Full Title: Sensitive Questions, Spillover Effects, and Asking About Citizenship on the U.S. Census
Short Title: Asking About Citizenship on the U.S. Census[*]

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Abstract

Many topics social scientists study are sensitive in nature. Although we know some people may be reluctant to respond to sensitive questions in surveys, we know less about how such questions could influence responses to *other* questions appearing later in a survey. In this study, we use the Trump administration's proposal to include a citizenship question on the 2020 Census to demonstrate how such spillover effects can undermine important survey-based estimates. Using a large survey experiment (n = 9,035 respondents), we find that asking about citizenship status significantly increases the percent of questions skipped and makes respondents less likely to report that members of their household are Hispanic. Not only does this demonstrate that sensitive questions can have important downstream effects on survey responses, but our results also speak to an important public policy debate that will likely arise in the future.

Keywords: sensitive questions, United States Census, citizenship question, demography, survey experiment

Replication files are available in the JOP Data Archive on Dataverse (http://thedata.harvard.edu/dvn/dv/jop). The empirical data has been successfully replicated by the JOP replication analyst.

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Introduction

Many topics of substantive political importance are sensitive in nature, meaning that researchers often must ask uncomfortable questions to better understand political behavior. Scholarship shows that question sensitivity is often influenced by three factors: social (un)desirability of the answers (Krumpal 2013), invasion of privacy (Singer, Van Hoewyk and Neugebauer 2003), and risk of disclosure to third parties (Tourangeau and Yan 2007). Underlying each of these factors is the supposition that some behaviors are more acceptable than others. For example, when researchers ask questions about "taboo" topics, respondents may suspect an invasion of privacy. Other questions may raise fears about the consequences of disclosing information to certain agencies or individuals, with respondents suspecting answers could be used against them. In such instances, even when survey administrators promise confidentiality, participants may be skeptical, especially when they believe they are uniquely at risk.

Although previous studies have shown sensitive questions can increase non-response rates (Rosenfeld, Imai and Shapiro 2016) and produce less truthful responses (Tourangeau and Yan 2007), less attention has been paid to how such questions can potentially affect answers to other items appearing later in the same survey, even ones seemingly unrelated. These "spillover" effects are important because one sensitive question could have broad effects on later important responses and, thus, on substantive research conclusions. In this study, we use a meta-analysis, simulation study, and an experiment centered around one such sensitive question – the Trump administration's proposal to include a citizenship question on the 2020 U.S. Census – to demonstrate how these spillover effects can undermine important survey-based estimates, like the number of Hispanics in the United States.

Evidence supporting such a relationship can be found in studies that show anonymity produces higher response rates and more truthful answers than confidentiality assurances (e.g, Ong and Weiss 2000). Although researchers assure respondents that their answers will remain private, the risk of exposure – however small – is higher when they provide identifying information. We argue similar concerns also lead sensitive questions to have downstream effects, adding an important new dimension to our understanding of survey research.

Background on Question Spillover Effects

According to Tourangeau and Yan (2007), question answers are produced in several stages beginning with

comprehension of the survey item and ending with a selected response once the respondent retrieves relevant material from memory. Spillover effects occur when previous survey questions affect the probability certain information enters into active memory when respondents formulate subsequent responses. Unlike traditional models, which assume that answers are based on preexisting evaluations stored in memory, we argue responses are actively formed when each question is asked (Tourangeau, Rips and Rasinski 2000), meaning that prior survey items can affect subsequent questions by influencing the accessibility of certain types of information. Ultimately, this can lead respondents to change their responses or drop out of a survey all together (De Leeuw, Hox and Huisman 2003).

This literature suggests sensitive questions appearing earlier in a survey could make later questions more sensitive than they would be otherwise. To borrow from the U.S. Census context, respondents might believe that answering questions about the citizenship status of themselves or members of their household could increase their chances of being investigated by the government, particularly if they live in a household with people identified as "Hispanic, Latino, or Spanish Origin" (to use the Census language). This concern could enter their active memory, making them more likely refuse to answer general household composition questions, regardless of actual citizenship status.

Although related to previous work on sensitive questions (e.g., Rosenfeld, Imai and Shapiro 2016), this example demonstrates the focal point of this study. Instead of considering increased non-response to the sensitive question itself, we aim to understand how sensitive questions can influence later answers to cognitively associated questions. The U.S. Census Bureau's proposal to include a question about citizenship status on the 2020 Census is useful in this regard because answers to a citizenship question could be cognitively linked to later questions about household composition. These answers are then used by federal and state governments to allocate funding and apportion congressional districts. If the spillover effects described in this study affect survey accuracy, then the citizenship question provides an important example of how such inaccuracies can carry meaningful policy consequences.

Application: Citizenship and the 2020 U.S. Census

Research Design

To test how asking a sensitive question – such as citizenship status of the respondent or members of his/her household – can affect responses later in the survey, we designed a pre-registered survey experiment that

mirrored the U.S. Census short form.[1] We randomly assigned half of the respondents (n = 4,497) to receive a "Citizenship Treatment" in which we asked, for each member of their household, "Is this person a citizen of the United States?" The other half (n = 4,538) did not receive the citizenship question for any household member. We independently randomized item order within each block of questions about different household members, thereby also randomizing the position of the citizenship question.

A third-party vendor (Qualtrics) recruited the survey panel and implemented the study in two waves. The first wave (n = 4,104) began on October 19, 2018 and targeted non-Hispanics (employing an English survey instrument), using self-reported demographic information maintained by Qualtrics. The race/ethnicity of the respondent and their country of origin is also determined using these data. The second wave (n = 4,931) began approximately one week later (on October 25, 2018) and targeted Hispanics (using English and Spanish survey instruments) in order to facilitate meaningful subgroup inferences. In Section S2 of the Supplemental Information (SI), we report balance statistics and demographic breakdowns for both survey waves.

An obvious difference between our study and the actual U.S. census is our status as academic researchers, which might lead to confidence among respondents that data would not be used for immigration purposes. To assess this, we also randomly assigned half of the respondents (n = 4,454) to receive a "Census Prompt" treatment, independent of the first randomization, consisting of a short note at the bottom of their consent form saying "Your responses will be shared with the U.S. Census Bureau," and requiring respondent consent. The other half (n = 4,581) received no prompt. (Additional details on survey logistics can be found in Sections S1-S2 of our SI.)[2]

Results

If sensitive questions increase concerns over exposure to risk, then respondents should be less willing to give identifying information or information that could attract government suspicion. Although the average respondent is unlikely aware of the increasing risk of Census re-identification, fears over confidentiality

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[1]In Section S1.7 of the SI, we explain differences between our approach and that of the Census. We also show in this section that our main results are not substantially influenced by these differences.

[2]Our experimental design was pre-registered with EGAP (Evidence in Governance and Politics) and added to the registry on October 23, 2018 (ID# 20181016AA). Additional hypotheses not discussed in the main text are discussed in Sections S3.2 and S3.3 in the SI. In these sections, we also explain a few deviations from our original pre-analysis plan, most notably our hypotheses related to the Census Prompt, which were difficult to test given the survey ultimately approved by Qualtrics.

leading up to the 2020 Census supports our underlying mechanism (Brown et al. 2018). We therefore expect the citizenship question will (1) increase item non-response and (2) lead to fewer Hispanic household members being reported. Moreover, these effects should be more pronounced among Hispanic respondents, especially those born outside the United States, since these groups were found to be especially sensitive to the citizenship question (Brown et al. 2018).

Treatment Effects on Item Non-Response

Beginning with Table 1, we operationalize survey item non-response as the percent of the survey questions for which the respondent did not submit a response. Since our Citizenship Treatment was not introduced until Q5, we only consider questions appearing after this question when assessing treatment effects. Using this measure, we find that receiving the Citizenship Treatment increases the share of questions skipped after Q5 by 3.15 percentage points (t-statistic = 3.897, p-value less than 0.001). Similar results are found when the dependent variable is the percent of respondents who completed at least 80% of the questions after Q5 with the Citizenship Treatment increasing this percentage by 3.35 percentage points (t-statistic = 3.978, p-value less than 0.001). These patterns suggest that introducing a sensitive question – like a question about U.S. citizenship – not only increases item non-response, but also leads to more respondents skipping a substantial proportion of the survey.[3]

Consistent with expectations, we also find this effect was more pronounced for Hispanics, who skipped 4.21 points more of the questions after the Citizenship Treatment was introduced (t-statistic = 3.494, p-value is less than 0.001). When we subset the data to Hispanics from Mexico or Central America, we find the percent of questions skipped increases further, to 11.04 percentage points (t-statistic = 3.298, p-value = 0.001). Since Hispanics who originate from Puerto Rico and Cuba tend to be U.S. Citizens, we also pre-registered this subgroup as an important point of comparison. As anticipated, the corresponding effect among Hispanics who listed Puerto Rico or Cuba as their birth country was far smaller: 1.78 percentage points (t-statistic = 0.566, p-value = 0.572). We also found a smaller difference of 2.15 percentage points for non-Hispanics (t-statistic = 2.360, p-value = 0.018).

Treatment Effects on Percent of Household Reported as Being Hispanic

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[3]We find the Census Prompt does not significantly affect the share of questions skipped after Q5 (t-statistic = 1.570, p-value = 0.116). However, in the SI, we show the Census Prompt does significantly increase the percent of questions skipped in Q1-Q4 (t-statistic = 2.433, p-value = 0.015) before the citizenship question is introduced.

In Table 2 we consider the effect of the citizenship question on the share of household members identified by the respondent as being of "Hispanic, Latino, or Spanish Origin." That is, we consider the percent of household members identified as Hispanic (as opposed to other ethnicities or non-responses) by each respondent. We again only consider household members whose race/ethnicity is assigned by the respondent after our Citizenship Treatment is introduced.

Table 2 shows – again consistent with spillover effects – that those receiving the Citizenship Treatment reported fewer Hispanic household members (31.01 percent of households) compared to those in the control condition (35.04, t-statistic = 4.244, p-value less than 0.001). Hispanic respondents receiving the Citizenship Treatment reported 5.95 percentage points fewer household members of Hispanic origin than their counterparts in the control conditions (59.38 vs. 53.43, t-statistic = 4.359, p-value less than 0.001). The corresponding difference among non-Hispanic respondents is smaller and less significant 1.38 points (8.43 vs. 9.81, t-statistic = 1.664, p-value = 0.096).

We again see larger, significant effects for Hispanics listing Mexico or a country in Central America as their country of birth. Here, respondents receiving the Citizenship Treatment reported 8.32 percentage points fewer household members of Hispanic origin (75.35 percent, compared to 83.67 percent in the control condition; t-statistic = 1.932, p-value = 0.055). Once again, among Hispanics who listed either Puerto Rico or Cuba as their birth country, the corresponding difference is smaller (4.41 points; 81.33 vs. 85.74 in the control condition) and insignificant (t-statistic = 1.077, p-value = 0.283).

Extrapolating to the 2020 U.S. Census

We now show how the downstream effects we have outlined above could influence important survey-based estimates, like the number of Hispanics in the United States. Since our survey purposefully oversampled Hispanics (51.10 percent of our sample) relative to the U.S. population (16.35 percent, as reported by the 2010 U.S. Census), we first created post-stratification weights to produce more nationally representative estimates. (We provide more details in Section S2-S3 of the SI.) Applying the estimated national-level treatment effect to the U.S. population, as reported by the 2010 U.S. Census (308,745,538), we estimate that asking about citizenship would reduce the number of Hispanics reported in the 2020 Census by 6,072,068 or 12.03 percent of the 2010 Hispanic population (50,477,594). The 95-percent confidence interval surrounding our estimate is 5,761,284 to 6,382,820, which represents a decrease of 11.41 to 12.64 percent relative to the 2010 Hispanic population. Although higher, our results are also consistent with a recent randomized

trial conducted by the U.S. Census Bureau (Poehler et al. 2020).[4]

Extrapolating to Other Sensitive Questions

We lastly consider how spillover effects could impact inferences made in other substantive applications. In Section S3.7 of the SI, we found 185 articles (or 13.63% percent) published from 2015 to 2020 in the *American Journal of Political Science*, *American Political Science Review*, and *Journal of Politics* that included at least one demographic variable previously identified in the literature as sensitive. This suggests the generalizability of the results reported in Tables 1 and 2, even in studies unrelated to the U.S. Census. For example, Rosenfeld, Imai and Shapiro (2016) asked indirect questions about a 2011 Mississippi abortion ballot initiative before respondents were asked directly whether they voted for the measure. The direct question always appeared later in the survey because "respondents refrained from answering indirect questions about politics at higher rates after the direct question was administered" (787). In Section S3.6 of the SI, we find evidence consistent with this statement. Specifically, we show that respondents were more likely to change their party identification when they were repeatedly asked sensitive questions and this effect is more pronounced for individuals who are most likely to be sensitive to ballot initiative questions.

However, these additional "doses" of sensitivity are different in nature to the treatment we used in our main application. In Section S3.8 of the SI, we therefore simulate how our treatment effect might influence the conclusions of eight studies selected from the aforementioned meta-analysis.[5] We ultimately find that, for each percentage point increase in sensitivity – as represented by the percent of respondents who dropped out of the survey after receiving a sensitive question – the confidence intervals surrounding the model coefficients presented in the papers would increase on average by 37.62 percent. When our simulation was conducted using the data from the two studies that actually included a citizenship question, the confidence intervals on the model coefficients increased by, on average, 111.39 percent. This means that we found the largest sensitivity effect in those studies that included a citizenship question.

Discussion and Conclusion

On July 21, 2020, President Donald Trump signed a memorandum barring undocumented immigrants from

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[4]Section S3.10 of the SI explains why our estimates differ from this study. Notably, Poehler et al. (2020) use pre-treatment questions to estimate their effects and the Supreme Court ruled against the citizenship question during their study.

[5]These eight were selected based on availability of reproducible code/data and diversity of methods. We also only considered surveys of the United States and choose studies across the various journals. See S3.8 in the SI for more details.

being used in congressional apportionment following the 2020 Census. This suggests that questions related to citizenship status and the U.S. Census are likely to continue for the foreseeable future. Using this example as an application and a companion simulation, we find sensitive questions can increase item non-response, decrease the reporting of certain demographic information, and dramatically increase the size of confidence intervals around key quantities of interest, ultimately making inference more difficult. We also find that, when applied to something as important as the 2020 U.S. Census, such spillover effects can have important policy implications. Lastly, similar effects likely also impact other studies, including the 185 published studies we identified in Section S3.7 of the SI.

Scholars sometimes need to know answers to uncomfortable questions in order to address important research topics. As we show here, however, sensitive survey questions could have important unintended consequences. This is not to say that such questions should be avoided. Rather, our study suggests caution: researchers should think carefully about how sensitive items could influence subsequent questions. We offer the citizenship question as one important example of how such effects can impact important policy outcomes, but additional work is needed to see whether other policy issues that rely on sensitive questions are similarly affected.

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Table 1: Treatment Effects on Item Non-Response

	Treatment	Control	Difference	t-statistic	p-value
Percent of All Questions Skipped					
All	30.03	26.88	3.15	3.897	< 0.001
Hispanic	38.84	34.35	4.49	3.674	< 0.001
Mexico/Central America	19.18	8.97	10.21	3.153	0.002
Puerto Rico/Cuba	12.96	12.10	0.86	0.276	0.783
Non-Hispanic	20.94	18.99	1.95	1.973	0.049
Percent of Respondents Who Skipped At Least 80% of the Questions					
All	21.70	18.36	3.35	3.978	< 0.001
Hispanic	29.98	24.91	5.06	3.862	< 0.001
Mexico/Central America	11.11	2.33	8.79	2.800	0.006
Puerto Rico/Cuba	6.25	3.77	2.48	0.853	0.395
Non-Hispanic	13.16	11.42	1.73	1.753	0.080

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last two columns report results from two-sample t-tests. Since the Citizenship Treatment was not introduced until Q5, we only consider questions appearing after this question when assessing treatment effects. More details about the measures used in this table can be found in Section S1.4 in the SI.

Table 2: Treatment Effects on Percent of Household Reported as Being Hispanic

	Treatment	Control	Difference	t-statistic	p-value
All	31.01	35.04	−4.03	4.224	< 0.001
Hispanic	53.43	59.38	−5.95	4.360	< 0.001
Mexico/Central America	75.35	83.67	−8.32	1.932	0.055
Puerto Rico/Cuba	81.33	85.74	−4.41	1.077	0.283
Non-Hispanic	8.43	9.81	−1.38	1.664	0.096

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last two columns report results from two-sample t-tests. More details about the measures used in this table can be found in Section S1.4 in the SI.

Supporting Information for:
Estimating the Effect of Asking About Citizenship on the U.S. Census: Results from a Randomized Controlled Trial

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S1 Materials and Methods

S1.1 EGAP and IRB

Our experimental design was pre-registered with EGAP (Evidence in Governance and Politics) and added to the registry on October 23, 2018 (ID# 20181016AA). The Harvard Institutional Review Board (IRB) questioned whether our Census prompt treatment was deception. Since the nature of the study meant that we were open to sharing data with pertinent public agencies and other relevant actors both inside and outside government, our treatment does not constitute deception. We have also arranged to share our findings with several former Census Bureau staff members, who, in turn, have indicated that they will share the data with current Census staff. In addition, all data (de-identified) would be made publicly available, per standard practices within social science. With these assurances, the Harvard IRB agreed to approve our proposed design and assigned our study the following protocol numbers: IRB18-1445, MOD18-1445-01, and MOD18-1445-02.

