30th ALTENBERG WORKSHOP IN THEORETICAL BIOLOGY

Quality & Quantity: Limits of Quantification in the Sciences

organized by

Richard R. Nelson, Werner Callebaut, and Isabella Sarto-Jackson

June 11-14, 2014

KLI Institute

Klosterneuburg, Austria

Welcome

to the 30th Altenberg Workshop in Theoretical Biology. The Altenberg Workshops are interdisciplinary meetings organized by the KLI Institute in Klosterneuburg, Austria. The workshop themes are selected for their potential impact on the advancement of biological theory. Leading experts in their fields are asked to invite a group of internationally recognized scientists for three days of open discussion in a relaxed atmosphere. By this procedure the KLI Institute intends to generate new conceptual advances and research initiatives in the biosciences. We are delighted that you are able to participate in this workshop, and we wish you a productive and enjoyable stay.

Gerd B. Müller
Chairman

The topic

The workshop will focus on the oppositions between "quantitative" and "qualitative" research that abound in the scientific and philosophical literature, but are often deeply misleading. By way of example, does it make sense to call the way organic molecules are characterized "qualitative" because much more than "numbers" is involved? On the other hand, can we claim that their characterization is "quantitative" only?

Scientific fields (and subfields within them) in the natural and social sciences differ substantially in the ways in which they rely on quantification, and to what extent. For instance, it is often suggested that in (parts of) physics, mathematics plays a "constitutive" role with respect to concepts and theories, and that this distinguishes physics from other sciences. Our main aim is to articulate a comparative framework that usefully accounts for the ontological, epistemological, and methodological aspects of "quality and quantity" talk, and enables a constructive debate on these and related issues.

Three rather different issues are often conflated in the arguments about quantity and quality: characterization of the phenomena; theory (articulation of causal relationships); and research methodology. Lord KELVIN was referring to the first issue when he said:

> When you can measure what you are speaking about, and express it in numbers, you know something about it: but when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of *science*, whatever the matter may be.

The use of quantities (counts or measures) to *characterize/describe the subject matter* a scientific field deals with—its "phenomenology"—should be distinguished clearly from the use of mathematics in its *theorizing*. Both often go together, but not necessarily so: In many sciences the subject matter is largely described quantitatively, but the theory is not mathematical. In other sciences mathematical models are used to suggest what is going on in a subject whose description is largely qualitative.

The use (or not) of quantitative research *methods* further complicates the picture. Many quantitative methods in the natural sciences rely on instruments that cannot capture exhaustively the features of the objects under study. In many fields researchers use measures they construct or select that they know are partial and inexact characterizations of the phenomena in which they really are interested in, in order to do statistical analysis.

The perceived divide between “quality” and “quantity” has roots in diverse intellectual heritages, including the Aristotelian tradition. One way of thinking that gained prominence after the Scientific Revolution is epitomized in GALILEO’s dictum that “*the book of nature is written in the language of mathematics*.” In this view, mathematical theorizing can provide the basis of a unified theory of nature formalized in a universal calculus whose axioms and rules can be applied independently of the object under study; but see ISRAEL (1996) on how the advent of modeling (LOTKA, VOLTERRA…) led to the evaporation of this dream.

Other scientists and scholars, rather following Francis BACON’s lead, have red-flagged the quantitative worldview, worrying that logic and mathematics might impose laws of their own to the detriment of the integrity of the object of research. Exclusively quantitative approaches, they fear, might unduly constrain or distort theory development, which requires a rich and deep understanding of phenomena, to be gained only through—iterative—quantitative *and* qualitative inquiries.

Quantitative research captures phenomena in mathematical terms that suggest *objectivity* (“the language of God”). But it is a non sequitur that quantification is able to avoid subjectivity on, say, individual, interpersonal, or political levels. In practice, scientists usually aim for consensus or inter-subjective agreement as imperfect proxies for objectivity. This is recognized in calls for “gold standards” or double blind, randomized approaches, indicating that subjectivity and objectivity are not opposites but, rather, mutually dependent in a dialectic manner. Moreover, quantitative research might imply a narrowing down and impoverishment of the research goal, and lead to missing out grossly on the “complete” description of phenomena (which is elusive anyway). In contrast, non-mathematical representations such as narratives (which may include analogy and metaphor) or non-numeric symbols (figures, graphics, etc.) that afford multi-dimensional descriptions constrict research less by enabling access to objects or processes from different methodological or cognitive angles.

> [Natural] phenomena are complex, our scientific interests in them are heterogeneous, and the number of possible ways of representing them … is large. (GRIESEMER 2000)

Finally, quantitative research is often suggested to be superior to its qualitative counterpart, supposedly justifying the ambition to "*measure what is measurable, and make measurable what is not so*" (GALILEO) in terms of *precision* and *rigor*. But what does this mean in practical terms? IQ is a good example of a measure of something that may not be measurable (at least not simply). Or consider the periodic table of elements: should we characterize is as “mathematical,” “quantitative,” “qualitative,” or as something distinct still?

