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On the application of mathematical models in maintenance

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Abstract

This paper may be seen as an appeal to maintenance modellers to work with maintenance engineers and managers on real problems. Such collaboration is essential if maintenance modelling is to be accepted within the engineering community. It is also particularly important in the design and building of maintenance management information systems if such systems are to be used to manage and operate maintenance policy in the new millennium. In this context, developing areas of maintenance modelling are discussed, namely: inspection maintenance; condition based maintenance; maintenance for multi-component systems; and maintenance management information systems. Some new models relating to capital replacement are also considered. Thus, we are concerned with the mathematical modelling of maintenance rather than with management processes relating to maintenance. Discussion of maintenance management information systems is included because of their importance in providing data for mathematical modelling and in implementing model-based maintenance policy. © 1997 Elsevier Science B.V.

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1. General observations

1.1. Introduction

It is exciting to be involved in the development of what is a growing subject. It has been recognized (Christer, 1984) that the subject of maintenance modelling has been a late developer. This is because maintenance is characterized by the fact that actions are carried out on plant and not product, and thus maintenance is viewed by many as a marginal activity. Difficulty arises when trying to relate spending on maintenance to changes in production performance. Of course, this is not to say that industrial performance cannot be improved through improvements in maintenance activity, and with maintenance expenditure on a par with defence budgets, the opportunities for cost savings are significant. Other authors (e.g. Pintelon and Gelders, 1992) have covered the rôle of maintenance modelling in an industrial context, and a framework for maintenance modelling has been proposed (Gits, 1986a,b,c).

There is, however, a fundamental challenge facing the mathematical modelling of maintenance as a subject. The success of the subject, which is strictly a branch of *applied* mathematics (Operational Research or Statistics, given your own personal viewpoint), can only be measured in terms of its impact upon the solution of real maintenance problems. New theory is appearing at an unprecedented rate (recent reviews confirm this, see Cho and Parlar, 1991, for example). If this theory is laying the groundwork for future applications, then this is perhaps sufficient justification for it. However, at present, too little attention is paid to data collection and to consideration of the usefulness of models for

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solving real problems through model fitting and validation (Ascher and Feingold, 1984). Too much attention is paid to the invention of new models, with little thought, it seems, as to their applicability.

Surely then, the mathematical modelling of maintenance is by definition *applied* mathematics, and it appears that much of the current literature is neither part of *pure* mathematics nor part of *applied* mathematics. Is it not the case that modelling developments must pass through the conceptual modelling stage to industrial application, with the associated validation and verification that this implies? Only then can they be considered as research developments in maintenance modelling.

Such a view of a developing area of *applied* mathematics is not new. For example, the comments of Tukey (1962) on the then developing field of Statistics are enlightening, and highly relevant to the field of mathematical modelling in maintenance. To paraphrase the strong views of this author, pieces of mathematical modelling in maintenance that "fail to contribute, or are not intended to contribute, even by a long and tortuous chain", to maintenance practice "must be judged as pieces of pure mathematics, and criticized according to its purest standards". Further, individual parts of mathematical modelling in maintenance "must look for their justification" toward either maintenance practice or *pure* mathematics: work "which obeys neither master, and there are those who deny the rule of both in their own work, cannot fail to be transient, to be doomed to sink out of sight".

Some technical developments may be needed, some what-if analysis may be done and prototyping studied; but all of these may not, for good reason, get as far as implementation.

It could be argued that mathematical modelling in maintenance faces a still deeper problem. This is, more often than not, the absence of sufficient data relating to the maintenance problem of interest (to the decision-maker) for plausible models to be fitted and validated. This is further hindered by the complexity of the models that are often proposed. Thus, how is mathematical modelling in maintenance to develop if it is to be judged by its success in tackling real problems, when there is insufficient information available to judge this success?

This paper seeks to highlight this fundamental problem facing the subject. Of course there is no quick fix; modellers must be encouraged to collaborate with managers and engineers, and such collaboration must be rewarded, not just through the successful application of models, but through the recognition of peers. The value of papers which address the modelling of maintenance decision problems but which do not address a real application must be considered carefully by scientific journals, through their editors and referees. Applied research has for too long been viewed as somehow less worthy or intellectually demanding than theoretical research; one suspects, however, that this view is strongest among those who have never attempted it.

Modellers should also consider what might be gained from:

1. restricting attention to simple models, and approximate solutions to problems of interest to decision-makers;
2. investing proportionally more effort working with engineers and managers, and in the collection of data relating to problems of interest.

By data here we mean not just specific figures relating to failure time, etc., but all information relating to the process of interest. In fact it is often the latter, rather than the former, that prove most enlightening in any practical context. The classic confusion over estimation of the "hazard" function from failure "times" (Ascher, 1992), when no information about the failure process is available, is a case in point.

