



Pythagoreanism, meaning and the appeal to number

Henderikus J. Stam*

Department of Psychology, University of Calgary, Calgary, Alberta, Canada T2N 1N4

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Abstract

The threat of Pythagoreanism hovers over the appeal to quantification, despite Yanchar's and Westerman's calls for a liberal, interpretive view of numbers. I discuss their articles by agreeing that the distinction between qualitative and quantitative methods is of limited utility, by noting various versions of interpretation in their articles, and by pressing the need to address questions of measurement. Without a coherent measurement strategy, the use of numbers remains at best ambiguous and at worst arbitrary. Although disagreeing with a number of issues in their respective positions, I welcome this opportunity to consider the broader question of measurement and its place in psychology.

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In his history of metaphysics, Aristotle noted that the Pythagoreans believed that number was the first principle. For the Pythagoreans, according to Aristotle, thought that the elements of numbers were the elements of all things; the whole of heaven is numbers. The accusation that one partakes in Pythagoreanism meant, in antiquity, that one was a member of the mystical cult associated with Pythagoras and his cosmology of the mathematical universe. Contemporary accusations of Pythagoreanism, however, are more circumspect (and less likely to endanger one's life); my colleague Charlie Martin recently referred to Pythagoreanism as that tendency of "limiting basic physical descriptions to

*Tel.: +1 403 220 5683; fax: +1 403 289 5570.

E-mail address: stam@ucalgary.ca.

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statistical or probabilistic claims concerning purported states” (Martin, 1997, p. 218), or worse, “an ontology of pure ‘probabilifyingnesses’” which consists in disembodied probability bundles.¹ Martin’s worry about dispositions is not an issue here (at least not explicitly) but I bring up the question of Pythagoreanism because in debates on quantification we can quickly lose sight of the ontological problems that bedevil us.

Michael Westerman and Stephen Yanchar have done us a great service by bringing a range of issues up to date. Their work has helped clarify these matters further and I expect this will be a useful entry for further debate and discussion. Hence, on that score alone I am grateful for the opportunity provided me to raise a number of vexing issues and concerns and I hope these will lead to a fruitful dialogue. I appreciate that Westerman’s and Yanchar’s articles are not identical even if, for the sake of argument, I will often treat them as if they advocate a similar position. I hope I will not do damage to their respective cases in doing so.

The debates that have exercised many psychologists for the better part of several decades are summarized here under the rubric of the tensions between qualitative and quantitative methods and the necessity of interpretation. Like Fisher (2003a, 2003b), Westerman and Yanchar wish to make a connection between hermeneutics and mathematics. As qualitative procedures come to find a stable home in the social sciences more generally, and in psychology in particular, it is extremely valuable to remind ourselves what we leave behind when we commit ourselves to qualitative methods and psychology’s version of Pythagoreanism. In this brief comment I wish to summarize what I take to be the most important elements of Westerman’s and Yanchar’s articles and then discuss three areas of concern: the qualitative–quantitative distinction, the problem of interpretation, and the question of measurement. I will conclude with a brief reflection on theory.

Yanchar (2006) argues for a new approach to quantitative research, an approach he calls “contextual-quantitative inquiry”. It is premised on the following argument: “there is no convincing reason to reject this possibility out of hand, that some quantitative strategies can provide useful resources for qualitative researchers, and that new forms of quantification can be adapted or developed to support this work” (Yanchar, 2006). I will call this the *weak* form of the argument for a contextual-quantitative inquiry, more on this later. Yanchar’s position is acknowledged as hermeneutic, that is, he is concerned with the experiential and practical aspects of human life and the central importance of human agency. He is also troubled that interpretive movements in psychology have aligned themselves with qualitative methods almost exclusively and rejected quantitative positions out of hand. This is because those committed to alternative positions have criticized traditional experimental research for its problematic assumptions and the methods attending this research have been viewed by these critics as equally problematic. Yanchar argues that those committed to alternative views have committed themselves to a “two-paradigm” view of psychology, a qualitative and a quantitative position (although he does not name anyone specifically here I believe this argument may function more as a straw-person argument in these two articles). Yanchar sees quantification as potentially useful to alternative, particularly hermeneutic paradigms. He is not advocating what is sometimes called the “mixed-methods” approach to research that encourages “the eclectic use of whatever method or methods seem to best address particular research questions in