S1.2 Sampling Design

A third-party vendor (Qualtrics) recruited the survey panel, which included actively managed research panels and panelists drawn from third-party vendors. We chose this approach partly because, compared to using an established polling firm, it reduced the possibility that we were surveying repeated or professional survey takers who would not reflect the population targeted by the U.S. Census – all U.S. residents. The panel was purposefully not restricted to U.S. citizens (just residents aged 18 or over).

The survey was conducted in two waves, each with an identical incentive structure. The first wave targeted non-Hispanics (using an English-only survey instrument), using self-reported demographic information maintained by Qualtrics. This group (n = 4,104) included 3,413 whites, 246 African Americans, 55 Hispanics, 181 Asians/Asian-Americans, 92 members of other racial groups, and 117 respondents who did not identify their race to Qualtrics. (Additional details regarding the demographic composition of our survey respondents can be found in Section S2 of the SI.)

The second wave targeted Hispanics with the goal of obtaining large enough numbers to make meaningful subgroup inferences. Moreover, because some Hispanics are primarily Spanish-speakers, second-wave respondents had the option of taking the survey either in English or Spanish. (Both versions are included in Section S1 of the SI.) The second wave included 4,931 respondents, which included 4,562 Hispanics, 13 whites, 3 African Americans, 1 Asian/Asian-American, 3 members of other racial groups, and 349 respondents who did not identify their race.

S1.3 Experimental Design

The survey was administered online via the Qualtrics platform and included at least 16 questions (or 17 for Hispanics). Our internet-only format follows the anticipated protocol of the 2020 U.S. Census, the bulk of which is expected to take place online. Following the Census format, the survey

first asks about the number of individuals in the respondent's household as of a certain date, those individuals' names (on our survey, their initials), the tenure status of the household, and which individual owns the home or pays rent. The survey then asks six demographic questions for each additional household member.

Household Member Citizenship Question ("Citizenship Treatment"). To evaluate the impact of asking about household members' citizenship on item non-response and response quality, we randomly assigned half of the respondents (n = 4,497) to a treatment condition in which we asked, for each member of their household, "Is this person a citizen of the United States?" Similar to the proposed 2020 Census question, we provided five answer categories, which are outlined in Section S1 of the SI. The other half (n = 4,538) were randomly assigned to a control condition that did not receive the citizenship question for any household member.

Within the subset of questions about household members, the exact placement of the citizenship question on the actual census is yet to be determined. Thus, for our survey, we randomly rotated the order in which the citizenship question appeared, conditional on the household member being asked about.

Data Sharing with Census Treatment ("Census Prompt" Treatment). Any treatment effect associated with a citizenship question may be smaller than if asked by the U.S. Census itself, since individuals might fear reprisal from the federal government but not from academic researchers with no government affiliations. We would thus expect smaller treatment effects associated with the citizenship treatment in our survey as opposed to the actual U.S. Census.

We assess this with another randomized intervention. Specifically, we randomly assigned half of respondents (n = 4,454) to receive, independently of the first randomization, a short note at the bottom of their consent form saying "Your responses will be shared with the U.S. Census Bureau." In order to proceed, we required that respondents indicate agreement. The other half (n = 4,581) received no prompt. This priming more closely mirrors the real-world scenario of the U.S. Census, in that respondents might expect that their data are held by a federal government agency (the Census Bureau). (Additional details regarding our experimental design can be found in Section S1 of the SI.)

Table S1: Respondents Randomly Assigned to Each Treatment

	Did Not Receive Citizenship Question	Received Citizenship Question	Total
Did Not Receive Census Prompt	2335	2246	4581
Received Census Prompt	2203	2251	4454
Total	4538	4497	9035

Note: This table shows our 2 × 2 experimental design. The number of respondents randomly assigned to each of our treatments is included in each cell.

As Table S1 summarizes, the final experiment was a 2 × 2 design, with respondents having an equal probability of assignment to any of the four conditions:

1. No questions on citizenship, no prompt about information sharing (n = 2,335)

2. Citizenship Treatment only (n = 2,246)

3. Census Prompt treatment only (n = 2,203)

4. Citizenship Treatment and Census Prompt treatment (n = 2,251)

Of 9,035 respondents, 4,497 received the citizenship question, statistically indistinguishable from the expected 0.50 proportion (χ^2 = 0.177, df = 1, p-value = 0.674). Of 9,035 respondents, 4,454 received the census prompt, a figure that is also statistically indistinguishable from the expected proportion (χ^2 = 1.757, df = 1, p-value = 0.185). In the next subsections we provide all the questions we used in both the English and Spanish versions of our survey.

It is also important to note that the dependent variables reported in Tables 1 and 2 in the main text involve various proportions of questions skipped. Since in the control condition the respondent does not receive either the question about citizenship or the census prompt, these respondents have shortest questionnaire. The second shortest questionnaire is for either those who only receive the citizenship treatment or census prompt since in both instances the survey length would be reduced. The only difference between these two groups is that the census prompt is asked a single time at the beginning of the survey, whereas the citizenship question is asked for each household member. Finally, the longest questionnaire is answered by respondents who received both the citizenship treatment and census prompt, meaning the denominator of the proportions of interest vary by the treatment.

Finally, within each demographic section the questions are randomized, meaning the questions asked about the age, race/ethnicity, gender, etc. of the house members are shuffled for each respondent. This means for those who are in the Citizenship Treatment group the citizenship question will sometimes appear after the relevant race/ethnicity questions for the first household member. Although the demographic questions for the second household member onward are also randomized, since the respondent always received questions about the first household member before the second household member the citizenship question has already been observed by all respondents in the Citizenship Treatment once they get to this point in the survey.

The census prompt is always received at the same point in the consent document, so this treatment is unaffected by any of the within block question randomization we use throughout the survey. We used such randomization to avoid question order effects that have been shown to be important by a number of studies (e.g., McFarland 1981; Jann and Hinz 2016). Given these concerned, we thought including such randomization was important, although we acknowledge it does make the randomization scheme of our experiment a little more difficult to interpret.

S1.4   Outcome Measures

In the main text, we rely on two outcome measures: (1) percent of all questions skipped and (2) percent of household reported as being Hispanic. For both measures, we took into consideration when the respondent received the citizenship question. This is somewhat challenging since for

each household member the demographic questions – including the question about citizenship – are randomized to avoid question ordering effects (see above).

For example, imagine Respondent A was in the Citizenship Treatment and had the following randomization for his/her two household members (M1 and M2):

- Household Member #1 (M1)

    – Gender (M1G)
    – U.S. Citizen (M1C)
    – Race/Ethnicity (M1R)
    – Age (M1A)

- Household Member #2 (M2)

    – Age (M2A)
    – Gender (M2G)
    – Race/Ethnicity (M2R)
    – U.S. Citizen (M2C)

In this example, the following would be used to calculate the percent skipped: M1C, M1R, M1A, M2A, M2G, M2R, and M2C. This is because the question about Household Member #1's gender occurs *before* the question asking about Household Member #1's citizenship. Since Household Member #2's demographic questions always occur after the respondent is asked a citizenship question, all of his/her responses are included in the percent skipped calculation.

Please also note that in this example the Race/Ethnicity question always occurs *after* the citizenship question. This means both household members are included in the percent hispanic calculation. To see this how this is calculated, imagine Respondent A gave the following responses:

- Household Member #1 (M1)

    – Gender = F (M1G)
    – U.S. Citizen = Yes (M1C)
    – Race/Ethnicity = NA (M1R)
    – Age (M1A) = NA

- Household Member #2 (M2)

    – Age (M2A) = NA
    – Gender (M2G) = NA
    – Race/Ethnicity (M2R) = NA
    – U.S. Citizen (M2C) = NA

If these were the response given, then we would say Respondent A skipped 85.71 percent of the remaining questions ($\frac{6}{7} = 0.8571$) after the citizenship question was introduced and 0 percent of his/her household members ($\frac{0}{2} = 0$) were reported as being Hispanic.

In the main text, we spend considerable time discussing this latter outcome because the respondents are always asked to enumerate their household members *before* any demographic questions – including the question about citizenship – are asked. This means that we know how many responses we should expect and how many responses we actually receive, making item non-response especially problematic for the U.S. Census. Indeed, the U.S. Census Bureau asks a single respondent to provide information about all of their household members. If a sensitive question leads them to omit some of that information, then it makes it difficult to properly estimate the demographic information of the household – including the number of Hispanic residents.

S1.5   English Survey

Our survey was constructed to mirror the Internet Self-Response (ISR) for the 2020 Census. Similar to the paper version, the first questions are about the respondent's household: the number of individuals living there as of a certain date (on the 2020 Census, it will be April 1, 2020, and on our survey, we used September 1, 2019), those individuals' names (on our survey, their initials), the tenure status of the household, and which individual owns the home or pays the rent (Person 1). Then the demographic questions are asked about each of the individuals in the order that the survey-taker listed them.

In addition to the basic structure of the survey, we also attempted to mirror the same look, feel, and skip logic of the ISR. For example, none of the demographic questions were forced-responses, meaning respondents could skip those questions and still receive the survey incentive. Below we provide the consent form we used for all survey respondents. At the bottom of this form is the Census prompt treatment. The questions we asked in the English version appear shortly thereafter. In those questions, the citizenship question appears as Question 5.

Consent Form

<u>**Key Information**</u>
The following is a short summary of this study to help you decide whether or not to be a part of this study. More detailed information is listed later on in this form.

Why am I being invited to take part in a research study?
We invite you to take part in a research study because you are a U.S. resident over 18. What should I know about a research study? Someone will explain this research study to you. Whether or not you take part is up to you. Your participation is completely voluntary. You can choose not to take part. You can agree to take part and later change your mind. Your decision will not be held against you. You can ask all the questions you want before you decide.

Why is this research being done?
This research is being done to estimate the response rates to surveys that include different types of questions.

How long will the research last and what will I need to do?
We expect that you will be in this research study for approximately seven to twenty minutes. You will be asked to complete a survey about your household and about some opinions on social issues.

Is there any way being in this study could be bad for me?
We don't believe there are any risks from participating in this research.

Will being in this study help me in any way?
There are no benefits to you from your taking part in this research. We cannot promise any benefits to others from your taking part in this research. However, possible benefits to others include more informed policymaking.

<u>**Detailed Information**</u>
The following is more detailed information about this study in addition to the information listed above.

What is the purpose of this research?
The purpose of this research is to learn more about what types of survey questions individuals are and are not comfortable responding to. Many institutions which administer surveys are concerned about the rate of individuals dropping out of surveys, because they are not able to learn as much information when individuals drop out of their surveys. Therefore, it is important to understand as much as possible about what types of survey questions lead to individuals dropping out of surveys.

How long will I take part in this research?
You will be asked to take an online survey one time, and the survey will take between five and fifteen minutes.

What can I expect if I take part in this research?
You can expect to take an online survey from your computer or mobile device that will take between seven and twenty minutes. This survey will ask you some questions about yourself and some questions about your attitudes.

What happens if I say yes, but I change my mind later?
You can leave the research at any time it will not be held against you.

If I take part in this research, how will my privacy be protected? What happens to the information you collect?
This study will not collect any personally identifying information. Nevertheless, efforts will be made to limit the use and disclosure of your Personal Information, including research study and medical records, to people who have a need to review this information. We cannot promise complete secrecy. Organizations that may inspect and copy your information include the IRB and other representatives of this organization, as well as the Harvard Kennedy School of Government and other representatives of this organization.

What else do I need to know?
You will be compensated the amount you agreed upon before you entered into the survey.

Who can I talk to?
If you have questions, concerns, or complaints, or think the research has hurt you, talk to the research team at:

[NAME 1]: [EMAIL] or [PHONE NUMBER]
[NAME 2]: [EMAIL] or [PHONE NUMBER]
[NAME 3]: [EMAIL] or [PHONE NUMBER]

This research has been reviewed and approved by the Harvard University Area Institutional Review Board ("IRB"). You may talk to them at (617) 496-2847 or cuhs@harvard.edu if: Your questions, concerns, or complaints are not being answered by the research team. You cannot reach the research team. You want to talk to someone besides the research team. You have questions about your rights as a research subject. You want to get information or provide input about this research.

You may download a copy of this information for your records by clicking here.

[*This is the Census prompt treatment. Randomly assigned to approximately half of the respondents. All others do not receive this question.*] Your responses will be shared with the U.S. Census Bureau:

☐ I understand that my responses will be shared with the U.S. Census Bureau

Questions

1. How many people were living or staying in this house, apartment, or mobile home on September 1, 2018?

2(a). Were there any additional people staying here on September 1, 2018 that you did not include in Question 1? *Mark all that apply*

- Children, related or unrelated, such as newborn babies, grandchildren, or foster children
- Relatives, such as adult children, cousins, or in-laws
- Nonrelatives, such as roommates or live-in babysitters
- People staying here temporarily
- No additional people

2(b). [*If R answered 2(a) with any answer except "No additional people".*] How many additional people?

3. Is this house, apartment, or mobile home –

- Owned by you or someone in this household with a mortgage or loan?
- Owned by you or someone in this household free and clear (without a mortgage or loan)?
- Rented?
- Occupied without payment of rent?

[*Beginning here, questions are asked for the number of household members listed in Question 1. The section always starts with Question 4(a), then Questions 5*-10 are randomized. After the last randomized question is asked for Person 1, then Question 4(a) is asked for the next household member. For Person 2 and above we add Questions 4(b)* and 4(c)* to the randomized questions (5*-10) and the section repeats.*]

Household Demographics Instructions: Please provide information for each person living here. If there is someone living here who pays the rent or owns this residence, start by listing him or her as Person 1. If the owner or the person who pays the rent does not live here, start by listing any adult living here as Person 1.

4(a). What are Person X's initials? *Print initials below*

First initial:

MI:

Last initial:

5*. [*This is the citizenship question treatment. Randomly assigned to approximately half of the respondents. All others do not receive this question.*] Is this person a citizen of the United States?

- Yes, born in the United States
- Yes, born in Puerto Rico, Guam, the U.S. Virgin Islands, or Northern Marianas
- Yes, born abroad of U.S. citizen parent or parents
- Yes, U.S. citizen by naturalization – *Print year of naturalization*
- No, not a U.S. citizen

6. What is this person's age? *For babies less than 1 year old, do not write the age in months. Write 0 as the age.*

7. What is this person's year of birth?

8. Is this person of Hispanic, Latino, or Spanish origin?

- No, not of Hispanic, Latino, or Spanish origin
- Yes, Mexican, Mexican Am., Chicano
- Yes, Puerto Rican
- Yes, Cuban
- Yes, another Hispanic, Latino, or Spanish origin – *Print, for example, Salvadoran, Dominican, Colombian, Guatemalan, Spaniard, Ecuadorian, etc.*

9. What is this person's race or ethnicity? *Select all boxes that apply and/or enter details as necessary. Note, you may report more than one group.*

- German
- Irish
- English
- Italian
- Polish
- French
- Other White – *Print, for example, Scottish, Norwegian, Dutch, etc.*

- African American
- Jamaican
- Haitian
- Nigerian
- Ethiopian
- Somali
- Other Black – *Print, for example, Ghanaian,*

South African, Barbadian, etc.

- Chinese
- Filipino
- Asian Indian
- Vietnamese
- Korean
- Japanese

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- Other Asian – *Print, for example, Pakistani, Cambodian, Hmong, etc.*
- American Indian – *Print, for example, Navajo Nation, Blackfeet Tribe, Muscogee (Creek) Nation, etc.*
- Alaskan Native – *Print, for example, Native Village of Barrow Inupiat Traditional Government, Tlingit, Orustaramuit Native Village, etc.*

- Central or South American Indian – *Print, for example, Mayan, Aztec, Taino, etc.*
- Lebanese
- Iranian
- Egyptian
- Syrian
- Moroccan
- Israeli
- Other Middle Eastern or North African – *Print, for example, Algerian, Iraqi, Kurdish, etc.*

- Native Hawaiian
- Samoan
- Chamorro
- Tongan
- Fijian
- Marshallese
- Other Pacific Islander – *Print, for example, Palauan, Tahitian, Chuukese, etc.*
- Some other race – *Print race or origin.*

10. What is this person's sex?

- Male
- Female

4(b).* [*Beginning with Person 2, we then add the following questions. These always appear after Question 4(a) and are randomized with Questions 5, 6, 7, 8, 9, and 10.*]

Does this person usually live or stay somewhere else? *Mark all that apply.*

- No
- Yes, for college
- Yes, for a military assignment
- Yes, for a job or business
- Yes, in a nursing home
- Yes, with a parent or other relative
- Yes, at a seasonal or second residence
- Yes, in a jail or prison
- Yes, for another reason

4(c).* How is this person related to Person 1?

