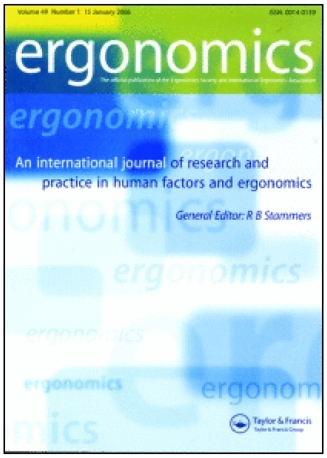

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From theory to practice: commentary on Bartlett (1962)

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1. Introduction

When I received Sir Frederic Bartlett's article, with an invitation to provide a commentary, I was initially puzzled, surprised that the piece was not familiar to me. However, I realised by page 2 that I had indeed read it before, as a young graduate student at the Applied Psychology Unit (APU) in Cambridge. But I am pretty sure that I have not referred to it since those days, which seems odd for what might be considered a landmark paper – thoughts on the future of ergonomics by arguably the most distinguished British psychologist of the 20th century. I do remember thinking at the time (with all the wisdom of the research greenhorn) that Sir Fred, then in his mid-70s, had not really taken the task very seriously. There just did not seem much there in terms of insights or analyses, or on the major recent engineering psychology developments, much of it arising directly out of wartime research at the APU. The list of omissions is surprising, especially given Bartlett's pivotal role in this work: little or nothing on the implications for the future of ground-breaking work on equipment design, pilot error, human-machine control, vigilance; nothing even on Bartlett's own work on the breakdown of complex skill under fatigue (Bartlett 1943).

On this, my second, reading such a callow appraisal seems a bit harsh. Of course, the content is somewhat slight and idiosyncratic, and certainly makes little meaningful contact with the understanding of an ergonomist trained in the last 10–20 years, but Bartlett does actually make a number of interesting points. What is striking for me is that, while his genuine lifelong concern with the practical benefits of psychology are prominent, everything that he says also has a clear basis in the theoretical and conceptual frameworks of the day. I believe (along with many others (e.g. Howell 1993, Wickens and Hollands 1999) that the exceptional progress made by ergonomics and human factors (I shall treat the terms synonymously for the purposes of the current paper) in their brief lifetimes is attributable in no small part to their firm foundation in strong theory and the development of a core of systematic fundamental research. In terms of the ergonomics thrust of Bartlett's essay, almost all the issues he addresses are concerned with engineering psychology; specifically with work changes brought about by automation in one of its guises – reduced involvement of operators in the control of tasks; the shift from direct to remote modes of communication; increased monitoring demands; increased complexity of decision making. These topics reflect the direct influence of information processing

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theories of human performance, which provided a stimulating framework for what APU researchers at the time referred to as 'applied experimental psychology'. Research on human performance carried out during the 1960s, 70s and 80s laid the basis for most of our modern understanding of the human operator: memory and attention; workload and task interference; vigilance and prolonged work; tracking; choice and decision making; and the maintenance of these functions under typical work conditions (stressors) such as noise, heat and loss of sleep (for comprehensive reviews see Boff *et al*. 1986, Wickens and Hollands 1999).

Faced with what is almost a projective test, I have identified two strands of personal relevance to me that run through Bartlett's essay. These relate to two fundamental theoretical constructs developed during the early 1960s: arousal and mental workload. These were exciting developments for the young researcher carrying out his PhD on stress and human performance. I will try to place them in the context of the period, and evaluate their development within human factors/ergonomics. As I consider them now, it is clear that they both relate to a certain way of thinking about performance under demanding work conditions, in which application is driven by theory and fundamental research. My contention is that this approach has, to a certain extent, been supplanted by the growing urgency of practical application, simplicity of decision making, and availability of off-the-shelf tests. It is not that these are inappropriate aims for a practical science, but there is an attendant danger that effective application may be compromised by a neglect of its theoretical foundations. A second thought is that, for me, the issues of arousal (or stress) and workload have always been inextricably linked, through the principle of adaptive regulation of goals and actions, though the two are usually treated separately within ergonomics. I shall return to this later.

2. Arousal, stress and performance

The first set of issues relates to the assessment of performance under stress and extreme conditions of work. My point of departure for this is Bartlett's expressed concern for the problems that might result from automation seriously reducing the overall level of external stimulation in the work environment. He refers to the large programme of work on sensory deprivation, notably carried at McGill University in Toronto in the early 1950s, which was the source of widespread interest at the time (see the review by Zubek 1969). Extreme reductions in the variation of perceptual and motor stimulation were shown to result in the dramatic breakdown of cognitive functioning, including disordered thinking and hallucinations. In hindsight, apart from the adaptation of such techniques by military research into brainwashing and interrogation, and some therapeutic applications, such as the use of floatation tanks to reduce stress, it seems clear that the work has had little lasting impact on mainstream psychology or ergonomics.

