

Comments and Discussion

Charles Schultze: Both of these papers have two parts. The first part of each deals with the competitive structure of the industry. The second parts are empirical, and I shall devote most of my comments to the empirical results. I will talk first about the paper by Erickson and his associates.

The U.S. definition of oil reserves is essentially a very conservative one. Reserves are the oil underlying wells already drilled. This isn't the precise definition; but, for all practical purposes, development has to be undertaken, and production wells have to be sunk, to "prove" reserves. As a consequence, Erickson is right to treat his reserves as a capital stock variable.

In Erickson's model, it takes a higher price to induce producers to hold more reserves—that is, a higher capital stock. He fits a lag model to this basic conceptual structure. But it seems to me that this procedure misses the point. Changes in production are principally due, not to variations in reserves with other factors held constant, but to scale movements in which the ratio of production to reserves is relatively unchanged. Variations in marginal costs must be explained basically by changes in the cost of finding and developing reserves.

Let me explain the process. A recovery ratio applied against discovered oil-in-place describes the proportion of that oil that can be extracted—that is, the proportion of oil-in-place that can be turned into proved reserves. The recovery ratio is then the relationship between the amount of oil ultimately developed as reserves and the amount of oil-in-place. It has been rising because of newer, more advanced techniques of recovery. In turn, a fraction of the stock of reserves is converted to production each year; currently, annual production is about 12 percent of reserves.

Most changes in reserves, and in production, come from a scale movement, in which the ratio of reserves to production is held constant. In the last fifteen years, the ratio of annual capacity production to reserves has stayed in a range of about 10.8 percent to a little over 12 percent. Erickson, in fact, interprets the results of his model as a scale movement when he says that (with a unitary price elasticity of reserves) "a 10 percent increase in price results in approximately a 10 percent increase in discoveries, reserves, and production." But the essential problem of a depletable resource is the cost of developing reserves. One might conceptualize oil production as coming from two industries: the first develops reserves, and the second produces oil from the reserves. The oil-producing industry can double output at a constant price if it can double reserves (and double other inputs) without having to pay a higher price for the new reserves. Only if the "reserve-producing" industry cannot develop reserves at a constant price, because of higher finding and development costs, will it have a rising supply curve. If this is true, the appropriate variable on the left-hand side of the estimating equation is discoveries (that is, annual gross additions to reserves), not the net stock of reserves. In fact, Erickson and Spann used such an approach in an earlier article.

As production takes place, existing reserves are depleted; maintaining production and the net stock of reserves requires continuing development of new reserves. If the depletion of original oil-in-place forces development of reserves from increasingly less favorable prospects, then the finding and developing costs of new reserves to replace depleted reserves will tend to rise. The shape of the supply curve for oil depends basically, then, on the extent to which the costs of developing new reserves rise as the cumulative volume of discoveries grows over time. Holding any given stock of reserves would cost more and more, because "replacement costs" steadily increase. If, as Erickson's equations imply, it costs more on average to hold a higher stock of reserves, then holding the same stock will also be subject to increasing costs as the ultimately depletable amount of original oil-in-place is discovered and developed.

Therefore, for the long run at least, a model cannot be consistent if it states that a higher stock of reserves will be held only in response to a higher price but that a constant level of stocks can be held (with a constant rate of annual depletion) without a rising supply price. The rising cost of developing reserves, which explains the rising supply price of higher stocks, will also raise the price for a constant level of stocks and production.

In summary, the basic question is the cost of developing new reserves. In terms of projecting the costs of Project Independence, I am not willing to accept the Erickson approach—fitting reserve stocks against price and user costs—because it omits finding and developing costs.

One other problem is the implication of Erickson's use of the prior period's reserve stock in a pooled time-series and cross-section model. The coefficient on that lagged variable may be distorted by major geological differences among regions: the District of Columbia does not have oil reserves as large as those in Texas. Even if the marginal costs were the same at the intensive margin, average costs will be quite different and will lead to different reserve development, depending on geology. Clearly, the best statistical "explanation" of why reserves in Region 1 and Region 2 differ in period t is the difference in those reserves in period $t - 1$. I suspect, in other words, that the lagged term picks up more than a lag. It picks up some geological differences, even though Erickson also uses regional dummies. I am not quite sure what interactions may be present in an equation that includes, along with regional dummies, a variable that itself is a good proxy for regional dummies; but I suspect a possible overestimation of the coefficient on the lagged term.

