Analysis of Group-Randomized Designs: A clinic-based breast cancer rescreening example

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ABSTRACT

Assigning multiple treatments/interventions to clinics or communities over time (rather than to individual subjects over time) involves contamination issues and several key reasons for considering the group-randomized designs (GRD) (MRC, 2002; Donner & Klar, 2000; and Murray, 1998): 1. The treatment/intervention to be studied is itself delivered to and affects *groups* of people rather than individuals; 2. The treatment/intervention is targeted at health professionals with the aim of studying its impact on patient outcomes; 3. The treatment/intervention is given to individuals but might affect others within that cluster (i.e., so-called contamination); 4. The target population is highly mobile so that individual follow-up would be prohibitively expensive. We utilize the SAS MIXED and GLIMMIX procedures to analyze group randomized designs. We use several illustrations, including a clinic-based breast cancer rescreening example. Rationale for the strategies and procedures used is given and limitations are discussed.

INTRODUCTION

When the investigator asks, "Can I apply treatments or interventions to individuals in the same clinic or community without bias?", the answer may not be so straightforward. In general, the answer will be that it would be very difficult if not almost impossible to guarantee uncompromised results, including contamination prevention (i.e., "leakage" of a treatment or intervention from one group to another). At the very least, provision should be made in accounting for the correlation induced by clinic/community classes.

In this section we give a brief overview of analysis of group randomized trials/designs. In the next section, we proceed using a breast cancer rescreening (BCR) data set to demonstrate analysis of these design types. Finally, we exhibit results and limitations.

UNIT OF ANALYSIS

Once the investigator identifies the unit of analysis that she posits will shed light on the study hypothesis, the design of the study experiment will revolve around collecting data on this unit of analysis. For example, in school-based studies, the research scientist wishing to study the effects of a new behavior modification method to enhance learning will wish to apply the method to whole classes within the school. The class would therefore be the unit of analysis. On the other hand, if the intent is to discern culturally-specific barriers to learning, the specific students (within the classes) would be the unit of analysis. Sometimes the distinction is not so clearly defined and, thus, the confusion and debate surrounding analysis of group-randomized designs (Murray, 1998).

Subsequently, the statistical analysis, including modeling, can be developed to correctly analyze the data based on the experimental design.

Because our particular application of group randomized trials is to binary outcome data (i.e., regular versus non-regular mammogram rescreening), we pay special attention to methods for binary outcome data as they relate to GRDs.

THEORY

Unadjusted Analysis. To better understand the process, we give the theory for normally distributed data (i.e., the outcome is continuous) not adjusting for covariates. For this, the time by condition model (after Murray, 1998) is:

$$Y_{i:jkl} = m + C_l + T_j + (TC)_{jl} + \tilde{G}_{k:l} + (\widetilde{T}\widetilde{G})_{jkl} + \tilde{e}_{i:jkl} \quad (1)$$

where $Y_{i:jk:l}$ is the measurement on the i^{th} woman nested within the k^{th} clinic/group and l^{th} condition where the left hand side (LHS) is expressed as a function of the grand mean and fixed effects of condition C, time T and time by condition interaction, TC. In addition, to account for any positive intraclass correlation (ICC), we include the random effects due to clinic/group, $\tilde{G}_{k:l}$, and the time by clinic/group interaction, $(\widetilde{T}\widetilde{G})_{k:l}$. That is, we are allowing women to be correlated within clinic group, within group across time and for random variation among the women. Failure to account for these as random effects will result in an inflated Type I error. Depending on the ICC (see discussion below), this can be substantial. Further assumptions regarding the random effects in this generalized linear mixed model are that each random effect is assumed independent and identically distributed (IID) with mean zero and constant variance, namely:

