

# The Nexus Misconceived

## Wittgenstein Made Silly

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**ABSTRACT.** Certainly, the issues raised by Wittgenstein's account of measurement deserve serious debate. However, for such dialogue to produce a satisfying scholarly yield, an accurate depiction of his philosophy is required. Unfortunately, the commentaries of Jost and Gustafson (1998) and Chow (1998) carry on a dubious tradition in the social sciences of misunderstanding Wittgenstein's most essential insights. The most crippling of these is a failure to grasp Wittgenstein's remarks on the autonomy of the conceptual (rules) with respect to the empirical, and the implication of this autonomy for the treatment of conceptual and empirical issues in science.

**KEY WORDS:** constitutive rule, internalism, measurement, Wittgenstein

Both the issue of what Wittgenstein said, and the implications for psychology of what he said, should be the stuff of vigorous debate, for Wittgenstein (at the very least) provides a stiff challenge to much of what we take for granted. The topic of measurement is of an importance that demands this kind of attention. Jost and Gustafson (1998, p. 463) claim that Wittgenstein has inspired sociologists and psychologists 'who have made careers out of doubting the scientific status of their own disciplines'. But the critiques of Chow (1998) and Jost and Gustafson (1998) are evidence of another disturbing trend, that being the increasing prevalence of critics and interpreters who lack the subtlety of thought to do justice to the issues raised by Wittgenstein. Jost and Gustafson's effort is equal parts misrepresentation of an analysis that calls for more than they have to offer and incomprehension of basic issues, topped off with the unjustified insinuation that the object of their critique manifests a nihilistic bent. Chow's installation, on the other hand, is merely a convoluted tour de force of misunderstanding. Where to begin?

## Wittgenstein vs Wishful Thinking

The majority of the confusion manifest in the two critiques is attributable to incomprehension of what is arguably the most important insight of Wittgenstein's philosophy. As Jost and Gustafson readily admit, rules play a role in measurement. But the nature of this role and its conceptual features (Wittgenstein's *internalism*) escapes them. While it was carefully explained in my original paper (Maraun, 1998, pp. 441–444), I will have to attempt once again to bring about understanding. But first, it is only proper to hammer out the kinks in the cartoonish depictions of Wittgenstein one encounters in the critiques. In the version of Jost and Gustafson, Wittgenstein is seen as being 'irked' by the idea that experimental psychology could provide answers to philosophical (metaphysical) problems (p. 465) and as providing criticism possibly more pertinent to a bygone era of psychology (p. 464). They view it as a deep insight that Wittgenstein's *philosophy* is compatible with a causal *theory* of intentional action (p. 465). Wittgenstein's remark that 'problem and method pass one another by' is, in the world of Jost and Gustafson, taken by the people as a warning against the possibility of the scientific study of mind and behaviour, and as having as its primary target psychological behaviorism (p. 465). Finally, Wittgenstein's 'musings' are conveniently taken as being at one with the basic aims of construct validation theory (p. 473). Chow (p. 486) contributes by asserting that 'Wittgenstein objected to psychologists' appeal to internal states, sense data, empirical data and experimentation.' However, all of this, to say the least, misses the mark.

What was Wittgenstein doing in psychology? An understanding of *the* remark solves this mystery. Wittgenstein was in the habit of seeking out confusions as fodder for his philosophical method, and psychology (and other sciences) was rich in offerings. This, in Wittgenstein's opinion, was primarily due to the fact that psychology was erected on a field of common-or-garden concepts that had 'messy' grammars. Regardless, endemic to psychology was *the* confusion, the confusion over the boundary between conceptual and empirical issues. The remark speaks to nothing other than this issue. As put by Baker and Hacker (1982):

The endemic sin of the experimental psychologist, the sin which explains and justifies Wittgenstein's remarks that 'problem and method pass one another by', is to neglect the conceptual investigations which are preconditions for fruitful, intelligible experiments. (p. 228)

