The effect of Employee Assistance Programs use on healthcare utilization

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Abstract:

Objective. To estimate the effect of Employee Assistance Program (EAP) use on healthcare utilization as measured by health claims.

Data Sources. A unique data set that combines individual-level information on EAP utilization, demographic information, and health insurance claims from 1991 to 1995 for all employees of a large midwestern employer.

Study Design. Using "fixed-effect" econometric models that control for unobserved differences between individuals' propensities to use healthcare resources and the EAP, we perform our analyses in two steps. First, for those employees who visited the EAP, we test whether post-EAP claims differ from pre-EAP claims. Second, we combine claims data of individuals who went to an EAP with those of individuals who did not use an EAP to test whether differences in utilization exist between EAP users and nonusers.

Data Collection Methods. From the EAP we obtained the date of first EAP contact for all employees who used the service, and from the company's human resources department we obtained limited demographic data on all employees. We obtained healthcare utilization claims data on all employees and their dependents from the company's two healthcare plans: a fee-for-service (FFS) plan and a health maintenance organization (HMO) plan.

Principal Findings. We found that going to an EAP substantially increases both the probability of an alcohol, drug abuse, or mental health (ADM) claim and the number of ADM claims in the same quarter as EAP contact. The increased probability of an ADM claim persists for approximately 11 quarters after the initial contact, while the increased ADM charges persist for approximately six quarters after the initial EAP contact.

Conclusions. Our results strongly suggest that the EAP is able to identify behavioral and other health problems that may affect workplace performance and prompt EAP users to access ADM and other healthcare. Consistent with the stated goals of many EAPs, including the one examined in this study, this process should improve individuals' health, family functioning, and workplace performance.

JEL Classification Code. I11

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Article:

Employee assistance programs (EAPs) are employer-sponsored programs intended to help workers who have substance abuse or other personal problems that may affect their job performance. Comprehensive EAPs engage in identification, assessment, motivation, referral, short-term counseling, monitoring, and follow-up activities (Roman and Blum 1985, 1988) and help with a variety of personal problems including family, emotional, financial, legal, and substance abuse concerns (Blum and Roman 1989). EAPs do not engage in long term treatment, nor do they provide healthcare services. Employees may refer themselves to an EAP or may be formally referred by supervisors. Self-referral to an EAP can reflect a purely private motivation to seek help or the informal but powerful urging of supervisors, coworkers, family members, or friends. Formal supervisor referrals consist of two types: referral arising from job performance problems and referral resulting from questionable or dangerous employee behavior in the workplace, often requiring the immediate attention of human resources.

EAPs have emerged as a popular means of addressing employee problems that have the potential to cause significant workplace consequences. Hartwell, Steele, French, et al. (1996) estimate that in 1993, 55 percent of all employees in U.S. private worksites with 50 or more employees were eligible to use EAP services. Hartwell, Steele, French, et al. (1995) estimated that this proportion increased to 67 percent by 1995.

Despite the common perception that EAPs lower health insurance costs for alcohol, drug abuse, and mental health treatment (McClellan 1990), few studies have rigorously evaluated the effect of EAP use on subsequent outcomes such as healthcare utilization (French, Zarkin, and Bray 1995; National Research Council/Institute of Medicine [NRC/IOM] 1994). Two widely cited studies of EAP effectiveness--McDonnell Douglas Corp. and Alexander and Alexander Group Consulting (1990), and Control Data Corporation [CDC] (1990)--have estimated the effect of EAP use on absenteeism and medical claims (the CDC study also looked at performance reviews and salary increases); but both studies have methodological weaknesses that limit the validity and reliability of their results (French, Zarkin, and Bray 1995). Other, more rigorous studies have either failed to find healthcare savings resulting from EAPs (e.g., McClellan 1990) or have not examined the effect of EAPs on healthcare costs (e.g., Ahn and Karris 1989). As noted in a 19 94 NRC/IOM report, "there is as yet no definitive study of the impact of EAP participation on employee work performance, absenteeism, or health claims" (p. 249).

This article estimates the effect of EAP use on healthcare utilization as measured by health claims. Because relatively little work has related EAP use to subsequent healthcare utilization, we draw on a closely related field, the alcoholism treatment literature, to aid in specifying the model and interpreting the results (e.g., Holder and Blose 1991, 1992; Holder and Cunningham 1992; Holder, Lennox, and Blose 1992). In addition, we draw on the program evaluation work of Heckman and Hotz (1989) and Heckman et al. (1998) to control for employees' self-selection into the EAP.

Based on the alcoholism treatment literature, we expect that individuals may experience one of two alternative healthcare utilization scenarios prior to going to an EAP. On the one hand, individuals may experience a time of increasing healthcare utilization just prior to going to an EAP. This increase in utilization just before an intervention takes place has been called the "pre-intervention ramp effect" by Holder, Lennox, and Blose (1992), and it reflects a period of time in which an individual both becomes progressively more debilitated and seeks healthcare through his or her healthcare provider. On the other hand, individuals may not experience a ramp effect before they consult an EAP either because their condition is stable and they do not require healthcare or because barriers to access exist that are sufficient to keep them from seeking care. These individuals, however, may experience an increase in the demand for healthcare that coincides with going to an EAP.

EAP participation may enable individuals who experience a pre-EAP ramp effect to address and resolve their problems within the EAP with minimal need for additional psychological or other healthcare services provided outside the EAP. In this case, healthcare utilization may decrease for these individuals relative to pre-EAIP levels. This decrease in healthcare utilization is the "post-EAP ramp effect," following Holder, Lennox, and Blose's (1992) use of the term in the context of alcoholism treatment. Based on the previous EAP literature, the predominant view is that EAP participation is likely to lead to this type of reduced healthcare utilization.

