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Book Reviews

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Book Reviews

MALCOLM McRAE

GORGOL, JOHN FRANCIS, *The Military-Industrial Firm: A Practical Theory and Model* (Manuscript prepared for publication by Ira Kleinfeld). New York: Praeger Publishers, Inc., 1972, 143 + xix pp. \$12.50.

This book springs from the author's dissatisfaction with the ability of existing economic theories to explain the operation of military-industrial firms (MIFs). The latter are firms engaged in developing and selling highly technical products to the Department of Defense (DOD) as sole customer. In this role, MIFs place technical competence above profit maximization, do not operate in a free market environment, and surrender a great deal of autonomy to DOD decision makers. The book's main sections are devoted to (1) Inadequacies in existing theories of the firm with respect to MIFs, (2) Hypotheses concerning the MIFs decision-making activities, (3) A theory and model of the MIF, (4) A simulation of MIF behavior, and (5) Conclusions.

The title and Table of Contents suggest a book useful to several audiences: those interested in the theory of the firm, MIF managers, and those interested in model building and simulation methodology. *The Military-Industrial Firm* does call attention to a relatively neglected but important class of enterprise and it knowledgeably discusses the relationship between the MIF and DOD. However, a number of deficiencies need to be overcome in order for the book to satisfy its implied audiences.

First of all, the premise that existing theory is inadequate and needs replacement would imply that the purposes or intended uses of the theories—existing and proposed—be stated, and that the inadequacies of existing theories be sharply identified. Is the theory intended to be normative, or descriptive of actual behavior? Will it be used chiefly for explanation, prediction, or as a guide to decision making? Does the theory work adequately by itself or is it intended as part of a larger theory of resource allocation? In what ways and to what extent are existing theories inadequate for the intended purposes? *The Military-Industrial Firm* virtually ignores discussion of specific purposes and uses, and the existing theory discussed looks very much like a straw man. It emphasizes neoclassical theory, intends to show resource allocation under different market conditions as part of general equilibrium theory, and highlights its shortcomings in describing the internal

behavior of one class of firms. No substantive discussion is given to the relevant monopsonistic case, and little or no argument is given for the implied inadequacies of newer theories, such as modifications of neoclassical theory, organization theory, decision theory, game theory and so forth. Although the implied inadequacies of present theories for particular purposes may indeed exist, no clear case is presented and the book begins on weaker ground than it might.

As a second point, hypotheses concerning MIF behavior would supply a stronger base for the theory and model if stated in unambiguous language. In addition, the hypotheses might be coupled with the type of evidence and decision rules to be used in rejecting or not rejecting them. Gorgol's five "hypotheses" are each chapter sections containing a variety of statements—a few tautological or ambiguous: "The MIF will survive only if it allocates sufficient resources for the development of new technical competence"; "[Technical competence] can be defined as the perception that the [DOD] has of the MIF in terms of the firm's ability to invent and discover". Tighter methodology at this point would clarify the hypotheses, allow better marshalling of evidence and lend credence to the statements.

The theory proposed by Gorgol consists of two sets of word descriptions of schematic diagrams which show the relationship between factors such as technical advance, production, political influence, negotiations, technical interchange and resources in one case and between engineering, marketing, production, management, DOD and the political superstructure in the other case. The relationships spring from the aforementioned hypotheses which in turn are based on the author's observations and some defense industry studies done in the mid-1960's. Although an empirical base is presented, no formal testing of the model was performed, nor is the model in a form which enables direct testing.

The simulation portrays the probability of success in winning a contract as a (unspecified) function of two factors: technical advance and political influence. The former is a function of resources allocated to "modification of specifications" (selling DOD on the firm's research potential) and "developing technical competence"; the latter is a function of resources allocated to political influence activity. The technical advance and political influence scales are integers corresponding to unspecified subjective benchmarks of achievement. The MIFs position on the technical advance or political influence scales rises by one unit if a random number is drawn which is less than a critical value of the form $R/(A+BR)$, where R is measured in composite units of appropriate resource allocation. In addition, a separate probability transition matrix shows probabilities of advancing between stages of technical capability each of which is insufficient to win a contract. The reader is given little or no advice concerning the meaning or operational character of the scales used, the probability assessments required, the functional forms or the parameter estimates needed.