1.2. Simple models

By simple models above we mean those with a small number of unknown parameters. For example, the age-based replacement model for a component, which is renewed on replacement and with, say, a two-parameter Weibull time to failure distribution (Barlow and Hunter, 1960), would be regarded by many as simple. Only a small number of observations of time to failure (~ 10, say) would be required to determine a near-optimal value of the critical age for preventive replacement (Baker and Scarf, 1995). However, even examples with a dataset of this size may be hard to find, particularly as in reality such observations of time to failure would be censored.

Component test data may be sufficient in number, but environmental factors are likely to be different than for components in situ. Thus for complex systems comprising many different components, the actual maintenance problem may just be the sensible organization of preventive maintenance work into collections of tasks based on subjective values for the critical ages for preventive replacement of components, rather than on the search for some optimal solution in terms of some precise (and thereby inadequate) criterion. Surprisingly perhaps, it is also hard to find extensive evidence of the use of the simple age-based replacement model in practice, although there are a few exceptions (Vanneste and Van Wassenhove, 1995; Christer and Keddie, 1985).

More complex models with a large number of parameters usually possess the characteristic of high correlations between parameter estimates; this indicates that the available data is unable to distinguish between equally plausible parameter combinations. Such models are difficult to resolve, and have low predictive power. See for example the "delay-time" model with inspection sensitivity parameter, p (Section 2.1). Often, sufficient data are not available to consider complex models; even if data are available, the maintenance policies implied by complex models (e.g. Christer and Wang, 1995a) may be difficult to implement in practice.

The main problem with focusing on simple models (point 1 of Section 1.1), however, is that the mathematical community seem intent on increasing complexity, making models more opaque, rather than striving for simplicity and transparency. I feel sure that engineers and managers themselves would be happier with simple and transparent models. The study of more complex models does have a rôle to play in that this may expose more of the true structure of the problem; this, therefore, can indicate the scope for simplification.

1.3. Subjective approach

Various authors have proposed methods for collecting data subjectively, that is using expert judgment (Christer and Waller, 1984; Van Noortwijk et al., 1992; Wang, 1997), and this approach is being used increasingly in situations in which objective data (maintenance history) are sparse. However the validity of such subjective data is sometimes suspect, particularly as in maintenance the "experts" used are those responsible for the current maintenance practice. Therefore their expert judgment must surely reflect the current practice rather than the true underlying engineering phenomena. One cannot accuse the maintenance engineer, whose subjective estimate of failure distribution leads to "optimal" policy close to current practice, of a fix; one should question, however, the role of the modeller who states that the solution thus found is optimal. One might state cynically that data collected subjectively is useful for fitting the complex models proposed; the subjective data may be used for the benefit of the modeller rather than the decision-maker. In this way the approach is perhaps no better than one in which a "numerical example" is given to illustrate the "optimality" of an approach. Numerous examples of the latter may be found in the literature on maintenance modelling.

Of course the subjective approach can be considered as one possible approach to investing more effort in collaboration and in data collection (point 2 of Section 1.1), and should be applauded from this point of view. Models based on properly collected and validated subjective data reflect all that is currently known and believed about the problem of interest; resulting policy is then based upon enhanced subjective opinion. However, the collection of sound objective data relating to the process or problem of interest is fundamental, not least for the calibration of subjective opinion. For this, maintenance management systems are necessary. If such systems are to be effective for the collection of data, modellers themselves must get involved; only through their involvement will it be possible to design a system which holds the data necessary for modelling. An example here is the prototype system developed by Jenkins et al. (1988), which has some optimization capability. However, in general the currently available systems are not used for analyzing historical data (Brown, 1990).

The subjective approach discussed here is still very much a quantitative approach and should not be confused with the "general" subjective or qualitative approach to problem solving, that is the soft OR approach. Here the subjective approach means the use of subjective data for estimation in specific models.

1.4. An integrated approach

The use of an integrated approach, very much applauded by Ormerod (1993), which looks at qualitative aspects of problems in detail, must also be encouraged. Such an approach might be summarized by the involvement of modellers in:

1. problem recognition;
2. design of data collection exercise;
3. design of systems for future information/data collection;
4. effective modelling using data collected;
5. comparison with competing techniques;
6. formulation of revised maintenance policy;
7. imparting ownership of models and policy on maintenance managers;
8. consideration of the payback from modelling exercise and gains due to the implementation of new maintenance policy.

