The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP*

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Abstract

We study how eligibility expansions in the U.S. Supplemental Nutrition Assistance Program affect take-up among previously eligible households and social welfare. Using an event study with administrative take-up data, we document a "woodwork effect": Expanding eligibility raises take-up among previously eligible households. An online experiment and administrative survey provide evidence that information frictions, rather than stigma, drive SNAP's woodwork effect. To interpret our findings, we develop a model of welfare programs with incomplete take-up and woodwork effects. Given our empirical results and certain modeling assumptions, expanding SNAP eligibility would raise social welfare.

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1 Introduction

Social welfare programs use eligibility rules to target assistance efficiently and to redistribute. However, eligibility rules may have the unintended consequence of reducing take-up rates even among eligible households. Indeed, there is a positive across-program correlation between U.S. welfare programs' income eligibility thresholds and take-up rates among the eligible population (Figure 1). Furthermore, a literature on health insurance documents "woodwork effects" where already-eligible households become more likely to enroll after eligibility expansions in public insurance programs (Frean et al., 2017; Sacarny et al., 2022).

If present, woodwork effects influence optimal program design. Without woodwork effects, the social planner trades off the program's social cost against the social benefit of providing the program to households at the margin of eligibility. With woodwork effects, looser eligibility rules raise take-up among the poorest households who are far from the margin, introducing a new trade-off between targeting and redistribution.

Do welfare program eligibility thresholds affect take-up among already-eligible households? How do such woodwork effects influence welfare analysis of eligibility thresholds? In this paper, we show that the income eligibility threshold affects take-up among households who are always eligible for SNAP. We explore the mechanisms underlying this take-up response using an online experiment and analysis of a government-commissioned survey on SNAP take-up. To interpret our findings, we propose a general model of welfare program participation with incomplete take-up. The model makes precise how mechanisms behind woodwork effects — namely, stigma and incomplete information — affect social welfare. It delivers formulas for the welfare impact of a marginal change to the eligibility threshold, which we calibrate with our empirical findings.

We focus on SNAP for several reasons. SNAP has an annual budget of \$70 billion and forms an important part of the U.S. safety net. Furthermore, SNAP eligibility rules are at the center of an active public discussion.[1] Finally, SNAP publishes anonymized public-use administrative data (the Department of Agriculture's Quality Control files), which we use for our main analysis.

We begin by providing evidence that eligibility expansions in SNAP raised take-up among the lowest-income individuals who are always SNAP-eligible. States can choose to expand SNAP's gross-income eligibility threshold beyond the federal minimum of 130% of the Federal Poverty Level (FPL). We focus on individuals at 50–115% of the FPL, a group eligible for SNAP in every state because they are poorer than the federal minimum eligibility. We use an event-study design with variation across states and years. Raising the eligibility threshold by 10 percentage points (pp) of the FPL (e.g., from 130% to 140%) boosts take-up by over 1 percent among the inframarginal group that was always eligible for SNAP. For every person who

[1]The Trump administration proposed eliminating state discretion in eligibility thresholds (Federal Register, 2019). During the coronavirus pandemic, the government relaxed some SNAP eligibility rules. As of Spring 2024, eight states passed universal school meals, a related food assistance program, in perpetuity (see https://frac.org/healthy-school-meals-for-all, accessed June 16, 2024).

joins SNAP because she becomes newly eligible, 0.99 already-eligible people join the program.[2]

We perform a clean placebo test. The policy change that permits states to change their SNAP eligibility threshold also gives other bureaucratic benefits to states without changing the threshold. States which adopt the policy without expanding SNAP eligibility see no increases in SNAP take-up.

We turn to uncovering the mechanisms underlying woodwork effects. One hypothesis, motivated by models of social signaling (e.g., Bursztyn and Jensen, 2017), is that raising the income threshold could reduce SNAP stigma. With less stringent eligibility rules, taking up SNAP no longer conveys as much information about one's type. To test this hypothesis, we conduct an online experiment with a nationally representative sample of more than 2,000 participants. We provide truthful information about the eligibility threshold in *one* state to shock beliefs about the mean eligibility threshold *across* states. The experimental variation increases participants' beliefs about the share of individuals who are eligible for SNAP in the entire U.S. by 9 percentage points on average (standard error: 0.8 pp) and moderately decreases an index of social stigma by -0.050 standard deviations (SE: 0.027, $p = 0.061$). Effects on stigma are larger among people who are likely SNAP-eligible. However, we find null results in a related experimental treatment and for other measures of stigma, so we interpret the experiment as providing mixed evidence overall.

A second hypothesis is that relaxing the eligibility restrictions increases information about SNAP. To test this hypothesis, we analyze microdata from the Food Stamp Program Access Study (FSPAS), a nationally representative survey on SNAP awareness and stigma among both SNAP enrollees and eligible non-enrollees conducted by the USDA's Economic Research Service (Bartlett et al., 2004). To our knowledge, this study is the first academic analysis of this rich dataset on SNAP take-up mechanisms. Using the FSPAS, we identify demographic groups that are likely subject to SNAP awareness and stigma. We find that the demographic groups with the largest woodwork effects are those with low levels of SNAP awareness and not the groups whose stigma is most sensitive to eligibility thresholds from the experiment. Thus, while the experiment suggests that the eligibility threshold perhaps has moderate effects on stigma, information appears to play a larger role in the decisions of people who newly take up.

To study the welfare implications of woodwork effects, we modify the framework of Finkelstein and Notowidigdo (2019). Households who are eligible for a welfare program enroll when the program benefit exceeds a private take-up cost (e.g., stigma) and they are informed about the program.[3] Both the cost and information may depend on the eligibility threshold. The model delivers formulas for the welfare impact of marginally raising the eligibility threshold with and without woodwork effects.

These formulas have intuitive interpretations. Without woodwork effects, a marginal increase in the eligibility threshold gives welfare gains to the newly eligible who enroll. Meanwhile, woodwork effects'

[2] We discuss this estimate in Section 2. Its magnitude is large because relatively few households at the margin of eligibility newly enroll when the means test is relaxed. That may be because other eligibility restrictions bind for this population. Also, benefit sizes are generally low for the newly eligible population.

[3] While we focus on stigma costs, our framework permits any cost that depends on the eligibility threshold. Another possible cost embedded within our framework is uncertainty about eligibility, as in Kleven and Kopczuk (2011).

social benefits depend on the mechanism. Suppose households newly enroll because of changing costs. Then new enrollees are just indifferent between taking up and not, so they do not value the eligibility expansion to first order — a standard envelope condition. However, those who would have enrolled anyway now pay lower stigma costs to take up. On the other hand, suppose that woodwork effects reflect improved awareness of the program. Then, the new take-up now confers first-order utility gains, as people who lacked awareness were not previously optimizing. In addition to the social benefits, the reform entails a social cost of provision plus possible fiscal externalities (e.g., labor supply responses), for both newly and already-eligible enrollees.

We conduct an illustrative calibration of these formulas by imposing assumptions on utility, ordeal costs, and stigma. We calibrate the welfare formula using the empirical magnitude of the woodwork effect in SNAP, together with our mechanisms analysis and several additional parameters. New take-up from woodwork effects enters our welfare formulas as both a social cost and a social benefit. An important takeaway for policy is that the presence or magnitude of woodwork effects is generally insufficient to perform a welfare analysis of eligibility expansions. To make progress, we use the FSPAS result that woodwork effects mostly come from misinformed households, rather than stigma reductions.

In our primary specification, making one additional household eligible for SNAP raises social welfare by about \$697. This estimate includes the mechanical cost of making the household eligible (\$67, noting that not all eligible households take up). In a mechanisms analysis, we find that welfare gains from woodwork effects are larger when they come from information rather than stigma. We also show how these welfare and mechanisms analyses rest on strong modeling and calibration assumptions, particularly about the redistributive benefits of SNAP and welfare cost of stigma.

Contributions and Related Literature. Our main contributions are that we empirically document woodwork effects in SNAP and study their welfare impacts in general. The lessons from SNAP may apply to other settings, as every social program makes some determination about program eligibility. Yet most of the literature on welfare program design imposes an exogenous eligibility threshold and studies the welfare impacts of other reforms to the program.[4]

Our work advances several literatures. First, we contribute to the literature on barriers to social program take-up (Nichols and Zeckhauser, 1982; Moffitt, 1983; Aizer, 2003; Currie, 2004; Heckman and Smith, 2004; Bhargava and Manoli, 2015; Friedrichsen et al., 2018; Finkelstein and Notowidigdo, 2019; Ko and Moffitt, 2024).[5] In the unemployment insurance literature, Kroft (2008) studies social spillovers — one potential

[4]To quantify this, we collected all 278 papers published in the *American Economic Review* between 2010–2018 and the *Quarterly Journal of Economics* between 2010–2019 that met one of 33 search terms about social welfare programs (see Figure A2 and Appendix A for details). Seventy-six of them were primarily about effects or design of social welfare programs, 49 of which involved the study of a specific policy instrument. Only 7 (14% of the 49) examined eligibility criteria as a policy instrument that the planner could manipulate to improve welfare. On the other hand, 25 of the 76 papers about welfare programs consider eligibility thresholds as a source of exogenous variation for estimating the program's treatment effect.

[5]We also add to public-economics research on the Supplemental Nutrition Assistance Program (see Currie 2003 and Bartfeld et al., eds 2016 for reviews). We draw on the SNAP data used in Ganong and Liebman (2018), who study how changes in the economic environment and program design affected enrollment through 2012. Recent research studies how SNAP receipt affects household members' nutrition, health or other outcomes (Almond et al., 2011; Hoynes et al., 2016; Bronchetti et al., 2019; Hastings et al., 2021; Bailey et al., 2024); whether the marginal propensity to consume food out of SNAP benefits differs from that out of cash (Hoynes and

microfoundation for woodwork effects — and explores how they affect optimal benefit size. Relative to Kroft (2008), we emphasize how the mechanism underlying peer effects influences social welfare, and we consider implications for the welfare impacts of eligibility threshold.[6,7]

Second, we link research on optimal program design to the growing literature in behavioral public economics (Bernheim and Taubinsky, 2018). Our analysis suggests that individuals' utility depends on social norms, and government policy plays an important role in shaping these norms, like in Lindbeck et al. (1999). Economists have only begun to explore how policy can influence psychological forces like shame or guilt, which may in turn have welfare consequences. For instance, our empirical evidence supports the hypothesis in sociology and history research that universal welfare programs like Social Security are less stigmatized because they are not means tested (e.g., Katz, 1986). Our analysis also highlights the roles of information versus stigma, and the empirical difficulty of disentangling them (e.g., Chandrasekhar et al., 2019). While leveraging the FSPAS data lets us make progress, future research that complements our work on mechanisms would be valuable.

Finally, we contribute to the small literature on woodwork effects. The health literature on Medicaid expansions finds evidence of woodwork effects (Aizer and Grogger, 2003; Sommers and Epstein, 2011; Frean et al., 2017; Sacarny et al., 2022), but it has not considered their implications for program design. Outside of the health literature, Leos-Urbel et al. (2013) and Marcus and Yewell (2021) find that eligibility expansions boost take-up among households already-eligible for free school breakfast or lunch programs.

2 Woodwork Effects in SNAP

2.1 SNAP Data

QC Data. We obtain the total number of people who participate in SNAP from the SNAP Quality Control (QC) files, which are administrative data from the U.S. Department of Agriculture on a random sample of SNAP participants (United States Department of Agriculture Food and Nutrition Service, 2019). The data record detailed information about household characteristics, benefit size, and incomes of SNAP participants. The data are a repeated cross-section, so we cannot study households over time. Using these files, we construct the total counts of program participants and those below a given income threshold, for each state

Schanzenbach, 2009; Hastings and Shapiro, 2018); and how SNAP affects recipients' labor supply (Hoynes and Schanzenbach, 2012; East, 2018; Harris, 2021; Gray et al., 2023). Homonoff and Somerville (2021) study the screening properties of the SNAP recertification process. This literature includes several papers studying stigma surrounding SNAP take-up and the rollout of the Electronic Benefits Transfer (Daponte et al., 1999; Currie and Grogger, 2001; Atasoy, 2009; Klerman and Danielson, 2011; Manchester and Mumford, 2012; Eck, 2019; Celhay et al., 2022). Ratcliffe et al. (2008) examine the effect of categorical eligibility on SNAP take-up but do not examine the effect of eligibility thresholds, nor do they focus on the already-eligible group.

[6]Other work studies the welfare impacts of eligibility rules in old-age insurance (Fetter and Lockwood, 2018) and disability insurance (Diamond and Sheshenski, 1995; Golosov and Tsyvinski, 2005; Low and Pistaferri, 2015).

[7]In many programs, the eligibility threshold is defined by the benefit size and the slope of the benefit schedule. Thus studies of benefit levels may also contribute to our understanding of eligibility thresholds. Our empirical setting isolates the effect of eligibility thresholds alone, since the benefit remains constant.

and year from 1996 until 2016, the last year for which systematic policy data are available.[8] The Quality Control files are administrative data. They record people's incomes and household size accurately, thereby addressing concerns about measurement error in survey data from Kreider et al. (2012), Meyer et al. (2015), and others. On the other hand, the dataset is relatively small at the state level. There are about 100,000 observations across 51 states and DC in each year from 2001 to 2016.[9] See Appendix A for more information about the dataset construction.

Sample and Outcomes. We begin with a sample of individuals with household income between 0%–130% of the FPL. We also study a sample including only individuals in households with income between 50%–115% of the FPL. We exclude individuals between 115%–130% of the FPL to address concerns about measurement error: we might consider an individual near the threshold as previously eligible, when in fact she is ineligible because of mismeasured income or other restrictions like asset tests. We focus on individuals above 50% of the FPL because take-up is high among individuals below 50% of the FPL, regardless of the state's eligibility threshold. Thus this group has less scope for increased take-up.

Using this sample, our main outcome is a measure of take-up *counts*. In particular, we use log total enrollment within specific income groups — in our main regression, among people earning 50–115% of the FPL. Almost all individuals in this range are eligible for SNAP in every state. This allows us to study inframarginal recipients; we are not counting increased enrollment among people who are newly eligible. Compared to take-up rates, this outcome has the advantage of not involving imperfect measures of the number of people who are *eligible* for the program in the outcome variable. Instead, we rely on the assumption that the number of people who are eligible regardless of the eligibility threshold (e.g., the number of people in households earning 50–115% FPL) is not correlated with the eligibility threshold beyond the controls we include.

We form take-up rates as a secondary outcome. Following Ganong and Liebman (2018), we divide the number enrolled (from the QC data) by the number of people within a given band of the income distribution in the state from the Current Population Survey's Annual Social and Economic Supplement (CPS ASEC) (Ruggles et al., 2020). That is, the denominator is the number of people in the CPS who are between 0–130% or 50–115% of the FPL. Crucially, the denominator does not exclude people who are otherwise ineligible for SNAP due to work requirements or asset histories. Thus, take-up rates are likely underestimates. There also may be measurement error in reported incomes in the CPS, an assumption we examine in detail.