$$\tilde{G}_{k:l} \sim N(0, s^2_{g:c}) \qquad (2)$$

$$\tilde{T}\tilde{G}_{jk:l} \sim N(0, s^2_{tg:c}) \qquad (3)$$

$$\tilde{e}_{i:jk:l} \sim N(0, s^2_{e}) \qquad (4)$$

If we assume I to be the effect due to the intervention and C the effect due to the controls, then the test for the intervention effect is defined as:

$$d = (\overline{Y}_{2I} - \overline{Y}_{1I}) - (\overline{Y}_{2C} - \overline{Y}_{1C}) \qquad (5)$$

with corresponding estimate given by

$$\hat{d} = (\overline{y}_{2I} - \overline{y}_{1I}) - (\overline{y}_{2C} - \overline{y}_{1C}) \qquad (6)$$

where we test

H_0: $d = 0$ versus the two-sided alternative H_A: $d \neq 0$.

and can evaluate the hypothesis using a Student's t distribution or using the F distribution with the expected mean squares information below.

The variance of this effect is given as

$$s^2_d = 4\left(\frac{MS_{tg:c}}{mg}\right) \qquad (7)$$

where $MS_{tg:c}$ reflects the "mean square" variation of condition nested within time by clinic/group and can be defined in the traditional ANOVA expected means squared sense as

$$MS_{tg:c} = s^2_e + m s^2_{tg:c} \qquad (8)$$

with associated (t-1)c(g-1) degrees of freedom (notice we put the number of conditions, c, in the middle to reflect the nesting) and where s^2_e is equal to the "residual error" variance and $ms^2_{tg:c}$ can be interpreted to be the weighted variance component due to condition nested within time by group/clinic.

To test for the intervention effect using the traditional ANOVA approach, we would like to see the variation between the time by condition interaction be greater than the variation in (6). To do this our F statistic would be

$$F = \frac{MS_{tc}}{MS_{tg:c}} \qquad (9)$$

with numerator degrees of freedom equal to (t-1)(c-1) and denominator degrees of freedom derived as (t-1)(g-1)c and where the time by condition mean square, MS_{tc}, is defined as

$$MS_{tc} = s^2_e + ms^2_{tg:c} + mgs^2_{tc} \qquad (10)$$

with (t-1)(c-1) degrees of freedom and s^2_{tc} taken as the variance due to the intervention and the rest as above.

Link and Distributional Assumptions

In the generalized linear (mixed) model context, we can assume different distributional assumptions depending on the type of outcome (Dobson, 2001). In our case where we have a binary outcome, we will use a logit link and binomial distribution for the generalized linear model. Therefore, we have the log of the odds of regular versus non-regular rescreening so that (1) becomes:

$$\log\left(\frac{p_{i:jk:l}}{1-p_{i:jk:l}}\right) = m + C_l + T_j + (TC)_{jl} + \tilde{G}_{k:l} + (\tilde{T}\tilde{G})_{jk:l} + \tilde{e}_{i:jk:l} \quad (11)$$

Covariate (Adjusted) Analysis

The covariate analysis will adjust the means based on potential biasing or confounding factors at both the group/clinic or subject level. The only difference in Model (1) then, will be the addition of the following fixed effect term:

$$\sum_{r=1}^{x} b_r (X_{ri:jk:l} - \overline{X}_r - \ldots) \qquad (12)$$

which subtracts the observed value and sample mean for the covariate(s). This is true since we are looking at "net" differences across time rather than individual differences as in a true pre-post nested cohort study. The intervention effect in (5) is then modified by subtracting the following term:

$$\sum_{r=1}^{x} b_r \left[(\overline{X}_{r.2I} - \overline{X}_{r.1I}) - (\overline{X}_{r.2C} - \overline{X}_{r.1C}) \right] \quad (13)$$

Intraclass Correlation (ICC). To get a better sense of the correlation induced by the women within clinics, we can also construct an estimate for the intraclass correlation (ICC) coefficient as follows:

$$I\hat{C}C_{m:tg:c} = \frac{\hat{s}^{\,2}_{tg:c}}{\hat{s}^{\,2}_{tg:c} + \hat{s}^{\,2}_{e}} \qquad (14)$$

Variance Inflation Factor (VIF). Similarly, we can calculate an estimate of the variance inflation factor (VIF) coefficient as follows:

$$V\hat{I}F_{m:tg:c} = [1 + (m-1) I\hat{C}C_{m:tg:c}] \qquad (15)$$

where m is the average number of women across all clinics. In the simplest case, The VIF will give the increase in the between-group variance that is due to the ICC.

