XII

The Roots of The Central Account

1. *Introduction*

The positive task of this book is to properly characterize latent variable modeling, and the negative, to tear down the Central Account. But the CA has flourished in part because of confusions in psychometrics in particular, and the social and behavioural sciences in general, over the facets of scientific investigation. These confusions have resulted in the acceptance of a great deal that is incoherent, "unobservability talk" a notable case in point. The most damaging confusion inherent to work in the social and behavioural sciences, and, by extension, psychometrics, the confusion at the root of so many false steps, centres on the distinction between conceptual issues on the one hand, and empirical issues on the other. The social and behavioural scientist, and, so too, the psychometrician, misunderstands the characters of the conceptual and empirical issues that permeate his work, and, as a result, frequently misdiagnoses the correct approach to addressing these issues. He does not know what to make of the psychological concepts that arise in his work. He does not know what kind of question he is addressing when he addresses a question whose answering requires a consideration of a concept's meaning, or ponders on the conditions under which a set of scores can rightly be said to be measurements of, say, dominance. He does not understand the place of such questions in relation to the mathematical innovations that represent progress in psychometrics.

This shortcoming has greatly militated against progress within the social and behavioural sciences, for it has resulted in countless futile efforts to try to solve what are conceptual problems through the building of empirical, scientific cases. And because conceptual clarity is a prerequisite to the carrying out of fruitful empirical work, the empirical work that should be the foundation for cumulative, progressive scientific efforts, has, time and again, been compromised. Psychometricians have, for example, advocated the mistaken belief that measurement claims are properly adjudicated through the accumulation of empirical evidence. As a result, rather than making strides in regard the clarification of the place of measurement in the social and behavioural sciences, they have invented many unrelated procedures that they have mislabeled as measurement. The aim of this section is to address the place of conceptual and empirical issues in the work of the social and behavioural scientist and the psychometrician.

2. *Empirical and Conceptual Issues*

The many issues addressed by a properly functioning empirical science include the description of the objects, entities, forces, and phenomena of natural reality, the construction of theories to explain this subject matter, the search for new truths about constituents of natural reality, and the making of discoveries of here-to-fore unknown features of natural reality. Scientific investigation is progressive. The empirical propositions of a science are adjudged to be more or less true on the basis of empirical evidence. Such propositions, regardless of the evidence offered in their support, are always provisional and open to rejection in light of new evidence.

The scientist conceptualizes his phenomena. This does *not* mean that he thinks, theorizes, or has hunches about his phenomena, although he certainly also does these things. It means, instead, that the phenomena he studies are denoted and organized by concepts (terms), both of the ordinary language and technical variety.[1] His observations, hypotheses and theories are expressed in language, and, hence, in terms of concepts. The hypothesis of astronomer Leverrier that "It is impossible to satisfy the observations of Uranus without introducing the action of a new Planet, *thus far unknown* [italics added]... Here are the elements of the orbit which I assign to the body..." (see Grosser, 1962) is an empirical speculation expressed in terms of the concepts *planet*, *orbit*, and *observation*, among others, these concepts combined in a sentence in a grammatically legitimate manner. Leverrier's hypothesis is intelligible because these concepts have employments that are fixed by linguistic rules (they have normative employments), these rules are grasped by competent speakers of language, and Leverrier has employed them correctly (in accordance with the rules). While Leverrier's claim is *intelligible*, it is an empirical issue whether or not it is correct.

A concept is a token in a linguistic practice, and language is a human creation. A concept is not a constituent of natural reality. The various employments of a given concept are fixed by linguistic rules. Rules are simply standards of correctness. They fix what is correct (and, hence, incorrect) in behavioural practices in which behaviour *can* be correct or incorrect. The rules of employment of concept "θ" are taught, learned, and made reference to in arguments over the meaning of concept "θ". When someone who is learning the language (or a special technical vocabulary) misapplies "θ", others can, and do, set him straight: "*That* is not how "θ" is used. *This* is what you say." To employ concept "θ" correctly is to employ it in accord with the linguistic rules that fix its sense, and to recognize an incorrect employment of "θ" is to recognize a departure from this normative employment. When we break from correct usage, the result is nonsense (literally, expressions to which language assigns no meaning). However, only a small proportion of the linguistic/grammatical rules that govern the employments of the concepts of language are codified. It is mistaken, for example, to view the rules that fix correct concept use as merely comprising the common grammatical rules taught in school. Once again, rules are simply standards of correctness, and, hence, to clarify the similarities and differences in the employments of two similar concepts is to clarify certain of the features of their rule governed employments.

The physicist lays down definitions for his novel technical concepts:

Definition: *alpha particle*. A positively charged nuclear particle, consisting of two protons bound to two neutrons.

In doing so, he provides rules for the employment of his terms. Classical convention reserves the term *definition* for a rule of employment that is of the necessary and sufficient variety. Thus, the rule (necessary and sufficient condition) that governs application of the concept *alpha particle* specifies that the concept is applied to an x, if and only if x is a positively charged nuclear

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1 An "ordinary language concept", e.g., *hope*, *car*, is a concept that is an element of the ordinary language taught to the members of the linguistic community in which the concept has an employment. A technical concept, e.g., *quark*, is a concept invented by the members of a specialized field of study, for use within the technical work of the field. The rules of employment of a technical concept are typically of the necessary and sufficient condition variety. Certain concepts have both technical and ordinary language senses (the technical and ordinary language senses are homonyms). In such cases it is essential to not lose track of the distinction, and relation, between these senses.

particle consisting of two protons bound to two neutrons. As will be seen, however, necessary and sufficient conditions are not the only type of rule of concept employment. The employments of psychological concepts are, notoriously, not governed by necessary and sufficient conditions, but, rather, by other distinct types of rule. Moreover, there is no end to the rules of correct employment that can be cited in regard a psychological concept, for a rule is simply a standard of correctness in a concept's use, and there are an endless variety of contexts in which the concept's employment might be at issue. Once again, in comparing and contrasting the correct employments of a set of related concepts such as *anger*, *rage*, and *fury*, one is reiterating some of the rules that fix the correct employments of these concepts.

If one were to apply the concept *alpha particle* to a sub-atomic particle consisting of one proton bound to one neutron, then one would be guilty of *misemploying the concept*: One's employment does not square with the rules that fix the correct employment of the concept. Another could justifiably point out this misuse, and offer correction. It would not be the case, however, that the two individuals were offering rival "theories" about the use of the concept *alpha particle*, nor that the incorrect party had failed to learn his *facts* about natural reality. For to know that one applies the concept *alpha particle* to a positively charged nuclear particle, consisting of two protons bound to two neutrons, is to grasp a rule of concept employment, not some fact about constituents of natural reality. Empirical evidence plays no role in this case. But this is not to make the weak claim that "there exists consensus in the use of concepts and signs". It is, instead, to say that, *expressions that lie outside of the rules that fix the correct employments of concepts are nonsense*. One cannot make even an initial intelligible linguistic move with a concept unless he is using it in a manner warranted by the linguistic rules that govern its correct employment, for the instant one steps beyond these rules, his utterances are not *wrong*, but no longer recognizable as English. One's attempts to make coherent claims about natural reality, e.g., alpha particles, are rendered nonsense in the face of concept misemployment. Expressions that violate the rules of language are nonsense and, therefore, make no claims about natural reality, and, hence, cannot be true or false.

The meaning of a concept is patent in an acceptable explanation as to how the concept is correctly employed. Correlatively, one's grasp of the meaning of a concept is patent in one's correct employments of the concept: "The meaning of an expression is not something deeper and more theoretical than what is patent in the accepted practice of explaining this expression; and this practice, like any normative practice, must be familiar to its participants, open to inspection, and surveyable" (Baker & Hacker, 1980, p.70). Hence, explanations of the meaning of a concept necessarily involve the reiteration of some portion of the rules that fix the correct employments of the concept: "Explanations are rules, but, of course, not always or even usually application rules. Their normativity consists in the fact that a rule given by an acceptable explanation provides a standard to judge correct uses of an expression. This may be by way of *grounds* of application, legitimacy of *substitution*, or *criteria of understanding*" (Baker & Hacker, 1980, p.77). To clarify the meaning of a concept is to clarify certain features of its correct employment, i.e., to reiterate how the concept is correctly employed in particular contexts. The grammars of concepts (their rules of correct employment) are not uniform in type. Each concept is correctly employed in its particular fashion, and, hence, what is involved in a clarification of its grammar will be unique to it. The following are examples.[2]

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2 This section owes a great debt to Baker and Hacker (1982), Ter Hark (1990), and Bennett and Hacker (2003).

Example 1 (Baker & Hacker, 1980, p.81): Consider the meaning of the preposition *on*, as in "on your mark", "on your guard", "on the table", "on Sunday", "on duty", etc. The first point to note is that the correct employments of the term *on* are not explained by providing necessary and sufficient conditions. There is no essence that runs through the different expressions in which *on* is featured. If one inquired as to *the* meaning of *on*, the response would be a call for an elaboration of the *context* of employment in which clarification is desired. There is no explanation which fulfills the role of standard of correct use in *all* standard contexts in which *on* is employed. Rather, understanding of the term is shown, and explanation given, by contextual paraphrastic explanation.

Example 2 Colour terms are explained, and an understanding of colour terms is manifested, by the correct employments of colour samples. "What is meant by *red*?": "This → [pointing at a swatch of red cloth] is red". It is a common misconception to view such expressions as describing a *property* of the swatch, or as expressing an empirical hypothesis. In fact, the expression "This → [pointing at a swatch of red cloth] is red" as a response to the question "What is meant by *red*?" shows a rule, i.e., standard of correctness, for the application of the term *red* (Hacker, 1986). To *describe* the cloth as being red would be an application of the concept *red*, and would presuppose a grasp of the rules of correct employment of the term *red* (one would already need to know how to use the term). This highlights a difference between the aims inherent to teaching, and applying, a concept. The aim of the former is to bring about understanding of the meaning of a concept by reiterating features of its grammar, while the aim of the latter is to employ the concept in the service of other aims. The latter presupposes the former. When used to explain the employment of colour terms, colour samples are internally related to colour terms, in the same way that metric terminology and canonical measures (e.g., the standard metre) are internally related. That is, such samples are concrete elements of grammar (Hacker, 1986). Thus, there exists no *evidential* recourse for a person who has been denied acknowledgment of his correct application of the term *red* to a red colour swatch. For his correct employment of the term does not rest on evidence, but, instead, manifests his grasp of a rule of correct employment of the term.

