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Abstract

Pre- and post-intervention data on health outcomes, absenteeism, and productivity from a longitudinal, quasi-experimental design field study of office workers was used to evaluate the economic consequences of two ergonomic interventions. Researchers assigned individuals in the study to three groups: a group that received an ergonomically designed chair and office ergonomics training; a group that received office ergonomics training only; and a control group. The results show that while training alone has neither a statistically significant effect on health nor productivity, the chair-with-training intervention substantially reduced pain and improved productivity. Neither intervention affected sick leave hours.

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I. INTRODUCTION

Despite the recent interest in ergonomic work standards by state and federal policymakers (California and Washington have recently passed statewide standards while the Bush administration recently rejected federal standards developed under the Clinton administration), economists have nearly ignored the effect of ergonomic interventions on productivity. A recent search of EconLit finds only 16 articles retrieved using “ergonomics” as a keyword, and a search using both “ergonomics” and “productivity” as keywords yielded zero hits, despite the fact that productivity is widely studied by economists and health effects are widely studied within the ergonomics and safety professions. This paper examines the economic impact of two ergonomic interventions using pre- and post-intervention data on productivity, absenteeism, and health from a quasi-experimental field study.

The findings presented here may be of interest to five different audiences: first, policymakers at both the federal and state level considering the social costs and benefits of ergonomic work standards; second, state and federal Occupational Safety and Health Agency regulators; third, health and safety corporate officers considering the type of work standards that might be most appropriate in an office setting; fourth, business managers seeking to improve the performance of their employees; and fifth, researchers interested in the relationship between individual health and economic outcomes.

While there are well-designed intervention studies in manufacturing or materials handling environments (cf. Daltroy et al. 1997; Loisel et al. 1997), there are few in office environments (for a review see Karsh et al. 2001; NRC 2001). The small number of office intervention studies has focused on either ergonomic training (Brisson et al. 1999; Hinman et al. 1997; Kamwendo and Linton 1991), alternate input devices (Rempel et al. 1999; Tittiranonda et al. 1999), or a broader set of office environment changes (Aaras et al. 1999, 2001; Nelson and Silverstein 1998; Rudakewych et al. 2001). The only office chair study followed a selected group of senior managers for two weeks after receiving chairs and found that the group of managers receiving the chairs reported lower discomfort levels (Ghahramani 1992).

In contrast to the paucity of research evaluating ergonomic chairs for office workspaces, there are a number of published studies evaluating computer ergonomics training. Evaluations of program effectiveness suggest positive changes in workstation configuration, chair adjustments, reduction in self-reported musculoskeletal symptoms, and repetitive strain injury incidence (e.g., Bayeh and Smith 1999; Bohr 2000; Brisson et al. 1999; Dortch and Trombly 1990; Green and Briggs 1989; Kukkonen et al. 1993; Robertson and Robinson 1995; Verbeek 1991). Ergonomic training and educational interventions have been advocated as potential prevention methods for reducing the incidence and severity of musculoskeletal injuries and, therefore, it is important to include training in an office ergonomics intervention (e.g., VanAkkerveeken 1985). Moreover, the evidence that exists from health researchers linking specific health measures or specific health promotion programs to individual productivity measures is sparse, as several recent reviews have noted (Warner et al. 1988; DeRango and Franzini 2002). Studies in this literature rely on either nonexperimental study design for inference (no control group) or limited measurement periods, and/or examine the intervention's effect on health or productivity, but not both simultaneously. The National Institute for Occupational Safety and Health (NORA 1996) identify the dearth of well-designed ergonomic interventions with cost-benefit evaluations as a critical research shortfall.

The microeconomic literature relating health to labor market outcomes usually assumes that improved health makes workers more productive and that more productive workers will receive better wages (see Currie and Madrian [1999] for a review of this literature). While many of these studies find that higher wages are correlated with good health (typically measured as self-reported health using a scale of excellent, good, fair, or poor), a growing literature (for examples, see Cockburn et al. 1999 and Berndt 2000) examines whether the postulated intermediate step, in which better health makes workers more productive, actually occurs. To the extent that this study finds that a specific health improvement, such as pain reduction, leads to more productive employees, the results presented here strengthen the existing microeconomic literature on health and wages.

This field study was unique in several dimensions. It was unusually comprehensive in that it simultaneously measured changes in knowledge of ergonomic principles, office space utilization, pain, absenteeism, and productivity (although the analysis in this paper covers only some of these outcomes). Furthermore, the study followed subjects for a relatively long time frame—11 months pre-intervention and 12 months post-intervention. The productivity outcome variable used (sales tax collections per effective workday) was an objective, rather than subjective, performance measure. Moreover, the productivity measure was dollar-denominated, making cost-benefit analysis straightforward. This was the first field study of a workplace health and ergonomic intervention to utilize a dollar-denominated productivity measure.

The total effects of the two interventions on monthly pain levels,[1] sick leave hours, and productivity were analyzed using difference-in-difference estimators that control for job characteristics, job tenure, gender, disability status, age, and years of education. In the health-mediated model, the effect of the two interventions on pain was estimated first. Second, the effect of pain on productivity was estimated. These two estimates were combined to calculate the health-mediated effect of the training-only intervention and the chair-with-training intervention. Results from both models indicated that the chair-with-training intervention reduced pain and improved productivity relative to the control group, but did not affect sick leave. The productivity benefits of the chair-with-training intervention were quite large compared to the intervention’s costs. Conservatively, the benefit flows indicate that the chair-with-training intervention paid for itself within nine working days. From the employer’s perspective, the benefits of the chair-with-training intervention were 25 times the size of the costs after 12 months. In contrast, the training-only intervention did not produce any statistically significant changes for any outcome studied.

The paper is organized as follows: Section II details the study design, the underlying theory of change developed by the research team, data used in this analysis, and the estimation strategies used to model the impact of the interventions on productivity. The total effects productivity model and health-mediated productivity model results are presented in sections III and IV, respectively. Sections V and VI discuss the absenteeism data analysis and cost-benefit analysis, respectively. Section VII offers discussion and conclusions.

