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Does asking about citizenship increase labor survey non-response?

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Abstract

The unsuccessful attempt to add a citizenship question to the 2020 US Census has drawn attention to citizenship questions on other surveys. Simultaneously, researchers have noted a secular increase in Current Population Survey (CPS) non-response. We combine these topics, studying the effect of the CPS citizenship question, added in the 1994 CPS redesign, on refusals. Direct panel regressions show states with higher rates of non-citizenship have higher refusal rates. An event-study regression discontinuity shows a 20-40% increase in refusals attributable to the redesign. Moreover, a difference-in-differences research design shows states with larger non-citizen and Hispanic populations were more affected by the redesign. These results imply the question causes non-citizens and Hispanics to refuse to participate in the survey disproportionately. Given the question appears to threaten the representativeness of the survey, we recommend there be a randomized controlled trial to precisely determine the question's effects.

Keywords Current population survey · Non-response · Survey refusal · Citizenship status · Immigration · Event study · Regression discontinuity · Panel data

JEL Classification C23: Panel Data Models · Spatio-temporal Models, C83: Survey Methods · Sampling Methods, J15: Economics of Minorities, Races, Indigenous Peoples, and Immigrants · Non-labor Discrimination

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1 Introduction

In the leadup to the 2020 Census, the Trump Administration attempted to add a citizenship question to the questionnaire. Critics argued the question would reduce response rates, particularly among vulnerable immigrant and Hispanic communities. A contentious legal battle concluded in 2019 when the Supreme Court ruled that the question could not be added (Berman 2019b).

This ruling did not settle all debate over the matter. Trump issued an executive order compelling the Census Bureau to investigate the issue using administrative data, such as state driver's license records, arguing states might desire to draw representative districts based on the location of eligible voters, not the population at large (Narea 2019). Unsurprisingly, this order was also challenged in court by civil rights groups such as the Mexican American Legal Defense and Educational Fund, who called it a "racially discriminatory scheme" to increase the political power of white people at the expense of minorities (Berman 2019a; Wang 2019a). Furthermore, in July 2020, the Trump administration released a memo attempting to prevent unauthorized immigrants from being counted in the Census for Congressional apportionment (Bahrampour 2020).

The newfound prominence of the issue has drawn attention to citizenship questions in other surveys. The 2019 Census Test Form, the American Community Survey (ACS), and the Current Population Survey (CPS) all feature citizenship questions (Wang 2019c). These questions have many similarities. For instance, the Census Test Form question, the proposed Census citizenship question, and the CPS question share the same five possible final answers, which are detailed in the Appendix (Wang 2019b, CPS Microdata Documentation).

The citizenship question was added to the CPS as part of the redesigned survey that took over as the official survey in January 1994. Save for smaller adjustments, this version of the CPS persists as of 2022. The redesign made questions more clear and consistent, allowed increased computerization, and implemented new questions. One of the new questions was the aforementioned citizenship question (Cohany et al. 1994). Based on concerns about the Census citizenship question's effects on survey participation, we aim to determine if the Current Population Survey's citizenship question is responsible for an increase in non-response, particularly among non-citizen and Hispanic individuals.

This question is important for several reasons beyond its political salience. In recent decades, non-response has risen across numerous surveys in many developed countries. The CPS is no exception (Tourangeau and Plewes 2013). Since 2010, the percentage of households not completing the survey has increased dramatically, primarily due to increased refusals (Bernhardt et al. 2021; Krueger et al. 2017). Refusals are a form of self-selection, and therefore could be distinctly non-random. This could make the final sample non-representative, potentially biasing headline statistics. In recent years, the United States immigrant population has been growing in both absolute and relative size. The Hispanic population, which contains the largest immigrant group, has also been growing in both respects as well (documented in Krogstad and Noe-Bustamante 2021; Budiman 2020 articles). It is important that the CPS properly

measure these groups for the accuracy of labor market statistics and to ensure that policy is inclusive and representative of the country.

We address this research question by using CPS microdata from 1989 to 2002, inclusive. We first aggregate our data at the national and state levels, and then employ a regression discontinuity approach to show that the 1994 redesign overall is associated with an increase in survey refusals on the order of 50-100 basis points. Panel regressions show that larger non-citizen populations are associated with higher refusal rates. Employing difference-in-difference designs, we find that states with greater non-citizen and Hispanic populations experienced disproportionately large increases precisely in January 1994, strongly implying the question played a key role.

This paper is organized as follows: Section 2 provides a background on US immigration, the CPS, non-response in surveys, and possible mechanisms for non-response associated with the citizenship question. Section 3 describes the data used and the estimation strategies employed. Section 4 presents the empirical results. Our event-study regression discontinuity and direct panel regression results establish that the redesign caused an increase in refusals, and refusals are associated with citizenship status. We then present results from a difference-in-difference research design relating disproportionate January 1994 increases in refusals to non-citizen and Hispanic populations. Section 5 summarizes the major empirical findings and policy implications. Finally, Section 6 discusses the main conclusions, limitations, and possible extensions. An Appendix presents additional tables and figures, and provides precise details about the citizenship question and our data.

2 Insights from the literature

In this section, we begin with a brief history of immigration policy in the United States. We then provide background and define key terms relating to the Current Population Survey and studies of survey methodology, including an in-depth look at the 1994 CPS redesign. Next, we review the related literature. We discuss the documented relationships between non-response, citizenship, and ethnicity in numerous surveys and censuses. We then go on to review the literature analyzing the measurement and effects of, as well as possible solutions to survey non-response.

2.1 Immigration policy in the United States

The United States has often been called a country of immigrants. However, for nearly as long, it has been a country of *anti*-immigrant sentiment as well. In the 1800s, millions immigrated to the United States, typically from Southern or Eastern Europe and Asia. This did not happen without backlash. By the 1880s, nativist sentiment had become a powerful political force (Young 2017).

Chinese immigration to the West Coast was met by intimidation, violence, and legislative assault. During the 1880s and 1890s, the Federal government passed laws that effectively prevented immigration from China for several decades and curtailed the rights of Chinese immigrants already living in the country. During the same period,

millions of immigrants from Southern and Eastern Europe arrived in the country, meeting similar backlash. Beginning in 1921, an immigration quota system was established. This system was strengthened in 1924 by a law that "effectively ended the great wave of 'new' European immigration" of the late 1800s.

Mexican immigration began later, but had become substantial by the 1890s. Perhaps surprisingly, there were few restrictions on immigration from the Western Hemisphere for decades, even as groups from the Eastern Hemisphere were subjected to quotas and literacy tests. This was not due to tolerance, as racialized nativist prejudice was even more pronounced, but rather has been attributed to economic factors. Demand for labor in the Southwest was high (partly due to *other* immigrant groups being restricted) in many industries and occupations. Mexican laborers were able to meet some of this demand. Pressure from business interests relying on their labor counteracted campaigning from anti-immigrant groups (Young 2017).