Example 3 The meaning of the concept *game* is explained by exemplification together with similarity clauses: e.g., "soccer and chess are games: both involve players, are played in accord with rules, and result in one player's winning (and the other's losing), or in a draw. Soccer, however, is a sport, whereas chess is a game of skill, etc., etc." Concepts whose meanings are explained in this way are called family resemblance concepts.

2a. Autonomy of concept meaning in regard facts

Linguistic rules are constitutive for correct concept employment (concept meaning). Linguistic expressions are judged as sensical or not in cmparison to linguistic rules. Expressions that accord with the rules of language have senses, and those that violate these rules do not. Empirical propositions (claims about natural reality) are expressed in language. The truth or falsity of a proposition is determined on the basis of empirical evidence, but a proposition can *be* true or false only if it can be determined what *counts* as evidence for its truth. But it is only possible to determine what counts as evidence for the truth of a proposition if it can be determined what it is that the proposition asserts. And one can understand what a proposition

asserts only if the proposition has a sense, i.e., if the terms of which it is comprised, and their combination, accord with the rules of language (Bennett & Hacker, 2003). That which is formed in violation of the rules of language (e.g., "the number two is more red than is purple") cannot have a sense, hence, cannot be a propositon, hence, cannot be judged as true or false.

In contrast to sensical propositions, the linguistic rules that fix the meanings of concepts are not adjudgeable as true or false, for they make no claims about natural reality. The meanings of concepts are neither constituents of natural reality, nor discoverable through the carrying out of empirical research. They, being as they are, fixed by linguistic rules, are autonomous of facts about natural reality. In particular, the rules that fix the correct employments of concept "θ" have no bearing on the empirical properties of the concept's referents (if the concept does in fact denote), and the properties of these referents have no direct bearing on the meaning of concept "θ". The referents of concept "θ" *are* constituents of natural reality and, hence, have properties that can, in principle, be discovered through scientific investigation. But these properties have no direct bearing on the rules that fix what concept "θ" does, in fact, denote (if, in fact, it denotes). On the contrary, fact f is a fact about θ-things only if it is a fact about constituents of natural reality denoted by concept "θ", and which constituents of natural reality are denoted by concept "θ" is given by the rules that fix the correct employments of concept "θ". Hence, the capacity to make discoveries about θ-things *presupposes* a grasp of the meaning (i.e., rules of correct employment) of concept "θ", for only given a grasp of the meaning of concept "θ" can one know *which* constituents of natural reality he is to make discoveries about. Facts about natural reality *can* have an indirect bearing on the linguistic rules that fix the meaning of concept "θ", by, for example, motivating humans to change these rules (Baker & Hacker, 1982).

2b. Empirical investigation and conceptual clarification

While the job of science is, indeed, to make discoveries about, and provide explanations of, aspects of natural reality, the scientist must concern himself with conceptual matters (the correct employments of concepts) because his scientific aims and products are expressed in language and, hence, in terms of concepts. Hence, when the scientist hypothesizes that γ-things have such and such properties, offers a theory as to why γ-things do what they have been observed to do, or sets out to prove the existence, in the far north, of γ-things, his claims are, in fact, about γ-things only if they are informed by a correct employment of the concept "γ". And to correctly employ concept "γ" presupposes that the scientist has grasped the rules that fix its correct employments. This is, of course, why countless scientific papers and texts in biology, chemistry, and physics, contain carefully stated definitions for key concepts. Given the correct employment of the concept "γ", it is a distinct, empirical, issue whether his (coherent) empirical claims are in fact true.

The linguistic rules that are constitutive for concept meaning are simply standards of correctness with respect linguistic behaviour, and, hence, are autonomous of the empirical phenomena of interest to the scientist. Neither concepts, nor the rules that fix their correct employments, are elements of natural reality, and, hence, neither concepts, nor the rules that fix their correct employments, are objects of empirical investigation. Neither concepts, nor the rules that fix their correct employments (i.e., their meanings), are discoverable, for, with respect the meaning of a concept, there is nothing *to* discover, at least not in the sense in which science can set about to discover, say, the chemical composition of a certain substance. The meaning of a concept is fixed by its rules of correct employment, and rules are laid down by humans, and are

open to scrutiny by one and all. One *learns* the meaning of a concept, is *reminded* of its meaning, *teaches* its meaning to others, etc. Physics did not, for example, discover the concept *alpha particle*, nor its correct employments, but, rather, invented the concept (laid down a rule for the correct employment of the term *alpha particle*). It *did* discover the material entities, and many facts about these entities, now known as alpha particles. The notion of a concept, or a concept's meaning, "existing", but unknown to humans, is nonsensical.

That the linguistic rules that are constitutive for concept meaning are autonomous of facts about constituents of natural reality does not imply that the scientist need pay no attention to conceptual matters. Rather, it implies that he has two distinct tasks, the clarification of the concepts that denote the phenomena of scientific interest to him, on the one hand, and the empirical investigation of these phenomena, on the other. If the aim of a program of investigation is the scientific study of η-things, then the aim of the program is to study phenomena signified by concept "η". If there exists uncertainty in regard the correct employments of concept "η", i.e., there exists confusion over its meaning, then there exists confusion over *what*, in natural reality, is the object of investigation. The consequence of research founded on such a compromised conceptual beginning is that it will always remain wholly unclear as to the meaning of any results generated by the research, and, in particular, what these results say about η-things. This is because any empirical findings generated in research are findings about η-things only if the constituents of natural reality to which the findings speak are, in fact, η-things. But to know which constituents of natural reality are, and are not, η-things is, eo ipso, to grasp to which constituents of natural reality concept "η" can rightly be applied, and this, in turn, presupposes a grasp of the rules that fix the correct employments of concept "η". In the event that the meaning of a particular concept is unclear, what is required are not further attempts to accumulate empirical evidence, nor the construction of theories about constituents of natural reality, but, rather, a clarification of the concept's rules of correct employment (Hacker, 1986, p.158). Making salient relevant features of a concept's correct employment constitutes a conceptual (grammatical) investigation. Einstein's pointing out that the correct employment of the concept *simultaneous* was unclear for the case of distantly occurring events, and his retooling of this concept so that it had a sense when applied to distantly occurring events, is perhaps the most famous case of a conceptual clarification freeing science to do its empirical work.

Consider the ordinary language concept *agreeable*. The correct employments of the concept *agreeable* are fixed by linguistic rules. These rules are known by the speakers of English, in the sense that a competent speaker can correctly employ the concept. But this does not mean that a competent speaker typically is in possession of a detailed understanding of the reticulation that these rules form (Baker & Hacker, 1982). On the contrary, the bringing of the meaning of a concept into relief is the product of an active program of conceptual clarification. Rules are simply standards of correctness. Most of the rules that govern the employments of psychological concepts are not anywhere encoded, and many are difficult to elucidate. There can exist certain contexts of application in which the rules do not settle what can and cannot be legitimately done with the concept. Importantly, whereas the rules of employment of the technical concepts of, say, physics, are standardly of the necessary and sufficient condition variety, those of ordinary language psychological terms are not. The behavioural criteria of the concept *agreeable* are "widely ramifying, lacking in unifying employment and not readily surveyable" (Baker and Hacker, 1982, p.229). There is not a finite, well bounded set of behaviours that are the instantiators of the concept. Moreover, the concept *agreeable* has a

number of distinct, but intertwined senses (consider: this was an agreeable evening, you are being falsely agreeable, he was not agreeable to the plan).

The complexity of the grammars of psychological concepts suggests that the task of clarification will not be an easy one. Of particular interest to the psychologist is the dispositional sense of the concept *agreeable*, as in "he is an agreeable sort", or "he is of an agreeable disposition", and as when the researcher characterizes his subjects in terms of their *agreeableness* (see, for example, *agreeableness* as it is used in the "Big Five" personality scheme). The psychologist's interest is in empirical phenomena denoted by the concept *agreeable*, but to bring scientific methodology to bear on the investigation of these phenomena, he must first grasp *which* phenomena are to be investigated. But to grasp which phenomena are the targets of investigation is to grasp the phenomena to which the concept *agreeable* is rightly applied in its dispositional sense, and this, in turn, is to grasp the linguistic rules that govern the dispositional employment of the concept. Hence, prior to launching scientific investigations into dispositional agreeableness, the scientist must clarify the rules that fix the correct employment of the concept *agreeable*, in its dispositional sense.

The following are a sampling of considerations relevant to a clarification of the correct employment of the dispositional sense of the concept *agreeable*, and, hence, to the carrying out of fruitful empirical investigations of dispositional agreeableness.

first/third person application: Certain psychological concepts, e.g., *pain*, *hope* and *desire*, *belief* and *attitude*, are characterized by a first/third person asymmetry of application. For these concepts, first person present tense utterances, e.g., "I have a desire to be a professional cyclist", are not reports on a state of affairs, but are, rather, *avowals*, and, hence, are standardly groundless. Typically, one does not claim, for example, that he desires a soda on the basis of evidence (introspective or otherwise). One *has* desires and needs and manifests them in action and avows them in speach.[3] First-person present tense utterances (e.g., "I want a cookie") are often, but not always, learned extensions of primitive pre-linguistic behaviour (the one year old's temper tantrum as his mother holds the bag of cookies from him) (Baker & Hacker, 1982, p.235). On the other hand, third person ascriptions, e.g., my claim that "he has a desire to be a professional cyclist", are justified by his behaviour in the appropriate circumstances, either his avowal to this effect or his displaying of behavioural criteria for the expression. To summarize, "I do not discover my intentions from introspective observation, I *form* them -and manifest them in speech or action (or keep silent, or abandon them). But my overt avowals of pain, recollection, intention, understanding, etc., constitute criteria for others to ascribe these psychological predicates to me. Their judgments are justified (non-inductively) by such (linguistic) behaviour in appropriate circumstances. My utterances are not *justified* by anything...for they do not rest on evidence of *any* kind" (Baker & Hacker, 1982, p.236).