In turning to the paper by Davidson and his associates, I want again to talk initially about the empirical results and their theoretical basis. The interesting part hinges on the estimate of the elasticity of supply. While several hypothetical estimates are given, the authors cite 1.6 as the value corresponding to the empirical findings. I want to consider that estimate because it is critical to the whole price-quantity projection.

Davidson and his associates apply an ingenious technique which uses the rent share to estimate supply elasticity. They assume a constant-elasticity production function, in which nature is the fixed factor and all other inputs are the variable factor. Then if one calculates the rent share, α , appropriately (amortizing those kinds of rents that are paid in advance) $(1 - \alpha)/\alpha$ is the supply elasticity.

In the first place, even if the elasticity of supply is correct, the equation has no lag. It is a timeless function. The authors could just as well put their 1980 results into 1974. Let me illustrate what is implied by their estimate of 1980 supply. With a price elasticity of supply of 1.6 and a demand elasticity of 0.5, they project domestic production of 16.9 million barrels a day in 1980. (I assume that Alaska is included in this total, so they are really projecting 14.9 MMB/d, or 5.4 billion barrels a year, for the rest of

the country. If Alaska is excluded, it makes my point even stronger.) At a production-reserves ratio of 13 percent, which allows for an increase from the current ratio of 12 percent, this projection implies that, between now and 1980, the United States will have developed a stock of 41.8 billion barrels of reserves, as compared with 25 billion to 26 billion at the end of 1973. In order to get this kind of development in net reserves, allowing for the fact that reserves converted into production must be replaced and allowing for just a little lag, annual gross additions to reserves must rise from 2.1 billion in 1973 to about 9 billion a year by 1979.

In order to expand reserves that much at a time when drilling depths are increasing, total drilling must expand even faster. Annual drilling may have to quintuple to achieve the Davidson results by 1980. Quite apart from the basic theoretical structure of the model, therefore, it is critical to inspect the lags before projecting results to any given year. Their result may be possible, but it depends on a massive increase in the rate of annual additions to reserves in a very short time.

In my view, Davidson's paper shares a basic problem with Erickson's in assuming that oil is like wheat. The ultimate stock of oil-in-place is treated like land. When more variable factors are applied, marginal cost rises, and generates rent. But this does not seem to me a fruitful concept for modeling depletable resources. It assumes, for example, that output could be continued at the same level at constant cost, unaffected by depletion of resources. In a depletable-resource industry, the same factors that imply a higher supply price for a higher output would also imply rising costs for a constant output unless they were offset by something like improved technology.

If the various discoverable oil prospects are of roughly equal quality, then long-run marginal costs would be constant. There would be no diminishing-return rents, although there might be scarcity rent if a sharp upturn in costs is expected at some future date. If, on the other hand, the quality of resources in place yet to be discovered is not uniform and geological science provides some advance knowledge of quality, then the long-run marginal costs of discovering resources will be rising. This rise may be offset by improved technology, or may be interrupted by surprises, like the opening of production offshore or in Alaska. Even with such offsets, it remains improper to treat a depletable resource like oil as a renewable resource like wheat.

Let me move on to the next point. To get the elasticity estimate, Davidson

and his associates use statistics for the federal outer continental shelf since, as they say, the largest growth in U.S. production will come from there. Now the outer shelf accounts for about one-eighth of oil and condensate production. They calculate the rent share of the value of output of offshore oil, take $(1 - \alpha)/\alpha$, and get their supply elasticity. But offshore oil fields yield a differential rent; and that is inconsistent with the assumption of a homogeneous fixed factor that underlies the rent-share method of estimating elasticity. Using their techniques, annualizing the lease bonuses, and spreading them over time, I calculated the rent share for onshore oil. I find a smaller rent share, and therefore a *higher* elasticity—2.4 compared with their 1.6. I suggest that that is an absurd result. The elasticity must, in fact, be a good deal higher for offshore than for onshore oil. Even accepting the rent-share technique, I see a major problem in trying to use it on data from a particular segment of the oil industry, particularly when that segment happens to be very flush and generates high differential rents.