METHODS & RESULTS

Setting. We conducted this pilot study at three community clinics in California which provide Pap smears, clinical breast exams and mammograms at no cost to low-income uninsured women. We refer to the clinics as Clinic I, Clinic II, and Clinic III.

Study Design. We used a group-randomized trial design using a *retrospective chart review* to establish pre- and post-intervention rescreening rates at each site. Considering the contamination issues discussed above, each clinic was randomly assigned one of two possible intervention conditions designed to increase client rescreening to Centers for Disease Control and Prevention (CDC) compliance. These conditions were a system redesign (SRD) and a system redesign plus a tailored telephone reminder (SRDTTR). The SRD involved a range of multilevel intervention components. Clinic "usual care" practices were evaluated prior to intervention. Given the limited resources to implement computerized tracking in health care settings, we implemented a manual tickler system since manual reminders have been shown to produce similar effects in increasing screening services. Specifically, the SRD condition (Clinics I & II) implemented manual tracking, appointment scheduling, a reminder card and call, and physician and staff delivery of breast health education. The SRDTTR condition (Clinic III) was responsible for an additional five to ten minute tailored counseling call to patients. A clinic staff person was hired at each site to assist with the intervention.

This pilot study compared these two interventions and did not include a per se control condition. In the terminology presented, the SRD takes the place of the control condition.

To calculate the rescreening rate for each clinic, we used a retrospective chart review to assess whether women returned for another mammogram within the prescribed or recommended time period. We used the period 10-18 months post baseline to determine compliance for each woman.

To estimate the pre-intervention rescreening rate, we randomly selected a sample of 546 women 50 and older from Clinics I and III (SRD condition) and the universe of 74 women 50 and older at Clinic II (SRDTTR condition) who obtained either initial or follow-up mammograms from January 1, 1999 through December 31, 1999.

We then conducted a *retrospective chart review* in the period January 1, 2000 through June 30, 2001, in order to establish the pre-intervention rescreening rates at each clinic. The pre-intervention sample was not exposed to any of the intervention components.

To estimate the post-intervention rescreening rates, we randomly selected a new sample of 796 women 40 years of age and older who came in for a mammogram between September 1, 2000 and August 31, 2001 for Clinics I and III and between November 1, 2000 and October 31, 2001, for Clinic II. The intervention phase was staggered across clinics, thus the post-intervention chart review varied accordingly. The final analysis includes only women 50 years and older.

Analysis Strategy. Due to the fact this was a pilot, our measured variables were few so that to show any intervention effects were real and not due to confounding factors, we used key demographic variables to show any general differences across clinics with respect to the outcome. In addition, we derived two measures of potential external influences (physical distance to clinic and poverty index.)

The derived measure of physical distance from the client's home to the clinic was a proxy for health care access to ensure there was no differential access by women (and to control for it if there were). This was done using the Great Circle Distance Formula, which calculates the distance between the client and clinic Zip Code centroids. Next, the derived poverty index was constructed using the U. S. Census 2000 definition for each Zip Code Tabulation Area (ZCTA) for each client. This was created to control for any disparities in the general economic level of each clinic's clientele.

