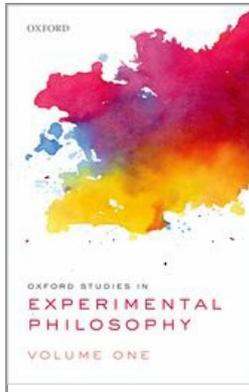


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## Experimental Philosophy\*

1935–1965

Taylor Murphy

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### [–] Abstract and Keywords

In the heyday of linguistic philosophy an experimental philosophy movement was born, and this chapter tells its story, both in its historical and philosophical context and as it is connected to controversies about experimental philosophy today. From its humble beginnings at the Vienna Circle, the movement matured into a vibrant research program at Oslo, and sought adventure at Berkeley thereafter. The harsh and uncharitable reaction it met is surprising but understandable in light of disciplinary tensions and the legacy of antipsychologism—sentiments and arguments which have reemerged today, albeit in modified form. Yet the research at Oslo remained unperturbed and it flourished in both its theory and its applications, which spanned the philosophical domain. The Berkeley years were short but intense, as exemplified by their engagement with ordinary-language philosophy, J.L. Austin, and the theory of communicative significance.

*Keywords:* empirical semantics, psychologism, intuition, methodology, interpretation, Arne Naess, Herman Tennessen

### Introduction

In the heydays of linguistic philosophy, a group of Norwegian philosophers saw that philosophers often relied on intuitions when speaking about the conceptual commitments and linguistic uses of ordinary people, despite enduring disagreement on the matter. Then there was the critical insight: “When philosophers offer conflicting answers to questions that have empirical components, empirical research is needed” (Naess, 1953a, p. vii). Calling themselves *empirical semanticists*, this group stood opposed to the “anti-empirically oriented armchair philosopher” and pointed “to the possibility of an ‘experimental philosophy’” (Tennessen, 1964, p. 290; Naess, 1938a, p. 161). They argued that it is “hardly sufficient that a single person registers his own reactions to this or that sentence, or makes pronouncements based on intuitions, or undertakes scattered observations of others’ usage” (Gullvåg, 1955, p. 343). And so off they went, testing the claims made by philosophers (**p.326**) and investigating how concepts are understood, defined, and used by ordinary people.

Empirical semantics is deeply similar to today’s experimental philosophy; it was linguistic philosophy’s experimental philosophy. Although the empirical semantics movement is not historically continuous with today’s experimental philosophy movement, empirical semanticists engaged with much the same subject matter, had similar motivations and aims, and encountered characteristic problems and objections at the interface between analytic philosophy and experimental psychology. This chapter spells out these connections and situates empirical semantics within the context of experimental philosophy and analytic philosophy.

Arne Naess led the empirical semantics movement. The most renowned Norwegian philosopher today, Naess was honoured with a state funeral and is well remembered both for pioneering the ecological movement and for his activism in the international peace movement (Stadler, 2009). Naess is also credited with bringing social science methods into Norway, and empirical semantics is considered an intellectual forerunner to sociolinguistics and corpus linguistics (Chapman, 2008, 2011; Thue, 2009). Along with Naess, the central empirical semanticists were Herman Tennessen, a psychologist with interests in logical analysis; Ingemund Gullvåg, a logician; and Harald Ofstad, a legal scholar turned moral philosopher (Ofstad, 1951, pp. 42–3; Tennessen, 1962).

This chapter is organized into three sections, roughly corresponding to three periods of empirical semantics research. I begin with the original inspiration for empirical semantics (around 1935–38), and survey its reception over the years. It grew from Naess’s own interests and his encounters with those he met in Vienna. Naess had an ongoing interest in behavioural psychology and the concept of truth, and gained further motivation to study the notion of truth experimentally in conversation with Tarski and others at the Vienna Circle. The overarching factor was the apparent reliance on intuition when assessing the way that terms are defined, conceived, and used by ordinary people. Naess’s work over the years was met with resistance, largely due to antipsychologistic

(or antinaturalistic) sentiments and arguments that became central to analytic philosophy when it parted ways with experimental psychology around the turn of the century. Empirical semanticists pointed to the **(p.327)** broadly empirical traditional conception of philosophy that existed prior to the rise of analytic philosophy. While the distinction between analytic conceptual analysis and synthetic sciences such as psychology was sensible, it was often used to depreciate empirical research on traditional philosophical topics. My account of what transpired in Vienna and Naess's intellectual development at this stage is drawn largely from his recollections, which were penned after World War II.

In the next section of the chapter I survey the work done by empirical semanticists in Oslo (around 1939–56). I sketch some basic ideas and methods and place this work in its social and political context. Despite the emphasis in sociopolitical topics, there was a vibrant research program on topics in analytic philosophy. The experimental studies here focused on understanding how people conceive of truth, democracy, synonymy, consciousness, and “testability” among physicists, to name just a few. I concentrate on one study of typological concepts for illustration, and note some contrasts with experimental philosophy. Perhaps most striking is that there was no attempt to access analyticities or conceptual truths from the experimental data—it was regular science and not, as it were, prior to it. However, it also embodied a unique analytical and philosophical approach of its own. For my sources here, I began with Tennessen's course textbooks and accounts by the empirical semanticists about their own research, as well as some archival material that includes original studies and data (as cited below). For the illustration on typological concepts, I partially translated some of Tennessen's work from Norwegian and used Tennessen's English summary (now available online).

Lastly, I turn to the period of work that followed at Berkeley (around 1957–61). Whereas the work in Oslo consisted in efforts to understand how people think about specific topics, at Berkeley the focus turned toward explanation of certain patterns of intuitive judgments about language found in analytic philosophy. As destiny would have it, there was a clash of sorts between empirical semanticists and ordinary-language philosophers, though this clash was also quite constructive. Although logical analysis could be distinguished from empirical semantics, ordinary-language philosophy certainly appeared to be about the same subject matter: ordinary language. Some of this work is obscure because it suddenly stopped around 1960, and much of it was done in connection with J.L. Austin (who died in February 1960). This section **(p.328)** draws heavily on archival material from this period, including seminar notes written at Berkeley, and drafts of work (some of which were not published). Again, much of this work is now online or available on request.

### Experimental Philosophy at the Vienna Circle

Much happened in this period, so a brief timeline of Naess's activities is in order (see Stadler, 2009). Naess spent his student years in Paris studying philosophy, psychology, mathematics, and astronomy. In 1933 he completed two Master's theses, one on truth and one on behavioral psychology. This work included some quantitative analysis of the usage of evidential expressions in science, such as “show,” “prove,” and “demonstrate” (Naess, 1933; cf. Overton, 2013). When Naess arrived in Vienna in 1934, he was invited

to participate in Moritz Schlick's Vienna Circle seminars. Here he continued to develop his interests in close conversation with Rudolf Carnap, Charles Morris, Otto Neurath, and Alfred Tarski, among others. Naess wrote his dissertation on the empirical study of scientific behaviour and presented this to the Vienna Circle in March 1936. Naess was also inspired to conduct experiments on the concept of truth, and presented some results at the Third International Congress for the Unity of Science (July 1937, Paris). In parallel he did research with the Viennese psychologist Egon Brunswick—also a participant at the Vienna Circle. Naess was to continue this research in exile in 1938–39 at Berkeley, where he worked in Edward C. Tolman's psychology laboratory.

According to Naess, one central idea of his was influenced by how the Vienna Circle seminars were structured. Participants worked toward gaining agreement on precise formulations of their philosophical positions, revising their formulations when there were diverging interpretations. As he later recalled it, this process led him to think that “we were not quite clear in our heads—that we in a sense were only vaguely aware of what we might be talking about” (Naess, 1993, p. 263). “Their quest for clarity and cordial cooperation in pursuit of knowledge led me to appreciate that ‘What do I mean?’ is an open question” (Naess, 2005a, p. lxiii f). Naess observed considerable shallowness of semantic intention and that there were often surprisingly diverse interpretations for each other's formulations. This led him to argue extensively against the **(p.329)** assumption that propositions were precisely grasped in *actual* thinking and understanding in his dissertation, *Cognition and Scientific Behaviour* (1936).

According to Naess, Vienna Circle participants often appealed to the ordinary, common-sense meanings and uses of words. He recalls finding it perplexing how “the logical empiricists [could] boast about a scientific attitude when they relied so much on intuition when speaking about the use of words” (2005b, p. 199). He was confident that empirical methods could be of use here, as well as for studying language more generally:

I believed that one could purge logical empiricism of its antiempirical tendencies by a program for purely empirical studies of linguistic usage. Precisely such research, without further intentions, seemed to me necessary (1) to counterbalance a form of “logical analysis” that strictly speaking was not logical, and (2) to create the preconditions for the construction of a system of exact concepts intended to cover all empirical fields of importance in the philosophical discussion.

(Naess, 2005b, p. 203)

In the Vienna Circle, Naess and Tarski discussed Tarski's recent work on the concept of truth in formalized languages. One of Tarski's desiderata linked his account to the ordinary concept of truth—his “material adequacy” condition. As Linsky put it:

The requirement of material adequacy is simply the requirement that the definition, once achieved, shall correspond more or less closely with that concept of truth which all of us have in mind before we ever undertake the task of explication.

(Linsky, 1952, p. 1)

Tarski believed that his account (roughly of the form, “p” is true iff p) does indeed “do justice to our intuitions” about truth and conforms with “common-sense usage,” thereby fulfilling his material adequacy criterion (Tarski, 1944). Naess doubted that this was really *the* common-sense notion of truth, as would be revealed by systematic questioning of non-philosophers. However, Naess also had a larger concern, for the Vienna Circle participants were not alone in their habit of referencing the views of non-philosophers as decisive in rejecting another’s formulation or position.

**(p.330)** Naess’s extensive investigations were published in his book “*Truth as Conceived by Those who are Not Professional Philosophers*” and in *Theoria* (Naess, 1938a, 1938b). Here Naess reports on the extent to which philosophers take “a standpoint to what the non-philosophers mean, stating that the theories of truth adhered to by their opponents contradict the basic structure of truth revealed among the non-philosophers” (Naess, 1938a, p. 165). He concluded that no agreement was coming any time soon from the armchair:

Under no conditions can we attribute any value to statements on these matters deduced from general philosophical views or from “intuition.” If one wishes to *know* something about the matter, the traditional methods of attack must be radically and definitely abandoned.