However, for individuals who are not experiencing a pre-EAP ramp effect, EAP participation may identify the need for additional psychological and other healthcare services outside the EAP; going to an EAP may reduce barriers to access for this additional care. Contrary to conventional wisdom, both effects will lead to a temporary increase in healthcare utilization, relative to pre-EAP utilization levels, that coincides with the initial EAP visit.

To estimate the effect of EAP use on healthcare utilization, we use a unique data set that combines individual-level information on EAP utilization, demographic information, and health insurance claims from 1991 to 1995 for a single employer. In contrast to the alcoholism treatment literature that analyzes average claims per period (Holder, Lennox, and Blose 1992), we focus on modeling individual-level claims. Taking advantage of individual-level data, we estimate fixed-effect econometric models that use only the variation in healthcare utilization within individuals over time. These models control for unobserved correlation that might exist between individuals in the propensity to use healthcare resources and in the timing of EAP use. Failure to correct for this correlation, if it exists, will bias our estimated coefficients. As we discuss further on, important differences appear in the results between the fixed-effect and standard regression models, which suggests that this correlation is empirically impor tant. This fixed-effect method using individual-level claims data represents a significant methodological advance over the aggregate time-series approaches commonly used in the alcohol treatment literature (for a review of this literature see Holder, Lennox, and Blose 1992).

We perform our fixed-effect analyses in two steps. First, we concentrate on the sample of the individuals who have gone to an EAP. Using this sample, we test whether or not healthcare claims (both total claims and alcohol, drug, or mental health [ADM]-only claims) increase with EAP participation. Second, we combine claims data from individuals who went to an EALP with those of persons who did not use an EAP to test whether differences in utilization exist between

EAP users and nonusers. This model allows us to assess whether the claims of individuals who seek help from an EAP are significantly different from those who do not go to an EAP.

METHODS

Data

The data used in this article were collected as part of a study funded by the National Institute on Alcohol Abuse and Alcoholism (NIAAA) to assess the effectiveness of specialized EAP services offered by a large midwestern employer. The employer offers EAP services to employees, retirees, and their family members. Marital and family issues, emotional problems, stress, substance abuse, financial and legal problems, and other job-related problems that may affect work performance are among the personal problems addressed by the EAP.

The EAP is committed to managing benefits costs by monitoring the services provided, evaluating outcomes of care, and critically selecting service providers. However, between 1991 and 1995--the years of our study--the EAP did not have a formal role in any of the organization's health insurance plans, and employees were not required to visit the EAP in order to gain access to ADM care. Based on reports provided by the EAP staff, 60 percent of EAP clients in 1991 referred themselves to the program, 22 percent of clients were referred by a supervisor or manager, 12 percent were referred by coworkers, and 5 percent were referred by the human resources department. By comparison, 49 percent of EAP client referrals nationwide are self/family referrals, 27 percent are informal/formal supervisor referrals, and 13.8 percent are peer referrals (Blum and Roman 1992). Thus, our study EAP's referral rates are similar to national averages.

Our study EAP generally provides up to six sessions of internal short-term counseling designed to assess the client's problems and help the client create a plan of action to address those problems. The EAP prefers not to provide short-term treatment of clients after the diagnosis has been made. Instead, the EAP believes that the client is best served by the EAP's accurately assessing the problem(s), linking the client with appropriate resources, and monitoring the client's progress. Thus, the EAP refers the majority of clients to external treatment providers.

As part of this study, we obtained the date of first EAP contact for all employees who used the EAP. We obtained limited demographic data on all employees from the company's human resources department. These data included birth date; hire date; termination date, if applicable; gender; and marital status as indicated on the employee's W2 form.

We also obtained healthcare utilization claims data on all employees and their dependents from the company's two healthcare plans, a fee-for-service plan and a health maintenance organization plan. Most employees were covered by the fee-for-service plan, but membership in the health maintenance organization plan increased steadily over our study period. We obtained data on all healthcare claims filed by employees or their dependents between January 1, 1991, and December 31, 1995. The data include the healthcare charge, service date, ICD-9 diagnosis codes, and dependent relationship. All charges were adjusted to 1995 dollars using the medical care index of the Consumer Price Index (Bureau of Labor Statistics 1998).

In our analysis we are primarily concerned with the healthcare utilization of employees at our study site. We have excluded all claims for dependents, but we have used dependent claims to identify the health insurance coverage status of employees. Thus, our analysis sample consists of all employees who had at least one claim, either for themselves or for a dependent, on either plan from January 1, 1991 through December 31, 1995.

We aggregated the charges from individual claims into quarterly charges based on the date of service. Employees may show no observed charges in a quarter either because they were not covered by one of the two insurance plans during that time or because they were covered but had no claims activity. These two possibilities have different implications for our analysis. If an employee was not covered by either health plan in a given quarter, then that employee's health claim should be included as a missing value for that quarter. If an employee was covered but chose not to use healthcare services, then that employee's charges should equal zero for that quarter. Unfortunately, the claims data did not include plan enrollment dates, so we were unable to distinguish between the two possibilities. To solve this problem, we assumed continuous coverage from either the date of hire or the date of the first observed claim, whichever came first, to the later of two dates, the date of termination or that of the last observ ed claim. Because a claim by a dependent (e.g., a spouse) would indicate the employee's enrollment, we included dependents' claims when identifying the first and last observed claim. We did not, however, include dependents' claims when we calculated healthcare charges for the employee. Finally, we set charges equal to zero in any covered quarter during which we did not observe a claim. This process allowed us to create a complete claims history for all employees in our analysis file.