The group variable is composed of the two intervention conditions, namely, a system redesign (SRD) and a system redesign plus a tailored telephone reminder (SRDTTR). Other measured adjustment factors considered were: family history of breast cancer, evidence that a Pap smear and mammogram were received at the clinic on the same date, prior history of breast problems and evidence of the clinician's mammography recommendation. We first examined bivariate relationships between the outcome variable, demographic and other potential correlates at both pre- and post-intervention. All statistical tests used a two-sided significance level of 0.10.

For modeling, we used a the generalized linear mixed model (GLIMM) using a logit link and binomial distribution (i.e., logistic regression with outcome regular v. non-regular mammography rescreening) using demographic and potential correlates as predictors. From this model, adjusted rescreening rates can be calculated. The condition nested within clinic term was treated as a random effect. This analysis is consistent with a group-randomized trial design where the clinic is repeated over time and conditions (or treatments) are nested within clinics.

The generalized linear mixed model follows.

Application. Our design is pre-post since we have collected data on rescreening mammography at baseline and then post-intervention. We attempted to fit a parsimonious model while at the same time achieving fit. The final models do not include covariates since these were not statistically significant (i.e., not significant adjusters) at the 0.10 significance level. We therefore exclude discussion of the covariate analysis.

Both the bivariate and the regression model analyses were run using The SAS System. The GLIMM logistic regression model was fit using the SAS GLIMMIX macro and procedure in order to control for intra-class correlation (ICC) (Varnell, et al. 2004; Murray, et al. 2004). The analyst may use the newer GLIMMIX procedure in SAS v9.1.3 (as an Internet download).

Following the nomenclature above, we look at three scenarios using two values of c=2 conditions with g=2 clinics per condition (actually we have one in one condition and two in the other in this pilot) and t=2 time periods for each clinic with measurements on individual women within each clinic across both time periods. This gives us denominator degrees of freedom (ddf) for the first fixed effect c(g-1)=2(2-1) = 2(1) =2 and for the second and third fixed effects the ddf will be (t-1)c(g-1) = (2-1)(2)(2-1) = (1)(2)(1) = 2.

The corresponding SAS code using the GLIMMIX macro are included in two equivalent realizations in Tables 1A and 2A. The corresponding Proc GLIMMIX procedure code are in Tables 1B and 2B.

Table 1A. GLIMMIX (Macro glmm800.sas):

```
%include "c:\sasmacro\glmm800.sas" ;

/*logistic regression with random
effects*/ ;
%glimmix(data=data.cenmerge ,
 stmts=%str(
 class clinic time condtn ;
 model madhere = condtn time condtn*time
       / ddf= 2,2,2  ddfm=residual ;
 RANDOM clinic(condtn)
time*clinic(condtn) ;
 lsmeans condtn*time / om e slice=time
                           slice=condtn
;
estimate '(I1-I0)-(C1-C0)'
   condtn*time -1 1 1 -1 / cl e ;
   ),
error=binomial,
link=logit
) ;
run ;
```

Table 1B. PROC GLIMMIX:

```
PROC GLIMMIX DATA=DATA.CENMERGE
            ORDER=DATA ODDSRATIO ;
  CLASS clinic time condtn ;
  MODEL madhere = condtn time condtn*time
        /dist=binomial link=logit
         ddf= 2,2,2  ddfm=residual ;
  NLOPTIONS technique=newrap ;
  RANDOM clinic(condtn)
         time*clinic(condtn) ;
  LSMEANS condtn*time / om e slice=time
                           slice=condtn ;
  CONTRAST '(I1-I0)-(C1-C0)'
           condtn*time -1 1 1 -1 / e ;
  ESTIMATE '(I1-I0)-(C1-C0)'
           condtn*time -1 1 1 -1 / cl e ;
RUN ;
```