(Naess, 1938a, p. 93)

In his effort to settle things, Naess interrogated and surveyed ordinary people; he asked them to explicitly state what they think truth is, to state what is common to all that is true, to make synonymy judgments, to evaluate others’ definitions; and he tried a variety of other techniques (see Appiah, 2008; Chapman, 2008, 2011; Stadler, 2009). Naess also investigated other factors such as age, gender, suggestibility, and education.<sup>1</sup>

Naess identified some thirty-seven truth theories, including those centering on what is provable, what is arrived at from one’s senses, what is learned, what serves life, what cannot become otherwise, what agrees with all the evidence, and what is agreed upon by consensus. He took this to show that there was just no thing that deserved to be called *the* common-sense theory or pre-philosophic conception of truth; so much, then, for the material adequacy condition:<sup>2</sup>

**(p.331)** It is therefore nonsensical to speak of *the* common sense view of the truth-notion. Equally nonsensical it is to speak of *the* view of the man in the street, of the uneducated, of the prephilosophic mind etc. No philosopher speaks of *the* philosophic view of the truth-notion...this would not, however, be any more ridiculous than to speak of *the* common sense view.

(Naess, 1938a, p. 85)

Instead of research on the ordinary conception of truth, Naess proposed some

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alternative projects: the development and acceptance of scientific hypotheses, the function of maxims and statements in ideological currents, formalization of evidential expressions in science, and conceptual clarification when it is useful to do so. As Naess later summarized his conclusions:

In *“Truth” as Conceived by Those who are Not Professional Philosophers*, I tried to show the inadequacy of intuitive methods employed by philosophers for the purpose of determining how “true” and related terms are conceived, defined and used by ordinary people. The exclusive use of intuitive methods for these purposes tends to result in an underestimation of the diverse trends of reflection among those who are not learned...Dialogues with those who are philosophically uneducated convinced me that acceptance of intuitions reported by the philosophically sophisticated about the verbal and conceptual habits of others leads to confusion and error.

(Naess, 1953a, p. vii)

It is worth highlighting the use of “intuition” by Naess here, as it differs from some current ways of using the term. Now, it was not uncommon to treat conformity with ordinary usage and “our intuitions” as decisive. Naess was certainly not the only one to notice this, nor was he alone in surveying such appeals and proposing empirical investigations. Richard Rudner (an American philosopher at Washington University in St. Louis) did so as well. Rudner surveyed a number of philosophers—Carnap, Goodman, Moore, Hempel—and asked how the intuitions that they appeal to could be justified, weighed against each other, and systematized (Rudner, 1950).

There is an important difference here between appealing to intuitions as such and treating them as evidence of the correctness of one’s analysis, **(p.332)** versus a method of reflecting from the armchair on what others would ordinarily say or think. Rudner (and Goodman) may have been concerned with the former, but Naess only had the latter use in mind. Naess did not view common-sense usage or shared intuitions as having any distinctive, epistemically significant role in philosophical analysis; instead he worried that the reliance on intuitions in making these claims was a source of fruitless controversy. As he put it:

It is not necessary to depart from philosophical pastures in order to see the need for trying out empirical procedures to discover the linguistic uses and conceptual commitments of the man on the street...[For example, see the disagreement in] articles in recent volumes of the periodicals *Mind*, *Analysis* and *Philosophical Review*. I do not contend that these philosophers in all cases *should* have investigated conventional usage by other means than intuition. I merely suggest that empirical procedures should be applied to empirical questions. When philosophers offer conflicting answers to questions that have empirical components, empirical research is needed...If intuitions are used, procedures should be devised by which intuitive results of different, presumably competent people can be compared. If the intuitive results seem to conflict or are difficult to delimit and

express, one should look for methods by which to avoid at least some of the intuitive components of the procedure.

...The kind of activity today referred to by names such as “logical analysis” and “conceptual clarification” is only partly deductive and axiomatic in character. Much of it seems to me to rest on intuitions about one’s own and others’ uses of terms and to contain recommendations or preferences in matters of terminology. The intuitional approach is excellent so long as the agreement in results is of the intersubjective, intercultural kind that characterizes some of the results in the formal or factual sciences. Such agreements, however, have not been obtained.

(Naess, 1953a, pp. vii-x)

The role of agreement with ordinary usage and our intuitions does not appear to amount to anything more than Aristotle’s use of *endoxa* as reasonable starting points in dialectical arguments (Hintikka, 1999). Naess did not regard intuitions and ordinary usage as anything more than conventional points of departure for one’s explications, construction of axiomatical systems, or other theorizing.<sup>3</sup>

### **(p.333)** Reception of empirical semantics in analytic philosophy

Reviews of Naess’s work were strikingly hostile, particularly at first. Briefly: J. Moore criticized Naess for not having “formulated his conclusions in any systematic fashion,” adding that “there are fewer misprints than are usually found in works of this character” (Moore, 1939). Malisoff began his review by stating that “this may be described as a psychological study” (Malisoff, 1939). Nagel predicted that Naess “will no doubt remain an outcast from the philosophic community and will have to find what solace he can in being a ‘mere’ scientist” (Nagel, 1939). Later reviews of his work barely differ in what they consider important to mention. Strawson also complained about Naess’s writing aptitude and was concerned that his (not inaccurate) summary was “parodying the author” (Strawson, 1954). Hutten wrote that it is a “social and psychological study about how people use words; it hardly touches upon the logical or philosophic issues involved” (Hutten, 1953). Chisholm stressed that although it is important for *linguistics* it does not have any clear relevance to *philosophical* questions (Chisholm, 1953). These reviews are short and do not engage much with the work.

To put these statements in context, consider the period of 1880–1920 as the time of an academic “power struggle” between philosophers and experimental psychologists (Kusch, 1995, 2011). While the turn of the century is remembered as a point of departure between philosophy and experimental psychology, it was also, as Sober put it, “a time of exile: while the psychologists were leaving, philosophers were slamming the door behind them” (Sober, 1978, p. 165). The rise and expansion of experimental psychology took place in philosophy departments and presented the vision of a new academic role that is part philosopher and part experimental scientist. This motivated many philosophers to argue for a strict separation between pure philosophy and experimental psychology, in a process of “role purification” (Kusch, 1995). Carnap’s vision is emblematic: “Now we shall eliminate the psychological questions also, not from the

region of knowledge, but from philosophy. Then, finally, philosophy will be reduced to logic alone (in a wide sense of this word)" (Carnap, 1935, p. 33). Psychologism, Carnap said, consists in the conflation of the task of logical analysis with the empirical questions of psychology.

**(p.334)** These early debates were couched in terms of "psychologism" or "psychologism," and climaxed in a controversial petition in 1913—one that was expressly "directed against the filling of chairs of philosophy with representatives of experimental psychology" (quoted in Kusch, 1995, p. 191). The outcome of these debates was a much-emphasized separation of "pure" philosophy from experimental psychology as distinct fields of study, with Frege and Husserl receiving credit for making the critical distinctions (see Kusch, 1995, 2011). Frege argued for a sharp distinction between logic/mathematics and psychology: mathematics and logic are neither parts of psychology nor are their objects defined, illuminated, justified, or proven true through psychology. One must, after all, distinguish between ideas of numbers and the numbers themselves. Frege acknowledged that knowledge of vague psychological processes may be of some interest, but rejected psychological interpretations of the analytic/synthetic and *a priori/a posteriori* distinctions; the difference is in how they are justified or proven true. In particular, they are justified without reference to matters of fact, psychological or otherwise. And at any rate, psychological laws do not evaluate thinking habits for their truth or falsehood—an *independent*, prior criterion is needed to evaluate them and to distinguish between being true and merely being taken-as-true. All good points to be sure, but this conception and these distinctions became central to philosophy, properly conceived.<sup>4</sup>

Naess recalls that his work was met with hostility and that he was often accused of psychologism. At the Third International Congress for the Unity of Science, Carnap even warned Naess not to present on his experimental studies (Naess, 1981, pp. 144–5). The use of questionnaires was scorned by "*genuine*" philosophers, Naess says, so much so that his department chair at Oslo threatened that he would not vote for Naess's tenure if he published his study on truth (Naess, 1983, p. 311). Naess published it anyway and his chair did not vote for his tenure.

Naess credits Morris's distinction between "pragmatics" and "semantics" with providing an easy way of dodging the potential relevance of his **(p.335)** empirical work (Morris, 1935; Naess, 1993). Morris introduced the trio of empirical, pragmatic, and formal dimensions of meaning, writing that "the meaning of a term is completely specified when it is known what objects the term designates, what expectations it produces in the persons for whom it has meaning, and what its connections are with other terms in the language of which it is part" (Morris, 1935, p. 278). The formal and pragmatic dimensions of meaning divided along the same lines as pure philosophy and experimental psychology; whereas semantics refers to the logical connections among terms in a language and is the domain of analyticity and the *a priori*, pragmatics is an empirical psychology of language which must not be confused with philosophy. Sellars's manner of discussing pragmatics and his concomitant concern with what counts as philosophy provides an idea of how many would

have approached Naess's work in what might be called "pragmatic semantics" (Apostel, 1953).

It is hardly necessary to point out that the additional tools for which we are looking are not to be found in the development which has come to be known as "pragmatics," for this is, on the whole, a branch of empirical science, a focusing of psychology and sociology on the phenomena subsumed under the empirical concept of language...Classical empiricism...confused the grammar of philosophical predicates by attempting to identify them with psychological predicates. In many cases the grammar was so seriously confused that certain of the more consequent empiricists can hardly be called philosophers.

(Sellars, 1947, pp. 645–6)

In response to this attitude Naess was quick to point out that "the term semantics is a catchword that does not convey any definite meaning," and that his work was not to be conflated with what is "legitimately done in pure logical analysis" (Naess, 1953a, p. i).

