



# Reflections on fifty years of operational research

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This paper provides a particular perspective on developments in operational research (OR) over the past 50 years and attempts to pick out significant milestones in that trajectory. Emphasis is placed on the UK experience (rather than taking a supposed perspective independent of interactions with any specific social context); on the significance of techniques and where they came from; and on the role of social, political, intellectual, and economic factors in what has been happening to and within OR.

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## Introduction

It is a sobering experience to have lived through periods which are now seen as history and to realize that one has oneself become part of that history. My involvement with operational research (OR) since 1961 (which does not quite cover the remit of this publication, but I did my best) is probably my main qualification for writing this overview paper. It follows that I cannot pretend to give a neutral, detached assessment: to do that would be to squander my natural advantage of actually having been there, or thereabouts, for a proportion of the events that must feature in the account. What follows will be personal, though I hope that readers will, nevertheless, find my perspective illuminating.

Personal perspectives can be limiting, and in writing this I will add the limitation of geography. This account is written from a UK vantage point. Since its origins in Britain in the late 1930s OR has spread round the world and, in particular, the greatest concentration of OR has, for decades, been in the United States. There is a tendency for this fact to dominate surveys of OR activity. For example, a forthcoming book of profiles of eminent operational researchers (Assad and Gass, 2009) largely excludes non-US residents. Some redressing of the balance is perhaps appropriate.

There will be regrettable omissions. Events which seemed of prime importance at the time will get no mention, and there are those who will find particular *lacunae* inexcusable. For example, there have been some 28 Presidents of Operational Research Society (ORS), who have without exception laboured with dedication and skill and not a little low cunning in the interests of the Society

and its members. Yet, I shall completely ignore almost all of them. So it goes.

## Structure

Even a relatively compact social activity such as OR has a multitude of strands within it. There can be no single narrative which connects in a cause-and-effect way the events and developments of most interest. This presents problems for structuring this account of 50 years. To maintain an element of discipline, what follows will be broadly chronological in its ordering, but by no means continuously so.

No significant human activity is performed in a vacuum. For this reason, I will at appropriate points peg my account to developments in the larger society which have conditioned our trajectory. Henry Ford may have said that history is just ‘one damn thing after another’; actually it is not clear who did first say that, but Ford certainly did say that ‘History is bunk’. Either way we should aspire to make more sense of events. I would have liked, therefore, to say that it will ‘emphasize the interplay among an array of political, intellectual, and economic factors’. But it has already been said, by Kirby (2007), in a paper which to some extent parallels the current one (though in the specifics our arguments differ considerably).

I will refer to a range of developments in the larger society, whether national or international, which in my view have conditioned the development of OR. To assist in reading the story it may help at the outset to summarize these events and tendencies but without, yet, spelling out their OR relevance:

1939–1945	Second World War
1945–1951	Post-war reconstruction and austerity
1951–1970	Long post-war boom
1965–1985	Student radicalism, industrial militancy
1970–1980	Post-war boom falters

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1980–1990	Thatcher/Reaganomics; defeat of trades unions
1980–	Entrepreneurialism and outsourcing replace planning
1980–	Privatization; target-based incentives in public sector
1990–	Collapse of Soviet Union; general decline in radicalism
1990–	Intensified globalization; Washington consensus on development

All dates are very approximate. I need to say that not all these events and tendencies are referred to directly in the text; some are included to give a more continuous sense of historical progression. I should also point out that the events selected are in many cases international, but have different significance for telling different national stories. As one obvious example, the national experience of the Second World War and its effect on the development of OR was different between the United States and Europe, and between different countries of Europe.

### The heroic period

The *Operational Research Quarterly (ORQ)*, fore-runner of *JORS*, was founded soon after the end of the heroic period of OR, which coincided more or less with that of the Second World War.

Foundation myths are common, and not only among primitive peoples (Rippin and Fleming, 2006). They serve various roles, not least to reinforce the adopted self-image of the city/nation/group. However, whatever the factual base, the myth is inevitably selective, and the subsequent promulgation of the myth has a conservative function.

The factual basis of the origins of OR has been explored in detail elsewhere (Kirby, 2003). OR grew out of the development of radar when, in 1937 or 1938, a small group of scientists and engineers were detached to use their technical skills to improve the actual operational performance of the radar system. Their initial successes led, first, to the group having the scope of its work steadily expanded, and then to the formation of OR sections throughout the British armed forces (and then the US military also). Watson-Watt (the inventor of radar) and Patrick Blackett have both been designated the ‘father of Operational Research’ but increasingly the claims of the latter have come to predominate. His group at Anti-Aircraft Command, with their very miscellaneous scientific backgrounds, came to be known as Blackett’s Circus—and from there he went on to set up new groups at Coastal Command and then the Admiralty. The effect of these analytic groups on the conduct of the war was substantial. When the wartime history of science was written (Crowther and Whiddington, 1947) one of the six chapters was given over to OR.