Furthermore this 'sin', every bit as prevalent today as when Wittgenstein was writing on psychology (see, e.g., Schanker, 1987; Ter Hark, 1990), is at the root of our mishandling of psychological measurement, and is rendered a virtual art form in construct validation theory. Comically, Jost and Gustafson protest that

Wittgenstein is famous for asserting that traditional philosophical problems are conceptual in nature and that the solutions to those problems must also be conceptual. This does not mean that any question that would interest a psychologist is also conceptual in nature. (p. 466)

I should hope not, since that would make psychologists *de facto* philosophers. The point is that the coherent study of the empirical questions of which in psychology we express interest presupposes the adequate treatment of often complicated conceptual issues. Wittgenstein was *not* interested in ragging on psychology, and certainly did not object to the psychologist's appeal to 'empirical data and experimentation' (Chow might want to read the relevant sources). On the contrary, he viewed the psychologist's job, empirical investigation, as wholly legitimate, and in no way in conflict with philosophy. On the other hand, he did view philosophy as potentially an *aid* to psychology, particularly in its capacity to resolve the logical conundrums that occur in the conceptual bedrock of empirical investigations, often crippling them. As Baker and Hacker (1982) state: 'the conceptual clarifications of the philosopher are, in principle, highly relevant to the psychologist's work' (p. 228). To Wittgenstein,

The positive task of philosophy of mind is the clarification of psychological concepts and the resolution of *philosophical* problems about the mind; it is not concerned with constructing *theories* about the mind which might complement or compete with empirical theories in experimental psychology. (Baker & Hacker, 1982, p. 229)

### Science Is Not a Synonym for Measurement

Jost and Gustafson portray my paper as a polemic against the possibility of psychology as a science. For example, they state that 'Maraun repeatedly ascribes to Wittgenstein the mistaken view that folk psychological concepts cannot be studied scientifically' (p. 475). This, however, is nothing more than a rhetorical ploy used in lieu of careful analysis. Regardless, when Jost and Gustafson ride out on their trusty steed to counter this fabricated challenge, one can only conclude that they are able to detect nothing more than the evaluative tone of my piece. For while I am certainly critical of psychological practice, neither the writings of Wittgenstein, nor anything in my paper debates the reality of psychology as science. Any discipline must carefully consider its practices, and my paper, on logical grounds, questions the coherence of certain types of measurement claims common to psychology. Do Jost and Gustafson really not understand that this is an entirely different issue?

For the record, science and measurement are distinct topics. And while some might insist that measurement is necessary for science, it is likely that they are confusing measurement with one of its prominent components,

representation. The representation of aspects of the empirical (e.g. by our data analytic models) is a prominent feature of psychological research, and the need to represent aspects of the empirical may be seen as the primary motivational factor behind the creation of the majority of our quantitative methodology. However, creating useful representations of that which is denoted by  $\sigma$  is not the same thing as measuring  $\sigma$ . Exactly why this is so was a topic of my paper. The superficiality of Jost and Gustafson's version of the issues is evident in their paraphrase of my case:

Maraun has argued simply that there must be initially agreed upon norms (that can be stated publicly) for how to define psychological constructs and how to measure them in order for these subsequent measurements to be considered meaningful, justified and correct. (p. 469)

If this *were* the point, there would be nothing to discuss, for operationism would already have solved the measurement problem. The argument made was that difficulties with psychological measurement arise as a result of our desire to measure common-or-garden concepts, and that the source of these difficulties is a conceptual feature known as *internalism* (discussed extensively by Wittgenstein).

