other empiricist". Moreover, it should be noted that in his John Dewey Lectures, Quine praises Dewey not for his pragmatism, but for his insight that one should study knowledge, mind, and meaning "in the same *empirical* spirit that animates natural science" (OR, 1968c, 26). See also Koskinen and Pihlström (2006, §1) and Godfrey-Smith (2014).

19 See also, (EBDQ, 1994b, 153) where Quine describes his lectures as "completely uncritical".

them up in the light of adverse experience.[20] In other words, even though Quine here still believes that a distinction between the analytic and the synthetic can be drawn, he already interprets the distinction behavioristically as one between sentences which are and sentences which are not candidates for revision when confronted with recalcitrant experience.[21]

4.7 QUINE'S NATURALISM IN THE EARLY 1940S

So Quine was a committed empiricist from the very start. Does this imply that he had also adopted a broadly naturalistic perspective? Before we move on and examine the development of his holism and realism, this section explores Quine's early ideas about the relation between science and philosophy. I show that his views were already strikingly naturalistic, albeit in a somewhat embryonic form. Some crucial elements, I argue, were still

20 See also, for example, Creath (1987, 485-6) and Hylton (2001). I follow Creath in characterizing Quine's definition as 'psychological' because, technically, Quine does not mention behaviorism in his lectures. Still this is what underlies Quine's definition. In "Truth by Convention", a paper that largely resembles his first lecture on Carnap, Quine writes that the apparent contrast between a priori and a posteriori truths (and thus the analytic and the synthetic) retains reality "*behavioristically* [...] as a contrast between more and less firmly accepted sentences" (TC, 1936, 102, my emphasis). Interestingly, Frost-Arnold (2011, 300n15) suggests that Quine's identification of the a priori with claims that can be held true come what may might be influenced by C. I. Lewis, who held that the a priori is that "which we can maintain in the face of all experience, come what will" (1929, 231). Recently, Lewis' influence on Quine's development has received quite some attention in the literature. See, for example, R. Sinclair (2012) and Murphey (2012, ch. 1).

21 See also "Notes on Existence and Necessity" (1943b) and Quine's lecture "On the Notion of an Analytic Statement' (1946c)': "We have to have some criterion of this kind: if users *behave* thus and so, then this is a semantical rule depending on meanings; otherwise it is a rule depending on fact rather than meanings, so that the statements whose truth follows from it are synthetic rather than analytic" (ONAS, 1946c, 33, my emphasis).

lacking—elements which, as we shall see, came to full development in the late 1940s and early 1950s.

We can get an excellent overview of Quine's position in the early 1940s from the notes, drafts, and lectures that are stored at Houghton Library.[22] For although Quine's publications in this period were still largely concerned with logic and semantics, the Houghton archives contain a wealth of unpublished material in which Quine explores more broadly philosophical topics.

For our present purposes, one of the most interesting manuscripts is a series of notes from 1944 in which Quine reflects on the relation between the philosopher's and the scientist's tasks in ontology, an issue which might have come up in his thinking through his criterion of ontological commitment. In the notes, fragments of which would later be used in both "On What There Is" and *Word and Object*, Quine argues for the position that the question "what is there?" is "broad enough to allow both philosopher[s] and scientist[s] to move about in it without treading on each other's toes" (ON*, 1944b, my transcription). He writes:

> The philosopher's task differs from that of the natural scientist or mathematician no less conspicuously than the tasks of these latter two differ from each other. The natural scientist and the mathematician both operate within an antecedently accepted conceptual scheme but their methods differ [...] The philosopher, finally, unlike these others, focuses his scrutiny on the conceptual scheme itself. Here is the task of making things explicit that had been tacit, and precise that had been vague; of uncov-

22 See especially (EJ*, 1937-1944) and (OM*, 1944-1951). Most items in these and other folders are autograph manuscripts, the majority of them related to Quine's planned book on ontology and semantics. For Quine's book plans, see his (April 19, 1945) letter to Goodman (QGC*, 1935-1994) and Murphey (2012, 53) .

> ering and resolving the paradoxes, smoothing out the kinks, lopping off the vestigial growths, clearing the ontological slums (November 5, 1944). (ON*, 1944b, my transcription)

Given the strong resemblance to his later ideas,[23] Quine's perspective on the philosopher's task here is already well developed. The same conclusion can be drawn with respect to Quine's ideas about the philosophers' vantage point:

> It is understandable, then, that the philosopher should seek points outside the world that imprisons natural scientist[s] and mathematician[s]. He would make himself independent of the conceptual scheme which it is his task to study and revise. "Give me που στω" Archimedes said, "and I will move the world". However there is no such cosmic exile. The philosopher cannot study and revise the fundamental conceptual scheme of science and common sense, without having meanwhile some conceptual scheme, whether the same or another no less in need of philosophical scrutiny, in which to work.[24] The philoso-

23 Cf. (WO, 1960b, §56): "What distinguishes between the ontological philosopher's concern and all this is only breadth of categories [...] it is scrutiny of this uncritical acceptance of the realm of physical objects itself, or of classes, etc., that devolves upon ontology. Here is the task of making explicit what had been tacit, and precise what had been vague; of exposing and resolving paradoxes, smoothing kinks, lopping off vestigial growths, clearing ontological slums".

24 Again, cf. (WO, 1960b, §56): "The philosopher's task differs from the others', then, in detail; but in no such drastic way as those suppose who imagine for the philosopher a vantage point outside the conceptual scheme that he takes in charge. There is no such cosmic exile. He cannot study and revise the fundamental conceptual scheme of science and common sense without having some conceptual scheme, whether the same or another no less in need of philosophical scrutiny, in which to work". Also interesting in this respect, is a note (from November 28, 1941) in which Quine writes that he, in his "tentative ontology", is "[s]tarting at the middle" (TO*, 1941, my transcription).

> pher is in the position rather, as Neurath says, "of a mariner who must rebuild his ship on the open sea [...]" (November 5, 1944). (ON*, 1944b, my transcription)

Again, the similarity with his later position is remarkable; like the fully naturalistic Quine, the early Quine is entirely committed to offering a completely science-immanent perspective.[25]

A question that naturally arises in the light of these strong similarities, however, is why Quine has never expressed these naturalistic propensities in his publications, i.e. the question why it takes him almost a decade before he is willing to publicly commit himself to a truly science-immanent philosophy? I think the answer to this question can be found in a different set of notes and drafts. They reveal that, although Quine *wants* to commit himself to a fully naturalistic position, he *cannot* do this because he has not yet succeeded in combining his naturalized conception of ontology with a plausibly naturalized conception of epistemology. To see this, consider the following three fragments:

> Here is a straightforward view, likely to be held by a physicist unspoiled by philosophy. The physicist—even

This anticipates the first section of *Word and Object*, where Quine claims that however we "[a]nalyze theory-building [...] we all must start in the middle".

25 At best, one can detect a difference in emphasis. Where the fully naturalistic Quine is prone to focus on the continuity between science and philosophy, Quine in these early notes is more inclined to emphasize their distinctness. For although they are both working immanently, the scientist and the philosopher do not yet seem to be concerned with the same project. Rather, as we have seen, Quine believes that the ontological question is 'broad enough to allow both philosopher[s] and scientist[s] to move about in it without treading on each other's toes'. This difference in emphasis is also exemplified in other passages of the note we have been looking at: "But if the philosopher has access to no transcendental vantage point, still his method differs in an important way from the methods of natural science and mathematics" (ON*, 1944b, my transcription).

> he—is not likely to say his atoms are more *real* than the tables, chairs, etc. [...] If physicists do not make the atoms more real than macroscopic objects, some physicists—tainted with philosophy—do make them less so [...] But the macroscopic objects are rather arbitrary as a basic reality, for certainly these are inferred from a yet more immediate zone in much the way that atoms were inferred from the macroscopic objects [...] In latter event we seem to have swung to complete contradiction of the point of view initially considered. Things are made up now not of atoms but of perceptions. Seemingly two rival theories of things, the atomic theory and the sensory theory. Materialism vs. empiricism. Realism vs. idealism (January 30, 1943). (TH*, 1943c, my transcription)

> Purpose of the book is to [...] dissociate ontology from epistemology so completely as to render it immune to the idealist (subjectivist) arguments (March 19, 1944). (WMB*, 1944d, my transcription)

> There is a sense in which physics might be said to be concerned with explaining the nature of reality. And who contests this? Primarily the Idealist [...] The Idealist would take the perceptions etc. rather as the basic reality, and derive things as constructions, logical constructs (Russell). The study of how to make these constructions is Epistemology. And things are composed not of atoms but of perceptions, sense qualia etc. (October 4, 1944). (SO*, 1944c, my transcription)

Quine in these fragments is worried about the objections of phenomenalist epistemology—objections which lead to the conclusion that his scientific ontology is somehow unreal after all. Where the fully naturalistic Quine replaces epistemological talk about sense data with its scientific analogue—the stimulation

of sensory receptors—Quine at this point does not yet have this solution at his disposal.[26] Quine's problem, in other words, is that even though he has succeeded in naturalizing metaphysics, showing how the philosopher does not require a transcendental perspective in ontology, he has not yet found a way to naturalize epistemology, i.e. to get rid of the phenomenalists' transcendental perspective.[27] It is because of this reason, I assume, that Quine in "On What There Is" settles for a pluralistic solution:

> the question what ontology to adopt still stands open, and the obvious counsel is tolerance and an experimental spirit. Let us by all means see how much of the physicalistic conceptual scheme can be reduced to a phenom-

26 To be sure, Quine does try to find ways to dismiss phenomenalism. In different fragments, Quine proposes different solutions to the phenomenalists' objections. One of the most interesting solutions from a contemporary perspective is the following (March 27, 1944): "Ontology & epistemology: how they are distinct & how they are mutually inclusive. Ontology is realistic, epistemology idealistic; but no contradiction" (OE*, 1944a, my transcription). This mutual inclusivity may remind us of Quine's later idea that epistemology and ontology are reciprocally contained (EN, 1969a, 83), although this fragment is too sketchy to attribute such a complex idea to Quine here. In one passage, Quine comes remarkably close to his later solution. See (TO*, 1941, my transcription): "Epistemology as a segment of a psychological study. Problem of priority. Answer in child psychology?" (November 1941).

27 For a different account of Quine's ideas in the early 1940s see Murphey (2012, 54-5), who seems to argue that Quine *himself* was a phenomenalist at this point. Cf. Frost-Arnold (2013, 35-6) and Ebbs (2015, 6-7). I think it is clear from the above passages, however, that Quine wants to get rid of the phenomenalists' objections, even if he does not see a satisfying way to do so. See also (TH*, 1943c, my transcription), where Quine after introducing the phenomenalist's perspective says: "Bear with me, dissenting reader; I am going to end up in agreement with *you*. But first let us see how this thing runs"; and (SO*, 1944c, my transcription): "we aren't throwing out philosophy with epistemology, leaving ourselves with nothing but physics. There remains ontological problem of essentially philosophical character, though *not* epistemological". Murphey's reading of Quine here might be caused by his being unaware of the above passages in which Quine expresses his commitment to a science-immanent perspective.

> enalistic one; still physics also naturally demands pursuing, irreducible *in toto* though it be. Let us see how, or to what degree, natural science may be rendered independent of platonistic mathematics; but let us also pursue mathematics and delve into its platonistic foundations. From among the various conceptual schemes best suited to these various pursuits, one—the phenomenalistic—claims epistemological priority [...] This point of view is one among various, corresponding to one among various interests and purposes. (OWTI, 1948, 19)

In what follows I shall argue that one of the crucial steps Quine had to take in integrating these different conceptual schemes into one single science-immanent perspective was to develop a thoroughly holistic conception of inquiry.

4.8 NARROW AND WIDE HOLISM

So let us look at the development of Quine's holism. Reading Quine's work on the nature of scientific inquiry from the 1930s, one might get the impression that he was already committed to holism in the early stages of his career. In his graduate school paper "Concepts and Working Hypotheses", for instance, Quine advances a view that seems pretty close to the holistic picture sketched in the last section of "Two Dogmas":

> If a recalcitrant item of experience, belonging to the field in question, should subsequently arise, modification somewhere in the system must take place, for it has been noted that a satisfactory conceptual system must accommodate every experience falling within the field. Thus it is that only the working hypothesis can stand which has endured without the emergence of any anomaly in the whole mass of experience since its inauguration. [...] In brief, one has a certain latitude as to where he may make

> his readjustments in the event of an experience recalcitrant to his system; and correspondingly there is some subjective option as to whether a chosen concept or a working hypothesis is to be branded as the point of 'error' in the antecedent system.[28] (CWH*, 1931)

Similarly, in his "Lectures on Carnap" (1934), Quine also seems to endorse a holistic picture of inquiry in arguing that "the accommodation of new discoveries in science is constantly occasioning revision of old hypotheses, old empirical laws", and that in general "we can choose, to *some* extent, where to revise, what principle to dislodge" (LC, 1934, 62-63).

Still, it would be a mistake to ascribe to the early Quine the radical holism he advanced in the 1950s, the crucial difference being that he did not yet apply his holism to logic and mathematics. Quine's holism was still of a narrow scope, applying only to the empirical sciences. Quine had better hopes of explaining the supposedly a priori character of logical and mathematical knowledge, like Carnap, in terms of analyticity.[29] In "Truth by Convention", for instance, Quine still embraces an

28 See also Isaac (2005, 212-20) and R. Sinclair (2012, 342). Quine's way of thinking here can, to some extent, be traced back even to 1927, when he wrote that "[m]an uses as an outline for his knowledge the natural relationship of all things, so far as he has been able to determine that relationship in the incompleteness of his data. This web—to change the metaphor—which he has thus succeeded in partially spinning, he reinforces with synthetic thread of his own manufacture: to wit, the conventional classifications and man-made systems of compilation which form so large a part of human knowledge. These two kinds of relationship—the natural and the artificial—work together in such a way that often they are not to be distinguished one from the other. Thus man formulates, for his own convenience, a general principle seeming to conform to the phenomena which he has observed and collected" (OK*, 1927).

29 Indeed, Carnap himself also combined his analyticity-based explanation of logical and mathematical knowledge with a narrow-scoped holism concerning the physical domain: "it is, in general, impossible to test even a single hypothetical sentence [...] *the test applies, at bottom, not to a single hypothesis*

analyticity-based account of logical and mathematical knowledge:

> There are statements which we choose to surrender last, if at all, in the course of revamping our sciences in the face of new discoveries; and among these there are some which *we will not surrender at all*, so basic are they to our whole conceptual scheme. Among the latter are to be counted the so-called truths of logic and mathematical truths [...] Now since these statements are destined to be maintained *independently of our observations of the world*, we may as well make use here of our technique of conventional truth assignment and thereby forestall awkward metaphysical questions as to our a priori insight into necessary truths. (TC, 1936, 102, my emphasis)

Although Quine here, as a committed empiricist, clearly does not want to invoke a metaphysical explanation of our supposedly a priori knowledge of logical and mathematical truths, he neither expands his holism to logic and mathematics so as to claim that our knowledge of those truths is ultimately a posteriori; he still believes that our logical and mathematical truths are 'maintained independently of our observations of the world'.[30]

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but to the whole system of physics as a system of hypotheses (Duhem, Poincaré)" (1934, 318).

30 This is not to say, of course, that Quine had not yet taken the possibility of an a posteriori explanation of logical truth into consideration. Carnap's notes of his discussion with Quine about *The Logical Syntax of Language* in Prague 1933, first discovered by Tennant (1994), reveal that Quine had already questioned Carnap's strict distinction between the analytic and the synthetic:

> He says after some reading of my "Syntax" MS:
> 1. *Is there a difference in principle between logical axioms and empirical sentences?* He thinks not. Perhaps I seek a distinction just for its utility, but it seems he is right: gradual difference: they are the sentences we want to hold fast (translation by Quine, TDR, 1991b, 391).

So the question that naturally arises is when Quine *did* come to accept a wide-scoped holism that applies to logic and mathematics. There has been considerable debate about this question in the literature. Richard Creath (1990, 31) argues that "[i]t was not until 1947, and then in private correspondence, that Quine came fully and finally to reject Carnap's doctrine that there are analytic truths", whereas Paolo Mancosu (2005, 331) points to a letter Quine wrote to J. H. Woodger in 1942, in which he argues that Carnap's "professedly fundamental cleavage between the analytic and the synthetic is an empty phrase" (QWC*, 1938-1982). Greg Frost-Arnold (2011, §5) defends an intermediate position. He argues that although Quine gave up on Carnap's *semantic* version of the analytic-synthetic distinction from the early 1940s onwards, he "was not yet willing to commit himself to the radical view of 'Two Dogmas' until shortly before writing that piece".[31]

In any case, even if Quine had already considered the possibility of gradualizing the analytic-synthetic distinction in the 1930s, he did not fully reject explanations in terms of analyticity until the late 1940s, as we shall see below.

31 Isaac (2005, 2011) defends still another hypothesis. According to him, Quine's motives for not publicly attacking the analytic-synthetic distinction until the early 1950s were largely political: "Up to the late 1940s, [Quine] had been content to mute his disquiet for the sake of presenting a united front on logical empiricism to the American academy" (2011, 274). Given Quine's public discontent with Carnap's semantic turn as well as any notion of analyticity that could not be explicated in terms of behavioral dispositions, however, I do not think that these political reasons can explain his refusal to reject the analytic-synthetic distinction in the early 1940s. Furthermore, also in private Quine was still actively seeking a behavioristically acceptable definition of synonymy at the time, as is exemplified by "Foundations of a Linguistic Theory of Meaning" (FLM*, 1943a), an unpublished manuscript from August 1943. In this paper, Quine attempts to formulate an empirically satisfying definition of synonymy, but fails to find one that lives up to his behavioristic standards. See Murphey (2012, 51-3). See also Quine's (August 14, 1943) letter to Church: "I would hope eventually for an empirical definition or criterion of synonymy as applied to natural languages" (QCC*, 1935-1994).

Whatever the exact timeline of his adoption of a wide-scoped holism, however, two series of events seem to have been particularly important for Quine's evolving ideas on the matter. First, in the academic year of 1940-1941, Quine regularly met up with Carnap and Tarski to discuss, among others, Carnap's forthcoming *Introduction to Semantics* (1942).[32] As Mancosu points out in his (2005, §2), Tarski at the time defended a view that comes close to Quine's wide-scoped holism in "Two Dogmas". Already in 1930, a note in Carnap's diary shows, Tarski held that "between tautological and empirical statements there is only a mere gradual and subjective distinction" (Haller, 1992). Even more revealing evidence that Tarski already defended something close to wide-scoped holism years before Quine came to accept the view, is a letter Tarski sent to Morton White in 1944:

> I think that I am ready to reject certain logical premisses (axioms) of our science in exactly the same circumstances in which I am ready to reject empirical premisses (e.g., physical hypotheses) [...] Explanation: we reject certain hypotheses or scientific theories if we notice either their inner inconsistency, or their disagreement with experience, or rather with individual statements obtained as results of certain experiences. No such experience can logically compel us to reject the theory: too many additional hypotheses [...] are always involved. [...] Axioms of logic are of so general a nature that they are rarely affected by such experiences in special domains. However, I don't see here any difference 'of principle'; I can imagine that certain new experiences of a very fundamental nature may make us inclined to change just some axioms of logic. And certain new developments in quantum me-

32 Carnap's dictation notes of these discussions are published and examined in Frost-Arnold (2013).

> chanics seem clearly to indicate this possibility. (White and Tarski, 1987, 31-2)

Looking back on his 1940-41 discussions with Tarski and Carnap, Quine recalls how he and Tarski argued "persistently with Carnap over his appeal to analyticity" in the opening pages of *Introduction to Semantics* (TDR, 1991b, 392). Surely, Quine must have learned about Tarski's wide-scoped holism somewhere in this period as well.[33]

Next to Tarski's influence, a second series of events seems to have contributed significantly to Quine's adoption of a wide-scoped holism, viz. his triangular correspondence with Morton White and Nelson Goodman in 1947. In a series of letters, Quine, White, and Goodman discussed, among other things, the prospects of Quine's search for a behavioristically acceptable definition of analyticity, a definition Quine still thought was needed in order to account for logical and mathematical knowledge. In May 1947, White asked Quine to comment on a manuscript that he would later publish as "On the Church-Frege Solution of the Paradox of Analysis" (1948). Briefly put, the paradox runs as follows. Consider the following two statements:

33 See also Frost-Arnold (2011, 301): "It seems unlikely that Tarski never voiced these views about logic in Quine's presence during their year together at Harvard". Frost-Arnold argues that the 1940-41 discussions were important for Quine's development in two different respects as well. First, Tarski presented to the group a proposal for a nominalist language, a language in which portions of arithmetic become synthetic, confronting Quine with the possibility of contracting the number of supposedly analytic truths. Second, the discussions revealed that Carnap had adopted a semantic approach to explicating analyticity, a move that conflicted with Quine's extensionalism, such that Quine came to "reject Carnap's then-current account of analyticity and perhaps made Quine even more suspicious in general of a notion he had begun to be skeptical about in 'Truth by Convention,' written when Carnap still accepted the extensional and syntactic approach" (ibid., 314).

(1) The attribute of being a brother is identical with the attribute of being a male sibling.

(2) The attribute of being a brother is identical with the attribute of being a brother.

Intuitively, (1) is informative whereas (2) is not. Yet, if (1) is true, then both statements say the same thing.[34]

Quine, who had already corresponded with White on the paradox in 1945, suggests that the paradox might be solved using C. I. Lewis and Carnap's distinction between intensional and structural synonymy.[35] The problem with this solution, however, is that Quine did not know of any behavioristically acceptable definition of intensional synonymy. Still, Quine's letter shows that he had not yet given up hope of finding such a definition:

> It's bad that we have no criterion of intensional synonymy; still, this frankly and visibly defective basis of discussion offers far more hope of clarity and progress, far less danger of mediaeval futility, than does the appeal to attributes, propositions, and meanings. (GQW, 1947, 339-40)

In response to both White and Quine, however, Goodman defended a much more stringent position than Quine, arguing that "the lack of any behavioristic criterion (or even the dimmest

34 In one letter, Quine shows that the paradox can also be formulated without invoking attributes: "An 'analysis' has the form $\ulcorner \zeta = \eta \urcorner$, where ζ and η are synonymous; therefore the whole analysis is synonymous with, or translatable into, the triviality $\ulcorner \zeta = \zeta \urcorner$" (GQW, 1947, 339).

35 Two statements are intensionally synonymous when they have the same intension, whereas structural synonymy is a narrower relation which depends on the statements' constituents and their syntactic order. All analytic statements have the same (null-)intension but not all analytic statements are structurally synonymous. See Lewis (1946) and Carnap (1947). Appealing to this distinction solves the paradox according to Quine because (1) and (2) above are intensionally but not structurally synonymous.

suggestion as to how one might be set up) is a sign that we are not at all clear as to what it is that we have to define". According to Goodman, the whole project of seeking acceptable definitions of analyticity and synonymy was to be rejected: "when Van uses a term and hopes for a behavioristic criterion he can't vaguely outline, he is employing a meaningless mark or noise on the ground that he needs it (like 'God') in his life and hopes that a meaning will be found for it" (ibid., 343).

Quine, obviously not very happy to be placed in the intensionalist camp by Goodman ("I have always been all for extension, with the world against me"), responded by backing Goodman's position. Quine now granted that he also "doesn't know how to apply 'analytic', much less define it" (ibid., 353-4). Goodman then, in his final letter, urges Quine to give up on the project of defining analyticity and to accept that the analytic-synthetic distinction simply cannot be drawn:

> If Van agrees that he not only doesn't know how to define "analytic" but doesn't know how to apply it either, what is it that he is hoping to find a behavioristic definition for? [...] he is looking for a behavioristic definition for which the test of adequacy will presumably be in accordance with a usage which he doesn't have before him. It seems to me he is then in the same position that he would be if he were to set out to define the Calubrian word "Phwanischk". (ibid., 356-7)

Of course, it is a matter of speculation how instrumental Goodman's pressure was.[36] Still, the fact is that Quine did adopt a wide-scoped holism shortly after Goodman's final letter.[37]

36 Especially Creath (1990, 35) and Isaac (2011, 275) emphasize the importance of the triangular correspondence to Quine's rejection of the analytic-synthetic distinction.