Many of the mostly young scientists who left their laboratories to enter OR achieved great distinction when they resumed their conventional careers after the war. They were to include

a remarkable crop of Fellows of the Royal Society and even several Nobel Laureates.

Their legacy to OR has been as an exemplar of focussed analytic support on an issue of supreme national, even international, priority. ‘Lively minds’, or so it seemed, could bring fresh insights that had eluded generals and admirals whose thinking was trapped within conventional tramlines. Subsequent generations of operational researchers, as they deployed their computer-based models, were invited to see themselves as inheritors of their mantle—free but responsible intellectuals, applying the natural scientific method to the common good.

A foundation myth is, of course, always self-serving—or we would scrap it and get a better one. This one allows us to catch some reflected glory from those who seized their hour. But it also obscures some aspects of the wartime experience that do not fit well with the post-war OR experience. In short

- (i) OR as practiced then and now exhibits some science-related virtues but certainly not the full scientific method—it has more in common with technology (Keys, 1989, 1998).
- (ii) None of the work done during Second World War employed any of the techniques we now think of as composing the OR toolbox. Indeed, Blackett himself became highly critical about the move towards optimizing techniques originating from the United States (Blackett, 1962).
- (iii) A significant number of those tools—in stock control and queuing theory for example—had arisen in industrial contexts before the Second World War, and so predated the emergence of OR. Even linear programming (LP) was first developed by Kantorovich in the Soviet Union in 1939, work which was central to his later Nobel Prize (Rosenhead, 1982).
- (iv) Many of the British scientists who joined the OR effort in the Second World War were socialists. Particularly influential was Bernal’s view that science could not deliver its full beneficent capability under capitalism (Bernal, 1939). Bernal himself worked in OR during the war. In the post-war period Blackett, despite his scientific eminence, was regarded as so politically suspect that he was not allowed to enter the United States, and was indeed arrested when the plane he was on had to make a refuelling stop there (Rosenhead, 1989a).

There are clearly other stories to tell here, and some of them have more richness than the conventional story of how OR developed.

### Austerity and post-war reconstruction

The Operational Research Club, fore-runner of the ORS, was formed as a result of a select dinner at the Athenaeum club in London in the autumn of 1947. By an extreme act of piety,

or maybe just by happenstance, there has been a tradition for several decades that the ex-Presidents of ORS welcome each new President at the start of her term—with lunch at the Athenaeum. Continuity is the thing.

I was born around the time that OR came into being. As it happens I subsequently met two of the four diners—Sir Charles Goodeve and the great Patrick Blackett. And I have written extensively about a third participant, Cecil Gordon. Sir Henry Tizard, a leading scientific mandarin of the period, escaped.

Cecil Gordon was the key protagonist of transferring the new approach of OR from the military to civilian government departments. In the end he was defeated, and in autumn 1948 left the Board of Trade for the University of Edinburgh. One of the civil servants, who achieved this victory over OR confided in a memo that his work had ‘achieved its purpose of ‘de-bunking’ the subject’. The focus of activity, he said, had now shifted to the newly formed OR Club, ‘which has decided to publish a quarterly bulletin on operational research, giving case studies, since it finds it hard to agree on anything else’ (Rosenhead, 1989a). With civil servants like these, who needs enemies?

The result was that, barring a few outposts (eg, the Building Research Establishment at Watford) OR developed in the United Kingdom largely outside government. Nor were there for quite some time significant centres of academic activity. Initially the nationalized industries were the most prominent users, though as time went on private industry and then non-industrial businesses grew in significance. And then, eventually, the public sector was enjoined by government to behave more like private businesses, which of course facilitated their privatization. But most of this is running well ahead of our timeline.

Certainly private industry has a low profile in the early volumes of the new ORQ. Also note the reference to the reporting of case studies by the civil servant quoted above. Techniques are largely absent; only gradually do a few papers on LP begin to appear.

There was of course a problem, in that most of the original operational researchers had left the field, and had not in any case developed or made use of any generic OR techniques during the war. If OR was to continue it needed to transmit some core ideas to new recruits. Blackett himself participated in a series of lectures on OR in Manchester (where he was a Professor) in 1949/1950—a sort of mini training course given in the evenings. It was still running in 1961, when I made my weekly pilgrimage over the Pennines from United Steels in Sheffield to attend.

By then some Universities had already started full-time Diploma and Masters courses, with LSE and Birmingham in the lead. And naturally they were stuffed full of techniques. Some were borrowed (with elaborations) from nearby disciplines, especially statistics; some, like stock control and queuing theory were recouped from prewar industrial developments; others (Critical Path Analysis, Dynamic Program-

ming, Game Theory and extrapolations from LP) were being developed in the United States, particularly at the RAND Corporation. Computer simulation, harder to teach than the others because less mathematical, was largely a British contribution to the repertoire. By stages these techniques came to define what OR ‘was’.

