MONTH-TO-MONTH RECIPIENCY TURNOVER IN THE ISDP

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The major impetus to the development of the Survey of Income and Program Participation (SIPP) was the need for more detailed and better quality income data than were available through current survey programs--most notably, the March income supplement to the Current Population Survey (CPS) (David, 1983; Ycas and Lininger, 1981). The SIPP itself has only been in the field since October of 1983, so there are not yet sufficient data for a thorough assessment of its performance. However, the precursor to the SIPP, the Income Survey Development Program (ISDP), is an available and underutilized data source offering a wealth of information to researchers with interests in a wide range of SIPP-related issues.

Background

This paper uses the 1979 Panel of the ISDP to examine a particular data quality problem concerning month-to-month turnover in the receipt of various income types. The basic question, first raised by Czajka (1982), is as follows: given six monthly observations over two consecutive survey waves (each of which covers retrospectively a 3-month period), what is the pattern of recipiency turnover in the resulting five pairs of months? Czajka's interpretation of tables prepared for another purpose by Lepkowski and Kalton (1981) was that in survey waves 1 and 2 of the 1979 panel there was "a pronounced tendency for reported program turnover to occur between waves more often than within waves--i.e., between months three and four rather than the four other pairs of months" (p. 93). Moore (1983), however, in a quantitative analysis of the Lepkowski and Kalton tables, failed to find the effect suggested by Czajka.[1/]

This discrepancy between the two investigations is attributable to differing interpretations of one of the response indicators in the tables--specifically, whether a particular code indicated "no data" (i.e., a case which could not be matched across the two waves) or "no receipt." Notwithstanding this confusion, two additional factors argued strongly for a more careful examination of the issue. First was the issue of completeness. For their work, Lepkowski and Kalton linked only the first two waves of the 1979 panel, leaving untouched waves 3, 4, and 5. A second shortcoming had to do with the quality of the linking operation itself. Lepkowski and Kalton had at their disposal only an early version of the ISDP data file, which contained numerous errors in the person identifier code crucial to the linking of survey records across waves.[2/]

Subsequent work carried out by Mathematica Policy Research, Inc., apparently corrected the problems with the person identifiers, resulting in the creation of a linked data file which had substantially more matches than the earlier file produced by the Michigan group. In addition, all five relevant waves of the 1979 Panel were included in the linking operation. The remainder of this paper analyzes and discusses tabulations derived from the later "definitive" edition of the 1979 ISDP data file to address more conclusively the issue of within-wave versus between-wave month-to-month income recipiency turnover.

Method and Results

The income types selected for analysis here were identical to the set used in the original Lepkowski and Kalton paper: the two major earned income categories (wage or salary income; self-employment or farm income), and 15 additional sources including all of the major government transfer programs (e.g., Social Security; Supplemental Security Income; unemployment compensation; veterans benefits; Aid to Families with Dependent Children (AFDC); food stamps; etc.). For these major programs, each respondent in two consecutive waves of the ISDP has six monthly observations; we use the term "month-pair" to refer to each pair of successive months. Thus, each set of linked waves includes five month-pairs, which can be designated as 1->2 and 2->3 (within survey wave n), 3->4 (the last month of wave n and the first first month of wave n+1), and 4->5 and 5->6 (within wave n+1). For each income type in each month-pair, a turnover rate ($p_{i(i+1)}$) was calculated as the number of adult sample persons[3/] who changed recipiency status with regard to income source X (i.e., who received income of type X in the first month of the pair but not in the second, or vice versa) divided by the total number of adult sample persons. The between-wave rate, p_{34}, was then compared to the average of the within-wave rates, $\bar{p} = 1/4 (p_{12} + p_{23} + p_{45} + p_{56})$. The difference between these two values, $p_{diff} = p_{34} - \bar{p}$, comprises the major variable of interest for this paper.