The Great Depression changed the dynamics of the situation, and fueled increased nativist activity. Governments began "repatriation drives," campaigns of harassment and coercion designed to lead to deportation. Hundreds of thousands (or even millions) of Mexican-Americans were pressured or forced into leaving the country. By some estimates, citizens, many of whom were children, constituted 60% of those removed (Balderrama and Rodriguez 2006). Approximately 1/3 of the Mexican population was removed from the country in a span of about 10 years. State, local, and federal governments broke all manner of existing laws, including international law and numerous Constitutional protections. This coerced removal left remaining Mexican immigrants with a strong distrust of the U.S. government, particularly law enforcement and immigration authorities. Reinforced by later actions, this distrust has persisted for decades (Johnson 2005).

The foreign-born population reached its lowest level in over a century in 1970. However, the landscape changed in 1965 when the Immigration and Nationality Act was passed, removing quotas and setting new criteria for migration. This led to increasing numbers of immigrants from the 1980s to 2000s, the majority of them from Asia and Central America. Mainly due to new quotas on Western Hemisphere migration, the number of undocumented immigrants increased as well. Such shifts were accompanied by legislative action. Congress passed immigration laws in 1986, 1990, and 1996. Among other provisions, these laws expanded resources for border patrol and broadened criteria for offenses leading to deportation (Young 2017).

As a tool of government, the Constitutionally-required Decennial Census, first conducted in 1790, has reflected the contemporary attitude towards immigration. In its earliest iterations, the Census focused on counting the number of free white males under age 16 and ages 16 and older, free white females, other free persons, and slaves. A citizenship question was first introduced in the 1820 census. Furthermore, a place-of-birth question started with the 1850 census, and year of entry was introduced in the 1890 census. Subsequent decennial censuses would ask about naturalization until it was omitted beginning in the 1960 census. However, the questions about citizenship and naturalization were included in the smaller but more detailed American Community Survey, which only goes to a sample of about 3.5 million households every year, starting in 2005 (Rogers 2018; Wolf and Cea 2019).

2.2 CPS background

As described in its official documentation,[1] "the Current Population Survey (CPS) is one of the oldest, largest, and most well-recognized surveys in the United States." Critical labor market statistics, including the unemployment rate, are derived from the CPS. While the CPS has undergone many changes, including the 1994 survey redesign, many aspects have remained stable, such as the monthly frequency, approximate sample size, and 8-month rotating sample ("Briefing Materials on the Redesigned Current Population Survey." Bureau of Labor Statistics Staff.). These factors provide intertemporal consistency and allow us to use the time series and panel data approaches we employ.

The CPS is not legally required. Those sampled may choose not to answer certain questions (item non-response), or they may refuse the survey in its entirety (unit non-response). Our focus is on unit non-response. However, all non-response is problematic. Non-response reduces sample size, requiring resources to be spent on surveying more people or costly follow-ups in order to achieve the necessary statistical power. Even more concerning is the possibility that those who do not respond are systematically different from responders. If responders are different from non-responders, and therefore the population as a whole, then the survey will not be representative of the population, and key statistics may be biased. If responders and non-responders differ in unexpected or difficult to measure ways, such bias may not be corrected even after the use of demographic-based weighting.

2.2.1 Critical terms & definitions

In the CPS, unit non-response occurs at the household level and is categorized into three types. A *Type-A* non-response[2] indicates a housing unit is occupied, but an interview was not completed. Type-B non-response indicates a housing unit was suitable for habitation, but temporarily unoccupied. Type-C non-responses occur when a housing unit is not fit for residential living, typically because it has been converted, destroyed, or moved. Each category contains several sub-categories. Of particular interest is Type-A Category-3: Refused. This corresponds to households whose residents explicitly refuse the survey. Because a refusal reflects a choice by households to self-select out of the sample, it is particularly worrisome to researchers.

2.2.2 1994 redesign overview

The redesigned CPS debuted in January 1994. This version largely persists as of 2022. The goals of the redesign were to use computerization to increase efficiency and accuracy, redefine variables for greater consistency, and collect new data. Two computerized interview modes were developed. In a Computer-Assisted Personal Interview,

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[1] The most recent official documentation is found in Technical Paper 77, while the most time-period accurate documentation is contained in Technical Paper 63RV.

[2] The official documentation uses both the terms non-interview and noninterview (e.g. Type-A non-interview), and makes a subtle distinction between a *non-interview* and *non-response*. For simplicity, we use the latter term to refer to both concepts.

an interviewer traveled to each housing unit and interviewed the residents using a laptop which automatically displayed questions and recorded input. Computer-Assisted Telephone Interviews were similar in nature (Cohany et al. 1994).

Computerization had three primary benefits. It improved consistency by standardizing how questions were asked while allowing for individualization to a person's unique circumstances. Computerization also reduced errors through built-in checks for internal consistency or unusual responses. Lastly, it reduced the response burden despite asking more questions by allowing individuals to briefly confirm previously stated information. This increased intertemporal consistency while allowing certain questions to be skipped if no change had occurred.

The questions themselves also changed. Questions were stated more precisely, such as by specifying the exact reference week for employment status, improving accuracy. Definitions for concepts themselves were modified, such by as adding new requirements for an individual to qualify as a "discouraged worker." Lastly, new questions were added, including those on multiple job-holding and citizenship status (Cohany et al. 1994).

2.2.3 Citizenship question format

The national origin and citizenship questions are asked in survey Part 2. Within Part 2., the Nativity and Immigration Section consists of the following questions:

- In what country (was/were) (name/you) born?
- In what country was (your/his/her) mother born?
- In what country was (your/his/her) father born?
- (Are / Is) (name/you) a CITIZEN of the United States?[3]
- (were/was) (name/you) born a citizen of the United States?
- Did (name/you) become a citizen of the United States through naturalization?
- When did (name/you) come to live in the United States?

It is important to note that if a person indicates they were born in the United States, the citizenship and naturalization questions are not asked for that particular person. The final citizenship variable (*PRCITSHP*) is a "recode" variable whose values are determined based off multiple questions, one of which is a direct question on citizenship. In the final data, it may take on one of the following five answers:

1. Native, born in the United States
2. Native, born in Puerto Rico or U.S. Outlying Area
3. Native, born abroad of American parent of parents
4. Foreign born, U.S. Citizen by naturalization
5. Foreign born, not a citizen of the United States

Further details regarding Citizenship Question are presented in Appendix 7.3. These include the assignment of interview/non-interview status and discussion of interviewing procedures, practices, and guidance.

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[3] This capitalization is in the questionnaire listing and in an image of the interviewing software in the interviews' manual.