### **Autonomy vs Accountability (Wittgenstein's Internalism)**

Jost and Gustafson express puzzlement over my characterization of the relation between the conceptual (grammatical) and the empirical. They state: 'Our own position is that Maraun overstates the case considerably when he argues that conceptual (or grammatical) issues in psychological measurement are independent of empirical issues' (p. 466); 'After having argued strenuously from the abstract that "empirically based argument is not relevant to the support of measurement claims", he back-pedals with statements like "it is not Wittgenstein's contention that empirical considerations are irrelevant to measurement"' (p. 467); 'All of this (as well as our points about reliability below) casts doubt on Maraun's assertion that "there is no such thing as *discovering* which actions constitute the legitimate measurement of  $\sigma$ , nor whether  $\sigma$  can, in fact, be measured"' (p. 471). Finally, they provide examples that apparently illustrate how the grammatical and empirical are not 'independent' (pp. 471–472). It is hard to know whether to laugh or cry, for the issue is Wittgenstein's *internalism*, its autonomy of grammar sub-component, and certainly *not* the *independence* of the empirical and conceptual. The disgrace here is that Wittgenstein's internalism is arguably his most essential philosophical insight, and motivates his careful analysis of the relation between criterion and symptom, science (e.g. theories, hypotheses, opinions, beliefs) and its conceptual precursors, concept and that which it denotes, pain and pain behaviour, and many other special cases.

Since my pages 441 to 444 were of little help to Jost and Gustafson, it might be salutary to hear from others on this matter. Ter Hark (1990) describes Wittgenstein's internalism as comprising the following two theses:

- (a) The meaning of words and signs is not given independently of constitutive rules, language-games, forms of life, but is internal to them. (b) The description of these rules, language-games, forms of life is not itself an empirical statement. (p. 67)

What are the implications of internalism?

Wittgenstein's internalism and the resulting autonomy of grammatical or constitutive rules have made it clear that forms of life are not founded by something external to themselves. Forms of life and grammatical rules are not foundations in the sense that they supposedly correspond to the 'objective facts' or the 'essence of nature'. For the grammatical rules themselves determine what is to be regarded as 'objective fact' and as the 'essence of nature'. (This is not to say that Wittgenstein does not recognize physical or physiological facts which are not part of a rule-guided practice. On the contrary, the grammatical rules *are* 'accountable to reality' in the sense that, if reality were quite different in certain respects, our rules and language-games would lose their value and meaning.) Forms of life and grammatical rules are 'foundations' in the sense that they constitute the meaning of our concepts in the first place and that if we were to change the rules, the concepts would have an entirely different meaning or no meaning at all. (Ter Hark, 1990, p. 67)

The point, then, is that a constitutive rule and that which accords with it are internally related, and this makes the relation autonomous of the empirical, even though such relations most certainly presuppose an empirical backdrop (the reason why was discussed in R4 of my original paper, p. 440). Autonomy of constitutive rules means that

... grammatical rules cannot be justified by a reference to or description of reality, since 'reference to', 'description of', and *a fortiori* 'justification' are themselves certain forms of language which also presuppose agreement in forms of life. (Ter Hark, 1990, p. 65)

Furthermore,

That grammar is autonomous, arbitrary, not justified by reference to reality is a deep leitmotif of Wittgenstein's work. That explanations are intra-linguistic (even though they often, as in ostensive definition, include partly concrete symbols) is merely an aspect of this general thesis. They belong to grammar. (Baker & Hacker, 1980, p. 76)

Jost and Gustafson do not understand the distinction between *autonomy* and *accountability*. The fact that rules are *constitutive* for the meanings of words and signs induces an autonomy with respect to the *justification* of the use of words and signs: All one can do is *reiterate* the rules for their correct use. To

note (trivially) that there is an empirical backdrop to a rule-guided practice is, to say the least, not the same thing. All in all, to have incorrectly taken the explication of a key conceptual feature of constitutive rules to be 'back-peddalling' is not a strong endorsement of one's readiness to engage in this debate.