Table 1 summarizes the results of a simple test of significance[4/] carried out on each p_{diff} for the 17 income types across all sets of linked survey waves[5/]. The message of Table 1 is unmistakeable. There is a strong and consistent tendency toward greater turnover in recipiency between survey waves than between months within a wave. Of the 85 p_{diff} observations in Table 1, 78 are positive (i.e., $p_{34} > \bar{p}$). Sixty-nine of the differences are significantly positive, 51 are significant at the $p<.01$ level or beyond. In contrast, only one difference is significant in the opposite direction.

Almost as obvious as the general trend in Table 1 are its two apparent exceptions. Six of the seven negative difference scores (including the only significantly negative value) are concentrated in two closely related income sources--educational benefits and Basic

Educational Opportunity Grants (BEOG). The only explanation we have for these outliers follows from the fact that they involve one-time payments at the beginning of school terms. Thus, their receipt may be more easily "dateable" than other income sources, and the single payment means that accurate reporting can never produce more between-wave than within-wave turnover. Aside from these relatively weak exceptions, however, it is clear that the great majority of income sources display an exaggerated turnover rate between survey waves. The important question then becomes: Why is this the case?

Discussion

Although it is perhaps the most commonly assumed explanation, response error is by no means the only possible source of the effects observed in this paper, nor is it necessarily the most likely source. In this final section, we briefly examine four potential contributors to greater between-wave than within-wave recipiency turnover: real underlying trends, edit and imputation procedures, person mismatches in linking data from successive survey waves, and response error.

Real underlying trends: Since this investigation is without the benefit of external validating information, we cannot demonstrate conclusively that the observed results indicate "error" as opposed to reflecting accurately real underlying trends in the events being measured. Two facts, however, render the latter hypothesis untenable: 1) a change in economic conditions or eligibility rules could produce an increase in recipiency turnover at a particular point in time, but it is difficult to imagine this happening periodically for a wide range of income types over an extended period of time; 2) the staggered interviewing schedule for the 1979 ISDP Panel (see Ycas and Lininger, 1981) further reduces this likelihood, since each calendar month over the life of the panel served as the first reference month of a wave for one set of respondents, the second reference month for another set, and the third month for a third set. In other words, each reference month in a survey wave combines data from three calendar months, so that any real change effects are present only in diluted form in three reference months.

Edit and imputation procedures: Three processing procedures possibly contributed to greater recipiency turnover between waves than within waves: reformatting edits to simplify and make consistent various data fields, imputation for person nonresponse, and imputation for item nonresponse.

The only known problem with the reformatting edits is that they were carried out independently for each wave; incorrect resolutions in the name of consistency thus may have artificially reduced turnover within waves, while reporting inconsistencies between waves were ignored. Another edit decision which may have contributed to the phenomenon of less turnover within waves than between waves was the following: if at least one "yes" was reported for an income type, and/or if at least one monthly amount was a valid nonzero amount, then any blank monthly recipiency indicators were set to "yes" and any blank monthly amounts were imputed using the average of the amounts reported in other months. The obvious effect of such a procedure is to reduce the apparent amount of change within a wave. Unfortunately, these edits were not identified on the data file. As a result, the extent to which they affected the results presented here is not known, although their combined impact is likely to be small.

Another possible contributor to the observed effect is the treatment of person noninterviews within interviewed households. Because there were, in fact, few such cases (only 298 in Wave 1), an imputation procedure was developed to substitute complete person records for the otherwise missing data. The procedure used reported demographic data as matching variables in a hot-deck assignment. Since each wave's data were processed independently, it is highly unlikely that an individual who was a nonrespondent in each of two consecutive waves would receive the same imputation donor for both waves. Consequently, some spurious wave-to-wave change could occur solely as an artifact of the independent processing.

The same argument applies to the case of item nonresponse within a person's record. The presence of valid data in one wave and the absence of valid data in the next (or vice versa) suggests possible problems for between-wave analyses because the ISDP imputation system did not take previous (subsequent) reporting patterns into account. In addition, if a respondent did not provide information for a specific item on two successive waves of interviewing, it is likely that different imputation donors provided the missing data in each wave.

