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MEMORANDUM FOR        The Distribution List

From:                     Arnold Jackson *[signed]*  
                              Acting Chief, Decennial Management Division

Subject:                  2010 Census Confidentiality Notification Experiment Report

Attached is the 2010 Census Confidentiality Notification Experiment Report. The Quality Process for the 2010 Census Test Evaluations, Experiments, and Assessments was applied to the methodology development and review process. The report is sound and appropriate for completeness and accuracy.

If you have questions about this report, please contact Joan Hill at (301) 763-4286.

Attachment

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# 2010 Census Confidentiality Notification Experiment Report

U.S. Census Bureau standards and quality process procedures were applied throughout the creation of this report.

Joan M. Hill  
Cynthia A. Rothhaas  
Frederic A. Lestina, Jr.  
Gianna S. Dusch

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**Decennial Statistical Studies Division**

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Ashley Landreth  
**Population Division**

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## EXECUTIVE SUMMARY

The purpose of the Confidentiality Notification Experiment was to determine the effect of confidentiality-related messages, written in language that respondents can easily understand, embedded in the cover letter that accompanied the initial census questionnaire mailing. The effect of two messages are of interest to this study: an administrative records-use message, which has not previously appeared in decennial production cover letters, and an alternative to the “standard” statistical purposes statement that was used in 2010 Census production materials. With regard to a administrative records data-linkage message, there is no definitive legal requirement that the U.S. Census Bureau provide respondents with a data-linkage notification; however, policy considerations come into play and stakeholders have recommended the inclusion of such a statement.

The Confidentiality Notification Experiment includes two experimental panels and a control panel. Each panel includes experimental cover letters used in the initial questionnaire mailing and the replacement questionnaire mailing. These experimental letters were sent to a sample of households in lieu of the production 2010 Census letters, which were the same as the experimental Control. All three panels were mailed according to the 2010 Census schedule. Two different treatments examine the main effects of the notification treatments. The first panel tests an added administrative records-use message within the cover letter while the second panel tests an added administrative records-use message in conjunction with an alternative statistical purpose message within the cover letter.

The results show that the experimental treatments affected neither mail return rates nor mail response rates. The experimental messages did not have an effect on respondent cooperation at the unit level. In terms of data quality, form completeness rates and item nonresponse rates results indicate that the experimental treatments had some effect on item nonresponse but we have no hypothesis-driven explanation for these results.

The item nonresponse rates for race were higher, in general, within the Medium Response Stratum (delineation of the sample into the high, medium, or low stratum was based on response propensity). The higher race item nonresponse for the “standard” statistical purposes message panel was driven by the item nonresponse for race for persons of Hispanic origin within the Medium Stratum. In addition, race item nonresponse was higher for the “alternative statistical purposes message panel for persons of Hispanic origin in the Low Response Stratum but we are unable to determine any clear, logical relationship to the treatments.

The administrative records-use message also appears to have increased item nonresponse to relationship, primarily within the Medium Response Stratum (each of the two treatment panels significantly differed from the Control). However, it is important to note that the item nonresponse for the relationship item is relatively low across all panels in that it is less than one percent.

The item nonresponse for the household-level items was not statistically significantly different across panels. In addition, the average household size across the experimental panels, as

measured by the population count data item on the first page of the questionnaire, was not significantly different among the panels at any level.

In terms of public reaction, to our knowledge, there were no discernible confidentiality concerns exhibited in 2010 via print media. Audio public reaction via media is more difficult to monitor. However, as far as we know, there were no consequential statements voiced over the air related to confidentiality concerns or the uses of census data.

Conducting the experiment in a census environment produced results that more closely predict the 2020 Census results compared to mid-decade tests. Although internal and external stakeholders may change the approach to informed consent messaging within the decade leading up to the 2020 Census, the results from this experiment can provide a benchmark for developing informed consent messages on record linking. While it may be useful to conduct mid-decade research on the underlying causal factors of the unexpected item nonresponse results, it is important to keep in mind that these item nonresponse differences are generally less than one percent (in most cases less than half a percent) and may not be of practical significance. We recommend moving forward toward 2020 with the revised statistics statement to describe uses of census data and the inclusion of the administrative records-use message on census materials. In addition, it is important to test how these messages affect response and data quality in other census data collection modes where the dissemination of this information may be more active (i.e., Internet respondents are required to click that the text was read) than passive (i.e., paper letter included in the mailout/mailback materials).

## **1. Introduction**

The purpose of the Confidentiality Notification Experiment was to determine the effect of confidentiality-related messages, written in language that respondents can easily understand, embedded in the cover letter that accompanied the initial census questionnaire mailing.

The following research question supported this study: “What is the effect on housing unit response rates of an administrative records-use message and alternative statistical purpose message in the initial questionnaire cover letter?” Specifically, the study looks at the effect of those messages on housing unit mail return rates to the mailout/mailback questionnaire and form completeness.

The effect of two messages are of interest to this study: an administrative records-use message, which had not previously appeared in decennial production cover letters, and an alternative to the “standard” statistical purposes statement that was used in 2010 Census production materials. The intent of these messages was to convey to respondents, in clear and understandable language, the meaning of the “statistical purposes” terminology that is used in the 2010 Census materials. The messages explain that statistical purposes includes use of census data in aggregate form, as well as usage involving linkage of the respondents’ individual data and data the U.S. Census Bureau receive from other federal agencies (i.e., administrative records). The Census Bureau performs this linkage in order to enhance census data with the administrative records data. These messages were developed with the intent that they should not, in and of themselves, depress unit or item response. This study was developed to assess the effect of those messages.

The results from this research will be used to develop statements for potential use in the 2020 Census materials. Results will also inform future policy decisions at the Census Bureau where issues of data protection and privacy are concerned. They will also provide general information on the topics of informed consent and administrative records-use to interested parties, such as the Federal Committee on Statistical Methods’ Subcommittee on Administrative Records.

## **2. Background**

Research literature demonstrates that at least two prominent factors can potentially influence response rates where the issue of confidentiality and privacy is concerned: the beliefs people hold and the words used to express pledges of confidentiality. First, the effect of privacy beliefs and attitudes has been studied in connection with the decennial census, and evidence suggests that privacy and confidentiality concerns have an effect on respondent behaviors. In 1990 and 2000, trust in the Census Bureau’s assurance of confidentiality predicted census mail response (Fay, Bates, and Moore, 1991; Singer, 2001). Martin (2001) found that respondents who had concerns about privacy were less likely to fill out a Census 2000 form completely and mail it back. Similarly, in Census 2000, Singer, (2001) found that four factors reliably predicted nonresponse: high privacy concerns, negative views on the Census Bureau’s confidentiality practices, disapproval of data sharing, and a lack of willingness to provide Social Security



Number. These findings suggest that item nonresponse (as well as unit nonresponse) may be connected with privacy and confidentiality concerns.