What does internalism have to do with measurement? Wittgenstein's case is that both grammatical constitutive rules and the methodological rules of measurement manifest internalism: 'In general, methods of measurement provide a special case to illuminate the thesis of the autonomy of grammar (BT 240 f.)' (Baker & Hacker, 1980, p. 284). In fact, the methodological rules of measurement are constitutive for measuring. What constitutes a correct claim as to how to measure is not *justified* with reference to the empirical. Whether one is *justified* in claiming that *this* is a measurement of Jim's height is determined by comparing actions (how they were taken) to the constitutive rules for measuring height. If numbers  $t_i$  are measurements of  $\sigma$ , they are so for only one reason: They were *taken* in conformity with the rules that are constitutive for measuring  $\sigma$ . Outside of the rules, one is not measuring at all. Now, certain concepts are internally related to measurement practices and others are not: 'Indeed, no concept of length is independent of some practice of measuring length . . . knowing what it is to measure length is not a matter of knowing what length is and what it is to measure something (PI p. 225)' (Baker & Hacker, 1980, p. 286). The internal link between concept and measurement practice ties measurement to meaning. One may not make a measurement claim without implicating constitutive rules, and hence making a claim about the meaning of a concept.

The rules that govern the use of common-or-garden psychological concepts are constitutive for their meanings. Indeed, they fix what may *legitimately* be done with these concepts, including their places in 'measurement sentences'. Because of internal relations it is not a matter of, for example, understanding what is meant by dominance, and knowing how to measure something. If a given common-or-garden psychological concept was legitimately linked to a measurement practice, this linkage would be an internal one, and hence be manifest in its grammar. There would be no argument over how to measure it. The rules for its measurement would be taught, along with its meaning, to children. Internalism means that one is not free to pronounce on the relation of rules of measurement to psychological concepts, without risking the possibility of changing the meanings of these concepts:

Modifying or technical rules like culinary rules are different: here one can obey other rules or flout existing rules and yet it is still possible to indicate a goal, even if it can now no longer be achieved, for example eatable food. With grammatical rules this is impossible: the goal of grammatical rules is conceptually dependent on the rules themselves: 'The aim of grammar is

just the aim of language' (TS 213, p. 194). Mistakes made by not obeying grammatical rules are excluded. Without grammatical rules there is no language at all, not just wrong or careless language. (Ter Hark, 1990, p. 66)

One cannot then justifiably side-step language and, for example, call a set of numbers *measurements* of dominance:

The confusion occurs for instance in psychology when on the one hand 'thinking' is used in the normal sense of the word and on the other hand is regarded as measurable in terms of (physiological) reactions. Wittgenstein does not say that this kind of measurement is impossible, but only that it involves an *entirely different* phenomenon from what 'thinking' is normally understood to mean. (Ter Hark, 1990, p. 32).

The numbers assigned to people on the basis of their taking a psychological test may be many things, and have many practical uses. But if one is talking about a common-or-garden concept, then such numbers are not measurements.

Throughout their critique, Jost and Gustafson fail to come to an understanding of constitutive rules, hence the lack of negotiability when it comes to constitutive rules, and, inter alia, the possibility of measuring common-or-garden concepts. The issue is *not* agreement in opinions, but in language-games and forms of life (Wittgenstein, 1953, §241). Now, one may, in certain rule guided practices, flout the rules for  $\Gamma$  and still be doing  $\Gamma$ , but only if the rules are *modifying* or *regulative* rules (see Ter Hark, 1990). Jost and Gustafson provide an example of a modifying rule when they refer to the norms for setting  $\alpha$  in statistical hypothesis testing. However, the meanings of psychological concepts are given not by modifying rules, but by constitutive rules, and 'Wittgenstein's analysis of the rules for psychological concepts shows that if these rules are changed, we can no longer say that the same activities or purposes are involved: in that case an entirely different game is being played' (Ter Hark, 1990, p. 281). Similarly, 'Without these rules the concepts do *not* yet have a meaning, and if different rules are "chosen", the concepts have an entirely *different* meaning or none at all' (Ter Hark, 1990, p. 282). Jost and Gustafson's assertion that 'Maraun offers no arguments whatsoever for why it is impossible to measure *constructs* that are used (or misused) by laypeople' (p. 473; emphasis added) is really just an admission of their incomprehension of the conceptual features of constitutive rules.

