From Robert Klitgaard, *So What? Useful Intellectuals in a Needy World* (unpublished ms).

9. The Quantitative-Qualitative Divide

Within many pastimes there are competing enthusiasms, cultural differences, even barriers of antipathy. Motorcycling has them. In a rude cartoon two bikers regard each other at a stoplight. One is a bearded renegade in a T-shirt straddling a huge Harley. The other is a sleek youngster in leathers on a boldly colored Japanese superbike. The cartoon shows each biker's thoughts. "Idiot," the Harley rider is thinking. "Idiot," thinks the racer.

They have this at least in common. Plus the fact that, as one motorcycle advertisement puts it, "No one has to ride a motorcycle."

Most of those whose pastime is research about social issues have something in common, too. They are academics, and no one has to be an academic. But social researchers form subcultures as strongly differentiated as the motorcyclists' choppers and cafe racers. And sometimes as hostile.

One line of differentiation concerns quantitative versus qualitative research. By "qualitative methods" many things can be meant, including participant observation, case studies, action research, open-ended interviews, experiential research, clinical studies, and ethnographic field work. Qualitative researchers—let's call them "quals"—come in many varieties but often share a disdain for exclusively quantitative research, for example when the unreliable results of large-scale surveys such as the census are statistically dredged for "significant" associations among reified "variables."

The techniques used in quantitative research quickly become highly specialized, and it is easy for quals to make embarrassing mistakes in discussing them. The quants mutter at the quals for not knowing how to think about multivariate relationships. For many quants the model is the hard sciences, where, as philosopher W.V. Quine notes, "numbers and other abstract objects bid fair to steal the show. Mathematics subsists in them, and serious hard science without serious mathematics is hard to imagine."[1] The literary critic George Steiner writes:

> The sciences and technologies which govern twentieth-century Western civilization have become "modern" and dominant in exact proportion to their mathematical formalization. Larger and larger domains of discovery, of scientific theory, of productive technological appliance have passed out of reach of verbal articulation and alphabetic notation.[2]

The quals respond that the quants, despite all their techniques, have come up with few precise estimates for any important parameters, in part because of weaknesses recognized by quants themselves in the caveat sections of their papers but conveniently forgotten in quant-qual debates.

But tabulating, even tabulating the quants' lack of success on their own terms, is not how the quals prefer to debate. Quals have a deeper misgiving. Quantitative research is wooden, tasteless, lifeless; it misses textures and flavors and individuals. Quals worry that quantitative techniques start to drive the problems being studied, the theories being developed, and the entire

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[1] W.V. Quine, *From Stimulus to Science* (Cambridge: Harvard University Press, 1995), p. 40.

[2] George Steiner, *Real Presences* (Chicago: University of Chicago Press, 1989), p. 114.

focus of the human sciences, which in turn (they aver) attracts the wrong kinds of people to important matters of theory and practice.

An Example

The quants have their own arguments. To show the extent to which the debate can be carried, consider an exchange that took place at a conference on data gathering in rural areas in India. Economist T.N. Srinivasan bristled when he heard anthropologists employ some of their favorite distinctions, such as nuanced, qualitative participant-observation versus clunky, quantitative surveys.

> If the description of nuances can vary with the observer in a conceptual sense, then there is no scientific point in attempting to describe them anyway. Conceptual subjectivity is to be distinguished sharply from the standard survey problem of "investigator bias" . . .
>
> The quantitative *versus* qualitative argument is again a phony one. If by qualitative one simply means an ordinal measure of ranking of a characteristic rather than a cardinal one, it is *still* quantitative. . .

But anthropologist Arjun Appadurai responded that "deeper" epistemological issues are involved. There is an analogy to the Heisenberg uncertainty principle (where the investigator distorts the reality he observes, making it fundamentally unknowable), which quantitative researchers tend to ignore. Appadurai questioned "whether problems of social life (and standard of living) can be reduced largely to their quantitative dimensions (and still remain significant)." Moreover, he asked "whether the problems of how rural people talk and think can be divorced from the fact that serious differences of world-view and terminology separate them from the social scientists who study them."

Srinivasan was unsympathetic. He refused to debate what he calls Appadurai's "interpretation" of the Heisenberg principle, and he did not "wish to quibble with him about what is 'knowable' since I believe, with the Hindu philosophers, that true knowledge lies in knowing what one does not know and cannot even know!"

