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Author: Charles R. Hulten

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Theory and Measurement
An Essay in Honor of Zvi Griliches

Charles R. Hulten

1.1 Introduction

Several revolutions in the field of economics occurred more or less simultaneously during the decades following World War II. One was the diffusion of formal mathematical techniques into economic theory with the Arrow-Debreu revolution in general equilibrium analysis and the Hicks-Samuelson revolution, which recast standard economics in a form that made quantitative analysis possible. At the same time, great advances were made in econometric analysis as techniques from statistics and agricultural economics were combined with the functional forms of the mathematical models. Computing power also increased dramatically during this period. Last, but certainly not least, the nation's macroeconomic statistics were compiled in 1948 into a coherent and internally consistent form in the National Income and Product Accounts (NIPA).

Many of the contributors to this transformation are Nobel laureates: Simon Kuznets, George Stigler, Milton Friedman, Tjalling Koopmans, and Richard Stone. Although the contributions of Zvi Griliches were regrettably not recognized with a Nobel Prize, he was a figure of comparable importance. The significance of his early research in the field of applied econometrics and in the study of the factors making for technological change were recognized in 1965 when he was awarded the John Bates Clark Medal. His many contributions are reviewed in the last book he completed before his death in 1999, *R&D, Education, and Productivity;* they cover a broad range of subjects but are organized on the central question of how

Charles R. Hulten is a professor of economics at the University of Maryland, chairman of the executive committee of the Conference on Research in Income and Wealth (CRIW), and a research associate of the National Bureau of Economic Research.

knowledge is generated, diffused, and embodied in production and growth processes.

The importance of careful measurement is another central theme in Griliches's research. This theme permeated his work in specific areas like human capital and R&D, and while it did not produce the kind of explicit results that could be easily assessed, in sum it constituted one of his most important legacies. Griliches was keenly aware of the injunction by Tjalling Koopmans (1947) that measurement without theory is to be avoided and made major contributions to developing the linkages between the two. His seminal 1967 paper with Dale Jorgenson on the measurement of productivity change is a virtual manifesto on the need for theory to guide measurement practice. Yet, at the same time, Griliches was also deeply concerned about the limitations on empirical work posed by inadequate data and the tendency for economists to substitute more theory for better data when the latter are found wanting. His presidential address to the American Economic Association (Griliches 1994) is a forceful reminder of the danger of theory without measurement in empirical work.

Many others have paid tribute to his contributions to the various topics on which he worked. I will pay my tribute to his contributions to measurement as a whole and to the importance he attached to the accuracy of economic data. In addition to reviewing some of the key issues of the theory-measurement dichotomy, my remarks will build on this theme by stressing the essential duality of theory and measurement and argue that this dichotomy itself is potentially deceptive. First of all, "getting the data right" often requires "getting the theory right." Second, theory and measurement are not separable aspects of economic activity in the sense that the accuracy of the data can affect subsequent economic decisions, and a complete economic theory must allow for possible feedback effects associated with inaccurate data. Finally, considerations of political economy come into play because the accuracy of economic measurement affects policy in a variety of ways, and changes in policy can, in turn, affect economic behavior.

1.2 The Quantitative Transformation

The convergence of theory, statistics, and data in the 1950s and 1960s involved more than the incorporation of new techniques and methods: it also involved a shift in the sense of what was possible. Koopmans, for example, starts his famous article on "Measurement without Theory" with the example of synergism between the empiricism of Tycho Brahe and the theory of Johannes Kepler in the development of celestial mechanics. While no one seriously believed that the laws of economics were as precise as those of physical science, there was a new sense of the possibilities of quantitative economics and a newfound faith there was enough stability in the

underlying economic structure that it is meaningful to give the "laws" of economics an exact mathematical form. And this translated into a program of research that encouraged the partnership of theory and measurement.

