Operational Research as Revelation

INAUGURAL ADDRESS BY THE PRESIDENT, 14 JANUARY 1970

STAFFORD BEER

INVOCATION

IF THERE is one speech in a garrulous career for which a professional man cannot subsequently excuse himself it is surely an inaugural address as president of his own professional society. Election to that office is the highest compliment he can receive, and I am deeply aware of this.

I have had a year since the election to reflect on the responsibilities, and on what I should say today. It has been a year for discarding three plausible speeches.

First, there was the witty speech full of in-jokes; next the overly clever speech making abstruse allusions; and last the obsequious speech of relief that I have joined the Establishment at last. These three speeches will never be spoken.

For a time, moreover, I found I was leaning emotionally on the stature of my predecessors. There is no lack of lustre there—a lustre in which I somehow feel entitled to share. I take this immediate opportunity to salute them all.

And especially I salute, and thank sincerely in your name, the most recent member of that distinguished band—our past-president, Bill Williams.

He has contributed massively to the Society's progress by exceeding hard work behind the scenes. And he hands on a rejuvenated society—poised, I think, for a virile leap into this new decade.

As for me, however: I am finally left alone up here with what I really think. If I did not take that loneliness seriously now I would be a fool; and I would be taking you who put me here for fools. My state of mind today has been hardly come by—in the practice of operational research, wherein I reach my silver jubilee next year. You cannot ask for more than to know what in all honesty is that state of mind. I cannot offer you less.

Its emotional tone, first of all, could be called a mood of controlled anger. I hope that this is both more dignified and more constructive than a feeling of mere irritation.

Operational research has in some respects been denatured by its own growth in respectability. I am angry about that and will explain my thinking later.

Secondly, it has in some respects been misrepresented—and not only by outside critics. That makes me angry too.

Above all, its capability has in large measure been wasted. I rate failure here, the responsibility for which we must ourselves largely bear, very seriously indeed. Here above all anger must be constructive.

I do not mean that the work we have all done is a waste. I mean that the issues to which scientific problem-solving is most profitably directed remain the accepted prerogative of intuition.

The favoured management style therefore remains the striking of consensus. This is a prudential technique, hopefully protecting the group from lunatic intuitions which individuals may entertain.

It operates at a terrible expense. The price paid is the suppression of right intuitions which lie outside the imagination, knowledge and experience of the consensus.

Under this style, management becomes a Mediocrity Machine. In the fair name of "participation" it emasculates the bold and far-sighted proposal and dissipates the innovative drive.

The constructive answer to this is not indeed a return to the rule of authority. It is the invocation of science to provide that kind of evaluation which would carry the consensus, and establish the management team in new paths.

I reserve my anger here for the Mediocrity Machine which invokes science—not to these ends at all, but for the increasingly efficient implementation of its own inadequate plans. We cannot afford this waste.

Today's world is short of capability rather than capital, of competence more than any other resource. To frivol away such competence in the scientific management of affairs as we have is itself a gross incompetence.

It is of a kind which would rank as legally culpable—if it were indeed capital rather than capability that had been squandered.

As it is, everyone is supposed to accept that society acts within ineluctable constraints. I do not know why this is. These constraints evidently act so that progress is satisfactory if any is made at all. I do not understand why this is either. We seem to have reversed an ancient maxim, so as to say: *non regredi est progredi.*

If I do not know *why* this is, perhaps I know *how* it is. The Mediocrity Machine calls for, selects and adopts only one kind of servant. I call him Acceptable Man. He operates smoothly and effectively within a small and esoteric group. He is the heir-apparent; he is imitative of the elders; he is competent in tribal lore. And he reflects the consensus.

Acceptable Man copes with challenge in a predictable way. He simply declares it invalid. It will come, all right, but not yet. Therefore the means for coping with challenge are all right, but are proposed too soon.

I have to tell Acceptable Man that the challenge will not wait—it is in our midst. Our milieu is fantastically expanded in every direction and there is crisis on every side. Our horizons for comprehension, for planning, for control are receding faster than we can conceive. No wonder that I see red: it must be a Doppler effect.

Acceptable Man is competent no longer. But Competent Man is not yet acceptable. Here lies the dilemma.

In the last presidential address you heard about operational research as genesis. It has bred many valuable activities, said the past-president, now carried on under other names. What then is left, and what is OR today? It must stand by what it reveals.

