Summary of the Papers

THE PAPERS in this issue of *Brooking Papers on Economic Activity: Microeconomics* have two main themes. The first five papers look at mergers, takeovers, and restructurings, and the implications of these for antitrust policy. Sanjai Bhagat, Andrei Shleifer, and Robert Vishny examine a sample of takeovers and mergers to assess the effect on employment and investment and to assess the extent of asset selloffs. Bronwyn Hall looks at the effects on research and development spending of these same changes and also considers the results for debt restructurings. Michael Katz and Janusz Ordover take up the same theme of R&D and technology development, but they consider the policy implications of joint ventures and consortia such as Sematech. Oliver Hart and Jean Tirole use analytical models to explain the motivations for vertical integration and whether there are likely to be efficiency gains or anticompetitive consequences from such mergers. And Michael Salinger considers the relationship between profits and concentration to see whether it is useful for antitrust policy.

The final two papers take up a different question. There is widespread concern over the low rate of saving and net investment in the U.S. economy, but in assessing this concern, there have been widely different estimates of the likely impact on productivity of changes in the rate of investment. The papers by Paul Romer and by Martin Baily and Charles Schultze look at some models and evidence that can help resolve these differing estimates.

Bhagat, Shleifer, and Vishny on the Effect of Restructuring

This paper looks at what happens to companies after they have been taken over, particularly when the takeovers are unfriendly. The authors examine 62 hostile takeovers between 1984 and 1986 in which the price paid or offered was more than $50 million. Not all the targets were acquired in the end—12 remained independent. The authors use multiple sources of data in examining their sample, including financial data from the securities industry, company reports, the 10K reports filed with the Securities and Exchange Commission, and reports in several business periodicals. They emphasize that the data are limited and that there may be effects of the takeovers that they are not uncovering.

When a hostile takeover is accomplished, there is a premium paid by the acquirer in addition to the market value of the company before the takeover bid. There is something of a puzzle in the literature as to why the acquiring companies are willing to pay these takeover premiums. The authors consider first differences in valuations by different parties as a possible cause of takeovers. For example, the market could be undervaluing the companies, so that takeovers occur to correct an undervaluation, not to make changes in operations. And, of course, a related possibility is that the acquiring companies are the ones making the incorrect evaluations: they may be overvaluing the companies they acquire. If this is the case, then the takeover premium received by the shareholders of the company being acquired will come partly out of the pockets of the shareholders of the company doing the acquiring.

In their sample the authors find that shareholders make substantial gains when the company they own is subject to a hostile takeover. The shareholders of the company doing the acquiring, on the other hand, have small losses (an average of $15 million on a purchase price of $1.74 billion). This means that takeovers are not purely redistributional. There are real gains from the takeovers, although on average they accrue to the target firm's shareholders. This result confirms the findings from earlier studies: hostile takeovers are typically followed by major changes in company operations.

The authors consider five possible ways in which an acquisition may raise the profitability of the combined firm. First, the acquisition may be made for strategic reasons. Acquiring a firm in the same industry will increase market share and may allow the combined company to raise its prices. The relaxation of antitrust rules in the 1980s could then be an explanation for the merger wave. Second, it could be that some companies have too many employees, and the managers are unwilling to take the necessary steps to reduce employment. The raider then lays off workers. Third, there may be gains involved in selling off parts of a company that has been acquired. The parts may yield a higher price in resale than they commanded as a single company. Fourth, there may be tax advantages from the takeover. Fifth, there may be changes made in the investment strategy of the firm. In particular, it has been argued that managers in some firms spend too much on investment in mature industries where the potential profits are not adequate. The raider then reduces investment or R&D to improve efficiency.

The authors look first at layoffs, measuring them as the sum of actual layoffs and early retirements from the divisions retained by the combined company. The actual layoffs are much the more important category, and the authors are able to separate out blue-collar and white-collar layoffs. They include all a firm's layoffs for three years after the takeover. The authors suspect that their estimates of layoffs may be too low: they ignore layoffs from the divisions of the acquiring company and they ignore those occurring in divisions that have been sold off. Also they rely on press or company reports of layoffs and may have missed some as a result. In one respect, however, they overestimate the effect of the takeovers. Some of the layoffs might have happened anyway.

The authors find evidence of layoffs in 28 of the 62 cases they study. Seven of these firms mentioned layoffs but gave no numbers. For the 21 target firms with specific figures, an average 1,262 employees were laid off, 5.7 percent of the work forces of these firms. This is a smaller percentage of the total employment of all the firms in the sample, of course. The authors find that most layoffs involve blue-collar workers, but the proportion of white-collar employment affected by layoffs is greater.

Having determined the number of layoffs that resulted from the takeovers, the authors assign a labor cost saving, using figures on wages and other compensation from the Department of Labor and other sources. The total labor cost saving is then based on the expected present value of the annual cost for each employee laid off.

Multiplying the number of layoffs by the cost saving per employee indicates total labor cost saving, an average of $37.5 million for those firms that did lay off workers. This was equal to 27 percent of the premium price paid for these firms. The authors stress the variability of their results across firms. Most of the firms in their sample had no reported layoffs, but some had labor cost savings larger than the premium paid for them. The possibility of reducing labor costs may be an important motivation for takeovers.

The authors also look at layoffs in companies that remained independent. Here layoffs were larger on average than for companies that were acquired. The companies that remained independent seem to have been forced to restructure their operations. Another interesting result comes from a comparison of white-knight takeovers and hostile takeovers. The authors find that white knights did not close headquarters; hostile takeovers did.

To summarize their results, the authors say that layoffs after takeovers are common and seem to be related to the takeovers, in that layoffs are much less frequent in a control group of firms not subject to takeovers or takeover attempts. Layoffs can account for between 10 and 20 percent of the takeover premium paid, averaging across all the firms in the sample.

The authors look next at the impact of selloffs of divisions after takeovers. Once again they consider a three-year period after the takeovers and restrict themselves to divestitures that were clearly from the company that had been taken over. They found that the average fraction of the acquisition price realized through selloffs was 0.3; the median was 0.17. There were no selloffs in about a third of the cases. In 17 cases, more than half the initial price of the company was reclaimed through selloffs, and in 3 cases more than 100 percent was recovered. Selloffs seemed more important for leveraged buyouts (LBOs) than for non-LBOs, indicating the pressure of debt service forces some sales.