This structuralist program is apparent in the development of empirical demand and production theory, as well as in the theory of growth. The early phases of this development transformed the largely graphical and heuristic description of the theory of the consumer and producer into a corresponding system of structural equations. This was followed by the effort to express the system in a form suitable for econometric analysis, in part by assigning specific functional forms to the demand and production systems and, more generally, to developing the nonstochastic specification of the system. In the estimation of production and investment functions, for example, ever more flexible functional forms were developed that allowed for more complex interactions between capital and labor inputs: the fixed-proportion and Cobb-Douglas forms were generalized into the constant elasticity of substitution (CES) function (and others), which in turn yielded to the translog and generalized Leontief forms. Duality theory was also developed during this period. The stochastic specification received relatively less attention at this time.

Which variables to include in the analysis, and how they should be defined and measured, was also an important part of the nonstochastic specification of econometric models. The debate over whether real output should be measured net or gross of economic depreciation and a parallel debate over capital were central to the famous exchanges between Jorgenson and Griliches (1967, 1972) and Denison (1972). Questions about the inclusion of research and development and its relation to the multifactor productivity residual should also be mentioned (Griliches 2000, chapter 4), as should the role of the Jorgenson (1963) user cost of capital in the accounting system (Christensen and Jorgenson 1969, 1970).

A key insight emerged from these debates: theory and measurement were "dual" in the sense that a testable theory of economic growth is associated with a set of data accounts that corresponded to that theory. In this view, theory and measurement imposed mutual constraints on each other. This duality is nowhere more apparent than in the fundamental national income accounting identity between the value of output and the value of factor input. This identity can be derived from a constant-returns-to-scale production function using Euler's theorem and the assumption of marginal cost pricing. Conversely, if one starts with the accounting identity, different structures of production are implied according to how the prices and quantities are actually measured and interpreted, as well as by what is included in the analysis. This is the Koopmans's injunction writ large.

1.3 "Data Woes"

Unfortunately, the partnership between theory and measurement proved far from equal, and Griliches devoted a significant part of his 1994 presidential address to the American Economic Association to a discussion of this issue. In a section titled "Data Woes," he asks the following question:

> Why are the data not better? The facts themselves are not in dispute. Every decade or so a prestigious commission or committee produces a report describing in detail various data difficulties and lacunae. (Griliches 1994, 14)

He acknowledges that he "really doesn't have good answers to this question," but goes on to offer three observations. First, "measurement problems are really hard"; second, economists have little influence over the budget for data collection activities, and the statistical agencies are balkanized; and, finally, speaking of the economics profession:

> We ourselves do not put enough emphasis on the value of data and data collection in our training of graduate students and in the reward structure of our profession. It is the preparation skill of the chef that catches the professional eye, not the quality of the materials in the meal, or the effort that went into procuring them. (Griliches 1994, 14)

This observation is an ironic inversion of Koopmans's injunction against "measurement without theory"; it is conceptual ingenuity that builds the careers of both the brilliant chef and the successful academic. The ingredients are of secondary importance, and this leads to the risk of too much theory without measurement, or, more accurately, theory with measurement so long as someone else finds the ingredients. The published data series provided by government statistical agencies are, after all, Samuelsonian public goods, and academic researchers are aware of the disincentives associated with the attendant appropriability/free-rider problem.[1]

This situation is well illustrated by the notoriously hard problems involved in measuring the output of the service-producing sectors of the economy. Griliches pointed out the true source of the difficulty: the inability to define in principle what exactly is meant when we speak of the out-

1. It should be emphasized that the neglect of measurement described by Griliches in the mid 1990s was relative to the emphasis it received in earlier periods. And many academic economists, like Griliches, did soldier on despite the diminished interest in measurement by academic economists as a whole. The Productivity Program at the National Bureau of Economic Research, directed by Griliches, and those associated with it deserve special mention in this regard. The same should be said of the CRIW, which was founded in the 1930s to work on the conceptual problems associated with the development of the national accounts and which has continued to promote research on data issues. However, it is also true that during the period with which Griliches was concerned, publication of data-oriented research in prestigious journals became more difficult, and courses on subjects like national income accounting disappeared from graduate curricula.

