

**Where the Rubber Hits the Road:
Notes on the Deployment Problem
In Workplace Studies**

Bob Anderson

Technical Report EPC-1998-108

To be published in:

*Workplace Studies:
Recovering Work Practice and Informing System Design,
Paul Luff, Jon Hindmarsh and Christian Heath (eds.),
Cambridge University Press.*

Copyright © Xerox Limited 1998

Xerox Research Centre Europe
Cambridge Laboratory
61 Regent Street
Cambridge CB2 1AB

Tel: +44 1223 341500
Fax: +44 1223 341510

WHERE THE RUBBER HITS THE ROAD

Notes on the Deployment Problem in Workplace Studies

**Bob Anderson
RXRC Cambridge**

October-December 1996

To be included in *Workplace Studies: Recovering Work Practice and Informing System Design* edited by Paul Luff, Jon Hindmarsh and Christian Heath; Cambridge University Press.

My thanks to Annette Adler, Ben Anderson, Sara Bly, Graham Button, William Newman, Wes Sharrock, and the various participants at the Colloquium on Workplace Studies held at Kings College, London at which some of the ideas were first bruited and suitably challenged. I should also probably thank Tom Rodden for failing to turn up at the Colloquium and thus giving me the opportunity to ramble on about my interests rather than act as discussant for his.

Generating Technology Innovation: The Context

I approach Workplace Studies with a viewpoint shaped by the research and development needs of a technology provider. I recognise the validity and the importance of other points of view, especially what is often (dismissively) called 'the academic' or 'the theoretical'.¹ I support and enjoy the work which is done in their name. But in the end, what drives my interest is the contribution which Workplace Studies should make to ensuring the commercial success of the work systems my Company might bring to market. And clearly, I do believe that they can and have made such a contribution. Moreover, because for a number of reasons I think this contribution could well be vital in the future², I want to see Workplace Studies not just continued but *supported* and *managed* in ways which will maximise their impact on the whole design and commercialisation process. In all that follows, then, I am arguing from this strongly positive position. I want to promote Workplace Studies within research and development. I want to secure their representation and voice in the design process. And of course I want to maximise the opportunity which their presence in design might give for improving both the process itself and the artifacts so designed – that is, the products we take to market. None of these, though, should be taken as a demand that all our Workplace Studies, let alone all studies of the workplace, should be reconstituted according to the framework I will outline. As I say, my concern here is with studies carried out as part of a clearly defined Research and Development process.

To achieve all of the objectives I have just outlined, I believe we have to go beyond simply building on the successes we have gained so far. We have to create a new sub-genre of sociology; a genre which, for want of a better term, I have been calling *Practical Sociology*³. This paper is about some of the things required to establish this genre as a *real engineering discipline*.⁴

¹ Indeed it would be odd if I didn't since I spent most of my previous life labouring in precisely those vineyards.

² The other papers in this volume clearly demonstrate both the specific form and broad scope of these contributions. From my own Company's viewpoint, reference should be made to Button and Sharrock (this volume) and Suchman (this volume).

³ Do not get hung up on this name. I invest nothing in it except insofar as it designates the intent and the domain. The practice could just as well be called Socio-Technical Systems Design – though others would then get equally outraged.

⁴ There is much which could be said justifying why I make this assertion. Why, that is, it has to be an engineering discipline. Additionally, there is already a whole paper which says what Practical Sociology is *not*. It is not an applied sociology. It is not a policy science. It is not operations research. It is certainly not market research and it is definitely not Chris Bryant's (Bryant, 1996) kind of practical sociology. I have neither space nor time to go into these now. So I will default to the classical academic strategy of argument by assertion. I am telling you where I am coming from and, starting from there, it seems to me that an engineering sub-genre is what we need. Disagreeing with my assumption is not of itself a refutation of my argument. To do that, one would have to show how it will not give me what I am looking for.

The R&D Garbage Can ⁵

Before we get to specifics, it might be just as well to sketch the current state of play within Corporate R&D. I do this first because I suspect it may not be very well appreciated or understood outside that domain, and second because it forms the context within which Practical Sociology will have to find its feet and place. By 'state of play', I do not mean the provenance of such generalised motivational exhortations as 'maximising return on assets', 'increasing shareholder value' and 'growing profitable revenue streams', which derive from the 'Corporate' part of Corporate R&D. Rather, I have in mind current sets of concerns about the relationships between the 'R' and the 'D' of R&D and the ways they should be framed. It is into these that Corporate sponsored studies of the workplace, at least, will have to fit.

