Keeping Track of Panel Members: An Experimental Test of a Between-Wave Contact Strategy

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The Panel Study of Income Dynamics (PSID) is a nationally representative longitudinal survey of approximately 9,000 families and their descendants that has been ongoing since 1968. Since 1969, families have been sent a mailing asking them to update or verify their contact information to keep track of their whereabouts between waves. Having updated contact information prior to data collection is associated with fewer call attempts and refusal conversion efforts, less tracking, and lower attrition. Given these apparent advantages, a study was designed in advance of the 2009 PSID field effort to improve the response rate of the contact update mailing. Families were randomly assigned to the following conditions: mailing design (traditional versus new), $10 as a prepaid versus postpaid incentive, timing and frequency of the mailing (July 2008 versus October 2008 versus both times) and whether or not they were sent a study newsletter. This article reports on findings with regard to response rates to the mailing and the effect on production outcomes including tracking rates and number of calls during 2009 under these different conditions, examines whether the treatment effects differ by key characteristics of panel members including likelihood of moving and anticipated difficulty in completing an interview, and provides some recommendations for the use of contact update strategies in panel studies.

Key words: Panel study; nonresponse; contact strategies; survey methods; attrition; tracking; field effort; respondent burden.

1. Introduction

Keeping track of sample persons between waves of data collection helps minimize attrition in longitudinal studies (Couper and Ofstedal 2009; Laurie et al. 1999). All things being equal, the longer the time between data collection waves, the greater the likelihood that sample persons have moved and the greater the difficulty in locating movers (Couper and Ofstedal 2009; Duncan and Kalton 1987). Movers are of particular concern in panel surveys as disproportionately missing them may result in biased measures of change. In order to capture residential changes between waves of data collection, the Panel Study of

Income Dynamics (PSID) has sent panel members a "contact information update" mailing every year since 1969. The mailing consists of a postage paid postcard with a label attached providing the last known address and telephone number of the respondent in the most recent wave of the study. In a brief letter that is included in the mailing, the respondent is asked to review the information, update or verify the address and telephone information, and return it to the study in order to receive a $10 postpaid check. About half of all panel members have responded to this between-wave mailing in recent years, with those who did respond being much less likely to require tracking and refusal conversion efforts, and about half as many calls to complete the interview, suggesting that the mailing may be effective.

Given the apparent advantage in having updated contact information prior to the start of data collection, we designed and implemented an experiment between the 2007 and 2009 interviews with the primary goal of improving the response rate of the contact update mailing and a secondary goal of examining the effects of new contact mailing design features on data collection effort in the 2009 round of interviews. Families were randomly assigned to the following conditions: mailing design (traditional versus new), $10 as a prepaid versus postpaid incentive, timing and frequency of the mailing (once in July 2008 versus once in October 2008 versus both times) and whether or not they were sent a study newsletter. This article reports on findings with regard to response rates to the mailing and the effect on production outcomes during 2009 under these different conditions.

2. Background

Various types of contact strategies to improve panel retention have been used in many studies (Couper and Ofstedal 2009; Laurie et al. 1999), with most research focusing on activities *during* waves of data collection, rather than *between* waves. Similarly, research has focused on different strategies for tracking sample persons, again usually at the start of data collection (e.g., Cotter et al. 2005; Ribisl et al. 1996). However, there are few examples of tests of between-wave contact strategies (for an exception, see Fumagalli et al. 2010). Our experimental design was informed by research from the survey methods literature on factors affecting nonresponse in the domains of design of respondent materials, incentives, and timing and frequency of requests.

First, drawing on research showing that respondent contact materials that are updated and aesthetically pleasing should increase the salience of the request, which may enhance cooperation (Dillman 2007), we hypothesized that families would be more responsive to a request to update their contact information that was modern and colorful compared to one that used a dated font and a black and white design. Second, based on research showing that under some conditions a prepaid incentive increases cooperation by heightening the salience of the incentive, as well as the respondent's sense of reciprocity (Laurie and Lynn 2009; Singer et al. 1999; Singer 2002), we hypothesized that a prepaid incentive may increase the rate of contact updates, as well as potentially reduce field effort during production. Third, the timing and frequency of the contact update request may affect cooperation. Ideally, the request occurs close enough to the upcoming data collection that most residential changes are captured, but not so close to the survey interview that perceptions of respondent burden are increased. On the other hand, the automated

forwarding of mail by the U.S. Postal Service expires one year after a move, so waiting too long may mean mail may not be forwarded to those who have moved in time for them to return the contact update request before data collection begins. As described below, two mailing conditions that manipulated time of the year were chosen.

With regard to frequency of contact, evidence on the optimal amount of respondent contact is scant. In the two-year timeline of a biennial survey such as the PSID and other large household surveys, panel members not only participate in a lengthy interview (and often additional supplements), but they are also sent a respondent newsletter, a contact information update, and a letter in advance of the upcoming interview. At what point do these multiple contacts increase perceptions of burden, or in fact, do they enhance perceptions of identification with the survey? It is likely that there are differential effects of these contact types (i.e., a newsletter versus a request for contact information) and perceptions of burden or identification may vary by characteristics of sample members. While the full set of research questions about whether the multiple contacts increase burden or enhance study identification are beyond the scope of this study, a follow-up mailing for nonresponders to the initial mailing was included to test the impact of the frequency of mailing on response. A final treatment condition was included as an additional manipulation of the amount of respondent contact of a different kind. In this condition, half the sample was mailed a respondent newsletter in February of the year after data collection, as is typically done in the PSID. The other half of the sample was not mailed the newsletter.

