The mediating role of job characteristics in job redesign interventions: A serendipitous quasi-experiment

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Summary
The aim of this paper is to examine the mediating role played by five key job characteristics in the relationship between employee participation in a job redesign intervention and employee well-being. In studies of job redesign interventions, it has been assumed that any effects of employee participation in job redesign on well-being are a result of changes in job characteristics rather than participation in change per se. It is therefore important to statistically test for mediation in job redesign intervention studies to help establish that the change in job characteristics is the mechanism through which job redesign interventions work. However, this has rarely been tested directly, either because data to allow tests of mediation have not been collected (e.g. assessments of job characteristics) or because data have been collected but mediation has not been tested using accepted procedures. This makes it unclear whether changes in job characteristics explain the effects. Results from multilevel analyses of a longitudinal 9-month long serendipitous quasi-experimental participative job redesign intervention showed that changes in job control, participation, skill utilization and feedback, but not task obstacles, were sufficient to account for the relationship between the intervention and employee well-being. Copyright © 2009 John Wiley & Sons, Ltd.

Introduction

There is a long history of using job redesign interventions to change job characteristics in the expectation that this will improve employee well-being (Parker & Wall, 1999). It is generally assumed that the change in job characteristics is the mechanism through which job redesign interventions affect employee well-being. Yet this is rarely tested; either because data to allow tests of mediation have not been collected (e.g. assessments of job characteristics) or because data have been collected but mediation has not been tested using accepted procedures (Baron & Kenny, 1986). To date, studies in which tests of mediation have been made have focused exclusively on changing one job characteristic, job control (Bond & Bunce, 2001; Logan & Ganster, 2005). However, many job redesign interventions aim to change multiple job characteristics on the assumption that this will produce a greater impact on
well-being (Semmer, 2003) but the efficacy of so doing has not been tested using required mediation procedures. Indeed, analysis might reveal that a change in one job characteristic accounts for all the change in well-being, implying that changing multiple job characteristics may not be an efficient and parsimonious approach. There are, therefore, two reasons why it is important to test for mediation in job redesign interventions that aim to change multiple job characteristics. First, it is important to establish that a change in job characteristics is the mechanism through which the job redesign intervention affects well-being, and that any change in well-being is not just a direct result of being involved in an intervention per se. This is particularly important in participative job redesign interventions, in which employees are involved in suggesting and implementing change, as improvements in well-being may result from the effects of increased involvement rather than changes in job design. Second, identifying whether the change in well-being is caused by a change in one or more job characteristics can help to understand better the efficacy of job redesign interventions. The specific aim of this paper is to examine the mediating role of multiple job characteristics in the relationship between employee participation in a job redesign intervention and employee well-being.

Job design theory and employee well-being

Job demands–resources theories of job design identify two types of job characteristic—job demands and job resources (Demerouti, Bakker, Nachreiner, & Schaufeli, 2001; Karasek & Theorell, 1990). Each is viewed as a determinant of employee well-being, i.e. longer-term levels of pleasant affect such as enthusiasm and contentment, and a lack of unpleasant affect such as anxiety and depression (Diener, Eunhook, Lucas, & Smith, 1999; Warr, 1990). Job demands are the physical, social and organizational aspects of work that require psychological and/or physical attention and effort, such as workload and task obstacles (Semmer, Zapf, & Dunckel, 1995). High demands have a generally negative effect on well-being. For example, the effort involved in overcoming many task obstacles (factors that inhibit task performance) depletes energy reserves and leads to lower well-being (Hockey, 1997; Lee & Ashforth, 1996). Job resources are the aspects of work that enable the person to manage job demands, facilitate the achievement of goals, promote learning and fulfill basic human needs (Deci & Ryan, 1985; Schaufeli & Bakker, 2004). They have a generally positive effect on employee well-being and include job control, feedback, participation, skill utilization and social support (Humphrey, Nahrgang, & Morgeson, 2007; Terry & Jimmieson, 1999; Van der Doef & Maes, 1999). For example, job control (discretion over the timing and methods of work tasks) and participation (the degree of influence over more distal issues in the work domain) enable employees to remove task obstacles, to manage demand in a way that is least effortful, and to fulfill their need for autonomy (Jackson, 1983; Karasek & Theorell, 1990). Other job resources, such as performance feedback (receiving information on task performance) and skill utilization (the opportunity to use existing skills and acquire new ones) help to promote skill development, to enable employees to cope better with demand and to fulfill employees’ need for competence (Hackman & Oldham, 1980; Holman & Wall, 2002).

1Job design theories that include job resources and job demands can be seen as a development of Hackman and Oldham’s (1980) job design theory as the range of job characteristics is expanded to include job demands and types of resource other than the five specified by Hackman and Oldham. For reviews of job design theory see Cooper, Dewe, and O’Driscoll (2001) and Parker and Wall (1999). Job design theories are also generally concerned with objective job characteristics, and job redesign interventions generally attempt to change objective job characteristics. However, evaluations of job design theories and job redesign interventions can only be based on perceptions of job characteristics. Thus, although the job redesign intervention reported in this study attempted to change objective job characteristics, the evaluation of that change was based on employees’ perceptions of job characteristics.
**Job redesign interventions**

Based on job design theory, job redesign interventions have been used to try to improve employee well-being by altering job characteristics. The strongest claims for the effectiveness of job redesign interventions come from studies using quasi-experimental designs (Semmer, 2003). Such studies have shown that more positive outcomes occur in experimental intervention groups than in control groups with regard to either employee well-being (Griffin, 1991; Workman & Bommer, 2004) or employee well-being and job characteristics (Campion & McClelland, 1991; Le Blanc, Hox, Taris, Schaufeli, & Peeters, 2007; Mikkelsen, Saksvik, & Landsbergis, 2000; Morgeson & Campion, 2002; Munz, Kohler, & Greenberg, 2001; Wall, Kemp, Jackson, & Clegg, 1986). However, only two studies have used mediation tests to test directly whether a change in job characteristics is the mechanism through which a job redesign intervention has its effect on employee well-being, and these studies have focused exclusively on changing one job characteristic, job control (Bond & Bunce, 2001; Logan & Ganster, 2005). Bond and Bunce (2001), for example, conducted a participative intervention in which employees identified and implemented changes solely in job control. It was shown that job control fully mediated the effect of the intervention on a measure of psychological well-being, mental health. It is therefore important to statistically test for mediation in job redesign intervention studies to help establish that the change in job characteristics is the mechanism through which job redesign interventions work. If job characteristics are not the mechanism through which job redesign interventions work, then alternative mechanisms need to be explicated. This is particularly the case with regard to participative job redesign interventions, as it needs to be established that the effects of employee participation in job redesign on well-being are a result of changes in job characteristics rather than participation in change per se.