put of banks, insurance companies, doctors, lawyers, teachers, and so on, which he termed the "hard-to-measure" sectors. It is relatively easy for theory to work with concepts like "real output" in an abstract way, without having to take on the hard issue of just what it really means or, significantly, to define the units in which the output of services are to be measured. If theory fails to help with this problem, how is the statistician to implement the Koopmans's injunction? As Griliches puts it:

> [I]t is not reasonable for us to expect the government to produce statistics in areas where concepts are mushy and where there is little professional agreement on what is to be measured and how. (Griliches 1994, 14)

Moreover, the difficulty of linking theory and measurement became progressively harder as theory evolved. The theoretical models of the early stages of the Hicks-Samuelson revolution tended to be highly aggregative, or based on the representative agent, and also tended to assume perfect competition (e.g., the aggregate production function underlying growth theory and growth accounting). This made for a tidy paradigm to guide the development of macroeconomic data, but theory also showed that the conditions that made for exact aggregation were highly unlikely (Fisher 1965).

Subsequent theoretical development took a more disaggregated view of the world, a world that is inherently more messy, with heterogenous agents and imperfect information and competition. Correspondingly, interest in measurement issues became more "micro" and cross-sectional (panel data) as well as more field specific. Academic interest in the quality of "official" macro statistical series waned, even as empirical work with microeconomic data sets increased. Moreover, the structural parameters of economic models became harder to identify as these models became more complicated, and the structuralist paradigm of the earlier period was challenged by reduced-form approaches. The current debate over the interpretation of regression coefficients in the hedonic price model is a recent example of the tension between the two approaches (Pakes 2003; Hulten 2003).

Thus, the profession's apparent neglect of data issues was, in part, a shift in emphasis from macro to micro levels of theory and measurement. Still, it would be hard to make a persuasive case that the economics profession placed much priority on its data during this period. As Zvi Griliches (1986) observed in his contribution to the *Handbook of Econometrics,* the term *data* is the plural form of the Latin word *datum,* which means "given," and one might further say that researchers in this period were generally happy to be given their data.

1.4 The Greenspan Critique

Both theory and data are important elements in the formulation of monetary and fiscal policy. Getting the theory "right" is a priority, as witnessed

by the debate of the Phillip's curve in the analysis of price inflation (the critiques of Friedman [1968] and Lucas [1976] are discussed below). But it is also important to know what the current rate of inflation actually is before deciding whether policy intervention is needed. It became increasingly apparent to policymakers in the 1980s and 1990s that the lack of reliable data was a binding constraint on policy formulation. The most pointed criticism came from Federal Reserve Board Chairman Alan Greenspan in the mid-1990s. He suggested, in remarks to the Senate Finance Committee in 1995, that the growth rate of the CPI might be biased upward by .5 to 1.5 percentage points per year (Greenspan 1995). This is a very significant bias given that the year-to-year change in the CPI averaged around 3 percent in the years immediately preceding Greenspan's remarks. An upward bias of this magnitude presented a rather different picture of price inflation and, as we shall see, had major implications for programs like Social Security, in which expenditures are indexed for inflation using the growth rate of the CPI.

After a panel of prominent economists concurred with the Greenspan's conjecture about 1 percentage point bias, a commission was subsequently established to investigate further (chaired by Michael Boskin, whose earlier efforts to improve the quality of economic data became known as the "Boskin Initiative"). The commission, of which Griliches was a member, also found a bias of around 1 percentage point, and attributed about half to methodological issues and half to a failure to capture dynamic improvements in product quality and the development of new goods.

This second source of bias in the CPI pointed to another dimension of the Greenspan critique: Greenspan (1998) also questioned the ability of existing macrodata to represent the true dynamism of the American economy, citing the implausibly low estimates of real output growth in the service sectors of the economy. The NIPA, for example, evolved at a time (the 1930s and 1940s) in which the production of tangible goods in manufacturing, agricultural, and natural resource sectors were the major source of gross domestic product (GDP; according to NIPA estimates, the service sectors accounted for 36 percent of GDP in 1947, shortly before the NIPA were launched, and were 56 percent by 1997). With this intersectoral shift came a shift away from the tangibility of output and toward the hard-to-measure intangible services where the units of real output, and thus economic growth, are hard to pin down.