Thus, in this study the manipulation of the mailing involved modifying aspects of the design, the incentive, the timing, and the frequency and amount of contact being made, with guidance from the survey methods literature. The key research questions that this experiment seeks to answer are the following:

1. Can we design a successful strategy that increases response rates to biennial requests for contact updates from families in a panel study?
2. What features of the protocol lead to improved response rates to the contact update request and reductions in data collection efforts, such as number of calls during production and the tracking of families?
3. Do these treatment effects differ by key characteristics of panel members, including likelihood of moving and anticipated difficulty in completing an interview?

3. Methods

3.1. Sample

The sample included in this experiment consisted of 8,166 families who participated in the 2007 Wave of the Panel Study of Income Dynamics (PSID) or who were identified as new families in 2007, making them eligible to be followed as a sample in 2009. The PSID is a longitudinal study of a nationally representative sample of U.S. families that began in 1968 and collects a variety of data on economic, health, and social behavior. The original 1968 PSID sample was drawn from two independent samples: a nationally representative sample of roughly 3,000 families designed by the Survey Research Center at the University of Michigan (the "SRC sample") and an over-sample of roughly 2,000

low-income African American families from the Survey of Economic Opportunity (the "SEO sample"). In 1997, 511 families who had immigrated to the U.S. after 1968 were added to the sample to enhance its national representativeness. The study is a genealogical panel, following the original 1968 sample and children in these households that grow up and form their own economically independent families (known as "split-offs"). Thus, the active sample is refreshed each wave through the addition of "split-offs" and includes families related both biologically and by marriage, with up to four generations of families participating in a given wave. Data have been collected annually 1968–1997 and biennially since 1999 through the most recent wave in 2011. The mode of data collection is via computer-assisted telephone interview for approximately 97% of the sample, with the balance interviewed in-person using computer-assisted personal interviewing. Data collection occurs over a nine-month period between March and December in odd calendar years.

3.2. Experimental Design

As shown in the first row of Table 1, the experimental design was comprised of four fully-crossed treatment conditions. First, to examine the effects of the aesthetic of the design of the mailing, half the families were sent a traditional black and white mailing, and the other half were sent a colorful, updated mailing. The appendix shows the text used for the postpaid incentive in both the traditional design and the new design. The text of the prepaid incentive for the traditional design read: "Please accept this $10 check as a thank you for returning the postcard to us!" The text of the prepaid incentive condition for the new design read: "We have included a $10 check in this envelope as a token of our sincere appreciation for returning the postcard, even if you have no changes to report." In all other respects the designs of the prepaid and postpaid mailings were identical. The mailing was sent to the individual who was the respondent in the prior wave. The second condition examined the effects of a prepaid or postpaid incentive by varying whether a $10 check for returning the contact information request was sent with the request or paid after receiving the information back from the respondent, to whom the check was made out. The third condition examined the timing and frequency of the mailing. Two times of year were chosen for the mailing. The first time was July 2008 which was the midway point between the end of the prior wave (i.e., December 2007) and beginning of the next wave (i.e., March 2009). The second time chosen was October 2008 which was as far into the year as was operationally feasible to update contact information before production interviewing began. An additional mailing condition sent families materials in July and followed up with a second mailing in October for nonresponders. This condition allowed an examination of the effect on response rates of two contacts versus one. Thus, one-third of the sample was mailed the contact update materials in July, one-third mailed in October, and one-third mailed initially in July with a follow-up mailing in October if there was no response from the July mailing. Finally, to further examine the effects of number of respondent contacts and potential burden, a respondent newsletter describing study results and upcoming data collection initiatives was sent to one-half of the panel members in February of 2008, with the other half not sent the newsletter.

Table 1.   Sample sizes and characteristics of families prior to treatment for each treatment condition group (N = 8,166)

| | Treatment conditions | | | | | | | | |
| | Traditional design? | | Prepaid? | | Timing and frequency of mailing | | | Sent newsletter? | |
	Yes	No	Yes	No	July only	October only	July and October	Yes	No
Number of families	4,137	4,029	4,051	4,115	2,718	2,756	2,692	4,058	4,108
Characteristics of families in 2007 (%)									
Likelihood of moving before 2009:									
Probably or definitely	29.9	29.3	28.9	30.3	29.9	28.6	30.4	28.9	30.3
None or uncertain	67.6	68.3	68.8	67.1	68.0	68.9	66.8	68.8	67.2
Missing	2.4	2.5	2.3	2.6	2.1	2.5	2.7	2.3	2.6
Family income is less than or equal to median	50.8	49.1	48.3	51.6	49.6	48.1	52.2	50.2	49.7
Number of calls in 2007 to finalize the case:									
1–3	31.2	30.1	30.9	30.4	31.4	30.1	30.5	30.8	30.6
4 or more	68.8	69.9	69.1	69.6	68.6	69.9	69.5	69.2	69.4
Age of head of family:									
Less than 45	52.5	52.1	52.5	52.2	52.1	52.0	52.9	52.1	52.5
45 or older	47.5	47.9	47.5	47.8	47.9	48.0	47.1	47.9	47.5
Sample types:									
SRC	63.8	63.9	63.8	63.9	63.2	66.5	61.9	62.3	65.4
SEO	28.7	29.4	29.1	29.0	30.7	25.9	30.4	30.2	27.9
Immigrant	7.5	6.7	7.1	7.1	6.0	7.6	7.7	7.5	6.7
Split-off family	7.0	6.4	7.0	6.5	6.8	6.7	6.6	6.6	6.8

