ESTIMATING DURATION OF FOOD STAMP SPELLS FROM THE SIPP[1]

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1. Introduction

Since its inception in 1984, the SIPP has been an important component of the federal statistical system (Short 1985). The SIPP is a continuing panel survey of the U.S. civilian noninstitutionalized population. A new panel starts each year, and members of original sample households are followed for seven or eight interviews conducted every four months. Bureau of the Census interviewers collect data at each wave on a substantial number of income sources. This report is limited to one type of income, that received from the Food Stamps program.

The 1987 panel covered seven waves of data collection, starting with an initial panel of approximately 12,000 households. The 1987 panel thus provides 28 consecutive monthly reports on income for sample members who remained in the panel for all seven waves. At the conclusion of each panel, a longitudinal file linking individual interviews across waves is created. Persons who responded at all panel waves are referred to as full panel respondents, and a weight is provided for these persons to allow longitudinal analysis. These "full panel respondents" have positive longitudinal weights, and are the subjects used in this investigation.

The focus of this research is on the estimation of the duration of spells of receipt of Food Stamps income over the 28 month period covered by the 1987 panel, and on how those estimated spell lengths are affected by a particular type of nonsampling error, the "seam effect" (Singh, Weidman, and Shapiro 1989; Kalton, Hill, and Miller 1990; Kasprzyk 1988). This effect arises because of the nature of the data collection mechanism. Sample individuals are interviewed every four months, reporting on receipt of income across the previous four month period. Changes in recipiency and income amounts between consecutive months have been observed to be much more frequent when the consecutive months are in different panel waves than when they are in the same panel wave. For example, changes in Food Stamp recipiency occur much more often between months 4 and 5, months collected in two different waves, than between months 1 and 2, months 2 and 3, or months 3 and 4. The seam effect occurs not only in the SIPP (Jabine, King, and Petroni 1990) but in many other panel surveys as well: in the Panel Study of Income Dynamics (PSID) (Coder and Ruggles 1988) and the Canadian Labour Market Activity Survey (LMAS) (Murray, Michaud, Egan, and Lemaitre 1991).

Young (1988) suggested three possible causes of the seam effect: constant wave response, changing interpretation of the question, and under-reporting. Constant wave response occurs when subjects accurately report information for the most recent month of recall, but then project that information back to the other prior recall months. Changing question interpretation occurs when respondents alter their perception of what is being asked from one wave to the next. Consequently, the response between waves may changes even though the phenomenon of interest (*e.g.*, Food Stamps recipiency) has not. Finally, under-reporting preceding or following a wave of accurate reporting may lead to changes between waves.

These three potential causes of the seam effect are quite similar in their outcome, and data are lacking in the SIPP to distinguish between them. Therefore, we must treat reported status changes as though they were accurate. Under this assumption, the "changing interpretation" and the "under-reporting" mechanisms are essentially just types of "constant wave response." Thus, regardless of the causal mechanism, the data need to be adjusted in such a way as to shift status changes from seam to non-seam months. Thus, we do not attempt to identify the causal mechanism in this research, but we do exploit certain identities under the "constant wave response" hypothesis which provide adjusted spell length estimates that are not only smoothed to reduce the seam effect but also are not biased upwards (*i..e.*, toward longer spell lengths) or downwards.

2. Kaplan-Meier Estimation

One method for spell length estimation is the Kaplan-Meier conditional probability technique (Ruggles 1989). The method uses data from spells which are observed to start during the panel period, including in estimation uncensored (*i.e.* spells starting and stopping in the panel period) and right censored (*i.e.,*, spells starting but not stopping in the panel period) spells. The method excludes initially censored (*i.e.*, spells in existence at the start of the panel) and doubly censored (*i.e.*, spells in existence at the start of the panel and lasting the entire panel length) spells. The SIPP data on Food Stamps allows the estimation of a discrete time hazard function estimated at each successive time point. The distribution of spell length and the survival function are derived from the estimated hazard function.