The behavioural criteria that justify the ascription of a psychological predicate to another individual are *not* symptoms of the concept, nor of some hidden constituent of natural reality that lies behind the concept. The relationship between concept and criteria is not empirical, but, rather, grammatical: The criteria are internally related to the concept, linked to the concept in grammar. Part of what it is to learn the employment of the concept is to learn the criteria by which the concept is instantiated in the third-person mode. The ascription of the desire to be a professional cyclist to another is not *entailed* by criteria of application, for an individual need not

3 The causal story behind a desire (say, e.g., the neuro-chemical precursors to the formation of a desire) is a different (empirical) issue!

manifest his desires. On the other hand, my expression of "hope for a quick resolution to the Iraqi crisis" is a groundless avowal. It is ungrammatical to question my avowal in the absence of disconfirming criteria (e.g., knowledge that I am well profiting from the sale of illegal arms to Iraq). The ascription of "hope for a quick resolution of the Iraqi crisis" to another, on the other hand, is justified by behaviour, either the individual's avowals to this effect or his displaying behavioural instantiators of the concept (e.g., his appearance holding a placard at anti-war demonstations). Criteria are, however, *defeasible* grammatical support for the application of a predicate (Baker & Hacker, 1982). The ascription to another individual of "hope for the resolution of the Iraqi conflict", supported by this individual's making a claim to this effect, and his participation in anti-war rallies, could be defeated by, e.g., knowledge of his behind the scenes arms dealings.

The employment of the concept *agreeable* is not characterized by a first/third person asymmetry. My claim that "I have an agreeable nature" is not a groundless avowal. It, like another's third person ascription of *agreeable* to me, is justified by my manifesting behavioural criteria. The concept *agreeable* shares this feature with concepts such as *intelligent*, *dominant*, and other dispositional and capacity terms. Whereas, in the absence of disconfirming behaviour, there is no logical basis for another individual to question my avowal that "I desire to be a movie star", it is perfectly reasonable for him to question my unsupported claim that "I am an agreeable sort." For the justification for *my* ascription of this predicate to myself is that I have, in fact, behaved agreeably, i.e., I have manifested some of the criteria that warrant ascription of the concept in the third-person mode.

location: Sensations have a bodily location (e.g., the pain is in my knee), while perceptions have no location, but are associated with a particular organ (e.g., visual perceptions with the eyes). In contrast, dispositions such as *agreeableness* neither have a location, nor are associated with an organ, nor are associated with an internal state or mechanism. These features of these concepts are neither empirical facts, nor hypotheses, but, rather, features of their correct employments. The rules that fix the correct employments of sensation terms prohibit their applications to anything not possessing a bodily location. Thus, one cannot correctly apply the term *sensation* to auditory perceptions (which have no location, but are, rather, associated with the ears).

subjection to will: Sensations are not subject to the will, but are, rather, characterized by a passivity. When one *has* a pain, he is experiencing a certain event. Perceptions are, to a certain degree, subject to the will, although when, for example, one takes in a vista one cannot actively edit what one is seeing. Since to be agreeable is to have behaved in a particular way, it makes a certain sense to speak of a person's agreeableness as being subject to his will. But this is not to say that some inner state is subject to the will, for the employment of the concept does not rest on inner states, but, rather, that it is possible to will oneself to change one's behaviour, thereby, in the long run, providing no further justification for the ascription of the predicate agreeable to oneself .

contrasts with similar concepts: Concepts are embedded in a ramifying network of connections with other concepts. An important feature of the clarification of the meaning of a concept is to distinguish its rules of correct employment from those of other similar concepts. Thus, in regard the concept *agreeable*, one would likely want to compare and contrast its correct employment with the correct employments of the concepts *likeable* and *affable*.

2c. Certainty and evidential support

Conceptual and empirical claims are supported in different ways, and can have conferred upon them different brands of certainty. Science is concerned with natural reality. The arbiter of the scientific case is empirical evidence. Regardless of the quality of the evidence brought forward in support of a scientific case, its acceptance as truth is always understood to be only provisional, and always rejectable or revisable in light of new evidence. It is merely convention to view the best case science can offer at any point in time as "truth". That is, "Scientific explanations are empirical, refutable by the facts, revisable in light of new discoveries, and never in principle (but only provisionally, historically) final" (Baker & Hacker, 1982, p.70). On the other hand, it makes no sense to ask whether a given concept, say, *dominance*, is true or false. For a concept is not a proposition. It makes no claims about natural reality that could turn out to be true or false. There is no *truth* in regard the meaning of a concept, there is simply its normative employment. Support for a claim about the meaning of a concept is not provided by empirical evidence, but, rather, by the reiteration of relevant aspects of the concept's normative employment, these known by all who have mastered the correct uses of the concept. Correlatively, it is nonsensical to envision the meaning of a concept as being potentially "incorrect", "not the true meaning", or "merely an imprecise version of the true meaning". For a concept's meaning is fixed wholly by linguistic rules. The illusion that it is not so is usually a result of confusing the meanings of concepts with characteristics of their referents (empirical claims made about a concept's referents may indeed be true or false, and empirical cases constructed about these referents, provisional and open to revision and further articulation). Because a concept's employment is fixed by linguistic rules, correct employments of the concept are instances of language, while employments that violate these rules are instances of nonsense.

2d. Signification and referent

Theoretical psychologist Siu L. Chow (1998, p.482) states that "Denotation serves a semantic function...However, this function of grammar does not give denotation a grammatical role", while psychologists Jost and Gustafson (1998, p.470) state that "If 'denotation' means 'reference', then it is just false that reference is established by grammar or syntax". But these comments are confused. Whether a concept signifies constituents of natural reality, and which constituents of natural reality it signifies if, in fact, it signifies, is solely a grammatical matter. A concept "γ" has a normative (rule governed) employment within a linguistic practice. What constitutes correct, and incorrect, employments of "γ" is fixed by the grammar of "γ". To correctly claim that concept "γ" signifies members of a class of constituents of natural reality, say x-things, is to grasp that "γ" is correctly *applied* to x-things. That is, it is to grasp a rule of correct employment of concept "γ". A concept "γ" that signifies is internally related to that which it signifies, say, x-things: To grasp that "γ" is correctly applied to x-things is to grasp part of the meaning of "γ", i.e., a rule that fixes a correct employment of "γ".

The teaching of the correct employments of a concept that signifies (a denotative concept), the explanations offered in regard its meaning, the justifications offered for its correct employments, etc., paradigmatically involve the use of samples and ostensive definition. What is meant by the term *flower*? Pointing to a daisy I reply, "*this* → is a flower", and then, pointing to a rose, "*this* → is a flower". This characteristic response is not, in this context, a *description*

of reality, it is not a description of properties of the objects to which I have gestured, but rather shows a feature of the correct employments of the concept *flower*. The purpose of the response is to teach an element of grammar (a rule of correct employment). To see this, consider the following test: Is there *evidence* that I could offer to support my assertion "*this* → is a flower"? No there is not, for I have not made an empirical claim that is now in need of evidential support. I have, rather, stated what I believe to be a rule of correct concept employment. I have either stated correctly or incorrectly, and which is the case is determined by comparison of my assertion to the linguistic rules that fix the employment of the concept *flower*. Any "evidence" I might provide would have to be about flowers, and to determine whether such evidence was, in fact, about flowers (even to call for such evidence) would presuppose an understanding of the meaning of *flower* (to settle this meaning question was the purpose of the original rule) (Ter Hark, 1990).

It is nonsensical to suggest that it is possible to both grasp the meaning of a denotative concept, and yet not know whether the concept has a class of referents, or what it signifies. On the other hand, given clarity in regard the meaning of a denotative concept, it is not nonsense to be unsure about whether or not its referents actually exist, or, if they have been proven to exist, their empirical properties. Questions about the *natures* of the members of the class of referents of "γ", if "γ" does, in fact, denote, are empirical issues, and, hence, are a matter for science. When the referents of a concept exist, scientists can study them, and when members of a class of referents are hypothesized to exist, scientists can attempt to prove their existence. However, by proving the existence of x, science has proven the existence of θ-things, only if the rules of application of the concept "θ" warrant application of "θ" to x. Similarly, by establishing a fact about x, science has established a fact about a θ-thing, only if the rules of application of the concept "θ" warrant application of "θ" to x.

2e. Hypothesis, theory, and law

The scientist's job involves the construction of useful explanatory theories, the formulation and testing of hypotheses, and the establishment of laws. A theory is a set of propositions offered as an explanation of some segment of natural reality. An hypothesis is a proposition about some element of natural reality. A law is a proposition about some aspect of natural reality that has been accepted as being true. Hypotheses, theories, and laws are about constituents of natural reality, and are provisionally and tentatively supported by empirical evidence. However, hypotheses, theories, and laws are expressed in language, and, hence, before they can be judged as true or false on the basis of evidence, they must have a sense (they must actually assert something). The truth (falsity) of an hypothesis, for example, can only be determined in the light of evidence *given* that its sense (meaning) has been settled. For if its sense has not been settled, it will remain unclear as to what it is asserting, and, hence, what would constitute evidence in support of that which it asserts. Whether a certain sentence formulates an intelligible hypothesis will depend upon its constituent technical and non-technical expressions and their mode of combination in the sentence (Bennett & Hacker, 2003). If an hypothesis is formulated in a manner that lies outside of language, then it is nonsensical, claims nothing, and, hence, cannot be addressed by science.

An hypothesis, theory, or law about constituent x of natural reality is an hypothesis, theory, or law about γ-things only if the rules of employment of concept "γ" warrant application of "γ" to x. Once again, fruiful empirical investigations into γ-things presupposes a grasp of the

rules of employment of concept "γ", for these rules determine to *which* constituents of natural reality concept "γ" can rightly be applied, and, hence, about which constituents of natural reality one is hypothesizing or theorizing when one hypothesizes or theorizes about γ-things. It has been discovered, through psychological experimentation, that "judgments of the loudness of sounds produced by particular events" are approximately logarithmically related to the intensity of these sounds. Consider the pre-requisites of the formulation of this law. As noted, to establish the existence of such a correlation, it must be possible to antecedently identify its relata, in this case "loudness of sounds" and "intensity of sounds".