3.3. Measures

3.3.1. Outcome Measures

Results for two sets of outcome measures with two dependent variables each are reported. The first set is referred to as "contact update outcomes" and captures information about respondent behavior in returning the contact information postcard or providing a new telephone number. This set consists of two outcomes, "postcard return" and "new telephone number." "Postcard return" is defined as a dummy variable coded as "yes" = 1 for instances when the respondent returned the postcard and either verified the current information, updated the information with a new address, or fixed the current information (e.g., changed "street" to "avenue"), and "no" = 0 for instances when no postcard was received back from the respondent. The overall rate of postcard return was 60.4 percent, with 10.8 percent of returners providing a new address, 16.4 percent providing an address fix, and 72.8 percent verifying their contact information.

   "New telephone number" is defined as a dummy variable coded "yes" = 1 for instances of receiving a postcard back from the respondent that includes the provision of a new telephone number that was not available in the prior wave, and "no" = 0 when a new telephone number was not provided, or a postcard was not returned. This latter group will by definition include panel members who have a new telephone number but do not provide it, as well as panel members who do not have a new telephone number. New telephone numbers were provided by 20.5 percent of the postcard returners and 12.4 percent of all respondents. Having an accurate telephone number at the beginning of field production is important because the PSID completes over 97 percent of its interviews over the telephone.

   A second set of measures was designed to assess the effect of the treatment conditions on two production outcomes. "Total calls in 2009" is a count variable capturing the full range of telephone calls that were made to reach the final disposition of the case during the 2009 field effort. The average number of calls in 2009 was 13.7, with a median of 6.0. A second variable was created called "Tracking required" to capture information about whether the sample case needed specialized interviewer effort during the field period to locate the case, whether due to the telephone number of record not being answered or out of order, or the respondent moved and no forwarding contact information was available. The variable is a dummy variable that allows the calculation of a tracking rate in the sample, with "yes" = 1, the case required tracking and "no" = 0, the case did not require tracking. Twenty-two percent of the families required tracking in 2009. The effects of the experiment on the overall 2009 interview rate were also examined but because the interview rate had such little variation, with 97.1 percent of the sample providing an interview, it was not possible to detect meaningful differences in the impact of the treatment conditions.

3.3.2. Measures to Assess Differential Impact

Two key measures were constructed to investigate whether the treatment conditions had differential effects for key subgroups on contact updates and production outcomes.

These variables were obtained from the public use data available at the PSID website (http://psidonline.org; Panel Study of Income Dynamics 2007) and were also included in all the models as covariates. A dummy variable for "likelihood of moving" was constructed using data from the prior wave. In 2007, respondents were asked to estimate the likelihood that they would move in the next couple of years: "Do you think you might move in the next couple of years?" If the respondent answered yes, they were then asked: "Would you say you definitely will move, probably will move, or are you more uncertain?" A dummy variable for likelihood of moving was created with "no" = 0 indicating "none" or "uncertain" likelihood of moving and "yes" = 1 indicating a "definite" or "probable" move. Couper and Ofstedal (2009) found this variable to be a strong predictor of the need for tracking.

The second measure to assess differential impact was expected difficulty in completing a 2009 interview as measured by "high versus low calls in 2007." The number of calls during 2007 production was highly skewed, with a mean of 10.3 and a median of six. A series of sensitivity analyses examined the cumulative effect of additional numbers of calls in 2007 on postcard returns in 2008 and number of calls in 2009. Each additional call in 2007 was associated with a linear reduction in rates of postcard returns, and an increase in the number of 2009 calls. Based on these results, an indicator variable was created to examine the differential effect of the treatment conditions for panel members who were high versus low on the number of calls during 2007. Families who had between one and three calls were categorized as "low" on an indicator variable and families with greater than three calls were categorized as "high."

3.3.3.   Control Variables

A series of control variables obtained from the public use data available at the PSID website (http://psidonline.org; Panel Study of Income Dynamics 2007) was included in all models. In addition to the dummy variables for likelihood of moving and high or low total number of calls in 2007, these variables included characteristics of PSID families such as family income, age of household head, and indicators for various sample types in the PSID. An indicator variable for whether the total family income reported in 2007 was equal to or below the median family income was created. Families at or below median income were coded "yes" = 1 and families who were above median income were coded "no" = 0. An indicator variable signifying whether the age of the head was under 45 ("yes" = 1) or 45 and older was created ("no" = 0). Three separate dummy variables that each identified types of sample members were included in the model. The first dummy variable coded families who were part of the original low-income Survey of Economic Opportunity (SEO) oversample as "yes" = 1 and coded families who were part of the original Survey Research Center (SRC) national probability sample as "no" = 0. A second dummy variable coded families who came from the 1997/1999 post-1968 immigrant refresher sample as "yes" = 1 and those who did not come from this sample as "no" = 0. Finally, a third dummy variable was included that coded families who were designated as split-off families during the 2007 wave as "yes" = 1 and nonsplit-off families as "no" = 0.