Mismatches: Technically, of course, although respondents do report month-to-month turnover within a survey wave, it is incorrect to refer to respondents' "reports" of between-wave turnover. These events are created by the computerized process which links together the data for specific individuals across survey waves. To the extent that people are incorrectly linked, a certain amount of artifactual turnover may appear in the month-pair which connects the two waves. Preliminary simulation work suggests that mismatching need not be extensive to produce within-wave versus between-wave differences of the magnitudes observed in Table 1. In fact, for most of the income types in this paper, a mismatch rate of 3 percent or less would produce an apparent increase in turnover quite comparable to the observed increase from within-wave month-pairs to between-wave pairs.

It is impossible after the fact to determine the impact of person mismatches on the estimates of between-wave turnover in the 1979 panel. Returning to the discrepancy between the early Lepkowski and Kalton data and the subsequent

refined file, one intriguing possibility is that although the former produced fewer matches than the latter, the matches that were completed may have been relatively error-free. If this were the case--that is, if the Michigan group somehow skimmed off the definite matches--then the appearance of heightened between-wave turnover in the later data file may simply reflect increased match errors. Clearly, evaluating the impact of match errors in turnover estimates from the SIPP will require maintaining data on the quality of the match for each person, perhaps in the form of a scale showing the number of variables which were identical across the linked waves.

Response error: Perhaps the most common explanation for the effects observed in this paper involves some form of recall bias. This was certainly Czajka's (1982) assumption. Presumably, a gestalt-like process operates in response to imperfect recall, leading respondents to report receipt for the entire 3-month period of a single wave as having been more stable than it really was. Such a process would work in two ways to produce more reports of between-wave than within-wave turnover: first, by reducing the number of within-wave turnover episodes (see Example 1); and second, by shifting the occurrence of turnover episodes to the between-wave period (Example 2).

	wave n			wave n+1		
Month:	1	2	3	4	5	6
Example 1						
actual receipt:	yes	no	yes	no	yes	no
reported receipt:	yes	yes	yes	no	no	no
Example 2						
actual receipt:	yes	yes	yes	yes	no	no
reported receipt:	yes	yes	yes	no	no	no

Although it is impossible with the available data to evaluate these notions directly, other research has demonstrated effects which appear to be related to the processes hypothesized to be at work here. Goudreau, Oberheu, and Vaughan (1984) report two results of interest from a survey of known AFDC recipients. First, those who failed to report receipt were likely to have received AFDC income for only part of the reference period of the survey. And second, the most common error in reporting income amounts was the tendency to report "the most recent payment for all three months of the reference period when payments actually varied" (p. 184).

A second, related response error possibility can be examined using the present data. According to this explanation, misreports of the type described above, while perhaps representing a general human tendency, are even more likely to occur when the respondent and the subject of the report are not the same person, and especially when different respondents provide the data for two consecutive survey waves. Table 2 summarizes the data regarding the role of proxy response in general, and changing respondents specifically, on elevated between-wave turnover. The results do not present a simple picture, but there is no evidence that self-response in consecutive waves erases the general effect observed in this paper. Note that with only one exception, all differences in column (c) are positive; that is, between-wave turnover is consistently greater than within-wave turnover even when attention is restricted to the constant self-response group.

Nor, in fact, is there consistent support for the weaker argument that self-response might at least reduce between-wave/within-wave turnover discrepancies. As shown in columns (j) and (m), the weight of the evidence is in the opposite direction. Only for the two earned income categories does proxy involvement strongly and consistently produce greater differences as compared to constant self-response.

Why the two general income types produce such disparate results is not clear. A plausible partial explanation--at least for the both-self/mixed-self-and-proxy comparison--is that a true change in recipiency for earned income also changes a person's availability for interview. For example, those who are not employed may be more readily available to be interviewed for self than those who are employed. Receipt of unearned income, on the other hand, is not associated with with the likelihood of finding a person at home; thus, recipiency turnover for unearned income is not associated with a corresponding change in response status.