37 In "Animadversions on the Notion of Meaning", given as a talk at a colloquium at the University of Pennsylvania in 1949 (where Goodman was based

4.9 UNREGENERATE REALISM

Let me briefly recap the main conclusions of our discussion thus far. Quine's naturalism as defined by NT and SI presupposes three substantive commitments: empiricism, holism, and realism. I have shown that Quine defended a strictly behavioristic version of empiricism and a narrow-scoped holism from the very beginning of his career. These commitments led him to seek a behavioristically acceptable definition of analyticity, such that we might explain our logical and mathematical knowledge in an empirically satisfying way. Somewhere in the late 1940s Quine, probably influenced by Tarski and Goodman, found a solution in extending his holism to logic and mathematics, thereby dissolving the need for a behavioristic explication of analyticity.

The resulting wide-scoped holism Quine defends in "Two Dogmas" did not only constitute a break with Carnapian logical positivism, it also represents a major step in Quine's growing dissatisfaction with first-philosophical perspectives and hence in the development of his naturalism. For even though an analyticity-based account of logical and mathematical knowledge is in line with the empiricist thesis that all our knowl-

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at the time), Quine presents many of his arguments from "Two Dogmas" against standard definitions of analyticity and, for the first time, proposes something very close to his now famous alternative:

> Another view, not distinguishing the [linguistic and factual] components: we have our sense experience, and our own system of beliefs [...] But it is underdetermined by experience. System as a whole must conform to experience along periphery; but disconformities can be repaired each by any of many changes of the system. We choose by two canons: 1) maximum elegance of whole system, 2) maximum conservationism. By 2), the more central principles resist change the more. These might be called the *more analytic*: matter of degree. (ANM, 1949, 155)

edge about the world is ultimately based on sense experience, such an account still aims to justify this logical and mathematical knowledge from an extra-scientific perspective.[38] Where Quine in his "Lectures on Carnap" was still defending the claim that an analyticity-based approach "has the importance of enabling us to pursue foundations of mathematics and the logic of science without encountering extra-logical questions as to the source of the validity of our a priori judgments" (LC, 1934, 66), the later Quine would reject any attempt to provide our logical and mathematical knowledge with an extra-scientific foundation.

Still, as I have argued in section 4.5, the holistic empiricism defended in "Two Dogmas" does not yet constitute a completely naturalistic perspective; without his unregenerate realism, Quine's position was still compatible with the instrumentalist pragmatism of James, Schiller, and Dewey. Indeed, Quine's remarks about physical objects being "comparable, epistemologically, to the gods of Homer" in "Two Dogmas" (TDE, 1951b, 44) are often interpreted as deeply instrumentalist. The problem, as we have seen in section 4.7, is that Quine had not yet found a way to naturalize epistemology. Even though Quine recognized that we are committed to physical objects via his criterion of ontological commitment, he had not yet found a way to get rid of the transcendental, distinctively epistemological point of view relative to which physical objects are myths.

The final stage in the development of Quine's naturalism, therefore, was to find a consistent way to reject the idea that we can picture the 'epistemological point of view' as a transcendental perspective which potentially undermines our realism

38 See also, Ebbs (2011a, 218n27): the problem with an analyticity-based account of logical and mathematical knowledge is that a definition of analyticity in a certain language system "is designed to explicate a conception of justification for accepting statements that is independent of the statement's explanatory contribution to a scientific theory—a conception of justification that [the naturalistic] Quine associates with first philosophy".

about physical objects. Quine came to adopt the idea that the epistemological point of view could be naturalized, could be incorporated into his wide-scoped holism, such that our talk about sense experience is translated in terms of the triggerings of our sensory receptors. That is, he came to espouse the view that we can be unregenerate realists about our physical and mathematical objects and still inquire into the relation between science and sense experience.

In the final two sections, I show that we can distinguish two phases in this process. First, in the late 1940s, Quine abandoned his attempts to develop a nominalistically acceptable account of mathematics, accepting that our commitments to physical and mathematical objects are epistemically on a par. Second, around 1952 Quine started defending the view that even our phenomenalistic posits, presupposed in most traditional epistemological perspectives, are not in any sense more basic than our mathematical and physical posits, thereby removing the final reason not to adopt a full-blooded naturalism.

4.10 FROM NOMINALISM TO REALISM

Let me start by considering Quine's evolving views on the ontological status of abstract objects. In the 1930s and 1940s, Quine was actively seeking a nominalistically acceptable account of mathematics. In his intellectual autobiography, Quine confirms that he already "felt a nominalist's discontent with classes" when he was visiting Vienna and Prague in the early 1930s (AWVQ, 1986a, 14). This uneasiness with abstract objects resulted in a series of lectures and papers,[39] which ultimately culminated in "Steps toward a Constructive Nominalism", a paper he wrote with Nelson Goodman (SCN, 1947).[40]

39 E.g. (N1*, 1937b), (LAOP, 1939a), (DE, 1939b), and (N2, 1946b).

40 For a detailed historical account of Quine's ideas about nominalism, see Decock (2002c, ch. 2) and Mancosu (2008).

For our present purposes, it is particularly interesting to examine Quine's *reasons* for seeking a nominalist interpretation of mathematics. Reviewing his lectures and papers, it becomes clear that these reasons were at least partly philosophical. In his 1946 lecture on nominalism, for example, Quine distinguishes between a 'mental' and a 'physical' version of nominalism, the former allowing only mental and the latter allowing only physical particulars, and sketches the motives behind these views:

> In the mental case [the nominalist's] motive may be an extreme sensationalism: what we are presented with are sensory events, and it is unphilosophical to assume entities beyond them, *in particular universals*. In the physical case, his mentality is likely to be that of Lord Kelvin, who insisted that he did not understand a process until it was reduced to terms of impact of bodies like billiard balls [...] Modern physics may seem to have cut the ground from under this physical type of nominalist, in abandoning even Kelvin's billiard balls [...] [b]ut the nominalist is capable of surviving this [...] [T]he nominalist reserves the right to refurbish this conceptual scheme [...] and to produce a substitute conceptual scheme which, while still theoretically adequate to the physicist's purposes, will not countenance any entities beyond those whose existence it is within the *physicist's* professional competence to assert. (N2, 1946b, 17-8)

Clearly, Quine here has not yet fully rejected first philosophy. Even though our best scientific theories quantify over abstract objects, there are philosophical reasons for either dismissing entities beyond our primary sense experiences or for refurbishing the physicist's conceptual scheme in nominalistically acceptable terms.[41] This first-philosophical attitude is expressed

41 Of course, this is not to say that Quine here embraces a traditional metaphysical perspective on the question of nominalism. See section 3.4. Yet although

even clearer a few paragraphs earlier, where Quine responds to the objection that classical mathematics indispensably quantifies over abstract objects:

> Now surely classical mathematics is part of science; and I have said that universals have to be admitted as values of its variables; so it follows that the thesis of nominalism is false. What has the nominalist to say to this? He need not give up yet; not *if he loves his nominalism more than his mathematics*. He can make his adjustment by repudiating as *philosophically unsound* those parts of science which resist his tenets; and his position remains strong so long as he can persuade us that these rejected parts of science are neither intrinsically desirable as ends nor necessary as means to other parts which *are* intrinsically desirable. (ibid., 17, my emphasis)

When Quine later specifies that the 'intrinsically desirable end' of science is effective prediction, it becomes clear that his position here is still compatible with the first-philosophical instrumentalist's view that theoretical posits beyond those needed for effective prediction are merely useful fictions.[42]

Quine does not defend a nominalistic position in the traditional sense, he does believe that there might be philosophical reasons for attempting "to set up a nominalistic language in which all of natural science can be expressed" (Quine, 1939b, 708).

42 See also Burgess (2008, 63): "the 1946 lecture gives no real reason that I can see why a nominalist should not be satisfied with instrumentalism", even though Quine, unlike the instrumentalist, actively seeks a reformulation of our scientific theories such that they do not quantify over abstract objects. Burgess also notes that Quine's arguments in the lecture are still first-philosophical: "we find Quine light-years away from the principle professed later [...] that epistemology should be 'naturalized', with the philosopher becoming a citizen of the scientific community. Quine's epistemology at this stage is thoroughly 'alienated', with the philosopher remaining a foreigner, passing judgment from the outside on soundness of its work" (ibid., 61). See also Mancosu (2008, 52), who notes that Quine's anti-Platonism here originates at least

Even though Quine actively sought a nominalist interpretation of classical mathematics, he was never satisfied with the results of his endeavours.[43] No doubt, this growing pessimism about the possibility of fulfilling the project played an important role in Quine's rejection of nominalism in the years after the publication of "Steps toward a Constructive Nominalism".[44] A 1948 letter to J. H. Woodger, however, indicates that holism also played an important role in Quine's rejection of nominalism:

> A brief reflection now on ontology. I suppose the question what ontology to accept is in principle similar to the question what system of physics or biology to accept: it turns finally on the relative elegance and simplicity with which the theory serves to group and correlate our sense data [...] Now *the positing of abstract entities (as values of variables) is the same kind of thing*. As an adjunct to natural science, classical mathematics is probably unnecessary; still it is simpler and more convenient than any fragmentary substitute that could be given meaning in nominalistic terms. Hence the motive—and a good one—for positing abstract entities (which classical mathematics) needs [...] These very relativistic and tolerant remarks differ in tone from passages in my paper with Goodman and

partly from "metaphysical qualms". It should be noted, however, that Quine, when it comes to mathematics, never completely abandoned the somewhat instrumentalist considerations he appeals to in his 1946-lecture. Maddy (1997), for example, has criticized Quine's mature position on mathematics because his later arguments too go against the *communis opinio* among mathematicians. I will discuss this argument in chapter 6.

43 In his autobiography, Quine explains how he and Goodman failed to give a complete nominalist account of proof theory, which assumes "strings of [s]igns without limit of length, whereas our program could countenance them only insofar as physically realized" (TML, 1985, 198).

44 When exactly Quine completely dispensed with nominalism turns out to be quite difficult to determine. See Decock (2002c, §2.3).

> even in my last letter, I expect. My ontological attitude seems to be evolving rather rapidly at the moment.[45] (QWC*, 1938-1982, my emphasis)

Quine's reflections here indicate that his acceptance of a wide-scoped holism after his triangular correspondence with Goodman and White, provided him with an argument for allowing abstract objects. If we evaluate our logical and mathematical theories solely in terms of their contribution to our best scientific theories, dismissing any extra-scientific justification in terms of analyticity, then there is no reason not to treat physical and mathematical objects on a par. After all, both play a similar role in 'grouping and correlating our sense data'. While the early Quine was a realist about physical objects but did not yet want to fully commit himself to the abstract objects of mathematics for philosophical reasons, his acceptance of a wide-scoped holism in the late 1940s seems to have removed his reasons not to extend his realism to abstract entities. Indeed, from his 1948 "On What There Is" onwards, Quine treats physical and mathematical objects as epistemically on a par.[46]

4.11 FROM PHENOMENALISM TO REALISM

Although Quine by the late 1940s adopted a realist position about both physical and mathematical objects, thereby taking yet another step toward accepting a fully naturalistic perspective, his realism was still in some sense classified. While Quine endorsed the view that "our acceptance of an ontology is [...] similar in principle to our acceptance of a scientific theory", he still had not been able to decisively dismiss the distinctively epistemological point of view from which "the ontologies of physical objects and mathematical objects are myths" (OWTI,

45 See also Mancosu (2008, 43).

46 See (FLPV, 1953a, 173-4) and (TML, 1985, 198).

1948, 16-19). If we work from within a phenomenalistic conceptual scheme and only allow sense data, Quine argued, then we cannot maintain our realism about physical and abstract entities. Quine, in other words, had not yet incorporated epistemology into his wide-scoped holism, such that his realism about physical and mathematical objects would become truly 'unregenerate'. What he needed was an alternative epistemology in which the justification of science does not ultimately depend on its relation to a phenomenalistic conceptual scheme.

The first glimmerings of such an alternative appear in Quine's "Lectures on David Hume's Philosophy" (LDHP, 1946a).[47] In these lectures, Quine presents Hume as claiming that in epistemology it is "[v]ain to seek a rational foundation" and that "[u]ltimately we can only describe psychological behavior, not justify it" (LDHP, 1946a, 134). Hume's philosophy inspires Quine to distinguish between two epistemological projects, one that reflects the traditional phenomenalist view that we ought to justify science in terms of sense data and one that comes remarkably close to the naturalistic position he was to adopt in the 1950s (here still called 'pragmatism'):

> the Humean point of view [...] develops into two divergent modern trends:
>
> Constructive empiricism: explain all meaningful scientific discourse by contextual definition on the basis finally of reference to direct experience.
>
> Pragmatism: abandon such a project as impossible, and say that our discourse is merely variously conditioned by experience without being reducible to empirical terms. Abandon, therefore, empirical criticism of concepts; instead, judge any form of discourse in terms of its *utility*—this utility being measured within empirical science by ordinary empirical methods. (LDHP, 1946a, 135)

47 See also, Pakaluk (1989).

When Quine later came to endorse the view that his holism blocks constructive empiricism because it entails that a typical theoretical sentence has no distinctive empirical content of its own, it might have led him to reconsider the pragmatist option. Indeed, there is some evidence for this. For just before he published "Two Dogmas", Quine seems to adopt something like this pragmatist option in "Identity, Ostension, and Hypostasis" (IOH, 1950a). Perhaps it is not a coincidence that this is also the paper in which he for the first time publicly uses Neurath's boat metaphor, the analogy he would later often use to illustrate his naturalism:

> we must not leap to the fatalistic conclusion that we are stuck with the conceptual scheme that we grew up in. We can change it bit by bit, plank by plank, though meanwhile there is nothing to carry us along but the evolving conceptual scheme itself. The philosopher's task was well compared by Neurath to that of a mariner who must rebuild his ship on the open sea [...] Our standard for appraising basic changes of conceptual scheme must be, not a realistic standard of correspondence to reality, but a pragmatic standard.[48] (IOH, 1950a, 78-9)

By 1950, therefore, Quine had adopted something like Hume's pragmatic naturalist epistemology; he endorsed the view that

48 The fact that Quine often uses Neurath's metaphor to illustrate his naturalism, might suggest that Neurath's writings have played an important role in Quine's development. Quine himself, however, claims there was no such influence. In a letter (April 18, 1986) to Koppelberg on the latter's (1987) book on, among others, the close relation between his and Neurath's philosophy, for example, Quine writes: "my reading of my predecessors has been very sporadic and inadequate. I was aware superficially of my affinity with Neurath, as you know, and I am glad now to see the degree to it and the detail. I was not appreciably influenced by him at the time; I had to grow into the point of view on my own, away from Carnap." (QKC*, 1981-1994, my transcription).

we cannot validate science by translating its concepts into the epistemologically more basic terms of sense data. Still, as we have seen in section 2.4, even in "Two Dogmas" Quine talked about "sense data" in describing the evidential boundaries of his newly developed 'empiricism without the dogmas' (TDE, 1951b, 44), suggesting that he still had not conclusively dispensed with the instrumentalism that is compatible with such a phenomenalistic epistemology.[49] The very last stage in the development of Quine's naturalism, therefore, consists in his adoption of the view that sense data are not epistemologically prior after all. This breakthrough finds its origin in "On Mental Entities", a paper Quine presented in 1952. In this paper, Quine sets out to answer the question of "whether we should affirm or deny that there are such things as *sensations*, these being conceived as immediate, subjective experiences" (OME, 1952a, 221). Against the phenomenalist picture, Quine here for the first time argues that sense data are posits too:

> the notion of pure sense datum is a pretty tenuous abstraction, a good deal more conjectural than the notion of an external object, a table or a sheep. [...] Epistemologists have wanted to posit a realm of sense data, situated somehow just me-ward of the physical stimulus, for fear of circularity: to view the physical stimulation rather than the sense datum as the end point of scientific evidence would be to make physical science rest for its evidence on physical science. But if with Neurath we accept this circularity, simply recognizing that the science of science is a science, then we dispose of the epistemological motive for assuming a realm of sense data.[50] (ibid., 225-6)

49 See also, Murphey (2012, 88-9, 92-3). Murphey especially points to Quine's adoption of a physicalistic definition of observation sentences as his motivation for dispensing with phenomenalism.

50 See also (PTE*, 1952b), a lecture from October 7, 1952: "We would do well to recognize that in seeking to isolate sense data we are not plumbing the

Quine here for the first time adopts a thoroughly naturalistic point of view. Epistemology is not a distinctively philosophical project responsible for validating our scientific theories and for blocking an unregenerate realism about physical and mathematical objects. Rather, Quine now endorses the view that epistemology is itself a science, and therefore cannot be conducted from some transcendental science-independent perspective.[51] Quine was still a committed empiricist, but he now adopted the view that the idea that "any evidence for science has its end points in the senses [...] is an insight which comes after physics, physiology, and psychology, not before" (OME, 1952a, 225).

As I have argued in section 4.5, the crucial argument underlying Quine's realism is the idea we cannot ask about the 'reality' and 'truth' of our scientific posits and theories in a distinctively philosophical way without stripping those concepts of their intelligibility. Our notions of 'reality' and 'truth' themselves depend on our scientific conceptual scheme. Not surprisingly, this argument also first appears in "On Mental Entities". Phenomenalist epistemologists, Quine argues, regard the realm of sense data as somehow more "real" than the external objects that are posited in order to organize our experiences. Quine now believes, however, that "it is a mistake to seek an immediately evident reality, somehow more immediately evident than the realm of external objects":

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depths of reality; we are engaged rather in empirical psychology, associating physical stimuli with human responses. From the laws of this science the sense data as intermediary hypothetical entities may, however, be deleted; they will not be missed."

51 See also (PR, 1955, 252-3): "It is by thinking within [our] unitary conceptual scheme itself, thinking about the process of the physical world, that we come to appreciate that the world can be evidenced only through stimulation of our senses [...] Epistemology, on this view, is not logically prior somehow to common sense or to the refined common sense which is science; it is part rather of the overall scientific enterprise".

> Unbemused by philosophy, we would all go along with Dr. Johnson, whose toe was his touchstone of reality. Everything, of course, is real; but there are sheep and there are no unicorns [...] there are odd numbers and there are no even primes other than 2. Such is the ordinary usage of the word 'real' [...] Failing some aberrant definition which is certainly not before us, this is the only usage we have to go on.[52] (OME, 1952a, 225)

Quine, in other words, fully embraced a naturalistic point of view; he adopted a perspective immanent to our scientific conceptual scheme and principally rejected any transcendental perspective on reality. His wide-scoped holism now not only included logic and mathematics, but also our ideas about the sensory basis of science. Combined with his argument for an unregenerate realism, Quine had definitively boarded Neurath's boat.

4.12 CONCLUSION

In this chapter, I have made a first stab at answering the question how Quine arrived at his naturalism. I have argued that Quinean naturalism presupposes three commitments—empiricism, holism, and realism—and have pieced together the evolution of his position by examining the origins of these commitments in his work. Building on Quine's early writings as well as on the existing literature on his early views, I have argued that although Quine has from the early beginnings defended a behavioristic version of empiricism and a narrow-scoped holism, it was not until the late 1940s that he, probably influenced by Tarski and Goodman, realized that he could broaden his holism to include logic and mathematics.

52 See also (SLS, 1954b, 233): "the terms 'reality' and 'evidence' owe their intelligibility to their applications in archaic common sense".

Once Quine had adopted this wide-scoped holism, he gradually started grasping the radically naturalistic consequences of his position. First, Quine came to reject attempts to find an analyticity-based account of our logical and mathematical knowledge, defending the view that there is no need to justify this knowledge outside its contribution to our overall scientific theories. Secondly, Quine abandoned his attempts to seek a nominalistically acceptable interpretation of mathematics, a project that was at least partially guided by first-philosophical motives. Instead, he came to defend the view that on a rigorously holistic picture of inquiry there is *no* reason *not* to treat physical and mathematical objects as epistemically on a par. Thirdly, from 1952 onwards, Quine adopted the view that a similar move could be made with respect to his epistemology; there is no distinct epistemological point of view which deals with objects (sense data) that are in any sense more 'real' than the objects posited in the sciences. Rather, Quine adopted the view that sense data are theoretical posits, that there is no meaningful extra-scientific notion of 'reality', and that the very empiricism he had defended from the beginning of his career, itself could only be plausibly defended from within the framework of science.

Part II

NATURALISM AND HOLISM

5

VARIETIES OF HOLISM

Summary: Having shown how holistic presuppositions play a crucial role in Quine's naturalism, in this chapter I zoom in on the question of how we are to understand these presuppositions. A great variety of holisms have been ascribed to Quine, all of them to some extent controversial. In this chapter, however, I argue (1) that at the core of Quine's holism is a relatively innocent observation about the logic of theory testing and (2) that even Quine's ideas about the scope of holism are not as radical as they often appear. Furthermore, I reconstruct some developments in Quine's position in later stages of his career, showing how he slightly changed his views about the breadth of holism, the analytic-synthetic distinction, and the nature of logical truth and inference.[1]

5.1 INTRODUCTION: THE 'DISCOVERY' OF VULCAN

During the mid-1800s, inquiry into the motion of the heavenly bodies was considered to be almost complete. With Newton's law of universal gravitation at hand, astronomers were able to predict and explain the planetary movements more precisely than ever. According to Newton, every body exerts a force upon every other body with a magnitude that is propor-

1 Section 6 of this chapter is based partly on "Katz's Revisability Paradox Dissolved" (Tamminga and Verhaegh, 2013) that appeared in the *Australasian Journal of Philosophy* (volume 91, issue 4, pp. 771-784), and partly based on "Quine: Philosophy of Logic" (under review-c). I would like to thank Lieven Decock, Catarina Dutilh Novaes, and the members of the WiP-seminar for their comments.

tional to the product of their masses and inversely proportional to the square of the distance between them. Newton's law implies that celestial bodies do not move in perfectly elliptical orbits but are subject to perturbations due to their mutually distorting gravitational effects. Astronomers were able to precisely calculate the planetary positions by incorporating these effects.

Initially, Newton's elegant theory had its complications: Jupiter and Saturn, for example, did not exactly seem to follow their predicted orbits. Most of these problems, however, could be explained away. The problem with Jupiter and Saturn was solved by Pierre-Simon Laplace, who took into account the mutual perturbations of these planets over long periods of time. As a result of improvements like these, Newton's theory of gravity seemed invincible; phenomena that at one moment threatened the theory, on closer examination turned out to strongly confirm it.

The most important evidence for Newtonian mechanics, however, was the way in which it lead to the discovery of Neptune. In the early nineteenth century, the astronomical community was confronted with the problem that all tables predicting the motion of Uranus were accurate for only brief periods of time. It appeared to be impossible to devise a theory of Uranus' orbit that accounted for all observations that had been made up until then.[2] This anomaly was considered to be so problematic that scholars such as Friedrich Bessel even proposed to adapt Newton's theory by adjusting the well-confirmed inverse square law.[3] Most astronomers, however, hypothesized that there had to be a yet undiscovered mass, probably another planet, that caused Uranus to move in its eccentric way. Urbain Le Verrier and John Couch Adams, a French and a British mathematician,

2 The planet, first observed by William Herschel in 1781, had been known for only a few decades at the time.

3 Bessell suggested that gravitational effects depend on the chemical constitution of the planet in question.

took up the challenge and independently computed an approximate position for a hypothetical planet which could explain Uranus' strange orbit. In September 1846, Le Verrier sent his conclusions to the German astronomer Johann Galle and only five days later the hypothetical planet was found within 1° of the predicted position. Neptune had been discovered.[4] After the great successes of Laplace, Newtonian mechanics had triumphed again and Le Verrier became widely known as a genius, as the man "who had discovered a star with the tip of his pen, without other instrument than the strength of his calculations alone".[5]

Only a few relatively minor problems remained for the Grand Newtonian Theory. One of these was an anomalous advance in the perihelion of Mercury.[6] The prediction of the transit of Mercury in 1786 by the French astronomer Jérôme Lalande, for example, was fifty-three minutes off, something that was considered an embarrassment (Fontenrose, 1973, 145). And although this error was reduced to no more than sixteen seconds by Le Verrier in 1845, about a year before he discovered Neptune, he was so disappointed with this result that he stopped his investigations and turned to the problems with the orbit of Uranus.

