# An Invitation to Theory

**Pierre Jinghong Liang (梁景宏)***

*Tepper School of Business at Carnegie Mellon University, USA*

**Abstract**

In this essay, I wish to invite young scholars to learn, use, and contribute to accounting theory. In this invitation, I argue theory has lineage, is important and can be fun. Its lineage comes from the post-WWII scientific revolution in management education and research. Theory is important because it is the successful interaction between theory and empirical work that ultimately advances an academic discipline. Theory can be fun because when done well, learning, using and contributing to theory can be an enjoyable activity for all scholars, either as consumers or as producers of theory.

## 1. A Little History

One way to view theory is that it is a coherent set of ideas that explains, or purports to explain, a set of real-world phenomena. The phenomena need not be obviously coherent on the surface (for example, the seemingly endless features of life forms on earth) but the theory needs to be coherent (for example, Darwin's theory of evolution).

As such, a theory offers a consistent, disciplined way to view the world, or a worldview. The worldview of business management changed dramatically after World War II. Partially aided by the Ford Foundation, American business education underwent a scientific transformation. Graduate School of Industrial Administration (now TepperSchool of Business) at Carnegie Tech (now Carnegie Mellon University) was part of a small group of business schools which broke new ground in management science and laid the foundation of the modern management education of today. The strategy at

* The author is an Associate Professor of Accounting on the faculty of the Tepper School of Business of Carnegie Mellon University, Pittsburgh, Pennsylvania, USA. The essay is written based on a few lectures on accounting theory given at various universities. The author thanks the audience at these lectures. In preparing the written manuscript, comments by Xijia Su of City University of Hong Kong have been extremely helpful. The essay also benefited from conversations with Marvin Goodfriend, Bennett McCallum of CarnegieMellonUniversity, and Feng Liu and Minghai Wei of SunYat-SenUniversity.

 

Carnegie was to invite scientists from base disciplines such as mathematics, economics, and psychology to study applied business problems theoretically. Individually and as a group, these scholars made well-known pioneering contributions including: Bounded Rationality (Simon, 1947), Statistical Auditing (Cyert and Trueblood, 1957), the Modigliani-Miller theorem (Miller and Modigliani, 1958), the Organizational Theory of the Firm (Cyert and March, 1963), Rational Expectations (Muth, 1961 and Lucas, 1972), and managerial accounting based on mathematical programming (Charnes, Cooper, and Ijiri, 1963 and Kaplan and Thompson, 1971).[1]

Based on the early effort of scholars at GSIA (Tepper) and at other institutions, the research and educational programs at modern business schools today are firmly grounded on three basic worldviews (what has been called a "tripod").

- Mathematics: operation research and mathematical programming (linear and non-linear) are the basis for production and operation management. Linear algebra underlies double-entry bookkeeping practices. Large-scale computing makes available the emerging fields of quantitative marketing, data-mining, and machine learning.
- Economics: economic decision-making under uncertainty, the bedrock for modern information economics, is the basis for many business functional areas such as accounting, finance, and marketing.
- Psychology: psychological and behavioral science at the individual level, from which group behavior derives, is the basis for modern organizational behavior research and the emerging interdisciplinary fields of behavioral economics and finance.

## 2. Worldview of Accounting

Within the broad framework of business management, accounting offers an amazing array of alternative worldviews. Ijiri (1967) and Mattessich (1964) stress the elegant mathematical structure of bookkeeping and accounting; a modern revival is found in Arya et al. (2000). Sterling (1970) emphasizes decision theory. Gonedes and Dopuch (1974) and Beaver (1981) emphasize accounting's informational role for capital markets, paralleling modern finance. Demski and Feltham (1976) pioneer an information economics approach, extended by Christensen and Demski (2002) and Christensen and Feltham (2002, 2007). Hilton (1985) incorporates mathematical psychology to the theory of accounting information choice and use. Watts and Zimmerman (1986) present a positive theory of accounting. Ball (1989) views accounting in the context of a firm serving as a specialist contracting intermediary. Sunder (1997) offers a theory of accounting based on a broad theory of contracts. More alternative worldviews are

[1] For additional reading, see Demski et al. (2002) for a discussion of the intellectual foundations of accounting and see Liang (2001) for a short but more focused background on the rise of information economics in accounting theory.

emerging as the academic field evolves along with the fluid accounting institutions it studies.