Many job redesign interventions aim to alter a range of job characteristics, as they are based on job design theories which assume that job characteristics have independent direct effects on well-being (Le Blanc et al., 2007; Mikkelsen et al., 2000). Multiple changes should therefore add up to a greater change in well-being. But studies of job redesign interventions that make multiple changes have not tested for mediation. So it is not clear which, if any, of the intervention induced changes in job characteristics are responsible for the change in well-being. Indeed, analysis might reveal that a change in one job characteristic alone accounts for all the change in well-being. If this were to be the case, it would suggest that interventions that change one job characteristic (e.g. job control) might be as equally effective but more parsimonious than broad-based job redesign interventions that aim to change multiple job characteristics. Testing for the mediating effects of multiple job characteristics using required statistical procedures can therefore enable a better understanding of the efficacy of broad-based job redesign interventions (although the relative effectiveness of job redesign interventions can be fully determined only by experimental study).

Evidence that the effects of interventions can be mediated by a wide range of job characteristics comes from Parker’s (2003) study of the introduction of lean manufacturing. However, this was not a job redesign intervention using the principles of job design to improve employee well-being. Rather, it was a management-led intervention that simplified jobs to improve production efficiency. The effect of the change was to increase job-related depression and this was fully mediated by decreases in three job characteristics—job control, skill utilization and participation in decision-making. Thus, although this study showed the mediating effects of multiple job characteristics in an organizational intervention, it was not a job redesign intervention aimed at improving employee well-being.

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2Job redesign interventions can be participative, in which the job holder is involved in the redesign process, or non-participative in which changes are imposed on the job holder.
Given the lack of studies in this area, there is a need to establish whether the effects of broad-based participative job redesign interventions on employee well-being are mediated by changes in multiple job characteristics. Such evidence will enable a better understanding and practical use of job redesign interventions.

**Hypotheses**

The general objective of the present study was to implement a participative job redesign intervention to enhance job design characteristics as a means of improving employee well-being. Our approach to job design was based on job demands–resources theory (Demerouti et al., 2001). Based on this theory, empirical evidence for the effects of job design on employee well-being (Humphrey et al., 2007; Parker & Wall, 1999) and on discussions with managers and employees, it was decided that the intervention would focus on trying to increase employees’ experience of four key job resources (job control, participation in decision-making, feedback and skill utilization) and reduce their experience of one key job demand (task obstacles). In particular, we expected that the job redesign intervention would enable employees to develop ideas about how to improve their job (e.g. new job tasks, new procedures) and — based on job crafting theory — that these ideas would need to be enacted or implemented by employees if their experience of job characteristics was to be altered (Wrzesniewski & Dutton, 2001). For example, by enacting new rules and procedures that permit employees to perform new tasks and allow greater freedom over the timing and order of tasks, it is likely that employees will experience an increase in job control. The first hypothesis is

**Hypothesis 1**: Employees in an experimental group that implement changes arising from a job redesign intervention will experience increases in job resources (job control, participation in decision-making, feedback and skill utilization) and reductions in job demand (task obstacles) when compared to employees in a control group in which these changes are not implemented.

As outlined earlier, job demands–resources theory predicts that increases in job resources will improve employee well-being. Job control and participation enable employees to manage demands in a way that is least effortful (Jackson, 1983; Karasek & Theorell, 1990), and performance feedback and skill utilization help to promote skill development, all of which enable employees to cope better with demand (Hackman & Oldham, 1980; Holman & Wall, 2002). In addition, job demands–resources theory predicts that decreases in job demand should improve wellbeing. Reduced task obstacles should lower task effort, thereby helping the person to maintain energy reserves and higher levels of wellbeing (Hockey, 1997; Lee & Ashforth, 1996). The second hypothesis is

**Hypothesis 2**: Increases in job resources and reductions in job demands will be associated with increases in employee well-being.

Hypotheses 1 and 2 imply that, as the job redesign intervention will affect job characteristics and as changes in job characteristics will affect well-being, then the effect of the intervention on well-being will be mediated by changes in job characteristics. However, the effect of the intervention on well-being may be caused by factors other than a change in job characteristics, e.g. a Hawthorne effect. It is therefore necessary to set a third hypothesis concerning the direct effect of the intervention on well-being, and a fourth hypothesis concerning the mediating effects of job characteristics in this relationship. Confirming Hypothesis 4 will strengthen the argument that the effects of the intervention...
are caused by changes in job characteristics and are not caused by other factors, e.g. a Hawthorne effect. Thus, our third and fourth hypotheses are

*Hypothesis 3*: Employees in an experimental group of a job redesign intervention will experience an increase in well-being when compared to employees in a control group.

*Hypothesis 4*: Changes in job characteristics will mediate the relationship between the job redesign intervention and employee well-being.

**Method**

**Organizational context**

The study was conducted in one department of a large UK company providing health insurance and health care. The department dealt with business customer’s health insurance policies and at the beginning of the study had approximately 240 employees (25 managers and team leaders, 215 team members) divided between five sections (three administrative sections dealing with policies and claims, a call centre dealing with incoming calls, and a support section dealing with post and incoming documents). The support section had 30 employees, the other sections had 50–60. All sections were subdivided into teams of about 10 employees.

The department had high labour turnover relative to the rest of the company. Management thought that the high level of turnover was caused by the low well-being of team members and that it also reduced the quality of work. Management was keen to improve the well-being of employees, but different attempts to do so had not produced any noticeable effect as indicated by the monthly measurement of job satisfaction (an indicator of subjective well-being, Faragher, Cass, & Cooper, 2005) in an anonymous company-wide survey. Having conducted research elsewhere in the company, we were asked by management to develop an intervention to improve the well-being of employees in the department.