Just as in other work estimating SNAP take-up, a possible limitation to the analysis is that we do not observe household assets or work histories, which can affect SNAP eligibility. First, federal rules restrict households with sufficient assets from participating in SNAP. In practice, only a small fraction of

[8] We construct our dataset by modifying the publicly available replication code for Ganong and Liebman (2018).

[9] Relative to comparable datasets, the QC data are best-suited for our analysis. The Survey of Income and Program Participation is not intended to be representative at the state level. The Panel Study of Income Dynamics and Current Population Survey both may be subject to measurement error about SNAP participation.

households are ineligible for SNAP under these asset histories. Second, under the Personal Responsibility and Work Opportunity Act (PRWORA), single households must meet certain work requirements to participate. However, the changes in these requirements do not coincide with changes in the eligibility threshold, and we also show that the results are similar among households with dependents. We also control for the requirements in robustness checks.

2.2 Policy Variation

Federal rules require that households below 130% of the FPL are eligible to participate in SNAP. Beginning in 2001, states had the option to expand eligibility to additional households up to 200% of the FPL under Broad-Based Categorical Eligibility (BBCE). The SNAP benefit schedule, which is set nationally, does not depend on a state's eligibility rules.

The eligibility thresholds relaxed under the BBCE correspond to gross income tests. Households must also pass a net income test: net of allowable deductions (e.g., an earnings deduction amounting to 20% of their earned income), their income must be below 100% of the Federal Poverty Level. The net income threshold always applies, regardless of the gross income test. Moreover, because the SNAP benefit size falls in net income and is not changed by the BBCE, many people who become newly eligible from the BBCE receive a small SNAP benefit.

Not every state that adopted the BBCE took the option to expand the eligibility threshold. In Section 2.5, we note that adopting the BBCE did entail additional changes to state welfare programs, but we reject that these changes can explain the woodwork effects we document here. Ultimately, 30 states expanded SNAP eligibility through the BBCE through 2016, four of which adjusted eligibility twice during this period (Figure A3A). Expansions occurred throughout the period, but they were especially likely to occur in 2001–2002 and 2010–2011. The states that do roll out an eligibility expansion are distributed across the country, although no states in the Great Plains region implement an expansion (Figure A3B). See Appendix A for more information about the BBCE policy.

2.3 Econometric Strategy

We estimate an event study using the variation in eligibility provided through the BBCE. We index each event by event-time τ, where $\tau = 0$ represents the first fully treated year. We set $\tau = -1$ in all years for untreated states. We define the "event eligibility rate" in each state s as the eligibility rate as a percent of the FPL after the BBCE expansion in treated states and the federal minimum (130%) in untreated states. We use a balanced panel: we limit the sample to the five years before and after treatment for treated states, and include all years in control states.[10] We normalize our coefficients relative to the year before the event and

[10] We drop the four states with two events in the event-study analysis, as well as the two states that have events too recently to have sufficient post-period data. This leaves 45 states (including the District of Columbia).

estimate:

$$y_{s,t,\tau} = \sum_{r \in \mathcal{R}} \eta^r \left(\mathbb{1}(\tau = r)_{s,t,\tau} \times \text{event eligibility rate}_s \right) + \delta_s + \gamma_t + X'_{s,t,\tau}\boldsymbol{\phi} + \varepsilon_{s,t,\tau} \tag{1}$$

where $\mathcal{R}$ is the set of event periods, s indexes states, t indexes years, event eligibility rate$_s$ measures the eligibility rate as a ratio of the FPL, δ_s is state fixed effects, and γ_t is year fixed effects.[11] We include X, a vector of additional linear controls for the state unemployment rate, the log of the number of people in a given income group in the state (measured in the CPS), SNAP outreach spending per person earning under 130% FPL in the states (log-transformed and interacted with an indicator for non-zero spending), and an index of other SNAP policies implemented around the same time (as in Ganong and Liebman (2018), henceforth the "Ganong-Liebman index").[12] In our primary estimates, we use $\ln(\text{enrollment}_{s,t,\tau})$ as the dependent variable ($y_{s,t,\tau}$). The coefficient of interest η^r represents the marginal effect of a 1 pp increase in the eligibility rate (expressed in terms of the FPL) on enrollment in event-time r. Our primary specifications are unweighted. We present standard errors clustered at the state level in this and all subsequent analyses that use state-year variation.

We also pool the data in this sample to estimate:

$$y_{s,t} = \eta \text{ eligibility rate}_{s,t} + \delta_s + \gamma_t + X'_{s,t}\boldsymbol{\phi} + \varepsilon_{s,t}. \tag{2}$$

The variable eligibility rate$_{s,t}$ represents the eligibility as a percent of the FPL in a given state-year, so η is the average effect on already-eligible people after an eligibility expansion.

Discussion of Identification and Controls. Our empirical strategy relies on a standard parallel trends identification assumption. Our state and year fixed effects remove fixed differences in outcomes across states and across years. The empirical strategy is valid if already-eligible households' log enrollment in treated and control states would have trended in parallel, conditional on controls, if not for the policy change. The specification in Equation (1) encodes a standard pre-trends test.

We include controls to address several important concerns. One concern is that states that impose the eligibility increase have faster population *growth* in the already-eligible sample. To address this concern, we control for the log count of the people within the already-eligible income group from the CPS. The economic environment and the policy environment are also relevant for SNAP take-up (Mabli et al., 2014; Ganong and Liebman, 2018). We control for the state unemployment rate to address the concern that states with eligibility increases have more financial distress. We include the Ganong-Liebman index to address the concern that states that expand eligibility may also impose other policies relevant to enrollment. We present robustness to additional threats to identification later in this section. Altogether, these controls do not have a dramatic effect on our results. The most important control is for the count of people who are eligible, which we show

[11] For instance, event eligibility rate$_s$ = 1.3 represents that the state has the minimum threshold of 130% of the FPL.

[12] The Ganong-Liebman index is the average of several indicators for the presence of different policies that may influence SNAP take-up, such as whether households can apply to SNAP online. See Appendix A for details on the variables that enter the index.

eliminates a modest pre-trend in our event study.

2.4 Results

Descriptive Evidence. Before presenting the formal estimates, we begin by visualizing woodwork effects in the raw data. In Figure 2A, we present total SNAP enrollment per 1,000 people (population-wide) in state-years with eligibility thresholds equal to 130% FPL versus above 130% FPL. We normalize the enrollment by the total population in all states with the relevant income rule to aggregate enrollment counts across states.

First, without the eligibility expansion, very few individuals with household income above 130% FPL take up the program, while with the eligibility expansion, mass appears above 130% FPL where individuals are newly eligible. This confirms that the QC data give sensible estimates of the enrollment counts, and that the eligibility changes relax a binding constraint for some individuals. Second, individuals *below* the threshold also enroll at higher rates with looser eligibility restrictions. These woodwork effects — the increased enrollment below the threshold — are the subject of our attention.[13]

Figure 2B presents a binscatter of the cross-sectional relationship between SNAP take-up among these already-eligible individuals (i.e., earning 0-130% FPL) and the state's eligibility threshold at the state-year level. We observe five different eligibility thresholds chosen by states between 1996–2016. Mean take-up is roughly 10 pp lower in states with eligibility at 130% of the FPL, the most stringent eligibility standard permitted under federal law.

Event-study Specifications. For confidence that the raw data reflect woodwork effects and are not driven by confounds, we turn to our event study (Equation (1)). We plot log enrollment among our already-eligible sample by event period, relative to event period -1 (Figure 3A). We find no evidence of pre-trends leading up to the policy change. After the policy change, enrollment increases steadily. Figure 3B shows that the effect is concentrated among people earning over 50% and less than 115% FPL. Our primary estimate is that raising the eligibility threshold by 10 pp of the FPL boosts the number enrolled by 1–2 percent in the five years following the policy change.

To show the effect of controls on our empirical estimates, we present in Figure A4 the event study with state and year fixed effects only (Panel A) and then add the control for the log of the total number of people between 50 to 115% of the FPL (Panel B). Overall the results are similar without controls. With no controls at all (Panel A), we see some visual evidence of a pre-trend prior to treatment, although the trend is small in magnitude and vanishes three years before treatment. Once we control for the log of the CPS population totals (Panel B), the pre-trend vanishes, and the results in Panel B are very close to those in Figure 3. Note that we are running log take-up regressions: the moderate importance of controlling for CPS population confirms that the denominator of a take-up regression matters and is not on-face concerning.

[13] We note a slight excess mass around 75% of the FPL, which may be an artifact of the QC data; however, woodwork effects appear throughout the income distribution.

The event-study figure suggests that results grow over time. The effects are larger in years 4–5 than years 1–3, suggesting that woodwork effects persist or grow in the medium-term.[14] Such effects might grow even several years later if, for instance, information takes time to spread or cascades once others become eligible. Alternatively, stigma might respond only slowly to changes in the threshold.

Placebo. A placebo test offers a useful validation of the above results. We observe nine states implement the BBCE *without* expanding eligibility beyond 130% of the FPL.[15] Most of these states adopted the BBCE around the same time as the states in the main event study (2009–2011). Thus, we study the effect of the BBCE in the states that did *not* expand eligibility but *did* implement the BBCE. To implement the placebo test, we show an event study (as in Equation (1)), where treatment represents states that implemented the BBCE but did not expand eligibility (Figure 3C).[16] We use log enrollment among the 0–130% of FPL sample as the dependent variable. This event study gives no effect; we find no evidence to support that the short- or long-term effects in placebo states are the same as the 5-year effect in states with an eligibility expansion.

The placebo test suggests that eligibility expansions, and not ancillary features of the BBCE, drive the results. We cannot completely rule out that the BBCE caused unobserved changes in outreach (not captured by our outreach control variable) or transaction costs (not captured by the vector of SNAP policy controls). But such forces would also be inconsistent with the placebo test, unless they only occurred in BBCE states that also raised the eligibility threshold.

Combined Estimates. The event-study specification and placebo test confirm the existence of woodwork effects. To obtain the pooled effect over all periods, and parsimoniously present robustness to different specifications, Table 1 presents estimates from Equation (2). Our preferred specification (Column 1) uses the sample used in the event study and includes state and year fixed effects, and controls for the state unemployment rate, outreach spending, and the Ganong-Liebman controls.[17] The independent variable is the eligibility threshold as a ratio of the Federal Poverty Level, so that increasing it by 1 corresponds to increasing the threshold by 100% of the FPL. For 50–115% FPL, we find that $\eta = 0.120$ and reject $\eta = 0$ at $p < 0.05$. These estimates suggest that raising the eligibility rate by 10 pp of the FPL (e.g., from 130% to 140%) boosts take up by 1.2 percent. The modal eligibility increase in our sample is from 130% to 200% of the FPL, which delivers a 8.4 percent increase in take-up among this sample ($0.7 \times 0.120 \approx 0.084$).

The 0–130% sample shows positive but insignificant results (t-statistic ≈ 1.6). The attenuated result in this sample is consistent with the sample's smaller magnitudes when averaging all post-period years in the

[14] Tests of the null hypothesis that $\eta^4 = \eta^3$ and $\eta^5 = \eta^3$ both reject with $p < 0.01$. A joint test for both hypotheses also rejects the null with $p < 0.01$.

[15] States can implement the BBCE for bureaucratic reasons, as the policy can simplify program administration, or to relax the SNAP assets test. See Appendix A.

[16] We exclude states that did increase eligibility from this test, so the regression includes 19 states. A handful of states which adopted BBCE without changing their eligibility thresholds at that point did expand eligibility at a later date. Here, we exclude these states, but the results are similar when they are included and we add a control for the eligibility threshold.

[17] We interact the log of outreach spending with an indicator for non-zero spending to address state-years with zero outreach spending (Chen and Roth, 2024).

event study.

The rest of Table 1 shows that our estimate of woodwork effects is robust to the particular choice of the specification. Column 2 separates the Ganong-Liebman index into separate indicators for each component variable. Column 3 reverts to the index form of these controls but adds new controls for lagged unemployment and the prevalence of waivers relaxing the SNAP work requirements for able-bodied adults without dependents (ABAWDs), beginning in 2010.[18,19] Column 4 excludes the years 2008–2011 (the Great Recession). Column 5 weights by state-year population. Column 6 computes the treatment effect as the difference between the average of the event study coefficients in the post period and the average of the coefficients in the pre period, weighting all post periods equally. Finally, Column 7 uses all years of data we have (a balanced panel of 50 states and D.C. from 1996–2016), instead of only the event study sample of a 5-year window around the eligibility increase. It also includes states that change eligibility several times or reduce eligibility. Throughout the table, the results are stable. Estimates of η range from 0.11 to 0.13 in the 50–115% FPL sample.

We also repeat the exercise for two different samples in Table A1 and find similar results. Panel A shows enrollment responses in the 0–115% FPL sample. The estimates are consistent with the main results but generally lower. This attenuation reflects that our dependent variable (SNAP enrollment) has less scope to rise when many people from 0–50% of the FPL already take up SNAP. Panel B assesses enrollment among households with children, as these people are likely not subject to the additional ABAWD work requirements that were relaxed and reimposed during the sample period. We see similar sized effects in this sample. Together, these results and Table 1 provide strong evidence of woodwork effects from the BBCE, as the effect persists across specifications and samples.

2.5 Robustness

We describe several robustness checks, relegating detailed analysis to Appendix B.3.

A first reason to be concerned is that the BBCE allows states to waive some rules on the maximum assets allowed for SNAP participation. If this aspect of the BBCE caused already-eligible households to newly enroll, we overstate the particular role of the eligibility change. In practice, these asset rules affect a small number of families.[20] Moreover, the placebo test in Figure 3C constitutes strong evidence that only the eligibility threshold, and not ancillary BBCE-related policies, are responsible for the take-up effect.

Relatedly, one might be concerned that BBCE expansions were bundled with other policies or coincide with changing economic conditions.[21] Using a placebo event study with the SNAP policy index as the

[18] We use data on ABAWD waivers from data generously shared by Harris (2021).

[19] Figure A5B also shows the event study where the sample includes only SNAP recipients in households with children.

[20] Ganong and Liebman (2018) find asset waivers were responsible for only a small share of increased take-up in recent years. Eslami (2015) estimates that 4 percent of people under 130% FPL who participate in SNAP are eligible only due to asset waivers (see computation in Ratcliffe et al. (2016)). There are a host of such asset waivers, including many not linked to the BBCE. But even assuming all these households were only eligible due to the BBCE, the asset waivers could not explain even half of the woodwork effects we find.

[21] We control for an index of eight other SNAP policies that occur during the same period, and show that this control does not

dependent variable, we find no evidence that the SNAP index increases after the eligibility expansions (Figure A6). Thus, for other policies to present a threat to identification, unobserved policy changes must coincide with eligibility expansions while observed policy changes do not. Along these lines, the appendix also presents evidence that economic conditions do not drive eligibility expansions. Finally, toward testing both these concerns, the appendix includes a version of a balance test. States that implement the BBCE do differ on observables from states that do not. However, states which raised their eligibility threshold *more* do not meaningfully differ on observables from states which raised their eligibility threshold *less*.