II. STUDY DESIGN, UNDERLYING THEORY OF CHANGE, AND DATA

Approximately 200 volunteers were recruited to participate in a study of ergonomics and productivity from a governmental agency that collects sales taxes, a State Department of Revenue (hence, DOR). Study participants were assigned by researchers to one of three groups: a control group; a group that received an office ergonomic training; and a group that received a highly adjustable chair and training (study design, interventions, and health effects are discussed in Amick et al., forthcoming). Health surveys developed by the study team were administered two months and one month immediately prior to group assignment and intervention implementation. Subsequently, the research team re-administered the same surveys during the second, seventh, and twelfth months post-intervention. The control group received the training intervention after the twelfth month of data collection. In addition, the agency managers provided administrative data on job characteristics, study participant demographic profiles, absenteeism, actual hours worked, and productivity.

Study participants in both the training-only and chair-with-training groups were trained in general office ergonomics knowledge with an emphasis on developing skills for recognizing office work risk factors, seating adjustment, and workstation arrangement. The training-only and chair-with-training group received identical training, except those in the training-only group were taught how to adjust their existing chairs while those in the chair-with-training group were taught how to adjust their new chairs. After the training was completed, study participants were responsible for making any subsequent changes to their workspace and working with the company's ergonomic resources. E-mail messages were sent out to remind workers about key ergonomic issues identified through a post-training knowledge exam and through workstation observations made after the intervention. The office ergonomic measures described are easily generalized to other firms in which office workers are seated.

Complete random assignment was not feasible in this study, since it was possible for educational information to be shared between members of the intervention groups and the control group. For example, workers who received office ergonomics training could potentially have shared their new information with coworkers nearby, especially if they happened to notice a coworker using a less than ideal working posture. Thus, where possible, all participants from the same building were assigned to the same treatment group. When this was not possible, people on different floors of the same building were assigned to different groups. Attempts were made to balance workload requirements and job descriptions across the three groups. The study design specified data collection on dependent and independent variables prior to the implementation of the two interventions in order to correct for any preexisting differences between treatment and control groups at baseline that may predict health and productivity.

To have been included in the study, each participant must have spent at least six hours a day sitting in an office chair and at least four hours a day computing, have been able to complete a questionnaire in English over the internet at work, and not have filed a worker's compensation claim in the last three months. Informed consent was transferred over the Internet as approved by the Liberty Mutual Research Institute for Safety of Human Subjects Committee. Furthermore, the company was required to provide researchers with detailed data on both an individual worker's productivity and work hours.

The quasi-experimental field study was conducted over a 15-month period, although data on production and absenteeism was obtained for the 11 months prior to the intervention, allowing for 23 months of data in all. Worker-month observations were excluded from the sample when employees switched from full-time to part-time work because part-time work is not compatible with the research protocol. This exclusion affected 243 worker-months, or about 10 workers per month over the entire sample. Furthermore, worker-month observations were excluded when employees collected over $50,000 in sales taxes per effective workday in a given month. Employees typically collected $34,000 total a month of sales taxes (see Table 1), so sales tax collections of $50,000 a day represent unusually large amounts that could potentially bias our results. This exclusion affected four worker-months, or about one worker every five months. Neither of these exclusions substantially affected the sign, size, or statistical significance of the results reported in the following paragraphs.

Data on productivity, absenteeism, worker, and job characteristics all came from administrative data. Data on health status, specifically bodily pain, came from surveys administered to participants at months –2 and –1 prior to the intervention and months 2, 7, and 12 post-intervention. Data on changes in office ergonomic knowledge, postures, work environment, and chair satisfaction pre- and post-intervention are not presented in this paper, but their collection is part of the study design. Figure 1 provides a graphical representation of the study timeline.

The study design and implementation was guided by a theory of change depicted graphically in Figure 2 (Amick et al. forthcoming). The theory proposes that office ergonomics training increases the worker's knowledge about ergonomics and motivates the worker to engage in behaviors that improve work effectiveness and reduce psychosocial and biomechanical strains (Robertson et al. 2002). Reduced postural loading and muscle fatigue should translate into improved health-related work role functioning, and consequently increased performance and productivity. Furthermore, office ergonomics training can lead to improvements in performance and productivity through other routes besides improved health, such as enhanced efficiency and satisfaction leading to increased worker motivation.

At this firm, employee performance is evaluated according to volume of sales taxes collected.[2] In order to construct the productivity measure, individual monthly sales tax collections were divided by the number of effective workdays per month, where an effective workday was defined as eight hours of work. Both total hours worked and sales tax collections were derived from administrative data that was provided for the 11 months before the intervention and the 12 months post-intervention for a total of 23 months of both sales tax collections and hours worked. It is important to note that we have data on actual hours worked by each individual; when we calculated the number of effective work days per month, we were not estimating work hours (based on a series of assumptions), but rather using data from time sheets. Tax collections per effective workday were used to measure productivity instead of total monthly sales tax collections, because it allowed us to distinguish between changes in monthly hours worked and changes in the efficiency of production per unit of time worked as potential sources for overall productivity gains.

A secondary outcome was sick leave hours per month. Sick leave was used as a measure of lost work time because of a very low incidence of workers' compensation claims. DOR managers indicated that there had not been lost work time at DOR due to a worker's compensation claim in at least ten years. Sick leave data came from administrative records on lost work time and was measured monthly. Leave codes accompany lost work time, revealing, for instance, if an employee missed work due to his or her own illness or the illness of another family member. We defined sick leave as lost work time associated with an employee's own illness. While these codes allow for the exclusion of absences due to vacations, maternity leave, or sick family members, they do not distinguish between work-related health conditions and non-work-related health conditions. Ergonomic measures may affect work-related lost time but should not affect non-work-related lost time. Hence, this outcome variable suffers from measurement error. While measurement error in the dependent variable increases the size of standard errors, thus posing a challenge to statistical significance, it does not impart a bias to the coefficient estimates (see for instance Greene 1990, pp. 294–295).

An intermediate outcome was health. Our health measure, freedom from pain, was collected from the administration of a series of questionnaires in months –2, –1, 2, 7, and 12, both pre- and post-intervention. This two-item scale assessed the degree of pain a person experienced within the past month (Ware et al. 1994). Respondents answered two questions: "During the past four weeks, how much did pain interfere with your normal work, including both work outside the home and housework?", and "How much bodily pain have you had during the past four weeks?" The answers were combined, weighted, and rescaled to vary between 0 and 100, with 100 indicating complete freedom from pain. U.S. norms are provided by Ware (1994).

