

# SECTION B

## Child and Adult Care Food Program (CACFP)

### Improper Payment Meal Claims Assessment

(OMB No.: 0584-NEW)

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## **SECTION B: COLLECTION OF INFORMATION USING STATISTICAL METHODS**

**B.1 Describe (including a numerical estimate) the potential respondent universe and any sampling or other respondent selection methods to be used. Data on the number of entities (e.g., establishments, State and local government units, households, or persons) in the universe covered by the collection and in the corresponding sample are to be provided in tabular form for the universe as a whole and for each of the strata in the proposed sample. Indicate expected response rates for the collection as a whole. If the collection had been conducted previously, include the actual response rate achieved during the last collection.**

### **1. Sampling Allocation and Precision**

In the context of the proposed study, the IPIA standards mean that we should be 90% certain that our national estimate of the percentage of improper claims will be within  $\pm 2.5$  percentage points of the true, but unknown, value. This range of values, or “precision” of the estimate, is a function of the variance of the estimate. It is well known that this variance will be greatest when the rate of improper billing is 50%. In such a situation, the necessary sample size from a very large population, if the design called for a simple random sample, would be 1,082. While the above result would be valid if the error rate were close to 0.50, a previous limited study placed the error rate closer to 5%. However, this might be slightly optimistic, given the non-representative character of that study. An assumption of a 10% error rate seems more realistic, and guards against a finding of a larger error rate. For a random sample, to estimate the improper meal claims rate at 90% with a  $\pm 2.5\%$  confidence interval, we would need 390 meal claims. The proposed design is not a random sample of claims for sound practical reasons, so the sample size will need to be much larger.

A number of issues make it undesirable to select meal claims at random, including the burden to each State and sponsor for acquiring administrative data needed to create the sample frame for the next level, as no master file of CACFP sponsors and FDCHs by State exists. This is neither practical nor feasible; so constructing the sample design will involve selecting States first, and then, from the selected States, acquiring a complete list of sponsors.

Sponsors will then be selected, and the lists of participating FDCHs will be obtained from these selected sponsors. The FDCHs can then be classified into Tier I (including Tier II FDCHs with only Tier I children), Tier II only, and Mixed-Tier FDCHs (classified as Tier II but containing some Tier I children).

The study requires separate estimates for each kind of FDCH tier level (Tier I and Tier II). Because the mixed-Tier homes vary considerably in their proportion of Tier I meals, and allocating a fixed number of FDCH homes by tier requires that a home be replaced by a home of the same type in the case of a refusal, we will obtain a measure of size based on the number or cost of Tier I and Tier II meals, and use it at each level. To get this measure we will obtain the proportion of all Tier I meals claimed by a sampling entity (State, sponsor, or FDCH) and the proportion of all Tier II meals claimed by the same entity and then take the average of the two. This procedure, instituted at each level, will lead to the expectation that 50% of the meals sampled are Tier I and 50% are Tier II. This is because States, sponsors, and FDCHs with a larger proportion of Tier II claims will be given a greater probability of selection. This will result in approximately equal samples from each tier.

We also take into account the design effect; i.e., the degree to which unequal weights and clustering makes a larger sample provide estimates equal to those of a random sample of a much smaller size. This design effect is inextricably bound to the concept of “intra-class correlation,” which, in this context, is a measure of the homogeneity of improper claim rates among homes associated with a particular sponsor, and of sponsors located in a particular State. Important here is the concept of an *effective* sample size. The effective sample size is equivalent to the sample size necessary if we were to draw claims through simple random selection. In this instance, and for a population parameter of 0.10 and a power and precision of 90%, with  $\pm 2.5$  percentage points, the effective sample size needed would be equal to 390 Tier I and 390 Tier II claims.