An illuminating episode is the 6-month visit that Pat Rivett, head of the National Coal Board (later British Coal) OR Executive (then and for 40 years the flagship UK OR group), made to the United States in the late 1950s. He spent 6 months at Russ Ackoff’s group at the Case Institute and ‘got’ techniques in the process. He came back to the United Kingdom and instituted a crash course in OR techniques, especially LP, for all the staff. The result was that practical work virtually came to a standstill for some weeks. But LP was rarely put to use at the Coal Board.

### The long post-war boom

The virtual demise of OR in UK civil government, described in the previous section, coincided with the onset of the Cold War. Indeed there is some evidence that the ejection of OR was connected with the ‘left’ associations of many of those practicing or urging the governmental practice of OR in the early post-war period (Rosenhead, 1989a). It was not just that particular individuals with a favourable orientation to the Soviet Union might now be perceived as security risks. It was that the whole idea of state intervention and planning became suspect.

Instead OR began to take hold in industry. In this achievement the indefatigable, nay evangelical, labours, in particular, of Sir Charles Goodeve were crucial (Kirby, 2000). The major nationalized industries became the key OR players—notably coal, as already mentioned, but also rail, electricity generation, gas, airlines, and so on. What was unacceptable in central government became the norm in these enormous publicly owned enterprises. This paradox is explained by the ‘managerialist’ structure which the Labour government devised for them, at arms length from government and with all vestige of popular involvement in their running safely excluded.

The early 1950s saw the start of the long post-war boom. Little by little OR began to penetrate private industry. In fact, steel had been from the early days the major exception to the public sector domination of UK OR. Like the nationalized industries, steel was seen as offering those ‘large systems of men, machines, material, and money’ (to quote from a long-standing ORS definition of the subject) which OR had been so successful with during the war. But now OR groups began to be formed outside heavy industry—in brewing, textiles, confectionery, and so on (Kirby, 2003).

At the start of this period, techniques were little known or practiced in groups. The predominant philosophy was one of problem—rather than technique—orientation. However, there began during this period the gradual and continuing elevation

of techniques as the rationale for the existence of OR, and as the material for the *ORQ*. In its early volumes, case studies were willingly published as evidence of further fields in which OR could be helpful. This contrasts with the present situation in which, more or less, a case study can only be accepted by *JORS* if it shows, or purports to do so, a theoretical innovation.

For many years Keynesian economics appeared to have banished recession, and economic growth was nearly continuous through to the end of the 1960s. This was a period in which exponential growth was widely but imprudently expected to be never ending. Corporations were willing to invest in their own OR groups (as well as other head office functions); and the first chair (Pat Rivett) and department of OR at a UK university was established at Lancaster. *JORS* membership soared. My membership number when I joined in 1961 was around 907 but by the end of that decade ORS was 3000 strong. And then it plateaued for over 30 years. Only in recent years has there been a 10% drop (though the membership composition within those numbers has changed radically—notably in the increase in the number of academics). Very similar patterns have occurred in the membership of OR societies across the developed world.

Over the period of rapid growth the optimism and ambition of the leaders of the profession, here as elsewhere, was boundless. Proposals were made for OR to tackle new fields, and more strategic issues in old fields. The fashion for corporate planning in industry, linking operations to corporate objectives in a top-down manner, seemed like one ideal opportunity. Inventory and production control and distribution (or logistics as they are now called) seemed by contrast less glamorous and tended to be neglected.

In the United States, one of the growth points for large-scale modelling had been the development of land-use/transport models to justify investment in major projects; indeed at one stage this was a precondition for the receipt of federal funds. These models made huge demands on data, computing power and theory (of the effect of the new infrastructure on travel and on locational behaviour). Sadly it was eventually realized that these models were impossible to maintain, and in any case simply failed to predict what actually happened. The death-knell for such models was sounded in a celebrated paper by Lee (1973).

I know of no such single turning point for OR in Britain. However, here also the stalling in the economy and in society membership was paralleled in the disappointment of OR's ambitions. There followed a period of general malaise—perhaps for clarity, and ominously, we should call it Malaise 1. (Kirby (2007) designates it rather as 'crisis.')

Operational researchers found themselves excluded from the tempting possibilities of setting the organizational direction by optimizing the corporate objective function. There followed some years of intense debate in which this riddle was endlessly explored. We were clever. We had excellent techniques and formidably powerful computers (we then thought). They found optima. Why did managers not want to

be given the best solutions? Why were we and our computer models not welcomed in the places where the really important decisions were made?

Various solutions were proposed. The dominant consensus was that there was nothing wrong with the product offering. Therefore, the problem must lie in awareness (the key managers did not know OR was around), or in communication (we were not doing a good enough job in promoting the value of what OR could do). Solutions proposed were, for example, running appreciation courses for managers or hiring a PR firm.