- Opposite-sex husband/wife/spouse
- Opposite-sex unmarried partner
- Same-sex husband/wife/spouse

- Same-sex unmarried partner
- Biological son or daughter
- Adopted son or daughter
- Stepson or stepdaughter
- Brother or sister
- Father or mother
- Grandchild
- Parent-in-law
- Son-in-law or daughter-in-law
- Other relative
- Roommate or housemate
- Foster child
- Other nonrelative

[*Beginning here, repeat sex, age, Hispanic origin, race, and citizenship questions for all enumerated household members.*]

11. Have you ever heard of the United States Census, or have you not heard of this?

   - I have heard of the United States Census
   - I have not heard of the United States Census

12(a). How likely are you to participate in the 2020 United States Census? By participate, we mean fill out and mail in a Census form or fill one out online. Would you say you...

   - Definitely will
   - Probably will
   - Might or might not
   - Probably will not
   - Definitely will not

12(b). [*If R answered 12(a) with "Might or might not," "Probably will not," or "Definitely will not".*] By participate, we mean fill out and mail in a Census form or fill one out online. Would you say someone else in your household...

   - Definitely will
   - Probably will
   - Might or might not
   - Probably will not

- Definitely will not

13. How important do you think the Census is for the United States? Would you say it is...

- Very important
- Somewhat important
- Not too important
- Not at all important
- Don't know enough to say

14. Do you believe that answering and sending back or completing online your United States Census form would...

- Personally benefit you
- Personally harm you
- Neither benefit or harm you
- Don't know enough to say

15(a). Do you believe that answering and sending back or completing online your United States Census form would...

- Benefit your community
- Harm your community
- Neither benefit or harm your community
- Don't know enough to say

15(b). [*If R answered 15(a) with "Benefit your community" or "Harm your community". Answer is piped into this question.*] Why do you say the Census would [benefit/harm] your community?

16. How concerned are you, if at all, that the Census Bureau not keep answers to the 2020 Census confidential?

- Extremely concerned
- Very concerned
- Somewhat concerned
- Not too concerned
- Not at all concerned
- Don't know enough to say

17. How concerned are you, if at all, that the Census Bureau will share answers to the 2020 Census with other government agencies?

- Extremely concerned
- Very concerned
- Somewhat concerned
- Not too concerned
- Not at all concerned
- Don't know enough to say

18. How concerned are you, if at all, that the answers you provide to the 2020 Census will be used against you?

- Extremely concerned
- Very concerned
- Somewhat concerned
- Not too concerned
- Not at all concerned
- Don't know enough to say

19(a). Do you think the results of the United States Census help one political party (the Republican Party or the Democratic Party) more than the other, or don't you think so?

- Yes
- No
- Don't know enough to say

19(b). [*If R answered 19(a) with "Yes".*] Which political party do you think the United States Census helps more?

20. As far as you know, is the Census used to determine whether someone is in this country legally, or is it not used for this?

- Yes, it is used to determine whether someone is in this country legally
- No, it is not used to determine whether someone is in this country legally
- Don't know enough to say

21. As far as you know, is the Census used to decide how many representatives each state will have in Congress, or is it not used for this?

- Yes, it is used to decide how many representatives each state will have in Congress
- No, it is not used to decide how many representatives each state will have in Congress
- Don't know enough to say

22. As far as you know, is the Census used to decide how much money communities will get from the government, or is it not used for this?

   - Yes, it is used to decide how much money communities will get from the government
   - No, it is not used to decide how much money communities will get from the government
   - Don't know enough to say

23. As far as you know, is the Census Bureau supposed to keep the personal information you provide on the 2020 Census form confidential, or are they not supposed to do that?

   - Yes, it is supposed to keep the personal information you provide on the 2020 Census form confidential
   - No, it is not supposed to keep the personal information you provide on the 2020 Census form confidential
   - Don't know enough to say

24. [*If R identified any household member as being of Hispanic, Latino, or Spanish Origin (see Question 8).*] Have you seen or heard anything recently from Hispanic/Latino civic, religious, media or community groups encouraging or discouraging you to from filling out your 2020 Census form?

   - Yes
   - No

S1.6 Spanish Survey

For the Spanish translation, we used the U.S. Census Bureau's own translation as much as possible. For any questions that did not appear on the Census short form, including our university-required consent form and debriefing materials, we obtained translations from a professional translation company, which we then vetted with the assistance of several native Spanish speakers. Only 210 respondents took the survey in Spanish.

Consent From

Información clave

Esta es una breve explicación del estudio para ayudarlo a decidir si usted desearía o no participar en el mismo. Incluimos información más detallada a continuación en este formulario.

¿Por qué fui invitado a participar en este estudio?

Lo invitamos a participar por el hecho de ser residente de los Estados Unidos y ser mayor de 18 años.

¿Qué debo saber sobre este estudio?

- Alguien le va a explicar este estudio.

- Participar o no en este estudio es su decisión.

- Su participación es completamente voluntaria.

- Puede elegir no participar.

- Puede participar y luego cambiar de opinión.

- Su decisión no se utilizará en su contra.

- Puede hacer todas las preguntas que desea antes de decidir si quiere participar.

¿Por qué se está haciendo esta investigación?

Esta investigación se está haciendo para medir las tasas de respuesta a encuestas que incluyen diferentes tipos de preguntas.

¿Cuánto durará la investigación y qué debo hacer?
Estimamos que participar en este estudio le tomará entre siete y veinte minutos. Se le pedirá completar una encuesta sobre su hogar y sus opiniones sobre asuntos sociales.

¿Puede este estudio perjudicarme de alguna manera?
No creemos que haya ningún riesgo de participar en este estudio.

$\neg\emptyset Participar en este estudio me ayudar\'a de alguna manera?$

$No\,hay\,beneficios\,para\,usted\,por\,participar\,en\,este\,estudio.\,Tampoco\,podemos\,prometer\,beneficios\,a\,otras\,perso$

Información detallada
A continuación se incluye información más detallada sobre este estudio además de lo previamente mencionado.

¿Cuál es el propósito de esta investigación?
El propósito de este estudio es aprender más sobre los tipos de preguntas en encuestas que las personas se sienten cómodas o incómodas en responder. A muchas instituciones que hacen encuestas les preocupa que las personas abandonen las mismas, ya que no pueden obtener suficiente información cuando los individuos abandonan sus encuestas. Por ello, es importante saber los más que se pueda sobre los tipos de preguntas hacen que las personas abandonen las encuestas.

¿Cuánto tiempo me tomará participar?
Se le solicitará que responda una encuesta en línea que le llevará entre siete y veinte minutos.

¿Qué puedo esperar si participo en este estudio?
Usted completará una encuesta desde su computadora o teléfono móvil. Esto le tomará entre siete y veinte minutos. La encuesta incluirá preguntas acerca de usted y algunas preguntas sobre sus opiniones.

¿Qué pasa si digo que sí, pero luego cambio de opinión?
Puede abandonar la investigación en cualquier momento en que no se llevará a cabo en su contra.

¿Si participo en el estudio, ¿cómo se protegerá mi privacidad? ¿Qué pasa con la información recolectada?

Este estudio no recopilará ningún tipo de información que permita identificarle personalmente. No obstante, se realizarán esfuerzos para limitar el uso y divulgación de su información personal, incluyendo los datos de la investigación y registros médicos, únicamente a las personas que necesiten acceder a la misma. No podemos prometer la ocultación completa de la información. Entre las organizaciones que podrían inspeccionar una copia de su información se encuentra la Junta de Revisión Institucional (IRB) de la Universidad de Harvard y otros representantes de la misma; así como la Escuela de Gobierno John F. Kennedy de la Universidad de Harvard y otros representantes de ésta.

¿Qué más necesito saber?

Será compensado por el monto acordado antes de que comience a responder la encuesta.

¿Con quién puedo hablar?

Si tiene preguntas, inquietudes o quejas, o cree que el estudio lo ha afectado de alguna manera comuníquese con el equipo de investigación:

[NAME 1]: [EMAIL] ó [PHONE NUMBER]
[NAME 2]: [EMAIL] ó [PHONE NUMBER]
[NAME 3]: [EMAIL] ó [PHONE NUMBER]

Este estudio ha sido revisado y aprobado por la Junta de Revisión Institucional de la Universidad de Harvard. Puede comunicarse con ellos al número telefónico (617) 496-2847 o por correo electrónico a cuhs@harvard.edu en los siguientes casos:

- El equipo de investigación no responde a sus dudas, inquietudes o quejas.

- No puede comunicarse con el equipo de investigación.

- Desea hablar con alguien fuera del equipo de investigación.

- Tiene dudas sobre sus derechos como sujeto de la investigación.

- Desea obtener información o dar su opinión sobre la investigación.

[*This is the Census prompt treatment. Randomly assigned to approximately half of the respondents. All others do not receive this question.*] Sus respuestas serán compartidas con el U.S. Census Bureau:

☐ Entiendo que mis respuestas serán compartidas con el U.S. Census Bureau

Questions

1. ¿Cuántas personas estaban viviendo o quedándose en esta casa, departamento o casa móvil el primero de septiembre de 2018?

2(a). ¿Había personas adicionales quedándose aquí el 1 de septiembre de 2018 que no incluyó en la Pregunta 1? *Indiquen todas que correspondan.*

   - Niños, emparentados o no, tales como bebŕs recién nacidos, nietos o niños acogidos (foster children)
   - Parientes adultos, tales como hijos mayores de edad, primos o parientes políticos
   - Personas adultas que no sean parientes, tales como compañeros de casa o cuarto, o niñeras que viven en el hogar
   - Personas que se quedan aquí temporalmente
   - No hay personas adicionales

2(b). [*If R answered 2(a) with any answer except "No additional people".*] ¿Cuántas personas adicionales?

3. ¿Es esta casa, departamento o casa móvil?

   - Propiedad suya o de alguien viviendo en esta casa con una hipoteca o crédito hipotecario? *Esto incluye los préstamos con la propiedad como garantía.*
   - Propiedad suya o de alguien en este casa totalmente pagada y sin deuda (sin una hipoteca o crédito hipotecario)?
   - Alquilado(a) o rentado(a)?
   - Ocupado(a) sin pago de alquiler o renta?

[*Beginning here, questions are asked for the number of household members listed in Question 1. The section always starts with Question 4(a), then Questions 5*-10 are randomized. After the last randomized question is asked for Person 1, then Question 4(a) is asked for the next household member. For Person 2 and above we add Questions 4(b)* and 4(c)* to the randomized questions (5*-10) and the section repeats.*]

Household Demographics Instructions: Por favor, provea información para cada persona que vive aquí. Si hay alguien que vive aquí que paga el alquiler (renta) o es propietario de esta vivienda, comience la lista con él o ella como la Persona 1. Si el propietario o la persona que paga el alquiler (renta) no vive aquí, comience la lista con cualquier adulto que viva aquí como la Persona 1.

4(a). ¿Cuáles son las iniciales de la Persona 1? *Escriba las iniciales a continuación.*

   Iniciales de nombre:

   Iniciales del apellido(s):

5*. [*This is the citizenship question treatment. Randomly assigned to approximately half of the respondents. All others do not receive this question.*] ¿Ciudadanía: Es esta persona ciudadana de los Estados Unidos?

- Sí, nació en los Estados Unidos
- Sí, nació en Puerto Rico, Guam, las Islas Vírgenes de los Estados Unidos o las Islas Marianas del Norte
- Sí, nació en el extranjero de padre o madre que es ciudadano(a) de los EE. UU.
- Sí, es ciudadana de los Estados Unidos por naturalización. *Escriba el año de naturalización*
- No, no es ciudadana de los Estados Unidos

6. ¿Edad: Cuál es la edad de esta persona? *Para bebés menores de un año, no escriba los meses de edad. Solo escriba 0.*

7. ¿Cuál es su fecha de nacimiento?

8. ¿Origen hispano: Es esta Persona de origen hispano, latino o español?

- No, no es de origen hispano, latino o español
- Sí, es mexicano, mexicano-americano, chicano
- Sí, es puertorriqueño
- Sí, es cubñ ano
- Sí, es de otro origen hispano, latino o español *Escriba, por ejemplo, salvadoreño, dominicano, colombiano, guatemalteco, español, ecuatoriano, etc.*

9. ¿Raza: Cuál es la raza de esta persona?

- Orígenes étnicos BLANCOS
- Alemán
- Irlandés
- Inglés
- Italiano
- Polaco
- Francés
- Otro: escriba, por ejemplo, escocés, noruego, holandés, etc.
- Orígenes étnicos NEGROS o AFROAMER-ICANOS
- Afroamericano
- Jamaiquino
- Haitiano
- Nigeriano
- Etíope
- Somalí
- Otro: escriba, por ejemplo, ghanés, sudafricano, barbadense, etc.
- Orígenes étnicos ASIÁTICOS
- Chino
- Chino
- Indio asiático
- Vietnamita
- Coreano
- Japonés
- Otro: escriba, por ejemplo, pakistaní, camboyano, hmong, etc.
- INDÍGENA DE LAS AMÉRICAS o NATIVO DE ALASKA

- Indígena americano: escriba, por ejemplo, Navajo Nation, Blackfeet Tribe, Muscogee (Creek) Nation, etc.
- Nativo de Alaska: Escriba, por ejemplo, Native Village of Barrow Inupiat Traditional Government, Tlingit, Orustaramuit Native Village, etc.
- Indígena de América Central o Sudamérica: escriba, por ejemplo, maya, azteca, taíno, etc.

- Orígenes étnicos del MEDIO ORIENTE O ÁFRICA DEL NORTE
- Libanés
- Iraní
- Egipcio
- Sirio
- Marroquí
- Israelí
- Otro: escriba, por ejemplo, argelino, iraquí, kurdo, etc.
- Orígenes étnicos NATIVO DE HAWÁI

o ISLEÑO DEL PACÍFICO
- Nativo de Hawái
- Samoano
- Chamorro
- Tongano
- Fiyiano
- de las Islas Marshall
- Otro: escriba, por ejemplo palauano, tahitiano, chuukés, etc.
- Alguna otra raza u origen étnico

10. ¿Sexo: Cuál es el sexo de esta persona?

- Masculino
- Feminino

4(b).* [*Beginning with Person 2, we then add the following questions. These always appear after Question 4(a) and are randomized with Questions 5, 6, 7, 8, 9, and 10.*]

¿Esta persona generalmente vive o se queda en algún otro lugar?

- No
- Sí, para ir a la universidad
- Sí, por una orden militar
- Sí, por un empleo o negocio
- Sí, en un hogar de ancianos o nursing home
- Sí, con el padre, la madre u otro pariente
- Sí, en una vivienda temporal o segunda residencia
- Sí, en una cárcel o prisión
- Sí, por alguna otra razón

4(c).* ¿Relación: Cómo está esta persona relacionada con la Persona 1?

- Esposo(a) del sexo opuesto
- Pareja no casada del sexo opuesto
- Esposo(a) del mismo sexo

- Pareja no casada del mismo sexo
- Hijo(a) biológico(a) de sangre
- Hijo(a) adoptivo(a)
- Hijastro(a)
- Hermano(a)
- Padre o madre
- Nieto(a)
- Suegro(a)
- Yerno o nuera
- Otro pariente
- Roommate o compañero(a) de casa
- Niño(a) acogidos (foster child)
- Otra persona que no es pariente

[*Beginning here, repeat sex, age, Hispanic origin, race, and citizenship questions for all enumerated household members.*]

11. ¿Ha oído mencionar el Censo de los Estados Unidos, o no lo ha oído mencionar?

    - He oído mencionar el Censo de los Estados Unidos
    - No he oído mencionar el Censo de los Estados Unidos

12(a). ¿Qué tan probable es que participe en el Censo de los Estados Unidos del año 2020? Por participar nos referimos a que llene y envíe por correo el formulario del Censo o a que llene el formulario en línea. ¿Diría usted que?

    - Definitivamente participaré
    - Probablemente participaré
    - Tal vez participaré
    - Probablemente no participaré
    - Definitivamente no participaré

12(b). [*If R answered 12(a) with "Might or might not," "Probably will not," or "Definitely will not".*] Por participar nos referimos a llenar y enviar por correo el formulario del Censo o llenar el formulario en línea. ¿Qué tan probable es que alguien más en su vivienda participe en el Censo del año 2020?

    - Definitivamente participará
    - Probablemente participará

- Tal vez participará
- Probablemente no participará
- Definitivamente no participará

13. ¿Qué tan importante cree que el Censo es para los Estados Unidos? ¿Diría que es?

- Muy importante
- Un poco importante
- No tan importante
- No importante
- No tiene suficiente conocimiento

14. ¿Cree que contestar y enviar su formulario del Censo de los Estados Unidos o llenar el formulario en línea puede?

- Beneficiarle personalmente
- Perjudicarle personalmente
- Ni beneficiarle ni perjudicarle a usted personalmente
- No tiene suficiente conocimiento

15(a). ¿Cree que contestar y enviar su formulario del Censo de los Estados Unidos o llenar el formulario en línea puede?