The beauty and attraction of theory is that no one single worldview is necessarily "*correct*." Christopher Sims offers a pragmatic view of theory as data compression. That is, scientific advances are "discoveries of ways to compress data concerning the natural world ... with minimal loss of information." (Sims 1996, p. 105) In other words, theory is useful because "it provides structure for organizing our thoughts about some set of phenomena." (Christensen and Demski, 2002, p. 6) It can be added that for different sets of phenomena, the worldview (or theory) which compresses the most data (or with the minimal loss of information) may turn out to be different. Theory offers a "buffet," not a single meal.

## 3. Interplay Between Theoretical and Empirical Work

### *3.1 Past Examples of Interplay*

Advances in an academic discipline depend critically on the successful interaction between theoretical and empirical explorations. There are many such examples in accounting and non-accounting fields.

- In physics, the classic example is the study of planetary motion by Tycho Brahe and Johannes Kepler. Nobel Laureate Tjallings Koopmans described their differences and outcome succinctly in his 1947 article "Measurement without Theory." According to Koopmans, "Tycho's main contribution was a systematic accumulation of careful measurements" (Koopmans, 1947, p. 161) even though he believed, incorrectly, "in the uniform circular motion as the natural basic principle underlying the course of celestial bodies." (p. 161). Kepler's more celebrated success was "due to his willingness to strike out for new models and hypothesis if such were needed to account for the observations." (p. 161) In this case, the combination of large scale empirical work and a daring theorist contributed to a theoretical success viewed from Sim's perspective: the eventual theory compresses a huge amount of the data with little loss of information.
- In macroeconomics, for a long time the Phillip's curve (that is, the inverse relation between the unemployment rate and inflation) was assumed to be a stable structural economic relation so the implied policy choices (policy makers could increase employment by inflating price levels via fiscal or monetary policies) seemed logical. However, theoreticians Milton Friedman, Edmund Phelps, and Robert Lucas, Jr. introduced the idea of expectations (thus the distinction between anticipated versus unanticipated inflation or policies) and reasoned that systematically exploiting the Phillips Curve to lower unemployment would only succeed temporarily at best and would certainly fail in the long-run. The validity of their reasoning was confirmed

by the actual macroeconomic experience of the 1970s. This understanding of stabilization policy is now the foundation for monetary policy in a number of countries in their efforts to achieve and maintain a low and stable inflation rate. More fundamentally, their insights show that empirical documentation of a relation does not necessarily establish a causal relation (and thus a policy implication). The lesson, embodied in the famous Lucas Critique (Lucas, 1972 and 1976), is that past observations of people's behavior and relations which econometricians study were influenced by past economic policies and institutions. Further and more importantly, people understand these influences in choosing the behavior which we study. When we propose changes to policies and to institutions, this will affect people's behavior and thus the relations themselves will change accordingly. For example, changing policies may not move the economy along a given Phillips Curve, but may shift the curve upwards or downwards instead. As a result, past relations may not predict people's behavior under the new policies. This is an example where deeper thinking in theory can reconcile the observable relations and policy implications from these relations. (See Phelps, 1987, p. 858-860 and McCallum, 1989, chapter 9 for curious readers on this macro topic.)