2.3 Survey non-response

Despite the importance of understanding bias in the CPS and the increasing prominence of citizenship questions, we believe we are the first to analyze the CPS citizenship question's effect on response. However, many papers study other aspects of these issues.

2.3.1 Non-response, citizenship, & immigration

Brown et al. (2019) studied this topic using the 2010 Census, American Community Survey (ACS), and administrative record data. They compared ACS responses to administrative data, finding that survey responses corresponding to administrative non-citizens had a higher likelihoods of skipping the question and an inconsistency with administrative records. These patterns led researchers to conclude this was driven by data confidentiality concerns. The authors argued this may cast doubt on the reliability of survey-based citizenship statistics. Their other approach compared the 2010 ACS (which included a citizenship question) to the 2010 Census (which did not). They employed a differences-in-differences design, comparing the Census-ACS difference in unit self-response rates of households with at least one (administrative) non-citizen to households with only citizens. They found the drop-off in response was nearly 12 percentage points larger for the former group. After various adjustments, they estimated the census citizenship question would make households with at-least one non-citizen 8 percentage points less likely to self-respond. This would lead to greater costs and lower the quality of the population count.

Baum et al. (2019) commissioned a randomized survey trial, creating multiple surveys that closely mimicked the Census form but differed on whether they included the citizenship question. The inclusion of the citizenship question was associated with a large increase in item non-response. These effects were largest among Hispanic individuals from Mexico and Central America. In particular, the citizenship question caused an increase in item non-response on race and ethnicity questions. Such results were stronger yet for Hispanic people born in Mexico and Central America. Similarly, the treatment caused a reduction in the percentage of individuals in a household who were reported as Hispanic, and which was the most pronounced among households with Central American and Mexican backgrounds. The authors projected that the census citizenship question would undercount the Hispanic population by 12%.

The 2020 Census Barriers, Attitudes, and Motivators Study Survey Report studied perceptions of the Census based on survey of over 17,000 respondents. While the citizenship question was not directly addressed, many of its findings can inform our understanding of its effects. In the survey, 10% of respondents falsely answered that the Census is used to locate people living in the country without documentation. More than half of respondents indicated they were at least "somewhat concerned" Census data would not remain confidential and/or would be shared with other government agencies. Both concerns were more prevalent among Hispanic people: while 22% of respondents were "very" or "extremely" concerned that Census answers would be used against them, this rose to 32% for Hispanic individuals, 34% for people born out of the United States, and 39% for those not proficient in English.

The qualitative counterpart to the CBAMS Survey was the 2020 CBAMS Focus Group Final Report, which directly addressed the citizenship question. Awareness of the citizenship question was low in non-immigrant origin focus groups, but high in immigrant-groups, especially the Spanish (U.S. Mainland) group. Across groups, most thought the question's purpose was to facilitate deportations of unauthorized immigrants. Many considered the question to be politically motivated and invasive. Those concerns were heightened in the Spanish (U.S. Mainland) focus group. Across citizens and non-citizens, the question made many participants "reconsider participation," as they viewed it as biased and racially charged. Among recent immigrants not having visas or permanent residency, more than 50% indicated that they would not finish the census if a citizenship question was present. Many citizens in this group reported they would avoid participation due to this question to protect vulnerable members of their households and communities. The main cause of this aversion was potential deportation. Overall, the report concluded "the question may impede participation among audiences with recent immigration history."

A close cousin of the Census is the American Community Survey, which has consistently featured a citizenship question. A 2018 report documented that nonresponse rates for the ACS citizenship question were distinctly higher than for any other question in each year from 2010 to 2016. Moreover, this gap has tended to widen over time, particularly after methodological changes implemented in 2013. Additionally, among selected questions, the Hispanic origin question had the *second* highest item-nonresponse rate every year 2010-2016 inclusive (O'Hare 2018).

Other research has looked at non-response and refusal in other surveys. Similar trends have affected many nations, and the "decline in survey response can be observed in all wealthy countries." In the U.S., the decline is found across government, academic, business and media surveys, and in both cross-sectional and panel designs. In the cross-sectional General Social Survey, responses fell from over 80% in the 1990s to near 70% in the 2000s, mostly driven by refusals. The Survey of Consumer Attitudes features a rotating design similar to the CPS. From 1979 to 2003, response rates declined by 0.74 percentage points per year on average. In the Study of Income and Program Participation non-response and refusals increased by 163% and 143%, respectively, from 1990 to 2009. The Michigan Panel Study of Income Dynamics saw a cumulative response drop from 76% to 58% from 1968 to 1988, partly due to attrition. Of note is the difference in response rates between the "core" and "immigrant" segments. Across survey groups, time and households, immigrant segment non-response rates were markedly higher for many types of non-response (Tourangeau and Plewes 2013).

The topic of non-response and immigration is not unique to the United States. Studying a survey in Denmark, Deding et al. (2008) concluded immigrants were less likely to be successfully contacted by surveyors, and less likely to participate once contacted. Immigrants from Pakistan were "especially difficult to contact," and Turkish immigrants had particularly high refusal rates. The authors concluded that efforts to combat non-response bias must account for immigrant populations and address both the initial contact and participation persuasion phases. Conversely, Bachmeier et al. (2014) obtained a null result. Analyzing the Los Angeles Family and Neighborhood Survey and the Survey of Income and Public Participation, the authors found introducing citizenship and legal status questions provided data which appeared to be

high quality and was consistent with independent estimates. In the SIPP, adding the citizenship question had no "appreciable 'chilling effect' on survey participation of unauthorized immigrant respondents."

Further research has looked at these topics, but from different angles. Pedraza et al. (2017) studied the salience of immigration status on healthcare utilization. They determined that Latino *citizens* are less likely to use healthcare services when immigration status is mentioned. They also found citizens may perceive information shared with healthcare providers might not be secure, particularly if they know someone who has been deported. It is plausible that similar anxieties could extend to the CPS.

2.3.2 Quantification & effects of non-response

Another branch of the literature has focused on measuring non-response, understanding its effects, and mitigating them. From 1998 to early 2020 there was a marked increase in CPS non-response, rising from approximately 22% to 31% of households. Particularly striking was the increase in refusals. From 2010 to 2020, refusals increased from less than 4% to nearly 14% of households (Bernhardt et al. 2021).

Previous studies have looked at CPS non-response during the 1994 redesign. Krueger et al. (2017) studied the effect of non-response on rotation group bias in the Current Population Survey. They found rotation group bias increased discretely in January 1994. The authors attributed this to a concurrent discrete jump in non-response, particularly Type-A non-response.