Table 2A. GLIMMIX (Macro glmm800.sas):

```
%include "c:\sasmacro\glmm800.sas" ;

/*logistic regression with random
effects*/ ;
%glimmix(data=data.cenmerge ,
 stmts=%str(
 class clinic time condtn ;
 model madhere = condtn time condtn*time
/ ddf= 2,2,2  ddfm=residual ;
 RANDOM int time /subject=clinic(condtn);
 lsmeans condtn*time / om e slice=time
```

```
                        slice=condtn ;
 estimate '(I1-I0)-(C1-C0)'
   condtn*time -1 1 1 -1 / cl e ;
   ),
error=binomial,
link=logit
) ;
run ;
```

Table 2b. PROC GLIMMIX:

```
PROC GLIMMIX DATA=DATA.CENMERGE
            ORDER=DATA ODDSRATIO ;
 CLASS clinic time condtn ;
 MODEL madhere = condtn time condtn*time
       /dist=binomial link=logit
        ddf= 2,2,2  ddfm=residual ;
 NLOPTIONS technique=newrap ;
 RANDOM int time /subject=clinic(condtn) ;
 LSMEANS condtn*time / om e slice=time
                            slice=condtn ;
 CONTRAST '(I1-I0)-(C1-C0)'
          condtn*time -1 1 1 -1 / e ;
 ESTIMATE '(I1-I0)-(C1-C0)'
           condtn*time -1 1 1 -1 / cl e ;
RUN ;
```

For Tables 1A, 1B, 2A and 2B, for the CLASS statement, we include CLINIC, TIME, and condition (CONDTN). That is, we treat each of these factors as categorical variables. The MODEL statement includes the outcome variable MADHERE [regular vs. non-regular rescreening: modeling the probability of regular rescreening], condition (SRD vs. SRDTTR), TIME (post vs. pre) and the condition by TIME interaction. Within the MODEL statement, we use the options ddf=2,2,2 to define the specific denominator degrees of freedom for each fixed effect. The LSMEANS requests the interaction of condition with TIME in order to correctly obtain the test for intervention effect. Within this request, the SLICE=TIME option will give individual tests for the main effects of condition at each time point. The SLICE=COND will generate a test of whether the linear time trend in each condition is zero. The ESTIMATE option will generate a table of coefficients that are used to compute the estimate. The tables differ in how the RANDOM effects are defined pursuant to formula (11). For Tables 1A and 1B , this statement includes CLINIC and TIME by CLINIC interaction with the appropriate nesting of condition within CLINIC. For Tables 2A and 2B, the equivalent but more efficient code includes a random intercept and TIME with the subject (unit of analysis) given as condition nested within clinic [i.e., clinic(condtn)]. The resulting output from the Table 2a SAS run is abbreviated below (using ODS RTF output).

Table 3a shows the dimensions or design information of the analysis. We see readily that the unit of analysis is the clinic (n=3) rather than the individuals within the clinic.

Table 3a. Dimensions	
Covariance Parameters	3
Columns in X	9
Columns in Z Per Subject	3
Subjects	3
Max Obs Per Subject	432

Table 3b shows the covariance parameter estimates. We used the code in Table 2b so that, within this table, the “Intercept” is the CLINIC(CONDTN) and “TIME” is the TIME*CLINIC(CONDTN) random effect.

Table 3b. Covariance Parameter Estimates

Cov Parm	Subject	Estimate
Intercept	CLINIC(CONDTN)	0.05167
TIME	CLINIC(CONDTN)	0.03090
Residual		1.0044

Using Table 3b, we can construct the estimate of the intraclass correlation (ICC) coefficient from formula (14) as follows:

$$I\hat{C}C_{m:tg:c} = \frac{\hat{s}^{\,2}_{tg:c}}{\hat{s}^{\,2}_{tg:c} + \hat{s}^{\,2}_{e}} = \frac{0.031}{0.031+1} = 0.03$$

Furthermore, an estimate of the variance inflation factor can be calculated from formula (15) as:

$$V\hat{I}F_{m:tg:c} = [1+(m-1)I\hat{C}C_{m:tg:c}] = [1+(119-1)0.03] = 4.54$$

Table 3c shows the Type III tests for the fixed effects. The overall time by condition F shows non-significance at the 0.05 significance level (p=0.23). Given this preliminary finding using the clinic as the unit of analysis, we should interpret any sub-analysis results with caution.