The authors also look at who bought the divisions that were sold off and deduce the motivations for the asset sales and purchases. They find that the selloffs after takeovers are often to firms in the same industry as the division being sold. These transfers of assets increase the amount of concentration in markets. The transfers do not seem to be directed at improving performance by improving incentives within organizations. The authors note that, in their sample, of $65 billion in assets that were transferred, 72 percent ended up in the hands of companies managing similar assets.

The authors do find that tax savings have sometimes been an important motivation for takeovers. Takeovers can provide tax gains for companies with tax losses or for those that have a large debt-tax shield. There were 17 cases in which the tax advantages of the takeovers represented at least 25 percent of the takeover premium. The authors believe that their analysis cannot to do justice to the matter of tax advantages, but the advantages are probably as important as layoffs in the overall picture of takeovers.

In the case of investment cuts, the authors find individual cases for which cutbacks in investment were significant. The oil industry is one example. The companies had large free cash flows that they were using to finance oil exploration that had a rather low expected rate of return. The oil and gas pipeline industry and the lumber industry were others that showed significant investment cuts following takeovers.

In concluding, the authors say they have not yet been able to pin down the reasons behind takeovers, but they have come to several conclusions about the process. First, takeovers allocate corporate assets to firms owning facilities in the same industry. Second, the raiders or merger buyout organizations serve to broker transfers of assets rather than actually to change the management of the companies themselves. Third, layoffs, tax advantages, and investment reductions are all sources of the gains from takeovers, most of which accrue to the shareholders of the firms being taken over.

Several participants in the meeting were concerned about the accuracy of the journalistic accounts used to measure the extent of layoffs or other consequences of the mergers and acquisitions. There was also considerable interest in the paper's finding that assets had been reallocated to firms within the same industry. Some people felt that monopoly power must be important. Others disagreed and suggested that efficiency gains were involved. Lawrence Summers in his discussion suggested that the authors had understated the tax advantages of mergers, and he outlined some additional tax gains.

Hall on the Effect of Restructuring on R&D Spending

Bronwyn Hall starts her study by noting that there is considerable concern that the wave of mergers and corporate restructurings that has taken place in the U.S. economy has had a detrimental impact on industrial R&D. There has been slow growth of R&D spending in the 1980s. The National Science Foundation estimates that real R&D spending has grown at only 2 percent a year in the past three or four years, compared with 7 percent a year from 1979 to 1984. This paper asks whether this concern about the link from financial changes to R&D is valid.

One argument says that the concern is not valid. According to finance theory, changes in the way companies finance their investments should not affect the decisions they make about the kind of investments they choose. The optimal financing decision can be made independently of the optimal investment decision. This argument can be questioned in practice, however, because corporate managers may make decisions that do not reflect only the interests of the shareholders.

Shareholders do not have full information about the investment options open to firms, and they find it difficult to decide whether changes in the profitability of firms are the result of good or bad management or of the inevitable uncertainty associated with any investment program. One consequence of this lack of information is that managers and shareholders may disagree over a firm's strategy. Managers have direct control over the firm's decisions and can impose their own judgments about whether the level of R&D spending or other investments is correct. However, if the market decides the managers' decisions are incorrect, the value of the company's shares in the market will fall. The company will then become vulnerable to a takeover by a raider who can "correct" the managers' decisions.

Discussions of the impact of corporate takeovers or restructurings often take either the managers' viewpoint or the viewpoint of the market or the raiders. When the managers are viewed as correct, the market is considered myopic—overly concerned with short-term profits and not concerned enough with the long-term survival and profitability of the company. In this view, managers have to defend R&D spending against the market's myopia. When the threat of takeovers increased in the 1980s, companies were forced to adopt financial strategies that raised indebtedness in order to discourage takeovers. This made the companies more vulnerable to default if the economy turned down, so the managers cut back on investments in R&D or related areas where the payoffs were long-term rather than short-term. This view concludes therefore that recent changes in the financial strategies of corporations have had an adverse effect on R&D, and that this effect is to be deplored.

The alternative viewpoint stresses that mangers may make the wrong decisions. They can direct the cash flow generated by their companies into investments that do not promise an adequate payoff. Managers want to build their own empires and increase the size of their own companies. They will invest in R&D in their own companies, for example, even when the technological opportunities are not all that good. It is better for the economy if these funds are paid out to shareholders or bondholders and can then be reallocated to growing and more profitable companies. Corporate restructuring means that a larger share of the company's cash flow will be earmarked for debt service and cannot be used for empire building by managers. This viewpoint, therefore, also concludes that increases in debt will reduce R&D spending but views this as desirable.

These two viewpoints are difficult to separate in empirical analysis because both predict a reduction in R&D. But Hall suggests that it is possible to look for indirect evidence to distinguish them. Is the stock market generally myopic with respect to R&D spending? What kinds of projects are cut?

Hall argues that the evidence suggests the market is not myopic about R&D. One set of studies has found that when a company announces a major long-term R&D project its share value increases. This evidence is not conclusive, however, because it gives no indication that the increase is appropriate in magnitude. Another set of studies looks at the way in which the market values R&D relative to other kinds of investments. These studies find that the market does not undervalue the long-term risky investments in R&D relative to other kinds of investments.

In her study Hall develops new evidence on the implications of corporate restructuring for industrial R&D. She examines data on about 2,500 firms from 1959 to 1987 using the Compustat file. She looks first at what the trends in R&D and financial structure have been among the firms in her sample. Hall focuses on manufacturing companies that were in the Compustat data base in at least one year from 1976 to 1987 and finds that about 580 of these firms had been acquired by other public firms. Another 130 or so had gone bankrupt or were liquidated, and another 250 had disappeared from the file as a result of delisting from the stock exchanges or because of name changes. For the firms that had remained in the sample, Hall was able to determine how much restructuring they had undergone by computing the change in long-term debt for each company in each year relative to the total value of its debt and equity at the beginning of the year. Firms with changes in this ratio larger than 75 percent in a year were considered to have restructured during that year.

Hall turns next to a form of restructuring that has been much in the news: when a company goes private by means of a leveraged buyout, or LBO. She finds 80 cases in her sample that were clearly LBOs and another 180 that probably were, although they are harder to identify with certainty. She finds that the number and size of LBOs has increased in recent years but that the direct effect of this kind of transaction for the manufacturing sector has been exaggerated. The magnitude of the assets or employment in companies that were involved with LBOs is not a large fraction of the manufacturing total. And those companies that were involved in such transactions were not doing much R&D before the LBOs took place. This is to be expected, says Hall, because raiders do not target industries or companies with high R&D investments and rapidly changing technology. They are looking for mature companies with large stable cash flows. Hall concludes therefore that LBOs do not have a major direct effect on the determination of R&D investments in the U.S. economy.