Many studies have found non-response can cause bias in important survey statistics. Hokayem et al. (2015) found the official poverty rate, derived from the March supplement of the CPS, was underestimated by a percentage point due to the relationship between low socio-economic status and non-response. Heffetz and Reeves (2018) found non-response biases key statistics in several major surveys. They utilized a design based on the number of attempts required to contact a household, inferring that harder-to-reach responders were relatively more similar to non-responders. Harder to reach responders were systematically different in their unemployment and labor force participation rates in the CPS, as well as in important measures in the Behavioral Risk Factor Surveillance System and Consumer Expenditure Survey. This was true even after controlling for typical demographic factors used in corrective weights.

2.4 Possible non-response mechanisms

In line with the literature, we hypothesize that the citizenship question will be associated with greater CPS refusals. We propose several reasons why the citizenship question would cause avoidance of the survey. Unauthorized immigrants or those with undocumented household members may believe the question may be used to target them for deportation. We use citizenship as our variable of interest, as legal status is not asked in the CPS. We speculate that any relationship between non-citizens and refusals is largely driven by that between unauthorized immigrants and refusals, and therefore that the latter relationship should be stronger.

Those who are not undocumented and do not have undocumented household members may still be made uncomfortable by the question. Immigrants with legal status (or their household members) may be concerned about immigration related repercussions if laws are changed or disregarded. Moreover, individuals who have undocumented friends or extended family members, who feel a connection to a community which has many unauthorized immigrants, or who believe the question is invasive or politically motivated may also refuse the CPS. Furthermore, there may be an issue of ambiguity[4] about the citizenship question in the CPS as well as in the ACS that could lead to a low response rate especially with respect to 'derivative citizens' (i.e. unmarried children under 18 who become citizens due to the naturalization of their parents though they do not go through it themselves). Via the last two mechanisms, even some *citizens* may refuse the survey due to the question. Insofar as we determine the effects of the question by comparing states with many non-citizens to states with few non-citizens, our results will be underestimated if this is true.

The citizenship question is asked at the end of the interview, after labor force questions have been answered. Therefore, refusing once the citizenship question has been asked will not result in a unit refusal. However, there are several scenarios in which the citizenship question *can* cause refusals. While *some* individuals have pre-existing familiarity with the CPS and its citizenship question, this likely applies to a very small portion of the population. More plausibly, prospective survey takers find out during the introduction that there are immigration-related questions. Most importantly, the CPS is a *repeated* cross section. Thus, an individual who decides not to participate after being asked the citizenship question in the first month will not be counted as a refusal for that month, but may be counted as a refusal during the next seven months in sample.

When faced with the citizenship question at the end of the initial interview, respondents may choose to not answer, answer incorrectly, and/or halt the interview. Going forward, respondents may choose to answer the question in subsequent interviews, refuse to answer the question in future interviews, or refuse to participate any longer going forward. As there will be an additional seven surveys after the first, it is possible that a person could do some combination of these. Our designs are intended to capture the margin where people refuse the survey entirely. However, this need not necessarily capture the entire scope of the issue, as it may be possible the citizenship question could induce non-participation in subsets of the survey.

3 Data, variables & methodology

In this section, we first introduce our data and discuss the methods used to prepare it for analysis. We then review the summary statistics and time series properties of the aggregated data. Lastly, we present our methodology. The models we employ include a regression discontinuity, direct panel regression, and difference in differences.

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[4] See Jasso and Rosenzweig (2020, pp. 646-647) for a clear explanation of why there could be the potential that the current citizenship question may have an adverse effect on response from derivative citizens. To make this point clear, we included the citizenship question/instructions to fill in Section 2.2.3 for January 1994 CPS.

3.1 Original data & preparation procedure

Our raw data are CPS monthly microdata from January 1989 to December 2002, inclusive. This includes information on 25,051,781 individual responses and household non-responses. The data used for analysis are aggregated data derived from the microdata. We collapse at both the state and national level, creating separate datasets. Aggregated variables are typically generated by assigning indicators to certain statuses observable in the microdata, and averaging over applicable households or individuals in a state (or the country). In every month, each state's data include numerous surveyed individuals, many of whom will respond to a question. Thus, by looking at the state (and national) level data, we create a balanced panel across all states and months, even when individuals may not respond to the survey or certain questions.

We provide detailed summary statistics for the state-level data in the Appendix 7.4. Because the national level data are derived from the same source, they have similar characteristics. The sample size for most state-level variables is 8568. This is due to the cross sectional size of 51 (50 states & Washington D.C) and the number of time periods being 168. Accordingly, the national data typically have a sample size of 168. Because some variables are not present for all survey months, they have fewer observations. This includes the non-citizenship percentage variable, which is present for all 5508 observations that occur after the citizenship question was introduced.

To fully consider the robustness of our results, we apply the demographic-based weights to the national-level data and re-estimated the event-study regression discontinuity model. Different weights are applied corresponding to if variables are at the individual or household level.[5]

3.2 Estimation strategy

Our investigative approach proceeds as follows. First, we employ an event-study regression discontinuity to assess if the 1994 redesign overall had a causal effect on refusals. We then draw a preliminary connection between the citizenship question and refusals via a multivariate panel regression featuring refusal rate and non-citizen population percentage. To uncover the relationship between those factors, we use a difference-in-differences design. These approaches work by relating the causal effect of the redesign to populations most affected by the citizenship question, implying the question is the underlying reason for such increases.

3.2.1 Event-study regression discontinuity

Our first model is an event-study regression discontinuity. The identifying assumptions of this model are (I) observations are equivalent before and after the threshold, except for the effects of the event occurring at the threshold, (II) nothing besides

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[5] Certain variables are allowed to remain unweighted. This is either because (a) they correspond to an variable directly pertaining to non-response (i.e. refusals) that the weights are meant to correct for but not adjust, or (b) there is a discontinuous shift in 1990 and/or 2000 unrelated to the CPS itself resulting from the weights being updated after new census results were applied.

the treatment coinciding with the threshold changes, and (III) the trend is correctly modelled before and after the threshold. The first assumption requires the CPS and those interviewed to be highly similar in December 1993 and January 1994 in ways that would relate to immigration. Since changes in demographics and response are slow-moving on a month-to-month basis, we believe this is satisfied. With regard to the second assumption, the CPS underwent a major redesign, and many changes could have influenced the rate of refusals. However, by considering the entire redesign, not just the citizenship question, to be the treatment, we argue this is satisfied. Moreover, we are aware of no other salient factor that may have also changed in January 1994. Lastly, we must assume the trend is modelled correctly before and after the threshold. We argue empirically in Section 4 this is satisfied. Since all assumptions are met, our event-study regression discontinuity design is valid, with the results applying to the cumulative survey redesign.