Table 3c. Type 3 Tests of Fixed Effects				
Effect	**Num DF**	**Den DF**	**F Value**	**Pr > F**
CONDTN	1	2	0.18	0.7128
TIME	1	2	3.42	0.2055
TIME*CONDTN	1	2	2.96	0.2276

Table 3d (in Appendix A) shows the effect of the interventions. The positive value of 0.79 indicates that the increase in rescreening rates from pre to post in the SRD condition is greater than that of the SRDTTR condition. Table 3e (Appendix A) exhibits the individual least- squares means (LSMEANS) portion of the output while Table 3f (Appendix A) gives the effect slices of the intervention effects at each time point. These are relevant if the omnibus F-test is significant. However, for a pilot such as this, it can be important to look at these effect slices for any trending towards significance with a larger set of clinics. As such, the SRD condition has a p-value of 0.0986, indicating this may be an important condition if we had more clinics (using clinic as the unit of analysis.)

Bivariate Results. The pre-post intervention bivariate analyses for the three clinics' demographic and mammography rescreening results are shown in Table 1 of Sabogal, et al. 2005. There was an age difference between pre- and post-intervention for Clinic II (p<.001). There was a higher proportion of older women at follow-up than at pre-intervention (and conversely, younger women at pre- than at post-intervention). All other demographic variables, except for marital status (Clinics I and II), were not different across pre- to post-intervention with respect to demographic composition and outcome (i.e., rescreening). Because marital status had large numbers of missing data at Clinic I, these results were not included. Clinic I showed a difference with respect to evidence that the patient received a Pap and mammogram test on the same date (43.5 percent pre-intervention v. 54.1 percent post-intervention; p=.002). Clinic I also showed a marginally significant difference on percent of women who received a Pap test and mammogram the same day (19.3 percent pre-intervention v. 27.3 percent post-intervention; p=0.049). All other comparisons were not statistically significant, including comparisons of these same variables across pre- to post-intervention with respect to percent rescreened. We also included derived health-care access variables "patient neighborhood-to-clinic distance" and "clinic poverty index" (CPI). There were no differences from pre- to post-intervention with respect to clinic access. Overall, these results indicate no significant differential effects.

As indicated above, the generalized linear mixed model logistic regression model-building process produced no additional statistically significant adjustment factors. Marital status was not included due to large numbers of missing data or evidence that the patient received a Pap and mammography rescreening or test on the same day, due to missing (i.e., empty cells) with the outcome. Condition (system redesign; system redesign + tailored telephone reminder), time (pre-post intervention) and intervention condition by time interaction (p=0.09), indicated a slight intervention effect (the condition by time interaction is necessary for correct evaluation of the intervention effect). The intra-cluster/clinic correlation (ICC) was 0.03 indicating the need for a correction.

DISCUSSION

Through a multifaceted system approach, mammography rescreening rates improved in the "system redesign" condition but not in the "system redesign + tailored telephone reminder" condition.

Our intervention also addresses patients' knowledge of and barriers toward mammography and the cultural contexts of care, all of which have previously been shown to motivate patients to engage in rescreening.

Although quality improvement in health care often involves implementing multiple interventions, assuming more time intensive or complex interventions are more effective than simpler approaches is debatable. For example, we found adding a telephone call to other interventions produced no significant improvement in rescreening rates in the SRDTTR condition. Other researchers have found little to no improvement in rescreening rates when using more complex interventions. Thus, inexpensive and time-efficient interventions such as simple reminder calls and patient education may be sufficient to improve rescreening rates. Nevertheless, due to the limitations of our study design, and the fact that this is a pilot study, we are cautious to rule out the effectiveness of the additional tailored telephone reminder included in the SRDTTR condition without further exploration.