Aims

In this workshop we want to argue for the claim that the dichotomy of “the qualitative versus the quantitative” that is entrenched in any scientific fields is deeply flawed, as it conflates several dimensions of research, including the characterization of the phenol-mena studied, theory formulation, and the methods applied. Antithetic thinking about these issues is probably fueled by the belief that the discrimination of quality and quantity adds some sort of valuation to the research in question. But this belief is deeply mistaken: qualitative and quantitative research are intertwined on multiple levels, and neither is more “fundamental,” really. A clear-cut separation will therefore be of little use for either of the two “camps,” as it is bound to result in oversimplification of complex issues while ignoring the judgmental impact of the language used for labeling.

Format

There will be 13 presentations, with 50 minutes allotted for each—roughly 10-15 minutes for each talk, followed by 35-40 minutes for questions on that talk and discussion. On Thursday we kick off with a joint introductory statement, addressing the aims and framework of the workshop, by the organizers; on Saturday we end with a general discussion, including publication plans.

To support discussion during the sessions, we encourage all participants to send a rough draft of their presentation and/or some materials that are relevant to their topic to the organizers in advance of the workshop, to be circulated among the participants.

Manuscript preparation and publication

The Altenberg Workshops in Theoretical Biology are fully sponsored by the KLI Institute. In turn, the Institute requires all participants to contribute a paper to a volume edited by the organizers. Altenberg Workshop results are usually published in the *Vienna Series in Theoretical Biology* (MIT Press), but given the very interdisciplinary character of the current workshop, in which biology is only one among several foci, the organizers have approached other publishers. The volume (definitive title to be determined still) will further develop the novel ideas and concepts generated as a result of the workshop. The contributors are not necessarily limited to the original participants; they may be complemented by experts on those topics that emerge as important and may include co-authors invited at the discretion of the participants. This procedure is intended to advance our understanding of the relation between quality and quantity, and the limits of quantification in the sciences. Because of the explicit interdisciplinary nature of the effort, the outcome should be attractive to a wide range of experts in the natural and social sciences as well as in the humanities.

We expect that participants will revise their drafts as a result of our discussions at the workshop and the ensuing review process (probably "round-robin," during which commentaries will be elicited for each paper from two selected members of the workshop). We are aiming for a January 5, 2015, date for receipt of finished manuscripts for publication. The length of the contributions should be approximately 8,000 words. The use of figures and photographs is highly encouraged. All contributions will be edited for style and content, and the figures, tables, and the like will be drafted in a common format. The editors will send specific instructions after the workshop.

Richard R. Nelson, Werner Callebaut, and Isabella Sarto-Jackson

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Quality & Quantity:
Limits of Quantification in the Sciences

Wednesday 11 June	Evening		
6.00 pm		Welcome reception and dinner at the KLI Institute	

Thursday 12 June	Morning	Philosophical and Historical Perspectives	Chair: Keller
9.30 am – 10.20 am	R. Nelson, Callebaut, Sarto-Jackson	Introductory Statement about the scope of the workshop	
10.20 am – 11.10 am	Callebaut	A Short History of Quality	
11.10 am – 11.40 am	Coffee		
11.40 am – 12:30 pm	Bookstein	No Quantification Without Qualification, and vice versa: A Multilayered Pattern Language for Tomorrow's Observational Biosciences	
12:30 pm – 2.30 pm	Lunch	at the KLI Institute	

Thursday 12 June	Afternoon	Qualitative Research in Biology	Chair: Bookstein
2.30 pm – 3.20 pm	Barwich	"The Standard Observer": Humans as Measurement Instruments for the Quantification of Odor Qualities	
3.20 pm – 4.10 pm	Albertazzi	Qualitative Appearances	
4.10 pm – 4:40 pm	Coffee		
4.40 pm – 5.30 pm	Richards	Between Quality and Quantity: The Archetype from Kant and Carus to D'Arcy Thompson and EvoDevo	
6.30 pm		Departure for Dinner at a Viennese Heurigen	

Friday 13 June	Morning	Q&Q Research in Economics	Chair: Callebaut
9.30 am – 10.20 am	Halsmayer	Making Economic Growth a Measurable Entity	
10.20 am – 11.10 am	R. Nelson	Description, Narrative, Numbers, and Models in Analysis of Long Run Economic Development	
11.10 am – 11.40 pm	Coffee		
11.40 am – 12.30 pm	Witt	Kinds of Causal Explanations and Their Affinity to Quantification: The Case of Economics	
12.30 pm – 2.30 pm	Lunch	at the KLI Institute	

Friday 13 June	Afternoon	Q&Q Methods	Chair: Sarto-Jackson
2.30 pm – 3.20 pm	K. Nelson	The Quantitative Emphasis in Psychology	
3.20 pm – 4.10 pm	Morin	"One Fact, One Vote! ": Quantitative Methods and the Democracy of Evidence	
4.10 pm – 4.40 pm	Coffee		
4.40 pm – 5.30 pm	Keller	Assessing Risk in the Absence of Quantifiability	
6.00 pm		Free evening for exploring Vienna	