¹Martin is concerned with the ontology imposed on us by Pythagoreanism. Michell simply defines Pythagoreanism as a belief among scientists that all attributes are quantitative (Michell, 1999).

context” (Yanchar, 2006). He resists this approach because it is not clear what criteria one might use to evaluate mixed-method studies, incompatible methods may yield incompatible results, and standards of rigor may be biased against methodological possibilities. He also argues that quantitative research has been neglected by alternative positions because practitioners of alternative views have been preoccupied with critiquing traditional views of science and quantification in the name of defending their own positions.

Contextual-quantitative inquiry then is, for Yanchar (2006), possible for three reasons: first, because certain aspects of human experience “can be meaningfully interpreted in terms of magnitude or frequency, the relevance of which bears directly on the social, moral, practical, and spiritual fabric of people’s lives.” Second, new quantitative strategies could be developed to investigate complex human experiences and, third, existing strategies could be reinterpreted. Finally, Yanchar (2006) argues that we should develop an alternative view of quantification, namely, one that emphasizes a shift “(a) from self-contained variables to dynamic modes, (b) from measurement to interpretation in context, (c) from internal validity to trustworthiness, and (d) from generalizability to transferability.”

Some preliminary comments are in order first. I referred to Yanchar’s view as a “weak view” to distinguish it from what *could* have been a strong view on quantification, namely the claim that the end of research is the eventual quantification of studies and phenomena for purposes of precise research, the aim of which is the explication of causal statements. Yanchar does not go down this road and qualitative research is not merely a preliminary to further quantitative research. On this view, quantitative studies are deemed useful with the caveat that “they can be interpreted, or reinterpreted, to fit within a contextual, interpretive framework” (Yanchar, 2006).

Westerman’s article attends to this problem in a slightly different way. He argues that a better view of quantitative research is to see it as inherently interpretive. This is true regardless of the kind of quantitative research in question, correlational, experimental, and so on. Furthermore, even critics of “positivism” do not see this interpretive aspect of quantitative research. The position advocated by Westerman, a hermeneutic perspective on practices, virtually demands the acceptance of quantitative methods.² Since the “understanding of psychological phenomena is irreducibly interpretive” (Westerman, 2006), and quantitative measures rely on interpretation, they are not mutually exclusive (although this argument I believe is not clearly made due to multiple meanings of “interpretation,” more on this below). Furthermore, Westerman argues that we can never fully “know” psychological phenomena, that is, our knowledge is always incomplete. He suggests a number of examples that demonstrate the interpretative nature of quantitative research, including research on “relational codes” (Westerman, 2006; e.g., observing patients in psychotherapy) and experiments (e.g., where the participants are “genuinely involved” in some activity). The theoretical frame espoused by Westerman (2006) is what he calls a “participatory approach” and a “participatory framework,” not to be confused by what has long been known as “participatory action research” derived from Lewin’s social psychology. Westerman (2006) means by a participatory approach “key features of

²Westerman’s notion of practices is inspired by Wittgenstein. See Maraun (1999) for an alternative take on the problem of measurement inspired by Wittgenstein.

theories that treat the person as involved in practical activities from the outset” where the literature on “scaffolding” provides a good illustration.

I think Westerman’s view is also a weak view on the use of quantification. The argument insists only that quantification shares with hermeneutics a necessary interpretive moment. It does not insist that quantification is necessary to hermeneutics or that a hermeneutic perspective on practices can only be furthered quantitatively. On this count then I see both Westerman’s and Yanchar’s articles as generally encouraging quantification without the insistence that characterizes most mainstream experimental research traditions. It simply encourages us not to reject quantitative data out of hand, in the same way that we use, for example, results from opinion polls to inform our discussions of political issues even as we know that this data may be incomplete, partial or “unscientific” to unknown degrees.