Demographic data were obtained from the administrative records of the employer. Workers' pre-intervention ages were used and fixed for the duration of the study. Gender was defined with an indicator variable (female = 1). Education was coded as years of education. Finally, a measure of whether the worker was classified as disabled according to the firm's own criteria was fixed pre-intervention.

Job information was also obtained from personnel data. Job tenure was measured in years. Job levels range from a low of one to a high of five; people with higher job levels generally have more supervisory responsibilities and thus spend less time collecting taxes. A dummy variable indicates whether an individual is a "collector." People who were not designated as "collectors" were still responsible for collecting sales taxes but had other duties, and generally had lower levels of sales tax collections; nevertheless, the firm's managers informed the research team that sales tax collections were still considered an important measure of productivity even for non-collectors.

Two strategies were used to estimate the effect the interventions had on productivity: a total effects model, in which regression adjusted group differences in total production pre- and post-intervention were compared; and a health-mediated model, in which the intervention was only allowed to affect production by changing SF-36 pain scores.

The two modeling strategies were motivated by a concern regarding Hawthorne effects which can occur when researchers monitor workers' production more closely than employers. Under these circumstances, the interventions' effect on production may be confounded by a higher work effort than would occur if the study had not been conducted. In addition, there may be psychological benefits to participants who view their inclusion in the study as evidence they are valued employees and participants may respond with improved productivity regardless of the underlying merits of the interventions. While these types of confounding factors may affect the total effects estimates of production increases, they are unlikely to affect the health-mediated estimates. While the total effects estimates include any post-intervention differences in production across treatment groups over and above preexisting pre-intervention differences, the health-mediated model estimates include only those improvements in productivity that are associated with improvements in the SF-36 pain score. Furthermore, with 3 post-intervention measures over 12 months, the sustainability of the intervention effects are being tested. All the confounding factors above would likely result in a transient effect. We would expect the novelty of the intervention to eventually subside, whereas only a "true effect" would remain in the long term.

Nevertheless, factors besides the Hawthorne effect may explain why total effects estimates are larger than health-mediated estimates. According to the proposed theory of change, improvements in training and seating equipment may lead to improved productivity by other means besides health. For example, the worker may use the office workspace more efficiently. Furthermore, the interventions may lead to higher levels of comfort and employee satisfaction, which in turn may lead to higher levels of productivity. A larger effect for the total effects model, as compared to the health-mediated model, would support the existence of such alternative routes of productivity improvement.

To estimate the total effects model, both fixed effects and random effects estimation methods were conducted for the sake of robustness. Productivity per effective workday was modeled as a function of demographic variables, job characteristics, treatment group assignment (training or chair-with-training group dummy variables), a post-intervention dummy variable (which is interacted with the treatment group dummies), and individual-specific dummy variables (the fixed or random effects).

To estimate the health-mediated model, a two-step method was used. In the first step (A), regression-adjusted SF-36 pain scores were compared pre- and post-intervention across treatment and control groups in order to estimate the interventions' effect on pain. In the second step (B), changes in individual production were associated with changes in reported SF-36 pain levels. Thus, the effect of the office ergonomic interventions in the health-mediated model was given by the product of $A \times B$.

The pre- and post-intervention variable means used for this paper appear in Tables 1 and 2, respectively. People in each group were in their mid-forties, college educated, and had similar levels of SF-36 pain (in the mid-sixties). However, there was a much higher level of collections per effective workday in the chair-with-training and control groups pre-intervention in comparison to those in the training-only group. This difference appears to be attributable to the fact that there were relatively few collectors in the training-only group and the fact that those in the training-only group are higher level managers. There are more women in the control group than in the other two groups as well. In general, people in all three groups had reasonably similar tenure levels (about 14 to 17 years on average). The final difference was that there were more people classified as "disabled" in the training-only group (20 percent) compared to both the control (3 percent) and the chair-with-training group (9 percent).

A comparison of the SF-36 pain scores of study participants by age group compared to U.S. national norms is given in Table 3. Participants aged 18–24, 55–64, and 65–74 had less or similar levels of pain compared to their national counterparts, but those aged 25–34, 35–44, 45–54, and 75+ appear to have more pain on average than similarly aged individuals in the United States as a whole. While study participants on average had higher levels of pain than national norms, these pain differences were not consistently higher for all age groups.

III. TOTAL EFFECTS MODEL

In this section of the paper, the effects of the chair-with-training and training-only interventions on production are captured using a difference-in-difference estimator. In this "total effects" model, production differences between groups are compared pre- and post-intervention, conditional on a set of control variables. The model captures the net effect of all influences on production changes over time.

The coefficient estimates from two models of productivity are found in Table 4. The coefficient estimates are derived from 23 months of productivity data provided by the firm's managers, 11 months of data prior to the intervention, and 12 months post-intervention. All the production data reflect individual tax collections rather than group averages. DOR managers indicated that sales tax collections were not seasonal, a contention that was verified by an examination of the data. Hence, no controls for quarter or month were included in the model.

The models differ depending on whether fixed or random effects were used or if a "post-intervention" stand-alone indicator variable was included. All columns report coefficient estimates and standard errors (in parentheses) from a difference-in-difference model in which pre-existing production differences between treatment groups are captured by the "chair-with-training" and "training only" variables, and the net post-intervention effects of the interventions are summarized in the "chair-with-training × post-intervention" and the "training × post-intervention" variables. Recall that coefficients on variables that are constant over time, such as female, age at the beginning of the study, tenure at the beginning of the study, disability status, education level at the beginning of the study, and treatment group assignment, are not identified in a fixed effects model. Hence, no coefficients are reported for those variables when fixed effects are used.