To fully determine the optimal design, we need not only a design effect, but also an intra-class correlation at every level of the design. We need a measure of the degree to which the rate of improper claims is similar for meals provided to the same children (as opposed to different children in the same FDCH), for children in

different FDCHs, for FDCHs from the same sponsor, and for sponsors in the same States. There are no data from which such information may be obtained; so the planned methodology consists of using the design effect of 5.9 found in the recent pilot study. If we use that design effect, we would need approximately 2,300 meal claims for each tier, or 4,600 meal claims in total. The maximum number of claims an FDCH can make for a child in a day is 3, but with absences taken into account this number would probably be closer to 2.7; so the total number of meals will be equal to  $16 \times 4 \times 4 \times 4 \times 2.7 \times 5 = 13,824$  meals. This assumes a 100% response rate from the parents. However, if we allow for 30% of the parent responses to be “I don’t know” or for some parent non-responses that cannot be compensated for, the number of meals is reduced to 9,677 meals, which is what we expect to receive and is slightly larger than twice the number required. This provides sufficient leeway to still yield valid estimates if some of the assumptions turn out unwarranted.

Based on these assumptions, our complete sampling strategy includes (1) a base national sample, (2) a feasibility subsample that meets the requirements stated by USDA/FNS to use a two-stage approach for the study. For the base national sample, we will select 16 States, 4 sponsors per State, 4 FDCHs per sponsor, and 4 children per FDCH. The feasibility study will consist of 8 States, 4 sponsors per State, 4 FDCHs per sponsor, and an average of 4 children, or half of the base national sample. The following exhibit presents the sample size for the feasibility study and national study (Exhibit B.1).

**Exhibit B.1: Summary of the Expected Sampling Design, by Study Phase (National and Feasibility Samples)**

Characteristics	States	Sponsors	FDCHs	Children	Meals	Days
<b>National Sample</b>						
<b>Structure</b>	16 States	4 sponsors per State	4 FDCHs per sponsor	4 children per FDCH	2.7 meals per day per child	5 days
<b>Sample Units</b>	16 States	64 sponsors	256 FDCHs	1,024 children	2,764.8 meals per day	13,824 total meals
<b>Feasibility Study Subsample</b>						
<b>Structure</b>	8 States	4 sponsors per State	4 FDCHs per sponsor	4 children per FDCH	2.7 meals per day per child	5 days
<b>Sample Units</b>	8 States	32 sponsors	128 FDCHs	512 children	1,382.4 meals per day	6,912 total meals

From the exhibit above, the number of sampling units for the national sample is estimated at 13,824 meals. It is expected that many parents will be unable to remember or will not know

whether their child was served a meal. Assuming a 30% rate of unable to remember or don't know by parents, the sample is reduced to 9,677. While this is more than twice the number that would be necessary if the design remained essentially the same, the assumptions were correct, and no further estimates were desired, other assumptions were considered in developing a sampling approach that supports the large number of meals. Consider the options for reducing the sample size. If the number of states, the number of sponsors, the number of homes or the number of parents per home were reduced, this would automatically increase the design effect due to clustering, thus requiring an increase in the sample size. The only reductions that would result in a smaller sample size would be to reduce the number of meals per day or the number of days per parent. Then the intra-class correlation would go down, and the estimated necessary sample size would also decrease. However, sampling meals from the administrative records is relatively inexpensive, and sampling fewer, given that FNS, the sponsor, and the home have been contacted, would result in minimal savings. Where some savings might be possible would be in asking the parents. But the possibility of parental error if reporting on specific days is greater than if given an entire week to go through. In addition, if we were to subsample days and the last two or three days prior to the call were selected, we would be adding a clustering level and a possible bias by day of the week. The assumptions that led us to the numbers we presented are based on previous studies, none of them identical to the proposed one. Thus it was decided that since the only cut in sample size that would not alter the design effect or the ability to predict would not save that much, the sample size and the design would be left intact. The sample size arrived at was also based on the need to obtain main estimates and the estimates of error by tier level in the program. Estimates by type of meal or by subpopulations could not be covered with the initially calculated sample size. Given these other considerations, it was decided that the larger sample size was desirable.