From my perspective, Corporate R&D appears to be being carried out within a highly charged force field. The components of that force field are associated with a number of transformations. The most prominent are:

From "voodoo innovation" to customer-centred research

In Economics, theories of innovation are, by and large, versions of – or adaptations to – Adam Smith's 'hidden hand'. In the long run, entrepreneurial profits will tend to be eradicated in perfect markets. This is because where 'surplus' profits are being made, suppliers will enter the marketplace and, being willing to accept lower profits (there is always someone willing to be worse off than yourself!), compete on price. Thus is the stability of 'normal' profits re-asserted in the marketplace. In his classic work *The Theory of Economic Organisation* (Schumpeter, 1934), Schumpeter described how, against such a background of stability, economic innovation and hence change occurs. In essence, this is the theory of mousetrap profits. If you are smart enough or lucky enough to invent, find, steal an innovative idea – a new and better mousetrap – the world will, for a short while at least, beat a path to your door. And you will become rich. Then as others in the "de-mussification" business notice your competitive advantage, they will attack your market by copying or improving on your product. Your surplus profits will decline: stability will be returned. The iron law of the marketplace, the hidden hand of competitive forces, will have moved things back into place once more.

As with mousetraps, so with silicon. Or so it has been proposed, especially by defendants of the free range of market forces such as George Gilder (Gilder, 1991) in his analysis of high technology businesses. There is no predicting where or how innovation will arise, nor who will gain advantage from it. The only thing to do, it seems, is to put enough bright people in a dark room, throw them resources and money, and leave them alone. If you are lucky and can wait long enough, a new idea, a radical innovation (the digital mousetrap?) will emerge and sweep the marketplace.

In a coruscating reply to Gilder, Charles Ferguson (Ferguson, 1988) called this 'a voodoo theory of R&D' because it places all the explanatory and causal weight on processes which are non-rational and beyond our control. Ferguson points out that inventing the mousetrap is

⁵ I've taken the allusion from Jim March (March & Olson, 1989) who describes all modern organisations as exhibiting many of the characteristics of rapidly spinning garbage cans.

less than half the task. As every Corporation knows only too well, it is not enough to have the invention. You have first to be able to recognise it *is* an innovation and second see how to turn it to business effect. Xerox discovered with the Alto, Ethernets, object oriented programming (but not, interestingly, laser printing) that focusing only on the technology in hand and failing to understand the opportunities it offers can cause one to ‘fumble the future’ (Smith & Alexander, 1988).

This realisation is now taking the form of a demand for “customer-focused research”, a strategy which both narrows horizons, in that it targets activity, and increases the probability that what is targeted can be brought to commercial success. These days, before an R&D project can be consecrated, it is increasingly necessary to demonstrate just how the proposed product and its related technology will fit within the business and work processes found in customer settings (Grady & Fincham, 1990).

From pipelines to scrums

Not only is our conception of innovation being transformed, so too is our model of how to manage it. Because we no longer believe in voodoo R&D, we no longer seek to maintain a ritually purified boundary between research and the commercialisation process. The old model we have jettisoned was essentially that the R&D process is a pipeline constantly being filled a long way upstream with ideas and inventions (in research) and from which products emerge downstream to be carried off to the marketplace. The boundary was felt to be important to maintain because otherwise research might get polluted by short-term demands from current product teams. At Boeing, the boundary maintenance tool took the form of creating small teams which were hidden from top level management and focused on advanced research. These became known as “skunk works”. A similar process was at work in the early design of the Alpha Chip at Digital (Quinn, 1979).

Currently, no clear alternative is being promoted in place of the pipeline image (which probably accounts for why it still has some lingering influence), but all of the ones currently being paraded (Roussell, Saad & Erickson, 1991) have a common form. They require research to be “close coupled” and “rapidly responsive” to the marketplace with, reciprocally, the R&D team loosely managed, fast moving, multidisciplinary and multifunctional (McGrath, 1995). Since many senior managers are terminally addicted to sporting metaphors, often they refer to the resulting organisation as a “scrum”. It is plain that what they expect is tight integration, rapid adaptation and the free flow of information.