The simulation output over 8 runs of 120 time intervals each was described by factors such as ratio of resources wasted to total resources, mean backlog, total resources allocated, average technical incompetence (maximum technical advance less attained technical advance), and, of course, number of contracts won. These outputs were studied under eight configurations of effectiveness in allocating resources to three categories: specification modification, technical advance, and political influence. The results show, for example,

that high effectiveness in technical advance appears most critical in winning contracts. Some results are rather surprising and suggest a larger number of runs or model modifications. For example, according to the model the number of contracts won was the same when resource effectiveness was low for all three categories as it was when resource effectiveness was high for all three categories. One appendix discusses the parallelism between the MIF and the Soviet firm, and another shows a flow chart for the simulation program.

The Military-Industrial Firm has a great deal of merit in pointing out the behavioral relationships between MIFs and the DOD and in reminding us of a large class of firms too often neglected in economic theory and model building. The book leaves one with the impression of a valuable early draft or research progress report. This impression is unfortunately correct and is due to the untimely death of its author John Gorgol. One can only hope that the development of ideas and models set forth in this book will soon be continued by others.

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PHELPS, THOMAS W., *100 to 1 in the Stock Market*. New York: McGraw-Hill Book Company, 1972, 288 pp., \$6.95.

"SMITH ADAM," *Supermoney*. New York: Random House, 1972, 301 pp., \$7.95.

If all the swingers in Adam Smith's world had read and digested Thomas Phelps's market tactics, that world of the stock-bond frenzy might be a different one. But if Phelps had read Smith's book — a modern counterpoint to the old *Wealth of Nations* — as he probably did *The Money Game*, he might have done some rephrasing.

Over the last ten years the swingers have sought stock market profits in every way possible, frequently with intemperance. This turbulent search is autopsied by Adam Smith as the reason for reconsidering the current expectations of growth in the availability of natural and financial resources. But while Thomas Phelps is content to re-examine Graham and Dodd's *Security Analysis* in the light of his method of finding the big winners, Adam Smith goes beyond. Smith credits that precious book too and then goes on to probe the reasons for the excesses. He chronicles those hectic weekends in 1970 when the marketplace stood on the brink, extends his story to the international front, and finally explains what is happening to the old capitalist spirit and why the future must be viewed with new eyes. If he is correct that capitalism, founded on the individual's wanting to maximize his profits, is on the wane, then Phelps sounds like the last cry of the Puritan Ethic.

100 to 1 in the Stock Market touts one investment strategy: "buy right and hold on." Trust your judgement in a big enough way to make it risky though profitable. Avoid time pressures; even buying low and selling high, swinging in and out of stocks, never makes as much money for the investor as buying right and hanging in there. Swinging does support your customer's man, the I.R.S., post office, telephone company and tranquilizer supply houses. But is that your reason for investing in the stock market? Or are you looking for more dollars?

Unfortunately this valuable advice to the public is bogged down in clichés, adages, "in other words." It doesn't rise out until two-thirds of the book is gone and by then some well-intentioned readers may have disappeared

into the quicksand. They may have gotten caught in the charts — perceptive and informative but poorly explained, or with a footnote to skip to Chapter VII and search there for chart explanations, when actually one only needed to hold out to Chapter V to understand what he had read back in Chapter II. Phelps spends so much time typing up the same information, reorganizing that same stock list of 365 “superstars,” that the reader turns impatient. Furthermore, if the reader had previously been exposed to a January-February 1964 article in the *Financial Analysts Journal* (“Optimum Investor — Stock Market Efficiency Standard” by Sherman Chottiner) he would have already been exposed to the brokerage penalty associated with trading.

Too far into the work Phelps explains the requisites and pitfalls in the selection of a stock which will grow into a 100 times winner. Look for a small start where geometric growth is possible, but not necessarily the stock under \$1. Beware of “accounting gimmickry.” Pay attention to a company’s price-earnings ratio and compare it to the P.E. ratio of the Dow Jones Industrial Average to discount current opinion of its anticipated growth. Study both fundamentals and technical information. Look for new technologies and “opportunities for profiting by capital leverage.” All this is good and true. But these are merely allusions to Graham and Dodd’s techniques, and overly simplistic ones at that. Here is nothing new for the seasoned investor while the examination of methods is too superficial for the novice, at whom this book appears to be directed.

