

Book Reviews*

Productivity of Cities

By Sung-Jong Kim, Aldershot, England: Ashgate Publishing Ltd., 1997, pp. 144, \$53.95

While the theory of productivity differentials has been well established in the field of urban economics, empirical evidence on productivity distribution has been lacking. Now, Sung-Jong Kim of Dankook University in *The Productivity of Cities* assembles a concise empirical investigation of urban productivity and provides a model that bridges the often wide gulf between theory and the actual empirical estimation of urban productivity.

Kim starts off with a concise but thorough review of the existing literature on urban productivity. The major issues in the study of spatial productivity are two-fold: What are the factors that account for urban-regional productivity differentials, and what are the methods of productivity measurement used in empirical studies.

Urban productivity differentials arise from differences in industry structure, capital intensity and vintage, the technology adoption rate, the quality of the labor force, and agglomeration economies. Most previous work on urban productivity has focused on agglomeration effects that arise out of the structural changes that take place in urban areas during the process of economic growth in the larger economy. Agglomeration economies are derived from economies of scale, which exist in large cities that offer locational advantages for the performance of certain types of economic activity.

Previous empirical studies of urban productivity have been plentiful, and Kim does a nice job of summarizing their major findings. Most have approached productivity in terms of labor productivity and have used a production function. Econometric methods for production function measurement of agglomeration economies are divided into two overall groups. The first estimates the agglomeration effects from the returns-to-scale parameter in the production function. Under this methodology, it was found that while metropolitan areas are places of concentration of various activities, the concentrations themselves are not homogeneous in mix—even in cities of similar populations. The other approach estimates agglomera-

tion effects via the Hicks-neutral productivity parameter, which models the external effects as initiating shifts in the efficiency parameter. Using this approach, it was found that each doubling of the population leads to an almost 6 percent increase in the productive resources of manufacturers within the MSA.

Kim's alternative to the previous two methods of estimation is a central theme in the book. Data Envelopment Analysis (DEA) is a relatively new technique for urban-regional researchers, so a systematic introduction to the methodology is outlined in some detail. DEA is based on linear programming theory and is quite useful in measuring relative efficiency for production relations involving multiple outputs and inputs in which classical regression cannot be applied straightforwardly. DEA is a nonparametric method of analysis and does not impose unknown structures on the data. The efficiency estimation procedure is based on the frontier relationship, which first identifies the most efficient units from the observed data and then constructs a linear production function based on the subset of the most productive units. Kim writes, "By definition, then, any points on the production frontier represent a feasible technique for combining the set of inputs to produce a set of outputs efficiently." The focus in this methodology is on the individual observation unit in efficiency estimation. For example, when evaluating 50 production units, DEA would utilize 50 multiple linear programming models with each observation appearing on the objective function, and it would calculate the best efficiency scores for each production unit.

After outlining the details of DEA techniques, Kim turns to an actual implementation of DEA in an effort to measure the comparative efficiency of fifty Korean cities. In the context of this empirical application, he also discusses a number of estimation-related problems encountered in practice. The results of the DEA show that large Korean cities are inefficient, and, in fact, the set of cities on the efficiency frontier are those of medium size. None of the fifty cities over 1 million in population are even close to that frontier. This analysis could have vitally important implications for economists working on government economic policy issues.

The next chapter deals with this variation in DEA efficiency scores among the cities by using the relative efficiency estimates as dependent variables in a regression analysis framework. The models were specified with three different agglomeration variables: popula-

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tion, density, and population potential. In the regression model with the density variable, Kim confirmed the “U-shaped” relationship between urban productivity and city size originally found with the DEA.

The final chapter of the book is Kim’s summary and his views on the policy implications of urban productivity measurement. He correctly notes that, “Empirical knowledge of the productivity structure in a national urban system is essential to both urban economists and urban planners.”

DEA techniques could prove powerful for urban economists or commercial real estate professionals in that this analysis could consider each city or MSA as an economic entity that produces comparable outputs using the same types of inputs. A DEA model could then estimate the relative productivity of a city from the actual measure of inputs and outputs. Furthermore, economists could then use the relative productivity estimates to test various hypotheses related to urban productivity. Through his discussion of the DEA methodology and his application of that methodology to a real-world example, Kim has provided a useful guide for economists and policymakers alike.

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The Managed Care Blues & How to Cure Them

By Walter A. Zelman and Robert A. Bereson, Washington, DC: Georgetown University Press, 1998, pp. 224, hard cover \$45.00, paper back, \$17.95.

Hillary Rodham Clinton’s health care task force had two goals: (1) require everyone to join a managed care plan; (2) require health plans to compete in a system called managed competition. The two authors of this book were part of that effort. Although the plan failed, the ideas flourished. There has been a managed care revolution, which is directly affecting the health care of 85 percent of American workers. Choice among competing managed care plans is also a reality for all federal workers, many state and local employees and an increasing number of employees of large companies.

