An Experiment to Improve Coverage Through Revised Roster Instructions

FINAL REPORT

This evaluation reports the results of research and analysis undertaken by the U.S. Census Bureau. It is part of a broad program, the Census 2000 Testing, Experimentation, and Evaluation (TXE) Program, designed to assess Census 2000 and to inform 2010 Census planning. Findings from the Census 2000 TXE Program reports are integrated into topic reports that provide context and background for broader interpretation of results.

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EXECUTIVE SUMMARY

This report describes an experiment in Census 2000 to improve household coverage by making the roster instructions presented on the short form more understandable and more likely to be read. Census residence rules are difficult to communicate to respondents because they may exclude core household members, and include persons who are not considered to be members. In addition, census residence rules do not follow any simple logic which is easily expressed to respondents. Previous experiments with alternate roster formats or with presentation of the residence rules have indicated that the presence of residence rules on the form (whether in extended or abbreviated form) has some effect on one category of coverage errors: erroneous enumerations. (Pausche, 1994, Alberti, 1997.) However, neither of these studies showed any effect on the rate of omissions. Cognitive testing of decennial census forms has indicated that many respondents do not read the roster instructions (Gerber et al, 1997.)

Our hypothesis was that making the roster instructions more understandable and formatting them to enhance readability would improve household coverage. A series of experimental roster formats were designed and cognitively tested. The experimental panel and the control panel consisted of 5,200 mailout cases each, stratified into high and low coverage areas, with oversampling of the low coverage areas. The best of these roster formats became the experimental panel (Attachment A) in this experiment, which was fielded along with a control short form (Attachment B). Coverage was measured by a specially developed telephone coverage reinterview. The reinterview sample consisted of cases that had completed and returned the census form, had phone numbers, and were not sent to large household followup. These cases were subsampled in the high coverage area stratum. The total sample size for the reinterview was 4,218 households.

Design features of the experimental roster instructions included:

- Double-banked, bulleted instructions (including some modifications in wording)
- Person-count box in Question 1 placed after the instructions
- Inclusion of a direction to read the instructions
- Enclosing the roster instructions and Question 1 in an outlined box

Our findings relate to specific questions presented below.

Did the experimental instructions affect mail response rates?

Alteration of the instructions would not be acceptable if mail response rates were decreased. The mail return rate for the experimental panel was 73.52 percent. The mail return rate for the control was 73.07 percent. These mail return rates are not statistically significantly different.

Did the experimental instructions affect response to Question 1?

It is critical that the box where respondents record the number of persons in their households be completed, since it serves to flag missing person-level data and to cue large household followup. Any increase in item nonresponse in this item would be unacceptable. The response for this item is significantly higher in the experimental form (99.20 percent) than in the control (98.22 percent) at the 0.01 level of significance.

Did the experimental instructions affect omissions?

Omissions are persons who should have been listed on the census but were not. Such persons would be identified in the reinterview. We are concerned both with the omission rates and their demographic characteristics.

There is no significant difference between the form types. However, the experimental form had a statistically significantly lower omission rate for Hispanics in the low coverage stratum, dropping from 3.23% to 1.00%. With a sixty-nine percent lower omission rate, the difference was statistically significant at the 0.05 level of significance.

Did the experimental instructions affect erroneous enumerations?

Erroneous enumerations are persons who are included on the census forms although they are not legitimate census day residents. They include persons who have spent most of their time elsewhere, or who were in group quarters where they should have been counted on Census Day. Examples are college students living away from home and persons in the military stationed elsewhere.

The coverage reinterview allowed us to learn in detail what kinds of persons are erroneously enumerated on both forms. Following intended questionnaire logic, the number of persons who were erroneously enumerated on both forms was 92, with approximately half in the control and half in the experiment. However, there were a number of cases in which interviewers had followed an incorrect path which often gave us enough information to assess enumeration status. When these were considered, there were 128 erroneous enumerations.

Demographic characteristics for erroneously enumerated persons were compared on the basis of gender, age, race, ethnicity, and relationship to the householder. After comparing demographic characteristics for erroneous enumerations specifically, the one statistically significant finding is that the percentage reported as age 18 to 35 higher in the experiment in low coverage areas. Although this finding is significant, it reflects minuscule cell counts, and for this reason, we believe that this result might not be replicated in a larger sample. It is possible that other demographic characteristics would have demonstrated statistically significant differences with a larger incidence of erroneous enumerations.

Recommendations:

- **These graphical means of presenting residence instructions should be implemented to encourage respondents to read the instructions.**

- **Further research should be conducted to examine the effectiveness and limitations (including possible overuse) of integrating instructions and specific questions using an outline box.**

- **Further research should be conducted in order to examine the relationship between graphical presentation and meaning in self-administered questionnaires.**

- **Differences between the effectiveness of these techniques in high and low coverage areas suggest that social factors (such as educational attainment) may influence the effectiveness of particular graphical techniques. Further research is needed to expand our understanding of these phenomena.**

- **Additional research should be conducted to better understand the way in which respondents naturally read questionnaires.**

1. Background

The aim of this paper is to describe an experiment in the decennial census to improve household coverage by making the roster instructions presented on the short form more understandable and more likely to be read.