> My point is simple: any debate about methods of studying rural change can be joined only if there is a common understanding among the participants at a *conceptual level* of what is to be studied. If the term "qualitative" as applied to a factor simply cloaks the conceptual fuzziness as to what that factor means, there is no point in attempting to assess how it has changed! . . . If the gaps between the language, terminology, thought processes, and the world-view of rural people are so different from those of the social scientists as to be indeed insurmountable, neither the survey method nor the participant observation can ever generate knowledge about rural folk.

Appadurai retorted: "One might mix metaphors here and suggest that the larger desert is a phony problem to the ostrich with his head in the sand." He quickly added, however: "Anthropologists, likewise, will have to worry a lot more about their long-standing fetish concerning 'holism,' a fetish I have criticized elsewhere." Appadurai refused to accept that "measurement is the *sine qua non* of social science." He concluded:

> This volume opens a dialogue which, in my judgment, is most important because it exposes our differences at the level of our ideologies of measurement, of epistemology,

> and, dare I say it, of "science" itself. Without admitting and addressing this problem, all talk of solutions, including my own, is probably over-optimistic.[3]

A frustrating exchange. I feel sympathy for both Srinivasan and Appadurai, by extension to both quants and quals. But I am bored by the methodological decrees. I wonder if quals and quants cannot find ways to move forward together in empirical research. Can't quants learn how intensive qualitative studies can help calibrate measurements and discover biases and omitted variables? Can't qualitative studies provide clues for large-scale quantitative research to follow up? And vice versa. Can't quals use quantitative research as a springboard for their own investigations? If the quals can step back a bit and gain some perspective, might they discover how many qualitative insights emerge from statistical techniques and mathematical models? In short, I wonder how we might leave behind the qualitative/quantitative debate and take our varied vehicles of thought with more of a sense of fun and adventure.

That's a lot of wondering, and I'm afraid this chapter proffers no solutions, if indeed solutions may be imagined to exist. Perhaps three humble and parochial examples, though, may suggest how quantitative and qualitative researchers might ride together, and like it.

Unusually Effective Schools

Let me begin with a somewhat historical example. In the late 1960s and early 1970s several path-breaking quantitative studies seemed to show that the quality of American public schools didn't matter.[4] Such student variables as social class, race, and rural-urban explained most of the variance in how much mathematics students learned and how much verbal competence they displayed on standardized tests. School variables such as pupil-teacher ratios, the education levels of the teachers, the quality of the libraries, and so forth made little difference. One estimate was that doubling the per-pupil expenditure would raise student achievement by only 1 percent.

A considerable debate ensued. Some critics savaged the achievement scores as partial measures of what schools were about (true) and as racially biased (false). They complained about missing variables in the statistical studies, which if controlled for would have made schools look much better (not yet demonstrated). Others argued that the schools were really about instilling social values such as punctuality and discipline as much as about achievement, so no surprise that "schooling in capitalist America" was cognitively disappointing. Yet another line of argument used the results to justify an attack on public schools, sometimes in the particular variations that had emerged in the United States, and sometimes in general.

After reviewing the empirical work and replicating some of it on several data sets, George Hall and I followed a different path. Even if on average schools make little difference to students' achievement, are there unusually effective schools? Could it be that most public schools are

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[3] T.N. Srinivasan, "On Studying Socio-Economic Change in Rural India," pp. 240, 245; and Arjun Appadurai, "Small-Scale Techniques and Large-Scale Objectives," pp. 276-8; both in *Conversations Between Economists and Anthropologists: Methodological Issues in Measuring Economic Change in Rural India* (Delhi: Oxford University Press, 1989).

[4] James S. Coleman *et al.*, *Equality of Educational Opportunity*, 2 vols. (Washington, D.C.: U.S. Government Printing Office, 1966). Frederick Mosteller and Daniel P. Moynihan, ed., *On Equality of Educational Opportunity* (New York: Vintage Books, 1972). Christopher Jencks *et al.*, *Inequality* (New York: Basic Books, 1971).

operating well inside their production possibility frontiers, that they could learn lessons from the success stories?[5]

Our idea was to look at schools over time and in different grades and see if some schools consistently outperformed the others, after controlling for students' background factors. In four data sets the answer was "yes." Some schools were consistently half to two-thirds of a standard deviation above what was expected, and this was much farther above expectation than could be accounted for by chance (or measurement error).

