

Problems and prospects of measurement in the study of culture

John W. Mohr · Amin Ghaziani

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Abstract What is the role of measurement in the sociology of culture and how can we sort out the complexities that distinguish qualitative from quantitative approaches to this domain? In this article, we compare the issues and concerns of contemporary scholars who work on matters of culture with the writings of a group of scholars who had prepared papers for a special symposium on scientific measurement held at the annual meeting of the American Association for the Advancement of Science (AAAS) back in 1956. We focus on three issues—the recurring need to reinvent measurement (as illustrated by the career of the psychologist S.S. Stevens), the linkage between qualitative and quantitative methods of analysis (as articulated in the writings of the sociologist Paul Lazarsfeld), and the assertion (by philosophers Ernst Cassirer and Peter Caws) that theorizing necessarily precedes measuring. We review a number of important advances in the way that measurement is theorized and implemented in the sociology of culture and we also point to a number of enduring dilemmas and conundrums that continue to occupy researchers in the field today.

Keywords Culture · Measurement · Qualitative and quantitative methods · Philosophy of science · S.S. Stevens · Paul Lazarsfeld · Ernst Cassirer

The idea that ‘social science must develop measurements’ has sometimes led to misunderstandings. Some optimists want to start measuring social phenomena immediately with all the precision of the most advanced sectors of physical science; some pessimists deny that man and his works can ever be measured at all, and recommend an entirely intuitive approach to the understanding of society.

J. W. Mohr (✉)

Department of Sociology, University of California, Santa Barbara, CA 93106-9430, USA
e-mail: mohr@soc.ucsb.edu

A. Ghaziani

Department of Sociology, University of British Columbia, Vancouver, Canada
e-mail: amin.ghaziani@ubc.ca

The false assumption underlying both positions is that science can be carried on only with one particular kind of device—the quantitative scale with equal intervals and a zero point—and that aside from this device there is nothing but a chaos of guesswork and intuition.

Lazarsfeld and Barton 1951, p. 155

Meeting about measurement (New York, 1956)

In the broad and boisterous world of modern science, the prosaic issue of measurement is rarely the main topic of conversation. But sometimes it is.¹ It was, in any case, one of the main topics at the annual meeting of the American Association for the Advancement of Science (AAAS) in December of 1956. In that year C. W. Churchman, Berkeley philosopher and systems theorist, organized a symposium of five panels “to examine and discuss important fundamental problems of measurement that have arisen in the disciplines of science” (Churchman and Ratoosh 1959, p. v). Churchman’s goal was to use the forum as an opportunity “to present contrasts in approaches to the problems of measurement. For this reason, the participants were chosen from different disciplines: e.g., physics, psychology, economics, accounting, philosophy. Furthermore, the participants were chosen because it was known that they had different viewpoints on the meaning and significance of measurement” (Churchman and Ratoosh 1959, p. v).² It seems quite likely that Churchman’s symposium was generated in response to the “Arden House Statement,” the 1951 call by senior AAAS leadership for a pivot in the organization’s focus, a shift to encourage more “meetings at which one branch of science is interpreted to the other branches of science, meetings at which are stressed the interrelations between the branches of science, meetings which cultivate borderline fields, and meetings at which the unifying theme would be central problems whose treatment requires the attack of several disciplines” (AAAS 2014a).³

¹ Another interesting case of measurement coming to the fore as a central focus of scientific interest occurred during the rise of the field of nanotechnology (see Harthorn and Mohr 2012).

² Churchman enlisted five panel leaders: Paul Lazarsfeld (measurement in the social sciences), Donald Davidson (measurement in the value sciences), Henry Margenau (measurement in the physical sciences), Philburn Ratoosh (formal aspects of measurement), and Sebastian B. Littauer (general aspects of measurement) who in turn recruited an impressive list of authors.

³ The Arden House Statement marked a major organizational shift for the AAAS. After the war, American scientific fields had begun to grow and professionalize at a rapid pace. “As more and more disciplinary societies began to go their own way and stopped meeting with AAAS it became apparent that, in the words of 1944–1945 president Anton J. Carlson, AAAS needed to ‘find another function or die’... Under the leadership of Warren Weaver, a mathematician and Rockefeller Foundation Executive ... the AAAS executive committee scheduled a special conference in September 1951 at Columbia University’s Arden House” (AAAS 2014b). In the summary statement, the AAAS leaders wrote, “in view of the present size and complexity of science, in view of the seriousness and importance of the relation of science to society, and in view of the unique inclusiveness of the AAAS, it seems clear that this organization should devote less of its energies to the more detailed and more isolated technical aspects of science, and devote more of its energies to broad problems that involve the whole of science, the relations of science to government, and indeed the relations of science to our society as a whole ... to try to “put science back together” (AAAS 2014a).

Whatever the specific reason, in the mid-1950s the topic of measurement was a central scientific problem. It merited the attention of a five panel symposium at the annual meetings, with a hope and an expectation that there would be utility in thinking about the topic in a coordinated fashion across disciplinary boundaries. Judging by the published versions of these conference papers (collected in Churchman and Ratoosh 1959), it seems clear that measurement *was* indeed an important matter of focus across the scientific disciplines in these years—but in very different ways. Authors included in the volume hold down contradictory sides of different philosophical debates (e.g., some promoted operational while others advocated constitutive theories of meaning) and many of the papers speak to wholly unrelated matters of concern. Even in the section devoted to what might seem to be the simple task of defining measurement as a concept, Churchman and Ratoosh admit that “the contrasts are quite noticeable. ‘Measurement’ clearly means a number of things.” The editors freely acknowledge the somewhat chaotic character of their collection and they make no excuse for it. It is, they write, “a book of contrasts (which) does not constitute a series of connected discourses on measurement” (Churchman and Ratoosh 1959, p. v). Instead it is a collection of papers that captures the unfinished state of the debate (or, series of debates) about scientific measurement at mid-century—a work in progress, not of consensus.

On the other hand, it is, as Churchman and Ratoosh also remind us, “as good a picture as possible of what the workers in the foundations of measurement are concerned about in the middle of the 20th century” (Churchman and Ratoosh 1959, p. v). Maybe so and, in any case, it is without doubt a fascinating time capsule that captures the intellectual concerns and styles of engagement of a range of scientific fields, all wound round the core theme of how to measure things.⁴ The easiest differences to observe across the chapters are those that divide scientific fields by level or type of scientific development. For example, the problem of measurement in physics appears as more of a technical matter, falling into the purview of specialists (in the physics of measurement and in the philosophy of measurement) which was not true in the social sciences where measurement appears to be much closer to the surface of active research advances across the field. Or, as the editors describe it, “Perhaps the most obvious aspect of the contrast is that the physicists are concerned with problems of measurement arising from accepted models, whereas the behavioral scientists are concerned with problems of measurement arising from proposed empirical studies” (Churchman and Ratoosh 1959, p. vi).