Only through such a comprehensive approach can the mathematical modelling of maintenance be deemed successful from a scientific point of view. Its effectiveness from an economic point of view will depend on the results of step 8, although there are those who would argue that since spending on maintenance is so high, and modelling so rare, it cannot fail to make an economic impact. Often successful implementation of just steps 1 and 8 is sufficient to give economic returns, even if this alone does not satisfy the curiosity of the modeller. In fact, even if other steps are successfully implemented, the significant payoff may be due to 1 rather than to 4. The techniques for tackling step 1 have been described in a maintenance context and are borrowed from quality management, e.g. cause and effect diagrams, Pareto charts (Vanneste and Van Wassenhove, 1995); Christer and Whitelaw (1983) call this snapshot modelling.

Ormerod (1993) suggests that the "root of the problem seems to lie in the way that OR/MS is perceived as theory, techniques and science rather than technology and practice grounded in the work place (i.e. for maintenance, the world of the engineer)". Really this perception is just indicative of too much emphasis in the past on the antithesis of points 1 and 2 of Section 1.1, and too little involvement in the integrated approach, steps 1–8 above.

1.5. Special growth areas

Can some areas of maintenance modelling be identified in which there have been recent developments and which offer unusual challenges and opportunities for growth?

The increase in the use of condition monitoring techniques within industry has been so extensive that it perhaps marks the beginning of a new era in maintenance management. The challenge for modellers here is to quantify the cost benefit of condition monitoring, through the embedding of such techniques in decision models. This development of truly condition based maintenance is made more difficult by the limited availability of failure data for monitored components.

Modelling the maintenance of complex multi-component systems offers many challenges. In practice many dependencies, both economic and stochastic, exist between components of the system. Techniques are required for looking at such systems in a systematic way, using simple models for modelling the maintenance of individual components, and combining the output of such modelling in order to schedule the maintenance of the system itself. Some progress has been made in this area (Dekker, 1995). Some maintenance models for multi-component systems have sought to model such dependency between components, both of an economic nature (Van der Duyn Schouten and Vanneste, 1990) and stochastic nature (Ozekici, 1988; Murthy and Nguyen, 1985). The difficulty with such opportunistic maintenance models lies in making the models simple enough to be both tractable and accessible to practitioners. Recently, Dekker et al. (1996) have reviewed maintenance models for systems with economic dependence.

Much recent work has been carried out in relation to inspection maintenance using the delay-time concept, and the simpler models have found application. The challenge is to now make such simple OR models available and accessible to practitioners (perhaps through the development of suitable software).

All of the above developments will make new demands on the design of maintenance management information systems (MMIS). For example, such systems will have to schedule the dynamic policies for complex systems which will be necessarily implied by use of condition based maintenance and opportunistic maintenance; this is significantly more difficult than scheduling the the static policies implied by block replacement and (routine) preventive maintenance, for example.

1.6. Examples

For some recent examples of extensive case studies one may look to, for example: Vanneste and Van Wassenhove (1995)—an integrated approach to modelling maintenance at a concrete products manufacturer, with particular application of age-based replacement; Christer et al. (1995)—modelling inspection maintenance at a copper products manufacturer; Perakis and Inozu (1991)—replacement modelling of components in marine diesels under particular operating conditions; Monplaisir and Arumugadasan (1994)—Markov modelling of condition based maintenance for locomotive diesel engines; Gopalaswamy et al. (1993)—component replacement for a vehicle fleet using multiple criteria decision making; Kececioglu and Sun (1995)—opportunistic replacement for ballbearing systems; Chilcott and Christer (1991)—maintenance modelling of coal face machinery; Redmond et al. (1997)—modelling the degradation of concrete structures.

Dekker (1996) provides a recent review of published case studies relating to mathematical modelling of maintenance (maintenance optimization models). This paper gives a useful account of the current state regarding applications of the subject, without directly appealing for much greater collaboration between modellers and decision-makers.

1.7. Capital replacement modelling

It is currently an open question as to whether capital replacement modelling, or economic life modelling, is part of the subject area of maintenance. Pintelon and Gelders (1992) label the area as long-term or strategic maintenance, and consider it as part of strategic planning. In strategic planning an organization is concerned with the provision of resources to safeguard future competitiveness. Although technological and economic factors may be the principal drivers for equipment replacement, maintenance costs and unavailability are also important here (Hsu, 1988). The maintenance issues discussed in the main in this paper can be thought of as operational or short-term maintenance (combining and scheduling maintenance activities) and tactical or medium-term maintenance (specification of a maintenance policy such as failure maintenance, inspection maintenance, age-based maintenance, condition based maintenance, etc.).

What is perhaps more interesting about capital replacement modelling is that many of the papers relating to this area that have appeared in the OR literature are application oriented (Eilon et al., 1966; Hastings, 1969; Jardine et al., 1976; Simms et al., 1984; Christer, 1988; Christer and Scarf, 1994; Scarf and Bouamra, 1995). The models themselves are generally very simple and thus offer little opportunity for mathematical exploration. This is the reverse of the situation in preventive maintenance, and again suggests that for maintenance as a whole the mathematicians have up to now dominated the problem solvers.

