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Assessing the Bias Associated with Alternative Contact Strategies in Telephone Time-Use Surveys

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Abstract

In most telephone time-use surveys, respondents are called on one day and asked to report on their activities during the previous day. Given that most respondents are not available on their initial calling day, this feature of telephone time-use surveys introduces the possibility that the probability of interviewing the respondent about a given reference day is correlated with the activities on that reference day. Further, noncontact bias is a more important consideration for time-use surveys than for other surveys, because time-use surveys cannot accept proxy responses. Therefore, it is essential that telephone time-use surveys have a strategy for making subsequent attempts to contact respondents. A contact strategy specifies the contact schedule and the field period. Previous literature has identified two schedules for making these subsequent attempts: a convenient-day schedule and a designated-day schedule. Most of these articles recommend the designated-day schedule, but there is little evidence to support this viewpoint. In this paper, I use computer simulations to examine the bias associated with the convenient-day schedule and three variations of the designated-day schedule. My results support using a designated-day schedule, and validate the recommendations of the previous literature. The convenient-day schedule introduces systematic bias: time spent in activities done away from home tends to be overestimated. More importantly, estimates generated using the convenient-day schedule are sensitive to the variance of the contact probability. In contrast a designated-day-with-postponement schedule generates very little bias, and is robust to a wide range of assumptions about the pattern of activities across days of the week.

Key Words: Telephone time-use surveys; Contact strategies; Bias; Computer simulations.

1. Introduction

Telephone time-use surveys present a unique data collection challenge because respondents are called on one day and asked to report on their activities during the previous day. The challenge arises because most respondents--about 75 percent (Kalton, page 95)--are not contacted on their original calling day, necessitating additional contact attempts. In most surveys, it does not matter when these additional attempts are made, because respondents are being asked to report about a fixed reference period. And in most surveys recall does not suffer too much if respondents are contacted several days after the initial calling day. But in time-use surveys, respondents' ability to recall their activities on a given day falls off dramatically after a day or so, which means that the respondent must be assigned a new reference day if no contact is made on the initial calling day. As we will see below, this scenario introduces the possibility that the probability of interviewing the respondent about a given reference day is correlated with the activities on that reference day. Therefore it is essential that these surveys have a strategy for making subsequent attempts to contact respondents that does not introduce bias.

Contact Strategies

What is a contact strategy? A contact strategy is comprised of a contact schedule and a field period. The contact schedule specifies which days of the week that contact attempts will be made, and the field period specifies the maximum number of weeks attempts will be made.

Contact schedules fall into two main categories: designated-day schedules and convenient-day schedules. Both types of schedule randomly assign each respondent to an initial calling day. If the respondent is contacted on the initial calling day, the interviewer attempts to collect information about the reference day, which is the day before the calling day. It is for subsequent contact attempts that these schedules differ.

Under a designated-day schedule, there are two approaches to making subsequent contact attempts. The interviewer could call the respondent on a later date, and ask the respondent to report activities for the original reference day. This approach maintains the original reference day, but extends the recall period. Harvey (1993, page 210) recommends allowing a recall period of no more than two days. The second approach is to postpone the interview and assign the respondent to a new reference day. Kalton (1985, page 95) recommends postponing the interview by exactly one week, so that the new reference day is the same day of the week as the original reference day.

These approaches are not mutually exclusive. For example, Statistics Canada's designated-day schedule allows interviewers to call respondents up to two days after the reference day (Statistics Canada 1999, page 17), and to postpone the interview by one week if the respondent cannot be reached after the second day of attempts. The interview can be postponed no more than three times (Statistics Canada, telephone conversation with survey methodologist at). To illustrate, if the initial reference day is Monday the 1st, the respondent is called on Tuesday the 2nd and, if necessary, on Wednesday the 3rd. If no interview is obtained on either of these days, the respondent is called on Tuesday the 9th and, if necessary, on Wednesday the 10th, and asked to report on activities done on Monday the 8th. This process continues until the respondent is interviewed, refuses, or until four weeks pass.

The convenient-day schedule does not maintain the designated reference day. If no contact is made, then the interviewer calls on the next day and each subsequent day until the respondent is contacted. Once contact is made, the interviewer attempts to collect the interview or, if the respondent is unwilling to complete the interview at that time, reschedule it to a day that is convenient for the respondent. The reference day is always the day prior to the interview. It is worth noting that because respondents are not likely to schedule interviews on busy days, allowing them to choose their interview day is really no different than the interviewer proposing consecutive days (or calling on consecutive days) until the potential respondent accepts. Hence, one may think of the convenient-day schedule as being functionally identical to an every-day contact attempt schedule.

A variant of the convenient-day schedule described above was used in the 1992-1994 Environmental Protection Agency (EPA) Time Diary Study conducted by the University of Maryland. Respondents were not assigned to an initial calling day. Instead, they were assigned to either the weekday or weekend sample. For example, those who were assigned to the weekend sample could be called on Sunday (to report about Saturday) or Monday (to report about Sunday). Interviewers were instructed to make at least 20 call attempts before finalizing the case as noncompleted (Triplett 1995, pages 3 and 4).

Most methodological papers argue in favor of using a designated-day schedule (Kinsley and O'Donnell 1983; Kalton 1985, page 95; Lyberg 1989; and Harvey 1993, page 209; and Harvey 1999, page 23). For example, Lyberg (1989, page 219) argues that the convenient-day schedule may introduce bias because "the respondent may choose a day when he/she is not busy, a day he/she is not engaged in socially unacceptable behavior, a day he/she thinks is representative, etc." Kinsley and O'Donnell (1983, page 78) argue that the convenient-day schedule could exaggerate the number of events taking place outside the home, because the respondent is more likely to be interviewed on a day that immediately follows a day that he or she was out of the house.