Here is my first reason for talking to you about operational research as revelation.

REVELATION (1)

To my mind the great revelation of operational research is the clarity with which it has exposed the shortcomings of the human brain as a means of dealing with complex situations, and the power of the alternative it offers.

That alternative is the construction of a scientific model, devised for the purpose in hand. It is astonishing but true that almost any scientific model, however exiguous and crude, quickly surpasses the capability of the brain to evaluate a complex situation in quantitative terms.

The set of punched cards on which the newly qualified physician may record details of every case with which he deals becomes a better diagnostician than he is himself after only a few hundred cases have been stored.

Men find it difficult to predict the behaviour of a small interactive servo-mechanism with a handful of variables, once feedback is introduced. If the transfer functions are non-linear and the responses are lagged, they find the task impossible.

Compare these two illustrations with the management task in running a large-scale enterprise. The models of this that we carry in our heads are insufficiently complex by orders of magnitude and insufficiently rich in interaction to an unthinkable degree. No wonder they are not of much use in procuring a discriminatory output.

The reason itself is understood. The slightly alkaline three-pound electro-chemical computer in the cranium has, if we are lucky, a capability to discriminate over a scale of nine in any dimension. To calculate the average is our *métier*; and if we can discriminate four points in either direction from there we are doing remarkably well. To these ends our brains employ ten thousand million logical elements, running on glucose at twenty-five watts. The discriminatory yield of nine points on a scale represents an output of 3·2 bits.

For comparison, a small business with five hundred binary inputs and five hundred binary outputs is capable of taking up more states than we can at all conceive. It is a number so large that if the whole terrestrial globe were and had always been a computer it could not have processed the data. (There is formal proof of this statement, based on Planck's constant as a limiting factor.)

The answer of man the manager to this problem is precisely *organization*. Proliferating variety is held in check by our organizational refusal to consider more than a tiny part of the problem at once. Nor will we normally consider more than one time epoch at the moment of decision. Any who tries to look more than a week or so ahead is likely to be written off as a visionary. Thus are great issues reduced to a scale with which our cranial computers feel they can cope.

But the subjective illusion of coping is not enough. There are implications for policy in an exploding technological milieu which range beyond the ken of this procedure in space, in time and in organizational stress.

In space, because technology now ramifies the effects of every action. In time, because it takes years for any policy to mature—so that the outcomes of many policies today are almost exactly out of phase with the management succession or the electoral cycle. I mean that praise and blame are likely to be apportioned to precisely the wrong set of people.

As to organizational stress, the point is that the space–time ramifications of policy and its exorbitant cost drive the level of decision inexorably upward, at the very moment when our collective managerial wisdom calls for driving it down.

The result is inevitably that the macro-systems of our firms and social institutions do not hang together, while the interaction of micro-systems is self-defeating.

Thus is lost every benefit of a putative integral policy. Industry loses the potential benefits of synergy. Government considered as moderating the national weal becomes incoherent. Administration is for ever inchoate.

All of this happens for want of a model, drawn at the level of systemic integration. It is not a necessarily elaborate model, but a model that can improve on a discriminatory output of 3·2 bits. It is a model that can cope with the interactions of perhaps ten to fifty variables in quantitative terms. You and I and the manager cannot do that. Science can. If operational research must stand by what it reveals, this is the overwhelming truth.

When we say that OR has produced a battery of techniques which have been notably successful, we refer exactly to this fact. Critical path analysis does precisely this. So does linear programming. Both techniques algorithmically relate the parts of a well-formulated system to create that whole which we cannot intuit, a whole greater than the sum of its parts.

As to our other famous methods, they have mostly enjoyed success because the models they use correctly convolve probabilities. And this is something further of which the brain is notoriously incapable.

I customarily seek to demonstrate this point to managerial audiences by inviting them to assess the chances that any two people in the room have the same birthday. They are hopelessly wrong, always, underestimating by ludicrous amounts. For as you know the chance is even when there are no more than twenty-three people present. And Feller's equations have never let me down in perhaps a hundred tests. At my last demonstration a few weeks ago a triple birthday emerged among the first twelve people questioned, after a consensus that the chance was less than a cat's in hell.

Let me now gather the threads of this argument together. I am saying that in operational research we combine the power of the model, applied at an integrative level, with the power of probability measurement. Then we have a tool which even in its crudest form immediately outclasses the brain in the task of quantitative evaluation.