After his mathematical discovery of Neptune, however, Le Verrier returned to the problem concerning Mercury with renewed confidence. The mathematician discovered that the mis-

4 The question of whether Adams is to be credited for the discovery alongside Le Verrier has been a matter of heated debate, often stirred up by strong nationalistic sentiments. Recent evidence suggests that Le Verrier's predictions were far more precise than Adams'. See Smith (1989) and Kollerstrom (2006).

5 Camille Flammarion, quoted in Baum and Sheehan (1997, 118).

6 The perihelion of a planet is the point in its elliptical orbit where it is nearest to the Sun. If Mercury and the Sun were the only bodies in our solar system, Mercury's orbit would be stationary and its perihelion would be a fixed point. Due to the influence of the other planets, however, Mercury's orbit precesses and has the form of a rosette, such that the perihelion can be seen as moving in a circle. The problem of the anomalous advance in the perihelion of Mercury is that Mercury's orbit precessed somewhat faster than predicted.

taken predictions of Mercury were caused by an anomalous advance in its perihelion and concluded that of the advance of 565 arcseconds per century only 526.7 arcseconds were caused by known planets. Again he inferred that there has to be some unidentified mass in the solar system. The obvious solution was to posit one or more yet to be discovered planets between the Sun and Mercury; bodies in this region would have the least effect on the orbits of the other planets, the motions of which were already perfectly in accord with Newton's theory.[7]

It was Edmond Lescarbault, an amateur astronomer, who was among the first to claim that he had observed one of these predicted objects transit the Sun. Le Verrier, after convincing himself of Lescarbault's capabilities as an astronomer, accepted the observation as legitimate, and named the planet 'Vulcan'.[8] For the second time in his prosperous career a planet predicted by Le Verrier's had been discovered. And again he was widely praised: "astronomers of all countries will unite in applauding this second triumphant conclusion to the theoretical inquiries of M. [Le Verrier]".[9] For a brief period, it was generally believed that the other planets needed to account for the anomalous advance in the perihelion of Mercury—the observed body was estimated to constitute only five percent of the needed mass—were soon to be discovered.

Instead of discovering more intra-Mercurial planets, however, the astronomical community began to doubt the existence of Vulcan. For one thing, the French astronomer Emmanuel Liais claimed that he had been observing the Sun at the exact

7 A large number of small planets constituting a ring of bodies was considered most plausible by Le Verrier; one big planet would probably have been too bright to account for the fact that it had not yet been observed. See Baum and Sheehan (1997, 137-9).

8 This name had been earlier proposed by the German astronomer Encke for the planet Neptune (Kollerstrom, 2009, 67) as well as by his French colleague Babinet as the name for an intra-Mercurial planet (Roseveare, 1982, 26-7).

9 *Monthly Notices of the Royal Astronomical Society*, quoted in Fontenrose (1973).

same time as Lescarbault in 1859 but that, despite using a better telescope, he had seen no unknown objects. A second reason was that on all four occasions on which Vulcan was predicted to transit the Sun in 1860, not one astronomer observed anything of significance. As a result of these events, the astronomical world became more and more divided. Some astronomers wholeheartedly kept defending the existence of Vulcan, while others remained highly critical. The first faction had its successes every once in a while. Examples are the supposed observation of Vulcan by the English amateur astronomer W. Lummis in 1862, and the simultaneous observation of an object by James Craig Watson and Lewis Swift—both acclaimed for their discoveries of several comets—during the Great Solar Eclipse of 1878. All these observations remained controversial however. In the end, most astronomers—including Le Verrier's successor Félix Tisserand—were convinced that the Mercury problem required another explanation, not in the least because the controversial objects often constituted not even one percent of the mass needed in the proposed Newtonian explanation.[10]

Many alternative explanations regarding the anomalous precession of Mercury's orbit were put forward. Simon Newcomb and Asaph Hall proposed to adjust the inverse square law whereas Hugo von Seeliger argued that the particles which cause zodiacal light together might constitute enough mass to explain the anomalous advance.[11] Although the latter hypothesis was accepted by most astronomers in 1906, it was Albert Einstein's theory of general relativity of 1915 that in the end provided the complete solution: the advance was a relativistic effect accentuated by Mercury's position close to the Sun.

10 For a more complete history of the increasing doubts about the Vulcan hypothesis, see Fontenrose (1973), Baum and Sheehan (1997, 145-223), and Linton (2004, 441-5).

11 For a discussion of these and other solutions, see Roseveare (1982).

I have discussed the above episode in celestial mechanics because this history is standardly taken to be a perfect illustration of evidential holism, the idea that hypotheses cannot be tested in isolation but only in conjunction with background theory.[12] Two similarly structured problems, the unexpected orbits of Uranus and Mercury, were solved in two radically different ways: one in which the main theory could be saved by giving up an auxiliary hypothesis (the number of planets) and one in which the main Newtonian theory had to be revised. Testing Newton's inverse square law, the episode teaches us, cannot be done without presupposing a wide range of background theories and assumptions: the existence of a certain number of planets, some detailed account of their masses and orbits, the adequacy of one's telescopes, the accuracy of one's methods of measuring angles, distances and time, the precision of one's observing skills, and the exactness of one's mathematical machinery.[13]

12 For some textbook discussions of evidential holism by means of this example, see Ray (1991, 90-8), Stanford (2009), and Massimi and Peacock (2015, 33-7). The popularity of the example seems to be mostly due to Lakatos (1970), who uses it to support the claim that even the most respected scientific theories may not be directly falsifiable in the Popperian sense. Quine uses the example, though in a slightly different context in (OAM, 1969c, 86): "There are two ways of rising to problems. Thus take the perturbations of Mercury. I suppose that before Einstein some astronomers pondered these with an eager curiosity, hoping that they might be a key to important traits of nature hitherto undetected, while other astronomers saw in them a vexatious anomaly and longed to see how to explain them away in terms of instrumental error".

13 See also Duhem (1914, 194), who has argued that comparing "calculated perturbations with the perturbations observed by means of the most precise instruments [...] will not only bear on this or that part of the Newtonian principle, but will involve all its parts at the same time; [...] the principles of dynamics [...] the propositions of optics, the statics of gases, and the theory of heat, which are necessary to justify the properties of telescopes in their construction, regulation, and correction". Duhem is generally viewed to be the first to have provided a systematic defense of evidential holism, whereas Quine is viewed to be the philosopher who has extended and popularized

Although evidential holism seems to be a quite straightforward idea, i.e. a relatively innocent observation about the logic of theory testing, the idea is controversial, especially in discussions about Quine's philosophy. The reason is that Quine is often read as arguing for an extremely radical version of holism, suggesting (1) that scope of holism should be extended to "the whole of science" (TDE, 1951b, 41), and (2) that confirmation and meaning are constituted holistically as well, ideas which are widely dismissed by philosophers of science and philosophers of language.[14]

In this chapter, I discuss several varieties of holism that have been attributed to Quine and argue that his views on the matter are less radical than is often suggested. I argue that Quine's holism is at its core a relatively innocent thesis about the logic of theory testing (sections 5.2-5.4), that Quine's ideas about the scope of holism are more nuanced than is often supposed (sections 5.5-5.6), and that Quine's supposedly holistic ideas about meaning are often misunderstood because of a failure to take into account his thoroughly naturalistic views about language (section 5.7).[15] In doing so, I provide an account of what Quine's holism precisely amounts to and show that the interplay between holism and naturalism in Quine's philosophy is more complex than we have thus far presupposed (section 5.8).

Apart from an examination of Quine's holism, this chapter also provides some further details about Quine's development.

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the view. It is because of this reason that evidential holism is also widely known as the Duhem-Quine thesis.

14 Quine is read as a confirmation holist by Esfeld (2001, ch. 2), Becker (2001), Colyvan (2001, §2.5), Parent (2008), and Eklund (2013), although most of them mistakenly use the terms 'Duhem-Quine thesis' and 'confirmation holism' interchangeably. Quine is read as a meaning holist by Okasha (2000), Cozzo (2002), De Rosa and Lepore (2004), Pagin (2006), and Jackman (2014). For some influential arguments against Quinean confirmation holism, see Maddy (1992), Sober (1999), and Achinstein (2001). The *loci classici* for arguments against meaning holism are Dummett (1973) and Fodor and Lepore (1992).

15 Quine's ideas about confirmation will be discussed in chapter 6.

Having discussed the early evolution of Quine's ever broadening holism in chapter 4, in this chapter I examine some changes in Quine's ideas about logic, analyticity, and theory testing in later stages of his career (sections 5.5-5.6). I show that although there are some significant changes in his views about the nature of logical truth and inference, they do not affect his general ideas about the ground of logical truth.

5.2 EVIDENTIAL HOLISM

Evidential holism is a thesis about the logical relation between theory and evidence; or, in more Quinean terms, about the relation between clusters of theoretical sentences and observation categoricals.[16] The logical relation between the two can be best described by what Morrison (2010) calls the prediction thesis and the falsification theses:[17]

(PT) *Prediction thesis*: one cannot deduce an observation categorical from a single hypothesis. Only clusters of theoretical sentences will imply observation categoricals.[18]

16 Observation categoricals are sentences of the form "Whenever P, Q", where P and Q are observation sentences such that the categorical expresses "the general expectation that whenever the one observation sentence holds, the other will be fulfilled as well". As examples of observation categoricals, Quine mentions 'When it snows, it's cold', 'Where there's smoke, there's fire', and 'When the sun rises, the birds sing' (FSS, 1995b, 25).

17 This dictinction is quite common in the literature, albeit under different names. See P. L. Quinn's (1974) distinction between a 'separability' and a 'falsifiability thesis' and Ariew's (1984) distinction between a 'non-separability' and a 'non-falsifiability thesis'.

18 See (WWI, 1986m, 168) and (FSS, 1995b, 45). There is one trivial exception to PT: if one combines all the theoretical sentences which together imply the observation categorical into one conjunction, this conjunction will imply the categorical by itself as well. See (FME, 1975a, 72) and (RJV, 1986g, 620).

(FT) *Falsification thesis*: whenever an observation categorical turns out to be false, one cannot logically determine which theoretical sentence is falsified. Rather, the cluster of theoretical sentences which implied the categorical is falsified as a whole.[19]

Applied to the example described above, PT states that Newton's inverse square law does not by itself imply anything about the orbits of Uranus and Mercury, whereas FT states that whenever one's predictions about these orbits turn out to be incorrect, one cannot logically determine whether one ought to revise Newton's law or an auxiliary hypothesis. As such, the two theses aptly explain how it is possible that two similarly structured problems, the unexpected orbits of Uranus and Mercury, were solved in two radically different ways.

How are PT and FT to be justified? For Quine, PT is simply an empirical fact, firmly supported by (1) scientific practice, as is evinced by our example of the rise and fall of Newton's inverse square law, and (2) by his account of language learning. As we have seen in section 2.7, Quine believes that to become a fully competent speaker of English one has to learn how to use one's observation sentences analytically. Now, according to Quine, this is a process that does not proceed by "continuous derivation", such that, if this process were "followed backward", it would "enable us to reduce scientific theory to sheer observation". Rather, it is a process that proceeds "by short leaps of analogy", such that a backward reduction is impossible in principle (NNK, 1975d, 267). The result is that non-holistic languages

19 See (PT, 1990g, 13-4): "the falsity of the observation categorical does not conclusively refute the hypothesis. What it refutes is the conjunction of sentences that was needed to imply the observation categorical. In order to retract that conjunction we do not have to retract the hypothesis in question; we could retract some other sentence of the conjunction instead".

are possible, but only on a holophrastic level, as Quine argues in a response to Nozick:

> [Nozick] asks whether a non-Duhemian language would be impossible for us. Let me say that the observation sentences, in my behaviorally defined sense, constitute already a rudimentary language of this kind [...] But I see no hope of a science comparable in power to our own that would not be subject to holism. (RRN, 1986i, 364)

The prediction thesis, in other words, is an empirical thesis; it is justified on the basis of observations of how scientists work and of how children learn their language.[20] It is probably because of this reason that even the strongest opponents to evidential holism admit that at least PT is true.[21]

5.3 FALSIFICATION AND SCIENTIFIC PRACTICE

Although there is widespread consensus about the justification of PT, there is no such agreement when FT is concerned. We can distinguish two types of argument against FT in the literature. The first argument points to scientific practice and claims that, in general, scientists *do* have reasons to discriminate between hypotheses which are and hypotheses which are not falsified

20 Cf. Gibson (1988, 32-42), who identifies a "scientific practices argument" and a "language learning argument" in Quine's work as well. Gibson also suggests a third argument for PT, an argument which is based on Quine's rejection of the analytic-synthetic distinction. I will come back to Quine's dismissal of the distinction in section 5.5.

21 See, for example, Sober (1999; 2000), who strongly criticizes FT but nevertheless maintains that "hypotheses rarely make observational predictions on their own; they require supplementation by auxiliary assumptions if they are to be tested. Schematically, it isn't the hypothesis H alone that predicts whether O will be true; rather, it is the conjunction H&A that has this implication" (Sober, 1999, 54).

whenever a prediction fails. According to Karl Popper, for example, the empirical successes of our theories determine which hypotheses ought be modified in the light of adverse experience:

> It seems to me quite clear that it is [...] through [the] temporary successes of our theories that we can be reasonably successful in attributing our refutations to definite portions of the theoretical maze. (For we are reasonably successful in this—a fact which must remain inexplicable for one who adopts Duhem's and Quine's views on the matter.) (Popper, 1963, 243-4)

In support of his claim, Popper contrasts two situations: one in which every prediction we test is almost immediately refuted and one in which most predictions turn out to be correct. The first situation, Popper argues, "would soon leave us bewildered and helpless" (ibid.). For if all our experiments were to have negative results, we would have absolutely no clue how to modify our theories. The circumstances are different in the second situation however. In this situation, which resembles everyday scientific practice, we *do* have a clue as to 'which portions of the theoretical maze' we should modify in response to failed predictions. Because there are so many successful predictions, large parts of our background knowledge have been corroborated extensively. If these parts of our system function as background in the testing of new and doubtful hypotheses, it is quite obvious that the latter should be modified when our predictions turn out to be false. If we have independent information about the reliability of our background beliefs, we do not have to worry about which hypothesis to falsify in the light of adverse experience.[22]

22 See also Glymour (1975, 403): "Scientists often claim that an experiment or observation tests certain hypotheses within a complex theory but not oth-

A similar objection is made by Sober, who argues that if FT were true, the distinction between hypotheses 'under test' and auxiliary hypotheses 'in use' would become irrelevant. For if we cannot determine which hypothesis to change when our observations conflict with our theory, there is no reason to distinguish between primary and auxiliary hypotheses in the first place:

> When scientists want to test one hypothesis against another, they don't simply invent auxiliary assumptions that permit the competing hypotheses to issue in predictions. Rather, they try to find auxiliary assumptions that they already have good reason to think are true. This means that the auxiliary assumptions used in a test and the hypotheses under test differ in their epistemological standing. The observational outcome favors one competing hypothesis over the others. But the test typically will not test the auxiliary assumptions at all [...] Typically, the auxiliary assumptions are epistemically independent of the test outcome. (Sober, 1999, 54)

To illustrate his point, Sober uses an example of a woman who thinks she might be pregnant and who uses a pregnancy test to check her 'hypothesis'. Although Sober concedes that, strictly speaking, a prediction about the outcome of the test can only be derived by presupposing a bunch of auxiliary hypotheses about the physical theory behind the test, he argues that the results of this test do not bear on these auxiliary hypotheses at all. Of

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ers. Relativity theorists, for example, are unanimous in the judgment that measurements of the gravitational red shift do not test the field equations of general relativity; psychoanalysts sometimes complain that experimental tests of Freudian theory are at best tests of rather peripheral hypotheses [and] astronomers do not regard observations of the positions of a single planet as a test of Kepler's third law [...] Observations are regarded as relevant to some hypotheses in a theory but not relevant to others in that same theory".

course no test is completely reliable; there will be some false positives and some false negatives. Still the woman's "degree of confidence in the reliability of the test procedure remains unchanged" whatever the outcome of the test in question (ibid., 55). In other words, while it could be reasonable for the woman to reject the test result by hypothesizing that it concerns a false positive or a false negative, it would be unreasonable for her to reject the result of the test by claiming that the degree of reliability of the test is wrongheaded: "it takes a very different experiment to assess the reliability of the test procedure" (ibid.).

From Quine's point of view, the above arguments rest on an erroneous presupposition. For they presuppose that FT implies that there is no reasonable way to decide which hypothesis to refute in the light of an adverse experience. This is a mistake, however, as Quine's point is only that there is no *logical* way to decide this. FT is a thesis about the *logic* of theory testing; it is perfectly compatible with the claim that there are other reasonable means for deciding between competing hypotheses.[23] In other words, Quine's claim is not that revising a well-confirmed background assumption is just as reasonable as revising the hypothesis under test. Rather, he merely makes the logical point that "we *do not have to* retract the hypothesis in question" and that we "*could* retract some other sentence of the conjunction instead" (PT, 1990g, 14, my emphasis). In fact, Quine has often attempted to list the 'pragmatic maxims' that describe our revision norms beyond the norms of logic.[24]

23 The same holds for Duhem (1914, 216-7): "Pure logic is not the only rule for our judgments; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices [...] constitute what is appropriately called good sense".

24 See (PR, 1955, 247), (WO, 1960b, §5), (WB, 1970, ch. 6), (TI, 1990h, 11), and (FSS, 1995b, 49). Although these writings contain different lists of pragmatic maxims, two of them appear on all these lists: "the maximization of simplicity and the minimization of mutilation" (TI, 1990h, 11). On several occasions, Quine

If we apply Quine's reply to Sober's example of the pregnancy test, we see that, although it would be *unreasonable* for the woman to change her belief in the reliability of the pregnancy test, this does not mean that *logically*, modifying this belief is not always an option.[25] Sober's distinction between hypotheses in use and hypotheses under test is therefore not a logical one. Rather, which hypotheses we regard to be under test reflects our judgment about which revision would make good sense in the light of adverse experience: "The scientist thinks of his experiment as a test specifically of his new hypothesis, but only because this was the sentence he was wondering about and is prepared to reject" (PT, 1990g, 14).

5.4 GRÜNBAUM'S ARGUMENT

Quine's falsification thesis thus turns out to be a straightforward, almost trivial, view about the *logic* of theory testing, a thesis which even Popper, Glymour, and Sober would be happy to accept provided that it is combined with the view that there are reasonable ways to decide between competing hypotheses beyond logic.

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has attempted to explicate these maxims. See, for example, (STCW, 1960a). In the end, however, he seems to have concluded that it is impossible to provide these norms with a formal explication: "No general calibration of either conservatism or simplicity is known, much less any comparative scale of the one against the other. For this reason alone—and it is not alone—there is no hope of a mechanical procedure for optimum hypothesizing" (FSS, 1995b, 49).

25 In fact, one could even imagine that there are circumstances in which it *would* be reasonable for the woman to change her beliefs about the reliability of the test. For instance, when the woman believes it to be extremely unlikely that she is pregnant and when she, just before taking the test, learns that many women have complained about the unusually high number of false positives. If the test then tells her that she is pregnant, it will be reasonable for her to slightly change her degree of belief in the reliability of the test, even if it remains an option that her test result is just a 'regular' false positive.

The triviality of FT becomes even clearer when we turn to a second argument that has been offered against it, an argument constructed by Adolf Grünbaum (1962). Suppose a physicist deduces an observation categorical O from a conjunction of theoretical sentences H&A and that she subsequently observes not-O. As we have seen, FT implies that the physicist cannot logically determine whether she has to revise H or one of the auxiliary hypotheses in A in resolving this conflict between theory and evidence. On Grünbaum's account, however, this diagnosis need not be correct. For Grünbaum argues, it would be correct only if there exists (1) an alternative H′ which together with A implies not-O, and (2) an alternative set of auxiliary hypotheses A′ which together with H implies not-O. In other words, FT's diagnosis is only correct when

(a) $(\exists A')((H\&A') \rightarrow \text{not-O})$, and
(b) $(\exists H')((H'\&A) \rightarrow \text{not-O})$

are true. To see this, suppose that (a) is false, e.g. because it turns out to be impossible to adapt the auxiliary hypotheses in such a way that the revised theory, combined with H, correctly predicts not-O. The physicist, in such a scenario, would have only one revision option left, namely H, and FT would turn out to be incorrect. After all, the physicist in this scenario, *pace* FT, *can* logically determine which hypothesis to revise in the light of her failed prediction.[26]

26 For a more concrete example of a scenario in which FT fails, see Grünbaum (1962, 23-33). For responses to this particular example, see Laudan (1965) and Giannoni (1967, 170-2). In my opinion, Grünbaum ignores the possibility that the physicist in the above example would, in theory, also have the option to dismiss not-O or even to revise the logic which led her to derive O from H and A. I ignore these difficulties with Grünbaum's argument in what follows, as it is Quine's response to the argument that I am primarily interested in here. The possibility of revising one's logic in the light of an adverse experience will be discussed in section 5.6.

Now, according to Grünbaum, Quine can only save FT by defending the claim that (a) and (b) can be made true by changing the meanings of the terms in H, H′, A or A′. Suppose for instance that H is the hypothesis that 'Ordinary buttermilk is highly toxic to humans' and that not-O is the observation that buttermilk is actually quite wholesome. According to Grünbaum, Quine could technically save FT by arguing that a scientist always has the option to change the rules of English. In this example, for instance, the scientist has the option to square H with not-O by changing "the intension of the term 'ordinary buttermilk' [to] that of the term 'arsenic' in its customary usage". The problem with this strategy, however, is that FT would then turn out to be "a thoroughly unenlightening truism" (Grünbaum, 1962, 20).[27] As a result, Grünbaum's argument implies that FT is either false (when changes of meaning are not allowed) or trivially true (when changes of meaning *are* allowed).

Quine has responded to Grünbaum's dilemma in a 1962-letter. He wholeheartedly chooses the latter option, admitting that FT is trivially true, and explains that he never intended to propose FT as a substantive thesis in the first place:[28]

> Your claim that the Duhem-Quine thesis, as you call it, is untenable if taken nontrivially, strikes me as persuasive. Certainly it is carefully argued. For my own part I would say that the thesis as I have used it *is* probably trivial. I haven't advanced it as an interesting thesis as such [...] I am not concerned even to avoid the trivial extreme of sustaining a law by changing a meaning; for the cleav-

27 Cf. Fodor and Lepore (1992, 47): "It's only epistemologically interesting that you could hold onto 'Burning is the liberation of phlogiston' in the face of Lavoisier's results if 'Burning is the liberation of philogiston' means that the burning is the liberation of phlogiston. It's no news that you could hold onto it in the face of those results if it means that Greycat has whiskers".

28 See also Gibson (1987, 67-8) and Becker (2001, 78-9). It should be noted that in (PT, 1990g, 16), Quine gives a slightly different response.

> age between meaning and fact is part of what, in such contexts, I am questioning. (CGC, 1962, 132)

Quine, in other words, admits that FT is trivial; there simply is no strict distinction between changes of fact and changes of meaning.[29] If Grünbaum is right in claiming that admitting semantic revisions trivializes FT, then Quine is happy to accept that FT is trivial.[30]

5.5 THE WHOLE OF SCIENCE?

So far, I have limited my discussion of evidential holism to the question of whether PT and FT are justified. I have argued that PT can be thought of as a simple empirical fact, firmly supported by observations about scientific practice and language

29 Does this imply that Quine also admits an extreme revision like the above described buttermilk-example as a live possibility? No it does not, because like other radical revisions—e.g. revisions of logical truths—such radical changes of meaning are constrained by Quine's maxim of minimum mutilation. Cf. Becker (2001, 79-80): "The expression 'buttermilk' has many inferential and confirmational relations, say, 'Buttermilk comes from cows', 'Buttermilk makes good pancakes', 'Buttermilk is fattening' [...] etc. Changing the meaning of 'buttermilk' to 'arsenic' would sever all of these inferential relations, creating incoherence among one's beliefs, and with no apparent motivation".