When we looked in more detail at the overachievers, Hall and I made some interesting discoveries. They tended to have smaller classes, better educated teachers, and more funds per student. Even though these variables did not explain much of the variance in achievement across all students in all schools, they were conspicuous in the unusually effective schools.

Our research provided a quantitative background and a justification for a host of case studies. Later investigators discovered that the unusually effective schools had powerful principals, who emphasized basic skills, discipline, and centralized control. This research turned out to augur good news and bad news for the educational establishment. Good news, because the establishment now had a way to escape the withering implications of the first wave of statistical studies: we showed that good schools both existed and had the characteristics educators had long said were important. Bad news, because the later "qualitative" research cut against the then-prevalent educational fancy for "classrooms-without-walls" and for de-emphasizing discipline and basic skills.

My point, however, concerns a useful joining of forces. Hall and I helped develop quantitative methods to identify candidates for qualitative research. Our techniques were limited in many ways, including of course the data at hand. As we noted, one could not be sure that the schools our methods identified were indeed "more effective." But those schools were a better bet than a random selection. And as it happened, they pointed the qualitative researchers in what turned out to be fruitful directions.

Social Integration and Disintegration

A second example concerns at once a more pressing and more ethereal concern: social integration and disintegration. At the 1995 summit on social development, world leaders addressed three issues: job creation, poverty, and social integration. The last category is a source of increasing concern. But what exactly is "social integration"?

The answer is not obvious. There is a distinctive sense that even as economic growth takes place, and perhaps because of the way it takes place, social disorders are increasing. Although such worries are not new, perhaps in the wake of the breakdown of most of the Communist system and perhaps in a *fin de siècle* mood, one seems to hear more about problems of "social disintegration" now than in the recent past. But despite horror stories such as Rwanda and the former Yugoslavia, some scholars deny that social disintegration is worsening. For example, sociologist Alex Inkeles' magisterial survey concludes that "in the overwhelming majority of cases the changes associated with these social forces have meant an improved quality of life for most

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[5] Robert Klitgaard and George R. Hall, *A Statistical Search for Unusually Effective Schools* (Santa Monica: The RAND Corporation, 1973), reprinted in shorter form in William B. Fairley and Frederick Mosteller, *Statistics and Public Policy* (Reading, MA: Addison-Wesley, 1977). Klitgaard and Hall, "Are There Unusually Effective Schools?" *Journal of Human Resources*, Vol. 10, No. 1 (Winter 1975).

people," ranging from objective measures of material welfare and political freedoms to self-reported satisfaction.[6]

Is social disintegration growing? To address such a question, ideally we would begin with a theory of social integration and then derive appropriate concepts, measures, and statistical tools. But like Joseph Tainter, who reviewed scholarly work on "the collapse of complex societies,"[7] Johannes Fedderke and I could not find plausible such theories.[8] And so our research strategy began from a different point. Can we develop a useful understanding of social integration through the study of patterns among a variety of possible indicators that are available in international data sets?

We assembled scores of imperfect indicators of regime instability, violence, political and civil rights, corruption, economic inequality, family stability, and general levels of economic prosperity. We even added the results of large-scale surveys of citizens' self-reported "satisfaction with life" and "happiness." Whether the indicators are invariably positive or negative is problematic, and we took no sides. Instead, we asked how various measures of these phenomena behave and interrelate.

Our first finding was that the available data are unsatisfactory in their validity, reliability, scope, and geographical coverage. Nonetheless, after numerous statistical explorations, we discovered what might be called two broad factors of social disintegration.

The first factor loads heavily on regime instability and the absence of basic rights. It turns out to correlate significantly with homicide rates, ethnic separatism, government corruption and inefficiency, hierarchical and non-individualistic cultural characteristics, "unhappiness" and dissatisfaction with life, and basic indicators of economic and educational development. Thus, Factor 1 seems to measure fundamental problems of social integration, linked with basic issues of political and economic development. This factor (constructed on data before 1985) significantly though imperfectly forecasts economic growth from 1987 to 1992.