## 1. Sampling Approach

**Sampling the States.** . The States that participate in the main study were sampled with probabilities proportional to size, using probability minimum replacement (i.e. sampling the very large States possibly more than once). The process of drawing the sample began with a list of States indicating the number of FDCHs and meals claimed in each State for each tier. A measure of size was calculated for each State. This measure of size was to be the average of the proportion of Tier I meals claimed in the State and the proportion of Tier II meals claimed in the State. For each State—combined or singularly—the measure of size was multiplied by the number of States to be sampled (i.e., 16) to provide the expectation of selection for the State. If this expectation was less than 1.0, it represented the probability that the State was selected once. If it was greater than 1.0, the integer indicates the number of times the State was selected with certainty, and the fractional part indicates the probability that the State was selected one additional time. The sum of all expectations added up to 16. When a State is selected twice (and 2 States were), 8 sponsors will be selected instead of 4. The sampling method used was Systematic Random Sampling. The sub-selection of States to include in the feasibility study will be done purposively. The 8 States in the feasibility study will be selected upfront to lay the groundwork for contact with the sponsors. First, of the States selected more than once, only one will be selected for the feasibility study. Then States that have participated in several rounds of the agency’s other CACFP assessment studies will be identified and flagged; of these States (for example California, Minnesota, Texas), up to two will be identified for the feasibility study. Representation from different FNS regions will also be a consideration for final selection, as it will be important to have different parts of the country represented. Exhibit B.2 presents the sample of States selected for the main study including the size, region, and the number of times selected.

Exhibit B.2. Sample of States To Be Included in the Data Collection

State/Region	Size	Region	Times Selected
Arizona	0.2204	7	1
California	1.9386	7	2
Colorado	0.1703	6	1

State/Region	Size	Region	Times Selected
Delaware	0.0751	2	1
Illinois	1.0042	4	1
Kansas	0.4649	6	1
Michigan	0.7777	4	1
Minnesota	2.0829	4	2
North Carolina	0.2612	3	1
Ohio	0.3622	4	1
Pennsylvania	0.2409	2	1
Rhode Island	0.0302	1	1
Texas	0.7324	5	1
Utah	0.2200	6	1

**Sampling the Sponsors.** Each State selected will be asked to provide a list of sponsors, the list of meal claims made by the sponsor’s FDCHs and the number of FDCHs of each tier associated with the sponsor.

The total Tier I and Tier II claims for the union of the 16 States (we are using the term, even though there will be fewer than 16 different States) will be obtained, and for each sponsor we will obtain a measure of size analogous to the one for States—the average of the proportion of Tier I claims in the 16 States combined and the proportion of Tier II claims in the 16 States combined. In each State, the measures of size will be added and the measure of size of the sponsor will be divided by the sum of the measures of size of all the sponsors in the State. We now let  $e_j = 4h_{ij} / (b_{i1} + b_{i2} + \dots + b_{in})$ , where  $e_{ij}$  is the expectation of sponsor  $j$  in State  $i$ ,  $h_i$  is the number of times that State  $i$  was selected, and  $b_{ij}$  is the measure of size of sponsor  $j$  in State  $i$ .

The integer part of  $e_{ij}$  is the number of times the sponsor will be sampled with certainty. The fractional part will determine the probability of an additional time. Pareto Sampling will be used to select the remaining cases. In the event that a sponsor has closed, a replacement will be selected. Ordinarily, this will be the next sponsor in the list using the ordering of the  $f_{ij}$ . However, if, as most frequently occurs, the FDCHs under the sponsor have primarily gone to 1 or more other sponsors, other procedures may have to be implemented. The most effective procedure would be to recalculate the probabilities and repeat the procedure using the same PRNs. This has a very large probability of including all of the sponsors previously selected. Thus, we have a contingency plan in case there is a radical change between the original sponsor sample and the time when the data collection must begin.