A similar problem occurs with the measurement of "knowledge" capital (Corrado, Hulten, and Sichel 2005). Much of the dynamism of the U.S. economy is the result of scientific and technological innovation, which is reflected in the rapid rate of product and process innovation arising from the commitment of resources to education, research, and development activities. As with services, the output associated with these activities is largely intangible and hard to measure: in what units should knowledge be

measured? In what units should innovation-induced quality differences in successive generations of computers be measured? The Boskin Commission estimated that half of its CPI bias was due to the mismeasurement of quality improvement and the introduction of new goods (Boskin et al. 1996), and Shapiro and Wilcox (1996), who produced similar estimates, likened the measure of quality change to "house-to-house combat." Moreover, estimates of investment by the U.S. private business sector during the 1990s suggest that expenditures for intangible capital were as large as spending for fixed capital. The latter is treated as a component of measured GDP, but intangible investment is not.

The U.S. statistical system has confronted these challenges: changes to the CPI have reduced the growth rate of the index by around three-quarters of a percentage point, again a rather sizeable change in percentage terms and a significant proportion of the bias estimated by the Boskin Commission. Programs have been initiated or planned at the Bureau of Economic Analysis that are aimed at more accurate measurement of service-sector output, at improving the way industry output is measured, and, more generally, at better characterization of the role of knowledge in the evolution of the economy (quality-adjustment of high-technology goods, capitalization of software expenditures, and the prospective incorporation of research and development [R&D] investment into the NIPA). Many of these changes have been made in the context of a renewed concern for the architecture or overall design of the statistical system and, in particular, with a concern for how theory shapes measurement.[2]

In sum, the years since Greenspan's critique of the mid-1990s have seen an acceleration of change in the macrostatistical system along Koopmansian lines. These years have also seen a renewal of the partnership between theory and measurement, and between academe and the statistical agencies. Though it is difficult to quantify, this renewal is reflected in the membership of the various CPI commissions, in the increased number of conferences and workshops attended by economists from both academe and the agencies (the CRIW has greatly expanded its activities in this area), and in the academic membership in agency advisory committees. These trends are part of the Griliches legacy.

2. The recent CRIW conference on "A New Architecture for the National Accounts," organized by Dale Jorgenson, Steve Landefeld, and William Nordhaus in 2004, is noteworthy in this regard, as is the paper by Fraumeni and Okubo (2005) presented at an earlier CRIW conference on "Capital and the New Economy." It is also worth noting, as a matter of historical perspective, that the "architecture" of the U.S. NIPA were strongly influenced by the data needs associated with the Great Depression and World War II. This influence left the NIPA with a distinctly Keynesian personality oriented to accounting for short-run expenditures flows. As interest shifted toward explaining the high rate of economic growth and innovation after World War II, growth and production theory became the theoretical paradigms for the design of the national accounts rather than Keynesian demand-management theory, and the problems of accounting for capital input and real output assumed a new design priority.

1.5 Koopmans Redux

The Koopmans's injunction is grounded in a view of theory and measurement inspired by the physical sciences: the data associated with a system, physical or economic, are a reflection of the principles that guide the evolution of the system, and thus these principles are the ones that must be used to organize the data used to study the system. However, while the analogy with physical science is instructive, it is flawed in one important respect. In the physical world, an atom of iron cannot decide to become a chlorine atom in order to maximize utility; in the economic world, a steel worker can decide to switch jobs and become a chemical-industry worker on the basis of the available information about the attractiveness of employment in the two industries. Moreover, if that information is inaccurate or erroneous, the error may well end up affecting the worker's future life.

In other words, there are feedback mechanisms in economic systems through which measurement error can affect the evolution of the economy. A complete description of the system must therefore include a theory of how measurement error and, more generally, how partial or inaccurate information interacts with the system as a whole.

Two feedback mechanisms are important in this regard. The first involves the perception of measurement error on the part of economic agents and their behavioral reaction. In general, each new round of official macrostatistics adds to the body of past statistics and is interpreted by economic agents in light of the perceived accuracy of those statistics. Subsequent decisions are based on the interpretation given to each increment to the decision makers' information set, and data bias can therefore become embedded in the evolution of subsequent behavior.