The problem of creating a household roster is common to many household surveys. Its importance lies in creating an appropriate list of persons about whom data is to be collected. However, the specific design and data demands of particular surveys often create specific rules of inclusion for various categories of individuals. For example, the roster may exclude certain persons who are considered core household members, or may include persons who are away for certain reasons but not for others. These rules, created to fulfill specific analytic or data requirements, do not necessarily mirror the way in which respondents would report on the membership of their households if left to themselves (Gerber, 1994; Gerber and Bates, 1994; Martin and Griffin, 1994). This creates the particular problem which the research described below attempts to examine: If there is a gap between the way in which respondents would naturally report their households and the data requirements of the survey, how can these requirements best be communicated to the respondent? This paper reports on developmental work to create a means of communicating decennial residence rules to respondents, and an experiment mounted in the census to test a revised form (Attachment A) against the Census 2000 form (Attachment B). This research was part of the Census 2000 Alternative Questionnaire Experiment. For this experiment, unedited and unimputed data were used.

This problem is particularly relevant for the decennial census, which is required not only to count the population, but to count it in the correct location as well. This has led to a complex network of decennial residence rules, which govern such issues as where college students should be counted; how to deal with the enumeration of persons connected with households who also have been present in institutional settings such as jails or hospitals; or distinguishing between the enumeration of crews of marine vessels vs. those on inland waterways. These rules do not follow any simple logic, and are far too lengthy to expect respondents to absorb in full. In addition, previous research has indicated that a number of these rules are clearly counterintuitive for respondents (Gerber et al., 1996). Classroom pretesting of the decennial questionnaire also indicates that respondents may not read the instructions (Gerber et al., 1997).

Since so much of the decennial census is collected in self-administered mode, the primary way of influencing respondent behavior to follow the residence rules lies in how the rules are worded and formatted on the questionnaire. This problem in communication consists of several elements. First, the roster instructions must be understandable to respondents. Second, the placement and format of the instructions on the page must encourage respondents to read the instructions. A third element, over which we have little control, is that respondents must be willing to follow the instructions, once they have read and understood them.

Past research on the design of the decennial mail questionnaire has looked at coverage issues. The 1994 Census Test attempted to improve coverage using a set of extended roster probes rather than instructions. However, this experimental form did not result in significant differences in gross error rates when compared with the control. Erroneous enumerations are persons counted on the roster, but should not have been counted, according to the residence rules. The erroneous enumeration rate for the experimental form was significantly smaller at the national level than the similar rate for the control. Omissions are persons omitted from the roster, but should have been included, according to the residence rules. No differences were found in the rate of omissions between the two (Pausche, 1994). In the National Content Test, three design versions were tested: a standard control with a full set of rules and a roster, a household count form with an abbreviated set of rules, and a household count box with no rules at all. Findings indicated that the estimated erroneous inclusions were greater in the forms with no rules than in the forms that present them in abbreviated form in households in High Coverage Areas. Thus, the presence of the rules can be demonstrated to have an effect on erroneous enumerations. The largest category of these erroneous enumerations was college students (Alberti, 1997). Both experiments found no differences in omission rates as a result of questionnaire design factors. Cognitive research has also been conducted to improve the wording of residence rules on the decennial questionnaire (Eisenhower et al, 1999). In approaching this work, our hypothesis was that we could improve coverage by affecting the reading behavior of respondents. That is, we hypothesized that improvements in coverage could be created by increasing respondents' attention to and understanding of the residence rules. Our expectation was therefore that the experimental panel would show fewer erroneous enumerations and fewer omissions than the control, Census 2000.

2. Methods

2.1 The redesign of the roster instructions:

In cooperation with our contractor, Westat, a series of cognitive interviews were undertaken to test and improve the format and wording of the decennial roster instructions. This section describes the results of the developmental research leading to the creation of the experimental roster. The aim of this research was to create and test new formats for the roster instructions without materially changing the content of those already adopted for inclusion on Census 2000. In addition, since we were required not to change any decennial content, innovations in this experiment were required to observe the spacing available on the census short form.

2.1.1 Respondents.

Because readability was an important factor in the development of the experimental form, half of the respondents in the cognitive study had less than a high school level of education. In addition, half of the respondents were living in "complex households;" i.e., households with members who were not all from the same nuclear family. This requirement resulted from previous research indicating that temporary, tenuously attached, or peripheral household members are often excluded from household

rosters. (Gerber et al. 1994, Gerber et al. 1996, Gerber et al, 1997.) Testing was accomplished in two rounds. There were 30 respondents in the first round of testing and 31 in the second round of testing. Respondents were interviewed using the following methods:

- Concurrent think-aloud: respondents were asked to say aloud what they were thinking and doing as they opened the package and completed the form. This procedure was used during Round I of testing.

- Retrospective think-aloud: In Round II probing occurred after respondents completed Question 1 in the questionnaire.

- Respondents were asked to provide definitions of some terms of relevance to the research.