Let me discuss briefly the problems with any econometric approach to estimating the cost of Project Independence and the future supply price of domestic oil production. Recent important developments have affected the supply price of domestic oil resources in opposite directions. First, the development of primary reserves—that is, reserves recoverable with natural-drive techniques—has been declining sharply, and costs have been rising. According to my estimates, primary reserves declined from about 25 billion barrels in 1959 to 19 billion in 1970, and to 15 billion in 1973. Discoveries of primary reserves fell from an average of about 1.7 billion a year in 1959–62 to about 600 million in 1973.

I estimate that the cost of developing a barrel of primary reserves has increased from about 88 cents to \$1.68 from 1959–62 through 1972. Using a 10 percent after-tax rate of return, that raw cost of developing a barrel of reserves translates into a rise from \$1.76 a barrel to \$3.36 a barrel in the cost of producing oil at a fixed (11½ percent) decline rate. Given a declining real price of oil, primary-reserve development fell very sharply. The rise in cost would have been even larger had not Texas and Louisiana eliminated the restrictions on production based on the market-demand factor. That action allowed annual production to rise from an average of 8 percent of reserves to 12 percent of reserves. At a 10 percent after-tax discount rate, the ability to recover oil faster was worth about 80 cents a barrel. So the rising cost has been partly offset by the removal of market-demand factors.

Second, there has been a huge growth of secondary production, which applies water-flood techniques to old fields, thereby increasing reserves and production. The marginal cost (at a 10 percent rate of return) of developing secondary reserves from existing wells has been only 40 to 55 cents a barrel, plus 15 cents a barrel because secondary costs are higher than primary costs. So a fairly substantial decline in development of new primary reserves was partly offset by the rise in secondary recovery. Unitization has also decreased the cost of developing and operating wells, and the opening up of offshore leases has provided new territories for development. A host of factors has affected costs. Some of these are temporary, some are not, but all are very difficult to pick up in any reduced-form econometric model.

One final point. There is an alternative explanation of the large decline in primary reserves, which fits in, somewhat, with the thesis of Davidson and his associates. Suppose I find a very large new field of oil. My geologist tells me there is a 99 percent chance that it contains a lot of oil. But, as I noted earlier, only as the property is developed by sinking production wells will reserves be generated.

Over the past ten years annual expenditures in constant dollars for exploring for new fields have been roughly constant. But the amount of development drilling—that is, developing the oil-in-place—has gone down steadily. Let me offer a hypothesis. Exploring for oil in the kind of situation prevailing during the last fifteen years is an overhead cost. A certain amount of money each year goes into exploration, depending on the price and all the other factors. But firms may or may not undertake the major expense of development—that is, drilling development wells for oil already discovered. The apparent rising costs and the reduction in output may result, not from withholding production from existing wells, but from failure to develop the oil already discovered by exploratory drilling. But since the oil hasn't been developed, it isn't counted in the reserve statistics.

Finally, Davidson and his associates make much of the point that, starting after 1970, statistics on offshore oil show a sharp increase in the number of shut-in wells. This is their major indication of speculative withholding. Now, it turns out that in December 1970, offshore producers went from state regulation in allowables to federal regulation, which allowed more production per well. Between 1970 and 1973, the number of wells decreased by 30 percent, but production rose by 7 percent. Production per well went up by almost 40 percent. My interpretation of that is straightforward. Because of the nature of state allowables, it paid to drill marginal

wells, but the same amount of oil was obtainable out of a smaller number of wells under the federal allowable schedule. It may be that producers in the last five or six years have been finding oil that they have not developed. And that may be a much more pervasive kind of speculative withholding than shut-in wells.