- In accounting, the relation between accounting and economic measurement has a long and varied history. Economists Irwin Fisher and Sir John Hicks have been influential in how early accounting theorists (such as William Paton and Sidney Alexander) view income measurement (that is, accounting measurement of income and capital must mirror economic concepts of income and capital). However, many years of practice and observation consistently prove that both accounting income and accounting asset values are far from their economic counterparts (such as stock returns and value). The empirical campaign in accounting, which started in the 1960's, had proceeded largely without much valuation theory linking accounting income and market observables such as prices and returns. The rise of modern accounting valuation theory, due to James Ohlson and his fellow co-authors such as Jerry Feltham, changed the landscape and ushered in a new era of empirical work based on a formal neoclassical theory of price reflecting accounting fundamentals. This is an example of empirical work leading theoretical work, followed by innovation in theoretical work, which leads to further and more refined empirical work.

### *3.2 How Theory Can Aid Empirical Work*

When applied well, empiricists benefit from theoretical guidance in every stage of a typical empirical examination:

- identifying a research question,
- developing research hypotheses,
- designing and conducting empirical tests, and

- interpreting tests findings

In each of these stages, theoretical understanding and discipline can play an important role and adds to the quality of the empirical work.

## Identifying a Research Question

Take the economic worldview as an example. The key to the economic worldview is trade-offs. Resources are scare so economic players trade off marginal benefit and marginal cost to reach (interior) solutions in most choice problems. As scientists studying choices made by economic players, researchers with an economic outlook must recognize that every choice they study (for example, corporate disclosure made by managers to the capital market or the pay-for-performance bonus rate chosen by the firm to compensate managers) is a result of a cost-benefit trade-off. In theory terms, the choice made by an economic player is a solution to some first-order-condition (FOC) equation. Further, any valuation (such as market prices) which depends on this type of choice must be recognized as functions of these solutions. This theoretical economic framing leads directly to the first stage of empirical work: identifying the research question.

Here the lesson is identifying an economic trade-off. When studying any economic/accounting phenomena, the first issue has to be what is the economic trade-off underlying the object of one's study. To be more precise, suppose the objects of the study are economic/accounting choices such as disclosure or bonus-rates. Let them be represented by the variable $x$. The first point is that $x$ must be a solution to a first-order-condition:

$$\max_{x \in X} V(x, a, b) \Rightarrow FOC: \frac{\partial}{\partial x} V = 0 \Rightarrow x^* = f(a, b) \qquad (1)$$

The framing forces researchers to think about (1) who are the players choosing $x$, (2) what are the factors going into function $V(.)$, and (3) are there constraints in the choice problem. For these choices to be interesting both theoretically and empirically, one would expect them not to be extreme choices generally (for example, full disclosure or non-disclosure; zero- or infinity-bonus rates for all firms at all times). In theoretical terms, we say the choices are interior or the optimal $x$ is not at the corners of the choices set $X$. In other words, whenever the research question is formulated, economic theory demands that we first ask whether or not the empirical relation to be examined already has a theoretical answer. For example, suppose one is about to study the determinants of corporate disclosure. It must be the case that disclosure has a both economic benefits and costs, and depending on the magnitude of these benefits and costs, different firms may arrive at different but individually optimal disclosure choices.

## Developing Research Hypotheses

Once the research question is formulated and an economic framework has been established, the next stage is to design specific empirical hypotheses to be examined. Here the formulated first-order-condition is particularly useful. In our general example, the simplest empirical hypotheses can be based on comparative statics in equation (1). That is, we first compute the comparative statics on the solution of the first-order-condition. For example, we compute:

$$\frac{\partial}{\partial a} x^* = \frac{\partial}{\partial a} f(a, b) \text{ and } \frac{\partial}{\partial b} x^* = \frac{\partial}{\partial b} f(a, b) \qquad (1a)$$

Theory predicts both the sign (positive or negative) and the magnitude (large or small) of partial-derivatives. These properties are the basis for empirical hypotheses. Sometimes theoretical properties may be ambiguous. For example, industry competition (say variable *b*) may affect both the marginal benefit and marginal cost of corporate disclosure (choice variable *x*), so how variable *b* affects choice variable *x* may become an "empirical" question. When data are collected on the choice variable *x* (disclosure or bonus-rate) and independent variables *a* and *b* (for example, industry competition, risk-profile of the firm), the first-order condition justifies a cross-sectional regression relating optimal choice of *x* to variables *a* and *b*, along with other scaling variables necessary for the empirical specification.