Meeting about measurement (Vancouver, 2012)

The articles collected for this special issue on “Measuring Culture” were also presented at a conference focused on issues of measurement, in this case, a symposium focused

⁴ This is not the first time that that collection of essays has been scrutinized by sociologists. Aaron Cicourel built much of the argument for his masterly essay on “Measurement and mathematics” (1964) on a close reading and critique of the Churchman and Ratoosh volume. Reading them again today, Cicourel’s admonishments about the dangers of an overly facile mixing of measurement practices across the social and natural sciences continue to be just as applicable and worthy of heeding in 2014 as they were when he wrote them a half century ago.

on the measuring of culture, organized by Amin Ghaziani and John Mohr and held at the University of British Columbia in fall 2012. The impetus for our gathering was our shared sense that although social scientific studies of “culture” have made great strides on conceptual clarification of the concept (its many meanings), we still have made but little progress on the problem of how to measure culture (the ways of operationalizing it, determining appropriate indicators for it, breaking it down into observable analytic units, and thus studying it) (Ghaziani 2009).⁵ How can we represent the social world and provide some observational grounding to it? How can we coordinate our theoretical concepts — something like “culture,” for instance, with our observational procedures for studying it? And how can we diversify our measurement outcomes to include theoretical, perceptual, numerical, statistical, and qualitative representations, all of which can advance our knowledge about the state of an empirical object?

Like Churchman, our aim was to bring together participants who “had different viewpoints on the meaning and significance of measurement” (1959, p. v). Unlike Churchman, we were focused on a specific methodological issue that is central to scholarly research in the social and human sciences—the so-called split between qualitative and quantitative methodologies.

To tackle the qualitative and quantitative divide head-on, we chose to draw from just one intellectual sub-community. Thus unlike the AAAS symposium, the disciplinary focus of our events was narrow. We included only sociologists among our panelists and, even within that set, we limited the conference to scholars who worked in the sub-field of the sociology of culture. This was strategic. Our thinking was that if we could contain the conversation and keep it within the bounds of a smaller group then perhaps the commonalities that tie us together as members of a sub-community—shared training, common discourse, overlapping scholarly literatures, and similar objects of investigation—would help us achieve a more productive dialogue across this methodological divide, open up a useful conversation about what the two styles of cultural sociology had to say to one another, and lead us to new insights and possibly some new programs for measuring culture.

An important advantage of working in the sub-field of cultural sociology is that the objects that come under investigation in this field inherently force us to confront the way in which more humanistic and interpretive sensibilities are essential to the doing of sociological work.⁶ Consider, for example, the types of research topics that are addressed by the papers collected here—talk of death (McDonnell), being in love

⁵ A second gathering (with the same participants) took place at the University of California, Santa Barbara in early 2014. Other principal participants in these conferences (not included as authors in the current issue) are Jennifer Lena (of Columbia University) and Neil Gross (at the University of British Columbia), both of whom provided papers, commentary, and leadership during the conferences themselves. Other colleagues (Jon Cruz, Simonetta Falasca-Zamponi, Craig Rawlings, and Sarah Thébaud—from the Sociology department at the University of California, Santa Barbara) offered valuable suggestions and critical commentary on the papers. Financial support for this project was provided by a grant from the American Sociological Association Fund for the Advancement of the Discipline and by the generous financial and institutional contributions of the University of British Columbia and the University of California, Santa Barbara.

⁶ The field of the sociology of culture is a logical choice for other reasons. It is an area that has grown rapidly over the last several decades. Established in 1987, the culture section is now the largest section of the American Sociological Association (and has been so for the last 5 years). Moreover the use of cultural theories and methodologies has taken root in a variety of other sub-areas of sociology—the study of formal organizations, social movements, and social networks, for example. Of course, the emergence of the culture section is itself more of an expression of the rise of a modern culturally grounded social science that has much deeper and wider roots, a topic that we cannot pursue here (see Friedland and Mohr 2004).

(Friedland et al.), being moral (Vaisey and Miles), being gay in a post-gay world (Ghaziani), recognizing a beautiful woman (Mears), getting a joke (Tavory), foretelling the future (Mische) or appreciating a new artistic genre (Lizardo). In all of these cases, the meaningfulness of what is being investigated, and thus the salience of a more qualitative and interpretative approach would be hard to deny. This is one reason why the sociology of culture is, in this sense, the point of the spear, the place where meaning-centric approaches (in the more traditional, humanistic and interpretive sense of that term) mix with more formalized (measurement based) styles of looking and knowing and understanding cultural forms.⁷

We focused on measurement because it draws attention to fundamental issues that lie at the interface of scientific and humanistic styles of knowing. With that as our orientation and our motivation, we began by asking our authors to think about how their own research in the sociology of culture engages with (or challenges) the scientific practices of measurement. We asked: What are the ways that we learn from culture and what, if any, is the role of measurement in that process? How *do* you measure culture? How might you *like* to measure culture (even if you do not do so now)? And finally, how *should* we measure culture? What are some best practices for measurement and what are some of the limits to measuring culture? The articles published here are the result of what has so far been a nearly 3-year continuing conversation about these questions. In a sense, we might say with Churchman and Ratoosh that these essays represent “as good a picture as possible” of where we are today in the field of the sociology of culture, at least with respect to matters of measurement. But, like the earlier edited volume on measurement, what we have to present here today is also more a work in progress than a work of consensus. But that might not be a bad thing. As Churchman and Ratoosh optimistically declare in describing their edited volume, “differences in opinion about what measurement means do not necessarily constitute confusion on the part of science. What something really means is the result of many different reflective viewpoints, as Hegel pointed out” (1959, pp. v-vi). This is an interesting point and it leads us to reflect on a few of the other ways in which the lessons from the 1956 conference are relevant to where we stand today. In this spirit, we briefly describe three topics that were part of the discussion of scientific measurement in 1956 that strike us as being still relevant: 1. the institutionalization and de-institutionalization of measurement practices, 2. the duality of quality and quantity and, 3. the significance of theory for the practice of measurement.

Reinvent measurement

It seems almost the unavoidable fate of the scientific approach to the world that each new and fruitful concept of measurement, which it gains and establishes, should be transformed at once into a thing-concept. Ever does it believe that the truth and the meaning of the physical concepts of magnitude are assured only when it permits certain absolute realities to correspond to them. Each creative

⁷ In this sense there is a strong parallel to the role that the Digital Humanities program has played in traditional fields of humanistic scholarship, see, for example, Liu (2013) and Moretti (2013).

epoch of physics discovers and formulates new characteristic measures for the totality of being and natural process, but each stands in danger of taking these preliminary and relative measures, these temporarily ultimate intellectual instruments of measurement, as definitive expressions of the ontologically real.

Cassirer 1923, p. 358

Sensation, it seems, must be cut into pieces that can be added and subtracted, like beans, or combined like weights in a scale pan.

Stevens 1975, p. 45

The Harvard psychologist S. S. Stevens contributed one of the more interesting chapters to Churchman and Ratoosh's volume. Most social scientists will recognize Stevens for his contribution to measurement theory; it was Stevens (1946) who gave us the (now) doxic taxonomy of measuring scales—the nominal, ordinal, ratio, and interval levels of measurement. This was a paradigm changing theorization of scientific measurement and yet during much of his career, Stevens's ideas about measurement were greeted with skepticism, resistance, and even, in one instance, official condemnation, for being beyond the bounds of responsible science. In his chapter, Stevens (1959) recounts how his methodological innovation emerged in the context of his fight against “the classical view of measurement . . . a view that dominated the scene until the 1930's” (Stevens 1959, p. 21). His narrative about the character of this conflict helps us to appreciate more Cassirer's observation (above) about the dangers of methodological ossification and it also helps us to understand better how and why it is that scientists need to regularly *reinvent measurement*.