3.4. Analysis Strategy

The first step in the analysis is to provide a description of the random assignment of the sample across the treatment conditions. In the second step, bivariate results of the effects of each of the four treatment conditions on the contact update and production outcomes are presented. As no significant interaction effects among the treatment conditions were found, the third step presents results from multivariate regression analyses predicting each of the four outcome measures from the four separate treatment conditions. These models control for variables that are known to predict the outcome measures, thereby increasing the precision of the estimated effects of the treatment conditions. The fourth step includes multivariate models identical to those estimated in step two except stratified by likelihood of moving and high versus low 2007 calls, with separate models for likely movers and non-movers, and high 2007 calls and low 2007 calls. For these models, the statistical significance of differential effects of the treatment conditions by likelihood of moving, and by high versus low 2007 calls are determined by also estimating models that interact these factors with the treatment conditions; results of these tests are reported in the tables. All multivariate models include controls for the variables described above. Estimates of the effects of the treatment conditions on the bivariate outcomes "Postcard return," "New telephone number," and "Tracking required" were calculated using logistic regression analysis. Because of its skewed distribution as a count variable, estimates of the effects of the treatment conditions on "Total calls in 2009" were calculated using Poisson regression analysis. Because the main goal of our analysis is to examine the effects of the experimental manipulations in the sample who received the postcard mailing, sample weights are not used in the models, although the control variables that are included represent the main variables that are used to construct sample weights.

4. Results

4.1. Random Assignment to Treatment Conditions

Eligible families were randomly assigned to four fully-crossed treatment conditions which defined a 2 ("design") × 2 ("incentive") × 3 ("timing and frequency") × 2 ("newsletter") factorial design. Because families in the PSID are related and may communicate with each other, all related families received assignment to the same treatment condition. Families were assigned to one of the 24 cells in the design. Table 1 displays the number and characteristics of families and their assignments within each treatment condition. The proportion of panel members assigned within each treatment condition is roughly equal. In addition, within each treatment condition the characteristics of those families that did and did not receive the treatment are quite similar, as illustrated in Table 1, indicating that the random assignment was successfully implemented. Table 1 also provides information on the sample composition, with about 30 percent of the sample indicating a high likelihood of moving in 2007, about 70 percent requiring four or more calls in 2007 to finalize their interview, and about 52 percent of the families having a household head younger than age 45. Approximately 64 percent of the families are from the SRC sample, 29 percent from the low-income SEO over-sample, and seven percent from the immigrant sample.

Nearly seven percent of the families are "split-offs", with the 2007 Wave their first wave of eligibility for participating in the PSID.

4.2. Contact Update and Production Outcomes After Treatment: Bivariate Results

Table 2 presents the proportion of panel members returning the postcard, providing a new telephone number, and requiring tracking, as well as the average number of telephone calls in 2009 to complete the case by each of the treatment conditions. There are four basic results of note in this table. First, the traditional mail design yielded a significantly *higher* proportion of panel members who returned the postcard (62.2%) compared to the new design (58.6%). Second, the July mailing with the October follow-up condition resulted in a significantly greater proportion of panel members returning the postcard (65.7%) and providing a new telephone number (15.0%) compared to mailing in July only (58.4% returning the postcard and 11.1% providing a new telephone number) or October only (57.3% returning the postcard and 11.2% providing a new telephone number). Third, whether a newsletter was sent and whether the incentive was prepaid or postpaid had no effect on any of the outcomes. Fourth, none of the treatment conditions affected the proportion requiring tracking or the average number of calls in 2009 to complete a case.

4.3. Main Effects of Treatment Conditions on Contact Updates and Production Outcomes: Multivariate Results

Multivariate regression models were estimated to examine the effects of the treatment conditions on the four dependent variables described above. Logistic regression was used to estimate models for obtaining a postcard return, a new telephone number, and being in tracking. Poisson regression was used to model the total number of calls during 2009 production. Each of the models included the set of control variables described earlier.

The results of the multivariate models are presented in Table 3. The results of the effects of the treatment conditions on *contact outcomes* are consistent with the bivariate results in Table 2 showing that the traditional design was associated with a significantly higher rate of postcard returns. In addition, panel members in the July and October mailing condition returned the postcard and provided a new telephone number at significantly greater rates than the October only mailing condition and the July only mailing condition (not shown in the table). Sending the newsletter and whether the incentive was prepaid or postpaid had no effect on rates of postcard returns or provision of new telephone numbers. Interestingly, the control variables had quite different effects on the outcomes as well. Likely movers, families below the median income, younger heads, and the low-income SEO oversample type all had significantly lower rates of postcard returns. Despite this pattern, these characteristics all significantly predicted higher rates of new telephone numbers, most likely due to the fact that these families change phone numbers more frequently. Families who required more than three calls to finalize in the prior wave, families from the immigrant refresher sample, and split-off families all had lower rates of both postcard returns and new telephone numbers.