Here is the point where I must turn aside to rebut the view, as common perhaps among scientists as among managers, that immense sophistication, time and expense are required to build useful models. It is not true.

The best-known examples of classical OR reveal the elegant simplicity of good science—the $e = mc^2$ that was there all the time. Then let us take care that scientific insight is not obfuscated by its own technique. Nor should we let our understanding vanish down the nearest data-drain to be lost in the viscera of a vast digital computer. Insight and understanding are precious commodities indeed: they are the outputs of cerebral computations that the brain handles very well.

I see several people here tonight who were doing successful operational research when any practical computer was still a mad dream. They were doing OR before linear programming was invented. (When what was originally called "the Dantzig method" was first discussed, I regret to say I did not know the George of that patronymic: I thought it was all something to do with the Polish Corridor.)

Then what was OR doing then? It was using science to solve problems in the conduct of affairs—whether at the tactical or the strategic level, whether the problems were about activities on the ground or policies in the head. That is what OR does today.

And so we have the name under which we work.

We call that work Operational (with a large O) because it is based in the world of genuine activity, the places where things actually happen. All good science, as distinguished from all good mysticism, is founded in empiricism. It involves actual observation, actual measurement and actual experiment. We call our work Research (with a capital R) because we deal with problems to which no one knows the answer. Doing that thing is *called* research.

Well, I am proud of this name. For years I wondered whether it should be dropped. In the 'fifties managers did not understand what it meant. Even later than that, I still glossed over the phrase, calling it *oh–ah* as a kind of meaningless grunt. We have all of us flirted with alternatives—of which the name "management science" was perhaps to be preferred. I even wrote a book about OR under that title.

But today I advise you to give up the quest for a pseudonym. Not only do we know what we are doing, but a great many managers do as well. Besides, after all, the name turns out to be pretty descriptive.

As for the name "management science", I now think it will go the same way as the name "scientific management" went in the 'thirties. That name became the battle cry of people on the make. The new one is now the banner under which is currently mounted a prestigious competition to project from hearsay evidence both the form and the pay-off of the improvement which a specific major company could expect from its use. The rules include the condition that no special observation of that organization is allowed. Measurement is therefore impossible, experiment is out of the question, and the validation of models cannot be attempted.

This kind of approach is unhappily sometimes the way of management. It is certainly not the way of science. Above all, it is neither operational nor research. I am thankful then for this terminological escape, and offer you a paean of praise to the name under which we have operated all along. Let's keep it. It means what it says and so do we. I invite you also to adhere to the revelation we know, and of which I have been speaking.

When I said earlier that our subject could be denatured by its own growth in respectability, I referred to a defensive and narcissistic preoccupation with established techniques. There is no need to take refuge in them, although there is a proper satisfaction in having set them on the shelf. Still less should we look upon a battery of techniques as our output, or judge our professional success in terms of their number and obscurity. You have noted a tendency to do this; so have I.

There are people who complain: "Look—a few simple techniques, and that is all it was about. Let's forget it." What is the point of reducing operational research to that kind of absurdity, and then complaining that it is absurd? I hope those who do this will not gash themselves on the very axes they are patently grinding.

For the professional, the output of OR will always be feasible solutions to problems, and nothing more. If there is a technological spin-off, so much the better. Stock controllers are most welcome to employ the techniques of inventory control. Indeed, did they not so do, they would be crazy. And if people have problems of resource allocation, let them use the algorithms which OR established for the purpose of computing the answers.

But let you and I, under the banner proclaiming our willingness to undertake proper research into actual operations, get on with solving new problems.

This is what we are for. The only question is whether other people know that too. I shall pitch my arguments about this at the level of government, although other branches of management are equivalently involved.

REVOLUTION

The printed heading for this section is not any longer "revelation" but "revolution". That is because Acceptable Man in his Mediocrity Machine is entrenched in the face of a challenge to which he cannot rise. He has to be shifted, and this will be a revolutionary move. If it cannot be accomplished by rationality, something unpleasant will assuredly happen. That is an inference—not a threat.

How do we ourselves stand in all this?

For a quarter of a century leading members of this Society have tried to gain an acceptance for operational research in the scientific management of civil affairs commensurate with its war-time contribution in the military field.