A second, independent factor we discovered loads on indicators of unrest that do not threaten regime stability. Strikes, riots, and political assassinations appear here instead of the coups, revolutions, and constitutional changes in Factor 1. Countries high in Factor 2 have many "crises" but turn out not to be particularly bad at civil and political rights, nor are their governments particularly inefficient or corrupt. There are larger numbers of political parties and big landowners, but agriculture tends to be less marginalized. Voter turnout is low, despite populations that are older and have higher levels of literacy and education than the average country in the world. These countries tend not to be plagued by issues of holding a country together, getting people educated and fed, and making government work for the people instead of against their interests. This second factor is not related to GDP growth. But countries scoring high on Factor 2 do exhibit various forms of unrest, political fractionalization, higher scores on the cultural "traits" of "intolerance of uncertainty" and "masculinity," considerable unhappiness and dissatisfaction with life, and an unequal distribution of land.

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[6] Alex Inkeles, "Industrialization, Modernization, and the Quality of Life," *International Journal of Comparative Sociology*, Vol. XXXIV, Nos. 1-2 (1993).

[7] Joseph A. Tainter, *The Collapse of Complex Societies* (Cambridge: Cambridge University Press, 1988).

[8] Robert Klitgaard and Johannes Fedderke, "Social Integration and Disintegration: An Exploratory Analysis of Cross-Country Data," *World Development*, Vol. 23, No. 3 (March 1995).

Interestingly, neither Factor 1 nor Factor 2 is related to many things we think of as aspects of social disintegration: ethnic fractionalization, reported rapes, levels of female wages compared to male wages, consumption of alcoholic spirits, officially reported levels of illegitimacy, percentage of single parents, indices of economic discrimination, the percentage of national income going to the bottom 40%, the percentage of the population in prison, or the emission of greenhouse gases. Nor are these variables strongly associated with each other.

Does rapid growth spawn social disintegration? We found that the fastest-growing quartile of countries from 1960 to 1985 did as well or better on almost every indicator of social integration, compared with the slowest-growing quartile.

How should one interpret such explorations? Fedderke and I called them "pre-theoretical warm-up exercises." We hoped our findings would constrain glib generalizations about social integration and disintegration. In particular, we argued that many of the phenomena of social disintegration that are increasingly discussed in the industrialized countries turn out not to be highly correlated among themselves nor do they seem to be the paramount issues of social integration facing the rest of the world. We suggested that different countries confront different challenges of social integration.

We emphasized the limitations of quantitative forays like ours. We were eager for case studies, for what might be called "clinical research." Countries very high and very low in both Factors 1 and 2 would be interesting candidates.

Clifford Geertz recently emphasized the need to investigate social disintegration by confronting cases.

> Secularism, commodification, corruption, selfishness, immorality, rootlessness, general estrangement from the sources of value, all the ills attributed to the modern form of life as it has taken shape in the West (and especially, everyone's hard case, in the United States), loom, or seem to, as imminent threats, and the risk of havoc looks at least as real as the promise of ease. It is not just the fact that progress, or its absence, is harder to measure than it is in matters where ICORs, Gini coefficients, GDP, and per capita income can at least be notionally calculated; it is that it is quite unclear how you might calculate (though there are always those who will try) such matters as political openness or oppressiveness, social vitality or enervation, aesthetic power or emptiness, spiritual depth or superficiality. You only know (if then) what these are when you are faced with specific examples of them, concrete and actual . . .[9]

Geertz and I would agree on the usefulness of thick descriptions of specific instances. But perhaps I would give more emphasis to quantitative and comparative work to help identify which examples to be "faced with," and which aspects of the "concrete and actual" to focus upon.

Studying Rural Poverty

In South Africa especially, but also in other countries, it is well known that some racial groups experience more poverty than others, and that rural people tend to be poorer than city dwellers. To some extent, in South Africa for example, part of the observed racial gap in average income (estimated at about 9.5:1 for whites to blacks) is in fact a rural-urban gap (many more blacks live

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[9] Clifford Geertz, *After the Fact: Two Countries, Four Decades, One Anthropologist* (Cambridge: Harvard University Press, 1995), pp. 142-3.

in impoverished rural areas, and the average *black* income in the Gauteng area around Johannesburg area is over 6 times that in the mostly rural Transkei).

Less studied are differences in the incidence of poverty within rural areas. Consider the area formerly known as KwaZulu, now part of the province of KwaZulu/Natal, which includes 175 "tribal areas." Almost exclusively black and largely rural, these areas are characterized by poverty that resembles Mozambique or Malawi more than urban areas of South Africa. In the 1991 census the average income in these tribal areas was only R573 (a little over $200 at then prevailing exchange rate).