Stevens was a psychologist, trained in what was then known as psycho-physics. After taking an undergraduate degree at Stanford, he spent 2 years as a graduate student at Harvard during which time he completed his PhD under E.G. Boring in the Department of Philosophy (in 1933). After spending another year at Harvard, “studying physiology under Hallowell Davis and another as a research fellow in physics” (Teghtsoonian 2001, p. 15105) Stevens was hired as an instructor in Harvard's newly founded Department of Psychology and promoted to the rank of full Professor a decade later.⁸ He spent his career working at the interface of biological sensations (e.g., of “objective” audible or visual stimuli) and the human interpretation of those sensations (the “subjective” experience of sounds, music, lights, colors, etc.). Measuring phenomena at this nexus of mind and body was Stevens's specialty and he is credited with the discovery of the “psychophysical power law” (also known as “Stevens' power law”), which specifies the mathematical relationship between the

⁸ Teghtsoonian (2001) traces Stevens's intellectual lineage back to Wilhelm Wundt who founded the first laboratory for psychological research at the University of Leipzig in 1879 and who is generally credited as the founder of the modern field of experimental psychology (Wundt trained E.B. Titchener, who trained Boring, who trained Stevens). Teghtsoonian also suggests that Stevens's 3 years of service as a Mormon missionary in Belgium and France (just a few years after the end of World War I) had a significant impact on his (stubborn) character and subsequent career success.

energy of a physical stimulus and the magnitude of its perceived effect (Teghtsoonian 2001).⁹

Stevens would have been 50 years old when he attended the AAAS conference in 1956. Aside from explaining the logic behind his theory of measurement, Stevens used part of his time to give a personal account of the controversy that arose early in his career over how he (and others in his camp) sought to create new ways of measuring physiological data. The conflict had to do with a debate over what could count as reliable evidence for scientific knowledge. Ultimately, this was a dispute about neo-Kantian theories of numbers and the epistemology of science. But in his essay Stevens summarizes his version of the disagreement in fairly simple terms. He explains the matter this way, “‘classical (measurement) theory’ grew quite naturally out of the evolution of the number system.... The important point for our present concern is the fact that some long-forgotten genius somehow contrived to build a number system—a formal model—to represent what he did with pebbles, fingers, or cattle. With the aid of this model, he measured the numerosity of his possessions” (Stevens 1959, p. 21). And, as Stevens explains, “it is scarcely surprising that the model turned out to be isomorphic with the empirical operations performable with such things as pebbles (from whose Latin name we derive our word *calculus*)” (p. 21).

In a fashion that would seem to fit well with Cassirer’s idea of measurement taking on the status of a thing-concept, as the theory and practice of measurement matured, so too did the current measurement regime begin to create limits on how measuring could happen. As Stevens tells this story, it was Hermann von Helmholtz who, in an effort to build a more solid mathematical foundation for the new science of measurement in the latter decades of the nineteenth century, “turned the matter around, so to speak, and implied that unless concrete operations could be mirrored by the mathematical laws of additivity, the operations did not qualify as measurement. In other words, instead of the mathematical model being made to agree with empirical operations, as had happened when arithmetic was in its infancy, the model has come to dictate what kinds of operations qualify as measurement” (Stevens 1975, pp. 44–45).¹⁰ From this bad beginning, things got worse. “Those who followed Helmholtz managed to tie measurement even more rigidly to the formal rules of addition, so that fundamental measurement was thought to be impossible unless additive operations could be carried out” (Stevens 1975, p.45). Thus, according to the canons of classical measurement theory, “direct or ‘fundamental’ measurement is possible only when the ‘axioms of additivity’ can be shown to be isomorphic with the manipulations we perform upon objects” (Stevens 1959, p.21). But this sets the bar pretty high. As Stevens complains, “only a few properties, such as length, weight, and electric resistance, are measurable in this fundamental way” (1959 p. 21).

⁹ Stevens distinguishes between two classes of sensory information. “The class of prothetic continua includes the ‘quantitative’ aspects of things, whereas the class of metathetic continua includes the ‘qualitative’ and positional aspects, such as pitch, apparent azimuth, and inclination.... On continua such as brightness, loudness, and heaviness, for example, we seem to progress along the continuum by a process of adding excitation to excitation—a prothetic process. The power function seems to be the general law relating subjective and physical magnitudes for Prothetic continua, but not necessarily for metathetic continua. Pitch, for example, is not a power function of frequency” (Stevens 1959, p. 37).

¹⁰ Note that Wundt was a student of Helmholtz.

Disregarding these limitations, Stevens pressed on with inventing new and different ways of measuring the linkage between subjectivity and objectivity in human sensory experience. But, as he recounts, “In those days the widely held view that true measurement exists only when the experimenter is able to perform a physical addition was blocking progress in psychophysics. The requirement of empirical addition seemed to some people to preclude the measurement of subjective brightness or loudness” (Stevens 1975, p. 50). This was not just a problem for psychophysics, “but also in much of psychology and the social sciences, there is a need to scale new kinds of variables and to measure the previously unmeasured. Procedures modeled on the counting and adding of beans do not suffice for the measurement of such concepts as the social status accorded a person by reason of the degree of his education. A man’s social status resides in the views and opinions of other people. If status is conferred by a consensus judgment, it cannot be measured by so-called additive procedures” (Stevens 1975, p. 46).

At some point the matter became quite heated. Around the time that Stevens was completing his dissertation at Harvard, the British counterpart to the AAAS, the British Association for the Advancement of Science (the BAAS), appointed ‘a distinguished committee of physicists and psychologists ... to assess the possibility of “quantitative estimates of sensory events”’ (Stevens 1975, p. 38). The committee deliberated for years, finally publishing its report in 1940. Stevens describes the outcome in these terms, “the committee split wide on the meaning of measurement, and some harsh words were written condemning the alleged measurement of sensation. In the final report, one member said, “I submit that any law purporting to express a quantitative relation between sensation intensity and stimulus intensity is not merely false but is in fact meaningless unless and until a meaning can be given to the concept of addition as applied to sensation”’ (1959, p. 22). “The conservative wing of the committee,” put the matter more bluntly, they “asked: ‘Why do not psychologists accept the natural and obvious conclusion that subjective measurements of loudness in numerical terms (like those of length ...) are mutually inconsistent and cannot be the basis of measurement?’” (1959, p. 23).

In contrast, Stevens wanted to shake up the theory of measurement and he pointed to the shifting models of mathematics itself as an example. Referring to the rise of topology theory and various non-Euclidean geometries, Stevens explains “Mathematics has at last freed itself of the earthy perspective under which it formerly sought to justify its laws by manipulations performed upon solid objects, and has taken off into the blue of pure abstraction—where it properly belongs” (p. 19). Stevens thinks measurement theory should follow suit. “If we can generalize our conception of measurement, perhaps it will gain power, just as mathematics has extended its reach and vitality by ever wider generalizations” (Stevens 1975, p. 46). In truth, the same sort of purely abstract freedom will not ever be possible for measurement theory, which “must remain anchored here below, for it deals with empirical matters” (1959, p. 19). But that does not mean that measurements must be tied down to old-style assumptions of physical embodiment and there is certainly “no requirement that measurement remain confined to the simpler problems of counting which first gave rise to it” (1959, p. 19). “How widely can the concept of measurement be generalized? My own efforts to push the outward bounds defining measurement have brought me through successive stages to the view that measurement occurs whenever an element from one domain is matched,

equated, or conjoined to an element of another domain. The matching operation becomes the key. At the heart of measurement is the same kind of operation we encountered when early man created numerals by matching number of strokes to number of camels or other objects. It is the same kind of operation that is involved when a subject is asked to signify by the force of his handgrip the apparent intensity of a light shining in his eyes” (1975, p. 46).