Although the authors appear to present a balanced view, their book is largely a defense of managed care, including a critique of attempts to curtail it with patient-bill-of-rights legislation. If managed care “is not as good as we think it can be, neither is it as bad as many people believe it to be,” they write. The remedy for its defects, they believe, is more widespread

adoption of managed competition, by which they mean an artificial market in which health plans compete but are required to charge the same premium to all enrollees (community rating) or at least the same premium to everyone of the same age (modified community rating). In any event, plans are prohibited from discriminating in any way based on an applicant’s health status.

Given their backgrounds, the authors are unlikely exponents of their views. Walter Zelman, who teaches at the School of Public Health at Harvard University, is a political scientist and former consumer advocate who once promoted Canadian-style national health insurance. Ironically, most advocates of national health insurance view a health care system managed by for-profit companies, as many HMOs are, as anathema. Robert Bereson is a former practicing physician who now directs the Center for Health Plans and Providers in the Health Care Financing Administration (the agency that runs Medicare). The irony here is that physicians as a group have been the most vocal in condemning managed care.

So why do Zelman and Bereson want managed care? They say third-party intervention is the only way to control health care costs. Under the old, fee-for-service insurance system, there was too much inappropriate care and too much inefficiency. The incentives for physicians were, “When in doubt, do it.” Managed care has reversed these incentives. Or has it only modified them? The economics here are straightforward. HMOs get a fixed fee in return for providing health care. The less care they provide, the higher their profits. The incentives are, “When in doubt, don’t do it.” Yet the authors have difficulty confronting that reality and its implications.

Part of the authors’ problem is today largely a nonproblem: inappropriate care. Studies of hospital records show that heart procedures that are “clearly inappropriate” (promise to cause the patient more harm than good) are performed no more than 4 percent of the time. But much of the time even the best-trained doctors, including specialists, disagree. The reason is that many medical procedures promise some benefits, but opinions differ as to whether they will be worth the cost, given a patient’s specific condition. Thus HMOs are constantly making cost-benefit decisions that in effect ration health care. How do they make these decisions? The HMOs won’t say. Zelman and Bereson recognize that when the economic incentives are to withhold care there is likely to be a quality problem.

Can managed competition solve the problem? The authors believe it can. But unlike real competition in free insurance markets, the “managed” competition model tries to ignore the financial implications of the fact that we are not all equally healthy. Under community rating, health plans will seek to avoid the

sick and attract the healthy—a condition Zelman and Bereson acknowledge, and bemoan. On balance, has managed care led to lower quality medicine than fee-for-service insurance? The authors say the evidence is mixed. However, economic theory predicts that the evidence will be mixed but in a systematic way. HMOs under managed competition have incentives to provide too much care for the healthy (from whom they make a profit) and too little care for the sick (from whom they incur losses). Although the evidence is skimpy, I believe it is broadly consistent with this prediction.

Is the only alternative to managed care runaway health care inflation? For other types of insurance, markets have found a better way. Under the casualty model, common in automobile and homeowners insurance, people share financial risk by paying some expenses out of pocket and relying on insurance only for catastrophic losses. These markets don't work perfectly. But they work better than managed care.

Zelman and Bereson do not consider the possibility that individual patients could manage a share of their own health care dollars through Medical Savings Accounts. The fact that physicians are serving as agents of third-party-payers rather than agents of their patients seems not to concern them. That physicians are being paid to deny care is of only passing interest. Physician complaints are generally dismissed as self-interested. Patients' insistence on choosing their physician (described as the "cult of choice") is treated as a problem rather than as part of a solution.

In general, this is a well-written book, with a lot of information conveyed in ways that will be helpful to lay readers. The authors give a history of how we moved from a market dominated by fee-for-service health insurance to one in which pure fee-for-service insurance is virtually extinct. Along the way they explain problems faced by physicians, how managed care companies have exploited market opportunities and why patient protection measures are being demanded. Although all this is seen through a pro-managed-care lens, the authors are not advocates of the status quo. They favor regulations that would guarantee appeal rights for patients. They propose freeing employees from their employer's health plan and allowing them choice, much like federal employees can choose among a dozen or so competing health plans today.

Nonspecialists, however, could be misled on several important points. For example, the authors repeat the canard that, because preventive medicine saves money, HMOs have a financial self-interest in providing it. The facts say otherwise. With few exceptions, preventive medicine adds to total costs; it doesn't reduce them. They also treat HMOs' fewer hospital days as *prima facie* evidence that HMOs have reduced costs. In fact, less hospital care is usually made

possible only by adding more physician therapy and drug therapy. Whether overall costs are lowered as a result is a separate empirical question. The discussion of HMO "report cards," including the Health Plan Employer Data and Information Set (HEDIS) published by the National Committee for Quality Assurance (NCQA), might also mislead. These so-called measures of quality mainly measure inputs, not outputs. For example, they tell how many healthy patients get screened for cancer, but they don't tell what happens to people after they get cancer.