- A list was made of all persons who the respondent could have included on the form, but did not include for one reason or another.

- Respondents were asked to complete a "Situations Test" in which they responded to a series of hypothetical situations asking them to determine if a person described in each situation should be counted according to census residence rules.

- Respondents were asked to rank order the various experimental forms on a variety of dimensions, including "eye-catching," "attractive format," "understandable directions," and "helpful directions."

2.1.2 Test Formats.

The following formats were tested in the first round of research:

- Box Format: In this version, Question 1 was placed in logical position, after the residence instructions. The question, instructions and answer box were placed within an outlined box, with a shaded ground.

- Double-Banked Format: Question 1 appeared, (also in logical position,) at the foot of the residence instructions, which were presented in two columns, labeled as ""Count these people" and "Do not count these people."

- Arrow version: Placing the response box in logical order, after the instructions, visually separates it from its response box. In order to connect the rules visually to the response box for Question 1, a set of arrows extended from the instructions to the response box.

Round II formats compared two alternate versions of the residence instructions with the control. The experimental formats included the Box Format (with some changes in wording,) the Census 2000 control and an additional version:

- The control: Census 2000 presented the residence instructions as a vertical list of rules separated by the phrases "Include" and "Do not include". In this version, the person-count box was placed directly after Question 1, and before the instructions.

- Hybrid Format: incorporated features of the Box and Double-Banked versions. (This version had an outline around a double banked list of rules.)

2.1.3 Cognitive Findings.

Cognitive findings from the first round of testing indicated that the Arrow Format, while it was eye-catching, was often confusing to respondents. As a result, it was dropped from the second round of testing. Both the Box and Double-Banked versions were quite effective in drawing the attention of respondents. The shaded ground and box outline which were used was effective in setting Question 1 off from the rest of the page, and unifying the tasks of reading the instructions and answering Question 1. Perhaps because of this, most respondents at least skimmed the rules before entering an answer in the person-count box. Respondents liked the Double-Banked form because they found the information easy to obtain, and it appeared distinctive. However, a small number of respondents had strong negative reactions to the Double Banked format, since they found that the text was harder to read.

In the second round of cognitive testing, respondents who completed both the Box version and the Hybrid formats generally indicated that they read or scanned the instructions before providing a response to Question 1. They reported being able to find information relevant to their unique situations easily. The Control (Census 2000) elicited the most problems in response. Specifically all but two of the respondents provided an answer to Question 1 prior to reading the directions. Two were required to change their answers to Question 1 after encountering the directions, which they found frustrating. Most respondents, however, proceeded directly to Question 2 without looking at the instructions at all. In rating the three forms on the dimensions described above, the respondents found both the Box and Hybrid formats to be more eye-catching and understandable than the Control.

Some changes in wording were also developed in the course of this cognitive research. First, some respondents did not understand the purpose of the instructions, and did not know what task they were connected with. It was therefore decided to include an instruction prior to Question 1 to direct them to read the instructions before answering. Other findings about content included:

- Respondents preferred to include the concept of "foster children" along with an inclusive statement asking them to list all children. The placement of the concept of "foster children" along with "roommates and boarders" struck them as wrong.

- Respondents preferred the terms "include" and "do not include" to "count" and "do not count." These terms, used in Census 2000, were retained.

- The term “correctional facility” was correctly identified by English-speaking respondents as a place where people undergo punishment for crimes. However, some thought it indicated a less severe institution than a prison. In addition, it was not familiar to some persons whose first language is not English. As a result, the example “jail” was added to the term “correctional facility.”

- The terms “housemate” and “roomer” were unfamiliar to some respondents. The terms “roommate” and “boarder” were familiar, and were therefore adopted for the experimental format.

2.2 Format Adopted for the Experiment:

The changes that were made included the following:

The instructions were placed in the logical order, between the first question and the answer box attached to it. The first question asks people to calculate the total number of persons living or staying in the household. This number serves as a control for the number of persons whose names are actually listed in the form. In Census 2000, the answer box for this question is placed directly after the question itself (in order to make sure that it was found by respondents). However, this made the roster instructions appear after the answer box, where presumably they might not be found until after the respondent had already created an answer. In order to integrate the instructions with the first question and the answer box (which was now separated from it) the entire sequence of the question, instructions and answer box was enclosed in a black line. The colored ground of the form was darkened somewhat in order to make the box stand out.

A direction to read the rules was introduced before the first question. This direction also attempted to give respondents the idea that census might have ideas about who was a resident that differed from their own. This was communicated in the phrase “according to our guidelines.”

The rules were somewhat reworded in order to make them more understandable to respondents. In particular, the language of the instructions was made more inclusive. For example, respondents were puzzled that the Census 2000 instructions only mentioned newborns and foster children: they wanted to see an instruction to list all children on the form. They also sometimes wanted to see an instruction to list everyone in the household. Census 2000 only provides instructions about selected categories of persons that are considered to be at risk of undercoverage.

The residence instructions were reformatted to appear in two bulleted lists, side by side within the instructions box. Respondents had the impression that the bulleted lists were shorter and contained less text. In addition, the double banking of the two lists brought the instructions to avoid erroneous enumerations (particularly the instruction about college students living away) to the top, right under the question. This was intended to make the instructions about erroneous enumerations easier for respondents to find.