**Job redesign intervention**

The job redesign intervention was based on the Scenarios Planning tool (Axtell, Pepper, Clegg, Wall, & Gardner, 2001; Nadin, Waterson, & Parker, 2001). Like other participative job redesign tools, two important features of this tool were: The participation of multiple organizational stakeholders to improve redesign solutions; and the introduction of job design theory so that participants can make better informed decisions (Israel, Baker, Goldenhar, Heaney, & Schurman, 1996). The job redesign process had two main phases: Assessment and redesign; and, implementation.

The assessment and redesign phase was conducted in a one day off-site meeting, facilitated by the research team, and repeated on three separate occasions so that all teams could participate. It involved each team identifying core job tasks and the obstacles that prevent effective working. The current job design was then rated (on a scale of 1–10) with regard to job characteristics and their effects on well-being and performance. The job characteristics were job control, skill utilization, feedback, participation, and task obstacles (for conceptual definitions of each job characteristic see pages 2 in the introduction). Teams then discussed the benefits of three job design scenarios: One aimed at maximizing well-being, one aimed at maximizing performance and one aimed at optimizing both.
Teams were then asked to suggest changes to the current job that would achieve the last objective. All suggestions were considered in terms of their effect on job characteristics and those rated most important by employees were adopted. Finally, the effect of these suggestions on job characteristics, well-being and performance was rated, and it was found that the suggestions led to a higher overall rating than that of the current scenario.

The implementation phase occurred in the following months, with teams given responsibility to implement the proposed job redesign changes. Two representatives per team agreed to monitor progress on job design changes, and to attend three implementation meetings (spread over 3 months) with the research team to discuss progress. The research team raised questions with management if employees were experiencing difficulty in implementation. The changes to job characteristics were: Job control—the adoption of new tasks and procedures (changing customer names and addresses, changing customer policy details, a new policy based task) and access to new customer information data bases without getting permission from a supervisor; for participation—involvement in design of a new IT system, team member setting of work schedules, team member setting of breaks; for skill utilization—training on the new tasks and IT systems previously outlined; for feedback—performance criteria specified more clearly, supervisor performance feedback increased to four sessions per month; and, for the removal of task obstacles—visiting teams in other departments to exchange understanding on information requirements, and procedural changes.

Research design

The study went through five stages (see Table 1). Briefly, the Time 1 questionnaire was administered one month before the 6-month long intervention. Discussions with management indicated that changes should be fully implemented within 6 months. As a short interval after an intervention may increase the ability to detect effects on well-being (Le Blanc et al., 2007), the Time 2 questionnaire was administered about 1 month after the intervention was fully implemented.

The serendipitous creation of a control group

Quasi-experimental interventions enable experimental groups (in which change is expected) to be compared against non-equivalent control groups (in which change is not expected) (Campbell & Stanley, 1966; Cook & Campbell, 1979; Shadish, Cook, & Campbell, 2002). If change in the dependent variable occurs in the experimental group and not the control group, then more confident causal attributions can be made to the intervention received by the experimental group. The type of non-equivalent control group used in many quasi-experiments is a no-treatment control group. A disadvantage of a no-treatment control is that a Hawthorne effect cannot be ruled out, as differences in outcome may be the result of experiencing an intervention rather than the nature of the intervention

Table 1. Outline of research process

<table>
<thead>
<tr>
<th>Stage</th>
<th>Procedure</th>
<th>Time</th>
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<tr>
<td>Stage 1</td>
<td>Employee briefing on study purposes</td>
<td>Month 1</td>
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<tr>
<td>Stage 2</td>
<td>Administration of the pre-test Time 1 questionnaire</td>
<td>Middle of Month 2</td>
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<tr>
<td>Stage 3</td>
<td>Job redesign phase 1: Off-site meetings to conduct assessment and redesign</td>
<td>Middle of Month 3</td>
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<tr>
<td>Stage 4</td>
<td>Job redesign phase 2: Implementation phase</td>
<td>End of Stage 3 to Month 8</td>
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<tr>
<td>Stage 5</td>
<td>Administration of the post-test Time 2 questionnaire</td>
<td>End of Month 9</td>
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per se (Roethlisberger & Dickson, 1939). This threat to validity can be overcome by using an inert-treatment control group that receives a treatment that is not expected to change the dependent variable (equivalent to the placebo condition used in medical research) (Shadish & Cook, 1999). In some studies the treatment received by the inert-treatment control group is similar to that received by the experimental group; in others it is quite different. But the key point is that the treatment received by the inert-treatment control group lacks the key active ingredient that is given to the experimental group (Schwartz, Chesney, Irvine, & Keefe, 1997).

Although we were unable to include a no-treatment control group or an inert-treatment control group from the outset (the organization wanted all employees to participate in and benefit from the job redesign intervention), a serendipitous event created an inert-treatment control group within the study. The event was an outsourcing initiative, announced and introduced by management just before the assessment and redesign phase (Stage 3, see Table 1) of the job redesign intervention, but unconnected to the job redesign intervention. The outsourcing initiative relocated some job tasks in two of the department’s administrative sections to another organization. The effect of the outsourcing initiative was to stop the key active ingredient of the job redesign intervention—the implementation of changes to job design—from occurring in the two affected sections. Without this key ingredient, the two affected sections became subject to a treatment that was inert with regard to well-being.

Evidence that the key active ingredient of the job redesign intervention did not occur in the two affected sections came from interviews and field involvement (e.g. attendance at meetings) with employees in these sections. This revealed opposition to the outsourcing initiative, as employees believed that it was a potential threat to their jobs and that the quality of the outsourced work would be inadequate. This probably explains why, during the first phase of the job redesign process, most suggestions from employees in the two affected sections centred on reversing the outsourcing initiative, not on job redesign. However, management stated explicitly that the outsourcing would not be aborted and so employee suggestions in this respect could not be implemented. More significant for our present purposes, as a result of managers’ refusal to consider employee suggestions on reversing the outsourcing initiative, employees in the two affected sections stated that they were unwilling to implement any of the few changes to job design that they had suggested as part of the job redesign intervention. Consequently, in the sections affected by outsourcing, the implementation of changes to job design did not occur (previous studies have also found that job redesign interventions affect certain groups and not others, e.g. Heaney, Price, & Rafferty, 1995). The lack of this key ingredient (i.e. the implementation of changes) therefore created an inert-treatment control group.