Two of these studies directly compare the designated-day and convenient-day schedules (Kinsley and O'Donnell 1983 and Lyberg 1989). In Kinsley and O'Donnell (1983, pages 72-78), the experimental design divided the sample into two groups. They found that the two schedules produced similar response rates, and that the demographic composition was similar for both samples. They also found that the estimated time spent away from home was much higher under the convenient-day schedule than under the designated-day schedule. But it is impossible to determine whether the convenient-day schedule overestimates time spent away from home, or if the designated-day schedule underestimates time spent away from home, because the truth is not known. In Lyberg (1989, page 213), two diaries were collected from each respondent. One was collected using a designated-day schedule and the other was collected using a convenient-day schedule. However, the convenient-day diaries were conducted by an interviewer, while the designated-day diaries were self-administered several days after the convenient-day interview. So it is impossible to determine whether any differences were due to differences in contact schedules or whether they were due to mode effects.

Two studies (Lyberg 1989, pages 221-223 and Laaksonen and Pääkkönen 1992, pages 87-94) investigate the effect of postponement on response rates. Both studies found that postponement increases response rates. Laaksonen and Pääkkönen (1992, page 93) also found that it was difficult to evaluate whether postponement introduces bias. Their results showed that respondents who postponed their interview spent less time on housekeeping and maintenance, and more time on shopping and errands. However, it is unclear whether these differences are the result of bias introduced by postponement, unobserved heterogeneity that is correlated with postponement probability, or simply random noise. In any case, they argued that the differences were small, so that any bias was small.

One advantage of the convenient-day schedule is that it is possible to make many contact attempts in a short period of time. In contrast, the designated-day schedule--as proposed--permits only one contact attempt per week. So it is natural to ask: Would it be reasonable to modify the designated-day schedule to allow some form of day-of-week substitution? For example, if the respondent cannot be reached on Tuesday to report about Monday, would it be acceptable to contact the respondent on, say, Thursday and ask the respondent to report about Wednesday? This modified schedule would allow for more contact attempts without having to extend the field period.

Because this type of substitution makes sense only if the substitute days are fairly similar to the original days, the first step was to determine which days, if any were similar to one another. In earlier work (Stewart 2000), I showed that Monday through Thursday are very similar to each other, Fridays are slightly different from the other weekdays, and Saturday and Sunday are very different from the weekdays and from each other. Hence, it would be reasonable to allow day-of-week substitution at least for Monday through Thursday.

Activity Bias and Noncontact Bias

When selecting a contact strategy, we need to be concerned with two types of bias: activity bias and noncontact bias. Activity bias occurs when the probability of contacting and interviewing a potential respondent on a particular day is correlated with the respondent's activities on that diary day. Note that here and throughout the paper, the term contact probability refers to the probability of a productive contact (one that results in an interview). Because I want to isolate the effects of using alternative contact strategies, I assume that respondents always agree to an interview when contacted. Noncontact bias occurs when differences in contact probabilities across individuals are caused by differences in activities across individuals. Two simple numerical examples will illustrate these biases.

Example 1 - Activity Bias: Suppose that potential respondents' days fall into two categories: hard-to-contact (HTC) days and easy-to-contact (ETC) days. Further suppose that interviewers never contact respondents on HTC days (i.e., that $P_H = 0$, where P_H is the contact probability on an HTC day), and that they always contact respondents on ETC days (i.e., that $P_E = 1$, where P_E is the contact probability on an ETC day). Finally, suppose that the probability that any day is an ETC day is 0.5, so that on average half of each potential respondent's days are ETC and half are HTC. Note that all potential respondents are identical in the sense that the probability that any given day is an ETC day is 0.5 for all potential respondents. For simplicity, I assume that the activities of a given day can be summarized by an "activity index," I_J, where $I_J = 1 - P_J$ $(J = H,E)$. The activity index represents time spent in activities that are negatively correlated with the contact probability. Thus, HTC days are days in which more time is spent in activities that are done away from home (working, shopping, active leisure, etc.), while ETC days are days in which more time is spent in activities that are done at home (housework, passive leisure, etc.). The average true activity index for the population of potential respondents is 0.5 ($= 0.5 \times 1 + 0.5 \times 0$).

If a convenient-day contact schedule is used and there is no limit on the number of call-backs, then HTC days are oversampled. To see why this occurs, it is instructive to work through the two possible contact sequences. If the initial contact attempt occurs on an ETC day, then the respondent is contacted and asked about the previous day (the diary day). Because HTC and ETC days are equally likely, on average half of these diary days will be HTC and the other half will be ETC. Therefore, the average activity index for the diary days of these respondents is equal to 0.5, which is the same as the population average. If, on the other hand, the initial contact day is an HTC day, then no interview takes place and the respondent is called on the following day. Contact attempts continue every day until the potential respondent is reached (on an ETC day). The average activity index for the diary days of these respondents is equal to one, because the respondent is always interviewed on an ETC day that immediately follows an HTC day. So if a given day is HTC (i.e., the potential respondent does a lot of activities away from home), then it is more likely that that day will be selected as the reference day. Hence, the probability of interviewing the potential respondent on a given reference day is correlated with the activities on that reference day. Since half of the initial contact attempts are made on HTC days and half are made on ETC days, the average activity index for the final sample is equal to 0.75 (= $0.5 \times 0.5 + 0.5 \times 1$).

Example 2 - Noncontact Bias: Now suppose that potential respondents differ with respect to their contact probabilities, and that the contact probabilities for each individual do not vary from day to day. Suppose also that half of all potential respondents are HTC, with $P_H = 0.25$, and that the other half are ETC, with $P_E = 0.75$. If we attempt to contact each potential respondent four times, given these probabilities, virtually all (99.6 percent) ETC potential respondents are contacted. In contrast, only 68.4 percent of HTC potential respondents are contacted. The overall contact rate is 84 percent ($99.6 \times 0.50 + 68.4 \times 0.50$), but the final sample is not representative: 59.3 percent of the sample are ETC and only 40.7 percent are HTC. Therefore, estimates based on this sample will tend to underestimate the time spent in activities done by HTC people, and overestimate the time spent in activities done by ETC people.

The biases described above not limited to time-use surveys. Although most surveys take steps to minimize noncontact bias, less attention has been devoted to activity bias. For example, in addition to their main focus on collecting event history information on employment, the National Longitudinal Surveys also include a few questions about labor force activities (employment and hours) during the week prior to the interview. Because these interviews tend to be scheduled at the convenience of the respondent, the respondent's activities during the reference week will be correlated with the probability of interviewing the respondent about that reference week. The intuition behind this correlation is exactly the same as that in Example 1. This correlation introduces bias into hours-worked estimates, although the direction of the bias is indeterminate. Hours worked per week tend to be overestimated for respondents who were unable to schedule an interview because of a heavy work schedule, and tend to be underestimated for respondents who were away on vacation. Activity bias is also an issue for travel surveys. Time spent away from home will tend to be overestimated if respondents are asked about, say, the four weeks prior to the interview. Asking respondents about a fixed reference period can eliminate this bias.