1. Qualitative and quantitative

Westerman and Yanchar also offer a useful corrective to the often-implicit dichotomy that pervades the literature on methods. The qualitative–quantitative distinction is unhelpful, having served more as a rhetorical ploy to denigrate qualitative (or alternatively, quantitative) work than to actually mark out a genuine difference. Both Westerman and Yanchar make this point, recognizing that qualitative research is often associated with forms of critique of traditional versions of psychology, but need not necessarily be opposed to quantification (see also Essex & Smythe, 1999; Michell, 2003).

How did we come by this awkward dichotomy in psychological research (a dichotomy that has its parallel in neighboring disciplines such as sociology)? In part, this dichotomy is a legacy of the post WWII extension of experimentation into all areas of psychology (see, e.g., Morawski, 1988; Stam, Radtke, & Lubek, 2000) and the downgrading of non-experimental methods as epistemologically inferior. By the 1960s, methods books routinely qualified experimental methods, and their associated quantitative procedures, as superior to those of qualitative methods largely on the basis of the advantage of experimental methods in explaining “causes.” This dichotomy was deeply embedded in the gradual standardization of experimentation that followed. In addition, the application of “small-sample” statistical testing procedures in psychological research became widely prevalent in the 1950s. The first statistical textbooks to promote these methods came into being at the same time as “tests of significance” became widely used in the psychological literature (see Halpin & Stam, 2006, for a brief overview). By 1960 these tests of significance had become a standard feature of psychological research and graduate training. By 1960, however, the first critiques of significance testing appeared in the literature and these have not abated (see Nickerson, 2000, for an overview). Among these is the realization that significance testing is a hybrid of two opposing theories; those propounded by Fisher on the one hand, and Neyman and Pearson on the other. Fisher saw an inductive role for the experiment, whereas Neyman and Pearson were interested in inductive behavior. By 1960 these two approaches were regarded as the major antinomy within frequentist (i.e., non-Bayesian) statistics (Halpin & Stam, 2006). Fisher and Neyman did not compromise their original positions, and the distinction between their theories has persisted in mathematical statistics. Thus, the adoption of statistical testing by psychological researchers was not a straightforward matter in so far as statisticians simultaneously propounded two incommensurate approaches to statistical testing that were combined in psychology’s

hybrid version of significance testing. The application of numbers to experimental results in psychology was dogged by controversy from its very inception.

This unique combination of experimental design with tests of significance, developed largely within psychology, gradually came to be known as *the* experimental method. That it is only one possible method among a great many is now obvious to anyone with knowledge of the development of methods in neighboring sciences, or in psychophysics for that matter. Hence, when we support the use of “quantitative methods”, it is important to determine just what is meant. The development of quantification in psychology is anything but straightforward and the problems associated with the development of tests of significance are themselves fraught with difficulties. Calling for quantification then means being clear on what one is not wishing to re-introduce, namely a series of mistaken views that have persisted into contemporary versions of psychology. On this count it was not always clear to me what *kind* of quantification either Westerman or Yanchar had in mind. Was it the application of numbers to objects and events alone, or do they advocate the use of null-hypothesis significance testing methods as part of a hermeneutic enquiry?

The qualitative–quantitative distinction is not only an artificial distinction, then, but is also not particularly helpful in identifying appropriate methods for particular research problems. The vast array of disciplines and approaches that now go by the name of science are clearly not wedded to single methods. Indeed, scientific fields or areas of investigation are themselves the unique formulation of problems that require solutions with methods developed to find answers to those problems. It is impossible, for example, to imagine the development of the field of biochemistry without chromatography, that technique developed in the early 20th century to determine the identity and concentration of molecules in a mixture. It would be irrelevant to ask if this is a qualitative or quantitative procedure, just as it would be irrelevant to ask if the Krebs cycle is qualitative or quantitative in nature. It is just the wrong kind of question, in part because the *qualities* in question are known and their *quantitative* relationship to one another is crucial for understanding the *qualities* involved.