If the hypothesized economic tradeoff were true and the collected sample contains enough variation in a andb (and with enough power), the empirical test would reject the null hypothesis that there is no relation between choice variable *x* and model parameter *a* (or the derivatives are zero).

## Designing and Conducting Empirical Tests

When designing and conducting empirical tests, theory can be helpful in more subtle ways. For example, theory may lead one to rethink empirical strategy if an additional layer is added. Suppose variable *a* is, in fact, a choice as well, either by the same firm or other firms or entities, that is:

$$\max_a U(a, b, c) \Rightarrow FOC: \frac{\partial}{\partial a} U = 0 \Rightarrow a^* = g(b, c) \qquad (2)$$

Combining equations (1) and (2) forms a structural specification of the economic relations of interests. Recognizing the endogenous nature of variable *a*, a reduced-form specification would become:

$$\begin{cases} x^* = f(\,g\,(b,\,c),\,b\,) = h\,(b,\,c) \\ a^* = g\,(b,\,c) \end{cases} \tag{3}$$

Within this simple setup[2], we can point to the challenges of the empirical design. First, we can identify the "endogeneity" problem in this study. That is, variable *a* is a choice variable as shown in equation (2) but is mistakenly treated as independent variable in equation (1): *a* is endogenous. Second, inferences based on the statistical relation between *x* and *a* (in equation 1) are, in fact, driven by the variation in variables *b* and *c*, which according to equation (3) jointly drive choice variables *x* and *a*. Based on this reasoning, empirical specifications can differ based on which variables are treated as choice variables (that is, solutions to first-order-conditions) and which are not. In summary, theoretical design and specifications determine empirical design and specifications.

One can also consider the case where many left-hand-side variables are not choices variables per se; value and price are good examples. That is, these variables are functions of choice variables (firm value is a function of disclosure or bonus-rates; a market clearing price is a function of many choices made by different individuals and firms). Re-injecting equation (3) into the value function of equations (1) and (2), we have

$$\begin{cases} V\,(x^*,\,a^*,\,b) = V\,(\,h\,(b,\,c),\,g\,(b,\,c),\,b\,) = m\,(b,\,c) \\ U\,(a^*,\,b,\,c) = U\,(\,g\,(b,\,c),\,b,\,c\,) = n\,(b,\,c) \end{cases} \tag{4}$$

Similar analysis, such as comparative statics, can be performed and empirical specifications can be derived based on equation (4). This iterative process is the key to a solid foundation for empirical investigation and to the resolution of existing empirical puzzles.

## Interpreting Test Findings

Viewed as a compression of data with error, even very good theory will "fail" some empirical tests when we look hard enough and shed light on the "error" part of the theory. When an empirical test rejects a prediction of a theory,[3] can we say such a failed

---

[2] Equation (2) can be more complicated than what is shown. For example, objective U(.) may be a function of the choice x. In that case, timing of these choices becomes important. Further, choices x and a may all represent a series of choices over a long period of time. In that case, dynamics become the key issue. Either way, the resulting structural relations can be incredibly complicated, posing both theoretical and empirical challenges.

[3] More appropriately, the test fails to reject the null hypothesis assuming the alternative hypothesis represents the theoretical prediction.

theory is of no use? Here the discussion by Stanford economist David M. Kreps (1990) is particularly useful. According to Kreps, knowing what assumptions lead to falsified conclusions is beneficial because "it is often a good place to begin to figure out what does" (p. 9). Without the failed first or second or third trials, there may not be the eventual successful theory at the 100th trial. Finally, "models that fail to predict because they lack certain realistic features can still help clarify the analyst's thinking about the features they do encompass, as long as the analyst is able to combine intuitively and informally what has been omitted from the model with what has been learned from it" (p. 9-10).