Thus, for Stevens, the scientific theory of measurement should be “enlarged to include as measurement the assignment of numerals to objects or events according to rule—any rule. Of course, the fact that numerals can be assigned under different rules leads to different kinds of scales and different kinds of measurement, not all of equal power and helpfulness. Nevertheless, provided a consistent rule is followed, some form of measurement is achieved” (1959, p. 19).¹¹ Working from these expectations of measurement, Stevens was able to forge ahead empirically to devise a measure of loudness as well as a unit for the loudness scale. “The remaining task was to formulate the theory of measurement capable of accommodating the measurements that scientists were actually producing” (Stevens 1975, p. 50). This was the situation that laid the foundation for the new theory of measurement scales for which Stevens is justly famous today.¹² As he explains his breakthrough, “The best way out seemed to be to approach the problem from another point of view, namely, that of invariance, and to classify scales of measurement in terms of the group of transformations that leave the scale form invariant.... A fourfold classification of scales based on this notion was worked out sometime around 1939 and was presented at the International Conference for the Unity of Science in 1941” (1959, p. 23).¹³

To summarize, Stevens’s reflections on the mismatch between the measurement needs of the humanistic sciences and the ossified assumptions of prior scientific traditions suggests one important lesson from the 1956 AAAS conference for contemporary debates about measuring culture. Existing assumptions about the practice of measurement may be hard to change because they appear as “ontologically real.” But that is no reason for complacency, as the example of S.S. Stevens illustrates. For science to advance there must be an ongoing and regular confrontation with the methodological assumptions of the field as well as a need to continually reinvent measurement.

¹¹ Stevens elaborates, “Under the modern view, the process of measurement is the process of mapping empirical properties or relations into a formal model. Measurement is possible only because there is a kind of isomorphism between (1) the empirical relations among properties of objects and events and (2) the properties of the formal game in which numerals are the pawns and operators the moves. As Russell put it: ‘Measurement demands some one-one relation between the numbers and magnitudes in question—a relation which may be direct or indirect, important or trivial, according to circumstances [Russell 1937, p.176]’” (Stevens 1959, p. 21).

¹² Stevens also credits his intellectual milieu. “In the meantime, unaware that the British committee was trying to settle the issue, some of us at Harvard were wrestling with similar problems. I remember especially some lively discussions with G.D. Birkhoff, R. Carnap, H. Feigl, C.G. Hempel and G. Bergmann. What I gained from these discussions was a conviction that a more general theory of measurement was needed, and that the definition of measurement should not be limited to one restricted class of empirical operations” (Stevens 1959, p. 23).

¹³ Stevens points out that when “Viewed in that perspective, we see that so-called fundamental measurement, the type that was modeled on the rules of additivity, amounts only to an important special case. It specifies particular rules in the form of operations that lead to the assignment of numbers to special magnitudes—numerosity length, weight, and a few others” (Stevens 1975, p. 47).

The continuum of quality and quantity

Looking at the material before us in all its richness of sense-data, we must decide what attributes of the concrete items we wish to observe and measure: do we want to study ‘this-ness’ or ‘that-ness’ or some other ‘-ness’?

Lazarsfeld and Barton 1951, p. 155

Churchman chose Paul Lazarsfeld to assemble the panel on measurement in the social sciences for the AAAS symposium. Lazarsfeld was a logical choice. As founding director of Columbia University’s Bureau of Applied Social Research, he was a key figure in promoting the development of a modern, quantitative style of social science in the post-war US. Lazarsfeld had earned his doctorate in Applied Mathematics from the University of Vienna in the mid-1920s where he had also (in 1927) set up a small research center in the department of psychology. Studies that he conducted through that center caught the attention of the Paris representative of the Rockefeller Foundation who facilitated Lazarsfeld’s visit to the United States on a fellowship in 1933–1934. During that time “the conservative party in Austria overthrew the constitution, outlawed the Socialist Party, and established an Italian-type fascism” (Lazarsfeld 1982, p. 16). Lazarsfeld, who was Jewish, was dismissed from his academic appointment and “most members of my family in Vienna were imprisoned” (p. 16). Rather than return to Austria, he cobbled together a series of situations that enabled him to stay on in the States. As Lazarsfeld recounts in his memoir, “With the help of Robert Lynd, Professor of Sociology at Columbia, I obtained a position in New Jersey as a supervisor of student relief work for the National Youth Administration, whose headquarters were at the University of Newark. In the fall of 1936, a Research Center was established at the university, with me as director. This center was patterned along the lines of its Viennese predecessor ...” (Lazarsfeld 1982, p. 16). From there he moved to Princeton (in 1937) to run what became the “Office of Radio Research” which moved to Columbia (in 1939) where it was transformed (in 1944) into the Bureau of Applied Social Research and where (in 1941) Lazarsfeld was offered a tenured faculty position in sociology.

Lazarsfeld’s influence on American sociology over the subsequent decade was substantial. Converse says of the Bureau of Applied Social Research, It “was not the first academic research center in sociology in an American university, but it was the most significant and influential for the growth of quantitative research in that discipline” (1987, p. 267). Lazarsfeld kept up a frenetic pace. Under his leadership the Bureau combined a wide variety of contracts with private clients (studies of marketing and commissions for private surveys) with government projects (especially during the war years) and large-scale academic studies such as the survey (in 1955) of over 2,400 social science faculty concerning their experiences of political intimidation during the McCarthy years (published as *The Academic Mind*). He collaborated with a broad range of social scientists, including his colleagues at Columbia (especially Robert Merton who was involved in managing the work of the Bureau) but also a number of other scholars such as Theodor Adorno, whom Lazarsfeld had brought to Princeton (in 1938) to work as the “part-time director of the music division” of the Office of

Radio Research (Lazarsfeld 1982, p. 58). He also published a prodigious amount of scholarly material, with a primary focus on the theory and practice of survey research, an area in which he authored a number of classic papers that were instrumental in helping to develop models for the formal analysis of survey data. He worked tirelessly to promote greater development and training in quantitative methods, receiving funding during the 1950s from both the Rockefeller and Ford foundations “to provide materials for the advanced training in social science research,” which led to the publication of a series of classic texts on sociological methodology (Lazarsfeld 1957b, p. xii).

In short, no one in the field of American sociology during this period was a more significant and forceful proponent of quantification than Lazarsfeld. And yet, paradoxically, one of the most striking features of his work was a deep commitment to qualitative research and his sustained refusal to disconnect the two methodological styles from one another. Note that Lazarsfeld was a genuinely liminal figure in this regard. His own research experiences had conditioned him to refuse this split. From his earliest professional training (under the direction of Professors Karl and Charlotte Bühler at the University of Vienna), Lazarsfeld grappled with the problem of moving between quality and quantity. Under the Böhlers’ leadership, the Vienna psychology department had “became a center for the application of psychology to social problems. We were continuously confronted with topics like these: how do young people acquire ‘work consciousness’ and finally vocational maturity? How does the behavior of parents affect the relationship among siblings? By what criteria do old people, looking back over their life, decide whether it has been meaningful? ... The answers were sought through concrete material: a large collection of diaries kept by young people, carefully recorded observations of family situations, detailed interviews with residents of old age homes, surveys in unemployed communities, and so on” (Lazarsfeld 1957a, p. xv).