Psychophysics more closely resembles these processes in psychology. Psychophysicists from the time of Wundt forward have attempted to develop mathematical techniques that appear to capture the major systematic features of human perception. Whether in the form of Fechner’s law, Stevens’ power function, Signal Detection Theory and so on, psychophysical calculations attempt to articulate precisely the relationship between perceptual experience and the nature of the physical stimuli in question. Note, however, that the perceptual judgments provided by research participants are not of the same order as those in, say, Newton’s second law of motion ($F = M \times A$). For example, Fechner’s psychophysical law ($R = k \log S$) serves an analytic purpose even as it does not provide us with the certainty of Newton’s law. By showing that the magnitude of a sensation is a linear function of the intensity of a stimulus, the researcher is able to pursue precise relationships, as has in fact been done for over 100 years. Certain predictions from the psychophysical law allow the researcher to pursue further questions, refine the relationships for different sensory domains, determine their limits, and so on (Robinson, 1985). Despite this precision, however, the psychophysical law has no direct physical relationship to the sensory organs. As we move further away from perceptual research into the broader realms of human psychology, the use of the word “law” becomes a simple heuristic for a set of relationships that, at best, bear some ordinal relationship to each other. Consider, for example, Teigen’s (1994) discussion of the Yerkes–Dodson law.

The question of which qualities are important and which quantities are to be measured then are not determined by the methods; normally we expect the methods to follow our scientific problems, not lead them, for that would be the proverbial dog wagging the tail. It is high time then that we abandon the dichotomous use of qualitative *versus* quantitative and I think both Westerman and Yanchar are correct on this point. But this merely raises the most obvious of questions, just how does one determine when to apply numbers to phenomena? I will address this question of measurement momentarily, first a brief interlude on interpretation.

2. Interpretation

If methods follow problems of interest, as they generally do in the sciences (with certain exceptions), then interpretation must surely be crucial for the case that Westerman and Yanchar wish to make. First, I want to note my agreement with the authors on raising the question of interpretation, for in important ways it clarifies the question of methods. However, I see at least two versions of interpretation that seem to run through their respective articles. On the one hand, interpretation is meant in the context of a hermeneutic position, one that privileges lived experience and the understanding of life from the sympathetic position of our own historical, unfolding experience. On the other hand, there are times when it appears that “understanding” is to be viewed in a rather ordinary way. For example, I understand in a quite straightforward way why children might visit a museum, what numbers referring to their preferences might mean in such a context, and so on (see Westerman’s article). Likewise I also understand Yanchar’s description of construct validity and content validity and how it could still be of use within a contextual interpretation. But the example given, the fluctuation of a “person’s action or ability” from “one situation to another” is rather mundane and does not require some hermeneutic account of understanding or meaning. This strikes me as a far distance from the richer question of understanding in hermeneutics implied by the work of Charles Taylor, Paul Ricoeur, Merleau-Ponty, and others whose work has infiltrated deeply into psychological hermeneutics. I had difficulty connecting the use of quantitative methods with the more interesting and rich tradition of hermeneutics that the authors take to be their theoretical account for it was never made clear just what difference a hermeneutic position might make. That is, most of what Westerman and Yanchar discuss I took to be uncontroversial with respect to interpretation. I will try to explain this by referring to a general scientific context where interpretation in this mundane sense is taken for granted.

All science is interpretive in at least the ordinary, background sense I referred to above. Indeed, even the logical positivists (or logical empiricists) were aware of the interpretive nature of quantification.³ Carl Hempel (1966, p. 88), one of members of the Vienna Circle, argued that theoretical terms were to be understood by “interpretative sentences” (1966, p. 88) although he meant by this something quite precise, namely a pretheoretical or

³Both Westerman and Yanchar refer to “positivism” as if it was a single philosophical position. Psychology, however, adopted a kind of late 19th century Machian positivism (emphasis on observation) and save for a few behaviorists, was largely aloof to developments in logical positivism, to psychology’s detriment. Had psychology remained aware of these developments it may have been quicker to abandon its singular emphasis on a single experimental model following WWII (cf. Smith, 1986).

“antecedently available” vocabulary. In discussing the problem of operational definitions that provide the necessary pretheoretical terms, Hempel noted that

the conception of the terms of a theory being individually interpreted by a finite number of operational criteria has to be abandoned in favor of the idea of a set of bridge principles that do not interpret the theoretical terms individually, but provide an indefinite variety of criteria of application by determining an equally indefinite variety of test implications for statements containing one or more of the theoretical terms (1966, p. 100).