2.3 The Telephone Reinterview Instrument:

The reinterview was designed to establish the most accurate possible roster for Census Day, April 1, 2000. This required the following steps:

First, a roster was collected for the current household membership. This step was taken as an aide for respondents' memories. Because it was necessary to key roster data from the mail portion of the questionnaire and transfer it to the reinterview instrument, the reinterview was not fielded until late July 2000. Therefore, a lag of four months or more had taken place since the respondents had completed Census 2000. We anticipated considerable memory decay. One technique of managing this was to begin with something easier to remember, the roster as of the time of the reinterview, and then to work backwards by asking about changes in the household since April 1.

The next step was to remove persons who had not lived there on April 1, and to ascertain if additional persons had moved out since April 1. The respondents were then asked an extended series of probes to assist in the discovery of additional persons who should have been included in the April 1st roster. These included probes about categories of persons thought to be frequently excluded from census rosters, and would constitute typical "omissions" from a census roster. The probes included questions about the following persons:

- Children: newborns, foster children, step children and children in joint custody
- Other relatives (cueing "aunts, uncles, grandparents, cousins, or any other kinds of relatives")
- Non-relatives (cueing "someone who rents a room from you or a friend staying with you temporarily while looking for a place to live")
- Mobile persons (cueing "any persons who were either temporarily away or moving around the beginning of April")
- Persons with no usual residence (cueing "people staying there who had no other permanent place to stay, even if you do not consider them to be regular members of your household").

This created an extended list of persons who were recalled by the respondent to have been connected with the household on April 1, but their true residence status had to be established by an additional set of questions. These questions were designed to discover erroneous enumerations in the list of names provided to us in the reinterview. Thus, respondents were probed to discover if any of the names they had provided to us were college students living away, members of the military living elsewhere, or persons who were in group quarters such as nursing homes, prisons or mental hospitals at the time of the census.

A final check on the residence status of persons still on the roster was then necessary. For each individual who had not been crossed off as an erroneous enumeration, two questions were asked. First, the reinterview asked if each individual had "another place to live or stay around the beginning of April." If they had no other place, and had been reported as living or staying there at the time of the census, they were counted as true residents. If another place to live or stay was reported, an additional question was asked: "On April 1st (were you/was Name) living or staying at that other place most of the time". If the answer to this was yes, we assumed that the sample household was not their primary place of residence, and they were not regarded as true residents.

Once we had established the roster of persons who we considered to be the true residents as reported to us in the reinterview, it was necessary to compare this list to the list provided on the census form.

During the main portion of the reinterview, the census roster was hidden from telephone interviews under a taped flap of paper. However, at this point, the interviewers opened the flap and reconciled the two lists of names. (Interviewer training had stressed the importance of not breaking this tape earlier in the interview, and both supervisors and interviewers later indicated that these instructions had been followed.) Persons who had not yet been mentioned in the interview were identified. The possibility still existed that these names had been placed on the census roster in error, so it was necessary to establish the true residence status of these additional persons. Questions were asked about such persons to establish if they had another place where they stayed most of the time, were away in college or the military, or were in group quarters in April 2000. At this point we had enough information to establish what we accepted as the true residence status of each name provided in the reinterview and in the original census form. It was only necessary to collect demographic information on true residents who had been identified as true residents only in the reinterview.

2.4 The Reinterview Sample:

The experimental and control panels consisted of approximately 5,200 mailout cases each. The national sample was stratified into high and low coverage areas (HCA and LCA), with oversampling of the low coverage areas. Overall, the high and low coverage area strata consisted of approximately 5,200 cases each.

The Westat sample consisted of cases that completed and returned the census form, had complete phone numbers for follow up, and had six or fewer persons listed in the household. The reinterview sample consisted of all cases in the low coverage area and all subsampled cases (subsampled randomly at a rate of 50%) in the high coverage area.

2.5 Estimation, Variance Estimation, and Significance Testing:

Households were sampled randomly at different rates within the two geographic strata for the reinterview. Weights were calculated by dividing the universe size by the sample size, where the universe is defined as the census mailout households in the United States.

Omission rates were calculated by dividing the number of omissions by the number of persons in the April 1st roster. Erroneous enumeration rates were calculated by dividing the number of erroneous enumerations by the number of persons in the census roster. Each of the four quantities used in calculating these rates is random. To compare rates across strata, standard errors were calculated using the statistical replication method of the stratified jackknife. As each household can have a variable number of erroneous enumerations, omissions, census persons, and April 1st persons, clusters were incorporated into the variance estimation at the household level.

Statistical significance testing was conducted on pairs of strata or treatments using a t-test that incorporates the covariance between the numerators and denominators in the calculation. The normal approximation to the t distribution was used to calculate p-values and establish statistical significance. A Bonferroni correction was made for multiple comparisons in the significance testing. The correction took account for analysis conducted by panel (control vs. experiment) for all areas, by panel for high coverage areas, and by panel for low coverage areas.