1.8. Teaching maintenance modelling

Should maintenance modelling be taught using a case study approach or a theoretical, technique oriented approach? Who should teach maintenance modelling? An opportunity exists to get these right from the start since the teaching of maintenance modelling to students of Engineering, Management and OR is only in its infancy.

For OR students, the presentation of theory as well as practice should be acceptable. For the engineers and managers, however, a case study orientation, using simple models in context, would not only be more acceptable to students, but make the subject more accessible to them as practitioners. This would encourage a sense of trust and ownership of the techniques amongst practitioners.

It is interesting to note that Tukey (1962) has much to say about the teaching of science which is relevant today, and it is perhaps surprising how little matters have moved on. In fact much of what Tukey had to say was a re-iteration of the beliefs of R.A. Fisher (see Box, 1978), in regard to Fisher's concerns about both the teaching of Statistics and developments in the subject. It was Fisher's belief that "responsibility for teaching statistical methods in our universities must be entrusted, certainly to highly trained mathematicians, but only to such mathematicians as have had sufficiently prolonged experience of practical research, and of responsibility for drawing conclusions from data, upon which practical action is to be taken".

2. Developments in maintenance modelling

2.1. Inspection maintenance

The modelling of inspection maintenance using the delay-time concept has been extensively studied in recent years (Baker and Wang, 1991; Baker and Wang, 1993; Baker and Christer, 1994). It has also been applied in case-related studies (Christer et al., 1995). Closely related models have been developed in the medical field (e.g. see Day and Walter, 1984). Models may be extended in numerous ways, beyond the reasonable limits of the data that is likely to be available in practice (see Baker, 1996a). Emphasis needs to be placed on keeping the modelling simple. For example, for the two-parameter delay-time model (Poisson process of defect arrivals with rate α; exponentially distributed delay-times with mean $1/\gamma$; perfect inspection) maximum likelihood estimation is straightforward when inspections are equally spaced, Δ time units apart. For a component observed over $(0, T)$ the maximum likelihood estimates satisfy:

$$\hat{\alpha} = \frac{n}{T},$$

$$\frac{(n-k)\hat{\gamma}\Delta}{e^{\hat{\gamma}\Delta} - 1} + \frac{\Sigma\hat{\gamma}t_i}{e^{\hat{\gamma}t_i} - 1} = (n-k),$$

where k failures are observed at times t_i $(i = 1, \dots, k)$ from the last inspection, and $n-k$ defects are found at inspections. Notice that if there are no failures $(k=0)$ then $\gamma \to 0$ and the estimate of mean delay-time is infinite. If all defects cause failures $(k=n)$ then the estimate of mean delay-time is zero. The resulting decision model is also easy to handle. The cost per unit time may be modelled as

$$D = \frac{c_1}{\Delta} + \alpha c_2(1 - P_D),$$

where c_1 and c_2 are the costs due to inspection and failure respectively, and $1 - P_D$ is the fraction of defects manifesting as failures. Here $P_D = (1 - e^{-\gamma\Delta})/\gamma\Delta$. This downtime model was proposed by Christer and Waller (1984) in an early discussion of the delay-time model. The optimal inspection interval, Δ^*, satisfies

$$(1 + \gamma\Delta^*)e^{-\gamma\Delta^*} = 1 - \frac{\gamma c_1}{\alpha c_2},$$

which has a solution provided $\gamma c_1 < \alpha c_2$. Otherwise it is optimal to never perform inspections.

If the assumptions in this simple model are not valid, and in practice this is usually so, then there are two possible routes:

1. extend the model with extra parameters, making greater demands on the available data;
2. use the simple model to obtain a "crude" approximation to the "optimum" policy.

For example, if defect arrivals cannot be assumed to follow a Poisson process then it is natural to consider a non-homogeneous Poisson process (NHPP) of defect arrivals, and this implies at least one more parameter. It also implies that equally spaced inspections are no longer optimal; if the rate of occurrence of defect arrivals is increasing, then the optimal inspection frequency must decrease with age (Baker et al., 1993; Christer and Wang, 1995a). In this case the former authors suggest that attention be restricted to the recent history of the plant over which an HPP model would be adequate, and a simple model used. Updating of the estimate of Δ^* would be required as the plant aged. The latter describe a method for specifying the times of all future inspections, with inspection frequency necessarily decreasing.