On page 474, Jost and Gustafson claim that 'One measures belief and confidence and their degrees by, among other things, choices and preferences (e.g., willingness to trade outcomes, engage in risks, etc.)' Chow completes the chord:

For example, variable  $X$  in [A1] and [A2] may be the palmar sweating (viz. the criterion variable) induced when students are told that they have failed

a psychology examination (i.e. the data collection context; see Table 1). It is used in establishing the criterion-related validity of the test because it is an accepted symptom of being anxious (even by the layperson). This choice is not inconsistent with the 'grammar' of using 'anxiety' in Wittgenstein's terms (see Baker & Hacker, 1982; Ter Hark, 1990). (p. 484).

If only things were so simple. With an alarming nonchalance, both parties attempt to drive a wedge into the heart of an internal relation, offering up correlates in place of criteria. The penalty for such behaviour is that they end up offering commentary not on *belief*, *confidence* and *anxiety* (as claimed), but on entirely different phenomena. Chow is deluding himself if he thinks that he can mislabel a symptom 'a feature of grammar' and conclude that 'this choice is not inconsistent with . . . Wittgenstein's terms' (p. 484). Indeed, the inconsistency arises not from Chow's choice of terms, but from his lack of understanding of their meanings. Does he really believe that he is contributing to the discussion of Wittgenstein's ideas by merely dropping in the term 'layperson'? The issue (once again) is not agreement in opinions (of laypeople or specialists), but the constitutive rules of language.

Of course, as I indicated in my paper (pp. 453–457), a psychologist is *not* obliged to employ common-or-garden concepts. He or she is quite clearly free to create technical concepts that no lay individual could hope to understand. But Jost and Gustafson are *way* off the mark in their conclusion that: 'For Wittgenstein, the only relevant point is that there are public contexts in which each language-game has a place; it is not a problem that scientists and ordinary people make use of different contexts when they speak about depression' (p. 474). Wittgenstein *harped* on the damage that may be done when a concept is employed in both an ordinary language and technical sense without the use of appropriate disclaimers. And with good reason, for this practice is destined to create *confusion*, which, in turn, has an insidious tendency to cripple a science: 'This tendency Wittgenstein condemned as "contempt for the individual case" which can lead to nought but confusion' (Baker & Hacker, 1982, p. 229). It is a mistake to take the conceptual confusion of our discipline as being 'preparadigmatic', for conceptual confusion is very different from disagreement over conceptual foundations.

The mishandling of Wittgenstein's internalism spawns a host of derivative confusions in the two critiques, several of which will be noted.

## Empirical vs Conceptual Issues

Just how deep the confusion goes is evident in the repeated conflation of issues of concept meaning and various types of empirical claim. Chow asserts that:



What is amiss in the ‘mischaracterization of measurement’ account is the absence of any acknowledgment that the said theorization is carried out at a level more abstract than the level at which ‘table’ is used. 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 the psychological phenomena denoted by their respective concepts. (pp. 482–483)

Jost and Gustafson help out with:

Maraun invents a sort of Meno’s paradox by arguing that one cannot study memory unless one understands what memory is, in which case there is no epistemic space for an inquiry to fill. The fact is that we cannot say what anxiety, dominance, empathy, memory or pain is in a suitably scientific psychological way until we have investigated. (p. 474)

and

We then refine our conception and note that some or all of the *platitudes of common sense* (Maraun’s ‘common-or-garden concept’ domain) may be false or, in other cases, simply unanticipated by common sense’. (p. 475; emphasis added)

It gets worse. On page 475, they ask:

What would it mean for findings to conform or not conform to the ‘grammar of remembering’? Does grammar settle the facts, before inquiry, about primacy and recency effects in recalling serial ordered items?