Phelps’ style is reporter-like, reflecting his background as editor and his extensive investing experience. He wants his 365 stocks to speak for themselves, prove the point by amassing repetitive truth. His quotable personalities present reiterations, interjected in choppy style. There is no humor beyond the numbers.

Adam Smith’s style is, however, more lively. His approach is through personality and story, not just numbers. His interviews are humorous, build to a point, as with Babson’s chronicle of the “skulduggery” between 1967 and 1969. Smith concludes, “I have the feeling that in Babson’s part of New England the stocks they put you into for not having common sense are the wooden ones down on the village green that are very uncomfortable for the hands and feet.” His visual imagery lightens the way, as where in days gone by, “the lodge brother bought a stock, the transaction would be recorded in the back room by a gentleman wearing gym shoes and a jacket indicating the freshman basketball team of Cardinal Hayes High School, who would lick his pencil as he recorded the trade.” The very choice of a pen name — for J. W. Goodman surfaces on the copyright page — indicates the eco-oriented symbolism throughout *Supermoney*.

Smith covers many of the same points along the way that Phelps does. The market is “manic-depressive,” he writes. So look for “Intrinsic Value” and ignore the market. When he hears “Securities must be studied on a minute to minute program,” Smith’s friend Warren Buffett wrote, “Wow! This sort of stuff makes me feel guilty when I go out for a Pepsi.” While Phelps emphasized that computers are good in technical analysis and historical review, both authors view the computer’s potential of generating long-term stock market profits as limited. Smith’s discussion of Beta Theory, a coefficient of stock volatility and risk, is a contrast to Phelps well-studied investments.

In *Supermoney* Adam Smith begins by explaining where the real stuff comes from. "Supercurrency is capitalized income, not sheltered income." If M_1 is "all the coins and currency in circulation outside the banks, plus demand deposits," and M_2 "includes savings accounts and time deposits in banks," then M_3 or Supercurrency "is the before-and-after of a stock going public, in at book and out at market." Where to look for M_3 ? "There will always be somebody whose company has gone public and gotten listed who can peel off some of the stuff and spread it around." It is one way to have bought and sold right.

With M_3 around, what could be wrong? In 1970, reports Adam Smith, the system almost ran out of money — illiquidity. Brokerage houses were dangerously tied up in red tape and lost securities. In 1968, there had been a buyers' panic in the stock market, followed by the other kind, after which a lot of wiser guys were sounding like Phelps. "They would no longer have such a limited view of history (which) . . . said that you don't sell good stocks in a decline because they'll come right back." They would no longer go into illiquid situations.

"Smith" himself shows what can happen when this greed for Supercurrency, evident between 1968 and 1970, is extended onto the international front in "How My Swiss Bank Blew \$40 Million and Went Broke." He writes of a lively fiasco in which expectations are never realized. The hopes had been "our bank would prosper and grow, then it would offer some of its shares to European investors . . . then the shares would be traded . . . and we would have Supercurrency. We would buy other banks and other services with Supercurrency instead of cash, and any time one of the stockholders wanted a chalet or a new cuckoo clock he would peel off a few shares of our Supercurrency." But throwing it all away on cocoa (!) speculations foiled the scheme and proved Phelps' points.

But what of the whole market system? There is an economic and cultural framework within which Phelps conceived and wrote his book. Its origin is back in the early '30's — when it seemed nothing could get worse. Phelps learned to be personally financially conservative, while the economy went up, up, up, excelsior. Consumption, earnings, G.N.P., all up. He advises holding on to stocks on the assumption that linear growth will prevail, the stock market is an ongoing continuum. And here fall the difference between Phelps and Smith. Smith has recorded the excesses caused by the Supercurrency and its handmaidens and then supports his thesis that the game is changing. Once "displacement" was sufficient to maximize stock gains; if the Vega plant was plagued with narcotics addiction, sell G.M., buy syringe manufacturers. But, says Smith, the schism falls deeper than between the stocks. The "rationality" is going; the rules of production are changing. We must soon hit the end of the spirit of capitalism, "cowboy economics" and the "illimitable plains" of infinite resources. "For many the traditional motivations of job security, money reward, and opportunity for personal advancement are proving insufficient," he quotes. The threat of the depression no longer hangs over executive or production heads. So the productivity curve begins to flatten not only because we are becoming a service economy, but because some of the motivations — the spirit and the fear — have gone out of the producers."