Saturday 14 June	Morning	Outlook	Chair: R. Nelson
9.30 am – 10.20 am	Sarto-Jackson	Transgressing the Borders of Quantification by Data Visualization	
10.20 am – 11.10 am	Wagensberg Lubinski	Is Quality Quantitatively Measurable?	
11.10 am – 11.40 am	Coffee		
11.40 am – 12.30 pm		General discussion and publication plans	
12.30 pm – 2.15 pm	Lunch	at the KLI Institute	
2.30 pm		Departure for Danube boat trip and dinner in Schloß Dürnstein	

Abstracts

Werner Callebaut
KLI Institute & University of Vienna

A Short History of Quality

We may not all agree (yet?) at this workshop with the conclusion of Fred BOOKSTEIN's abstract, which—generalized from biology to scientific endeavors in general—reads: "If it is to optimally serve valid … reasoning, the qualitative must be the quantitative; and, obversely, every sufficiently profound scientific quantification is qualitative too." But I think we all agree—although this point is readily forgotten by many Q/Q dichotomists—that qualities that are orderable (say, natural numbers by order of magnitude: 1, 2, 3, …) by this operation become quantities. Natural numbers are not "more" quantitative than, say, the colors red and green (understood physicalistically in terms of wavelengths), which are equally orderable; it's just that it is easier for many of us to screen off the qualitative from the quantitative in the former than in the latter case. Enters the distinction between primary (observer-independent) and secondary (observer-dependent) qualities, which can be traced back to pre-Socratic philosophy.

But what is quality? Pondering the kind of knowledge that is handled by "exact science" at a time when materialism and realism were deemed obsolete by philosophers of physics, Arthur EDDINGTON (1929) remarked that "… the poetry fades out of the problem, and by the time the serious application of exact science begins we are left with only pointer readings.… The whole subject-matter of exact science consists of pointer readings and similar indications." The philosopher A. Cornelius BENJAMIN concurred that by abstracting from objects, science "loses an important feature of them." Since the artist's, the religious believer's, and the mystic's attempts to capture this "elusive element" are "essentially emotional and irrational," BENJAMIN saw it as the philosopher's task to "pursue this qualitative element by *rational* techniques":

> The philosopher must restore to the world its expansiveness and durational character, which the scientist has replaced by meter sticks and clocks; he must repopulate it with heat and movement and pushes and pulls, which the scientist has eliminated in favor of molecules and differential velocities and pointer readings. Only through this supplementation can one understand the world in its totality. (BENJAMIN 1937)

Our own views on the Q/Q conundrum at this workshop, I take it, differ from EDDINGTON's and BENJAMIN's in at least three ways:

(1) In science, quality and quantity are intrinsically intertwined.

(2) Nor should philosophy be reduced to "the qualitative." Corollary: contrary to, say, Michael POLANYI's or Marjorie GRENE's work, analytic philosophy in the last century or so has seriously neglected the qualitative.

(3) "Emotions," "irrationality," and related notions are as plausible subjects of scientific and philosophical investigation as very much else, especially if one takes into account their evolutionary dimension.

My short history of the concept of quality is avowedly presentist (tuned to the objectives of this workshop), western-biased (due to my profound ignorance of non-western philosophies and cultures), and philosophy-centered (for lack of space and time).

I begin by situating "quality" in the theories of categories of ARISTOTLE (every uncombined, "categorematic" expression refers to entities falling into at least one of ten classes: substance, quantity, quality, relation, place, time, posture, state, action, and passion) and KANT (expressions of statements are universal, particular, or singular in quantity; affirmative, negative, or infinite in quality; categorical, hypothetical, or disjunctive in the relation of their parts; and problematic, assertoric, or apodictic in modality). In the grand scheme of KANT's *Critique of Pure Reason*, each function of the understanding yields a category in one of the four major divisions, quantity, quality, relation, and modality. I ask if there is anything in this grand scheme that we can profitably use today, focusing on the role of Q&Q in HEGEL's and ENGELS' dialectics.

Next I survey the history of the distinction between primary and secondary qualities from DEMOCRITUS' "by convention" to GALILEI, HOBBES, DESCARTES, LOCKE, BERKELEY and others to BERGSON to the "qualia" in current philosophy of mind.

What are the take-home lessons from this historical exercise? I round off by juxtaposing my results to some present-day cases and discussions of the handling of qualitative data and research methods, and conclude that the price paid for the oblivion of the history of quality in current science and philosophy is high.