30 One could question whether Quine already thought of FT as a trivial thesis when he, in "Two Dogmas", claimed that "[a]ny statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" (TDE, 1951b, 43). I believe he did. For in the paper, Quine never mentions the requirement that a language should stay "semantically stable" (Grünbaum, 1962, 20) when we are making 'drastic adjustments in the system'. Furthermore, in the introduction to the first edition of *Methods of Logic*, Quine already considers the possibility that some revisions can better be thought of as "the adoption of a new conceptual scheme, the imposition of new meanings on old words" (ML1, 1950c, xiv). I thank Gary Ebbs for both suggesting this problem and its solution to me.

learning, and that FT is an almost trivial thesis about the logic of theory testing, a thesis which simply follows from PT.[31]

The controversy surrounding Quine's evidential holism is not only invoked by its justificatory status however. A significant part of the philosophical dispute concerns the *scope* of Quine's thesis. Quine's version of evidential holism is controversial precisely because he seems to extend its scope to the whole of science. In "Two Dogmas", for instance, Quine talks about "*total science*" being like "a field of force whose boundary conditions are experience"; about "*the whole of science*" as "the unit of empirical significance"; and about "*no statement*" being "immune to revision" (TDE, 1951b, 42-3, my emphasis).[32] Quine, in other words, seems to argue that only science as a whole can imply an observation categorical, and that, as a result, only science as a whole is falsified in the light of a failed prediction. This appears to have some implausible consequences. It implies, for example, that when 19th-century astronomers were considering solutions to the problem with Mercury's orbit, they could also have blamed their ideas about chemistry or molecular biology.[33]

Does Quine really believe that the scope of evidential holism should be extended to "[t]he totality of our so-called knowledge or beliefs" (TDE, 1951b, 42)? In order to answer this question unambiguously we must distinguish between two ways in which holism might be extended to the whole of science, i.e. between two senses of 'scope':

31 Indeed, FT is a logical consequence of PT: if only a cluster of hypotheses H&A implies the observation categorical O, then one cannot conclude not-H from not-O; by modus tollens, only not-(H&A) follows from the failed prediction.

32 In contrast to Duhem, who is generally read as restricting the scope of evidential holism to physics.

33 For a similar worry, see Morrison (2010, 341).

(Scope$_1$) The size of the set of theoretical sentences that is required in the deduction of *every* observation categorical.

In my definition of PT and FT above, I have talked about 'clusters of theoretical sentences' that imply observation categoricals and about 'clusters of theoretical sentences' which are falsified as a whole in the light of a failed prediction. The first question about the scope of Quine's thesis, is the question of the size of these clusters, the question of how much background theory we presuppose whenever we test a hypothesis.[34]

The second question about the scope of Quine's thesis, on the other hand, is weaker and asks about the number of theoretical sentences which are revisable *in principle*:

(Scope$_2$) The size of the set of theoretical sentences that is required in the deduction of *at least one* observation categorical.

In order to grasp the distinction between scope$_1$ and scope$_2$, note that the scope$_1$-set of theoretical sentences is a subset of the scope$_2$-set of theoretical sentences. After all, if a theoretical sentence is required in the deduction of all observation categoricals (scope$_1$), it will by definition play a role in the deduction of at least one observation categorical (scope$_2$). The converse is not the case as there might be theoretical sentences that play a role in the deduction of only a few but not all observation categoricals. Such theoretical sentences are in principle vulnerable to revision, but not *every* time a random categorical proves mistaken.[35]

34 Quine sometimes uses the term 'breadth' in discussing this question: "Another reservation regarding the Duhem thesis has to do with breadth. If it is only jointly as a theory that the scientific statements imply their observable consequences, how inclusive does that theory have to be?" (EESW, 1975c, 229)

35 One might even distinguish a third type of scope when one considers Quine's remark that "[s]cience is neither discontinuous nor monolithic [but] variously

Now, "Two Dogmas" is often read as an argument for the claim that the scope$_1$ of holism is our complete set of theoretical sentences. This reading seems to be fair as Quine, in the paper, explicitly claims that "the unit of empirical significance is the whole of science" (TDE, 1991b, 42), thereby suggesting, as he later seems to acknowledge, that "our scientific system of the world [is] involved *en bloc* in *every* prediction" (FME, 1975a, 71, my emphasis).

In looking back on "Two Dogmas", however, Quine has argued that he regrets this claim and proposes to restrict the scope$_1$ of his holism:

> Looking back on ["Two Dogmas"], one thing I regret is my needlessly strong statement of holism [...] In later writings I have invoked not the whole of science but chunks of it, clusters of sentences just inclusive enough to have critical semantic mass.[36] By this I mean a cluster sufficient to imply an observable effect of an observable experimental condition. (TDR, 1991b, 393)

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jointed, and loose in the joints in varying degrees" (EESW, 1975c, 230). Quine here is not talking about either the scope$_1$ or the scope$_2$-sense of holism. For suppose that the scope$_2$ of holism is the whole of science but that the scope$_1$ of holism is only moderate, consisting of relatively small clusters of theoretical sentences (this is Quine's position as I will show in the remainder of this chapter). Then the question arises how much overlap there exists between these different middle-sized clusters. If science is 'monolithic', then every theoretical sentence will share at least one cluster with every other theoretical sentence, whereas if science is 'discontinuous', then there will be at least two clusters A and B such that sentences in A will never form a cluster with sentences in B and vice versa. In this chapter, I will ignore this third sense of scope. In chapter 6, however, I will take up this topic, albeit in a less abstract form.

36 Indeed from *Word and Object* onwards Quine has always emphasized that "some middle-sized scrap of theory usually will embody all the connections that are likely to affect our adjudications of a given sentence" (WO, 1960b, 13). See also (CGC, 1962, 132), (EESW, 1975c, 229-30), and (RHP, 1986f, 427).

In his work after "Two Dogmas", in other words, Quine softens his tone and restricts the scope$_1$ of his holism to middle-sized clusters, dismissing the view that every theoretical sentence is vulnerable to revision when a prediction turns out to be false.[37]

Quine's switch to a more moderate variant of holism in restricting the scope$_1$ of his thesis has lead some commentators to claim that he fundamentally changed his views.[38] I believe that this is a mistake, however, as Quine's holism in the scope$_1$-sense does not do any epistemological work in his philosophy. Rather, it is Quine's universal revisability thesis, i.e. his strong holism in the 'scope$_2$-sense', that is epistemologically significant.

To see this, reconsider the two dogmas Quine wants to dismiss in "Two Dogmas of Empiricism". The first dogma Quine aims to reject is the analytic-synthetic distinction. In section 6 of his paper, Quine argues for the conditional claim that *if* his holistic picture of inquiry is correct, "it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements which hold come what may". This is folly, according to Quine, because on the holistic picture "[a]ny statement can be held true come what may" and, conversely, "no statement is immune to revision" (ibid., 43). Now, for Quine's argument here to be valid, he does not require a strong holism in the scope$_1$-sense; a moderate holism is sufficient for Quine to establish that one can hold any

37 It should be noted, however, that Quine still maintains that *legalistically speaking*, the unit of empirical significance *is* the whole of science. See (FME, 1975a, 71), (RJV, 1986g, 619), and (TDR, 1991b, 393). What Quine means by this is that the scope$_1$ of holism is the whole of science in the sense we have discussed in section 5.4, i.e. that we *can* revise any statement in the light of adverse experience if we allow changes of meaning.

38 See Fogelin (1997, 550), who suggests that Quine's shift constitutes a "fundamental change in position". According to Fogelin, there is a tension between naturalism and holism in Quine's philosophy, and the shift reflects a "growing commitment to a naturalistic standpoint" (Fogelin, 2004, 32). The supposed tension between holism and naturalism is discussed in chapter 6.

statement true come what may. After all, if a middle-sized cluster of theoretical sentences is always involved in the derivation of an observation categorical, one has the logical freedom to decide never to revise a particular hypothesis. Quine *does* need a strong holism in the scope$_2$-sense, however, if he wants to maintain that 'no statement is immune revision'. For if the scope of Quine's holism would be moderate in this sense, he could not maintain that any statement will be revisable in principle.[39]

A different way to make the same point is to ask why the analytic-synthetic distinction was so important to Carnap and the logical positivists. As Quine views the matter, Carnap required the analytic-synthetic distinction in order to be able to solve an age-old problem for empiricism, namely the problem of how to explain our logical and mathematical knowledge. In arguing that our logical laws and mathematical statements are analytic, the positivist could maintain that these statements are meaningful while remaining faithful to the empiricists' core idea that all our knowledge about the world originates in sense experience. After all, if logical and mathematical statements are true in virtue of meaning, then they do not claim anything about the world.[40]

Now, according to Quine, Carnap's need for an analytic-synthetic distinction is caused by the second dogma.[41] After all, if

39 The identification between strong holism in the scope$_2$-sense and universal revisability is not strict; there might be revisable hypotheses that are not directly required in the deduction of at least one observation categorical. For Quine in his later work maintains that there might be acceptable scientific hypotheses which cannot directly "[join] forces with other plausible hypotheses to form a testable set". These hypotheses "may be prompted by symmetries and analogies, or as welcome unifying links in the structure of the theory" (FSS, 1995b, 49). In the remainder of this chapter, I will ignore this caveat.

40 See (QSM, 1988, 26): "How, Carnap asked, can mathematics be meaningful despite lacking empirical context? His answer was that mathematics is analytic".

41 In "Afterthoughts", Quine calls the second dogma "the real villain of the piece" (AT*, 1987a, 10, my transcription).

all synthetic statements are to be tested in isolation, there will be statements, i.e. our mathematical and logical statements, that are never tested and hence statements which are meaningless from a strictly verificationist point of view. Yet when we give up on the second dogma—and note that it is enough here to combine a moderate holism in the scope$_1$-sense with a strong holism in the scope$_2$-sense—we can explain why our logical and mathematical statements are meaningful without appealing to the notion of analyticity; they are meaningful because they play an important role in squaring theory with evidence.[42]

5.6 UNIVERSAL REVISABILITY

Quine, in other words, is correct when he classifies his early holism as "needlessly strong" (TDR, 1991b, 393). All he requires in dismissing the dogmas of empiricism are a moderate holism in the scope$_1$-sense and a strong holism in the scope$_2$-sense. In the previous section, I have shown that Quine defends a moderate holism in the scope$_1$-sense. In this section, I argue that he also subscribes to the second thesis, i.e. that he allows no exception to his claim that in principle "no statement is immune to revision" (TDE, 1951b, 43).

At first glance, Quine *does* seem to allow exceptions to universal revisability. One class of sentences that seems immune to revision in Quine's work, for instance, is the class of logical

42 See (RGH, 1986d, 206-7): "the second dogma creates a need for analyticity as a key notion of epistemology, and [...] the need lapses when we heed Duhem and set the second dogma aside. For given the second dogma, analyticity is needed to account for the meaningfulness of logical and mathematical truths, which are clearly devoid of empirical content. But when we drop the second dogma and see logic and mathematics rather as meshing with physics and other sciences for the joint implication of observable consequence, the question of limiting empirical content to some sentences at the expense of others no longer arises". See also (QSM, 1988, 27): "Once we appreciate holism, *even moderate holism*, the notion of analyticity ceases to be vital to epistemology".

truths.[43] In *Pursuit of Truth*, for example, Quine explicitly claims that logical truths are exempted from revision:

> Over-logicizing, we may picture the accommodation of a failed observation categorical as follows. We have before us some set S of purported truths that was found jointly to imply the false categorical. Implication may be taken here simply as deducibility by the logic of truth functions, quantification, and identity [...] Now some one or more of the sentences in S are going to have to be rescinded. *We exempt some members of S from this threat on determining that the fateful implication still holds without their help.* Any purely logical truth is thus exempted, since it adds nothing to what S would logically imply anyway. (PT, 1990g, 14, my emphasis)

Quine, in other words, argues that logical truths are not completely on a par with ordinary scientific statements. Even if we were to decide to remove a logical truth ϕ from our belief set in the light of an unexpected experimental result, this would be a pointless manoeuvre. For ϕ would simply pop up again as it is directly implied by the underlying 'logic of truth functions, quantification, and identity'. No matter how often one decides to rescind ϕ, one will never get rid of it because it follows from the empty set.[44]

43 Another possible exception to universal revisability is the class of observation sentences. I have already dismissed this option in the sections 2.6 and 2.7, however, in arguing that although *holophrastic* observation sentences are individually tied to observation (and thereby unrevisable), any competent speaker of a language will understand her observation sentences *analytically*. See also (RES, 1999a, 262-3), where Quine argues that although he has often "set this detail aside and proceeded much as if they were uniformly infallible", he *does* maintain that "observation sentences [...] are themselves irreducibly theoretic to various degrees".

44 See also (TR, 1994e, 431): "Even mathematical truths share [...] in the empirical meaning of sciences where they are applied [...] This cannot be said

Quine's view on the revision of logical truths here does not necessarily imply that logic becomes *absolutely* irrevocable however. For it might still be possible to revise the underlying logic which causes ϕ to pop up every time; if ϕ automatically follows from one's underlying logic L, then it might still be possible to revise ϕ via a revision of L, even though it is impossible to revise ϕ directly.[45]

Nothing in the above quoted passage thus directly implies that, when it comes to universal revisability, Quine makes a substantive exception for logic; indirect revisions of logic remain a possibility. Still the question whether or not Quine allows such indirect modifications has been a matter of some debate. For Quine sometimes seems to suggest that some logical principles cannot be revised even in this indirect way; for example when he claims that the idea of someone accepting a sentence of the form 'p and not p' is "meaningless" (CLT, 1954a, 102), or when he claims that logic is *analytic* in that a deviant logician who "tries to deny ['p and not p']", only "changes the subject" (PL, 1970c, 81).[46]

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of logical truths. Any sentence already implies any logical truth, and thus gains no further implying power by being conjoined with it". Note that this constitutes a modification of Quine's early 1950s view, when he still held that the logical truths which express our logical laws are to be viewed as statements in the web, statements themselves open to revision when push comes to shove. See (ML1, 1950c, xiv) and (TDE, 1951b, 42). The later Quine does not view logical laws as "simply certain further statements of the system" (ibid.); logic *does* have "a special status" in our theory of the world, viz. providing "the link between theory and experiment" (TDR, 1991b, 394). This makes sense as logical laws cannot be merely logical truths, as Quine himself had already argued in (TC, 1936, §4), using Carroll's (1895) parable. For a more detailed account of Quine's development on these issues, see Creath (2004, §3) and Tamminga and Verhaegh (2013, §4).

45 One could, for instance, modify L's underlying consequence relation. See Tamminga and Verhaegh (2013).

46 See also (TDR, 1991b, 396): "Anyone who goes counter to *modus ponens*, or who affirms a conjunction and denies one of its components, is simply flouting what he learned in learning to use 'if' and 'and'".

It is sometimes thought that these Quinean ideas about logic are a consequence of his late view that it *is* possible—*pace* "Two Dogmas"—to defend a limited notion of analyticity. For Quine, from *The Roots of Reference* (1973) onwards, has argued that some sentences *can* be called analytic: if every member of a language community learns that a certain sentence is true by learning how to use one or more of its component terms, then there are no obstacles toward counting the sentence as true in virtue of meaning.[47] Central to Quine's early holism was the idea that we cannot strictly distinguish between changes of theory and changes of meaning.[48] Quine's renewed ideas about analyticity, however, partly go against this view in acknowledging that a change of logic *can* be a pure change of meaning. For if '$p \wedge q \models p$' is analytic in Quine's sense, then to deny this logical law is simply to change the meaning of 'and', not to propose a change of theory.

In response to these new Quinean ideas about analyticity, scholars have distinguished between what Arnold and Shapiro (2007) call a 'logic-friendly' and a 'radical' Quine:

> It is sometimes said that there are two, competing versions of [...] Quine's unrelenting empiricism [...] [The] *logic-friendly* Quine holds that logical truths and, presumably, logical inferences are analytic in the traditional sense: they are true solely in virtue of the meaning of the logical terminology. Consequently, logical truths are knowable a priori, and, importantly, they are incorrigible, and so immune from revision. No amount of empirical data can get

47 See (RR, 1973, §21). Quine's views here are partly influenced by Putnam's (1962) ideas about one-criterion words. Quine credits Putnam in (WO, 1960b, §12) and (RHP, 1986f, 427).

48 Recall Quine's letter to Grünbaum: "I am not concerned even to avoid the trivial extreme of sustaining a law by changing a meaning; for the cleavage between meaning and fact is part of what, in such contexts, I am questioning" (CGC, 1962, 132).

> us to revise them. [...] The other, *radical* version of Quine does not exempt logic from the attack on analyticity and a priority. Logical truths and inferences are themselves part of the web of belief, and the same methodology applies to logic as to any other part of the web [...] Everything, including logic, is up for grabs in our struggle for holistic confirmation.[49] (Arnold and Shapiro, 2007, 276-7)

There has been considerable controversy about the extent to which the logic-friendly and the radical Quine are compatible, and if not, which one of these characters best approaches the 'real Quine'.[50] With one exception, however, all scholars have overlooked the fact that Quine has answered this question explicitly on two occasions.[51] According to Quine, the two perspectives on logic are perfectly compatible because his renewed talk about analyticity does not have any significant epistemological consequences. Even if changes of logic are now viewed as changes of meaning, the fact that logical laws are analytic does not do any work in Quine's views about the epistemological status of logic. If we are unwilling to treat a logical revision as a change of theory, this only reflects how deeply embedded the laws of logic are in our system of beliefs; the maxim of minimum mutilation still suffices to account for the nature of logical truth. Logical positivists required a notion of analyticity to account for the meaningfulness of logic, a requirement that was necessitated by their idea that each empirically significant

49 See also Haack (1977), who distinguishes between a 'conservative' and a 'radical' Quine; and Parent (2008), who contrasts a 'Principle of Logical Charity' with a 'Revisability Doctrine'.

50 See Putnam (1976, 92n1), Haack (1977), Kitcher (1984), Berger (1990), Shapiro (2000, 334), Burgess (2004), Maddy (2005, 443), Weir (2005, 463), Arnold and Shapiro (2007), Parent (2008), and Tamminga and Verhaegh (2013).

51 See (RS1, 1968d) and (CB, 1990a). The exception I am referring to is Parent (2008). Still even he takes into account only one of these two papers.

sentence has a distinct empirical content.[52] But once we have adopted evidential holism and have dropped the positivist's dogma, we do not require the notion of analyticity to do any explanatory work. The ground of logical truth still lies in its "meshing with physics and the other sciences for the joint implication of observable consequences" (RGH, 1986d, 207).

Quine's point here can be illustrated using his ideas about translation. Consider a lexicographer who aims to translate a native tribe's language into English; and suppose that all members of the tribe are inclined to assent to 'q ka bu q', which seems to mean 'p and not p' if the linguist would follow the translation manual she has drawn up thus far. Now, in response to this situation, the linguist has at least two options. She can either stick to her earlier conclusions and interpret the natives as accepting contradictions; or she might take the natives' utterances as evidence that her existing translation manual cannot be correct. Now, according to Quine, it would be absurd to choose the former option:

> if any evidence can count against a lexicographer's adoption of 'and' and 'not' as translations of 'ka' and 'bu', certainly the natives' acceptance of 'q ka bu q' as true counts overwhelmingly. We are left with the meaninglessness of the doctrine of there being pre-logical peoples; pre-logicality is a trait injected by bad translators. (CLT, 1954a, 190)

If Quine is right, then we cannot but conclude that the natives agree with us when it comes to logic. The very idea of an empirical observation that would justify the lexicographer in ascribing to them an alternative framework is ruled out from the beginning. Yet (and here lies the explanation of why Quine deems

52 At least they did before Carnap adopted a narrow-scoped holism in his (1934). See chapter 4, footnote 29. I thank Hanjo Glock for this qualification.

his renewed account of analyticity to be epistemologically irrelevant) the fact that we choose to interpret the natives as agreeing with us when it comes to logic, does not tell us anything about the *ground* of logical truth. We only interpret the native in terms of our logic because it is a basic *pragmatic* rule to interpret one another as charitably as possible:

> What is interesting to ponder is the connection between this rigidity of logic in translation and the question of the immunity of logic to revision [...] generally, we are well advised in translation to choose among our indeterminates in such a way, when we can, that sentences which natives assent to as a matter of course become translated into English sentences that likewise go without saying. This policy is regularly reflected in domestic communication: when our compatriot denies something that would seem to go without saying, we are apt to decide that his idiolect of English deviates on some word [...] We see, then, how it is that 'Save logical truth' is both a convention and a wise one. And we see also that it gives logical truths no epistemological status distinct from that of any obvious truths of a so-called factual kind.[53] (RS1, 1968d, 317-8)

Confronted with the objection that his ideas about the relation between logic and translation appear to be in conflict with universal revisability (and hence a strong holism in the scope$_2$-sense), therefore, Quine responds by showing that the two the-

53 See also (CB, 1990a, 36): "Is change of logic a change of language, or is it a change of substantive theory on a par with changes in physics [...] I have seemed to oscillate between those positions. But are they really two positions? If someone persists in a simple logical falsehood, we do indeed conclude that he has mislearned our language or is tampering with it. But this is equally the way with any obvious falsehood, logical or not. [...] But its being verbal in that sense conflicts none with my claim [...] that logic is integral to our system of the world and accessible to change in the same way as the rest".

ses can be easily combined. His views concerning the interpretation of deviant logicians (both domestic and abroad) are not intended to express anything fundamental about the epistemological status of logic. Rather, Quine only intends to show that translation practices are constrained by the principle of charity, a principle closely related to the maxim of minimum mutilation in theory revision; in updating our theories and in making sense of one another we are inclined to 'save the obvious', nothing more, nothing less. Obviousness is not a trait that is exclusive to logic, nor does the fact that a truth is obvious imply that it cannot be revised: "Obviousness resists change but does not preclude it" (CB, 1990a, 36).[54]

5.7 MEANING HOLISM

Let me briefly recap the main conclusions of our discussion thus far. I have argued that Quine's evidential holism is a thesis about the relation between theory and evidence as described by PT and FT, both relatively uncontroversial views about the *logic* of theory testing. More controversial are Quine's ideas about the *scope* of evidential holism. Still Quine has softened his tone on this issue by adopting a more moderate holism, by slightly changing his views about the nature of logic, and by admitting a limited notion of analyticity. These modifications, however, have not affected his ideas about the ground of logical truth such that his holism still implies that all statements, even our

54 In response to the above sketched dilemma of Arnold and Shapiro (2007), we can thus say that the 'logic-friendly Quine' and the 'radical Quine' are perfectly compatible. Their description of these 'two Quines' ought to be adapted however. For although the logic-friendly Quine argues that logic is analytic, he does not believe that it is "analytic in the traditional sense", nor does he believe that they "are knowable a priori" or that they are "incorrigible, and so immune from revisions". Conversely, the radical version of Quine *does* "exempt logic from the attack on analyticity".

logical truths, are ultimately a posteriori and revisable in principle.

In the above sections, I have often referenced Quine's dismissal of the distinction between changes of fact and changes of meaning. Both Quine's rejection of the analytic-synthetic distinction and his response to Grünbaum show that he is blurring "the cleavage between meaning and fact" (CGC, 1962, 132).[55] This invites the question of whether Quine's evidential holism can be extended to a holism about meaning as well, i.e. whether Quine's rejection of the distinction between changes of fact and changes of meaning combined with his evidential holism implies that he also subscribes to

(MH) *Meaning holism*: The meaning of the sentences of a theory θ are established together by the totality of relations between the sentences of θ as well as the relation between θ and the sensory evidence on which θ is based.