Effects, such as those mentioned above, are based on very severe environmental manipulations, and Bartlett, quite rightly, does not generalise unduly from them to the much milder reductions of input associated with new work environments. Instead, he suggests that we specifically need to test whether something similar might occur with this more limited manipulation: '... one of the principal streams of ergonomics research ... must be concerned with partial and specific removal or reduction of outside stimulation' (Bartlett 1962: 507). By this he had in mind something like work arrangements where direct (side by side) communication is replaced with remote contact, or the reduced staffing of modern control consoles. As far as I am aware, nothing in the design of normal working environments over the past 40 years has come close to matching the sensory deprivation of the McGill studies, including the low-event inspection tasks that characterised some earlier assembly line procedures. Even in space research, specifically mentioned by Bartlett as an area where such problems may be serious because of its intrinsic isolation and confinement, only mild impairment of cognitive performance is generally observed (Manzey and Lorenz 1998). In retrospect, there is no doubt that the problem is almost certainly overstated, and we have to assume that Bartlett, like many at the time, was caught up in the drama of the research programme and its findings. The work on sensory deprivation is the origin of the term 'underload' in relation to work environments – the assumption that there can be too little to do, as well as too much. The problem has recently resurfaced in research on underload resulting from increasing automation (e.g. Young and Stanton 2002), with indications of impairment, for example in the recovery of manual control. However, it is likely that these may be attributable to problems of workload management rather than low levels of stimulation *per se*.

This sensory deprivation research had its origins in Hebb's (1955) hypothesis that a minimum level of background stimulation was necessary for arousal of the cortex, and for effective mental functioning. Hebb championed the construct of generalised arousal, with strong support from a flurry of exciting and influential papers (e.g. Duffy 1957, Easterbrook 1959, Malmo 1959). The construct of general arousal became very popular in applied experimental psychology during the 1960s as a model for effects of stressors on performance. There was a belief that arousal would help us understand apparent discrepancies in findings from studies of effects of stressors through its supposed 'inverted U' relationship with performance (e.g. Broadbent 1963). This has come to be accepted as a familiar part of the ergonomics landscape, and is restated regularly whenever issues of arousal or stress are discussed. It is now clear, however, that the widespread optimism about the explanatory usefulness of arousal was misplaced. Even setting aside the long-established failure of the assumed unitary, non-specific mechanism (Lacey 1967), there never has been any clear basis for the inverted-U relationship (Hockey 1984, Neiss 1988). The use of 'over-arousal' to account for positive effects of stressors or negative effects of incentives (Broadbent 1963, Corcoran 1962, Wilkinson 1963) seemed insightful at the time, but has long been understood to be an artefact of a shift in the direction of attention with high levels of the stressor, away from the task and towards the stressor itself, or the internal anxiety state generated by it (Hockey 1984, Näätänen 1973, Neiss 1988) – what Neiss refers to as 'side-effects'; a specific distraction state associated with worry or bodily discomfort, rather than simply a high level of a general state. The myth of the inverted U relationship has been perpetuated in almost every ergonomics text that refers to these issues, and is still widely accepted within the discipline. The title of Neiss's (1990) paper is *Ending arousal's reign of terror*. This may be putting it a bit strongly, but it surely is about time that ergonomics addressed such issues with greater objectivity and purpose. The unthinking acceptance of this insidious myth has merely served to undermine attempts to address problems associated with the impact of environmental conditions at work.

Awareness of the impact of environmental stressors on human performance is, of course, well-established within the ergonomics curriculum, but the theoretical basis for such effects has been largely neglected. In general, the 'quick and dirty' story (the one based on arousal) has been too readily accepted, possibly because the real one is more complex; less neat and tidy. It has long been realised, for example, that part of effect of stressors is the individual's response to the perceived threat to task goals, giving rise to compensatory strategies on the part of the individual (Easterbrook 1959, Hancock and Warm 1989, Kahneman 1973, Teichner 1968). The compensatory control model (e.g. Hockey 1997, 2005) was developed to accommodate the various processes involved in adapting to such threats in a task setting, and the costs of different control options to both task and operator. Instead of the construct of arousal, it suggests the intervening variable of 'operator functional state' (OFS), in recognition of the 'intrinsic relationship between human task performance and the background state of the individual' (Hockey *et al*. 2003). The OFS framework is referred to in more detail in the next section, as a basis for examining workload management. A key finding is that physiological activation is a function of operator effort, rather than the presence of a stressor *per se*: in other words, it occurs only when effort is exerted as a planned intervention to prevent the stressor from interfering with performance.