3. Limitations

Several restrictions were placed on the experiment. First, since we did not have a large number of experimental panels available, the redesigned rules were tested as a package. We therefore cannot separate the effects of wording vs. reformatting of the rules in our analysis. Second, since this experiment took place in the live Census, we could not radically alter the content of the rules. Third, in reformatting the rules, we were limited in our ability to change the placement or size of the residence instructions, since they had to fit into set decennial content. The coverage implications of these presentations of the rules were investigated using a telephone coverage reinterview. In order to establish the true residence status of all individuals listed on the census form, we had to capture the Census 2000 data (after the census closeout date), before a reinterview could occur to establish errors in coverage. This led to an unavoidable delay between the respondents' creation of the two rosters we wished to compare.

An additional limitation to the experiment was that we were only able to use part of the mail universe. We did not have the resources to accomplish our own large household follow up in the experiment. Therefore, census forms that were returned with more than six persons on the roster were diverted into large household followup and did not remain part of our sample.

Due to the design of the experiment and reinterview, results can only be projected to the mail universe. In addition, approximately 280 cases were not eligible for reinterview because there was no telephone contact information. Due to a probable combination of sampling (1,737 cases), our inability to interview in Spanish (134 cases), our inability to reinterview large households (265 cases), and a high number of noncontacts (1,126 cases), our overall demographic proportion of Hispanics may not match those in the Decennial Census.

4. Results

In assessing the functioning of the new residence instructions, the following elements must be taken into consideration: mail return rates, response rates to the first question on the census form, omission rates, and erroneous enumeration rates.

4.1 Response Rates:

Improvements in coverage must not affect the overall performance of the form. Thus, any deterioration of the mail return rate, even if accompanied by improvements in coverage, would not be acceptable. The mail return rate for the experimental panel was 73.52 percent. The mail return rate for the control was 73.07 percent. These mail return rates are not statistically significantly different. The mail return rates for the two panels in the High Coverage Area was approximately 76 percent and in the Low Coverage Area, approximately 59 percent. Differences in the rates between the two panels were not statistically significantly different, either in the High Coverage Area or the Low Coverage Area.

Overall, the total sample size sent out for reinterview was 4,218 households: 2,128 cases in the control and 2,090 cases in the experiment. There were 2,958 completed interviews: 1,497 completed cases in the control and 1,461 cases in the experiment. The Westat reinterview response rate was approximately 70%. This represents a response rate of 70.35% in the control and 69.90% in the experiment. Noninterviews included noncontacts, refusals, and language problems. There were 134 cases which could not be completed because of language problems, primarily Spanish-speaking respondents.

4.2 Response Rates to the First Question on the Census form:

One of the alterations in the experimental version of the form was the placement of the box where respondents were to record the number of persons in their households. It is critical that this item be filled by respondents since it serves to flag missing person level data and to cue large household followup. Any increase in item nonresponse in this item would be unacceptable. Table 1 shows that the response for this item is significantly higher in the experimental form than in the control. This demonstrates that the format integrating the instructions with the first question were highly effective.

Table 1: Percent of Respondents Who Answered the First Question on the Census

Panel	**Stratum**		
	All Areas	**HCA**	**LCA**
Control	98.22%	98.58%	96.26%
Experiment	99.20%	99.40%	98.04%
Total	98.71%	98.99%	97.14%
Control vs. Experiment: Statistical Comparison	$p < .01$	$p < .05$	$p < .05$

4.3 Demographic Characteristics of Correctly Enumerated Persons:

We have no direct way to tell who was included on the form as a result of the experimental intervention: therefore we must look at the overall demographic composition of the households to estimate what kind of individuals were being brought in by the experimental and control forms. This analysis follows what we know about the kinds of persons likely to be omitted from census rosters: young minority males and non-relatives were of particular interest to us. Tables 2.A and 2.B display the statistically significant findings.

There appears to be no difference between control and experimental panels in either stratum for age, gender or race. In the low coverage area, there were significantly more persons of Hispanic origin in the experiment than in the control.

Table 2.A: Percentage Reported As Hispanics

Panel	Stratum		
	All Areas	HCA	LCA
Control	8.36%	4.77%	25.44%
Experiment	10.42%	5.91%	31.66%
Total	9.38%	5.33%	28.51%
Control vs. Experiment: Statistical Comparison	$p < .05$	Not Signif	$p < .01$

Also, slightly more people reported living with relatives in the experiment than in the control. Relationship is only asked for Persons 2-6; i.e., not for Person 1 in the household.

Table 2.B: Percentage of Persons Reported As Living With Relatives

Panel	Stratum		
	All Areas	HCA	LCA
Control	90.12%	90.73%	87.37%
Experiment	92.37%	93.02%	89.48%
Total	91.21%	91.84%	88.41%
Control vs. Experiment: Statistical Comparison	$p < .10$	Not Signif	Not Signif

4.4 Omissions:

Omissions are persons who should have been listed on the census but were not. Such persons would be identified in the Westat reinterview. We are concerned both with the omission rates and their demographic characteristics.

