# Contributions to the paper 'Uneconomic gold production in South Africa' * by H. L. MONRO

## Contribution by B. K. Martus†

I congratulate the author on his interesting paper. In my view, the establishment of the level at which South African gold output is at some kind of optimum is an interesting though somewhat academic exercise. The methodology that the author proposes for the establishment of this level is generally pretty well accepted, but there are some dissenting opinions. For example, the economist Orcutt maintains that 'doing econometrics is like trying to learn the laws of electricity by playing the radio'.

Nevertheless, the science is generally well accepted and is an extremely powerful tool *if used correctly*. As will become clear, I have some severe reservations about this paper on that account. Before dealing with those aspects, though, I would like to spend some time discussing the author's basic propositions.

*Basic Propositions*

The author points out that greater supplies of a commodity on a market result in lower prices. He therefore appeals for lower South African production, which, if his equations are correct, will result in higher overall profit. The pursuit of higher profit is undeniably of major concern to private industry. However, the State must also consider the impact of lower production levels on State revenue, foreign exchange, and unemployment, as well as several other important factors.

Thus, while I fully support the author's efforts to estimate optimum South African gold production levels, I am decidedly uneasy about what he calls 'optimum' production. For reasons I have just outlined, the optimum level from the State's point of view may be much higher than that which simply earns the maximum total profits for the *mining* industry.

The author estimates the actual optimum level of gold production in 1982 but considers that target to be unattainable. He is almost certainly correct in saying that the South African Government could not accept such a low level of gold output. In any case, an agreement to limit production *directly* in this way could be effected only by cartellization of South African production, with the many problems that that would entail.

But is the author's 'optimum' an optimum even from the point of view of the mining industry? The answer is *no* except by sheer accident. The reason is that optima in the mining industry can be calculated only on a dynamic basis, because the temporal effects of changes in production levels (or other factors) cannot be ignored. Thus, the correct basis for the establishment of optimum profit levels is over the life of a mine. This is a fact that the industry acknowledges by calculating optimum profitability, and therefore optimum production levels, by the method of present value (P.V.).

Fortunately, the author may have recognized these fatal flaws in his definition of 'optimum' output because he proposes a less ambitious *indirect* way of limiting production. This involves the absorption of all operating mines by the relevant holding companies and (presumably) an appropriate tax structure.

The proposal seems to have possibilities; however, in practice, production targets in individual mines will continue to be set on the basis of a number of projections, notably productive capacity and estimations of the gold price P.V.'d over the life of the mine. The response of individual mines to the author's proposal (even allowing for a different tax structure) may thus be more conservative than he imagines. Therefore, in practice, some form of inter-group agreement (perhaps cartellization) would probably still be required in order to limit the total gold production.

If reconciling vastly disparate shareholder interests by amalgamating mines and then battling with the infinitely more difficult problem of cartellization were not daunting enough, the author's proposal faces a third problem. This is the degree of scepticism that is likely to be generated by this model. If a decision-maker is uncertain about the validity of any concept, he is unlikely to do anything that is irrevocable.

Nevertheless, to operate at or near the true optimum production levels is clearly a desirable objective, and the remainder of my comments are therefore intended to be helpful in improving the quality of the model and thus making it more likely that decision-makers will act upon the findings derived from it.

I would therefore like to make some general comments about econometric modelling.

*Dynamic Model*

As pointed out, the mining industry *must* use dynamic models. The author's equations do have some dynamic features but the overall model is essentially static.

*Model Specification*

One of the most critical and criticized areas in econometric modelling is the specification of the model. The experts are agreed that the general characteristics of the model should be specified *a priori*, that is *before* the estimation of the coefficients from the data is attempted. This ensures that the model is kept 'honest' and minimizes what is called 'data mining'. The latter term relates

* Published in the *J. S. Afr. Inst. Min. Metall.*, vol. 84, no. 5. May 1984. pp. 113–124.

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 SA ISSN 0038–223X/$3.00 + 0.00.

to the practice of including *any* variable in a model if it shows correlation with the dependent variable, whether this correlation is a reflection of a *true* relationship or not. If there is no true relationship, the inclusion of such a variable has the effect of improving the $R^2$ value without giving an actual improvement in the explanatory power of the model.

To forestall accusations that the modeller is indulging in data mining, he must therefore satisfactorily explain the meaning of all relationships in practical terms.