Aside from dealing with falsified theory, our problem may extend to theories which are too complicated to be tested with any power or with any economic significance. Another issue can be that a theory can appear to be so simple that its predictions are too obvious to test. These also post challenges for empirical work and may frustrate empirical researchers. What happens when some theories have never been and perhaps will never be tested empirically or in a lab? Viewed as data-compression devices, even untested theories may be useful as long as it helps us organize our thoughts, ideas or intuitions and helps put in perspectives things that have been personally or casually observed.

The lesson here is that there is a judgment side of conducting social science research. There is no set formula to build a model or design an empirical specification. It all depends on what one seeks to better understand. As such, any finding from an empirical test is a prisoner of its own prejudice and limitations of the underlying (explicitly or implicitly stated) theory. As researchers, we restrict our attention to certain main effects when analyzing the phenomena of our interest to achieve scientific precision. For the better, the findings help us understand the economic relation within our narrow scope of study. For the worse, the findings may lead us to miss the more importation relation or factors that we deliberately assume away.

At this stage, theory is helpful because of its own explicit acknowledgement of assumptions. As a data compression device, all theories suffer loss of information in the process. Even if a theory is successful in the sense that empirical data may support its predictions, it nevertheless misses some part of the social phenomena in reality. It is our job to always search for improved data-compression devices. As such, theoretical considerations at this stage make us more humble and perhaps fan the desire for better future research.

## 4. A Separate Theory for China?

As researchers interested in institutions in China or other emerging markets, we are often tempted to ask whether "western" theoretical predictions can be generalized to China, and whether we need to develop our own (Chinese) theoretical foundations on which (Chinese) empirical studies must follow. As a general idea, I believe this kind of thinking is dangerous and represents an impediment to progress, especially in the area of

higher learning. But as a specific idea on how general theory should be specialized to the specific institutional features associated with emerging markets like China, I view such adaptations as necessary and a catalyst for progress. Further, when such institutional features change in response to the gradual maturity of emerging markets, the general theorymust be reapplied accordingly.

As a general principle, this kind of idea belongs to a type of philosophy called Exceptionalism. It has many variations. In the rapid rise of Great Britain during the 1800s, British Exceptionalism was an integral part of its national myth, as well as a key explanation of how a geographically small country emerged as a great commercial and maritime power. In political philosophy, American Exceptionalism is the theory that the United States is an exception to the norm and deserved a special space among the nations of the world because of its unique immigrant-based population, its geopolitical location, its unique founding, its unique political and religious institutions, and its apparent departure from some assumed norms of national development.

The idea that China can be an exception from assumed general economic relations is very similar to the earlier Exceptionalism, either the British or American version, and is typically associated with the rise of a young nation/economy. But reviewingthe history, it seems the law of nature always prevails and these Exceptionalisms have proved to be quite outdated ideas. Good theories stand the test of time and space no matter how strange they may initially appear and bad theories won't no matter how "official" they can be. Viewed in such fashion, I believe we should doubt the validity of such Exceptionalism claims when stated in general terms. The "Lucas Critique," which is an important innovation in economic theory in the past 50 years, does not have any national boundaries. Agency theory deals with economic problems emanating from information asymmetry among economic agents, which again, do not have any national boundaries.

However, viewed at the level of a specific issue, the "exception" idea may actually be quite useful. Every economy at some stage will have its unique institutional features, such as the existence of strong state- and family-controlled economic entities or the lack of market institutions like property rights protection. These institutional features are typically very hard and slow to change (but they do change); and these institutions do affect economic relations. In our earlier example, suppose the local institution is such that variable a is not a choice variable at all. Then as a result, equation (1), instead of (3), is the correct empirical specification to be tested. As such, economic theories and resultant empirical relations must take the existence and impact of these institutions into account. However, emerging economies, by definition, have a temporal property because the name suggests that in time these economies would emerge and become established economies where stable and progressive institutions would prevail and so will the prevailing economic relations suitable for such a mature and developed economy. By then, we will no longer need to worry about "Chinese" economics versus "Western" economics; they will simply be economics.