The hazard function **h(t)** in the present context is the probability that a spell of Food Stamps recipiency ends at a month t, given that the spell lasts t months or more. The hazard function is estimated by the proportion of spells that end at month t among all spells known to have lasted t months or more. The estimation includes both uncensored spells and right censored spells that last at least $(t+1)$ months. Right censored spells lasting t months cannot be used since it cannot be determined if they lasted t months or longer.

The discrete time Kaplan-Meier estimate for the hazard function **h(t)** is given by

$$\hat{h}(t) = \frac{d_t}{\sum_{x=t}^{\infty} d_x + \sum_{x=t+1}^{\infty} c_x}$$

where d_t denotes the number of spells ending at time t, and c_t denotes the number of censored spells at time t. The survival function **S(t)** is the probability that a spell will last for more than t months, and is estimated from $\hat{\mathbf{h}}(\mathbf{t})$ as

$$\hat{S}(t) = \prod_{x=1}^{t} [1 - \hat{h}(x)]$$

The SIPP data are weighted to compensate for unequal probabilities of selection, nonresponse, and other design features. The estimation procedure for the hazard function was suitably modified to allow estimates to be made that incorporated these weights in the procedure.

3. The Food Stamp Program

The investigation reported here for Food Stamps was conducted for a large number of other programs as well. The Food Stamp program was selected for presentation here because it is one of the largest programs reported in the SIPP. Thus there are a large number of both short and long spells available for analysis. Table 1 shows the number and distributions of various types of Food Stamp spells available in the 1987 SIPP. Nearly one-quarter of the persons having Food Stamp spells reported two or more such spells during the panel. Further, there are 3,501 Food Stamp spells reported in the 1987 SIPP, 1,768 of which are uncensored or right censored and therefore used in the Kaplan-Meier hazard rate estimation method.

The seam effect on spell starts and stops is readily apparent in Table 1. Nearly one-half of the starts (43.8%) occur in the most recent month of recall, while more than one-half of the stops (53.9%) occur in the most recent month.

Table 1
Availability of Data for Food Stamps Spells in the 1987 SIPP Panel

	N	%
Persons Having:		
Only One spell	2018	75.8
Two spells	491	18.4
Three spells	128	4.8
Four spells	18	0.7
Five or more spells	9	0.3
TOTAL	2664	100
Spells which are:		
Uncensored	1052	30.0
Right censored	716	20.5
Initially censored	930	26.6
Doubly censored	803	22.9
TOTAL	3501	100.0
Spell starts at:		
Recall month one	342	19.3
Recall month two	371	21.0
Recall month three	280	15.8
Recall month four	775	43.8
TOTAL	1768	100.0
Spell stops at:		
Recall month one	340	17.2
Recall month two	306	15.4
Recall month three	267	13.5
Recall month four	1069	53.9
TOTAL	1982	100.0

Table 2 presents the unadjusted Kaplan-Meier estimates for the hazard and survival functions for Food Stamp spell length. There is an obvious "heaping effect" in the hazard function for months four and eight. These heaps are less apparent in the survival function, but the large jump in the survival function between months three and four is of concern. The survival function is a multiplicative function of hazard rates, and thus a large discrepancy in even one hazard rate may have unfortunate effects on the remainder of the survival function.

The seam effect on unadjusted spell length estimates in Table 2 is not as large as its effect on the number of starts and stops. This is due to both starts and stops occurring on the seam. If only the spell stop or start occurs on a seam, then a spell which may be distorted by the seam effect will not appear as an obvious seam problem in the estimated spell length distribution. The complexities of the seam effect on spell length estimates from this consideration alone strongly indicates that an adjustment procedure to reduce the distortions of the seam effect is critical for proper examination of spell lengths in the SIPP.