A certain amount has been done, which I earnestly wish to acknowledge. There have been valuable if isolated results in a few departments of state. The same can be said about a very few local and regional authorities, the Greater London Council at their head.

But anyone soaked in the early history of OR and its immediate post-war promise undoubtedly feels let down. This is the cause for anger with which I began. And yet the feeling of anger almost dissolves into feelings of bewilderment and regret. *Mais ou sont les neiges d'antan?*

By the end of the 'forties it might have seemed that the days of positive anti-science in government were past. It was said at that time, with amused disgust, that we had seen the last of them. A memorandum issued within the Civil Service in 1934 had supposedly recommended that senior officers should not sit in conference with scientists. Ridiculous, everyone was saying, by 1949.

But less than two years ago a distinguished civil servant—our own then president—was on this very occasion concerned lest a myth arise that operational research were less than an "integral and essential part" of management.

The fact is that we had not after all gained the point. We have not gained it to this day.

When Churchill came to write the story of the air war, in which human valour reached that high peak which his own immortal words had already enshrined, he had something more to say. It concerned the "secret war", fought by the scientists who had saved the war from being lost before it could be won.

"Only with difficulty", he wrote in *Their Finest Hour*, "is it comprehended even now by those outside the small high scientific circles concerned."

Just a few of the subsequent managers of civil government affairs—a Stafford Cripps for example—ever took the point. Few others indeed have reposed a jot of faith in the ability of science to tackle the problems of peace.

Yet science is only just beginning, by the accepted scale of man's cultural span on earth. Its potential contribution to the handling of affairs is not assessed. The public, and especially the young, are encouraged to believe that science (rather than its ignorant application) has brought us all to the edge of disaster.

Even so the nation was entitled, in the name of that managerial revelation and consequent revolution in the conduct of war, to the extensive development of governmental OR in the service of peace-time society.

It did not happen to scale. Moreover, despite many protestations of its wish to advance further now, made by both politicians and permanent officials from time to time, government seems impotent to act convincingly.

That is supposing that it really wants to do anything of the kind. Speaking personally, I shall find it easier to believe in the good intentions of government when it faces up to the financial realities of the market place where professional OR staff is concerned. It is a sad task to sit on advisory boards and to watch the directors wrestling with the special martyrdom which government scientists are called upon to bear.

Those of us who are martyred only on isolated days can afford it in the national interest. But I think it should be public knowledge that the government assessment of the professional value of a senior OR consultant on these occasions is exactly *one-tenth* of his standard market value. And I am sorry too for those many senior men who find themselves constantly apologizing for this absurdity.

I do not speak of what has been happening nationally in the last few years with easy hindsight. What would happen was predictable. In 1964 we had the "affluent society" which had "never had it so good". In a letter to *The Times* published while the OR Conference was in session at Cambridge during that year, I said: "It is all very well to be affluent; but an affluent anachronism will burn brightly away to indigence."

Sure enough, we did just that. And we are still an anachronism.

I went on in support of the plan—remember it?—to modernize Britain. "We need operational research teams of outstanding ability", I said, "working on problems of decision and control at the national level. Because these problems are usually discussed in economic terms, they are currently assumed to be purely economic problems: but they are not. Interdisciplinary scientific teams are needed to evaluate issues subject to conflicting criteria."

We got the new government, but not the new policy.

Today, it seems to me that people at all levels of influence and having all shades of political opinion know that the reform of government machinery is mandatory and urgent. It cannot be done without the sort of competence OR has.

We recently had a Minister at Mintech who knew it. Dr. Jeremy Bray wrote a reasoned, important and unsensational book about the problem which was published last week. He was sacked for his pains.

Acceptable Man is no longer competent, while Competent Man is not yet acceptable.

The fact is that we have become inured to threats, and find it easier to sidestep challenge than to meet it. The consequence is very strange, and I invite you to contemplate this remark:

Theory is the only reality countenanced by our culture.

This means that the accepted account of affairs becomes more real to those concerned than the truth on the ground. And the facts have to fit this theory, rather than that the theory should at all be changed.

It is the hard-headed practical man rather than the scientist who has all the theories these days. He "knows". The scientist knows no more than that he *doesn't* know—until he has made the study.

Consider now with me some apparent realities of our time which turn out to be theoretical constructs in this sense. I start with something remarkably concrete to be called a theory—yet that is what it is.