Furthermore, if the perfect inspection assumption is dropped, then this implies one more parameter. This may be modelled, for example, in two ways. Firstly, by adopting a mixture model for the delay-time distribution, in which any defect has probability $1-p$ of having a zero delay-time (thus a proportion $1-p$ of defects will never be detected at inspection). The distribution function of the delay-time, h, is then $F(h) = 1 - pe^{-\gamma h}$ $(h \geq 0)$. Alternatively, the inspections themselves may be modelled so that they detect any defect present with probability p. Both these models suffer from the problem that the estimate of the sensitivity parameter p can be highly correlated with the estimate of $1/\gamma$, the mean delay-time. A large dataset is generally required to estimate these parameters. The latter model is also more complex in that defects have to be tracked, in the likelihood, through all subsequent inspections. This is because if $p < 1$, then a defect may have arisen at any time previous to the current inspection, and not necessarily since the last inspection (as would be the case if $p = 1$).

With the experience gained so far, sufficient objective data rarely exist for estimating the parameters in a complex model (perhaps even a model with more than two parameters); if data do exist, they are often unreliable.

Thus option 2 above may be the most sensible approach in general. The approximation to Δ^*, the optimum inspection frequency, would provide an upper bound for the frequency of inspection since as p (in the above models) decreases from 1, Δ^* increases. In the limit as $p \to 0$ then inspection is completely ineffective and $\Delta^* \to \infty$. This model bias in the estimate of Δ^* would be conservative from an engineering point of view, with inspection recommended more frequently that is likely to be necessary. If the bias were the other way, then recommending the implementation of calculated policy would be difficult. It should be noted that the cost functions are often very flat in the neighbourhood of Δ^*, and this also supports the adoption of an approximation to Δ^*. Another possible approach would be to present to engineers optimum policy for various values of p (assumed known in the model), and to leave the choice of which policy to implement in their hands.

2.2. *Single and multi-component replacement / repair models*

There has been little advance in the *application* of single and multi-component replacement models. The huge literature, constantly growing and frequently reviewed (McCall, 1965; Pierskalla and Voelker, 1979; Sherif and Smith, 1981; Valdez-Flores and Feldman, 1989; Cho and Parlar, 1991), is of little value, it seems, to the practitioner, other than in very specialized cases. Thus although this is the most studied field from a mathematical modelling in maintenance point of view, much of the work is of mathematical interest only, exploring the consequences of a modelling format. This is because little attention is paid to data collection and the fitting of such models (Baker and Christer, 1994). This enables authors to invent new models with little thought as to their applicability to real problems.

The important component replacement models, age-based replacement (Barlow and Hunter, 1960), block replacement with or without minimal repair (Barlow and Proschan, 1965) and modified block replacement (Berg and Epstein, 1976) can now be routinely applied in practice, given subjective or objective data regarding failure time distributions of components of interest. Little evidence exists that this is done widely however.

Also recent advances have been made regarding the replacement of components in multi-component systems (Ozekici, 1988). For such systems combining the replacement of certain components may be appropriate for economic reasons (shared setups) or for probabilistic reasons (failure dependence). Some recent work has been done on opportunistic replacement of components with economic dependence (e.g. Van der Duyn Schouten and Vanneste, 1990). In order to model systems for which there is failure dependence between components, authors have considered multi-variate failure models (Murthy and Nguyen, 1985; Nakagawa and Murthy, 1993; Murthy and Wilson, 1994). Again emphasis needs to be placed on simple, applicable models and useful approximations to such models.

The debate regarding component and system renewal on repair/replacement still rages. Good advice (Ascher and Feingold, 1984) is to test the renewal assumption when failure and maintenance data are available. When it is invalid, then some simple models for ageing systems are available, for example: a non-homogeneous Poisson process with log-linear rate or power-law rate (the latter is called the power-law process). See Ascher and Kobbacy (1995) for a recent application of such models to preventive maintenance for a complex system, with implied decreasing interval between inspections. For a recent discussion of the power-law process, with applications, see Baker (1996b).

2.3. Condition based maintenance

The use of condition monitoring techniques has increased rapidly over recent years. Requirements for production performance have risen, plant have increased in cost and complexity, and the downtime available for routine maintenance (preventive replacement, inspection and adjustment) has been squeezed. Condition monitoring is seen as the appropriate technique in these circumstances. For example, production engineers want to know if plant will run "until the end of the week", not that a stoppage is necessary now because "component X is *due* for replacement". Also important from a psychological point of view, is that condition monitoring can reduce the uncertainty operators feel about the current state of plant. Knowledge about the vibration levels of a certain critical bearing, say, gives engineers confidence about its operation in the short term.