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Fred BOOKSTEIN
University of Washington & University of Vienna

No Quantification Without Qualification, and vice versa: A Multilayered Pattern Language for Tomorrow's Observational Biosciences

"Identical twins are much more similar than any microscopic sections from corresponding sites you can lay through either of them."
— Paul WEISS (1956)

In ordinary scholarly parlance, "quantification" can refer either to a process or to representation by the "quantity," like 5 or 3.14159, that stands for some output of that process. The word "qualification" of my title is meant as dual in the same way: both as a process and as the "qualities," like diseases or species, that emerge from that process. It is tempting to set up an antinomy here. After all, the joints at which (according to PLATO) we carve Nature delimit qualities, not quantities. But that temptation must be resisted, and any opposition of qualitative versus quantitative rejected in favor of a much more tolerant and ingenious interweaving. Over the course of our workshop I will be recommending certain methods of information theory (and its 19th-century precursors) as conveying the potential reunion of tactics suggested by my title. In an alternate metaphor, I will be praising methods that bridge the Q/Q divide rather than requiring us to sequester ourselves on one side or the other. In the Weiss quote above, twinning is qualitative (a discrete event), and microscopy is one of our many unsuccessful attempts at finding Nature's "joints" within human biology, but only quantities submit to the phrasing "much more" that drives this bon mot.

Today's biological academics have badly mistaken the main point here. Typically we teach our students one pier or the other of an inspissated biometrics that vitiates the bridge I have in mind. At one extreme, we have the databases of genomics, proteomics, and all the other contemporary -omic sciences, which, as far as I can tell, remain as overwhelmingly alphabetical as they were when the idea of their “code” was first decoded. At the other extreme, we have academic biometrics and biostatistics, which focus laser-like on scientifically unproductive ideas like sampling or statistical significance testing at the cost of gaining any insight into the pattern languages according to which we formulate the categories under which those very samples and simplistic questions were originally formulated.

Bridges over the Q/Q divide have been suggested in the past. Thermodynamics offers a notation in the formalism of modeling open systems far from equilibrium, systems that may have many stable states upon which they perseverate or around which they cycle. My predecessor Walter ELSASSER, physicist and biophysicist, grappled frontally with this paradox in the course of two great argumentative essays. In his 1975 book he noted how the art of bioscience inheres to a surprising degree in the single craftsmanly choice of what to measure from the unimaginably broad spectrum of choices proferred by any system having the endless multi-scale heterogeneities of organisms. Later, toward the end of his life, he highlighted the interplay between our two domains even more starkly, noting that whereas the other natural sciences study aspects of space-time, matter, and energy, the biological sciences add to this list the fundamentally different category of nonstructural memory, by which he meant, essentially, the offspring’s copy of the parent’s adult form. Importantly, this Elsasserian category of memory is qualitative, a matter of matching a present pattern to a past representation: a representation that is a stable emergent of whatever thermodynamics actually characterize the organism’s physiology at shorter time scales. To ELSASSER, the interplay between the qualitative and the quantitative is thus at the root of any theoretical biology that is worthy of the name. I commend his views to you.

This interplay of the continua of physics with discretizations (qualifications) like species, life cycle, or memory copies over into my profession of “quantitative biomedical data analysis” via a device that, although dating to the end of the 19th century, remains far from fully exploited even today. Yesterday’s favored model of complete disorder was the bell curve (the normal or Gaussian distribution), which describes measured values. Tomorrow's should be the Wishart distribution, which describes the disorder of pattern analyses (in this case, organized sums of products) of arrays of numbers each distributed on its own bell curve. In a more familiar language, this is the disorder of the co-variance matrix, a generalization of the correlation coefficient. Theoretical biology has not paid enough attention to either of these. When the measures driving a Wishart representation's cross-products are a meaningful roster of quantities, the result of the analysis of their co-variances can be any or all of the following: a statement of principal aspects of variation, a statement of deviations from such a model of common principal aspects, a count of how many such patterns might be needed to account adequately for some phenomenon, and an assessment of the uncertainties of all the preceding quantities.

Morphometrics, the branch of biometry with which I am most closely associated, can be reformulated (this is what my 2015 book is attempting to do) as one of the best extant examples of what we're trying to accomplish more generally: a rhetorical system for combining the discrete and the continuous, the individual and the species, from the embryo to, let us say, the professor emeritus, from the Tertiary to the present. All of my own work in morphometrics combines the qualitative (species names or syndromes, anatomical labels) with the quantitative (computational homology, growth, and form) without any possibility of drawing a boundary between them.

In light of this I would venture the following generalization. It is possible that our best contemporary rhetorics of explanation in biology resemble morphometrics in their frank combination of carefully (i.e., qualitatively) supervised parallel quantifications that, taken together, result in new qualifications, leading in turn to new quantifications, and so on.

In this view our best modern models of explanation across a wide range of natural and social sciences are likely to be the network models that combine the qualitative language of nodes and their names with the quantitative language of links and their causal chains in a manner that is completely integrated by the time it arrives in the textbooks. Good research is necessarily both qualitative and quantitative at the same time. A well-defined experiment is a matter of qualifications; a well-calibrated set of measurements is one that allows one or more than one qualitative network of quantified patterns to be extracted.