Turning next to firms involved in mergers and acquisitions, Hall summarizes the results that she found in some earlier research on mergers and acquisitions. There, her results did not indicate that mergers and acquisitions lead to significant reductions in R&D spending. And in looking at the nature of the acquisitions, she found that companies tend to take over companies that are doing R&D of the same kind that they themselves are doing. This suggests that the acquisitions are motivated by a desire to increase the efficiency of the combined R&D operation or to acquire technology that has benefits synergistic with a company's existing R&D program.

A study by the National Science Foundation issued in 1989 had reached rather different conclusions concerning the impact of mergers and acquisitions, and Hall comments on why this is the case. The most important reason is that the NSF study looked at the amount of R&D performed whereas she was interested in R&D intensity, that is, the amount of R&D relative to the size of a company. There are significant size reductions following mergers and acquisitions, and R&D declines along with size. Hall's argument was that it does not decline disproportionately.

In this new analysis Hall reexamines this same issue to see if her earlier results are repeated for the more recent sample and finds that there is a small reduction of R&D intensity as a result of the mergers. Companies performing R&D cut their R&D intensity by about half a percent following an acquisition. Hall then explores whether this could be the result of the elimination of duplicative R&D efforts and finds evidence that seems to go against this idea. She also explores whether the reduction of R&D persisted or was reversed. She finds that it persisted.

Hall concludes by noting that the difference between the new results and her earlier results is that the nature of the mergers and acquisitions has changed. In the 1980s, firms making acquisitions are less oriented to R&D.

Hall looks next at the relation between leverage and R&D. Do companies that shift their financial structure toward greater debt rather than equity reduce their commitment to R&D? She reports strong evidence that companies increasing their leverage do reduce their R&D intensity, and this reduction reflects a long-term change. Moreover, the effects can be pronounced. For companies that increase their debt by amounts equal to 50–100 percent of the size of their capital stock (there are 220 such cases in her sample), R&D intensity drops between a quarter and a third.

Hall started out by pointing to the concern about the slowing in the growth of R&D spending among U.S. companies in the past few years, and the results of this study do indicate that increases in leverage may be responsible for an important fraction of this slowdown. For the 250 firms in her sample that had large debt increases, the drop in R&D associated with this decline is about $1 billion, equal to 2.5 percent of the total and a big enough change to affect the picture of aggregate R&D spending.

Hall gives an important reservation about her results that was also raised as a matter of concern in the meeting. It is not certain that increases in leverage are causing the reduction in R&D spending. The firms that decide to restructure may have been planning to reduce their R&D investment anyway. A firm in a maturing line of business may decide to change its corporate strategy, simultaneously raising leverage and reducing R&D costs.

Katz and Ordover on Cooperative R&D

Several studies have found that the social rate of return to R&D is much higher than the private rate of return. This means that there is a large gap between the private incentive to perform R&D and the social incentive to increase the total amount of R&D being done in the economy. When a single firm is deciding whether to develop a new technology, it does not take into account the fact that other companies, and ultimately consumers, will benefit from its activities. It looks only at the private return.

The gap between private and social returns can arise for several reasons. First, companies can often imitate a new technology without paying a royalty. Second, the profitability of one technology may depend on access to related technologies. Third, government intervention may have limited the ability of U.S. companies to cooperate in technology development. And fourth, a company may not be able to appropriate the surplus from its innovation even it it has a watertight patent or other protection.

The authors discuss this last point first. A system under which one company discovers an innovation and other companies are then licensed to produce the product is described by the authors as a form of ex post cooperation. And in theory such a system looks to be by far the best alternative for technology policy, preserving the benefits of competition while allowing the widespread diffusion of new technology. The way to close the gap between the private and social returns to R&D would then involve efforts to strengthen the protection of intellectual property. It might also be possible to give firms more leeway in the structure of licensing agreements, such as restrictions on how the patent is to be used.

The authors point out that the policy of strengthening the protection given to intellectual property has in fact been followed in the United States in recent years. In particular, the court of appeals has upheld 80 percent of the patent rights cases since 1982, compared with 30 percent before that.

Even though the idea of strengthening intellectual property rights looks like a good way to close the gap between the social and private returns to R&D, the authors argue that in practice this approach has problems. Even a watertight patent does not provide an adequate mechanism for rewarding innovation. First, the holder of the patent cannot charge different royalties to different users of the technology and thus charges a royalty that is too high relative to the socially efficient royalty. Second, by its nature information is a difficult commodity to trade. Its value is hard for the buyer to assess before the purchase, and the information is difficult to take back if it is simply loaned out.

These problems arise because even a watertight patent does not provide enough protection. Other problems can arise because such a patent, in a sense, provides too much protection. For example, restrictive licensing agreements could be a vehicle for anticompetitive behavior. Watertight patents can also discourage diffusion of technology by raising the cost of information. Policy toward intellectual property must both provide incentives for R&D and encourage the diffusion of new knowledge.

The authors conclude therefore that ex post cooperation, by means of patents and copyrights and licensing and royalties, is not a complete solution to narrowing the gap between the private and social returns to R&D. Other alternatives also have to be explored.

The first possible response is that direct subsidies can be given for R&D. Indeed this is currently done to a minor extent in the form of the R&D tax credit. But one difficulty is that a subsidy will not solve the diffusion problem. Ideally, new technological developments should be widely applied. A subsidy may increase the R&D done by individual companies separately and will reduce the gap between the private and social returns to R&D, but it does not provide the benefits of cooperation. The authors also note that a subsidy may not encourage the R&D that is most socially useful.

The second way to narrow the gap between social and private returns might be to allow companies more scope to cooperate in technology development, a form of ex ante cooperation. Consortia to perform basic research or traditional joint ventures with specific development goals have recently been the subject of an active policy debate that led to the National Cooperative Research Act of 1984. The authors note, however, that this act puts clear limits on the kinds of cooperation allowed.

Currently several pieces of legislation are pending that would provide greater antitrust protection for cooperative research, including the specific effort to encourage the development of high-definition television.

Katz and Ordover argue that because neither subsidies nor stronger intellectual property rights provide a solution for the problems created by technology development, the third policy option is worth exploring, and they turn to economic theory to see what it can say about the issues. They develop a simple model that shows the two sources of difference between the private and social returns from R&D. The innovating firm ignores the benefits to consumers from its R&D and ignores the effect on the profits of other firms. An industrywide coalition that performed R&D would avoid the second of these two effects, although in general it will perform too little R&D because it will ignore the first effect.