It is worth noting that noncontact bias is a more important consideration for time-use surveys than for other surveys, because, unlike most other surveys, time-use surveys cannot accept proxy responses. If proxy responses could be accepted then data on HTC individuals could be collected from proxies, who may be easier to contact. This would weaken the correlation between the individual's activities and the probability of collecting data about that individual.

The rest of the paper is organized as follows. In Section 2, I introduce four contact strategies, and use simple simulations to assess the activity bias associated with each strategy. In Section 3, I augment the simulations with data from the May 1997 Work Schedule Supplement to the Current Population Survey and the 1995 University of Maryland Time Diary Study, and examine how the bias varies by specific activity. I also decompose the overall bias to assess the relative contribution of activity bias and noncontact bias. Section 4 summarizes the results and makes recommendations.

2. Contact Strategies, Correlated Activities, and Activity Bias

In this section, I compare the activity bias associated with the convenient-day schedule and each of the three variants of the designated-day schedules. These schedules are defined as follows:

1. Convenient-day (CD): Attempt to contact potential respondents every day following the initial contact attempt until the potential respondent is contacted or until the field period ends.
2. Designated-day (DD): Attempt to contact respondents only once (no subsequent attempts).
3. Designated-day with postponement (DDP): Attempt to contact potential respondents on the same day of the week as the initial attempt until the potential respondent is contacted or until the field period ends (as recommended by Kalton 1985, page 95).
4. Designated-day with postponement and substitution (DDPS): Attempt to contact potential respondents every other day following the initial contact attempt until the potential respondent is contacted or until the field period ends.

The DDPS strategy assumes alternating Tuesday/Thursday and Wednesday/Friday contact days. Whether the first week is Tuesday/Thursday or Wednesday/Friday depends on the start day, which is randomly assigned.

As we saw in Example 1, it is straightforward to show that a convenient-day schedule can introduce activity bias into time-use estimates when the base contact probability was the same each day (0.5) except for random noise (+0.5 with probability ½ or –0.5 with probability ½). Even though Stewart (2000, pages 5-9) shows that Monday through Thursday are very similar on average, it is likely that the contact probabilities for some individuals vary systematically by day each week. For example, some individuals may be hard to contact on Monday, Wednesday, and Friday of each week. This systematic variation makes it considerably more complicated to determine whether sample estimates are biased, and to determine the direction and extent of that bias. One could model contact strategies and analytically solve for the bias under different assumptions about the pattern of contact probabilities. However, this is a cumbersome process, because each assumption about the pattern of contact probabilities across days would require a separate solution. In contrast, computer simulations are an ideal way to assess the bias associated with alternative contact strategies under different assumptions about the pattern of contact probabilities. The computer program is simpler and produces more intuitive results than the analytical solution. And it is a simple matter to modify the program to allow for different patterns. In Section 3, I add realism to the simulations by incorporating real time-use data--something that would be impossible to do when taking an analytical approach.

Simulations

My simulation strategy was very straightforward. First, I created four weeks worth of "data" for each of 10,000 potential respondents. Because I am focusing on contact strategies, I ignore the sampling procedures and assume that the sample of potential respondents is representative of the population. The simulations are designed to compare the four contact schedules above, so I assumed that the "week" is five days long. Eligible diary days were restricted to Monday through Thursday, because, as noted above, these days are the most similar to each other. The next step was to simulate attempts to contact these respondents using the four contact schedules described above. Finally, I compared the estimates generated using each schedule to the true sample values.

To simplify the simulations I abstracted from specific activities, as in the examples above, and characterized each day using an activity index, I_J, (J = H,E) that ranges from 0 to 1. The activity index $I_J = 1 - P_J$, where P_J is the probability of contacting and interviewing the respondent. To simulate the variation in activities across days, the contact probability on a given day is:

$$P_J = \overline{P}_J + \varepsilon,$$

where $\overline{P}_J$ is the average contact probability on an HTC (J = H) or an ETC (J = E) day, and $\varepsilon \sim U(-\hat{\varepsilon}, \hat{\varepsilon})$. I assume that $\overline{P}_H < \overline{P}_E$, which means that, on average, respondents are less likely to be contacted on HTC days than on ETC days. To insure that contact probabilities lie in the [0,1] interval, I set $\hat{\varepsilon}$ so that $\hat{\varepsilon} < \min(\overline{P}_H, 1 - \overline{P}_E)$.

There are many assumptions one can make regarding the pattern of activities across days. The simplest case is where all days are identical except for random noise. But as noted above, it is possible that potential respondents are systematically harder to contact on some days than others. To cover a wide range of activity patterns, I ran the simulations under following eight assumptions about the pattern of HTC and ETC days in each of the four weeks:

1. Actual values of the activity index are distributed as U(0,1), so that the average value is 0.5.
2. The first two days of every week are HTC and the last three days are ETC (HHEEE).
3. The first three days of every week are HTC and the last two days are ETC (HHHEE).
4. The first four days of every week are HTC and the last day is ETC (HHHHE).
5. The first day of every week is ETC and the last four are HTC (EHHHH).
6. The first two days of every week are ETC and the last three are HTC (EEHHH).
7. The first three days of every week are ETC and the last two are HTC (EEEHH).
8. For half the sample Monday, Wednesday, and Friday are HTC and Tuesday and Thursday are ETC (HEHEH). For the other half of the sample the reverse is true (EHEHE).

In pattern 1, the base probability of contacting the respondent is the same, so that all of the variation in probabilities is due to the random term. In patterns 2-7, HTC days are grouped together at either the beginning of the week or at the end of the week. And in pattern 8, the base probabilities alternate between HTC and ETC days. To allow me to focus on activity bias, I run separate simulations for each of the 8 patterns described above. Thus, within a simulation all individuals have the same pattern of base probabilities.