One concern with the coverage assumptions just described lies in the implied conclusion that coverage begins or ends with employment. Our thinking here may be invalid for a number of reasons (e.g., COBRA continuation of coverage). To examine the sensitivity of our results to this assumption, we also created an analysis file in which we assumed continuous coverage from the first observed claim to the last observed claim. We found that our results were not sensitive to our coverage assumptions.

The four dependent variables in our analyses each represent an indicator variable for (1) whether or not any healthcare use occurred in a quarter, (2) whether or not any ADM utilization occurred in a quarter, (3) the natural logarithm of total charges that were billed within a quarter, and (4) the natural logarithm of ADM charges for the quarter. ICD-9 diagnosis codes were used to distinguish ADM charges from other charges, according to the criteria described in Larson et al. (1997). Using data from the human resources department, we created indicator variables for an employee's sex and marital status and a variable measuring the employee's age.

Because previous research has shown significant differences in healthcare utilization between individuals in FFS plans and those in HMOs (e.g., Goldman 1995), we controlled for the type of health plan. Because we did not observe the actual date on which coverage in one plan ended and coverage in another began, we assumed that HMO enrollment began in the quarter with the first observed HMO claim and ended in the quarter with the last observed HMO claim. If we observed both an FFS and an HMO claim in a single quarter, we assigned the quarter as an FFS quarter. Based on this algorithm, we created an indicator variable equal to one if the employee was enrolled in the HMO in that quarter and zero otherwise.

To assess the sensitivity of our results to this algorithm, we also conducted our analyses under the assumption that HMO coverage began immediately after the last observed FFS claim and that it ended immediately before the first observed FFS claim. Quarters with both FFS and HMO claims were assigned as HMO quarters. Under either algorithm our results were essentially the same, so we report only those results based on the first algorithm.

Finally, we created a set of indicator variables to measure the potential pre- or post-EAP ramp effects. We created an indicator variable for each quarter prior to EAP contact and an indicator variable for each quarter after EAP contact. We also created an indicator variable for the quarter in which the individual first visited the EAR Employees who did not visit the EAP were assigned zeros for all of these indicator variables. To control for possible year-specific effects, we created an indicator variable for each calendar year.

Table 1 provides descriptive statistics of the variables used in our analysis. The first column refers to the sample of 488 individuals who went to the EAP at some point between 1991 and 1995 (the "EAP only" sample); this consists of 7,963 person-quarters of data. Column 2 refers to those individuals in our data set who did not go to the EAP (n = 2,882), or 41,171 person-quarters of data. The data in each sample were drawn approximately proportionately from each year and had about the same overall HMO utilization rate, 18 percent. Before they visited the EAP, the EAP sample had approximately the same percentage of quarters with positive charges and approximately the same level of total charges and ln(total charges) as the non-EAP sample. After they visited the EAP, the EAP sample was significantly more likely to have a positive claim and had significantly higher In (total charges) than the non-EAP sample. A similar pattern holds for ADM claims activity.

Table 1: Sample Characteristics

Variable	*EAP Only (s.d.)* *(N=488, N•T=7,963)*	*Non-EAP (s.d.)* *(N=2,882, N•T=41,171)*
Employee Characteristics		
Percent male	10.7% (30.9%)	14.6% (35.3%)
Percent single	49.0% (50.0%)	47.1% (49.9%)
Mean age	36.9 (8.9)	37.4 (11.1)
Percent who visited the EAP	100.0%	0.0% (0.0%)
Time Series Characteristics		
Quarters occurring in 1991	18.1% (38.5%)	18.5% (38.9%)
Quarters occurring in 1992	20.9% (40.7%)	19.6% (39.7%)
Quarters occurring in 1993	21.2% (40.9%)	20.3% (40.2%)
Quarters occurring in 1994	20.9% (40.7%)	21.0% (40.7%)
Quarters occurring in 1995	18.8% (39.1%)	20.6% (40.5%)
HMO quarters	18.0% (38.4%)	17.5% (38.0%)
Total Claims Activity		
Quarters with positive charges		
All quarters	57.2%*** (49.5%)	46.3% (49.9%)
Pre-EAP quarters	48.5% (50.0%)	
Post-EAP quarters	63.6%*** (48.1%)	
Mean charge for charges > 0		
All quarters	$1,971.56 ($6,902.57)	$1,962.55 ($8,386.63)
Pre-EAP quarters	$1,900.26 ($7,480.39)	
Post-EAP quarters	$2,010.84 ($6,563.58)	

continued

Table 1: Continued

Variable	EAP Only (s.d.) (N=488, N•T=7,963)	Non-EAP (s.d.) (N=2,882, N•T=41,171)
Mean log of charges for charges > 0		
All quarters	6.30*** (1.51)	6.10 (1.56)
Pre-EAP quarters	6.21* (1.48)	
Post-EAP quarters	6.35*** (1.52)	
ADM Claims Activity		
Quarters with positive ADM charges		
All quarters	19.7%*** (39.8%)	6.5% (24.7%)
Pre-EAP quarters	9.8%** (29.8%)	
Post-EAP quarters	26.9%*** (44.4%)	
Mean ADM charge for charges > 0		
All quarters	$1,372.49 ($3,992.94)	$1,618.17 ($6,452.65)
Pre-EAP quarters	$1,075.78* ($3,120.17)	
Post-EAP quarters	$1,450.73 ($4,190.49)	
Mean log of ADM charges for charges > 0		
All quarters	6.16* (1.37)	6.01 (1.49)
Pre-EAP quarters	5.80 (1.46)	
Post-EAP quarters	6.25*** (1.34)	

*Significantly different from Non-EAP at the .10 level; **significantly different from Non-EAP at the .05 level; ***significantly different from Non-EAP at the .01 level.