Second, in addition to respondents' attitudes about privacy, it appears the extent of the confidentiality promise (and its limitations and associated risks) also affects respondents' willingness to cooperate. Singer (1978) found that assurances of absolute confidentiality significantly decreased item nonresponse rates and resulted in better quality data for sensitive questions. The same study revealed that a confidentiality assurance paired with a qualifying statement (i.e., answers would be confidential, "except as required by law") depressed item response rates. In another experimental study, a panel given the message that other agencies might have access to their data produced a lower response rate (although not statistically significant) compared to a panel that received an assurance of confidentiality "in perpetuity" (National Research Council, 1979).

Furthermore, research suggests that confidentiality assurances may be counterproductive in some cases, appearing to raise, rather than lower, suspicion (Singer et al., 1992). There is some evidence that suggests stronger and more detailed assurances of confidentiality, when used in connection with surveys containing nonsensitive questions, raise suspicion and concern among respondents. Perhaps this is because they wondered if their data might fall into the wrong hands (Frey, 1986; Singer, Hippler, and Schwarz, 1992).

With these studies in mind, in 2006, staff at the Census Bureau set out to create a cohesive set of confidentiality messages, written in plain language, intended for use in the 2010 Census mailing package. Toward that goal, the Statistical Research Division (SRD) led an inter-divisional Working Group designed to review the relevant internal and external research related to this topic, consider both legal and policy issues that impact this type of legally-required messaging, and arrive at a set of messages that could be cognitively pre-tested and eventually integrated into the 2010 Census mailing package. The Decennial Management Division asked SRD to use plain language to rewrite, and then cognitively pretest, the legally-required messages included in the cover letter to the 2010 Census mail form. Major stakeholders across the Census Bureau, including the Policy and Legal Office, worked with SRD to develop the scope, main concepts, and syntax of these messages.

At that time, these stakeholders were interested in applying recommendations made by the National Research Council (1993) regarding the communication of certain legally- or ethically-required messages to respondents. Some recommendations were as broad as to be "clear and accurate" in conveying these messages, but others were as specific as informing respondents that their data may be linked with records from other sources. Stakeholders from both the Policy and Legal Offices were particularly interested in clarifying the meaning of the standard statement "...your responses will only be used for statistical purposes," because several years of SRD research suggested respondents found it difficult to understand the phrase "statistical purposes." They decided the "statistical purposes" phrase should be rewritten for clarity and that the topic of administrative records-use would have to be mentioned. As a result, the revised language told respondents their census responses would be used to "produce statistics," and they were also told that other government agencies may give the Census Bureau their information to improve the results. Based on the results of 50 cognitive interviews, the Working Group recommended

various revisions to the standard letter package used for the 2006 Census Test (Landreth, Gerber, and DeMaio, 2008).

In May 2007, the Data Stewardship Executive Policy Committee (DSEP) approved many of the Working Group's recommendations for use in the 2010 Census. For example, the front of the letter carried a streamlined confidentiality statement, while more technical messages were moved to the back of the letter. Two recommendations, however, were rejected: proposed language informing respondents about possible linkage of their answers to data from other agencies and the alternative statistical purposes statement. These recommended messages were rejected because the language had not been field-tested and could negatively affect mail response in 2010 Census production.

With regard to an administrative records data-linkage message, there is no definitive legal requirement that the Census Bureau provide respondents with a data-linkage notification; however, policy considerations come into play and stakeholders have recommended the inclusion of such a message. The American Community Survey currently includes a data-linkage message in the brochures that are included in its mailing package and in the brochure that interviewers leave with respondents. The Census Bureau tested both general and specific data-linkage messages in the Census 2000 Social Security Number, Privacy Attitudes, and Notification Experiment. Results indicated that item nonresponse did not suffer as a result. The general statement about data linkage depressed mail response rates slightly, while the specific statement about data linkage did not (Guarino, Hill, and Woltman, 2001).

The revised data-linkage message tested in the 2010 Census experiment performed well in qualitative testing, but needed to be field-tested before it could be used in future censuses. Testing the revised language in the 2010 Census gives us a better picture of the likely effect of including a linkage statement in the next census.

With regard to a statistical purpose message, the 2010 Census production letters used the statement "Your answers will only be used for statistical purposes, and for no other purpose." The SRD and the Working Group recommended that this statement in particular should be revised because past research consistently indicated the phrase "for statistical purposes" was not well understood by respondents (Gerber, 2002 and 2003, and Landreth, 2001). As a result, the 2006 cognitive pre-testing research evaluated an alternative statement ("...to produce statistics") with positive results; the alternative statement was consistently and correctly interpreted by most respondents. In October of 2008, the DSEP approved experimental testing to be conducted in the 2010 Census of a revised version of the 2006 recommended wording: "your answers will only be used to produce statistics" (Styles, 2008).

The Confidentiality Notification Experiment was designed to determine if the Census Bureau can provide what the agency considers improved versions of intelligible and informative statements to respondents on data linkage and the usage of census data, without impacting return rates or data quality. The statements are intended to provide respondents with specific information (i.e., informed consent) without significantly reducing public confidence and, consequently, mail response rates.

The experiment was intentionally conducted within a census environment where advertising and media could affect respondents' concerns about the confidentiality of their answers. Conducting the experiment in a census environment produces results that would more closely predict 2020 Census results compared to limited mid-decade tests. Although internal and external stakeholders may change the approach to informed consent messaging within the decade leading up to the 2020 Census, the results from this experiment can provide a benchmark for developing wording for messages informing respondents.

### **3. Methodology**

#### **3.1 Panel Design**

The mailing strategy for the Confidentiality Notification panels was similar to the production 2010 Census mail strategy. Each sampled household was mailed an advance letter, an initial questionnaire package, and a reminder postcard. The non-responding sample households were also mailed a replacement questionnaire containing the specific experimental treatment that corresponded to the initial questionnaire treatment. Sample households received census questionnaires that were identical in content to the 2010 Census mailout/mailback questionnaire.

The effects of the treatments on mail response and return rates are evaluated through the use of three panels as part of the 2010 Census Program for Evaluations and Experiments (CPEX).

The Confidentiality Notification Experiment includes two experimental panels and a control panel. Each panel included experimental cover letters used in the initial questionnaire mailing and the replacement questionnaire mailing. These experimental letters were sent to a sample of households in lieu of the production 2010 Census letters, which were the same as the experimental Control. All three panels were mailed according to the 2010 Census schedule. The Control panel was also used in conjunction with the 2010 CPEX Deadline Messaging and Compressed Schedule Experiment (Stokes, Bentley, and Reiser, 2009) and the Census 2000 Form Replication Alternative Questionnaire Experiment Panel (Reiser, Stokes, Compton, and Bentley, 2009).

Two different treatments examined the main effects of the notification treatments. One letter tested an added administrative records-use message while the other tested an added administrative records-use message in conjunction with an alternative statistical purpose message. Table 1 provides a summary description of the Control panel and two treatment panels (refer to Appendices for actual cover letter content).