The author has done this to some extent, but I do not believe that his gold-supply equations, for example, satisfactorily explain medium- to long-term supply phenomena. I would expect, for practical reasons, that the lag between a high gold price and the extra production that it triggers would usually be longer than the one year that the author finds (in his all other non-Communist supply equations). In South Africa, where additions to capacity admittedly take a longer time to install than in other areas, the period would be from perhaps one to six years. The author's 'Latin American' supply equation shows supply at any time as effectively depending on all earlier prices. However, his equations again suggest that the *major* impact of a price change is a short-term one.

With respect to Soviet supplies, I found the author's arguments reasonably convincing as a description of Moscow's short-term strategy. I am entirely unconvinced that this same reasoning could describe their long-term strategy. For example, assume that high gold prices have encouraged the expansion of Soviet gold mines. Surely, under such circumstances, more Soviet gold would in future find its way to market?

If this model is to be used, as the author hopes, for the setting of practical *long-term* targets, it will have to be capable of adequately predicting long-term market changes.

*Direction of Causality*

In the physical world, certain events 'cause' other events to happen. For example, among married men, coming home at midnight in a drunk and disorderly state 'causes' bumps on the head.

An example in the context of this paper is that capital expenditure on mining facilities 'causes' extra mine tonnage to come on stream some time from one to six years later. Cause and effect are quite unambiguous in this case.

An important case where direction of causality is not always clear is in price establishment; for example, does demand affect price, or price affect demand? This is a more important question than it seems because the empirical equations linking price and demand differ depending on which is considered to be the cause and which the effect. In the paper, gold demand is always shown as being dependent on (or caused by) price. This is not always true: there are examples where high demand pushed prices higher, as the author acknowledges when he refers to the 'bandwagon' effect.

There are tests for the establishment of the direction of causality, and they should be applied to the data.

Of course, it is also possible, and perhaps even likely, that the price and the quantity bought and sold are decided simultaneously. That too can be handled by econometric methods but, again, the equations connecting price and quantity would be different from the forms established in the paper.

*Structural Break*

The use of econometric methods generally requires that the process being modelled has remained stable (not necessarily static) over the period. If not, it is said that there has been a 'structural break'. This situation can be handled mathematically, but at least three data points are required on each side of the 'break'.

A change of coefficients based on one or two points, as is done in numerous equations in this paper, is not permissible.

*Diagnostic Statistics*

The tests for a good model include the $R^2$ and $t$-test quoted in the paper. However these are not infallible tests. I have already mentioned some problems that $R^2$ and $t$ do not detect. Several other tests must be passed successfully before one can claim to have an acceptable model. These include lack of

*multicollinearity*, where the explanatory (or independent) variables are correlated with one another;

*heteroscedasticity*, when the variances of the stochastic disturbance terms are not constant over the sample or are infinite,

*serial correlation*, where successive disturbance terms are not independent of one another.

The presence of any of the above make $R^2$ and $t$ unreliable estimates of the accuracy of a model. It is therefore a standard requirement in this kind of work that statistics should be provided showing that the model is free of the above problems.

*Conclusion*

The author suggested that more data are needed and more work must be done on his model before definitive conclusions can be reached. I agree with that view.

## Contribution by D. M. Hawkins*

The thesis of the paper is a very interesting one, but, before it is used as the basis of any firm proposals to restructure the mining industry, which changes may be irreversible, one should be very sure that the basis of the calculations is sound.

I believe this is not the case, and that the model fitted is far from having been firmly verified. There are many grounds for statistical and economic objection to the methodology. The objections from economics (for example, the question of how reliable is the regression coefficient obtained for an endogenous variable if this is subsequently made exogenous), I shall leave for others better qualified to make, and concentrate on the major statistical flaws as I perceive them.

The paper makes very extensive use of multiple

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regression with selection of variables. This is a difficult area, which has taxed statistical theoreticians for many years, and which is still not by any means fully resolved. It is, however, very well established that stepwise regression as implemented in the paper is almost certain to end with a number of irrelevant predictors in the set selected, and with biased estimates of the genuinely relevant predictors.

In the paper, it is suggested that a regression coefficient that is twice its standard error is about significant. In fact, even if one has a large number of observations (so that the Student *t*-statistics used in stepwise regression are acceptably near to normal), this rule is dangerously misleading. It ensures that *any single predictor* will be introduced only if roughly 95 per cent significant. However, there is not a single predictor, and, with many predictors to 'bite at the 95 per cent cherry', it becomes virtually certain that the regression will contain a number of irrelevant terms. Each of the irrelevant predictors will have a 5 per cent chance of entering the regression; so if, say, 40 predictors are available, one can expect 2 irrelevant ones to be selected.