The latter phenomenon is signified by the concept *sound intensity*, a technical concept whose definition is provided by physics, to wit, "the energy per second transported through a unit area by the sound wave created by a particular source (the unit area perpendicular to the source)" (Bueche, 1972). Sound intensity, then, can be seen to be equivalent to "power per unit area", and since power is measured in watts, the units of measurement of sound intensity are watts per metre-squared (w/m^2). The rustle of leaves produces an energy of, roughly, 10^{-11} w/m^2, while a jackhammer produces 10^{-2} w/m^2. On the other hand, to make correct judgments about the loudness of sounds, to compare the loudness of one sound to another, is a perceptual capacity. To come to possess this capacity, and exercise it in the course of life, presuposses a great deal, including the proper functioning of various physiological structures (the auditory organs and their neural accompaniments), and the mastery of the correct employments of ordinary language auditory perceptual predicates. Given non-pathological physiological structures, children can usually be taught the correct uses of expressions such as "it is very loud", "the noise is deafening", "it is so loud, I can't hear a thing", etc., in much the same way that they are taught colour terms: Through the use of samples. For example, the mother and child encounter a construction site in which a very loud machine is at work, and the mother comes very close to the child and says "oh, that *is* loud". She sees that the child is upset by the rackett and tells him "put your hands over your ears". In doing so, she is teaching her child the correct employments of auditory perceptual predicates. If something is corrupt in the child's neurology, then he may be unable to learn the correct employments of auditory perceptual predicates, and, hence, may fail to develop the capacity to make judgments about the loudness of sounds. The law linking the loudness of sounds to their intensity presupposes normative employments of the concepts that denote the phenomena involved. In this case, the rules that fix the normative employment of *sound intensity* come from physics, and those of *sound loudness* (its cognates and related auditory perceptual terminology), from ordinary language.

On the other hand, *hypotheses, theories, and laws about constituents of natural reality γ have no direct bearing on the meaning of concept "γ"*. The meaning of concept "γ" is fixed by its linguistic rules of employment, and such rules, laid down by humans, are autonomous of facts about natural reality. The meanings of the expressions contained in an hypothesis are independent of the beliefs of the hypothesizer, the truth or falsity of the claim asserted in the hypothesis (if it asserts anything), and the properties of any entities denoted by the constituent concepts.

2f. Existence

The concept *existence* has many different senses. Consider the following paradigm case involving the existence of a material entity.

(Definition): *meson*. A sub-atomic particle containing one quark and one antiquark.

The employment of the concept *meson* is governed by necessary and sufficient conditions, these conditions given in the definition above.

The scientist can inquire whether: i) there exists a material entity to which the concept *meson*, defined as above, can be applied; ii) (equivalently) there exists at least one material entity that is rightly called a meson (concept *meson*, defined as above); iii) (equivalently) there is in existence at least one referent of the concept *meson* (defined as above). These are essentially equivalent ways of asking a question about the existence in nature of a material entity, and, since it is about nature, this question is properly addressed by scientific, empirical inquiry. It is undeniably one of the jobs of science to answer questions regarding the existence of material entities, forces, sources of energy, etc. Observation, hypothesis formulation and testing, theory construction, and many other tools, can be brought to bear on such a question. *But for science to answer whether there does, indeed, exist in nature at least one γ-thing (e.g., inorganic material entity, life form), scientists must grasp the rules that fix the correct employments of the concept "γ"*. For only then will they grasp what is meant by the question, what they are to search for, what does and does not constitute relevant evidence. If scientist A claims that "I have proven that γ-things exist", and, in support of his claim, produces object x from his samples container along with the exclamation "I present to you a γ-thing", he has, in fact, presented a γ-thing only if the rules of employment of concept "γ" warrant application of concept "γ" to x. At the time Mendeleyev provided a criterion of application for the concept *scandium*, and hypothesized the existence in nature of this element, its existence had not yet been established. But Mendeleyev's criterion allowed scientists to go about trying to prove the existence of scandium because they knew *what* they were looking for, namely an element to which application of concept *scandium* was warranted by Mendeleyev's criterion.

2g. Correlates

A constituent of natural reality can co-occur with other constituents of natural reality. While the co-occurrence of two constituents of natural reality can rightly be viewed as an empirical phenomenon, what *constitutes* a co-occurrence is a matter for humans to decide. For this is a matter of laying down rules for the correct employment of the concept *co-occurrence*. Within the social and behavioral sciences claims of co-occurrence usually rest on the reported values of various quantitative paraphrases of the concept *co-occurrence* (correlation coefficients of various types) in samples drawn from populations under study. The logic at work is roughly as follows: i) Let two random variates, **X** and **Y**, represent two constituents of natural reality, φ and τ, respectively. That is, let the distributions of **X** and **Y** in population P_T under study contain scores that are signified by concepts "φ" and "τ", respectively; ii) A real valued function, ρ(**X**,**Y**), defined on the [-1,1] interval, a correlation coefficient, is invented to quantify the degree of covariation of **X** and **Y**; iii) If $\rho(\mathbf{X},\mathbf{Y})\neq 0$ in P_T, **Y** is a correlate of **X** (and **X** of **Y**) and constituents φ and τ are said to co-occur.

Once again, the variates are only of interest to the scientist in so much as they represent the natural phenomena he wishes to understand. Note also that concepts do not have correlates, nor do they co-occur. Certain concepts signify constituents of natural reality, and the study of the complex web of co-occurences of these constituents is one of the tasks of the scientist. In

investigating which constituents of natural reality co-occur with constituents x, science is investigating what co-occurs with θ-things, only if the rules of application of the concept "θ" warrant application of "θ" to x. Note also that, in regard the usual correlation coefficient based treatments of co-occurrence, this means that the correlation between **X** and **Y** is *about* the co-occurrence of constituents of natural reality φ and τ, only if the scores on **X** and **Y** are signified by concepts "φ" and "τ", respectively. That a correlation can rightly be interpreted as a fact about two constituents of natural reality presupposes a great deal on the conceptual side of the ledger.

2h. Causality

A phenomenon (some constituent of natural reality) can have causes, other phenomena that bring about its occurrence. Once again, causality can rightly be viewed as an empirical phenomenon, but humans decide upon what it *means* for one thing to cause another (i.e., they create the rules for the employment of the concept *cause*, and other related concepts (*stimulus*, *sufficient condition*, *antecedent event*)). Social and behavioral scientists attempt to study the causal relations that exist amongst constituents of natural reality through the employment of various quantitative paraphrases of the concept *cause* and the representation of natural phenomena by variates. However, neither concepts nor variates have causes. Certain concepts signify constituents of natural reality, the latter possessing causes. It is one of the tasks of science to come to discover, describe, and explain the causes of constituents of natural reality. It is essential to remember, however, that the investigation of a causal claim to the effect that "(constituent of natural reality) A is a cause of (constituent of natural reality) B" presupposes the capacity to antecedently identify A and B, which, in turn, presupposes an understanding of the rules of employment of the concepts that denote each of A and B. For, otherwise, it is wholly unclear what such a claim means, and, therefore, what would constitute evidence to support such a claim. This point can be made in a slightly different way, by noting that, in investigating the causes of natural phenomenon x, science is investigating the causes of θ-phenomena, only if the rules of application of the concept "θ" warrant application of "θ" to x.

3. *Confusions that have undermined the efforts of psychometricians*

The failure to grasp the natures of conceptual and empirical issues, and, consequently, the place of these issues in scientific work, has produced within psychometrics a number of confusions that have militated against the realization of the stated aim of the discipline, the development of "psychology as a quantitative rational science".[4] The vast majority of these confusions are conceptual in nature, existing at the interface of the mathematics that are created so plentifully by the discipline, and their application to empirical problems in the social and behavioural sciences. The English language contains a very large number of concepts that can be called psychological concepts: examples include *agreeable (ness)*, *dominant (ance, ate)*, *gregarious*, *hope*, *wish*, *desire*, *reason*, *infer*, *ponder*, *recall*, *forget*, *sad*, *happy*, *nervous*, *anxious*. These are the concepts in terms of which humans conceptualize their own, and others', psychological lives. To be able to ponder on one's own psychology, and the psychologies of others, is a gift of language, and without this gift, the social and behavioural sciences could not have been conceived of, let alone begun. The social scientist who claims an interest in, for

4 This is the raison d'etre of the discipline as stated in the journal *Psychometrika*.

example, memory would, apparently, be claiming an interest in phenomena denoted by the ordinary language concept *memory* (its cognates and related terminology), a concept that has been in the language for approximately six-hundred years. Certainly English speaking humans have been discussing memory phenomena for far longer a period of time than the life of the discipline of psychology. If the social scientist is interest in memory phenomena and this phenomena is *not* denoted by the ordinary language concept *memory* (its cognates and related terminology), than the onus of clarification is on her.

The fact that the social and behavioural sciences are founded on a bed of ordinary language psychological concepts has meant that the psychometrician, whose job it is to produce quantitative tools for use in these sciences, has been forced to try to make sense of the place of these concepts in scientific work, and with respect quantitative tools. The practice of latent variable modeling is perhaps the most notable instance of his failure in this regard. For the psychometrician, under the influence of the Central Account, believes that he possesses technologies whereby, when certain requirements are met, desires, traits, abilities, capacities, dispositions, etc., can be detected and measured. The Central Account wrongly portrays the latent variable model as a detector of properties/attributes (causes), the scores that comprise the distribution of random variates $\boldsymbol{\theta}$ to $\underline{\mathbf{X}}$ as measurements with respect these alleged properties/attributes (causes), and urges the modeller to "interpret the latent variate", i.e., to apply an ordinary language concept name to that which he believes he has detected. Confusions over the place of psychological concepts in the work that is to be undertaken have led to these, and many other, misguided attempts at solutions to problems and pseudo-problems. Herein are presented examples of conceptual/empirical confusions.

3a. Psychological concepts are often mischaracterized as being "primitive or folk theories", hunches, connotations/feelings, thoughts, "common sense", or essences to be discovered.