4. A Seam Effect Adjustment

Considerable effort has been devoted to describing the seam effect (Petroni, Carmondy, and Huggins 1989), but little attention has been given to the practical effect on results obtained from a standard analysis of spell length data, such as that presented in Table 2. The seam is widely used as a covariate in multivariate hazard models Long 1990a; Long 1990b; McBride and Swartz 1990), but it has not yet been incorporated into simple spell length estimates.

A standard smoothing function, or the use of parametric survival functions, would compensate spell length estimates for the seam effect. However, neither of these approaches is without serious drawbacks. Further, there is some understanding of the mechanism through which the seam effect distorts the hazard and the survival functions.

The constant wave response model underlying the adjustment to be proposed here assumes that changes reported on or off Food Stamps at the 1st, 2nd, or 3rd recall month are accurate, but that some portion of the reports at 4th month should have been reported at another month. The adjustment procedure takes part of the starts and stops reported at the 4th month and redistributes them across other months in the same wave.

Let $R(i)$ denote the proportion of reported spell starts (or stops) at the ith recall month, and let $E(i)$ denote the expected proportion of spell starts (or stops) at the ith recall month. Spell starts (or stops) are defined to be the first month on spell (off spell) after at least one month off spell (on spell). Therefore, there can be no starts or stops in the first month of the panel. Thus there are 27 total months available for spell starts (or stops). The expected proportion of starts (or stops) at the 1st, 2nd, and 3rd recall month is 7/27, since there are 7 out of these 27 months which are 1st, 2nd, or 3rd recall months. Thus, $E(1) = E(2) = E(3) = 7/27$, while $E(4) = 6/27$.

Because of the rotation system under which the SIPP data is collected, both $R(i)$ and $E(i)$ will be robust to seasonal variation. In addition, an brief analysis not shown here shows that $R(i)$ does not vary greatly from wave to wave.

Table 2
Unadjusted and Adjusted Kaplan-Meier Estimates For Food Stamp Program Spell Lengths in the 1987 SIPP Panel

Month	Survival Function S(t)		Hazard Function h(t)	
	Unadj.	Adjusted	Unadj.	Adjusted
0	100.0	100.0		
1	87.4	88.5	12.6	11.5
2	79.8	81.2	8.7	8.2
3	72.6	72.4	9.0	10.9
4	57.6	62.8	20.7	13.3
5	51.7	54.7	10.2	12.8
6	49.2	50.4	4.9	8.0
7	45.9	46.4	6.6	7.8
8	42.5	42.8	7.4	7.8
9	40.4	39.6	5.1	7.5
10	36.9	36.3	8.7	8.3
11	34.8	34.1	5.7	6.0
12	32.6	32.0	6.3	6.3

While some nonsampling error may exist in those reporting spell starts (or stops) at the 1st, 2nd, or 3rd months, these errors cannot be attributed to the seam. Under the constant wave response model, the seam effect acts upon both starts (and stops) by telescoping events which should have been reported in one of the first three months of recall to the fourth month instead.

Let $A(i)$ denote the actual or true proportion of spell starts (or stops) at the ith month of recall. $A(i)$ is "unobserved" in the sense that the observed start month may not necessarily be the same as the actual start month. (Spells which are completely unobserved are not recaptured by this approach.) Consider a transition matrix of π_{ji} which represent the conditional probability that a start (or stop) actually occurring in recall month i is reported in month j. That is, let $\pi_{ji} \equiv Pr\{R(j)|A(i)\}$. Then, under the constant wave response model, the transition matrix of these conditional probabilities is given by

$$\Pi = \begin{bmatrix} (1-\pi_{14}) & 0 & 0 & \pi_{14} \\ 0 & (1-\pi_{24}) & 0 & \pi_{24} \\ 0 & 0 & (1-\pi_{34}) & \pi_{34} \\ 0 & 0 & 0 & 1 \end{bmatrix}$$

Weidman (1986) proposes a similar matrix and its value in assessing spell lengths. We extend that code here with direct application of Kaplan-Meier estimation under the "constant wave response" model for the seam effect. Suppose that we estimate the unknown $A(i)$ using the expected probabilities of reporting, $E(i)$. Then, if the assumption of equal probabilities of spell starts (or stops) across recall months is correct, and given that spell starts cannot be observed in the first month of the panel, it is possible to solve for the unknown π_{ji} in **Π**.