**Table 1. Description of Confidentiality Notification Experiment Panels**

PANEL	TREATMENT
Control	<i>Control:</i> 2010 Census form and cover letter
CN1	<i>Confidentiality Notification 1:</i> 2010 Census form and a cover letter that tested the 2010 standard statistical purposes message (“Your answers will only be used <b>for statistical purposes, and for no other purpose</b> ”) on the front with the addition of the following administrative records-use message on the back:  <i>[To improve census results, other government agencies may give us additional information about your household. The additional information we receive is legally protected under Title 13, just like your census answers.]</i>
CN2	<i>Confidentiality Notification 2:</i> 2010 Census form and a cover letter that tested the experimental statistical purposes message (“Your answers will only be used <b>to produce statistics</b> ”) on the front with the addition of the same administrative records-use message as in CN1 on the back:  <i>[To improve census results, other government agencies may give us additional information about your household. The additional information we receive is legally protected under Title 13, just like your census answers.]</i>

These two panels are designed to determine whether the administrative records-use message affects response. In order to determine whether this message could potentially reduce response, we paired it with a letter that uses the standard statistical purposes message in CN1 and the alternative statistical purposes language in CN2. We did not intend to differentiate the effect of the alternative statistical purposes language, in part, because we do not expect that particular alternative wording would have any effect on response. In contrast, the administrative records-use linkage message was deemed more likely to negatively influence respondent compliance, although our hope was that the administrative records linkage message also would not affect response rates. When the alternative statistical purposes message was developed, staff from both the Policy and Legal Offices felt strongly that the administrative records message must be paired with the alternative statistical purposes message (CN2) in the spirit of full disclosure. Thus, the experiment was not designed to isolate the effect of the alternative statistical purposes language.

### 3.2 Sample Design

The sample for this experiment was only selected from within mailout/mailback enumeration areas including the 50 states and the District of Columbia. While the majority of the nation was represented in the sampling frame, Puerto Rico and the Island areas, as well as non-mailout/mailback areas such as remote Alaska, were not represented in this experiment. Thus, the results can only be generalized to the mailout/mailback universe.

The stratification for the sample design of this experiment was based on response propensity. The strata were defined as the areas delineated by the 2010 Census replacement mailing strategy: high response areas did not receive a replacement mail form; medium response areas received a

targeted replacement mailing to nonrespondents by a certain date; and low response areas received a blanket replacement mailing to all housing units, regardless of their response status. See Zajac and Letourneau (2008) for further details on the identification of the replacement mailing housing units. We used this stratification since it partitions by response propensity, which is a key measure for our experimental treatments.

Although we selected our sample based on the delineation of the 2010 Census replacement mailing strategy, as stated earlier, all nonrespondents in this experiment received a targeted replacement mailing. Since the experimental treatments could be correlated with response propensity, the 2010 Census three-tiered replacement strategy, which was defined by response propensity, may have confounded the compliance results and therefore was not implemented for the replacement mailing within the experimental panels. All panels were exposed to the same targeted replacement mailing strategy in order to maintain comparability of stratum results. (Note that replacement cover letter treatments corresponded to initial cover letter treatments within each panel.) This targeted replacement strategy is more realistic for the 2020 Census since the three-tiered design was implemented for the 2010 Census to alleviate operational concerns.

Regarding sample sizes for each panel, the target stratum sample sizes were originally about 5,000 housing units in the “High Response Stratum,” 5,000 housing units in the “Medium Response Stratum,” and 10,000 housing units in the “Low Response Stratum.” The sample allocation for these panels utilized a substantial oversampling of low response areas so that we could effectively measure any response rate changes for the traditionally hard-to-enumerate stratum. Actual sample sizes varied from the target sizes due to the incremental sampling scheme necessitated by the iterative address frame development (this unanticipated sampling process change, which involved sampling from each local census office’s housing units as they were added to the sampling frame database, had only a minor impact on sample sizes in the end). See Table 2 for the actual mailout size for each panel and stratum (Compton, 2009).

**Table 2. Mailout Sample Sizes (Number of Housing Units) by Panel and Stratum**

PANEL	STRATUM			
	Total	High Response	Medium Response	Low Response
<b>Control</b>	18,129	6,344	3,952	7,833
<b>CN1</b>	18,129	6,344	3,952	7,833
<b>CN2</b>	18,128	6,344	3,952	7,832
<b>Total</b>	54,386	19,032	11,856	23,498

Source: CPEX Sample File

### 3.3 Evaluation Measures

We conducted a variety of analyses in order to evaluate the success of the notification treatments. All three panels were evaluated based on respondent cooperation (as measured by return rates), form completeness, and public reaction.

### 3.3.1 Mail Return and Response Rates

The mail return rate is an indicator of respondent cooperation for this experiment. Mail return rates were the primary analytical measure used to evaluate the success of the treatment panels. Return rates, which are one measure of census cooperation, indicate if respondents in one panel are more (or less) likely to respond than those in another panel.

Return rates were calculated for the initial questionnaires and the replacement questionnaires, as well as for the initial and replacement questionnaires combined. The initial questionnaire was sent out to all experimental cases in the initial mailout. For each experimental case, a replacement questionnaire was sent if no response was received from the initial mailout. The rates exclude unmailable questionnaires and questionnaires acquired through Telephone Questionnaire Assistance (TQA). Mail response rates were also evaluated as an alternative measure of respondent cooperation. Essentially, the mail response rates include all housing units in the mailout universe, whereas the mail return rates include only occupied housing units.

The following formula was used to calculate return rates:

$$\text{Mail Return Rate} = \frac{\text{Unduplicated nonblank experimental mail returns}}{\text{Occupied Housing Units in Universe}} * 100$$

The following formula was used to calculate response rates:

$$\text{Mail Response Rate} = \frac{\text{Unduplicated nonblank experimental mail returns}}{\text{Housing Units in Universe}} * 100$$

Our hypothesis was that mail return and response rates for the experimental panels would not be statistically significantly different from the Control. Any differences are attributed to the specific set of treatments since all other factors are held constant for the experimental and Control panels.

### 3.3.2 Form Completeness and Average Household Size

A second evaluation measure is the proportion of the form completed by respondents. We calculated a form completeness statistic to evaluate whether the different notification treatments had an effect on data completeness for the census data. Form completeness was calculated at the person level and at the housing unit level. The person-level form completeness statistic is defined as the total number of person-level data items reported (nonblank) for each person. These items are relationship, sex, age/date of birth, Hispanic origin, and race. Note that item nonresponse rates for each item are also provided.

During the design phase of this experiment, we assumed that the items completed by the respondent would not be affected by the treatment messages on the questionnaire cover page. We evaluated this assumption because it is possible that the confidentiality concerns associated with the experimental messages could result in respondents' being less forthcoming with regard to supplying answers on the census questionnaire.

In addition, we examined the average household size across the experimental panels, as measured by the population count item on the first page of the questionnaire. Similar to form completeness, our assumption was that the number of household members listed by the respondent would not be affected by the treatment messages on the questionnaire cover page. We checked this assumption because the confidentiality concerns associated with the experimental messages could result in the respondents' being less forthcoming with regard to listing household members who may be tenuously attached to the household.

### *3.3.3 Public Reaction via Media*

We attempted to identify public reaction to the confidentiality notification messages by monitoring articles from the Public Information Office (PIO) daily media clips. We investigated various sources of negative public feedback. This information is anecdotal and not generalizable; we did not have the resources to develop and implement a comprehensive, systematic approach to provide actual estimates of negative public reaction.