It is important to realize, however, that an industrywide coalition may perform less R&D than would be the case without the coalition. This is because R&D provides a competitive advantage to the firm performing it, and this motive is lost if there is a coalition. When some firms are part of a coalition and others are not also causes problems: the firms left out may be hurt by the coalition.

The simple model illuminates some of the issues involved in ex post cooperation but does not deal with others. Katz and Ordover, therefore, turn to more complex models, and the findings from these can be summarized as follows.

— When innovators are product-market competitors and intellectual property rights are strong, cooperative decisionmaking will decrease the incentives for R&D. If intellectual property rights are weak, incentives are increased. The authors stress that these findings ignore some potential anticompetitive effects of cooperative agreements and do not consider international cooperation.

— Ex post sharing of the results of R&D will widen dissemination and give greater incentives for R&D than ex ante cooperation.

— Ex post sharing of results can break down, particularly when secrecy is the main protection of intellectual property. There may also be inefficiency in ex post sharing of results if a firm that holds a patent bargains with potential licensees.

— When there are extensive technological spillovers, firms may not want to joint a cooperative research venture. They can do better by borrowing the technology once it has been developed.

Having come to some conclusions from their theoretical analysis, Katz and Ordover next analyze how cooperative research ventures have worked out in practice. Starting with projects that have been registered under the National Cooperative Research Act, they find that U.S. firms have not rushed to sign up for the protection given under the act. There were 159 registrations through the end of 1989, a small number compared with the total joint ventures undertaken by U.S. firms since the passage of the act (140 in the semiconductor industry alone). The act is not providing additional protection that firms consider very valuable. And firms may also be concerned about the information they must reveal when they register. The kind of joint ventures that were registered, the authors conclude, are not primarily ones where competitive spillovers are important. They also conclude that the ventures were not intended to retard innovation.

Katz and Ordover turn next to case studies of Sematech, the Microelectronics and Computer Corporation (MCC), and the Very Large Scale Integration (VLSI) consortium in Japan. These three are atypical of joint ventures in that there are many participants. They also involve research on computers and electronics, for which patent protection is not very strong, and were all responses to perceived international threats to the viability of the domestic industries. The research has been focused on developing knowledge that can be used for commercial products or processes rather than on developing the products or processes themselves.

With the VLSI project, the Japanese electronics industry attempted to catch up with the United States in the manufacture of 64K RAMS. The R&D goals were clear, and the technology had already been developed within the U.S. industry. Despite this, the project had difficulties getting started because each of the participants had knowledge it did not want to reveal and some of them had links to U.S. firms. In addition, the idea of joint research within the same laboratory was novel. Once the project did get started, however, it was successful and brought the Japanese companies up to the state of the art. Katz and Ordover credit the Japanese Ministry of International Trade and Industry with ensuring success by adding government funding support and by playing referee to encourage cooperative behavior by the participants.

Sematech was a response to the Japanese dominance of commodity RAMS—DRAMS and SRAMS. Fourteen of the largest U.S. chip producers participated, and the Department of Defense played an important role in the organizational structure that was established. Sematech also has links with the semiconductor material manufacturers, and both government and private funds have been committed to the program. Katz and Ordover conclude that, at first glance, the dangers from anticompetitive behavior are not great in the case of Sematech: the global nature of the industry means that it would be difficult for the companies to exert any monopoly power, even though most of the U.S. industry has participated. This conclusion has to be qualified, however, if the American companies are able to lobby for trade restraints.

The structure of Sematech makes it unlikely that it would be used to inhibit other R&D. The members are able to do any research they want outside the consortium, and several are working on parallel projects. There may, however, be some concern about the effect of Sematech on companies that are not part of the consortium.

MCC was organized in 1982 and began operations in 1983 as a response to the Japanese project to develop a fifth generation computer. The participants also thought that they could gain ground on IBM and AT&T, firms that were not invited to participate. The desire to share development costs rather than concerns about appropriability seem to have motivated the participants. Unlike the other two projects, MCC has a large independent staff; it is an independent research organization and has had considerable freedom in determining its research. But this freedom has created a problem because the staff has developed its own agenda, so that even though the organization has created several new technologies, they have not yet been incorporated into commercial projects. In response to these difficulties, MCC has restructured itself and is trying to become more relevant to commercial innovation.

Katz and Ordover conclude their study by pointing to the weaknesses of current theorizing in dealing with the matter of joint research. The case studies indicate that the internal organization of the joint research ventures has been very important to their success or failure, a subject on which theory has been weak. Theory has also been weak on antitrust policy because it has not been able to deal with the complex nature of technology development, particularly the relationship between firms and the downstream industries that they supply.

In the meeting, considerable opposition was expressed to the idea of encouraging U.S. companies to collaborate more extensively on research. It was feared that such collaboration would undermine the competitive environment of the U.S. economy and could lead to collusive behavior. Participants echoed the authors' conclusion that the theory presented in the paper had not been closely connected to the empirical case studies.

Hart and Tirole on Vertical Integration

Antitrust policy has historically been concerned with ensuring that an adequate number of companies are competing in the production and sale of a given product or related group of products. When two companies in the same industry or line of business merge, this horizontal merger may be reviewed by the U.S. Department of Justice to determine whether competition in the industry has been reduced to the point where consumers' interests are adversely affected.

The vertical merger of a company with a supplier does not directly lead to any change in market shares in either the upstream industry (the suppliers) or the downstream industry (the users), so antitrust policy has generally regarded such mergers as changes made to enhance efficiency rather than to increase market share or to gain the power to raise prices. Some vertical mergers have been scrutinized by policymakers or have involved legal action, but in general there has not been much opposition to vertical mergers. Some theorists have suggested, however, that the possibility of adverse effects from vertical mergers should be taken more seriously. In their paper Oliver Hart and Jean Tirole systematically explore this issue to find out the conditions under which vertical integration can lead to anticompetitive behavior.

The authors first set their framework of analysis. They note that in principle two companies may not actually have to merge in order to work together to increase profits. But in practice it is difficult to achieve cooperation without an actual merger, so vertical mergers are motivated by the desire for greater cooperation. Mergers have costs as well as benefits, however. Combining operations can be expensive and the merged operation may find it difficult to maintain incentives. Firms in the Hart and Tirole model, therefore, have to weigh these costs and benefits.

There are two upstream and two downstream firms in the authors' basic model. The two intermediate (upstream) firms' goods are identical, as are the goods of the two final (downstream) firms. The upstream firms both produce under conditions of constant unit costs, although these costs are not the same in the two firms. The first model that is examined using this basic framework shows how a downstream firm has an incentive to merge with the lower-cost supplier of intermediate products to restrict output and gain a higher price and additional profit in the downstream market. It is not hard, the authors conclude, to find examples where vertical integration can be motivated by the opportunity it gives to firms to reduce output and increase price.