From “big bets” to investment options

As R&D has been drawn into the mainstream of the Company (a response to both the need to control the asset and the realisation of its strategic value), so the ways of thinking there have leaked back into research. Nowhere is this more visible than in the emergence of a new rationalisation for R&D decision making. If you like, instead of seeing R&D as a kind of *prospecting* for ideas, some of which will require major gambles and promise equally significant pay-offs (what in the R&D literature are called “bet the Company” decisions), the approach now is to distribute and manage risk by treating R&D as a series of *investment options* (Dixit & Pindyck, 1995). During its lifetime, any individual project is treated as part of a structured portfolio of more or less “risky” options to invest. As the project proceeds, the investor (i.e. the Company) can choose whether to exercise the

option. When seeking funding for any project, then, the R&D manager is less likely to argue in terms of the size of the opportunity (its big bang) and almost certainly not in terms of the novelty of the technology – its whizziness (Quinn, 1979). Rather it is the structure of carefully laid down points at which information will be fed back to enable the Company to make the decision to “call” or “put” the option which will be emphasised.

From “killer apps” and point products to sustainable competitive advantage

What the options model does is to militate against betting the Company on a single throw, or, relatedly, to assume that success can only be achieved through the serial discovery of new services or usages which capture customers’ imagination (often called “killer apps” – or applications). To put it the other way around, it demands that R&D be configured to ensure that there is a continuity of success. The reason for this is a larger shift in strategic thinking which has gone on in Corporations. Rather than viewing their “product set” as a series of individual forays into a market or series of markets, Corporations plan only to enter markets where they can maintain sustainable competitive advantage. By definition, a one-off success is not sustainable and hence not strategic. Since one of the primordial sources of success is the differentiation produced by continuous innovation within the product set, R&D is now being looked to to provide such sustainable strategies. What R&D provides the Corporation is called (in the jargon) *innovative product platforms* (Myers & Rosenbloom, 1996).

All of the tendencies illustrated above have a common concern to integrate “upstream R&D” with “downstream market understanding”. It is hardly surprising, then, that some research managers have seen the social sciences, especially those elements which concern themselves with work and the work setting, as a powerful resource to draw upon in achieving this aim. What they have seen, though, may not necessarily be what social science thinks it is actually offering, or capable of offering, or indeed what it is best at.⁶ What R&D managers would like to have could probably be most succinctly described as *marketplace intelligence* and *socio-ergonomics*. And what they are looking for is its distillation into forms which the engineering and design disciplines can first accommodate and second deploy.

There is one obvious response to all this. Begin a campaign of re-education to show R&D managers how much they have misunderstood social science. There are lots of arguments in favour of such a strategy and several crucial ones against. For me, probably the most telling is that, should we be successful, not only do we risk strangling the golden goose (so to speak), we will also have lost an opportunity to try for something really interesting. So, rather than insisting that Mohammed go to the mountain by getting engineering to accommodate social science, I would propose the opposite tack. Let’s see what we can do to take the mountain to Mohammed, social science to engineering. Let’s see, that is, what it would take to build an engineering discipline which would fulfil the functions which market intelligence and socio-ergonomics are shorthand for. Let’s see, that is, if we can make a practical sociology.

The Problem of Institutionalisation

Several years ago, when musing upon much the same order of problem, Lisl Klein (Klein, 1993) argued that it was first and foremost a matter of institutionalisation. It really isn’t all

⁶ I’ve been round this one before (Anderson, 1997).

that difficult to get an arbitrary engineering group to talk with some random social scientists. By and large, engineers are pleasant and well-mannered people. And social scientists occasionally have surprisingly useful things to say (Sommerville, Rodden, Sawyer & Bentley, 1992). But the workaday world of engineering projects is such that unless the exchange, the collaboration, is institutionalised it will always remain at the fleeting level of pleasantries and hallway conversation. To have impact, it has to be embedded in the design process and to do that it has to be a legitimated, sanctioned and part of what some sociologists themselves call “the moral order of design” (Harper, 1992). Klein proposes that this requires changes in the training of engineers (and social scientists), changes to the structure of funding and resourcing, and changes to the standards and codes of practice enforced with regard to designed artifacts. All of which sounds like a tall order. But to make matters worse, it will also require quite wide-ranging changes in the character and conception of the social science being done in order to bring it under the auspices (as we are prone to say) of the engineering rubric. If you think Klein’s asks were hard ones, wait till you see what the latter imply, for they determine the re-shaping which social science will have to undertake to be an engineering discipline and to have the position and influence I believe it both aspires to and should have. For Workplace Studies in Corporate R&D, to coin a phrase, they mark where the rubber hits the road.