The joints at which PLATO would carve Nature are necessarily discrete. Yet the quantitative engines we need for localizing these have existed at the foundation of biometrics since the 1890s, were formalized by SHANNON in 1947 and then AKAIKE in 1974 along with everything else in the world of information, and today merely await our closer attention in order to continue bridging the qualitative and the quantitative here in the new century. One cannot make scientific progress without continually refreshing that bridge. Indeed the bridge I have in mind is not limited to biology, but characterizes all the enduring achievements of the natural sciences: the periodic table, the Hertzsprung-Russell chart of stars, the very geometry of the Double Helix itself.

Return, finally, to the quote from WEISS in my epigraph. What makes identical twins "identical" is a qualification; but similarity (or, better, dissimilarity) is degree of similarity, thus intrinsically quantitative, whether in the morphometrics I love, in systems biology, or anyplace else where I have ever checked. One cannot speak one language without at the same time implicitly or explicitly speaking the other. If it is to optimally serve valid biological reasoning, the qualitative must be the quantitative; and, obversely, every sufficiently profound scientific quantification is qualitative too.

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“The Standard Observer”: Humans as Measurement Instruments for the Quantification of Qualities. The Case of Olfaction as a Diagnostic Tool for Neurodegenerative Diseases

Changes in smell perception are a first symptom and a potential diagnostic tool for pre-clinical detection of major neurodegenerative disorders (DOTY et al. 1991). Differences in the course of hyposmias (reduced ability to smell) may also aid in differentiating disorders with similar clinical symptoms such as Parkinson’s, Lewy Body Disease, and Alzheimer’s (WESTERVELT et al. 2003). A basic requirement for comparing abilities and differences of perception between healthy and ill test subjects is the design of standardized identification set of test odors (DOTY et al. 1984), and a reliable way to assess human sensory performances in identifying and discriminating odor qualities.

Nonetheless, the measurement of odor perceptions in humans is notoriously difficult. Little agreement exists in how to measure odor quality (WISE et al. 2001). This lack of standardization in both the profiling of odor quality and the assessment of human sensory performances poses a severe handicap in neurobiological research for exploring how changes in qualitative experiences may indicate and distinguish neurological disorders. An extensive study of olfactory loss, for example, found that the choices of test kit produced different research outcomes (LÖTSCH et al. 2008). Addressing the specific issues underlying olfactory performance tests and the quantification of odor quality, this paper focuses on the role of human test subjects as “measurement instruments,” and the apparent impossibility of eliminating bias in human sensory performances. Although considered as such—the expression of “humans as measurement instruments” appears in several psychophysical papers—a critical exposition of the implications of this conception is not elaborated. Biomedical interest in deviating olfactory performances as related to neurodegenerative diseases renders this neglect all too visible. Instead of ruling out differences within people's perceptions, variations among people's perceptions itself become center focus. These differences are not to be explained away as illusions or minor statistical variabilities but, quite the opposite, are to be assessed as systematic patterns that ought to be objectified. The measurement standard against which deviations become systematized, however, is through comparison with "the standard observer."

The aim of this paper is to analyze the implications of humans as measurement instruments for understanding the underlying problems of observation in quantifying sensory quality. These problems point at a lack of understanding some qualitative aspects of olfaction that might further suggest a correction of what is considered the objective basis of measurement and comparison.

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Qualitative Appearances

The demand for a science of qualities (i.e., of the subjective experiences of the surrounding world) has not yet received a satisfactory response. Such a science in fact requires a largely critical stance on the mainstream view of an objective trustable reality, universally founded and univocally expressed in quantitative terms and in a third person account. The main unasked question in science is still that of the levels of reality and the sciences correlated to each different level; and of the methodology/methodologies to adopt in scientific research according to the (ontologically) different observables under consideration. Psychophysics (FECHNER 1860), for example, concerns the S-R level, and its methodology is simply unable to grasp the complexity of qualitative appearances. MARTIN SELIGMAN, past President of the American Psychological Association, has recently proposed overturning the entire discipline of psychology from being a primarily past-oriented field to a primarily future-oriented one (SELIGMAN et al. 2013). Still blurred, however, are the boundaries of the future. Can we assume that the time is ripe for a second radical Copernican revolution, placing qualities instead of psycho-physical quantities as the main concern of psychologists (ALBERTAZZI 2013a,b), and asking for new forms of measurement and proper metrics for what are essentially semantic dimensions of our being aware and understanding of the environment we live in?