To carry out the event-study regression discontinuity, we estimate regression equations of the following form:

$$REF_t = \beta_0 + \beta_1 SurveyMonth_t + \beta_2 Post_t + \boldsymbol{\beta} X + \lambda_t + \epsilon_t \tag{1a}$$

$$REF_{it} = \beta_0 + \beta_1 SurveyMonth_t + \beta_2 Post_t + \boldsymbol{\beta} X + \lambda_t + \lambda_i + \epsilon_{it} \tag{1b}$$

The first equation applies to the time-series national-level data, and the next equation applies to the state-level panel data. The variables (listed as presented for the state-level data) are defined as follows: REF_{it} is the refusal rate in state i and month t. $SurveyMonth_{it}$ is a linear time trend, $Post_t$ is an indicator for months occurring during and after 1994, and X is a vector of controls including demographic and economic variables. We also may include fixed effects across the temporal (month or year) and spatial (state or division) dimensions, represented by λ_t or λ_i. Based on Krueger et al. (2017) and our general hypothesis, our prior is that the time trend will have a positive coefficient. Furthermore, due to the addition of the citizenship question, we expect β_2 to be positive.

To test the robustness of these results, we estimate several alternate specifications. These include using Type-A Non-Response as the dependent variable (Appendix 7.5), multiple threshold bandwidths (7.6.1), and polynomial discontinuity specifications (7.6.2), which are based on the national data. We also present a model documenting a discontinuity in the Hispanic population reported (Appendix 7.6.3), based on state level data.[6]

3.2.2 Direct panel regressions

To begin relating our findings to the citizenship question specifically, we employ multivariate panel regressions as follows:

$$REF_{it} = \beta_0 + \beta_1 SurveyMonth_t + \beta_2 NonCitizen_{it} + \boldsymbol{\beta} X + \lambda_t + \lambda_i + \epsilon_{it} \tag{2}$$

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[6] Alternative versions based on the other dataset (either national or state-level) may be made available upon request.

The variable $NonCitizen_{it}$ indicates the percentage of sampled individuals in each state each month who reported not being a United States citizen. We expect it to be positively related with the refusal rates, as states with more non-citizens have larger populations that are disproportionately likely to refuse the survey. While not a research design, this model would indicate that controlling for other factors, states with higher levels of non-citizens should have higher rates of refusal. This would in turn be consistent with the citizenship question causing non-response among non-citizen and Hispanic populations.

One concern is the interplay between variables due to the non-observation of refusers' characteristics. We cannot observe the citizenship status of those who refuse, and so cannot identify non-citizens who refuse as non-citizens. If our prior is true, then states with high refusal rates due to many non-citizens will have their non-citizen populations disproportionately and artificially reduced in the observed data. This may in turn affect our estimated coefficients.

In response to this concern, we make three observations. First, such bias-inducing interplay could only exist if there *is* a relationship between the dependent and independent variables. Thus, there seems to be little chance this could affect the probability of a Type-I error. Accordingly, a qualitative conclusion of effect or non-effect would remain valid. Secondly, the effects would be small, as both non-citizens and refusers each represent less than 10% of the population during the relevant time period. Thus, any distortion of aggregate statistics would be on the order of tenths of a percentage point.[7]

To more directly investigate this possible source of distortion, we conducted simulations featuring randomly generated data based on our priors. Using the simulated data, we compare regressions that are analogous to those we can run with real-world data to those with the simulated refusers included in the sample. After 2,000 iterations, the average coefficients for the regressions with and without the unobservable data differed by less than 1 percentage point. Both estimates were within a percentage point of the true difference in refusal rate between citizens and non-citizens built into the simulation. This was true for simulations with both exaggerated and more plausible assumptions for refusal rates, non-citizen populations, and their relationship. A simulation with a built-in lack of relationship showed the average regression closely approximated the correct null result, implying false positives would not be artificially likely. These findings indicate that such interplay does not meaningfully affect our results.[8]

3.2.3 Differences-in-differences

Our targeted research design model is the difference-in-differences. Our design is not a classic "diff-in-diff," as there is no traditional control group: the treatment (the citizenship question) was implemented across all states simultaneously, along with

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[7] Consider a scenario with 1000 surveyed individuals, 5% of whom are non-citizens. If 96% of citizens and 92% of non-citizens respond, we would observe 912 citizens and 46 non-citizens, yielding an observed non-citizen percentage of 4.8%.

[8] Results of these simulations are available upon request.

the redesign as a whole. Instead, treatment and control groups are differentiated by the degree to which they are affected by the question, or exposure to the treatment. If the citizenship question has an effect on refusals, then we expect it act through the mechanism found in the literature, whereby individuals fear that divulging citizenship and immigration information may lead to legal or immigration-related repercussions. This concern applies predominantly to immigrants and especially non-citizens, and those who have family or other close ties to non-citizens.

The control group is therefore states with small Hispanic or non-citizen populations. These states are relatively less affected for two reasons. First, because those born in the United States are not asked the citizenship question, control states will likely have fewer people asked the question at any point. Second, because such states have fewer non-citizens or Hispanic people, they contain fewer people for whom the question, if and when it is asked, would likely evoke anxiety and concern. The treatment group is states with large Hispanic and non-citizen populations, which would experience the greatest effect of the citizenship question, as they will tend to have more individuals asked the question, and more individuals concerned about the question. We then estimate a straightforward difference-in-differences model as follows:

$$REF_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post \times Treat_{it} + \boldsymbol{\beta X} + \lambda_t + \lambda_i + \epsilon_{it} \quad (3)$$

To determine the control and treatment groups, we rank states by *five* characteristics that likely indicate high sensitivity to the citizenship question. The two most important of the five are December 1993 Hispanic population,[9] and January 1994 non-citizen population.[10] We then assign the 20, 15, or 10 states with the smallest population percentages to the control group, and an equal number of the states with the largest percentages to the treatment group. Additionally, we estimate a multiple treatment-group specification, where only states that are in the treatment group for at least three out of the five categories are considered to be "treated." Likewise, only states in control groups for at least 3 categories are assigned to the control group.[11][12] This is done to minimize noise, as each characteristic on its own imperfectly measures the true sensitivity to the citizenship question.

For this model, we have the following priors. Due to the large effect-size of the jump in 1994 found in the literature, we expect that both groups will experience some increase at that time, and so β_1 will be positive. We hypothesize that states with greater vulnerable populations are more likely to avoid government surveys in general, implying that β_2 may be positive as well. Our strongest and most important prior is that

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[9] When possible, we use data prior to the redesign to minimize the chance of non-response influencing treatment group assignment. This is not possible for non-citizen population, which was unobservable before the question was added.

[10] The other three are Born in the United States population (1994), born in Mexico population (1994), and citizenship question revisions (1994).

[11] For each of the 5 categories, the same amount of states (20, 15, 10) are used to assigned each treatments/control status. However, it need not be the case that 10 states are in the top (bottom) 10 across all characteristics.

[12] With 5 characteristics, it is impossible to be in 3 or more treatment groups and also be in 3+ control groups.

after the introduction of the citizenship question, treated states (with larger populations exposed to the impact of the question) will experience a differentially large increase in refusals, making β_3 positive.