Torgerson provides the following description of the place of concepts in psychological science:

> The concepts of theoretical interest tend to lack empirical meaning, whereas the corresponding concepts with precise empirical meaning often lack theoretical import. One of the great problems in the development of a science is the discovery or invention of constructs that have ... both.
>
> The more "theoretical" constructs are often not far removed from simple common-sense or prescientific conceptions. Though they have a great deal of common-sense meaning attached to them, the meaning is not specified precisely. The terms are thus somewhat vague, and more often than not are complex. Before a satisfactory state of affairs is reached, it is necessary somehow to transform these inexact, complex concepts into exact ones which can be specified precisely. This is what Carnap (1950) calls the task of *explication*. Though this task seems common to all sciences, it is particularly acute in those disciplines that are in their initial stages of development. It is especially true at the present time in the social and behavioral sciences, where an immense amount of time has been devoted to construction of complex and elaborate theoretical superstructures based on unexplicated, inexact constructs (1958, p.8).

Torgerson claims that the more theoretical constructs, "those of interest", are often not far removed from common sense. But what he is, in fact, referring to are, not surprisingly, the concepts of ordinary language. It is these concepts, not anything to do with "common sense", that denote phenomena of interest to the psychologist. Because Torgerson fails to grasp the relation between empirical and conceptual issues, he confusedly employs the expression "empirical meaning", suggests that concepts can be evaluated for their "theoretical import", equates concepts with constructs without clarifying what he means by either, and speaks of concepts as having "common-sense meaning". If there exists a sensible reading of the expression "empirical meaning", Torgerson does not indicate what it would be. It has the ring of a conflation of empirical and conceptual issues, for the meaning of a concept is not a constituent of natural reality, and, hence, is not an empirical issue. Concepts do not get their meanings from constituents of natural reality (the empirical), but rather from their normative roles in language. Certain concepts do, of course, signify constituents of natural reality. But the characteristics of these referents have no implications for the meanings of such concepts. Similarly, it is unclear what Torgerson means by a concept's having "theoretical import". Certainly, a concept can be featured in an important theory (or, for that matter, any combination of important and unimportant theories) and the existence of a certain concept might stimulate scientists to invent important theories. So too, empirical discoveries sometimes necessitate the wholesale revision of a conceptual system currently in use in scientific work. If this is what he means, then this is innocuous. However, since, paradigmatically, it is discoveries about features of natural reality that have theoretical import, in so much as discoveries can lead to the refutation of, or revision of, one or more theories, it is likely that Torgerson has simply conflated conceptual and empirical issues. Finally, under no charitable reading of these expressions can a concept be said to have "common-sense meaning" (as if there are various brands of concept meaning, among them common sense and, alternatively, "true" or "rigorous" meaning). A concept has a meaning, and its meaning is made clear by reiterating the linguistic rules that fix its correct employments. A concept is not an opinion or proposition, and, hence, cannot coherently be evaluated for its common sensicalness. It is not a primitive or folk theory. It does not lie on the same continuum as primitive/folk and sophisticated/scientific theorizing.

Theories, hunches, and hypotheses *do* express claims about the world, and, hence, can, at least in principle, be shown to be false (true) or unsophisticated (sophisticated). A primitive theory can be shown to be wrong-headed, and, eventually replaced by a sophisticated scientific effort. The Copernican account of the movement of the planets, which depicted them as having spherical orbits about an imaginary center point, was eventually shown to be wrong-headed in light of evidence gathered by Tycho Brahe, this evidence sculpted into a scientific case by Johannes Kepler. The truth of the hypothesis "women are, in general, less dominant than men" is, at least in principle, adjudgable by empirical evidence. If it were true, a common-sense (or sophisticated) explanation for it could be offered. But the concept *dominance* is neither true, nor false. Nor does it possess a "common sense meaning". It simply has a meaning, and this meaning is manifest in its correct employments. There can be no "real" meaning of the concept *dominance* beyond that fixed by the concept's grammar, for even the phrase "real meaning of the concept *dominance*" depends for its sense on the meaning assigned to the concept *dominance* by the rules of ordinary language. Theories and hypotheses are stated in terms of concepts, and, hence, their *coherence* (but not, if they are coherent, their truth/falsity) rests on the correct employments of the concepts in terms of which they are expressed.

Confusion over these issues is widespread within the social and behavioural sciences. In an earlier exchange on some of these issues, Jost and Gustafson (1998, p.475) discuss the place of theoretical innovation in science and conclude that "We then refine our conception and note that some or all of the platitudes of common sense…('common-or-garden concept' domain) may be false or, in other cases, simply unanticipated by common sense." In this quote, Jost and Gustafson begin by conflating "concept meaning" with "conception", the latter having a sense, in this context, that runs roughly parallel with "account of", "opinion in regard", or "hunch about". They follow this initial mis-step by conflating "the platitudes of common sense" with "the meanings of concepts". The "platitudes of common sense" are claims about the world, and, hence, are correctly characterized as either true or false. But the meaning of a concept (i.e., its employment in a linguistic practice) is neither true, nor false, and certainly is not a set of empirical propositions which could be "unanticipated by common sense".

Psychologist Siu L. Chow takes precisely the same mis-steps in his assertion that "It is important to realize this because what is true at the level of using psychological concepts in everyday life need not be true at the level of investigating empirically psychological phenomena denoted by their respective concepts" (1998, p.483). Once again, concepts can neither be true, nor false, for they make no empirical claims. They, therefore, can neither square, nor be at odds, with *facts* about the constituents of natural reality that they denote (if they do, in fact, denote). If concept "ψ" denotes ψ-things, these being constituents of natural reality, then the rules of employment of concept "ψ" settle *which* constituents of natural reality are to be studied in a study of ψ-things (namely, those to which the rules of employment of concept "ψ" warrant application of concept "ψ"). But these rules, standards of correctness in the use of concept "ψ" in "everyday life", make no claims about natural reality, and, hence, cannot be "true" or "false".

It is, similarly, commonplace for researchers to misrepresent the meaning of a concept as a "connotation" or "feeling". But this is to confuse the normative, rule governed, employment of a concept, with emotional *accompaniments* of the concept's use. When I employ the proper name "Verona", I can practically taste risotto, but neither risotto, nor my imagining of its taste, has any bearing on the correct employment (i.e., the meaning) of the proper name Verona. My imagining of the taste of risotto is an accompaniment to *my* uttering of the name Verona, and, of course, the name Verona will likely stir in Joe and Sue different emotional accompaniments.

McDonald and Mulaik's (1979, p.305) discussion of variate selection includes the claim that "...they do not begin with a set of relatively clear psychological concepts and then select variables for their study that on the basis of theory should contain or be determined by these concepts." But this sentence contains the false implication that "whether a variable can rightly be said to be signified by a given concept is a matter for theory to decide", this being equivalent to the mistaken claim that "theory determines the grounds of application of concepts." This sentence also contains the outright incoherence of the claims that "variables contain concepts" and "concepts determine variables". And so it goes. In discussing the notion of a domain of variates, McDonald and Mulaik refer to the "...empirical content of the additional items..." (1979, p.306). What could be meant by "empirical content"? Items do not *have* "empirical content" any more than they have "empirical meaning." At best, it might be the case that particular responses to items provide grounds for the instantiation of particular concepts.

Neither psychological concepts, nor concepts in general, are *thoughts*. Rather, thoughts, on any of a number of topics, are *expressed* in language, and, hence, in terms of concepts. Thus, one's capacity to form and express one's thoughts presupposes mastery of the employments of the concepts contained in these expressions. Thinking is a capacity which presupposes linguistic

mastery: "As the linguistic superstructure evolves, the *possibility* of thought and experience grow and expand. As temporal expressions are mastered, the child becomes *capable* of wanting not only *this* toy, here and now, but a toy of such-and-such features next Christmas" (Baker & Hacker, 1982, p.236). A psychological concept does not stand for any "essence" whatsoever. Certain concepts denote features of reality, and this simply means that their rules of employment warrant their application to these features.

Contrary to what is implied in many discussions found within the social and behavioral sciences of the place of concepts in science, language is not an annoying obstruction to the comprehension of natural reality. Rather, what is meant by *natural reality*, and what is meant by references to particular constituents of natural reality, is fixed in language. Whatever can be said about the constituents of natural reality can be expressed in language, and, hence, presupposes language. The capacity to make, reveal, and record discoveries about the constituents of natural reality presupposes language. Whatever one might *feel* lies beyond that which can be said, cannot be articulated. One cannot step out beyond language and say anything about reality. Language provides the concepts in terms of which both the regular citizen and the sophisticated scientist can make claims about the features of natural reality that these concepts denote. Hence, it is not the case that the messiness of the grammar of a psychological concept such as, e.g., *gregariousness*, is obscuring the social scientist from seeing the "truth" about dispositional gregariousness. For if he is interested in the phenomena of dispositional gregariousness, then he is interested in precisely those features of natural reality denoted by the concept *gregariousness*, i.e., those features of natural reality to which the grammar of the concept warrants the concepts application. Phenomena *not* denoted by the concept *gregariousness* are not the phenomena of dispositional gregariousness.

3b. "We are not interested in the concept *dominance*, but in dominace as it exists in the world."

The social and behavioural scientist is a scientist and, hence, one of his jobs is to make *empirical* discoveries. He is, therefore, correct in affirming his interest in "dominance as it exists in the world". However, the implied *disinterest* in the concept *dominance*, and other ordinary language psychological concepts, is gravely misplaced. For, if his investigations are to bear fruit, the scientist will need to know *which* features of natural reality to study. Because his stated interest is to make discoveries about dominance, his stated interest is in making discoveries about phenomena signified by the concept *dominance*. And these phenomena are precisely those to which application of the concept *dominance* is warranted by its grammar. Thus, if he is to realize his aim of making contributions to our understanding of "dominance as it exists in the world", he will first have to come to an understanding of the linguistic rules that fix the correct employment of the concept *dominance*. If he fails to do so, he will be unable to justify his claim that the "evidence" he has generated in his studies bears on "dominance as it exists in the world", just as the physicist who claims to have made amazing discoveries about mesons is unlikely to win a Nobel prize if it turns out that he has been studying particles containing no anti-quarks (whatever it is that he has been studying, it certainly hasn't been mesons).

3c. "We currently don't know what *anxiety* means. We are engaged in scientific work to answer this question" (Cronbach & Meehl, 1955,);

"because factor analysis has commonly been treated as a theory-generating device; that is, it has been treated as a device for the *post facto discovery of the psychological concepts* [italics added] that explain the correlations of the variables one has chosen to measure" (McDonald & Mulaik, 1979, p.298)

"Jackson and Maraun say that item intercorrelations have nothing to do with the meaning of items. Test constructors know this is not so. The more similar the meaning (to the test constructor and presumably the respondent) the higher the correlation between the items" (Zuckerman, 1996).