A case in point is the science of color, which varies from colorimetry (wavelength-receptor mapping, prediction of physical color mixing, etc.) to the semantic dimension of color appearances. This should make us aware that endogenous meanings can appear even into the professional use of the term (are we considering the quality of the color or the manipulation of intensities of channels?). The various color systems convey different quantitative or qualitative information, and they are substantially irreducible to each other. Color qualia can only be classified purely phenomenologically, on the basis of their visual similarity (e.g., Hering, the original Munsell atlas), but neither of these differences apparently derives from either physical properties of radiation or physiological processes. Colorimetry (BOYNTON 1979; BRAINARD 1995) does not deal with this issue. The semantic, emotional, and aesthetic valences such as warmth/coldness, pleasantness/unpleasantness, lightness/heaviness, wetness/dryness, etc., of their modes of appearance (KATZ 1930), however, are so widespread as to suggest an evolutionary spandrel (GOULD & LEWONTIN 1979), or an adaptive response to the environment by which we perceive and remember objects and relations (DE MOUILPIED 1924; FOGDEN & FOGDEN 1974; NEISSER 1976; FOX 1979; SHEPARD 1992; CHANGIZI et al. 2006; HURLBERT 2013), on the basis of *qualitative information cues*. Revolutionary ideas were already present in VON UEXKÜLL's (1934) concept of functional tone, and were methodologically addressed by LORENZ (1977) in his claim to view both sides of the mirror.

I shall present some experiments on the relationships between shape and color as case studies of the deep built-in cross-modal network in *perceiving*. The rationale emerging from the results is that if the analysis of the semantic dimensions is conducted systematically (BURCHETT 2002), in parallel with analysis of the material properties of color and shape (MAUSFELD 2010), it may contribute to revision of the ecological theory of perception (GIBSON 1977, 1979) on qualitative bases.

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Between Quality and Quantity:
The Archetype from Kant and Carus to D'Arcy Thompson and EvoDevo

KANT had suggested that an archetypal pattern might undergo dynamic transformation and thereby produce an "evolution" of species. However, he didn't believe any evidence supported this scenario. GOETHE's morphological ideas developed in a way similar to KANT's, and ignited the imagination of Carl Gustav CARUS—an extraordinary anatomist and artist—who made these incipient conceptions graphic through his morphological study of vertebrates. From his studies he produced a series of graphic analyses—ultimately a kind of geometrical representation—that gave substance to what became known as the vertebrate archetype. In England, Richard OWEN adopted the theory of the vertebrate archetype (borrowing heavily from CARUS without attribution), and DARWIN transformed it into an argument for evolutionary transformation. The same pattern of argument made its way to the 20th century in the work of D'ARCY THOMPSON and current EvoDevo conceptions. The theory of the archetype and its modern equivalents stand between a qualitative and a quantitative mode of explanation.

Verena HALSMAYER
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Making Economic Growth a Measurable Entity

Though the wealth of the nation or the economic power of the national economy was an early category for discussion, it was only after 1950 that the Gross National Product (GNP) moved from being a category of economic analysis to being an everyday concern to government. Concurrently, “economic growth” became a thoroughly defined, measurable, and amenable entity. As part of my dissertation project I develop a series of case studies relating to the early history of “measuring economic growth” in the United States. In these case studies I seek to account for the multifarious interconnections between modeling, measuring and economic governance by looking at the making of the respective data, tools and instruments as historically contingent social processes.

My contribution to the Q&Q workshop will start with the beginnings of national income accounting and the first attempts to “measure growth” at the National Bureau of Economic Research (NBER). My narrative will then move to the use of Robert Solow’s “simple model of economic growth” (1956)—a by-product of dynamic programming research undertaken at RAND—as an instrument for measuring GNP growth on the basis of aggregated time series data. A third episode will deal with the stabilization of the neoclassical growth model as the conventional instrument for measuring growth once national income accounts were standardized under the auspices of international economic agencies such as the OECD.

Despite various extensions and adaptations, the basic structure of “growth accounting” stayed the same: A closed smoothly-working equilibrium system based on the assumptions of perfect competition and perfect foresight, whose growth-rate only changed via the motivation of exogenous factors. This kind of empirical work was not intended to provide a test of the neoclassical growth model, the aggregate production function, or certain equilibrium assumptions. Rather, it was a way of interpreting given data, assuming that they were generated from an aggregate production function. My three historical case studies make visible a shift from an earlier concern with observational uncertainty and problems of measurement to the passive consumption of data. “Growth accounting” was standardized to such an extent that it became accepted econometric work to construct a “simple” and “plausible” model and then to insert figures from a set of time series or cross section data—mostly from secondary or tertiary sources—in a curve-fitting procedure. The consequences of that shift are debated in the correspondence between the protagonists of my case studies and their critics. Their contemporary views on the benefits and pitfalls of (different ways of) measuring “economic growth” will conclude my contribution to the workshop.

Richard NELSON
Columbia University

Description, Narrative, Numbers, and Models in Analysis of Long Run Economic Development

I will begin with some general observations. Fields of science differ greatly in the way numbers and mathematical formulations are used. In some, numbers are the heart of the characterization of the phenomena studied. In others, numbers are sprinkled through what is largely a verbal description or narrative. In some, mathematics is the expression of a "law" believed to hold. In others, mathematic-cal models are allegories aimed to help thinking, with the theory (in the sense of beliefs about the gist of what is going on) expressed verbally.