It is not really clear how many predictors were available in this analysis, but the analysis has 5 supply variables, 8 demand variables, 11 independent economic variables, and 4 scrap variables. In addition, there are some of these same variables lagged by one year (which in effect doubles the number of variables), and the use of different regression coefficients in different sub-periods of the 11 years. Quite how many potentially usable new variables this latter option introduces altogether is not clear but, as 10 per regression, it amounts to 50 variables in the supply equations alone.

All this leads to an enormous number of variables, with the consequence that the reported regressions are undoubtedly overfitted and contain irrelevant variables, with a consequent bias in the coefficients of all variables.

In fact, as the sample size is small, the 'two standard error' rule is even less adequate, in that the correct cut-off for a 95 per cent significance is larger than two standard errors, and so the probability of including an irrelevant variable is greater than the assumed level. A further problem with small data sets is that the inclusion of an irrelevant variable with a fortuitously high correlation with the dependent variable leads to a downward bias in the estimated standard deviation, and this in turn inflates the *t* values of all the variables and opens the door for further irrelevant variables to enter.

A further technical problem is the use of ordinary least squares on the time series, whose residuals are, contrary to the assumptions of the regression model, correlated. This ignoring of the correlations means that the *t*-tests used are in any event misleading because they do not have *t* distributions. The effect of the likely positive autocorrelation is to make irrelevant variables in the final regression even more likely.

Thus, the apparently very high multiple correlations do not actually provide adequate verification of the model, and there must be considerable doubt about the presence of many of the terms in the models, and about the true values of the regression coefficients of them all.

Perhaps the recent experience of the Hunt brothers in the silver market and of OPEC in oil should serve as a warning about the elasticity of supply (from unsuspected reservoirs of scrap in the former, and from new sources of supply in the latter) to really sharp increases in price. By far most of the gold ever extracted is still above ground in amounts that probably no-one can gauge at all accurately. There are very large potentially mineable reserves of gold around the world, waiting for an attractive price and enough lead time to come onto the market. I think only the very brave would assume that the short series of annual price and supply history we have had since demonetization (and in which South African supply has been endogenous rather than exogenous) can give sufficiently precise estimates of these elasticities to raise confidence that a unilateral reduction by South Africa in the supply will not simply lead to an effortless replacement by other suppliers.

## Contribution by P. J. Niewenhuizen*

The hypothesis advanced by Mr Monro that the demonetization of gold in August 1971 introduced a major new policy variable, namely the supply–price/demand–price relationships, into the decision-making process touches on a matter that has never really been given serious thought by the industry. The insensitivity of the industry to the supply–price/demand–price relationships as a major policy variable is partially an outflow of the historic role of gold in the international monetary system and the fact that marketing of the metal is still being effected very unilaterally by the South African central bank. Technical, corporate financial considerations, and balance-of-payments considerations, also play an important role in the view of policy makers that the production of gold cannot be revised downwards in order to optimize the overall economic yield, especially over time.

The period 1972–1982 covered by the study is a relatively short period in which to study supply–price/demand–price relationships. As far as the structure and organization of the gold market are concerned, three (possibly more) distinct phases can be identified. In (1968)–1974, trading activity was mainly restricted to the physical market, which was in many ways a market in the process of being established and geographically apportioned between London and Zurich. The period 1974–1980 was dominated by the steep rise in the price of energy and its great balance of payments redistributional effects, the demolishment of the fixed exchange rate system of Bretton Woods, and the re-alignment of exchange rates, high interest and inflation rates, and the liberalization of private gold ownership in the U.S.A. The advent of the futures market in 1974, and its development over the next few years, introduced a further new element into the market. The market for gold over the period 1972–1982 is thus anything but homogeneous. The determination of the price elasticity of supply and demand in a market as volatile as the gold market is methodologically most difficult, if not impossible.

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Of even more importance, though, is the functioning of the futures market for gold. This very important segment of the gold market, on which about 40 kt of gold is being traded annually, is nothing but a securities market dominated at any particular point in time by either bullish or bearish sentiments. It is thus a market characterized by a distinctive asymetrical distribution of market forces only remotely related to the physical supply of gold. This segment of the total market is an important instrument for the satisfaction of a large part of the overall demand (speculative and industrial) for gold. The role of the physical supply of gold in this particular market would seem to be of minor importance, more than 95 per cent of the total transaction volume being of a purely speculative nature. The real demand for gold (see Table II of Mr Monro's study), and the speculative demand for securities bearing an increasingly indirect relationship to gold in its physical form, are two distinctly separate market phenomena. The supply and demand relationships within these two markets are most difficult to capture within the confines of the elasticity concept.