Hart and Tirole then look at variants or extensions of this basic model. They note that there may be bandwagon effects: when one firm merges with its supplier, the other may have an incentive to merge with the remaining supplier to sustain its supply base. Then the authors consider what happens in situations in which the downstream firms bargain with the upstream firms over the price that will be paid for the intermediate good. In this case there is a new motive for vertical integration, even when costs of production are the same in the two upstream firms. An upstream firm may want to merge with a downstream firm to ensure a market for its product. The result will be a reduction in the profit of the other upstream firm, which could force it to leave the industry, creating a monopoly for the single firm created by the two merger partners.

The last variant of the basic model considers the possibility of capacity limitations. If the upstream suppliers have limited production capacity, a downstream firm may want to merge with one to guarantee its supply source.

Hart and Tirole next analyze the implications for welfare of these alternative possibilities. There are three possible ways the mergers described in the models could have adverse impacts on welfare. The first occurs when the merger leads to a restriction of output and an increase in price. The second applies if one of the firms that is not a party to a merger is pushed into leaving the industry. This then creates a monopoly that also restricts output. The third cost from mergers is that the mergers themselves are expensive.

There are also potential gains from mergers. When one firm leaves the industry, the total cost of production in the industry can be reduced by decreasing rent-seeking behavior. Mergers may also provide market guarantees that encourage investment, and these, too, will reduce overall industry costs.

Given these offsetting costs and benefits, the authors argue that it is hard to come up with clear-cut prescriptions for antitrust policy. Their theory can give some guidance, however, as to when vertical mergers are likely to be used to reduce competition. This is most likely to happen when the merging firms are large and efficient (have low marginal costs of production or low costs of adding to capacity) relative to the firms that do not merge. In such cases mergers should be subject to serious scrutiny by the antitrust authorities. Additionally, policymakers should look carefully at cases in which mergers are likely to drive other firms out of the industry. For example, if an upstream firm is the major source of supply to several customers and one of those customers proposes to merge with it, other competitors in the downstream industry could be hurt.

Having set out the theory, the authors provide three case studies to see if any of the predictions of the model apply in practice and whether the model can provide lessons for policy. The first study is of the cement and ready-mixed concrete industry. In the early 1960s a large number of vertical mergers took place, with the upstream (cement) firms acquiring the downstream (concrete) firms. This activity led to a report by the Federal Trade Commission on the industry in 1966. The cement industry produces a homogeneous output, and production requires large-scale operations, so that the industry was rather concentrated. There was some overbuilding in the early 1960s, leading to significant overcapacity by 1965. There were more firms in the concrete industry, but still the industry was fairly concentrated.

The wave of mergers that took place in the mid-1960s seems to have been motivated by cement producers' desire to obtain a guaranteed market. Once the mergers started, bandwagon effects were created as producers found it necessary to merge if they were to obtain a share of the contracting market. Some companies also dropped out of the industry after a large customer had been bought out. Hart and Tirole find several ways, therefore, in which the results from the case study correspond to the predictions of their model and thus support its validity. One observation from the cement industry that did not fit with their analysis is that after the mergers, many of the concrete producers continued to buy cement from companies other than their merger partners.

The authors suggest that this divergence between the theory and observation probably came about because, unlike the assumption of the model, in practice unit costs were not constant.

The second case study discussed the computer reservation systems operated by the airlines. The largest were the Sabre system of American Airlines and the Apollo system of United Airlines, but smaller systems also operated. By 1984, there were complaints that the systems were being used to channel traffic to the airlines operating them and away from airlines competing most directly with the host airline. The biases in the systems were partly monetary; the Sabre system, for instance, charged much larger fees to some airlines than to others. There was also bias in the order in which alternative flights were displayed to travel agents. In 1984 eleven airlines filed suit against American Airlines charging discrimination, and the Civil Aeronautics Board subsequently set up regulations for the operations of the computer reservations systems.

According to the authors, the CAB was responding appropriately to a threat to airline competition. An upstream monopolist had control over an essential element in supply (the ''essential facility''), and no partcular efficiency gains were likely to result from the vertical integration between the airlines and the reservations systems. Based on their analysis, the authors find that more market power was the motive for operating as an integrated unit.

The third case study goes back to the days when railroads were the main form of long-distance land transportation. In the early years of this century, the Terminal Company controlled a bridge across the Mississippi River at St. Louis. This bridge was the only way across the river at that point and was available at reasonable cost.

When the Terminal Company was acquired by a group of railroads, the United States filed suit out of concern that the vertical integration would be used to monopolize railroad traffic in the area. In its ruling on the case, the Supreme Court did not require that the acquiring railroads divest themselves of the bridge, but it did require that competing railroads be given adequate access to it. Based on their analysis, the authors argue that the Supreme Court was correct to be concerned about monopolization of the railroad market because it seems to have been the only plausible motive for the merger.

In discussing the paper, participants in the meeting commended the authors for developing a fascinating theoretical analysis, but they questioned whether the theory was quite ready for empirical prime time. The case studies concerned many firms, but the theoretical models discussed only two upstream and two downstream firms: it is very hard to go from two firms to many firms.

Salinger on the Profits-Concentration Relationship

Michael Salinger's paper starts, as Hart's and Tirole's did, with the idea that when an industry has too few competing companies, those companies can raise prices at the expense of the interests of customers. Antitrust policy must be a watchdog for consumers' interests. What has always been difficult, however, is to determine the point at which a given market allows anticompetitive pricing. Most markets have a substantial number of firms operating, but many of the firms may be small, so that the bulk of the market is supplied by a few large firms. Markets in which a large fraction of total sales comes from a few firms are said to be concentrated, or to have a high concentration ratio. The concentration ratio is often defined as the fraction of sales accounted for by the largest four firms, and this has then been used as a guide to antitrust policy. When the ratio is too high, based on some stated criterion, there has been either an active policy of deconcentration or restrictions on mergers.

Advocates of a vigorous antitrust policy argue that companies in concentrated industries routinely collude informally to raise profits and that active enforcement of the antitrust statutes is essential to protect the interests of consumers. These advocates claim that enforcement has been very weak in recent years and that the result has been an increase in collusive behavior.

The relationship between profits and concentration has figured in this debate. It has been found empirically that the greater the concentration, the higher the profits. And supporters of a vigorous antitrust policy argue that this evidence supports the position that companies are able to raise prices if only a few firms dominate a market.