[Insert Table 1 about here]

Table 1 shows the results from a representative subset of the 153 simulations I ran. The first four columns show the average contact probability on HTC and ETC days, the value of $\hat{\varepsilon}$, and the true average activity index. The remaining columns contain estimates of the bias associated with the four contact schedules. I computed the bias as the difference between the estimated amount of time spent in each activity and the true amount of time spent in each activity, and expressed that difference as a percentage of the true value. Entries with an asterisk indicate that the bias statistically different from the zero at the 5 percent level.

Pattern 1 - Identical Base Probabilities with Random Noise

This pattern is essentially the same as in the numerical example above. The main result is that all of the contact schedules generate unbiased estimates for the average activity index, except the CD schedule. As expected, the CD schedule overestimates the average activity index. More importantly, when using the CD schedule, the estimated average activity index--and hence the bias when activities are uncorrelated across days--*is positively correlated with the variance of ε.* As the variance increases from 0.003 ($\hat{\varepsilon}$ = 0.1) to 0.083 ($\hat{\varepsilon}$ = 0.5), the bias increases from less than 1 percent to 15 percent. One can see the intuition behind this result by noting that a large negative realization of ε on a particular day makes it less likely that the respondent will be contacted on that day, and hence, more likely that that day will become the diary day. None of the other contact schedules are sensitive to the variance of ε.

Patterns 2-7 - Grouped Base Probabilities

The results are mixed when HTC days are grouped at either the beginning or the end of the week. In the simulations where $\overline{P}_E - \overline{P}_H$ is relatively small (0.2) all of the contact schedules perform reasonably well. The absolute value of the bias is less than 3 percent in all cases. However, when $\overline{P}_E - \overline{P}_H$ is relatively large (0.5), there are significant differences in the bias associated with each contact schedule. The DDP schedule performs the best overall. The bias exceeds 5 percent (in absolute value) only in pattern 7 (EEEHH), for which the bias is −5.5 percent. In contrast, when using the DD and DDPS schedules, the bias is in the 10-14 percent range in patterns 2 (HHEEE), 3 (HHHEE), and in the 16-20 percent range in patterns 6 (EEHHH), and 7 (EEEHH). The differences between the DD and DDPS schedules and the DDP schedule for these patterns are significant, both statistically and in practical terms. In patterns 4 (HHHHE) and 5 (EHHHH) the DDP schedule performs slightly worse than the DD and DDPS schedules, but the bias is so small (less than 1.5 percent) that the difference is of no practical significance. The CD schedule fares somewhat better than the DD and DDPS schedules. The bias is less than 5 percent, except in patterns 6 and 7, where the bias is in the 11-18 percent range. As in pattern 1 above, the estimated average activity index increases with the variance of ε under the CD schedule, but not under any of the other schedules. And as can be seen from Table 1, in patterns where the bias is negative (patterns 6 and 7), an increase in the variance of ε decreases the bias.

Pattern 8 - Alternating Base Probabilities

All of the contact schedules generate biased estimates, because ETC days are undersampled. As above, all of the schedules perform reasonably well when $\overline{P}_E - \overline{P}_H$ is relatively small. The bias is in the 5-8 percent range for all schedules except DDP, for which the bias is about 1 percent. However, when $\overline{P}_E - \overline{P}_H$ is large, all of the contact schedules generate significant bias. The bias for the DDP schedule is higher than for the other patterns (about 10 percent), but it is smaller than the 25-35 percent bias for the other schedules. Again, these differences are significant statistically, and they are significant in practical terms.

The reason that the DDPS schedule generates a large activity bias is that contact attempts are made on two HTC days and then on two ETC days (or the reverse). This pattern results in contacting respondents on a relatively large fraction of ETC days, and hence, diary days will be disproportionately HTC days. Not surprisingly, if the DDPS schedule is modified so the respondent is contacted on the same two days each week, there is virtually no bias.

It is clear from these simulations that the activity bias associated with each contact strategy depends on the pattern of activities across days, the contact probabilities on HTC and ETC days, and the variance of those probabilities. However, it is also clear that the DDP schedule outperforms the other schedules regardless of the pattern assumed. If each pattern is viewed as a different type of respondent, then the overall bias (which includes both activity and noncontact bias) depends on the relative frequency of each type in the population. Information on the incidence of each type would allow me to measure the overall bias, and, for each strategy, decompose the overall bias it into the portion due to activity bias, and the portion due to noncontact bias. I do this in the next section.

3. Augmented Simulations

If one is willing to make some additional assumptions, it is possible to augment the simulations using data from other sources. The first assumption is that individuals' work schedules are a reasonable proxy for the patterns of HTC and ETC days, so that work days correspond to HTC days and nonwork days correspond to ETC days. The second assumption is that it is possible to replicate an individual's week by taking one day from each of five individuals.

I used data from the May 1997 Work Schedule Supplement to the Current Population Survey (CPS) to obtain information about individuals' work schedules. Note that because I need to know the prevalence of each type of schedule for the entire population, I also included nonworkers. Table 2 shows the patterns of work (W) days and nonwork (N) days from the May 1997 CPS. Approximately 88 percent of all individuals fall into two patterns. Forty-eight percent work all five weekdays, and 39 percent do not work any weekdays. Another 4 percent work four weekdays and have either Friday or Monday off. The remaining individuals do not exhibit any discernible pattern. To simplify the simulations, I assumed that individuals either worked all 5 weekdays (workers) or that they did not work any weekdays (nonworkers).

[Insert Table 2 about here]

To generate information on individual activities, I used data from the 1992-94 EPA Time Diary Study that was conducted by the University of Maryland. This dataset contains time-diaries for a sample of 7,408 adults (see Triplett 1995, pages 1-4, for a description of the survey). Because each individual was interviewed only once, I have only one observation per person. So I used the following repeated sampling method to construct 8 weeks worth of data for a sample of 18,974 "individuals." I divided the diary data into workdays and nonwork days. A diary day was considered a workday if the individual did any paid work during the day. Workdays were assigned to workers and nonwork days were assigned to nonworkers.