Although Naess's empirical semantics was generally met with resistance and scepticism about its relevance for analytic philosophy, it was viewed quite amicably among his closer Vienna Circle peers.<sup>5</sup> Tarski, for instance, said of material adequacy that it "can be settled scientifically, though of course not by a deductive procedure, but with the help of the statistical questionnaire method. As a matter of fact, such research has **(p.336)** been carried on [by Naess]" (Tarski, 1944, p. 360). Carnap advocated for an empirical approach to the application conditions or intension of natural language terms, and endorsed Naess's work as exemplary (Carnap, 1955a).<sup>6</sup> Carnap thought that a theory of pragmatics was needed not just for psychology and linguistics, but also for analytic philosophy due to the latter's focus on natural language (Carnap, 1955b; cf. Lutz, 2009). Carnap viewed knowledge about these "pragmatical concepts" as instrumental in inspiring and informing one's explications. He also saw it as instrumental in furnishing a practical justification for an explication, as one may attend to the function that these concepts are serving. Empirical semanticists agreed with Carnap's assessment (e.g., Naess, 1953a; Tennessen, 1960a).

It is perhaps no surprise that ordinary-language philosophers discovered a way to reformulate psychologism in their own terms. Ryle did this explicitly when he introduced a distinction between *use* and *usage* (Ryle, 1953). In the revolt against psychologism, he says, linguistic "vogues" have evolved: first from talk of concepts to talk of meanings, and now to talk of uses. Ryle attributes psychologism to these misleading verbal vogues.<sup>7</sup> Yet *use* has a critical advantage in that it contrasts with *misuse* and so is clearly "normative" (i.e., evaluative), whereas *usage* is merely a descriptive type of linguistic anthropology and sociology and is of "no philosophical interest." If usage is not in accordance with use the folk are mistaken. And analysis of use is not informed by analysis of usage anyway, as descriptions of usages *presuppose* descriptions of uses. To be sure, Naess and Tennessen did refer to themselves as specifically analyzing "usages" (Naess, 1949; Tennessen, 1949a). Tennessen saw Ryle's distinction as "fruitful, thought-economical" but

did not find a difference in the use/usage of “use” and “usage” in ordinary language (“Ryle’s Dilemma”; Tennessen, 1965). But as Tennessen’s colleague Max Wright quickly pointed out, this interpretation presupposed the answer. Either there is no difference and the results show it, or there is a **(p.337)** difference and the results show that the folk are mistaken; “the appeal was, in any case, philosophically pointless” (Wright, 1967).

Eventually, three reviews appeared that critically engaged with empirical semantics, and these are familiar in their arguments and concern with what counts as philosophy.<sup>8</sup> Since these reviews present arguments in some detail, it is useful to highlight how they pose their concerns. Apostel was worried about the ordinary concept of synonymy (Apostel, 1953). There is just no clear way to avoid the apparent circle when moving from descriptive “occurrence synonymy” (or similar types) to the more ambitious “normative synonymy” (synonymy according to a rule)—especially concerning the meaning of “synonymy” itself. Crockett brought up a similar point about the missing bridge to analyticity, which he says Naess “quite naturally wishes” to cross by “counting noses.”

The question remains, however, as to whether Naess has made any positive contribution to analytic philosophy...What is the philosophical point of these surveys? [It] is not at all clear that the description of a stock use of an expression is assisted by counting the noses of those who employ it in this way.

...Let us suppose that in a questionnaire, call it QS1A, one hundred per cent of the subjects say that the following sentences, chosen by them from other similar sentences, express the same assertion: [P, Q]. Then we may say that [P] and [Q] are QS1A-synonymous, and this will be a shorthand way of referring to the above results. Naess, quite naturally, wishes to say more than this, and what he wishes to say is that these tests results are relevant confirmatory evidence for the synonymy of these expressions. Here we need a clear-cut hypothesis of the meaning of “synonymy” as it is used in the preceding sentence, and Naess’s failure to provide such a hypothesis in this and other cases makes one wonder what can be the usefulness of his techniques.

(Crockett, 1959, pp. 109–110)

In another review, Toulmin complained that Naess’s studies were only able to handle “descriptive” statements and raised the possibility of error, thereby questioning the project’s philosophical relevance. What do the studies *prove* about the correctness of basic math, or equivalently, the use of language and the nature of our concepts?

**(p.338)** Exactly what Mr. Naess takes to be their relevance, is to one’s sorrow, left unclear—“The question of relevancy is complicated,” he says. What makes it so puzzling and tantalizing is Naess’s vagueness about the point of the investigation for philosophy.

...One must hope that, before Naess gets too immersed into the practical work of framing and using more and more similar questionnaires, he will sit down and tell

us what exactly they *are* designed to prove. Until that is done, it will be easy for philosophers to ignore his work. “Even if 25.8% of persons are found to give the sum of two and two as five,” they will argue, “that would leave the correctness of the formula ‘ $2 + 2 = 4$ ’ in formal arithmetic unaffected; surely also, the fact that quite a number of people were prepared to give some sense to the statement ‘Jones knows the time assiduously’ would not destroy the familiar, established use of terms, which rules out the collocation of such a verb and adverb?” And it would be a pity if Naess’s work were to be entirely ignored, for, reading through the paper, one certainly feels that the reactions of his answerers proves *something* about the nature of our concepts...At the moment, all one can do about the larger aspects of his work is to suspend judgment.

(Toulmin, 1956, p. 118)

Indeed, history repeats itself for those in analytic philosophy who undertake empirical investigation into concepts, which are presupposed in evaluating or interpreting people’s performance (Alexander et al., 2010; Kauppinen, 2007; Machery, 2008). These concerns pose problems for empirical semantics and experimental philosophy to the extent that the aim is to access some ambitious form of *a priori* analyticity or conceptual truth by means of experimental psychological investigation. But in the case of empirical semantics, there was just no attempt pursue such a “mentalist” project (Alexander et al., 2010). Naess, after all, rejected that goal in his work on the notion of truth, and his students and collaborators agreed.

Empirical semanticists were well aware of the above difficulties and had nothing against the “laudable effort to stamp out every trace of psychologism” (Naess, 1954, p. 55). What empirical semanticists deplored was the persistent concern for what counts as philosophy, and they always insisted that the philosopher who uses the methods of science “need not stop being a philosopher for that reason” (Naess, 1961a, p. 173). In their analysis of some of Hume’s texts, they close with the observation that the isolation of philosophy from psychological research is “one of the paradoxes of contemporary philosophy” (Naess and Naess, 1960, p. 146). Throughout much of the work of empirical **(p.339)** semantics they advocated a return to a traditional and broadly empirical conception of philosophy. Naess sums up this view well:

There is a tendency to look upon deductive and axiomatical procedures as somehow more philosophical than empirical ones, and this has undermined the position of the broad empirical traditions (Aristotle, Ockham, Locke, Berkeley, Hume, Bentham, John Stuart Mill), which in my view deserve a strong representation in contemporary culture. The charge of psychologism against thinkers of this tradition is well founded, but has been largely misapplied. It has discouraged research into genuinely empirical components of question complexes of a mixed formal and empirical character.

...Very roughly, one may distinguish a deductive, an intuitional, and an empirical component in the writings of analytical philosophers. Even in those cases in which

deductions and intuitions can help us considerably, consistent neglect of the empirical component will bring research toward stagnation. If empirical studies are neglected, we shall see much intelligent debate along intuitionist lines, but less of that process that many of us find so inspiring in the history of philosophy and science: the development of new branches of reliable knowledge as a result of combined philosophical and scientific efforts....Critics who would assume that the methods described in this book aim at solving questions that the intuitively and deductively operating logician has not been able to solve, mistake the intention.

(Naess, 1953a, pp. iii–iv)

In *The Function of Moral Philosophy: A Plea for Integration of Philosophical Analysis and Empirical Research* (1958), Ofstad begins with the classical conception of philosophy:

Among the ancient Greeks the philosophy of morals did not exist as a specific discipline. Socrates was not only a philosopher of morals, he was a psychologist too, and also a sociologist and a political scientist...The attempt to answer such questions led them into empirical as well as analytical problems. They accepted no definite limits for their speculations in these areas. Why should they? It is we who have tried to distinguish carefully between questions of analysis of language and those of an empirical nature, and split up the study of man into a number of different sciences. [The] view that the philosopher, *qua* scientist, cannot assert pure norms or value-statements...has dominated important parts of Anglo-American and Scandinavian moral philosophy for nearly fifty years, and so it may be useful to take it up for evaluation...It has stimulated contacts with such other branches of philosophy as logic and semantics, but the connections with psychology and the social sciences have been almost broken. The training of moral philosophers might be changed so that their education would qualify them for taking part in team-work with logicians, semanticists, psychologists and social scientists.

(Ofstad, 1958, pp. 35–7)

**(p.340)** Ofstad then surveys research programs in various disciplines: meta-ethics, communication and argumentation, moral deliberation and decision-making processes, beliefs and ethical behaviour, and so on, still insisting on methodological pluralism:

Whether research of this kind is called “moral philosophy” or not, seems, however, rather unimportant. The important thing is that there ought to be a close connection between such studies and investigations which are more central to moral philosophy...For the philosophical significance of such work, it is important to preserve all the subtleties which are compatible with the exploitation of the research-instruments developed within the social science.

(Ofstad, 1958, p. 40)

In today’s experimental philosophy, one also finds a nearly identical dialectic, exemplified

by Knobe's response to Kaupinnen—that is to say, the response that such difficulties, while interesting, are largely a red herring; where this is accompanied by the advocacy of a traditional, broadly empirical conception of philosophy (Knobe and Nichols, 2008; Knobe, 2007; Knobe et al., 2012). Again, the issue for empirical semanticists was not that the questions that were raised were not interesting or difficult ones (given relevant aims), but that these objections have been misapplied and so have discouraged otherwise valuable empirical research.