The relationship of profits and concentration has played an important role in antitrust policymaking. In 1969 the Neal report used the relationship as a basis for its recommendation that there be an active policy of deconcentration in which firms that had market shares over 15 percent would be broken up when the four-firm concentration ratio in a market was over 70 percent. These recommendations never became legislation, but they did inspire the Justice Department and the Federal Trade Commission to bring charges of monopolization—including the IBM case and the case against the breakfast cereal manufacturers.

There have been two lines of criticism of an active antitrust policy and the use of the profits-concentration relationship as a guide for such policy. The first is to argue that the tendency for profits to be higher in concentrated industries merely reflects the normal working of markets and does not indicate the existence of monopoly power. For example, industries experiencing technological change will be above average in profitability and will be changing in structure. The firms that have been the most successful innovators or are run most efficiently will be growing, and the unsuccessful ones will be shrinking—concentration, in other words, will be rising. The observed relation between concentration and profits, in this view, is a sign of a dynamic adjustment.

A related argument can be used to extend this idea to the case in which there is stable market equilibrium. After adjustments have taken place, some firms will have long-run cost advantages. These firms will be large and will have above-average profits, but the profits are a result of the lower costs, not of monopoly power. An implication of this view is that small firms in concentrated industries will not earn above-average profits. Tests have indicated that indeed it is only the firms with large market shares that earn high profits in concentrated industries. The results of these tests have been very important in affecting antitrust policy in practice. It is now accepted by many economists that firms with large market shares and high profits are more efficient or technologically advanced.

The second line of criticism of the profits-concentration relationship is that profits cannot be measured accurately enough to make valid comparisons across industries. Thus the relationship itself is suspect and should not be used as a basis for antitrust policy.

Michael Salinger argues that the profits-concentration relationship is still important. First, it is still affecting the application of antitrust policy. Second, he believes, the relationship may provide a useful additional guide to policy, as well as the leading alternative, which is to look at individual industries in isolation.

Salinger argues that examining the profits-concentration relationship can illuminate the competitive structure of markets. His first point is an analytical one. He argues that the tests that find market share and not concentration to affect profits do not indicate whether markets are competitive. The tests do not rule out monopolistic pricing. Suppose one accepts the idea that in many industries some firms will have lower costs than others. These firms will tend to be large and have large market shares, but not all of the firms in the industry will necessarily have excess profits. There are many possible equilibria, says Salinger, where the dominant companies will act strategically, that is, adjust their prices and production to increase profits. If the large firms do this, small firms will enter the competitive fringe until they are once more earning a competitive return. But a new market equilibrium will have emerged in which the large firms are earning monopoly profits and the small firms in the competitive fringe are not. Salinger argues that when concentration is high, the dominant firms can be expected to behave strategically. On this basis, therefore, he argues that the profits-concentration relationship can still indicate the existence of noncompetitive behavior.

Salinger then turns to the empirical difficulties created by the endogeneity of concentration, that is, the fact that the degree of concentration may be the result of high profits in an industry rather than the other way around. There may be other factors in an industry that are leading to both high profits and high concentration—the nature of the technology, for example. One important other factor that Salinger judges must be considered is imports. He argues that high profits in an industry will lead to an increase in the level of imports.

Salinger turns next to the empirical findings. He uses data from the Census Bureau's panel and considers primarily the years from 1972 to 1984. For this period previous researchers have found a shift in the profits-concentration relationship. The impact of concentration on profits seems to have sharply declined or even disappeared. Salinger calculates measures of industry concentration that account for the share of imports in an industry, and once this is done, he finds that the profits-concentration relationship is restored. He concludes that the growth of foreign imports has been the reason that firms in industries that have a few dominant domestic companies can no longer earn high profits.

Having argued that the profits-concentration relationship remains intact once imports are accounted for, Salinger looks at one of the hypotheses about this relationship, namely that it reflects the dynamic adjustment of industries. He uses as a test the idea put forward by Sam Peltzman that in industries that are adjusting, there will be a relationship between changes in prices and changes in concentration. Peltzman has argued that major structural changes in an industry will cause large changes—either increases or decreases in concentration—and declines in relative prices. Salinger's results are consistent with Peltzman's idea. Changes in concentration do correlate with relative price declines.

Salinger also looks behind the correlation between concentration and price changes and finds a link to wages. His results indicate that wage increases are greater when concentration is increasing. Or to put it another way, the industries that have suffered declines in concentration (particularly where imports have become important) are those in which wages have increased more slowly.

Salinger does report results, however, that are counter to one version of the short-run adjustment view of the concentration-profits relationship. He finds that the extent of concentration in an industry in 1972 is important to profits in 1982. If the profits-concentration relationship is reflecting the fruits of successful innovation, he notes, these fruits are lasting a very long time.

The paper concludes with a look at the implications of the results for policy. The dynamic effects of competition, Salinger finds, do give rise to market power over some period of time. This is hardly a new conclusion, but one that he judges is being lost in the debate over whether markets are perfectly competitive. Antitrust policy is not now focused on deconcentration; there is little support for breaking up large firms. Policy toward mergers is relevant and Salinger argues that his findings do not support the hypothesis that mergers among firms in the same industry will raise efficiency. Structural changes in an industry resulting from a developing technology will lower prices and may increase concentration, but mergers will increase the monopoly power of dominant firms without generating any identifiable cost-reducing benefits.

In his comments at the meeting, Sam Peltzman noted that this paper was one of very few attempts to bring together the two main theories about concentration, collusion, and efficiency. Both he and Richard Caves pointed to the odd relationship among margins, concentration, and capital intensity that was showing up in Salinger's results. Many participants at the meeting questioned the validity of the profits-concentration relationship for policy analysis, pointing to measurement problems and the difficulties created by the dynamic nature of competition.

Romer on Capital and Productivity

Paul Romer starts his discussion by reviewing some of his earlier work, in which he argued that increases in the labor force might slow the pace of technological change and that increases in capital might speed it up. This study develops new theory and evidence on these questions.

Romer points out that technology is, in one important respect, a public good in that it has the characteristic of nonrivalry. When it is used by one firm, it can still be used by another firm without affecting the productivity of the first firm. The other characteristic of pure public goods is nonexcludability, and technology could have this characteristic too. All knowledge used in production could be derived from basic research funded by the government or as a side effect of other activities (Robert Lucas assumes that technology is produced as a side effect of education). If no one could be excluded from using technology, there would be no market incentive for R&D.