Mondays were drawn from the Monday observations, Tuesdays were drawn from Tuesday observations, etc. No observation was used more than once for a given individual, but the same observation could be used for more than one individual. The final sample proportions look fairly similar to the proportions from the CPS. Fifty-eight percent of individuals in the final sample were workers and 42 percent were nonworkers, which is reasonably close to the ratio of workers to nonworkers (1.38 vs. 1.23) in the CPS.

To compute the contact probabilities, it was necessary to make a third assumption. Following Pothoff, Manton, and Woodbury (1993, page 1198), I assumed that the contact probability is equal to the number of minutes spent in activities done at home (excluding sleeping) divided by the time spent in all activities other than sleep. This process for generating contact probabilities has two important properties: (1) the contact probability for a given day is related to the activities done on that day, and (2) one group of potential respondents (workers) has a lower probability of a productive contact (0.36 vs. 0.72).

[Insert Tables 3a and 3b about here]

Tables 3a and 3b summarize the bias estimates from the augmented simulations. Table 3a shows the bias estimates assuming a 4-week field period, and Table 3b shows the same estimates assuming an 8-week field period. Each of the first four columns contains estimates of the bias associated with the four contact schedules. The entries for each schedule and each 1-digit activity include estimates of the activity bias for workers and nonworkers, and an estimate of the overall bias. The overall bias includes noncontact bias, so it is possible that the overall bias is larger (or smaller) than the activity bias for either group. I computed the bias as the difference between the estimated amount of time spent in each activity and the true amount of time spent in each activity, and expressed that difference as a percentage of the true value. As with the previous set of simulations, an asterisk indicates that the bias is significantly different from the zero at the 5 percent level. The fifth column shows the true time spent in each activity by group and overall.

Comparing Tables 3a and 3b, we can see that the main difference is that, except for the DD strategy for which the field period is irrelevant, the overall bias is smaller when the field period is 8 weeks. This smaller overall bias is due mainly to the increased number of contact attempts, which disproportionately increases the probability that workers are contacted and makes the sample more representative (see Table 4). In contrast, estimates of the activity bias associated with the various contact strategies are not sensitive to the length of the contact period. So for the rest of this discussion, I will focus on the results in Table 3b.

The DD strategy generated virtually no activity bias. There were a few activities--Active Leisure, Entertainment/Socializing, Organizational Activities, Education/Training, and Active Child Care for workers, and Active Child Care for nonworkers--for which the activity bias was rather large, but none of these bias estimates are statistically significant. The overall bias for the DD strategy is quite large for most activities, which, as we will see below, is primarily due to noncontact bias.

Comparing the other three strategies, one can see two patterns emerge. First, activity bias is significantly smaller (and generally not statistically significant) when using the DDP strategy or the DDPS strategy than when using the CD strategy. Second, the bias in the CD estimates follows the expected pattern. The bias tends to be positive for activities that are done away from home (Active Leisure, Entertainment/Socializing, Organizational Activities, Education/Training, Purchasing Goods/Services, and Paid Work), and negative for activities done at home (Passive Leisure, Personal Care, Active Child Care, and Housework). This pattern is consistent with research cited in the introduction that finds that reported time spent away from home is greater under a convenience-day strategy than under a designated-day strategy. More important, it is now clear that this finding is due to bias in convenient-day strategies rather than bias in designated-day strategies.

[Insert Table 4 about here]

Noncontact Bias

In general, the contact rate increases and the sample becomes more representative as the number of contact attempts increases (see Table 4). The contact rate is the lowest under the DD strategy (40 percent), and the sample is the least representative. Under both the DDP and the DDPS schedules, the contact rate increases and the sample becomes more representative as the field period increases from 4 to 8 weeks. Using a DDPS schedule with an 8-week field period (16 contact attempts) results in a contact rate of 80 percent and a representative sample. Not surprisingly, the sample generated by the DDP schedule with an 8 week field period is virtually identical to the one generated by the DDPS schedule with a 4 week field period.

Activity Bias vs. Noncontact Bias

To get a clearer picture of the contribution of each type of bias to the overall bias, I decomposed the overall bias into the portion due to activity bias, the portion due to noncontact bias, and the portion due to the interaction between the two biases. The overall bias for activity *a* and group *g* (workers or nonworkers) is given by:

$$F_g X_{ag} - F_g^* X_{ag}^* = \underbrace{F_g^*\left(X_{ag} - X_{ag}^*\right)}_{\text{Activity}} + \underbrace{X_{ag}^*\left(F_g - F_g^*\right)}_{\text{Sample}} + \underbrace{\left(F_g - F_g^*\right)\left(X_{ag} - X_{ag}^*\right)}_{\text{Interaction}},$$

where F_g is the fraction of the sample in group *g*, and X_{ag} is the time spent in activity *a* by group *g*, and asterisks indicate the true values. The total bias for activity *a* is obtained by summing this expression over workers and nonworkers, and is given by:

$$\sum_{g=W,N}\left(F_g X_{ag} - F_g^* X_{ag}^*\right) = \sum_{g=W,N} F_g^*\left(X_{ag} - X_{ag}^*\right) + \sum_{g=W,N} X_{ag}^*\left(F_g - F_g^*\right) + \sum_{g=W,N}\left(F_g - F_g^*\right)\left(X_{ag} - X_{ag}^*\right),$$

[Insert Table 5 about here]

There are several things to take from these decompositions (shown in Table 5). First, under the CD schedule, all of the overall bias is due to activity bias. The large number of contact attempts virtually guarantees a representative sample, so that increasing the field period from 4 to 8 weeks does not make much difference. In contrast, noncontact bias accounts for all of the bias under the DD schedule. Under both the DDP schedule and the DDPS schedule there is virtually no activity bias, and noncontact bias decreases dramatically as the field period is increased from 4 to 8 weeks. Not surprisingly, the noncontact bias for the DDP schedule with an 8-week field period is about the same as the noncontact bias under the DDPS schedule with a 4-week field period. In these simulations, the sample becomes fully representative when the field period is long enough to allow 16 contact attempts. Finally, the small magnitude of the interaction terms reflects the fact that activity and noncontact biases associated with each contact strategy are negatively correlated.