The phenomena referred to above cannot be ignored in a study of the pricing of gold. The methodological problems involved in their incorporation would seem to be so complex that no solution, taking into account the present state of knowledge and theoretical tools, seems to be possible.

These remarks do not suggest that the thoughts expressed by Mr Monro should be summarily disregarded. Mr Monro's great knowledge of the industry and the gold market compensates to a large extent for the present inability of economic science to deal with marketing phenomena as complex and volatile as those relating to gold.

### Contribution by B. G. Kingsman*

As a co-worker on the development of an econometric model for supply and demand in the world gold market with Mr H. L. Monro, it would perhaps be useful for me to make some comments on the various points raised in the discussions on his paper. Although this research work has not been fully completed, the results appeared to raise pertinent questions concerning the policies of the gold-mining industry. Monro felt that these were of such interest and significance that they should be aired for public discussion as soon as possible. His preliminary conclusions were that mining profits for the industry could be increased at a lower level of output per year. Whether a move in this direction is desirable overall is a very complex question indeed. It requires very careful and detailed study. Nevertheless, the paper, allowing for all the caveats one would make on the validity of the econometric model, presents a strong case that such a consideration and investigation should be made.

I offer no comments on that question, but confine my remarks to the points raised about the econometric model itself.

The development of a meaningful econometric model for the supply and demand of a commodity is a very difficult task. It is even more difficult in the case of gold because of the scarcity of data compared with those for other commodities, and because of its role as a store of monetary value over time as well as the normal uses for industrial and private consumption. Before such a model is used for changes in policy or operations, it is right that it should be subjected to searching examination. The comments of Dr Krige, D. M. Hawkins, and B. K. Martus are useful contributions to this process.

Econometric modelling is now a well-established and powerful technique, as Martus points out. Both he and Hawkins refer to the importance of specifying *a priori* the general characteristics of the model to be estimated before the coefficients of the model are calculated. 'Data mining' should be kept to a minimum and, if further variables are included, they must have a satisfactory explanation in practical terms. All relationships should be based on generally accepted economic theory or be consistent with current practical knowledge.

The models were constructed on this basis. That the amount of a commodity consumed depends on the consumer's ability to pay, his income, and the price of the commodity is a basis of economic theory. In the case of Western jewellery demand, this was replaced by Frieman's 'permanent income hypothesis', that consumers adjust their spending from one year to the next by only a fraction of the actual change in their income, combined with Cagan's adaptive expectations model[1]. It is this that leads to the lagged demand variable on the right-hand side of the equation. This is now a well-accepted piece of economic theory. The model for Western investment is based on the generally accepted assumption that gold maintains its value in real terms over time. An investor is concerned with determining what proportion of his portfolio to put into gold compared with stocks and banks, etc. It is clear that the ratio of inflation to the bank interest rate will play a major role in his decision on these proportions. The odd result here is the negative coefficient of income. Continuation of the research since the paper was written suggests that this effect can be better represented by a negative time trend. This could be due to a rush into gold when it was first demonetized and an adjustment of investment portfolios to a more rational proportion to invest in gold since then. Alternatively, the many different forms of investment developed over the past ten years have reduced the attractions of holding gold. The model for Eastern jewellery demand is basically a well-established model of money spent versus income. Money spent is appropriate here because the price of gold jewellery is not substantially above the value of its gold content. It is weighted by the ratio of the lagged to the current price on the grounds that a fall in price will encourage more interest in gold seeing a potential profit when prices rise again.

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SA ISSN 0038–223X/$3.00 + 0.00.

These brief remarks are merely to demonstrate that the models presented were chosen originally on good economic grounds and were not the result of trying a whole range of possible variables. Unfortunately, the needs of publication to keep the article short may well have given the wrong impression on the research among some readers. It was felt at the time, probably wrongly with hindsight, that a longer, more-detailed paper would have obscured some of the major points that Monro wished to communicate. A paper discussing the model in more detail is in preparation, and may enable better discussion to be made of the economic bases of the models given in the paper.

The comments just made apply to some of the inadequacies of the diagnostic statistics presented in Monro's paper to which attention was drawn by the earlier contributors. Again, it was felt that a more extensive description of the statistical analysis was not appropriate at that stage. It is well appreciated that requiring a coefficient to be twice its standard error as the sole test of the validity of a regression model is somewhat inadequate. This was intended only as a rough rule-of-thumb for those readers unfamiliar with regression methodology. Whenever stepwise regressions were used, any added or subtracted variable was tested for its significance by analysis-of-variance methods.