The authors also fail to address the central issue surrounding managed care: To what extent can objectively derived protocols substitute for the subjective opinions of practicing physicians? Like many defenders of managed care, Zelman and Bereson believe that physician opinions are often based on outdated medical school training, custom and habit. But is there an alternative? Not long ago, the hope among technocrats was that guidelines developed by experts could substitute for the judgment of physicians. Yet the most recent studies show that expert opinions vary widely, depending on which experts are consulted. Even those in the vanguard in promoting guidelines now admit that they cannot reliably be used in making decisions about individual patients. Practicing physicians know that administrative pressures to follow the guidelines are strong and persistent. A physician who allows professional judgment to override the guidelines on too many occasions might soon be looking for other work.

While public opinion seems to indicate Americans are suffering from the "managed care blues," the authors believe we can learn to live with them. They do not consider whether turning health care into a real market could be the appropriate antidote.

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Investment Intelligence from Insider Trading

By H. Nejat Seyhun, Cambridge, MA: MIT Press, 1998, pp. 402, \$29.95.

Can insider-trading signals provide valuable investment information? Most business economists know that corporate insiders have more and better quality information about their companies' future than outside investors. The question is whether the publicly available information on how those insiders use that information in their own stock trades can profit outside investors? The author's answer is that insider trading information is valuable, more valuable than several other valuation measures, and can be used to improve investment

returns.

Make no mistake, this is not a get rich quick stock system by a fly-by-night, self-proclaimed guru. The book is a scholarly study, from a foremost publisher, by a leading academic expert on insider stock trading. The study is based on a massive data set going back twenty-one years, covering all of the reported insider transactions of all publicly traded firms in the United States. The author is Chair of Finance and Professor at the University of Michigan. Other business economists might recognize his work on the “January effect” if they are interested in modeling day or monthly trading patterns.

Investment Intelligence begins at ground zero for each subject. For example, the first section of the introduction is thoughtfully titled, “Why is there a need for a book on insider trading?” One reason is to answer the question posed in the first sentence of this review. Another reason is to understand what other information can be learned from insider trading data, e.g., “Do insiders profit from their own trades?” (They do), and “Do they tend to manipulate stock prices?” (They don’t). Another reason for the book is to develop methods that outside investors can use to improve their investment results using insider-trading data.

The book begins by dispelling two common misperceptions: (1) all insider trading is illegal; (2) if insiders make money, then it must be illegal and thus they only report unprofitable transactions. In the same section, the definition of insiders is explained and the types of transactions that are and are not regulated are discussed. It also introduces what insiders can do, e.g., purchase stocks or options, and what they can’t do, e.g., trade on nonpublic information or sell short. The reader is introduced to the concepts of market timing and market allocation (in the sense of diversification) within the context of insider-trading information. The first chapter is an introduction to the data set, a brief overview of the regulatory history and the landscape of the insider-trading world. It ends with a brief introduction to the debate concerning the impacts of increased governmental regulation of insider trading. We now have the background to see if insider trading predicts future stock returns.

Stock prices rise following both purchases and sales by insiders. What we learn is that stock prices rise more after insiders’ net purchases than after net sales. Thus, insiders earn profits from their legal trading activities. These returns are greater than returns for the overall market. In typical straightforward language the author says, “Overall, insider purchases are good news, while insider sales are bad news for future stock performance.” Of course there is much more to the arguments and discussions, e.g., the passive versus active nature of some insider trading and the fact that signals can conflict. However, while the language is clear, the

complexity of concepts is not hidden.

These general concepts are expanded to more specific applications. We learn that, of the four classifications of insiders, top executives are the most informed, followed by officers and directors, with large shareholders the least informed as measured by their investment returns. The explanation is that top executives base their decisions on first-hand day-to-day operations rather than changes in official strategy or other factors that might be known at lower levels. In summarizing several of the factors, the results seem to indicate that the interaction between inside purchases-sales, identity of insiders, volume of the trade, and firm size are most important. Each of these influences exerts a separate, independent and additive influence on the profitability of insider trading.

A particularly interesting chapter is devoted to the stock market crash of 1987. The objective was to use insider trading to understand better the causes of the 22.6 percent fall in the Dow-Jones Industrial Average on October 19, 1987, and the subsequent rise. The view is that typical investors, not insiders, were “spooked” by the previous two-week’s price fall. Insiders were large purchasers of shares immediately after the crash and not only provided liquidity to the market but also increased confidence in the market at a critical time.

Can one use insider trading data along with information on P/E ratios, book-to-market ratios, momentum, or dividend yields to improve investment results? The answer is sometimes yes and sometimes no. When the author has clear results, you are not left in the dark. For example, in the case of dividend yields, after an extensive review of the data and very well measured analyses, the author’s conclusion is, “Our overall advice to the typical stock market investor who follows insider trading is to ignore dividend yields.” This statement is sure to send some readers into orbit. Nevertheless, this language is all too infrequent in both the academic and investment world, where the rewards for being almost exactly right are small compared to the penalties for being slightly wrong.

The book is disarmingly straightforward. It asks clear questions and gives clear answers. It is much more than well written, it is understandable. It is much more than a book on how investors can use insider-trading data to improve investment returns. It is an introduction to the academic study of security analysis, with the mathematics, statistics and unnecessary equivocation removed. The conclusion of the book is that insider trading information can improve investment results, and it works best as part of an overall investment strategy rather than as a substitute for security analysis.

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