It is shocking that both critiques accept so readily the mistaken view that ‘common-or-garden concept domain’ (constitutive rules?) is just another way of speaking of ‘everyday beliefs’. Evidently, Wittgenstein has been wasting his breath. To review, on the one hand there is concept meaning. Meaning is established by constitutive rules. The relation between constitutive rules and signs and words is internal (see pp. 492–496 above). On the other hand, there are ideas, including those people have about the empirical, expressed in terms of concepts (and, of course, whose meanings are given by rules). The constitutive rules of grammar are not then ‘platitudes of common sense’, theories, beliefs, opinions, hypotheses, and certainly have nothing to do with ‘epistemic space’. In Ter Hark’s (1990) terms, ‘A constitutive rule for the use of a word is not the same as an empirical statement in which the word is used. The first statement is logical, the second empirical; the logical statement is constitutive for the empirical one’ (p. 67). Jost and Gustafson should carefully consider how this bears on their claim that ‘we cannot say what anxiety, dominance, empathy, memory or pain is in a suitably scientific psychological way until we have investigated’. It is clear that they are confusing the constitutive grammatical rules that fix the meaning of concept  $\theta$  with empirical facts conceptualized in terms of  $\theta$ . Empirical investigations generate facts, and facts are about phenomena. But phenomena are conceptualized, and to conceptualize one

must understand the meanings of concepts. Their error here is akin to claiming that ‘the mountain is a place where skiers go’, and then forgetting that to know this *fact* about mountains presupposed a criterion for *mountain* (i.e. the concept’s meaning).

### Teaching/Learning vs Researching/Discovering

The fundamental distinction between constitutive and methodological rules and empirical issues has strong conceptual links to the clusters of *researching/discovering* and *learning/teaching*. Predictably, then, Jost and Gustafson waste little time in mercilessly conflating these clusters, most egregiously in their examples of page 469. *Of course* rules are learned and taught (see my pages 437–441), but they are not *discovered*:

Criteria are not objects of knowledge but a standard of knowledge. . .one cannot learn a criterion as an object of knowledge, since any information that is supplied—wet trousers, raindrops on the window, etc.—presupposes familiarity with the criterion. (Ter Hark, 1990, p. 32)

One provides *evidence* to support the claim that one has made an empirical discovery, but not so a criterion. To support the claim that one grasps a criterion one must already understand the criterion, for all one may do is *reiterate* one’s understanding. Support for the claim is nothing but a segment of language in which the concept is used correctly. More technically, one cannot *discover* a criterion because to establish that one does indeed grasp the criterion requires a comparison to the criterion itself. This is why children do not discover or research their language, but instead are taught their language. They practise it, are drilled on its nuances, are corrected when they make mistakes, and eventually master it. In Ter Hark’s (1990) terms,

The description of a form of life is a clarification of a practice which we already ‘know’. The ‘knowledge’ this produces is not comparable with the information given by empirical statements, but is ‘knowledge’ in the sense of recollection or explication. (p. 69)

On page 471 of Jost and Gustafson’s paper, this conflation is transposed into the domain of measurement, with an insistent series of claims that one may indeed discover how to measure. They claim, for instance, that ‘The history of science teaches us that only scientific experience will help to determine whether phenomena as diverse as the movement of electrons and unconscious thought processes could be measured reliably.’ But this suggests nothing about whether  $\sigma$  could be measured (*what* does  $\sigma$  mean without constitutive rules?), but instead whether, given the background empirical conditions, it would be fruitful to *lay down* constitutive rules of measurement (it would not be so if the world was such that reliability was

not achievable; see Maraun, 1998, p. 443). Jost and Gustafson should study this difference. Moreover, this type of thinking is permissible for *technical concepts*, but certainly not so for common-or-garden psychological concepts. For such concepts, the constitutive rules already exist, and allow (as they stand) no leeway to free-wheel because they are internally related to the linguistic behaviour that conforms to them: 'the misunderstanding is based on a failure to recognize the internal relation between rule and action, between order and execution, or between pain and its expression' (Ter Hark, 1990, p. 46).