The baseline model of productivity excludes the "post-intervention" stand-alone indicator variable and is found in columns 1 and 3 in Table 4. This specification is preferred because there was no reason to expect a change in post-intervention production for the control group. In this baseline model, point estimates for the "chair-with-training × post-intervention" variable range from \$324.44 to \$353.11 per effective workday, while point estimates for the "training × post-intervention" variable range from \$151.01 to \$155.69 per effective workday. In the case of the training-only intervention, none of the post-intervention coefficient estimates are statistically significant. In the case of the "chair-with-training" intervention, both are statistically significant at the 5 percent level. Columns 2 and 4 report the two sets of coefficient estimates which incorporate a stand-alone "post-intervention" variable. In both the fixed and random effects models, the "post-intervention" coefficient estimates are quantitatively small (indicating a possible upward drift of \$45 or \$36 in tax collections per effective workday post-intervention, respectively), and are not statistically significant. Thus, excluding a stand-alone "post-intervention" variable from the model appears warranted. Nevertheless, it should be noted that including the "post-intervention" stand-alone variable raises the *p*-value on the "chair-training × post-intervention" coefficient to 0.11 and 0.13 respectively, in the cases of the fixed and random effects specifications. This set of estimates is available from the authors upon request.

Furthermore, a series of regressions using alternate specifications of the baseline model were run to test the results' robustness. Eight different models were estimated, using tax collections levels (as above) or the natural log of collections, using a sample in which non-collectors were included (as above) or a sample in which non-collectors were excluded and using fixed or random effects to control for individual heterogeneity. The eight models correspond to all the possible permutations of these three binary choices (2 × 2 × 2 = 8). In all eight cases, the coefficient on the "chair-with-training × post-intervention" variable was significant at the 5 percent level. Moreover, the size of the coefficients is comparable to the size of the productivity effects reported in Table 4. For the "training only × post-intervention" variable, the results were more mixed. In two cases, the coefficients were significant at the 5 percent level, in two other cases the coefficients were significant at the 10 percent level, and in the other four cases, the coefficients were not significant at a meaningful level.

Another specification of the baseline model was run in order to examine whether the productivity effects faded with time. Thus, we added two variables to the baseline model in Table 4 which consisted of a time variable interacted with both the "chair-with-training × post-intervention" and the "training only" variables. The "time" variable takes on values from one to twelve for each of the post-intervention months, and is zero otherwise. In this specification, the effect of an intervention is expressed both as a constant (on the "chair-with-training × post-intervention" and the "training only × post-intervention" variable) and as something that varies with time (the post-intervention treatment group variables interacted with the time variable). In this specification, evidence of a fading treatment effect would be expressed as a negative and statistically significant coefficient on these two new time-variant variables. In fact, the coefficients on these time-variant variables are positive but not statically significant for both treatment groups and for both the fixed effects and random effects models. Thus, at least in the time frame of our study, there is no evidence that the productivity gains are short-lived.

IV. HEALTH-MEDIATED EFFECTS MODEL

This section of the paper analyzes the effects of the two interventions on the SF-36 pain score and the relationship between the SF-36 pain score and production. In the first step, pain scores are modeled as a function of gender, age, tenure at the agency, disability status, years of education, job characteristics (collector and level), treatment group assignment (chair-with-training and training-only), and treatment group assignments interacted with a post-intervention dummy variable using fixed effects and random effects estimates. The results of these estimations are found in Table 5.

The pain regressions in Table 5 follow the same form as the productivity regressions in Table 4, using the same dependent variables and the same panel regression techniques. As before, preexisting differences in pain scores between groups are reflected in the “chair with training” and “training only” dummy variables, while the effect of the interventions on pain are summarized by the “chair-with-training × post-intervention” and “training × post-intervention” variables. This baseline model excludes the “post-intervention” stand-alone variable because there was no expectation that pain scores would change in the control group post-intervention. The coefficient estimates from this baseline model in columns 1 and 3 indicate that the chair-with-training intervention reduced pain by 5.95 to 6.23 points, and the training-only intervention reduced pain by 1.83 to 2.12 points, depending on whether random or fixed effects are used (recall that higher scores of the SF-36 score correspond to lower levels of pain). In the case of the chair-with-training intervention, both estimates are significant at the 5 percent level. In the case of the training-only intervention, neither estimate is statistically significant.

An alternative specification including a stand-alone “post-intervention” dummy variable is found in columns 2 and 4. This specification allows for the possibility of a secular trend in pain scores over time, which could, in theory, confound the estimates of the interventions’ impact on pain. The coefficient point estimates on the "post-intervention" stand-alone variable indicate an unexpected, moderate drift in pain scores among the controls. Controlling for post-intervention changes in pain scores among those who did not receive any intervention reduces the estimated impact of the chair-with-training and training-only interventions by 2 points, suggesting caution when interpreting the "chair-with-training × post-intervention" and "training only × post-intervention" coefficients in the baseline model. Nevertheless, one cannot rule out the possibility that the observed change in post-intervention pain among the controls may be due to random noise given that the coefficient estimates on the "post-intervention" stand-alone variable are not statistically significant. A larger sample would have been necessary to resolve this issue.

Table 6 contains the coefficient estimates and standard errors of a regression of tax collections per effective workday on the same set of demographic and job characteristic variables as in Table 5, plus pain scores. The estimates found here indicate that a one-point improvement in pain is associated with either a $13.25 or $19.14 increase in production per effective workday depending on whether fixed or random effects were used.

With these numbers in hand, we can calculate the health-mediated effect of the chair-with-training intervention. The health-mediated estimate of the productivity gain derived from the training-only intervention is assumed to be zero, given that there is no statistically significant relationship between the training-only intervention and post-intervention improvements in pain. For simplicity, we limit the discussion here to the fixed effects baseline model in Table 5 (column 1) and the fixed effects model in Table 6 (column 1), although similar numbers can be easily obtained using the numbers from the other regressions. In Table 5, the estimated coefficient indicates that the chair-with-training intervention reduces pain by 6.23 points. In Table 6, a one-point reduction in pain is associated with an increase in tax collections of $19.14 per effective workday. Thus, the health-mediated effect of the chair-with-training intervention is 6.23 × $19.14 = $119.24 per effective workday.