Conclusion

This paper has demonstrated the existence of some data quality problems in the 1979 Panel of the ISDP, at least when data are examined from more than one survey wave at a time. We have as yet no definitive explanation for these problems, but only a list of possible causes: edit, imputation, and processing procedures; matching difficulties; and response errors. It is likely, of course, that all contributed to the observed effects.

Although modelled in many ways on the 1979 Panel, the SIPP has adopted several modifications which may reduce the problem of heightened turnover in income recipiency between survey waves. First, the SIPP questionnaire includes procedures by which information brought forward from the previous interview can be verified and corrected, if necessary, at the time of interview. The identification and correction of incorrect information was not systematically addressed in the ISDP. Second, the SIPP exercises much tighter control on the sample than did the ISDP, through an improved control numbering system, and improved check-in procedures in Census Regional Offices. These new procedures should help keep mismatches to a minimum in linking consecutive survey waves.

In the future, as SIPP data become available we will monitor them closely for evidence of the type of problem we have demonstrated here. In addition, we will seek to ensure that data which might help pinpoint the cause of the problem (for example, match certainty indicators and edit and imputation flags) are systematically gathered and maintained. We are also planning a more active program of investigation--a record check study matching selected SIPP income receipt and amount data with existing administrative records. Such a study will contribute greatly to our understanding of the quality of SIPP responses, and will provide valuable direction to the development of any ameliorative actions to improve the quality of the SIPP.

Technical Note on Significance Testing Procedures: The following assumptions guided procedures for testing the significance of the between-wave versus within-wave difference in turnover rates:

Suppose five observations have common variance σ^2 and common correlation ρ. Then

the variance of the average of four

$$= \frac{4\sigma^2 + 12\rho\sigma^2}{16} = \frac{\sigma^2}{4}(1+3\rho)$$

and

the variance of the average of four minus the fifth

$$= \sigma^2 + \sigma^2/4\ (1+3\rho)\ 2\rho\sigma^2$$

$$= (5/4)\sigma^2(1-\rho).$$

In this illustrative example, the effect of positive covariance among the estimates is to reduce the variance below the sum of the variances of the two components. For the tests in Table 1, the variance of the difference was estimated by

$$Var_{diff} = 1/N\ [1/16\ (p_{12}(1-p_{12}) + p_{23}(1-p_{23}) + p_{45}(1-p_{45}) + p_{56}(1-p_{56})) + p_{34}(1-p_{34})]$$

where N = the number of adult sample persons in the two consecutive waves and $p_{i(i+1)}$ = the turnover rate for month-pair i and i+1

which ignores all covariances, and thus is likely to be conservative as compared to the illustrative example.

FOOTNOTES

1/ In fact, if the analysis indicated any consistent tendency, it was quite the opposite of that proposed by Czajka--less turnover in the month-pair which linked the two survey waves than in those within a single wave.

2/ Some suggestive evidence on the extent of this problem can be seen in the fact that about 20 percent of the entries in the Lepkowski and Kalton tables are of the "no match" variety, with data available for only one of the two waves. In fact, it was the frequency of this outcome which led Czajka to believe that the supposedly "no match" cases were actually "no receipt," since the code "occurs too often to reflect simply a failure to match records between waves" (Czajka, personal communication, 1983).

3/ Excluded from the tallies are the special subsamples of persons selected from lists of program participants, and persons who were not adult household members during both of the consecutive survey waves. Sample weights were not used for the tallies, and all analyses used the unweighted survey data.

4/ See the Technical Note regarding the procedures for significance testing.

5/ An explanation is in order regarding the last column of Table 1. In the design of the 1979 Panel, a randomly selected one-third of the sample was not administered a wave 4 interview, but skipped directly from wave 3 to wave 5. Thus, the first two sets of linked survey waves--1&2 and 2&3--contain the full respondent sample, sets 3&4 and 4&5 contain two-thirds of the sample, and set 3&5 contains one-third of the sample.