Invariably, techniques to date amount to monitoring some condition-related variable(s), X, over time and initiating repair or replacement when X exceeds a preset level, c. Much of the modelling effort has gone into determining the appropriate variable(s) to monitor (Chen et al., 1994); the design of systems for condition monitoring data acquisition (Drake et al., 1995); condition monitoring data diagnosis (Harrison, 1995; Li and Li, 1995); and how to implement computerized condition monitoring (Meher-Homji et al., 1994). The references cited here are given as examples of such work. No modelling is used to determine c; no cost considerations are used explicitly in the decision process. The critical level, c, is chosen on the basis of subjective opinion and on the recommendations of component supplier and monitoring equipment manufacturers. What is required is an integrated approach for predicting condition and for embedding this within a model of the decision process itself.

Such an approach can be outlined as follows. Given the operating history up to time t, $\{X_{t-}\}$, the task is to model the residual life, y_t, of the subsystem/component conditional on the current operating history. It is then necessary to embed this model in an appropriate decision model which takes account of the cost (or downtime or uptime or availability or safety or risk) associated with failure and preventive maintenance. Some preliminary work has been done in this area (Park, 1993; Christer and Wang, 1995b; Kumar and Westberg, 1997). Other work has looked at determining optimum inspection epochs, where data is not recorded continuously (Pellegrin, 1992; Christer and Wang, 1992; Coolen and Dekker, 1995), and at the prediction problem in particular (Christer et al., 1996). The last reference uses a state space model for the condition and condition-related variables and the Kalman filter to predict the residual life, y_t, given the condition-related history to date, $\{X_{t-}\}$; it also differs from the previous references in that it considers a real application.

It is natural to consider such a model of residual life given covariate (condition-related) information as an extension of age-based replacement. Age may be one component of the many-valued process $\{X_{t-}\}$ and optimal policy, when it exists, will be characterized by some region $\mathscr{C}$, so that the component is replaced when $\{X_{t-}\} \in \mathscr{C}$. Modelling the hazard as a function of age (as in age-based replacement) and other covariates (condition-related) would allow the specification of such a critical region in a relatively straightforward manner, in principle. Optimum policy would reduce to that of age-based replacement when true condition appears unrelated to the monitored covariates, based on available data. Furthermore, the ageing of systems can be accommodated by including a subset of covariates which reflect system age, e.g. number of previous repairs/replacements since new; note that in certain circumstances such an approach implies that the hazard immediately after failure returns to as-new, even though subsequently the system may age more quickly (see Newby, 1993). The key problem is having sufficient data on failures and covariates in order to estimate model parameters and hence adequately estimate the critical region; this is a non-trivial aspect of the problem, if not the most critical, for practical application.

Proportional hazards modelling (one particular and natural form for modelling the hazard) has been used to look at this problem (Kumar and Westberg, 1997; Love and Guo, 1991; Makis and Jardine, 1991). Accelerated life models (Kalbfleisch and Prentice, 1980) could also be used here, and may be more appropriate since the analogy between accelerated life testing, where these models originate, and condition monitoring is a close one. Another candidate, particularly in the case of an ageing system, may be the proportional intensities model, in which the intensity function of a non-homogeneous Poisson process is considered as comprising a baseline intensity (typically corresponding to a power-law or log-linear process) and a multiplicative factor involving covariates (Ascher et al., 1995; Lindsey, 1995).

The main criticism of the work to date on proportional hazards modelling in condition based maintenance is that the conditional residual life is determined by the current hazard, that is, the current values of condition-related variables (and the full condition history is not used). Thus this does not capture the essence of the problem as illustrated in Fig. 1. Either it is necessary to forecast the hazard given the development of the estimated hazard to date, or the condition-related covariates must reflect recent history, that is for example $X_{t-} = Z\{^1_t, Z^1_{t-1}, Z^2_t, Z^2_{t-1}, \ldots\}$, say, where $Z^1_\cdot$, $Z^2_\cdot$, $Z^3 d\cdot$, ... are condition-related variables. This is essentially an open problem, although some prototype modelling has been carried out (Wang et al., 1996).

The numerous model extensions and complications discussed in the literature relating to stochastic replacement/repair could be incorporated into such condition based maintenance models. This would lead to numerous mathematical papers, but as discussed earlier, applications should provide the motivation here.