### **The External Place of Classical Test Theory, Predictive Aims and Consensus**

In an ingenious attempt to circumvent the logical necessity of internal relations, Jost and Gustafson review the importance of classical test theory desiderata (pp. 471, 473), predictive aims (pp. 471, 472), and the consensus of practising psychologists (pp. 470, 471). I would be the first to agree on the importance of the first two of these topics, my degree being in psychometrics, and my primary teaching responsibilities centred in this domain. However, as painful as it is to admit, they do not bear on the justification of measurement claims in psychology. More specifically, what psychometrics offers is *external* to the internal relation between constitutive rules of measurement and behaviour that accords with these rules. Psychometrics is therefore not relevant: 'That is why it is meaningless to talk about founding such rules via extra-linguistic facts' (Ter Hark, 1990, p. 65).

My strong inclination is to brush off these forays as merely fatuous. Jost and Gustafson misunderstand Wittgenstein's case (see pp. 492–496 above), and so are in no position to offer serious commentary. They argue that 'If these numbers were found to show no reliability or predictive validity among female samples, then they will not be thought of as "legitimate" measures of depression, because depression is considered to be generally similar in women and men' (p. 473). Not so. Predictive success is the goal in an entirely different game. Moreover, it is a game that is governed not by constitutive rules, but by modifying rules. In this game, *all* that matters is that I am able to predict, and if I choose to flout accepted guidelines for arriving at a strong predictive case, I still may arrive at my goal (predictive success). Not so for measurement: If I flout rules of measurement, then I am not merely measuring poorly, but instead am not measuring at all. Furthermore, the measurement game is standardly prior to the empirical predictive game. To make empirical discoveries about prediction, for example about the linear predictability of height from weight, *presupposes*

that the constituents already can be measured. For if one cannot support the relevant measurement claims, then one cannot support the claim that *this* evidence (e.g. *this* correlation) is relevant to the understanding of the predictive relationship of height and weight. Generally speaking, to be able to measure  $\sigma$  in no way guarantees its relevance (in a predictive sense, or otherwise) to science, any more than being able to count gold ingots guarantees one wealth.

I have no idea why Jost and Gustafson view it as relevant that 'The purpose of social dominance research, for instance, is not to demonstrate that it is possible to obtain different scores (measurements) for different individuals, but to demonstrate empirical connections between those measurements and other variables of conceptual and theoretical interest' (p. 472). But I think the fact that they believe it to be so speaks volumes on their understanding. The demonstration of empirical connections is undoubtedly an important aim of science, but has no bearing on the logic of measurement (it presupposes measurement). Furthermore, if every individual in the world was exactly 6 ft tall, this fact could be noted just because we are able to measure the heights of individuals. The result itself would have no bearing on what it is to measure height. On page 473, Jost and Gustafson suggest that 'Contrary to what Maraun suggests, Wittgenstein was sensitive to at least one empirical issue having to do with measurement, and that is the issue of reliability.' But this is in no way contrary to what I suggest, unless one is philosophically tone-deaf. Jost and Gustafson must learn the distinction between *reasons* and *justifications* (Maraun, 1998, p. 444). Measurement practices may be established for various reasons, including their compatibility with empirical background conditions. However, once rules are laid down, the justification of measurement claims is made with reference to the rules. On the other hand, classical reliability, a technical concept founded on the notion of person-specific propensity distributions, does not even bear on the issue of the *stability* of empirical background conditions, which one *might* legitimately claim was a necessary accompaniment to the laying down of rules of measurement.

The same irrelevance of case characterizes Jost and Gustafson's manoeuvring on page 472, which includes a weak-kneed appeal to the authority of the very individuals who accept the version of measurement that is the target of my paper. The issue is not *prescriptive rules*, but constitutive rules, and not *validity* (a technical innovation), but measurement. If 'validity' could so easily be substituted in for 'measurement', then there would be no need for discussion, for one could solve the measurement problem by consulting the APA guidelines on validity. The fact that it can't be is what has moved so many of our technically most gifted to look elsewhere for answers. I urge Jost and Gustafson to hold the lecture on psychometric theory, and devote some time to learning what is meant by an internal relation.

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