Robert E. Hall: I shall discuss mainly the paper by Davidson and his associates. It begins with a critique of free markets, and argues that the market cannot be trusted to do anything right on oil. It builds a pretty strong case for some kind of government intervention, possibly including price controls. I find that argument unconvincing.

The efficient market solution can be derived only from a very complicated intertemporal model, with lots of special assumptions, including the operation of forward markets. Clearly, not all of those assumptions are met; Davidson reasons, therefore, that all of the conclusions that follow are wrong.

Of course, the conclusions could be right even though the assumptions and the argument are wrong. Efficiency really depends, not on the detailed assumptions, but on the economic principle that no opportunity for profit remains permanently unexploited. Davidson's paper offers no diagnosis of a failure of this economic principle, and hence makes no case, as far as I can see, that a free market does not perform reasonably well. There is a lot of slippage in its performance, but not enough to justify reliance on price controls. I see a plain element of perversity in controls as they have been, and are now, administered with regard to U.S. petroleum.

According to the study by William Nordhaus (*BPEA*, 3:1973), the efficient competitive price of petroleum should be less than \$2 a barrel. In fact, the price is at least quadruple that. If the free market is doing its job, how can the price be so far above the Nordhaus prescription?

The first reason is monopoly, which is excluded in the Nordhaus diagnosis. What we have is a monopoly or some kind of highly effective cartel, operating with a competitive fringe. This means that the monopolist sees as his demand function the difference between the market demand function and the competitive supply function. The existence of a competitive fringe in petroleum is demonstrated in the Erickson paper. There is no significant barrier to entry in this business, and hence production will rise up to the point where the competitors' marginal cost equals price. The problem is that monopolists control such a large share of the market, especially through

the multinational companies, that the price is still much higher than their marginal cost. The major U.S. companies control about 50 percent of U.S. petroleum, and that portion may be part of the monopoly supply that is set against the demand function that OPEC and the majors face. As a result, the monopolists can drive the prices way up, even though the competitive fringe raises its supply up to the point at which its marginal cost equals price. Despite some competition, monopoly is able to quadruple the price compared with what Nordhaus estimates it should be.

It follows that the nation needs to do something about monopoly in the oil business. I endorse that part of Davidson's policy prescription—to put as much oil as possible under diversified ownership and, in particular, to keep as much as possible out of the existing cartel.

I am very skeptical about the alleged effect of speculation that Davidson and his associates stress so much. I strongly doubt that, in a competitive market, a speculative bubble could develop and no one would foresee its eventually bursting. I see no evidence to support the claim that even competitive suppliers are now withholding output because they believe the price is going to rise further. I believe that the competitive suppliers are now riding up a very steep short-run marginal cost schedule. They are producing at marginal costs equal to the high price, and there is no speculative element in that. I can't accept the whole business about speculation with a capital "S"; I have never found it a convincing part of Keynes, and I find it a no more convincing part of Davidson.

The authors propose to solve this problem with price controls even if the market is made competitive. They offer the ingenious suggestion that speculation can be broken by imposing price ceilings that rise less rapidly than the interest rate. But then speculation against the removal of price controls becomes the real danger. People cannot conceivably be convinced that price controls will last forever. Hence, controls will always generate speculation against the probability that, because of shortages, they will break down and be eliminated.

The amazing conclusion of this study is that Project Independence will bring oil prices down close to their pre-embargo levels. As Davidson recognizes, that conclusion is in complete conflict with the study at MIT that I have been involved in, which estimates that Project Independence will result in oil prices of about \$12.

As he points out, part of this difference is that we at MIT treated 1973 as a point of long-run equilibrium. That assumption on our part seems

quite clearly wrong. But I doubt that much of the discrepancy can be explained by taking 1971 as a more plausible point of long-run equilibrium. It would take a lot of work to identify the elements on both the supply and demand sides that account for the large discrepancies between the findings of this study and of previous studies.

The elasticity estimates themselves do not seem to explain the difference. Like Charles Schultze, I am uneasy about the notion of a fairly simple static supply function for petroleum. There is very high substitutability between the supply of petroleum today and the supply a year from now; the intertemporal cross-elasticity must be at least 10, and might be as high as 50. A one-year supply elasticity is not the relevant number for this analysis. Much more thought is needed to develop an intertemporal supply function that takes into account the very high cross-elasticity.