How much variation is there across these tribal areas? Are some doing relatively well? What might explain this? Apart from factors such as rainfall and soil quality, which are in some sense beyond the control of local leaders, could the quality of the local tribal authority make a difference? Or aspects of what might be called the local sociocultural setting?

As a prelude to the kinds of case studies and ethnographic research that would be needed to answer such questions, Amanda Fitschen and I carried out a quantitative study of per capita income across tribal areas.[10] We found considerable variation in per capita income. The tribal area whose average income is at the 25th percentile of all tribal areas has an income of only R376. In contrast, the 75th percentile tribal area has a per capita income that is more than twice as high, R775. Average incomes in tribal areas range from R60 to R3218.

We added to the census data information from an agricultural college about average rainfall and soil quality, then used a geographical information system to aggregate these data by tribal area boundaries. We regressed per capita income against rainfall, soil quality, population density, the proportion of the population that is female (many men migrate to cities), the proportion with secondary school diplomas, and the literacy rate. A bunch of tribal areas were three standard deviations higher than expected. A bunch of others were well over two standard deviations below expectation. Might these over- and underachievers, so to speak, represent something more than random noise or measurement error? Are there unusually effective tribal areas?

We plotted the residuals from this regression on the map of KwaZulu/Natal. We looked especially at the top quintile and bottom quintile of residuals. We examined their proximity to cities and towns. Proximity did matter, it seemed, but we also found many cases where a top quintile area was right beside a bottom quintile area. We overlaid the road network. We overlaid the location of clinics and of schools. In some cases we thought we could "explain" why one tribal area was doing better than another alongside—the district road ran right through a market town. In other cases we couldn't.

Two lines of additional work followed. The first is to gather more data at the local level, such as public investment and expenditure, the extent of non-government organizations, crop yields, and so forth. The second is to carry out some pilot studies of the "sociocultural setting" at the local level. In seven villages, several South African students and I pretested a protocol for detailed interviews with "key informants" and a survey of attitudes and values. In its design we drew upon an international conference on how to measure the sociocultural setting.[11] In the pretesting

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[10] Robert Klitgaard and Amanda Fitschen, "Exploring Income Variations across Traditional Authorities in KwaZulu-Natal, South Africa" *Development Southern Africa*, Vol. 14, No. 3 (October 1997).

[11] Robert Klitgaard, ed., *Assessing Cultures*. IRIS Research Report No. 2. College Park: Center for Institutional Reform and the Informal Sector, University of Maryland, 1993.

we found significant variations across villages in such things as polygamy, attitudes toward outsiders, and perceptions of the quality of local leadership.

Statistical and geographical research techniques can be used to identify over- and underachievers and target those tribal areas for more in-depth, local-level research. Once again, quantitative methods can identify promising starting points for qualitative research.

Riding Together

Synergies can of course run from qualitative research to the quantitative. A clinical report triggers a survey in medicine; Freud's story of Hans and his little widdler stimulates (among other things) disconfirming data. In the human sciences examples might be cited from anthropology (Benedict's categories), psychology (Piaget's stages of development), and economics (Doeringer and Piore's dual labor markets, not to mention Marx's history of England).[12]

Qualitative techniques are being used to calibrate the accuracy and bias of the U.S. census. Anthropologists help survey researchers "translate" questions into forms that are more readily appreciated by members of different cultural groups. They also help contextualize the information-gathering process itself, trying to break it away from the straight-jacket of standardized questionnaires. Qualitative techniques may help map out the cognitive domains and sociocultural norms for different subpopulations, which can in turn provide insight into the content of the study and improve the conceptualization of the research issues involved. It is not as easy, however, as saying that the dense qualitative research can be counted on to get a more accurate answer than the clunky census survey. Statistician William Kruskal notes several problems.