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TABLE 1: BETWEEN-WAVE (p_{34}) AND AVERAGE WITHIN-WAVE ($\bar{p}$) MONTH-TO-MONTH RECIPIENCY TURNOVER RATES, AND ANALYSIS OF DIFFERENCE OF RATES (p_{diff}), FOR SEVENTEEN INCOME SOURCES IN FIVE SETS OF LINKED SURVEY WAVES (1979 PANEL, ISDP)

	Linked Survey Waves														
	1 & 2			2 & 3			3 & 4			4 & 5			3 & 5[1]		
Monthly Turnover Rate:	p_{34}	$\bar{p}$	p_{diff}[2]	p_{34}	$\bar{p}$	p_{diff}[2]	p_{34}	$\bar{p}$	p_{diff}[2]	p_{34}	$\bar{p}$	p_{diff}[2]	p_{34}	$\bar{p}$	p_{diff}[2]
Income Type:															
Wage and salary	8.98%	2.99	**	10.10	3.41	**	10.22	3.58	**	7.51	3.01	**	9.03	1.51	**
Self employment and farm	2.99	0.22	**	3.08	0.29	**	2.85	0.26	**	1.99	0.34	**	3.18	0.32	**
Social Security	1.21	0.13	**	1.05	0.11	**	0.98	0.13	**	0.61	0.15	**	1.18	0.08	**
Federal SSI	0.46	0.04	**	0.36	0.04	**	0.20	0.04	*	0.16	0.03	*	0.26	0.02	*
Unemployment compensation	1.12	0.67	**	0.99	0.56	**	0.89	0.59	*	0.97	0.73	*	0.77	0.58	
Veterans benefits	0.31	0.03	**	0.24	0.03	**	0.16	0.02	*	0.16	0.03	*	0.12	0.02	
Workmans compensation	0.47	0.23	**	0.23	0.15		0.26	0.15		0.14	0.15	-	0.26	0.18	
AFDC	0.33	0.07	**	0.34	0.08	**	0.26	0.08	*	0.34	0.06	**	0.46	0.07	**
Child support	0.37	0.14	**	0.35	0.13	**	0.34	0.14	*	0.25	0.13	*	0.26	0.09	*
Employer or union pension	0.27	0.04	**	0.23	0.04	**	0.27	0.05	**	0.20	0.04	*	0.29	0.02	*
Educational benefits	0.25	0.27	-	0.149	0.153	-	0.20	0.14		0.16	0.17	-	0.07	0.16	-
BEOG	0.48	0.21	**	0.09	0.16	⊖	0.30	0.15	*	0.42	0.35		0.24	0.25	-
Food stamps	1.47	0.48	**	1.23	0.34	**	1.21	0.36	**	0.98	0.35	**	1.13	0.28	**
Rental income	0.56	0.04	**	0.76	0.04	**	0.29	0.07	**	0.44	0.08	**	0.14	0.01	*
Assist. from relatives, friends	0.75	0.37	**	0.63	0.15	**	0.40	0.09	**	0.32	0.13	*	0.53	0.15	*
Lump sum payments	1.38	0.95	**	1.48	1.01	**	1.25	0.80	**	1.00	0.93		1.16	1.08	
Incidental or casual earnings	0.74	0.31	**	0.97	0.38	**	1.13	0.43	**	0.93	0.45	**	0.94	0.45	*
Number of Cases	13,157			12,751			8,568			8,639			4,154		

[1] See text footnote 5.

[2] See the Technical Note for a description of significance testing procedures, results of which are symbolized as follows:

**	$p_{34} > \bar{p}$, $z > 3.3$	($p < .01$)
*	$p_{34} > \bar{p}$, $2.0 < z < 3.3$	($p < .05$)
[blank]	$p_{34} > \bar{p}$, $0 < z < 2.0$	(n.s.)
-	$p_{34} < \bar{p}$, $-2.0 < z < 0$	(n.s.)
⊖	$p_{34} < \bar{p}$, $z < -2.0$	($p < .05$)