It is, of course, easy to be clever after the event. But knowledge of OR's prehistory could have provided part of the answer. One fore-runner of OR was Scientific Management. Under the leadership of Frederick Taylor in the United States from the 1890s to the start of the First World War, Scientific Management sought to revolutionize the conduct of management (Taylor, 1947). Operations that had been left to custom and practice became the object of stop-watch study. Scientific Management developed not only time-and-motion but innovations in job design, reward systems and many other aspects of the control of work—for example, the Gantt chart. Taylor himself proposed a central 'planning department', staffed by engineers like himself, as the nerve centre of the enterprise.

It did not happen. Many of the tools and techniques developed by Taylor and his disciples were indeed adopted piecemeal by management and still remain in the organizational repertoire. But the integrated planning system that he envisaged was not implemented anywhere. Putting ourselves in the position of those managers of 100 years ago, we can perhaps appreciate a doubt they may have had as to whether engineers would have the rounded competence to, in effect, take charge. And no doubt at all, from a power perspective, as to whether they should cede control to these technicians.

Fifty plus years after that, a similar scenario unrolled for OR.

### Choppy water

Events, processes and even milestones do not always fall into neatly defined periods. They sprawl, or arrive rather earlier or later than the other features that it would be helpful to describe them with.

Malaise 1 was long drawn-out; indeed it maybe that it was not so much pulled out of as accommodated to. That is, the OR communities in the United Kingdom and round the world became used to their straitened circumstances and came to regard them as their natural habitat.

Large-scale modelling enterprises still took place during the 1970s. In the United Kingdom one of the most prominent of these was the Balance of Care model, intended to guide health service agencies in the allocation of categories of patients between institutional and community care. (See Rosenhead,

1984.) It was the most resource intensive non-military OR study carried out within government upto that point. Naturally, this study started as a LP model, which in effect tried to minimize costs. A more elaborate version followed, which enabled fractional parts of the clinically recommended care packages to be delivered, and also tried to infer a non-cost objective function. However, the solutions proved unstable against small changes in parameters. A cut-down version was then developed which removed the optimization routine, and allowed the model to be used in ‘what-if’ mode. As such it continues to be modestly useful.

This lesson—that it was better to provide tools which empower clients than to produce solutions based on computational virtuosity—was gradually gaining ground at this time. However it was by no means received wisdom by the time the UK OR community was hit by the Ackoff Papers (Ackoff 1979a, b). These were surely the two most influential papers ever published in *JORS*. In fact, their impact was felt most powerfully the year before, when Ackoff delivered them at the beginning and end of that year’s ORS Annual Conference.

Ackoff Paper no. 1 at the start of the Conference delivered a devastating critique of the contextual insensitivity of OR, as well as its lack of ambition to make a difference to society. This by now traditional OR restricted itself to mathematical operations on quantitative data, and hence was being progressively relegated to the lower tier of organizational concerns, where such data was indeed repetitively generated. The effect of this attack was amplified by Ackoff’s emphatic delivery. Ackoff’s second paper at the end of the Conference offered his alternative (which he called Interactive Planning) to the orthodox OR approach. However, the genie was not put back even into this different bottle. Conference buzzed with discussion of the demolition job they had all witnessed.

The shock waves continued once delegates left. In May of 1979, a conference on ‘The Ackoff Papers’ was held at Bath. In December, the OR Society held an official National Event at the Royal Society on the topic ‘The Ackoff proposals: is the Debate over?’ Some of the papers presented later appeared in print—see Tobin *et al* (1980) and Dando and Bennett (1981).

It is no exaggeration to say that for British OR the Ackoff Papers were a significant milestone. Afterwards, the OR community became more intellectually alert and agile. Once a dialogue on the basis for and purpose of analytic support was opened, it became inescapable to employ abstract non-quantifiable concepts on which the operations were logical or forensic, but not mathematical. Ideas from systems thinking were part of this change, and there was a revival of interest in the relationship between OR and the Social Sciences. This culminated in an International Conference on the topic (Jackson *et al*, 1989).

*JORS* became an, no the, important journal for the development of this discourse. One small symptom of this was the role of the Comments section. Previously this had served a valuable function for correspondents to dispute the validity of proofs, and the like. Now it began also to host intense and

sometimes erudite discussions in which the implications of taking different points of view on the process and conduct of OR were explored.

### New vistas

It is undoubtedly the case that there have been major new developments in the more technical branches of OR since the burst of activity in the 1950s. Thus, Goal Programming and Data Envelopment Analysis were largely the invention of Charnes and Cooper in the United States, and System Dynamics sprang from J Forrester at MIT; Combinatorial Optimization and Decision Analysis had a somewhat more distributed parentage.

British operational researchers working in these fields have of course made extremely valuable contributions to the development of these and other fields. Though it is perhaps invidious to name names, notable UK work has included Land in branch and bound, Kendall and Cox in applications of queuing theory, and Kelly on routing in network queues. Other areas where UK researchers have opened up new fields include bandit problems, hidden faults models, credit scoring, etc. However, I cannot resist the conclusion that in weight and significance of achievement there is no comparison. It is a sobering thought that there is no major technical field since computer simulation in the early 1960s, which predominantly originated with work carried out outside the United States.