A potential threat to the validity of using this group as an inert-treatment control is that the outsourcing could have had an effect on well-being in its own right, i.e. the outsourcing was not inert. The outsourcing initiative could have increased staff concerns about job security and lowered well-being. But such decreases would happen only if the threat to jobs was undiminished in the long term. This was not the case. By the time of the second questionnaire it was apparent to employees that outsourcing would not be extended because of management concerns about the quality of the outsourced work (which eventually led to the outsourcing being reversed). Employee concerns consequently abated and so the effects of the outsourcing initiative did not last long enough to be detectable by the time of the second questionnaire. The outsourcing might also have altered employees’ perceptions of their job, as it involved the removal of tasks. For example, it might have lowered the perception of variety or task identity, i.e. the sense that one is completing a whole and identifiable task (Hackman & Oldham, 1980). Again, this does not appear to have been the case, as the outsourcing was relatively limited in scope, and involved tasks that were not core to employees’ jobs. Moreover, employees were still involved in the outsourced tasks, as they often had to correct the mistakes made by employees in the outsourcing company. As such, the outsourcing initiative is likely to have had little effect on job design. Thus, in the short term, the outsourcing initiative prevented changes to job design.
being implemented, but in the longer-term it did not prove to be a major issue for well-being. This argument is supported by evidence from our findings (reported later) that no statistically significant change in job characteristics or well-being occurred in the inert-treatment control group.

**Participants**

Participants were team members of all five sections. At Time 1, the survey was completed by 188 of 215 potential respondents, giving a response rate of 87 per cent. There were 151 women and 37 men. The average age of participants was 33.56 years, and their average tenure was 39.76 months. At Time 2, all team members were given the opportunity to complete the survey, including those who did not respond at Time 1 or had joined since Time 1. The survey was completed by 173 of 203 potential respondents, giving a response rate of 85 per cent. At Time 2 there were 132 women and 41 men, their average age was 33.34 years and their average tenure was 36.89 months. The longitudinal sample, comprising those who responded at both Time 1 and 2, was 119. There were 71 team members in the experimental group and 48 in the control group. Of the 188 Time 1 respondents, 144 potential respondents remained at Time 2, as 44 employees had either moved elsewhere within the organization, quit voluntarily, taken maternity leave, or were on sick leave. So the response rate for the Time 1 respondents remaining at Time 2 was 83 per cent.

**Statistical analysis**

We tested our hypotheses using multilevel regression modelling. The data had two-levels, with measurement occasions (level-1) nested within individuals (level-2) (Raudenbush, Bryk, Cheong, & Congdon, 1999). Shadish (2002) recommends using this approach to analyse quasi-experiments. Advantages of this approach over repeated measures ANOVA (the technique traditionally used to analyse longitudinal quasi-experiments) are that variance is correctly partitioned to the different levels and that it is robust against violations of homoschedasticity and sphericity, which means that effect sizes can be estimated more accurately and Type 1 error rates are reduced (Queneç & van den Bergh, 2004; Snijders & Bosker, 2003). In all multi-level analyses, level-1 and level-2 variables were grand mean centred (Hoffman & Gavin, 1998).

To test for the treatment effects of the intervention (e.g. Hypotheses 1 and 3) we followed the procedure used by Le Blanc et al. (2007). This involves conducting a moderation analysis at level-1. To do this we created dummy variables representing time of measurement (i.e. pre- and post-intervention) and group membership (i.e. experimental or control group), and an interaction term that is the product of the two dummy variables. When the interaction term is added to the equation a significant relationship indicates that change over time in the experimental group is significantly different from that of the control group. Random effects of the slopes of the independent variables were fixed (i.e. the random variation of slopes between individuals was fixed) and individual controls (age, gender and tenure) were included as level-2 predictors of the dependent variable. An example of the equation for the moderation analysis can be seen in the Appendix. We also wanted to check that any reported change in working conditions was restricted to those areas covered by the intervention. This would provide greater confidence that an intervention-induced halo effect, that makes employees who experience change in one aspect of their job more inclined to report change in other areas, did not occur. To do this, we examined a variable which the intervention was not meant to change, namely, inter-team task dependency, i.e. the extent to which an employee relies on people in other teams to get his or her job done and perceives that people in other teams rely on him or her to get their job done.
(Ancona & Caldwell, 1992; Hoegl & Weinkauf, 2005). Finding no effect of the intervention on this variable will provide greater confidence that an intervention-induced halo effect has not occurred.

To test for direct effects (Hypothesis 2) we regressed the dependent variable on the level-1 independent variables of interest, included time of measurement and group membership (i.e. experimental or inert-treatment group) as level-1 controls, and age, gender and tenure as level-2 controls on the dependent variable. To test for mediation (Hypothesis 4) we followed the procedures as recommended by Baron and Kenny (1986) and Krull and Mackinnon (2001) supported by the Sobel (1982) test. Step 1 examined the effect of the independent variable (intervention) on the dependent variable (well-being) using the moderation analysis outlined earlier. Step 2 examined the effect of the independent variable on the mediator (job design) variables using the moderation analysis outlined earlier. Step 3 examined the effect of the mediator on the dependent variable not only using the procedure for testing direct effects outlined earlier but also including the independent variable (i.e. interaction term). At Step 4 we examined whether the relationship between the independent and dependent variables became non-significant when a single mediator variable was added to the equation.

**Measures**

All the items from the main level-1 measures are shown in the Appendix.

**Job-related well-being**

Job-related well-being was measured using Warr’s (1990) established 12-item measure relating to pleasant (e.g. enthusiasm, contentment) and unpleasant (e.g. anxiety, miserable) affect. For each item, participants were asked to rate the extent to which they had experienced that type of affect in the last month. A high score indicates greater well-being. Cronbach’s $\alpha$ at T1 and T2 were .91 and .91, respectively. On the assumption that well-being is related to voluntary turnover (Griffeth, Hom, & Gaertner, 2000), further evidence for the validity of the scale in this study comes from our finding that, of Time 1 employees, those who had voluntarily quit the organization by Time 2 ($M = 3.02$) had a lower level of job-related well-being than those still with the organization at Time 2 ($M = 3.37$) ($t = 2.37$, $df = 185$, $p < .05$).