For the *production outcomes*, total calls during 2009 were significantly reduced for families who were sent the traditional design compared to the new design, prepaid versus postpaid, July only or July and October compared to October only (and for July and

Table 2. Contact update and production outcomes after treatment for each treatment condition group (N = 8, 166)

| | Treatment conditions | | | | | | | | |
| | Traditional design? | | Prepaid? | | Timing and frequency of mailing | | | Sent newsletter? | |
	Yes	No	Yes	No	July only	October only	July and October	Yes	No
Contact update outcomes (%)									
Returned postcard and gave new address, fix, or verification	62.2	58.6*	61.2	59.7	58.4	57.3	65.7(a,b)	60.5	60.3
Obtained new telephone number	11.8	13.0	12.4	12.4	11.1	11.2	15.0(a,b)	12.3	12.4
Production outcomes in 2009									
% Required tracking	22.2	21.7	21.9	22.1	22.0	21.2	22.7	21.4	22.5
Average number of calls to complete case	13.6	13.9	13.5	13.9	13.6	14.1	13.5	13.5	14.0

* indicates statistically significant difference at the 0.01 level using a *t*-test.

(a,b) indicates statistical significance using a *t*-test at $p < .01$ level for July and October relative to July only (a) and statistical significance at $p < .01$ level relative to October only (b).

Table 3. Main effects of treatment conditions on contact update and production outcomes (N = 8, 166)

	Contact update outcomes		Production outcomes in 2009	
	Returned postcard and gave update, fix, or verification: OR	Obtained new telephone number: OR	Tracking required: OR	Total calls: Poisson *b*
Treatment conditions				
Traditional design	1.18**	0.87*	1.02	−0.010*
Prepaid	1.06	1.03	1.02	−0.028**
Timing and frequency of mailing				
July only	1.08	0.96	1.02	−0.035**
July and October	1.56**	1.36**	1.04	−0.052**
October only (reference)				
Sent newsletter	1.04	1.00	0.92	−0.037**
Control variables				
Likelihood of moving before 2009:				
Probably or definitely	0.72**	1.65**	1.45**	0.033**
None or uncertain (reference)				
Missing	0.73*	1.23	1.47**	0.161**
Family income is less than or equal to median	0.81**	1.50**	1.64**	−0.124**
Number of '07 calls to finalize the case:				
1–3 (reference)				
4 or more	0.55**	0.84*	1.78**	0.620**
Age of head of family:				
Less than 45	0.57**	1.25**	2.01**	0.456**
45 or older (reference)				
Sample types:				
a. SRC (reference)				
SEO	0.46**	1.18*	1.67**	0.202**
b. Original PSID sample (reference)				
Immigrant sample	0.46**	0.82	1.34**	0.182**
c. Non Split-off family (reference)				
Split-off family	0.63**	0.71*	1.26**	0.01
Mean of dependent variable	0.604	0.124	0.220	13.7

OR = odds ratio, Poisson *b* is the unstandardized Poisson regression coefficient. ** = $p < 0.01$, * = $p < 0.05$.

October compared to July only; not shown in the table), and who were sent the newsletter compared to those who were not. None of the treatment conditions affected rates of tracking. In terms of the control variables, the number of calls and tracking rates were higher among families who were likely to move, needed four or more calls in the prior wave, had young heads, and were from the SEO and immigrant samples. Those with income at or below the median had significantly fewer calls during production but higher rates of tracking compared with families with income above the median.

4.4. Differential Effects of Treatment Conditions on Contact Updates and Production Outcomes: Stratified Models and Interaction Models

As described above, the most consistently observed treatment effect was for the July and October mailing condition, which increased the amount of contact update information collected in advance of 2009 production and reduced the number of calls needed to finalize the case in 2009. The traditional design was associated with a significant increase in postcard returns, a significant reduction in the number of calls in 2009, and also a modestly significant reduction in the likelihood of obtaining a new telephone number. Finally, prepaying families to return the card and sending them a study newsletter reduced the number of calls in 2009. How did these effects differ by various subgroups of PSID respondents and families? Can we identify subgroups that are particularly responsive to aspects of the treatment conditions to develop strategies for optimizing contact update requests that are tailored to these subgroups?

The potentially differential effects of the treatment conditions on contact update and production outcomes were examined by two key characteristics known to be associated with nonresponse and field effort: likelihood of moving and total number of calls during the prior wave of data collection.

4.4.1. Effects of Treatment Conditions by Mover Status

As shown in Table 3, families with a high likelihood of moving were less likely to return the postcard, more likely to provide a new telephone number and more likely to need tracking and have higher calls during production than non-movers. Do any of the treatment conditions help increase rates of returns and telephone numbers, and reduce tracking and calls for families who are likely to move?

Table 4 shows the results of multivariate regression models that examine whether the treatment conditions have differential effects on the outcomes by mover status. We suppress from the table the estimates of the control variables to conserve space. The results show that prepaying reduced tracking for likely movers only ($p = .08$). There are consistent differential effects of the treatment conditions on the number of total calls in 2009 for likely movers and nonmovers. Here we see that all of the significant effects of the treatment conditions except for "sent newsletter" observed in the main effects model in Table 4 are important only for panel members who are likely movers. Total calls for likely movers only are reduced by the traditional design, a prepaid incentive, and both July only and July and October timing.