### Empirical Semantics

Despite some resistance, Naess was appointed as chair of philosophy at the University of Oslo in 1939, and here he continued to work on empirical semantics with his students and collaborators. This work culminated in his monograph *Interpretation and Preciseness* (Naess, 1953a), and later, *Communication and Argument* (Naess, 1966). Empirical semantics was highly influential in Norway; for nearly twenty-five years, an introductory version of *Interpretation and Preciseness* had served as the obligatory text for graduate students who intend to take any other major examination at “any Norwegian university, at most advanced great-schools, some teacher colleges and at all the military staff colleges Norway” (Tennesen, 1962, p. 1). After introducing some relevant terminology and an overview of the methods, I survey the work done in this period.

**(p.341)** Empirical semantics views communication in terms of a *sender, signal, and receiver*—particularly the interpretation of the signal by the receiver (or sender), where interpretations are modeled in set theoretic terms. Interpretations can be discovered experimentally with the use of questionnaires, such as through judgments of the form “Q may be synonymous with P” and “when you read P, did you take this to mean Q?” An interpretation Q of P is a *precization* (is more precise) when the synonymic alternatives to Q are a proper subset of the synonymic alternatives to P. An expression is a *plausible interpretation* of another roughly when it would be judged as potentially synonymous by many interpreters. Importantly, precizations and interpretations may be depicted in tree-like maps, which encode direction and depth of precizations (Figure 12.1). Strictly speaking, interpretations are properties of individuals, plausible interpretations are properties of groups, and the preciseness of expressions are given by social usages (Gullvåg, 1983).

*Definiteness* or *depth of intention* refers to the precization operative in an interpreter/speaker, as evinced by the point at which the individual **(p.342)**

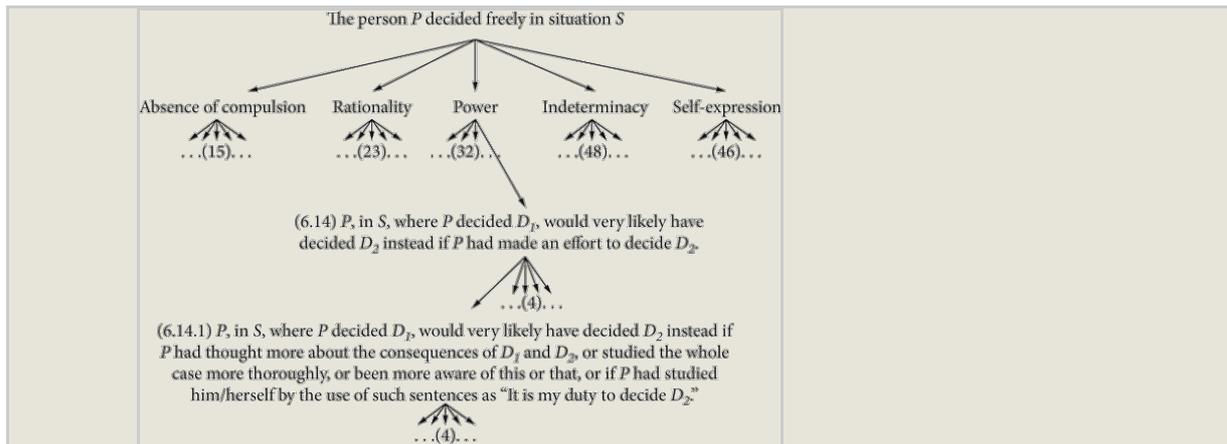


Figure 12.1 Tree-like diagram of precizations, modeled from Ofstad’s precizations of the sentence scheme “The person P decided freely in situation S” (Ofstad, 1961). Numbers below branches refer to the count of precizations that have been omitted. Also omitted is “freedom as virtue” in the top branch.

becomes indecisive among more precise formulations of what they mean or understand. Is noonish 14 or 15 minutes past noon? One may be indecisive about the matter and unable to answer the question, and this indecisiveness is the hallmark of indefiniteness. The concept was influenced by Peirce and his definition of “vagueness” (Gullvåg and Naess, 1996; Peirce, 1902, p. 748). It is more precisely a type of “process vagueness” found in the interpreter’s head (cf. Sorensen, 1990). Depth of intention was later explained in terms of the conceptual framework underlying an individual’s ability to discriminate in perception and thought and to access finer distinctions between types of situations (Gullvåg, 1983; Tennessen and Gullvåg, 1959).

Much use was made of the concepts of definiteness of intention and preciseness in diagnosing and explaining fruitless disagreement, and they were central to how empirical semanticists thought about conceptual change and scientific development. It also underpinned their views of the value, role, and limits of philosophical analysis (Gullvåg, 1988; cf. Howe, 2010; Naess, 1936; Tennessen, 1973). The centrality of definiteness of intention and their empirical approach to language encouraged empirical semanticists to interpret statements about conceptual or analytic truths as optative rather than, say, indicative, constative, apodictic, or anamnestic in character, though they were also fervent methodological pluralists.

Empirical semantics research would typically begin with a survey of uses alongside definitions and commentaries, dubbed “occurrence analysis” and “metaoccurrence analysis” respectively. The distinction between occurrence and metaoccurrence analysis distinguishes the projects of understanding how a term is used versus understanding how it is defined and conceived of by its users, so as to be able to diagnose discrepancies. This first stage was coupled with a detailed discussion of the interpretation of certain texts of interest (“elementary analysis”). There was a tendency to map out the space of possible interpretations by substituting precizations of component parts of expressions and then narrowing the list down. The goal was often not just to find out what

any individual actually thought but to map out the space of what one could possibly think or mean more precisely.<sup>9</sup> The whole process can be done from armchair up to this point in the procedure.

**(p.343)** Once some hypotheses were formed about usages/underlying concepts and any psychological or social factors of interest, these were tested with questionnaires, with interviews, or simply by revisiting the original occurrences. Usually a panel of interpreters coded occurrences under the usage rules or underlying concepts. Quite often a goal was to understand how people interpret and understand each other in fairly general terms (e.g., how does considering someone as out-group or in-group affect one's direction of interpretation of key words in political argumentation?).

The result of the investigation would be a map of precisations and underlying concepts, plus the effects of such factors as personality and philosophical positions. This served as a basis for making further recommendations and evaluations, such as for facilitating good political or scientific discourse through an increase in precision, for estimating the convincingness of arguments and appeal of political slogans ("market analysis"), or for diagnosing unnoticed ambiguities and connotations that arise from indefiniteness of intention.

The scope and detail of this work is impressive. Naess's analysis of Zaslavski's usage of "democratie" in *La démocratie soviétique* considered all 192 occurrences (Naess, 1953a, pp. 300–49). Tennessen's study on "the system of private enterprise" surveyed two years of annual newspapers in Norway for occurrences before constructing "the longest questionnaire ever given in Norway" (Naess, 1964, p. 7). Under the six conceptions of private enterprise identified, they coded 7,667 occurrences and analyzed them with respect to political party and profession (Tennessen and Gullvåg, 1959, p. 23). Siri and Arne Naess classified 661 sentences in Hume's *Treatise of Human Nature* into normative, declarative (analytic, synthetic), and other linguistic categories (Naess and Naess, 1960). Tennessen, Ofstad, Gullvåg, and Bay (1950) investigated nationalism and its relationship with psychological, sociological, and economic factors. They tested sixty-three hypotheses in ten surveys, most with about eighty questions and 500 questionnaires per survey. Analyzing the wealth of data at the time proved quite difficult, as one can imagine.

### **(p.344)** Empirical semantics research at Oslo

Naess's appointment as chair of philosophy at Oslo was soon followed by a five-year Nazi occupation. This, of course, impacted everyone working with Naess during his wartime seminars. Indeed, some of Naess's students did not survive the war and their studies were published posthumously.<sup>10</sup> It is in this context that a "unique interdisciplinary milieu developed, combining an emphasis on general theory and methodology with a strong concern for social and political problems" (Bay, 1958, p. vii). This attitude is reflected in the subject matter and the goals of many of their studies. For example, at the end of Tennessen's investigation of the attitudes of lawyers to the trials of Quislings<sup>11</sup> in Norway, he concluded:

The emotional impetus behind this kind of work is linked to the hope that our efforts

will incorporate research with a relatively direct struggle for humanist ideals. Regardless of whether we call our surveys opinion polls or attitude analysis or scientific studies in the areas between sociology, social psychology, semantics, and ethics, the hope is that if these examinations were performed on a larger scale, they would effectively contribute to the eradication of antagonism throughout society.

(Tennessen, 1950, my translation)

Naess and his students helped establish the Institute for Social Research in Norway in 1950, and Naess led UNESCO's Philosophical Analysis of Fundamental Concepts project to study concepts of democracy, nationalism, and liberty.

Still, throughout this period there was also a vibrant empirical semantics research program in philosophy, and this can be divided into some rough categories (Table 12.1). In philosophy of language, there was an **(p.345)**

**Table 12.1. Overview of topics and sample of works in empirical semantics. Asterisks signify experiments that use questionnaires and interviews. For some other bibliographies and overviews, see Ofstad (1951), Naess (1953a, pp. viii-ix), and the journals *Synthese*, *Theoria*, *The Philosophical Review*, and *Inquiry* for the 1940s through the 1960s**

Sample of empirical semantics research

**Philosophy of language**

- Verification of statements on ordinary language (Gullvåg, 1955; Mates, 1958b).
- \* “True,” “perfectly certain,” and “extremely probable” (Naess, 1953b).
- \* “Or” (Naess, 1961b).
- \* Common sense theories and “truth” (Naess, 1938a, 1938b).
- \* The intuitive concept of synonymity (Naess, 1956, 1957).
- \* Ordinary-language philosophers’ claims about ordinary language (Austin and Naess, 1964; Tennessen et al., 1964; Tennessen, 1959a, 1965).

**Ethics, action theory, and freedom of the will**

- Aesthetics and ethics in Kierkegaard’s *Either/Or* (Ofstad and Löfgren, 1965).
- “What is virtue?” in Plato’s *Meno* (Grimm, 1962, 1964).
- Free will and “The person *P* decided freely in the situation *S*” (Ofstad, 1953, 1961, 1967).
- Verifiability and objectivity of descriptive and normative claims, relationship to “reality” (Naess, 1959; Ofstad, 1951; Wickström-Nielsen, 1948).