I have a few comments on the paper by Erickson and his associates. I concur in their conclusions about the competitiveness of the oil market. In particular, I agree that special tax provisions do not have very much to do with the competitiveness of the industry; in fact, I am surprised that the notion that they do is so hard to dispel. Also, I find their estimate of a supply elasticity a little below unity more acceptable than the estimate Davidson uses.

But percentage depletion presents a problem. I thought the reason that some of the international oil companies were willing to give up percentage depletion is that, after deduction of the royalty payments they make to foreign governments, nothing is left over to be taxed. Apparently, oil companies are very successful in assigning their costs to their foreign operations. Hence, the added benefits of percentage depletion may be small; even without that provision, they might still pay essentially no taxes. I doubt that the treatment of depletion in the formula for the user cost of reserves takes appropriate account of this fact.

Paul Davidson: Let me start with a response to Charles Schultze's question. He sees a problem in applying a Marshallian analysis to oil production over time. He argues, in essence, that maintaining a constant production flow and replacing reserves tend to shift the long-run supply curve up over time, contrary to our model. It seems to me, however, that Schultze presented some good reasons why supply was shifting down over time, such as the introduction of secondary recovery, unitization, and offshore drilling.

In part, this is an answer to Hall's question about our differences with the MIT study. That study may have suggested that supply was shifting up over time. Historically, I find very little evidence of a major shift in either direction, certainly not over a decade. In the absence of such evidence, I think it is fair to argue that the supply curve is pretty stable.

The search for reserves does not have to yield diminishing returns. The oil industry does not necessarily develop the least-cost oil reservoirs first. The company may take leases to block competitors from getting to these reservoirs, and then just sit on them as long as they can. Ultimately, they have to decide to drill somewhere, and then they may arbitrarily drill on Block A and not on Block B, even though the anticipated costs of finding might be just as low on Block B. So I don't think that the marginal cost of finding reserves is going to go up very rapidly, although I would concede rising costs if we were considering a period of a century, rather than a decade.

Schultze suggested that the large number of shut-in wells developed suddenly when federal prorationing replaced state prorationing. But why should that in itself cause producers to shut down wells? Offshore wells are efficient, and are not offset wells. The amount brought up in one well affects neither recovery nor the marginal efficiency of another well. Producers could have brought up twice as much oil once they were free of state controls. They didn't. They shut in those wells, and held up the price of oil in the absence of state regulation. In terms of the bottleneck question, it is important to recognize that these shut-in wells are already available for production. It merely requires someone to turn them on. They could produce significant quantities without any delay for exploration or development.

Let me turn to the important question of reserves. I know that some companies have two sets of reserve statistics—one that is reported to the industry and a higher one that is used for internal planning. This assertion is very hard to prove about oil reserves, because the producers are not required to file reports with a public agency.

One of the benefits of natural-gas regulation, however, is that the producers are required to file reports and hence the assertion of underreporting of reserves can be documented. In testimony before the Senate Subcommittee on Integrated Oil Operations (December 13, 1973), FPC economist David Schwartz provided substantial evidence of underreporting of re-

serves by many gas producers. In one of many examples he cited, one company reported that, as of June 30, 1973, it had zero uncommitted proved reserves in a particular offshore area. A few days later the company entered into an interstate contract to sell natural gas from that property and therefore applied for certification with the FPC. On July 5, five days after reporting zero uncommitted reserves, the company's application for certification estimated the proved reserves to be 3 billion cubic feet per year for the next twenty years.

This is just one of many documented instances that suggest that the statistic on proved reserves is a very unreliable indicator of natural-gas reserves. The problem may be as bad for petroleum, but that cannot be demonstrated because the data are collected by the industry. On the other hand, production statistics—particularly for offshore oil, where the government is the owner—are reasonably accurate.