- Beneficiar a su comunidad
- Perjudicar a su comunidad
- Ni beneficiar ni perjudicar a su comunidad
- No tiene suficiente conocimiento

15(b). [*If R answered 15(a) with "Benefit your community" or "Harm your community". Answer is piped into this question.*] ¿Por qué dijo que el Censo podría beneficiar/ perjudicar a su comunidad?

16. ¿Qué tan preocupado(a) está que el Census Bureau (La Oficina del Censo) no mantendrá confidenciales sus respuestas al Censo del 2020?

- Profundamente preocupado(a)
- Muy preocupado(a)
- Algo preocupado(a)
- No tan preocupado(a)
- Nada preocupado(a)
- No tiene suficiente conocimiento

17. ¿Qué tan preocupado(a) está que el Census Bureau (La Oficina del Censo) compartirá sus respuestas al Censo del 2020 con otras agencias gubernamentales?

- Profundamente preocupado(a)
- Muy preocupado(a)
- Algo preocupado(a)
- No tan preocupado(a)
- Nada preocupado(a)
- No tiene suficiente conocimiento

18. ¿Qué tan preocupado(a) está que sus respuestas al Censo del 2020 se usarán en su contra?

- Profundamente preocupado(a)
- Muy preocupado(a)
- Algo preocupado(a)
- No tan preocupado(a)
- Nada preocupado(a)
- No tiene suficiente conocimiento

19(a). ¿Cree usted que los resultados del Censo de los Estados Unidos ayudan más a un partido político (el Partido Republicano o el Partido Demócrata) que a otro, o cree que no es así?

- Sí
- No
- No tiene suficiente conocimiento

19(b). [*If R answered 19(a) with "Yes".*] ¿En su opinión, qué partido se beneficiará más por los resultados del Censo de los Estados Unidos?

20. Por lo que usted sabe, ¿se utiliza el Censo para determinar si alguien está en este país legalmente, o no se utiliza así?

- Sí, se utiliza para determinar si alguien está en este país legalmente
- No, no se utiliza para determinar si alguien está en este país legalmente
- No tiene suficiente conocimiento

21. Por lo que usted sabe, ¿se utiliza el Censo para determinar el número de representantes que cada estado tendrá en el Congreso, o no se utiliza así?

- Sí, se utiliza para determinar el número de representantes que cada estado tendrá en el Congreso

- No, no se utiliza para determinar el número de representantes que cada estado tendrá en el Congreso
- No tiene suficiente conocimiento

22. Por lo que usted sabe, ¿se utiliza el Censo para determinar la cantidad de dinero que las comunidades recibirán del gobierno, o no se utiliza así?

   - Sí, se utiliza para determinar la cantidad de dinero que las comunidades recibirán del gobierno
   - No, no se utiliza para determinar la cantidad de dinero que las comunidades recibirán del gobierno
   - No tiene suficiente conocimiento

23. Por lo que usted sabe, ¿El Census Bureau (La Oficina del Censo) tiene que guardar la información personal que usted proporcionó en el formulario del Censo del 2020 de manera confidencial, o no tiene que hacerlo así?

   - Sí, tiene que guardar la información personal proporcionada en el formulario del Censo del 2020 de manera confidencial
   - No, no tiene que guardar la información personal proporcionada en el formulario del Censo del 2020 de manera confidencial
   - No tiene suficiente conocimiento

24. [*If R identified any household member as being of Hispanic, Latino, or Spanish Origin (see Question 8).*] ¿Ha visto u oído algo recientemente de grupos cívicos, religiosos, medios de comunicación o grupos de la comunidad hispana/latina alentándole o desalentándole a llenar su formulario del Censo del año 2020?

   - Sí
   - No

S1.7 Comparing our Survey to the Census Short-Form

Although we attempted to mirror the Census short form in our study, there are several distances worth mentioning. First, the Census short form asks the questions in the same order for each household member. Their order is as follows:

Print name of Person X

Does the person usually live or stay somewhere else?

How is the person related to Person 1?

What is this person's sex?

Table S2: Treatment Effects on Item Non-Response (Excluding Census Opinion Questions)

	Treatment	Control	Difference	t-statistic	p-value
Percent of All Questions Skipped					
All	27.63	23.87	3.75	4.424	< 0.001
Hispanic	36.67	31.32	5.36	4.197	< 0.001
Mexico/Central America	15.83	5.12	10.71	3.307	0.001
Puerto Rico/Cuba	9.29	8.01	1.27	0.415	0.679
Non-Hispanic	18.28	16.01	2.27	2.169	0.030
Percent of Respondents Who Skipped At Least 80% of the Questions					
All	23.50	18.58	4.93	5.757	< 0.001
Hispanic	31.82	24.23	7.59	5.762	< 0.001
Mexico/Central America	11.11	2.33	8.79	2.800	0.006
Puerto Rico/Cuba	5.47	1.89	3.58	1.418	0.157
Non-Hispanic	14.92	12.60	2.32	2.236	0.025

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last two columns report results from two-sample t-tests. Since the Citizenship Treatment was not introduced until Q5, we only consider questions appearing after this question (excluding the block of Census opinion questions) when assessing treatment effects. More details about the measures used in this table can be found in Section S1.4 in the SI.

What is this person's age and what is this person's date of birth?

Is this person of Hispanic, Latino, or Spanish origin?

What is this person's race?

In our study, we shuffled the question order due to well-known concerns over question-order effects (e.g., McFarland 1981; Jann and Hinz 2016).

Second, we asked fourteen questions about the 2020 Census and the Census Bureau more broadly. These questions always appeared after the basic demographic questions included in the Census short form and were included because we were also interested in determining whether the citizenship question would entice certain demographic groups to speak out against the Census Bureau.

Since the Census opinion questions always occur after the household demographic questions these additional questions do not affect the results reported in Table 2 in the main text. However, Table 1 could be impacted since this table reports the overall level of non-response. Given that, we replicate this table in Table S2 excluding the Census opinion questions when calculating the outcomes, meaning the percent skipped only includes the household demographic questions that appear after the citizenship question was introduced.

Generally speaking, our results are stronger when the Census opinion questions are excluded from the measure. Ultimately, this suggests that the inclusion of the Census opinion questions did not change the substantive conclusions of the study. Please see Section S3.3 for a discussion of how the citizenship question affected opinions towards the 2020 Census and the U.S. Census Bureau itself.

S2 Demographic and Balance Statistics

S2.1 Demographics

For race/ethnicity data, we relied on data provided by the vendor. We cross-checked their data with the responses to our survey, finding strong correspondence between reported race/ethnicity of the household and the vendor-provided race/ethnicity.

Table S3: Respondents' Race/Ethnicity and Partisanship by Survey Wave

	Wave 1	Wave 2
Race/Ethnicity		
Hispanic	55 (1.34%)	4,562 (92.52%)
African-American/Black	246 (5.99%)	3 (0.06%)
Asian-American/Asian	181 (4.41%)	1 (0.02%)
White	3,413 (83.16%)	13 (0.26%)
Native American/Inuit/Aleut	25 (0.61%)	1 (0.02%)
Native Hawaiian/Pacific Islander	10 (0.24%)	0 (0.00%)
Other	57 (1.39%)	2 (0.04%)
Not Provided	117 (2.85%)	349 (7.08%)
Partisanship		
Democrat	1,129 (27.51%)	1,526 (30.95%)
Republican	1,027 (25.02%)	653 (13.24%)
Independent	913 (22.25%)	758 (15.37%)
Other	75 (1.83%)	134 (2.72%)
Not Provided	960 (23.39%)	1,860 (37.72%)
Country of Birth		
North America		
All	2,916 (71.05%)	2,348 (47.62%)
United States	2,898 (70.61%)	2,346 (47.58%)
Canada	18 (0.44%)	2 (0.04%)
Latin America		
All	19 (0.46%)	621 (12.59%)
Mexico	1 (0.02%)	156 (3.16%)
Central America (Excl. Mexico)	1 (0.02%)	51 (1.03%)
Cuba	2 (0.05%)	74 (1.50%)
Puerto Rico	0 (0.00%)	159 (3.22%)
Caribbean (Excl. Cuba/Puerto Rico)	11 (0.27%)	52 (1.05%)
South America	4 (0.10%)	129 (2.62%)
Europe		
All	91 (2.22%)	83 (1.68%)
Northern Europe	21 (0.51%)	9 (0.18%)
Southern Europe	29 (0.71%)	62 (1.26%)
Eastern Europe	15 (0.37%)	6 (0.12%)
Western Europe	26 (0.63%)	6 (0.12%)
Africa		
All	18 (0.44%)	55 (1.12%)
Northern Africa	9 (0.22%)	27 (0.55%)
Middle Africa	7 (0.17%)	23 (0.47%)

Table S3 – *Continued from previous page*

	Wave 1	Wave 2
Eastern Africa	0 (0.00%)	4 (0.08%)
Western Africa	2 (0.05%)	1 (0.02%)
Asia		
All	75 (1.83%)	44 (0.89%)
Central Asia	0 (0.00%)	1 (0.02%)
Southern Asia	22 (0.54%)	10 (0.20%)
South-Eastern Asia	18 (0.44%)	10 (0.20%)
Eastern Asia	28 (0.68%)	7 (0.14%)
Western Asia	7 (0.17%)	16 (0.32%)
Oceania		
All	1 (0.02%)	5 (0.10%)
Australia/New Zealand	1 (0.02%)	3 (0.06%)
Micronesia	0 (0.00%)	2 (0.04%)
Not Provided	984 (23.98%)	1,775 (36.00%)
Total	4104	4931

Table S3 shows the demographic and partisan composition of the first and second waves of our survey. Of these variables, the most important is race/ethnicity where 1.34% of the first wave of our survey was identified as "Hispanic" by Qualtrics which is substantially less than the percent identified as "Hispanic" in the second wave (92.52%). Other noticeable differences are found in the partisan breakdown where 25.02% of the first wave of our survey were identified as Republicans which is 11.78 percentage points higher than the percent in second wave (13.24%). With that said, we found a reasonable number of respondents identified as Democrats. More specifically, Qualtrics identified 27.51% and 30.95% as being Democrats in the first and second waves of the survey, respectively. This is important since Hispanics tend to identify as being members of the Democratic party, meaning partisanship could be correlated with the survey wave.

Table S4: Respondents' Race/Ethnicity and Partisanship Compared to 2010 Census

	Survey	Census
Race/Ethnicity		
Hispanic	4,617 (51.10%)	50,477,594 (16.35%)
African-American/Black	249 (2.76%)	38,929,319 (12.61%)
Asian-American/Asian	182 (2.01%)	10,242,998 (3.32%)
White	3,426 (37.92%)	196,817,552 (63.75%)
Native American/Inuit/Aleut	26 (0.29%)	29,32,248 (0.95%)
Native Hawaiian/Pacific Islander	10 (0.11%)	540,013 (0.17%)
Other	59 (0.65%)	8,488,805 (2.75%)
Not Provided	466 (5.16%)	317,009 (0.10%)

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Table S4 – *Continued from previous page*		
	Survey	Census
Partisanship		
Democrat	2,655 (29.39%)	123,251,219 (39.92%)
Republican	1,680 (18.59%)	72,524,327 (23.49%)
Independent	1,671 (18.49%)	96,328,608 (31.20%)
Other	209 (2.31%)	83,97,879 (2.72%)
Not Provided	2,820 (31.21%)	82,43,505 (2.67%)
Country of Birth		
North America		
All	5,264 (58.26%)	269,596,539 (87.32%)
United States	5,244 (58.04%)	268,789,539 (87.06%)
Canada	20 (0.22%)	807,000 (0.26%)
Latin America		
All	640 (7.08%)	21,224,000 (6.87%)
Mexico	157 (1.74%)	11,711,000 (3.79%)
Central America (Excl. Mexico)	52 (0.58%)	3,053,000 (0.99%)
Caribbean	298 (3.30%)	3,731,000 (1.21%)
South America	133 (1.47%)	2,730,000 (0.88%)
Europe		
All	174 (1.93%)	4,817,000 (1.56%)
Africa		
All	73 (0.81%)	1,607,000 (0.52%)
Asia		
All	119 (1.32%)	11,284,000 (3.65%)
Oceania		
All	6 (0.07%)	201,376 (0.07%)
Not Provided	2,759 (30.54%)	15,625 (0.01%)
Total	9035	308,745,538

Table S4 compares the demographic and partisan composition of both waves of our survey to the 2010 Census estimates. Since the U. S. Census Bureau does not ask about partisanship, we imputed the total number of Democrats, Republicans, and Independents using the 2010 population (308,745,538) and weighted party identification estimates from the 2008 American National Election Studies (ANES). For example, in the nationally weighted version of the 2008 ANES 39.92% of respondents said they identified with the Democratic Party. Multiplying that percentage by 308,745,538 yields an estimated 123,251,219 Democrats. We repeated this process for Republicans, Independents, and people who identified with another party ("Other") or did not provide an answer ("Not Provided"). The nationally weighted 2008 ANES were used to impute each of these categories.

Our sample is similar to the 2010 Census in some categories, but different in others. For example, in the 2010 Census 6.87% of the population was born in Latin American which is very close to the percent in our survey (7.08%). We find similar results for respondents Qualtrics identified as being born in Central American countries (excluding Mexico) that we define as Belize, Costa Rica, Dominican Republic, El Salvador, Guatemala, Honduras, Nicaragua, and Panama. In the 2010 Census, 0.99% of the population was born in these countries, whereas the same can be said for 0.58% of our respondents. There are a few other examples where our sample is similar to the 2010 Census (e.g., Asian-Americans, etc.), but our sample was mostly different leading us to employ post-stratification weights (see discussion in Section S3.1) before extrapolating our results to the 2020 Census.

S2.2   Balance Statistics

Balance statistics were calculated using the **MatchBalance** function of the **Matching** library in the **R** statistical software language. These are presented in Tables S5 and S6 for the citizenship question and Census prompt, respectively. In the second and third columns, we report the means for the treatment ($\bar{X}_T$) and control ($\bar{X}_C$) groups for each of the variables listed in the first column. Unadjusted and Bonferroni-corrected p-values are reported in the fourth (p) and fifth ($\hat{p}$) columns, respectively. We also estimated pairwise interactions for all variables, but none of these were statistically significant at the 0.05-level.

Table S5: Balance Statistics for Citizenship Question Treatment

Variable	$\bar{X}_{\texttt{Treatment}}$	$\bar{X}_{\texttt{Control}}$	p-value	$\hat{p}$-value
Wave	0.540	0.552	0.266	1.000
Race/Ethnicity				
Hispanic	0.508	0.514	0.584	1.000
African-American/Black	0.026	0.029	0.526	1.000
Asian-American/Asian	0.022	0.019	0.267	1.000
White	0.382	0.376	0.550	1.000
Native American/Inuit/Aleut	0.003	0.003	0.712	1.000
Native Hawaiian/Pacific Islander	0.001	0.001	0.518	1.000
Other	0.022	0.025	0.325	1.000
Not Provided	0.050	0.053	0.638	1.000
Party				
Democrat	0.294	0.294	0.981	1.000
Republican	0.187	0.185	0.753	1.000
Independent	0.183	0.187	0.562	1.000
Other	0.022	0.025	0.325	1.000
Not Provided	0.315	0.310	0.605	1.000
Country of Birth				
North America				

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Table S5 – *Continued from previous page*

Variable	$\bar{X}_{\texttt{Treatment}}$	$\bar{X}_{\texttt{Control}}$	p-value	$\hat{p}$-value
Canada	0.002	0.002	0.669	1.000
United States	0.579	0.582	0.795	1.000
Latin America				
Mexico	0.017	0.017	0.982	1.000
Central America (Excl. Mexico)	0.004	0.007	0.055	1.000
Cuba	0.009	0.008	0.787	1.000
Puerto Rico	0.020	0.015	0.082	1.000
Caribbean (Excl. Cuba/Puerto Rico)	0.007	0.007	0.928	1.000
South America	0.014	0.015	0.576	1.000
Europe				
Northern Europe	0.004	0.002	0.064	1.000
Southern Europe	0.010	0.010	0.785	1.000
Eastern Europe	0.002	0.003	0.283	1.000
Western Europe	0.003	0.004	0.164	1.000
Africa				
Northern Africa	0.003	0.005	0.329	1.000
Middle Africa	0.003	0.003	0.980	1.000
Eastern African	0.001	>0.001	0.314	1.000
Western Africa	>0.001	>0.001	0.568	1.000
Asia				
Central Asia	>0.001	0.000	0.317	1.000
Southern Asia	0.004	0.003	0.277	1.000
South-Eastern Asia	0.003	0.003	0.723	1.000
Eastern Asia	0.003	0.004	0.412	1.000
Western Asia	0.003	0.002	0.818	1.000
Oceania				
Australia/New Zealand	>0.001	>0.001	0.993	1.000
Micronesia	>0.001	>0.001	0.995	1.000
Not Provided	0.308	0.303	0.656	1.000

Note: Unadjusted and Bonferroni-corrected p-values from two-sample t-tests reported in the last two columns. No pairwise interactions were statistically significant at the 0.05-level.