Estimation of the predicted number of calls for each of the treatment conditions, while holding the other conditions constant at their mean values, provides practical information

Table 4. Treatment effects on contact update and production outcomes for those likely to move (N = 2,418) and those not likely to move (N = 5,748)

	Contact update outcomes				Production outcomes in 2009			
	Returned postcard and gave update, fix, or verification: OR		Obtained new telephone number: OR		Tracking required: OR		Total calls: Poisson b	
	Likely to move	Not likely to move	Likely to move	Not likely to move	Likely to move	Not likely to move	Likely to move	Not likely to move
Treatment conditions								
Traditional design	1.36**	1.11[c]	0.90	0.86	0.92	1.09	-0.053**	0.006[a]
Prepaid	0.92	1.14[b]	0.97	1.07	0.86	1.13	-0.106**	0.010[a]
Timing and frequency of mailing								
July only	1.08	1.08	0.88	1.03	1.03	1.03	-0.127**	0.014[a]
July and October	1.53**	1.57**	1.38*	1.35**	1.07	1.02	-0.130**	-0.012[a]
October only (reference)								
Sent newsletter	1.09	1.01	1.10	0.95	0.90	0.94	0.012	-0.062**[a]

OR = odds ratio, Poisson b is the unstandardized Poisson regression coefficient. Chi-square test. ** = $p < 0.01$, * = $p < 0.05$, and a and b indicate statistically significant differences in effect of treatment condition between likely movers and non-movers at the 0.01 and 0.05 levels, respectively. All models also include the control variables contained in Table 3.

about the magnitude of these effects. For likely movers, the average number of calls is reduced by the traditional design by 0.8 calls, by a prepaid incentive by 1.6 calls, by the July timing by 1.9 calls, and by the July and October timing by nearly 2.0 calls. Finally, the effect of the newsletter in reducing total calls is of statistical significance only for those not likely to move, reducing the average number of calls by 0.8.

4.4.2.   Effects of Treatment Conditions by Number of Total Calls During 2007 Production

In Table 3, we saw that families who had four or more calls during 2007 data collection were significantly less likely to return the postcard and to provide a new telephone number, and had higher rates of tracking and more calls during 2009 production compared to families who had fewer than four calls. The following results describe the differential effect of the treatments on these outcomes for families who varied in the number of calls in the prior wave.

The results in Table 5 show that there are two statistically significant differential effects of the timing of the mailing conditions by 2007 call status. Compared to the October mailing condition, families that had a high number of calls in 2007 had significantly fewer 2009 calls if they were in the July mailing condition or the July and October mailing condition. Estimation of the predicted number of calls shows a reduction in calls by nearly a full call for the July mailing condition and 1.7 calls for the July and October mailing condition, relative to the October mailing condition, for families who had a high number of calls in 2007. Families with a low number of calls in 2007 show the opposite pattern, with the July mailing condition and July and October mailing condition increasing calls in 2009 relative to mailing in October only. Calls in 2009 were predicted to increase by approximately .10 for families in the July mailing condition, and by .20 for families in the July and October mailing condition, relative to the October only condition.

5.   Discussion

The goals of this study were to attempt to increase cooperation to a request for contact information, document the features that influenced cooperation, and examine whether there were sample subgroups that differed in responsiveness to the treatment conditions. One important caveat to note at the outset is that the experiment evaluated in this study was conducted during a time when the PSID had a two-year period between interviews, making the generalization of results to surveys that have different lengths of time between interviews, such as commonly used annual surveys, uncertain. First, we found modest success in designing a new contact update protocol that increased the overall response rate by approximately 7–10 percentage points in the overall sample compared to the protocol that had been in place for decades in the PSID. Second, we found modest effects of the experimental manipulations on three of the four key outcomes of interest across the overall sample. To summarize, we found a consistent positive effect of the timing of the mailing, such that mailing in July and mailing in July and October relative to mailing only in October increased postcard returns, new telephone numbers, and reduced total calls in 2009. All of the experimental manipulations produced independent, statistically significant reductions in total calls in 2009. None of the experimental manipulations

Table 5. Treatment effects on contact update and production outcomes by low number of 2007 calls (N = 2,505) and high number of 2007 calls (N = 5,661)

	Contact update outcomes				Production outcomes in 2009			
	Returned postcard and gave update, fix, or verification: OR		Obtained new telephone number: OR		Tracking required: OR		Total calls: Poisson *b*	
	One to three calls in 2007	Four or more calls in 2007	One to three calls in 2007	Four or more calls in 2007	One to three calls in 2007	Four or more calls in 2007	One to three calls in 2007	Four or more calls in 2007
Treatment conditions								
Traditional design	1.17	1.19**	0.79	0.92	1.17	0.98	0.002	-0.016*
Prepaid	0.99	1.09	0.93	1.08	0.98	1.02	-0.017	-0.032**
Timing & frequency of mailing								
July only	1.10	1.07	0.72*	1.10[b]	1.26	0.97	0.093**	-0.058**[a]
July and October	1.71**	1.51**	1.29	1.40**	1.19	1.01	0.201**	-0.107**[a]
October only (reference)								
Sent newsletter	1.10	1.01	1.02	0.99	0.85	0.95	-0.054**	-0.033**

OR = odds ratio, Poisson *b* is the unstandardized Poisson regression coefficient. Chi-square test. ** = $p < 0.01$, * = $p < 0.05$, and a and b indicate statistically significant differences in effect of treatment condition between call groups at the 0.01 and 0.05 level, respectively. All models also include the control variables contained in Table 3.

affected rates of tracking in 2009. And finally, there were differential effects of some of the treatment conditions by mover status and number of calls in the prior wave to complete the case.