Let me respond to a few of Hall's comments. I cannot accept his conclusion that profit opportunity brings about efficiency in oil, because I believe monopolies prevent that result. There are differences between a joint monopoly and pure competition. The existence of a brand-name premium in gasoline until a few months ago suggests that even the retail market is not purely competitive. Certainly at the wellhead there are serious departures from pure competition despite the ability of firms to enter as strippers. The scope of the competitive fringe can be overestimated.

As for price controls, if they are announced as temporary and are administered by people with no faith in them, they clearly will encourage user costs. But price controls can work if people are convinced that they are permanent. And I think they can be convinced of that if the government is clever and has regulators who believe in regulation.

I agree with Hall that the cross-elasticity of supply between alternative years is high. That implies that user cost can be fantastically high in this situation. Hence, in order to have stable production flows, some policy must be devised to convince producers that they cannot profit because of user cost.

Laurence H. Falk: I would like to comment on Charles Schultze's assumption of a constant ratio of reserves to production. I see no reason at all why the ratio should be constant. According to the U.S. Geological Survey, producible U.S. reserves (prior to the recent price rises) may be as high as

500 billion barrels. Compare that with the 36 billion barrels of proved reserves currently reported by the companies. Proved reserves can be altered very suddenly, thus changing the reserves-to-production ratio.

We don't know where big oil deposits are located. Before the Ghawar field in Saudi Arabia was discovered, no reserves existed there, and no structure there was identified by geologists as a particularly good prospect. That discovery suddenly increased world reserves by more than twice existing proved U.S. reserves. As this suggests, the reserves-to-production ratio can change dramatically with new discoveries. And the ratio will always appear lower than it actually is because of underreporting.

Edward W. Erickson: I want to support some of the price estimates made by Paul Davidson and then qualify that support substantially. I participated in an effort to predict average bonus bids for three offshore lease sales, two in 1972 and one in 1973. It turned out that the assumption that the bidding firms expected to get \$5 a barrel for oil and \$0.65 per thousand cubic feet for natural gas yielded amazingly accurate forecasts. On the equimarginal principle, this suggests that the marginal cost of developing onshore oil reserves in 1972 and 1973 was about \$5 a barrel, quite close to the number Davidson calculates.

I submit, however, that the price was appropriate for 1972 or 1973, rather than for 1980, and for offshore oil, rather than for all domestic oil. The domestic supply curve is drifting up and to the left, because the United States is a relatively old petroleum province. Companies have drilled the least-cost prospects first, and the exhaustion of the inventory of prospects is only partially offset by technological change. Many small technological changes are occurring now but none is as cost-saving as the invention of the rotary drill, and they are not sufficient to offset the drilling up of the available prospects. Conditional on OPEC cartel prices at current levels, this process will make the 1980 market-clearing price much more than the \$5 a barrel appropriate to the conditions existing in 1972 and early 1973.

The key issue for 1980 production is the rate of offshore leasing. Lease sales amounted to 1.7 million acres in the first ten months of 1974, a sharp increase over 1973, and may rise as high as 10 million acres by 1980. That would provide some relatively low-cost reserves. But even offshore development runs into diminishing marginal returns, because the companies

deplete the best prospects first. They are moving into a margin of deeper-water and higher-cost development. Furthermore, since offshore wells now produce only about 12 percent of domestic oil and condensate, their cost cannot substantially diminish the overall domestic cost.

I would be much more confident about relying on the market to generate the best possible outcome than on some price-control scheme. I submit the natural-gas case as evidence in that regard.

General Discussion

There was a lively discussion concerning the market power of the major U.S. oil companies. Edward Erickson argued that industry concentration ratios were small and that because there were about twenty large companies in the industry, they would find it hard to exercise effective market control. The historically observed problems of resource misallocation in the domestic petroleum industry have been due more to TIPRO (Texas Independent Producers and Royalty Owners) than to the major U.S. oil companies. Paul Davidson would not agree that the existence of twenty prominent firms precluded effective cartel action, arguing that even a large cartel can stick together if the firms have common objectives. On the international oil scene, Erickson and Arnold Packer thought the power of the major companies was small in comparison with the power of the OPEC cartel. Michael Wachter argued that historically, if not currently, the major firms and OPEC have been acting in coordination and that the United States helps sustain the international cartel by dealing with OPEC through the oil majors.