3. Mental workload and performance assessment

The second conceptual development of the 1960s that I will consider here is the marked progress in the assessment of the demands of mental work. This followed the demonstration that performance measures did not always reflect obvious differences in the demands of two tasks A and B (notably, in the language of the period, when neither exceeded the channel capacity of the operator). Bartlett refers to Kalsbeek's use of dual task methods (Schouten *et al*. 1962), in order to investigate the pattern of interference between two tasks. This was one of a range of techniques developed over this period that enhanced our ability to assess the human response to the demands of working environment (e.g. Brown and Poulton 1961, Knowles 1963, Leplat and Sperandio 1967). For example, the subsidiary task method allowed investigators to conclude that task A was more demanding than task B (even though neither produced errors when tested alone) if a subsidiary task was performed less effectively when paired with A than with B: see Gopher and Donchin (1986) and O'Donnell and Eggemeier (1986) for comprehensive reviews.

This early work was driven by a focus on major theoretical issues, concerned with attentional capacity, mechanisms of task interference, the nature of information processing resources, and so on, culminating in a NATO workshop (Moray 1979). Since that time, as Bartlett anticipated, what he referred to as 'behaviour load' (Bartlett 1962: 509) and we now call mental workload (or simply workload) has become one of the central features of ergonomics assessments, and practitioners are routinely expected to be able to carry out such assessments in a variety of situations. I have my doubts about just how well we do this. Our concern today is, understandably, primarily with practical assessment, and much less with theory; with prediction rather than cause-effect analysis of the processes involved. At a recent HFES Europe Chapter meeting, Neville Moray made the claim that the ergonomics/human factors community has actually been *over*-concerned with theory (and pursuit of the 'grand truth'), and neglected the more realistic and valuable goal of being able to make effective predictions (Moray 2007). I suspect that the truth lies somewhere in between, but, in any case, Moray is in a position to say this as one who has had his feet planted firmly in both domains: his strong emphasis on prediction has always been driven by a clearly-defined theoretical awareness. My own feeling is that workload assessments are sometimes carried out by practitioners without a real sense of purpose or rationale—with why they really need to measure workload, with what it will tell them, and in what ways that information will help in changing the working conditions. Pickup *et al.* (2005) make a similar point about the growing tendency for the indiscriminate use of workload methods, without concern for the context or fit to the empirical problem. Such problems may be overstated, but I am convinced that awareness of, and familiarity with, the theoretical origins of the workload construct (as with any other) puts the practitioner in a better position for making intelligent use of the many tools now available.

From the very beginning, there has been considerable uncertainty about how to interpret workload in terms of practical measurement. The term is used to refer to both the objective demands imposed by the task and the subjective experience of the operator. Subjective experience may further refer to the perception of demands, or of the effort exerted (or of the level of effort that the task requires), or even the level of stress or frustration experienced, as in the Subjective Workload Assessment Technique, SWAT (Reid and Nygren 1988). Another approach focuses on the level of psychophysiological response of the operator, typically in terms of cardiovascular or EEG changes. Sometimes, as in the ubiquitous NASA TLX (Hart and Staveland 1988) mental workload may even include the operator's self-rated performance. To an outsider (an engineer?) this looks like a mess; the use of the catch-all 'workload' to cover all these separate functions is normally excused by the widespread agreement that workload is a 'multidimensional' construct. I have to express some reservations about this usage. Although it appears self-evident, such a label may, in some cases, simply excuse us from trying to work out the functional relationships between these very different aspects of the human response to task demands. Instead, we might consider the process of workload management as a framework or rubric, in which to explore these interactions. To understand what is happening when we change a task environment we need to be interested in all the constituent elements of the adaptive response: objective demand; the subjective appraisal of demand; stable operator characteristics that mediate the operator's strategic response to the task; level of commitment to task goals; level of effort exerted; and ensuing physiological costs of goal maintenance.

4. Operator functional state as a framework for workload

This brings me back to arousal (or, rather, to operator functional state, OFS), since I cannot see any way to understand the dynamics of mental workload without considering its relation to the physiological costs of effort, and the after effects of fatigue. Bartlett assumed that the new workload methods would provide more effective ways of studying fatigue and arousal ('central nervous wakefulness'; Bartlett 1962, p. 509). For the most part this has not happened, though there are indications that such an approach might be fruitful (Broadbent 1971, 1979, Hockey and Earle 2006). I would suggest that what is needed is an OFS-style modelling of the various processes involved in workload management, allowing us deduce what the causal linkages are whenever an operator takes on a task and tries to maintain it for some time. I offer, as an example of this approach, the scheme shown in figure 1, based on the compensatory control theory: see Hockey (1997, 2005) for a full description of the model and regulatory processes involved.