For maximal robustness, we estimate a conceptually similar model in Appendix 7.9. This model differs in two primary ways: (1) it includes all states, not specific subsets, and (2) it uses a continuous interaction term (interacting *post* and December 1993 Hispanic Population or January 1994 non-citizen population) and not a binary control/treatment framework to asset sensitivity to the citizenship question.

4 Empirical results

The event-study regression discontinuity results, show a large and persistent jump in refusals in January 1994, causally attributable to the redesign as a whole. Direct panel regression results show that states with higher non-citizen percentages have higher refusals rates. Finally, the difference-in-differences research designs show that states with larger Hispanic and non-citizen populations, and therefore more exposure to the effects of the citizenship question, had larger increases at the time of the redesign.

4.1 Event-study discontinuity models

4.1.1 National-level data results

The event-study regression discontinuity results feature a large instantaneous increase in the national refusal rate at the point of the redesign in 1994. Not only is the increase abrupt, but it is sustained across the sample period. Figure 1 shows the evolution of the national refusal rate and Type-A non-response rate[13] over time, as well as the predicted values obtained from regressing each dependent variable with a linear time trend and a threshold (*Post*) indicator. The result is striking: precisely at January 1994, there are large increases in both time series. While both series display seasonality, their mean values after January 1994 are distinctly higher than before the redesign.

To formalize this, we estimated regression models based off of Eq. (1a) from Section 3. Testing for statistical issues revealed all models in Table 1 (below) experienced serial correlation. To correct for these issues, we executed all six regressions using robust standard errors. Regression results reveal the jump at 1994 was highly statistically significant. Estimates were robust in magnitude and significance to the inclusion of month-fixed effects and a vector of controls.[14] Additional robustness exercises presented in the Appendix show that the discontinuity is present across a variety of time bandwidths (7.6.1), and when using differential polynomial time controls (7.6.2). Results are also significant when not using weighted variables, and when using Type-A Non-Response instead of refusals as the dependent variable (Appendix 7.5).

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[13] Results for Type-A non-response are included in the Appendix.

[14] Controls include average household size & income, educational attainment, labor force status, and race/ethnicity variables.
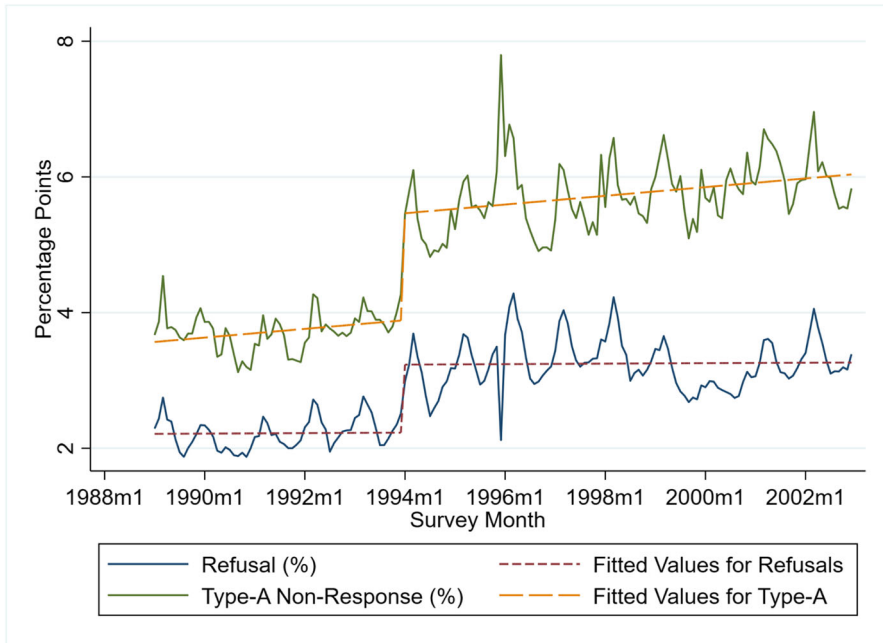
Fig. 1 Type-A Non-Response & Refusals with Fitted Values (National Data)

Table 1 Event-Study regression discontinuity results (weighted national data)

	Refusal (%) (1)	Refusal (%) (2)	Refusal (%) (3)
Survey Month	0.00029	0.0010	0.0297***
	(0.00099)	(0.00071)	(0.0063)
Post	1.004***	0.942***	0.668***
	(0.104)	(0.073)	(0.070)
Constant	2.111***	1.898***	263.5***
	(0.375)	(0.268)	(58.82)
Observations	168	168	168
R-squared	0.685	0.835	0.928
Month FE	No	Yes	Yes
Weighted Demographic Controls	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in parentheses
Coefficient estimates in percentage points

The third regression discontinuity assumption requires the trend to be modelled correctly before and after the threshold. Due to the high goodness of fit ($R^2 > 0.90$), we argue that this assumption is satisfied. This is affirmed visually by Fig. 2, which shows actual and fitted values from Table 1 Model (3). Fitted values closely track the actual values before and after the threshold. At the threshold, the fitted values jump upwards, but do not deviate from the actual data themselves, except for one month during the 1995 government shutdown. As the trend is modelled correctly during the sample period, any jump is attributable to the intercept and not to a mis-specified model.

4.1.2 State-level data results

We then estimated the Eq. (1b) regressions using the state-level specification. The results showed a statistically significant increase in refusals at the threshold that was robust to the addition of a variety of controls. Table 2 features corrected regression results for the state-level event-study regression discontinuity models with refusals as the dependent variable. The discontinuous jump in refusal rates was estimated to be between 86 and 92 basis points. Intercept estimates were stable as controls were added, and remained highly significant. Conversely, the coefficient on the linear time trend is changed in the specification including controls. We attribute this to some of the additional controls that increase over time (e.g. family income) absorbing the previous effect of the time trend. The same models were also estimated using
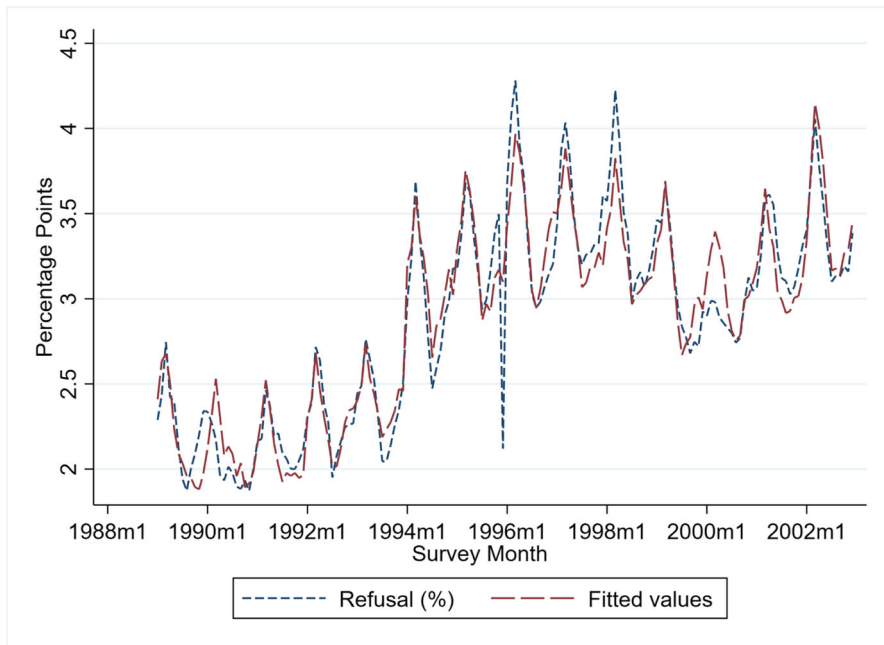