Another concern about an analysis of take-up pertains to the role of measurement error in take-up rates. To mitigate these concerns, we typically use QC numerators as our dependent variable, in Table 1. However, the size of the eligible population, a control in our analyses, is measured imperfectly in the CPS. A simulation shows that only an implausible amount of measurement error in the size of the eligible population, exactly coinciding with the event and only in treated states, could explain our result.[22]

Finally, we note that concerns about negative weights are unlikely to apply in our setting, since: (i) there is a large pool of never-treated units, and (ii) we do not have always-treated units. As a check we implement the heterogeneity-robust stacked estimator from Cengiz et al. (2019) and the Sun and Abraham (2021) estimator. We obtain similar results (Appendix Figure A8). The Sun and Abraham (2021) estimator delivers somewhat larger results in years 4 and 5, but the confidence interval safely contains the original point estimates.

2.6 Heterogeneous Effects and Compliers

From which part of the income distribution do woodwork effects arise? We present treatment effect heterogeneity by household income (Figure A7). We estimate a version of Equation (2), using take-up *rates* instead of log enrollment counts so that the values are more directly comparable across income groups with different base rates. Take-up rates increase most among those earning 130–160% FPL, who are barely ineligible before an expansion. The effect in this group is larger than the largest effect in the already-eligible population, among those earning 100–130% FPL. However, even after the expansion, take-up in the newly eligible group is still much lower than any other group. We also see that the treatment effect size is increasing with household income within the already-eligible sample; however, this may partially reflect that the base take-up rate is much lower among households with relatively more income.

As another way of summarizing the targeting of eligibility expansions, we regress enrollees' characteristics on the threshold (Table 2). Besides income, other characteristics including gender, race, and age, do not change.[23]

diminish the magnitude of the effect in Table 1.

[22]Controlling for the size of the eligible population could also be problematic if individuals move in response to eligibility expansions. The literature on such "welfare magnetism" is mixed (Meyer, 1998; Agersnap et al., 2020; McPherson, 2024).

[23]We note that Table 2 shows no evidence of an increase in the share of enrollees whose SNAP certification period is less than 6 months, suggesting that new enrollees do not have more volatile income. This is evidence against an alternative explanation that the

Consistent with Figure A7, Table 2 shows a significant positive effect on the average poverty level of enrollees. The magnitude of the effect on income indicates that, if the means test were raised by 10 percentage points (from 130% FPL to 140% FPL, for example), the average gross income of SNAP recipients would increase by 0.07% FPL. A simple back-of-the-envelope calculation suggests that compliers thus have 1 percentile higher income on average than those who would have enrolled regardless.[24]

Together, Figure A7 and Table 2 suggest that woodwork effects bring in relatively higher-income households and are not well-targeted on this dimension. If woodwork effects reduce ordeals or information frictions, this evidence accords with the screening results in, e.g., Finkelstein and Notowidigdo (2019). This result contributes to a contested literature on the screening properties of ordeals and information interventions (see discussion in Rafkin et al., 2024).

As Section 4 formalizes, the incidence of woodwork effects in terms of complier households' rank in the income distribution is insufficient for welfare analysis. Such households could still be making mistakes. If woodwork-effect compliers are sufficiently needy relative to the average household in society, eligibility expansion can raise welfare. But the observed targeting does suggest that woodwork effects are probably less favorable for welfare than if woodwork effects were concentrated among the poorest households.

2.7 Interpreting the Magnitude of the Results

We now provide three ways of interpreting the magnitude of the results.

Take-up Elasticity. We estimate the elasticity of take-up with respect to the share of the population who is eligible. The elasticity will also play an important role in the theoretical model.

We employ an instrumental variables approach to estimate this elasticity. The share of the population eligible for SNAP is affected by confounding conditions which also affect the number of people below a certain income level. The eligibility expansions provide plausibly exogenous shocks to the share eligible. Thus we instrument for the log share eligible for SNAP using the state-and-year-specific income cutoff as a ratio of the Federal Poverty Level. The exclusion restriction is that eligibility expansions are not associated with take-up of already-eligible people except through changes in the share eligible.

We return to Equation (2) from Section 2. We use a log-log specification, with $\ln(\text{take-up})$ and $\ln(\text{share eligible})$ as the dependent and independent variables, respectively. The estimating equation is:

$$\ln(\text{take-up})_{s,t} = \eta \ln(\text{share eligible})_{s,t} + X'_{s,t}\boldsymbol{\phi} + \delta_s + \gamma_t + \varepsilon_{s,t}, \tag{3}$$

where we instrument for $\ln(\text{share eligible})$ using the state eligibility threshold as a ratio of the FPL. Here η represents an elasticity rather than a level effect.

higher threshold reduces uncertainty about eligibility.

[24]To see this, note that our main estimates imply that after a 10 pp increase in the means test, 1% of all enrollees are newly enrolled. In the 2016 CPS, a 7% higher FPL (= 0.07 ÷ 0.01) off the control-group average of 80% FPL corresponds to a change from the 12th to the 13th percentile of the household income distribution.

We present the IV estimates for the 0–130% sample using all the data (Table 3, Panel A) as well as the event-study sample (Panel B). We document a strong first stage: in the full sample, increasing the eligibility threshold by 10% of the FPL increases the share of a state population that is eligible by 7.28% (t-stat = 21.41), with similar results for the event-study sample. Our 2SLS estimate in the full sample is $\eta = 0.130$ (SE: 0.067); the estimate in the event-study sample is $\eta = 0.121$ (SE: 0.079). We also document that simple OLS regressions of log take-up on the log share eligible have the opposite sign, likely due to the omitted variables bias we described above.

Comparison to Woodwork Effects in Medicaid. Sacarny et al. (2022) find that about 0.1 previously-eligible children enter Medicaid for every adult who entered Medicaid from the Oregon Health Insurance experiment. To compare to this point estimate, we employ the magnitude of the woodwork effect among the entire already-eligible population (Table 1A).[25] We find that almost one (estimate: 0.99, standard error: 0.58) already-eligible person between 0–130% of the FPL is induced to take up the program for every newly eligible person who takes up the program.

We cannot reject that the treatment effects equal those in Sacarny et al. (2022). Even so, our point estimate is that woodwork effects in this setting are almost ten times larger than in Sacarny et al. (2022), which warrants discussion. Altogether, we have no reason to expect that woodwork effects will be of the same magnitude across programs and over time. Importantly, in this setting, expanding the SNAP eligibility threshold for gross income does not loosen other eligibility criteria (e.g., the net income threshold). These criteria may bind for people with higher incomes. As a result, eligibility expansions can cause higher take-up among the already-eligible population without many newly eligible people joining the program. Relatedly, the benefit sizes for the newly eligible population are small, so they may not be induced to join.

Comparison to Outreach Spending. Another way of benchmarking our effects is to compare the take-up from woodwork effects to the take-up from direct SNAP spending on information and outreach. For an effect of outreach on take-up, we turn to the randomized control trial run by Finkelstein and Notowidigdo (2019), where the authors find that sending mailers to people who are likely eligible for SNAP but not enrolled boosts enrollment. They calculate that their outreach intervention costs about \$20 per additional enrollee. At this rate, it would cost about \$74 million to increase enrollment in the already-eligible population by the same amount as raising the income eligibility threshold from 130% FPL to 200% FPL.[26]

On the one hand, \$74 million is a fraction of the total annual spending on SNAP (\$70 billion in 2016). On the other hand, it is more than four times what all states combined spent on outreach in 2016 (\$17.4 million).

[25] Let the point estimate for the entire already-eligible population from Table 1A, Column 1 be $\hat{\eta}_i$. We then estimate a version of Equation (2), using the log of the total number of people on the program as the dependent variable (and controlling for the log of the number of people below 130% of the FPL from the CPS). Let the point estimate from this regression be $\hat{\eta}_t$. We then present $\frac{\hat{\eta}_i}{\hat{\eta}_t - \hat{\eta}_i}$, where the denominator represents the increase in the marginal population and the numerator represents the increase in the already-eligible population.

[26] There were around 44 million SNAP enrollees in 2016. To derive the number of new enrollees from such an increase, we multiply 44 million by the increase in take-up (8.4%) implied by our estimates in Table 1 at the modal eligibility threshold increase (130% to 200% FPL). Finally, we multiply this by \$20 per additional enrollee to arrive at \$74 million.

Finally, the mechanical cost of raising eligibility goes to program recipients who are newly eligible. But the mechanical cost of outreach does not go to program recipients.

Cost-Effectiveness. We conduct a back-of-the-envelope calculation to compare the mechanical costs of two natural interventions to raise take-up of the already-eligible population by 1 pp: raising the eligibility threshold and raising the benefit size. We find that the raising take-up by way of the eligibility threshold is a costlier intervention than inducing the same amount of take-up by raising benefits (Table 4). Using the η estimated in Equation (3), we calculate that to increase take-up by 1 percentage point, an additional 4.3 pp of the US population would need to be eligible for SNAP. If take-up in the newly eligible population is similar to take-up among people who are just barely eligible (36%), and the benefit size is similar to the benefit size in this group ($707 per person-month, calculated from the QC data), then this intervention costs an additional $3.6 billion per year. We assume that the elasticity of take-up with respect to the benefit size is 0.5.[27] To get a 1 pp increase in take-up, the benefit size would need to increase by $56 per year for 44 million SNAP enrollees — costing $2.5 billion per year.

3 Mechanisms: Information and Stigma

Why are there woodwork effects? The question relates to a long-standing literature on incomplete take-up of social programs that categorizes barriers to take-up into incomplete information, stigma, and other ordeal costs.

One hypothesis is that the eligibility change affects stigma around SNAP take-up. For example, it is possible that when SNAP becomes available for relatively wealthier people, SNAP no longer conveys as much of a negative signal. We test this hypothesis using an online experiment in which we exogenously change participants' beliefs about the SNAP means test. A second hypothesis is that changing the eligibility threshold increases the information about the program. For example, because more people are eligible, people can more easily obtain information about how to apply from friends or family. We test this hypothesis by making novel use of USDA survey data on SNAP stigma and information.

3.1 Online Experiment: Evidence of Stigma

We present evidence from an online experiment that the eligibility threshold may affect perceived stigma around SNAP take-up.[28]

[27]See Krueger and Meyer (2002) in the context of Unemployment Insurance and worker's compensation programs. We know of no more recent estimate for SNAP.

[28]We used the survey provider Lucid. We ran the experiment in March 2020. The onset of the coronavirus pandemic should not complicate treatment-control differences via randomized information provision.

3.1.1 Experiment Design

The experiment induces variation in participants' beliefs about the share of people who are income-eligible for SNAP. Then, we study how raising people's beliefs about the share eligible affects self-reported stigma.[29] Figure A12 summarizes the experiment design. Experiment data are available at the AEA Data and Code repository (Anders and Rafkin, 2024).

High- and Low-Share Experiment. Our main experiment was embedded in a question asking respondents to report what share of Americans they thought were income-eligible for SNAP in 2016.[30] On this page of the survey, all respondents were given a truthful hint: *"In 2016, in one of the U.S. states, roughly [X] of the population had low enough income that they could qualify for SNAP."*

X was randomly either 15% or 38%, which were the highest and lowest state-level eligibility shares we see in the administrative SNAP data from 2016. We refer to those participants who saw the 38% hint as those in the "high-share" treatment.

Belief Elicitation. After implementing the treatment, we conduct a manipulation check by eliciting people's beliefs about the share of people eligible for SNAP. We asked: *"In 2016, how many out of every 100 people (in all U.S. states) do you think have low enough income that they could qualify...?"*

Belief-Correction Experiment. Following the belief elicitation, we included another randomization. We informed a random subset of participants about the correct share (27%, as per our calculations combining the CPS and the SNAP Policy Database). Depending on their prior beliefs, this "belief-correction" treatment is intended to cause participants to update up or down about the share of people who are eligible. We included the belief-correction experiment because recent papers, e.g., Bursztyn et al. (2020), use similar belief corrections to manipulate people's prior beliefs. The cross-randomization of these experiments proves difficult to interpret, because we manipulate people's beliefs with the high- and low-share experiment before correcting them with the belief-correction experiment. We relegate discussion of the belief-correction experiment to the Appendix.

Stigma Elicitation. We asked respondents to rate their agreement, on a scale from 1 to 9, to a series of eight statements about SNAP: *(1) I would prefer not to use food stamps because I would rather be self-reliant and not accept help from the government; (2) I believe that people should do what they can to avoid being on food stamps; it is better to make it on your own; (3) Most people believe that someone who uses food stamps is just as hard-working as the average citizen; (4) If I used food stamps, I would be concerned that people would treat me disrespectfully at stores; (5) Most people believe that someone who uses food stamps does so because of circumstances outside their control; (6) Most people think less of a person who uses food stamps; (7) Most people who use food stamps would go out of their*

[29] The complete survey instruments are available from the authors' websites.

[30] Reports were incentivized as follows: participants were told at the beginning of the survey that a lottery would be conducted among respondents who answered a factual question correctly, and the winner would have $50 donated to her choice of charity.

way to prevent others knowing about their food stamp receipt; (8) If I used food stamps, I would avoid telling other people about it. We modeled these questions after the questionnaire in the Food Stamp Program Access Study (Bartlett et al., 2004).

We aggregate the statements into two indices: (i) "first-order stigma," which ask respondents about their own attitudes (statements 1, 2, 4, and 8 above), and (ii) "second-order stigma," which asks respondents about others' attitudes (statements 3, 5, 6, and 7).[31] We standardize these outcomes using the mean and standard deviation of the control group and then average the standardized values as in Kling et al. (2007). We also show the effects on an aggregated index.

Either first- or second-order stigma could play a role in woodwork effects, depending on the model. If people care about social image and take-up is partly observable, the extent to which others condone or sanction SNAP may affect take-up costs. With first-order stigma, people may have a hedonic aversion to SNAP that does not depend on others' views. Such aversion could easily influence take-up if modeled as a direct take-up cost.

Sample Construction and Balance. We drop participants who fail either of two pre-registered attention checks, as well as those who did not provide a prior or respond to all stigma questions. Our final sample has 2,131 participants (79% of the original sample). Table A4 summarizes these sample limitations and confirms that attrition, inattention, and non-response were balanced between treatment and control. Appendix A describes the data cleaning in more detail.

The sample is balanced across the high-share treatment (joint p-value: 0.94) and has a relatively similar composition as the U.S. on average (Table A5). In some tests, we restrict the sample only to the 512 people below 130% of the FPL, because this already-eligible subgroup is relevant for for woodwork effects. Among this subgroup only, a joint F-test suggests experimental imbalance (p-value: 0.02).[32] The experiment was randomized but not stratified, and any imbalance in this subgroup occurred by chance. To address the lack of balance when studying treatment effects in this subgroup, we present robustness tests that control for available demographics. We stress that the experimental treatment is balanced in the full sample, and we emphasize results from the full sample as a result.