**Job design variables**

All job design measures were based on employee perceptions. Job control was a 9-item measure that assessed the extent to which employees have discretion over methods used, the timing of work and customer interaction. It was based on a measure by Jackson, Wall, Martin, and Davids (1993) but modified to reflect the service context (Holman, 2002). Cronbach’s $\alpha$ was .88 at T1 and .89 at T2. Participation was a three-item scale that assessed employees’ involvement in decisions about their job, team and department (Parker, 2003). Cronbach’s $\alpha$ was .90 at T1 and .87 at T2. Skill utilization was a four-item scale that assessed the opportunity to use existing skills and develop new ones (O’Brien, 1986). Cronbach’s $\alpha$ was .87 at T1 and .88 at T2. Feedback was a four-item measure assessing the extent to which employees received feedback on their performance from any source (Holman, Chissick, & Totterdell, 2002). Cronbach’s $\alpha$ was .90 at T1 and .89 at T2. Task obstacles was a six-item measure assessing whether task performance was affected by a lack of information and by problems and interruptions from colleagues and computer systems. It was based on a measure developed by Semmer et al. (1995). Cronbach’s $\alpha$ was .72 at T1 and .80 at T2. Inter-team task interdependence was an individual-level three-item measure that assessed the extent to which an employee relies on people in other teams to get his or her job done and perceives that people in other teams rely on him or her to get
their job done. It was based on a measure of intra-team task interdependence developed by Campion (1988). Cronbach’s $\alpha$ was .79 at T1 and .82 at T2.

Level-2 measures included age, tenure and gender, as previous research indicates that they affect well-being (Warr, 1990).

**Results**

Table 2 shows the correlations among variables within the whole Time 1 sample and within the whole Time 2 sample. In keeping with job design theory, all the job characteristics were related in the expected direction to well-being on both occasions. Descriptive statistics for the experimental and control conditions in Table 3 show that, in keeping with the study’s hypotheses, job characteristics improved over time in the experimental but not the control group. Formal tests of the hypotheses were conducted through multilevel analyses, to which we now turn.

Hypothesis 1 stated that the intervention would increase job resources and reduce job demand in the experimental group but not the control group. The results of the analysis for treatment effects revealed that Hypothesis 1 was supported in four out of five instances (see Table 4, Models 1–5), as there was a significant interaction effect for job control ($\beta = .31, p < .01$), participation ($\beta = .51, p < .01$), skill utilization ($\beta = .52, p < .01$) and feedback ($\beta = .78, p < .01$) though not for task obstacles. Plots of the interactions (which were of a similar form) showed that job characteristics increased over time in the experimental group but remained essentially unchanged over time in the control group. Examples of two interactions effects—for feedback and skill utilization—are shown in Figure 1. The results also demonstrated that men reported significantly less change in job control ($\beta = -.31, p < .05$) and feedback ($\beta = -.56, p < .01$), and more change in task obstacles ($\beta = .33, p < .01$).

Hypothesis 2, that changes in job resources and job demands would be associated with changes in employee well-being, was largely confirmed. When assessed independently of each other, all job characteristic variables were significantly associated with well-being: Job control ($\beta = .24, p < .01$; pseudo-$R^2 = 9$ per cent), participation ($\beta = .27, p < .01$; pseudo-$R^2 = 13$ per cent), skill utilization ($\beta = .25, p < .01$; pseudo-$R^2 = 22$ per cent), feedback ($\beta = .16, p < .01$; pseudo-$R^2 = 11$ per cent) and task obstacles ($\beta = .16, p < .01$; pseudo-$R^2 = 17$ per cent) (Pseudo-$R^2$ refers to change in total

Table 2. Correlations between main study variables at Time 1 and Time 2

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<td>4. Tenure</td>
<td>.03</td>
<td>.37</td>
<td>-.14</td>
<td></td>
<td>-.03</td>
<td>.04</td>
<td>.23</td>
<td>.03</td>
<td>.01</td>
<td>.05</td>
</tr>
<tr>
<td>5. Job control</td>
<td>.32</td>
<td>-.04</td>
<td>-.19</td>
<td>.03</td>
<td></td>
<td>.30</td>
<td>.22</td>
<td>.28</td>
<td>-.19</td>
<td>-.05</td>
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<tr>
<td>6. Participation</td>
<td>.40</td>
<td>.03</td>
<td>-.10</td>
<td>-.01</td>
<td>.45</td>
<td></td>
<td>.26</td>
<td>.07</td>
<td>.01</td>
<td>.17</td>
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<tr>
<td>7. Skill utilization</td>
<td>.49</td>
<td>.17</td>
<td>-.13</td>
<td>.18</td>
<td>.44</td>
<td>.37</td>
<td></td>
<td>.23</td>
<td>-.21</td>
<td>-.10</td>
</tr>
<tr>
<td>8. Feedback</td>
<td>.37</td>
<td>.11</td>
<td>-.20</td>
<td>-.06</td>
<td>.12</td>
<td>.19</td>
<td>.22</td>
<td></td>
<td>-.35</td>
<td>-.02</td>
</tr>
<tr>
<td>9. Task obstacles</td>
<td>-.43</td>
<td>.09</td>
<td>.16</td>
<td>.00</td>
<td>-.21</td>
<td>-.21</td>
<td>.18</td>
<td>-.26</td>
<td></td>
<td>.34</td>
</tr>
<tr>
<td>10. Inter-team task interdependence</td>
<td>-.06</td>
<td>.03</td>
<td>.09</td>
<td>.16</td>
<td>.01</td>
<td>.05</td>
<td>.01</td>
<td>.00</td>
<td>.17</td>
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</tr>
</tbody>
</table>

*Note*: Correlations below the diagonal, Time 1 ($N = 188$), correlations over .14 significant at $p < .05$. Correlations above the diagonal, Time 2 ($N = 173$), correlations over .15 significant at $p < .05$. Copyright © 2009 John Wiley & Sons, Ltd. J. Organiz. Behav. 31, 84–105 (2010) DOI: 10.1002/job
When entered as a group (see Table 5, Model 3), all job characteristics remained significant except for job control, and collectively accounted for an additional 37 per cent of the variance in well-being above that explained by the background factors.