TABLE 2: AVERAGE BETWEEN-WAVE (p_{34}) AND WITHIN-WAVE ($\bar{p}$) MONTHLY TURNOVER RATES, AND AVERAGE DIFFERENCE OF RATES (p_{diff}), BY RESPONDENT PATTERN IN CONSECUTIVE WAVES; AND COMPARISON OF DIFFERENCES FOR PROXY SITUATIONS VERSUS CONSISTENT SELF RESPONSE (1979 PANEL, ISDP)

	Average[1] Monthly Turnover Rates (p_{34} and $\bar{p}$) and Differences[2] (p_{diff})									Comparison[2] of p_{diff} for for Proxy Versus Self-Response Situations			
	Respondent Pattern in Consecutive Waves									p_{diff} (s-p and p-s) minus p_{diff} (s-s)		p_{diff} (p-p) minus p_{diff} (s-s)	
	Self-Self			Self-Proxy & Proxy-Self			Proxy-Proxy						
Income Type	Average p_{34} (a)	Average $\bar{p}$ (b)	Average p_{diff} (c)	Average p_{34} (d)	Average $\bar{p}$ (e)	Average p_{diff} (f)	Average p_{34} (g)	Average $\bar{p}$ (h)	Average p_{diff} (i)	Average effect (j)	Number of + differences (out of 5)[3] (k)	Average effect (m)	Number of + differences (out of 5)[3] (n)
Wage and salary	6.54	2.88	3.66	15.52	3.87	11.66	13.38	3.83	9.54	8.00	5	5.88	5
Self employment and farm	2.53	0.31	2.22	3.57	0.29	3.28	3.18	0.19	3.00	1.06	5	0.78	5
Social Security	1.03	0.12	0.90	1.22	0.12	1.10	0.73	0.09	0.64	0.20	4	-0.27	0
Federal SSI	0.32	0.04	0.29	0.24	0.03	0.21	0.20	0.02	0.17	-0.07	1	-0.11	1
Unemployment compensation	0.93	0.61	0.32	1.20	0.81	0.40	0.75	0.51	0.24	0.08	3	-0.08	2
Veterans benefits	0.20	0.03	0.18	0.21	0.03	0.19	0.13	0.00	0.13	0.01	4	-0.05	1
Workmans compensation	0.29	0.18	0.12	0.26	0.20	0.05	0.19	0.12	0.06	-0.07	2	-0.06	2
AFDC	0.41	0.08	0.33	0.30	0.06	0.24	0.11	0.01	0.09	-0.09	1	-0.25	0
Child support	0.43	0.18	0.25	0.14	0.02	0.12	0.04	0.01	0.03	-0.12	0	-0.21	0
Employer or union pension	0.31	0.03	0.27	0.23	0.01	0.22	0.05	0.04	0.01	-0.04	2	-0.26	0
Educational benefits	0.13	0.15	-0.03	0.31	0.28	0.02	0.19	0.17	0.02	0.05	3	0.04	3
BEOG	0.18	0.14	0.05	0.47	0.47	0.00	0.60	0.35	0.24	-0.05	1	0.19	5
Food stamps	1.37	0.45	0.92	1.02	0.25	0.76	0.73	0.12	0.61	-0.16	1	-0.31	1
Rental income	0.46	0.05	0.41	0.46	0.04	0.42	0.32	0.01	0.31	0.01	3	-0.10	1
Assist. from relatives, friends	0.65	0.22	0.43	0.39	0.15	0.24	0.16	0.03	0.13	-0.18	1	-0.29	0
Lump sum payments	1.41	1.10	0.32	1.02	0.68	0.34	0.83	0.65	0.19	0.02	3	-0.13	1
Incidental or casual earnings	0.93	0.41	0.52	0.91	0.33	0.58	1.00	0.38	0.61	0.06	2	0.09	4

[1]/Average rates are computed as the sum of the rates derived from each set of consecutive waves divided by five.

[2]/Minor discrepancies in some differences are due to rounding.

[3]/Columns (k) and (n) provide evidence on the consistency of proxy involvement effects across the five sets of consecutive waves. Entries indicate the number of times the p_{diff} for the proxy situation exceeds the p_{diff} for the self-self pattern.