Econometric Strategy. In our primary specification, we simply compare the difference in means across treatments:

$$y_i = \beta \mathbb{1}(\text{high})_i + \gamma \mathbb{1}(\text{truth})_i + \varepsilon_i, \tag{4}$$

for individual i, where β represents the coefficient of interest. In robustness exercises, we estimate a version of Equation (4) with additional demographic controls. We conduct inference using robust standard errors.

[31] We reverse the scale for questions 3 and 5 so that positive numbers always indicate more stigma. Statement 4 could perhaps be categorized under either index. Separately, in a minor deviation from preregistration, we include four statements under the "self-stigma" index rather than two. We make this choice to more parsimoniously partition the set of questions. We also show results question-by-question.

[32] The most imbalanced covariate is that the high-share treatment is less concentrated in the Northeast region than the low-share treatment (p-value of difference: 0.02).

3.1.2 Experiment Results

Beliefs About Eligibility. The high-share treatment successfully moved beliefs about eligibility (Figure A13). Both groups report beliefs that are slightly overestimated but reasonable; the mean for the low group is that about 30% of people are eligible, and the mean for the high group is that about 39% are eligible. The raw difference in means is 9.21 pp (SE: 0.80, p-value < 0.001). The standard deviation of beliefs in the control group is 19.8 pp, so the treatment raised the beliefs by a sizable 0.47 standard deviations. Moreover, while the low- and high-share treatments anchored a large fraction of people toward the numbers we provided them (15% and 38%), it also moved beliefs for others throughout the distribution.

Stigma. First, we note that responses to the eight stigma statements are somewhat but not overwhelmingly correlated (Figure A14), so each question may contain independent information about the participants' views. A concern is that participants simply anchor to their responses on the first question since the question order was not randomized. In fact, while we find that responses to the second question are relatively correlated with the first question (correlation ≈ 0.65), other questions do not display a large correlation with the first question.

Increasing individuals' beliefs about the share of Americans eligible for SNAP moderately decreases their self-reported second-order stigma (Figure 4A). Aggregating the results into indices, the high-state treatment reduced second-order stigma by -0.050 standard deviations (SE: 0.027, $p = 0.061$). Effects are larger in magnitude among the 512 participants below 130% FPL (point estimate: -0.109, SE: 0.058, $p = 0.061$).

The treatment effects for second-order beliefs are similar across questions that form the second-order index. In the full sample, the high-share treatment reduces stigma the most in the question about whether most people believe recipients "go out of their way to prevent others knowing about their food stamp receipt." We find larger effects among people who have ever taken up SNAP, men, and Democrats, although treatment effect heterogeneity is not generally significant (Figure A15). On the other hand, we find positive but statistically insignificant results on first-order stigma (Panel B).

We summarize these results in Table A7, and we find very similar results when we include demographic controls (Table A8). Moreover, when we aggregate the second- and first-order stigma results into a combined index, we find no statistical evidence of an average effect on stigma, although the point estimate is negative. The null result is mechanically driven by the null or slightly positive effect on first-order stigma.

3.1.3 Experiment Conclusions and Caveats

This experiment suggests that aspects of transfer program design, like the eligibility threshold, may affect stigma. Evidence about stigma in social welfare programs remains elusive (Currie, 2004; Bhargava and Manoli, 2015; Celhay et al., 2022), so the experiment is a contribution in its own right.

Still, the evidence we provide is not dispositive. First, we find no effects on first-order stigma. First-order beliefs about, say, whether one should accept help from the government may represent deep-seated aspects of one's identity. It is therefore not surprising that people's first-order beliefs may be hard to move in a light-touch survey experiment. Second, the belief-correction experiment does not provide evidence that the means test affects stigma (see results in Appendix C). Third, we classify Statement 4 as first-order beliefs about stigma, but it could also capture second-order beliefs, and reclassifying the statement would further attenuate the results. Fourth, as with any online experiment, one may worry about external validity and what stigma views in a questionnaire really measure.

3.2 Stigma, Information, and Take-Up

In this section, we show that the subgroups whose stigma decreases the most in the online experiment do not have the largest changes in take-up in the administrative data used in Section 2. Instead, those subgroups who appear to have the lowest stigma about SNAP, and those that are least likely to have information about SNAP, are those that see the largest changes in take-up. Together, this suggests that the means-test affects take-up largely via an information channel.

Data. For this exercise, we include data from an additional source, the USDA's Food Stamp Program Access Study (FSPAS) (Bartlett et al., 2004). To our knowledge, this is the first academic study of the FSPAS, which the USDA's Economic Research Service generously shared with us. The USDA's FSPAS involved phone and in-person interviews conducted in 2001 with a reference month of June 2000. Since the analysis of woodwork effects uses QC data from 1996–2016, the FSPAS data occur toward the beginning of the sample period. We use data from two subsurveys: one of a random sample of approved SNAP applicants, and another of a nationally representative sample of likely eligible nonparticipants.[33]

Both surveys ask respondents a series of four questions about their perceived stigma around SNAP and a number of questions about the information they have about SNAP. We consider respondents who report any feelings of stigma to be affected by stigma ("stigma types"). We consider any nonparticipants who reported a lack of information about any of three information questions to be affected by information frictions ("information types").[34] Anyone who participates in SNAP is not an information type. Types are not mutually exclusive.

The data include demographic information, including gender, age, race, marital status, and number of children. Because we also have these variables, as well as household income, in our online experiment and in the administrative SNAP data, we can compare statistics at the demographic cell level between datasets. Each cell is defined by the gender and age (binned into 18-30 year-olds, 31-65, and 65-100) of the household

[33] Among nonparticipants deemed to be eligible from an initial screener, 96.3% completed the survey. Among applicants randomly sampled from lists provided by SNAP offices, 56.7% were reached and completed the survey. We analyze a sample of 1,585 respondents who either answered questions about stigma or answered questions about information (and have non-missing weights).

[34] These asked whether participants had heard of SNAP; whether they thought they were eligible for SNAP; and whether they knew where to go to get SNAP benefits.

head; whether or not the household head is a non-Hispanic white; the household composition (married adult with children, unmarried adult with children, or adult(s) without children); and, where available, the income decile of the household when compared to the distribution of incomes in the US Current Population Survey.

Descriptives. Figure A9 shows the stigma and information statements presented to FSPAS respondents and the share of respondents who agree with each statement. About 40% of the sample agree with at least one of the stigma statements, leading us to categorize them as being affected by stigma. Of those who agree with any stigma statements, almost half (47%) agreed with only one, and another 29% agreed with two. Meanwhile, about 61% of the nonparticipant sample disagreed with any of the information statements, leading us to categorize them as being affected by information.

We show additional descriptive statistics by whether we consider the respondent to be affected by stigma (Table A2). Those who report any stigma are more likely to be white, are on average younger, and are more likely to have children in their household. Notably, those who report any stigma are *more* likely to be enrolled in SNAP.

Results. Next, we study whether demographic cells with many stigma or information types have larger woodwork effects (the binned scatterplots in Figures 5).[35] First, we find that cells with many stigma types have smaller woodwork effects (Panel A), and cells with many information types have larger woodwork effects (Panel B). While the relationships are noisy, we can statistically reject that the slopes are equal to zero at $p < 0.05$ and $p < 0.1$, respectively. The fact that the cells with many stigma types are statistically *less* likely to have large woodwork effects is particularly suggestive that woodwork effects are not driven by stigma. To complete the story, Figure 5C presents the correlation between the treatment effect from the online experiment (i.e., the effect of the perceived means test on reported stigma) and the treatment effect from the main analysis (the effect of the means test on take-up). Subgroups with the largest reductions in stigma when the means test increases do *not* have the largest woodwork effects.

We suggest caution, as we combine a small-scale experiment with a noisy survey. Possible measurement error might attenuate results for stigma, leading us to underestimate its contribution to woodwork effects.

Discussion. We find no evidence that stigma contributes to woodwork effects. We find suggestive evidence that information drives woodwork effects. Nevertheless, the experiment suggests that increases in the eligibility threshold could decrease stigma costs. How should we interpret these facts? We use a model to conduct welfare analysis.

[35] Appendix C gives details. Because these binned scatterplots plot cell-level coefficients estimated with error, we conduct our tests weighting by the inverse of the product of the variances of the coefficients, also discussed in the Appendix.

4 Model

We develop a model of program take-up in the spirit of Finkelstein and Notowidigdo (2019). Our goal is to study the welfare impact of a marginal change in the program's eligibility threshold when woodwork effects are present. Households enroll if they are aware of the program and they face low enough enrollment costs. Woodwork effects reflect that changes in the eligibility threshold increase information and/or reduce ordeal costs (stigma). Proofs and extensions are in Appendix E.

4.1 Set-up

Households have exogenous and observed type θ. Types are distributed uniformly on the unit interval, $\theta \sim U[0,1]$.[36] All households with $\theta < m$, where m is the "eligibility threshold," are eligible for exogenous program benefit B_θ.[37] Following the notation in Finkelstein and Notowidigdo (2019), household type θ gets indirect utility u_θ^A if they take up the benefit (that is, the program accepts them) and u_θ^N if not.

Ordeals and Information. Type θ faces take-up or ordeal cost $c_\theta \in \mathbb{R}$ drawn from distribution $H_\theta(\cdot \mid m)$, which has finite first moment and is continuously differentiable in its argument and in m. Here we further specialize $H_\theta(\cdot \mid m)$ to have bounded support but relax this assumption in the Appendix. We often call c_θ a stigma cost, since we are especially interested in how the eligibility threshold may reduce stigma. But the model is agnostic about whether c_θ is stigma or other ordeals like the time-costs of applying.[38]

Households can misperceive take-up costs. With probability p_θ^i, the household is informed about the program and correctly perceives that she faces cost $\hat{c}_\theta = c_\theta$. With probability $1 - p_\theta^i$, the household is not informed about the program and incorrectly perceives that she faces cost $\hat{c}_\theta = \infty$. We assume that the probability of correctly perceiving take-up costs is unrelated to the take-up cost.

Timing and Household Optimization. Households draw a perceived take-up cost. They take up if take-up utility, net of perceived costs, exceeds utility from not taking up: $u_\theta^A - \hat{c}_\theta > u_\theta^N$. Write $p_\theta^s := H_\theta(u_\theta^A - u_\theta^N \mid m)$, so p_θ^s denotes the enrollment probability if they correctly perceive stigma costs. The expression $p_\theta^i p_\theta^s$ is then the overall take-up probability. Woodwork effects mean that changes in m reduce costs, so that $\frac{dp_\theta^s}{dm} > 0$, and/or increase information, so that $\frac{dp_\theta^i}{dm} > 0$.

[36] This is without loss of generality for any continuous type distribution, as we can map θ onto quantiles of the distribution.

[37] One way of justifying an exogenous benefit and endogenous eligibility threshold is that states control the threshold, while the benefit schedule is set nationally. We model the state's problem.

[38] Some transaction costs may depend on m, e.g., if more stores accept SNAP once more people become eligible. If the threshold reduces uncertainty about eligibility, that might yield an increase in the expected net benefit or an increase in awareness, depending on the model.

4.2 Welfare

We assume a utilitarian social planner. Redistributive motives still emerge from the curvature of the utility function. Following Finkelstein and Notowidigdo (2019), a money-metric social welfare function is:

$$W = \int_0^m \left[\underbrace{p^i_\theta p^s_\theta \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] \right]}_{\text{Enrollees}} + \underbrace{(1 - p^i_\theta p^s_\theta) u^N_\theta}_{\text{Non-enrollees}} \right] d\theta + \underbrace{\int_m^1 u^N_\theta d\theta}_{\text{Ineligible}} - \text{Fiscal Cost}, \tag{5}$$

where u^A_θ and u^N_θ are utility rescaled by $\frac{1}{E[u']}$. The fiscal cost term is the total government spending in society. In this sense, u^A_θ and u^N_θ are money-metric utilities for a representative household who bears the full fiscal cost.[39] We can alternatively view W as an *ex ante* welfare measure, evaluated before households realize their income or take-up costs.

This approach takes an explicit stance on the gains from redistribution. Society values transferring to households with high marginal utility of consumption more than to households with low marginal utility, based on the levels of utilities u^A_θ and u^N_θ and the scaling factor $E[u']$.

To understand Equation (5), observe that among eligible types $\theta < m$, share $p^i_\theta p^s_\theta$ takes up the program. These households receive the benefits but must pay the take-up costs, leaving type θ with utility $u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta]$ on average. Remaining type-θ households get u^N_θ. For households who enroll, society bears the cost of program provision.

Define G_θ as the net fiscal cost of program enrollees versus non-enrollees, inclusive of all benefits provided, administrative costs, and tax revenues collected (e.g., labor-supply responses). For simplicity, assume that changing the eligibility threshold only changes behavior among enrollees; for instance, non-enrollees do not modify labor supply with a higher threshold.

We now summarize the cost and benefits of raising the eligibility threshold:

Proposition 1. *Society's willingness to pay for a marginal increase in the eligibility threshold is:*

$$\begin{aligned} \frac{dW}{dm} = & \underbrace{\int_0^m \frac{dp^i_\theta}{dm} p^s_\theta \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta \right] d\theta}_{\text{(i) woodwork effect (information channel only)}} + \underbrace{\int_0^m \int_{c < u^A_\theta - u^N_\theta} p^i_\theta \frac{dH_\theta(c \mid m)}{dm} dc d\theta}_{\text{(ii) stigma reduction}} \\ & + \underbrace{p^i_m p^s_m \left[u^A_m - E[c_m \mid c_m < u^A_m - u^N_m] - u^N_m \right]}_{\text{(iii) newly eligible and enrolled (benefits)}} - \underbrace{\int_0^m \left[\frac{d\left(p^i_\theta p^s_\theta\right)}{dm} G_\theta + p^i_\theta p^s_\theta \frac{dG_\theta}{dm} \right] d\theta}_{\text{(iv) woodwork effect (costs)}} - \underbrace{p^i_m p^s_m G_m}_{\text{(v) newly eligible (costs)}}. \end{aligned} \tag{6}$$

"Society's willingness to pay" in the proposition text means that Equation (6) gives the willingness to pay of the aforementioned representative household for the utility change from the policy reform.

[39] Our representative household has marginal utility equal to the average marginal utility of society. Specifically, $E[u']$ denotes the average expected marginal utility across types and states of the world: $E[u'] := \int_0^m \left(p^i_\theta p^s_\theta (u^A_\theta)' + (1 - p^i_\theta p^s_\theta)(u^N_\theta)'\right) d\theta + \int_m^1 (u^N_\theta)' d\theta$.