Hypothesis 3, that employees in an experimental group of a job redesign intervention will experience an increase in well-being when compared to employees in a control group, was confirmed. The results in Table 5 (Model 2) showed that the intervention had a significant positive association with well-being ($\beta = .24, p < .05$). This result is represented in Figure 2 and shows that well-being increased over time in the experimental group but remained essentially unchanged over time in the control group.

Table 4. Multilevel models for effect of intervention on employee perceived job characteristics

<table>
<thead>
<tr>
<th>Outcome variable</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>$\beta$</td>
<td>SE</td>
<td>$\beta$</td>
<td>SE</td>
<td>$\beta$</td>
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<td>Intercept</td>
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<td>.05</td>
<td>2.37**</td>
<td>.06</td>
<td>4.85**</td>
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<td>Controls</td>
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<td>Age</td>
<td>.00</td>
<td>.00</td>
<td>.00</td>
<td>.00</td>
<td>.01</td>
</tr>
<tr>
<td>Gender (Male = )</td>
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<td>.10</td>
<td>.06</td>
<td>.16</td>
<td>-30</td>
</tr>
<tr>
<td>Tenure</td>
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<td>.00</td>
<td>.00</td>
<td>.00</td>
<td>.01</td>
</tr>
<tr>
<td>Time and intervention</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Time of measurement</td>
<td>.31**</td>
<td>.08</td>
<td>.26*</td>
<td>.11</td>
<td>.25**</td>
</tr>
<tr>
<td>Experimental group</td>
<td>-29**</td>
<td>.12</td>
<td>-15</td>
<td>.14</td>
<td>-17</td>
</tr>
<tr>
<td>Interaction term</td>
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<td>.13</td>
<td>.51**</td>
<td>.16</td>
<td>.52**</td>
</tr>
<tr>
<td>Pseudo $\Delta R^2$</td>
<td>3%</td>
<td>4%</td>
<td>7%</td>
<td>3%</td>
<td>0%</td>
</tr>
</tbody>
</table>

$^1$Indicates significant difference between experimental group and control group within time point, $p < .05$.

$^2$Indicates significant difference across time within group, $p < .05$.

variance accounted for). When entered as a group (see Table 5, Model 3), all job characteristics remained significant except for job control, and collectively accounted for an additional 37 per cent of the variance in well-being above that explained by the background factors.

Hypothesis 3, that employees in an experimental group of a job redesign intervention will experience an increase in well-being when compared to employees in a control group, was confirmed. The results in Table 5 (Model 2) showed that the intervention had a significant positive association with well-being ($\beta = .24, p < .05$). This result is represented in Figure 2 and shows that well-being increased over time in the experimental group but remained essentially unchanged over time in the control group.

Table 3. Comparison of means between experimental and control groups at each time point for main study variables in the longitudinal sample

<table>
<thead>
<tr>
<th>Job characteristic</th>
<th>Longitudinal sample</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Experimental group Mean (S.E)</td>
<td>Control group Mean (SE)</td>
</tr>
<tr>
<td>Well-being 1</td>
<td>3.23 (.09)</td>
<td>3.35 (.11)</td>
</tr>
<tr>
<td>Well-being 2</td>
<td>3.41 (.08)</td>
<td>3.31 (.11)</td>
</tr>
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<td>Job control 1</td>
<td>3.24 (.10)</td>
<td>3.45 (.11)</td>
</tr>
<tr>
<td>Job control 2</td>
<td>3.51 (.10)</td>
<td>3.50 (.12)</td>
</tr>
<tr>
<td>Participation 1</td>
<td>2.34 (.11)</td>
<td>2.30 (.15)</td>
</tr>
<tr>
<td>Participation 2</td>
<td>2.54 (.11)</td>
<td>2.13 (.12)</td>
</tr>
<tr>
<td>Skill utilization 1</td>
<td>4.76 (.15)</td>
<td>5.09 (.19)</td>
</tr>
<tr>
<td>Skill utilization 2</td>
<td>4.96 (.14)</td>
<td>4.83 (.19)</td>
</tr>
<tr>
<td>Feedback 1</td>
<td>4.60 (.16)</td>
<td>4.98 (.21)</td>
</tr>
<tr>
<td>Feedback 2</td>
<td>5.34 (.14)</td>
<td>4.78 (.18)</td>
</tr>
<tr>
<td>Task obstacles 1</td>
<td>2.66 (.09)</td>
<td>2.69 (.11)</td>
</tr>
<tr>
<td>Task obstacles 2</td>
<td>2.58 (.10)</td>
<td>2.57 (.12)</td>
</tr>
</tbody>
</table>

$^1$Indicates significant difference between experimental group and control group within time point, $p < .05$.

$^2$Indicates significant difference across time within group, $p < .05$.

Hypothesis 4, that job characteristics would mediate the effect of the intervention on well-being, was largely confirmed. Following the procedure for testing mediation recommended by Baron and Kenny (1986), the test of the first step showed that the independent variable (i.e. the intervention) had a significant effect on the dependent variable (i.e. well-being) ($\beta = .24, p < .05$). The result of this analysis is shown in Table 6, Model 1. The test of the second step showed that the independent variable was related to all mediators except task obstacles (see Table 4, Models 1–5). The test of Step 3 showed that all mediators were related significantly to the dependent variable, well-being (job control, $\beta = .23, p < .01$; participation, $\beta = .26, p < .01$; skill utilization, $\beta = .24, p < .01$; feedback, $\beta = .16, p < .01$; task obstacles, $\beta = .26, p < .01$). All variables except job obstacles met the criteria for inclusion in Step 4. The test for Step 4 showed that including the job characteristic of interest into the equation resulted in the coefficient of the interaction term becoming statistically non-significant in each case (see Table 6, Models 2–5). Sobel tests revealed that the change in the significance of the independent variable due to the introduction of the mediator was significant in each case: Job control, $z = 1.99, p < .05$; participation, $z = 2.74, p < .01$; skill utilization, $z = 2.57, p < .01$; and, feedback, $z = 2.74, p < .01$. Overall, job control, participation, skill utilization and feedback were each shown to mediate the effect of the intervention on well-being, whereas job obstacles did not mediate the effect of the intervention.
on well-being. The indirect effect size and total effect size for each test of mediation were, respectively: job control as mediator, $\beta = .07$ and .23; participation as mediator, $\beta = .13$ and .24; skill utilization as mediator, $\beta = .12$ and .25; and feedback as mediator, $\beta = .12$ and .25 (Krull & MacKinnon, 2001).