Fig. 2 Refusals with Model (3) Fitted Values (National Data)

Table 2 Event-study regression discontinuity results (state data)

	Refusal (%) (1)	Refusal (%) (2)	Refusal (%) (3)	Refusal (%) (4)
Post	0.920***	0.861***	0.861***	0.870***
	(0.096)	(0.095)	(0.095)	(0.109)
Survey Month	0.00096	0.00167	0.00167	-0.00658***
	(0.00127)	(0.00127)	(0.00127)	(0.00224)
Constant	1.718***	1.371***	1.345**	-6.056
	(0.500)	(0.504)	(0.586)	(5.318)
Observations	8,568	8,568	8,568	8,568
R-squared	0.161	0.195	0.330	0.516
Month FE	No	Yes	Yes	Yes
Division FE	No	No	Yes	Yes
Demographic Controls	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1

Robust standard errors in parentheses

Coefficient estimates in percentage points. State-clustered standard errors

Type-A non-response as the dependent variable, resulting in large and statistically significant threshold coefficient estimates. These findings affirm the redesign caused a large increase in both refusals and Type-A non-response more generally.

4.2 Direct panel regressions

Using the national-level data, we estimate regressions based on Eq. (2), featuring state survey refusals and Type-A Non-Response as the dependent variables and controls including year, month, and Census Division fixed effects, as well as state demographic and economic controls.[15] Due to collinearity with year fixed effects, the linear time trend is only included in specifications excluding them. The variable for Non-Citizen population percentage is only available after 1994, so the regression sample period is 1994-2002, inclusive.

Across all specifications, greater portions of non-citizens in a state are consistently associated with higher refusal rates. Table 3 provides the regression results. The corrected results are highly significant. Coefficient point estimates ranged from 5 to 13 basis points. Focusing on the specification with the most controls, we estimate a one percentage point change in a state's non-citizen population is associated with a 5-6 basis point change in refusals, holding all else constant. Like the event-study regression discontinuity models, regressions using Type-A non-response yield similar results, with somewhat larger and similarly significant coefficients. As a robustness check, we performed regressions (included in Appendix 7.7) using a state's population

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[15] The direct panel regression controls include slightly different education variables than previous regressions.

Table 3 Direct panel regression results

	Refusal (%) (1)	Refusal (%) (2)
Non-Citizen Pop. (%)	0.131***	0.055***
	(0.026)	(0.020)
Constant	1.81***	-10.94
	(0.27)	(6.73)
Observations	5,508	5,508
R-squared	0.360	0.452
Month FE	Yes	Yes
Year FE	Yes	Yes
Division FE	Yes	Yes
Demographic Controls	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in parentheses
State-clustered standard errors

born in the United States instead of non-citizen population. Results are statistically significant in the expected direction, and consistent with our other findings.

4.3 Difference-in-differences models

We estimated nine difference-in-differences regressions based on Eq. (3). These all included demographic/economic controls and were corrected as before. They varied both in the size of the treatment and control groups, and also the characteristic used to rank the states for group assignment. Specifically, we used December 1993 Hispanic population (Table 4), January 1994 non-citizen population (Table 5), and multiple treatment groups (Table 6). For robustness, we estimated a model where the treatment and control groups were assigned by the percentage of a state's population born in the United States. Results are included in Appendix 7.8.2. A critical assumption of the difference-in-differences design is the existence of common trends. We provide strong graphical evidence showing common trends between treatment and control groups before the redesign in Appendix 7.8.1.

The regressions grouped by Hispanic population showed statistical significance in the most critical coefficients. The estimate for the treatment group was typically positive and significant (most importantly in the specification with the smallest groups and therefore largest differences between them), indicating treatment states had higher refusal rates prior to the redesign. Most importantly, the coefficient for the interaction of the treatment group and post threshold indicators was positive in all models and highly significant in those with the largest differences between treatment and control groups, as is logical as the groups become more distinct.

Difference-in-differences regressions using state non-citizen population had even more striking results. Before the redesign, treatment states had refusal rates 38-123 basis points higher than the control group. Moreover, the states with the largest non-citizen

Table 4 Difference-in-differences results assigned by Hispanic population

	Refusal (%) (1)	Refusal (%) (2)	Refusal (%) (3)
Treatment ('93 Hispanic Pop.)	0.0297	-0.212***	0.684***
	(0.0368)	(0.0484)	(0.0972)
Post	-0.447***	-0.845***	0.691***
	(0.0654)	(0.0935)	(0.124)
Post × Treat.	0.0119	0.101**	0.259***
	(0.0381)	(0.0460)	(0.0544)
Constant	-8.879***	-5.503***	-5.856**
	(1.608)	(1.936)	(2.406)
Observations	6,720	5,040	3,360
R-squared	0.574	0.595	0.681
Month/Year/Division FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in parentheses
Coefficient estimates in percentage points

Table 5 Difference-in-differences results assigned by non-citizen population

	Refusal (%) (1)	Refusal (%) (2)	Refusal (%) (3)
Treatment ('94 Non-Citizen Pop.)	0.527***	0.386***	1.228***
	(0.039)	(0.051)	(0.077)
Post	-0.086	-0.176**	1.293***
	(0.069)	(0.079)	(0.138)
Post × Treat.	0.201***	0.278***	0.122**
	(0.040)	(0.047)	(0.061)
Constant	-6.429***	-8.831***	10.86***
	(1.675)	(1.931)	(2.342)
Observations	6,720	5,040	3,360
R-squared	0.555	0.591	0.678
Month/Year/Division FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in parentheses
Coefficient estimates in percentage points

Table 6 Difference-in-differences results assigned by multiple treatment groups

	Refusal (%) (1)	Refusal (%) (2)	Refusal (%) (3)
Treatment (3+ Treat Groups)	0.428***	0.174***	0.965***
	(0.040)	(0.049)	(0.087)
Post	-0.0095	0.0941	1.404***
	(0.0671)	(0.110)	(0.128)
Post × Treat.	0.148***	0.233***	0.331***
	(0.039)	(0.045)	(0.054)
Constant	-4.119**	-6.749***	-4.403*
	(1.617)	(1.870)	(2.267)
Observations	7,056	5,208	3,024
R-squared	0.562	0.586	0.711
Month/Year/Division FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes

*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Robust standard errors in parentheses
Coefficient estimates in percentage points

populations experienced an extra increase of about 12-28 basis points on top of the increase experienced by control states. We further interpret these results in Section 5.