## 5. Theory Is Fun!

How can theory be fun? Many of us have a painful experience reading theory papers with all those mathematical expressions and Greek symbols that are so hard to even pronounce. Equating theory and math is another myth about theoretical work in social science. Yes, knowing mathematics helps to construct precise arguments about somewhat complicated economic arguments. Yes, being better at mathematics sometimes makes theory researchers better. But as most successful theorists would attest, the most important and fun part of theory may not be the math at all. The key to theory (thus the key to reading, learning, and using theory) is the recognition of a critical issue of interest (or idea) and the ability to think through the issue in a logical and coherent manner. It just happens that mathematics has become, as a convention, the most convenient and direct way to communicate such ideas. But math is never at the center stage. Good ideas are.

As scientists, we are both consumers and producers of research. As producers of research, we rationally choose to be specialists ("carnivore" or "herbivores" of a certain kind), exploiting comparative advantage and economies of scale. As consumers of research, we should choose to be generalists ("omnivores" in the literal sense: meaning *eater of everything*).

To accounting scholars, this invitation to theory is wide and open for both: as producers and as consumers, perhaps especially the later. Just as more and better consumption of empirical work makes a better producer of theoretical research, more and better consumption of theoretical work makes a better producer of empirical research. So accept the invitation and look around, you may find lots of fun as well as clues for your own work, empirical or theoretical.

It is always exciting to study theory, especially now. Just as the world economic environment is experiencing a much-needed rebalancing, thus creating an opportunity to equalize the past disparity of wealth and development, the existing theoretical order in accounting is rebalancing given the stress on accounting institutions created by the changing economic environment of the world. While the economic use of information remains as the core of accounting phenomena,[4] the rebalancing brings about new research dimensions. Here are a few examples of the emerging and exciting new opportunities for accounting research both in theory and in empirical work.

- The role of accounting in macro- or aggregate behavior is an emerging topic both in theory and empirical work, especially in light of the financial crisis in the past two years. This is significant for accounting because for a long time accounting research has been mostly focused on individual firm/entity level, as opposed to the macro-

[4] For an expanded argument for this point, see my article in a 2008 issue of China Accounting Review, co-authored with Professor Xiao-Jun Zhang of UC-Berkeley (Liang and Zhang, 2008).

or economy-wide level. With this direction, learning from macro- and monetary-economics will become necessary for accounting researchers.

- Similarly, early theoretical focus on specific managerial control, largely based on agency theory, has shifted to become a much broader examination of the governance structure of either corporate or public entities. Theory is much needed in formulating this general governance issue with a mixture of markets and contracting approaches in a dynamic setting.
- Finally, reputation has become an increasingly important factor of production in the study of many economic phenomena. Examples are auditors' reputation for quality assurance services, firms' reputation to disclose quality information, and regulators' reputation to follow through on policy commitments. Advances in repeated game theory have made the systematic examination of reputation both feasible and more conducive to applications such as accounting.

With so much to learn and so much promise, I believe it is quite fun to be a student of theory today.

In the end, having fun is what ultimately makes good research happen. When we learn production functions in economics, a two-factor function is typical: output is a function of capital (K) and labor (L). If one were to consider the production function of good research, what would it look like? Capital is needed, researchers need a building to work, a chair to sit on and a computer to run programs, etc. Labor is obviously needed as all scholars, especially young scholars, need time to develop their skills and time to apply their skills to frontier questions. But to produce good research, physical capital and repetitive labor are typically not enough, unlike the production of many other goods. Good research is typically a result of some group of creative individuals having a lot of fun working. In other words, good research requires PASSION. So go out there and have fun!

## References

Arya, A., Fellingham, J. C., Glover, J. C., Schroeder, D. A., Strang, G., 2000. Inferring transactions from financial statements. Contemporary Accounting Research17 (3): 365-385.