4. Summary and Recommendations

Telephone time-use surveys have unique characteristics that make data collection more challenging. Unlike most other surveys, time-use surveys cannot accept proxy responses, so it is more likely that the probability of contacting a potential respondent is correlated with his or her activities. And because telephone time-use surveys ask respondents to report on their activities during the previous day, it is possible that the probability of interviewing the respondent about a given reference day will be correlated with the activities on that reference day. In this paper, I showed how these characteristics can generate noncontact bias and activity bias. I then used two sets of computer simulations to show that the extent of these biases depends on the survey's strategy for contacting potential respondents.

In the first set of simulations, I showed that the extent of the bias associated with any given contact schedule depends on the pattern of easy-to-contact (ETC) and hard-to-contact (HTC) days. The designated-day-with-postponement (DDP) schedule outperformed the other contact schedules for all of the activity patterns I examined. These simulations also showed that estimates generated using a convenient-day (CD) schedule are sensitive to the within-person variance of the contact probability. Estimates of the time spent in activities that are positively correlated with the contact probability (for example, activities done at home) decrease as the variance increases. In contrast, estimates generated by other contact schedules are not sensitive to the within-person variance of the contact probability.

Given the results of the simple simulations, it is clear that the overall bias for the different contact strategies depends on the relative frequency of each pattern in the population. Direct data on these patterns do not exist, so I augmented the first set of simulations using CPS data on work schedules and actual time use data from the 1992-94 EPA Time Diary Study. The results from the augmented simulations confirm those from the simple simulations, and show how the bias can affect estimates of time spent in specific activities. As expected, the CD contact strategy introduces systematic activity bias into time-use estimates. The time spent in activities done at home is underestimated, while time spent in activities done away from home is overestimated. There is no systematic activity bias in the samples generated by the DDP and designated-day-with-postponement-and-substitution (DDPS) strategies. The simulations also show that increasing the number of contact attempts reduces noncontact bias.

These results clearly show that the choice of contact strategy matters and point to two recommendations.

First, time-use surveys should use the DDP schedule. The DDP schedule generates less activity bias than the other contact schedules under all of the activity patterns tested. The DDPS schedule performed nearly as well in the more common activity patterns. But given that contact rates and field costs are a function of the number of contact attempts, the DDPS offers no cost advantage over the DDP schedule. Hence, there is no reason to choose the DDPS schedule over the DDP schedule.

Second, time-use surveys need to take steps to minimize noncontact bias. Because noncontact bias is largely a function of the number of contact attempts, an obvious way to minimize noncontact bias would be to increase the number of contact attempts. I will not elaborate on this point, because other authors have looked at this issue in depth. For example, Bauman, Lavradas, and Merkle (1993) show that age and employment status are related to the number of callbacks and that additional callbacks generate a more representative sample, and Botman, Massey, and Kalsbeek (1989) propose a method for determining the optimal number of callbacks. Another alternative would be to try to increase the probability of contacting potential respondents. This could be done by determining when respondents are likely to be home and calling at those times, or by allowing respondents to call on their designated interview day. Paying incentives is another way to make potential respondents become "more available." A less costly approach to minimizing noncontact bias would be to adjust sample weights. Pothoff, Manton, and Woodbury (1993) show that, when the variable being measured is correlated (across individuals) with the contact probability, weighting based on the number of callbacks is practical and effective. In the end, the correct mix of these approaches will depend on the constraints facing the survey manager.

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Table 1: Activity bias associated with each contact strategy under alternative assumptions about the correlation of activities across days

Activity Pattern	Average Contact Probability: Hard-to-contact days	Average Contact Probability: Easy-to-contact days	ε-hat	True Average Activity Index	Estimated Bias (Expressed as a percent of the true activity index): CD	DD	DDP	DDPS
Identical Base Probabilities								
	0.50		0.10	0.500	0.7 *	-0.1	0.0	0.1
	0.50		0.30	0.500	5.3 *	-0.3	0.1	0.2
	0.50		0.50	0.500	15.1 *	-0.9	0.4	0.7
Grouped Base Probabilities								
HHEEE	0.75	0.25	0.05	0.500	0.7	-10.7 *	-4.7 *	-13.8 *
	0.75	0.25	0.25	0.500	5.2 *	-10.9 *	-4.8 *	-13.9 *
	0.60	0.40	0.05	0.500	-0.1	-2.2 *	-0.7 *	-2.8 *
	0.60	0.40	0.20	0.500	2.5 *	-2.6 *	-0.7 *	-2.5 *
HHHEE	0.75	0.25	0.05	0.625	-2.7 *	-9.7 *	-4.0 *	-12.7 *
	0.75	0.25	0.25	0.625	0.8	-10.3 *	-4.1 *	-12.8 *
	0.60	0.40	0.05	0.550	-0.4 *	-1.8 *	-0.6 *	-2.5 *
	0.60	0.40	0.20	0.550	1.9 *	-2.4 *	-0.5	-2.2 *
HHHHE	0.75	0.25	0.05	0.750	0.1	-0.1	0.1	0.0
	0.75	0.25	0.25	0.750	2.3 *	-0.5	0.2	0.2
	0.60	0.40	0.05	0.600	0.1 *	0.0	0.0	0.0
	0.60	0.40	0.20	0.600	1.9 *	-0.3	0.2	0.2
EHHHH	0.75	0.25	0.05	0.625	1.7 *	1.0	1.4 *	0.7
	0.75	0.25	0.25	0.625	4.2 *	-0.3	1.2 *	0.7
	0.60	0.40	0.05	0.550	1.1 *	0.3	0.5 *	0.3
	0.60	0.40	0.20	0.550	2.9 *	0.0	0.6 *	0.4
EEHHH	0.75	0.25	0.05	0.500	-18.2 *	-17.1 *	-4.3 *	-21.7 *
	0.75	0.25	0.25	0.500	-15.9 *	-17.9 *	-4.5 *	-20.9 *
	0.60	0.40	0.05	0.500	-2.0 *	-2.2 *	-0.4	-2.6 *
	0.60	0.40	0.20	0.500	-0.4	-2.4 *	-0.3	-2.6 *
EEEHH	0.75	0.25	0.05	0.375	-16.6 *	-17.6 *	-5.5 *	-20.3 *
	0.75	0.25	0.25	0.375	-11.4 *	-17.6 *	-5.6 *	-19.6 *
	0.60	0.40	0.05	0.450	-2.0 *	-2.3 *	-0.4	-2.5 *
	0.60	0.40	0.20	0.450	0.0	-2.5 *	-0.5	-2.5 *
Alternating Base Probabilities								
HEHEH/ EHEHE	0.75	0.25	0.05	0.500	31.5 *	26.4 *	9.6 *	28.5 *
	0.75	0.25	0.25	0.500	34.7 *	26.5 *	9.7 *	29.4 *
	0.60	0.40	0.05	0.500	5.6 *	4.5 *	1.3 *	5.1 *
	0.60	0.40	0.20	0.500	7.8 *	4.3 *	1.2 *	5.1 *

Note: Asterisks indicates that the estimated average activity index is statistically different from the true value at the 5% level.