Prima facie, Quine seems to subscribe to something like MH. In *Word and Object*, for instance, he argues that "there is in general no sense in equating a sentence of a theory θ with a sentence S given apart from θ" because if S is not directly linked to sense experience, "S is meaningless except relative to its own theory; meaningless intertheoretically" (WO, 1960b, §6).[56]

In the literature, Quine is also widely read as a meaning holist. For there is an almost general consensus that (1) Quine subscribes to a verificationist theory of meaning, and (2) that if one combines evidential holism with verificationism, meaning holism is the result. The idea behind (2) is that if one combines the thesis that the meaning of a sentence is constituted

55 Except, as we have seen, when it comes to logic and one-criterion words.

56 See also (EN, 1969a, 80), where Quine argues on the basis of the assumption that "the English sentences of a theory have their meaning only together as a body".

by the evidence for its truth (verificationism) with the idea that sentences admit of evidence only in clusters (evidential holism), one automatically obtains the idea that sentences admit of meaning only in clusters as well (meaning holism).[57] Indeed, Quine himself comes close to advancing something like the above argument in "Epistemology Naturalized":

> If we recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth, and if we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of theoretical sentences is the natural conclusion.[58] (EN, 1969a, 80-1)

Although Quine here speaks about the indeterminacy of translation, his reasoning seems to apply to meaning holism as well. After all, proposing a translation for a language L is to propose a theory of meaning for L, and the very idea behind the indeterminacy of translation is that there can be no direct evidence for translation proposals on the sentential level. In choosing between translations for L, and thus in choosing between theories of meaning for L, linguists can only decide between translation manuals as a whole.[59]

The above considerations strongly suggest that we ought to think of Quine as a meaning holist. Still I believe that we must

57 For a discussion of this argument, see Føllesdal (1973, 290-1), Gibson (1982, 80-81), Becker (2001, 76), Cozzo (2002), De Rosa and Lepore (2004), Pagin (2006), and Morrison (2010, 338). Most of these authors also believe that the argument is valid. An exception is Fodor and Lepore (1992). For a response to Fodor and Lepore, however, see Harrell (1996) and Okasha (2000).

58 See also, (RRG, 1986j, 155-6), where Quine attributes this argument to Føllesdal and claims that although "Føllesdal mistrusts this defense because of doubts about verificationism [...] I find it attractive".

59 Cf. Hookway (1988, 166). See also De Rosa and Lepore (2004, §2) for a defense of the claim that Quine "himself refers to his meaning holism as the thesis of the indeterminacy of translation".

be cautious in ascribing MH to Quine. After all, it is certainly misleading to speak about Quine as having a substantive theory of meaning in the first place.[60] If we read verificationism as a substantive theory of meaning, and meaning holism as the thesis that the *meaning* of a sentence S is constituted by S′ contribution to the theory as a whole, then it would definitely be problematic to attribute it Quine. For Quine submits that sentences do not have meanings.[61]

Still if carefully formulated, it *is* correct to say that Quine's holism has *semantic implications* and that these implications follow from his ideas about (1) evidential holism, and (2) his verificationism broadly construed. For although Quine believes that we have no need for meanings, he does not deny that our utterances are *meaningful*: "I feel no reluctance toward refusing to admit meanings, for I do not not thereby deny that words and statements are meaningful" (OWTI, 1948, 11).[62]

60 For similar worries, see Hookway (1988, 165-6), McDermott (2001, §8), Becker (2001, 81n1), Hylton (2007, 56-8), Kemp (2010, 290), Ebbs (2011b, 622), and N. Sinclair (2012, 557). Next to rejecting an interpretation of Quine in which his verificationism functions a substantive theory of meaning, we should also dismiss a reading of Quine in which he uses verificationism to distinguish between the meaningful and the meaningless. For as Hylton (forthcoming) convincingly shows, Quine does not allow any strict criterion of significance. See also chapter 3, footnote 28.

61 See (OWTI, 1948, 11-2) and (PML, 1953d, 47-8). Note that Quine has in principle no problem with the hypostasization of meanings. The problem however is that there are no clear identity-criteria for meanings, i.e. that it is unclear when two sentences have and when they do not have the same meaning. See (RWA, 1986k, 73): "Synonymy, not hypostasis, is the rub".

62 See also (WO, 1960b, §43), where Quine dismisses Grice and Strawson's (1956) claim that "if we are to give up the notion of sentence-synonymy as senseless, we must give up the notion of sentence-significance (of a sentence having meaning) as senseless too". Quine dismisses this argument as fallacious: "it is argued that if we can speak of a sentence as meaningful, or as having meaning, then there must be a meaning that it has, and this meaning will be identical with or distinct from the meaning that another sentence has. This is urged without any evident attempt to define synonymy in terms of

So if we are to make sense of MH, we ought to read it not as a thesis about sentence-meaning. Rather, we should view MH as the thesis that "the *empirical content* of sentences [cannot] in general be sorted out distributively, sentence by sentence" (CGC, 1962, 132, my emphasis).[63] The key is to interpret Quine's verificationism not as a substantive theory of meaning or as a strict criterion of significance but as a restriction on his theory of language learning, the basics of which were sketched in section 2.7:

> Should [we] abandon the verification theory of meaning? Certainly not. The sort of meaning that is basic to [...] the learning of one's own language, is necessarily empirical meaning and nothing more. A child learns his first words and sentences by hearing and using them in the presence of appropriate stimuli. [...] Surely one has no choice but to be an empiricist so far as one's theory of linguistic meaning is concerned. (EN, 1969a)

Verificationism, in others words, is an expression of Quine's view that any plausible theory of significance, i.e. any reasonable explanation of our understanding of the sentences we use, must be sought in terms of dispositions to behavior.[64] And the reason behind this restriction, according to Quine, the reason

meaningfulness" (WO, 1960b, 206-7). For an extensive discussion of Quine's argument here, see Ebbs (2011b).

63 Quine's reluctance to be identified as a meaning holist and his empirical reinterpretation of the doctrine is nicely summarized in his answer to a question about semantic holism in an interview: "it usually takes a substantial bundle of sentences about the world, to imply [...] observable consequences [...] Such is my semantic holism, though I don't recall using the phrase; namely, that sentences have empirical meaning only jointly" (IQJ*, 1990e, my transcription).

64 See (MVD, 1975b, 248): "The behavioural level [...] is what we must settle for in our descriptions of language, in our formulation of language rules, and in our explications of semantical terms. It is here, if anywhere, that we must give our account of the understanding of an expression [...] These things

as to why any theory of language learning can only appeal to publicly observable behavior, is naturalism, i.e. Quine's commitment "to approach semantical matters in the empirical spirit of natural science" (PPLT, 1970b, 8):

> I hold [...] that the behaviorist approach is mandatory. In psychology one may or may not be a behaviorist, but in linguistics one has no choice. Each of us learns his language by observing other people's verbal behavior and having his own faltering verbal behavior observed and reinforced or corrected [...] There is nothing in linguistic meaning beyond what is to be gleaned from overt behavior in observable circumstances.[65] (PT, 1990g, 37-8)

need to be explained, if at all, in behavioural terms; in terms of dispositions to overt gross behaviour".

65 Quine is generally read as a rigorous behaviorist on the basis of passages like these. Hylton (2007, §4.4.) is one of the few to have argued that such a characterization is unfair. Quine's behaviorism, Hylton argues, is nothing more than the thesis that any theory of language should start from the "undeniable" fact that "language is learnt by infants who receive information about the world only through their sensory stimulations". Hylton also credits Katz, "one of Quine's most vehement critics" for having appreciated this. See Katz (1990, 179-80):

> I shall [...] make no objection to Quine's statement that 'the behaviorist approach is mandatory'. The behaviorism he has in mind here is not the dreaded reductive doctrine of days gone by, but merely a way of putting the study of language on a par with other sciences by requiring the linguist's theoretical constructions to be justified on the basis of objective evidence in the form of overt behavior of speakers [...] Quine's behaviorism is thus a behaviorism one can live with. [...] Quine's behaviorism merely takes linguists out of their armchairs and puts them in the field facing the task of having to arrive at a theory of language on the basis of the overt behavior of its speakers in overt circumstance.

The question, according to Katz, is thus not whether Quine's approach is correct, but whether his claims about *what* 'can be gleaned from overt behavior' are correct. For an insightful discussion of the historical background of Quine's behaviorism, see Decock (2010).

In Quine's case, in sum, evidential holism can be said to lead to MH only when it is combined with a particular variant of verificationism. Quine's verificationism should not be interpreted as a substantive theory of meaning or as a strict criterion of significance but as a mildly behaviorist restriction on theories of signficance, a restriction which follows from Quine's naturalism.

5.8 CONCLUSION

When Quine, in the final stages of his career, was asked what problems he would choose to work on had he been twenty-five years old, he answered that he would like to follow out the "schema of empirical checking of scientific theory by deduction of observation categoricals; carrying it out in detail in application to some limited but substantial part of science, such as perhaps Newtonian mechanics" (ICQ*, 1993a). In this chapter, I have examined Quine's more philosophical ideas about the logic of theory testing, attempting to provide a detailed account of his holism from both a historical and a systematic perspective.

I have shown that Quine's core idea that all statements are epistemically on a par (are all ultimately aposteriori and revisable in principle)—the idea which played an important role in my reconstruction of Quine's development (chapter 4)—essentially depends on his evidential holism, which is itself a relatively innocent, empirically supported idea about the logic of theory-testing. And although Quine *does* require a wide-scoped holism in showing that all statements, even our logical and mathematical truths, are ultimately evaluated on the basis of what they contribute to our best scientific theories, this holism needs to be wide-scoped only in the sense of what I have called 'scope_2', i.e. in the sense that any statement will be required in the deduction of at least one observation categorical. As such,

Quine's views on the matter are less radical than is often suggested.

A similar conclusion can be drawn about Quine's holism with respect to meaning. In the chapters 2 and 3, I have argued that Quine's argument against traditional epistemology and metaphysics essentially presupposes holism. Quine, we saw, rejects transcendental perspectives because distinctively philosophical talk about 'evidence, 'truth', and 'reality' is empirically empty. In this chapter we have seen what underlies this type of holism. Quine's evidential holism becomes semantic when it is combined with his particular variant of verificationism, a thesis which itself largely follows from Quine's naturalistic theory of language learning. As a result, Quine's philosophy is more complex than we have thus far presupposed: not only do holistic presuppositions play a crucial role in his naturalism, Quine's holism itself cannot be understood without taking into account his naturalism either, a claim that will be further developed in chapter 6.

6

SCIENCE, SCIENTISM, AND SETS

Summary: In this final chapter, I discuss and evaluate two arguments which aim to show that there exists a fundamental tension between Quine's holism and his naturalism. First, I discuss Penelope Maddy's argument that Quine's naturalism is *too weak*. A true naturalist, Maddy argues, should take scientific practices at face value, not evaluate them in terms of their contribution to science as a whole. Against Maddy, I argue that holistic pictures of inquiry *can* accommodate what scientists are doing and that pluralistic versions of naturalism face deep-rooted problems. Secondly, I discuss Susan Haack's argument that Quine's naturalism is *too strong*. According to Haack, Quine unconsciously vacillates between two notion of 'science', something which pushes him into the direction of an implausibly strong scientism. Against Haack, I argue that Quine's naturalism is more moderate than it might appear and should not be interpreted as scientistic.[1]

6.1 INTRODUCTION

In the previous chapters, we have seen how naturalism and holism intertwine in Quine's philosophy. Holism plays a crucial role in Quine's defusion of the science-philosophy distinction

1 The second half of this chapter is an adapted version of the paper "Rafts and Cruise Ships: Quine's Naturalism Disambiguated" (under review-d). I thank the people who attended my talk at the Free University Amsterdam for their valuable comments. Section 6 is partly based on "Quine, Putnam, and the Naturalization of Metaphysics" (2013) that appeared in *Metaphysics or Modernity?* (eds. S. Baumgartner, T. Heisenberg, and S. Krebs, pp. 249-269).

(chapter 1), in his explicit arguments against traditional epistemology and metaphysics (chapters 2-3), and in the early development of his naturalism (chapter 4). Conversely, we have seen that Quine's views about holism themselves partly rely on his naturalistic theory of language learning (chapter 5).

Not everyone is convinced that holism and naturalism are this closely related however. In fact, many have argued that there exists a fundamental tension between the two: where naturalism seems to be a thoroughly uncompromising thesis about the epistemic status of the (natural) sciences, i.e. a scientistic worldview in sheep's clothing, holism seems to be a relatively tolerant thesis—a view about theory selection that is compatible with a wide range of philosophical perspectives.[2] That is, where naturalism seems to rule out some theses or types of inquiry as unscientific, there seems to be no reason why holism cannot rule them back in.[3]

If the above line of reasoning is correct, then there are two ways to attack Quine's philosophy on Quinean grounds. On the one hand, one might appeal to naturalism and argue that Quine's holism supports views that are evidently extra-scientific—something a true naturalist, by definition, should never allow. On the other hand, one might appeal to holism and argue that Quine's naturalism puts unnecessary constraints on his the-

2 See Fogelin (1997), who distinguishes in Quine's philosophy two conflicting components: a "hard component", consisting of physicalism, extensionalism, naturalism, and behaviorism; and a "soft component" consisting of holism, universal revisability, and the indeterminacy theses. See also Weir (2014, 114-5), who argues (1) that 'the heart of Quine's naturalism is a reductionist form of scientism" and (2) that there is in Quine's philosophy "a tension (indeed an outright inconsistency) between a hard-line reductionism from which he never broke free and the holistic anti-reductionism which is his official position".

3 Lewis' (1986) modal realism, for instance, appears to be a characteristic example of a distinctively philosophical thesis that is primarily defended on the basis of broadly holistic considerations in the Quinean sense. See Rosen (2014, §4) and Williamson (2014, 9-11).

orizing, constraints that needlessly impede scientific progress as naturalism dismisses some legitimate types of inquiry as unscientific.

In this final chapter, I discuss and evaluate what I believe to be the best-developed versions of both types of argument. I start out with an argument of Penelope Maddy, who zooms in on Quine's ideas about the justification of set theory and who signals that these ideas strongly conflict with the views of the set theorists themselves. According to Maddy, Quine's ideas about set theory are heavily influenced by his holism, something that is unjustified from a naturalistic perspective, according to which any scientific enterprise should be evaluated on its own terms (sections 6.2-6.3). In response to this argument, I show (1) that holistic pictures of inquiry *can* accommodate what set theorists are doing and (2) that pluralistic versions of naturalism face profound problems which have led Maddy herself to commit to a type of holism that is not in any sense weaker than the holism to which Quine subscribes (sections 6.4-6.6).

Next, I turn to an argument of Susan Haack, who zooms in on Quine's views on the justification of natural science and signals that his implausibly strong naturalism forces him to reject this issue as illegitimate (sections 6.7-6.8). Haack, who herself defends a modest variant of naturalism—a variant which she justifies on the basis of Quine's holism—argues that Quine unconsciously vacillates between two notions of 'science', and that it is this vacillation which explains his unjustifiably strong variant of naturalism. After having examined Quine's responses to Haack, I argue that Haack and Quine are talking past each other and that once this mutual misunderstanding is cleared up, Quine's naturalism turns out to be more modest, and hence less scientistic, than it may have first appeared (sections 6.9-6.12).

6.2 JUSTIFYING THE AXIOMS OF SET THEORY

Let me start with Maddy's argument for the conclusion that Quine's naturalism is needlessly curbed. Maddy arrives at this conclusion in her pursuit of an answer to the question of what justifies the axioms of set theory, the branch of mathematics which is often taken too serve as a foundation for mathematics as a whole.[4]

The justification of mathematical knowledge has always been a central concern of empirically-minded philosophers. After all, mathematics seems to be an indisputable counterexample to the empiricists' core idea that all our knowledge about the world ultimately originates in sense experience. As we have seen, even the Carnap-Quine debate about the analytic-synthetic distinction can, to some extent, be understood as a conflict about how we are to account for our mathematical knowledge.[5] Maddy, however, transforms this traditional empiricist question into a question about the justification of the axioms of set theory. For she argues, if all mathematics is unified by set theory, and all set theory can be derived from its axioms, some of the empiricists' most central worries concerning the justification of mathemat-

4 For instance by Quine himself. See (STL, 1963, 4) and (FSS, 1995b, 86-7). In particular, Quine thinks of set theory as providing an 'ontological reduction' of mathematics. See (ORWN, 1964). Note that set theory's foundational role thus need not be spelled out in terms of its axioms being *more certain* than our mathematical truths. See (OR, 1968c, 43). This is something Maddy herself also emphasizes; for her, set theory is primarily foundational because it plays a strong unifying role. See Maddy (1997, ch. I.2).

5 See (QSM, 1988, 26): "How, Carnap asked, can mathematics be meaningful despite lacking empirical context? His answer was that mathematics is analytic. Holism's answer is that mathematics, insofar as applied in science, imbibes the shared empirical content of the critical masses to which it contributes". This stick-figure summary already nicely illustrates why Maddy disagrees with Quine. Maddy is not satisfied with Quine's 'holistic answer'; she wants a naturalistic answer.

ics boil down to question of how the axioms of set theory are justified:

> The truths of mathematics hold a place of honour in our collective web of belief. While our fondest empirical beliefs [...] are all supported by definitive accumulations of good empirical evidence [...] in the mathematical case [...] we must reach fundamental assumptions [...] The astounding achievement of the foundational studies of the late nineteenth and early twentieth centuries was the discovery that these fundamental assumptions could themselves be proved from a standpoint more fundamental still, that of the theory of sets. The idea is simple: the objects of any branch of classical mathematics—numbers, functions, spaces, algebraic structures—can be modelled as sets, and resulting versions of the standard theorems can be proved in set theory. So the most fundamental of the fundamental assumptions of mathematics, the only such assumptions that truly cannot be proved, are the axioms of the theory of sets itself. In this sense, then, our much-valued mathematical knowledge rests on [...] the set-theoretic axioms [...] what justifies [these] axioms? (Maddy, 1997, 1-2)

This fundamental question about the grounds of the axioms of set theory is particularly pressing because there are quite a few mathematical questions that cannot be settled solely on the basis of classical set theory, i.e. on the basis of Zermelo-Fraenkel set theory with the axiom of choice (ZFC). The most notorious example of such an 'independent question' is Cantor's Continuum Hypothesis (CH), which can neither be proved nor be disproved in ZFC.[6] This situation has led to a search for additional axioms that *do* decide such problems. The question, however, is

6 CH is the hypothesis that (assuming a well-ordered continuum) the smallest cardinal number greater than $\aleph_0$ (the cardinality of the integers) is $\aleph_1$ (the

how we are to decide between different axiom candidates. Answering this question naturally depends on one's views about what justifies the axioms of set theory.[7]

As a case study, Maddy focuses on one particular axiom candidate, viz. the axiom of constructibility (hereafter $V = L$).[8] *Prima facie*, this axiom candidate seems to be a good choice because $V = L$ *does* decide most independent questions. The continuum hypothesis, for example, can be proved from ZFC + $V = L$.[9] Moreover, $V = L$ also seems to be a good candidate from a holistic perspective, as Quine has argued:

> sentences such as the continuum hypothesis [...] which are independent of [ZFC], can still be submitted to the considerations of simplicity, economy, and naturalness that contribute to the molding of scientific theories generally. Such considerations support [...] $V = L$. (PT, 1990g, 95)

In fact, Quine not only argues that these considerations 'support' $V = L$, he argues that we should adopt $V = L$ (or an

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cardinality of the reals). Gödel (1939) proved that ZFC cannot disprove CH; Cohen (1963, 1964) proved that ZFC cannot prove CH either.

7 Intuitively, one might simply suggest that one ought to look for self-evident axioms. Maddy believes this to be impossible, however, as not even the existing axioms of ZFC were accepted because they are self-evident. See Maddy (1988a,b). See also (STL, 1963, 4): "The natural attitude on the question what classes exist is that any open sentence determines a class. Since this is discredited [viz. by Russell's paradox], we have to be deliberate about our axioms of class existence and explicit about reasoning from them; intuition is not in general to be trusted here".

8 L is the union of all stages L_α, such that at every stage $\alpha + 1$, one uses only those subsets of L_α which are definable by a first-order formula whose quantifiers range over, and whose parameters are drawn from L_α. $V = L$, then, states that L describes the total universe of sets V, and thus that every set is constructible. See Maddy (1997, 64-5).

9 In fact, the continuum hypothesis can be proved from ZF + $V = L$ as the axiom of constructibility implies the axiom of choice.

equally plausible candidate axiom) precisely *because* it satisfies these maxims. For according to Quine, these are the only considerations there are: "In set theory [...] we find ourselves engaged in something very like convention [...] We find ourselves making deliberate choices and setting them forth *unaccompanied by any attempt at justification other than in terms of elegance and convenience*" (CLT, 1954a, 117, my emphasis).[10]

Yet although $V = L$ decides many independent questions and Quine believes it to be an interesting axiom candidate from a holistic perspective, the axiom is rejected by the majority of set theorists.[11] How can this be? One of Maddy's main philosophical goals is to answer the following two questions: (1) Is the question whether or not to adopt $V = L$ a genuine question? That is, is there a fact of the matter with respect to the truth of $V = L$? (2) If the question whether or not to adopt $V = L$ is a genuine question, how are we to answer it? We shall see that Maddy has given different answers to these questions at different stages of her career. In what follows, I examine these answers and show how she has come to develop an argument against Quine's holistic evaluation in the process.

10 See also Ebbs (2011a). Quine has maintained this attitude throughout his career. See (RCP, 1986c, 400) and (IV, 1991a, 243). Note that the important point here is Quine's selection criterion, not his commitment to $V = L$. For he does consider alternatives: "More sweeping economies have been envisioned by Hermann Weyl, Paul Lorenz, Errett Bishop, and currently Hao Wang and Solomon Feferman, who would establish that all mathematical needs of science can be supplied on the meager basis of what has come to be known as predicative set theory. Such gains are of a piece with the simplifications and economies that are hailed as progress within natural science itself. It is a matter of tightening and streamlining our global system of the world" (PT, 1990g, 95). See also (RHW, 1986e, 648). See Decock (2002a, §§3-4) for a history of Quine's evolving views about set theory.

11 For a list of set theorists that have rejected the axiom, see Maddy (1997, I.6).

6.3 REALISM AND NATURALISM

Maddy started out as a 'set-theoretic realist', a view inspired by Gödel's platonism and Quine's indispensability argument for the existence of mathematical objects.[12] In *Realism in Mathematics* (1990), Maddy answers the above two questions from this realist perspective. In response to the first question, she argues that the problem of whether or not to adopt $V = L$ is a genuine problem because there is a fact of the matter about $V = L$ in the objectively existing world of sets; and in response to the second question, she argues that it is an "open problem" (*pace* Quine) whether or not $V = L$ is indeed the correct axiom (Maddy, 1990, ch. 4.5).