One possible explanation for the lack of improvement in mammography rescreening rates with the "tailored telephone reminder" condition may relate to our initial estimate of that clinic's pre-intervention rescreening rates. The clinic's billing data, used for site selection purposes, indicates a clinic rescreening rate of fewer than 35 percent, whereas the subsequent pre-intervention chart review revealed a rescreening rate of 45.6 percent, a figure too high for the clinic to have qualified for study participation had this information been available at the study's inception.

While limitations exist with our choice of a cross-sectional design in terms of comparable samples and generalizability, it was necessary given that this was a pilot and the limited sample sizes within clinics, available project resources, and mobility of the population. The cross-sectional design appears to have validity across clinics as the populations had a somewhat similar demographic profile within clinics in terms of insurance status, income, education, access, race, and ethnicity. However, other characteristics such as knowledge of breast health, perceptions, and motivation to screen can vary among samples leaving this design choice with limitations.

This multilevel intervention is unique because it addresses the interconnections among patients and clinic settings to achieve improved rescreening behavior, an approach related to the focal point framework which emphasizes the importance of acknowledging the multilevel context in behavior change. In addition, few studies have demonstrated the potential of multi-component system interventions to improve cancer rescreening within underserved communities. We were able to standardize core components of the intervention despite diversity in clinic settings, staff makeup, and operational practices, all important considerations noted in the literature (Glasgow et al., 2003). To overcome the overestimation of rescreening mammograms in underserved groups the current pilot study used patient medical records data to measure changes in rescreening rates.

CONCLUSIONS

Group-randomized designs need to be analyzed differently than the usual nested cohort designs. If the analyst does not choose the "correct" unit of analysis, they may be producing spurious results, though "correct" is a point of controversy and is left to the analyst to decide.

In our analysis, we noted a slight intervention effect for the SRD condition, using the clinic as the unit of analysis. The evidence points to the less complex SRD condition as an important consideration for developing future intervention studies for mammography rescreening.

LIMITATIONS

Currently, the Proc Glimmix procedure is only available as a download from The SAS Institute in SAS/STAT version 9.1.3. The GLIMMIX macro can be downloaded from the Internet in pre and post SAS version 8.

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APPENDICES

Appendix A.

Table 3d. Estimates								
Label	**Estimate**	**Standard Error**	**DF**	**t Value**	**Pr > |t|**	**Alpha**	**Lower**	**Upper**
(SRD1-SRD0)-(TTR1-TTR0)	0.7927	0.4609	2	1.72	0.2276	0.05	-1.1906	2.7760

Table 3e. Least Squares Means								
Effect	**BASELINE OR FOLLOW-UP**	**Intervention Condition**	**Margins**	**Estimate**	**Standard Error**	**DF**	**t Value**	**Pr > |t|**
TIME*CONDTN	Baseline	SRD	WORK._DS	-0.7783	0.2617	2	-2.97	0.0969
TIME*CONDTN	Baseline	SRDTTR	WORK._DS	-0.2268	0.3397	2	-0.67	0.5731
TIME*CONDTN	Follow-up	SRD	WORK._DS	0.04448	0.2604	2	0.17	0.8801
TIME*CONDTN	Follow-up	SRDTTR	WORK._DS	-0.1967	0.3498	2	-0.56	0.6305

Table 3f. Tests of Effect Slices							
Effect	**BASELINE OR FOLLOW-UP**	**Intervention Condition**	**Margins**	**Num DF**	**Den DF**	**F Value**	**Pr > F**
TIME*CONDTN	Baseline		WORK._DS	1	2	1.65	0.3272
TIME*CONDTN	Follow-up		WORK._DS	1	2	0.31	0.6358
TIME*CONDTN		SRD	WORK._DS	1	2	8.67	0.0986
TIME*CONDTN		SRDTTR	WORK._DS	1	2	0.01	0.9421