## **3.4 Variance Estimation**

Due to the stratification in the sampling design, standard errors should typically be lower than those produced from a simple random sample. However, the homogeneity of results within a household for person-level statistics typically increase the standard errors since the majority of person information within a household is typically provided by one respondent. To account for these factors, we used a stratified jackknife replication procedure. Due to software and processing limitations, we used a random groups method to create the replicates. The random groups method involved sorting housing units in the order they were selected and reassigning them to 250 different groups, or replicates. This was more efficient than creating one replicate for each housing unit (i.e., primary selection unit), which would have resulted in tens of thousands of replicates.

To help ensure the validity of statistical inference when making multiple panel comparisons, we used a multiple comparison procedure. The family or group of comparisons must exhibit a stronger level of evidence against the null hypothesis in order for an individual comparison to be deemed "significant," which compensates for the number of comparisons being made. The multiple comparison procedure is intended to control for statistically significant differences between panels by reducing the probability of a Type I error or "false positive" (i.e., the null hypothesis is incorrectly rejected when it is true). The Dunn procedure was used for this analysis since all panels were compared to each other and the number of comparisons within a family was small. We used the Dunn procedure to control to an alpha error rate of 0.10 per family of comparisons.

## **4. Limitations**

The following limitations should be taken into account when interpreting the results from the experiment:

- The experimental questionnaires were provided in English only. The optimal design would have included Spanish/English bilingual questionnaires, as well as questionnaires in other languages since the treatments may differentially affect those respondents who might need language assistance. During the design phase of the experiment, the Census Bureau lacked resources and was confronted with timing constraints with some of our systems and, thus, could not include experimental questionnaires in languages other than English. However, we do not believe that this fatally biased the results of the experiment.
- Since the experiment was conducted as applied research in a census environment, events (both planned and unexpected) that occurred during the data collection phase may have affected the research results. Media and advertising play an important role in the public's willingness to participate in a census. The 2010 Census advertising campaign distributed confidentiality messages to various population groups. We cannot accurately measure whether the impact on respondents' census participation specifically due to confidentiality concerns was uniformly distributed across the population or whether it differentially affected specific population groups. However, this experiment's sample design includes panels that cut across population groups. Thus, we are able to measure significant changes in response rates due to the confidentiality treatments compared to the Control group, even in the presence of changes due to the advertisement campaign. However, we cannot differentiate the effect of advertising and the cover letter treatments.

## 5. Results

### 5.1 Universe

The universe for the mail response analysis consists of housing units that were selected in sample and mailed back a questionnaire (See Table 2). The universe excludes housing units considered unmailable, as well as housing units that were identified as having called TQA for assistance (as the assistance provided by an agent could have potentially compromised the experiment). The exclusions total 783 cases. For all other analyses, the universe consists of the occupied housing unit subset of the larger universe described above. Table 3 below shows the number of occupied housing units in the universe for mail return rate estimates, the form completeness estimates, and within household coverage estimates.

**Table 3. Mailout Sample Sizes (Number of Occupied Housing Units) Excluding Unmailables and TQA Callers**

PANEL	Total	STRATUM		
		High Response	Medium Response	Low Response
<b>Control</b>	15,299	5,784	3,409	6,106
<b>CN1</b>	15,304	5,787	3,414	6,103
<b>CN2</b>	15,350	5,798	3,416	6,136
<b>Total</b>	45,953	17,369	10,239	18,345

Source: CPEX Sample File



## 5.2 Mail Return and Mail Response Rates

Mail return rates were the primary analytical measure used to evaluate the success of the treatment panels. Return rates, which are one measure of census cooperation, indicate if respondents in one panel are more (or less) likely to respond than those in another panel.

Table 4 contains mail return rate estimates by panel for the initial and replacement questionnaires, as well as the combined results, at the national level.

**Table 4. Mail Return Rates (Percent) and Panel Differences by Mailing**

PANEL	Initial Mailing	Replacement Mailing	Overall
<b>Control</b>	71.4 (0.40)	6.8 (0.23)	78.2 (0.35)
<b>CN1</b>	71.8 (0.39)	7.1 (0.24)	78.9 (0.36)
<b>CN2</b>	71.8 (0.39)	7.0 (0.22)	78.9 (0.37)
<b>Differences</b>			
<b>Control – CN1</b>	-0.4 (0.56)	-0.3 (0.32)	-0.7 (0.50)
<b>Control – CN2</b>	-0.4 (0.57)	-0.3 (0.33)	-0.7 (0.51)
<b>CN1 – CN2</b>	<-0.1 (0.57)	<0.1 (0.32)	<0.1 (0.53)

Source: CPEX Sample and Response Files

\*No statistically significant difference exists between panels for a pairwise comparison error rate of  $\alpha=0.10$ .

Table 5 contains mail return rate estimates by panel for the initial and replacement questionnaires, as well as the combined results, within each stratum.

**Table 5. Mail Return Rates (Percent) and Panel Differences by Stratum by Mailing**

PANEL	HIGH STRATUM			MEDIUM STRATUM			LOW STRATUM		
	Initial Mailing	Replacement Mailing	Overall	Initial Mailing	Replacement Mailing	Overall	Initial Mailing	Replacement Mailing	Overall
<b>Control</b>	76.3 (0.57)	6.7 (0.33)	83.1 (0.46)	67.9 (0.87)	6.5 (0.44)	74.5 (0.82)	61.0 (0.58)	7.2 (0.32)	68.2 (0.59)
<b>CN1</b>	77.4 (0.54)	6.4 (0.34)	83.8 (0.48)	67.0 (0.81)	8.3 (0.48)	75.4 (0.80)	61.0 (0.65)	7.6 (0.36)	68.6 (0.59)
<b>CN2</b>	76.6 (0.56)	6.6 (0.33)	83.3 (0.51)	68.6 (0.82)	7.3 (0.42)	75.9 (0.80)	61.4 (0.63)	8.0 (0.32)	69.4 (0.62)
<b>Differences</b>									
<b>Control – CN1</b>	-1.1 (0.78)	0.3 (0.46)	-0.7 (0.67)	0.9 (1.20)	<b>-1.8 (0.64)*</b>	-0.9 (1.22)	<-0.1 (0.83)	-0.4 (0.47)	-0.4 (0.77)
<b>Control – CN2</b>	-0.3 (0.80)	0.1 (0.50)	-0.2 (0.69)	-0.7 (1.23)	-0.7 (0.63)	-1.4 (1.19)	-0.5 (0.87)	-0.8 (0.44)	-1.2 (0.82)
<b>CN1 – CN2</b>	0.8 (0.80)	-0.2 (0.47)	0.5 (0.74)	-1.6 (1.18)	1.1 (0.61)	-0.5 (1.17)	-0.4 (0.92)	-0.4 (0.47)	-0.8 (0.87)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

There were no statistically significant differences in overall mail return rates at the national and stratum levels. However, for the replacement mailing, there was a significantly higher return rate for the CN1 panel (containing the standard statistical purposes message) compared to the Control panel in the Medium Stratum. Although we applied the Dunn’s statistical adjustment to control the error rate per family of comparisons, which reduces the chances of spurious significant differences (i.e., false positives), this difference remained significant. In addition, the direction of this difference (i.e., CN1 had a higher return rate than the Control in this cell) is counterintuitive since one would expect the Control return rate to be higher than the experimental panel with the inclusion of the administrative records-use message. Thus, we have no hypothesis-based explanation for this difference and we attribute it to an effect of random sampling error.