In practice, of course, some R&D is commercially funded, indicating that technology is at least partially excludable. Firms that perform R&D do get some benefit from it that other firms do not get. In the model he uses in this paper, Romer assumes that technology is generated by private firms, is nonrival, and is partially excludable. These assumptions are made explicit by assuming that research generates designs for producers' durable goods. Once these designs are completed, the goods themselves can be manufactured with a production function that has constant returns to scale. The factors of production are labor, the total of human capital, and the producers' durable goods that have been made in the past and are available for production. The cost of the design is recouped by charging a price that is higher than the production cost. The output being produced can be allocated one-for-one either to consumption or to units of producers' durable goods.

New designs for the producers' durables are produced by units of human capital, so that the rate of increase of the number of new designs in the economy is proportional to the amount of human capital devoted to the design activity (research).

Romer examines the nature of a balanced growth path in his model economy and finds the following results. First, the amount of human capital used in research increases with the amount of human capital in the economy—in fact it increases more than proportionally. This means, he says, that if two isolated economies are combined into one, the amount of human capital devoted to research will increase and so will the rate of growth. This comes about because the value of incurring a fixed design cost depends upon the size of the market. Researchers in separated countries may engage in redundant research efforts.

A second feature of the model that assumes a Cobb-Douglas production function is that the level of research in the economy and hence the rate of growth will depend on interest rates. Reducing the rate of interest will encourage research efforts and increase growth. By contrast, a subsidy to capital accumulation will have no impact on the steady-state growth path and neither will changes in the size of the labor force. These results are not robust, however, to changes in the specification. A constant elasticity of substitution production function will imply that if human capital and labor are complements in the production of output, then an increase in the size of the labor force will reduce the amount of human capital used in research and will reduce the long-run rate of growth of the economy.

Romer turns next to look at some evidence on growth patterns. He admits at the outset that the relationship between the variables in his model and the available data is not terribly close. In particular, the model deals with differences in the sizes of the labor forces, whereas available data are on rates of growth of the labor forces. This difference is not serious, he argues, because the same mechanism that causes an increase in the labor force to drive human capital out of research will also operate if the rate of growth of the labor force increases.

Romer first presents data on the relationship between the rate of growth of output per hour and the rate of growth of the labor force for the U.S. economy. Averages over twenty years are used to eliminate cyclical variations. He finds that higher rates of labor force growth are associated with lower rates of productivity growth. Romer judges that the causality here is that the major changes in labor force growth were the result of changes in immigration and demographics (notably the baby boom). His explanation, based on the modeling, is that during periods when the labor force is growing rapidly relative to the stock of human capital, a smaller fraction of the labor force is devoted to research. In terms of the recent U.S. experience, trained people have moved out of applied sciences and engineering and into law, medicine, and management. The difference between the rates of growth in Europe and the United States, he claims, supports his idea: there was a much slower rate of labor force growth in Europe.

Romer then turns to examine a regression of output growth on the share of investment in income and the rate of population growth in a large cross-section of countries. His model predicts, he argues, that when there are exogenous changes in the rate of technical change, the coefficient on the investment share should be in excess of one-third. If the variations in the investment share are independent of the rate of technical change, the coefficient should be less than 0.1. In his results the coefficient is in the range of 0.1 to 0.2, and the coefficients are highly significant. Romer concludes that both sources of variation are present.

The coefficient on population growth is surprisingly large, he finds, larger even than one would expect from standard neoclassical theory. The developing countries that make up the bulk of his sample have had higher rates of income growth and higher rates of population growth since World War II than was the case in the prewar period. This could reflect the impact of higher incomes on population growth rates.

In his conclusion Romer argues for the importance of finding a unified explanation of productivity behavior over different time periods and across different countries. His overall explanation is that applied research effort responds to incentives. An increase in the size of the market increases the incentive for research and hence the growth rate. For reasons that he feels are not clear, a higher rate of income growth also leads to a higher rate of saving and investment, but because this investment is not being stimulated by technological change, it results in a decline in the rate of return to capital.

Turning to the slow growth in U.S. productivity in recent years, Romer says a crude way of putting the problem is that in the United States there are too many lawyers and MBAs and not enough engineers.

Moreover, the investment tax credit is not going to help.

In commenting on the paper Zvi Griliches and Ernst Berndt commended Romer for his efforts to integrate growth theory with industrial organization. Charles Schultze and Martin Baily commented that while the empirical relationship between innovation and R&D has been demonstrated at the microeconomic level, it has proven hard to find a relationship at the aggregate level between the rate of productivity increase and the amount of resources devoted to technology development. And some participants doubted that the shift from engineering to law and management was really a response to the increase in the rate of labor force growth.

Baily and Schultze on Capital and Productivity

For the past few years the rate of net investment in the U.S. economy has been very low and the rate of national saving even lower. Many people, including Martin Baily and Charles Schultze, have argued that this situation should be changed. The drain on national saving coming from the federal budget deficit should be eliminated, thereby curbing foreign borrowing, lowering interest rates, and encouraging investment. In this paper, however, Baily and Schultze express concern that the productivity gains that can be expected from increasing the rate of investment in business fixed capital are being exaggerated. The case for increased saving and investment should be based on a realistic assessment of what this will accomplish.

They proceeded as follows: Has the experience of actual economic growth in the U.S. economy contradicted the simple growth model on which the standard estimates of the contribution of capital growth to productivity are based? What accounts for the different estimates of the contribution of capital made by Edward Denison and by Dale Jorgenson and his colleagues? Does the assumption of capital-embodied technology or imperfect markets lead to a change in the estimates of capital's contribution to growth?

The paper begins by reviewing the main empirical implications of the simple neoclassical model of economic growth, looking at the actual historical record of U.S. growth from 1889 to the present. The simple growth model provided the basis for the standard estimates of multifactor productivity (MFP) growth, which calculate the extent to which the growth of output exceeds the contributions attributable to the growth of capital and labor inputs.

The MFP calculations use the neoclassical model by estimating the contribution of capital to growth based on capital income's share in total income. And as a first test of this procedure, the authors plot the rate of MFP growth against the rates of labor force growth and capital input growth. They look at the nonfarm business sector and at manufacturing. They find no tendency for periods of rapid capital accumulation to coincide with periods of rapid MFP growth. This suggests that the neoclassical model is not underestimating the contribution of capital to productivity.

The authors do find a weak tendency for periods of rapid labor force growth to correspond to periods of slow productivity growth, as Paul Romer has noted in his paper in this volume and elsewhere. But Baily and Schultze do not find this to be a very robust result.