There is no significant difference in overall omissions between the form types. The omission rate for the control was 1.13% and the omission rate for the experiment was 1.21%. However, the experimental form had a statistically significantly lower omission rate for Hispanics in the low coverage stratum. The omission rate for Hispanics by form type and strata is presented in Table 3.A.

Table 3.A.: Omission Rates for Hispanics by Panel and Strata

Panel	Stratum		
	Total	HCA	LCA
Control	3.54%	3.90%	3.23%
Experiment	2.55%	4.26%	1.00%
Total	3.00%	4.09%	2.02%
Control vs. Experiment: Statistical Comparison	Not Signif	Not Signif	$p < .05$

When looking at the kinds of persons who were omitted completely and who were found by the coverage reinterview, it can be determined what kinds of persons the two forms still miss. Any differences that are found can be an indication of how the form is functioning.

After comparing demographic characteristics for omitted persons, the one statistically significant finding, as shown in Table 3.B., is that the percentage reported as White was higher in the control in high coverage areas. Although this finding is significant, it reflects minuscule cell counts, and for this reason, we do not believe that this result would be replicated in a larger sample. It is possible that other demographic characteristics would have demonstrated statistically significant differences with a larger sample of omissions.

The factor "White only" was used to dichotomize race results in a very small sample, especially for omissions and erroneous enumerations. Similar analyses were not conducted on other racial groups because statistical significance was likely to be artifactual in such small samples.

Table 3.B: Percentage of Omitted Persons Reported As "White Only"

Panel	Stratum		
	Total	HCA	LCA
Control	58.42%	92.31%	13.33%
Experiment	39.67%	47.06%	21.95%
Total	48.86%	66.86%	16.92%
Control vs. Experiment: Statistical Comparison	Not Signif	$p < .01$	Not Signif

4.5 Erroneous enumerations:

Erroneous enumerations are persons who are included on the census forms although they are not legitimate census day residents. They include persons who have spent most of their time elsewhere, or who were in group quarters where they should have been counted on Census Day. Examples are college students living away from home and persons in the military stationed elsewhere.

No significant differences are found in erroneous enumerations by panel. The erroneous enumeration rate for the control was 0.40% and the erroneous enumeration rate for the experiment was 0.39%.

The coverage reinterview allowed us to learn in detail what kinds of persons are erroneously enumerated on both forms. Following intended questionnaire logic, the number of persons who were erroneously enumerated on both forms was 92, with approximately half in the control and half in the experiment. However, there were a number of cases in which interviewers had followed an incorrect path which often gave us enough information to assess enumeration status. When these were considered, there were 128 erroneous enumerations.

Demographic characteristics for erroneously enumerated persons were compared on the basis of gender, age, race, ethnicity, and relationship to the householder. After comparing demographic characteristics for erroneous enumerations specifically, the one statistically significant finding is that the percentage reported as age 18 to 35 is higher in the experiment in low coverage areas (see Table 4.A below). Although this finding is significant, it reflects minuscule cell counts, and for this reason, we believe that this result might not be replicated in a larger sample. It is possible that other demographic characteristics would have demonstrated statistically significant differences with a larger incidence of erroneous enumerations.

Table 4.A: Percentage of Erroneous Enumerations Reported as Age 18 to 35

Panel	**Stratum**		
	Total	**HCA**	**LCA**
Control	47.87%	55.56%	29.79%
Experiment	48.45%	44.44%	58.14%
Total	48.15%	50.05%	43.64%
Control vs. Experiment: Statistical Comparison	Not Signif	Not Signif	$p < .05$

To consider the effect of age on erroneous enumerations, we categorized age into three groups: 0-17, 18-35, and 36+. These categories were selected to consider the effect of mobility for children, young adults including college students, and the rest of the population. We did not restrict college age to a narrower focus because we were also interested in other highly mobile young adults. We uncovered statistically significant differences only in the 18 to 35 age group.

It is interesting to look further at the distribution of reasons given for erroneous enumerations for the 57 cases included in age 18 to 35. Although we cannot explain the significant findings, the distribution of reasons is suggestive for further research on erroneous enumerations.

Table 4.B shows that there are a number of college students in low coverage areas. This is not usually assumed. The table also shows that the largest number of erroneous enumerations are not accounted for by the categories of erroneous enumerations which are usually included in residence rules research, such as college, military, and various group quarters institutions. This suggests that highly mobile people account for a substantial proportion of erroneous enumerations. This confirms previously conducted qualitative research that has demonstrated that young adults in this age group may be highly mobile for other reasons than college.

Table 4.B: Frequency of Reasons Givens for Erroneous Enumerations for Persons Age 18 to 35

	Total	Control HCA	Control LCA	Experiment HCA	Experiment LCA
College	23	3	6	5	9
Military	4	1	2	0	1
Institution (GQ)	2	0	0	0	2
Unknown	28	6	6	3	13
Total	57	10	14	8	25

5. Recommendations and Conclusions

The experimental format for the residence instructions shows promise. It lowered the omissions rate for Hispanics in the low coverage stratum, while not affecting the mail back response rate. In addition, the experimental format significantly improved item response in the box recording the number of persons in the household. Lowering the number of omissions for Hispanics, especially in areas with low coverage, can have a beneficial impact in lowering the undercount.