To calculate balance statistics for the country of birth, we grouped country into regions as defined by the World Bank Development Indicators which were obtained from the **countrycode** library in the **R** statistical software language. The only changes made to the World Bank regions were in North America where we separated the United States from Canada and used Latin American subdivisions that were more consistent with our survey. Ultimately, our respondents were born in 100 different countries and 24 different regions.

Table S6: Balance Statistics for Census Prompt Treatment

Variable	$\bar{X}_{\texttt{Treatment}}$	$\bar{X}_{\texttt{Control}}$	p-value	$\hat{p}$-value
Wave	0.545	0.546	0.904	1.000
Race/Ethnicity				
Hispanic	0.510	0.512	0.898	1.000
African-American/Black	0.026	0.029	0.260	1.000
Asian-American/Asian	0.019	0.021	0.479	1.000
White	0.383	0.376	0.486	1.000
Native American/Inuit/Aleut	0.002	0.003	0.474	1.000
Native Hawaiian/Pacific Islander	0.001	0.001	0.965	1.000
Other	0.023	0.024	0.776	1.000
Not Provided	0.053	0.050	0.551	1.000
Party				
Democrat	0.295	0.293	0.848	1.000
Republican	0.190	0.182	0.309	1.000
Independent	0.189	0.181	0.297	1.000
Other	0.023	0.024	0.776	1.000
Not Provided	0.303	0.321	0.068	1.000
Country of Birth				
North America				
Canada	0.002	0.003	0.199	1.000
United States	0.576	0.584	0.439	1.000
Latin America				
Mexico	0.018	0.017	0.796	1.000
Central America (Excl. Mexico)	0.005	0.007	0.311	1.000
Cuba	0.008	0.009	0.915	1.000
Puerto Rico	0.019	0.017	0.460	1.000
Caribbean (Excl. Cuba/Puerto Rico)	0.007	0.007	0.789	1.000
South America	0.013	0.016	0.251	1.000
Europe				
Northern Europe	0.004	0.003	0.420	1.000
Southern Europe	0.009	0.011	0.415	1.000
Eastern Europe	0.003	0.002	0.249	1.000
Western Europe	0.004	0.003	0.665	1.000
Africa				
Northern Africa	0.004	0.004	0.676	1.000
Middle Africa	0.004	0.003	0.242	1.000
Eastern African	>0.001	0.001	0.328	1.000
Western Africa	>0.001	>0.001	0.579	1.000

Table S6 – *Continued from previous page*

Variable	$\bar{X}_{\texttt{Treatment}}$	$\bar{X}_{\texttt{Control}}$	p-value	$\hat{p}$-value
Asia				
Central Asia	>0.001	0.000	0.317	1.000
Southern Asia	0.004	0.003	0.432	1.000
South-Eastern Asia	0.003	0.003	0.651	1.000
Eastern Asia	0.005	0.003	0.206	1.000
Western Asia	0.002	0.003	0.576	1.000
Oceania				
Australia/New Zealand	0.000	0.001	0.045	1.000
Micronesia	>0.001	0.000	0.157	1.000
Not Provided	0.309	0.302	0.468	1.000

Note: Unadjusted and Bonferroni-corrected p-values from two-sample t-tests reported in the last two columns. No pairwise interactions were statistically significant at the 0.05-level.

Beginning with Table S5, all Bonferroni-corrected p-values are well above the 0.05 threshold and approximate 1. Not only does this demonstrate we have reasonable balance across the treatment and control groups for our Citizenship Treatment, but none of the unadjusted p-values are below 0.05 which gives us additional confidence that our sample is equally distributed across both conditions. A similar results is found in Table S6. Again, the Bonferroni-corrected p-values approach 1 and only one unadjusted p-value is below the 0.05-level (respondents from Australia/New Zealand). This demonstrates the characteristics of the respondents who received the Census prompt are essentially the same as those who did not receive this treatment.

S3 Additional Analyses

S3.1 Survey Weights

In order to make our sample more nationally representative, we created post-stratification weights using the **rake** function from the **survey** library in the **R** statistical software language. Generally speaking, a raking algorithm take known population distributions and creates sample weights in order to make the sample's marginal distributions identical to their counterparts in the population. The process is iterative, meaning initial weights are created to make the marginal distribution of the first variable identical in the sample and population. Those weights are then adjusted so the marginal distributions of the second variable matches the population distribution. So forth and so on, until the algorithm converges and you have a weighted sample where the marginal distributions of the variables you provide are identical to those in the population.

Our vendor was able to provide the following information for our respondents: Race/Ethnicity, Country of Birth, Party Identification, Zip Code, and Religion. Of these variables, we had the best coverage for Race/Ethnicity and Zip Code which was provided for 94.99% and 95.03% of our respondents, respectively. From there, the information provided by Qualtrics becomes increasingly

sparse. Party identification was not provided for 31.21% of our respondents. Qualtrics did not identify the country of birth for 22.39% of our respondents. Not only was religion not provided for 43.00% of our respondents, but we also could not find population distributions for all 20 religions our vendor provided.

Given these limitations, we created post-stratification weights using the following variables: (1) Race/Ethnicity, (2) Zip Code, and (3) whether the individual identified as a Democrat. The raking algorithm would not converge in a reasonable amount of time using the other configurations which is why we focused our efforts on these three variables. Using our weighted sample, we then estimated the effect of receiving the citizenship question on the percentage of the household reported as being of Hispanic, Latino, or Spanish origin using the **svyttest** function from the **survey** library mentioned above. When this was done, we still found the citizenship question lead to a significant underreporting (1.97 percentage points) of Hispanic household members (t-statistic = 3.205, p-value = 0.001) which gives some evidence that the results from our experiment have some generalizability.

To extrapolate to the 2020 Census we used the **svymean** function from the **survey** library to derive weighted estimates (and their standard errors) of the proportion of the household that is Hispanic under treatment (11.85) and control (13.82). We then multiplied the total population according to the 2010 Census (308,745,538) by these weighted estimates yielding an estimated 36,587,056 and 42,659,124 Hispanics, respectively. We then subtracted the treatment estimate (36,587,056) from the control estimate (42,659,124) to arrive at 6,072,068 fewer Hispanics reported.

S3.2 Census Opinion Questions

At the end of the survey, we asked our respondents 13 questions (or 14 questions when Hispanic household members were listed) about their opinions of the 2020 Census and the Census Bureau more broadly. Generally speaking, respondents skipped a large number of these questions, meaning respondents who did offer opinions were likely different from those who did not. This caveat aside, we found Hispanics were generally more concerned about the 2020 Census.

Figure S1 shows Hispanic respondents generally are (1) less likely to participate in the 2020 Census and more concerned (2) their 2020 Census answers will be used against them, (3) the Census Bureau will share their answers with other government agencies, and (4) their answers will not be kept confidential. We think these additional results clearly demonstrate Hispanics are a "hard to count" population meaning any changes to the Census that could disproportionally affect this population should be made cautiously.

S3.3 Additional Hypotheses

In our pre-analysis plan, we registered several hypotheses and expectations. In this section, we will discuss some of the hypotheses that we did *not* discuss in the main text.

Figure S1: Hispanic Respondents Are Generally More Concerned About the 2020 Census
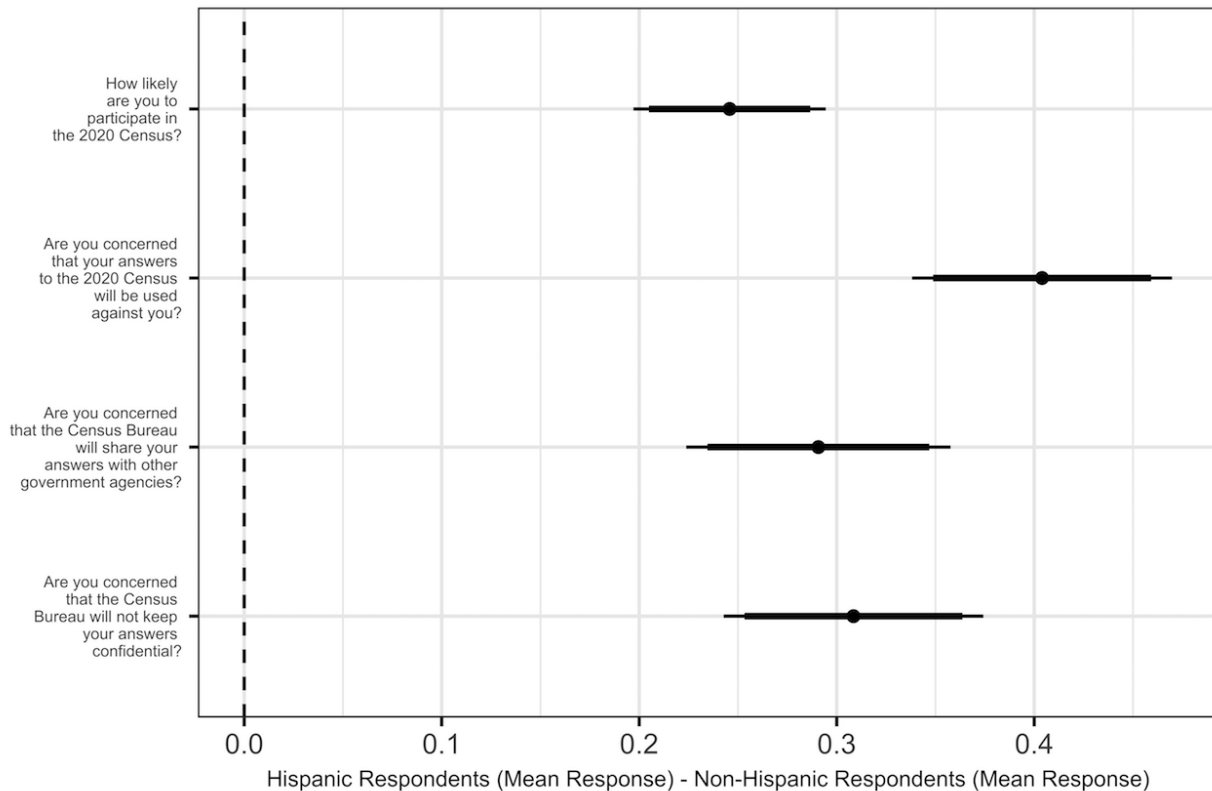
Note: Difference in the average response for Hispanic and non-Hispanic respondents. All questions are scaled so higher values mean a more negative answer. For example, "How likely are you to participate in the 2020 Census?" is coded as 1 = "Definitely will" and 5 = "Definitely will note." Similarly, "Are you concerned that the Census Bureau will not keep your answers confidential?" is coded as 1 = "Extremely concerned" and 5 = "Not at all concerned." The full questions are reported in Section S1.5 of the Supporting Material (SI). Thicker (━) and thinner (—) lines represent 90 and 95-percent confidence intervals, respectively.

Survey respondents who receive the Census prompt are less likely to begin the survey.

In our pre-registration we hypothesized that the invitees who receive the Census prompt will begin the survey at a rate of 4%, but we found this to be intractable given some unforeseen limitations to our survey. More specifically, we were unable to determine who received the link to the survey, meaning we only have data on those who actually started the survey. This makes it difficult, if not impossible, to directly test this hypothesis.

To gain some traction, we calculated the proportion of early questions skipped by respondents. Here, we define Questions 1, 2, and 3 as "early" questions. Respondents receiving the Census prompt skipped 8.68 percent of these questions, whereas those not receiving the Census prompt skipped 7.33 percent. This 1.35 percentage point difference is statistically significant at the 0.02-level (t-statistic = 2.523, p-value = 0.012).

We also conducted an additional analysis on the number of reported household members. Respondents who receive the Census Prompt, however, report smaller household sizes – an average household size of 2.75 under the treatment condition compared to 2.83 under the control condition, a significant drop (t-statistic = 2.07, p-value = 0.038). The drop is somewhat larger among Hispanics (3.04 under treatment vs. 3.15 under the control), although this difference is only statistically significant at the 0.07-level (t-statistic = 1.826, p-value = 0.068). We find a comparable, albeit smaller and insignificant, pattern among non-Hispanics (2.45 under the treatment vs. 2.50 for the control group; t-statistic = 1.028, p-value = 0.304).

Survey respondents who receive the citizenship question will answer fewer questions.

In the main text, we operationalize this hypothesis using the percent of questions skipped, but in our pre-registration we suggested those who receive the citizenship question should complete the survey at a rate of 80%. We tested this hypothesis by creating a dummy variable that equals 1 when respondents skipped more than 80 percent of the questions. Using this variable, we found 18.59 percent of respondents who received the citizenship question skipped more than 80 percent of the questions. Of those who did not receive the citizenship question, 14.94 percent skipped more than 80 percent of the questions. The 3.65 percentage point difference is also statistically significant (χ^2 = 9.788, p-value = 0.002).

We also conducted additional analyses regarding questions about respondent's age and date-of-birth. Conditional on the number of household members initially reported, respondents who received the Citizenship Treatment were significantly more likely to skip the questions concerning household members' ages, on average by 3.32 percentage points (t-statistic = 4.111, p-value less than 0.001). The effect is stronger among Hispanics, who experience an increase in questions skipped of 4.56 percentage points (t-statistic = 3.597, p-value less than 0.001).

We again find large, significant effects among Hispanic respondents who report being born in either Mexico or a Central American country, where receiving the Citizenship Treatment increases the percent of questions skipped on household members' ages by 10.95 percentage points (t-statistic = 3.274, p-value = 0.001). The corresponding increase among Hispanic respondents listing Cuba or Puerto Rico as their birth country is 0.14 (t-statistic = 0.045, p-value = 0.964). We again find a smaller 2.27 percentage point effect among non-Hispanics (t-statistic = 2.423, p-value

= 0.015).

Those who receive both the citizenship question and census prompt should show more pronounced effects.

In order to test this hypothesis, we estimated a simple linear regression where the proportion of questions skipped was regressed on the interaction between our Citizenship and Census Prompt Treatment. Ultimately, the interaction was not statistically significant at the 0.05-level (t-statistic = 1.392, p-value = 0.164) which we expect may be due to a number of factors.

First, we were unable to re-contact respondents who dropped out after receiving the Census Prompt, but before receiving the Citizenship Treatment. Consequently, of those who received the Census Prompt, we only observe the effect of the Citizenship Treatment for those who responded the *least* to the Census Prompt.

Second, the Census Prompt is likely underpowered. The concern from the Census Bureau is that individuals will receive either an email or envelope that says "United States Census Bureau." That is quite a bit different from our treatment that simply added a checkbox at the bottom of a 2-page description of how we will protect their confidentiality. Those confidentiality assurances and the Institutional Review Board (IRB) likely altered or diminished the effect of the checkbox we introduced.

Third, the effect of the Census Prompt is likely more pronounced on unit non-response. Individuals see an email or envelope that says "United States Census Bureau" and simply do not respond. Our survey is designed to test item non-response, but we cannot effectively measure unit non-response since we do not know who received the survey link and decided not to participate.

Finally, our respondents are not only paid to complete the survey, but they are part of a Qualtrics panel. Both factors give individuals strong incentives to complete the survey and given their repeated exposure to survey instruments they are also less likely to be concerned that they would see any detrimental effects from their participation. Consequently, when we say we will protect their confidentiality they likely have some confidences in those assurances, otherwise they would not be members of a Qualtrics panel.

We anticipate that respondents identifying as Democrat/Leaning Democratic are more likely to respond to either or both treatments negatively than are those who identify as Republican/Leans Republican.

We found some evidence of partisan differences. Democrats who received the citizenship question skipped 25.16 percent of the questions which is 3.14 percentage points more than the percent of questions skipped by those those who did not receive the citizenship question (22.02). Hispanics who were identify as Democrats by Qualtrics skipped 5.64 percentage points more questions when they received the citizenship question (t-statistic = 2.808, p-value = 0.005). No significant effects were found for Hispanics who identified as Republican (t-statistic = 1.114, p-value = 0.266) or Independent (t-statistic = 0.203, p-value = 0.839).

We anticipate little or no treatment effect among Puerto Ricans and Cuban Americans, but a negative treatment effect for both treatments among Mexicans and Central Americans.

Among the respondents from Mexico and Central America ($n = 240$), the citizenship question does seem to affect their response rate. More specifically, Hispanics who Qualtrics identified as being from either Mexico or Central American skipped 9.93 percent of the questions when they did not receive the citizenship question. When the citizenship question was randomly assigned this percentage increased to 20.97 percent and this 11.04 percentage point difference was statistically significant at the 0.05-level (t-statistic = 3.298, p-value = 0.001). When Hispanics who Qualtrics identifies as originating from Puerto Rico or Cuba ($n = 235$) received the citizenship question they skipped 13.56 percent of the survey. Under the control condition, these same respondents skipped 11.78 percent of the survey. This insignificant difference (t-statistic = 0.566, p-value = 0.572) and the significant difference for Hispanic respondents originating from Mexico and Central American provides evidence consistent with our pre-registered hypothesis.

We anticipate that Latina/o and non-whites will respond differently to the attitude questions we posed than non-Latina/o and whites, regardless of treatment status.