To return to the problem of data collection: sufficient data (relating to failures) for estimating the parameters of a conditional residual life model are unlikely to exist in most, if not all, practical cases. Collecting data may be an option but at a cost. Few

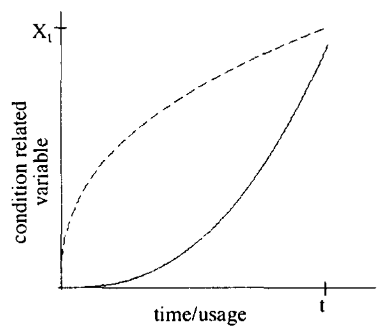

Fig. 1. Conceptual view of two different condition histories: condition history 1, with large expected residual life (– – –); and condition history 2, with small expected residual life (———).

operators are prepared to let components run to failure, and often replaced components are not in a "failed" state. Also that information most closely related to true condition or wear is likely to be more costly to collect. Oil analysis of a gearbox sump is significantly easier than an internal inspection. Thus it may be necessary to build the cost of data collection into the decision model, as well as the cost of failure and preventive maintenance. Thus the initial question of interest for condition monitoring of a particular component ought to be not how to formulate decision policy (i.e. specify the critical region), but whether monitoring will be effective; that is, whether it will reduce costs in the long run when all costs (downtime) are accounted for. The costs of data collection need to be balanced against the expected gains as a result of operating a policy which is nearer to the true unknown optimal policy. Some ideas for approaching this problem area discussed in Baker and Scarf (1995).

3. Maintenance management information systems

With the advent of condition monitoring, and with the development of appropriate decision models, new demands will be put on maintenance management information systems (MMIS). The large number of such systems available at present (Kobbacy et al., 1995) can usually:

1. track components;
2. provide logistic support e.g. spares inventory;
3. store maintenance history;
4. alarm predetermined maintenance activity;
5. produce management reports.

The extent to which the above are accomplished varies. Notably point 4 is not usually a consequence of modelling, but rather the result of manufacturer and user recommendations. The suitability of available software is determined by the scale of the system requiring maintenance. Although a small number of systems available are able to

6. analyze maintenance history,
7. determine "optimal" policy for components and subsystems,

such systems do not offer solutions for large complex systems with many sub-system and component interactions. This is essentially because they require continual intervention from the user. For a complex plant an MMIS will also have to:

8. incorporate expert opinion in a knowledge base;
9. incorporate subjective data from experts;
10. combine maintenance activities into schedules;
11. update schedules with the occurrence of events such as failures and unscheduled replacements;
12. plan resources;
13. measure the effectiveness of maintenance activity.

To perform 6 and 7 requires the definition of suitable classes of maintenance models. The ability to perform 8 and 9 is necessary firstly in relation to models, which will require updating with experience of the plant and the information system. Secondly, this is necessary in order to make up for the shortfall in objective data. Even as the system "learns", and maintenance history extends back, modification, cannibalization, re-design and changing operating conditions will invariably imply that appropriate data will always be in short supply. Some recent progress has been made in connection with 8 (Betanov et al., 1993; Kobbacy et al., 1995; Streichfuss and Burgwinkel, 1995).

The combination of maintenance activities, 10, may be approached in a manner described by Dekker (1995). Here the author restricts attention to maintenance activities for which the next execution moment is determined from the last. Such a static combination of maintenance activities may be far from optimal if failure maintenance and condition based maintenance have to be carried out independently. Concentration on component replacement, overhaul and inspection at fixed intervals is just an appropriate starting point from which to consider the modelling of the combination of maintenance activities.

In order to update schedules with the occurrence of events that cannot be predicted in advance (failures, replacements dictated by condition monitoring or age) an approach is required which can dynamically combine, in an opportunistic manner, the routine (periodic) maintenance with the aperiodic maintenance. Such a system would mimic the decision making of an experienced maintenance engineer (component/subsystem A has failed and stopped the plant, is it cost-effective to replace B, overhaul C, inspect D, at this opportunity?), but in an optimal or near-optimal manner. Such an idea has been followed in the context of age-based replacement for a multi-component system with a small number of components (Dekker et al., 1993). Earlier ideas along these lines were discussed by Dekker et al. (1991), and more recently Wildeman et al. (1997). By routine (periodic) maintenance here, we mean those maintenance activities for which the next execution moment is determined from the last (replacement, overhaul, inspection and adjustment at fixed intervals). By aperiodic we mean maintenance activity for which the timing cannot be known in advance (age-based replacement, condition based replacement and failure correction).

4. Developments in capital replacement modelling

Methods now exist for tackling the multi-equipment replacement problem, and such models have been applied in practice (e.g. Simms et al., 1984). The multi-equipment replacement problem considers the replacement of plant in the context of the fleet within which the plant is operating. Such models are necessary because the replacement of an individual plant, or some subset of the fleet (subfleet), has cost implications not just for the replaced and replacement plant but also for the rest of the operating fleet.