Tables 6 and 7 contain the mail response rates by panel for the entire sample and by stratum, respectively. The mail response rates include all housing units in the mailout universe, whereas the mail return rates presented previously included only occupied housing units. The mail response rate is typically used as a survey implementation benchmark since its converse is roughly the workload for non-responding cases that require follow-up during a census.

**Table 6. Mail Response Rates (Percent) and Panel Differences by Mailing**

PANEL	Initial Mailing	Replacement Mailing	Overall
<b>Control</b>	63.1 (0.40)	6.0 (0.20)	69.1 (0.36)
<b>CN1</b>	63.4 (0.39)	6.3 (0.21)	69.7 (0.37)
<b>CN2</b>	63.6 (0.38)	6.2 (0.20)	69.9 (0.36)
<b>Differences</b>			
<b>Control – CN1</b>	-0.3 (0.55)	-0.3 (0.28)	-0.6 (0.52)
<b>Control – CN2</b>	-0.6 (0.56)	-0.2 (0.30)	-0.8 (0.52)
<b>CN1 – CN2</b>	-0.2 (0.54)	<0.1 (0.29)	-0.2 (0.54)

Source: CPEX Sample and Response Files

\* No statistically significant difference exists between panels for a pairwise comparison error rate of  $\alpha=0.10$ .

**Table 7. Mail Response Rates (Percent) and Panel Differences by Stratum by Mailing**

PANEL	HIGH STRATUM			MEDIUM STRATUM			LOW STRATUM		
	Initial Mailing	Replace-ment Mailing	Overall	Initial Mailing	Replace-ment Mailing	Overall	Initial Mailing	Replace-ment Mailing	Overall
<b>Control</b>	70.1 (0.59)	6.2 (0.30)	76.3 (0.51)	59.5 (0.85)	5.7 (0.39)	65.3 (0.82)	48.7 (0.54)	5.8 (0.26)	54.5 (0.56)
<b>CN1</b>	71.1 (0.56)	5.9 (0.32)	76.9 (0.53)	58.8 (0.80)	7.3 (0.42)	66.1 (0.81)	48.7 (0.58)	6.1 (0.29)	54.8 (0.56)
<b>CN2</b>	70.5 (0.55)	6.1 (0.30)	76.6 (0.52)	60.3 (0.79)	6.4 (0.37)	66.7 (0.80)	49.3 (0.57)	6.4 (0.26)	55.7 (0.58)
<b>Differences</b>									
<b>Control – CN1</b>	-1.0 (0.79)	0.3 (0.43)	-0.6 (0.72)	0.8 (1.21)	<b>-1.6 (0.57)*</b>	-0.8 (1.25)	<-0.1 (0.75)	-0.3 (0.39)	-0.3 (0.74)
<b>Control – CN2</b>	-0.4 (0.82)	0.1 (0.46)	-0.3 (0.74)	-0.7 (1.16)	-0.7 (0.56)	-1.4 (1.16)	-0.7 (0.81)	-0.6 (0.35)	-1.2 (0.82)
<b>CN1 – CN2</b>	0.5 (0.79)	-0.2 (0.44)	0.3 (0.76)	-1.5 (1.16)	0.9 (0.54)	-0.6 (1.20)	-0.7 (0.80)	-0.3 (0.38)	-0.9 (0.80)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

There were no statistically significant differences in overall mail response rates at the national and stratum levels. However, for the replacement mailing, there was a significantly higher response rate for the CN1 panel compared to the Control panel in the Medium Stratum, which is consistent with the mail return rate results. Similarly, we attribute this difference to an effect of random sampling error.

### 5.3 Form Completeness

Two measures are used to compare data quality across treatment panels: form completeness and item nonresponse. The average household size statistic is also presented to compare respondent-reported household size across treatment panels. The hypothesis was that the proportion of items completed by the respondent, as well as the respondent-reported household size, would not be affected by the treatment messages on the questionnaire cover page.

Table 8 below shows the person-level form completeness results. The person items included in this analysis are relationship, sex, age/date of birth, race, and Hispanic origin. Note that only unique person records with at least two data items are included in the person-level analysis.

The person-level form completeness statistic ranges from one to five, with one representing one of the five person-level data items reported and five representing all person-level data items reported. It is important to note that Person 1 is not asked the relationship question. For the purposes of this analysis, relationship for Person 1 is treated as having been reported. Therefore, it is impossible for Person 1 to report zero items. Consequently, Table 8 has separate displays for Person 1 and Persons 2 through 6. Responses for Persons 7 through 12 are not included in this analysis because they only include a subset of the items asked of Persons 1 through 6.

**Table 8. Percent of Person Records by Number of Items Reported and Differences by Panel**

PANEL		NUMBER OF ITEMS REPORTED	
		All 5 Items	1 through 4 Items
<b>Control</b>	Person 1	92.5 (0.26)	7.5 (0.26)
	Persons 2-6	89.9 (0.36)	10.1 (0.36)
<b>CN1</b>	Person 1	92.4 (0.28)	7.6 (0.28)
	Persons 2-6	88.3 (0.39)	11.7 (0.39)
<b>CN2</b>	Person 1	92.4 (0.26)	7.6 (0.26)
	Persons 2-6	88.9 (0.36)	11.1 (0.36)
<b>Differences</b>			
<b>Control-CN1</b>	Person 1	0.2 (0.39)	-0.2 (0.39)
	Persons 2-6	<b>1.6 (0.54)*</b>	<b>-1.6 (0.54)*</b>
<b>Control-CN2</b>	Person 1	0.2 (0.37)	-0.2 (0.37)
	Persons 2-6	1.0 (0.52)	-1.0 (0.52)
<b>CN1-CN2</b>	Person 1	<0.1 (0.38)	<-0.1 (0.38)
	Persons 2-6	-0.6 (0.51)	0.6 (0.51)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

The percent of person records for which all data were provided was significantly lower in the CN1 panel for Persons 2 through 6 compared to the Control panel. This result is directly associated with the significantly higher item nonresponse for the race item in the CN1 panel, as described in the item nonresponse discussion below Table 9.

Table 9 below shows the person-level item nonresponse results. Relationship for Person 1 is treated as having been reported since Person 1 is defined as the reference person (i.e., household member relationship data are based on Person 1). Responses for Persons 7 through 12 are not included in Table 9 because they only include a subset of the items asked of Persons 1 through 6.