The multiple-treatment groups specification showed similar results as previous models. Being in the overall treatment group (being in three of more treatment groups based on the five categories) was associated with substantially higher baseline refusal rates than control group states. The post variable alone was inconsistently significant, indicating that control states alone may have experienced little discontinuity at the time of the redesign. Conversely, the interaction term is consistently significant and positive. It begins at 15 basis points, and as the treatment/control groups become smaller and therefore farther apart on average, grows to 33 basis points. Thus, it seems the effect of the redesign on refusals was concentrated in the states that are most sensitive to immigration-related questions.

Conceptually similar robustness exercises with a continuous interaction term are presented in Appendix 7.9. Conditioning on numerous controls, the interaction terms were positive and usually significant, indicating that states with higher Hispanic and non-citizen populations experienced differentially higher refusal rates.

5 Summary of key findings and policy implications

Applying event-study regression discontinuity point estimates (around 50-100 percentage points) at the threshold to the observed Dec. 1993 refusal rate of 2.51% posits a 20-40% increase in refusals is attributable to the redesign term. Per the event-study design, this is the *causal* effect of the entire redesign on refusals.

Coefficient estimates from the direct panel regressions estimate that each successive percentage point of non-citizen population causes a 5-6 basis point increase in refusals. In January 1994, the average state non-citizen percentage was 3.23%. Applying this to the direct panel results implies the question caused a 16-20 basis point increase. Using the December 1993 national refusal rate of 2.51%, we project the citizenship question caused refusals to rise by 6-8%. Event-study regression discontinuity models projected the redesign overall caused a 50-100 basis point increase. If we take the increase to be 75 basis points, we can estimate that 21-27% of the total increase is attributable to the citizenship question.

By 2020, the refusal rate had increased five-fold since 1993, and routinely exceeded 14%. We note that the size of the foreign-born population (which includes both non-citizens and naturalized citizens) has continued to grow in both absolute size and as a percentage of the American population (Lange and Yeganeh 2018). The Hispanic population has also grown at a high rate. These factors suggest that the total effect of the citizenship question may have grown substantially since 1994. Assuming the relative percentage of refusals caused by the citizenship question is the same, then if we apply the finding that 6-8% of refusals are driven by the citizenship question and apply it to a 13% refusal rate, we find 0.7-1.1% of *all* households may be refusing due to the citizenship question specifically. This is large, but similar in magnitude to the 2% reduction in response projected by Brown et al. for the effect of the question on the Census. Because the effects are primarily in states with larger non-citizen and Hispanic populations, we hypothesize that this rate would be several times higher for those groups.

While the potential for the question's harm has never been greater, the policy implications are not straightforward. The citizenship question can have legitimate uses in measuring the unique labor force, educational, and other characteristics of non-citizens. To identify this information, the Bureau of Labor Statistics must be able to identify non-citizens. However, the BLS also has a goal of measuring the entire United States labor market, including non-citizens and Hispanic individuals. While adding the citizenship question allows researchers to better identify information from non-citizens, they receive less overall information about them. Thus, as non-citizens and Hispanic people comprise a vital part of the labor force, the addition of the question means that labor force statistics are vulnerable to bias.

The optimal policy therefore depends on the size of the effect on refusals, and the priorities of the BLS. If the objective is to have the most representative and inclusive survey with the most accurate statistics, then it seems best to eliminate the citizenship question. However, if information obtained from the CPS about non-citizens' characteristics is valuable and inaccessible elsewhere, then the question may have a redeeming feature. While this research shows the question has caused an increase in refusals, it does not prove that labor market statistics are distorted. While non-citizens and Hispanic individuals may have different labor market characteristics, giving the potential for bias, we have not attempted to determine if statistics are *actually* biased due to refusals. Because the CPS is weighted to correct for non-response, it is possible any bias is mitigated.

Given these findings, we advise the Census Bureau to take seriously the adverse effects on response and inclusiveness associated with the citizenship question. We

recommend a randomized controlled trial be conducted to determine the exact causal effect of the question, evaluate the bias caused, and make an informed analysis to determine if the citizenship question warrants continuation.

6 Conclusion

Survey non-response and refusal has been a growing problem in social science research. Increases in non-responses have plagued surveys with different designs, topics, and structures from all kinds of institutions and across countries. Such problems can make samples non-representative, potentially casting doubt on the reliability of survey-derived statistics. Similarly, immigration is a politically salient issue, and one which has effects on survey participation in the United States and in other nations. In the U.S., the proposed 2020 Census citizenship question was a flash-point that directed attention to the role of such questions and the effects on participation. We study if the citizenship question had a detectable effect on refusals in the Current Population Survey.

Our empirical results are consistent with increased survey refusals arising from the citizenship question. Event-study regression discontinuity designs provided strong evidence that the 1994 CPS redesign as a whole caused a large, sustained, and significant 50-100 basis point (20-40%) increase in refusals. We then examined the relationship between state non-citizenship percent and refusal rate. Across specifications, states with greater non-citizen percentages had significantly higher refusal rates.

To more precisely evaluate the relationship between the *citizenship question* and refusals, we a difference-in-difference research design. Broadly, these results show that when the citizenship question was added in 1994, states with larger populations exposed to the citizenship question experienced larger increases in refusals precisely at the redesign. Difference-in-differences results demonstrate that treatment states more exposed to the effects of the question experienced an additional increase at the threshold. To adjust for non-response bias, the BLS uses weighting to correct for measurable discrepancies between the survey sample and established national demographics. These weights are used to determine the final published labor force statistics. In light of the citizenship question being connected to increases in non-response, it would be important for future research to determine if they are successfully correcting for this problem. We also recommend there be a randomized controlled trial in the CPS to precisely measure the deleterious effects of the citizenship question on response and determine if the question should be removed.

drafts. An earlier version of this paper was presented at the June 2020 virtual sessions of the Western Economic Association Annual Conference, and benefited immensely by the discussant comments of Nicole Simpson of Colgate University. A previous version of this paper was circulated as IZA Discussion Paper #13350 (June 2020). The usual caveats apply.

Declarations

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