Disregarding Mulaik and McDonald's mischaracterization of psychological concepts as theories (devices that "explain the correlations..."), the misconceptions expressed in these quotes are that: i) Facts about the referents of a psychological concept are relevant to an understanding of the meaning of the concept; ii) The meanings of psychological concepts are *out there*, and, as with any feature of natural reality, can be discovered through the doing of science. These misconceptions arise from a failure to distinguish between a concept, an element of language, whose normative employment is fixed by rules, and its class of referents, given that it does, in fact, denote. These miconceptions were enshrined in the construct validation program of Cronbach and Meehl (1955).

The scientist's primary job in regard anxiety is to study those features of natural reality (behavioural phenomena) that are *denoted* by the concept *anxiety*. The scientist could potentially come to discover that men over forty are more anxious than women, and that high anxiety tends to accompany poor performances in certain test situations. The scientist must provide the empirical evidence to support these claims of discovery. These are the ends toward which the tools of science, in the hands of a capable scientist, can be put. However, to be able to make the case that f is a fact about anxiety presupposes a grasp of the meaning of the concept *anxiety*. For f is not a fact about anxiety unless it is a fact about a feature, ρ, of natural reality denoted by the concept *anxiety*. And to be able to make the case that ρ is denoted by the concept *anxiety* presupposes a grasp of the rules of correct employment of the concept *anxiety*.

Science will not be making empirical discoveries about the meaning of the concept anxiety because the meaning of *anxiety* is not an element of natural reality. It is not *out there* to be discovered and investigated. The concept *anxiety* is an element of language, and language is a human creation. Its meaning, what one means by the term, is fixed by linguistic rules, and is patent in appropriate explanations of the correct employments of the term. Such explanations are reiterations of the rules that fix the correct employment of the concept *anxiety*. Concepts have normative, rule governed employments, and, hence, are familiar (although not always transparent) to, and open to inspection by, language speakers. A clarification of the meaning of *anxiety* is brought about by engaging in grammatical investigations, i.e., by clarifying the concept's rules of correct employment.

Facts about that which is denoted by *anxiety*, e.g., that men over forty are more anxious than women, have no direct bearing on the meaning of the concept *anxiety*. Grammatical rules are "autonomous, arbitrary, not justified by reference to reality" (Baker & Hacker, 1980, p.76). Thus, it is deeply confused to suggest, as do Zuckerman (1996) and so many others, that the values of correlations can have any relevance to the meanings of concepts. The former are summaries of certain features of joint distributions of variates, while the latter are fixed by the rules of language. To correlate properties of alpha particles with other physical properties

reveals facts about natural reality. But the meaning of the concept *alpha particle* is fixed by the linguistic rules that fix the correct employment of the concept *alpha particle*. It is precisely because of the autonomousness of the rules that fix the sense of the concept "η" that denotes constituents of natural reality, say, η-things, that a fact, f_η, about η-things *remains* a fact about η-things after science comes to know fact f_η. For, if knowledge of f_η altered the meaning of "η", i.e., directly altered the rules that fix what constitutes correct employment of "η", then, given knowledge of f_η, "η" would no longer denote in the same way, and f_η, based as it was on a different, previous criterion of η-hood, would no longer *be* a fact about η-things. The autonomous linguistic rules that fix the correct employment of denotative concept "η" fix *which* constituents of natural reality are rightly called η-things. *Given* this conceptual foundation, the scientist must come to understand the empirical natures of η-things.

If the social and behavioural scientist is to successfully investigate, e.g., capacities, dispositions, attitudes and emotions, then he faces two markedly different tasks: i) He must clarify the correct employment of the concept that denotes the phenomena of interest to him (so that he is clear about *what* he is to investigate); ii) He must employ the tools of science to investigate this phenomena. However, the space age social or behavioural scientist is likely to view such an account of meaning as unsophisticated at best. He may tell himself that the most powerful tools available for coming to an understanding of pretty much anything are the tools of science.[5] Issues of meaning are, he is likely to feel, at the least *both* conceptual and empirical in nature, and, hence, open, at least to some degree, to being addressed by empirical, scientific investigation. This middle ground position is profoundly mistaken.

The rules of language fix the correct employments of concepts, and they are autonomous of facts about the constituents of natural reality denoted by concepts. Imagine that, via a scientific investigation, I accumulate evidence that I believe constitutes a discovery about the meaning of the concept *anxiety*. I claim, based on my empirical evidence, that *anxiety* is correctly applied in accord with rule r_h. Could this truly be a discovery, and new fact, about the meaning of *anxiety*? No it could not. My discovery is a discovery about the meaning of *anxiety* only if r_h is a rule that does, in fact, fix some feature of the employment of the concept *anxiety*. But to be able to *recognize* r_h as a rule that fixes some feature of the employment of *anxiety* is already to grasp part of the meaning of the concept *anxiety* (to wit, that r_h is indeed a rule for the correct employment of the concept *anxiety*). The situation is *not* akin to, say, the discovery of a new stellar body, that, until its discovery, was unknown to humans. For, by their very nature, meanings are settled by rules laid down by humans *prior* to the use of linguistic signs. One either does, or does not, grasp these rules, but, in either case, no *discoveries* are required.

The widespread belief that facts about constituents of natural reality both constitute knowledge about these constituents and progressively and directly modify the meanings of the very concepts that denote these constituents is not only badly confused, but trivializes scientific practice. If, after many years of attempts to do so, science has still not proven the existence of , e.g., φ-things, this in no way suggests that the concept "φ" that denotes φ-things is problematic and in need of modification. It suggests, rather, that either φ-things are hard to find in nature, the scientific approach that has been used in the search for φ-things is in need of modification, or that φ-things simply do not exist. In fact, to entertain any of these possibilities pressuposes a meaning for the concept "φ" that is antecedent and fixed in language. Similarly, while newly

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5 He would do well to examine the operation of other sciences. If he did, he would observe the striking fact that a not insignificant portion of science is non-empirical.

collected facts about φ-things that happen not to square with received theory about φ-things might undermine such theory, they could not possibly undermine the *meaning* of the concept "φ" that denotes φ-things. To modify the concept itself in the face of disappointing empirical outcomes is to trivialize the scientific process. Imagine the possibilities: i) In the space race pitting the Americans against the Russians, the Russians declare in 1964 that they are the first on the moon (New definition: *moon*: The highest point on the highest mountain in Russia); ii) Professor Ernest Failsley declares that he has conclusive evidence that there does exist life in space (New definition: *space*: That area between the outside wall of Ernest's house, and the local pub).

The employment of the dubious technique of re-definition in the face of unhappy empirical outcomes makes it possible for theories to be unfalsifiable. Consider, once again, the case of phlogiston theory. Phlogiston theory posited the existence of an invisible substance possessed by all combustible objects, that is released by these objects during combustion. The concept *phlogiston*, then, denoted a constituent of natural reality whose existence had not been established. But phlogiston theory was shown to be false, and the concept phlogiston was shown not to denote anything that exists in nature. Now, imagine that in the face of phlogiston theory's imminent demise, the concept *phlogiston* had been altered so as to denote something *other* than what it had originally denoted, something perhaps that was *known* to exist. Obviously, then, the providing of evidence for the existence of this new thing would have to have been taken as support for phlogiston theory. But this surely would have been a trivial and hollow victory.

3d. "In spite of the great social and scientific usefulness of psychological tests it must be acknowledged that for the most part we have had very inadequate ideas as to what it is they actually measure. The plea is frequently offered in defense of tests that, by analogy, we do not know the whole truth about electricity and yet we do not question the right of the physicist or the engineer to measure it." (Thurstone, 1954, p.470)

"In any theory of latent traits, one supposes that human behavior can be accounted for, to a substantial degree, by isolating certain consistent and stable human characteristics, or *traits*, and by using a person's values on those traits to predict or explain his performances in relevant situations" (Thurstone, 1954, p.537)

The first quote expresses the Central Account notion that psychological tests measure things, but that the humans that construct and use these tests may not know *what* they measure. Tests are, as it were, "nets" which are cast into the sea of psychological phenomena in confidence that something will be caught. The problem is thought to be that the "fish" that make up the haul are "unobservable", and, hence, cannot be unproblematically identified. However, as is clear from the second quote, the net-like tests are likely to catch traits, the latter viewed as entities residing in natural reality. It was argued in Chapters VII to IX that this notion is incoherent. One *takes* measurements of property κ with an instrument created to measure κ, expresses such measurements in appropriate units, and expresses the results in language. One can do things *incorrectly* and produce a result that is not a measurement of κ. The point is that where there can be correct and incorrect behaviour there must be human created standards of correctness for these activities. Standards of correctness establish what it means to take measurements of κ (and, hence, to make errors in attempting to measure κ), to express the results of measurement operations in appropriate units (and, hence, to err in the employment of

units of measurement), to make mistakes, etc. The status of an instrument as a measuring tool is enshrined in a normative practice of measuring and, hence, there can be no doubt about the status of an instrument as a measurement instrument if, indeed, it plays this role, nor, similarly, *what* it can be used to measure if it can be used to measure something. These issues are not empirical issues and, hence, evidence about natural reality plays no role in their adjudication. Furthermore, while there may well exist stable patterns of behaviour currently not denoted by any psychological concept, as it stands, what it is to grasp the traits a person can have is nothing other than to have mastered the employment of the trait-terms of language. For traits are nothing other than those things that are denoted by the trait-terms of ordinary language. It is language that provides the tools in terms of which scientists can describe the psychological features of individuals.

The point to note, here, is that the incoherence of Thurstone's portrayal results from his failure to grasp the relation between conceptual and empirical issues in science. He misportrays the grammatical relations that link measurement instruments to the properties they measure within measurement practices, as empirical facts to be discovered. His inability to grasp the relationship between conceptual and empirical features of science is further manifest in his reference to electricity. That physicists can measure certain of the properties of electricity (e.g., potential and resistance) results from the concepts that denote these properties being embedded in normative practices of measurement. The rules that fix the correct employments of these concepts *warrant* their appearance in measurement sentences. The fact that humans have layed down rules that fix correct behaviour within the practice of, say, measuring electrical resistance, allows science to then study the empirical properties of electrical resistance. Facts about electrical resistance have no bearing on the meaning of the concept *electrical resistance*, nor its place within the normative practice of measuring electrical resistances.