Lazarsfeld recounts how, in his role as an assistant to the Böhlers, “I was in charge of training students to handle such material. Little precedent for this task could be found in the tradition of the social sciences. The categories were more complex than those usually treated by quantitative methods; because they were what is called today ‘qualitative attributes,’ no standard correlation techniques could be used. Furthermore, the goal was not to find isolated relationships. The results had to hang together, each as part of a consistent whole. This situation led, not to the development of new formulae, but to a kind of empirical work in which qualitative analysis is guided by conceptual schemes and in which each empirical procedure is scrutinized as to its logical implications” (Lazarsfeld 1957a, p. xvi). Lazarsfeld maintained this sensibility even after he moved to the United States and became more actively involved with conducting and theorizing survey research. He never lost sight of the ways in which these formalized measures represented artificial assemblages. “Before we can investigate the presence or absence of some attribute in a person or a social situation, or before we can rank objects or measure them in terms of some variable, we must form the concept of that variable. Looking at the material before us in all its richness of sense-data, we must decide what attributes of the concrete items we wish to observe and measure: do we want to study ‘this-ness’ or ‘that-ness’ or some other ‘-ness’?” (Lazarsfeld and Barton 1951, p. 155). How to decide indeed? It is ultimately a highly qualitative matter, “The precise origin of our notion of this-ness or that-ness may be extremely varied, but it usually seems to

involve combining many particular experiences into the category which promises greater understanding and control of events” (Lazarsfeld and Barton 1951, p. 155–156).

Lazarsfeld assumed that there was no hard and fast distinction between humanistic and scientific styles of knowledge but, instead, a continuum of methods, “a direct line of logical continuity from qualitative classification to the most rigorous forms of measurement, by way of intermediate devices of systematic ratings, ranking scales, multidimensional classifications, typologies, and simple quantitative indices” (Lazarsfeld and Barton 1951, p. 155). According to Lazarsfeld it is the proper task of the social scientist to systematically build up a better and more effective collection of measurement tools to get more precise ways of measuring and, thus, of formally analyzing social life and social experience. The goal was to move from the more qualitative towards the more quantitative, but only when conditions allow for that progression to occur. Frequently, however, they do not. As Lazarsfeld notes, “At the present stage of the social sciences a great deal of research must be of an exploratory nature, aiming at qualitative answers to such questions as the following: what goes on in a certain situation? ... Where research contains exploratory elements, the researcher will be faced by an array of raw data for which ready-made theoretical categories will not exist. He must formulate categories before he can do anything else” (Lazarsfeld and Barton 1951, p. 156).

This explains why Lazarsfeld was so keenly interested in the ways that qualitative researchers approached their data. It is because, “in this wider sense of ‘measurement,’ social phenomena are being measured every day by both theoretically oriented and applied social researchers.” This then presents itself as a strategy, “One way to develop social science measurement is to systematize these commonly performed research procedures, by codifying exactly what successful researchers do in carrying out the simpler forms of measurement and exploring their logical implications” (Lazarsfeld and Barton 1951, p. 155).

As the example of Paul Lazarsfeld demonstrates, a second lesson for us to take away from the 1956 measurement symposium—a lesson that seems just as relevant today as it did a half century ago—is that the divide between qualitative and quantitative measures should not be understood as a hard and fast analytic difference, but viewed instead as a methodological continuum that moves slowly up the measurement ladder as we devise more and better forms of sociological understanding. Thus, qualitative and quantitative scholars have much to share and to learn from one another.

Measures must be “conceived and sought”

Measurement presupposes something to be measured, and, unless we know what that something is, no measurement can have any significance.

Caws 1959, p. 3

The first chapter in Churchman and Ratoosh’s collection is an essay by the philosopher Peter Caws entitled “Definition and measurement in physics” (1959). Even

though the subject matter might seem somewhat far afield, in point of fact this essay also holds out another useful lesson for contemporary discussions of measuring culture.

Like Stevens, Caws wants nothing to do with the classical measurement model and he is eager to move toward more modern (generalized) styles of measurement, but his critique comes in from the opposite direction. For Stevens the classical model puts too much constraint on his ability to devise measurement operations that yield interpretable results. For Caws, all measurement is necessarily an abstraction and Stevens' attempt to ground his model in a set of measurement operations cannot escape this basic ground truth. Caws complains, "such formulations have led to the belief that measurement really has one foot in the outside world, and that it can, therefore, give us information about the link between the external and the theoretical" (Caws 1959, p. 4). For Caws, this is an unacceptable philosophical position. "Operationism adopted a fallacy that was almost a reversion to the idea of real definition—the principle that the physical entity in fact is the set of operations by which it is measured. Logical empiricism, on every hand, has tended toward a different but equally unfortunate extreme, which in its most exaggerated form consists in reducing the physical entity to a name or linguistic symbol" (Caws 1959, p. 5).

Caws's definition of measurement is founded on the premise that there is no outside to the theoretical assumptions that we necessarily bring to the task of measurement—there is no such thing as an innocent measurement. But, at the same time, there is an important distinction to make between the concept of measurement and the concept of definition since many of the confusions that bedevil measurement theory come from confusions over these two ideas. Caws explains, "They both have the character of leading to relations which set the entities of science in order with respect to one another. ... Definition, in general, is concerned with the systematic order of the conceptual schemes of science, and with the nature of the relations between different entities. Measurement has a more limited function, that of establishing metrical order among different manifestations of particular properties, and of making scientific events amenable to mathematical description" (Caws 1959, p. 3).¹⁴ And thus, for Caws the "essential function" of measurement "is the setting in order of a class of events with respect to its exhibition of a particular property, and this entails the discovery of an ordered class the elements of which can be put in a one-to-one correlation with events in question. The most obvious available ordered class is that of finite real numbers" (Caws 1959, p.5).¹⁵ Thus, a formal definition of measurement "is the assignment of particular mathematical characteristics to conceptual entities in such a way as to permit (1) an unambiguous mathematical description of every situation involving the entity and (2) the arrangement of all occurrences of it in a quasi-serial order" (Caws 1959, pp. 5–6).

Notice that this is a very general definition that "makes no reference to the means whereby the mathematical characteristic is arrived at—it simply says what

¹⁴ "... one of the most striking similarities between definition and measurement. Definition requires the replacement of one symbol in an expression by another symbol or symbols; measurement requires the replacement of a symbol by a number, itself also a symbol. It is not far from this point to an identification of the two processes" (Caws 1959, p. 6).