I refer to Euston Station. This is someone's *idea* of what a great railway terminal *ought* to be like. It is the sort of grandiose dream that Stephenson might have had of the future if he had not had more sense. It does not work. It does not meet the manifest need. It is a theory.

Consider the Prices and Incomes Policy. There is a theory, well based on certain principles which themselves are unassailable. But how theoretical can you get? This theory turns out to repudiate one of the few cybernetically valid mechanisms for self-regulation that the economy has ever had. It turns out to entail that all growth is both minuscule and uniform, which cannot be true. It takes a system of pay differentials, frozen by accident out of history and patently unjust, as inviolable.

Return to something quite concrete once more. What is a motor-car but a theory? It is an ergonomic nonsense sold on sex-appeal. I for one would like a motor-car which I could drive without acute discomfort and a pervading sense of personal hazard. But this theoretical car—the one I have to drive—makes no provision for me, only for some theoretical driver.

Consider the taxation system, which is not only a theory but a completely incomprehensible theory at that. It absorbs by now an unbelievable proportion of the national effort, one way and another. That is one mark of its sheer impracticability. But the theory leads, as every taxpayer discovers, to the most bizarre consequences in our personal lives. Never mind: the theory is right, and the facts must be forced to fit it.

Back to a concrete theory: the jumbo-jet which I saw with my own eyes this week. What are the practical realities of long-distance travel? They are about getting to and from international airports. No-one's theory encompasses this hard fact. So we take the easy and costly path, following our technological noses from Mach 1 to Mach 2 into the noise-polluted yonder.

My sixth example is the health service, organized on a theory about the structure of the medical professions ill-mixed with a theory about the pharmaceutical industry. The patient barely figures in this theoretical construct. He would stand a better chance if he were fighting fit to face the rigours of his treatment.

Finally, and supremely, I offer you the theoretical construct of:

The Egalitarian Society, in which I passionately believe. But the theory which is this accepted reality turns out to deal with equitable shares in poverty. There are no parameters in the model to manipulate the creation of wealth.

What do I propose should be done about all this?

Firstly I propose the undertaking of operational research in the construction of better theories which more nearly match the realities on the ground. Research into operations is the bridge between theory and practice.

Real-life OR is not done in order to satisfy the requirements of a Ph.D. board. Good OR is not done to support anyone's policy.

OR is empirical science: it is about observation, measurement and experiment. It is concerned with finding ways to make the worse somewhat better. It narrows the area of risk. It studies the vulnerability of alternative policies to a range of possible futures. It looks at the real world where the action is, where real people live under real constraints.

Does this offend mathematical purists? My answer is: let us first get the sign right.

Does this offend those charged with government? My answer is: let us manage the real world, and not a theoretical conception of what ought to count as reality.

Secondly I propose that all this should be done in a proper context. By this I mean a context which is both interdisciplinary and interfunctional, a context which is above all interdepartmental.

This calls in question the theoretical construct of government. And why not?

Is the citizen cared for by Health and Social Security not the same citizen protected by the police? And is he not to be educated?

Of course there must be organizational divisions of government, as of any large undertaking. But what measures do we take to see that divisions are not divisive? How do we achieve coherence and an integral policy?

The answer is by drawing our models at the level of integration. *They will be meta-systemic models.*

Now we shall come to the outcry. The super-theoreticians who count as practical men will say it is too theoretical.

Was the meta-model of battle linking separate encounters and spread out on a vast map in the war-time Operations Room too theoretical? It worked.

I envision a government operations centre, laid out on comparable lines, relating the pieces of the national problem in an integral way.

Industrial managements could have this room if they wanted it; so could a new kind of Cabinet Office—one freed at last from the theoretical constructs of economic folk-lore.

Do not be dismayed by the size of the task, nor recoil from a plan having less than academic perfection.

We have only to improve on the brain's unaided performance before our model is useful. Can you solve a simultaneous linear equation in only three variables in your head? Can our finest managers really juggle, as they often believe, with—say—even seven variables at a time? They cannot. And how large must a system be to compete with a discriminatory output of 3·2 bits?

This plan would create a new model of organization, scientifically based on genuinely physiological lines. It would quantify its model in terms of lagged, non-linear feedback loops, according to calculations scientifically based on genuine servomechanics. It would drive its display of the model with an analogue computer costing £50,000 for outright purchase.