In her second book, however, Maddy changes her mind with respect to the latter answer. She now argues for the stronger conclusion that a set-theoretic realist (still *pace* Quine) *should* reject $V = L$. By means of an insightful historical sketch, Maddy shows how mathematicians over the last two centuries gradually came to reject a methodological maxim she calls 'definabilism', i.e."the requirement that every mathematical object be definable in a certain uniform way" (1997, 116). According to Maddy, mathematicians slowly came to reject this principle be-

12 For Gödel's realism, see his (1947). Quine's indispensability argument has been briefly discussed in section 4.10. Recall Quine's switch from nominalism to realism in his (1948) letter to Woodger: "A brief reflection now on ontology. I suppose the question what ontology to accept is in principle similar to the question what system of physics or biology to accept: it turns finally on the relative elegance and simplicity with which the theory serves to group and correlate our sense data [...] Now the positing of abstract entities (as values of variables) is the same kind of thing" (QWC*, 1938-1982). Later, Quine would add that we do not only need to accept abstract entities because they contribute to our best scientific theories, but that he sees "no way of meeting the needs of scientific theory, let alone those of everyday discourse, without admitting universals irreducibly into our ontology" (SID, 1980b, 450). The term 'indispensability argument' is from Putnam (1971, 347).

cause it turned out to be unnecessarily restricting.[13] Now, because $V = L$ is an axiom that is related to this old-fashioned methodological maxim—it requires every set to be definable in a uniform way—Maddy concludes that the axiom should not be adopted:

> The connection between $V = L$ and Definabilism is obvious. The Axiom of Constructibility states that sets are definable in a uniform, indeed, in a predicative, way. In contemporary mathematics, the assumption that $V = L$ appears as a throw-back to an especially strong from of a now-discredited methodological maxim [...] [H]istory supports the expectation that it would limit the development of mathematics. Under these circumstances, the deep and widespread resistance to adding $V = L$ as a new axiom seems perfectly rational. (Maddy, 1997, 129)

Maddy's set-theoretic realism has a naturalistic flavor: it refuses to recommend a reform of mathematics on non-mathematical grounds. That is, it suggests that set-theorists are right in rejecting $V = L$ and it argues that they should reject it *because* it is not in line with contemporary mathematical norms.

Still Maddy believes that the position is not naturalistic enough. According to Maddy, set-theoretic realism is flawed because it does not do any work in her methodological argument against $V = L$; it only complicates the argument by invoking additional philosophical constraints. After all, if it is correct that $V = L$ is rejected by most set theorists because it is perceived as unnecessarily restricting, this in no way implies that

13 One of the many examples Maddy discusses is the debate between Poincaré and Zermelo over the use of impredicative definitions. According to Poincaré (1909, 63), mathematicians should "[n]ever consider any objects but those capable of being defined in a finite number of words", whereas Zermelo pointed out that those definitions are widely used in set theory and analysis. See Maddy (1997, 126). Today few mathematicians still reject impredicative definitions.

$V = L$ is also false of the mind-independent set-theoretic world. A set-theoretic realist, in other words, requires an additional argument to show that the maxims on which the set theorists rely are reliable maxims for determining the nature of this independently existing world of sets.[14]

Furthermore, Maddy argues, even if the set-theoretic realist *were* to succeed in showing that the mathematician's procedures reliably track mathematical truth, the resulting position would still not be satisfying from a naturalistic perspective. For although it would not recommend any reform to mathematical practice, it still does attempt to *defend* mathematical practice on the basis of a distinctively philosophical argument. That is, the resulting position would defend the view that there is a fact of the matter about whether or not to adopt $V = L$ on the basis of the distinctively philosophical thesis that there exists a mind-independent world of sets. According to Maddy, however, true naturalists should not only abstain from *criticizing* scientific and mathematical practice, they should also eschew the practice of philosophically *defending* science and mathematics:

> set-theoretic realism was intended to be a 'naturalistic' theory: e.g. it steadfastly refuses to recommend reform of mathematics on philosophical grounds [...] But now

14 Note that this epistemological issue is not a problem for Quine's brand of realism. For as he notes in his response to a related worry from Wilfrid Sellars, on his holistic picture we do not require an independent argument for the reliability of our scientific and mathematical procedures: "An epistemological account of our talk of numbers or classes is to be sought rather in inferential or semantical connections between sentences that contain references to numbers or classes and sentences that are more sensitive to observational evidence" (SBLM, 1980a, 29). See also Quine's reading notes on Benacerraf's (1973) "Mathematical Truth", the paper which formulates the latter's influential dilemma for mathematical realism: "I part company at [Benacerraf's] causal theory of knowledge and reference. The world causes sensory stimulations, yes; but then natural science and mathematics together and on a par, accommodate the stimulations holistically" (NR*, undated, my transcription).

> another failing comes into view: though it recommends no reforms, it does attempt to defend mathematical practice on the basis of a philosophical realism about sets. It took me a very long time to realize that if philosophy cannot criticize, it cannot defend, either. (Maddy, 1997, 191-2)

In response to these and other arguments,[15] Maddy develops a new and rigorously naturalistic alternative to set-theoretic realism. This new position, which she dubs *mathematical naturalism*, can best be characterized by comparing it with Quine's (FME, 1975a, 72) definition of naturalism according to which 'science is not answerable to any supra-scientific tribunal':

> What I propose here is a mathematical naturalism that extends the same respect to mathematical practice that the Quinean naturalist extends to scientific practice [...] Where Quine holds that science is not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method, the mathematical naturalist adds that mathematics is not answerable to any extra-mathematical tribunal and not in need of any justification beyond proof and the axiomatic method. Where Quine takes science to be independent of first philosophy, my naturalist takes mathematics to be independent of both first philosophy and

15 It should be noted that in summarizing Maddy's case against set-theoretic realism, I have ignored one important argument, viz. Maddy's rejection of the indispensability argument underlying her set-theoretic realism. According to Maddy (1992, 1994, 1995), indispensability arguments strongly conflict with both scientific and mathematical practice and therefore cannot be part of a naturalistic perspective on mathematics. For some convincing responses, see Resnik (1997, ch. 4), Colyvan (2001), and Decock (2002b). I have chosen not to discuss this argument because Maddy's attack on the indispensabilty argument should not be construed as an attack on Quinean holism, the subject of this chapter. See Dieveney (2007) and Morrison (2012).

> natural science (including the naturalized philosophy that is continuous with science)—in short, from any external standard. (Maddy, 1997, 184)

Maddy's mathematical naturalism, in other words, adopts Quine's criticism of extra-scientific evaluations of science and applies it to mathematics. If science should not be evaluated on the basis of extra-scientific norms, then neither should mathematics be evaluated from a vantage point outside mathematics.

As a result, Maddy's naturalistic answers to the two main questions about $V = L$ as posed in section 6.2 are surprisingly simple: the question whether or not to adopt $V = L$ is a genuine question as set-theorists take it to be a genuine question;[16] and we should reject $V = L$ because set-theorists have strong mathematical reasons for rejecting it.[17]

The opposition between Quine and Maddy is a conflict between two types of naturalism. Where Quine's defends a *holistic* naturalism by submitting set theory to "the considerations

16 Of course there are mathematical questions which are not considered to be genuine questions by mathematicians. The question whether or not we are to adopt the parallel postulate seems to be a case in point: there are Euclidean geometries that satisfy the parallel postulate and non-Euclidean geometries that do not. In set theory, however, the situation is different, or so Maddy argues: "given that set theory is (at least partly) designed to provide a foundation for classical mathematics, to provide a single arena for mathematical existence and proof, it does make sense to try to make our theory of sets as decisive as possible, to try to choose between alternative axioms" (Maddy, 2007, 355). Whether Maddy's conclusion is correct here is a matter of debate. Mathematicians of a formalist bent, for example, reject the presupposition that there is one correct set of axioms for set theory. See Robinson (1969), Cohen (1971), and Mostowski (1967).

17 In the remainder of *Naturalism in Mathematics*, Maddy supports this conclusion by explicating the norms that set-theorists use in their debates to defend or reject certain axioms. She concludes that set-theorists often appeal to two important norms: MAXIMIZE (provide a maximal framework which admits as many sets as possible) and UNIFY (provide a single unified theory in which as many open mathematical questions are resolved). She argues that $V = L$ is rejected because it conflicts with MAXIMIZE.

of simplicity, economy, and naturalness that contribute to the molding of scientific theories generally" (PT, 1990g, 95), Maddy's mathematical naturalism is *pluralistic*; scientific theories are evaluated using scientific norms and set-theoretic axioms are evaluated using set-theoretic norms. According to Maddy, only mathematical naturalism can account for what set theorists are actually doing. In what follows, I present two arguments for the conclusion that Maddy is mistaken and that holistic variants of naturalism are to be preferred over pluralistic ones.

6.4 PURE MATHEMATICS VS. PURE ASTROLOGY

Let me start with the question *why* Maddy believes that debates concerning the adoption of new set-theoretic axioms ought to be resolved exclusively by intra-mathematical means, i.e. why a self-sufficient mathematics would be more valuable than a mathematics that is influenced by philosophical and/or scientific argument.[18] This question arises because even if it were the case that mathematicians *are* not influenced by any supra-mathematical norms, this does not necessarily entail that their theories are thereby valuable. Many types of inquiry are largely sealed from outside influence without being considered to be valuable; think of astrology and alternative medicine. The challenge to the mathematical naturalist is to provide reasons for the claim that mathematics (and set theory in particular) are different, i.e. reasons for the claim that a completely self-sufficient

18 Rosen (1999) has pointed out that mathematical naturalism can be interpreted as either a *descriptive* or a *normative* theory. In this section and the next, I interpret mathematical naturalism normatively—i.e. as defending the claim that the set-theoretic axioms *should* be evaluated solely on mathematical grounds—as it is this version that provides the most formidable challenge to Quine's holistic naturalism. Another reason not to discuss the descriptive thesis here is that it is false; mathematicians often *are* influenced by extra-mathematical considerations. See Hale (1999, 395) and Paseau (2013, 28).

mathematics would be valuable whereas a completely self-sufficient astrology is not.

Maddy's initial response to this challenge is to point out that mathematics (as unified by set theory) is a *successful* practice and that "successful practice[s] should be understood and evaluated on [their] own terms" (Maddy, 1997, 201). The problem with this response, however, is that is unclear how we are to explicate the term 'successful' in this context. It seems that as a mathematical naturalist, Maddy can only claim that mathematics is successful by its own lights. Yet as many commentators have pointed out, this internal sense of 'successful' is problematic because it does not single out mathematical practice.[19] Astrology and theology, for example, could make similar claims in so far as they consider themselves successful by their own standards.

Maddy, foreseeing this objection, examines the possibility of adopting an 'astrological naturalism', a position according to which we ought to judge astrology solely on the basis of astrological norms, rejecting extra-astrological, scientific considerations.[20] Maddy responds to this challenge by pointing at the special relation between mathematics and natural science: "Mathematics is staggeringly useful, seemingly indispensable, to the practice of natural science, while astrology is not" (ibid., 204-5).

19 See Dieterle (1999), Rosen (1999), and Tappenden (2001).

20 Of course, there is a difference between astrology and mathematics. Astrologers make claims that directly conflict with our best scientific theories, i.e. claims about causal interactions between celestial bodies and our everyday lives, whereas mathematicians do not. Yet as Maddy admits, one can imagine a subtler interpretation of the subject in which astrology is the science of "certain supernatural vibrations that *don't* interact causally with ordinary phenomena" (1997, 204, my emphasis). On this interpretation, astrology (let's call it 'pure astrology') also does not compete with the sciences. The challenge now comes down to answering the question why, if we adopt mathematical naturalism, we should not also adopt 'pure astrological naturalism'.

Several commentators have questioned whether the mathematical naturalist can legitimately respond in this way.

> Maddy seems to want to have her cake and eat it. The reason for mathematics' credibility is supposed to be its application in science. But why should the fact that mathematics features in our best science be a reason for believing mathematicians' utterances—that is, a reason for taking them to be true? The suspicion is that if featuring in our best science is the mark of credibility, it should be scientific standards that ultimately determine mathematical theories' acceptability. (Paseau, 2013, 30)

In other words, although Maddy rejects the evaluation of mathematics on the basis of scientific standards, she needs to appeal to science to justify adopting mathematical naturalism without being also committed to 'astrological' naturalism, something which puts pressure on her to acknowledge that scientific norms should also play a role in our decision whether or not to adopt a given axiom.[21]

In later work, Maddy (2007; 2011) has responded to this objection. After having slightly modified the way in which she characterizes her naturalism,[22] Maddy argues that although both

21 See also Dieterle (1999, 130) and Tappenden (2001, 496). Another problem is that Maddy's response only seems to work for the applied parts of mathematics, and not for the "the more gratuitous flights of higher set theory" that Quine so effectively "inactivates" by adopting $V = L$ (PT, 1990g, 95). Maddy tries to save the unapplied parts of mathematics by claiming that her "mathematical naturalist sees mathematics as a *unified* undertaking" (Maddy, 1997, 205, my emphasis), but, as Rosen notes, such a response "will not move the Quinean [who wants] to know why on earth the distinctive theoretical concerns that function within higher set theory [...] should count without further argument as genuine reasons for belief" (Rosen, 1999, 472-3).

22 Shortly after *Naturalism in Mathematics*, Maddy (2001; 2003; 2007) gives up on the idea of naturalism as a distinctive doctrine. She comes to view naturalism as an attitude—somewhat reminiscent of van Fraassen (2002)—and

pure astrology and mathematics are apt subjects of sociological and anthropological study, there is a difference between the two:

> What sets pure mathematics apart, obviously, is its apparently essential contribution to our scientific description of the world. Because of this added feature the Second Philosopher's [see footnote 22] treatment of pure mathematics in sociology, anthropology [...] and so on—the treatment that runs parallel to her approach to pure astrology [...]—will not be enough. In the course of her examination of her own best methods, she will need an account of how and why pure mathematics plays the role it does. (Maddy, 2007, 346-7)

In a footnote, she adds: "This is not a reversion to a Quinean [naturalism], because the conclusion is only that mathematics is *different* from pure astrology, not that mathematics is *confirmed*" (ibid., 346n4, my emphasis). Maddy's response, in short, is that her appeal to mathematics' use in science is not meant as a justification of mathematical claims, rather it is meant as a descriptive difference which justifies her treating mathematics in a different way than astrology.

This response is unsatisfactory. Recall that the challenge under discussion is a challenge to someone who has already adopted mathematical naturalism. We ask this person why she does not also adopt astrological naturalism. Now, if the answer is

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introduces a naturalistic character, a 'Second Philosopher', who begins from "commonsense perception and proceeds from there to systematic observation, active experimentation, theory formation and testing, working all the while to assess, correct, and improve her methods as she goes" (ibid., 2). She contrasts this position, among others, with the Quinean naturalist who, she argues, is *not* a 'busy sailor' from birth (ibid., 87). I have already argued against this characterization of Quine's naturalism in chapter 2. For my present purposes—assessing the relative benefits of holistic and pluralistic naturalism—the details of Maddy's later version of naturalism are irrelevant.

(i) that mathematics makes an essential contribution to science whereas astrology does not, and (ii) that this is a mere descriptive difference (not one justifying any mathematical claim), then one can wonder what good a reason this is not to adopt astrological naturalism as well. After all, there are many descriptive differences between astrology and mathematics which do not contribute to the justification of the one enterprise over the other (e.g. the fact that Nancy Reagan regularly consulted an astrologer and not a mathematician); and these differences do not seem to be particularly relevant in our answering the question whether or not to adopt the one type of naturalism over the other. So why would this one descriptive difference—the fact that only mathematics essentially contributes to science—be a reason to adopt the one but not the other if it is not intended to be a justification of the mathematician's claims. It seems that Maddy either has no grounds to reject astrological naturalism or has to admit that the role mathematics plays in science contributes to its being a valuable enterprise, which would in turn pressure Maddy to admit that scientific norms should also play a role in our decision whether or not to adopt certain set-theoretic axioms.

6.5 MAD MATHEMATICS

This dilemma becomes more forceful when we consider a second challenge for the mathematical naturalist. The first challenge addressed the question why we, under the assumption that we should adopt mathematical naturalism, should not also adopt (pure) astrological naturalism. Now, we turn to the question why we should adopt a mathematical naturalism in the first place. What if, the challenge asks, mathematicians would collectively go mad and dismiss the requirement that set theory should be consistent? Would the mathematical naturalist have

any means to oppose such a development? Maddy takes this challenge into consideration and gives the following response:

> suppose mathematicians decided to reject the old maxim against inconsistency—so that both '2 + 2 = 4' and '2 + 2 = 5' could be accepted—on the grounds that this would have a sociological benefit for the self-esteem of school children. This would seem a blatant invasion of mathematics by non-mathematical considerations, but if mathematicians themselves insisted that this was not so, that they were pursuing a legitimate mathematical goal, [...] I find nothing in the mathematical naturalism presented here that provides grounds for protest. (Maddy, 1997, 198n9)

According to Maddy, in other words, mathematical claims are by definition immune to scientific revision, even if these claims are obviously in conflict with our scientific body of knowledge. But why, some commentators ask, would anyone adopt mathematical naturalism if this is the consequence? Neil Tennant, for example, argues that Maddy here reduces her position to 'absurdity':

> Such is the price of buying into Quineanism without its unrelenting [...] holism. For Quine, it is the evidential holism in our theory of nature that truly naturalizes other areas of thought, such as mathematics. Natural science as a whole has to be understood in its own terms. To be anti-holistic, and separate mathematics off from science as a whole, as Maddy does, is to divert the springs of naturalism at their very source. (Tennant, 2000, 329-30)

According to Tennant, mathematics is simply too valuable to leave important decisions only to mathematicians:

> as Maddy herself points out [...] mathematics is 'staggeringly useful, seemingly indispensable, to the practice of natural science'. One might add also: to engineering and technology; to medical diagnostics; to the financial markets; to actuarial science; and to a host of other areas of human activity in which everyone's interests and concerns are engaged. So [...] those outside the community of professional mathematicians have a permanent and legitimate concern in the nature of the norms governing the latter's practice. In a word: mathematicians cannot be allowed to be a law unto themselves. What they do is too important, and ought to be subject to outside constraints designed to protect everyone's interests. (ibid., 328-9)

In other words, there seems to be no reason to accept mathematical naturalism if it leads to the conclusion that mathematics is in principle immune to scientific revision, i.e. if it leads us to exclude the possibility that we can revise some portion of mathematics in the future if it turns out that this would benefit the progress of science.[23]

Again, Maddy replies in her later work:

> One might worry that [the assumption that the actual methods of mathematics are the ones that should be followed] leaves the well-being of science at the mercy of the mathematician's whim [...] I think this concern mistakes

23 Of course, in practice, scientists virtually never propose to revise any applied mathematics; they merely revise its interpretation. As Dieterle notes, however, this does not mean that scientists would never propose to revise *pure* mathematics if this were to benefit their particular scientific goals: "It is true that mathematical claims that occur within science are not typically subjected to correction. If a hypothesis is disconfirmed, it is the empirical portion that is corrected, not the mathematical part. But [...] this is relevant only to applied mathematics. There is no evidence for the claim that a scientific naturalist *does* or *ought* to regard the methods and practices (and claims) of unapplied mathematics as immune to correction" (Dieterle, 1999, 134, my emphasis).

> the attitudes of pure mathematicians. Though they may not be primarily motivated by physical applications, providing tools for natural sciences remains one among the overarching goals of the practice of mathematics. (2007, 349-50)

Maddy makes a small concession here; she admits that providing tools for the natural sciences is one of the goals of mathematical practice. This reinforces my critique on Maddy's response to the astrological challenge above. For if providing tools for the natural sciences is one of the goals of mathematics, again there is pressure on Maddy to admit our decision whether or not to adopt a certain set-theoretic axiom should be ultimately constrained by scientific norms as well.

Maddy's response to the problem is not yet complete however. For even though 'providing tools for natural sciences remains one among the overarching goals of the practice of mathematics', one could imagine a situation in which mathematicians collectively decide to drop this goal. How would Maddy's Second Philosopher reply in such a scenario?

> What if mathematicians were to decide that the goal of providing tools for natural science should be outweighed by some other worthy objective, whatever that might be?[24] [...] One unhappy thought seems to me unavoidable: if the Second Philosopher couldn't somehow persuade these hypothetical mathematicians in terms of other shared goals and values [...] she would have no extraordinary means by which to convince them that they are wrong. In the case of the astrologers' star-based explanations of human behavior, she can show by her methods why they are misguided, but she cannot do so, as the astrologer would insist, without appeal to her methods

24 In a note, Maddy (2007, 350n16) refers to her '2 + 2 = 5' example.

> [...] similarly, she can show by her mathematical methods why these hypothetical wayward mathematicians are wrong, but she cannot do so, as would be required to return to the fold, without appeal to the very methods they have forsaken. (ibid., 350)

The situation is not as hopeless as this passage suggests, however, because

> [t]here's nothing in the strange tale told so far to determine whether or not the practice of these wayward souls would continue to be called 'mathematics', and of course the word doesn't matter. What is clear is that the new practice, whatever it's called, wouldn't play the same role in the Second Philosopher's investigation of the world as the discipline we call 'mathematics' now plays. Presumably the evolved practice would end up more or less comparable to 'pure astrology' and the Second Philosopher would have no interest beyond the [...] anthropological [...] Furthermore, for purposes of her ongoing investigation of the world, she and her fellows would need to reinvent a practice more or less the same as what we now call 'mathematics', and that practice would command precisely the attention previously awarded to the discipline that wandered off. (ibid., 350-1)

Maddy's argument here, in sum, boils down to the claim that if mathematicians would start pursuing goals that are harmful for science, the mathematical naturalist should correct those mathematicians by appealing to the goals of mathematics that are currently in play. If these mathematicians were to insist on their newly adopted goals, then the mathematical naturalist could just leave them to their new practices and continue to practice mathematics in the old-fashioned way. This is, of course, completely in conflict with the mathematical naturalists' core commitment.

Combining our two arguments, we can conclude that a truly pluralistic version of mathematical naturalism cannot be maintained. To see this, consider a group of mathematicians pursuing a new line of research that is generally considered to be completely irrelevant to science. Now there are two options: either (a) this new line of research is still compatible with the most general mathematical norms which have, over the past centuries, produced results that have strongly contributed to scientific progress, or (b) this new line of research is conducted on the basis of completely new mathematical norms. The upshot of the above arguments is that in both scenarios, mathematical naturalism collapses into Quinean holism. For in scenario (b), Maddy will reject the new mathematical norms because they do not contribute to scientific progress; and in scenario (a), Maddy only accepts the new line of research because the mathematical norms with which it is compatible *have* always contributed to scientific progress. In both scenarios, therefore, Maddy, like the Quinean naturalist, evaluates the mathematical enterprise ultimately in terms of its role in science as a whole.

To be fair, there is still a difference between Quine and Maddy; Quine proposes to ignore set theory's norms altogether, evaluating its axiom candidates solely in terms of 'simplicity, economy, and naturalness', whereas Maddy's set theorists are only *indirectly* influenced by scientific norms.[25] Regardless of how one

25 Still even on this issue Quine and Maddy's positions are not as far apart as one might think. For as Quine has claimed on multiple occasions, his proposal to restrict set theory for pragmatic reasons does not imply that he views all mathematics beyond the strictures of the axiom of constructibility as meaningless: "The strictures of economy are no threat to the starry-eyed set theorist for whom the sky is the limit. They do not declare any of his sentences meaningless; they merely slant our distribution of truth values so as to favor economy. His tracing of implications among them still makes proof-theoretic sense and can be methodologically illuminating as well as exhilarating" (FSS, 1995b, 56). See also (IV, 1991a, 243).

evaluates the Quine-Maddy debate on *this* issue, however, it is a debate within and not a debate about holistic naturalism.[26]

6.6 OPEN-MINDED NATURALISM

Maddy is not the only philosopher to have proposed a pluralistic variant of naturalism. In recent years there has been a broad movement of philosophers who have urged for a more 'pluralistic', 'liberal', or 'open minded' naturalism.[27] The spirit of this 'pluralistic movement' is perhaps best captured by Putnam (1981, 204), who proposes to change Neurath's metaphor by talking "not of a single boat but of a *fleet* of boats". It is my contention that the above conclusion about Maddy's mathematical naturalism extends to these alternative pluralisms as well: even if different fields of inquiry use different norms in everyday practice, in the end all sciences will have to appeal to the same overarching norms if they are to be taken seriously as sciences, i.e. if they are to be seen as part of what Quine, in Wittgensteinian spirit, has called "the game of science" (PT, 1990g, 20).[28]

26 For a similar conclusion from a different angle, see Decock (2002b, 243-5).

27 See, for example, some of the essays collected in De Caro and Macarthur (2004b, 2010).

28 A similar point can be made about *ontological* variants of pluralism. In response to Goodman's (1978) pluralism about 'world versions', for example, Quine argues that, ontologically, all these world versions will ultimately have to answer to physics because "nothing happens in the world, not the flutter of an eyelid, not the flicker of a thought, without some redistribution of microphysical states" (OW, 1978, 98). For according to Quine, it is almost a constitutive rule of the game of science that it is the physicist's job to develop a theory that is complete: "If the physicist suspected there was any event that did not consist in a redistribution of the elementary states allowed for by his physical theory, he would seek a way of supplementing his theory. Full coverage in this sense is the very business of physics, and only of physics" (ibid.). See also (RHP, 1986f, 430-1). Note that Quine's emphasis on the 'full coverage' of physics here does not imply that he is a reductionist. See (FM, 1977, 279).