Furthermore, in explaining the actions of agents (as in the case of psychology) Hempel argued that one needs to know something of the motives and beliefs of that agent, not only the agent’s behavior or physiology. Interpretation in this latter sense means merely knowing something about the agent in question, but this is hardly straightforward, a problem Hempel sidesteps. The term “pretheoretical” does not solve the problem for Hempel, of course and opens up the vexing problem that our skills and capacities as members of some human community makes the explication of pretheoretical knowledge in psychology indefinitely open. (I say indefinitely open rather than infinite because we are bounded by our bodies and our location as apt participants in a particular version of social life.)

The dilemma then is as follows: interpretation is widely acknowledged, even by the logical positivists as a necessary component of scientific theory building and testing. They argued that the problem of the open-ended nature of interpretation and the limitless foundations to an interpretive enterprise was amenable to a solution that did not appeal to metaphysics. That is, it was possible, in principle, to specify the meaning of theoretical terms through the precise use of pretheoretical or bridge principles. The spectacular demise of logical positivism seems like so much history, although it is important to remember why: the underdetermination of theory by data cleared the way for a thorough re-evaluation of the scientific enterprise followed by what was called holism, namely the view that our theories do not confront the world piecemeal but rather as a whole. Adjustments can always be made elsewhere in the system when we find that our results fail to confirm a favorite hypothesis. As a consequence, science is thoroughly interpretive. The question then is, how does empirical adequacy continue to rein in an otherwise limitless and open science—a question that has continued to preoccupy those in the philosophy and sociology of science. Regardless of how one answers this question, that is, with a version of pragmatism, instrumentalism or otherwise, the open and interpretive nature of science is not in question.

Both Westerman and Yanchar as I have already noted, are committed to more profound notions of interpretation than that implied by the more mundane sense of interpretation and they argue that their views on psychology are informed by a rich tradition of hermeneutics committed to notions of agency and lived experience. I share these commitments, in part, for I agree that psychology has been impoverished by a narrow functionalism that does not take seriously the most compelling and historically important features of human life. Yet I do not see how their theoretical commitments have need of a connection with a particular form of research, whether it involves quantification or not. What I have called the weak version of their argument is simply the case that quantitative methods can be consistent with hermeneutic accounts of human action, what Yanchar calls “contextual-quantitative studies” and what Westerman has labeled an “explicitly

interpretive quantitative research.” But quantitative methods and approaches are also consistent with traditional behaviorist research, cognitive functionalist versions of research, so-called humanistic research, neuroscience studies, psychophysics, clinical trials of psychotherapy, and so on.

I am not arguing that a hermeneutic psychology is explicitly opposed to quantification—this could be construed as anti-hermeneutic in the first instance. Nor am I arguing that numbers have no place in a hermeneutic or broadly interpretive analysis. What is missing from Westerman’s and Yanchar’s articles is an account of just how quantitative research is especially important, or just relevant, to their interpretive versions of psychology. I realize that this is a matter that might take more than an article or two to describe but I found the absence of detailed examples in their articles telling. Without such examples it was difficult to know what a hermeneutically inspired quantitative study might look like. I suspect that this is due to another vexing issue, namely the nature of measurement.

3. Measurement

The most important issue that these articles can bring to light is the need to clarify the question of measurement. This, rather than quantification, seems to me to be the problem at the core of the issues raised by Westerman and Yanchar. Yanchar (2006) is clearly aware of this as he remarks early on in his article that he is not interested in smuggling in the “quantitative imperative”. My worry is not that he is smuggling it in through the back door but rather that it is walking in through the front door, head held high. Perhaps this is an unfair criticism, but in the absence of clear examples of just what the approach advocated looks like, I find it hard to avoid this conclusion.