¹⁵ Caws writes, "I'm not sure we always ought to demand a number; certainly, as I hope to show later, we are not bound to use one" (1959, p. 6).

measurement is, objectively” (p. 6).¹⁶ This is an important feature of Caws’s definition. This maneuver enables him to escape the epistemological conundrums of the operationists and the logical empiricists. Caws explains the difference: “The true function of measurement is to link mathematics and physics. Observe that it is not a question of establishing a connection between the empirical and the theoretical, which is what operationism claimed to do.... What measurement does is to connect two parts of theoretical knowledge, the mathematical and the conceptual, imparting relevance to one and precision to the other” (Caws 1959, p. 13).¹⁷

To understand this argument it is helpful to go back to the work of Ernst Cassirer who Caws relies on in making his own case. Cassirer published his classic work *Substance and Function* (in German) in 1910. That book examined the shifting philosophical assumptions of scientific thought, tracing over the course of many centuries, the philosophical move away from an orientation (that Cassirer identifies as originating with Aristotle) to think of the world in terms of substances towards the emergence of a modern scientific approach which sees the physical world in terms of relational systems. Cassirer shows how this begins during the renaissance, and comes to full expression with the rise of modern Einsteinian physics. A decade after the publication of *Substance and Function*, Cassirer added a long supplement to the text (more than a hundred pages), entitled “Einstein’s theory of relativity considered from the epistemological standpoint.” This is the text that Caws mainly cites in his essay.

Cassirer, who is often described as last in the line of the great neo-Kantian philosophers, develops an extraordinarily sophisticated set of arguments about the ways in which relational thinking changed the history of science. But one of Cassirer’s main points is to insist upon the power of deep (neo-Kantian) ordering principles that structure our ability to experience reality and to generate a framework for understanding.¹⁸ In the history of science, it is the building up of theoretical constructs that allow us to perceive information in a fashion that is not so much channeled through our senses, as it is channeled through our conceptual sense-making systems. Cassirer writes,

That concepts, such as those of mass and force, the atom or the ether, the magnetic or electrical potential, even concepts, like those of pressure or of temperature, are no simple thing–concepts, no copies of particular contents given in perception: this scarcely needs any further explanation. ... For any, even the simplest, measurement must rest on certain theoretical presuppositions of certain “principles,” “hypotheses,” or “axioms,” which it does not take from the world of sense, but which it brings to this world as postulates of thought. In this sense, the reality of the physicist stands over against the reality of immediate perception as something through and through mediated; as a system, not of existing things or

¹⁶ “To it, as objective, must be added what Cassirer calls a ‘subjective index,’ stating the conditions under which it has validity” (Caws 1959, p. 6).

¹⁷ Caws writes, “Measurement thus assigns, in a stipulative fashion, mathematical characteristics to conceptual entities, and enables us to apply what we learn about the organization of the world in one realm to the clarification of the other” (1959, pp. 13–14).

¹⁸ One of the most distinctive characteristics of Cassirer’s work is that he managed to push the Kantian problem of cognitive structure out of the head and into the culture, especially in his work on the philosophy of symbolic forms (Mohr 2010).

properties, but of abstract intellectual symbols, which serve to express certain relations of magnitude and measure, certain functional *coördinations* and dependencies of phenomena (Cassirer 1923, p. 357).

Whenever we set out to measure something we have to already have some set of expectations about what it is, what it means, and what it means to “measure it.”

Everywhere physical thought must determine for itself its own standards of measurement before it proceeds to observation. There must be established a certain standpoint for the comparison and correlation of magnitudes; certain constants must be established at least hypothetically and in preliminary fashion before a concrete measurement can take place. In this sense, each measurement contains a purely ideal element; it is not so much with the sensuous instruments of measurement that we measure natural processes as with our own thoughts. The instruments of measurement are, as it were, only the visible embodiments of these thoughts, for each of them involves its own theory and offers correct and useful results only in so far as this theory is assumed to be valid. It is not clocks and physical measuring-rods but principles and postulates that are the real instruments of measurement. For in the multiplicity and mutability of natural phenomena, thought *possesses* a relatively fixed standpoint only by *taking* it (Cassirer 1923, p. 364–365).

And so a final bit of wisdom that comes down to us over this past half century from the AAAS symposium is a reminder of just how important are our theories to our ability to measure. What Peter Caws (and Ernst Cassirer) teach us is that there is no such thing as an innocent measurement and that, ultimately, it is with our minds (and not our instruments) that any form of measurement must begin.¹⁹ As Cassirer says when discussing measurement standards, “...whatever particular properties they may have, no constants are immediately given, but all must be *conceived* and *sought* before they can be found in experience” (Cassirer 1923, p. 359–360).

Some new ways to measure culture

We turn finally to consider the articles that are collected here in this special issue. As a way to organize this discussion we (once again) return to the three topics that we discussed in the case of the Churchman and Ratoosh volume and we ask whether these issues retain their salience, a half century later, as scholars grapple with the question of

¹⁹ One of the ways that we would want to consider interrogating this perspective would be to ask how the rise of practice theory (e.g., Bourdieu 1977, etc.) over the last century will have had an impact on these styles of understanding. This is not the place to take up an extended discussion of this speculation but our quick answer would be to suggest that there is a duality here, in the sense that Cassirer’s philosophical approach still carries water, but that it needs to be complemented by a perspective (such as Bourdieu’s) that emphasizes the ways in which the experience of material embodiment also have an impact on the ways that Kantian categories of understanding come to be constructed. Both are likely to be true.

how to measure culture. What if anything is different now? In what ways has our approach to measurement (and theorizing about measurement) changed between then and now and what (if anything) does this tell us about our current attempts to measure culture?

We start with the first issue, the problem of the ossification of measurement regimes and the periodic need to re-examine, re-confront, and re-invent measurement practices. Thinking about the articles collected here, we are struck at the many different ways in which, in fact, our authors *do* see a need to overthrow existing measurement regimes. Indeed, one of the most exciting qualities of this collection is that all the papers seek to challenge, de-stabilize, or push to reconfigure some existing practice or convention for measuring culture.

Take, as an example, the article by Terence McDonnell. McDonnell was studying AIDS campaigns in Ghana and he set about his task in a conventional manner. He gathered together local residents in small focus groups and showed them images and texts of AIDS campaign billboards and posters and started recording the groups' responses. McDonnell explains that the answers he got back were nearly always just paraphrases of the original texts and thus not an effective way to find out what local Ghanaians understood about AIDS. Thinking on his feet, McDonnell shifted tactics. Instead of having his focus groups respond to prepared images he asked them instead to draw their own AIDS poster and to talk about it as they did. The information was far richer, especially when it came to the task of identifying deeper structures of "automatic cognition." McDonnell explains that the most important factor was being able to identify moments of collective resonance. He writes, "The exercise drawing an AIDS poster proved an effective method for revealing moments of resonance. To capture resonance requires that we observe people using culture in real time, as an unfolding sequence of deliberation, choice and action." Thus, much like S.S. Stevens, McDonnell focused on the research task at hand, and determined to find a way to match what he wanted to know with what he was able to measure—even if that meant changing the methodological conventions for measurement. McDonnell describes his new approach to measuring culture as a "productive methods" style of data generation.

Steve Vaisey and Andrew Miles offer another example of researchers who are actively inventing new measurement methods to pursue the study of culture. In this case the focus is on the analysis of "moral culture," which they differentiate into: (1) moral goods ("bright lights") including the kinds of things that people value and seek to pursue in their pursuit of the good life and, (2) moral prohibitions ("bright lines") that point to the kinds of moral violations and transgressions that people must avoid. This article presents a particularly interesting example because it highlights the embrace by sociologists of culture of innovative new measurement tools that are borrowed from an adjacent discipline and then creatively redeployed. Specifically Vaisey and Miles draw upon the elaborate and detailed research program that was developed by the psychologist Jonathan Haidt and his colleagues over the course of many years to devise measures of moral sentiments. In their article, Vaisey and Miles show how this basic (and transplanted) innovation in measurement practices has a wide variety of applications for sociologists that potentially span across a range of very different types of data sources.