V. LOST WORK TIME

Tables 7 and 8 provide a total effects and health-mediated effects model of the two interventions on monthly hours of sick leave, the measure of absenteeism provided by the firm. The form of these two models is analogous to the total effects and health-mediated effects models of productivity, except that they predict sick leave hours per month rather than sales tax collections per effective workday. An examination of the "chair-with-training × post-intervention" and "training × post-intervention" coefficients in Table 7 reveals that none of the coefficients are quantitatively large (for example, sick hours are reduced by 0.16 hours in Column 1, or 0.02 workdays per month) or statistically significant at a reasonable level. A similar conclusion can be found in Table 8. While the coefficient estimates on the "chair-with-training × post-intervention" and "training × post-intervention" variables are statistically significant, the point estimates imply a relatively trivial change in sick leave hours per month compared to the gains in on-the-job productivity reported in the previous two sections. For instance, the fixed effects estimate implies a 0.04 hours reduction in sick leave hours per month per point of SF-36 pain reduced. This implies a total monthly change of sick leave of 0.04 × 6.23 = 0.25 hours per month.

VI. COST-BENEFIT ANALYSIS

Table 9 summarizes our findings and puts them in context. The average amount (not regression adjusted) of individual collections per effective workday in the 11 months prior to the interventions was $1,993.98. This number will serve as the base value used in our calculations of the percentage increase in production due to the chair-with-training intervention. Our estimate from the health-mediated model of productivity indicates that the chair-with-training intervention led to a $119.24 increase in sales tax collections per effective workday, or a 6 percent increase over the pre-intervention base figure. Our estimate from the total effects model indicates a $353.11 increase in sales tax collections per effective workday, or a 17.7 percent increase over the pre-intervention base figure.

The benefit-to-cost ratio at one year after the intervention is calculated using fixed effects estimates from the baseline model only. A benefit-to-cost ratio greater than one indicates a positive return on investment while a number less than one indicates an economic loss. The chair itself cost $800 per person and the direct costs of the trainers (their time and travel expenses) amounted to $200 per participant. The participants' average hourly wage is $21.49/hour. Thus, the labor costs of the 90-minute training session averaged $21/hour × 1.5 hours = $32 per participant. The intervention benefits include reductions in absenteeism (0) and increases in on-the-job production. Using the more conservative estimate of increased production from the health-mediated model of $119.24 per workday and the administrative data's per-person average of 17.75 effective workdays per month, the average monthly benefit flow is $119.24 × 17.75 = $2,116.51 per month or $2,116.51 × 12 = $25,398.12 per year. Thus, the benefit-to-cost ratio for the chair-with-training intervention is $25,398/($800+$200+$32) = 24.61. In other words, benefits from the chair-with-training intervention are approximately 25 times larger than costs in the first year.

The large size of the benefit-to-cost ratio may reflect political constraints on staffing levels unique to the public sector. It is plausible that state legislatures may understaff departments of revenue due to budget pressures and political concerns, leading to a marginal product of labor that is considerably higher than a sales tax collector's wage. The marginal product of labor in private firms may be much closer to the wage rate. In such cases, the daily benefits of the chair-with-training intervention can be approximated by multiplying the percentage increase on-the-job daily production by the wage rate. The benefit after one year is this number multiplied by the average number of days worked in a month times 12. Using the 6 percent increase in production from the health-mediated model, this "wage replacement" method yields a daily benefit of $21.49/hour × 0.06 × 8 hours = $10.32, which is about 12 times smaller than the benefit estimated previously of $119.24. Taking the wage rate and number of days worked per month from the study above, the benefit-to-cost ratio after the first year would be ($10.32 × 17.75 days per month × 12 months)/($800 + $200 + $32) = 2.13. Thus, the lower productivity gain estimates from the health-mediated model imply that the “chair-with-training intervention” would pay for itself within six months in a firm similar to this agency where the marginal product of labor equaled the wage.

VII. DISCUSSION AND CONCLUSION

The productivity gains associated with the chair-with-training intervention are similar to the gains reported in two other studies. Dainoff (1990) conducted a series of laboratory experiments in which the office productivity of subjects was monitored using different office configurations. He found a 17.5 percent productivity increase in subjects working in an ergonomically optimal setting compared to one which was ergonomically suboptimal, a number which is comparable to the total effects estimate productivity increase (17.7 percent) associated with the chair-with-training intervention. Niemela et al. (2002) report non-experimental evidence that a renovation of a harbor storage facility resulted in a 9 percent post-intervention productivity increase compared to pre-intervention levels. Nevertheless, it is important to consider that prior studies primarily focused on health outcomes and conducted productivity analysis in an opportunistic *post hoc* fashion. In contrast, this study was specifically designed to assess the productivity effects of a well-designed intervention.

Aaras (1994) provides cost-benefit calculations derived from a 12-year, non-experimental field study of a Swedish telephone manufacturer and finds that workplace redesign substantially reduces turnover rates and sick leave absences. By comparison, we find no effect of the interventions on sick leave hours. After 12 years, Aaras calculates that the benefits to the employer were nine times larger than the costs, implying a breakeven point of a little over a year compared to less than six months in this study when the wage replacement method is used. Nevertheless, it is difficult to directly compare the benefit-to-cost ratios derived from Aaras’ calculations to our own because of the differences in specific interventions, study time frames, and productivity outcome variables.

There are three important factors to note concerning the calculations of productivity impacts reported. First, the independent calculation of the health-mediated model estimates acts as a type of validation of the total effects estimates. While there are theoretical reasons to expect that the health-mediated effects would be smaller than the total effects estimates, there was no guarantee that the empirical estimates would conform to this theoretical supposition. The fact that two independent methods of calculating the interventions' effects yield internally consistent results provides evidence of the reliability of both sets of estimates. The reverse would be true had the health-mediated estimates been larger than the total effects estimates. Second, about a third (from row E in Table 9, 6.0/17.7 = 0.339) of the total effect of the "chair-with-training intervention" on productivity can be explained by improvements in pain scores alone, leaving aside any improvements in work space utilization, job satisfaction, comfort, or fatigue that may have led to increased production. Third, there are potentially large production gains from an ergonomic intervention, even when the intervention has no effect on lost work time. Previous estimates of the social costs of work-related musculoskeletal disorders (such as back and repetitive strain injuries) have relied mostly on estimates of the dollar value of lost work time associated with such disorders.[3] The results from this study suggest that such calculations of social costs suffer from a substantial downward bias. Furthermore, these results show that ergonomic interventions do not necessarily need to reduce lost work time in order to produce a substantial economic benefit to employers; information that is germane to work environments in which lost work time is low.