Multicollinearity is an inevitable problem in all analyses of economic time series. The question is always whether it has a significant effect on the coefficients. There are a number of tests for this. One test, based on Frisch's confluence analysis, is whether the inclusion of other variables seriously changes the coefficient of an existing variable in the relationship. The models generally meet this criterion, the major exception being the scrap term in the Eastern jewellery model.

One test for the establishment of homoscedasticity is the Spearman rank-correlation test. This is based on the calculation of the rank-correlation between the residual errors and the independent variables. There has so far been little evidence that this is a serious problem for any model. Serial correlation between the error terms in the model can be tested by the Durbin Watson statistic. There was little evidence that this was a serious problem in any of the models chosen.

One very important caveat must be made to the results quoted above. There were only eleven data points in the analysis. The various diagnostic tests, with the exception of the $R^2$, corrected $R^2$, and $t$-test, are based on much larger sample sizes. So one must not attach too much importance to the above results. They can be regarded only as an indication that the models are not significantly affected by the undesirable properties mentioned. They cannot be taken as giving conclusive proof of this. A further difficulty and caveat is that many of the various diagnostic tests were developed for the linear (or log-linear) regression model. Their statistical properties and validity may be rather different for non-linear regression models such as for Western investment and Eastern jewellery.

The correspondents adversely draw attention to the number of variables needed in each of the separate models. However, it would be somewhat naive and unreasonable in such a complex structure as the world gold market to expect demands to be explained by a single variable. A major problem, unlike that which exists for most other metals, is that the data published in the *Gold* reports until recently covered only the net demands, i.e. usages of newly mined gold. Any demand model must include all the gold consumed, including that provided by the recycling of scrap gold. Data on this are extremely limited. Lower bounds on the amount of such scrap gold can be inferred from the *Gold* reports for the years prior to 1980. This shows that very substantial amounts were recycled in the early seventies. Ignoring the demands satisfied from this source would indeed nullify any demand models produced. Because of the virtual lack of data on this, it is impossible to make any statistical tests on the scrap-recycling models. One has to judge whether the basic model and the figures generated appear reasonable.

One of the scrap models presented in the paper requires some further comment. I would not claim that the scrap-recycling model for the Eastern jewellery model provides an explanation of behaviour, but merely that it produces a time series identical or similar to the actual values that occurred. The values derived for scrap can be modelled via Nerlove's partial adjustment model[2] with the behavioural aspect being a simple price ratio. This basically states that holders of gold take some time to be convinced of the benefits of selling gold, and continue selling for a little time after prices have risen again. Such inertia in behaviour is common in economic systems. The price ratio determining behaviour is the ratio of the current year's price to the previous highest price. Because of the limited data, it is not possible to include this type of standard economic model in a reduced-form equation. It has to be modelled via the device of an intermediate scrap-generation model time series. This has serious multicollinearity between the two variables of income and price, and hence the attendant problems that this causes. Reasonable coefficients can be obtained by choosing those values that best fit the very limited scrap data that are available.

The use of dummy variables in an econometric model for structural changes in the situation is a well-established and accepted technique in economics. Gold usage in dentistry is dominated by West Germany particularly, followed by Japan and the U.S.A. to a lesser extent. Usage in the first two countries is affected strongly by the State health-insurance regulations concerning the amount of gold dental work allowed. These insulate consumers from the full effects of increases in the price of gold. The changes in these regulations that did occur can best be represented by dummy variables. It is possible, of course, that these also affected the elasticities of price and income. It is well recognized that further data points after the structural break of 1980 are required for this to be estimated properly, and hence for reliable forecasts to be given of future usages. Nevertheless, at any point in time one must do the best possible with the data then available. A further point is that the high prices of 1980 may have brought about technological changes. Because of the limited information on gold usages in dentistry, at present this can, again, be treated only as a structural

break in the time series. Since no structural break apparently occurred after the first peak price in 1973, this may be a less significant factor than the changes in health-insurance regulations.

Similarly, for electronics and other industrial demands there were undoubtedly technological changes following the high peak prices of 1973 and 1980. Thus, in this case there were two structural breaks. Again, two or three further years of data are needed to confirm that behaviour after the second structural break continues as modelled in the equation given. High prices clearly caused the changes in unit usage. However, it is obviously a fairly complex process that one would not expect to model simply, even if there were an adequate number of structural breaks to work on. Interestingly, although the peak price of 1980 in real terms was much higher than that of 1973, the effect in reducing the overall usage levels was much smaller. This suggests that the scope for further economies is fairly small if prices rise above the $650 per ounce of 1980.