Specification and Estimation Issues

Because almost 50 percent of our observations on total healthcare utilization and more than 80 percent of our observations on ADM utilization are zeros, we cannot estimate simple ordinary least squares models of healthcare charges. Instead, we estimate two-part models in which the decision to file a claim is modeled in the first part using logit models, and the natural logarithm of charges conditional on having filed a claim is modeled in the second part (see Manning, Newhouse, Duan, et al. 1987).

Although the two-part model is a common technique for modeling healthcare utilization (e.g., Duan et al. 1983; Manning, Newhouse, Duan, et al. 1987), other techniques for modeling panel data with a large proportion of zeros exist (Jones 1998; Goldman 1995; Nijman and Verbeek 1992; Grootendorst 1997). These alternative techniques require some form of identifying assumptions, through distributional assumptions, functional form assumptions, or restrictions on the coefficients. For example, the panel data Tobit suggested by Grootendorst assumes that the error term is normally distributed, that the correlation between the two equations is one, and that the two equations have the same coefficient vector. Because we were unwilling to make the stringent assumptions required for these alternative techniques, we decided to use the two-part model.

Another important consideration is the potential for selection bias. The key question we are attempting to answer is this: What is the mean effect of EAP participation on healthcare utilization for those who went to the EAP? To find the answer we need to compare the mean claims of those who went to the EAP to the mean claims of the same group of individuals if they had not gone to the EAP (see Heckman et al. 1998; and Heckman and Hotz 1989, in the context of estimating the effect of training on wages). If EAP participation were randomly assigned, we could simply compare the mean claims of those who participated in the EAP to the claims of those who did not. However, EAP participation is not randomly determined among employees; we expect that individuals who go to the EAP are more likely to require healthcare services than typical employees. This suggests the potential for correlation between the EAP participation variables and the error term in both the logit and ln(charges) specifications. This potential co rrelation is the source of selection bias, and failure to account for it may bias our parameter estimates.

Heckman and Hotz (1989) noted that several estimators are available when selection depends on unobservables, as we hypothesize in the context of our study. One estimator is the fixed-effect estimator, which is motivated by a model in which the error term is composed of a person-specific, time-invariant component or a fixed effect plus a random component that is independent of all other variables. In this model, the person-specific fixed effect is presumed to be the source of the selection (i.e., it accounts for the correlation between the propensity to use the EAP and both the likelihood and level of healthcare claims). In the results section further on, we report on a test described by Heckman and Hotz (1989) that examines whether or not the fixed-effect model is a reasonable solution to the selection bias problem.

We estimate the decision to file a claim using a fixed-effect, or conditional logit, specification (McFadden 1973; Greene 1990; StataCorp 1997), and we estimate the natural logarithm of charges using fixed-effect linear regression (Hsiao 1988; StataCorp 1997). Because the fixed effect captures all time-invariant factors for a given individual, the demographic variables in Table 1 are perfectly collinear with the fixed effect and cannot be included in our models. Similarly, because age always moves in increments of one from one year to the next within an individual, it becomes collinear with the four calendar-year indicator variables and cannot be included in our models.

Our specifications for both the logit and the linear models are of the form:

$$\begin{aligned} Y_{jt} = f\,[&b_0 + b_1 \times \textit{Pre-EAP}(Q1\text{–}Q8)_j + b_2 \times \textit{EAP}_j \\ &+ b_3 \times \textit{Post-EAP}(Q1\text{–}Q12)_j + b_4 \times \textit{HMO}_{jt} \\ &+ b_5 \times \textit{Year Indicators} + v_j + e_{jt}] \end{aligned} \qquad (1)$$

where Y_{jt} represents either a 0/1 indicator variable (charge/no charge) or the natural logarithm of charges, conditional on a positive charge, and *f* […] is either the logit transformation or a linear function. The b's represent parameters associated with each of the regressors. Pre-EAP $(Q1\text{-}Q8)_j$ represents eight dummy variables that are equal to one in each of the eight quarters prior to EAP utilization (to capture any pre-EAP ramp effect); EAP_j is a dummy variable equal to one in the quarter in which the individual first uses the EAP; Post -EAP$(Q1\text{-}Q12)_j$ is a vector of 12 dummy variables that are equal to one in each of the 12 quarters post-EAP utilization.[1] The specification allows a quarter-specific, pre- and post-EAP ramp effect. This specification is more general than simply assuming a linear time trend pre- or post-EAP. The HMO variable is an indicator variable that is one if the individual is covered by an HMO in that quarter and zero otherwise. The year indicators are year-specific indicator variables that capture changes in the structure of health insurance benefits or other changes that may affect healthcare utilization; v_j is a person-specific fixed effect; and e_{jt} is a mean zero iid error term.

We conducted a specification search to determine the most parsimonious set of pre-EAP and post-EAP indicator variables that adequately described the effects of the EAP. Starting with the eighth quarter pre- and the twelfth quarter post-EAP utilization, we tested for the joint significance of groups of quarterly indicators, four quarters at a time. If we failed to reject the hypothesis that the quarterly effects were jointly equal to zero, we set the coefficients to zero. For both samples, this process resulted in a logit specification that included four pre-EAP dummies and twelve post-EAP dummies and a log-linear specification that included four pre-EAP dummies and eight post-EAP dummies.

We also conducted additional specification tests (available upon request). These included a test of fixed versus random effects and a test for attrition bias (Verbeek and Nijman 1992). We strongly rejected the random-effect model in favor of the fixed-effect model. We also concluded that attrition bias is not a significant factor in our models.