This makes the development of not one but two new fields—Problem Structuring Methods (PSMs), often referred to as ‘soft OR’; and Community OR—in the United Kingdom during the 1980s a matter of some note. Neither of these is a technique in the sense that mathematical programming is, and the latter is to do with an extension of the client base. However, set against the differential fecundity of US OR over the preceding decades they are clearly noteworthy. Each has roots in earlier periods, but their coming into bloom, as it were, in this period seems to owe a good deal to the creativity and openness unleashed by the Ackoff Papers.

In what follows readers should bear in mind that as a participant in both of these developments I am an inherently unreliable witness.

### PSMs

Over all the nearly 50 years that I can speak for, British OR has had a distinctive character when compared with its US or indeed its European equivalents. To avoid undue length I shall focus on the comparison with the United States. In the United Kingdom, academia plays and has played a far less dominant role in the community. One indicator is the alternation between academics and practitioners as Presidents of the Society, no longer completely regular but still broadly adhered to. And indeed the current President is a practitioner. Academics have maintained a close connection with the world of practice, by a variety of mechanisms. For many years these included the recruitment of academics from middle-rank

to senior practitioners; however, the increasing target-based pressure on universities has now made this less feasible.

The greater sense of pragmatism which this relationship engendered can be seen at its fullest in the development of the Strategic Choice Approach (SCA) the first of the bundle of approaches now known as PSMs to be developed. It originated from the Institute for Operational Research (IOR), a joint venture of the UK Operational Research Society and the Tavistock Institute for Human Relations, brokered in 1962 by Russ Ackoff on an earlier UK visit. Early in its life IOR obtained a Research Council-funded opportunity to observe over an extended period how decision-makers (in a substantial local government unit) debated and came to conclusions. From this beginning the SCA has been developed and honed over 40 years. It now consists of an array of tools and an articulated interactive process to assist decision-makers to make progress through their deliberations (Friend and Jessop, 1969; Friend and Hickling, 2005). Though this body of work has merited a special edition of the journal *Planning Theory* (Mandelbaum, 2004), it is remarkable almost as much for its lack of theory in the normal sense of the word as for its extremely sophisticated decision support.

Other component PSMs were developed independently but in parallel—Soft Systems Methodology from the 1970s (Checkland, 1981), Robustness Analysis from the late 1960s (Gupta and Rosenhead, 1968), Strategic Options Development and Analysis from the early 1980s (Eden *et al.*, 1983), and Metagames (later Drama Theory) from its roots in the 1970s (Howard, 1971). Thus, virtually all the elements of this development were in place, at least in their early stages of development, well before the Ackoff Papers. Yet, it was only in the ferment that resulted from the Ackoff intervention that their commonality of stance became clear. Indeed they, rather than the proposals in Ackoff Paper no. 2, became the distinctive response of the UK community to the challenge set in Ackoff Paper no. 1.

When gathered together in Rosenhead (1989b)—see also Rosenhead and Mingers (2001)—they could be recognized as a paradigm shift within OR (Kirby, 2007). The first edition of *Rational Analysis for a Problematic World* was identified by the then editor of the *JORS* as the most referenced book by the Journal's authors in the following decade (Ranyard, 2000). Five of the 13 authors whose papers in *JORS* had been cited more than 100 times during 1981–1999 were contributors to *Rational Analysis for a Problematic World Revisited* (Ranyard, 2001).

In summary, PSMs are model-based group decision support methods which

- can structure complexity of content and represent it in a transparent manner;
- are deployed in a facilitated group environment;
- develop model structure interactively;
- incorporate tools to encourage participation and generate commitment to actions.

Virtually none of these attributes of 'soft OR' apply to traditional OR approaches, increasingly, known as 'hard OR'.

These characteristics gave PSMs access to classes of problems that 'hard' OR techniques could not reach—where the problem 'is' could not be determined consensually ahead of and independently of the process of intervention. It now became possible to deal with messy problems where different stakeholders legitimately could (and did) hold contrasting perspectives on what factors were relevant, what objectives might be desirable, and so on. So rather than tackling supposedly objective problems of forecasting, allocation, in process throughput and the like, what was needed and possible was to deal with problem identification, system design, partial commitment under uncertainty, cooperation, and conflict.

There was now, however, a dual challenge for the consultant. ('Analyst' as a job description no longer quite fits.) She needed to have a firm grasp of the technical possibilities of the structuring language being deployed (and of other PSMs that might prove useful as the engagement evolved). But she also needed to have a natural or trained sensitivity to group processes and to the delicate but powerful potentialities of the facilitator role. Time management was of the essence, as the existence of the group was a rare, expensive, and possibly irreproducible opportunity. A successful intervention would be one in which the structuring exercise had generated a clarity and confidence to take action in what in many cases had previously seemed a daunting and confusing morass.