> Here, specially chosen and trained observers go to small population clumps and get to know families well so that census information can be checked or calibrated in fine detail. I think this is a highly promising approach, but there are at least three difficulties. First the ethnographer might appear to be an intrusive, big-brother agent of the distant inimical government. Second is the ever-present possibility that the selection and training process through which the observers inevitably pass will tend to select for people of particular kinds whose traits will bias the end results. Third, ambiguities of definition will forcefully come to our attention.[13]

To which one might add that the problems of identification and inference are not overcome by having even infinite information on a very few cases.[14] Whether we are quants or quals, a condition of much empirical research in the human sciences is the lack of a plausible theory to guide statistical modeling. We have hunches and vague hypotheses, but seldom anything that resembles a scientific theory. We have seen three examples. Does anyone have a detailed and believable theory for how family background factors and school characteristics might combine,

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[12] Statistician John Tukey puts the point more generally. "Unless exploratory data analysis uncovers indications, usually quantitative ones, there is likely to be nothing for confirmatory data analysis to consider." John W. Tukey, *Exploratory Data Analysis* (Reading, MA: Addison-Wesley, 1977), p. 3.

[13] William Kruskal, "Introduction" in *Measurement Errors in Surveys*, ed. Biemer, *et al.* (New York: John Wiley & Sons, 1991), p. xxxii.

[14] Charles F. Manski, *Identification Problems in the Social Sciences* (Cambridge: Harvard University Press, 1995).

under various conditions, to produce various kinds of academic achievement? For what causes social integration and disintegration? For the variations in poverty across tribal areas?

In our complicated multivariate world, where not only are there many sources of variation but the arrows of effect can run in both directions, estimation without a theory is at best an exploration, at worst the manufacture of erroneous inferences. One reaction is to abandon statistical hypothesis testing (the hypotheses are incomplete, the testing therefore biased in unknown ways). Edward Leamer's memorable admonition was the title of a paper in the *American Economic Review*: "Let's Take the Con out of Econometrics."[15] But we might also say, let's take the con out of the case study. The pretense of sure empirical estimation is false for qualitative as well as quantitative methodologies.

And yet we do learn from research of both kinds. I cannot say, without much more evidence than the three humble examples I've offered, to what degree it is useful (defined how?) intentionally to combine (in what ways?) quantitative and qualitative research. Lacking such evidence, my point is to suggest that, despite what has become almost a cultural divide, investigators of both camps *can* usefully combine forces.

True, we can guess that this breach stems in part from non-scientific factors, whatever the adherents say—from differences in taste, from different abilities, from a process of academic specialization that has turned intolerant. But is there also a kind of stubbornness here, which a moment's reflection (and three small examples?) might help us overcome?

When I listen to debates between adherents of quantitative and qualitative methods, I'm reminded of the bikers muttering at the stoplight. My hunch is that it's time to get our Harleys and the Kawasakis in motion, enjoying together what motorcycles are all about—the journey, the discovery, the results. We should stop trying to decree what truth is and get on the road to uncovering it.[16] Riding together in the human sciences, not only will we learn more and help solve more social problems, but we can leave behind a lot of negative energy wasted at the stoplights.

The Harley tattoo puts it this way: "Live to ride, ride to live."

Speaking of science, Friedrich Nietzsche made a similar point. To paraphrase: whether we are after laws through quantitative research or precious stones through qualitative methods, the real point is not getting to the other side of the earth but the discoveries we make en route. And then to quote: "For this reason Lessing, the most honest of theoretical men, dared to say that he took greater delight in the quest for truth than in the truth itself. He thus revealed the fundamental secret of science, to the astonishment and irritation of scientists."[17]

Or of some scientists, anyway. Surely not the physicist Richard Feynman:

> The work is not done for the sake of an application. It is done for the excitement of what is found out. Perhaps most of you know this. But to those of you who do not know it, it is

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[15] Edward Leamer, "Let's Take the Con out of Econometrics," *American Economic Review*, Vol. 73, No. 1 (March 1983).

[16] "Science is seen as pursuing and discovering truth rather than as decreeing it," notes Quine. This idiom "fittingly vivifies scientific method, the method of interrogating nature by conjecture and experiment and abiding by the consequences." Quine, *From Stimulus to Science*, p. 67.

[17] Friedrich Nietzche, *The Birth of Tragedy*, §15 (trans. Shaun Whiteside) (London: Penguin, 1993).

almost impossible for me to convey in a lecture this important aspect, this exciting part, the real reason for science. And without understanding this, you miss the whole point. You cannot understand science and its relation to anything else unless you understand and appreciate the great adventure of our time. You do not live in your time unless you understand that this is a tremendous adventure and a wild and exciting thing."[18]

[18] Richard P. Feynman, *The Meaning of It All: Thoughts of a Citizen Scientist.* Reading, MA: Addison-Wesley, 1998, p. 9.