This hypothesis is discussed in Section S3.2 of the SI.

We anticipate respondents receiving either or both treatment conditions, relative to the control conditions will have different attitudes towards the Census.

We did not find evidence consistent with hypothesis. When we asked "How likely are you to participate in the 2020 United States Census? By participate, we mean fill out and mail in a Census form or fill one out online?" there were 5 response options (other than "Don't Know") ranging from "Definitely will" (1) to "Definitely will not" (5).

For this question we found no statistically significant difference between the mean responses for those who did (1.73) and did not (1.74) receive the citizenship question (t-statistic = 0.390, p-value = 0.697). The same can be said for the Census prompt. Although the effect is more pronounced, we again found no significant difference between the mean responses for the treatment (1.71) and control (1.75) groups (t-statistic = 1.890, p-value = 0.060).

We found the same results when we asked "How concerned are you, if at all, that the answers you provide to the 2020 Census will be used against you?" For this question there were again 5 response options (other than "Don't Know") ranging from "Extremely concerned (1)" to "Not at all concerned" (5). To make this variable comparable to the other questions, we inverted the scale so higher values implied greater concern.

Again, we found no statistically significant difference between the mean responses for those who did (2.22) and did not (2.24) receive the citizenship question (t-statistic = 0.700, p-value = 0.484). The same can be said for the Census prompt. Although the effect is more pronounced, we again found no significant difference between the mean responses for the treatment (2.21) and control (2.25) groups (t-statistic = 1.257, p-value = 0.209).

When we asked "How concerned are you, if at all, that the Census Bureau will share answers to the 2020 Census with other government agencies?" we found the same results. For this question the 5 response options (other than "Don't Know") were again inverted so "Extremely concerned" was re-coded as 5 and "Not at all concerned" was re-coded as 1. This was done to make this question comparable to the rest with higher values implying more concern.

We again found no statistically significant difference between the mean responses for those who did (2.61) and did not (2.64) receive the citizenship question (t-statistic = 1.114, p-value = 0.265). The same can be said for the Census prompt where no significant difference between the mean responses for the treatment (2.60) and control (2.65) groups (t-statistic = 1.220, p-value = 0.223).

Finally, neither treatment significantly affected responses to the following question "How concerned are you, if at all, that the Census Bureau will not keep answers to the 2020 Census confidential?" The 5 response options (other than "Don't Know") were again inverted so "Extremely concerned" was re-coded as 5 and "Not at all concerned" was re-coded as 1.

For this question we found no statistically significant difference between the mean responses for those who did (2.70) and did not (2.70) receive the citizenship question (t-statistic = 0.087, p-value = 0.931). The same can be said for the Census prompt. We again found no significant difference between the mean responses for the treatment (2.71) and control (2.69) groups (t-statistic = 0.748, p-value = 0.454).

Although these results are not consistent with our pre-registered expectations, the non-response rate in this section is noticeably higher than the rest. For example, respondents skipped 36.58% of the questions regarding their opinions towards the Census which is much higher than the 8.00% of questions skipped in the first part of our survey. Since we do not know the opinions of these missing respondents, it is difficult to say how this affects our original hypothesis, but we can say that the respondents who are answering these questions are likely different from those who did not.

S3.4   Congressional District Analysis

Since Hispanic populations are unevenly distributed across the United States, we were interested in whether certain congressional districts will be disproportionally affected by introducing the citizenship question. Although we have at least 1 respondent in all 435 congressional districts, our sample is not balanced across all districts making it difficult to properly estimate the effect of receiving the citizenship question within a single congressional district. One way to achieve this end is to subset our data by congressional district and then re-estimate the effect of receiving the citizenship question on the percent of the household reported as being of Hispanic, Latino, or Spanish Origin. We did this for all 435 congressional districts and identified the 10 districts where our Citizenship Treatment had the most pronounced effect.

Table S7 reports these initial results. Not only do districts from California represent 8 of the 10 districts most affected by our Citizenship Treatment, but many of the statistically significant differences exist within Southern California. Using an unadjusted p-value from a two-sample t-test, we find that California's 1st, 4th, 22nd, 37th, 38th, and 39th districts all show significant ($p < 0.05$) declines in the percentage of household members reported as being Hispanic when respondents

Table S7: Congressional Districts with Largest Marginal Effect (Citizenship Question)

District	Treatment (N)	Control (N)	Treatment (Mean)	Control (Mean)	Diff.	p-value
CA-22	9	13	25.00	78.57	-53.57	0.01
CA-48	7	3	35.71	88.89	-53.17	0.11
CA-37	8	6	2.50	54.17	-51.67	0.01
CA-1	6	6	12.50	63.89	-51.39	0.03
CA-4	7	10	20.00	70.00	-50.00	0.04
OK-4	2	8	16.67	62.50	-45.83	0.27
CA-39	9	7	22.22	66.67	-44.44	0.05
CA-38	10	7	46.07	88.10	-42.02	0.03
TX-35	10	13	43.33	84.62	-41.28	0.03
CA-45	5	11	15.00	53.03	-38.03	0.08

Note: This table shows the congressional districts where respondents seemed to respond the most to our Citizenship Treatment.

in those districts received the citizenship question. A similar decline is found in California's 45th district, but it is only statistically significant at the 0.10-level. All of these districts except for California's 1st, 4th and 22nd are in the Los Angeles metropolitan area and California's 22nd district is around 176 miles (or a 3 hour drive) away.

S3.5 Direct Costs Analysis

In this subsection, we try to extrapolate the costs of undercounting 6,072,068 Hispanics in the 2020 Census. It is difficult to say precisely how much it would cost, but we do our best using 2000 and 2010 Census estimates. At the high-end, re-contacting these households could cost anywhere between \$1,088,515,183 to \$1,287,212,181 in projected dollars which is a large percentage of the approximately \$5 billion Census operating budget.

The Census Bureau spent \$1,589,397,886 following up with 47,235,198 households in 2010, equalling a per-household rate of \$33.65 for the first followup. Households that had to be recontacted two and three times cost the Census Bureau \$84.09, and \$142.53, respectively. If we assume the 6,072,068 fewer Hispanics all live in unique households, then introducing the citizenship question could cost the Census Bureau up to \$204,325,088 in order to fill in the missing information. If these same households need to be followed-up with two and three times, then the citizenship question could cost the Census Bureau \$510,600,198 and \$865,451,852, respectively. Using the first followup rate (one additional contact at \$33.64) and the 95-percent confidence interval we report in the main text (5,761,284 to 6,382,820), filling in the missing information generated from introducing the citizenship question could cost anywhere between \$193,867,207 to \$214,781,893 assuming each missing Hispanic lives in a unique household. If we use the third followup rate (three additional contacts equaling \$142.53 for a single household), contacting these missing Hispanic household members could cost anywhere between \$821,155,809 to \$909,743,335 in 2010 dollars.

Between the 2000 and 2010 Census, the rate for conducting a single household followup increased from $26.58 to $33.65 which represents a 26.59 percent increase over the 2000 Census first followup rate. The 2010 Census second and third followup rates also increased by 90.20 and 96.32 percent based on the costs for the 2000 Census. If we assume the 2020 Census will see similar rate increases, then we can predict the first, second, and third followups will cost $42.60, $159.94, and $279.81 per household, respectively. Using the first predicted followup rate for the 2020 Census (one additional contact at $42.60) and the estimated 95-percent confidence interval we report in the main text (5,761,284 to 6,382,820), filling in the missing information generated by the citizenship question could cost anywhere between $245,430,698 to $271,908,132 assuming each missing Hispanic lives in a unique household. If we use the third predicted followup rate for the 2020 Census (three additional contacts equaling $279.81 for a single household), contacting these missing Hispanic household members could cost anywhere between $1,612,064,876 to $1,785,976,864 in projected dollars.

S3.6   Re-Analysis of Rosenfeld, Imai, and Shapiro (2016)

We are not the first study to discuss whether sensitive questions increase item non-response. Generally speaking, most studies of item nonresponse have looked at how respondent characteristics, interviewer effects, question ordering and question wording increase the number of invalid responses provided by the respondent (for review, see Shoemaker, Eichholz and Skewes 2002). Since sensitive questions can produce anxiety in respondents, respondents may omit responses to avoid embarrassment and decrease the risk of exposure or simply to protect their privacy (Tourangeau and Yan 2007). This can lead some respondents to provide more socially acceptable answers – something we ultimately find in the re-analysis presented in this section – or by simply refusing to answer sensitive questions. Such censorship is precisely why Shen and Truex (2020) used item non-response to create a self-censorship index, ultimately finding self-censorship is essentially the same in many authoritarian regimes which calls into question the degree these citizens falsify answers to support the regime.

Given the potential consequences of asking sensitive questions, Shen and Truex (2020) outlines several studies that have addressed the best ways to ask about socially undesirable behavior or especially personal issues. Indirect techniques, like list experiments (Blair and Imai 2012; Corstange 2009), endorsement experiments (Bullock, Imai and Shapiro 2011) and randomized response (Gingerich 2010), have all been offered as ways to elicit truthful responses by providing respondents some level of protection with respect to their answers. Ultimately, these studies have found such approaches tend to provide more accurate results, suggesting researchers do not have to abandon sensitive subjects all together.

In this subsection we re-analyze an experimental design from a study in this latter group. In Rosenfeld, Imai and Shapiro (2016), the authors used a list experiment, endorsement experiment, and randomized response to elicit opinions on the 2011 Mississippi Personhood Initiative that attempted to define a "person" in the State Constitution to include fertilized human eggs. They assessed the accuracy and bias of these approaches using validated voting data. Ultimately, they found direct questioning – similar to what was proposed with regards to the citizenship question – lead to significant underestimates of the actual vote outcome, whereas the list experiment, ran-

domized response and endorsement experiment all yielded better results. They also found the list experiment was more prone to social desirability and the randomized response performed surprisingly well.

This study uses the replication data from Rosenfeld, Imai and Shapiro (2016) to determine whether the spillover effects we isolated with respect to the citizenship question can also be found in this study. As we mention in the main text, even though what we identify below is more akin to sensitivity "doses," we think the broader importance of this study – as indicated by the 132 citations – makes it especially useful for testing the generalizability of our main findings. For those interested in conducting a similar analysis, the replication data for the study we replicated can be found on Harvard's Dataverse (`https://doi.org/10.7910/DVN/29911`).

As Rosenfeld, Imai and Shapiro (2016) explain on page 787, in the list experiment the control group was the following questions:

> Here is a list of four things that some people have done and some people have not. Please listen to them and tell me HOW MANY of them you have done in the past two years. Do not tell me which you have and have not done. Just tell me how many:
>
> - Discussed politics with family or friends;
> - Cast a ballot for Governor Phil Bryant;
> - Paid dues to a union;
> - Given money to a Tea Party candidate or organization.

The treatment group was given the exact same script, but the following item was added to the list:

> - Voted 'YES' on the 'Personhood' Initiative on the November 11 Mississippi General Election Ballot

As Rosenfeld, Imai and Shapiro (2016) explain on page 788, in the endorsement experiment the control group was asked the following questions:

> We'd like to get your overall opinion on some people in the news. As I read each name, please say if you have a very favorable, somewhat favorable, somewhat unfavorable, or very unfavorable opinion of each person.
>
> Phil Bryant, Governor of Mississippi?
> > Very favorable
> > Somewhat favorable
> > Don't know/no opinion
> > Somewhat unfavorable
> > Very unfavorable
> > Refused

The treatment group was given the exact same script, but information was added that Governor Bryant supported the personhood amendment:

> Phil Bryant, Governor of Mississippi, who campaigned in favor of the 'Personhood'
> Initiative on the 2011 Mississippi General Election Ballot?

As Rosenfeld, Imai and Shapiro (2016) explain on page 787, the direct question was administered as follows:

> Did you vote YES or NO on the Personhood Initiative, which appeared on the November
> 2011 Mississippi General Election ballot?
>
> Voted Yes
>
> Voted No
>
> Did not vote
>
> Don't know
>
> Refused

In this re-analysis, we are interested in respondents who received *both* the list and endorsement treatments versus those who received neither. More specifically, in the tables below the "Treatment" group is respondents who received the list treatment, endorsement treatment, and the direction question (list experiment + endorsement experiment + direct question) , whereas respondents who received the list control, endorsement control, and the direct question (list control + endorsement control + direct question) are labeled as the "Control" group. Since the direct question was always asked, we do not have a pure control group, but we think this comparison is the easiest for the purpose of this supplemental analysis.

The dependent variable is whether or not the individual changed their party identification on the survey. As part of their study, Rosenfeld, Imai and Shapiro (2016) obtained Mississippi's state voter history file that not only contains information about whether an individual voted, but also whether they identified as a "Democrat," "Republican," or "Other." This re-analysis is limited to respondents who the state of Mississippi recored as either Democrats or Republicans. We then coded whether those individuals did (1) or did not (0) change their party identification on the survey.

For our survey experiment, we actually had no information about the actual number of Hispanic household members. All we had was the percent of the household the respondent reported as being of Hispanic, Latino, or Spanish origin. This re-analysis is more ideal since we know what to expect with respect to party identification, so we know whether certain groups are underreporting in response to receiving the sensitive treatment. If sensitive questions have spill over effects, then in this re-analysis we should expect party identification to potentially be affected since experiment what is be manipulated is a sensitive *partisan* question.

Beginning with Table S8, we find Democrats and Republicans are more likely to change their party identification on the survey when they receive the list and endorsement experiments. Although this difference is only statistically significant at the 0.08-level, it is consistent with the spill over effects we saw in our citizenship question experiment. Table S9 shows this overall relationship is much more pronounced for individuals who voted against the initiative. According to

Table S8: Treatment Effects on Self-Reported Party Identification

	Treatment	Control	Difference	t-statistic	p-value
All	0.42	0.32	0.10	3.00	0.08
Female	0.40	0.27	0.13	2.25	0.13
Male	0.47	0.46	0.02	0.02	0.90
Democrats	0.33	0.19	0.14	2.20	0.14
Female	0.24	0.17	0.07	0.36	0.55
Male	0.22	0.09	0.13	0.67	0.41
Republicans	0.47	0.38	0.09	1.40	0.24
Female	0.49	0.33	0.15	1.77	0.18
Male	0.46	0.42	0.04	0.19	0.66

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last two columns report results from two-sample t-tests.

the authors "Based on interviews and their knowledge of Mississippi politics, most commentators expected the [personhood] initiative to pass easily" (785), meaning those who voted against the initiative believed they were in the minority when they answered the survey.

When the data was subsetted to this group, we found over 50 percent of respondents changed their party identification when they received the list and endorsement treatments, which is over twice the rate as those in the control group. Although it is impossible to say whether these changes in party identification are due the sensitivity of the question, we note no significant differences were found among those who voted in favor of the initiative. This suggests there is something about voting against the initiative that made survey respondents especially sensitive to the list and endorsement treatments.

One alternative explanation could be that people's party identification actually changed in response to the personhood initiative. If this were the case, then we should only find a difference in Republicans since by voting "no" they would be more likely to be on the opposite side of their party. Although there is a lot of party switching from Republicans, we find that Democrats also tend to switch their party identification. Indeed, 31 percent of Democrats who received the list and experiment treatment switched their party on the survey as compared to only 11 percent in the control. Not only is this over twice the rate, but it is more pronounced among female Democrats who are more likely to be against the initiative a priori which gives them little incentive to change.

The citizenship question was especially sensitive to some demographic groups leading to underreports. Given the partisan debate surrounding the personhood initiative, it is reasonable to expect partisans to be more sensitive to questions asking them to express their opinions about the initiative, especially for those who felt their opinions were in the minority. Ultimately, this is what we found and is consistent with our broader argument with respect to spill over effects. With that said, Rosenfeld, Imai and Shapiro (2016)'s study was not designed to answer such questions, so we view these results are purely suggestive.

Table S9: Treatment Effects on Self-Reported Party Identification (Voted No)

	Treatment	Control	Difference	t-statistic	p-value
All	0.53	0.25	0.28	6.00	0.01
Female	0.50	0.17	0.33	3.32	0.07
Male	0.54	0.29	0.25	3.09	0.08
Democrats	0.31	0.11	0.20	2.10	0.15
Female	0.43	0.14	0.29	1.40	0.24
Male	0.22	0.09	0.13	0.67	0.41
Republicans	0.67	0.39	0.28	3.20	0.07
Female	0.56	0.20	0.36	1.66	0.20
Male	0.73	0.46	0.27	2.16	0.14

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last
two columns report results from two-sample t-tests.

Table S10: Treatment Effects on Self-Reported Party Identification (Voted Yes)

	Treatment	Control	Difference	t-statistic	p-value
All	0.42	0.42	0.00	0.00	0.98
Female	0.35	0.37	-0.01	0.01	0.92
Male	0.47	0.46	0.02	0.02	0.90
Democrats	0.47	0.36	0.12	0.45	0.50
Female	0.22	0.25	-0.03	0.02	0.89
Male	0.70	0.50	0.20	0.64	0.42
Republicans	0.40	0.43	-0.04	0.13	0.72
Female	0.41	0.41	0.00	0.00	1.00
Male	0.38	0.45	-0.06	0.23	0.63

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last
two columns report results from two-sample t-tests.