The first and most important insights from Proposition 1 emerge from how it clarifies the roles of the mechanisms, information and stigma. The envelope theorem plays a key role in many sufficient-statistics equations of this form (Chetty, 2006; Kleven, 2021). The envelope theorem typically implies that behavioral responses only enter social welfare via their effect on the government budget, to first order, and private welfare gains from behavioral responses are zero. However, the envelope theorem hinges on household optimization. If woodwork effects come from already-eligible households who enroll because they now correctly perceive costs, there are first-order welfare consequences (term (i)). These households were not previously optimizing, so the envelope theorem does not apply. Notice, however, that Proposition 1 does *not* include first-order gains from woodwork effects driven by decreased stigma, since these newly enrolled individuals were just indifferent between enrolling and not enrolling.

Distinct from woodwork effects, term (ii) reflects that already-enrolled households do enjoy first-order welfare benefits when costs fall, unlike households at the margin of take-up who get no private gains. Term (ii) captures society's willingness to pay for reduced stigma among already-enrolled households.

In addition to these mechanisms, eligibility expansion mechanically benefits the newly eligible households who enroll (term (iii)). These households could not get benefits before the reform, and the social planner values raising their utility.

Finally, Proposition 1 explains the costs of expanding the eligibility threshold. First, the households who change behavior may impose a fiscal externality (term (iv)). These terms capture both the direct cost of providing the benefit to new households as well as other behavioral responses to the program. For instance, behavioral responses could include labor-supply reductions among those who become aware of the program. Notably, the cost of the woodwork effect depends on the total behavioral response ($d\left(p_\theta^i p_\theta^s G_\theta\right)$), and not the mechanisms. Second, the households who newly enroll because they are newly eligible impose a mechanical fiscal cost (term (v)).

Welfare Impact Without Woodwork Effects. To develop intuition and policy implications, we now study a social planner who is unaware of woodwork effects. This unaware social planner estimates dW/dm as if terms (i), (ii), and (iv) were zero. If there really are woodwork effects, is she underestimating the welfare impact of raising the threshold? The answer depends on whether or not the sum of terms (i), (ii), and (iv) is positive.

Corollary 1. *Let dW^u/dm be the unaware social planner's estimate of the welfare impact of raising the eligibility threshold — that is, the planner's estimate if she (perhaps incorrectly) assumes terms (i), (ii), and (iv) in Equation (6) are zero. Then the true welfare impact of raising the eligibility threshold exceeds the unaware estimate ($dW/dm >$*

dW^u/dm) if and only if

$$\underbrace{\int_0^m \frac{dp^i_\theta}{dm} p^s_\theta \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta\right] d\theta}_{\text{(i) woodwork effect (information channel)}} + \underbrace{\int_0^m \int_{c<u^A_\theta - u^N_\theta} p^i_\theta \frac{dH_\theta(c \mid m)}{dm} dc d\theta}_{\text{(ii) stigma reduction}} - \underbrace{\int_0^m \left[\frac{d(p^i_\theta p^s_\theta)}{dm} G_\theta + p^i_\theta p^s_\theta \frac{dG_\theta}{dm}\right] d\theta}_{\text{(iv) woodwork effect (costs)}} > 0. \tag{7}$$

Corollary 1 shows that woodwork effects are a motive to set a higher threshold under two conditions. First, if many already-eligible people misoptimize, then woodwork effects give them welfare (term (i)). One wrinkle is that the government has to pay for their take-up (term (iv)), so it also has to be the case that these people are sufficiently needy (i.e., have sufficiently high marginal utility) that the transfer is on net beneficial. Second, raising the threshold can reduce ordeal costs (term (ii)) among already enrolled households. On the other hand, if many of the newly enrolled people are just indifferent to enrolling, that reduces the welfare impact, since they impose a fiscal externality (term (iv)) without receiving any welfare gain. Other fiscal externalities from woodwork effects — e.g., via large labor-supply responses among newly informed households — would further reduce the motive to set a higher threshold.

Connecting Woodwork Effects to Policy. A key takeaway from the model is that the existence of woodwork effects alone does not imply that expanding eligibility is socially desirable or undesirable. Rather, isolating whether the woodwork effects come from stigma or information, and for which households, matters for welfare conclusions. For instance, if the woodwork effect reflects increased awareness, big woodwork effects imply big welfare gains. However, if the woodwork effect reflects changing stigma costs, this implication no longer goes through. If changing the threshold affects stigma costs only for households at the margin of taking up ($u^A_\theta - c_\theta \approx u^N_\theta$), that would yield a big woodwork effect but a small social net benefit. If changing the threshold affects stigma costs only for households who already enroll ($u^A_\theta - c \gg u^N_\theta$), that would yield a small woodwork effect but a big social net benefit. Thus, the existence or magnitude of woodwork effects is not a sufficient statistic for welfare or policy recommendations.

When is the existence of woodwork effects sufficient to conclude that the threshold should rise? A natural case, formalized in Appendix E.2, is when woodwork effects only come from the information channel.

4.3 Discussion

Connection to Finkelstein and Notowidigdo (2019). Like Finkelstein and Notowidigdo (2019), we stress the distinction between utility-maximizing non-participation (driven by ordeals) versus non-participation that results from household optimization mistakes (driven by information). They focus on the targeting properties of the ordeals and misperceptions directly. We focus on a reform's perhaps unintended ordeal and information spillovers on a population the reform itself does not directly affect. While similar insights appear in their model, we particularly stress the first-order effect of the policy reform on already-enrolled households' costs. Finally, since we study a different policy reform, our model includes welfare considerations not included in Finkelstein and Notowidigdo (2019), like the welfare gains and costs from the marginal types' enrollment.

Targeting. Proposition 1 also shows how targeting of woodwork effects affects the welfare impacts of the policy reform. In the language of Allcott et al. (2019), there are two ways for the reform to be well-targeted. First, it can have a "corrective motive," meaning woodwork effects correct households who were making mistakes. Second, it can have a "redistributive motive," meaning woodwork effects come from households who are poor. Absent mistakes and excluding the role of stigma, woodwork effects are harmful for social welfare because of the envelope logic above. With nonzero mistakes, the social gains from the information channel are larger when lack of information ($1 - p^i$) or the woodwork effect due to changes in information (dp^i) positively covaries with need (term (i)). Relatedly, the social costs from the woodwork effect are larger when the woodwork effect positively covaries with benefit size (term (iv)).[40] Altogether, even if less-needy households enroll due to woodwork effects, that does not necessarily imply that eligibility expansion is "poorly targeted" altogether, as long as they are needier than average and making mistakes.

The proposition also shows how the new enrollment driven by newly eligible enrollees ($\theta = m$) has ambiguous implications for welfare. On the one hand, newly eligible enrollees are less needy. On the other hand, they all capture the full benefit from the program; that is, none of them are subject to an envelope theorem. Whether these new enrollees are net positive or negative depends on whether the social value of providing them with the benefit exceeds the social cost of provision.

Stigma Interventions. Like in Finkelstein and Notowidigdo (2019), Equation (6) clarifies the welfare impacts of stigma interventions in general, beyond this particular policy reform. In this and many models, stigma is like any other ordeal. Consequently, due to the envelope theorem, policies that reduce stigma and raise take-up have no first-order welfare impact on households whose behavior changes. The policies actually reduce welfare because those households still have a fiscal cost.

Destigmatization interventions raise welfare only in two main cases. First, if households who always enroll are willing to pay for the stigma reduction. Second, if households are making mistakes, in which case there are first-order gains from correcting those mistakes.

The take-up response to a destigmatization intervention is not a sufficient statistic for welfare. In fact, large take-up responses can easily imply the intervention reduces welfare.

Modeling of Information Frictions. We model information frictions as awareness of the program. Unaware households will not enroll in the program no matter how high the utility benefits are. When she becomes aware and enrolls, she captures the full net utility benefit of the program. Different models of information give different welfare implications. For example, suppose information frictions are instead cast as noisy

[40] Term (ii) also embeds a subtler targeting force, which captures the extent to which stigma reductions are concentrated among those who are aware of the program (that is, those with high p^i_θ).

misperceptions of benefit size, as in Finkelstein and Notowidigdo (2019). Then the utility gain to the previously eligible households who newly enroll is bounded above in relation to the size of the misperception. Our model is a useful benchmark, but it may imply that we present an upper bound on the welfare gains from woodwork effects. Better empirical estimates and richer models of misperceptions about welfare programs would improve welfare calculations.

5 Empirical Model Calibration

Our model provides a framework for quantifying welfare effects of eligibility expansions. To calibrate the model, we use data on take-up rates, the SNAP benefit schedule, the income distribution, awareness of SNAP, and the empirical woodwork effect. We then estimate the social value of a marginal increase in the current SNAP eligibility threshold. This exercise requires strong assumptions, so we view the analysis as illustrative and highlight where more data would be useful.

5.1 Calibration

For calibration, we restrict certain forms of heterogeneity, use parametric expressions for utility and ordeal costs, and use external estimates of other parameters from the literature. We discuss the main steps here, leaving details to Appendix D.

Much of the inputs to Equation (6) can be read from the QC/CPS data and estimates of the woodwork effect described in Section 2. We operationalize our calibrations by forming 100 types θ, representing ranks of the income distribution. For each type, we observe type-specific take-up probabilities and average benefit sizes. We assume the eligibility threshold is $m = 0.27$ from the 2016 CPS, so the 28th rank is at the margin.

We develop a specialized version of Equation (6) expressed in terms of woodwork-effect elasticities. We make two important restrictions. First, we impose a constant woodwork-effect elasticity (elasticity $= 0.12$, Table 3B), rather than unrestricted woodwork effect derivatives. We relax this constant-elasticity assumption in sensitivity analyses. Second, we assume all fiscal costs operate through take-up responses. We thus abstract from how providing SNAP could induce other behavioral responses, like labor supply changes, that affect the government budget. Positing small labor-supply responses is empirically plausible (Gray et al., 2023). Then, fiscal costs are given by take-up rates, SNAP benefits and a measure of the administrative costs of SNAP from Isaacs (2008).

To calibrate take-up costs, we impose that ordeal costs are given by an intercept ($75, Finkelstein and Notowidigdo, 2019) and a mean-zero logit error. We calibrate the logit scale parameter by matching empirical take-up probabilities, which then implies a distribution of costs conditional on enrollment.

To calibrate the role of stigma versus information, we use empirical take-up probabilities and data on SNAP awareness from the FSPAS. We also need to determine how much of the woodwork effect comes from stigma. We assume that one third of the woodwork effect elasticity comes from stigma, and two thirds come from information, to match the FSPAS finding that information drives woodwork effects.

To calibrate utility, we posit a constant relative risk aversion utility function over income and benefits, and we assume the household values SNAP benefits the same way as money. We impose a consumption floor (here, income floor) of $2,000 since CRRA utility is sensitive to very low values (Finkelstein et al., 2019). To calibrate the risk aversion parameter, we use an "inverse-optimum" approach (Bourguignon and Spadaro, 2012; Hendren, 2020). We assume that the social planner sets the eligibility threshold optimally but mistakenly believes there is no impact on those already eligible. That is, we select the risk aversion parameter which sets terms (iii) plus (v) in Equation (6) equal to zero (see Corollary 1 and related discussion). This exercise delivers the sensible result that risk aversion $\gamma \approx 2.5$, within the range of 1 to 4 often used in public finance (Chetty and Finkelstein, 2013).

The inverse-optimum approach is a principled way to select a risk aversion parameter. But the exercise still imposes an assumption about what the planner optimizes, so we also show sensitivity to γ.

Calibrating the welfare impact of stigma reductions (term (ii) in Equation (6)) is challenging. We conservatively assume a low willingness to pay to avoid stigma. Suppose that woodwork effects came entirely from stigma. Under this case, we posit that already-eligible enrollees would gain 2% of the net utility from enrolling in SNAP if the program were made available to 10 percentage points more of the population. Since we assume woodwork effects primarily come from information, we adjust this 2% figure by multiplying by the one-third of woodwork effects we attribute to stigma. As we have great uncertainty about these calibration choices, we show sensitivity to the magnitude of term (ii).

To convert the thought experiment in Proposition 1 to the present empirical context, we view the gross income threshold as the eligibility threshold m. Other barriers to enrollment, e.g. the net income threshold, do not change. In principle, we can think of those other tests as differentially binding ordeal costs across household type. In practice, we calibrate the same mean ordeal cost across households. We leave better modeling of multiple eligibility requirements to future work.

5.2 Results: Welfare Impact of Eligibility Increases

We study the welfare impact of a policy reform that makes one additional household, at the margin of eligibility, newly eligible (Table 5). As we scale utility by the average marginal utility in society, each entry in the table is a money-metric corresponding to the same welfare concept as in Section 4.[41] We split up the costs and benefits of the reform by whether they come from marginals or already-eligibles. We also distinguish the social benefits of the reform that owe to already-eligible households' stigma reductions (term (ii), Equation (6)).

[41] Each entry is society's willingness to pay (a representative household's *ex ante* willingness to pay) to make one household at the threshold eligible.

The inverse-optimum approach equates the social costs and benefits of the reform from households at the eligiblity threshold. That is, we choose the risk aversion parameter so that Column 1 equals Column 2, and Column 6 is 0.

Our first conclusion is that under our baseline assumptions, there would be considerable welfare gains from increasing the eligibility threshold (Row 1). Making one more household eligible would confer \$697 in social welfare. This gain includes the costs of providing SNAP, and due to the inverse-optimum assumption, comes entirely from net social benefits from already-eligible households. The \$697 in net social welfare is also sizeable when compared with the \$67 in mechanical fiscal costs to households at the eligibility threshold.

The magnitudes are plausible. Focus on the costs. Recall our back-of-the-envelope calculation that for every person who joined the program due to newly becoming eligible, almost one already-eligible households joined out of the woodwork. Column 5, the already-eligible households' total cost, is about 3.5 times larger than Column 2, the marginals' total cost. That is because one already-eligible household joins out of the woodwork for every marginal who joins, and their benefits are about 4 times larger than marginals'.

Why are the benefits large relative to the costs? Our calibration suggests that average marginal utility in society $(E[u'])$ is approximately the marginal utility of the 15th-percentile household, so society benefits from redistributing to many already-eligibles. Compared with the average benefit size of roughly \$3,000 at the bottom, the social gains from redistribution are of reasonable magnitudes.

As another way of interpreting Table 5, one can read off a welfare-weighted Marginal Value of Public Funds ("Welfare Impact") by calculating ratios of social benefits and social costs. The Welfare Impact of the reform is (Columns 1 + 3 + 4) $\div$ (Columns 2 + 5). Our primary estimate is that the Welfare Impact of the reform is about 3.3 overall if we include all welfare effects in the calculation. These moderate welfare-weighted MVPFs further suggest that the social benefits, while large, are plausible. We caution that these Welfare Impacts are not comparable to MVPFs in the SNAP literature, as we take an explicit stance on redistribution when we compare willingness to pays across households with different incomes.

To study what drives these results, and probe sensitivity, we vary inputs into our calibration. To start, when we halve or double the woodwork effect elasticity (Rows 2 and 3), we find that this change proportionately rescales Columns 3 and 5, the welfare benefits and costs from already-eligibles. In this case, as the social benefits of already-eligibles' enrollment exceed the social cost, smaller woodwork effect elasticities would reduce the welfare impact of the reform.