**Table 9. Person-level Item Nonresponse Rates and Differences (Percent) by Panel**

PANEL	Relationship	Sex	Age/Year-of-Birth	Hispanic Origin	Race
<b>Control</b>	0.6 (0.06)	1.7 (0.09)	0.7 (0.07)	4.6 (0.20)	3.0 (0.19)
<b>CN1</b>	0.8 (0.07)	2.1 (0.10)	0.7 (0.08)	4.7 (0.21)	3.7 (0.22)
<b>CN2</b>	0.9 (0.08)	1.9 (0.09)	0.7 (0.08)	4.7 (0.21)	3.3 (0.19)
<b>Differences</b>					
<b>Control – CN1</b>	-0.2 (0.10)	<b>-0.4 (0.14)*</b>	-0.1 (0.10)	-0.1 (0.30)	<b>-0.7 (0.28)*</b>
<b>Control – CN2</b>	<b>-0.3 (0.10)*</b>	-0.1 (0.12)	-0.1 (0.10)	-0.1 (0.30)	-0.3 (0.26)
<b>CN1 – CN2</b>	-0.1 (0.11)	0.2 (0.14)	<0.1 (0.10)	<0.1 (0.30)	0.4 (0.28)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

The national-level item nonresponse rate for race within the CN1 panel was significantly higher than that within the Control panel. The higher race item nonresponse for CN1 seems to be driven by the item nonresponse for race for persons who self-identified as being of Hispanic origin, as shown in Table 10 below.

**Table 10. Race Item Nonresponse Rates and Differences (Percent) by Panel by Hispanic Origin\*\***

PANEL	RACE RESPONSE RATE	
	Hispanic Origin	Not Hispanic Origin
<b>Control</b>	15.3 (1.12)	0.6 (0.07)
<b>CN1</b>	19.8 (1.27)	0.5 (0.06)
<b>CN2</b>	17.0 (1.13)	0.5 (0.06)
<b>Differences</b>		
<b>Control – CN1</b>	<b>-4.5 (1.71)*</b>	<0.1 (0.09)
<b>Control – CN2</b>	-1.6 (1.55)	<0.1 (0.09)
<b>CN1 – CN2</b>	2.8 (1.70)	<0.1 (0.09)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

\*\*Note that persons who did not respond to the Hispanic Origin question are excluded. Since item nonresponse rates for Hispanic origin are not significantly different across panels at national and stratum levels, the panel domains for this table are presumably comparable.

We know from previous research that people who self-identify as Hispanic are generally less likely to answer the race question since they do not view race as a separate construct (Humes, 2009). Based on this, we would expect a higher race item nonresponse across all panels and not just for CN1. We know from additional analysis that the panels have comparable percentages of persons who identified as Hispanic and comparable Hispanic item nonresponse rates so we can eliminate demographic or population differences as a driver of the higher race item nonresponse.

In terms of the stratum results for race item nonresponse, Table 11 below shows that the race nonresponse is significantly higher for CN1 compared to the Control in the Medium Response Stratum and significantly higher for CN2 compared to the Control in the Low Response Stratum.

**Table 11. Race Item Nonresponse Rates and Differences (Percent) by Panel by Stratum**

PANEL	High Stratum	Medium Stratum	Low Stratum
<b>Control</b>	2.7 (0.25)	3.0 (0.40)	4.4 (0.34)
<b>CN1</b>	2.9 (0.28)	4.8 (0.50)	5.5 (0.39)
<b>CN2</b>	2.4 (0.23)	4.1 (0.47)	5.7 (0.41)
<b>Differences</b>			
<b>Control – CN1</b>	-0.2 (0.37)	<b>-1.9 (0.64)*</b>	-1.1 (0.54)
<b>Control – CN2</b>	0.3 (0.33)	-1.2 (0.59)	<b>-1.3 (0.53)*</b>
<b>CN1 – CN2</b>	0.5 (0.36)	0.7 (0.65)	-0.2 (0.57)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with controlling to an error rate of  $\alpha=0.10$  per family of comparisons.

In addition, race item nonresponse is also higher for CN2 compared to the Control panel for persons of Hispanic origin in the Low Response Stratum (this is based on our examination of race nonresponse by Hispanic origin by stratum).

Interestingly, race item nonresponse differences appear for Persons 2 through 6 but are not significantly different for Person 1 only. Since one respondent typically completes a census form for other household members (Person 1 is the respondent in the majority of cases), we would expect consistent item nonresponse results across household members if the treatments raised suspicion related to confidentiality.

In all of the data we examined in an attempt to find explanation for the unexpected race item nonresponse results, there were no differences in race item nonresponse when comparing the two experimental panels (which differ only by the statistical purposes message). However, we have mixed results for CN1 and CN2 compared to the Control within various subpopulations; we were unable to determine any clear, logical relationship to the treatments.

The relationship item nonresponse rate is significantly higher for the CN2 panel compared to the Control panel at the national level. Upon further analysis, we found that this difference is isolated within the Medium Response Stratum where CN2, as well as CN1, have significantly higher relationship item nonresponse compared to the Control panel. All significant differences hold when restricting the analysis to Person 1 only. Since results show no differences in relationship item nonresponse when comparing the two experimental panels (which differ only by the statistical purposes message), the differences in item nonresponse to relationship are presumably related to the experimental administrative records-use message. However, it is important to note that the item nonresponse for the relationship item is relatively low across all panels in that it is less than one percent. Even the significantly lower item nonresponse rates for the treatments generally differ from the Control by less than one percent.

The sex item nonresponse rate is also significantly higher for the CN1 panel compared to the Control panel at the national level, but it is not different at the stratum level. The percentage of males (and females) across panels and strata are not significantly different so the sex distributions do not appear to be differentially related to the item nonresponse rates. We have no hypothesis-driven explanation for the higher national-level item nonresponse rate for the CN1 panel compared to the Control.

Finally, additional analysis revealed that the person-level item nonresponse does not appear to be clustered within the same person boxes, that is, the item nonresponse for race, relationship, and age does not appear to be missing together for the same people. Thus, those persons without a response to race are no more likely to have the relationship (or sex) item blank. This seems counterintuitive since one would expect that, if the administrative records-use messages raised suspicion (rather than increased confidence), respondents would be more likely to leave more than one item blank for each person, or at least withhold information perceived as more personal identifiers such as age or date of birth. We are not seeing these patterns in the person-level item nonresponse results.

In addition, item nonresponse for person name is not significantly different across panels. This result seems counterintuitive since one would expect name to be viewed as one of the more sensitive items and consequently more likely to be affected by the presence of the administrative records-use message.

Table 12 below shows the housing unit form completeness results. The housing unit items included in this analysis are tenure and population count. The household-level data completeness statistic is defined as the total number of housing unit items reported for each occupied housing unit. Therefore, this statistic ranges from zero to two, with zero representing no housing unit data items reported and two representing both tenure and population count reported.