In the more qualitative of the sciences there often are strong pressures to become more quantitative and mathematical. But it is possible to go too far in this direction. The example I will use is analysis by economists of long-run economic development. As Verena HALSMAYER will document, until the 1950s the treatment of economic growth by economists was largely description and narrative expressed verbally, with numbers as part of that narrative. These numbers included things like the production of coal, the number of patents, average wage rates, the price of wheat. And the "theory" was largely expressed verbally. In graduate education in economics, economic growth or development generally was treated in courses in economic history.

The development of National Product statistics during World War II for the first time gave economists something they could use as a quantitative "measure" of total output, and how output changes over time. As HALSMAYER will recount, this led to a revolution in the way economists described and analyzed economic growth. Growth now was characterized quantitatively. The analysis took the form of formal models and econometrics. Courses in economic growth emerged structured along these lines. A parallel development was the decline of economic history as a subject studied and taught in economics departments. Relatedly, economists doing research on economic growth came to know less and less economic history.

But there is the basic question of whether, or the extent to which, the economic development experience of nations can be characterized adequately in terms of a time series of a single measure of aggregate output. Many economists would argue that, at the least, one needs to recognize that the output of an economy consists of many different types of goods and services, and an important characteristic of economic growth is large changes in that mix, as new industries get created and some old ones die out. But my question is more radical that. Can one understand economic growth without recognizing aspects of qualitative economic history? Can one understand, for example, the very rapid economic growth that occurred in the US after World War II without recognition of the new government policies of large-scale research support? Is a description of that rapid growth adequate if it does not include specific attention to the development of the computer and of microelectronics? Without consideration of Bell Labs and the consequences of the antitrust agreement that was signed by AT&T? Without recognition and explanation of the rise of American research universities to world leadership in these and other fields? Articles in economic journals on economic growth contain nothing on these topics (there are a very few exceptions).

I want to highlight that the point I am making here does not deny, or even downplay, that the development and use of national product statistics and techniques like growth accounting are useful vehicles for understanding economic growth. They most certainly are. Rather, my argument is that, if they are used alone, they simplify the subject matter far too much. The kind of description and knowledge we get from them is a useful part of the way we can understand economic growth, but other techniques and orientations provide extremely important knowledge that these cannot. And prominent among these is largely qualitative, descriptive and narrative, economic history.

I would argue that the situation is the same in a number of other sciences. What is required there is a mix of techniques and perceptions. Numbers and mathematical analyses are part of our understanding, but far from the whole understanding, unless we decide to blind ourselves. My belief is that is what is happening in economics and some of the other social and behavioral sciences.

Ulrich WITT
Max Planck Institute of Economics

Kinds of Causal Explanations and Their Affinity to Quantification: The Case of Economics

I have in mind to go beyond economics by connecting to biology, using Niko TINBERGEN's scheme of four explanations in biology. The different types of explanations that TINBERGEN suggests for the analysis of animal behavior are useful also for explaining economic behavior and economic activities more generally. The differences between the types in terms of their affinity to quantitative theorizing are quite obvious in economics, but may be similarly obvious also in the biological sciences.

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Katherine NELSON
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The Quantitative Emphasis in Psychology

This contribution focuses on the negative effects of the obsession in psychology (as well as in other social sciences) with quantitative measures of constructed variables often vaguely understood based on terms in common language use. Two traditional areas are illustrative: intelligence as measured in IQ tests, and human memory that was for almost 100 years measured primarily in terms of nonsense word lists, or better, lists of real words. I argue that the problem persists in the high value attached to quantification, and its judged significance in "p-values," and the low value attached to qualitative or descriptive studies and the exploration of theory. Contemporary work in Developmental Psychology will be cited in support.

Olivier MORIN
KLI Institute

“One Fact, one Vote!”:
Quantitative Methods and the Democracy of Evidence

At their best, quantitative methods in empirical disciplines can be described as little "republics of facts"—as ways of giving equal voice to a large number of observations relevant to the evaluation of a theory: One fact, one vote. The alternative is to let one's favorite examples or counter-examples decide the fate of hypotheses (monarchy), or simply to give up on testing hypotheses (anarchy). Both monarchy and anarchy are viable ways of doing research; they are commendable when one's hypotheses are beyond controversy, or not specific enough to be tested yet—but these two regimes eventually erode the credit of theories.

Actual republics need to make tough choices regarding their voters: they choose, not just who they shall invite to the vote (the problem of *inclusion*), but also what exactly they shall count as a voter, which could be men, human beings, adults, families, towns, or many other things (the problem of *individuation*). They try to avoid situations where most voters would be influenced by one or two sources of information and propaganda (the problem of *independence*). They need to consult voters with questions that everyone can answer in commensurate terms (the problem of *measure*).