**Sampling the FDCHs.** Once sponsors are selected, ICF International will contact them and obtain a list of FDCHs and their amount of Tier I and Tier II claims. A PRN will be assigned to each FDCH, as well as a size



measure equal to the average of the proportion of Tier I claims in the 64 sampled sponsors or combinations of sponsors and the proportion of Tier II claims in the 64 sampled sponsors or combinations of sponsors. The design calls for sampling 4 FDCHs per sponsor if the supplemental design is not used, and 8 if it is used. The Pareto Sampling design will assume 4 FDCHs per sponsor. Thus, a probability of selection will be calculated in a way parallel to that used for sponsors. The measure of size will again be inflated for Tier II meals. Let  $q_{ijk}$  be the proportion of Tier I claims in FDCH  $k$  under sponsor  $j$  in State  $i$ . Let  $p_k$  be the probability of selection of home  $k$ , calculated by the formula  $p_{ijk} = 4h_{ij}b_{ijk} / (b_{ij1} + b_{ij2} + \dots + b_{ijn})$  where  $p_{ijk}$  is the probability of selection of FDCH  $k$  in sponsor  $j$  in State  $i$ ,  $h$  is the number of times the sponsor was selected, and  $b$  is the measure of size. Now if we add the products  $q_{ijk}p_{ijk}$  across all of the sponsors and divide the total by 256 (the number of FDCHs to be selected) we have an estimate of the proportion of the meals that will be in Tier I. The target would be 50%. Once the initial probabilities are calculated, the geographical distributions of the FDCHs for each sponsor will be examined. Clusters of at least 16 FDCHs will be identified. For each sponsor sampled once, a cluster will be selected with the Probability Proportional to Size method, using the sum of the probabilities of selection of its members as a measure of size. Now the probabilities of selection of each FDCH are divided by the probability of selection of the cluster to obtain the final probability of selection. If a sponsor was selected more than once, 2 clusters may have to be selected. If an FDCH is found to have closed or is unable or unwilling to participate, it will be replaced by the next FDCH in the Pareto Sampling ordering.

**Selection of Children and Meals.** Once the sponsors are selected, 4 children will be selected from each FDCH. The enrollment rosters will have been obtained from each FDCH; 4 children will be selected from the roster, excluding the providers' children, and care will be taken to select only 1 child per family. In many FDCHs there will be fewer than 4 families, thus wherever possible, additional parents will be selected from other FDCHs. Note that the increase in the number of children to be selected is meant to compensate for lower enrollment rates in some FDCHs, and not for non-response. A non-respondent family may be replaced within the FDCH (since during the observation, every child present will be recorded). If the enrollment in an FDCH is lower than 4, we will use ordered sampling (Pareto Sampling approach) to assign a child from another FDCH. The dates during

which observations are made will be chosen purposively rather than randomly based on the meal service pattern of the provider.

**Pareto Sampling.** As mentioned previously, a key consideration for the sampling plan is the ability to identify necessary replacements for the sample, for sponsors, FDCHs, and parents. If one uses an approach that orders the frame at a given stage and selects the first  $n$ , one can simply address non-response by selecting the next unit in the frame. The sampling approach, which can also be used to control the overlap between samples, is the use of Permanent Random Numbers (PRNs). Suppose one has a set of units to be sampled and a probability of selection for each unit, where the probabilities add up to  $n$ , and one wishes to sample  $n$  units. Let  $p$  represent the probability of selection of a unit and  $r$  represent its PRN, assigned between 0 and 1. One can then calculate  $q = (r - pr)/(p - pr)$  and select the  $n$  units with the lowest values of  $q$ . The number  $q$  is the Pareto number by which we sort the units and select the ones with the lowest values of  $q$ . Even when the values of  $p$  change from one sample to another (for example, in one sample we select units by tier and in another we do not), the use of Pareto Sampling assures us that our sample will maximally overlap (if we use the same PRNs), or minimally overlap (if we use  $1 - r$ ). More importantly for this study, since the units are sampled by randomly sorting, if a unit has closed or is otherwise unable to participate, the order can be preserved and the next unit is sampled. This can be applied to both sponsors and FDCHs.