We next permit a heterogeneous woodwork effect elasticity, to incorporate the observed finding that higher-income households are more responsive to the threshold (Figure A7, see Table 5 notes for implementation details). Such heterogeneity reduces the welfare gains by 42% (Row 4 vs. Row 1). Given the large standard errors on woodwork effects by income group, we start with the constant-elasticity assumption as a benchmark. But this assumption is probably not conservative for our main conclusion.

We proceed by examining how mechanisms drive the welfare effects (Rows 5–7). If woodwork effects exclusively owe to information, then households who join capture large gains but there is no stigma reduction (Row 5, Column 4). If woodwork effects exclusively owe to stigma, then already-eligibles who join capture nothing in welfare (Row 7, Column 3), but already-eligibles who stay on the program now get stigma reductions. In this calibration, we find that reduced stigma (Column 4) is less important than welfare gains from enrollment among people who made mistakes (Column 3). Some of these conclusions hinge on our assumption that willingness to pay to avoid stigma is low, that is, Column 4 is moderate in magnitude. However, if Column 4 were large in magnitude, that would only amplify our primary result that raising the threshold generates welfare.

The magnitudes of the welfare analysis are sensitive to calibration assumptions. When we shrink or amplify the social benefits from stigma reductions, the welfare gains shift somewhat, from \$610 to \$873 (Rows 8 and 9). With half the risk aversion parameter implied by the inverse-optimum assumption (Row 10), the welfare impact of the policy reform falls to \$365. Imposing uniform ordeal costs, rather than logit, means the reform would have much smaller net effects on welfare (Row 16, see table notes for details). Uniform costs are an extreme assumption, as they imply that the average ordeal cost conditional on enrollment erodes half the net welfare gains. Nevertheless, the welfare conclusions' sensitivity to calibration choices suggests caution.

6 Conclusion

We document the existence of woodwork effects in SNAP. We find that the woodwork effects arise from increased information after states relax eligibility thresholds, and our online experiment also finds that relaxing eligibility thresholds reduces stigma. We develop a general model for studying the welfare impact of raising program eligibility. We apply our model to SNAP and find that, granting several parametric and calibration assumptions, raising the threshold likely increases welfare.

All social programs, even universal ones, make some determination about eligibility. This threshold is often chosen by the planner and thus is not an exogenous feature of the policy environment. As a result, our normative insights have applications in many areas in public economics. An especially important result is that the presence of woodwork effects alone is insufficient to conclude that raising the eligibility threshold would increase welfare. Rather, the mechanisms — behavioral biases versus classical ordeal reductions — are often crucial.

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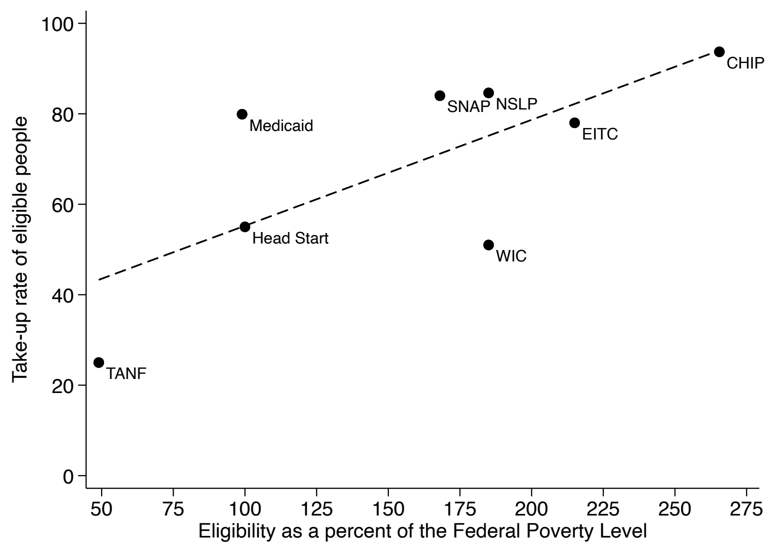
7 Figures

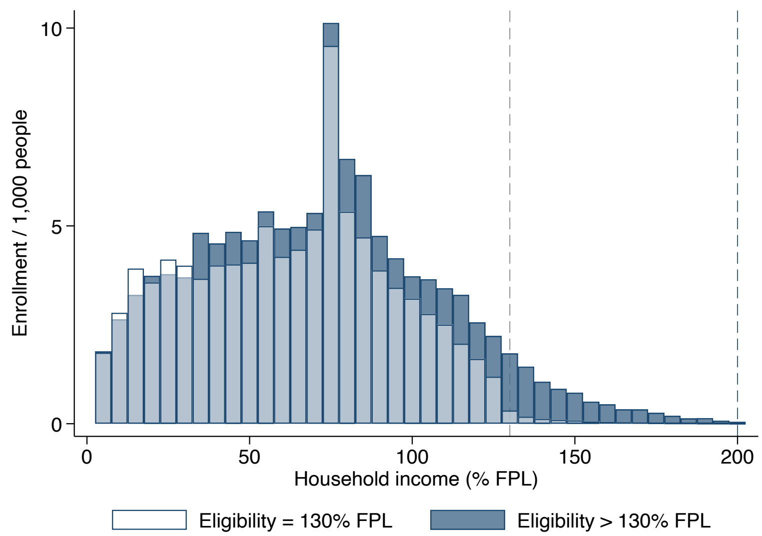
Figure 1: Eligibility Thresholds and Program Take-Up



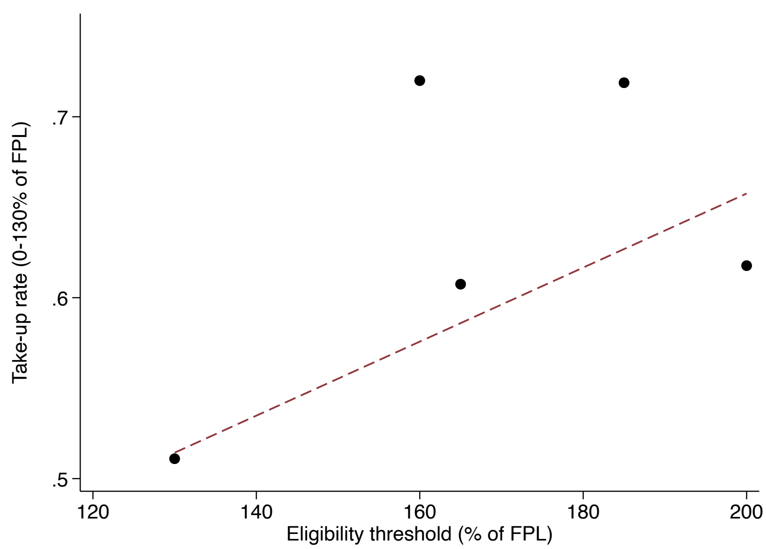
The figure shows income eligibility thresholds as a percent of the Federal Poverty Level (FPL) for the largest U.S. means-tested social programs against estimates of their national take-up rates, compiled from different sources. Take-up rates are estimated out of the eligible population for each program. Where the eligibility threshold is defined in dollars (e.g., EITC, TANF), the figure shows the threshold as in terms of percent of the FPL for a family of three. Some programs (e.g., WIC, TANF) are restricted to certain subgroups in addition to imposing income thresholds — for example, families only — or have additional requirements. See Appendix A for additional information on constructing these data. The estimates for TANF and Head Start require particular caution, so the appendix shows a best-fit line excluding them.

Figure 2: Descriptive Evidence of Higher Enrollment Among Already-Eligible Households with Expanded Eligibility

(A) Enrollment by Household Income and Eligibility Threshold

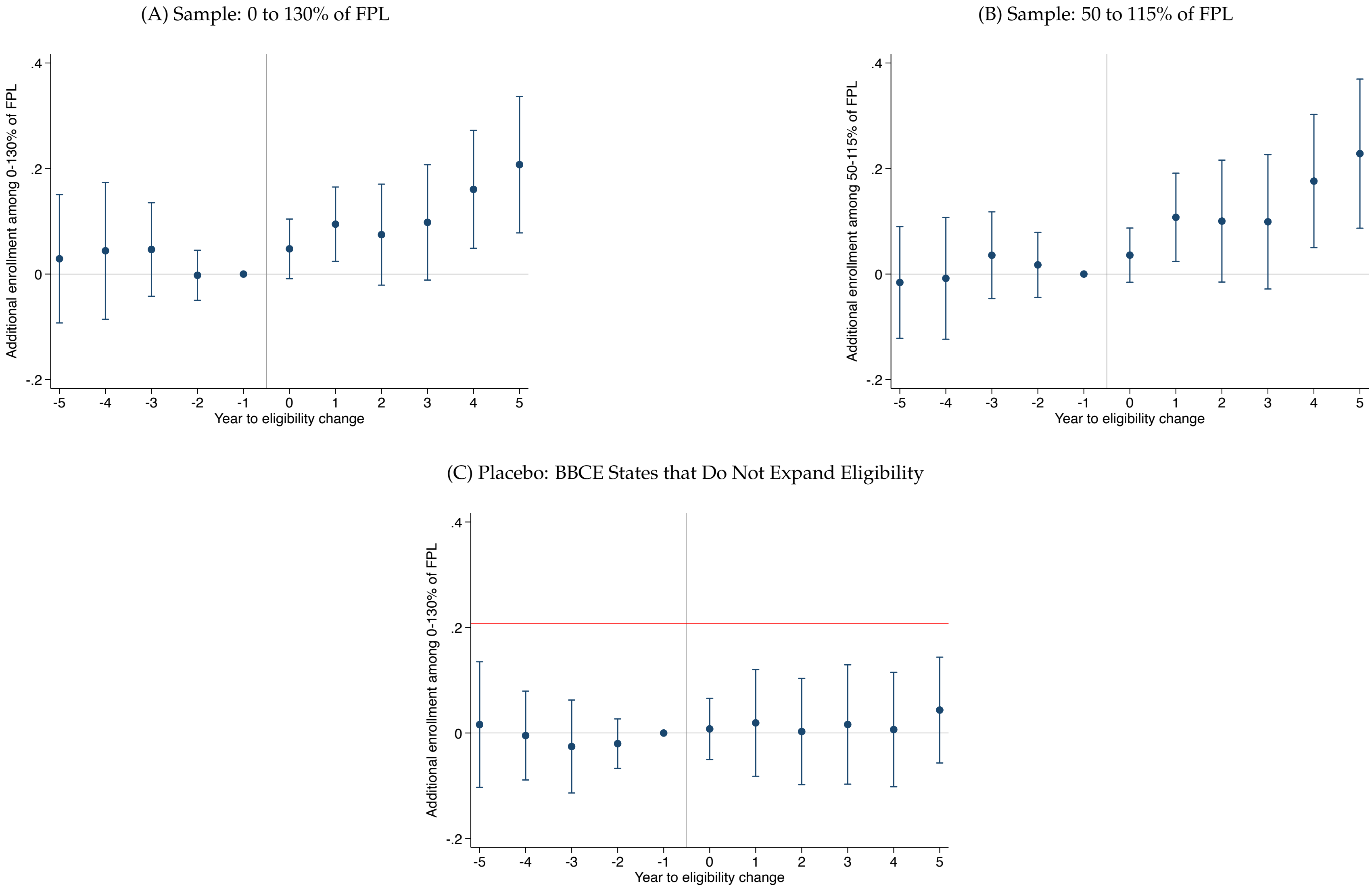


(B) Take-up by Eligibility Threshold



This figure presents the relationship between the eligibility threshold and SNAP take-up and enrollment. Panel A shows SNAP enrollment per 1,000 people in states and years where the eligibility threshold is 130% of the Federal Poverty Level (FPL) versus above 130% (and up to 200%). Each bar takes the number of people in the USDA Quality Control (QC) data whose household income is in each income bin, divided by the total population (i.e., all people, with any household income) in all state-years with the indicated eligibility regime. The data are limited to the sample we use in the main event study, and household income is top-coded at 200% FPL in the USDA QC data. Panel B shows average take-up among those earning 0–130% of the FPL in states with each eligibility threshold observed in the data. The USDA Quality Control data provide estimates of the numerator for the outcome (take-up counts, by state-year), and the Current Population Survey data provide estimates of the denominator (total counts of individuals within this sample).

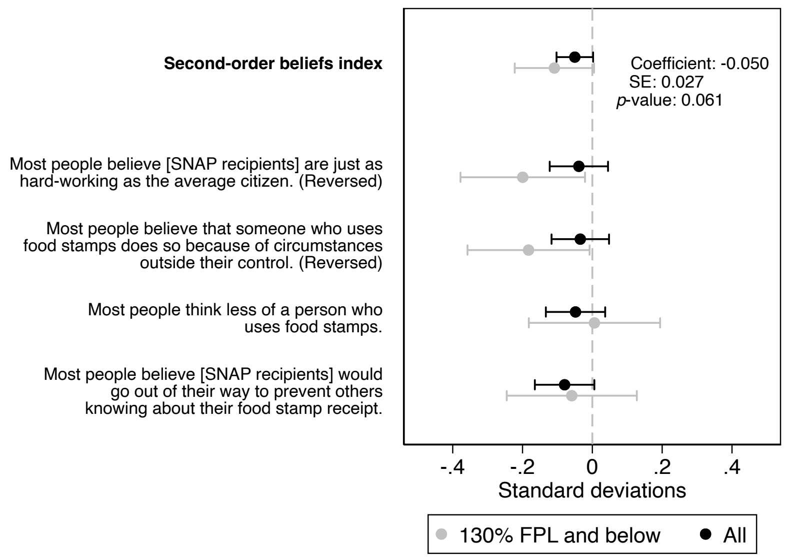
Figure 3: Event Study of Changes to Eligibility Threshold



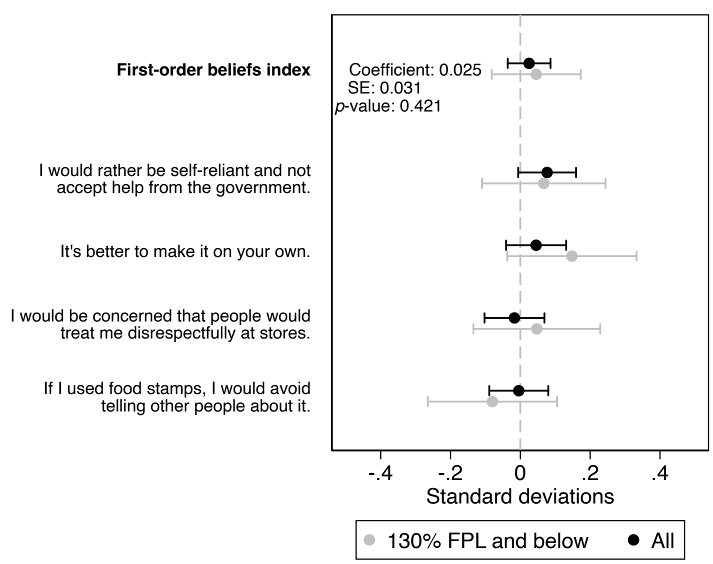
This figure presents the event-study estimate of η (Equation (1)), the effect of the eligibility rate on already-eligible individuals' take-up. Panel A presents results for the sample of individuals from 0–130% of the Federal Poverty Level (FPL); Panel B presents results for 50–115% of the FPL. Panel C presents a placebo event study, using the nine states that adopt the Broad Based Categorical Eligibility policy but do not expand eligibility (see Section 2). The red line in Panel C plots the 5-year point estimate from Panel A. The minimum eligibility in all states is 130% of the FPL. Standard errors are robust to heteroskedasticity and clustered by state. Bars show 95% confidence intervals.