Because individuals are not randomly assigned to EAP participation, we require nonexperimental comparison groups. In this article we have two nonexperimental comparison groups: those employees who went to the EAP and those who did not. In the first case (reported in Table 2, in the Results section), we estimate Equation 1 using the EAP-only sample. This is equivalent to using the reference time periods--those quarters before the pre-EAP indicator variables and after the post-EAP indicator variables--to serve as a proxy for the healthcare claims of individuals if they had not gone to the EAP. These healthcare claims are arguably representative of the "steady-state" level of healthcare claims.

In the second case, we also estimate Equation 1 with a combined EAP and non-EAP sample (reported in Table 3, under Results). In this case, individuals who did not use the EAP are the comparison group.[2] As we note in the next section, our results are very similar across the two comparison groups.

RESULTS

Figure 1 represents the plotted mean natural logarithm of ADM charges plus one dollar (ln[*ADM charges plus $1*]) (the dollar was added to charges to avoid dropping those observations that had zero-dollar claims in a quarter) per quarter for 12 quarters before and after the first contact date with the EAP. Figure 2 shows a similar plot of the mean natural logarithm of total charges plus one dollar. Note the increase in claims that occurs coincident with t = 0, the time of first EAP contact, for both ln (*ADM charges plus $1*) and ln(*total charges plus $1*). We see that (ln[*ADM charges plus $1*]) experiences a much larger percentage increase at the time of EAP contact than does ln (*total charges plus $1*). We also see that ln(*ADM charges plus $1*) decreases over the post-EAP quarters without falling back to the mean pre-EAP levels, while ln(*total charges plus $1*) drops slightly in the third quarter post-EAP and then increases again.

Figure 1: The Mean Natural Logarithm of Alcohol Drug or Mental Health Charges plus One Dollar

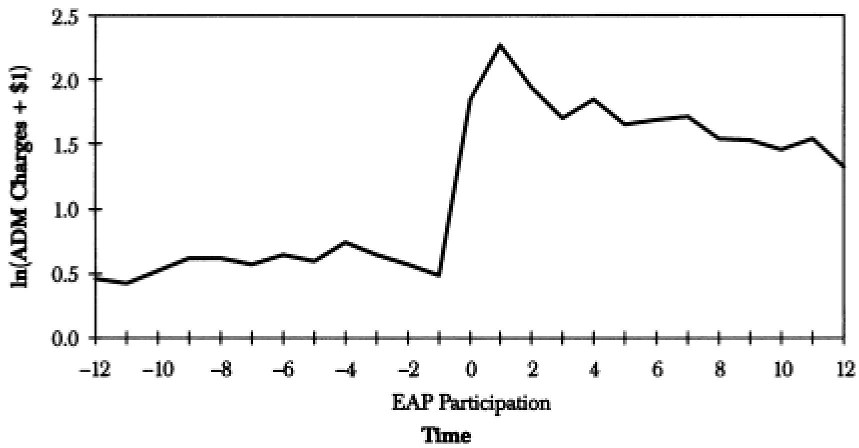

Figure 2: The Mean Natural Logarithm of Total Healthcare Charges plus One Dollar

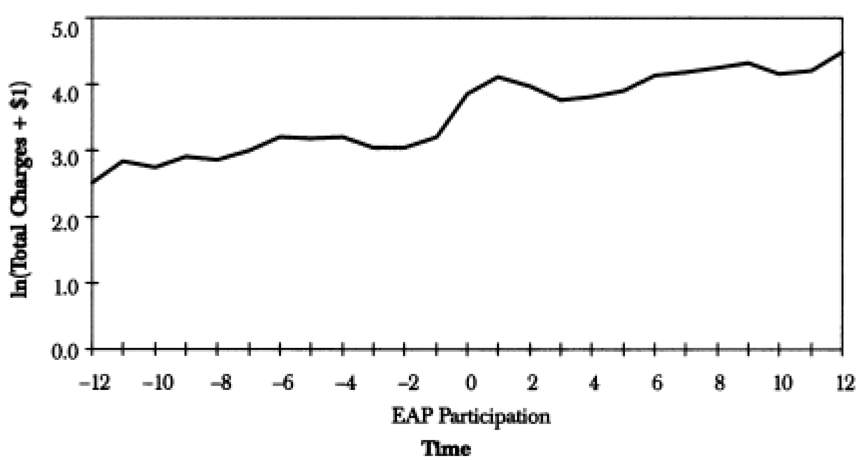

Table 2 presents results of the two-part models for ADM utilization and total utilization for the EAP-only group. As discussed earlier, each model includes a person-level fixed effect. Unlike the findings for alcoholism treatment, we find no evidence here that healthcare utilization increased prior to EAP access (the pre-EAP ramp effect). Our results show no significant increase in healthcare utilization, either ADM only or total utilization, prior to EAP access. However, the probability of an ADM visit is lower one period prior to contacting an EAP.

In contrast, both total and ADM healthcare utilization increase substantially in the quarter during which the employee first consults with the EAP. The probability of an ADM visit increases substantially. Looking at the odds ratio, individuals who went to the EAP were 5.9 times more likely to have a positive health claim in the quarter during which they first consulted the EAP than in the reference time periods; ln(*ADM charges*) are approximately 150 percent[3] higher in the quarter of first EAP contact The odds ratio of an ADM visit increases even more one quarter post-EAP (10.0 times), and the increased odds ratios of an ADM visit continue for approximately three years. ln(*ADM charges*) post-EAIP remain significantly higher for approximately six quarters, but the magnitude of the differential decreases steadily. The total utilization results, which include ADM visits, change in parallel with the ADM results, but the magnitude of the results is smaller.