To argue that the game of science is ultimately bound by some unifying overarching norms is not to argue that these norms play a crucial role in everyday scientific practice. Quine's picture certainly allows that different sciences appeal to different methods in furthering their own causes and thereby, indirectly, science as a whole.[29] Nor is it to deny that these overarching norms are fallible: future discoveries could certainly lead scientists to give up on the idea that all their inquiries should be seen, ultimately, as part of one unified enterprise, as part of one collective endeavour to predict and explain our experiences in a systematic way. The purpose of the first part of this chapter, however, has been to argue that no such radical revision is needed in order to accommodate the pluralistic naturalists' observation that different sciences make use of different norms.[30]

6.7 HAACK'S ARGUMENT

Let us turn to a second way in which present-day philosophers have urged for a more open-minded naturalism. Where the pluralists urge for more open-mindedness toward various types of inquiry by arguing against Quine's holism, there is also a movement which argues against Quine's supposedly strong scientistic version of naturalism, a version which leads him to dismiss many legitimate types of inquiry as unscientific.

In the second half of this chapter, I discuss and evaluate Susan Haack's argument for a more open-minded naturalism in

29 Recall that for Quine science is "neither discontinuous *nor monolithic* [but] variously jointed, and loose in the joints in varying degree" (EESW, 1975c, 230, my emphasis). See section 5.5.

30 This is something Putnam also recognizes. For his adapted boat metaphor continues as follows: in the fleet of boats "people are passing supplies and tools from one boat to another and shout advice and encouragement (and discouragement) to each other [...] Its all a bit chaotic; but since it is a fleet, no one is ever totally out of signalling distance from all other boats. There is, in short, both collectivity and individual responsibility" (Putnam, 1981, 204).

this second sense.[31] In a series of papers, Haack has argued that Quine's naturalism, at least when applied to epistemology, contains a "deep-seated and significant ambivalence" (1993b, 353). According to Haack, Quine unconsciously shifts between two notions of 'science'. In some places, Quine seems to use the notion broadly, referring to empirical inquiry in general, whereas in other places he speaks narrowly about the 'natural sciences'. When Quine uses the notion broadly he seems committed to a modest variant of naturalism, arguing that we may freely use our best scientific theories in resolving the traditional problems of epistemology. Yet when Quine defines naturalism using the narrow notion of science, Haack argues, he is committed to an implausible "revolutionary scientism" in which traditional epistemological problems are rejected as illegitimate (1998, 50-1). Because questions about the epistemic status of natural science cannot be answered from within natural science itself, Quine seems forced to maintain that such questions are ill-conceived, and hence to presuppose that the natural sciences have a privileged epistemic status.

Haack's alternative to Quine, her modest 'aposteriorist' variant of naturalism, *does* acknowledge the legitimacy of those epistemological questions. She argues that natural science is "epistemologically distinguished", not privileged, "the point being that distinction, unlike privilege, is earned" (1997, 497). In terms of the boat metaphor we could say that where the pluralists wanted to *split up* Neurath's ship into a fleet of self-sufficient boats, Haack argues that the ship should be *expanded*. That is, where Quine often pictures the naturalist as a busy sailor on the restricted raft of natural science, Haack views the naturalist as a passenger on a giant cruise ship, a multi-

31 See also, for example, Stroud (1996), Almeder (1998), and De Caro and Macarthur (2004a). Since Haack presents the most detailed argument and because Quine has responded to Haack on two occasions, I mostly limit myself to the Quine-Haack debate.

chambered ocean liner in which the natural sciences only encompass a few compartments. Quine cannot answer questions about the epistemic status of natural science because there is no external perspective from which to inspect his raft. Haack's aposteriorist naturalism, on the other hand, *does* allow such questions because her ship contains compartments with windows looking out on the cabins of natural science.

On two occasions, Quine has responded to Haack, arguing that he *is* committed to a broad notion of science and hence a modest variant of naturalism. Yet in the light of Haack's forceful arguments, Quine's replies appear somewhat disappointing. Quine does not attempt to dissolve the supposed ambiguity in his position, nor does he respond to Haack's objection that his naturalism wrongly grants natural science a privileged epistemological status (CH, 1990b, 128). If anything, his replies seem to confirm Haack's diagnosis. For immediately after claiming that he intends to use 'science' "[v]ery broadly almost always", he argues that he sees "epistemic standards as internal to the *natural* sciences" (RTE, 1997a, 225, my emphasis).

In what follows, I formulate a more satisfying response to Haack on Quine's behalf. I argue (1) that Quine does not believe the natural sciences to be epistemically privileged, and (2) that this claim need not conflict with his dismissal of some traditional epistemological questions as illegitimate. I argue that Haack and Quine have been talking past each other because they have different conceptions of what constitutes a traditional epistemological question. Once this mutual misunderstanding has been cleared up, Quine's naturalism will turn out to be more modest, and hence less scientistic, than it might have appeared at first sight.

The remainder of this chapter is structured as follows. After having introduced Haack's objections to Quine's ambivalent ideas about naturalism (section 6.8), I argue that Quine is genuinely committed to a modest variant of naturalism. My

argument proceeds in three steps. First I show that Quine misunderstood Haack's objection and that his responses need not be interpreted as confirming her diagnosis (section 6.9). Second, I argue that Quine's epistemology should not be described as scientistic, given his ideas about justification and the relation between science and common sense (sections 6.10-6.11). Third, I argue that Quine's rejection of some traditional epistemological questions makes perfect sense even from within Haack's modest aposteriorist perspective (section 6.12).

6.8 APOSTERIORISM VS. SCIENTISM

A natural starting point for our discussion is Haack's taxonomy of naturalisms in epistemology. Haack distinguishes between two notions of 'science' and categorizes the different ways in which we might say science and epistemology are continuous.[32] Haack's first notion of 'science' tracks the way in which the concept is most commonly used in the English-speaking world and is roughly coextensional with the *natural* sciences.[33] The second notion of 'science', on the other hand, refers to our empirical beliefs in general, including, next to the natural sciences, also our everyday empirical beliefs, history, and the humanities. This second notion, in other words, refers to our "web of empirical beliefs" in general (Haack, 1993a, 172). In order to distinguish between the two notions, Haack dubs the first narrow usage '*science*' and the second broader usage 'SCIENCE'.

32 Haack is not the only philosopher to have structured the way in which epistemologists use the term 'naturalism'. Maffie (1990), for example, gives an overview of six different kinds of 'continuity' to which naturalists have appealed.

33 It should be noted that Haack's first notion also includes the cognitive sciences and might therefore not be completely identical to "*natural* science" as usually defined. I take it that Haack is just following Quine here, who also lists psychology as one of the natural sciences. See (EN, 1969a, 83).

Haack uses her *science*-SCIENCE distinction to differentiate between three variants of naturalism of increasing strength:

> (RFA) *Reformist aposteriorist naturalism*: the thesis that traditional problems of epistemology can be resolved within SCIENCE.
>
> (RFS) *Reformist scientistic naturalism*: the thesis that traditional problems of epistemology can be resolved within *science*.
>
> (RVS) *Revolutionary scientistic naturalism*: the thesis that traditional problems of epistemology are illegitimate or misconceived, and should be abandoned, to be replaced by questions of *science* (ibid., 167-8).

RFA and RFS are both reformist because they propose to replace the traditional a priori *methods* of epistemology by the methods of either SCIENCE or *science*, whereas RVS is revolutionary because it proposes to abandon the traditional epistemologists' *problems*. RFS and RVS are both scientistic because they make epistemology an enterprise internal to *science*, whereas RFA does not, positioning epistemology more broadly within the web of empirical belief.[34]

Haack herself defends RFA, and justifies her position using Quine's holistic picture of inquiry:

> reformist, aposteriorist naturalism is a straightforward consequence of [...Quine's] gradualist conception of phi-

34 It should be noted that Haack's catalogue of naturalisms is substantially richer than presented here. For one thing, Haack also identifies two variants of naturalism that are weaker than RFA. Second, for all variants of naturalism defined above, Haack differentiates between a narrow and a broad version depending on whether the thesis applies to all or only some problems of traditional epistemology. Because these further distinctions are not relevant for assessing the differences between Haack and Quine, however, I have left them out in my overview.

> losophy as differing only in degree of generality and abstraction, not in the metaphysical or epistemological status of the truths it seeks, from the natural sciences. (Haack, 1993a, 171)

According to Haack, in other words, Quine's holism only supports the thesis that epistemology, metaphysics, and science are epistemically on a par, not that epistemology and metaphysics are projects *within* science narrowly conceived. They are all projects within SCIENCE, not *science*.[35] Still Haack complains, Quine himself often seems to defend a variant of naturalism that is closer to RFS or RVS; for example, when he in "Epistemology Naturalized" proposes to abandon traditional epistemology and settle for psychology: "Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science" (EN, 1969a, 82).

So how might we explain the gap between Quine's holistic picture of inquiry, which only justifies RFA, and his flirtations with stronger variants of naturalism? According to Haack, Quine shifts from RFA to RFS and even RVS due to his ambiguous use of the notion of 'science':

> Here is a sketch of my diagnosis [...] Quine uses the term 'science' ambiguously, sometimes in the usual sense, to refer to those disciplines ordinarily classified as sciences, sometimes in a broader sense, to refer to our presumed empirical knowledge, generally [...] This explains how Quine shifts, apparently unselfconsciously, from aposteriorism to scientism. Because the traditional problems of epistemology do not lend themselves readily or obviously to resolution within the psychological or biological sciences of cognition, however, Quine then finds himself,

35 See also Strawson (1990, 310), who, in response to Quine's claim that philosophy is continuous with science, remarks "'continuous with', not 'identical with'". Haack approvingly cites Strawson's comment in her (1998, 50).

> in his scientistic mood, under pressure to shift and narrow the questions with which he is concerned—to such a point that continuity with the familiar questions of epistemology is broken, and Quine finds himself tempted to cast doubt on the legitimacy of the old projects. This explains how Quine shifts, apparently unselfconsciously, from a reformist to a revolutionary stance. (Haack, 1993a, 171-2)

Haack, in other words, argues that Quine first moves from RfA to RfS because he fails to differentiate between SCIENCE and *science*, speaking about the natural sciences where he should have spoken about our empirical beliefs in general. Quine is then pressed to shift from RfS to RvS because some traditional epistemological problems obviously cannot be resolved within *science*.

Haack illustrates her diagnosis by means of Quine's argument against scepticism, an argument that is supposed to show that sceptical worries are relatively innocent because they arise from within science.[36] Haack argues that Quine's argument is ambiguous precisely because he shifts in the ways identified above. First, Quine moves from a claim about empirical knowledge in general to a claim about natural science. Consider, for example, the following passage:

> Doubt prompts the theory of knowledge, yes; but knowledge, also, was what prompted the doubt. Scepticism is an offshoot of science. The basis for scepticism is the awareness of illusion, the discovery that we must not always believe our eyes. Scepticism battens on mirages, on seemingly bent sticks in water, on rainbows, afterimages, double images, dreams. But in what sense are these illusions? [...] Illusions are illusions only relative

36 See section 2.5.

> to a prior acceptance of genuine bodies with which to contrast them [...] Common sense about bodies is thus needed as a springboard for scepticism [...] Epistemology is best looked on, then, as an enterprise within natural science.(NNK, 1975d, 257-8)

In claiming that scepticism is an offshoot of 'science', Haack argues, Quine seems to be referring to SCIENCE, since he equates the term with both "knowledge" and "common sense about bodies". Yet he concludes that epistemology is to be seen as an enterprise within "natural science". As a result, Quine shifts from the thesis that the philosopher requires SCIENCE as 'a springboard for scepticism' to the conclusion that scepticism is problem within *science*. Quine here, in other words, shifts from RFA to RFS.

According to Haack, Quine's failure to distinguish between SCIENCE and *science* here, leads to an ambiguous position about the legitimacy of the traditional epistemological problem posed by the sceptic. In "Things and Their Place in Theories", for example, Quine defends the reformist view that "[r]adical scepticism [...] is not of itself incoherent", granting the legitimacy of the traditional epistemological problem (TTPT, 1981d, 21). From within our broad empirical web of knowledge (from within SCIENCE) this position makes sense. After all, Quine's sceptic uses common sense experiential knowledge about illusions to claim that we might be mistaken about other knowledge as well, which seems perfectly legitimate. Quine, in these moods, only argues that "the skeptic may be seen [...] as overreacting when he repudiates knowledge across the board" (ibid.), which seems fair given that the sceptic himself presupposes SCIENCE in getting his argument of the ground. Yet on the very same page of his paper, Quine also seems to claim that scepticism *is* an incoherent position:

> Transcendental argument, or what purports to be first philosophy, tends generally to take on [the] status of immanent epistemology insofar as I succeed in making sense of it. What evaporates is the transcendental question of the reality of the external world. (ibid.)

In this passage, Haack argues, Quine does *not* seem willing to concede that scepticism is coherent. Rather, he claims that the very question of the reality of the external world 'evaporates' within a naturalized epistemology. According to Haack, Quine's move here is exemplary of the second shift distinguished above (i.e. from RFS to RvS): because the problem of scepticism does not make sense from within *science*, he is pressed to deny the legitimacy of the sceptical problem: "From the point of view of scientistic naturalism, Cartesian hyperbolic skepticism must be ruled senseless" (1993b, 343). Haack's reformist aposteriorist naturalism, on the other hand, can maintain that sceptical doubts are legitimate, while using Quine's diagnosis that the sceptic is overreacting because he himself presupposes SCIENCE.

Haack's problem with Quine's scientistic variant of naturalism, therefore, is that it cannot do justice to the traditional epistemologists' problems. This is especially clear when one considers the question whether natural science has a special epistemic status. According to Haack, this question "seems patently legitimate" but it is "implausible to suppose" that it can be settled "exclusively by or exclusively within *science*" (1993a, 187). For examining the epistemic status of *science* from within *science* is just as circular as examining the epistemic status of a Sacred Text by reference to that Text itself, or so Haack argues:

> Qua scientistic naturalist [...] Quine can allow no *supra-scientific* tribunal; he can only inform us that [*science*] is certified by *science* itself. Reflecting, however, that that what is said in the Sacred Text is true may be certified

> as true by the Sacred Text itself, one realizes that this is scarcely the reassurance for which one hoped. (Haack, 1993b, 250)

Because scientistic naturalism does not allow a perspective outside *science*, Quine seems committed to the claim that *science* is epistemically privileged; he cannot but presuppose the legitimacy of *science* from the outset. This problem does not seem to arise for the modest aposteriorist naturalist, RFA above, however. Haack *can* appeal to a perspective outside *science* (i.e. SCIENCE) in order to examine the epistemic status of *science*. And from within this broader perspective, she does not have to conclude that *science* is privileged:

> *science* has had spectacular successes, has come up with deep, broad and detailed explanatory hypotheses which are anchored by observation and which interlock surprisingly with each other. [...] *Science*, as I see it, has done rather well, by and large, at satisfying the criteria by which we judge the justification of empirical beliefs [...] These criteria are not internal to, nor restricted to, *science*, they are the criteria we use in appraising the evidence for everyday empirical beliefs as well. (Haack, 1993a, 187)

The aposteriorist naturalist, in other words, can appeal to her SCIENTIFIC criteria of good evidence to explain why *science* has been so successful; she does not, like Quine, have to *presuppose* that *science* is epistemically privileged.

6.9 QUINE'S REPLIES

Haack's arguments seem to pose a dilemma for Quine. Either he has to abandon his identification of epistemology with psychology and stick with an aposteriorist variant of naturalism or he has to explain why a stronger variant of naturalism does

not commit him to the claim that *science* is epistemically privileged.[37] In his responses to Haack, however, Quine (CH, 1990b; RTE, 1997a) seems to vacillate between these two options. On the one hand, it looks as though Quine commits himself to an aposteriorist variant of naturalism. In his first reply, for example, he ignores Haack's claim that "Epistemology Naturalized" seems "to include reformist as well as revolutionary elements" (Haack, 1990, 112), and argues that he is "happy" to be classified as an aposteriorist reformist naturalist (CH, 1990b, 128). Similarly, in his second reply, Quine waves away Haack's allegation that he uses "'science' ambiguously" (Haack, 1997, 503), by claiming that he intends 'science' "[v]ery widely almost always—even including history". Yet in response to Haack's explicit question whether he sees epistemic standards as internal to the *natural* sciences, on the other hand, Quine answers affirmatively: "yes, as engineering is" (RTE, 1997a, 255).

As a result, Quine's responses can be interpreted as confirming Haack's diagnosis that his ideas about naturalism are ambivalent. In this interpretation, Quine is committed to an implausibly strong scientism, but when push comes to shove he appeals to a broad notion of 'science' and a modest variant of naturalism in order to escape the unwelcome consequences of his position. In what follows, however, I argue that this interpretation is misguided and show that a more charitable reading is possible; a reading in which Quine, like Haack, is genuinely committed to an aposteriorist naturalism.

As a first step toward this alternative interpretation, let me dissolve the apparent inconsistency in Quine's replies to Haack. The key to understanding Quine's answer to Haack's question

37 There is also a third option. Quine could also bite the bullet and maintain that natural science *is* epistemically privileged. I do not believe that this is an attractive option for Quine, however. Quine does not allow that our ideas about science are unempirical dogmas; they should themselves be based on science. See section 4.3.

about the source of our epistemic standards, is his reference to engineering. This indicates that Quine has misunderstood Haack's question. For Quine has always used his engineering analogy in response to a slightly different objection, viz. the complaint that his naturalized epistemology cannot account for our epistemic standards *at all*. Consider, for example, Quine's response to Morton White's (1986) suggestion that his naturalized epistemology has no way to account for our normative epistemic standards:

> Naturalization of epistemology does not jettison the normative and settle for the indiscriminate description of ongoing procedures. For me normative epistemology is a branch of engineering. It is the technology of truth-seeking, or, in a more cautiously epistemological term, prediction. Like any technology, it makes free use of whatever scientific findings may suit its purpose. [...] There is no question here of ultimate value, as in morals; it is a matter of efficacy for an ulterior end, truth or prediction. The normative here, as elsewhere in engineering, becomes descriptive when the terminal parameter is expressed.[38] (RMW, 1986h, 664-5)

In this passage, Quine does not use the engineering analogy to show that epistemic standards are internal to *science* instead of SCIENCE like he seems to do in his response to Haack. After all, in the passage he merely claims that a naturalist may make 'free use' of scientific data. Rather, he uses it to show that normative epistemology is not lost in the naturalization process.[39]

When Quine, in his response to Haack, confirms that epistemic standards are internal to the natural sciences 'as engineering is', therefore, he seems to misinterpret Haack's ques-

38 Quine uses the engineering analogy for the same purposes in (IKL*, 1980c, 19), (AT*, 1987a), and (PT, 1990g, 19).

39 How Quine saves normative epistemology will be discussed in section 6.11.

tion. Haack asks whether our epistemic standards are internal to *science* or SCIENCE, but Quine interprets the question as one about *whether* the naturalized epistemologist can account for epistemic standards *at all*. This reading is confirmed in *From Stimulus to Science*, where Quine explicitly claims that normative epistemology is a project within SCIENCE, not *science*: "A normative domain within epistemology survives the conversion to naturalism [...] Normative epistemology is the art or technology *not only of science, in the austere sense of the word, but of rational belief generally*" (FSS, 1995b, 49-50, my emphasis).

6.10 SCIENCE VS. *science*

I have argued that Quine's replies to Haack need not confirm her diagnosis that his ideas about naturalism are ambivalent. His responses are also compatible with an interpretation in which he *does* fully subscribe to an aposteriorist variant of naturalism. In what follows, I argue that this reading is the only correct one. To do so, I reconstruct what would have been Quine's response if he had interpreted Haack's question correctly. As I will make clear, Quine's work unambiguously shows that he would have agreed with Haack that epistemic standards are internal to SCIENCE and therefore that he does not believe *science* to be epistemically privileged. My argument proceeds in two steps. In the present section, I argue that, although Quine often sloppily uses the two notions of science interchangeably, his epistemology cannot be described as scientistic on the basis of his ideas about the relation between SCIENCE and *science*. In the final two sections, I show that when we apply our conclusions from chapter 2, Quine's revolutionary talk of abandoning certain epistemological problems as being misconceived makes perfect sense even from within an aposteriorist naturalism.

Let me start with Quine's ideas about the relation between SCIENCE and *science*. Throughout his career, Quine has always

emphasized that the difference between the two is one of degree and not kind. The evidential standards of *science*, Quine argues, are nothing more than a refinement of the norms we all use in our everyday inquiries:

> science is itself a continuation of common sense. The scientist is indistinguishable from the common man in his sense of evidence, except that the scientist is more careful. This increased care is not a revision of evidential standards, but only the more patient and systematic collection and use of what anyone would deem to be evidence. If the scientist sometimes overrules something which a superstitious layman might have called evidence, this may simply be because the scientist has other and contrary evidence which, if patiently presented to the layman bit by bit, would be conceded superior. (SLS, 1954b, 233)

Quine does not believe in a special *scientific* method. Rather, the norms of *science* are continuous with the methods that guide us in our everyday inquiries.[40] Given Quine's genetic approach to epistemology, this conclusion should come as no surprise. For if Quine is right that "the evidential relation is virtually enacted [...] in the learning", that there is "a partnership between the theory of language learning and the theory of scientific evidence" (NNK, 1975d, 264), then our best *scientific* methods are literally an outgrowth of the way in which we have all learned our common sense theories of the world. After all, in learning our language, we all first start by acquiring a theory of the world in which everything consists of middle-sized physical objects. It is only later in life that the scientist gets "ahead of common

40 See also, for example, Quine's (WO, 1960b, 3): "science is self-conscious common sense"; his (NK, 1969b, 129): "[science] [...] differs from common sense only in degree of methodological sophistication"; and his (WIB, 1984d, 310): "science is refined common sense".

sense" by introducing "system into his quest and scrutiny of evidence", thereby broadening "the knowledge which the man in the street already enjoys, in moderation, in relation to the commonplace things around him" (SLS, 1954b, 229, 233).

Quine, therefore, has no incentive to strictly distinguish between SCIENCE and *science*. Indeed, whenever he reflects explicitly about his notion of 'science', he always claims that he intends to use it broadly.[41] So how then, might we explain Quine's vacillation between broad and narrow uses of science when he is not explicitly contemplating his use of the notion? Ironically, the explanation is provided by Haack herself. Recall that Haack grounds her aposteriorist naturalism on Quine's 'gradualist conception of philosophy as differing only in degree of generality and abstraction [...] from the natural sciences'. Now, according to Haack, it is this very gradualism which "disinclines [Quine] to attach much significance to the distinction between the broader and the narrower use" (Haack, 1993a, 171):

> Gradualism is the thesis that philosophy is essentially like, is continuous with, empirical inquiry generally; since the natural sciences constitute a major and central part of such inquiry, gradualism highlights the similarity in method and purpose between philosophy and the natural sciences. It thus encourages Quine to use the term "science", which ordinarily refers to those disciplines classified as natural sciences, as a convenient way of referring to our beliefs about the world, quite generally. (Haack, 1993b, 339)

In other words, it is because *science* has so much in common with our ordinary empirical inquiries, that Quine has no reason

41 Besides the claim in his response to Haack, he also commits himself to a broad notion of 'science' in (FSS, 1995b, 49), (NLOM, 1995c, 462), and in a letter (May 31, 1988) to Hookway (QHWC*, 1988), in which he protests against the narrow conception of science that is ascribed to him in Hookway (1988).

to draw a strict distinction between *science* and SCIENCE. Our common sense theories about the world are, for Quine, already primitive scientific theories.[42]

6.11 A DEFLATIONARY THEORY OF JUSTIFICATION

Quine's epistemology, therefore, cannot be described as scientistic on the basis of his ideas about the nature of *science*. According to Quine, there is no reason to distinguish strictly between our ordinary empirical and our natural scientific inquiries. So far so good. Yet Haack's objection to Quine still stands. Haack's problem with Quine's naturalism is that he often seems to dismiss some legitimate epistemological questions as being misconceived. For example, when he proposes to "settle for psychology" and to examine how we in fact construct our scientific theories out of our sensory evidence (EN, 1969a, 75), or when he writes that his naturalized epistemology "evaporates [...] the transcendental question of the reality of the external world" (TTPT, 1981d, 22). So even if Quine has all the *resources* for a modest aposteriorism, he still seems committed to the revolutionary claim that some traditional problems of epistemology *are* illegitimate. In this section and the next, however, I show that this final matter of contention can be dissolved as well. Building on my conclusions from chapter 2, I argue that a closer examination of what Quine does and does not reject when he dismisses 'traditional epistemology' will show that his revolutionary claims also make perfect sense from an aposteriorist perspective.