In discussions of quantitative methods, the problem of measure usually takes center stage. This paper will focus on inclusion, individuation, and independence. In recent efforts to explore culture quantitatively, these three issues loom large. Part of what makes quantitative approaches attractive is the possibility of including a range of objects that is beyond the reach of traditional methods (a study conducted with Google Ngram, for instance, is informed by millions or books). This means that problems of inclusion, so far left to the whim of tradition or circumstance, now should be settled in theoretically motivated ways. Individuation also raises problems that traditional methods could ignore: when studying the evolution of languages, alphabets, or art forms, we need to start with clear criteria specifying under what conditions an item will count as one item. These criteria will of necessity include a modicum of arbitrariness, which I will argue is not as worrying as it might seem. The problem of *data independence* (known in the cultural sciences as Galton's problem) is more serious, and often underestimated.

In the new quantitative approaches to culture, issues are still being debated that were settled in other disciplines long ago (their solution black-boxed in software or taken for granted in textbooks). In studying them, I hope to show that the heart of quantitative methods is not in their measurement tools but in their way of choosing and grouping observations. The kind of quantitative research that I am interested in, like democratic voting, uses numbers and measurements but is not about numbers or measurements: it is about trying to give a voice to enough relevant facts.

Evelyn Fox KELLER
Massachusetts Institute of Technology

Assessing Risk in the Absence of Quantifiability

Low-probability and high impact events pose an especially difficult challenge for conventionally accepted methods of rational risk analysis. The challenge arises from the difficulty (often impossibility) of computing either the magnitude of such events, the probability of their occurrence, or their cost. Yet policy depends crucially on assessing the risk (and cost) of such events. The question I wish to pose is: how we can proceed in the absence of such quantitative assessments?

Isabella SARTO-JACKSON
KLI Institute

Transgressing the Borders of Quantification by Data Visualization

"Biology has long been the stepchild of the natural sciences. Compared with mathematical proofs, physical formulae, and the molecules of chemistry, biology, like life itself, has often seemed unquantifiable, unpredictable, and messy. Yet, scientists have striven gallantly to pin biology down through the application of the other natural sciences. Ever since Gregor MENDEL … biology has slowly transformed itself into a 'hard' science."

— Hunter (2010)

Physics is usually conceived as the key example of a science that deals with universal, invariable, and thus "hard" facts. It is therefore not surprising that biologists make much effort to incorporate mathematics and computation to convert biology into a more quantifiable science. Such endeavors (wrongly) accept a clear separation between qualitative and quantitative research and disregard their multi-faceted interdependence. In my manuscript I will give an example from optical physics (microscopy), but the reasoning goes for many digital imaging methods including techniques in astronomy, medical imaging, or motion tracking.

Microscopy has historically been a qualitative technique, but with the advent of digital microscopy, progress in camera technology, and refined labeling and imaging methods, biological specimens can be measured and characterized by a vast variety of parameters. Yet, observation processes in physical measurements are far from perfect due to photonic noise, optical distortions, or fuzziness deriving from the instruments, the samples investigated, or the underlying procedure. To achieve an ideal representation of the original object, mathematic algorithms are applied on the recorded signals resulting in deconvolved data. The algorithm-based processes of deconvolution require an assumption of a perfect optical path through the instrument that is convolved with a theoretical distortion resulting in the original, undistorted object. It is, however, impossible to calculate the correct theoretical distortion in practice and, usually experimental estimations based on some qualitative, prior “knowledge” about the original object are exploited. This example illustrates the entanglement between qualitative and quantitative approaches that make a Q/Q devide in many ways futile and counterproductive, and supports the claim by BRYMAN (1984) that the Q/Q distinction is a convention rather than an epistemological separation.

Moving from microscopy to visualization techniques in general, it is truly remarkable how rapidly data visualization techniques have gained importance in biology over the last decades. This can be ascribed to a peculiarity of human cognition: Visual representations (photographs, graphs, diagrams, etc.) allow inherent information to be processed *immediately* across many levels of abstraction, because the visual system integrates sensory cues into a perceptory whole. In contrast, numerals, or equations, (and also narratives) are *serial* representations in which the sequence of the characters determines the meaning of the representations (PERINI 2005) requiring slower, serial processing. To support this line of arguments, I will show that images are salient and can dominate numerical descriptions when presented simultaneously.

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Jorge WAGENSBERG LUBINSKI
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Is Quality Quantitatively Measurable?

In many aspects of culture the question arises of how to measure the importance, relevance, or merit of a piece of knowledge, be it a work of art, a scientific theory, an essay, or just a cultural show. The term *Best Seller* was coined using a unique parameter: the number of consumers. Many publishers base their work almost exclusively on the number of readers, almost all museums display the total number of visitors as the unique indication of the quality of their offer, etc. However, we can always try to improve our approach. Scientific journals, for example, define further parameters as the number of quotations of the published papers. The issue has its philosophical depth. The question is: is quality quantitatively measurable? The discussion will be based on thirty years of direct experience in the domain of the modern scientific museums.