The Problem of Deployment

I want to frame all this in terms of the problem of deployment. That is, I want to ask how we might envisage (or indeed expect) shaping the growing body of work, findings, and techniques associated with Workplace Studies to be used by the predominantly practical engineering disciplines we are seeking to engage. To get the discussion going (actually to get it going yet again, because in fact it has come and gone several times in the general debates), I want to pose things from the viewpoint of the practising engineering project manager. I do this not because I think it the only way to position and scope the problems to be addressed, nor because I wish to dismiss the broader epistemological and methodological questions which the attempt to relate intellectual pursuits always throws up. Aligning the design, the engineering, and the sociological outlooks is a deeply fascinating problem. For the moment, though, I will impose a self-denying ordinance. Instead, I will ask how we might fit the sociological stance (as evidenced in Workplace Studies) with endemically praxeological orientation of the engineering project manager.

A final introductory word: this time one of caution. Some people will be disappointed that I do not have any immediate programmes to offer. There are no ready answers or cut and dried recommendations in my knapsack. This is not because I have not thought about these questions, nor grappled with them *as practical matters*, for I have. Indeed, it is precisely because I have thought about them and have grappled with them as matters of practical management in a research and development setting that I know one really has to be suspicious of ready answers (especially if you don’t know how *rough* they might be) and cut and dried recommendations (no matter how *pressed* one is for them).

The Project Manager’s Perspective

Broadly and very briefly summarised, the engineering project manager’s perspective is constituted by the need to provide a schedule of products or deliverables

- on time;
- within budget; and
- to specification.

And while there are very many other good engineering (and non-engineering) desiderata for the effectiveness of project management, management of the constraints of time, cost, specification and schedule are paramount.⁷

Put as broadly and briefly, this characterisation of the engineering project manager's outlook may seem both less than impressive and less than perceptive [though a brief consultation of Fred Brooks' *The Mythical Man Month* (Brooks, 1982) might lead us to be somewhat more confident in the observation and its applicability]. We need to dig a little deeper and excavate the implications (or explicate the imprecations if you prefer) behind these points. When I say *the management of a schedule of deliverables* is central to the project manager's perspective, I mean no more than that the role of the manager is called into being in virtue of the requirement to attend to the monitoring and scheduling of activities. Engineering projects are (literally) composites. Or, to put it another way, the engineering approach to any complex problem (and at some level all real design problems are complex) is to divide the problem into manageable (and hence simpler) parts (modules), bring the requisite and relevant skills to bear upon each part, and then recompose the solutions thus created into the designed or engineered artifact. Project management is approached in the same top-down/bottom up/divide and conquer manner as any other engineering problem (Newell & Card, 1985). Problem decomposition and re-composition creates the need for co-ordination and control. That is the task of the project manager.

The impetus to modularisation which is (I claim) a, but not *the*, characteristic of the engineering outlook places a number of requirements upon the disciplines which contribute to the production of the eventual artifact. Prime among these is concatenation. That is, each discipline is viewed as under an obligation to integrate its results with those of others in consistent, predictable, rule-governed, systematic (that is *disciplined*) ways. The contributions line up and are "plug compatible", as my engineering friends say.

There is an associated aspect of this which should not go unremarked. For the engineering project manager, *the solution is all*. Disciplines are viewed as resources, as means to ends, and the problems such disciplines take up and take on in like manner. The engineering project manager is not motivated by an interest in the problem or endeavour *for its own sake*. This sharply contrasts, perhaps not surprisingly, with much of the current *raison d'être* of Workplace Studies.

To be an engineering discipline, Workplace Studies will have to adapt to modularisation for them to be deployed within project teams. Perforce, then, we will have to re-think some of what often appears to be definitive of the present genre.

⁷ In fact, with colleagues I have examined some of these in the context of particular engineering projects (Sharrock & Anderson, 1996). Other related issues are raised in (Bucciarelli, 1994).