Let me first examine what Quine is *not* dismissing when he claims that we should settle for psychology. In naturalizing epistemology, Quine is not abandoning traditional questions

42 Indeed, Quine often speaks about our ordinary object-based ontology as "rudimentary physical science". See, for example, (NNK, 1975d, 258) and (FSS, 1995b, 15).

about justification. That is, he does not *replace* normative epistemology with descriptive psychology. Quine's epistemology remains deeply normative. For not only has he always explictly insisted that the "[n]aturalization of epistemology does not jettison the normative" (RMW, 1986h, 664-5), also in his general theorizing he has shown how deeply concerned he is with the norms that guide us in our scientific theorizing beyond the strictures of logic.[43]

How we should think about Quine's normative epistemology becomes clear when we take seriously his commitment to a radically science-immanent perspective, i.e. his commitment to the view that "we can never do better than occupy the standpoint of some theory or other, the best we can muster at the time" (WO, 1960b, 22). It is generally recognized that this commitment leads Quine to adopt a deflationary theory of truth.[44] Until quite recently, however, most scholars failed to recognize that this commitment also leads Quine to adopt a *deflationary theory of justification*, i.e. a theory which does not seek a substantive extra-scientific explanation for the justification of some of our statements beyond their being included in or excluded by our best scientific theory of the world.[45]

Rather than interpreting Quine as offering a deflationary theory of justification, scholars usually interpret him as *rejecting* any talk of justification whatsoever as his project of examining how theory and evidence are actually related is thought to be incompatible with any normative programme. What is overlooked, therefore, is that this project *is* compatible with a deflationary notion of justification, a notion according to which facts about how we *actually* construct theory from evidence coincide

43 See the sections 6.9 and 5.3 respectively.

44 See section 3.6.

45 See, for example, Johnsen (2005), Gregory (2008), Ebbs (2011b), and R. Sinclair (2014). Ebbs aptly dubs Quine's theory a "minimalist understanding of justification" (p. 630).

with facts about how we *should* do this. When Quine defines naturalism as the view according to which our best scientific theories "are not in need of any justification *beyond* observation and the hypthetico-deductive method" (FME, 1975a, 72), he is not rejecting talk about justification; he is only rejecting substantive extra-scientific theories of justification. Our best scientific theories are not in need of any extra-scientific justification because they are already justified *in virtue of* their being our best scientific theories.[46]

6.12 'TRADITIONAL EPISTEMOLOGY' REVISITED

Returning to Haack's accusation that Quine rejects some legitimate epistemological questions, we see now that this is largely a mistake. Quine does not abandon normative epistemology; he only rejects traditional epistemology when it is interpreted as first philosophy; i.e. when it is interpreted as a SCIENCE-independent inquiry, taking place outside our empirical web of belief, and aiming to justify SCIENCE from a SCIENCE-independent perspective:

> I think of the basic tenet [of naturalism] as a negative one, namely that we can't hope for any evidence, any avenue to truth higher than or more fundamental than ordinary scientific method itself. The method of prediction, and then experimental testing of the predictions. So that the traditional epistemological quest for something firmer than science that would serve as a justification of

46 See also Johnsen (2005, 88): "so far is [Quine] from proposing to abandon the normative that he is proposing instead to *discover* the norms that govern theorizing by discovering the norms that we conform to in our theorizing [...] What he is [...] proposing is to *enlist the aid of psychology* in addressing the burden of epistemology: psychology will identify the norms we adhere to, and philosophy will tell us that, *by virtue of* their being the ones we adhere to, they are the ones we *are to* adhere to".

> scientific method is dismissed as a mistake. (IWVQ, 1994c, 71-2)

Quine has no problem with any epistemological project in particular. He only rejects the idea that these projects can be carried out in a SCIENCE-independent fashion and ought to result in a substantive notion of justification.

As we have seen in section 6.8, Haack accuses Quine of both allowing and dismissing sceptical questions, sometimes even on the very same page. Now let us re-examine his claims with the above interpretation in mind. When Quine argues that his naturalized epistemology "evaporates the transcendental question of the reality of the external world" (TTPT, 1981d, 22), we should note that he is explicitly talking about a *transcendental* question. And when he, on the same page, argues that "radical scepticism [...] is not of itself incoherent", he again qualifies his claim by adding that these sceptical doubts "would still be *immanent*, and of a piece with the scientific endeavour" (ibid., my emphasis). Quine, in other words, allows scepticism when it is interpreted immanently, but dismisses it when it is interpreted transcendentally. When the sceptic argues that there is no absolute SCIENCE-independent foundation for science, no Cartesian certainty, Quine can simply dismiss the argument as illegitimate. After all, his naturalism implies that the very idea of such an external vantage point is a mistake. But when the sceptic admits that her doubts about science arise from within our empirical web of beliefs, because science seems "vulnerable to illusion on its own showing" (ibid.), Quine no longer has any reason to believe that the sceptic's questions are incoherent.[47]

In dismissing 'traditional epistemology', therefore, Quine is primarily rejecting traditional *interpretations* of epistemology according to which it is the philosopher's task to validate science from some science-independent perspective. Quine's ideas, on

47 See section 2.5.

this reading, are perfectly consistent with Haack's modest variant of naturalism. After all, as we have seen, Haack's aposteriorism too is based on Quine's "conception of philosophy as differing only in degree of generality and abstraction, not in the metaphysical or epistemological status of the truths it seeks, from the natural sciences" (Haack, 1993a, 171). In rejecting the traditional conception of epistemology, in other words, Haack too claims that there is no SCIENCE-independent foundation for SCIENCE. Indeed, when Haack reflects on the possibility of vindicating her own criteria of justification, she argues that her attempts will always be conditional on what she herself takes "to know about human subjects and their cognitive abilities and limitations". The best any naturalist can do is to investigate "the satisfactoriness of our criteria of justification from within the web of belief" (Haack, 1990, 122-4).[48]

When Haack and Quine discuss the latter's revolutionary proposals, therefore, they are talking past each other. Quine's claim that some problems of 'traditional epistemology' are illegitimate does not constitute a rejection of traditional epistemological talk about justification and scepticism. Rather, he dismisses traditional *interpretations* of justification and scepticism, interpretations which Haack, as a naturalist, dismisses herself as well. More schematically, any theory about justification will be (1) a *scientific* theory, (2) a SCIENTIFIC but not a *scientific* theory, or (3) a first philosophical theory. Haack interprets Quine as arguing against the idea that epistemological theories can be of type (2) and as rejecting the traditional problems of justification

48 As a result, Haack's comparison of the claim that we should examine the epistemic status of *science* from within *science* itself with the claim that we should examine the epistemic status of a Sacred Text by reference to that Text itself (1993b, 250) is not entirely fair. For Haack's position itself implies that we ought to examine the epistemic status of SCIENCE from within SCIENCE itself. It is perhaps because of this reason that Haack's 'Sacred Text-argument' is omitted from her (1993a, ch. 6), even though this chapter largely resembles her (1993b) in other respects.

and scepticism because there is no plausible reinterpretation of those problems within *science*.[49] But in fact, Quine is merely arguing against the idea that epistemological theories can be of type (3), a claim with which not even a modest naturalist in Haack's sense will disagree.[50]

This mutual misunderstanding about what defines 'traditional epistemology' is probably best explained by the different contexts in which Quine and Haack conducted their inquiries. In much of his early work, Quine was partly out to defend his naturalism against a conception of epistemology in which "the epistemologist's goal is *validation* of the grounds of em-

49 That it is a mistake to think that Quine sees philosophical problems as problems within *science* also becomes clear in the Fara interviews. In response to Horwich's suggestion that he "think[s] of philosophy as part of natural science", for example, Quine explicitly denies that the characterization is correct: "No, that is not the characterization I intend. I may occasionally say things which sound very much like that, or even exactly like that. But what I mean is that philosophy is *continuous with* science" (ICQ*, 1993a, my emphasis).

50 Of course Haack and Quine can disagree about the details of Quine's deflationary theory about justification. Just as there are many deflationary theories of truth, there are many ways in which one might set up an SCIENCE-immanent theory of justification. In broad outlines, however, Quine and Haack agree. In *Evidence and Inquiry*, Haack (1993a) develops what she calls a "foundherentist" notion of justification; an epistemological theory intermediate between foundationalism and coherentism which, according to her, "can combine their strong points and avoid their weaknesses" (1993a, 13). In his response to Haack, Quine explicitly expresses his sympathy with foundherentism:

> Susan Haack's classification [...] recognizes my foundationalism, which consists in my appreciation (it can scarcely be called a thesis) that the checkpoints of beliefs are sensory observations [...] On the other hand my coherentism is evident in my holism, however moderate. So I do indeed combine foundationalism with coherentism, as I should think it evident that one must. (CH, 1990b, 128)

See also Quine's letter (December 6, 1993) to Haack, in which he claims that her "blend of foundationalism and coherentism (spare me the word!) strikes me as the way to go" (QHC*, 1975-1997, my transcription).

pirical science" (EN, 1969a, 75, my emphasis). His main concern, therefore, was the distinction between theories of type (1) or (2) on the one hand, and theories of type (3) on the other. Haack, on the other hand, wrote *Evidence and Inquiry* in the early 1990s, when broadly anti-Cartesian interpretations of epistemology were already commonplace.[51] She primarily intends to defend her ideas about the nature of philosophy against approaches that have become too sceptical about epistemology.[52] In other words, she takes Quine's rejection of type-(3) statements for granted and focuses more narrowly on the distinction between statements of type-(1) and type-(2), a distinction to which Quine is not sensitive given his ideas about the relation between *science* and SCIENCE (section 6.10). Because Quine and Haack have different distinctions in mind, they are talking past each other. Quine misunderstands Haack's question about the source of our epistemic standards as a question about whether the naturalized epistemologist can account for epistemic standards in terms of either type-(1) or type-(2) statements; and Haack misunderstands Quine when he abandons certain type-(3) statements as misconceived.

Now this mutual misunderstanding is cleared up, we can see that, although Quine and Haack's epistemologies differ in emphasis, they have a common enemy: the traditional Cartesian epistemologist who dismisses any attempt to incorporate sci-

51 Indeed, in the early 1990s, Quine claims that he is of "*that large minority or small majority* who repudiate the Cartesian dream of a foundation for scientific certainty firmer than scientific method itself" (PT, 1990g, 19, my emphasis).

52 Next to her chapter on Quine, two other chapters of Haack's *Evidence and Inquiry* are concerned with defending her conception against such sceptics; e.g one chapter is concerned with reliabilist rejections of internalism and one chapter is concerned with Rorty's "vulgar pragmatism". Note that Quine would be critical about these sceptical theories, *qua* theories about justication, as well. For Quine's rejection of Rorty's (1979, 202) claim that his theory is devoid of talk of justification, see (LMAP, 1990f, 151). For a suggestion about how Quine would have responsed to reliabilism, see Johnsen (2005, 92-3).

entific results in our theories of knowledge as circular. Both believe that our theories of justification should be immanent and both reject the circularity charge as being misconceived. There are no external vantage points; even the epistemologist who is interested in ratifying our criteria of justification can only examine and evaluate our epistemic norms from within the broad web of empirical beliefs in which those very norms are employed. Both Quine and Haack, in other words, are modest naturalists.

7

CONCLUSION

The history of twentieth-century analytic philosophy is often depicted as a history of two broadly diverging projects: the formal, science-minded philosophy of the logical positivists and the common-sense philosophy of the ordinary language philosophers. Quine's philosophy, though distinct from positivism in its rejection of the two dogmas, in many respects falls into the former category. Not only are his views undeniably science-minded, Quine also adopts the positivists' rigorous methodology, i.e. their insistence on employing the standards of clarity and precision that are most commonly displayed in the sciences.

Given this coarse-grained classification as well as his strong emphasis on the natural sciences in the construction of a regimented language, it is not surprising that Quine's naturalism is often depicted as some sort of scientism in sheep's clothing, as an implausibly deferential worldview in which every type of inquiry is ultimately evaluated in terms of its merit for the sciences narrowly conceived. Indeed, in contemporary philosophy Quinean naturalism is often identified with a staunch rejection of normative epistemology and with an austerely eliminativist form of physicalism.

In this book, however, I have argued that this is a mistake. I have offered a reading of Quine's naturalism in which the adoption of a radically *immanent* perspective lies at the heart of his philosophy. First and foremost, Quinean naturalism is a rejection of the transcendental, i.e. a dismissal of any philosophy that purports to offer an outside vantage point. Quine

rejects traditional philosophy because it is presupposes an external perspective, not because it is inherently unscientific.

In response to the question as to wherein our perspective should be immanent, Quine's answer, of course, is 'science'. It is partly because of this reason that his naturalism is often interpreted as scientistic. I have argued, however, that such a reading presents Quine's philosophy the wrong way around. Even if we, like Quine, adopt a strictly science-immanent perspective, a question that remains to be answered is how broad our conception of science ought to be. Quine's notion of science, we saw, is quite broad as it encompasses what Haack (1993a, 172) calls our complete 'web of empirical beliefs', including common sense.[1] It is only *after* adopting such a broadly science-immanent perspective that Quine, in constructing his regimented language, starts making choices that many contemporary philosophers have argued to be unduly restrictive. Quine's ideas about the *ultima facie* bounds of science, in other words, are established from within as well (Ricketts, 1982).

When it comes to his *naturalism*, therefore, Quine's position is more closely aligned with ordinary language philosophy than has been often supposed. Even though Quine and the ordinary language philosophers exhibit a difference in style as well as a difference in emphasis—in *Word and Object*, Quine worries that ordinary language philosophers "exalt ordinary language to the exclusion of one of its own traits: its disposition to keep on evolving" (WO, 1960b, 3)—both acknowledge that "we all must start in the middle" and eschew transcendental perspectives (ibid., 4). Indeed, in reflecting on the role of naturalism in twentieth-century analytic philosophy, Quine has claimed that

1 It is therefore not surprising that Quine, when confronted with Strawson's call for a more liberal variant of naturalism, argued that Strawson "wouldn't find the discrepancy" between scientific and liberal naturalism if he had shifted "to a more liberal [...] conception of science" (ICQ*, 1993a).

"certainly the ordinary language philosophers [. . .] are as naturalistic as one could wish" (ICQ*, 1993a).

Reading Quine's naturalism as a radically science-immanent philosophy sheds new light on his arguments against traditional epistemology and metaphysics. I have argued that Quine does not reject the traditionalists' views out of despair but because they crucially presuppose an extra-scientific vantage point. In epistemology, both the sceptic who questions our knowledge about the external world and the empiricist who tries to justify this knowledge by reconstructing it from sense data rely on the viability of such a transcendental perspective. The sceptic presupposes that she can challenge science without presupposing science, whereas the empiricist presupposes that she can answer this challenge by reducing our theory to some science-independent sensory language. In a similar fashion, Quine dismisses traditional metaphysics. For in asking what reality is *really* like, the traditional metaphysicist is asking us to set aside our ordinary perspective on reality.

To be fair, not many traditional epistemologists and metaphysicists will have recognized the way in which Quine has depicted their projects. After all, to a certain extent it is commonplace in philosophy that there are no Archimedean vantage points. Quine's contribution, however, lies not so much in the nature of his critique as in the rigor with which he extinguishes transcendental theorizing. Quine's adoption of an absolute science-immanent approach has led him to dismiss a broad range of widely accepted presuppositions. To name only a few of the examples I have discussed: he has adopted a strictly deflationary theory of truth, linguistic understanding, and justification; and rejected the distinctions between science and the logic of science, between internal and practical external questions, and between science and epistemology. Even if many contemporary philosophers reject transcendental perspectives in theory, in other words, Quine believes that they do not always practice

what they preach. Quine's naturalism is radical not because it is *science*-immanent, but because it is science-*immanent*.

Beside my interpretation of Quine's naturalism, a second major theme in this study has been the interplay between naturalism and holism in Quine's philosophy. The two themes are connected because it is holism that explains *why* Quine adopts a science-immanent approach. For not only do his arguments against traditional epistemology and metaphysics crucially rely on holistic reasoning—according to Quine we cannot make sense of the first philosophers' extra-scientific use of concepts like 'evidence', 'truth' and 'reality'—also in his own development, Quine's growing naturalism goes hand in hand with his gradual adoption of an ever broadening holism. Scrutinizing both his published and unpublished work, I have reconstructed Quine's development and shown how his position evolved from a strictly behaviorist version of empiricism as well as a narrow-scoped holism to a picture of inquiry in which all statements—even our mathematical, ontological, and epistemological statements—are epistemically on a par.

The idea that all statements are epistemically on a par might be understood in two ways: as the view that every statement is always up for revision whenever an observation categorical turns out to be false and as the view that every statement is at least in principle vulnerable to revision. I have argued that Quine, in order to maintain his critique of the science-philosophy distinction, does not require the former view, i.e. does not need the view that "our scientific system of the world [is] involved *en bloc* in *every* prediction (FME, 1975a, 71). Rather, what Quine requires is universal revisability. And indeed, where Quine gave up the former view by adopting a moderate holism, he maintained the latter by showing how all statements, our logical truths in particular, remain revisable even if they are analytic.

The firm connection between holism and naturalism also played a central role in my critical discussion of Maddy. I have argued that Maddy's call for mathematical naturalism—a philosophy of mathematics that aims to do better justice to actual set theoretic practice—is unacceptable precisely because she fails to acknowledge that all statements will ultimately be evaluated in terms of their contribution to science as a whole. Although Quine in this response requires the extra assumption that science is not discontinuous, I have argued that there is no reason to conclude that this assumption should be given up solely because scientists and mathematicians appeal to different norms in their everyday inquiries.

In claiming that Quine's science-immanent approach lies at the heart of his philosophy and in claiming that this approach relies on his holism, I am not implying that this is the only way to understand his views or that naturalism and holism play some sort of foundational role in his philosophy. Quine's antifoundationalism extends to his own views as well; there are no fundamental building blocks from which everything else can be derived. In presenting his philosophy in a systematic way, one could also legitimately start out with his "attack on standard philosophical views about meaning" (Harman, 1967, 124), his "naturalistic-behavioristic thesis of language" (Gibson, 1988, 1), or his logical pragmatist "conception of human beings and human action" (Glock, 2003, 1-2). I have chosen the present approach because naturalism seems to be Quine's most influential legacy and because doubts about his 'taste for desert landscapes' seem to play a crucial role in the popularity of so called liberal variants of naturalism today. In reinterpreting Quine's naturalism, therefore, I have not only aimed to contribute to the developing field of Quine studies, I have also aimed to shed new light on the philosophical presuppositions that underlie today's dominant metaphilosophy.

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SAMENVATTING

Wat is de relatie tussen filosofie en wetenschap? Bestaat er een specifiek filosofische onderzoeksmethode die ons kennis kan brengen die we niet kunnen opdoen in de wetenschap? Is filosofie de moeder aller wetenschappen? Of is filosofie zelf een tak van wetenschap en dient ze haar theorieën te integreren met de belangrijkste resultaten uit de biologie, de natuurkunde en de psychologie?

Gedurende de tweede helft van de twintigste eeuw vindt er binnen de analytische filosofie een 'naturalistische' omwenteling plaats in het denken over deze vragen. Waar analytisch-georiënteerde filosofen van oudsher een strikt onderscheid hanteren tussen de empirische onderzoeksmethoden van de wetenschappen en de logisch-conceptuele analyses van de filosofie, denken de meeste filosofen vandaag de dag dat dit onderscheid op zijn best gradueel is. Er bestaat geen transcendent filosofisch perspectief op de werkelijkheid; natuurwetenschappers, psychologen en filosofen zitten allemaal in hetzelfde schuitje.

Hoewel deze positie geenszins nieuw is, is de hedendaagse populariteit van het naturalisme voor een groot deel terug te voeren op het werk van de Amerikaanse filosoof Willard Van Orman Quine (1908-2000). Als geen ander heeft Quine laten zien (1) hoe we kunnen nadenken over de graduele relatie tussen filosofie en wetenschap, (2) waarom er geen scherp onderscheid gemaakt kan worden tussen empirische en conceptuele kennis, en (3) hoe we traditioneel filosofische domeinen als de metafysica en de epistemologie kunnen ombouwen tot naturalistisch respectabele disciplines.

Ofschoon een groot deel van de hedendaagse analytische filosofie op zijn minst methodologisch gezien schatplichtig is

aan Quines werk, bestaat er geen systematische studie die zich volledig richt op de ontwikkeling van en de filosofische vooronderstellingen in zijn naturalisme. Dit proefschrift poogt hierin verandering te brengen. Op basis van zowel gepubliceerd werk als ongepubliceerde artikelen, notities en brieven uit zijn persoonlijke archief, biedt dit proefschrift een uitvoerige historische reconstructie van de ontwikkeling van Quines naturalisme, een nieuwe interpretatie van zijn argumenten en een systematische analyse van de presupposities die ten grondslag liggen aan zijn positie.

Na een schets van de naturalistische omwenteling in de analytische filosofie (hoofdstuk 1), reconstrueer ik in het eerste deel van dit proefschrift Quines argumenten tegen de traditionele epistemologie (hoofdstuk 2) en metafysica (hoofdstuk 3). Ik laat zien dat Quines argumenten principiëler zijn dan vaak is verondersteld en dat zijn argumenten grotendeels rusten op zijn 'holistische' stelling dat wetenschappelijke hypotheses nooit individueel toetsbaar zijn. De sterke relatie tussen naturalisme en holisme speelt eveneens een belangrijke rol in mijn historische reconstructie van de ontwikkeling van Quines naturalisme (hoofdstuk 4), die laat zien dat de evolutie van zijn positie met betrekking tot de relatie tussen filosofie en wetenschap op cruciale momenten wordt gedreven door de ontwikkeling van zijn holisme.

Nadat heb ik aangetoond op welke manieren holisme een rol speelt in zijn naturalisme, zoom ik in op de vraag hoe we Quines holisme zelf moeten begrijpen (hoofdstuk 5). Ik laat zien dat veel van de theorieën die aan Quine zijn toegeschreven op dit gebied—hoewel controversieel op het eerste gezicht—terug te voeren zijn op relatief onschuldige ideeën over de wetenschappelijke praktijk. Tevens laat ik zien hoe Quine, in zijn latere werk, wijzigingen doorvoert in zijn ideeën over conceptuele en logische waarheden. Ik eindig dit proefschrift met een ontmanteling van twee argumenten die pogen aan te tonen dat

er een fundamentele spanning bestaat tussen Quines holisme en naturalisme (hoofdstuk 6) en een concluderend hoofdstuk waarin ik beargumenteer dat het radicale karakter van Quines positie niet zozeer ligt in zijn focus op het wetenschappelijke karakter van de filosofie, maar in zijn categorische verwerping van transcendentale perspectieven (hoofdstuk 7).

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