PSMs are still young but are generating an impressive track record both practically and in research terms, and in international spread. For applications see, in particular, Vidal, 2004. In the academic community it has spawned both new developments of particular PSMs, and a theory and practice of combining methods (Mingers and Gill, 1997). Wide acceptance is shown by streams of papers at national and international OR conferences, and special issues of journals both in Europe and Latin America.

Perhaps, the most surprising accolade came from the UK's Engineering and Physical Sciences Research Council (EPSRC), the heartland of British 'hard' OR. In 2004, EPSRC convened an international review panel with members from New Zealand, the United States, France, and the Netherlands as well as Britain. Among its other conclusions it found 'soft OR' to be among the strong sub-fields of the discipline here. But it went further—it identified 'soft OR' as one of only two 'unique selling points of significant strength within the British OR research agenda' (EPSRC, 2004).

### *Community OR*

The genesis of this novel strand of OR practice lay, directly, in the observation that OR tended to work almost

exclusively for work organizations that were controlled through a hierarchical structure. Those interests of individuals or groups not served by these organizations did not, therefore, receive the advantage of OR-based analysis. Indeed where the goals and practices of organizations clashed with those of more loosely associated groups, the latter could find themselves OR-ed against.

Perceptions do not just happen, and this one had a prehistory of some 15 years. Indeed the history of Community OR is entangled with the history of PSMs. There are two dimensions to this entanglement: that many of the individuals involved in launching one of these fields also had roles in the other; and that PSMs bulk disproportionately large in the practice repertoire of Community OR.

The 1960s and 1970s were a period of significant youth radicalization round the world. Even though OR presented and thought of itself as scientific and apolitical, a generation of young operational researchers were not immune to these tendencies in their environment. There were some who thought that if their political views were serious then they should apply also to their own economic link with the rest of society—their work.

In Britain, in 1970, the ORS under the Presidency of Stafford Beer undertook a major internal shake-up. Part of this was to be the establishment of professional grades of membership with examinations and periods of apprenticeship. Later this transmuted into a proposal for ORS to establish a Register of Practitioners. The proponents of these changes were principally consultants and government operational researchers who each saw legitimate advantages in having letters after their names. The proposal had the full blessing of the ORS officers and committee, but was opposed by a loose formation of younger operational researchers infused with the values of democracy and active participation. A long-drawn out conflict ensued (the details are not particularly edifying) which ended in December 1973 with overwhelming votes against all versions of the proposal. (It is worth noting that at precisely the same time a crisis of comparable magnitude shook the Operational Research Society of America, with the focus on possible disciplinary action against members who used their analytic skills to criticize the US Anti-Ballistic Missile policy (Ezrahi, 1980).)

Back in the United Kingdom, the activists who had opposed professionalization developed a proposal for an Institute of Critical OR to work for marginalized social elements. This proposal was put to ORS—to no effect. And there it rested until a number of senior OR figures, including consecutive Presidents, raised the professionalization proposal once more. In 1984 your author was prevailed upon, by those opposed to professionalization, to stand for the forthcoming vacancy for ORS President, and was elected to general consternation. (There has been no contested Presidential ORS election since.)

My election manifesto put forward ideas drawn in part from the 'Institute of Critical OR' proposal of nearly 10 years

previous. These were then amplified in my Presidential address (Rosenhead, 1986). This advocated

- (i) addressing OR to a broader range of clients than had so far been achieved, and in particular to grass-roots groups characterized by lack of resources, and consensual and non-hierarchical decision-making;
- (ii) developing further the incipient participative model-based methods that enabled groups to reach agreement on the nature of their shared problem;
- (iii) applying OR to the overwhelming predicaments with a systemic aspect which perplex society, rather than principally to questions of efficiency within individual organizations.

Item (iii) remains as a challenge to OR. Item (ii) was an early advertisement for PSMs, as yet, barely recognized as a coherent whole. Item (i) gave a mandate for ORS to develop what became known as Community OR. The Community Operational Research Unit was set up at Northern College in Barnsley, though now relocated to the University of Lincoln. Over the subsequent 20 years there has been a unit at the University of Hull, and a Community OR Network. Work has been carried out, for example, for tenants and residents groups, for a parent teacher association, for single issue protest groups, for voluntary organizations, for a community redevelopment bid, for a feminist collective, for an anti-poverty alliance and so on. See Mar Molinero (1988), Thunhurst and Ritchie (1992), Thunhurst *et al* (1992), Ritchie *et al* (1994), Bowen (1995), Rosenhead and White (1996), and Midgley and Ochoa-Arias (2004).

That clientele is worth considering for a moment. Contrast it with the usual sponsors of model-based analytic work. Contrast it, for example, with the winners of the annual Edelman Prize, the practice prize offered by the US society INFORMS. Counting back from 2008, the winners have been Netherlands Railways, Memorial Sloane Kettering Cancer Center, Warner Robbins Air Logistics Center, General Motors, Motorola, Canadian Pacific Railroad, Continental Airlines, and Merrill Lynch. An equivalent prize in the United Kingdom, were it to exist, would very likely have a different mix between private, public, and government. However, it would surely consist, just the same, of powerful organizations whose members are paid to work for them and are within a top-down control structure. Community OR opens a window on the rest of the social world.