We were interested in why job control was significantly related to well-being when entered on its own, but not when entered with the other job characteristics. Previous research indicates that skill
utilization mediates the relationship between job control and well-being (Holman & Wall, 2002; Parker, 2003). Further analysis showed that skill utilization partially mediated the relationship between job control and well-being, since when skill utilization was entered, the significance of job control dropped from $t = 4.31, p < .001$ to $t = 2.33, p < .05$ (Sobel test $z = 3.86, p < .001$). Our provisional explanation for the failure of job control to account for variance independently of the other variables is that it is a precursor of skill utilization.

Further analyses examined potential threats to the validity of our findings. Using $t$-tests we examined non-equivalence between the experimental and control groups at Time 1. Apart from job control (see Table 3) there were no significant differences between the two groups in the main study variables. It is therefore unlikely that differences in demographic variables or job type between the two groups explain each group’s reaction to the changes.

To examine possible non-random sampling effects of participant attrition, we followed the procedure recommended by Goodman and Blum (1996). The first step involved testing for the presence of non-random sampling by conducting a logistic regression. The dependent variable was a dichotomous variable representing those present at Time 1 and 2 (i.e. stayers) and those who responded at Time 1 but opted out or had left at Time 2 (i.e. leavers), and all study variables represented the independent and dependent variables. The result showed that none of the variables were related significantly to staying in or leaving the sample. Although this suggests that the data are missing at random (Goodman & Blum, 1996), it can be recalled that those with low well-being were more likely to quit, suggesting that data are missing non-randomly. We therefore went on to test Steps 2–4 of Goodman and Blum’s procedure which are used when data are missing non-randomly. The results indicated that there was no evidence for the effects of non-random sampling on means (Step 2, $t$-tests between stayers and leavers using main study variables), variances (Step 3, differences in variances of main study variables between whole Time 1 sample and stayers) or the relationships amongst independent variables at Time 1 and the dependent variable at Time 1. (Step 4, $t$-tests to compare coefficient of independent variable from hierarchical regression using whole sample with coefficient of same independent variable from hierarchical regression using stayer sample. Each regression includes all independent variables). We also examined whether the well-being of those employees in the groups who voluntarily quit (as well the well-being of those who had moved elsewhere in the organization, not responded due to sickness,
stayed and responded, or stayed and not responded) differed between the control and experimental groups. T-tests revealed no significant differences. We then examined whether employees in the control group were more or less likely by Time 2 to have voluntarily quit (or moved elsewhere etc.). Using a $\chi^2$-test, no difference was found ($\chi^2 = 1.57$, df = 4, $p > .10$). It is therefore unlikely that the different changes in well-being between the control and experimental group occurred because those who quit in the control group had lower well-being than those who quit the experimental group; or were simply a result of employees in the experimental group being more likely to quit (or moving elsewhere etc.) than employees in the control group. Thus, while data are missing at non-random, it appears unlikely that participant attrition affected the results of the study.

Our final analysis to test for intervention induced halo effects (i.e. that employees in the experimental condition who experience change in one aspect of their job being are more inclined to report change in other aspects) showed that the intervention had no effect on inter-team task interdependence, a work characteristic not expected to change as a result of the intervention. This result, and the fact that no change occurred in job obstacles, provides greater confidence that the reported change in each job characteristic reflected a real change in each job characteristic and was not a result of an intervention-induced halo effect.

**Discussion**

This study makes two important contributions. First, it demonstrates that the effect of a job redesign intervention on employee well-being was mediated by multiple improvements in job characteristics, i.e. job control, skill utilization, participation and feedback. The study therefore provides further support for the practical utility of using participative job redesign interventions (such as the Scenarios Planning Tool) to achieve multiple changes in job characteristics as a means of improving employee well-being. This builds on previous studies analysing the mediating effects of job characteristics in job redesign interventions but which have focused solely on job control (Bond & Bunce, 2001; Logan & Ganster, 2005). Second, the study provides further support for job demands–resources models of job design (Demerouti et al., 2001), since changes in both job demands and resources were associated with changes in well-being. Each job characteristic independently contributed to an improvement in well-being. Another positive finding was that the intervention and change in job characteristics accounted for twenty-nine percent of the change in well-being, a moderate effect size. However, a larger effect of the intervention might have been obtained but for two limiting factors. First, changes to work organization were constrained by the organization’s information technology system, such that work procedures could not be altered with ease. Second, many task obstacles occurred at the interface with other departments. But a lack of managerial prerogative over these departments meant that suggestions regarding this interface (e.g. cross functional roles) could not be implemented.

The quasi-experimental design of this study is a particular strength and the use of an inert-treatment control group helps to rule out the possibility of a Hawthorne effect, and as both groups received equivalent attention, it is unlikely that researchers unwittingly influenced each group in different ways. A number of other potential threats to validity were also eliminated, including the non-equivalence of samples at Time 1 and an intervention-induced halo effect, while our analyses showed that any non-random sampling effects of participant attrition were unlikely to have influenced the results. The confounding nature of the outsourcing initiative is also another potential limitation but it is unlikely that the outsourcing initiative caused the results, as the outsourcing initiative was inert with regard to well-being. The use of the inert-treatment control group created by this event does not therefore represent a

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serious threat to the central findings of this paper. The outsourcing initiative was a serendipitous event, and given the difficulty of running quasi-experiments in organizations, Evans (1975) argues that it is important to take advantage of such events. However, future job redesign intervention studies would benefit from having a factorial or fully crossed design to test the separate and interactive effects of the independent variables, and from the planned inclusion of an inert-treatment control group.