## 2. Non-Response Analysis

There are three levels at which non-response is possible, and where response bias can affect the results: 1) There is the sampling of sponsors, where it is possible that cooperating sponsors could be different from sponsors that refuse to cooperate. 2) There is the sampling of FDCHs, where the FDCH is not available (perhaps because the sponsor's monitor is unable to be present) or for some other reason. 3) Parents may choose to not cooperate with the study. Response bias is indicated when there is reason to believe that the respondents are different from the non-respondents (i.e., they would have given different answers or yielded different results). While this cannot be determined precisely (because no data are available for non-respondents), there are indicators that suggest a non-response bias exists that would allow for adjustments to control the bias. The non-response analysis requires

variables available for both respondents and non-respondents (but not for units that are not sampled at all). The following variables are possible candidates for weighting categories for non-response adjustments:

- 1) Sponsors (number of FDCHs, proportion of Tier I meals)
- 2) FDCHs (tier of the FDCH, number of children, presence of the owner's own children, number of meals claimed for the week of the study—also obtained from the sponsors for non-respondent homes).
- 3) Children (number of siblings in the FDCH, length of their enrollment).

**B.2 Describe the procedures for the collection of information including—**

- **Statistical methodology for stratification and sample selection**
- **Estimation procedure**
  
- **Degree of accuracy needed for the purpose described in the justification**
- **Unusual problems requiring specialized sampling procedures**
  
- **Any use of periodic (less-frequent-than-annual) data collection cycles to reduce burden.**

Because no national sampling frame exists, the frame must be developed by obtaining selected administrative records on the number and status of sponsors, FDCHs, and children currently enrolled in the FDCH. Therefore, each program level must be contacted to obtain the necessary records. Data collection tasks consist of conducting in-home observations at sampled FDCHs, conducting parent-recall interviews with the subsample at each FDCH, and obtaining 3 months of meal claims for each sampled FDCH from sponsors.

**States.** FNS will notify its 7 Regional Offices who, in turn, will inform State agencies about the research. The 16 sampled States will be contacted using an official letter that advises them of the study, requests their participation, and lists the information needed to select the sample of sponsors. Confirmation and follow-up calls will be made to confirm the data request. The data being requested consist of the list of CACFP sponsors of FDCHs (including full address and telephone number) in the State and the total number of FDCHs supported by each of these sponsors (by tiering level) along with supporting documentation. Appendix A provides a copy of the letters and contact scripts for this data request.

**Sponsors.** During the first contact, we will seek to obtain the necessary data for the sample of FDCHs. The 64 sampled sponsors will be contacted using an official letter that advises them of the study, requests their

participation and lists the information needed to select the sample of FDCHs. Confirmation and follow-up calls will be made to confirm the data request. Once the 4 FDCH providers and replacement cases associated with the sponsor have been selected, the sponsor will be contacted again, via letter, to obtain more detailed information about these FDCHs. Sponsors will receive a second letter identifying the selected FDCHs and requesting the following administrative data for each selected FDCH: provider CACFP application/agreement information, tiering determination documentation, participant information, and 3 months of meal claims. Sponsors (n=64) will receive a follow-up contact telephone call to obtain the 3-month meal claim records for selected FDCHs. These records are expected to be transmitted electronically when feasible and via mail if electronic copies are not available. Meal claim records will be abstracted and data entered by ICF International staff.

Those sponsors associated with the 8 States selected for the feasibility study will receive a third contact as a part of the study procedures for the validation of the parent-recall interview data. A sponsor will be contacted via telephone to confirm the dates for observation at a FDCH and to arrange for the sponsor's monitor to accompany the visit if needed. For the feasibility study, only 32 sponsors will receive this contact. Observation will not be conducted with the balance of the sample for the full national study. Appendix B includes the contact letters and telephone scripts for sponsors.