Both PSMs and Community OR remain a small part of OR practice in the United Kingdom. (Smaller elsewhere, and invisible in the United States.) The reasons are multiple. There is the culture of our practitioner groups, and of the university departments which provide the entry points to practice. There are the expectations of actual and potential client groups, based, for example, on previous experience, as to what sort of services OR can offer. And for Community as opposed to conventional OR there is the differential

opportunities for employment and a viable career. Nevertheless, both have established a toe-hold and show signs of take-up in other countries.

## Malaise 2

I have devoted some space to these developments, because I think they offer significant future opportunities for OR. However, it would be completely untrue to suggest that they were the predominant concern of OR communities in the years since 1980, either nationally or internationally. The concern, rather, has been with a perceived decline in the profession's embeddedness in society. I have called this 'Malaise 2'.

I think we can distinguish Malaise 1 from Malaise 2 as follows. Malaise 1 was a crisis of disappointed expectations—it became clear that OR as then understood would not take over or even significantly penetrate decision-making at the highest levels in public or private enterprises, or in government. Malaise 2, which manifested itself from the early 1980s at least to the end of the century, was more sombre. It saw evidence of shrinking horizons and opportunities for OR, and expressed trepidation about how far this tendency might go. The two major UK 'milestones' of this malaise were the Commission on the Future Practice of Operational Research (Operational Research Society, 1986), and the project on The Success and Survival of Operational Research Groups (Fildes and Ranyard, 1997). The former, for example, 'was established out of a widespread apprehension of an impending, or even an actual, crisis confronting operational research'. These milestones can be seen, I think, as reactions to stages in the restructuring of work, both globally and in Britain.

In the early 1980s, President Reagan in the United States and Margaret Thatcher in the United Kingdom pursued the shock therapy of vigorous monetarist, anti-union, and anti-protectionist policies. In Britain manufacturing industry was stripped out; there was the sharpest 1 year drop in output since 1921; and unemployment went up inexorably to over 3 million. Many of OR's traditional clients, if they survived, were forced to cut head office overheads. Many long-standing OR groups were lost.

There was more to come. From 1984, the privatization took place of virtually all the main publicly owned utilities—telecoms, gas, water, electricity, British Airways, steel, etc. By the end of Thatcher's term of office in 1990, there had been more than 40 privatizations, with the railways to follow under her successor. OR had been particularly well entrenched in the publicly owned industries. Some of the OR groups survived and flourished, for example, at BA. But where an industry was reconfigured as competitive groups, rather than as a monopoly, there was no obvious home for the previous central analytic group. (The group at British Rail is a counter-example: it stayed in being to service the multiple train operating companies.) And the national flagship OR

group at British Coal could not survive the virtual elimination of that industry after the 1984–1985 miners strike.

And yet there was more. Before these changes Britain had been a substantial power in manufacturing and extraction. Afterwards, it was an economy based around financial, business and personal services. Accompanying this transformation was a change in management structure and style. The top-down bureaucratic organization held together by planning was replaced as an ideal, and in a good deal of practice, by enterprises with flat organizational structures, configured into relatively autonomous units with an entrepreneurial mission (see, eg, Mintzberg, 1994).

These two reports represented valiant attempts by the representative body of the UK OR community to comprehend the changes in its environment, to assess their significance for the longer-term future of OR, and to devise policies and stratagems to guard against identified threats. In fact, not a great deal happened in terms of identifiable policy shifts by the ORS itself. Perhaps their major effect was to generate within the community at large an awareness of the changing shape of OR's potential clientele, and the need to adapt it.

It can also be argued that the development of PSMs, under way at the time that these reports were commissioned, was in itself an adaptation to the new conditions. Traditional OR was largely predicated on the existence of a client organization with unified objectives and able to implement recommended decisions through its chain of command. PSMs, by contrast, were appropriate for more fluid decision contexts. Representatives of different stakeholders, each of whom had a share of the relevant information and responsibility, could use PSMs to agree a problem specification and a way forward in cases where hierarchical control was not available or appropriate.

British OR also received an unexpected fillip from the Conservative government of Margaret Thatcher. OR, having in effect been ejected from civilian government after the Second World War, had found its way back under the Wilson Labour government of 1964. The climate was by now more conducive to analysis and to planning. The election of Edward Heath's Conservatives in 1970 made little change—and indeed in 1971 Heath set up the Central Policy Review Staff, otherwise known as the 'Think Tank'. This was intended to provide high-powered advice to Cabinet through a stronger central policy capability. (Several of its members had OR experience, though no recognizably OR studies were carried out.)