For any wave of the panel, let **A** denote the vector of actual probabilities, **E** the vector of expected probabilities, and **R** the vector of reported probabilities of a spell start (or stop). That is, $\hat{A}^T = E^T = (7/27,\ 7/27,\ 7/27,\ 6/27)$ and $R^T = (R(1),R(2),R(3),R(4))$. Observe that the probability of an actual start (or stop) in month i is given as $R(j) = \sum_{i=1}^{4} \pi_{ji} \cdot \hat{A}(i)$. That is, $R = E^T\,\Pi$. Solving for the π_{ji} in **Π** leads to the following result:

$$\pi_{14} = 1 - \frac{R(1)}{E(1)},\quad \pi_{24} = 1 - \frac{R(2)}{E(2)},\ \text{and}$$
$$\pi_{34} = 1 - \frac{R(3)}{E(3)}$$

Alternatively, the conditional distribution of an actual spell start (or stop) in month j given a reported spell start (or stop) in month i may be expressed as

$$\gamma_{ji} = Pr(A(i)\,|R(j)) = \frac{E(i)\pi_{ij}}{R(j)}$$

with corresponding matrix of conditional probabilities

$$\Gamma = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ \frac{E(1)-R(1)}{R(4)} & \frac{E(2)-R(2)}{R(4)} & \frac{E(3)-R(3)}{R(4)} & \frac{E(4)}{R(4)} \end{bmatrix}$$

These conditional probabilities are consistent with the assumption that observations reported off the seam (not at recall month 4) were reported accurately.

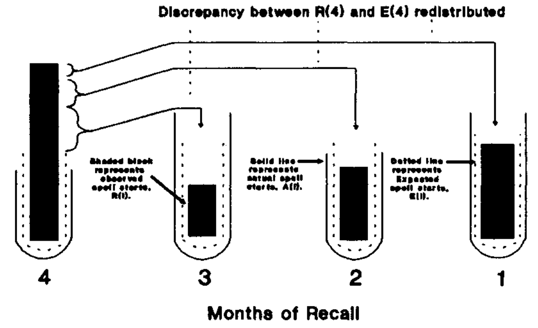
This model can be applied to participant weights to redistribute spell starts (or stops) from the 4th month of recall to other months in the wave. For example, a reported spell start at month 5 (recall of 4 months) is broken into 4 spell starts with the following weights:

Spell Start Month	Weight
5	$\omega\gamma_{44}$
6	$\omega\gamma_{43}$
7	$\omega\gamma_{42}$
8	$\omega\gamma_{41}$

A less rigorous presentation of the technique is shown in Figure 1 for spell starts. The dotted line "buckets" represent the expected spell starts, $E(i)$. The arrow points to $E(1)$, the expected proportion of spells starting in month one. $E(4)$ is slightly smaller than $E(1)$, $E(2)$, and $E(3)$, because spell starts cannot be observed in the first month of the panel, and the first month of the panel corresponds to the fourth month of recall. The solid line "bucket" represents the actual proportion of spell starts, $A(i)$, occurring in a given month. The pointer is on $A(2)$. The actual proportions and the expected proportions are portrayed as almost identical because of the large sample size for most of the programs studied here, and the sampling error for these proportions should be very small. The shaded block in the "buckets" represents the observed values, $R(i)$.

$R(4)$ is so large that the $E(4)$ "bucket" is overflowing. Since the $E(1)$, $E(2)$, and $E(3)$ buckets were not adequately filled by $R(1)$, $R(2)$, and $R(3)$, the weights for the observations filling $E(4)$ need to be redistributed to the other "buckets." For the Food Stamp program, 19% of spell starts were observed in the first month of recall, but the expected number of spell starts in the first month of recall is 7/27.