Perhaps most importantly, the findings of this study suggest that firms may benefit substantially by improving the seating of their office workers in conjunction with a training program in office ergonomic principles and practices, even if these firms do not have workers who suffer from acute musculoskeletal disorders. In contrast, the training-only benefits are less clear. Not only are the point estimates of such benefits smaller than those of the chair-with-training intervention, albeit in the right directions of reducing pain and enhancing productivity, such estimates are not statistically significant. While the point estimates reported from the total effects estimation results suggest a substantial productivity impact for the training-only intervention, a study with a larger sample size would be needed to provide the statistical power necessary to conclusively show that training alone provides a productivity benefit.

Figure 1

Study Timeline

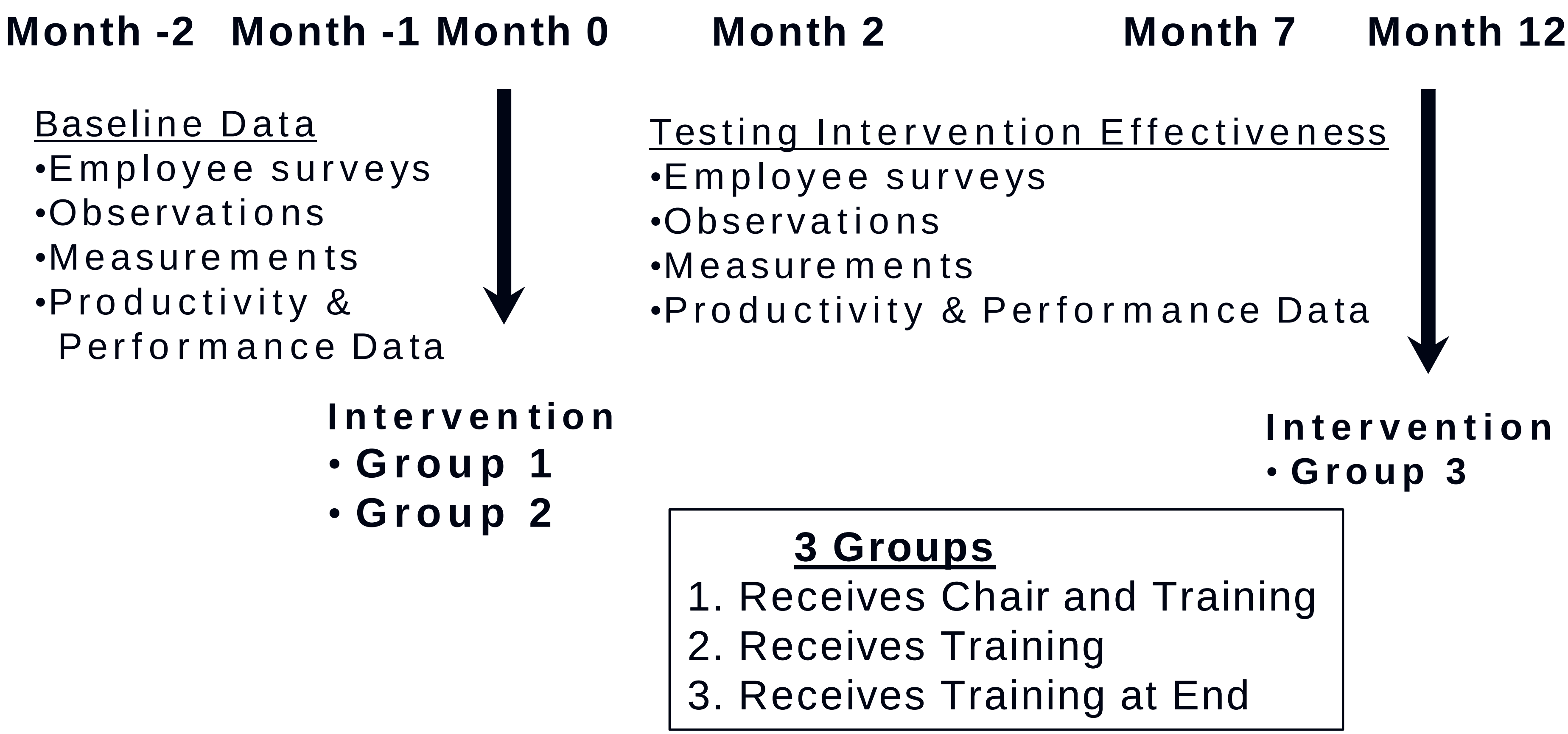

Figure 2
The Model of Change
(from Amick et al. 2002)