S3.7   Meta-Analysis

Although political scientists have long noted the problems associated with sensitive questions (Rosenfeld, Imai and Shapiro 2016; Aronow et al. 2015; Blair and Imai 2012; Blair, Imai and Zhou 2015; Bullock, Imai and Shapiro 2011; Blair, Imai and Lyall 2014; Blair, Coppock and Moor 2020), less attention has been paid to the sensitivity of some demographic variables (for review, see Tourangeau and Yan 2007). Of these, questions about respondent income has received the most attention (for review, see Bound, Brown and Mathiowetz 2001; Moore, Stinson and Welniak 2000). Not only are such questions considered sensitive because of high non-response rates (Juster and Smith 1997), but many believe it is impolite to ask about someone else's income (van Melis-Wright and Stone 1993) and ultimately worrying about breached of confidentiality when such questions are asked (Brehm 2009). Moreover, telephone surveys are found to have higher rates of nonresponse for income questions as compared to face-to-face interviews (Jordan, Marcus and Reeder 1980), suggesting the mode of the interview also likely effects the willingness of respondents to answer some demographic questions. Ultimately, item nonresponse associated with income questions can create unreliable economic indicators (Micklewright and Schnepf 2010), especially for low-income populations (Mathiowetz, Brown and Bound 2002).

Similar effects have been found for questions about race/ethnicity (Martin, DeMaio and Campanelli 1990; Tourangeau and Smith 1996), most work in this area has focused on the unwillingness of racial/ethnic minorities to answers demographic questions more generally (Lor et al. 2017). Although a variety of cognitive and motivational factors undoubtedly influence the decision to respond to demographic questions (Moore, Stinson and Welniak 2000), the argument advanced in this study centers on fears of confidentiality (Singer, Hippler and Schwarz 1992) and how such fears vary as demographic questions become more and less "threatening" (Singer, Mathiowetz and Couper 1993; Tourangeau, Rips and Rasinski 2000). More specifically, we argue that contextual factors can make some questions – like asking about citizenship – seem more dangerous, especially to certain groups (Rodriguez 2000). Ultimately, this makes some groups – like Hispanics – more difficult to count (Ramirez and Ennis 2010), but also raises the broader question of whether demographic self-reports should be used in the first place.

This question is especially relevant to education researchers who have questioned the validity of education self-reports (Cole and Gonyea 2010; Cole, Rocconi and Gonyea 2012; Miller 2011; Somers et al. 2020). Again, research in this area finds that education self-reports become less reliable as performance decreases (Kuncel, Credé and Thomas 2005), meaning those who are at the lowest end of an educational scale are more likely to either provide a more socially desirable response (Gonyea 2005) skip the question altogether (Hamilton 1981). Consequently, previous studies have considered better ways to ask certain demographic questions (Moore 2006) and have looked at whether demographic questions are more likely to be answered when placed at the beginning (Babbie 2015) or end of a survey (Stoutenbourgh 2008). Although research on question placement has produced mixed results (Teclaw, Price and Osatuke 2012; Dillman 2011), the depth of the work in this area suggests political scientists should actively consider when and how demographic questions are sensitive to their respondents.

For example, Table S11 shows the number of respondents who refused to answer questions asking about their income, race and education in the most recent American National Election Study (ANES). For a point of comparison, we also provide the same count for party identifica-

Table S11: Demographic Item Nonresponse in 2016 American National Election Study (ANES)

	Income	Race	Education	Party ID	Total
Face-to-Face					
Refused = Y	53 (4.49%)	4 (0.34%)	0 (0.00%)	9 (0.76%)	66 (5.59%)
Refused = N	1127 (95.51%)	1176 (99.66%)	1180 (100.00%)	1171 (99.24%)	1114 (94.41%)
	1180	1180	1180	1180	1180
Web					
Refused = Y	137 (4.43%)	29 (0.94%)	15 (0.49%)	3 (0.10%)	184 (5.95%)
Refused = N	2953 (95.57%)	3061 (99.06%)	3075 (99.51%)	3087 (99.90%)	2906 (94.05%)
	3090	3090	3090	3090	3090
Face-to-Face + Web					
Refused = Y	190 (4.45%)	33 (0.77%)	15 (0.35%)	12 (0.28%)	250 (5.85%)
Refused = N	2953 (69.16%)	3061 (71.69%)	3075 (72.01%)	3087 (72.30%)	4020 (94.15%)
	4270	4270	4270	4270	4270

Note: Reports item nonresponse for income, race, education and party ID for the 2016 American National Election Study (ANES). Number of respondents are reported in each cell with column percents in parentheses. For example, 137 of the 3,090 respondents (or 4.43%) who took the survey online did not answer the question about family income.

tion. Beginning with income, we find that respondents are significantly less likely to report their household income than their party identification ($\chi^2 = 158.85, p < 0.001$). The same is true for their race/ethnicity ($\chi^2 = 8.94, p = 0.003$). Although – overall – there is no significant difference between the response rate for party identification and education ($\chi^2 = 0.15, p = 0.700$), when the data is restricted to those who took the survey online a significant difference is found ($\chi^2 = 6.74, p = 0.009$). Not only is this consistent with previous work on internet response rates for sensitive questions (for review, see Sakshaug, Yan and Tourangeau 2010), but it also helps partially explain why the effect sizes reported in the main text are slightly larger than those recently reported by the U.S. Census Bureau (Poehler et al. 2020)

Table S12: Studies From AJPS, APSR, and JOP Including at least One Sensitive Question (2015-2020)

Study	Education	Income	Race
American Journal of Political Science			
2020 (N = 44)			
Egan (2020)	X		
Ferrali et al. (2020)	X	X	
Hoffmann et al. (2020)	X		
Jung (2020)	X	X	

Continued on next page

Table S12 – *Continued from previous page*

Study	Education	Income	Race
Laustsen and Petersen (2020)	X		
Orr and Huber (2020)		X	X
Ward (2020)	X	X	
2019 (N = 60)			
Banks and Hicks (2019)	X	X	X
Broockman, Ferenstein and Malhotra (2019)	X	X	
Grewal et al. (2019)	X	X	
Groenendyk (2019)	X	X	X
Jones, Victor and Vannette (2019)	X	X	X
Pardos-Prado and Xena (2019)	X	X	
Velez and Newman (2019)	X	X	X
2018 (N = 65)			
Balmas (2018)	X	X	
Barnes and Hicks (2018)	X	X	
Bisgaard and Slothuus (2018)	X	X	
Campbell et al. (2018)	X	X	X
Dassonneville and McAllister (2018)	X		
Djupe, Neiheisel and Sokhey (2018)	X	X	X
Druckman, Levendusky and McLain (2018)	X	X	X
Hatemi and Fazekas (2018)	X	X	X
Hjortskov, Andersen and Jakobsen (2018)	X	X	X
Klar (2018)	X	X	X
Kosmidis (2018)	X		
McConnell et al. (2018)	X		X
Peisakhin and Rozenas (2018)	X	X	X
Reeves and Rogowski (2018)	X	X	X
Rosenfeld (2018)	X	X	
2017 (N = 64)			
Alt and Iversen (2017)	X	X	X
Barber, Canes-Wrone and Thrower (2017)	X	X	X
Bearce and Tuxhorn (2017)	X	X	X
Christenson and Kriner (2017a)	X		X
Christenson and Kriner (2017b)	X		X
Flavin and Hartney (2017)		X	X
Gaikwad and Nellis (2017)	X	X	
Garand, Xu and Davis (2017)	X	X	X
Jensen and Petersen (2017)	X		
Karpowitz, Monson and Preece (2017)	X	X	X

Continued on next page

Table S12 – *Continued from previous page*

Study	Education	Income	Race
Kertzer and Zeitzoff (2017)	X	X	X
Kim and Margalit (2017)	X	X	X
Lelkes, Sood and Iyengar (2017)	X	X	X
Mendelberg, McCabe and Thal (2017)	X	X	X
Mondak et al. (2017)	X	X	X
Pérez and Tavits (2017)		X	X
Ryan (2017*b*)		X	
Ryan (2017*a*)	X	X	X
Smidt (2017)	X	X	X
2016 (N = 66)			
Bishin et al. (2016)	X	X	X
Broockman and Ryan (2016)	X	X	X
Holbein and Hillygus (2016)	X	X	X
Koch and Nicholson (2016)	X	X	X
Miller, Saunders and Farhart (2016)	X	X	X
Rueda and Stegmueller (2016)	X	X	
2015 (N = 67)			
Branton et al. (2015)	X	X	X
Gibson and Nelson (2015)	X		X
Christenson and Glick (2015)	X	X	X
Grose et al. (2015)	X		X
Hirano et al. (2015)	X	X	X
Hainmueller and Hopkins (2015)	X	X	X
Iyengar and Westwood (2015)	X	X	X
Kasara and Suryanarayan (2015)	X	X	
Levy et al. (2015)	X		
Mason (2015)		X	X
Newman, Johnston and Lown (2015)	X	X	X
Pérez (2015)	X	X	X
Somer-Topcu (2015)	X		
Tesler (2015)	X	X	X
American Political Science Review			
2020 (N = 60)			
Akee et al. (2020)	X	X	X
Claassen (2020)	X	X	
Eggers and Vivyan (2020)	X	X	
Fournier, Soroka and Nir (2020)	X		X
Graham and Svolik (2020)	X	X	X

Continued on next page

Table S12 – *Continued from previous page*

Study	Education	Income	Race
Kalla and Broockman (2020)	X	X	X
Lyall, Zhou and Imai (2020)	X	X	X
Peyton (2020)	X	X	X
Simas, Clifford and Kirkland (2020)	X	X	X
Thal (2020)	X	X	X
Tomz and Weeks (2020)	X		X
2019 (N = 71)			
Arias et al. (2019)	X		X
Barber and Pope (2019)	X	X	X
Blair, Karim and Morse (2019)	X		
Carlson (2019)	X		X
de Benedictis-Kessner et al. (2019)	X	X	X
de Benedictis-Kessner and Hankinson (2019)	X	X	X
Hobbs and Lajevardi (2019)	X	X	X
Larsen et al. (2019)	X		X
Maxwell (2019)	X	X	X
Ward (2019)	X		
2018 (N = 71)			
Anoll (2018)	X	X	X
Auerbach and Thachil (2018)	X	X	
Butler and Hassell (2018)	X		
Enos and Gidron (2018)	X	X	X
Hankinson (2018)		X	X
Mo and Conn (2018)	X	X	X
Scacco and Warren (2018)	X	X	
Tertytchnaya et al. (2018)	X	X	
2017 (N = 49)			
Aarøe, Petersen and Arceneaux (2017)	X	X	X
Goren and Chapp (2017)	X		X
Healy, Persson and Snowberg (2017)	X	X	X
Holbein (2017)	X	X	X
Rosenfeld (2017)	X	X	
2016 (N = 54)			
Blattman and Annan (2016)	X	X	
Carnes and Lupu (2016)	X	X	X
Davenport (2016)		X	X

Table S12 – *Continued from previous page*

Study	Education	Income	Race
2015 (N = 47)			
Baker (2015)	X		
Bloom, Arikan and Courtemanche (2015)	X		X
Huddy, Mason and Aarøe (2015)	X	X	X
McEntire, Leiby and Krain (2015)	X		
Peffley et al. (2015)	X	X	
Journal of Politics			
2020 (N = 111)			
Bauer (2020)			X
Condon and Wichowsky (2020)	X	X	X
Rodon and Sanjaume-Calvet (2020)	X	X	X
2019 (N = 130)			
Chudy, Piston and Shipper (2019)	X	X	X
Clifford, Kirkland and Simas (2019)	X	X	X
Krupnikov and Levine (2019)	X	X	
Lajevardi and Abrajano (2019)	X	X	X
Newman and Malhotra (2019)	X		X
Ryan (2019)	X	X	X
2018 (N = 123)			
Ahler and Sood (2018)	X	X	X
Bakker and Lelkes (2018)	X	X	
Boudreau and MacKenzie (2018)	X	X	
Canelo, Hansford and Nicholson (2018)	X		
Goldman (2018)	X	X	X
Intawan and Nicholson (2018)	X	X	
Iyengar, Konitzer and Tedin (2018)	X	X	X
Kam and Burge (2018)	X	X	X
Levendusky (2018)	X	X	X
Margolis (2018*a*)	X	X	X
Margolis (2018*b*)	X	X	X
Piston et al. (2018)	X	X	
Schleiter and Tavits (2018)	X	X	
Suhay and Garretson (2018)	X	X	X
2017 (N = 109)			
Alvarez, Levin and Núñez (2017)	X	X	X
Burden et al. (2017)	X	X	
Burden, Ono and Yamada (2017)	X	X	X

Continued on next page

Table S12 – *Continued from previous page*

Study	Education	Income	Race
Busby, Druckman and Fredendall (2017)		X	X
Coe et al. (2017)	X		X
De Kadt (2017)	X	X	X
Doherty et al. (2017)	X		X
Emmenegger, Marx and Schraff (2017)	X	X	X
Falcó-Gimeno and Muñoz (2017)	X		
Gschwend, Meffert and Stoetzer (2017)	X	X	
Kuo, Malhotra and Mo (2017)	X	X	X
Lelkes and Westwood (2017)	X		X
Lerman and McCabe (2017)	X	X	X
Morgan and Kelly (2017)	X		X
Mummolo and Nall (2017)	X	X	X
Myers (2017)	X	X	X
Peterson (2017)	X	X	X
Ponte et al. (2017)	X	X	X
Weitz-Shapiro and Winters (2017)	X	X	
West (2017)	X	X	X
2016 (N = 84)			
Acharya, Blackwell and Sen (2016)	X	X	X
Alt, Marshall and Lassen (2016)	X	X	
Baker et al. (2016)	X	X	
Barnes and Córdova (2016)	X	X	
Bisgaard, Sønderskov and Dinesen (2016)	X	X	X
Corstange (2016)	X	X	
Driscoll and Maliniak (2016)	X		
Fortunato, Stevenson and Vonnahme (2016)	X		
Hafner-Burton, Victor and LeVeck (2016)	X	X	
Häusermann, Kurer and Schwander (2016)	X	X	X
Johnston and Ballard (2016)	X	X	X
Kam and Estes (2016)	X	X	X
Lyons et al. (2016)	X	X	
McCann and Chavez (2016)	X	X	X
Mérola and Helgason (2016)		X	
Mummolo (2016)	X	X	X
Reeves and Rogowski (2016)	X	X	X
Simonovits and Kézdi (2016)	X	X	X
Sirin, Villalobos and Valentino (2016)	X	X	X
Stephens-Dougan (2016)	X	X	X
2015 (N = 82)			

Table S12 – *Continued from previous page*

Study	Education	Income	Race
Abrajano (2015)	X	X	X
Bisgaard (2015)	X	X	
Carlin and Moseley (2015)	X	X	X
Carlin and Singh (2015)		X	
Carnes and Sadin (2015)	X	X	X
Cavaillé and Trump (2015)		X	
Chong et al. (2015)	X	X	
Conroy-Krutz and Moehler (2015)	X	X	X
Hayes and Lawless (2015)	X	X	X
Just and Anderson (2015)	X	X	X
Rocha, Knoll and Wrinkle (2015)	X		X
Wilson and Hobolt (2015)	X	X	

Regardless of whether demographic nonresponse is higher or lower for internet surveys, one thing is clear: political scientists consistently ask respondents questions about income, race and education without mentioning many of the troubles associated with these questions. More specifically, Table S12 reports every study published between 2015 and 2020 in the American Journal of Political Science, Journal of Politics and American Political Science Review that asked respondents to report at least one of these troublesome demographic questions. In total, 185 of 1357 included at least one question about income, education or race. Although some omissions are undoubtedly warranted, we found it especially problematic that in the 151 that asked respondents a question about their income there was little to no discussion of the literature surrounding the sensitivity of that question. In the main text, we use the citizenship question to demonstrate the potential consequences of asking such questions and in the next subsection we generalize this finding using an applicable simulation.

S3.8   Simulation Study

In order to assess the broader implications of the findings we outline in the main text, we conducted a simulation study using code and data from eight studies published in the *American Journal of Political Science* and the *Journal of Politics* between 2015-2019. These studies were selected using two criteria.

First, we only included studies that used a large-N survey of the United States. Although we expect sensitive questions to have spillover effects in other countries and within laboratory settings, we chose these more traditional American political surveys because they were more similar to the proposed deployment of the citizenship question by the U.S. Census Bureau. Second, we selected studies with diverse methodological approaches. For example, Campbell et al. (2018) uses a structural equation model, whereas a list experiment is used by Burden, Ono and Yamada (2017). We did this to determine whether spillover effects are more pronounced in some methodological

approaches versus others. Finally, the studies must have a clear figure or table that outlines results from models that include either education, income or race as a control variable. This criteria ensures that readers can clearly draw parallels between the original studies and the results of our simulation.