The major point one can make on drawing implications from the econometric model is that the changes in usage resulting from an increased price level from dentistry, electronics, and other industrial uses, the categories where dummy variables are needed for the modelling of structural breaks, are small relative to the other usage models where no dummy variables are required.

In modelling the supply side for a mineral commodity, one has to distinguish between mine output in a year and mine capacity. These may be the same if a mine is worked to its full capacity, but not necessarily so if a mining group has a choice of the grades of ore that it can work in any year. The former situation would be true of alluvial mining, as in Brazil in recent years. Indeed, in that case the concept of mine capacity is not particularly meaningful and useful. The latter situation applies to deep-lode mines as in South Africa.

Alluvial and placer mining is carried out by smaller companies and individuals, and requires far less capital expenditure on equipment than deep-lode mining. Open-cast mining uses equipment that can be employed easily on civil-engineering projects. In all these cases, high or very low gold prices can lead to rapid responses in output. It is thus not unreasonable to expect a response to the previous or indeed current year's prices.

In the case of deep-lode mines, the mining companies presumably try to plan ahead from which parts of which mines to extract ore or which grades to extract. Thus, one would expect output to depend primarily on the previous year's price, although a significant change in the gold price in the current year could lead to some adjustment of the current year's output plans. However, it may just not be possible to adjust the output level as quickly as the mining companies ideally would wish. It is also costly to close down a mine or re-open it, so that a period of sustained higher or lower prices may be required for significant changes to be made. It is likely that there will be only a partial response of output in any year to the change in price compared with what the mine operators ideally might like to do. This process can be modelled by Nerlove's partial-adjustment model[3], now a well-known and much-used piece of economic theory. This process applied to South African output from 1971 to 1982 gave the following equation:

$$MP_t = 9{,}64\ MP_{t-1}^{0{,}645}\ P_{t-1}^{-0{,}062}\ .$$
$$\qquad\qquad\qquad\qquad\qquad\qquad (0{,}112)\quad (0{,}030)$$

This equation had an $R^2$ value of 0,955 and a corrected $R^2$ of 0,945. The R.M.S. % error was 3 per cent. The Durbin Watson statistic at 1,48 indicates that there is little serial correlation in the errors, even allowing for the small sample size.

No other price lags gave any significant improvements in the statistical goodness of fit. However, because the lagged mine production is included in the model on the right-hand side, longer lagged prices are implicitly included in the model. The results obtained suggest that the major impact of a price change is a short-term one.

The longer time lags mentioned by the earlier commentators really apply to mine capacity rather than to mine output. Unfortunately, there is no time series of mine capacity available from which to model this effect. It could be that, in South Africa, the bringing of new mine capacity on stream has been a continuous gradual effect with no significant increases, of say 10 per cent or more, in any one year.

Such an effect was present for the rest of the world mine output. There was undoubtedly an increase in mine capacity in 1981 and 1982 following the high prices of 1979 and 1980, either by bringing new mines on stream or re-opening mines closed earlier owing to the relatively low prices of the middle and late seventies. However, because of the complete lack of data on mine capacities in each year, this can be modelled only by a dummy variable. The model here, if valid, suggests that mine operators in the rest of the world are able to change their output from year to year more easily than in South Africa, or they have adopted policies of normally being much more flexible in their output levels.

It is hoped that these remarks have helped to clarify some of the points raised by the previous correspondents, and so assist in a better-informed debate on Monro's suggestions. It is important to examine the policies and output prospects of other gold producers to see whether any step change in behaviour is likely to occur. The sort of changes in outputs suggested and the resulting price changes are likely to be within the patterns seen over the past ten years. It is also useful to specify these changes in some formal policy and then evaluate their effects via a dynamic simulation of the econometric models. As with all econometric models, research should be continued and efforts made to gain further data.

*References*


1. CAGAN, P. The monetary dynamics of hyper inflations. *Studies in the quantity theory of money*. Friedman, M.(ed.). Chicago University Press, 1956.
2. NERLOVE, M. Estimates of the elasticities of supply of selected agricultural commodities. *Journal of Farm Economics*, vol. 38, 1956.
3. NERLOVE, M. Distributed lags and demand analysis. USDA, *Agricultural handbook* no. 141, Washington, 1958.