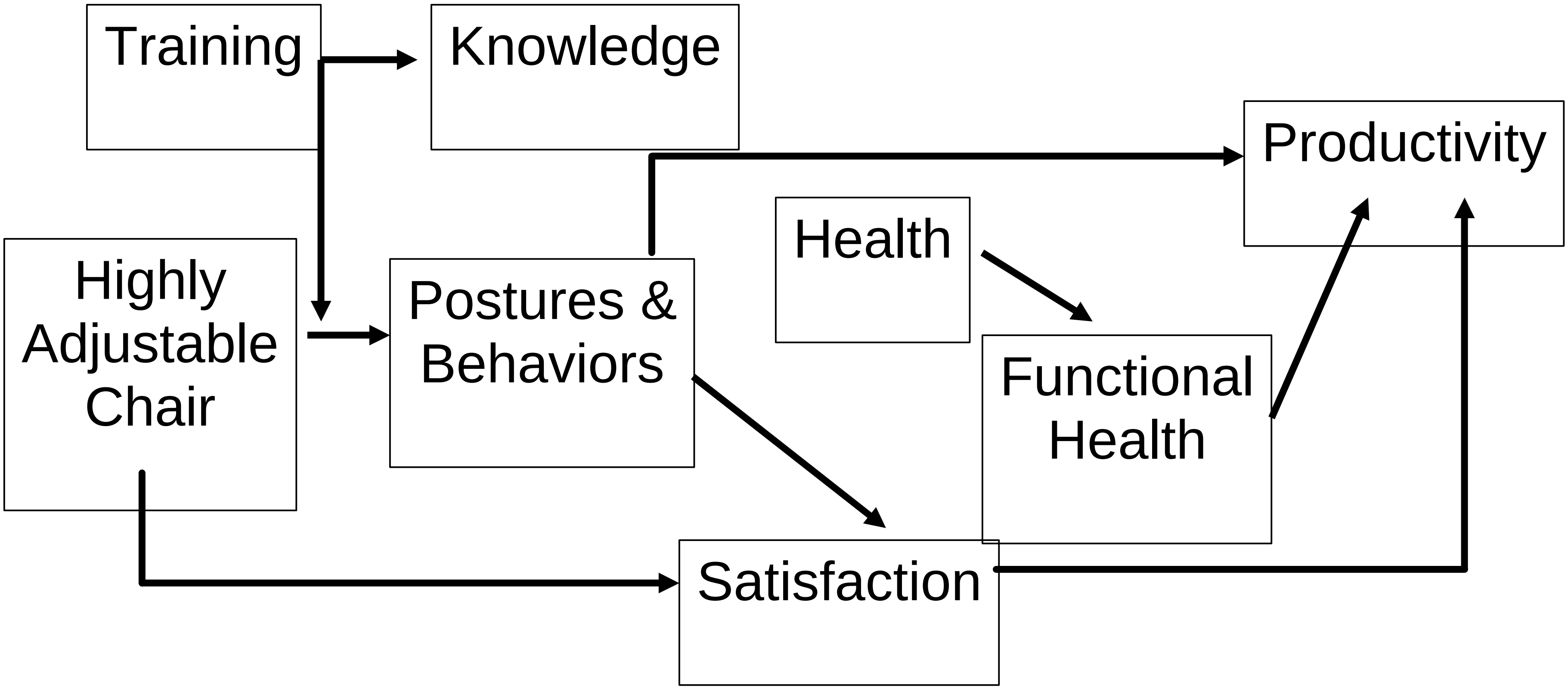

Table 1 Means for Regression Variables (Pre-intervention Data for March and April 2001)

Variable	Total sample	Chair and training	Training only	Control group
Age [a]	47.47	46.77	49.01	46.92
Female [a]	0.58	0.52	0.57	0.69
Tenure [a]	15.88	14.06	16.81	17.65
Disabled [a]	0.11	0.09	0.2	0.03
Years of education [a]	15.03	15.32	15.31	14.25
Collector [a]	0.44	0.47	0.19	0.65
Level [a]	3.28	3.31	3.56	2.93
SF-36 pain score [b]	65.71	66.8	4.66	64.87
Monthly sales tax collected [a]	34509.50	36277.84	22793.71	37394.13
Production per effective day [a]	1940.53	2000.7	1162.94	2144.02
Hours of sick leave [a]	4.69	4.42	4.37	5.45
	(*N*=208)	(*N*=88)	(*N*=61)	(*N*=59)

[a] Means calculated using 11 months of data (July 2000–May 2001).
[b] Means calculated using only 2 survey months (March and April 2001).

Table 2 Means for Regression Variables (Post-intervention Data for July 2001, December 2001, and May 2002)

Variable	Total sample	Chair and training	Training only	Control group
Age [a]	47.47	46.84	48.83	46.98
Female [a]	0.59	0.52	0.57	0.7
Tenure [a]	15.83	13.97	16.71	17.61
Disabled [a]	0.11	0.09	0.2	0.03
Years of education [a]	15.03	15.31	15.32	14.27
Collector [a]	0.45	0.5	0.19	0.64
Level [a]	3.3	3.38	3.54	2.95
SF-36 pain score [b]	69.44	72.38	67.56	66.35
Monthly sales tax collected [a]	34183.67	40098.56	23686.19	34091.47
Production per effective day a	2128.83	2362.64	1306.3	2187.46
Hours of sick leave [a]	4.45	4.23	4.31	4.91
	(*N*=208)	(*N*=88)	(*N*=61)	(*N*=59)

[a] Means calculated using 12 months of data (June 2001–May 2002).
[b] Means calculated using only 3 survey months (July and December 2001, May 2002).

Table 3 The SF-36 Pain Scores of Study Participants and National Norms by Age Group

Age group	National means[a]	DOR means	P-value for test of difference in means
Ages 18–24	80.82	96	0.0321
Ages 25–34	81.35	70.83	0.0006
Ages 35–44	77.06	67.41	0
Ages 45–54	73.12	67.87	0
Ages 55–64	67.51	67.71	0.9053
Ages 65–74	68.49	73.5	0.3467
Ages 75 +	60.88	44.67	0.0475

NOTE: DOR participants, excluding monthly hours worked < 20. Average production per effective day > 50,000, and part-time workers.

[a] National means reported in Ware (1993).

Table 4 Total Effects Model Production per Effective Workday (Production data taken from July 2000 to May 2002)

	Fixed effects	Fixed effects with post-intervention indicator	Random effects	Random effects with post-intervention indicator
Constant	2,463.24**	2,470.40**	–2,164.64	–2,177.92
	(657.09)	(657.57)	(2,437.78)	(2,447.95)
Female	—	—	–258.29	–258.76
			(456.99)	(458.77)
Age	—	—	20.43	20.42
			(27.60)	(27.71)
Tenure	—	—	27.56	27.55
			(27.85)	(27.96)
Disabled	—	—	422.02	423.31
			(722.72)	(725.49)
Education	—	—	186.58	187.01
			(126.87)	(127.36)
Collector	237.93	237.01	1,261.15**	1,256.54**
	(405.92)	(406.00)	(315.59)	(316.07)
Level	–211.75	–217.03	–168.11	–170.37
	(195.08)	(195.77)	(149.05)	(149.56)
Chair and training	—	—	–385.91	–367.07
			(434.33)	(441.89)
Training only	—	—	–803.98	–786.46
			(603.52)	(609.93)
Post-intervention indicator	—	45.46	—	35.63
		(137.74)		(137.64)
Chair-training × post-Intervention	353.11**	307.75	324.44**	288.95
	(134.24)	(192.14)	(134.17)	(192.14)
Training × post-intervention	151.01	105.55	155.69	120.02
	(240.01)	(276.77)	(240.03)	(276.75)
Observations	2502	2502	2502	2502
Overall R^2	0.0125	0.0124	0.1246	0.1243

Standard errors in parentheses; * = significant at 10%; ** = significant at 5%.

Table 5 Health-Mediated Model, Step 1: Effect of Intervention on SF-36 Pain Score
(Health data taken from survey months: March 2001, April 2001, July 2001, December 2001, and May 2002.)