Amin Ghaziani demonstrates a different kind of measurement dilemma in the study of culture. In this article, Ghaziani studies the culture of gay neighborhoods (or

gayborhoods) and he comes to his project with a particular puzzle. Ghaziani describes an emerging “post-gay” trend among some contemporary urban centers in which gays and lesbians are increasingly leaving gayborhoods behind and choosing other urban neighborhoods. The dilemma for Ghaziani comes from not knowing how then to go about qualitatively measuring the cultural vitality of gayborhoods. He draws on interview and public opinion data and shows that a focus on information regarding anchor institutions (e.g., “particular bars, bookstores, and bathhouses, which are the primary engines of community building”) and neighborhood commemorations “such as the tax-funded rainbow color pylons that city officials installed in Chicago’s gayborhood,” provide an effective way to measure the character and vitality of urban gay communities in a historical period characterized by a cultural logic that suppresses sexual differences between gays and straights.

The article by Ann Mische offers still a different type of problem. In this essay, Mische points out that the sociological study of collective memory has been powerfully developed over the last two decades but that the complementary arena of analyzing collective possible futures has been almost entirely neglected. This raises interesting methodological questions such as how do you measure something that has not happened yet? She suggests that to be effective a study will need to focus on “the effects of talk on future projections—not just in retrospective account-making, but in the jostling, jockeying, listening, learning, and arguing talk that unfolds in relational settings and is located in larger contentious fields.” Mische proposes the notion of identifying “sites of hyperprojectivity” (“communicative settings, somewhat removed from the flow of day-to-day activity, in which the explicit purpose of talk is to locate problems, visualize alternative pathways, and consider their consequences and desirability”) as a new approach to measuring culture. In her example, she draws on an online archive of transcripts and published texts from the United Nations Conference on Sustainable Development and a parallel “People’s Summit” meeting (both held in Rio de Janeiro in 2012). Mische combs through these materials looking for new ways to measure meanings within a textual corpus, measures that might facilitate an analysis of different styles of talking about the future. In this article, Mische propose an approach that cross-references grammatical styles with what she calls different “dimensions of projectivity.” In the process, Mische opens the door to a whole new type of cultural analysis as well as a new set of measurement tools for the study of culture.

In his article, Christopher Bail provides another example of a potentially radical reinvention of measurement strategies for the study of culture. Bail’s paper addresses the possibility of using “Big Data” sources as a way to begin to measure culture. As Bail points out, we are at the very early stage of a truly remarkable data revolution in which massive amounts of textual and other forms of social information downloaded from the internet (or a variety of other “Big Data” repositories such as Google Books, LEXIS-NEXIS, or the Vanderbilt Television News Archives) will be accessible (and analyzable) for sociologists who are interested in the study of culture. Bail develops a set of possible theoretical agendas for this type of research and he summarizes the current state of the methodological field. He also provides a brief inventory of Big Data software tools that cultural sociologists have available to them. As with the article by Vaisey and Miles, Bail explains that while the analytic foundations of cultural measurement tools may have been pioneered and developed by other disciplines (e.g., computer scientists and computational linguistics in this case), it falls to sociologists to

re-think and re-invent thoughtfully the actual measurement and data analysis procedures that can be used for measuring cultural forms that are distributed across Big Data information stockpiles.

In each of these articles we find, much as we saw in the case of S.S. Stevens, sociologists who are hard at work, actively questioning and reconfiguring the ossified assumptions of established measurement regimes. Some of those regimes are simply outmoded, others were created by sociologists who were not interested in thinking about culture. Some methodologies were designed by non-sociologists who were pursuing entirely different goals. Indeed, all the articles in the current special issue push back, in some way, against existing measurement conventions for studying culture. But they also do more than this. For example, the question of how to merge or balance qualitative and quantitative approaches to measurement, which we discussed with regards to Paul Lazarsfeld's work, is very much alive and still in question today, but our appreciation of the nuances of the matter are also far more sophisticated now than had been true in Lazarfeld's era.

Consider, in this context, the article by Ashley Mears. Mears studies the fashion industry and the women who work as fashion models. In this paper, she explicitly tries to think about a mix of four different (qualitative and quantitative) methodologies that she has used for studying the industry—interviews, ethnography, observant participation, and network analysis. The fundamental issue that Mears investigates has to do with the analysis of taste—which women are selected for which opportunities according to what subjective vagaries of implicit understanding. Mears find that interviews are a good source for obtaining narrative accounts of what people do and why they do it but she also finds that the information obtained tends to consist of largely post-hoc justifications for actions taken. In contrast, ethnography enables a researcher to “access situated behavior to see how people actually behave” in the context of the interpretive contingency of immediate situations, and thus it is a method that provides a highly important complement to the data collected from interviews. Against both these style of gathering cultural data, Mears also highlights the power of carnal ethnography, a mode of data gathering in which one's own bodily presence and engagement with an immediate situation allows the researcher to understand how meanings and interactions emerge out of the lived material experience of shared corporeality. Finally, Mears considers social network analysis. Once again, gathering and analyzing network data provides a different means for understanding the field of study, in this case a method for seeing how taste patterns are structured across a broad and hierarchically ordered social field. This style of knowledge acquisition is distinct from but also highly complementary to the other ways of knowing that Mears employs. Her article pushes back against Lazarsfeld's presumption that increasing levels of formal measurement is always a good thing. Instead Mears argues that different methods have different advantages and that the astute researcher will understand what forms of knowledge, providing what kinds of insights are needed for what kinds of research questions. Mears advocates mixing and matching different styles of measurement according to the specific goals and needs of the research problem.

But are these different methods on a qualitative-quantitative continuum as Lazarsfeld suggests? The article by Iddo Tavory takes on this question directly. Tavory writes about humor, about what makes something funny. In his paper, he both provides

an elegant theorization of funniness and an application of the model to an empirical case—AIDS humor in Malawi. Tavory identifies four features of humor: 1) Humor is a play on form, a means of creating a “*bi-sociation*” (a linkage) between two worlds of meaning. 2) Humor does not resolve the tension between these multiple meanings, but rather it sustains the tension, “allowing people to stay on both sides of the fence at once.” Tavory writes, “it is ambiguity and polysemy, rather than clarity, that gives humor its edge.” 3) Humor tells us something about social life, and thus, citing Mary Douglas, Tavory writes “there is ever only a joke because there is ‘a joke in the structure.’” And, finally, 4) “humor can be located on a spectrum of its reach,” all jokes are inside jokes, at some level and in some way. Tavory then usefully lays out the sociological implications of this theorization. Borrowing some ideas from Kenneth Burke, Tavory identifies four elements of situations that people in a social world might share—common ideas about the actors, objects, acts, and the moods of a situation. He illustrates his approach by walking us through his empirical case, explaining how and why AIDS is funny in Malawi. But, how do we measure all of this? That is the question. Tavory is optimistic about our prospects for measuring certain features of humor. Pointing to recent developments in the application of text mining algorithms to Big Data as a possible method of cultural analysis, Tavory suggests that such developments hold out the promise of being able to measure bi-sociations (the multiple meanings of words and phrases) and also to create a mapping of the structure of expectations about these duality structures. And thus Tavory suggests our measurement efforts might well lead to being able to create algorithmic models that will be “able to recognize likely instances of humor.” But what these quantitative models would *not* be able to do is understand the “things that everyone ‘feels’ but people often do *not* make explicit.” This gap means that we are necessarily drawn back again to qualitative procedures. Says Tavory, “... to understand not only the vehicle through which a joke is made but why it may be funny, we must move from the distancing that measurement must entail, either to spend enough time in the social space we are interested in and begin to grasp some of the jokes, or to ask people what is so funny about the jokes they laugh at.” Like Mears, Tavory sees a virtue to employing multiple methods but he also sees a region of human experience that is essentially un-measurable.