**FDCHs.** FDCHs (n=256) will be contacted via letter to obtain child enrollment and meal service records. FDCHs will be contacted by telephone to confirm the data request and to ascertain the best means for transmitting necessary data. Half of the total sample of FDCHs (n=128) will be included in the feasibility study. Appendix C provides the contact letter and telephone scripts for FDCHs

**Onsite Observation.** With the 32 sponsors sampled for the feasibility study (n=32), a one-time 2-day in-home observation will be conducted using similar procedures as the ones used for the monitoring visit typically conducted by the sponsor. This visit will be scheduled for a target week within a month. The in-home observation will be conducted to record the number of children being served meals and the type of meals being served during two mealtimes on each day of the observation. The onsite observation form will be completed on paper with a pencil, by the data collector while in the home to minimize disruption to the day care environment. After

completing the observations, the data will be entered into an automated computer system by the data collector and will be transmitted electronically to ICF International headquarters. Appendix D contains the onsite observation data collection instrument.

**Parent-Recall Telephone Interview.** For the feasibility study, parents (n=512) will be contacted during the same target week when the observation is scheduled to occur. The 12-minute parent-recall interview will be conducted by telephone with the parents of sampled children in each sampled FDCH. Calls to parents will begin on the Sunday following their target week and will continue for a total of 4 days. For the full national study, the balance of the national sample of parents (n=512) will be contacted within a selected target week corresponding to the monthly meal claim reporting period. The parent recall telephone interview will ask parents to report on the attendance of their children at the sampled FDCH and the meals their children received during the observation week while in care. Parents will also provide the names and grade levels of their other children enrolled in the sampled FDCH and a way to confirm the quality of the link and assure parents' recall is of the sampled children and not their other children enrolled in the same FDCH. The parent-recall interview for the national study will be conducted using the same procedures used for the feasibility study. The total sample size is (n=1024). The parent-recall interview questionnaire is in Appendix E and enrollment abstraction form is in Appendix F.

**Power Analysis.** The feasibility study is essentially a measurement study to compare meal counts by parent recall vs. onsite observation. Since national representativeness is not required, the statistical precision is for detecting between-group differences. Power analysis thus for this analysis is proxy of that used for statistical comparison of means or percentages by groups. The data collected will be in a hierarchical structure where meals are nested within children, children within homes, and homes within sponsor. With a focus on parent (equivalent to child) meal recalls, the study design will use a two-level design for power analysis: meals at level 1 and children at level 2. As proposed, with 512 parents whose children to be served an average of two meals per day in two days that are covered by the onsite observation, there will be a total of 2,048 meals. With a difference in cell percentage between cell b and c

in the 2X2 crosstab assumed as .15 (for McNemar's test) and with two hypothetical levels of intra-class correlation, the study should be able to achieve sufficient power:

- With an intra-class correlation of .02, the power would be about .90.
- With an intra-class correlation of .04, the power would be about .80.

**B.3 Describe methods to maximize response rates and to deal with issues of non-response. The accuracy and reliability of information collected must be shown to be adequate for intended uses. For collections based on sampling, a special justification must be provided for any collection that will not yield “reliable” data that can be generalized to the universe studied.**

The expected response rate among States and sponsors is 100%. The expected response rate among FDCHs is 95%. States, sponsors and FDCHs will be reminded that their participation in the study is required as a condition of Section 305 of S.3307 Child Reauthorization Act – “Healthy, Hunger-Free Kids Act of 2010, which states “ that States, State educational agencies, local educational agencies, schools, institutions, facilities, and contractors participating in programs authorized under this Act and the Child Nutrition Act of 1966 ([42 U.S.C. 1771](#) et seq.) shall cooperate with officials and contractors acting on behalf of the Secretary, in the conduct of evaluations and studies under those Acts.” Based on this requirement, a State/sponsor/FDCH provider participating in the Child and Adult Care Food Program (CACFP) is required to cooperate with officials and contractors acting on behalf of the Secretary in the conduct of evaluations and studies, as required under Section 28 of the Richard B. Russell National School Lunch Act (42 U.S.C 1769i). Refusal conversion techniques will be used as needed and emphasize that the study is not punitive. Similar techniques will be used with FDCHs, in addition to an informational sheet about the study, which is included in Appendix C. **FDCH providers will also receive a stipend up to \$75, which will offset time and material costs incurred due to their participation in the study. The stipend is offered with the understanding that copies will be reimbursed at 10 cents each, and reasonable faxing charges will be covered.**