Yanchar uses the term as defined by Michell (1999, 2000); it refers to Michell’s critique of the notion that measurement is necessary for a discipline to be scientific. It is derived from his fundamental disagreement with the practical syllogism, that “all science is quantitative; psychology aspires to be a science; therefore, psychology must be quantitative” (Michell, 1999, p. 39). Neither Westerman and Yanchar take this view explicitly since they argue for an acceptance of quantitative methods alongside qualitative methods. However, the issue is not settled on this score, for what is it that is being quantified? The very act of *counting* is not *measurement*. The kinds of examples provided by Westerman and Yanchar, such as “the number of times a student tries before giving up on a task” (Yanchar, 2006), or that of museum exhibits and the preferences that are equally interesting to girls and boys (Westerman, 2006), are not clearly measures of anything so much as they are the application of numbers to events. As Michell has argued, whether in fact something is measurable is itself an open, empirical question. Since much of what constitutes measurement in psychology consists of frequency data, measurement is assumed, not given. For Michell (1999, p. 17), the issue comes precisely to this question: measurement exists only when there are “real numerical relations (ratios) between things (magnitudes of attributes), and not the attempt to construct conventional numerical relations when they do not otherwise exist.”

It is obvious that the critique of the quantitative imperative puts into question the measurement practices of most of psychology. However, most researchers who advance and defend qualitative methods do not in fact have a sophisticated argument about the nature of measurement in mind; this problem rarely plays a role in writings on qualitative research. Instead, their arguments are concerned with preserving the integrity (however

defined) of experience as well as preserving a language or narrative that is minimized or diminished in quantitative research. Indeed, the argument is often an ethical one; participants in quantitative research not only lose their “voice” but their uniqueness is replaced by the researcher’s perspective, the “view from nowhere.” Nonetheless, if we are to return to a tradition of quantitative research to supplement or advance the questions brought to bear by qualitative research as Westerman and Yanchar propose, then it behooves us to be explicit about what it is, if anything, we are measuring. To simply adopt a liberal view on quantification because it is not explicitly *opposed* to qualitative traditions is not an in principle adoption of genuine measurement so much as it is a return to counting strategies that, in the long run confuse the issues. Hence, quantification is fine, so long as you are indeed measuring entities that are quantifiable. This is further confused in psychology, however, through the convention of creating variables in an ad hoc fashion.

The questions here are complex and I can only touch briefly on the major themes. The extensive use of variables in psychology creates the possibility for assigning numbers without regard for things. It allows the *probabilifyingnesses* of psychology to proceed through a process of abstraction. When we create two groups, even if those groups have natural members such as the girls and boys visiting Westerman’s hypothetical museum, we collect numerical responses from those two groups and compare them. The measures compared are aggregates and hence cannot be brought to bear on any individual in those groups except in the most generic (and hence abstract) way. The aggregate, the variable to be explained (e.g., “interest in exhibits”), has an abstract relation to the individuals who contributed to the numerical value of that variable.⁴ As such, we can multiply the number of variables indefinitely since they are created by the experimental setup and are not features of the world in the way a neuron or a rock might be—that is, we have not justified our measurement by demonstrating that our object of interest (preferences) are indeed quantifiable. We could measure many different versions of “interest” (e.g., interest in modern art, interest in sculpture, interest in the museum’s tour guide, and so on) by simply modifying a single question on a questionnaire. In each case we would create a new variable, we could intercorrelate these with other measures, determine their reliability and validity, and so on. We could even discuss their interpretive relation to one another. But since they are not true “measures” they never refer back to some concrete feature of the world; instead they are abstract functional entities that serve the ends of the research.

The solutions to such problems are complex and can be recovered in theories of measurement such as conjoint measurement theory (seriously solving the question of the assignment of numbers to objects and specifying the degree to which this assignment is unique). The history here is lengthy (cf. Fisher, 2003a, 2003b; Luce, Bush, & Galanter, 1963). Of course, one may settle for the consensus that assigning numbers to things is “arbitrary” and conduct business as usual; a time-worn strategy in the social sciences. But it is precisely this “business as usual” that has led many researchers to abandon the practice of assigning numbers to events and then treating the aggregates derived from those numbers as genuine quantitative representations of the things themselves. The interpretive problems associated with the failure of psychology to take measurement seriously are legion. Imagine a world where we each developed our own measures of length or temperature (in fact, a problem that bedeviled the pre-industrial world). At what point do we realize we are no longer talking about the same “thing”? How is this different from

⁴See Danziger (1990) for a history of this development.

the multitude of personality “measures” that exist in the literature (notions of reliability and validity notwithstanding, since these only prolong the search for common measures without resolving the fundamental dilemma)?