The Questions of Deployment

The primary considerations which a project manager would bring to the deployment of social science skills within a specific engineering project are:

1. Access;
2. Qualification; and
3. Implementation.

Each of these is, naturally, a dense mass of subsidiary issues. The most prominent are:

- 1.1. Where do I get it?
- 1.2. What are the trade-offs between having my own and getting it shrinkwrapped off the shelf?
- 1.3. How much do I really need?
- 1.4. Can I re-use it in the future?
- 1.5. If it is not shrink-wrapped can I re-use someone else's?
- 2.1. How do I know what sort I need?
- 2.2. How do I estimate fitness for purpose?
- 2.3. What are the 5 key issues I should target it on?
- 3.1. Where does it fit and who should it report to?
- 3.2. When should I bring it in?
- 3.3. How do I know when what I've got is "good enough"?

The point about these questions is not that they are hard to answer or that answering them will pose undue stress on a fledgling sub-genre. It is just that within Workplace Studies little or no attention has been paid to them *as institutional matters*. Unlike other disciplines to whom we might look for analogies (e.g., psychology, ergonomics), Workplace Studies does not have a body of commonsense recipe knowledge on which it can draw to help the project manager answer these questions or to estimate how far has been gone in getting to answers. This does not mean that we *have to find* answers to these questions. Neither does it mean that since we might not like their answers, we have *to find* answers *before* others do it for us. Rather, it is that in answering these and similar questions, I believe Workplace Studies will begin to re-shape itself as a practical, engineering discipline.

What's Going On Here?

In case you hadn't spotted it, these managerialist questions imply trade-offs between costs as well as benefits. And you can't get the benefits (funding, impact and influence) without settling for the costs (external direction, scoping and shaping). Let's walk through them and see what might be involved.

Access

Central to the question of access are the professional processes for the dissemination of method, technique and generalisability of results. Taking the last first. We have, by now, piled a fair heap of studies. What, in general terms can we say they show us? To my mind, and I'll re-phrase the old systems engineer's apothegm, we've got back pretty much what we've put in, to wit extensive and robust demonstrations of the possibility of analyses of the social organisation of technological innovation and technology use. While this is definitely not garbage, I'd hesitate to say that adding yet more demonstrations to those already in hand was actually building anything up in any way (other than the pile).

Of course, the analysis of why this might be so can go in many ways. Perhaps we still need an integrating theoretical frame of reference? Perhaps we have just sprinkle sampled the domain? Perhaps (and I for one feel this is the most likely) the investigations we have undertaken were not designed to be generalisable (let alone cumulative)?

Let me be extremely careful here. I am not proposing that Workplace Studies have to adopt some simple or even simplistic version of inductive generalisability. What I am saying is that they have got to figure out how new and unknown problems can be brought under the auspices of known and solved ones.⁸ For all I know, this might be a matter of fixing determinant criteria for the formation of a kind of case-based reasoning rooted in ideal types.

Then there is the question of specification. If we are able to frame general statements, how do we reduce them to the specifics of each new instance? What, to coin another term, is our theory of "the theory of the case"? What are our analogues to the psycho-physical "laws" of ergonomics (Fitt's Law; Fitts, 1954) or the "regularities" of psychology (Miller's rule; Miller, 1956)? This is where my (perhaps too glib) reference to ideal types might actually have a grain of sense in it. Moving forward here will mean working out what the appropriate categorical system should be and how it might be extensible.

Method and technique are, of course, closely related. Do Workplace Studies have *methods* in the usual sense? Or are they more craft-like? And if so, does that have implications for whoever may be called upon to carry them out (see the qualification issue below)? If I look across the body of work currently in place, I find it just as hard to see the use of a method (let alone a replicable one) as I do to see the build up of cumulative findings.⁹ Probably the two are associated. There are similarities (family resemblances, you might say) between studies

⁸ Continually emphasising the uniquely identifying characteristics of each contexture really gets in the way here. It is both distracting and unhelpful since it is often taken to imply (wrongly in my view) that comparison is impossible *tout court*.

⁹ Again, though, let me try and trap a distracting and very misleading hare before it gets away. I am not, definitely not, saying that Workplace Studies as Workplace Studies are deficient because they do not seem to display commonality of method and cumulation of findings. As forms of conventional Social Science, such attributes might well be improper ones for them to have. But as components of an engineering discipline, their lack certainly would be a handicap, to say the least. Second, while I am at it, I might as well scoop up a related and equally misleading distraction. A lack of universal method or cumulativity does not imply a lack of rigour, analytic systematicity or, should one aspire to it, scientificity. Universal method and cumulative findings are desiderata of certain forms of science not science itself.

of ground control facilities, air traffic control centres, underground railway control centres, engineering shops, print factories, and the like. But the methods adopted in each go no further than that. And of course this variety is motivated as a matter of principle. Each study is (at least in ambition) designed to be *uniquely adequate* (more or less) to its phenomena. The net result is that not only do you have to learn by doing (which is probably the only way you can learn) but there are no principles to be drawn out other than the unique adequacy one.