It is unclear why Hispanics were the only segment of the population substantially affected by the change in the questionnaire. Further research would be necessary to disentangle the reasons for the effects that were seen here. Because only two panels were available to us, there is no way to tell whether the lower number of omissions were due to verbal changes or to changes in format which promoted the reading of the form. Additional research would be necessary to find ways to improve respondents performance with regards to erroneous enumerations.

Specific recommendations include the following:

- These graphical means of presenting residence instructions should be implemented to encourage respondents to read the instructions.

- Further research should be conducted to examine the effectiveness and limitations (including possible overuse) of integrating instructions and specific questions using an outline box.

- Further research should be conducted in order to examine the relationship between graphical presentation and meaning in self-administered questionnaires.

- Differences between the effectiveness of these techniques in high and low coverage areas suggest that social factors (such as educational attainment) may influence the effectiveness of particular graphical techniques. Further research is needed to expand our understanding of these phenomena.

- Additional research should be conducted to better understand the way in which respondents naturally read questionnaires.

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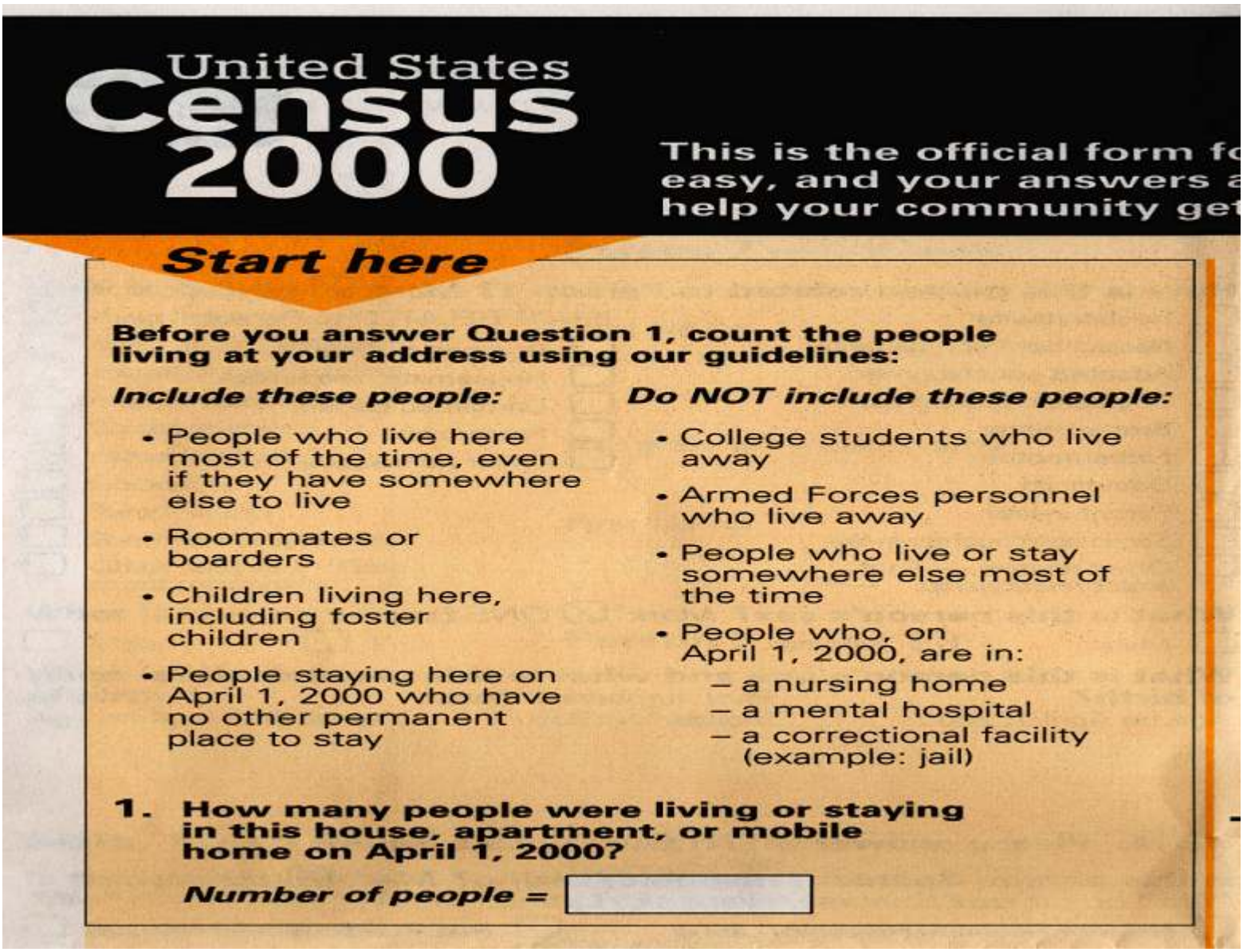
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This is the official form fo
easy, and your answers a
help your community get