That some researchers have turned to “qualitative research” does not mean there is a necessary divide between quantitative research and qualitative research (even if a rhetorical divide exists). Qualitative research (itself not a single kind of research strategy) is one way of reproducing in research format the kinds of crucial features of life about which one wants to know something important. But one could easily call this something else; it is *research* in the sense that one seeks to uncover formalities, regularities, qualities, natures, kinds, types, sorts or categories of existence, being or life that are of interest both to the participants as well as the researchers in question. It is not just something that is *opposed* to quantitative measures. In short, without having solved the measurement problem, the nature of quantitative research is often ambiguous.

4. Theory

I am certainly not alone in suggesting that there is no easy solution to the dilemma created by quantification in psychology. In addition, sometimes the movement to adopt qualitative methods has led to new and different problems, equally intractable. After all, it is not method that is at fault if one’s theoretical frame and observational capacities are not capable of distinguishing meaningful features of the world. Many, but not all, qualitative researchers are concerned about meaning, agency and the moral dimensions of human life and cannot see a way to capture those in the framework of traditional methods. However, poor theorizing and badly considered research is not much of an improvement over the blind adherence to quantitative methods. Loosely constituted qualitative research, where qualitative often means no more than “what someone has told me”, can be just as confusing as poorly designed experimental research and, at worst, leads to a form of academic journalism.

On the other hand, there are very complex research programs that have been developed out of alternative theoretical conceptions. Genuinely considered phenomenological research is not only possible, but widely practiced as the work of Amedeo Giorgi and others has shown. Conversation analysis was originally inspired by Garfinkel’s ethnomethodology (Garfinkel, 1967) and has spawned fruitful, intricate, as well as applied, forms of research that continue to mature.⁵ Beginning with the work of Harvey Sacks, its offshoots in the form of discourse analysis have led to entirely new lines of research, new journals, and new research communities. Indeed, to choose one example out of this tradition would be impossible given the vast network of researchers now working in this field. Other examples include ethnography in the sociology and anthropology traditions as well as a wide range of participant observation strategies.

All research, for it to be research of some consequence, requires the development of theoretically viable positions that guide the interests of the investigators. Methods in this respect are secondary, although methods can indeed become attached to an entire research program such that a phenomenon becomes firmly linked with a method (e.g., paired associate learning; personality scales and the five-factor theory of personality; galvanic

⁵For the sake of argument I have simplified the relationship between Garfinkel’s theory and conversation analysis. Naturally, a more fulsome account is required to trace the influences on conversation analysis.

skin response). Could we even think of “factors” of personality without the use of personality inventories and factor analysis? Communities of investigation, or “epistemic cultures” in the words of Knorr Cetina (1999), have agreed upon ways of knowledge construction. It brings me to my final concern: the power of science has been, in part, a power of social relations. It is the possibility of inter-laboratory cooperation as well as competition that has spawned its successes. Without a network of cooperation on foundational issues, competition and hence the advancement of broad research programs becomes extremely difficult. The debate on qualitative/quantitative methods threatens to continue in its fragmented and limited scope, skirting about the margins of psychology with minimal constraints—it is a good example of a debate without foundations. Without some fundamental agreement on questions of theory it is unlikely that this debate will come to any conclusion soon, unless sheer cognitive fatigue wins the day. A consequence of this dichotomy is that there is little movement out of this conundrum; merely investigators going their own way and following independent paths of inquiry that have little to do with each other. It is not the case that we need to either resolve or eliminate the debate and the dichotomy by fiat; the dichotomy is just not helpful. The real question remains: if we are to use *numbers* or *qualities* in our research then we must have good reasons for doing so; what are these? And the one does not, ipso facto, rule out the possibility of using the other.

In one of the works attributed to Aristotle (1984, p. 1868), *Magna Moralia*, he argues that Pythagoras “attempted to speak concerning virtue, but he did not speak correctly for bringing virtues into correspondence with numbers, he did not make any distinct”. Virtues are indeed beyond number since it is doubtful that they can be shown to be genuinely measurable. We should reject the temptation of Pythagoreanism for so long as the allure of numbers are aligned with the reigning ethos of scientism, the costs are simply too high.

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