Much of growth theory has been concerned with the properties of steady-state growth paths, that is, where output is growing at a constant rate and the rate of profit on capital is constant. But Baily and Schultze find that these are forces that tend to push the economy toward a steady-state equilibrium; those forces abate very slowly. As a consequence, the historical variations in the rate of labor force growth and the rate of technological change that the economy has experienced have prevented it from achieving steady growth in practice. From 1948 to 1968, for example, the rate of capital accumulation was slower than the rate that maintains a constant rate of profit, so that the rate of profit was actually rising. Since 1968, however, a decline in the rate of technological change not matched by a corresponding slowdown in capital accumulation has pushed down the rate of profit. These findings, the authors argue, provide support for the view that there are diminishing returns to capital, an assumption of the neoclassical model that has been challenged by recent theorists.

Baily and Schultze also point to an implication of their findings for future growth. Unless there is an increase in the rate of technical change, the nonfarm business sector of the U.S. economy is likely to suffer from some combination of a decline in its rate of profit and its rate of output and productivity growth.

The simple neoclassical growth model assigns much of growth to an unexplained residual. This residual is called technical or technological change but in practice could cover many different things; it is really a measure of our ignorance. Baily and Schultze leave the simple growth model, therefore, and turn to a comparison of the frameworks used by Edward Denison and Dale Jorgenson. Denison and Jorgenson have developed analyses that have made progress in reducing the growth residual. However, these efforts have come to very different conclusions about the contribution of capital to growth, and Baily and Schultze ask why this is the case.

In Jorgenson's framework, growth in the capital input explains a very large fraction of the growth in output and productivity. This is not the case in Denison's framework. On the face of it, the difference in conclusions would seem to come from differences in the way capital is treated. Jorgenson and his coauthors, Frank Gollop and Barbara Fraumeni, use translog capital indexes that take account of differences in the rates of return to different types of capital; Denison distinguishes only three types of capital and aggregates them rather simply. In addition, the two approaches take different views of how the efficiency of capital declines over time. It turns out, however, that these differences in the treatment of capital do not account for much of the difference in the conclusions.

The main sources of the different assessments of the contribution of capital to productivity growth are that Jorgenson looks at gross output and uses the gross share of capital income to assess capital's contribution to growth whereas Denison uses these concepts net of depreciation. And Jorgenson includes residential housing and consumer durables in his analysis, which are very capital-intensive, whereas Denison excludes these sectors.

Baily and Schultze emphasize that they are not taking a position on which of these alternatives is correct. Their view is that the two frameworks have been used to answer different questions. They find that if one takes the Jorgenson framework and asks, for example, what the effect would be of an increase in nonresidential business fixed investment on the net output of the business sector, the answer will be very similar to the answer obtained from Denison's framework.

Baily and Schultze next consider the extent to which the contribution of capital growth to productivity should be reconsidered when technology is embodied in capital. This is a question that has been studied in the past, and the surprising result that has emerged is that capital's contribution to steady growth is not larger in this case than it is when technology is disembodied. However, embodiment can make a difference to an analysis of growth in economies that start out well below the technological frontier. In this regard the authors look specifically at the convergence of the European and Japanese economy to U.S. levels of productivity.

There is a very strong relationship between the share of investment in output and the rate of growth of output or productivity among these countries. This evidence has been used to suggest that increased investment in the United States would give a bigger boost to productivity than is suggested by the neoclassical growth model.

Baily and Schultze note, first, the objections that have been made to the idea that the economies of the world are converging to the same level of output. Critics point out that most countries are not in fact converging. Poor countries are remaining relatively poor. Baily and Schultze concede that the convergence model cannot be applied to all countries. But the reason, they argue, is that there are various factors in developing countries that inhibit growth and that the convergence idea is valid for Europe and Japan.

Under the convergence hypothesis, European and Japanese economies were able to achieve rapid growth because there were technologies available to them that had already been developed in the United States. The high rates of investment in these countries were part of the transmission mechanism by which new technologies were brought into their economies. Technology was, to an important extent, embodied in capital; investment did have a very high payoff. But this same payoff could not be expected for the U.S. economy, which was on the technological frontier. And as Europe and Japan approach the U.S. level of productivity, one can expect to see, and indeed does see, a decline in the payoff to investment in these countries.

The final reason for reassessing the role of capital in growth is the lack of perfect competition in labor and product markets. The neoclassical growth model assumes that wages and profit rates are determined competitively—indeed this is the basis for using capital and labor shares of income to estimate the contributions of these factors to output growth. Baily and Schultze concede that in practice markets are not perfectly competitive. In fact, in their assessment of the neoclassical model they find some evidence that this assumption of the neoclassical model is incorrect. But they point out that imperfections in product and labor markets will have offsetting effects. In product markets, imperfect competition means that measured capital income will overstate the contribution of capital to growth. In labor markets, wage premiums achieved by groups of workers will mean that labor income will overstate the contribution of labor to output growth and hence lead to an understatement of capital's contribution. The authors review the analyses of Robert Hall for product markets and Lawrence Summers for labor markets and conclude that there is no clear case for using a figure for the elasticity of output with respect to changes in capital that is very different from its income share. There may be a more important effect of imperfections on the allocation of resources by industry. They do, however, agree on the need for additional research on the association between industry wage premiums and the degree of capital intensity noted by Summers.

The authors' conclusion is that standard estimates of the contribution of capital to the growth of output and productivity remain valid. Any increase in the share of output that is devoted to the accumulation of nonresidential business capital will have only a modest effect on productivity in this sector. Despite this, the authors say that these modest gains are still important. The gains in productivity from all sources are very modest at present, and increasing the level of national saving and investment will lead to permanently higher levels of productivity.

The authors were criticized in the meeting by Dale Jorgenson for supporting an outdated approach to capital aggregation, an approach that omits the "most important impact of higher rates of investment, namely, substitution among different types of capital." Jorgenson pointed to the analysis in the book he wrote with Frank Gollop and Barbara Fraumeni to support this criticism. Edward Denison criticized the authors' discussion of capital embodiment and convergence. In his view the convergence of the European economies and Japan's economy to U.S. levels of productivity resulted from reductions of inefficiency in agriculture and small business and the economies of large-scale production. The authors responded to Jorgenson's criticism by noting that they had based their conclusions on the data and results in the Jorgenson, Gollop, and Fraumeni book. They felt that Jorgenson had misunderstood their intention, which was not to support one treatment of capital over another. They had found that the alternative analyses give very similar estimates of the contribution of capital to productivity growth if the same concepts of output and capital are used.