Table 2: Distribution of work schedules

Activity Pattern					Percent	Cumulative Percent
M	Tu	W	Th	F		
-	-	-	-	-	39.40	39.40
W	W	W	W	W	48.11	87.51
W	W	W	W	-	2.63	90.14
-	W	W	W	W	1.63	91.77
W	W	W	-	-	0.81	92.58
W	W	-	-	-	0.26	92.84
-	-	-	W	W	0.37	93.21
-	-	W	W	W	0.68	93.89
W	-	W	-	W	0.49	94.38
-	W	-	W	-	0.25	94.63
-	-	-	-	W	0.51	95.14
W	-	-	-	-	0.25	95.39
W	W	-	W	W	0.73	96.12
W	-	-		W	0.36	96.48
W	-	-	W	W	0.70	97.18
Other patterns					2.82	100.00
Total					100.00	

Note: A "W" indicates a workday, and a "-" indicates a nonwork day. Author's tabulations from the May 1997 Work Schedule Supplement to the CPS. Observations were weighted using supplement weights. The sample size is 89,746 observations.

Table 3a: Estimated bias - augmented simulations (4 week field period)

Activity/Emp. Status	CD	DD	DDP	DDPS	Time Spent in Activity (Truth)
Passive Leisure					
Nonworkers	-8.44% *	0.12%	-1.54%	-1.03%	314.72
Workers	-5.40% *	1.07%	0.43%	0.82%	152.04
Overall	-8.62% *	13.56% *	2.53% *	0.38%	220.70
Active Leisure					
Nonworkers	9.80% *	-2.75%	0.99%	-0.66%	65.94
Workers	-0.07%	-7.34%	-4.69%	1.91%	26.89
Overall	4.03% *	11.75% *	3.31%	1.08%	43.37
Entertainment/Socializing					
Nonworkers	19.41% *	-2.01%	-0.25%	-1.20%	67.30
Workers	8.63% *	7.14%	5.21%	3.72%	27.87
Overall	13.11% *	15.78% *	5.64% *	1.37%	44.51
Organizational Activities					
Nonworkers	19.58% *	-0.98%	9.00%	3.84%	19.25
Workers	13.77% *	6.95%	7.17%	7.48%	8.72
Overall	15.24% *	15.26% *	12.37% *	5.99%	13.16
Education/Training					
Nonworkers	32.77% *	-0.42%	12.54% *	8.92% *	43.60
Workers	-1.17%	7.63%	0.57%	1.59%	13.16
Overall	19.17% *	22.02% *	15.39% *	8.00% *	26.01
Personal Care					
Nonworkers	-0.50%	-0.29%	-0.49%	-0.44%	663.04
Workers	-0.52% *	0.01%	-0.06%	-0.13%	580.71
Overall	-0.79% *	2.20% *	0.34%	-0.15%	615.46
Purchasing Goods/Services					
Nonworkers	12.62% *	1.35%	0.11%	-1.28%	72.98
Workers	-4.05%	4.62%	-3.62%	-5.43% *	23.28
Overall	4.67% *	22.36% *	4.25% *	-1.49%	44.25
Active Child Care					
Nonworkers	-7.89% *	5.11%	-1.06%	-0.54%	24.13
Workers	-7.69% *	-6.05%	-4.09%	-0.92%	12.64
Overall	-9.09% *	14.21% *	0.77%	-0.09%	17.49
Housework					
Nonworkers	-8.88% *	1.71%	0.33%	2.27%	169.04
Workers	-10.55% *	0.85%	-2.03%	-0.14%	57.92
Overall	-11.49% *	20.77% *	4.53% *	2.52% *	104.82
Paid Work					
Nonworkers					
Workers	2.95% *	-0.77%	0.25%	-0.27%	536.77
Overall	6.74% *	31.44% *	-7.74% *	-1.87% *	310.22

Note: Asterisks indicates that the bias in the estimated time spent in the activity is significantly different from zero at the 5% level.

Table 3b: Estimated bias - augmented simulations (8 week field period)

Activity/Emp. Status	CD	DD	DDP	DDPS	Time Spent in Activity (Truth)
Passive Leisure					
Nonworkers	-8.63% *	-0.09%	-1.62%	-1.21%	315.38
Workers	-5.24% *	1.28%	0.39%	1.10%	151.72
Overall	-8.72% *	-13.51% *	-0.35%	-0.31%	220.79
Active Leisure					
Nonworkers	10.62% *	-2.03%	1.76%	0.06%	65.46
Workers	0.00%	-7.29%	-3.50%	2.21%	26.87
Overall	4.49% *	12.30% *	0.50%	0.82%	43.16
Entertainment/Socializing					
Nonworkers	19.77% *	-1.72%	-0.15%	-0.91%	67.10
Workers	8.09% *	6.64%	5.52%	2.76%	28.00
Overall	13.06% *	15.80% *	2.47%	0.40%	44.50
Organizational Activities					
Nonworkers	18.92% *	-1.53%	8.59%	3.25%	19.36
Workers	14.03% *	7.00%	3.18%	7.25%	8.72
Overall	14.89% *	14.88% *	7.14% *	4.76%	13.21
Education/Training					
Nonworkers	33.56% *	0.18%	12.91% *	9.55% *	43.34
Workers	-0.72%	8.24%	0.77%	2.01%	13.09
Overall	19.73% *	22.74% *	10.29% *	7.32% *	25.86
Personal Care					
Nonworkers	-0.50%	-0.29%	-0.48%	-0.44%	663.03
Workers	-0.55% *	0.00%	-0.08%	-0.16%	580.81
Overall	-0.82% *	2.20% *	-0.17%	-0.29%	615.51
Purchasing Goods/Services					
Nonworkers	12.64% *	1.36%	-0.09%	-1.28%	72.97
Workers	-4.41%	4.23%	-3.66%	-5.45% *	23.36
Overall	4.48% *	22.23% *	-0.42%	-2.58%	44.30
Active Child Care					
Nonworkers	-7.67% *	5.36%	-1.04%	-0.31%	24.07
Workers	-8.02% *	-6.18%	-4.98%	-1.65%	12.66
Overall	-9.14% *	14.30% *	-2.23%	-0.89%	17.48
Housework					
Nonworkers	-9.02% *	1.55%	0.20%	2.10%	169.30
Workers	-10.55% *	0.80%	-2.15%	-0.20%	57.95
Overall	-11.64% *	20.63% *	0.17%	1.34%	104.94
Paid Work					
Nonworkers					
Workers	2.96% *	-0.78%	0.30%	-0.26%	536.82
Overall	6.86% *	-31.44% *	-0.86%	-0.22%	310.25