In effect, all this seems to mean that we must tell our engineering project manager 'you have no choice but to have one of your own'. And the trouble with that is that it will make Workplace Studies *very expensive* and very slow to be disseminated and assimilated.

Qualification

In one sense this too is about professionalisation.¹⁰ What are the standards which you would expect a journeyman practitioner to have achieved? What should they know and be able to do? Is there (heaven help us) a basic 'tool box', 'kit bag', 'medicine chest' (choose your favourite metaphorical domain together with its portmanteaux) of approaches, techniques and formats? Or does the work depend only on having someone with skills, flair, insight, and extraordinary talent; your own shaman, so to speak? If so, we are looking for a 'genius' engineering discipline and that is an oxymoron. No one in their right mind builds an engineering discipline which can only be populated by geniuses (or geni, bottled or otherwise, for that matter).

One way of thinking about what is involved here, could be to ask how Workplace Studies would fare under something like the Software Engineering Institute's Capability Maturity Model (CMM). I say this in the full recognition that a recent issue of IEEE Software (July 1996) was given over to examining just why software engineering remains in such a relatively primitive state with regard to performance measurement systems such as CMM. So it is not that software engineering provides a strong model to follow. Rather that the practices (especially the project management practices) built into the CMM are those which any engineering discipline should be attempting to follow (or so it seems reasonable to claim).

When using the CMM, assessors look for:

- demonstration that a clear, integrated and consistent methodology is followed throughout the project;
- demonstration that project tracking and risk analysis is built into the assessment processes;
- demonstration that defined measures of progress are deployed;
- demonstration that the requirements for the project have been clearly identified; and
- demonstration that quality of outputs is measured.

¹⁰ Notice it is professionalisation *not* professionalism. It has to do with certification of the field not enforcing rules of conduct - though, of course the two are related.

Just for the sake of sharpness, I will re-phrase these as a set of questions relevant to the Workplace Studies.

1. What are their methodologies and against which problem sets have Workplace Studies been deployed and validated? What domains of problem are *not* amenable to treatment from this perspective? What are the criteria of integration and consistency for their use in large-scale and small-scale projects?
2. What are the key risks associated with schedule slippage and performance failure for Workplace Studies? Which critical components will be disabled and in what order?
3. How do Workplace Studies identify and order milestones? Can progress be assessed other than in categorical terms? What specific project management techniques have been endorsed for Workplace Studies?
4. What form does requirements specification for Workplace Studies take? What processes should be put in place to prevent requirements drift?
5. What are the forms of output? Can Workplace Studies contribute to the determining of benchmarks, scenarios, forms of solution, etc. (Newman, 1996)? What are the criteria for determining levels of quality and how do Workplace Studies compare the quality of output from different contributions or from different sub-projects?

The last question is, of course, about the results which you can expect an investigation to provide. Do we know which critical parameters we should select (Newman, 1997)? Here there is an interesting tie back to one of the earlier questions. Do we know how to map the requirements of a project onto the choice of an investigative approach in order to isolate a chosen set of critical parameters?

The tricky question with regard to qualification is not that we do not know how to tell a good analysis from a poor one. We have all seen enough PhD theses and novice field reports to be able to do that. What we lack are standards for the application of the high quality reports and findings we have been compiling to the design of artefacts. That is, we lack standards by which to judge what contribution the reports and findings make to *the improvement* of the artefact. If I put it in terms which are becoming increasingly familiar to all of us, can we say, crisply and succinctly, what *the value add* of Workplace Studies is for the design of specific artefacts?

I recognise that in part this is an unfair question. Many Workplace Studies are as much about the process of design as they are the specifics of design. But that is just where the CMM becomes so relevant. What do Workplace Studies offer in the way of direct and practicable advice for how to improve design and development processes against criteria of assessment such as those encapsulated in CMM?