The HMO variable is insignificant in the ADM conditional logit specification, but it is negatively related to ln(*ADM charges*). A similar result holds for the ln(*total charges*) regression (column 4), but HMO has a surprisingly positive and significant relationship with the probability of any utilization (column 3). Note, however, that our parameters of interest--the coefficients on the pre- and post-EAP variables--are robust to the inclusion or deletion of the HMO variable from the specifications.

In further analyses not shown (but available upon request), we compared the conditional logit and fixed-effect results to simple logit and ordinary least squares (OLS) specifications of the ADM models that do not control for individual fixed effects. Although the qualitative pattern of significant coefficients was similar in both, the magnitudes of the coefficients were very different. For ADM utilization, the magnitude of the simple logit coefficients was smaller than the conditional logit coefficients from pre-EAP through five quarters post-EAP, but from six quarters post-EAP the conditional logit coefficients were smaller than the simple logit results. The estimated odds ratio of having a positive health claim one quarter after an EAP visit was 4.8 with the simple logit specification and 10.0 with the conditional logit specification. For the ln(*ADM charges*) results, the fixed-effect results were larger in magnitude than the comparable non-fixed-effect results. Although the timing was slightly different, a similar pattern held for the total utilization specifications. These results suggest that the simple logit and OLS results are affected by a correlation between individual fixed effects and the parameter of interest.

Table 2: Results for ADM and Total Healthcare Utilization of the EAP-Only Sample

	ADM Utilization (s.d.)		*Total Utilization (s.d.)*	
	Conditional Logit	*ln(*ADM Charges*)*	*Conditional Logit*	*ln(*Total Charges*)*
Intercept†	– (–)	5.879*** (0.089)	– (–)	6.331*** (0.056)
4th quarter before EAP visit	0.125 (0.230)	−0.014 (0.199)	0.023 (0.157)	0.106 (0.113)
3rd quarter before EAP visit	−0.169 (0.237)	0.267 (0.210)	−0.106 (0.150)	0.037 (0.111)
2nd quarter before EAP visit	−0.430* (0.231)	0.094 (0.198)	−0.072 (0.142)	0.120 (0.103)
1st quarter before EAP visit	−0.768*** (0.237)	0.065 (0.210)	0.215 (0.136)	0.005 (0.098)
Quarter of first EAP visit	1.767*** (0.169)	0.911*** (0.131)	0.644*** (0.135)	0.218** (0.089)
1st quarter after EAP visit	2.305*** (0.173)	0.803*** (0.122)	0.799*** (0.141)	0.331*** (0.089)
2nd quarter after EAP visit	1.751*** (0.178)	0.696*** (0.129)	0.693*** (0.147)	0.162* (0.093)
3rd quarter after EAP visit	1.360*** (0.185)	0.439*** (0.134)	0.399*** (0.150)	0.076 (0.096)
4th quarter after EAP visit	1.472*** (0.192)	0.241* (0.136)	0.425*** (0.159)	−0.053 (0.100)
5th quarter after EAP visit	1.012*** (0.202)	0.368** (0.144)	0.299* (0.165)	0.037 (0.104)
6th quarter after EAP visit	0.969*** (0.208)	0.256* (0.148)	0.384** (0.172)	−0.012 (0.105)
7th quarter after EAP visit	0.989*** (0.214)	0.093 (0.147)	0.389** (0.178)	−0.032 (0.107)
8th quarter after EAP visit	0.723*** (0.224)	−0.188 (0.154)	0.319* (0.185)	−0.018 (0.111)
9th quarter after EAP visit	0.608*** (0.233)	– (–)	0.286 (0.194)	– (–)
10th quarter after EAP visit	0.516** (0.243)	– (–)	0.230 (0.201)	– (–)
11th quarter after EAP visit	0.646** (0.252)	– (–)	0.124 (0.208)	– (–)
12th quarter after EAP visit	0.377 (0.278)	– (–)	0.375* (0.227)	– (–)

continued

Table 2: Continued

	ADM Utilization (s.d.)		Total Utilization (s.d.)	
	Conditional Logit	ln(ADM Charges)	Conditional Logit	ln(Total Charges)
Quarter occurred in 1991	−0.990***	−0.312***	−0.789***	−0.223***
	(0.148)	(0.115)	(0.109)	(0.072)
Quarter occurred in 1992	−0.480***	−0.265***	−0.596***	−0.131**
	(0.125)	(0.094)	(0.096)	(0.064)
Quarter occurred in 1994	−0.241**	−0.492***	−0.372***	0.280***
	(0.120)	(0.091)	(0.095)	(0.062)
Quarter occurred in 1995	−0.534***	0.117	−0.693***	−0.034
	(0.139)	(0.113)	(0.107)	(0.072)
HMO	−4.20E-04	−0.671***	1.770***	−0.445***
	(0.173)	(0.248)	(0.119)	(0.094)

*Significant at the .10 level; **significant at the .05 level; ***significant at the .01 level.

†In fixed-effects models the intercept cannot be uniquely identified from the person-specific components (fixed effects). StataCorp does not report an intercept for conditional logit, but in the fixed-effects regression model, StataCorp reports the average of the person-specific components as the intercept.

Table 3 presents the results of combining the EAP and the non-EAP samples. These results allow us to examine whether significant differences exist between the healthcare utilization of individuals who went to the EAP and those who did not. As in Table 2, the pre- and post-EAP variables are defined only for individuals who went to the EAP. Individuals who did not use the EAP are the excluded group in Table 3.[4]

As was the case with the EAP-only sample, no evidence exists of a positive ramp effect prior to EAP access; as with the results discussed earlier, evidence exists of a decrease in the probability of ADM utilization prior to contacting the EAP. Similarly, the post-EAP results are qualitatively similar to the results in Table 2 for both the ADM and total utilization models. The magnitude of the parameter estimates is larger in Table 3, with the biggest differences occurring in the later quarters post-EAP for ADM utilization. Importantly, the results show that the healthcare utilization of those who went to the EAP, as measured by either the discrete or the continuous measures, remained at or above the level of those who did not go to the EAP.