**Philosophy of science**

- Evidential expressions in science (Naess, 1933).
- Normative, analytic, and synthetic sentences in Hume’s *Treatise* (Naess and Naess, 1960).
- “Consciousness” in the psychology of perception (Fluge, 1945, in Norwegian).

- \* “Testability” in physics (Lövestad, 1945; see also Naess, 1953a, pp. 374–6).

- \* Concepts of type/typicality (Tennessen, 1949b, in Norwegian).

### **Social, political, and legal philosophy**

- Foundations for the science and empirical semantics of law (Aubert, 1943).

- Concepts of legal norm (Ofstad, 1949, 1950).

- Critical examination of Nietzsche’s “Will to power” philosophy (Haaland, 1947; see also Naess, 1953a, p. 266).

- Examination of dialectical materialism, by Arne Torvik (Ofstad, 1951, p. 41).

- \* Businesspersons’ views of white-collar crime’s status as “crime” (Aubert, 1952).

- \* “The system of private enterprise” (Tennessen, 1949c, 1959b).

- \* Lawyers’ attitudes to prosecution of Quislings in Norway (Tennessen, 1950).

### **UNESCO and Norwegian Institute for Social Research**

- Gandhi’s ethics (Naess, 1958).

- Freedom and liberty (Bay, 1958).

- \* Concepts of democracy (Naess et al., 1956; Naess, 1953a, pp. 300–49; Rokkan and McKeon, 1951).

- \* Nationalism (Bay et al., 1950).

### **Philosophy and education**

- Precization in education, and educational development of the concept of truth (Grimm, 1955).

- \* The effect of philosophy education on students’ philosophical positions (Fain and Kaelin, 1960).

**(p.346)** effort to understand statements about ordinary-language rules, de-precization in ethical statements, and studies on particular terms such as “true,” “or,” and “synonymity”. In ethics and action theory there are interests in whether people “produce” decisions, “ought” and “can,” the freedom of the will, and relationships between interpretations of descriptive and normative statements. In philosophy of science, topics included consciousness and perception, the “testability” of physical laws, and the concept of “type” in psychology. They even tested whether scientists end up at observation sentences through repeated questioning (they do not). In social and political philosophy they investigated concepts of crime, dialectical materialism, Nietzsche’s “Wille zur Macht,” and interpretations of “legal norm” in law. The diverse themes in empirical semantics span traditional and contemporary philosophical topics, not unlike experimental philosophy today.

Tennessen’s “Concepts of Type” aptly illustrates the process and style of empirical semantics research (Tennessen, 1949b). This study begins by stating that its motive is to take a comparative and evaluative approach to typological methods, hypotheses, and research programs, using tools from experimental psychology. Tennessen surveys occurrences of and commentaries on “type,” “typical,” and closely related terms using

sources from philosophy and psychology, encyclopaedias, and newspapers. Then he surveys the range of possible precisations of the type concept using a schematic formula and substituting (p.347) interpretations of key components. This is coupled with an analysis of specific typologies in psychology to understand how they function in research. There is also a wider interest in non-cognitive aspects of meaning, such as the relationships among binaries such as female/male, young/old, plant/animal, passive/active, and their relationship to the types of integration disorders distinguished in psychology. After formulating hypotheses about some underlying concepts or usages, Tennessen reports on the 669 questionnaires that were completed by students, farmers, and professionals.

Tennessen identifies four main type concepts, which seem to be roughly as follows: (1) a characteristically descriptive individual of the type or exemplar; (2) typicality in the sense of statistical mode or set of most expected features; (3) those distinguishing properties with high sensitivity and specificity; (4) a “class” that is individuated according to some constitutive qualities of that class. Tennessen subsequently evaluates the work of typologists and philosophers. For instance, he criticizes Hempel and Oppenheim for failing to distinguish between (2) and (4) in their logical analysis. He also recommends that typologists in psychology use precization (3), given their explanatory and diagnostic aims, and shows how a number of them unknowingly shift between usages. Although Tennessen closes by saying this methodology is a valuable and much needed contribution to philosophical and logical analysis, he places special emphasis on its limited role. In particular, this strategy makes the process of discovery explicit by including the techniques for surveying the possible directions of precization. It also uses experimental methods to test the descriptive adequacy of one’s delimited usages or underlying concepts. This is meant, among other things, to fix the mysterious absence of a methodology section (the “method of revelation”) in logical analysis publications (Tennessen, 1949a).

One program in today’s experimental philosophy focuses on understanding how people think about specific philosophical topics of interest, and this aim is well represented in the research done at Oslo. However, the studies performed in empirical semantics are unlike experimental philosophy in their concentration on language systems, at least when theorizing about their work. But in practice they did not sharply distinguish between meanings and concepts, nor did they distinguish between predication of a term and application of a concept, nor the use of a term and deployment of a concept, etc. Where these studies (p.348) are *experimental*, they are in the tradition of behaviourist psychology and social science. The measurements are between variables such as personality and philosophical views and their effects on questionnaire results and other behaviour. The theories contain no cognitive architecture, identification of cognitive systems, or information-processing models, and this is typical of the time. This is also true of the work done at Berkeley (discussed below).

There is also a complete absence of any attempt to establish analyticities or conceptual truths. Empirical semanticists were happy to keep their results descriptive and

understand various parts of the world, and never aimed directly at the more ambitious kind of philosophical upshot that was expected of them. In general, they did not find projects of refining intuitions or increasing definiteness of intention in conceptual analysis worthwhile, except as a means to greater clarity and precision for some specified purpose. This overall attitude is clearly expressed by Naess on the notions of synonymy and analyticity:

Benson Mates contends “that one is justified in saying that there are ‘intuitive’ notions of analyticity and synonymy.” This empirical hypothesis about the existence of certain phenomena or kinds of phenomena is tenable, so far as I can see. Mates has an intuitive notion of synonymy; I have had several in my life, and there is reason to believe that all of them have much in common. On the other hand, there is no reason to believe that the various intuited entities are identical or near identical...If both of us should assert that our own intuitions are more adequate or more nearly similar to the intuitions of respectable authorities, there would be disagreement between us. But by what kind of discussions or research can a disagreement about the nature of an intuited entity be settled? Fortunately, collective research does not seem to presuppose that all intuitions are shared by all researchers, or that they should even know of the differences, or that the intuited entities should be definite in outline and content. Thus, there may in the years to come be much fruitful research concerning synonymy by researchers with partially different intuitive notions of synonymy...It is our contention that sound methodology does not require strict conformity of research terminology to prior intuitions. One may even say that strict conformity is not possible because of the indefiniteness of the intuitions.

(Naess, 1957)

Indeed, he argued that intuitions about universal interchangeability *salva veritate* are not universally held and that there are lax and broad notions of synonymy. Naess hypothesized that the thought that this intuition is universal owes to overestimation of the definiteness of intention (**p.349**) in unqualified statements, thereby mistaking unqualifiedness for generality or absoluteness (as with “lying is wrong”). Indefiniteness was also a major point of contention between empirical semanticists and ordinary-language philosophers. Although use/usage is a distinction between correct language use and actual performance, when taken as an empirical hypothesis about ordinary language it turned out to be below the definiteness of intention of its users (Tennessen, 1965). Whereas Ryle saw the ordinary concept of voluntary as somehow shaped by its role in blaming (as shown by illustrations of presumably competent usage), empirical semanticists would see this as unduly precise, i.e., as more definite than or even contrary to how ordinary folk define “voluntary” or conceive of voluntariness (Mates, 1958b).

### Empirical Semantics at Berkeley

The short period from 1957 to 1961 at Berkeley was another highly fruitful episode in empirical semantics. Much work originally done in Norwegian at Oslo was also published in English by Tennessen and Naess (the two main experimentalists).<sup>12</sup> Whereas empirical

semanticists had previously focused on logical analysis and the social and political applications of their techniques to specific concepts, at Berkeley the focus shifted to ordinary-language philosophy and to explaining why people have the intuitions about linguistic expressions that they do. One goal in this section is to outline this explanatory project. The work done at Berkeley is also a little piece of lost history that is interesting in its own right, and it has some parallels with debates in experimental philosophy.

The main cohort of empirical semanticists went to Berkeley in 1957.<sup>13</sup> Tennesen took a professorship at Berkeley in the Department of Speech (now named the Department of Rhetoric). Naess also became a part-time visiting professor in philosophy, and Gullvåg a visiting scholar. Here they worked alongside philosophers David Rynin, Isabelle Hungerland, Benson Mates, and John Searle. Everyone here was critical of ordinary-**(p.350)** language philosophy.<sup>14</sup> John Austin was a visiting professor in 1958 and inspired much of the empirical study that transpired in these years (Tennesen, 1959a).

Upon arrival, Tennesen and Gullvåg participated in Mates's 1957 seminar on Austin's "A Plea for Excuses" (1956). In this seminar they concentrated on ordinary-language philosophy and their own methodology. Notes from the seminar show that they focused intensively on *how* Austin was arguing for his positions:

Each set of hypotheses, it will be noticed, contains statements about usage and statements about actions: e.g., that it is generally not permissible to use adverbs in descriptions of normal actions, and some (or most) actions are neither voluntary nor involuntary. One occasionally feels that, for Austin, the two kinds of statements, which he simply juxtaposes, have some close logical relation: one kind is evidence for the truth of the other kind, for instance.

(Tennesen et al., 1964, pp. 106–7)

They also found it striking that "Austin's favourite method appears to be telling little stories and asking people what they should say in the described situation; by means of sets of stories, he finds himself able to elicit general agreement as to 'what we should say when'" (Tennesen et al., 1964, p. 103).