This behavioral effect can be thought of as an economic bias, and the key point is that the behavioral bias may be different from the underlying statistical bias. A statistical bias in the CPI might, for example, cause people to alter their behavior relative to what it would have been if the statistical bias had been zero, but there is no straightforward link between the size of the measurement error and the behavioral reaction to it. Indeed, a systematic statistical bias in the CPI may not have any economic bias at all if agents are not "fooled" by the error and therefore do not alter their behavior. This result might seem, at first glance, to be counterintuitive, but it is in fact an extension of the Friedman analysis of the Phillips curve and the Lucas (1976) critique of policy effectiveness. Friedman and Lucas argued that a tradeoff between inflation and unemployment assumes a high degree of consumer ignorance and that rational consumers would "pierce the veil of money" and focus on the real economy, leaving unemployment unchanged at its natural rate as inflation accelerated. The same logic applies to measurement errors. If agents are not fooled into changing their behav-

ior by a policy-induced increase in the rate of inflation, they would not be fooled by an error-induced increase.

As a more concrete example of this critique, suppose that every agent perceives that the growth of the CPI is biased upward by 1 percentage point each year and that recipients of cost-of-living wage adjustments (COLAs) therefore believe that their COLAs overcompensated them for the effects of price inflation. But so would those bargaining with them in wage-setting negotiations, and the final wage bargain might well reflect this mutual knowledge, leaving the *total* compensation package (basic wage, COLA, and benefit packages) invariant to the bias. Moreover, if the CPI were returned to its unbiased growth rate on the advice of experts and both sides perceived the change, the previous *total* compensation package would be unchanged even though the components would be adjusted to reflect the new unbiased COLA.

This line of reasoning presumes full rationality and complete information. A data bias may have a different effect in other models of economic decision making. If agents are completely ignorant of the 1 percentage point CPI bias, negotiations would proceed as though the biased COLA were an accurate compensation for inflation, and the bias would not be offset by a reduction in the other components of total compensation. Moreover, removing the CPI bias would now affect the distribution of income and most likely change the subsequent evolution of incomes and relative prices.

The possibility of the endogenous feedback of measurement to theory implies that the degree of measurement "accuracy" is an organic and nonseparable aspect of the economic system, and the data are not just external characteristics of the system. This, in turn, opens a new frontier beyond the Koopmans's injunction to be explored in future debates over the meaning of data accuracy and the corresponding role of theory.

Future research must also come to terms with a second type of data feedback mechanism: the possibility that economic agents may want to change official statistics instead of changing their economic behavior. Where the first type of feedback described previously operates directly through the choices of individual economic agents, the second operates indirectly through collective choice in the political arena. The opportunity for the political-economy feedback effect arises because many of the relevant macroeconomic statistics are produced by government agencies, and agency budgets and programs are subject to the control of the executive and legislative branches of the federal government, which, in turn, are elected by the very agents whose welfare and decisions may be affected by the accuracy of the data.

The possibility that some agents may exert pressure through the political process puts the issue of data accuracy on a slippery political slope. It

is tempting to stand on the moral high ground and assert that the national statistical system should be insulated from the political process and reflect only the experts' assessment of best-practice measurement technique. However, while this position may seem unassailable from the heights of the ivory tower and, while it embodies a large dose of truth, it ignores the larger reality of the national statistical system: like all other aspects of government in a democratic system, a nation's official statistics are subject to the consent of the governed. It is ultimately up to the governed to decide whether to accept expert advice about statistics like the CPI (or, more accurately, which expert's advice to choose). The public may be well advised to insulate the statistical system from the vicissitudes of politics, but it is their choice to make.

The history of the CPI over the last decade again illustrates the importance of this political economy issues. Perhaps the most startling finding of the Boskin Commission was the cost to the government of the estimated CPI bias: approximately $1 trillion over twelve years in Social Security and other COLAs based on the CPI. Taken as an unintended government outlay, the estimated bias would rank as one of the largest federal government programs. Conversely, the consequence of removing the CPI bias to those receiving COLAs or inflation escalators based on the CPI was equally $1 trillion. Surely those whose future pensions are reduced have a right to know the reasons for the reduction and to challenge the expert opinion supporting the change with their own analysis to tell their side of the story.