The expected response rate for parents is 80%. Once again, refusal conversion techniques will be used by the researchers to build rapport and emphasize that the study is not punitive. Because validation efforts of the parent-

recall method are based upon a parent being able to accurately recall the attendance of her child at day care for a specific week, the same week onsite observations are conducted, the parent interview will be conducted as a telephone survey in order to assure quick response to the questionnaire. The interview will be conducted within a 5-day window after the close of the observation week to preserve the parents' ability to recall their children's attendance more accurately. Telephone interviews will also allow for more than one completion attempt to be made with respondents during the short period of time which will assist in obtaining a higher response rate. Mail surveys, which achieve much lower response rates, would not be appropriate to collect this data, given the need to preserve parent's cognitive recall in verifying the meals consumed by their child for a prior week. Pre-survey letters or publicity materials in regards to the parent-recall interview component of the study are not being distributed to family day care home providers or parents of the sampled children prior to participation. Pre-notification of this component of the study to either the family day care home providers or the parents might jeopardize the validity of their responses. The agency believes that pre-notification will notably alter parent responses, thus nullifying the possibility of identifying erroneous meal claims.

**B.4 Describe any tests of procedures or methods to be undertaken. Testing is encouraged as an effective means of refining collections of information to minimize burden and improve utility. Tests must be approved if they call for answers to identical questions from 10 or more respondents. A proposed test or set of test may be submitted for approval separately or in combination with the main collection of information.**

The pretest assessed the field procedures and protocols to be used to contact and recruit states, sponsors, parents and FDCH providers as well as the response time required for the data collection tools for the feasibility and main study. Each represented a distinct type of respondent that required the use of different approaches and data collection instruments in the pretest for each distinct type of respondent. FDCHs are the basic unit of analysis for the meal claims IPIA assessment; however, states and sponsors were contacted in the pretest to secure permission and cooperation to solicit information from FDCH providers and parents of children attending FDCHs. The pretest of the FDCH meal claims observation and provider survey instruments required the collection of verification and meals claims data from parents of children attending the FDCHs and sponsors. The pretest was conducted within OMB guidelines with three state agencies, four sponsoring organizations, nine FDCHs, and

nine parent/guardians. The pretest States were Virginia, Maryland, and the District of Columbia. These States were identified after the national sample was drawn, and are excluded from the national study. Selected States and sponsors were contacted in order to identify 9 FDCHs and 9 parents for the pretest. State agencies and sponsors received the contact letters and were asked to provide the requested data items electronically or in paper format. They were also asked about the level of burden associated with these requests and the preferred methods to submit data files. The pretest included the selection of FDCHs that were different in terms of program tiering status (either a Tier 1 or Tier 2 home), varied in the number of children enrolled, the types of meal served by the provider, and the ethnic diversity of providers. In-home observation visits and parent-recall interviews were conducted with 9 respondents each. The parent-recall instrument was tested with 9 parents, while the observation protocol was tested with 9 FDCHs. Nine total pretests were conducted for each distinct instrument/respondent. Study team members recorded any issues that were raised during the pretest with the data collection tools and revisions were considered as appropriate. Empirical estimates of respondent burden were also obtained through the administration of the study protocols for each respondent level. Non-response rates were calculated based on the participation rates in prior studies conducted with the same populations<sup>1</sup>.

**B.5 Provide the name and telephone number of individuals consulted on statistical aspects of the design and the name of the agency unit, contractor(s), grantee(s), or other person(s) who will actually collect and/or analyze the information for the agency.**

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<sup>1</sup> U.S. Department of Agriculture, Food and Nutrition Service, Office of Research and Analysis, *Child and Adult Care Food Program (CACFP): Improper Payments Data Collection Pilot Project*, by Rhoda Cohen, Lara Hulsey, Stacie Feldman, Claudia Gentile and John Hall. Project Officer, Fred Lesnett Alexandria, VA: September 2009.



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