Ball, R., 1989. The firm as a specialist contracting intermediary: Application to accounting and auditing. Working Paper, University of Rochester.

Beaver, W. H., 1981.Financial reporting: An accounting revolution. Englewood Cliffs, NJ: Prentice-Hall.

Charnes, A., Cooper, W. W.,Ijiri, Y, 1963. Breakeven budgeting and programming to goals. Journal of Accounting Research 1:16-43.

Christense, J., Demski, J., 2002. Accounting theory: An information content perspective. New York, NY: McGraw-Hill/Irwin.

Christense, P., Feltham, G., 2002. Economics of accounting: Information in markets, Vol. 1. New York, NY: Springer.

Christense, P., Feltham, G., 2007. Economics of accounting: Performance evaluation, Vol. 2. New York, NY: Springer.

Cyert, R., March, J. G., 1963.A behavioral theory of the firm.Maldon, MA: Blackwell Publishers.
Cyert, R., Trueblood, R. M., 1957. Sampling technique in accounting. New York, NY: Prentice Hall.
Demski, J. S., Feltham, G. A., 1976. Cost determination: A conceptual approach. 1st ed. Ed. Ames, Iowa: Iowa State University Press.
Demski, J., Fellingham, J., Glover, J., Ijiri, Y., Liang, P. J., Sunder, S., 2002.Some thoughts on the intellectual foundations of accounting.Accounting Horizons 16 (2):157-168.
Gonedes, N., Dopuch, N., 1974. Capital market equilibrium, informationproduction and selecting accounting techniques: Theoretical framework and review of empirical work. Journal of Accounting Research 12 (Supp): 48-169.
Hilton, R., 1985. Probabilistic choice models and information. Sarasota, FL: American Accounting Association.
Ijiri, Y., 1967. The foundations of accounting measurement: A mathematical, economic, and behavioral inquiry. Houston, TX: Scholars Book Co.
Kaplan, R. M., Thompson, G. L., 1971. Overhead allocation via mathematical programming models. Accounting Review 46 (1):352-364.
Koopmans,T., 1947.Measurement without theory.Review of Economics and Statistics29 (3):161-172.
Kreps, D., 1990. A course in microeconomic theory. Princeton, NJ: Princeton University Press.
Liang, P. J., 2001. Accounting recognition: An information content perspective. Accounting Horizons15 (3): 223-242.
Liang, P. J., Zhang, X. J., 2008. Information economics and accounting measurement: Ablueprint for scholarly research. China Accounting Review6 (1):93-101.
Lucas, R.E., 1972.Expectations and the neutrality of money. Journal of Economic Theory 4: 103-124.
Lucas, R.E., 1976. Econometric policy evaluation: A critique. Carnegie-Rochester Conference Series on Public Policy 1: 19-46.
Mattessich, R., 1964. Accounting and analytical methods. Homewood, IL: Irwin.
McCallum, B., 1989. Monetary economics: Theory and policy. New York, NY: Prentice Hall.
Miller, M. H., Modigliani, F., 1958.The cost of capital, corporation finance, and the theory of investment. American Economic Review 48 (June):261-297.
Muth, J., 1961. Rational expectations and the theory of price movements.Econometrica 29:315-335.
Phelps, E., 1987. Phillips curves, in The new palgrave: Adictionary of economics. London:Macmillan.
Simon, H. A., 1947. Administrative behavior:A study of decision-making processes in administrative organization. New York: Macmillan Co.
Sims, C., 1996. Macroeconomics and methodology. Journal of Economic Perspectives 10: 105-120.
Sterling, R. R., 1970. Theory of the measurement of enterprise income. Lawrence, KS: The University of Kansas Press.
Sunder, S., 1997. Theory of accounting and control. Cincinnati, OH: Thomson Press.
Watts, R., Zimmerman, J., 1986. Positive accounting theory. Englewood Cliffs, NJ: Prentice Hall.