The effect of a prepaid versus promised incentive was restricted to total calls in 2009, and had no effect on postcard returns or receiving new telephone numbers. This is consistent with the results of Fumagalli and colleagues (2010) who tested prepaid versus promised incentives with variable amounts in the British Household Panel Survey, and similarly found no significant effects. One reason for the lack of effect of a prepaid versus promised incentive in the PSID may be that panel members have been in the study for many years and have developed trust that the study will deliver on its promise to provide the incentive in a timely manner after the completion of the interview. Contact update mailings in the PSID over the years have always used a promised incentive and the switch to a prepaid incentive of the same amount might not have been a change salient enough to produce a change in behavior. Both the prepaid and promised incentives were paid by check, and it may be that cash would have increased the salience of a prepaid incentive. However, the prepaid incentive did significantly reduce the number of calls by about eight-tenths of a call needed to finalize the case in 2009, which suggests some slight efficiency gain from this strategy. Another dynamic was observed that seemed to offset this modest gain. Approximately 15% of the families assigned to the prepaid incentive condition cashed their checks without returning their postcards. However, the fact that the check was cashed provided some confirmation of accuracy of the address on file, and is in this way similar to receiving a postcard that provided a verification of the current contact update information, and not an update. Nonetheless, the effects of the prepaid incentive were not consistently different from those of the promised incentive.

The fact that the newsletter mailing had limited effects across the outcomes suggests that its benefits may not be direct. That is, the newsletter may serve to keep panel members engaged, and may help interviewers to gain cooperation at each wave, but without this translating into a reduction of effort (i.e., number of calls, tracking rates). Fumagalli et al. (2010) tested a standard respondent report (similar in function to the PSID newsletter) versus a targeted report (aimed at younger persons, and those identified as "busy"). They found a positive effect of the latter on response rates for young people, but not the other targeted groups, suggesting that contact materials that are specific to particular groups of respondents may hold some promise in increasing cooperation.

We found systematic differences between the timing conditions, in both the bivariate and multivariate analyses. The July and October mailing proved to be more effective than the July only or October only mailing, increasing rates of return of the postcard by 7.3–8.4 percentage points (from 57.3% for October only and 58.4% for July only to 65.7% for July and October), and provision of new telephone numbers by nearly 4 percentage points (from 11.1% in July only and 11.2% in October only to 15.0% in July and October). Given that the incentive was not provided a second time for those who got two mailings, one explanation may simply be the positive effect of an additional contact attempt, consistent with the mail survey literature (e.g., Dillman 2007). Mailing to nonresponders was an inexpensive way of increasing response to the mailing. About 58 percent of the families in the July and October mailing condition responded to the initial mailing in July. The follow-up mailing in October to the families that did not respond in July produced a fairly

substantial increase in the overall postcard return rate of about 8 percent and in the new telephone number rate of about 4 percent, and reduced total calls to completion but not tracking rates in 2009 in the multivariate model.

Finally, the traditional design of the postcard was associated with significantly higher rates of postcard return and in the multivariate model, significantly fewer total calls, than the new design. One *post hoc* explanation for this counter-intuitive finding pointing to the effects of burden is that the newer design, while printed in color with more modern-looking font, required additional instructions on how to detach, fold and seal the postcard before returning, while the traditional design required only the latter two steps. In other words, the additional burden due to the complexity of the new design may have dissuaded panel members from returning it.

We focused on two key subgroups – those who said they were likely to move and those who needed greater effort to be interviewed in 2007 – to see if the contact update manipulations had differential effects. These two groups are those hypothesized to be most in need of updated contact information in advance of the start of production interviewing. Here again we find mixed results, depending on the outcome variables we examined. There were few effects of any of the treatment conditions on rates of postcard returns or new telephone numbers, with only the July and October mailing showing consistent positive effects across all groups. Looking at calling effort, however, for the likely movers, the prepaid incentive, the July mailing or July and October mailing, and the traditional design are all associated with significantly fewer calls to completion in 2009, whereas for those not likely to move, the newsletter is associated with significantly fewer calls. For those who required more effort (four or more calls) in 2007, the newsletter, the prepaid incentive, and the July mailing and the July and October mailing are all associated with significantly fewer calls to completion in 2009. This suggests that some of the manipulations have an added benefit in terms of reducing effort (and therefore cost) for those at greatest risk (movers, and those requiring more effort in 2007).