**Table 12. Percent of Housing Records by Number of Items Reported and Differences by Panel**

PANEL	NUMBER OF ITEMS REPORTED		
	2 of 2 (All items reported)	1 of 2	0 of 2 (No items reported)
<b>Control</b>	96.8 (0.18)	2.7 (0.17)	0.4 (0.06)
<b>CN1</b>	96.8 (0.17)	2.8 (0.16)	0.4 (0.06)
<b>CN2</b>	96.6 (0.19)	2.9 (0.17)	0.5 (0.07)
<b>Differences</b>			
<b>Control – CN1</b>	0.1 (0.25)	-0.1 (0.24)	<0.1 (0.09)
<b>Control – CN2</b>	0.3 (0.27)	-0.2 (0.25)	-0.1 (0.09)
<b>CN1 – CN2</b>	0.2 (0.25)	-0.1 (0.23)	-0.1 (0.09)

Source: CPEX Sample and Response Files

\* No statistically significant difference exists between panels for a pair-wise comparison error rate of  $\alpha=0.10$ .

Table 13 below shows item nonresponse results for the tenure and population count items.

**Table 13. Housing-level Item Nonresponse Rates and Differences (Percent) by Panel**

PANEL	Tenure	Population Count
<b>Control</b>	2.2 (0.15)	1.4 (0.11)
<b>CN1</b>	2.3 (0.14)	1.3 (0.11)
<b>CN2</b>	2.4 (0.15)	1.6 (0.13)
<b>Differences</b>		
<b>Control – CN1</b>	-0.1 (0.20)	<0.1 (0.16)
<b>Control – CN2</b>	-0.1 (0.21)	-0.2 (0.18)
<b>CN1 – CN2</b>	-0.1 (0.21)	-0.2 (0.17)

Source: CPEX Sample and Response Files

\*Denotes statistically significant difference between panels with a pair-wise comparison error rate of  $\alpha=0.10$ .

There were no statistically significant differences in household-level form completion rates or household-level item nonresponse rates at the national level or within each stratum.

Table 14 below presents the average household size across panels, as measured by the respondent-provided within-household population count.

**Table 14. Average Respondent-reported Population Count by Panel – Overall and by Stratum**

PANEL	AVERAGE WITHIN-HOUSEHOLD POPULATION COUNT			
	Overall	High	Medium	Low
<b>Control</b>	2.6 (0.02)	2.6 (0.02)	2.5 (0.03)	2.5 (0.02)
<b>CN1</b>	2.6 (0.02)	2.6 (0.02)	2.5 (0.03)	2.5 (0.03)
<b>CN2</b>	2.6 (0.02)	2.6 (0.03)	2.5 (0.03)	2.5 (0.03)
<b>Differences</b>				
<b>Control – CN1</b>	<0.1 (0.02)	<0.1 (0.03)	<0.1 (0.05)	<-0.1 (0.03)
<b>Control – CN2</b>	<0.1 (0.02)	<0.1 (0.03)	<-0.1 (0.04)	<0.1 (0.04)
<b>CN1 – CN2</b>	<0.1 (0.03)	<0.1 (0.04)	<-0.1 (0.05)	<0.1 (0.04)

Source: CPEX Sample and Response Files

\* No statistically significant difference exists between panels for a pair-wise comparison error rate of  $\alpha=0.10$ .

The average household size across the experimental panels as measured by the population count item on the first page of the questionnaire was not significantly different among the panels. Thus, the number of household members listed by the respondent was not affected by the treatment messages on the questionnaire cover page.

#### **5.4 Public Reaction via Media**

In an attempt to identify public reaction to the confidentiality notification messages, we monitored articles from the PIO daily media clips. The purpose of this search was to identify any media activity regarding the experimental confidentiality terminology and the additional administrative records-use message. The media activity was searched during 2010 from January 30 through April 5, which was the timeframe when most activity related to this topic would have occurred. Overall, we did not find activity that was directly related to the experimental messages in the questionnaire cover letter, but we did find information related to informed consent as the topic relates to the 2010 Census.

To our knowledge, there were no discernible confidentiality concerns exhibited in 2010 via print media. Audio public reaction via media is more difficult to monitor. However, as far as we know, there were no significant statements voiced over the air related to confidentiality concerns or the uses of census data.

### **6. Related Assessments, Evaluations, and/or Experiments**

The 2010 Census Mail Response and Return Rate Assessment and the Deadline Messaging and Compressed Mailing Schedule Experiment are related studies.



## 7. Lessons Learned, Conclusions, and Recommendations

The results show that the experimental treatments affected neither mail return rates nor mail response rates. Mail return rates were the primary analytical measure used to evaluate the success of the treatment panels. Thus, the experimental messages did not have an effect on respondent cooperation.

In terms of data quality, form completeness and item nonresponse rate results indicate that the experimental treatments had a slight effect on item nonresponse. The item nonresponse rates for race were higher, in general, within the Medium Response Stratum. The higher race item nonresponse for CN1 was driven by the item nonresponse for race for persons of Hispanic origin within the Medium Stratum. In addition, race item nonresponse was also higher for CN2 for persons of Hispanic origin in the Low Response Stratum. In sum, we are unable to determine any clear, logical relationship to the treatments.

The administrative records-use message also appears to have increased item nonresponse to relationship, primarily within the Medium Response Stratum. Since results show no differences in relationship item nonresponse when comparing the two experimental panels (which differ only by the statistical purposes message), the differences in item nonresponse for relationship are presumably related to the experimental administrative records-use message. However, it is important to note that the item nonresponse for the relationship item is relatively low across all panels in that it is less than one percent.

Although specific causality is difficult to identify, it is possible that confidentiality concerns associated with the experimental messages could have resulted in respondents' being less forthcoming with regard to supplying answers on the 2010 Census questionnaire. However, the average household size across the experimental panels, as measured by the population count data item on the first page of the questionnaire, was not significantly different among the panels at any level. Thus, the number of household members for which the respondent provided at least some data was not affected by the treatment messages on the questionnaire cover page.

At first glance, these results seem counterintuitive. We would not expect respondents to deem the relationship, sex, and race questions more invasive or sensitive than the name, date of birth, and Hispanic origin questions, after having been sensitized to privacy/confidentiality concerns in reading the administrative records-use paragraph. At minimum, census analysts, with knowledge about which data items are key variables for record linkage activities (e.g., name with middle initial and date of birth), would expect respondents exposed to the administrative records-use message to omit date of birth, at the very least.

However, these results may not be counterintuitive based on an alternative hypothesis. Respondents exposed to the treatments may have been affected by the administrative records-use paragraph, but not enough to fail to mail their forms back. It is possible a small portion of respondents felt obligated to comply by sending the form back but, in light of the administrative records-use paragraph, they decided to answer only a subset of the questions that, in their estimation, the Census Bureau had a need (or right) to know. In this case, respondents may have decided the government did not need additional information about the nature of their

relationships to others in the household, household members' sexes, and races (for Hispanics only). This effect may not have been strong enough to show as significant in both treatment panels across all items but appeared significant in a less consistent pattern. Although speculative at this point, further qualitative research may be appropriate for lending support to (or refuting) this explanation.

In the Census 2000 Social Security Number, Privacy Attitudes, and Notification Experiment, the Census Bureau tested both general and specific data-linkage statements. The 2000 results indicated that item nonresponse did not suffer as a result. However, the general statement about data linkage depressed mail response rates slightly, while the specific statement about data linkage did not (Guarino et al., 2001). In the 2010 Confidentiality Notification Experiment we see a different trend. The 2010 results generally suggest that mail return and response rates were not affected by the administrative records-use messages or the experimental statistical purposes message although several data items had significantly higher item nonresponse.