Scarf and Christer (1995) classify the composition of a fleet as singular (one operating plant), multiple identical (homogeneous) or multiple non-identical (inhomogeneous), and classify replacement policies as single plant replacement, entire fleet replacement or subfleet replacement. With such considerations contextual, and identified a priori at the problem

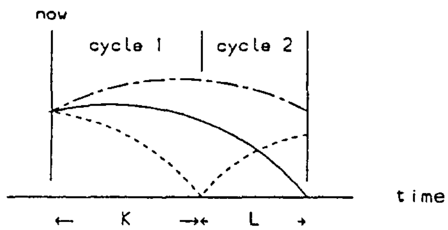

Fig. 2. Two-"cycle" subfleet replacement model with variable planning horizon of length $K+L$: --- operate and sell subfleet 1, buy and operate first replacement subfleet; —— operate and sell subfleet 2, buy second replacement subfleet; - — - operate rest of fleet.

recognition stage (point 1 of Section 1.4), approaches to consider appropriate modelling techniques given various such contexts are required.

For example, Scarf and Bouamra (1995) consider subfleet replacement for an inhomogeneous fleet of buses using a two-cycle model. In this model costs are considered over two "operate–sell–buy" cycles of variable length, K and L (see Fig. 2). The principal decision variables are the choice of subfleet to be replaced first and the length of the first cycle, K (the time to first replacement from the present). Such a model, for the replacement of a single or "typical" plant, was first proposed by Christer and Goodbody (1980). The formulation in Scarf and Bouamra (1995) is flexible in that: the plant in a particular subfleet may themselves be of differing ages and specification; technological change is allowed for in that costs relating to replacement plant may be assigned as appropriate; the assignment of equipment to subfleets would be under the control of the fleet operator, along with some indication of the subfleets which are candidates for replacement. The model can also allow, in principle, for the retirement of subfleets as occasional spares, and for the possibility of subfleets comprising single plant, which may be appropriate if the fleet is small.

A difficulty with such models is that the optimum value of the horizon length, $K+L$, depends on the choice of subfleets to be replaced. This can lead to difficulties with interpretation across differing replacement schedules (choices of subfleets to be replaced first and second). Fixing the length of the horizon, H say, and allowing the number of replacement cycles to vary, may be a way of resolving this problem. A recent application of such a model is considered in Scarf and Hashem (1996).

It is important also to consider circumstances in which decision variables are constrained, perhaps for reasons of technical obsolescence (changing requirements for plant), or alternatively a management imposed minimum acceptable life. These models can allow simple consideration of the cost implications of such sub-optimal policies.

When the fleet comprises a single plant, or the replacement of the entire fleet is considered, models such as that described in Christer and Scarf (1994) may be used. When plant are to be replaced singly, then, given individual operating cost history, repair limit replacement policies (Hastings, 1969; Mahon and Bailey, 1975; Jardine et al., 1976) may be considered. Of course, the replacement of plant singly may be impractical in certain cases.

With the models discussed, technological change can be modelled in a direct and simple manner, since prediction is only required over relatively short planning horizons. However care is required when the planning horizon is short, because "end-of-horizon" effects may be influential. Using a fixed planning horizon, and considering the resulting optimum policy as a function of the horizon length, can allow the decision maker to choose a "robust" optimum policy. Finally it should be pointed out that although we have been discussing replacement strategies, these models are also appropriate for equipment upgrade and refurbishment decisions such as major re-designs, costing substantial sums of money and therefore requiring both justification and strategic planning.

5. Concluding remarks

This article is an appeal to maintenance modellers, and those working in the mathematical modelling of maintenance, to go out an attempt to solve real maintenance-related problems. This, I believe, should be the main priority of the majority working in the area. It implies that the models themselves take a lower priority than the understanding of the process of interest to the decision-maker. This has not often been the case in the past where, even with the best of intentions, so called applications and case studies appear to have been motivated by the need to find an application for a particular model, rather than by the solution of the problem of interest to the engineer or manager.

On the other hand, I am not suggesting that all maintenance modelling should be case-study based. This is partly because decision-makers may be reluctant to take model-based developments through to implementation. Also, operational problems themselves may only require a small proportion of modelling and such work does not score highly among academics, where emphasis in the past has been on theoretical research. However, it is my belief that the mathematical modelling of maintenance should be driven by applications, because the mathematical modelling of maintenance is, itself, applied mathematics. Theoretical research which arises as a result of such model development will then receive sufficient justification.

Applied research has always been regarded as the poorer cousin of theoretical (basic) research, with funding from research awarding bodies more difficult to obtain. Applied research by definition involves collaboration and cooperation between scientists and industry. This can make the outcome of the research more difficult to predict and the work can take longer to complete. A shift of emphasis away from theoretical research towards applied research is required among peers. This shift must occur if modellers and academics are to take up the challenge, if they are to bring the modelling of maintenance to bear on the problems of maintenance management in the new millennium, and if they are going to impart a sense of ownership of the models and information systems among the engineers requiring and using them. This applies not just to maintenance modellers, but equally to Operational Research modellers in other areas.

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