3e. "Indicators" and the "real thing".

Dispositional terms such as *agreeable*, *dominant*, *consciencious*, *brilliant*, etc., are often portrayed in the social and behavioural sciences as standing for something inside the individual, the ascription of these concepts to a given individual seen as an hypothesis that the individual possesses this inner (unobservable) thing. Chow (1998, p.483), for example, expresses his belief that what the concept *intelligence* denotes is "...some hypothetical structure, mechanism, or state...". If not inside the individual, then the "real thing", for which a concept is but a façade, is an element of an unobservable domain (this is the CAC and CAM). Thus, the social and behavioural scientist speaks of there being "symptoms" or "indicators" of dominance, and attempts to assemble a list of these indicators that can be used, often as input into a latent variable analysis, to make inferences about the "real entity" that the concept stands for. But this portrayal is badly confused. The grounds for the correct ascription of, for example, the term (very-, somewhat-, moderately-) *agreeable* to an individual in the present tense, third-person mode are fixed by linguistic rules, for linguistic rules fix the correct employments of concepts.

The normative, rule governed employments of dispositional concepts do not square with the portrayal of their third-person ascriptions as being "hypotheses of inner mechanisms, states, or structures." In particular, a dispositional concept's third person present-tense ascription to another individual is not justified by anything *inside* the individual, nor, certainly, to platonic essences in unobservable domains, but, rather, by the individual's having behaved in a certain

way, in particular circumstances (i.e., on the basis of criterial behaviours[6]). *The behavioural criteria of a concept are not "indicators", but are, rather, grammatically (internally) related to the concept. That is, their relations to the concept they instantiate are fixed in grammar.* Language users learn the behavioural criteria that instantiate a dispositional concept as part of their learning to correctly employ the concept. Part of the meaning of the concept *dominant*, for example, is elucidated by describing the behavioural circumstances that justify its third-person, present tense ascription to another individual.

Psychometricians act as if there does not exist a language that contains such concepts, as if they themselves discovered such concepts, and, with a bit of luck, will eventually reveal to humanity their meanings. Like the little boy who attributes his great height to his ability to fly, rather than the fact that he is standing on his father's shoulders, the psychometrician misascribes his capacity to formulate a coherent hypothesis regarding the causes of dominant behaviours, assemble a list of "hypothesized indicators of intelligence", make a coherent claim about agreeable individuals, or note that "writhing tends to go with pain", to his "scientific hunches", rather than their proper source, his capacity to correctly employ psychological predicates, a capacity he shares with other language speakers. Such concepts can appear, legitimately or otherwise, in such sentences, because those who put them there are speakers of the language of which these concepts are a part.

There is no "real" (unobservable) dominance that lies separate from whatever is signified by the concept *dominance*, for "… there is no more to the meaning of an expression than its correct use, a description of the grounds justifying the use of a psychological predicate (of the criteria for its application) is an explanation of its meaning" (Baker & Hacker, 1982, p.235). The misportrayal of dispositional concepts as standing for an essence of one sort or another likely results from a failure to distinguish between the meanings of these concepts, and the possible causes of, and phenomena co-occuring with, their behavioural criteria. The criteria of a concept are linked to the concept in grammar. They are logico-grammatical grounds for ascribing the concept to another individual. On the other hand, the fact that these criteria have causes, or phenomena with which they co-occur, is a contingent, empirical matter. Notice that the discovery of a cause of *dominant* behaviours *presupposes* the capacity to antecedently identify dominant behaviours. The researcher must grasp which behaviours are, and are not, dominant behaviours, and this is equivalent to grasping the behavioural criteria of the concept *dominant*, which, in turn, is equivalent to grasping some of the rules that fix the correct employment of the concept *dominant*. To put this another way, the supposition, and subsequent discovery, of a cause of behaviours $\{b_1,b_2,...,b_t\}$ is not a supposition, and subsequent discovery, of a cause of dominant behaviours unless $\{b_1,b_2,...,b_t\}$ are, in fact, signified by (instantiate) the concept *dominant*. But to correctly claim that $\{b_1,b_2,...,b_t\}$ are signified by (instantiate) the concept *dominant* presupposes knowledge as to the correct employment (meaning) of the concept *dominant*, and, in particular, that its criteria of application are, in fact, $\{b_1,b_2,...,b_t\}$.

One of the tasks of the social and behavioural scientist is to investigate the causes, be they physiological or otherwise, of behaviour. The behaviours that are criterial for (instantiate) a given dispositional concept might have a single, shared cause, or multiple, distinct causes. However, *the causal story of these behaviours, while certainly a matter for science to resolve, is a wholly separate issue from that of the meaning of the dispositional concept the behaviours instantiate*. The cause or causes of dominant behaviours is not the "real" dominance, but, rather,

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6 A behavioural criterion is "…a presumptive implication, non-inductive evidence supporting a judgment." (Baker & Hacker, 1982, p.235).

the cause or causes of dominant behaviours. If behaviour γ has been shown to co-occur with behaviour δ, the latter, a behavioural criterion for dispositional concept "φ", then there are various senses in which γ can rightly be said to be a symptom or indicator of an individual's being φ. One sense is that γ can be used to predict the occurence of an individual's being φ. This would be a contingent, inductive relation. However, the only thing that *justifies* ascription of concept "φ" to another is his having performed behaviours criterial for the ascription of "φ". What is meant by an individual's being φ is spelled out in terms of the criteria of ascription of "φ", not in terms of the causes of, or phenomena co-occuring with, the behavioural criteria of "φ". If some chemical process in the brain, τ, turns out to be the cause of a range of dominant behaviours (criteria that justify the third-person, present-tense ascription of *dominant* to an individual), then science has not discovered the *true* dominance, but, rather, the cause of these particular dominant behaviours.

The following, taken from a web based structural equation modeling discussion group, is representative of the confusions that exist in psychometrics over the relationship between the conceptual and empirical facets of psychometric work:

1. "something" is hypothesised as causing a range of phenomena (behaviours)that are observed in individuals - we can verbally descibe them as having something to do with differences in confidence, assertiveness,self-awareness, pride, capable etc.

2. These phenomena seem to be related one another - we impute a cause.

3. We cannot name the cause "?" - but we infer that the phenomena/behaviours can be jointly described as constituting "Self-Esteem". Alternatively, we name the cause of the phenomena as "Self-Esteem".

4. We have a range of outcomes/phenomena that seem to be the outcome of this cause - "?" or "Self-Esteem".

In (1), what are (poorly explicated) behavioural criteria (confidence, assertiveness, self-awareness, pride, capability) for the third-person, present-tense ascription of the concept *self-esteem* to another individual, are described as a "range of phenomena", as if psychometrics cleverly thought up this collection (this, despite the fact that, according to the Oxford English Dictionary, the concept *self-esteem* appeared in literature as early as 1657). In (2), these behaviours are described as "seeming to relate to each other", as if this constitutes a remarkable empirical discovery. In fact, they relate to each other as a result of language (just as do being "angry", "steamed up", "red with fury", and "outraged": One is grammatically justified in ascribing to Bill the property of being "steamed up" when Bill is angry (part of what it means to be steamed up is to be angry)). These "phenomena" are criteria of ascription for the same concept, and this means that they are bound tightly together in a web of *conceptual* implication (see Bennett & Hacker, 2003, for many such examples). For example, it is ungrammatical to grant ascription of *high self-esteem* to an individual while at the same time deny ascription of *confident*. This is why the phenomena that these concepts denote are *seen* as "self-esteem phenomena".

In (3), a little game is played that a "mere label", *self-esteem*, is invented to denote the, currently unknown, cause of the "range of phenomena." But this is preposterous. The concept *self-esteem* has had, for a very long time, a normative, rule governed employment ("oft times nothing profits more Then self-esteem, grounded on just and right Well manag'd", Milton, 1667). There are self-esteem phenomena to be studied by psychologists because of this fact, not because of insights from empirical science. And nothing in the normative, rule governed employment of the concept *self-esteem* establishes the concept as a denoter of a (currently unknown) cause. The normative, rule governed employment of the concept *self-esteem* does establish it as instantiated in the third-person, present-tense mode by behavioural criteria, those "phenomena" that are, in (3), perversely hypothesized to be caused by self-esteem itself! The teaching and learning of the concept *self-esteem*, practices in which the rules of correct employment of the concept are referred to and reitereated, do not involve mention of the cause, known or unknown, of the behavioural criteria of the concept. They do involve mention of the criteria for the ascription of the concept to another individual. Thus, when teaching the correct employment of the concept *self-esteem*, one will make mention of confidence, assertiveness, and self-awareness. The discovery of a cause of (high) self-esteem, if one exists, is solely the task of science, and has no bearing on the meaning of the concept *self-esteem*. However, if such a discovery were to be made, it would presuppose an understanding of the rules that fix the correct employments of the concept *self-esteem*, for these rules establish what are and are not self-esteem phenomena (criteria of the concept), i.e., the phenomena whose causes are sought.

The concept *self-esteem* and its behavioural criteria are internally related: Part of what it is to grasp the meaning of the concept is to be able to spell out the criterial grounds for its instantiation. To teach the employment of the concept one must teach at least some of its criteria. To state that "an individual who has high self-esteem is confident, feels good about herself, has a favourable opinion of herself" is to provide a paraphrastic elucidation of part of the meaning of the concept. It is to reiterate certain of the rules of correct employment of the concept. The rules of language fix that behaviours {a,b,c,..d} are grounds for ascribing the predicate *high self-esteem* to another. No inference is involved, for the relation of given concept to its instantiators is not empirical, but grammatical. On the other hand, {a,b,c,..d}, instantiators of, say, *high self-esteem*, may have single or multiple causes, and the job of the behavioural scientist is indeed to discover these causes (i.e., to work out their causal story).