Note: Asterisks indicates that the bias in the estimated time spent in the activity is significantly different from zero at the 5% level.

Table 4: Contact Rate Summary - Augmented Simulations

Field Period		CD	DD	DDP	DDPS	Truth
4 weeks	Contact Rate	89.68	40.35	71.79	78.39	
	Percent Nonworkers	40.08	60.07	46.82	43.14	42.21
	Percent Workers	59.92	39.93	53.18	56.86	57.79
8 weeks	Contact Rate	89.79	40.35	78.87	80.17	
	Percent Nonworkers	40.02	60.07	42.88	42.19	42.21
	Percent Workers	59.98	39.93	57.12	57.81	57.79

Table 5: Bias Decomposition - Augmented Simulations

	4-week field period				8-week field period			
	Total Bias	Activity Bias	Noncontact Bias	Interaction	Total Bias	Activity Bias	Noncontact Bias	Interaction
Passive Leisure								
CD	-8.62	-7.23	-1.57	0.18	-8.72	-7.29	-1.62	0.19
DD	13.56	0.50	13.16	-0.10	13.51	0.46	13.24	-0.18
DDP	2.53	-0.75	3.40	-0.11	-0.35	-0.83	0.50	-0.02
DDPS	0.38	-0.29	0.69	-0.02	-0.31	-0.30	-0.01	0.00
Active Leisure								
CD	4.03	6.27	-1.92	-0.32	4.49	6.80	-1.96	-0.35
DD	11.75	-4.40	16.08	0.06	12.30	-3.92	15.97	0.26
DDP	3.31	-1.05	4.15	0.20	0.50	-0.13	0.60	0.03
DDPS	1.08	0.26	0.84	-0.02	0.82	0.83	-0.02	0.00
Entertainment/Socializing								
CD	13.11	15.51	-1.89	-0.51	13.06	15.53	-1.92	-0.54
DD	15.78	1.30	15.82	-1.34	15.80	1.32	15.69	-1.21
DDP	5.64	1.72	4.08	-0.17	2.47	1.91	0.59	-0.02
DDPS	1.37	0.58	0.82	-0.04	0.40	0.42	-0.02	0.00
Organizational Activities								
CD	15.24	17.36	-1.70	-0.42	14.89	17.06	-1.76	-0.40
DD	15.26	2.05	14.28	-1.08	14.88	1.72	14.39	-1.23
DDP	12.37	8.30	3.69	0.39	7.14	6.53	0.54	0.07
DDPS	5.99	5.24	0.74	0.01	4.76	4.77	-0.02	0.00
Education & Training								
CD	19.17	22.84	-2.49	-1.18	19.73	23.53	-2.56	-1.24
DD	22.02	1.94	20.90	-0.82	22.74	2.54	20.90	-0.69
DDP	15.39	9.04	5.40	0.96	10.29	9.36	0.78	0.14
DDPS	8.00	6.78	1.09	0.13	7.32	7.35	-0.02	0.00
Personal Care								
CD	-0.79	-0.51	-0.28	0.00	-0.82	-0.53	-0.29	0.00
DD	2.20	-0.13	2.39	-0.06	2.20	-0.13	2.39	-0.06
DDP	0.34	-0.26	0.62	-0.02	-0.17	-0.26	0.09	0.00
DDPS	-0.15	-0.27	0.12	0.00	-0.29	-0.29	0.00	0.00
Purchasing Goods/Services								
CD	4.67	7.55	-2.39	-0.49	4.48	7.44	-2.45	-0.51
DD	22.36	2.34	20.06	-0.04	22.23	2.23	20.00	0.00
DDP	4.25	-1.02	5.18	0.10	-0.42	-1.18	0.75	0.01
DDPS	-1.49	-2.54	1.04	0.01	-2.58	-2.55	-0.02	0.00
Active Child Care								
CD	-9.09	-7.81	-1.40	0.11	-9.14	-7.82	-1.43	0.10
DD	14.21	0.45	11.72	2.04	14.30	0.53	11.66	2.12
DDP	0.77	-2.32	3.03	0.07	-2.23	-2.69	0.44	0.01
DDPS	-0.09	-0.69	0.61	0.00	-0.89	-0.87	-0.01	0.00
Housework								
CD	-11.49	-9.42	-2.26	0.18	-11.64	-9.51	-2.32	0.19
DD	20.77	1.43	18.93	0.41	20.63	1.31	18.95	0.37
DDP	4.53	-0.43	4.89	0.08	0.17	-0.55	0.71	0.01
DDPS	2.52	1.50	0.99	0.03	1.34	1.36	-0.02	0.00
Paid Work								
CD	6.74	2.95	3.69	0.11	6.86	2.96	3.79	0.11
DD	-31.43	-0.77	-30.90	0.24	-31.44	-0.78	-30.90	0.24
DDP	-7.74	0.25	-7.98	-0.02	-0.86	0.30	-1.16	0.00
DDPS	-1.87	-0.27	-1.61	0.00	-0.22	-0.26	0.03	0.00