From 1979, the Thatcher government set about dismantling the conditions under which OR had prospered, both in government and the public agencies and industries. (In 1983 she abolished the CPRS, and no government since then has recreated any equivalent.) Explicit government planning was contrary at least in principle to the ideal of the primacy of market forces. It might, therefore, have been expected that government OR would shrivel and decay during her premiership. In fact, the reverse happened. OR in civilian government entered into a golden period of rapid and continuous expansion.

The apparent paradox is explained by a change in the nature of the OR work undertaken. The replacement of planning by market-driven competition is no simple matter, especially where there is a natural monopoly (eg gas, water, and electricity supply). When these industries were privatized, complex mechanisms with associated calculations were needed to allow market-like processes to operate. There are other parts of the public sector for which neither Thatcher nor her Conservative and Labour successors have (as yet) been able to complete the privatization—eg police, prisons, health service, and education. Here the analytic problem was different: to set up quantitative targets with associated data collection and monitoring which could serve as substitutes for the price signals in a market. Another big growth area has been in attempts to control expenditure on social security and so on. Although some more conventional OR work within government has continued, the striking growth has been in analytic work concerned to make the Thatcherian revolution operational.

It therefore seems only appropriate to identify the election of Margaret Thatcher in 1979 to be a milestone for British OR.

### The Britishness of British OR

Organizational restructurings similar to those which produced Malaise 2 in Britain took place contemporaneously in the economy of the United States. However, the official response was quite different there from that of ORS. The most significant statement was from INFORM's Committee on the Next Decade in Operations Research (CONDOR). This was an almost uniformly optimistic message based on OR's existing strengths: further development of techniques of greater virtuosity and wider scope would see OR through to a positive future (CONDOR, 1988).

A more introspective paper in the US OR literature (Corbett and van Wassenhove, 1993) puts this response in perspective. The authors suggested that OR had been losing the attention of decision-makers, and that this could well be related to the process known as 'professional regression' (Abbott, 1988) by which professions withdraw into themselves. Internal status rankings based on the knowledge system that gives the profession its special claim tend to be correlated with remoteness from practical concerns and implementation. The CONDOR report illustrates this tendency in operation in OR.

A theme which has emerged periodically in this selective historical account of OR has been that OR is not the same everywhere. Specifically, it is not the same in the United States and the United Kingdom. OR is a social activity. Its practice at a particular place and time is influenced by the society in which it is embedded, and also by the inheritance of ways of doing things acquired through academic or practitioner apprenticeship.

The United States is by far the most powerful OR community in the world, both numerically and in terms of the

contributions they have made to OR's technical repertoire. An added factor, for non-US academics, is that the differential citation by US academics of papers in their national journals makes the latter, bibliometrically, the world's 'top' OR journals. This exerts a pressure gradient on research around the world—to get papers into these 'top' journals and to enhance one's career, it is necessary to select one's research programme to fit with their priorities. This influence towards conformity and away from diversity is not conducive to the health of OR in countries other than the United States (and maybe not in the United States either).

There is, in fact, a distinctive OR tradition in the United Kingdom, some elements of which have been touched on in this paper. Broadly speaking, the balance between academics and practitioners within the community has been different from that in the United States. One indicator, already mentioned, is the long-term alternation of academic and practitioner Presidents of the OR Society. Another is the makeup of the membership. A former INFORMS President has stated that 75% of that Society's membership have PhDs (cited in Sodhi and Tang, 2008). A survey of OR Society members in 2007 suggests that around 28% have PhDs, but only 11% have PhDs in OR (Operational Research Society, 2007).

One aspect of the UK OR community frequently remarked on down the years is the strength and breadth of the relationship between the academic and practitioner groups within it. It seems to me that this has generated, even among those who are developing theory and methods, a primary concern with problem solving or even with problem formulation. There are, of course, those who are concerned virtually exclusively with the challenges of what can be done differently, better or quicker within their domain of expertise. But the general tone is against theory and technique (just) for its own sake. There is an anchor to the potential practical utility of particular engagements with theory.

It could be argued that this characteristic of UK OR is both a strength and a weakness. The weakness, relative to the record of the United States, is a lesser intensity of technical creativity. The strength is the maintenance of the dialectical link between theory and practice which strengthens both, and should help to counter the tendency to professional regression.

### Conclusion

It is the nature of history that it has no conclusion. It is also true that any conclusions one attempts to draw are necessarily time-bound. There is no fixed viewpoint from which an objective perspective can be found. As illustration of this, I should in fairness draw the attention of readers to an earlier historical analysis of OR of which I was co-author (Rosenhead and Thunhurst, 1982). There are some differences, and not all because OR has in the mean time added more than 50% to its life experience.

This paper is my current attempt to make some sense of the radical transformations that have engulfed an analytic practice

that has kept the same name while drastically reinventing itself. No that is not quite right. OR has not simply reinvented itself, it has at the same time been subject to redesign by larger forces at work in society, which themselves are evolving under their own dynamic. This has been said before: 'Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves but under circumstances directly encountered, given and transmitted from the past' (Marx, 1963). I think that will do as a conclusion.

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