Figure One:
Application of the Seam Adjustment



Thus $R(1) = 0.19 < E(1) = 0.26$. We also see that $R(4) = 0.44$. Suppose that an observed spell start at month of recall 4 has a weight for the individual with the spell start of 4,400. This weighted observation would be replaced by four new observations, starting at each of the four possible months of recall. The observation at the 1st month of recall will have a weight of 700 reflecting the contribution of the seam case towards the discrepancy between the expected 0.26 and the observed 0.19.

5. Applying the Model to Survival Analysis

In order to apply this model to survival data, the adjustment must be made to both spell start and stop values before spell lengths (stop month minus start month) are calculated. It is assumed that the probability a participant's spell begins on the seam is independent from the probability that the spell ends on the seam, intuitively a very poor assumption. However, a comparison of spell starts and spell stops for uncensored spells suggests that the assumption may be adequate. A more thorough investigation of this assumption (Kalton, Miller, and Lepkowski 1992) suggested that the assumption is adequate not only for the Food Stamp program, but for all other program spells studied as well.

The adjustment redistributes spell starts and stops on the seam across four months. Spell length is a function of spell starts and stops. Under the assumption of independence between starts and stops across months, participants who have both starts and stops on the seam will actually be distributed across 16 observations. This represents combinations of possible true start and true stop times. Participants who have a start on the seam, but not a stop on the seam (or vice versa) will be distributed across four observations. Participants with neither a stop nor a start on the seam will remain as a single observation.

It may be a concern that this procedure could create some zero or negative valued spell lengths. However, this is impossible, with the exception of one case. If both the spell start and the spell stop are on the seam they must be in different waves. Since the seam adjustment only creates new starts and stops in the same waves as the reported waves, positive spell lengths are guaranteed. Similarly if the start is off the seam, but the stop is not, the seam start and stop will be in different waves. The spell start and stop are only in the same wave if the spell starts on the seam and lasts 3 months or less. Nonpositive spells created by redistributing the start times of these spells are treated as spells of length one.

The application of this model is shown in comparison to the unadjusted spell length estimates in Table 2. While a small degree of heaping still exists in the hazard function at wave 4, the heaping phenomenon observed for the unadjusted Kaplan-Meier estimates is strikingly reduced. Although the unadjusted distribution is not as smooth as the adjusted distribution, it is not strongly biased toward longer or shorter spells. This may be attributed to the fact that spell stops are approximately as likely to appear on the seam as spell stops. Spell stops on the seam underestimate the spell length, whereas, spell starts on the seam overestimate the spell length. Returning again to Table 1 we see that spell stops on the seam are somewhat more probable than spell starts on the seam. It is not surprising therefore that the adjusted model estimates 62.8% of spells will last over four months compared to an estimate of 57.6% in the unadjusted model.

Torelli and Trivellato (1990) to suggest a heaping model to account for the seam effect. Their model applies the heaping effect to the spell lengths rather than the spell starts and spell stops which are the root of the problem. While the model that they propose may do an adequate job of smoothing the distribution function, the mechanism which caused the heaping is ignored to some extent. Those interested in correcting the seam problem at the design level in future studies may wish to examine Murray *et al.* (1991), who describe steps taken in the Canadian LMAS study to reduce the seam problem.

6. Conclusions

This research has shown that estimates of spell length data are not robust to non-sampling error, but that simple adjustments may be made to correct for this problem. Measurement definition and population choices provide a trickier situation for researchers. It is recommended that these choices be considered carefully before proceeding with an analysis of spell length data.

An effort has been made in this research to consider some of the simplest non-parametric estimates of spell length data very thoroughly. The findings here should be of concern to researchers plunging more immediately into multivariate hazard models. If these differences exist at the univariate level with very large sample sizes, they may well lead to more serious difficulties with more complex estimators.

Footnotes

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