One limitation that we cannot deal with is the non-random allocation of employees to groups, so differences in outcome between groups may be caused by a treatment/selection interaction effect (Cook & Campbell, 1979; Shadish et al., 2002). But this specific threat seems unlikely as we found no major differences in the main study variables at Time 1 between the experimental and control groups. Another limitation is that the job design and well-being measures were self-report. This raises the possibility that common method variance may artifically inflate the relations between these variables; but the main reason to doubt that this is a problem is the existence of non-significant zero-order correlations between some variables. If common method variance were operative we would expect positive associations across the variables in general and an equivalent change in the experimental and control groups, which together would militate against mediation effects. Further validity for the main criterion variable is provided by our finding that employees at Time 1 who had voluntarily quit the organization by Time 2 had a significantly lower level of job-related well-being than those still employed by the organization at Time 2. One more limitation is the relatively small sample size, an implication of which is that it cannot be concluded firmly that job characteristics fully mediate the effect of the intervention on well-being; a larger sample may have led to a finding of partial rather than full mediation. It would also have been beneficial to conduct a simultaneous test of the separate meditational paths of each job characteristic to get a more precise indication of their indirect and total effects (Preacher & Hayes, 2008), although we are not aware of a test suitable for a multi-level context. The relative contribution of each job characteristic to the change in well-being is also difficult to state with certainty, as it cannot be assumed that the manipulation of each job characteristic was of equal strength.

Future research could examine the alternative mechanisms through which job redesign interventions have their effects. One plausible mechanism is the psychological contract (Rousseau, 1995). The provision by an organization of a positive and healthy work environment is likely to form an important part of an employee’s psychological contract. A job redesign intervention may signal to employees that the organization is fulfilling its duty of care towards them (Schalk, 2004), or it may be viewed as an unacceptable alteration of existing work conditions (Barnett, Gordon, Gareis, & Morgan, 2004); and the absence of an intervention on job design might be viewed as a breach of expectation as well (Turnley & Feldman, 2000). An intervention may therefore meet or breach the employee’s psychological contract, and such changes have been shown to be associated with, respectively, higher and lower employee well-being (Conway & Briner, 2002; Lambert, Edwards, & Cable, 2003).

In conclusion, this study has shown that a change in job characteristics was a mechanism through which a participative job redesign intervention had its effect on employee well-being. Although few other job redesign intervention studies explicitly test this mechanism, the findings of this study offers reassurance that job redesign interventions do have effective outcomes because they change job characteristics. The possibility that job redesign interventions may have effects through alternative mechanisms requires further investigation, and attending to these alternative mechanisms may enhance the utility of job redesign.

Acknowledgements

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David J. Holman is a Senior Lecturer at the Institute of Work Psychology and the Management School, University of Sheffield. His primary interests are in the nature and effects of work design, work design in call centers, workplace redesign, and the nature, regulation and effects of affect at work.

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Toby D. Wall was Director of the Institute of Work Psychology, University of Sheffield, from 1996 to 2007, and is Emeritus Professor there as well as Visiting Professor at Leeds University Business School. His interests encompass: work design, employee well-being and performance; human resource management and organisational performance; and applied research design and methods.

References


THE MEDIATING ROLE OF JOB CHARACTERISTICS


Appendix

1. Example of multi-level equation for testing effect of intervention on job characteristic
   
   Level 1: Job Control \( i = \beta_0j + \beta_1j (Time_{ij}) + \beta_2j (Group_{ij}) + \beta_3j (Interaction\ term\ ij) + R_{ij} \)
   
   Level 2: \( \beta_0j = \gamma_{00} + \gamma_{01}(Age_{j}) + \gamma_{02}(Gender_{j}) + \gamma_{03}(Tenure_{j}) + U_{0j} \)
   
   \( \beta_1j = \gamma_{10} \)
   
   \( \beta_2j = \gamma_{20} \)
   
   \( \beta_3j = \gamma_{30} \)

2. Specific items for level-1 measures

**Job control**

To what extent:

1. Do you set your own pace of working?
2. Do you have control over when to take breaks?
3. Can you control how much work you do?
4. Can you vary how you do your work?
5. Do you plan your own work?
6. Can you choose the methods to use in carrying out your work?
7. Can you choose what to say to a customer?
8. Can you choose among a variety of tasks to do?
9. Do you control when and how much you interact with others at work?

   A five-point response scale was used (‘not a lot’ to ‘a great deal’).

**Participation**

To what extent:

1. Can you influence decisions about the long-term plans and direction for your team?
2. Can you influence decisions about the long-term plans for your business area?
3. Can you influence decisions about work procedures?

   A five-point response scale was used (‘not a lot’ to ‘a great deal’).
Skill utilization

To what extent do you agree or disagree with the following statements:

1. I make full use of my skills.
2. I get the opportunity to develop new skills.
3. I get challenged by my job.
4. I use a variety of skills in my job.

A seven-point response scale was used (‘strongly disagree’ to ‘strongly agree’).

Feedback

To what extent do you agree or disagree with the following statements:

1. I receive frequent feedback on my performance.
2. The feedback I receive about my performance is constructive.
3. I am satisfied with the amount of feedback I receive about my performance.
4. I am satisfied with the way in which feedback about my performance is given.

A seven-point response scale was used (‘strongly disagree’ to ‘strongly agree’).

Task obstacles

To what extent:

1. Is your work affected by changes to tasks and procedures?
2. Do you have to deal with unnecessary interruptions?
3. Does a lack of support from others affect your work?
4. Do computer problems affect your work?
5. Does not being able to do certain tasks affect the quality of your work?
6. Do you spend a lot of time on acquiring information or materials to do your job?

A five-point response scale was used (‘not at all’ to ‘a great deal’).

Inter-team task interdependence

To what extent do you agree that:

1. I need to talk to people outside my team to get my work done.
2. People outside my team need to talk to me to get their work done.
3. I cannot get my tasks done without information or materials from members of other teams.

A five-point response scale was used (‘strongly disagree’ to ‘strongly agree’).

Job-related well-being

During past month, how often has work made you feel:

1. Tense
2. Miserable
3. Depressed
4. Optimistic
5. Calm
6. Relaxed
7. Worried
8. Enthusiastic
9. Anxious
10. Comfortable
11. Gloomy
12. Motivated

A five-point response scale was used (‘never’ to ‘all of the time’) and negatively worded items are reverse coded.