Objective external demands are interpreted by an operator (depending on their skill and familiarity with the task) and implemented as a set of task goals. A separate decision is made on the level of effort required to drive the task, informed by familiarity with the

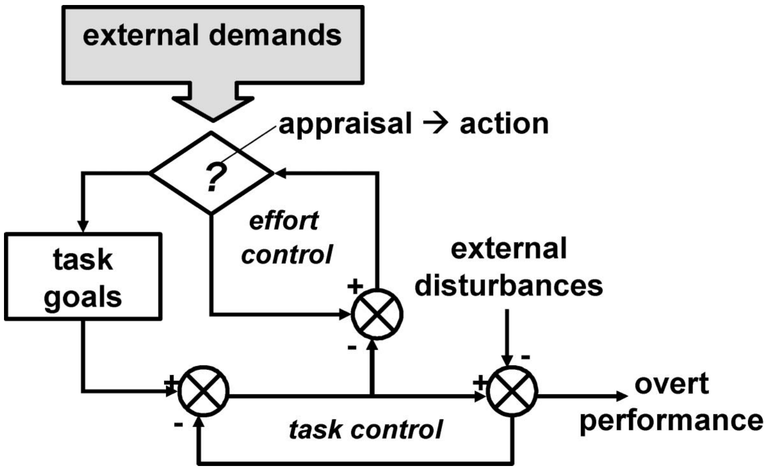

Figure 1. Simplified model of the control processes involved in adaptive task management, showing separate feedback loops for task actions and effort. See text for explanation.

situation, and also the current functional state of the operator. For example, less effort would be needed if the task is well-learned; more if he or she is already tired. In addition to the usual need to fine-tune performance on the basis of negative feedback, the level of effort actually required will vary over time, and need to be increased (an executive decision that over-rides the effort preset) if unexpected disturbances are experienced, such as sudden surges in demand or an increase in the level of environmental stressors (sensed as increased effort demands). A higher level of effort attracts greater regulatory costs in terms of autonomic or brain processes involved in the control of performance, resulting in the more rapid onset of fatigue. Low effort options will then become more attractive, so that a shift to a reduced (more readily manageable) goal may occur, with a consequent reduction of costs and recovery from fatigue. For task environments where a reduced goal would not be a viable option (e.g. safety critical systems) the high effort state would need to be sustained, resulting in increased levels of physiological costs and greater carry-over effects of fatigue.

Such a scheme allows us to look beneath the surface of any workload management scenario. This is critical if we are, for example, to understand the causes of failures of performance. Of course, in practice, we will rarely have time or facilities for doing all this, and I do not suggest that the method is straightforward. In fact, some of the measurements required still prove difficult to achieve (for example, real time changes in effort and goal adjustment). But, assuming that something of (at least) this complexity underlies the behaviour of the operator in managing changing task demands under changing environmental and internal state conditions, I am not sure whether much can be gained from summarising the whole situation in terms of TLX scores, or by measuring heart rate. Of course, sometimes that may be all we can do, but we still need to be aware of what we get from it, and of its place in the overall scheme of things. It is relatively easy to tell that one task is experienced as more demanding than another, but we surely need to know why this is the case: it may be objectively more difficult; operators may be less skilled or familiar with it; they may have worked harder on it (because the goal was more important to them; because the interface was less supportive; because the task came at the end of a shift when they were already tired). At least, it would be useful, as standard practice, to separate effort judgements from assessment of demands, and to obtain separate estimates of external demands (objective difficulty) and perceived load (subjectively determined task goals). The advantage of clearly-articulated theory in the design of assessment procedures is exemplified by Wall *et al.*'s (1996) use of quasi-objective ratings of ask dimensions to clarify the linkage between subjective demands and strain in questionnaire studies.

5. Concluding remarks

In conclusion, I believe that these two constructs, arousal and workload, may have been mishandled by the ergonomics community. At the time that Bartlett was writing, each promised much for the development of both theory and method. Over the past 20 years or so, our increasing trivialisation of arousal has masked the serious need to consider the influence of operator state on performance. Meanwhile, our current enthusiasm with workload as a tool has restricted our awareness of the relationship between different measures. The reality is that these are two sets of complex ideas with a background of detailed theoretical argument and extensive fundamental research. As a context for assessing specific practical work situations, we need to reacquaint ourselves with this background. It should then be clear that a convincing analysis of the processes underlying the management of sustained work tasks can only be achieved by bringing the two ideas together, as complementary aspects of an adaptive task management process.

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