The main worry in Mates's seminar was the lack of explicitness about the way in which these vignettes were paired with usage hypotheses. Nearly a decade earlier, Tennesen ran some experiments to investigate problems with assessing usage from the armchair, and found a tendency to conflate evidence with illustration (Tennesen, 1949a).<sup>15</sup> As Naess summarized it:

**(p.351)** This mechanism radically destroys the function of definitions. Instead of giving us precise and tenable hypotheses for language usages to be tested by observing usages of the rule in a language, a definitoform sentence is looked upon as a formulation, the meaning of which is to be understood by means of the definiendum within the language. As a result, there is a tendency to accept uncritically and without any qualifications whatever subsumptions are explicitly or

implicitly asserted.

(Naess, 1953a, pp. 278–9)

They apparently achieved similar results with Austinian vignettes, and so stressed that this distinction needs to at least be made fully explicit in philosophical writing and seminars such as his (Tennessee, 1959c, 1959d).

In order to explore Austin's method more carefully, they surveyed possible interpretations of Austin's approach. They stressed that what is important is not just what Austin meant but any other interpretations as well, as was standard practice. Some examples:

(2) Usually, use of any modifying expression is *not permissible*. (2a) It would sound odd. (2b) It is not in accordance with communication norms in English. (2c) It would be meaningless. (2d) It would be false... (5a) Most people *do* not ever use "voluntary" and "involuntary" in describing more than a small number of cases. (5b) Can not meaningfully be used outside of such cases. (5c) Most people do not use the terms so that they ever say it was voluntary or involuntary.

(Tennessee et al., 1964, pp. 101–2; results in Tennessee, 1965)

Their diagnosis of the bleak prospects of the more ambitious claims of ordinary-language philosophy, as they saw it, turned on an ambiguity about what is "correct."

Of course it is true that, if an action has a certain characteristic, then some statement is "correct", i.e., true, namely, one which attributes that characteristic to that action. But on any other meaning of "correct," this does not follow, nor does the converse relation hold: that a locution is "permissible" does not allow us to infer anything about the world... Simply put: we feel it necessary to distinguish between what one would say and what one could say, and to insist that knowing the former does not give us complete information about the latter.

(Tennessee et al., 1964, pp. 107–8)

While the Mates seminar went on, Stanley Cavell was expressing high praise for ordinary-language philosophy (OLP) just a few doors away. So **(p.352)** Rynin arranged a debate at the 1957 Pacific APA, and they told Cavell he would have to defend OLP against Mates (Cavell, 1958; Mates, 1958b).<sup>16</sup>

Mates's paper draws from the seminar notes, but the target shifts to Ryle and his apparently irritating use/usage distinction (Ryle, 1953). Mates argues that Ryle's claims about use are descriptive after all, and accuses OLP of conflating semantics and pragmatics (conversational implicatures).<sup>17</sup> He then distinguishes between extensional and intensional methods of studying ordinary language, and complains that OLP focuses only on the extensional method. The extensional method is occurrence analysis, and the intensional method is metaoccurrence analysis with a Socratic twist. The intensional

method involves asking participants what they mean and presenting cases that might make them revise and precisify their accounts. Ordinary-language philosophy does not account for the conflict between methods, Mates charges. Both methods are incorporated into empirical semantics (Hungerland, 1960; Tennessen, 1959e). The Socratic dialogue was first proposed by Toulmin to transgress merely “descriptive” statements by investigating the semantics and entailment relations endorsed by the folk’s own lights (cf. Naess, 1961b; Toulmin, 1956). The Socratic dialogue model is revived in Kauppinen’s discussion of experimental philosophy for the same purpose (Kauppinen, 2007).

### Experiments on language judgments

By 1959, at least fifteen projects and 4,500 questionnaires and interviews had been administered at Berkeley on topics including the analytic/synthetic distinction, definiteness of intention, verbal rigidity and argumentation patterns in religious and political discourse, and “P can decide to do x” (see Tennessen, 1959a, p. 287). Tennessen used these to argue “against any tendencies to narrow down the field of permissible communication by employing rigid, *a priori* norms or rules for ‘what can possibly be said (p.353) and meant’” because “the whole thing is most often a question of general (including hermeneutical) imagination” (Stern, 1969; Tennessen, 1959a, pp. 276–7, 1961). Tennessen’s target is exemplified by his appraisal of the Russell–Strawson controversy over Russell’s theory of descriptions.

It has always been clear that whatever advantages this proposal might have, they have *nothing* to do with analyses or hypotheses about (common or “ordinary”) language usages. *None the less*, the following passage is found in Strawson’s “On Referring” (p. 330):

Now suppose some one were in fact to say *to* you with perfectly serious air: “The king of France is wise.” Would you say, “That’s untrue”? I think it’s quite certain that you wouldn’t.

Strawson is wrong: Of about 1,500 informants tested in some recent experiments no one seemed to act in accordance with Strawson’s predictions...Strawson, one might say, has opened the door a crack to the vast field of empirical investigations of language, taken a peep in, and, after (almost immediately) having shut the door, he reports: “Russell is wrong: The Theory of Descriptions is fatally *incorrect* because one would not (could not? should not? ought not to?) utter, and/or mean: “The present King of France is wise is false’!” However, in his own attempt at a “solution to this puzzle” Strawson seems absolutely uninterested in what “one” would or would not say.

(Tennessen, 1960b, pp. 187–8)<sup>18</sup>

By dividing up the mechanisms underlying intuitions about language and explaining the tendency to reject statements for purely language reasons, Tennessen and collaborators aimed to show that these judgments were quite irrelevant as to a statement’s tenability.

After surveying plausible interpretations of “what should we say” and similar locutions, they identified three general lines of defense: ineffability, infrequency, and impermissibility. A sample of each follows.

Under the ineffability approach, a tenuous connection was found in the “verbal rigidity” hypotheses of Vygotsky, Piaget, and Frazer, according to which children and “primitive societies” cannot separate word forms from word meanings. Tennessen found that children just have a strong tendency to adhere to the *permissibility* rather than *potentiality* direction of interpretation of “can” in such questions as “can you call a cat a dog?” (Tennessen, 1959a, pp. 266–72). The order of questions was sufficient to make the difference, and once clarified there was no **(p.354)** evidence of verbal rigidity for English speakers. However, there *was* an analogous form of rigidity on the side of experimenters, as they were apparently unaware of these language ambiguities and did not imagine such alternative interpretations.

The infrequency approach draws on what is “never said” to formulate language rules. This is what Austin reportedly suggested at Oslo (October 1959), in a debate with Naess over some of Tennessen’s experiments on “voluntary yawning”:

**Austin:**

The subjects gave wrong answers concerning their own use of expressions, e.g.: when saying they would never use “he yawned voluntarily” as a description of a perfectly ordinary event of yawning *because* it is perfectly obvious that such yawnings *are* voluntary. *Actually the subjects would not say it because it cannot be said.*

**Naess:**

The subjects interpreted “he yawned voluntarily” as synonymous with “he was not forced to yawn” and thus conceived it as obvious that he was not forced to (and therefore *not worth while saying*).

**Austin:**

But then they do not know well enough the expression “voluntary.” It is too difficult a word, maybe. Better use “clumsy.”

**Naess:**

Suggestion to HT: New experiments...

**Austin:**

...Rules (grammatical or others) do not exist as rules. *We say there is a rule against saying x when x is never said.* What is against the language system *cannot* be true or false or obvious. *Thus “He yawned voluntarily” cannot be true or false*

*or obvious.*

**Naess:**

Tennessen investigates communication rather than language systems. What happens if something actually *is said* (uttered) which “never is said”?...

**Austin:**

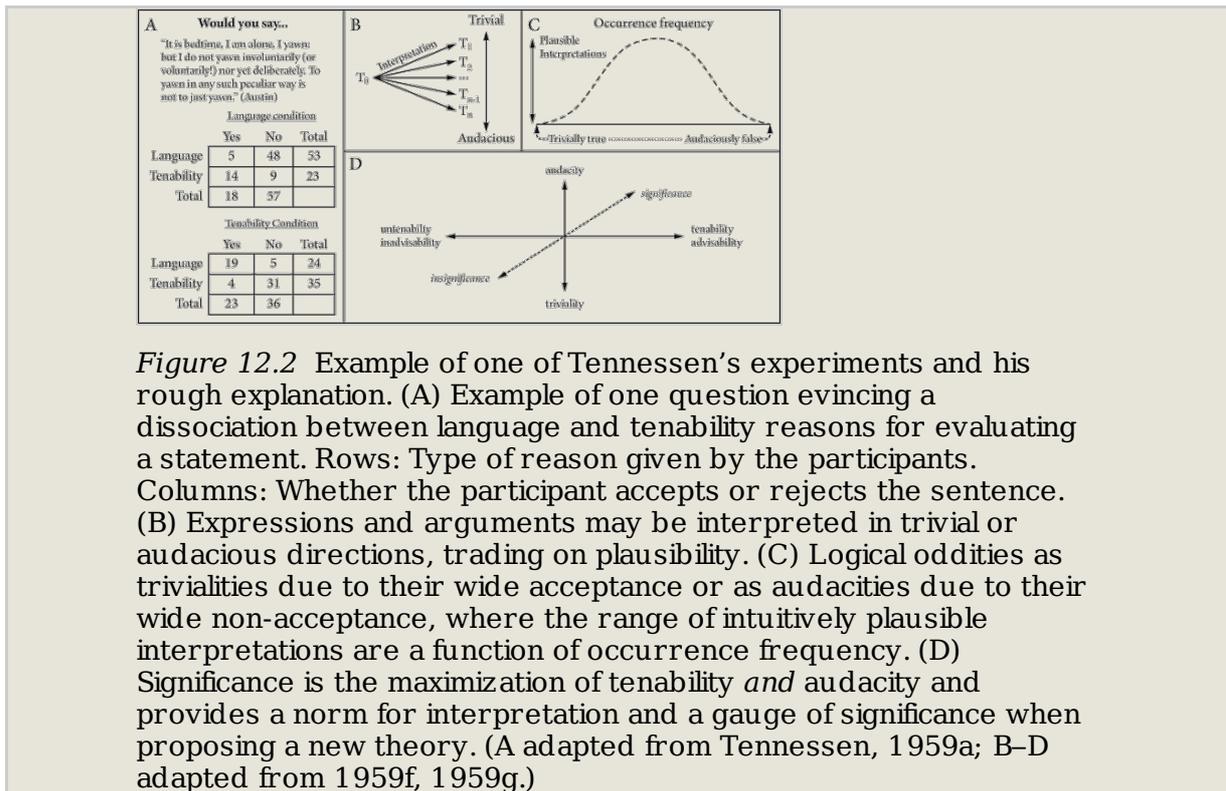
Suppose that someone yawned in a standard way and there is nothing exceptional (i.e., the case is a standard case). If we ask people to describe completely as possible what happens, they will *never* add “clumsily” or “voluntarily.” If we say “but did he or did he not yawn clumsily (or voluntarily)” they would find the terms *inapplicable*, the sentences neither true nor false.