The last lesson of the AAAS symposium also comes into play as we think about measuring culture. As Caws and Cassirer argued, we must theorize before we can measure and we see that very dynamic being played out in multiple ways in the articles we have assembled here. Consider the article by Omar Lizardo. For many years now sociologists have analyzed social networks and used the formal analysis of network structures as a way of understanding and explaining social behavior. Usually culture was absent from these analyses or, in some cases, network structure was used as a way to predict people’s relationship to culture. But with the cultural turn in American sociology, a new theoretical framework began to take shape, a framework that sees the cultural dimensions of social life as establishing the foundation for how social structures like networks come to form up in the first place. Lizardo’s article follows in this line. He offers an elegant and paradigm shifting inversion of Ronald Burt’s traditional analysis of the efficacy of network positions that link across structural holes, providing instead a suggestion of how an analysis of the content of culture that defines

a person (in the context of the broader, overall network of shared cultural tastes) can give us a way of measuring a person's network potentiality or their likelihood of connectivity within a networked world of others who have similar (and different) patterns of cultural tastes. Here we have a case, as Cassirer would well appreciate, of a shift in theory that necessitates a change in the way that measurement happens.

Much the same is true of Frederick Wherry's article. Wherry gives us a broad overview of what happened to the formal study of markets once cultural factors were brought to the fore. Instead of the traditional vision of markets as rationalized structures of choice and deliberation in which meanings have been emptied out, Wherry shows that the cultural turn in economic sociology brought about a gestalt shift, a change in the way that formal measurement practices can be employed to analyze the logic of markets. Wherry highlights three ways in which this has happened, one is through the focus on what he describes as "discursive inflection points," or moments when the anchored meanings (or logics) of a field give way to some new metaphorical foundation that shifts the orienting gestalt for participants in a market. He also points to the utility of studying "breached ritual sequences," a concept that he borrows from Harold Garfinkel's notion of disruptions to taken for granted normative expectations. Finally, Wherry emphasizes the importance of relational analysis (of the sort that Viviana Zelizer has pioneered). In all of these cases, theory clears the way for new ways to measure.

Theory also precedes measurement in the article by Roger Friedland, John Mohr, Henk Roose, and Paolo Gardinali. In this case, the issue has to do with the study of institutional logics that describe the deep level systems of implicit understanding that operate to provide meaning and orientation in a given institutional space. The concept has been the object of theorization and research by sociologists for some 20 years now but in this paper, Friedland and his colleagues emphasize a new approach to theorizing institutional logics, an approach that highlights the idea that a logic is composed of a troika of duality relations linking subjects, objects, and practices. It is the concept of objects (or substances) that is especially important here because substances are defined as deep ontologies of understanding that do not exist except insofar as they are enacted through practices and embodied within identities. After shifting the theory of institutional logics in this manner, so too must Friedland et al. change the way that they measure. In this article, Friedland and colleagues use their new theorization as a way to motivate a different approach to measuring institutional logics, an approach that they then test out through a study of the cultures of love that operate among a sample of American university students.

Conclusion

All sciences depend on repertoires of measurement. It is only through conventions of measurement that data can be accumulated and it is only through the systematic analysis of data that science can be accomplished. Although it is frequently ossified, uncontested, unnoticed, and forgotten, scientific measurement is, in fact, the very foundation for all scientific work. By drawing attention to Churchman's AAAS symposium on scientific measurement, from a half century ago we have sought to gain a little perspective on the ways in which the problems and prospects of measurement that we confront in the sociology of culture today can be understood. What we have

found is that there are some problems of scientific measurement that persist and endure, plaguing mid-twentieth century physicists as doggedly as they continue to confound contemporary sociologists of culture. But there are also issues in which we have made significant progress and these include our capacity to think in a more nuanced way about the complementarities of diverse methods and styles of measurement.

To compare the current day with the 1950s seems quite apt. The social sciences are coming around once again to address issues of measurement for many of the same reasons that the matter had come into focus a half century ago. We are today, like we were once before, in the midst of a renaissance in methods and measurement. Before, it was the Second World War and its aftermath that provided a primary catalyst for change.²⁰ Today it is the Internet and the relentless slide towards a digitally mediated social world that is upending the conventions and assumptions of the craft of research. In both cases, the way that we as social scientists approach and make sense out of social life will undergo a profound transformation. The articles gathered in this special issue represent the future of social science and in many ways they serve to point us toward new modalities of formal measurement and approaches to interpretive cultural analysis. But none of this is at all straight-forward, none of it is uncontested, and none of it is as simple as we might initially imagine. It is, as Lazarsfeld reminds us, a process that is lengthy and uneven. “The job ... will be a long one, and will involve switching back and forth between concrete categories closely adapted to the data themselves and general categories able to tie in with other fields of experience, until both concrete applicability and generality are obtained. The immediate problem is to get the raw data classified in some reasonable preliminary way, so that it can be communicated, cross tabulated, and thought about (Lazarsfeld and Barton 1951, p. 156).

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²⁰ In the aftermath of World War II (in which large-scale public science projects had played prominent roles), and the growth of the Cold War (with the emergence of new crises such as Sputnik), the United States was entering something of a Golden Age of academic science. Thanks in part to the educational benefits provided to Veterans through the G.I. Bill, universities and colleges were growing (and hiring), public investment in science was becoming institutionalized (through the founding of organizations such as the National Science Foundation) and research science was making significant advances across all fronts (Appel 2000; Clark 2008; Douglass 2007; Geiger 1993; Zachary 1997).

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John Mohr (Ph.D., Yale University) is Professor in the Department of Sociology at the University of California, Santa Barbara. His primary interest is in the empirical study of meaning systems and he has focused on developing applications of relational (especially network) analysis to the study of discourse in institutional systems that include the history of the welfare state, the development of post affirmative action policies in higher education, and the field of nanotechnology. Mohr is currently working (with Ronald Breiger, Robin Wagner-Pacifici, and Petko Bogdanov) on applying modern text-mining tools to analyze US statements about national security, on analyzing the relationship between practice and happiness within a Buddhist monastery (with Josep Rodriguez) and on further understanding the institutional logic of love (with Roger Friedland).

Amin Ghaziani is Associate Professor of Sociology at the University of British Columbia, where he is also affiliated with Institute for Advanced Studies. Ghaziani has published articles in the *American Sociological Review*, *Social Problems*, *Sociological Forum*, *Contexts*, and *Theory and Society*. He is author of *The Dividends of Dissent: How Conflict and Culture Work in Lesbian and Gay Marches on Washington* (Chicago, 2008). His newest book, entitled *There Goes the Gayborhood?*, is forthcoming in August 2014 with Princeton University Press (Princeton Studies in Cultural Sociology Series).