Table 3: Results for ADM and Total Healthcare Utilization for the Combined EAP and Non-EAP Samples

	ADM Utilization (s.d.)		Total Utilization (s.d.)	
	Conditional Logit	*ln(*ADM Charges*)*	*Conditional Logit*	*ln(*Total Charges*)*
Intercept†	–	5.904***	–	6.179***
	(–)	(0.073)	(–)	(0.026)
4th quarter before EAP visit‡	–	–	–	–
	(–)	(–)	(–)	(–)
3rd quarter before EAP visit	−0.282	0.261	−0.139	−0.071
	(0.302)	(0.269)	(0.201)	(0.151)
2nd quarter before EAP visit	−0.507*	0.105	−0.106	0.028
	(0.300)	(0.263)	(0.196)	(0.148)
1st quarter before EAP visit	−0.809***	0.050	0.191	−0.079
	(0.307)	(0.274)	(0.193)	(0.144)
Quarter of first EAP visit	1.800***	0.933***	0.643***	0.145
	(0.258)	(0.225)	(0.193)	(0.138)
1st quarter after EAP visit	2.404***	0.876***	0.822***	0.281**
	(0.263)	(0.222)	(0.197)	(0.140)
2nd quarter after EAP visit	1.871***	0.805***	0.734***	0.122
	(0.265)	(0.228)	(0.200)	(0.143)
3rd quarter after EAP visit	1.521***	0.587**	0.432**	0.035
	(0.270)	(0.233)	(0.202)	(0.146)
4th quarter after EAP visit	1.659***	0.427*	0.456**	−0.104
	(0.274)	(0.234)	(0.208)	(0.149)
5th quarter after EAP visit	1.209***	0.546**	0.310	−0.003
	(0.281)	(0.242)	(0.213)	(0.154)
6th quarter after EAP visit	1.192***	0.519**	0.420*	−0.052
	(0.285)	(0.247)	(0.218)	(0.155)
7th quarter after EAP visit	1.246***	0.452*	0.420*	−0.033
	(0.290)	(0.248)	(0.223)	(0.157)
8th quarter after EAP visit	1.000***	0.262	0.320	−0.003
	(0.296)	(0.259)	(0.227)	(0.161)
9th quarter after EAP visit	0.884***	–	0.253	–
	(0.303)	(–)	(0.235)	(–)
10th quarter after EAP visit	0.803**	–	0.188	–
	(0.312)	(–)	(0.241)	(–)
11th quarter after EAP visit	0.958***	–	0.087	–
	(0.321)	(–)	(0.248)	(–)
12th quarter after EAP visit	0.691**	–	0.333	–
	(0.343)	(–)	(0.265)	(–)

continued

Table 3: Continued

	ADM Utilization (s.d.)		Total Utilization (s.d.)	
	Conditional Logit	ln(ADM Charges)	Conditional Logit	ln(Total Charges)
Quarter occurred in 1991	−0.535*** (0.080)	0.171** (0.074)	−0.421*** (0.042)	−0.053 (0.033)
Quarter occurred in 1992	−0.321*** (0.073)	−0.054 (0.062)	−0.463*** (0.040)	−0.097*** (0.031)
Quarter occurred in 1994	−0.230*** (0.072)	0.239*** (0.062)	−0.174*** (0.039)	0.281*** (0.030)
Quarter occurred in 1995	−0.579*** (0.084)	−0.235*** (0.078)	−0.461*** (0.043)	−0.112*** (0.035)
HMO	−0.122 (0.123)	−0.471** (0.191)	1.874*** (0.055)	−0.365*** (0.048)

*Significant at the .10 level; **significant at the .05 level; ***significant at the .01 level.

†In fixed-effects models the intercept cannot be uniquely identified from the person-specific components (fixed effects). StataCorp does not report an intercept for conditional logit, but in the fixed-effects regression model, StataCorp reports the average of the person-specific components as the intercept.

‡Due to collinearity with the fixed effect, we have omitted the fourth quarter pre-EAP indicator variable.

Finally, we performed a test, proposed by Heckman and Hotz (1989), of the assumption that the fixed effect controls for selection bias. The test is based on the premise that after controlling for sample selection, the comparison and intervention groups should be statistically equivalent prior to the intervention. To implement the test, Heckman and Hotz suggest estimating an "intervention effect" prior to the actual intervention. If selection bias is adequately controlled, the estimated "intervention effect" should be zero in the time periods prior to the intervention. If the intervention effect is not zero, then some form of selection bias is still present. We performed this test by estimating a variant of the models presented in Table 3 in which we include the EAP and non-EAP sample, but we limited the EAP sample to its pre-EAP quarters. We then tested the joint significance of the pre-EAP variables. All pre-EAP variables were jointly insignificant in all models except the total charges conditional logit in which they were significant at the .05 level. Based on this finding, we conclude that our fixed-effect models have adequately controlled for selection bias for the ADM models, but have not fit as well for the total claims models.

DISCUSSION

This article presents the first detailed evaluation of the effect of EAP use on subsequent healthcare utilization. In contrast to results found in the alcoholism treatment literature (Holder, Lennox, and Blase 1992), we did not find an increase in health claims prior to employees' using the EAP. However, our results show clearly that going to an EAP coincides with an increase in both the probability of an ADM claim and the dollar amount of ADM claims (conditional on a claim) in the same quarter as EAP contact. The increased probability of an ADM health claim persists for approximately 11 quarters after the initial EAP contact, while the increased ADM charges persist for approximately six quarters after the initial EAP contact.