(Austin and Naess, 1964)<sup>19</sup>

**(p.355)** These potential problems and predictions were the subject of further experiments, of course. In one case, they hosted fake discussion groups on social problems among students (accompanied by a hidden tape recorder), and aimed to have the locution “voluntary yawning” occur as naturally as possible (Tennessen, 1965, pp. 234–6). Contrary to Austin’s predictions, the statements were accepted as meaningful.<sup>20</sup>

Lastly, consider the impermissibility approach. Tennessen and collaborators predicted a dissociation between two sources underlying the evaluation of a statement: (1) a grammatical and idiomatic/literal direction of interpretation, and (2) a tenability direction of interpretation. So some experiments began with either (1) a “logical-maniacal” lecture on how people often assert nonsense and contradictions, or (2) a “common-sensical” lecture on how the most important thing is to understand what someone means. Participants then classified sentences as tautologies, contradictions, or nonsense, or as conveying factual synthetic statements. Participants were also asked to provide their reasons, and these were coded as language or tenability reasons for rejecting the statement. These experiments showed that participants could adopt and switch between the two interpretive attitudes and exposed tendencies for pseudo-disagreement when these distinct sources or attitudes explain the disagreement (Figure 12.2A; Tennessen, 1959a, pp. 280–4, 1959f).

So Tennessen identified two mechanisms or processes, one fast and intuitive and the other requiring a bit of reflection and imagination, and these underlie the difference between language and tenability directions of interpretation. Tennessen took the upshot of his studies to show that so far there are no empirical grounds for a theory of linguistic necessity or for linguistic restrictivism; they are either empirically unsupported or they split between two sources of judgments about the acceptability of a statement. The reasons and features these sources track do not tell us anything about each other; if a statement is ungrammatical, counterintuitive, or goes against ordinary usage in one way or another, this tells **(p.356)**



*Figure 12.2* Example of one of Tennessen’s experiments and his rough explanation. (A) Example of one question evincing a dissociation between language and tenability reasons for evaluating a statement. Rows: Type of reason given by the participants. Columns: Whether the participant accepts or rejects the sentence. (B) Expressions and arguments may be interpreted in trivial or audacious directions, trading on plausibility. (C) Logical oddities as trivialities due to their wide acceptance or as audacities due to their wide non-acceptance, where the range of intuitively plausible interpretations are a function of occurrence frequency. (D) Significance is the maximization of tenability *and* audacity and provides a norm for interpretation and a gauge of significance when proposing a new theory. (A adapted from Tennessen, 1959a; B–D adapted from 1959f, 1959g.)

us nothing about its tenability, and vice versa. Tennessen insisted on Carnap’s principle of tolerance for matters linguistic and conceptual, and defended the viability of Russell’s approach against Strawson-style ordinary-language refutations (Tennessen, 1960a).

More interesting to Tennessen was what the ineffability, infrequency, and impermissibility approaches *do* get right, and to this end he offered some tentative “explanations of the fact that there are linguistic expressions, locutions, formulations which intuitively or discursively sound *odd* or even ‘logically odd’” (Tennessen, 1959f, p. 369). Tennessen viewed this as part of his “attack on the method of revelation” (the use of intuition) in philosophy. He said:

A particularly interesting situation arises, when [historically] philosophically interesting problems—linguistic or non-linguistic—have not yet been tackled by the scientists within any ramificulated branch of existing science disciplines. **(p.357)** The prim and proper philosopher, then, who insists on an *a priori* attitude, has to choose between keeping his hands clean, at the cost of ignorance on relevant matters, or to engage in empirical research himself. It seems that confronted with this choice-situation, most analytical philosophers, and in particular the so called “ordinary language” oriented philosophers, choose ignorance as the lesser of the two evils. The present paper is partly meant as an attempt to indicate what may be gained for philosophy by choosing the more earthly, *a posteriori*, attitude, employing empirical investigations after the pattern of the social (and other “soft”) sciences, and developing the available methods and techniques to fit within a philosophical frame of reference.

(Tennessen, 1960a, pp. 496–7)

Tennessen's tentative proposal was that the quick, unreflective judgments about linguistic or "logical" oddness can be explained by a person's linguistic and conceptual habits plus the factors governing the remarkability of an expression (Tennessen, 1959f, 1959g, 1963). For example, "P is cultivating weeds" was labeled as "logically odd" by Nowell-Smith, despite this being a perfectly natural way to describe a normal and important activity undertaken by many anti-weed-spray producing companies (Tennessen, 1959f). It is just because Nowell-Smith is not a worker at such a company and that this is never remarkable that a tension is intuited by him, and this explanation holds quite generally as a first approximation.

Here is the gist of Tennessen's theory. The "audacity" or "triviality" of a statement is a measure of how widely accepted the statement is among one's audience. In general, interpretations of statements, arguments, and theories tend to leave open a continuum between audaciously false and trivially true directions of interpretation—as he previously noticed in his previous work on nationalism, "the system of private enterprise," and elsewhere (Figure 12.2B). The range of fast and intuitive interpretations is a function of that expression's remarkability among situations that a person regularly encounters, so that these intuitive semantic judgments are a reflection of one's linguistic and conceptual habits. However, the *whole* range of plausible interpretations is not immediately obvious and may require deliberation and a little imagination (Figure 12.2C). What makes a "hypothesis" or "proposal" *significant* is that it is sufficiently audacious while still tenable (Figure 12.2D).

Tennessen used the assumed rule of significance as a benchmark for successful interpretation; if someone propounds a contradiction or obvious absurdity and significance is assumed, the interpreter is forced to **(p.358)** engage in a roundabout interpretation by figuring out what the sender is "up to" and intends to convey. He applied this schema to various cases and accounted for witty sayings and double entendres as a mismatch between intuitive and reflective interpretations, to explain the value in audacity for special emphasis, and to give reasons why one would ever remark a truism, obvious falsehood, or something completely irrelevant (Tennessen, 1959a, 1960b, 1965). The difference between mere sensationalism and significance was accounted for in terms of a difference between *prima facie* and actual tenability (Barnes and Robinson, 1972; Tennessen, 1959g, 1973, 1984). Something as audacious as "photons are both particles and waves" or "neuroscientists discover free will is an illusion" is truly significant only if it is tenable in spite of its audacity; only if it does not receive its audacity by trading on untenable implications or rigging spurious interpretations. Tennessen and Naess were very interested in how this view could apply to theory construction in social science (see Tennessen, 1960a, pp. 496–7).

It seems that this was the last major period of development in empirical semantics. The Department of Speech that was home to Rynin, Tennessen, Hungerland, and others underwent restructuring in around 1960 and the Norwegian empirical semanticists all moved to different institutions. Many of the books and collections of experiments that

Tennessen repeatedly cites throughout his studies were no longer pursued after 1959–60 and were never completed.<sup>21</sup> This is perhaps unfortunate, as manuscripts show that they were aiming to develop a theory of “contextual pragmatic implications,” which they identified as an area for which a scientific theory is lacking (Tennessen, 1959e, 1959h). Hungerland made some key insights into conversational implicatures and the manuscripts show enthusiasm for systematizing these (Hungerland, 1960).<sup>22</sup>

To sum up, the empirical semanticists at Berkeley viewed ordinary-language philosophers as appealing to ordinary usage and tested these claims, often with negative or uncertain results. The constructive project (**p.359**) to explain sources of language intuitions immediately followed. Much earlier was the discovery that the interpretation mechanism tends to treat evidence and illustration of usage indiscriminately. At Berkeley they explored a dissociation between language and tenability-based judgments in evaluating a statement, plus the various other studies on usage frequency and remarkability (see Tennessen, 1959a, 1959f). These were used to provide some tentative explanations for the patterns of counterintuitivity in terms of usage and a distinction between fast intuitive language judgments and reflective interpretations, which was then applied to conversational and scientific contexts. By better understanding the sources of the intuitions in these cases, the hope was to clarify when they have a legitimate role in argumentation and theorizing.

### Summary and Conclusion

Empirical semantics had an interesting history from its launch in Vienna, its development and applications at Oslo, and through its disagreement with the methods of ordinary-language philosophy at Berkeley. Naess was motivated by philosophers’ appeal to the way terms are conceived, defined, and used by ordinary people, as exemplified by Tarski’s material adequacy condition. For Naess and the other empirical semanticists, intuitions about common sense and the pre-theoretic views of ordinary people were not sufficient, especially given enduring disagreement on such matters. The conclusion was underwhelming: there is just as much variation and indefiniteness in the minds of ordinary people.

The empirical techniques were valuable for other reasons, and so Naess continued his work with collaborators at Oslo. Empirical semanticists had success in projects with UNESCO on concepts with social and political significance, in the study of science and law, and in their inquiries into questions of traditional philosophical interest. Later work at Berkeley involved developing theories to understand disagreements arising from the claims of ordinary-language philosophers. The research done at Oslo and Berkeley was extensive and broad in its scope. It was very constructive as well; part of this was of necessity due to the lack of pre-existing methods and theories for their purposes, part of this was due to a commitment to pluralism about the methods and subject matter of philosophy, and part of this owed to the view that long-term constructive (**p.360**) cooperation of many workers is just as important in philosophy as it is in science (Naess, 1953a).