3f. "Who can, in practice, recognize an extrovert personality?" (Lazarsfeld, 1959, p.477)

"Trait concepts develop as follows: "We experience, say, anxiety, and its role in our own course of action (R). We observe how other people act in situations (S) which would, we know, bring on our anxieties; we notice that their reaction R is similar to ours. As a result, we file away in our minds that as a rule such stimuli S are likely to be followed by responses R. We "explain" such S-R sequences with the help of an intervening variable: anxiety" (Lazarsfeld, 1959, p.477)

As was described in Chapter III, Lazarsfeld believes that psychological concepts either are, or denote, unobservable essences residing in natural reality. Not surprisingly, then, he completely misconceives of what it is to correctly claim that someone is an extrovert or that someone is anxious. Who can recognize an extrovert? Any competent user of the language. For to possess the capacity to *recognize* an extrovert is nothing but to grasp the grounds for ascribing the concept *extrovert* to another. And to grasp the grounds for ascribing the concept *extrovert* to

another is to grasp some of the rules that fix the correct employment of the concept. The concept *extrovert* is ascribed to another in the third person present tense mode on the basis of behavioural criteria. In the learning of how to correctly employ the concept *extrovert* one learns some of the criteria that justify ascription of the concept to another. That is, Lazarsfeld's question is a conceptual question. The problem is that the rules that fix the correct employments of psychological concepts are not of the necessary and sufficient variety. The grammars of psychological concepts are complex, and this perhaps makes them unsuitable for use in science.

Lazarsfeld's inability to grasp the difference between a conceptual and empirical issue makes a mockery of his second quote. Here he discusses how trait concepts develop. He envisions people experiencing anxiety and observing others in circumstances they know would bring on their own anxieties, and from this, explaining certain S-R consistencies with reference to the intervening variable anxiety. However, *anxiety* is not an intervening variable, but, rather, an ordinary language psychological concept. This should be obvious, for, it it weren't so, people could hardly be expected to possess the capacity to "recognize that they are experiencing anxiety" or "recognize circumstances as those that would bring about their own anxieties". To even grasp the meaning of such expressions presupposes a grasp of the rules that fix the correct employments of the concept *anxiety*. That is, Lazarsfeld plays at defining *anxiety* (as an intervening variable) while, in fact, his "definition" presupposes the meaning of the concept as fixed in ordinary language.

3g. "The tendency to speak in terms of level of extraversion as a comprehensive statement about an individual implies a fair degree of coherence (depending on the researcher's model of extraversion) in personal experience, behavior, perception by others, and/or physiological reactivity" (McGrath, p.10).

This is confused. The grounds of correct ascription to an individual of the concept *extraversion* is a conceptual issue, and, hence, is settled through a consideration of linguistic rules. There is in existence no linguistic rule that makes any mention of coherence in personal experience, behavior, perception by others, and/or physiological reactivity, let alone models of extraversion. The concept *extraversion* is a dispositional concept, and is justifiablly ascribed to an individual when the individual has manifested behavior criteria for the concept, these criteria learned in the learning of the correct employment of the concept (Baker & Hacker, 1982).

McGrath (p.8) further describes an extravert as one who "experiences great pleasure in social situations", is "likely to engage in behaviors leading to social contact", and is "perceived by others as outgoing", and worries about whether an individual who manifests only two of these criteria is, in fact, "truly" an extravert. But the lack of covariation amongst the instantiators of the concept *extravert* poses no difficulties with respect the correct ascription of the concept, for, once again, the rules that fix the correct employments of the concept are mute with respect to the issue of covariation (language users are not taught about covariation as a prerequisite to their learning how to correctly employ the concept). The issue of the covariation of distinct expressions of extraversion is an empirical issue that pressupposes a conceptual issue, to wit, the capacity to identify behaviors *as* expressions of extraversion (just as the capacity to study the magnetic properties of scandium presupposes the capacity to identify scandium in nature).

3h. "A disposition toward dominance is not the same thing as the concept of dominance. The concept is something people could well discuss in detail to determine what it means, and then

apply it in the measurement of dispositions or in the measurement of actions or in the measurement of situations or in the writing of textbooks or legal opinions. The disposition is something that depends on the concept, but that has the additional property of being susceptible to measurement" (from a review)

It is not clear what this means. The concept *dominance* has a range of correct employments, including a dispositional one (as when I say "he is a dominant s.o.b." or "she is constantly dominating proceedings"). The dispositional employment of the concept can be contrasted with its application to individual behavioural acts ("you completely dominated that meeting"). One correctly ascribes the concept *dominant* to an individual (and, in this case, to one's self) in its dispositional sense given that he has behaved in a manner criterial for the ascription of the concept in its dispositional sense. One learns these criteria in the learning of the correct employment of the concept in its dispositional sense. Nothing in the rules that fix the correct employment of the concept *dominant* give a sense to the notion that there is something *else* called "the disposition", nor, certainly, that this something else is "susceptible to measurement."

3i. The meanings of "lay" psychological "notions" (concepts) have no bearing on a technical science such as psychology. The psychologist need pay no attention to such "primitive conceptions/theories".

A common response to the claim that the social and behavioral scientist standardly mishandles the psychological concepts that inform his work, and that this has had a detrimental effect on his empirical work, has been to scoff and rebut that this criticism has little bearing on the technical scientific work that, nowadays, constitutes work in the social and behavioural sciences. To quote, "Psychologists are not *required* to use the concept in the same way that laypeople do..." (Jost and Gustafson, 1998, p.474). This is correct, but to be a valid defense against the charge that the social and behavioural scientist confuses conceptual and empirical issues, thereby doing violence to the concepts that populate his empirical work, and, thus, undermining the empirical work itself, there must be evidence that he does, in fact, *employ* these concepts in normative ways that differ from that of ordinary language (i.e., as technical homynyms of the ordinary language senses). The scientist must be able to provide coherent explanations of these novel, technical employments, and, in particular, indicate the ways in which his usage departs from that of ordinary language. However, the social and behavioural scientist does not do this. On the contrary, his discussions of his empirical work *presuppose* the ordinary language employments, this being a chief reason he can market his work to the general population as "relevant." The social scientist who claims to have made strides in understanding the causes of bullying is employing precisely the same concept as would arise in any discussion of bullying amongst parents in the playground. This is made clear by the (ordinary language) grammatical implications manifest in his use of the term (for example, the co-ascription of a range of negative evaluative terms).

Thus, the social and behavioural scientist, while certainly free to employ any conceptual scheme he pleases, does not choose to employ novel technical terms. In distinction to, say, quantum physics, the social and behavioural sciences were not founded on a bed of technical concepts. They arose from a desire to explain the psychological phenomena of interest to one and all (attitudes, desires, intelligence, anger, grief, love, etc.) and such phenomena are precisely

those phenomena denoted by ordinary language psychological concepts. If the aim *is* still to study scientifically phenomena denoted by the concepts of ordinary language, and there is no evidence that it is not, then the attitude expressed in (3i) is misplaced, because:

a. To study, say, anger is to study precisely those behavioural phenomena denoted by the concept *anger*, and to grasp which behavioural phenomena are denoted by the concept *anger* is to grasp the rules that fix the correct employment of the concept *anger*. To put this differently, in studying x, one is studying a φ-thing only if the rules of employment of concept "φ" warrant application of "φ" to x.

b. If a scientist fails to grasp the rules of correct employment of the concept *anger*, then, inter alia, he fails to grasp *which* behavioural phenomena are to be studied in a study of anger. He literally does no know to which phenomena his attentions should be directed.

While there are areas of research within the social and behavioural sciences in which the work is, for the most part, organized by technical concepts, these are exceptions to the rule. The very intelligibility of the vast majority of the theories, hypotheses, and discussions produced within the social and behavioural sciences depends upon an understanding, by both researchers and audience alike, of the ordinary language employments of psychological concepts. If this were not the case, then why should the social and behavioural researcher retain the use of ordinary language concept labels? The psychologist who's work is organized not by the ordinary concept *memory*, but, instead, by some technical concept, can, with no loss of intelligibility, forfeit his use of the term "memory", and replace it with something else, e.g., *mnemomon*, defined as follows {...}. All he requires is a novel concept-name, and the stipulation that this concept is to be employed in some particular way. However, the marketability of psychology rests on the belief that the psychologist is making discoveries about psychological phenomena of interest to the general public, and these phenomena are those that are signified by ordinary language psychological concepts. It should be remembered that the personality psychologist Raymond Cattell *did* employ proper technical terms, and it is hardly coincidental that his work, rather than being a foundation upon which future work now rests, has always been viewed as an oddity, eventually fading to obscurity. The technical concept *parmia*, for example, denotes phenomena that is of interest neither to the average citizen, nor the average psychologist. Whatever phenomenon it denotes can hardly be expected to be as interesting to the general public as the phenomena denoted by ordinary language concepts such as *agreeableness*, *intelligence*, *dominance*, and *unhappiness*.

The psychologist's professed disinterest in, and frequent departures from, ordinary language senses of psychological concepts are not the product of the careful formulation of technical homynyms, but, rather, conceptual *confusion* exacerbated by profound misunderstanding over the place of concepts in science. The scientist need not study phenomena denoted by ordinary language concepts. But if he wishes to study, e.g., self-esteem, then he wishes to study precisely the phenomena denoted by the ordinary lnaguage concept *self-esteem*. Good science involves a great deal that is non-empirical, from the formulation of laws that are idealizations (e.g., Newton's first law of motion), to the conceptual work that frees science from confusion, so that it may do its empirical bidding (e.g., Einstein's analysis of the concept *simultaneity*; Mach's revealing of the circularity inherent to Newton's definition of the concept *mass*). The only thing of relevance to the true scientist is getting it right. Any attitude that

obstructs this aim, including misplaced contempt for the conceptual work that is a hallmark of great science, is an unscientific attitude. It appears that there has only been one psychometrician who has had an adequate grasp of the relationship between the empirical and conceptual components of science, and that would be Louis Guttman. Guttman devised his facet analysis as a tool for the clarification of the conceptual (definitional) foundations of his work. He was unfailingly careful to distinguish this component of science from the empirical studies that would yield facts about the phenomena denoted by the nodes of his facet analytic definitions. Perhaps, then, it is not surprising that McDonald (1996, p.596) would describe Guttman's separation of conceptual and empirical issues as "...an incongruous union between precise facet-analytic *empirical conceptions* [italics added] and such crude exploratory devices as smallest space analysis..."