Start here

Before you answer Question 1, count the people living at your address using our guidelines:

Include these people:

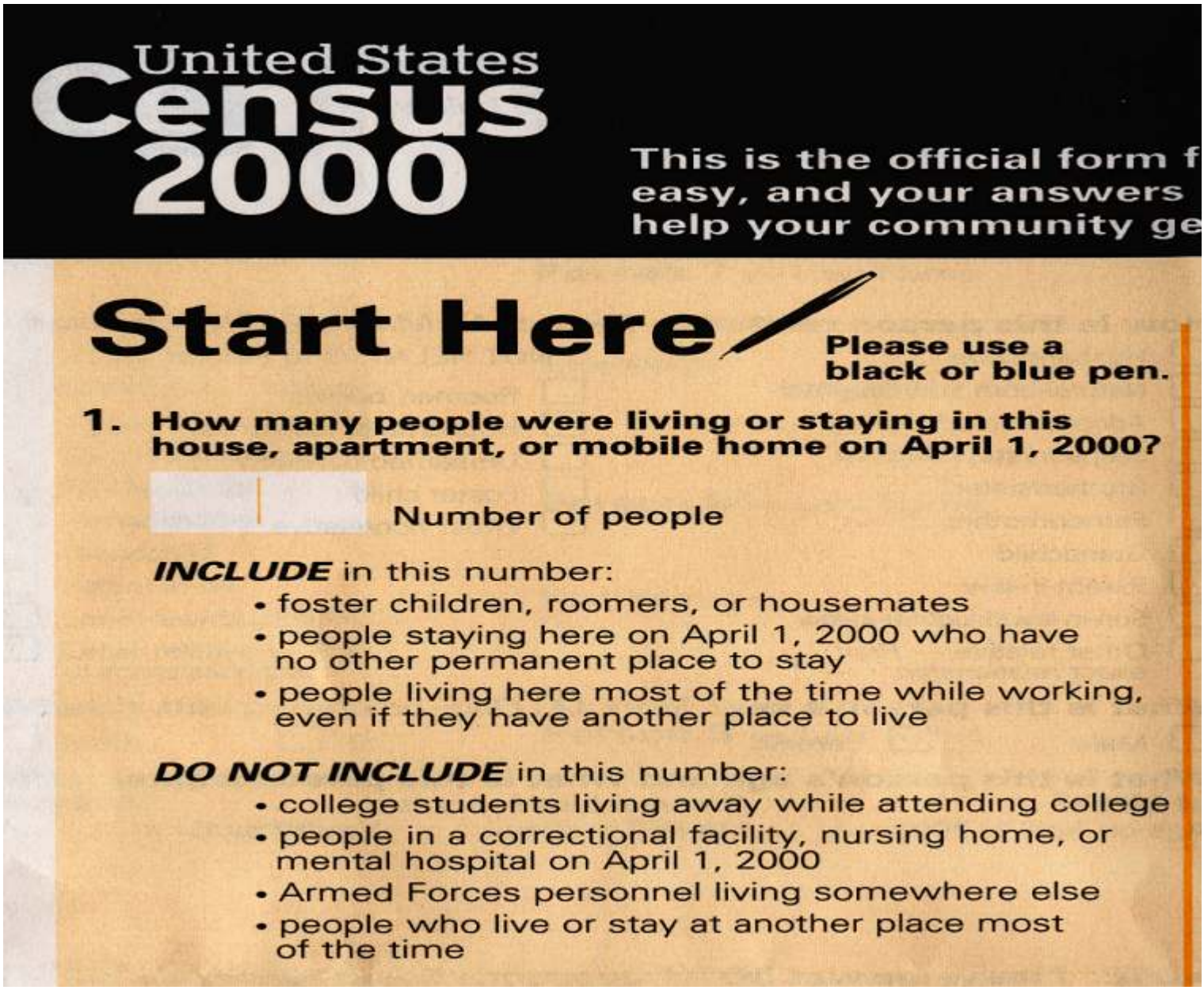
- People who live here most of the time, even if they have somewhere else to live
- Roommates or boarders
- Children living here, including foster children
- People staying here on April 1, 2000 who have no other permanent place to stay

Do NOT include these people:

- College students who live away
- Armed Forces personnel who live away
- People who live or stay somewhere else most of the time
- People who, on April 1, 2000, are in:
  - a nursing home
  - a mental hospital
  - a correctional facility (example: jail)

1. How many people were living or staying in this house, apartment, or mobile home on April 1, 2000?

Number of people = [ ]



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This is the official form f
easy, and your answers
help your community ge

Start Here

Please use a
black or blue pen.

1. How many people were living or staying in this house, apartment, or mobile home on April 1, 2000?

Number of people

INCLUDE in this number:

- foster children, roomers, or housemates
- people staying here on April 1, 2000 who have no other permanent place to stay
- people living here most of the time while working, even if they have another place to live

DO NOT INCLUDE in this number:

- college students living away while attending college
- people in a correctional facility, nursing home, or mental hospital on April 1, 2000
- Armed Forces personnel living somewhere else
- people who live or stay at another place most of the time