We also evaluated whether those who went to an EAP had lower healthcare utilization post-EAP contact compared to the utilization levels of those who did not go to the EAR. In contrast to the findings of the alcoholism treatment literature, we did not find a cost offset associated with going to the EAP. Our findings suggest that health claims increased coincident with going to the EAP and decreased some time thereafter, but that claims did not decrease below previous levels or below the level of those who did not go to the EAP. In comparing our results with those found in the alcoholism treatment literature, we conjecture that the typical individual going to alcoholism treatment may be more debilitated than the typical individual who uses an EAP.

Although we do not have direct evidence on our conjecture, the notion that alcoholics entering treatment are more debilitated than the typical individual visiting an EAP is consistent with research on EAP processes and goals. As we noted in the introduction, EAPs provide a wide range of services, including identification, assessment, motivation, referral, short-term counseling, monitoring, and follow-up activities (Roman and Blum 1985, 1988) and help with a variety of personal problems, including family, emotional, financial, legal, and substance abuse concerns (Blum and Roman 1989). Many of these services provided by EAPs are intended to increase access to care, while many of the problems they address are not likely to be associated with healthcare utilization prior to EAP use. Furthermore, EAPs are workplace-based programs that focus on early intervention in work performance issues (Roman and Blum 1988). For this reason, the typical employed EAP client, who after all must be able to hold down a job, is likely to be less debilitated than the typical alcoholic examined in the alcoholism treatment literature, who may or may not be employed. Future research should compare and contrast the healthcare utilization of alcoholics and EAP clients.

Our study has several limitations that may affect the ability to generalize and use our results. First, our results are limited by the potential of selection bias. Individuals' EAP status was not randomly assigned. Clearly, the design would be stronger if that were the case; however, the practical problems of implementing this strategy in a community-based setting would be daunting and such a design may not be ethically defensible. To help control for the non-random assignment of individuals to an EAP, we estimated fixed-effect logit and OLS specifications. These models assume that the selection can be modeled with a person-specific fixed component that affects both the decision to use an EAP and the timing and extent of healthcare claims. As suggested by Heckman and Hotz (1989), we tested the assumptions of this model and found that we could not reject the fixed-effect model for the ADM specifications; however, future work should evaluate whether our results are robust to alternative selection models.

Second, we have a short panel duration of five years. To make the most of our data, we analyzed individual-level claims records and used panel data techniques that have not been applied in related studies in the alcohol treatment literature. Although we feel that we have made methodological improvements in the evaluation of this type of intervention, we hope that future work will draw on additional years of panel data.

Third, we analyze only one EAP, so our results cannot be generalized to other EAPs. Future work needs to evaluate the same research question with a larger number of EAPs. In doing so, researchers must recognize that two key data sources are required to duplicate our study: health claims for all individuals at a worksite and EAP records for the same worksite. Obtaining and combining these two data sources is likely to be difficult and therefore may impede future research on this topic.

Fourth, sponsors and potential sponsors of EAPs (e.g., employers and unions) must recognize that our analysis is not intended to be a complete evaluation of the benefits of EAPs. We focus solely on medical expenditures. A full evaluation of the benefits of an EAP should also include the potentially large and important improvements in individual and family well-being and the increases in workplace productivity.

In spite of these limitations, our results suggest that EAPs are able to identify psychological and other health problems that may affect workplace performance and motivate EAP users to seek access to ADM and other healthcare services. Consistent with the stated goals of many EAPs, including the one examined in this study, this process should improve individuals' health, family functioning, and workplace performance.

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NOTES

(1.) We limited our specification to eight quarters pre- and 12 quarters post-EAP because small sample sizes prevented the analysis of longer time frames.

(2.) Heckman et al. (1998) note that other methods of developing comparison groups exist. These include matching intervention and nonintervention individuals on the basis of observed variables as well as on their probability of participating in the intervention (e.g., propensity scores).

(3.) The percentage differential associated with a particular pre- or post-EAP quarter is e^{β}- 1. As noted by Manning, Newhouse, Duan, et al. (1987), this retransformation remains valid only if the error term is homoscedastic. We tested for heteroscedasticity following Davidson and MacKinnon (1993: 560-63) and StataCorp (1997: 394) by regressing the squared residuals on the squared, cubic, and quartic fitted values of the dependent variable and then testing their joint significance. Unlike a ½-sum-of-squared-residuals test statistic, the joint test of the significance of the fitted values does not assume normality of the underlying errors. The results of this testing indicated that the total utilization In(*charges*) models in Table 2 were homoscedastic, but all other models were not. To examine the effect of heteroscedasticity on our results, we computed the smearing factor suggested by Manning (1998) for all of the pre- and post-EAP coefficients. For the total utilization models in Ta ble 2, these smearing coefficients were approximately one, as expected. For the other models, these coefficients were generally less than one, ranging from 0.32 to 1.57, with a mean smearing factor of s = 0.52 for the post-EAP coefficients for ADM utilization in Table 2, *s* = 0.89 for total utilization in Table 3, and *s* = 0.49 for ADM utilization in Table 3. Although we discuss the percentage differential as being e^{β}- 1, a more conservative estimate is *s* x e^{β}- 1.

(4.) To make the non-EAP group the reference group, we dropped those quarters before the pre-EAP indicator variables and after the post-EAP indicator variables for the EAP sample. Because of collinearity with the fixed effect, we have omitted the pre-4 indicator variable.

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