James Tobin questioned the theoretical basis for Davidson's contention that a conglomerate energy company will suppress an alternative energy source to protect the value of its oil reserves, or the value of OPEC oil reserves in which it has a concession. A conglomerate will introduce a new technology or new source when its marginal full cost is lower than the marginal variable cost of its existing operations. It will continue to operate its old sources so long as their marginal variable costs can be reduced, by partial curtailment of their operations, to the level of the marginal costs of the new operations. These are the same conditions under which a new technology would be introduced under perfect competition. The competi-

tion and conglomerate cases differ only if the two technologies are mutually exclusive. Marina Whitman and Davidson thought that while this analysis might apply if the new technology were already in existence, the conglomerate might not be willing to make as large an investment as a competitor to develop an alternative energy source that would diminish the value of the conglomerate's oil reserves.

Tobin began an extended discussion by asking Davidson to clarify his definition of "zero user cost." He and other participants objected to Davidson's excluding from the price of oil under "zero user cost" the diminishing marginal-returns rents associated with an exhaustible resource. As a result of this exclusion, the price of oil for Davidson is only the marginal factor cost plus a monopoly markup. By that definition, Christopher Sims said, "zero user cost" may not be a desirable goal, because it would exhaust reserves prematurely. Davidson argued that oil may be exhaustible over a very long time horizon, like 200 years; but over normal time horizons, oil should not be modeled in the aggregate as exhaustible. He noted that even the sun will burn out eventually, but that doesn't mean we should analyze sunlight as an exhaustible resource. And, to support his view that future reserves were so uncertain that they should not be modeled as exhaustible, he referred to a Bureau of Mines report of a geological formation under South Carolina that might be as productive as the oil reservoirs of Saudi Arabia.

Saul Hymans was unwilling to accept Erickson's user-cost formula without some test of the sensitivity of the regression to alterations in the formula. He said complex user-cost expressions are frequently inserted in regressions without any knowledge of whether the estimation succeeds because of, or in spite of, the form of the expression. Hymans also argued that, in a proper model of supply, current production and the discovery rate of new oil would be jointly determined.

Sims and Robert Spann were not so ready as Davidson to dismiss speculation by oil producers as socially undesirable. When producers act upon the information that prices are going to change, they tend to even out the long-term path of prices. Spann said that the recent energy crunch might have been alleviated if producers had had incentives to speculate against the possibility of an Arab oil shutoff. If they could have counted on prices being allowed to rise freely, Americans would have stockpiled oil to sell at high embargo prices, increasing the supply of oil and diminishing the price. Arthur Okun thought Spann's scenario was academic because no

one foresaw a cessation of oil imports before the embargo. Franco Modigliani agreed with Davidson that a speculative bubble could persist for a long time and could generate false market signals.

The failure of U.S. supply to respond thus far to higher prices was discussed. Erickson argued that the weak supply response was a market failure arising from price controls, including the present control of prices for "old" oil. Okun noted that there is now an incentive not to increase production on an oil property if production will not exceed 100 percent of its 1972 production level, the condition that would free the price on further production increments as well as on matching quantities of old oil. Charles Schultze suggested that stripper wells are now being drilled because their free price makes them more profitable, even though they are more costly and less productive. Producers also appear to be speculating against removal of price controls: drilling is up 40 percent, but production is down 6 or 7 percent.

R. J. Gordon questioned whether Davidson's production forecasts for 1980 could possibly be achieved even if his prescription for policy were followed, in view of the massive investment that would be required. Whitman wanted to know what environment would be necessary to achieve self-sufficiency at Davidson's prices by 1980. Would the price of foreign oil matter and would the United States have to pledge price supports for domestic oil? Whitman and Packer were concerned that we be prepared for the eventuality of someday producing ten-dollar oil rather than importing five- or even two-dollar oil in a future with self-sufficiency achieved and the OPEC cartel broken.