Implementation

Three questions are paramount here: Where does it fit? When do I use it? and Who does it report to? Answers to the first question are likely to turn on what we think is the main leverage the research offers. Are we talking about market intelligence? Are we excavating the

“unarticulated needs” of some market segment? Are we defining the requirements and specifications for some envisioned product? Are we collecting evaluations of some extant and potential products? The answers here (and this is not a complete list) will give fairly strong guidance to the selection of results required (what are we looking for and how important is it?) together with the investigative tools and techniques designed to provide them.

Once we have identified the points of leverage, we can then get a lot closer to answering the timing question. All product delivery processes have a phased structure with decision points or “gates” through which the project has to pass to gain further funding. Which gate reviews are Workplace Studies best at supporting? Do we know? Does it matter? (The answer to the latter is very definitely “yes” for both political and pragmatic reasons. Unless we can identify the contribution made to phase gate reviews, we will never be able to answer the “value add” question above.)

The Stopping Rule Problem

Gate reviews are, of course, an organisational solution to the stopping rule problem. Pursuing any solution, fine tuning it, gathering data, can be endless tasks. We can always justify “another pass”, a “further search”, “an update”. The question is, though, what does the incremental information, experience or understanding actually give us? Can we see not just what we will get from the work but what value it will contribute to the final goal? It follows that for Workplace Studies the stopping rule problem is not binary in form. It is not whether to have studies but how many to have and for how long and to what depth? Are the targets, the “deliverables”, the “requirements” for the studies specified clearly enough and at a level which will enable us to determine when or if we have the information required for the task in hand? If we are not yet in a position to do this, what would it take to get us there?

Management Lines

Formal management and reporting processes fall out from the previous two issues in the sense that the data which is collected and the generalisations which they support have eventually to be directed towards some pragmatic product development concerns. In the past, for the most part we have attempted to include Workplace Studies generally within the ambit of usability analysis and more especially end user requirements specification. While this was (and is) not wrong, it is not to my mind the most advantageous point of leverage to go after simply because for requirements to be collected and usability to be assessed *we must have already defined the product and its ‘use architecture’*¹¹. More and more, I see it is just this definitional process to which Workplace Studies can make its most valuable contribution. The point of interception of the development process is not, then, usability but the formation of **product and market vision**. Workplace Studies should contribute right up front to shaping the value proposition for an offering and its supporting technology. Indeed, Workplace Studies can and should be the owners of that process.

¹¹A term I am stealing quite unashamedly from Annette Adler. For closely related work, see Dourish, Adler & Smith (1996).

This may sound somewhat odd, since the generation of market and product visions and strategy is, usually, the domain of marketing groups rather than engineering ones. Under the old pipeline model, such reservations would have been justified. However, in all of the emerging models, these functional silos are being broken down and responsibility for all aspects is being shared across an integrated product planning group. Moreover, since marketing groups are concerned with fit of offerings into settings, take up by users and, of course, with capturing and understanding the voice of the customer, that makes them more than natural allies for Workplace Studies. In addition, since, as one Development Manager put it to me “Engineering gets its marching orders from marketing”, contributing to the shaping of market and product vision is not just a natural leverage point; it is a critically important one.

Conclusion

Although the tradition of studying the use of technologies in actual workplaces goes back a long way, it is only relatively recently that we have started to think in explicit terms of drawing specific technology recommendations (as opposed to generalised ones or policy recommendations) from our analyses. And we have come a long way in a very short time. We have built up a substantial body of detailed investigations. We have honed a number of exploratory techniques. We are beginning to understand how to derive requirements from our findings. What we have not done, though, or better, what we have not done very much, is to think about the institutionalisation and associated professionalisation of Workplace Studies as a practical sociological contribution to an engineering effort. These are the lines of thinking I have been following in this paper. I have argued that the processes of institutionalisation and professionalisation will require attention to be paid to questions which have not featured much in the discussions thus far. In particular, they will require practitioners to become much more explicit about issues of goodness of fit between studies and design problems, criteria of assessment of outputs from studies, and, of course, the form which such outputs should take for particular purposes. If I have done nothing else, my hope is that I have sketched the rough order of concerns which these throw up and indicated something of what would be needed to resolve them.