Table S13: Studies Used For Simulation

Study	Figure/ Table	Model	Page	Education	Income	Race
Burden, Ono and Yamada (2017)	Figure 1	1	1077	✓	✓	✓
Campbell et al. (2018)	Table 4	1	562	✓	✓	✓
Hatemi and Fazekas (2018)	Figure 4	6	883	✓	✓	✓
Kam and Burge (2017)	Table 1	1	317	✓	✓	✓
Kuo, Malhorta and Hyunjung Mo (2016)	Table 1	1	24	✓	✓	✓
McCann and Nishikawa Chávez (2016)	Table 2	1	1202	✓	✓	✓
Rocha, Knoll and Wrinkle (2015)	Table 1	1	907	✓		✓
Tomz and Weeks (2019)	Table 1	2	186	✓		✓

Note: This table shows the studies used in the simulation described on page S57. The last three columns indicate the sensitive variables included in the original study. More details about the studies can be found in the associated discussion (see pages S55–S48). Both Kuo, Malhorta and Hyunjung Mo (2016) and McCann and Nishikawa Chávez (2016) included a citizenship question. All other studies did not.

Table S13 includes information about all the studies we included in our simulation as well as the specific model we used in the analysis below. For each of these studies, we first obtained the replication files from their respective Dataverses. Using these files, we then identified the code that reproduced the target model. Once we were able to successfully replicate the original results, our simulation consisted of the following steps for each sensitive variable:

1. Take a random sample with replacement of the entire dataset

2. Randomly assign a survey location to all variables

3. Randomly assign a percentage of respondents who will drop out of the survey in response to a sensitive variable

4. For these respondents, enter `NAs` for all variables after the sensitive variable using the random locations created in Step #2

5. Re-estimate the target model using the data produced in the previous step

6. The coefficients from this re-estimated model are then stored in a vector

These steps were repeated 10,000 times for all variables associated with the respondent's education, income and race. The percentage of respondents who could drop out of the survey was restricted to the following set: $\{0.01, 0.02, 0.03, 0.04, 0.05, 0.06, 0.07, 0.08, 0.09, 0.10\}$, meaning the lowest dropout rate was 1 percent and the highest was 10 percent. Please refer to Algorithm 1 for the complete simulation.

Algorithm 1: Simulating Spillover Effects Produced by Sensitive Variables

Data: Model (M) and Survey (S) with M Respondents (R $\in$ R$_1$, R$_2$, ..., R$_m$) and N Variables (X $\in$ X$_1$, X$_2$, ..., X$_n$)

**Result:** 10,000 simulated bootstrapped coefficients from M

Results = {};

for *sensitive_variable in* {*education, income, race*} **do**

    for *sensitive_percent in* {*0.01,0.02,...0.10*} **do**

        while *10,000 > boot* **do**

            boot = boot + 1;

            Sample S with replacement ($\hat{S}$) yielding $\hat{R}$ and $\hat{X}$;

            Randomly assign Location ($\hat{L}$) to $\hat{X}$ ($\hat{L} \in \hat{L}_1, \hat{L}_2, ..., \hat{L}_n$);

            for *variable in* $\hat{X}$ **do**

                if *variable* $\neq$ *sensitive_variable* **then**

                    if $\hat{L}_{variable} > \hat{L}_{sensitive\_variable}$ **then**

                        Sample variable equal to sensitive_percent times length of $\hat{R}$ ($\widehat{variable}$);

                        Set $\widehat{variable}$ to NA;

                    end

                else

                    Sample variable equal to sensitive_percent times length of $\hat{R}$ ($\widehat{variable}$);

                    Set $\widehat{variable}$ to NA;

                end

            end

            Estimate M using $\hat{S}$ and extract coefficients ($\hat{C}$);

            Add $\hat{C}$ to Results

        end

    end

end

To help understand these steps, imagine there is a 10-question survey of 100 respondents and there is one question asking about the respondent's race. In Step #1, we would randomly sample all 100 rows with replacement. We would then randomly assign a number between 1 and 10 to all questions in the survey. Let's assume in the first iteration of the simulation that 5 percent of the respondents will dropout once they are asked about their race. In this same iteration, let's also assume that the race variable was randomly assigned a survey location of 4. Then, for 5 percent of the respondents, variables with assigned locations greater than or equal to 4 would be given NAs – resulting in a dataset that is identical to the original bootstrapped dataset, but for seven questions where 5 percent of the responses from 5 percent of the respondents are now entered as missing. This dataset is then used to re-estimate the target model, yielding simulated bootstrapped coefficients.

The results are show in Figure S2. Here, the quantities of interest are the average size of the 95% confidence intervals from the original model and the average size of the 95% confidence intervals produced by the simulation outlined in Algorithm 1. More specifically, we are interested in the percent change, meaning we take the difference between the average size of the simulated

Figure S2: Hispanic Respondents Are Generally More Concerned About the 2020 Census
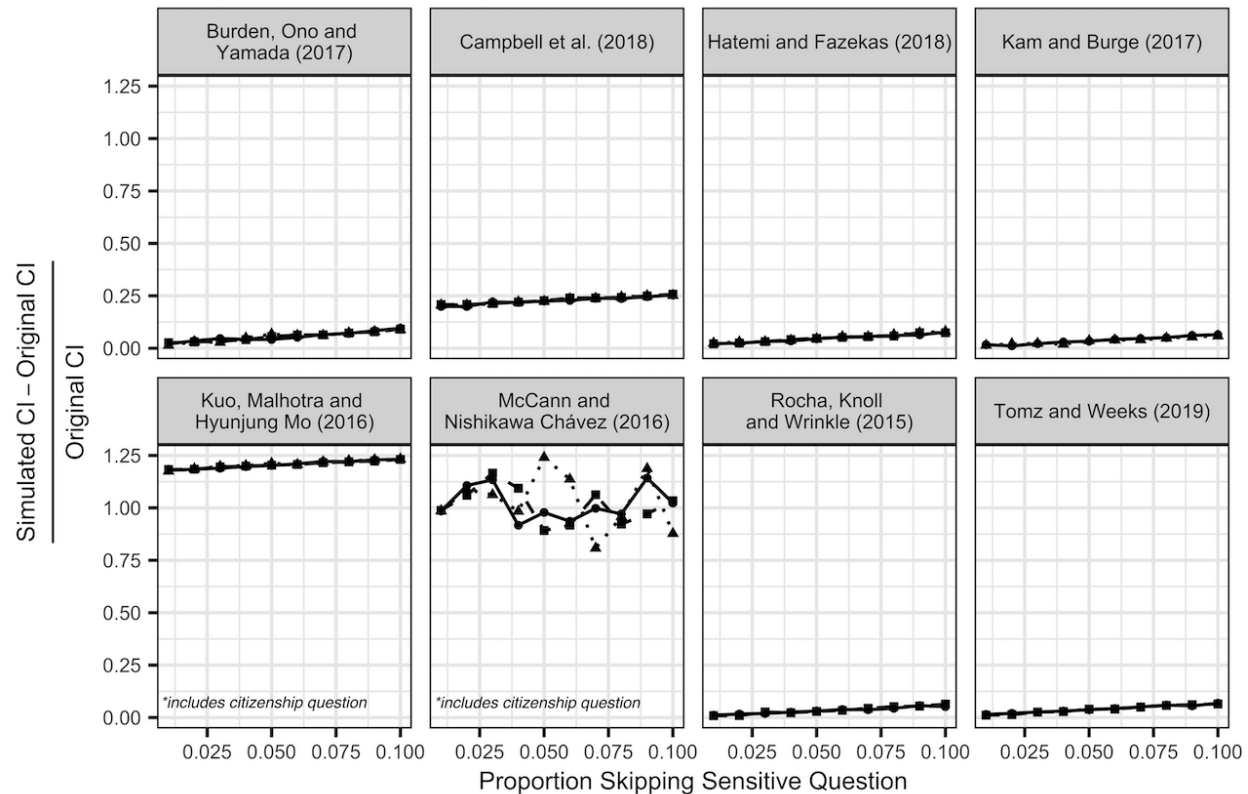
Note: Difference in the simulated confidence intervals and original confidence intervals by proportion of missingness due to three sensitive questions. Y-axis represents this difference as a percent increase/decrease relative to the original confidence interval. X-axis is the proportion of respondents who dropped out after the sensitive question was asked. Education, income and race are the sensitive variables represented by the solid lines with circles (━•━), dotted lines with triangles (·▲·) and dashed lines with squares (━■━), respectively. Please see Table S13 and associated discussion (see pgs S55–S48) for details about the studies used in this simulation. Both Kuo, Malhorta and Hyunjung Mo (2016) and McCann and Nishikawa Chávez (2016) included a citizenship question. All other studies did not.

95% confidence intervals and the average size of the original 95% confidence intervals and divide by that difference by the latter. For example, if the average size of the 95% confidence intervals produced by the original model is 1 and the average size of the 95% confidence intervals produced by our simulation is 1.5, then the percent increase would be 0.50 ($\frac{1.5-1}{1} = 0.50$). This suggests the sensitive variables in the original model increase the average confidence interval by 50%, assuming our simulation is an accurate reflection of the spillover effects described in this study.

We focus on the potential expansion of confidence intervals due to the inclusion of sensitive variables because this is precisely what many worried about with respect to the citizenship question and the 2020 U.S. Census. Indeed, if the citizenship question caused some respondents to drop out of the survey, then it would could create wildly different estimates of certain quantities of interest, like the percent of the population that is Hispanic. Said differently, the confidence intervals around these quantities of interest would increase with the inclusion of the citizenship question which is precisely what our simulation is attempting to assess. With that said, all simulations are abstract representations of such effects which is why we use this simulation to demonstrate one way the spillover effects we identified with respect to the citizenship question could potentially impact political science research.

Looking to Figure S2, we found – on average – when 1 percent of respondents dropout of the survey in response to a sensitive question the 95% confidence intervals increase by 34.84%. This means the simulated confidence intervals are over thirty percent larger than the original confidence intervals even when only a small percentage of respondents dropout of the survey. Moreover, since the location of the questions are randomized in our simulation this simulated effect is the average we would expect independent of where the sensitive question appears in the survey. In the main text we found this citizenship question lead Hispanics to be underreported by around 8 percent. If this percent is used in our simulation, then we would expect the 95% confidence intervals to increase by 37.87% which would likely effect the precision of estimates of various quantities of interest, including the relationship between important independent and dependent variables.

One criticism of these results is that they are likely being heavily influenced by the simulations associated with Kuo, Malhotra and Mo (2017) and McCann and Chavez (2016). Indeed, when our simulation results are restricted to these two studies, then a 1 percent drop out increases – on average – the size of the 95% confidence intervals by 108.28%, as compared to the originals. This is over three times the size of the average effect (34.84%) when the same percent is used. Although we do not have a good explanation for why the simulated effects are so high in these studies, it is worth noting that these are the only two studies that actually asked respondents whether they are U.S. citizens. Similarly, McCann and Chavez (2016) is the only study we include in our simulation that utilized multiple imputation which is why there is much more variability in that study's simulated effects as compared to the others. This suggests multiple imputation may not fully address the problems associated with sensitive questions and could potentially exacerbate them.

S3.9   Re-Analysis Using Coppock (2019) Coding

After reading Coppock et al. (2019), we were concerned about post-treatment bias, so – following a useful suggestion from an anonymous reviewer, we re-estimated Table 2 in the main text using

Table S14: Treatment Effects on Percent of Household Reported as Being Hispanic

	Treatment	Control	Difference	t-statistic	p-value
All	30.88	32.97	-2.08	2.289	< 0.023
Hispanic	53.03	55.33	-2.30	1.750	< 0.081
Mexico/Central America	78.86	83.67	-4.81	1.196	< 0.233
Puerto Rico/Cuba	85.64	85.74	-0.100	0.027	< 0.979
Non-Hispanic	8.000	9.320	-1.320	1.681	< 0.093

Note: Treatment mean, control mean, and the difference between the two are shown in the first three columns. Last two columns report results from two-sample t-tests.

the following coding:

> When the subject answers the question about a family member's race/ethnicity AND reports Hispanic he/she is coded as 1 and he/she is coded as 0 either if the subject does not answer the question OR does answer and reports Hispanic.

As shown in Table S14, our results remain largely the same as those reported in the main text. Although the results for Hispanics who list either Mexico or a country in Central American as their country of origin are less significant when the outcome variable is coded in this way.

S3.10 Comparing our Study to Previous Studies on Census Non-Response

Several studies have been conducted both inside and outside of the U.S. Census Bureau considering the potential consequences of adding a citizenship question to the 2020 Census. Overall, some find evidence of lower census participation among immigrant populations (Van Hook et al. 2014), particularly due to concerns – especially among nonwhites (Singer, Mathiowetz and Couper 1993) – over privacy and the confidentiality of information they provide to the census (Singer, Van Hoewyk and Neugebauer 2003). However, since the U.S. Census has not asked about citizenship status on the decennial census recently, the best estimates on the effect of asking about citizenship on non-response or attrition rates comes from other sources. Among these is the American Community Survey (ACS), also administered by the U.S. Census Bureau, that routinely asks about household members' place of birth, citizenship, and year of entry. Some research employing ACS data suggests that asking about citizenship on the census may reduce participation among households with noncitizen members (Brown et al. 2018) while disproportionately affecting nonresponse rates among non-whites (O'Hare 2018). Indeed, prior research (Van Hook et al. 2014) has found that both the Census and ACS substantially undercount Hispanic immigrants and their children.

All of these prior findings are, however, derived from observational data and cross-survey comparisons which are ill-suited for estimating the causal effect of including (or not including) questions that ask about residents' citizenship. Given that, a randomized controlled trial (RCT) that randomly manipulates the presence of citizenship questions is much better suited to estimate

causal effects. In 2019, the Census Bureau conducted such a trial, the results of which were recently reported in January 2020 (Poehler et al. 2020). Although – for the most part – the estimated effect sizes were smaller, the results of this RCT is consistent with what is reported in Tables 1 and 2 and throughout the SI.

More specifically, Poehler et al. (2020) found the citizenship question significantly increased non-response in areas with more than 49.1 percent Hispanic residents (see Table 12) and the largest effect was found in Los Angeles (see Table 14) which is consistent with what we find Section S3.4 in the SI. Perhaps most importantly, the Census Bureau also found Person 1 was less likely to self-identify as being Hispanic when they received the citizenship question (see Table 20) and break-offs were significantly more likely (see Table 29), especially in questions asking about the respondent's demographics (see Table 30). Collectively, these results provide more evidence underlining the importance of spillover effects and further bolster the results reported in our study.

However, our study differs from Poehler et al. (2020) in three main ways. First, the Census Bureau only reports results for Person 1 (or the person answering the survey), whereas our study shows spillover effects can also influence the amount of information respondents provide about their household members. Second, the Census Bureau has no way to verify whether the respondent is actually Hispanic. Instead, the Poehler et al. (2020) uses tract-level demographic data that is less accurate than the individual-level demographic data provided by Qualtrics. Finally, and perhaps most importantly, the citizenship question was always the last question asked after collecting a person's name, sex, age, Hispanic origin, and race (Poehler et al. 2020, 2), meaning all effects are estimated using pre-treatment questions. Collectively, these differences may explain why the Census Bureau's estimate is smaller than what we found in this study, but more research is needed to better understand the effects of these differences.

Although there are compelling reasons why our citizenship question application makes a significant contribution to the previous literature reported by the Census, our study also raises the broader question of the academics' role in real-life policy discussions. Ultimately, the academic community not only brings alternative methodologies to policy discussions, but they also can provide independent and transparent verification which is helpful, especially when dealing with complex policy issues like the citizenship question and the 2020 U.S. Census. Moreover, policymakers face fundamental uncertainty regarding a large number of sensitive policy issues, like citizenship, abortion, sexual behavior, or race/ethnic identity. Although the Census Bureau can help address many of these uncertainties, the academic community can undoubtedly handle some of this load.

Simply put – when it comes to complex policy discussions, more – not less – input is needed. This is especially true with a citizenship question that exists in an ever-changing political environment. Indeed, the Poehler et al. (2020) began on June 13, 2019 which is more than a year after President Trump announced that a citizenship question was going to be added to the 2020 Census (Wang 2018). Our study was conducted in the fall of 2018 when the citizenship question was likely much more salient, especially since the Supreme Court ruled on June 27, 2019 that adding a citizenship question to the 2020 Census would violate federal law (Liptak 2019). Rather than saying one study is better than another, we think both studies provide an important snapshot into the sensitivity of the citizenship question which is why our study makes an important contribution to this broader policy discussion.

For example, the effect sizes found by Poehler et al. (2020) are substantially less than those

reported in Tables 1 and 2 in the main text. One reason why this may be the case is due to the oversampling of Hispanics in our study. More specifically, 8.8% of respondents in Poehler et al. (2020) self-identified as Hispanic (23), whereas 56.41% of the respondents in our study identified as Hispanic. Similar to the Census, we too are interested in how the citizenship question generally influences response rates, but we also knew that this effect would be more pronounced in Hispanics which is why we oversampled this population in our study. This is not meant to suggest that our results are more valid, but instead to provide yet another reason why academic research is an important complement to government studies, like the one recently published by Poehler et al. (2020).

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