All of this, finally, may well provoke a professional gasp from you. If so, I beg you to reflect on your own theoretical construct of what is allowed to count as the reality of operational research itself.

REVELATION (2)

I come in conclusion to the second reason why this talk is entitled: operational research as revelation. This is the biblical reference. It has to do with an apocalyptic vision.

No one can any longer say whether mankind can survive. The world's leading scientists in the relevant fields seem agreed about this: man has created for himself a set of political and military crises, a set of environmental crises, and a set of societary crises which it may prove impossible to contain.

We act as though we had accommodated to the thermonuclear threat, although it is one which computably grows. The environmental threats (pollution by pesticides, by sewage, by noise, by carcinogenic urban air) are all predictable consequences of existing, researchable systems that have been underwritten by our civilization. The same is true of race problems, the issue of poverty and the rising tide of population.

All these systemic outcomes are quantifiable aspects of computable systems.

But none of them is computable in the head. We must have the models of which I earlier spoke. That means operational research. Social upheaval and threatening violence point dramatically to the need. But the Mediocrity Machine "knows" that even a major revolt of the young need not disturb us. The consensus will prevail.

The consensus will soon *be* the young.

If we apply Revelation (1) to the apocalyptic Revelation (2) we shall see how we may move. Outcomes for society are latent in the systems running now. They can be calculated. If we seek alternatives to those outcomes, we can calculate the systemic design for producing them. The technique of prognostication is not required—provided we have built those models. It is a matter for science.

At the end of this month I am going to Washington to make a presentation to the House of Representatives' Committee on Science and Astronautics. I would like to read you a few sentences put together from this presentation.

> "At present, the most obtrusive outcome of the system we have is gross instability—of institutional relationships and of the economy. This cannot last. The society we have known will either collapse, or it will be overthrown.

In either case a new kind of society will emerge, with new modes of control; and the risk is that it will be a society which no one actually chose, and which we probably will not like.

"The risk which faces us today is the probability that society will yet refuse to study the systemic generators of human doom, and will disregard the capability which already exists competent to bring these many but interrelated forms of crisis under governance.

"The systems we have already started, which we nourish and foster, are grinding society to powder. It might sound macabre to suggest that computers will finish the job of turning this planet into a paradise after human life has been extinguished.

"But that vision is little more macabre than the situation we already have, when we sit in the comfort of affluent homes and cause satellites to transmit to us live pictures of children starving to death and human beings being blown to pieces."

The solutions I shall offer in Washington are those I offer in my own country from this platform tonight. We must get on with *research* into these *operations*—before it is too late. And if every member of the Operational Research Society were so engaged, that effort would not yet be out of scale.

The action required entails big thinking—on the part of government, of managers and of scientists alike. Big thinking has unhappily become a solecism in Britain. That is why our big thinkers are so often away from home. They come and go where they are wanted.

But they are needed here.

I have often heard it said by the most elevated people in this land that the capacity to do this kind of work does not exist. That is not true. In so far as the capacity may be inaccessible at the minute, I have the temerity to say to these elevated personages:

You drove it away: you bring it back.

There are in fact two standard answers to the whole of my address to you tonight which will predictably be churned out by the Mediocrity Machines of various institutions.

The first I have referred to already. It says that while (of course) the argument and the proposal are absolutely right they come *too soon*. The solution will work one day. Just now the market is not ready for it.

You're telling me the market is not ready for it. That is just what I am complaining about. If we want revelations tonight, that is exactly why our country is steadily, systematically, failing to make the international grade.

The second answer I shall get—and predictably before I leave this room—is that I have oversold operational research. In so far as you my friends follow the lead I am trying to give, you too will have to live with that accusation. You will be told that overselling is a wicked disservice to the cause.

Never mind.

Overselling the science which offers one hope in a doom-laden situation is a venial sin. It is better than selling science short in a desperate attempt to look dignified and mature. It is very much better than selling Britain short.

The sheer immodesty of the claim to be always modest nauseates me. While I am in this chair I shall do what I think I ought to do, regardless of those very British murmurs of well-bred complaint.

I hope that you will do the same.

If it gets uncomfortable, ask yourselves this:

Who are these people, these managers and ministers, who dare to say that we have oversold OR? How would they know?

They haven't even tried it.