References

- Anderson, R. *Work, Ethnography and System Design*. in A. Kent and J.G. Williams, eds., *Encyclopedia of Microcomputers*, Volume 20, 1997, pp 159-183, Dekker Godfrey.
- Brooks, F. *The Mythical Man Month*. Reading, UK: Addison Wesley, 1982.
- Bryant. C. *Practical Sociology, Post Empiricism and the reconstruction of Theory as Application*. Oxford, UK: Polity Press, 1996.
- Bucciarelli, L. *Designing Engineers*. Cambridge, Mass: The MIT Press, 1994.
- Button G. & Sharrock. W. In: *Workplace Studies: Recovering Work Practice and Informing System Design* edited by Paul Luff, Jon Hindmarsh and Christian Heath; Cambridge University Press, 1998.
- Dixit, A. & Pindyck, R. *The Options Approach to Capital Investment*. Harvard Business Review, May-Jun 1995, pp. 105-115.
- Dourish, P., Adler, A. & Smith, B.C., *Designing User Interfaces Around Reflective Accounts*. Proceedings of Reflection '96, San Francisco, May 1996.

- Ferguson, C.H. *From the People who brought you Voodoo Economics*. Harvard Business Review. May-June 1988, pp. 55-62.
- Fitts, P. *The information capacity of the human motor system in controlling amplitude of movement*. Journal of Experimental Psychology 47,381-391, 1954.
- Gilder, G. *Into the Telecosm*. Harvard Business Review March-April 1991, pp. 150-161.
- Grady, D. & Fincham, T. *Making R & D Pay*. The McKinsey Quarterly, No.3, 1990, pp. 22-29.
- Harper, R. *Looking at Ourselves*. Proceedings of Conference on Computer Supported Cooperative Work. New York: ACM, 1992, pp. 330-337.
- Klein, L. On the Collaboration between scientists and engineers. In E. Trist and H. Murray, *The Social Engagement of Social Science*. Philadelphia: University of Pennsylvania Press, 1993, pp. 369-384.
- March, J. & Olson, J. *Rediscovering Institutions. The Organisational basis of Politics*. New York: The Free Press, 1989.
- McGrath, M. *Product Strategy for High Technology Companies*. Irwin Professional Publishing, 1995.
- Miller, G.A. *The Magical number seven, plus or minus two*. Psychological Review 63:81-97, 1956.
- Myers, M. & Rosenbloom, R. *Re-thinking the Role of Research in the Engines of Innovation*. R.S. Rosenbloom & N.J. Spencer (eds.) Harvard Business School Press, 1996, pp. 14-18.
- Newell, A. & Card, S. *Prospects for Psychological Science in HCI*. Human-Computer Interaction, Vol. 3, pp. 210-242, 1985.
- Newman, W. *The Place of Interactive Computing in Tomorrow's Computer Science*. Computing Tomorrow. Wand, I. and Milner, R. Cambridge, UK: Cambridge University Press, 1996.
- Newman, W. *What Application Designers Know: Some Thoughts on Innovation in Interactive Systems*. Proceedings of Software Ergonomie '97, Dresden, 1997.
- Quinn, J.B. *Technological Innovation, Entrepreneurship, and Strategy*. In: Generating Technological Innovation. Roberts, E. (ed.) New York: Oxford University Press, 1987 pp. 117-134. Originally published in Sloan Management Review, Spring 1979, Vol. 20, No.3.
- Roussell, P., Saad, V. & Erickson, T. *Third Generation R & D: Managing the link to corporate strategy*. Cambridge, Mass: Harvard Business School Press, 1991.
- Sharrock, W. & Anderson, R. *Organisational Innovation and the Articulation of the Design Space*. In: Design Rational Concepts, Techniques and Use. Moran, T. & Carroll J. (eds.) Mahwah, NJ : Lawrence Erlbaum Associates, Inc., 1996, pp. 429-451.
- Schumpeter, J. *The Theory of Economic Development*. Cambridge, Mass: Harvard University Press, 1934.
- Smith, D. & Alexander, R. *Fumbling the Future: How Xerox invented then ignored the first personal computer*. William Morrow & Company Inc., 1988.

Sommerville, I., Rodden, T., Sawyer, P. & Bentley, R., *Sociologists can be Surprisingly Useful in Interactive Systems Design*. In: Proceedings of HCI'92, York, UK, 1992.

Suchman, L. In: *Workplace Studies: Recovering Work Practice and Informing System Design*, edited by Paul Luff, Jon Hindmarsh and Christian Heath; Cambridge University Press, 1998.