I have highlighted the lamentable dialectic that surrounded empirical semantics

throughout the period of logical analysis and linguistic philosophy. Empirical semanticists advocated a return to the traditional conception of philosophy that accepted the philosophical legitimacy of both empirical and analytical questions. Their kind of approach was marginalized, often inadvertently to be sure, due to the expectation that *a priori* analytical results should follow from or be the aim of scientific investigation in matters philosophical—as if, for instance, in developing a scientific theory or explanation of how people define, conceive, and use certain terms and their cognates, it would not be philosophy proper unless it also went toward proving some contentious logical, semantic, or conceptual truth without begging the question. That was the worry with analyticity and synonymy, with deriving use from usage, the correctness of math from math performance, or other forms of psychologism. Although some of the dialectic persists today (Kauppinen, 2007; Knobe, 2007), the philosophical climate and attitude has definitely improved.

Compared to experimental philosophy, there is an absence of any “positive” or “negative” mentalist program, where these both involve taking intuitions to have some distinctive evidential role in conceptual analysis or discovering analyticities, and the negative program casting doubt on such mentalist programs (Alexander et al., 2010; Cappelen, 2012; Machery, 2008). Whether or not this is an accurate picture of today’s experimental philosophy movement, it was at any rate not a topic of interest in the eyes of empirical semanticists. And there are many reasons why, including their view of indefiniteness of intention in semantics, their survey of possible universal normative conclusions that might be drawn from descriptions of language, and their broadly Carnapian attitude to theory construction—not to mention their attention to differences in directions of interpretation and preciseness (compare Chalmers, 2011).

Today there are two broad explanatory goals found in experimental philosophy that are concerned with understanding *how* people think about philosophical topics and explaining *why* they think the way they do about them. Both were well represented in empirical semantics, though of course in the form of a behaviourist psychology that **(p.361)** emphasizes language usage. Much of the research at Oslo was concept-driven research on how closely related terms are defined, conceived, and used by people, and was motivated by an interest in improving political discourse and contributing to science and philosophy. The work done at Berkeley exemplified the interest in explaining why people have the intuitions they do about the acceptability of a statement. Although many experiments directly tested the claims made by philosophers about ordinary language and thought, this tended to function as a rhetorical point of departure for subsequent theorizing and explanation.

At the center of empirical semantics and experimental philosophy is the use of the latest tools from psychology and social science, and a return to a traditional conception of philosophy as one that engages with both analytical and empirical questions. Empirical semantics had a small following and faced some difficulties with experiment construction and interpretation of the evidence, and most of the actual experiments in philosophy were done in connection with just a few philosophers, among them Naess and Tennessen. In

contrast, today's experimental philosophy has much wider appeal and has far better tools at its disposal, and the experiments and researchers already outnumber the work in empirical semantics by a wide margin. Despite empirical semantics' successes, Ernest Nagel was correct when he predicted that Naess would "no doubt remain an outcast from the philosophic community and will have to find what solace he can in being a 'mere' scientist" (Nagel, 1939). Needless to say, experimental philosophy has much brighter days to look forward to.

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### Notes:

Special thanks to Adam Morton and Jeff Pelletier’s summertime seminar on empirical semantics and experimental philosophy at the University of Alberta in 2009. Here I became aware of the manuscripts, seminar notes, and original studies that were left by Herman Tennessen, which served as valuable material for understanding the research at Oslo and at Berkeley. Some of this material has been placed online on my website, and is otherwise available on request. I would also like to thank the anonymous reviewers and editors for their insightful and helpful comments.

(<sup>1</sup>) As Ernest Nagel elegantly summarized: “It also contains many delicious morsels: for example, Dr. Naess found that school children at the age of puberty are capable of discussing the problem of truth with as much aplomb, though without the technical language, as philosophers with reputation; that the theory of truth as *adaequatio rei et intellectus* was propounded to him, except for the jargon, by a school-girl of sixteen; that his women test-persons had a greater tendency than men to believe in “absolute truth”; and that the criticisms by his test-persons of statements by their fellows, when these statements were torn completely out of their context, were not unlike those made by professional philosophers upon the writings of their colleagues.” (E. Nagel, 1939, p. 78)

(<sup>2</sup>) On Tarski, Naess pointed out that the group in this vicinity is unified by its function in conversation as a means of affirming something stated or as otherwise avoiding redundancy. Tarski responded that participants likely misunderstood, and proposed another test (Tarski, 1944, p. 360).

(<sup>3</sup>) For this reason, it is incorrect to count Naess as an early proponent of “experimental philosophy,” characterized as advocating empirical studies of non-philosophers’ “intuitions about philosophical cases,” or as subscribing to the view that philosophers appeal to intuitions as such as evidence (Cappelen, 2012, p. 219).

(<sup>4</sup>) Empirical semanticists had much to say about Frege and Husserl. Frege's views about vaguely grasped propositions receive treatment in Gullvåg's formalization of depth of intention as discussed later in the chapter, using Barwise and Perry's situation semantics (Gullvåg, 1983). See also Naess on Husserl's apodictic evidence of ideal laws from the perspective of empirical semantics (Naess, 1954).

(<sup>5</sup>) The views of many Vienna Circle members such as Carnap, Schlick, Nagel and Neurath (who were in agreement with Naess's approach) did not gain much credence in German academic philosophy because of this contrasting attitude (Kusch, 1995, pp. 222–6).

(<sup>6</sup>) Naess (1953b) also thanks Carnap for input into experimental hypotheses. Probably this input was given at the 1937 Congress in Paris, where Naess presented some preliminary results.

(<sup>7</sup>) That is to say, when philosophers have remarked such things as “the concept of P” or “the meaning of “P,” they have never been talking about some concept or term that stands in relation to P, but P itself. Ryle's take on this phenomenon is similar to that of Timothy Williamson and Herman Cappelen.

(<sup>8</sup>) Apostel, Crockett, and Toulmin all have positive things to say in the rest of their reviews, as does Mates (Mates, 1958a). Of course, Quine influentially noted that it is not at all clear how empirical investigation can solve disputes about meaning, and his influence shows up in some of the reviews (cf. Naess, 1957; Quine, 1951).

(<sup>9</sup>) Indeed, when Naess talked of the “possibility of an ‘experimental philosophy’” in his study of truth he referred to a developmental psychology of conceptual systems—one which *begins* with the “embryonic form” of philosophical positions found among non-philosophers (Naess, 1938a, p. 161).

(<sup>10</sup>) In a 1945 letter to Otto Neurath, Naess wrote of his wartime experiences: “I am still somewhat groggy and disheartened because of lost friends and collaborators, but I hope soon to recover. The very brilliant young philosopher Ludvig Lövestad died this year. He was my close friend in all kinds of work, also the ‘illegal.’ He was tortured to death, remaining silent about my hiding-place. Another close friend and collaborator in philosophy, Wickström-Nielsen, was killed when jumping from parachute. He came from England and jumped with documents and Russell's new book on Truth etc. and Lundberg's new book on the methods of sociology. Also other young people who wished to go on with philosophy and mathematics are missing. This field got an exceptionally hard blow.” (Quoted in Stadler, 2009, p. 20.)

(<sup>11</sup>) Quislings were members of Vidkun Quisling's collaborationist party during the Axis occupation of Norway in World War II.

(<sup>12</sup>) Much of this was facilitated by Naess's editorship at *Synthese*, and Naess's new journal, *Inquiry*.

(<sup>13</sup>) This is aside from Ofstad, who went to the University of Stockholm in 1955.

(<sup>14</sup>) Their colleague Stanley Cavell described this period as one of being engaged in “all but continuous argument, sometimes consisting of friendly exchange sometimes of (temporarily, but you couldn’t be sure) estranging dispute” (Cavell, 1999, p. xxiii). “[I was surprised by the] outrage [OLP] produced in my older colleagues. Outrage is what it was. This was evident in my colleague Benson Mates’s contempt, echoed in his older friend David Rynin’s exasperation” (Cavell, 2010, p. 372).

(<sup>15</sup>) Some of the experiments went as follows: “The word *x* seems to be used in different ways. Occasionally it is used in the sense of *y*, as for instance in the sentence: ‘...’. We inserted a sentence which made it seem preposterous to believe that the word was used as indicated in the text. In spite of this, there was a tendency among the respondents to agree to the subsumability. Some of the questionnaires contained questions of the following kind: ‘Do you think this *x* is a good or bad example of *y* being used in the sense of *z*?’” (Tenessen and Gullvåg, 1959, p. 3).

(<sup>16</sup>) Although it launched his career, Cavell remembers it thus: “Rynin issued this invitation—summons rather—coming upstairs and down the hall from his to my office in Dwinelle Hall, at the end of a conversation that he began by noting that since I arrived in town I had been saying a lot of extravagant things about this new work on ordinary language...The impression of anger in such exchanges never left me.” (Cavell, 2010, pp. 372–3.)

(<sup>17</sup>) This appears to be the first time conversational implicatures, though not named as such, are explicitly used as arguments against ordinary-language philosophy.

(<sup>18</sup>) The “1,500” number presumably comes from its regular inclusion in studies at Berkeley.

(<sup>19</sup>) The full transcript and other materials are available on the author’s website.

(<sup>20</sup>) Austin planned to study the interviews apparently supported occurrence analysis in dictionaries (Austin and Naess, 1964). Indeed, Austin saw his approach as one that would be absorbed into a larger scientific enterprise (see Naess, 1961 a, p. 197). Unfortunately Austin passed away shortly after these debates. At the time it was an open secret at Oxford that Austin was seriously considering moving to Berkeley, having reportedly remarked that he “could build an empire there” (T. Nagel, 2009), and having expressed concern with a lack of a next generation of like-minded philosophers at Oxford (Chapman, 2009).

(<sup>21</sup>) This is aside from *Objectivity*, which was published by a small San Francisco publisher.

(<sup>22</sup>) Hungerland argued that a satisfactory account of contextual implication depends crucially on what one can infer about the speaker’s beliefs given that the norms of

conversation, whatever they are, are still not violated (see also Chapman, 2008, 2009).