Taken together, the results of this experiment indicate that the use of a contact update protocol that includes a follow-up mailing across the entire sample of a panel study may yield fewer calls and thus potentially lower operational costs during data collection. We generated a basic estimate of the cost-effectiveness of the follow-up mailing by simply calculating the approximate point at which the follow-up mailing is beneficial in terms of its cost-savings in number of calls. We first estimated the approximate cost of the difference in the follow-up mailing compared to the July only mailing (approximately $14,000). We also estimated the predicted reduction in calls across the entire sample as a consequence of the follow-up mailing compared to the July only mailing (0.5 of a call). Across the entire sample of 8,166 families, the follow-up mailing would result in a savings of approximately 4,083 calls (i.e., $0.5 \times 8,166$). Thus, the point at which the follow-up mailing would be cost-effective would be equal to the cost of the mailing divided by the number of calls saved, which is about $3.70. This amount is most certainly less than the cost of a single call made during production interviewing, indicating that the follow-up mailing is likely to be a cost-effective strategy.
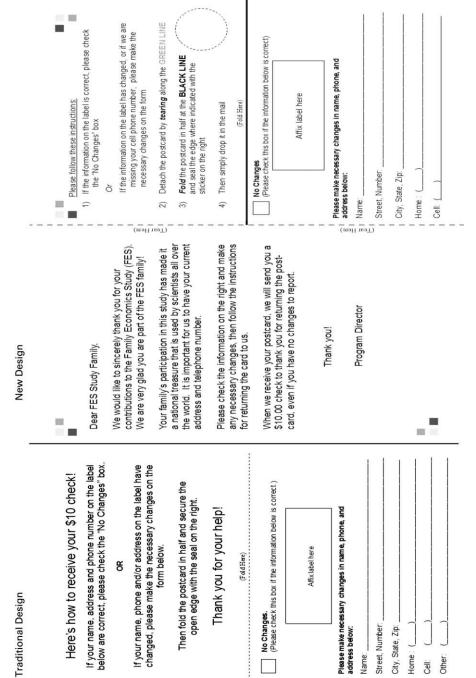
Building on this finding, the PSID included a follow-up mailing for all nonresponding families as part of the 2010 contact update protocol. Moreover, while the current experiment provided some suggestive evidence on the importance of incentives, the

results in regard to the superiority of prepaid versus postpaid were inconclusive, and because there was no condition with $0 incentive, the overall utility of the importance of the incentive could not be tested. Based on this, a new experiment in PSID was implemented in 2010 that manipulated the amount of the postpaid incentive for postcard return, including no incentive, $10 (status quo), and $20 (a higher amount). A fourth condition randomly assigned a portion of families to receive no postcard mailing in order to examine the overall utility of the mailing on production outcomes in 2011. This experimental design will enable us to better estimate the cost-effectiveness of the strategies. The postcard design (for those receiving it) was uniform across the incentive conditions, and included an updated font with the information about the incentive prominently placed, and without the lengthy instructions for returning. All families have also been sent the respondent newsletter based on interviewer sentiment that the newsletter is favorably received. The lack of evidence in the current study on the usefulness of the newsletter makes this dimension of respondent contact a candidate for a future experiment that should include manipulations of content and design.

Finally, the fact of there being differential effects of the various treatment conditions for those at high risk of nonresponse supports the development of tailored protocols to facilitate contact with these panel families. For example, prepaying likely movers and high-effort families may pay off in terms of reduced field effort. However, the differential treatment of family members in an ongoing panel may have unexpected results. For example, not all related sample families exhibit the same characteristics with regards to being high- or low-effort families. Treating one sample family in a particular way, with differential incentives, as one example, may lead to an expectation by a related sample family that they will be treated similarly, even if doing so is not optimal scientifically or operationally. Moreover, it is clear that the use of strategies that are tailored to various subgroups within the sample should include a consideration of the operational costs and benefits of their implementation. Future research is needed to further specify the potential effects of tailored strategies that can identify this balance.

APPENDIX

Traditional Design

New Design

Here's how to receive your $10 check!

If your name, address and phone number on the label below are correct, please check the "No Changes" box.

OR

If your name, phone and/or address on the label have changed, please make the necessary changes on the form below.

Then fold the postcard in half and secure the open edge with the seal on the right.

Thank you for your help!

(Fold Here)

☐ No Changes.
(Please check this box if the information below is correct.)

Affix label here

Please make necessary changes in name, phone, and address below:

Name: _____

Street, Number: _____

City, State, Zip: _____

Home : (___) _____

Cell: (___) _____

Other: (___) _____

Dear FES Study Family,

We would like to sincerely thank you for your contributions to the Family Economics Study (FES). We are very glad you are part of the FES family!

Your family's participation in this study has made it a national treasure that is used by scientists all over the world. It is important for us to have your current address and telephone number.

Please check the information on the right and make any necessary changes, then follow the instructions for returning the card to us.

When we receive your postcard, we will send you a $10.00 check to thank you for returning the post-card, even if you have no changes to report.

Thank you!

Program Director

Please follow these instructions:

1) If the information on the label is correct, please check the "No Changes" box

   Or

   If the information on the label has changed, or if we are missing your cell phone number, please make the necessary changes on the form

2) Detach the postcard by *tearing* along the GREEN LINE

3) *Fold* the postcard in half at the **BLACK LINE** and seal the edge where indicated with the sticker on the right

4) Then simply drop it in the mail

(Fold Here)

☐ No Changes
(Please check this box if the information below is correct)

Affix label here

Please make necessary changes in name, phone, and address below:

Name: _____

Street, Number: _____

City, State, Zip: _____

Home : (___) _____

Cell: (___) _____

(Tear Here)

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