Figure 4: Effect of High-Share Treatment on Stigma

(A) Second-order beliefs



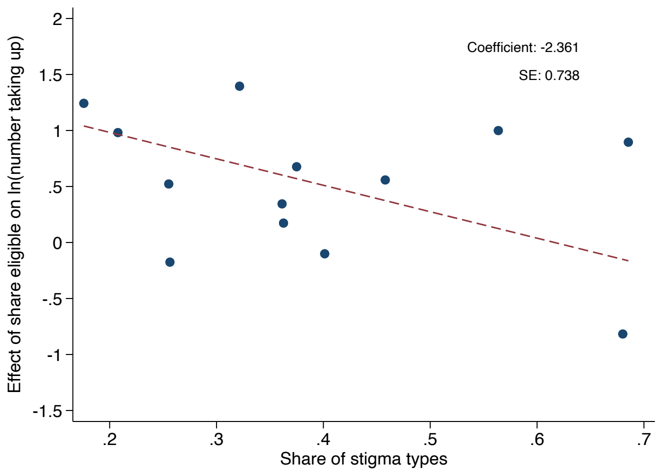
(B) First-order beliefs



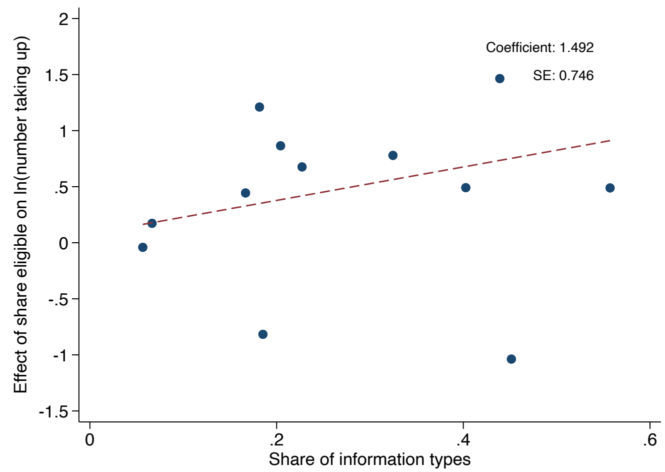
This figure presents results from the online experiment. It shows the effect of the "high-share" treatment (where respondents were randomly given a hint that *increased* their reported beliefs about the share of Americans who are eligible for SNAP) on agreement with each statement in the stigma instrument (Equation (4)). Outcomes marked with "(Reversed)" were reverse-coded so that for all items, a higher score indicates more stigma. The coefficients correspond to a reduced-form (intent-to-treat) estimate and do not account for the amount by which the treatment moved people's beliefs about the share of Americans who are eligible for SNAP. Each outcome is in units of standard deviations, and the indices average the set of outcomes displayed in each panel. Bars plot 95% confidence intervals.

Figure 5: Woodwork Effects Heterogeneity by Demographic Cell

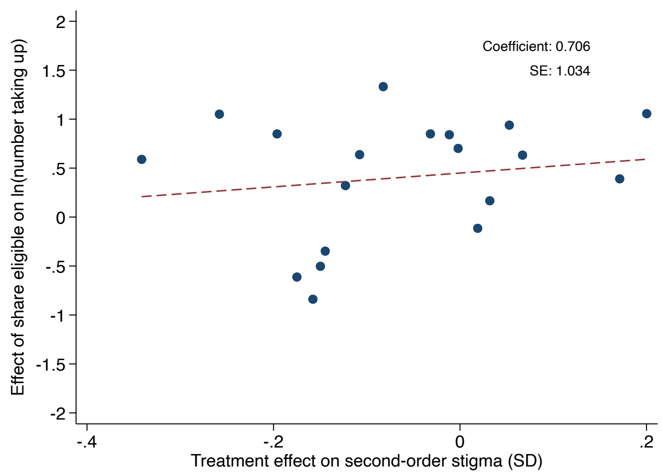
(A) Share Stigma Type vs. Woodwork Effects



(B) Share Information Type vs. Woodwork Effects



(C) Experiment Treatment Effect vs. Woodwork Effects



Panels A and B show binned scatter plots of the correlation between the subgroup-specific woodwork effects with the share of respondents in the USDA FSPAS survey who reported (A) any stigma and (B) less than complete information. Panel C shows the correlation between the subgroup-specific woodwork effects and the subgroup-specific treatment effect in the online experiment. Subgroups are defined by household head age bin, gender, and race/ethnicity (non-Hispanic white vs. other), as well as by their household composition and income decile in the national distribution. Estimates are weighted by the inverse of the product of the variances of the cell-level coefficients; see Appendix C for details.

8 Tables

Table 1: Estimates of the Woodwork Effect

	(1) Main estimate	(2) Extra controls	(3) Waivers, lag unemp.	(4) Excludes recession	(5) Weighted	(6) Avg of coefficients	(7) All data
Panel A. 0–130% FPL							
Income limit (% FPL) / 100	0.093	0.095*	0.079	0.095	0.080	0.084	0.090*
	(0.057)	(0.056)	(0.055)	(0.060)	(0.072)	(0.064)	(0.049)
Panel B. 50–115% FPL							
Income limit (% FPL) / 100	0.120**	0.124**	0.106**	0.129**	0.114*	0.117**	0.124**
	(0.053)	(0.056)	(0.051)	(0.055)	(0.064)	(0.058)	(0.048)
Observations	705	705	680	628	705	705	1071
N states	45	45	45	45	45	45	51

This table shows the effect of the eligibility threshold on log enrollment among the already-eligible population (0–130% FPL in Panel A and 50–115% FPL in Panel B). Column 1 estimates Equation (2), and the following columns present various extensions to show robustness. Column 2 separates the Ganong-Liebman policy index into separate indicators. Column 3 includes a control for the previous year's unemployment rate in each state and a control for the population-weighted average number of months a state had ABAWD work requirement waivers in effect. Column 4 excludes years 2008–2011, during the Great Recession. Column 5 weights observations by the state-year population. Column 6 presents the difference between the average pre- and post-period event study coefficients. Finally, Column 7 estimates Equation (2) using all the data available instead of only the event study sample. Standard errors are robust to heteroskedasticity and clustered by state.*, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table 2: Effects on Demographic Composition (50–115% FPL)

	(1) Female	(2) Black	(3) Age	(4) Has child	(5) Avg net income	(6) % FPL	(7) Certification $\leq$ 6 mo.
Income limit (% FPL) / 100	-0.002	0.058	0.312	0.000	-27.577	0.735**	0.020
	(0.004)	(0.064)	(0.434)	(0.010)	(20.361)	(0.307)	(0.107)
Baseline mean	0.59	0.22	28.94	0.71	817.41	79.62	0.40
Observations	705	705	705	705	705	705	705
R^2	0.70	0.81	0.85	0.84	0.89	0.70	0.67

This table presents results from estimating the effect of the SNAP eligibility threshold on the composition of enrollees earning 50–115% FPL. The columns present estimates of Equation (2) with the indicated outcome variable: Column 1 shows the effect of the eligibility threshold on the fraction of the 50–115% FPL enrollee sample who are female, and so on. The independent variable is the eligibility threshold as a ratio of the Federal Poverty Level, so that increasing by 1 corresponds to increasing the eligibility threshold from, e.g., 130% FPL to 230% FPL. "Baseline mean" refers to the average of the outcome indicated by the column in state-years where the eligibility threshold is 130% FPL. Outcomes are calculated using the USDA's Quality Control (QC) data, limiting the data to households earning 50-115% FPL. Standard errors are robust to heteroskedasticity and clustered by state. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table 3: Estimates of the Take-up Elasticity with Respect to Eligibility Threshold

	OLS	IV		
		First Stage	Reduced Form	2SLS
Panel A. All data				
ln(Share eligible)	-0.104*			0.130*
	(0.060)			(0.067)
Income limit (% FPL) / 100		0.728***	0.095*	
		(0.034)	(0.048)	
Observations	1071	1071	1071	1071
Panel B. Event study sample				
ln(Share eligible)	-0.144**			0.121
	(0.068)			(0.079)
Income limit (% FPL) / 100		0.752***	0.091	
		(0.038)	(0.059)	
Observations	705	705	705	705

This table presents estimation results for η, the elasticity of take-up with respect to the share of the population who are eligible, controlling for the covariates included in Equation (3). We estimate this elasticity using the eligibility threshold as an instrument for the share of residents in a state who are eligible for SNAP. The first column shows results from a naïve OLS regression of ln(take-up), among the 0–130% FPL population, on ln(share eligible). The second column presents the first stage — the coefficient from a regression of ln(share eligible) on the eligibility threshold as a % of the Federal Poverty Level (FPL). The third column, the reduced form, gives the relationship between the eligibility threshold and ln(take-up). The final column gives the 2SLS estimate, our final estimate for η. Standard errors are robust to heteroskedasticity and clustered by state. ** and *** indicate $p < 0.05$ and 0.01, respectively.

Table 4: Cost-Effectiveness Calculation

	Eligibility threshold	**Benefit size**
Change required for 1 pp take-up increase	4.3 pp	\$56 per person-year
1. Number of people affected	13.9 million (newly eligible)	44.2 million (enrolled)
2. Take-up among affected people	36%	100%
3. Cost per person-year	\$707	\$56
4. **Mechanical cost of intervention** (= Row 1 × Row 2 × Row 3)	\$3.6 billion	\$2.5 billion

This table shows the cost-effectiveness of increasing take-up by raising the means test versus by increasing the benefit size. Let m be the share of the U.S. population eligible for SNAP, B be the benefit size per person, and p be the take-up probability. The top row shows the required change in the instrument (m or B) to achieve a one percentage-point increase in take-up. We calculate this row by noting that $\eta = \frac{dp}{dm}\frac{m}{p}$ and rearranging to solve for dm when $dp = 0.01$ (and likewise for B). The remaining rows show the mechanical cost to the program (without including the costs incurred by woodwork effects) of changing these instruments. The number of people enrolled in 2016 (44.2 million) was found in Oliveira (2017). When using the means test m to increase take-up, 13.9 million more people become eligible, but we estimate only 36% of those would take-up. When using the benefit size B, benefits are increased for all program participants. (Note that this value differs from the value in Appendix Figure A7 because the appendix figure presents an unweighted average of state take-up rates.) The cost per person uses averages from the QC data. The final row of the table shows the total mechanical cost for each policy tool, which multiplies rows 1-3.

Table 5: Welfare Effects of Woodwork Effects: Calibration

	At Threshold		Already Eligible			Social Welfare		
	Social benefits (1)	Social costs (2)	Social benefits from take-up (3)	Social benefits from stigma reduction (4)	Social costs (5)	At threshold (= 1 - 2) (6)	Already eligible (= 3 + 4 - 5) (7)	Overall (= 6 + 7) (8)
1. Primary	**66.7**	**66.7**	**753**	**176**	**232**	**0**	**697**	**697**
2. Half woodwork elasticity	66.7	66.7	377	176	116	0	437	437
3. Double woodwork elasticity	66.7	66.7	1,507	176	463	0	1,219	1,219
4. Heterogeneous woodwork elasticity	66.7	66.7	527	176	298	0	404	404
5. No woodwork from stigma	66.7	66.7	1,130	0	232	0	898	898
6. Double woodwork from stigma	66.7	66.7	377	352	232	0	497	497
7. All woodwork from stigma	66.7	66.7	0	527	232	0	296	296
8. Half WTP for stigma change	66.7	66.7	753	87.9	232	0	610	610
9. Double WTP for stigma change	66.7	66.7	753	352	232	0	873	873
10. Half risk aversion	196	66.7	379	88.4	232	129	236	365
11. Double risk aversion	0.89	66.7	749	175	232	-65.8	692	626
12. Half income floor	28.5	66.7	737	172	232	-38.2	678	639
13. Double income floor	119	66.7	594	139	232	52.5	501	553
14. Half hassle costs	75.6	66.7	764	178	232	8.8	711	719
15. Double hassle costs	49.6	66.7	733	171	232	-17.2	672	655
16. Uniform idiosyncratic costs	4.6	66.7	346	80.8	232	-62.1	195	133

The table presents the social welfare from a policy reform that makes one household, at the margin of eligibility, newly eligible for SNAP. Each entry is in units of $2016.

- In our primary estimate, the woodwork effect elasticity is 0.12 (Table 3), and 2/3 of this elasticity comes from information rather than stigma (see Appendix D). We use an inverse-optimum approach that equates Columns 1 and 2 in the primary specification, by setting risk aversion parameter $\gamma \approx 2.5$.
- Row 1 gives results from our primary specification. Columns 1 and 2 show the social benefits and costs from households at the threshold (terms (iii) and (v) in Equation (6)), whereas Columns 3, 4, and 5 show the social benefits and costs from households who are already eligible (terms (i), (ii), and (iv) in Equation (6)). Subsequent rows vary parameters or assumptions in isolation.
- Rows 2 and 3 halve and double the woodwork elasticity from the primary estimate. In Row 4, we let the woodwork effect elasticity vary by income. We assume that the elasticity is half as large as the primary elasticity for quantiles 1–9, equal to the primary elasticity for quantiles 10–18, and 10 times as large as the primary elasiticity for quantiles 19 and above.
- In Rows 5–7, we adjust the parameter that governs how much of the woodwork effect elasticity comes from stigma versus information. In Row 5, all the woodwork effect comes from information. In Row 6, 2/3 of the woodwork effect comes from stigma. In Row 7, all the woodwork effect comes from stigma.
- Rows 8–9 halve and double our primary estimate of society's willingness to pay for the change in stigma, which is term (ii) in Equation (6). We operationalize this by doubling or halving the parameter ψ in Equation (D.6).
- Rows 12 and 13 halve or double the income floor from $2,000 per individual in the household. Rows 14 and 15 halve and double the hassle cost from our primary estimate of $75.
- In Row 16, we assume that stigma costs are drawn from a uniform distribution. Then stigma costs conditional on enrollment always erode half the net welfare gain.

Online Appendix: Not Intended for Publication

A Data and Institutional Context

A.1 SNAP Sample Construction

We build off the sample in Ganong and Liebman (2018), and adapt their public-use code and data associated with the published paper. We extend the sample to 2016. Our main outcome (the number of people enrolled in SNAP, for different income groups) uses the USDA's Quality Control (QC) data from 1996–2016. The QC data provides information on the household's income as a fraction of the FPL. We use the QC data (together with its household weights) to obtain counts of the number of people in a given state-year that enroll in SNAP who are within some income band (as a fraction of the FPL).