While it is tempting to interpret these findings as evidence that respondents never read the administrative records-use message, because we presume respondents in general ignore extraneous material (in this case, a cover letter), there is quantitative evidence to the contrary. A 2000 decennial study on marketing strategy reported that 58 percent of respondents reported receiving *and* reading the Census 2000 advance letter (Martin and Rivers, 2001). Even if a social desirability effect slightly inflates the proportion of respondents reporting having read the letter, the finding remains impressive. Thus, it is prudent to be cautious of any potential impact these messages may have on response rates and data quality.

The 2010 Confidentiality Notification Experiment determined that the Census Bureau can provide what the agency considers improved versions of intelligible and informative statements to respondents on data linkage and the usage of census data, without lowering return rates. The statements are intended to provide respondents with specific information (i.e., informed consent) without significantly reducing public confidence and, consequently, mail return rates. Although there was an isolated effect of the messages in the Medium Response Stratum for race (and relationship), which was driven by Hispanic respondents, the quality of the household-level items and most of the person-level items, as well as the respondent-provided within-household coverage, was not significantly affected.

Conducting the experiment in a census environment produced results that more closely predict 2020 Census results compared to mid-decade tests, despite changes in respondents' confidentiality concerns over the decade. Although internal and external stakeholders may change the approach to informed consent messaging within the decade leading up to the 2020 Census, the results from this experiment can provide a benchmark for developing informed consent messages on record linking. While it may be useful to conduct mid-decade research on the underlying causal factors of the unexpected item nonresponse results, it is important to keep in mind that these item nonresponse differences are generally less than one percent (in most cases less than half a percent) and may not be of practical significance.

We recommend moving toward 2020 with the revised statistics statement to describe uses of census data and the inclusion of the administrative records-use message in the English-language

census materials only. In addition, we recommend future research be undertaken on this topic in two critical areas.

First, additional research with these messages is highly recommended before they are placed in translated census or survey materials. Cognitive and field testing should be carried out with translated messages in linguistically isolated households to better understand the nonresponse effects that might appear with these populations due to socio-cultural and translation issues.

Second, before including these messages in other census data collection modes, it is important to test how these messages affect response and data quality. More research will be needed to understand response behavior when respondents' interactions with these messages become more active, thereby increasing its saliency for respondents (i.e., Internet respondents are required to click that the text was read) compared to the more passive and less salient approach used in this experiment (i.e., paper letter included in the mailout/mailback materials).

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## Appendix A – 2010 Census Questionnaire Cover Letter Content – Front Page

Draft 6 (7-30-2009)

March 15, 2010

A message from the Director, U.S. Census Bureau . . .

This is your official 2010 Census form. We need your help to count everyone in the United States by providing basic information about all the people living in this house or apartment. **Please complete and mail back the enclosed census form today.**

Your answers are important. Census results are used to decide the number of representatives each state has in the U.S. Congress. The amount of government money your neighborhood receives also depends on these answers. That money is used for services for children and the elderly, roads, and many other local needs.

Your answers are confidential. This means the Census Bureau cannot give out information that identifies you or your household. Your answers will only be used for statistical purposes, and no other purpose. The back of this letter contains more information about protecting your data.

## **Appendix A (continued) – 2010 Census Questionnaire Cover Letter Content – Back Page**

### **Your Answers Are Confidential**

Federal law protects your privacy and keeps your answers confidential (Title 13, United States Code, Sections 9 and 214). The answers you give on the census form cannot be obtained by law enforcement or tax collection agencies. Your answers cannot be used in court. They cannot be obtained with a Freedom of Information Act (FOIA) request.

As allowed by law, census data becomes public after 72 years (Title 44, United States Code, Section 2108). This information can be used for family history and other types of historical research.

Please visit our Web site at <[www.census.gov/2010census](http://www.census.gov/2010census)> and click on "Protecting Your Answers" to learn more about our privacy policy and data protection.

## Appendix B – CN1 Panel Cover Letter Content – Front Page

March 15, 2010

A message from the Director, U.S. Census Bureau . . .

This is your official 2010 Census form. We need your help to count everyone in the United States by providing basic information about all the people living in this house or apartment. **Please complete and mail back the enclosed census form today.**

Your answers are important. Census results are used to decide the number of representatives each state has in the U.S. Congress. The amount of government money your neighborhood receives also depends on these answers. That money is used for services for children and the elderly, roads, and many other local needs.

Your answers are confidential. This means the Census Bureau cannot give out information that identifies you or your household. Your answers will only be used for statistical purposes, and no other purpose. The back of this letter contains more information about protecting your data.

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## **Appendix B (continued) – CN1 Panel Cover Letter Content – Back Page**

### **Your Answers Are Confidential**

Federal law protects your privacy and keeps your answers confidential (Title 13, United States Code, Sections 9 and 214). The answers you give on the census form cannot be obtained by law enforcement or tax collection agencies. Your answers cannot be used in court. They cannot be obtained with a Freedom of Information Act (FOIA) request.

To improve census results, other government agencies may give us additional information about your household. The additional information we receive is legally protected under Title 13, just like your census answers.

As allowed by law, census data becomes public after 72 years (Title 44, United States Code, Section 2108). This information can be used for family history and other types of historical research.

Please visit our Web site at <[www.census.gov/2010census](http://www.census.gov/2010census)> and click on "Protecting Your Answers" to learn more about our privacy policy and data protection.



## Appendix C – CN2 Panel Cover Letter Content – Front Page

Draft 4 (7-22-2009)

March 15, 2010

A message from the Director, U.S. Census Bureau . . .

This is your official 2010 Census form. We need your help to count everyone in the United States by providing basic information about all the people living in this house or apartment. **Please complete and mail back the enclosed census form today.**

Your answers are important. Census results are used to decide the number of representatives each state has in the U.S. Congress. The amount of government money your neighborhood receives also depends on these answers. That money is used for services for children and the elderly, roads, and many other local needs.

Your answers are confidential. This means the Census Bureau cannot give out information that identifies you or your household. Your answers will only be used to produce statistics. The back of this letter contains more information about protecting your data.

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## **Appendix C (continued) – CN2 Panel Cover Letter Content – Back Page**

### **Your Answers Are Confidential**

Federal law protects your privacy and keeps your answers confidential (Title 13, United States Code, Sections 9 and 214). The answers you give on the census form cannot be obtained by law enforcement or tax collection agencies. Your answers cannot be used in court. They cannot be obtained with a Freedom of Information Act (FOIA) request.

To improve census results, other government agencies may give us additional information about your household. The additional information we receive is legally protected under Title 13, just like your census answers.

As allowed by law, census data becomes public after 72 years (Title 44, United States Code, Section 2108). This information can be used for family history and other types of historical research.

Please visit our Web site at <[www.census.gov/2010census](http://www.census.gov/2010census)> and click on "Protecting Your Answers" to learn more about our privacy policy and data protection.