Smith, with insight and breadth of references, goes beneath that market strategy of buy right, hold on, to question the fundamental approach to our

economy. With history and experience to guide him, Smith recommends an awareness of the changing times, although "awareness is not a traditional sensitivity in the rational preciseness of a game played with numbers." Phelps advises that the profits are there despite the economy and the market. All one must do is have "vision, courage, patience, extraordinary tenacity." It is difficult to trust to the sufficiency of these qualities if one has found Smith's arguments convincing. For while Phelps is content to review the history, Smith's text questions whether history once again will repeat itself.

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LARSON, RICHARD C., *Urban Police Patrol Analysis*. Cambridge: The M.I.T. Press, 1972, 289pp., \$12.50

A country physician in Idaho once claimed that most great surgeons do not have one or two major tricks in their bag but rather have a large collection of small techniques and tricks. Richard Larson has demonstrated in his book that he is a superior surgeon in dealing with some of the interrelated problems confronting a modern metropolitan police force.

Urban Police Patrol Analysis was started, in part, when the author was a victim of a grand larceny in the Fall of 1965. His ensuing (though unsuccessful) efforts to locate the larcenists gave rise to an interest in a specific problem area facing an urban police department: patrol analysis. This interest led to a Master's thesis and a Ph.D. dissertation (both M.I.T.) which concentrated on some vexing police "management" problems in the area. His employment with the New York City Rand Institute has permitted additional work in the area.

The book is divided into eight chapters:

- 1—System Description, Objectives, and Performance Criteria
- 2—Police Patrol Operations in a Hypothetical City
- 3—Travel Time Models
- 4—Models of Preventive Patrol
- 5—Patrol Allocation Algorithm
- 6—The Simulation Model
- 7—Evaluating Technological Innovations: Automatic Car Locator Systems
- 8—Intersector Cooperation and Repositioning

A substantial amount of the material in these chapters is not new. The author has incorporated in the text articles and papers that have been presented in both journals and private papers. In my view this is a strong point of the work since much of it has been subject to critical analysis and review prior to appearing in this book. However, the impression should not be made that this is merely a collection of previous efforts. There is new material but more importantly the entire work is integrated, logical, and forms a very cohesive, very readable volume.

The essential question raised by the author is: What assistance can be offered to a police commander in order to allocate a scarce resource (police controls) more efficiently? The direction of Richard Larson's extensive study is to show how the combination of computer-based quantitative models and experienced police commanders can improve the allocation and dispatching of patrols. A major interest is in improving crime protection and response

without sacrificing emergency service quality. The importance of this problem is obvious when viewing current statistics on crime and costs of supporting a patrol car for 24 hours in the field.

The continued use of relatively simple formulas by police departments, Larson argues, is insufficient to answer a host of important questions: What is the effect of eliminating a patrol car? Or of moving it to another precinct? Or of assigning it to a new patrol area? The author's approach helps to answer these questions. However, the police commander is still left in the traditional posture of having to take the results of the model and view them in light of his objectives—he must still make the decision. The hope of this work is that the commander will make better decisions based on better information and analysis.

Larson has been able to achieve several solid points in the presentation of his material. First, he has maintained the difficult balance between the qualitative and quantitative aspects of the problem. There is no sterile presentation of mathematical formulas nor is there a totally narrative discussion of different points of view or alternatives. Second, although many cases deal with hypothetical situations, problems, or model simulations, a surprising amount of realism is gained by frequent references to actual metropolitan areas (mainly Boston and New York) coupled with actual data from empirical studies. Furthermore, there is a very substantial introduction and use of police terminology which creates a sizable vocabulary on the part of the reader. Third, there is support for the material by numerous references to related works provided in the footnotes. Fourth, there is a range of quantitative techniques that commences with a negative exponential distribution of service time at the scene of an incident and becomes more sophisticated. However, the highest degree of sophistication is not extreme and is well within the bounds of both understanding and acceptance. Finally, there is an extensive glossary of terms and annotated bibliography at the conclusion of the text.

Hopefully this text will provide some of the analysis theory and format for many police departments. However, once analysis is started there will be a considerable lag before the full benefit is felt. Some of Larson's methods (for New York City) were developed in 1968 and 1969 but they have yet to be totally placed in use. That will require a year or more.

M.J.M.