	Fixed effects	Fixed effects with post-intervention indicator	Random effects	Random effects with post-intervention indicator
Constant	62.68**	62.62**	72.10**	70.79**
	(7.02)	(7.02)	(15.42)	(15.41)
Female	—	—	–0.82	–0.79
			(2.73)	(2.72)
Age	—	—	–0.15	–0.15
			(0.16)	(0.16)
Tenure	—	—	0.17	0.17
			(0.17)	(0.17)
Disabled	—	—	–5.54	–5.54
			(3.88)	(3.86)
Education	—	—	–0.11	–0.11
			(0.85)	(0.85)
Collector	7.97	7.72	–4.98**	–5.01**
	(8.13)	(8.12)	(2.47)	(2.46)
Level	–0.09	–0.16	1.36	1.34
	(2.14)	(2.14)	(1.20)	(1.19)
Chair with training	—	—	–0.11	1.18
			(3.19)	(3.38)
Training only	—	—	–1.71	–0.42
			(3.74)	(3.91)
Post-intervention indicator	—	2.48	—	2.18
		(1.96)		(1.95)
Chair-with-training × post-intervention	6.23**	3.75	5.95**	3.77
	(1.48)	(2.45)	(1.46)	(2.44)
Training × post-intervention	1.83	–0.65	2.12	–0.06
	(1.93)	(2.75)	(1.92)	(2.74)
Observations	855	855	855	855
Overall R^2	0.0013	0.0017	0.0538	0.0541

Standard errors in parentheses; * = significant at 10%; ** = significant at 5%.

Table 6 Health-Mediated Model, Step 2: Effect of SF-36 Pain Score on Production per Effective Workday (Health data taken from survey months: March 2001, April 2001, July 2001, December 2001, and May 2002.)

	Fixed effects	Random effects
Constant	727.39	–2,825.24
	(1154.52)	(2,657.94)
Female	—	–262.11
		(492.85)
Age	—	16.70
		(29.98)
Tenure	—	48.35*
		(29.36)
Disabled	—	–52.71
		(795.72)
Education	—	98.18
		(138.33)
Collector	945.86	2,260.05**
	(1,075.64)	(479.31)
Level	–250.17	–194.88
	(337.38)	(198.30)
SF-36 pain score	19.14**	13.25**
	(5.73)	(5.21)
Observations	503	503
R-squared	0.0509	0.1501
Overall R^2		

NOTE: Standard errors in parentheses; * = significant at 10%; ** = significant at 5%.

Table 7 Monthly Hours of Sick Leave (Hours of sick leave taken July 2000 to May 2002.)

	Fixed effects	Fixed effects with post-intervention indicator	Random effects	Random effects with post-intervention indicator
Constant	3.95**	4.06**	9.59**	9.93**
	(1.32)	(1.32)	(2.32)	(2.33)
Female	—	—	0.19	0.19
			(0.41)	(0.41)
Age	—	—	–0.03	–0.03
			(0.02)	(0.02)
Tenure	—	—	–0.03	–0.03
			(0.03)	(0.03)
Disabled	—	—	1.45**	1.45**
			(0.58)	(0.58)
Education	—	—	–0.19	–0.19
			(0.13)	(0.13)
Collector	0.12	0.01	0.34	0.32
	(1.04)	(1.04)	(0.37)	(0.37)
Level	0.18	0.19	–0.14	–0.14
	(0.40)	(0.40)	(0.20)	(0.20)
Chair and training	—	—	–0.49	–0.85
			(0.49)	(0.53)
Training only	—	—	–0.41	–0.76
			(0.57)	(0.60)
Post-intervention indicator	—	–0.68*	—	–0.67*
		(0.36)		(0.36)
Chair-with-training × post-intervention	–0.16	0.52	–0.16	0.51
	(0.29)	(0.47)	(0.29)	(0.46)
Training × post-intervention	–0.02	0.66	0.00	0.67
	(0.36)	(0.51)	(0.36)	(0.51)
Observations	4429	4429	4429	4429
Overall R^2	0.0001	0.0006	0.0146	0.0153

Standard errors in parentheses; * = significant at 10%; ** = significant at 5.

Table 8 Monthly Hours of Sick Leave and SF-36 Pain Scores
(Health data taken from survey months: March 2001, April 2001, July 2001, December 2001, and May 2002.)

	Fixed effects	**Random effects**
Constant	7.08*	8.38**
	(3.35)	(3.78)
Female	—	0.40
		(0.64)
Age	—	–0.03
		(0.04)
Tenure	—	–0.00
		(0.04)
Disabled	—	1.40
		(0.91)
Education	—	–0.01
		(0.20)
Collector	1.87	0.85
	(3.68)	(0.57)
Level	0.22	–0.18
	(0.96)	(0.32)
SF-36 pain score	–0.04**	–0.03**
	(0.02)	(0.01)
Observations	855	855
Overall R^2	0.0165	0.0241

Standard errors in parentheses; * = significant at 10%; ** = significant at 5%.

Table 9 Percentage Increase in Production, Chair and Training Intervention

	Health Effects	Total Effects
A. Change in production per day per change in SF-36 pain score (Table 6, fixed effects)	$19.14	—
B. Change in pain score per intervention (Table 5, fixed effects)	6.23	—
C. Average total benefit per day (A × B)	$119.24	$ 353.11
D. Predicted average daily production, pre-intervention	$1,993.98	$1,993.98
E. Percentage increase in production (C/D)	6.0%	17.7%

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Endnotes

[1] While the analysis here focused on changes in monthly pain levels, Amick et al., forthcoming, analyzed the effect of these two interventions on the daily growth in pain scores. The pain scores used in both papers are distinct and come from different sets of questions administered during the study. The pain score used here is a monthly average, while the pain score used by Amick et al., forthcoming, is tabulated three times a day for five days a week during each of the survey months.

[2] A few support staff did participate in the study. While these individuals contributed to the analysis of health, they were excluded from the productivity analysis because there was no data on their production.

[3] Boden and Galizzi (1999) show that workers' post-injury wages are depressed relative to baseline after suffering a MSD that causes them to miss time from work. While this study is often cited as a source of productivity loss, on-the-job productivity losses due to chronic pain are almost never calculated.