The implication of these political economy considerations for economic measurement are illustrated by a National Research Council (NRC) committee empaneled to study the CPI in light of the Boskin Commission report. The NRC report *At What Price?* (2002) produced two competing visions of the CPI, with different implications for the distribution of Social Security benefits. The first is based on the economic theory cost-of-living index (COLI), the second on the traditional view of the CPI as a fixed basket of products priced in successive periods, whose composition is updated from time to time (termed by the NRC commission the "cost-of-goods index," or COGI). The former seeks to ground the CPI as firmly as possible in economic theory, following Koopmans, and provides conceptual support for the policy of reducing the bias in the CPI and thereby *decreasing* COLAs based on the CPI. The COGI approach, on the other hand, focuses on the shortcomings of theory (e.g., the near impossibility of aggregating data derived from heterogenous consumers) and opts for the more heuristic basis for the CPI—a Laspeyres index applied to a fixed (or slowly changing) market basket of goods and services. It thus provides conceptual support for the larger COLAs of the pre-Greenspan–Boskin Commission period.

The weight of opinion of the economics profession is undoubtedly on the side of the COLI approach. This is the standard textbook solution, and

it gives the COLI approach the perception of scientific credibility with the public as a whole or, at least, with its representatives in Congress who exercise political oversight over the Bureau of Labor Statistics. It should be remembered, in this regard, that the changes in the CPI precipitated by the Boskin Commission emanated from the political process (the Senate Finance Committee), not the expert community of economists.

The "perceived credibility" standard was explicitly invoked by the NRC report in its discussion of the use of price hedonic techniques in the CPI program. It endorsed price hedonics as the most promising method for adjusting CPI prices for changes in product quality, but also cautioned the Bureau of Labor Statistics to proceed slowly in actually implementing the technique because hedonic regressions sometimes produce "strange-looking variable coefficients [which] could be indicative of larger problems" (NRC 2002, 142). The NRC panel did not elaborate on this doctrine, nor did it define "perceived credibility," but it did comment that "it is hard to know when a hedonic function is good enough for CPI work" (NRC 2002, 143). However, the report presumably has in mind both the need for professional consensus on the "science" involved and acceptance of this consensus by the public at large.

My own conclusion is that the doctrine of perceived credibility applies to measurement issues far beyond its application by the NRC report to price-hedonic problems (Hulten 2003). I have argued, more generally, that the adage that "an old tax is a good tax" also applies to data. People adjust their behavior in response to a tax, winners and losers are sorted out as the tax matures, and changes are negotiated and unforeseen defects are ameliorated. A change in the statistical system operates in much the same way: people adjust their behavior in light of the new data (the first type of feedback effect), and the modifications may be made as the data "matures" (the second feedback effect). The roles of the Boskin Commission and the NRC panel should be seen in this light.

1.6 Conclusion

I conclude my tribute to Zvi Griliches with the observation that the quality of data mattered a lot to him. He was bothered by the propensity of many empirical studies to use what he called "found data," and knew that the quality of the ingredients was at least as important as the method of preparation, to use his own metaphor. He had an uncanny ability in seminars to "sniff" out bad "ingredients" even when they were artfully concealed.

He was one of the few prominent economists insisting on the importance of linking theory and measurement. This insistence was particularly important during the period in which academic interest in macroeconomic data was on the wane. This situation has begun to change as the realization

grows that (a) macroeconomic policy is constrained by inadequate data as much as by inadequate theory, and (b) that problems of economic measurement are often caused by inadequate theory.

The presence of potential feedback effects makes the theory-measurement nexus even more difficult. As models of limited information penetrate more deeply into the core of economic thinking, the importance of these feedback effects will become more and more apparent and will force theorists to incorporate the issue of data accuracy into their theoretical models. The field of economic measurement, which Griliches nurtured with such care and dedication, is (one hopes) set to bloom again.

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