In our welfare exercise and in some supplemental analyses, we are interested in SNAP take-up *rates*. For these, we treat the QC data as the numerator in the take-up rate, and form the denominator from the CPS, which contains the count of people within a household income band in each state and year.

Our data on state-level SNAP policies, including the income eligibility threshold and other policies (e.g., outreach spending), come from the USDA's SNAP Policy Database (2019).

The QC data include individuals in the household who are not in the SNAP unit. As in Ganong and Liebman (2018), we include these individuals as taking up SNAP. Many of these individuals are relatives of the individuals in the SNAP unit and may, in practice, have their consumption subsidized by SNAP. Results are very similar if we limit only to individuals in the SNAP unit.

A.2 Broad Based Categorical Eligibility

We provide more information about the BBCE provision that permits states to expand SNAP eligibility.

Broad Based Categorical Eligibility permits states to expand eligibility using Temporary Assistance for Needy Families (TANF) or State Maintenance of Effort (MOE) budgets. States cannot expand eligibility beyond 200% of the FPL.

There are two concerns about other effects of the BBCE that could affect our analysis of woodwork effects. In practice, states are legally required to fund small auxiliary services (e.g., telephone hotlines) using TANF/MOE funds in order to grant eligibility to more people in SNAP. Congressional Research Service (2019) writes:

> "As of July 2019, 42 jurisdictions have implemented what the U.S. Department of Agriculture (USDA) has called "broad-based" categorical eligibility. These jurisdictions generally make all households with incomes below a state-determined income threshold eligible for SNAP. States do this by providing households with a low-cost TANF-funded benefit or service such as a brochure or referral to a telephone hotline. There are varying income eligibility thresholds within states that convey "broad-based" categorical eligibility, though no state may have a gross income limit above 200% of the federal poverty guidelines."

The first concern, which we address in Section 2.5, is that this policy requires that SNAP administrators must notify households that they are eligible. In practice, the policy discussion around BBCE centers around the eligibility expansion, and the notification of receipt may not be much different than typical state efforts to notify recipients, especially for households below 115% of the FPL. The core of our robustness tests uses states that are treated with BBCE but do not expand eligibility. We find no evidence take-up increases in these states.

A secondary concern is that BBCE expansions sometimes waive asset rules. We also address this concern in Section B.3.

A.3 Components of SNAP Policy Index

We use the SNAP policy index defined in Ganong and Liebman (2018), but without the BBCE. It is the average of dummies for each of seven policies. Six policies are directly from the SNAP Policy Database (2019). These are defined to be 1 if at least some parts of the state use the policy:

- At least one household vehicle is exempted from the asset test.

- Households with at least one recipient of Supplemental Security Income can use a simplified application for SNAP.
- Households can recertify with a telephone interview instead of a face-to-face interview.
- Households can apply to SNAP online.
- The state has fewer requirements for reporting changes in household earnings.
- There are call centers in the state for households to ask questions about SNAP, and in some places, recertify.

The final policy is a dummy if fewer than 20% of households have a certification period of 3 months or less, indicating that only a low share of SNAP households in the state must recertify at frequent intervals.

The index averages all seven policies except for when information about vehicle exemptions is unavailable; in this case, we average the remaining six.

In cases in which the index varies throughout the year, we use the minimum of the index in that year.

A.4 Experiment Sample Construction

We document several data cleaning decisions.

- A small number of participants had missing information about their household size or composition. We assume people with missing information were single, non-married, with no children (so had a household size of 1).
- A small number of participants had missing income. We assume they were in the bottom income bin and therefore had an income of $7,500.
- We top-coded household size at 6 because the most number of children that participants could report was 4.
- Incomes were top-coded at $250,000. We assume these participants had incomes of $300,000.
- Fewer than five participants took the experiment multiple times, and we drop them.
- **Attention checks.** The attention checks are the following. First, before treatment, we tell people: *"In this survey, we will ask you about your beliefs and attitudes about the Supplemental Nutrition Assistance Program (SNAP), also known as food stamps."* After eliciting the preferred charity (the incentive), we ask: *"What does SNAP stand for?"*. There are four multiple choice responses: *"Sufficiently Noisy Animal Parties"*; *"Supplementary Names Artful Program"*; *"Supplemental Nutrition Assistance Program"*; *"Salty Noodles And Pasta."* We drop the 106 participants who answer the acronym question incorrectly. Second, we drop the 145 participants who report that either 0 or 100% of people in the U.S. are eligible for SNAP.
- **Below 130% FPL Sample.** To form the already-eligible sample of experiment respondents, we predicted the relevant 2020 poverty threshold for each respondent using (1) the midpoint of their household income bin and (2) their household size, constructed via their marital status and number of kids. Anyone who reported a household income bin with a midpoint below 1.3 $\times$ the result is included in the sample of respondents under 130% FPL. This may have excluded some respondents from the already-eligible sample if they were also living with or supporting parents or elders.

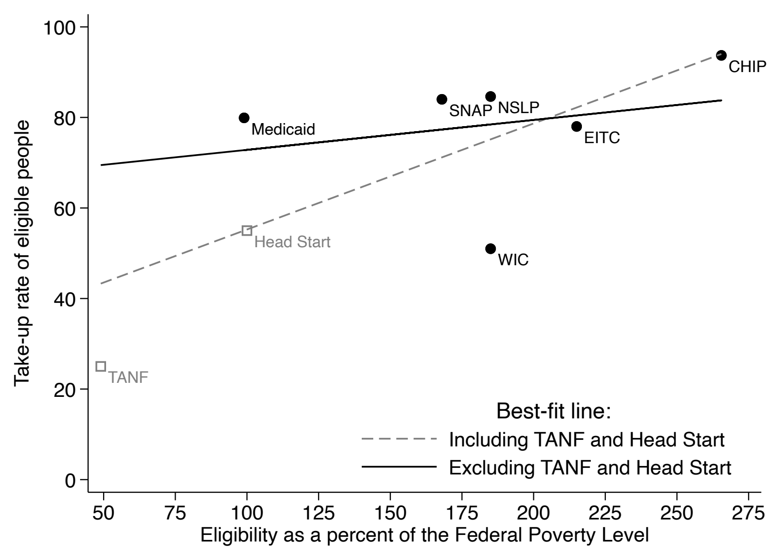
A.5 Figure 1 Details

We collected income eligibility rules and take-up rates from various sources for a subset of U.S. social programs. To the extent possible, all values are from 2016. The set of programs was determined by the following process: We began by limiting to programs with FY 2016 budgets over $5 billion. We eliminated tax credits. Then we eliminated the following programs for specific reasons. We eliminated Section 8 Housing because the notion of participation is difficult to define where there are long wait lists and barriers to take-up are very high (often requiring moving). We eliminated Old Age Assistance and Social Security because income-based means tests are not meaningful for a population that often does not work and lives in households with other earners. Finally, we eliminated Pell Grants because eligibility is not based on a specific income threshold.

In programs with different eligibility thresholds per state, the level plotted is the population-weighted average of those thresholds. The SNAP take-up rate displayed here is higher than that used in our paper because the USDA uses a more involved and restrictive method for assessing eligibility than we do; our empirical results are not affected by a denominator that is too large.

Eligibility and take-up rates are particularly difficult to estimate for TANF and Head Start. Below, we show how excluding TANF and Head Start would affect Figure 1.

Figure A1: Eligibility Thresholds and Program Take-Up



The figure replicates Figure 1, with an additional line of best fit fitted without data from Head Start and TANF.

Finally, we provide additional details on our construction of the data in Figure 1:

- CHIP
  - Eligibility data are from Brooks et al. (2016), Table 1, which gives income thresholds for children's eligibility to receive Medicaid or CHIP benefits, assuming a family of 3. In some states, the income threshold varies for different subgroups. The figure uses a population-weighted average of all the states' highest income thresholds.
  - The take-up rate is from Appendix Exhibit 1 of Haley et al. (2018), also as referenced by The Kaiser Family Foundation (KFF).
- EITC
  - Eligibility is calculated using the IRS.gov EITC maximum allowable AGI for a family of three.
  - The take-up rate is from the IRS.gov "About EITC" webpage (Internal Revenue Service, 2020), estimated by the Census Bureau using the CPS.
- Head Start
  - Eligibility is generally 100% of the FPL (HHS).

- The take-up rate was calculated as follows:
  1. Participation rates are 35% (Child Trends, 2018), calculated using the total number of children enrolled in Head Start divided by the total number of children in poverty (ages 3-5).
  2. However, Head Start is oversubscribed. We use details from the Head Start Impact Study (U.S. Department of Health and Human Services, 2010): this study found that 85% of Head Start centers were oversubscribed. Within oversubscribed Head Start centers, the study randomized 60% of applicants into acceptance, while the remaining 40% were wait listed. In some centers, not all applicants were included in the randomization; in others, there were not enough applicants to attain this ratio in the randomization. We assume that take-up is 35*(1)*(15%) + 35*(10/6)*(85%). That is, the take-up rate is 35% among the 15% of centers which were not oversubscribed and 35*(10/6) in the oversubscribed centers, on average.

- Medicaid (parents only)
  - Eligibility data are from Brooks et al. (2016), Table 5, which gives income thresholds for parents' eligibility to receive Medicaid or CHIP benefits, assuming a family of 3. In some states, the income threshold varies for different subgroups. The figure uses a population-weighted average of all the states' highest income thresholds for parents.
  - The take-up rate is from Appendix Exhibit 2 of Haley et al. (2018), as referenced by KFF.

- NSLP (National School Lunch Program)
  - Eligibility for free lunch is 130% FPL in most districts; eligibility for reduced-price lunch is 185% FPL in most districts.
  - The take-up rate is calculated as follows:
    1. First, we take the total number of students eligible for free or reduced-price lunch in the 2015-2016 school year, according to Table 204.10 in National Center for Education Statistics (2017). This is around 26 million.
    2. We take the average number of free and reduced-price meals served daily in 2016, provided by the USDA Food and Nutrition Service: around 22 million (United States Department of Agriculture Food and Nutrition Service, 2020a).
    3. The take-up rate is 22 / 26

- SNAP
  - Eligibility data use a population-weighted average of states' eligibility thresholds.
  - The take-up rate is from Cunnyngham (2019), which gives estimates of 2016 take-up rates.

- TANF (Temporary Assistance for Needy Families)
  - Eligibility data are from Giannarelli et al. (2017), which provides, for all states, the income cutoff in dollars for TANF initial eligibility for a family of three. These cutoffs were converted to percent of the 2016 Federal Poverty Level for a family of three. The final eligibility level is the population-weighted average of these.
  - The take-up rate estimate comes from Giannarelli (2019).

- WIC (The Special Supplemental Nutrition Program for Women, Infants, and Children)
  - Eligibility is capped at 185% of the FPL.
  - The take-up rate is an estimate from the USDA FNS (United States Department of Agriculture Food and Nutrition Service, 2020b).

A.6 Figure A2 Details

Using JSTOR and EBSCO, a research assistant collected all *AER* and *QJE* papers that met one of 33 search terms according to the search engine.[42] The search terms were: "welfare program," "social insurance," "social program," "social assistance," "social welfare," "social benefit," "income threshold," "participation threshold," "means-testing threshold," "means-tested program," "means-tested welfare," "means-tested benefit," "means-tested subsidy," "income means testing," "eligibility rule," "eligibility threshold," "eligibility criteria," "eligibility criterion," "eligibility requirement," "woodwork effect," "program eligibility," "program benefit," "program subsidy," "program duration," "optimal program," "optimal provision," "benefit schedule," "program schedule," "public insurance," "program take-up," "incomplete take-up," "welfare take-up," "benefit take-up."

We limit the sampling frame to the 2010–2018 *AER* and 2010–2019 *QJE*. Appendix A.6 provides the search terms. On the authors' websites, we also provide a spreadsheet of all the papers, their inclusion criteria, and how we classified them. We also provide a list of judgment calls involved in this exercise and our rationale for our decision. We exclude the papers and proceedings but include comments. We exclude the 2019 AER because it was not available on JSTOR or EBSCO. We then read the abstract and/or introduction of each of the 278 papers that met at least one of the 33 search terms. We determine whether a paper was about a social welfare program.

We impose the following additional criteria when categorizing papers.

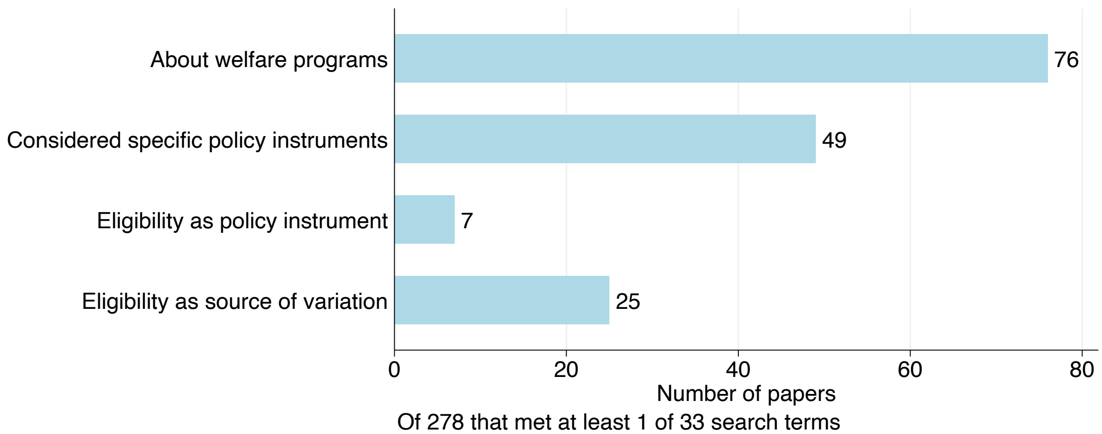
- We exclude papers that are principally about optimal income or capital taxation.
- We exclude transfers that are not intended to alleviate poverty (e.g., the effects of giving people computers).
- We exclude papers about credit market restrictions only, such as papers about mortgage deductions. We do include papers about consumer bankruptcy.
- We exclude papers about search and matching in labor markets if they do not have a substantial social insurance angle (e.g., UI).
- Because of the important theoretical connection between optimal social insurance and welfare design, we include papers that are about private insurance markets (including health insurance), as long as they have a significant angle about optimal policy.
- We define "program eligibility" as rules that determine whether a person has access to a social program. We do not consider eligibility to include access to different plan choices within a health program; our decision to exclude these papers is conservative, since they would only estimate a treatment effect using eligibility but not use optimal eligibility as an instrument.

[42]The research assistant also searched the downloaded PDFs to see which search terms were most often met. Two of the papers that the search engines specified met the search terms did not actually include the search terms in the downloaded PDF, perhaps due to a bug in the search engine. Neither paper was deemed to be about social welfare programs so this issue does not substantively affect the conclusions.

B Empirics Appendix

B.1 Additional Figures

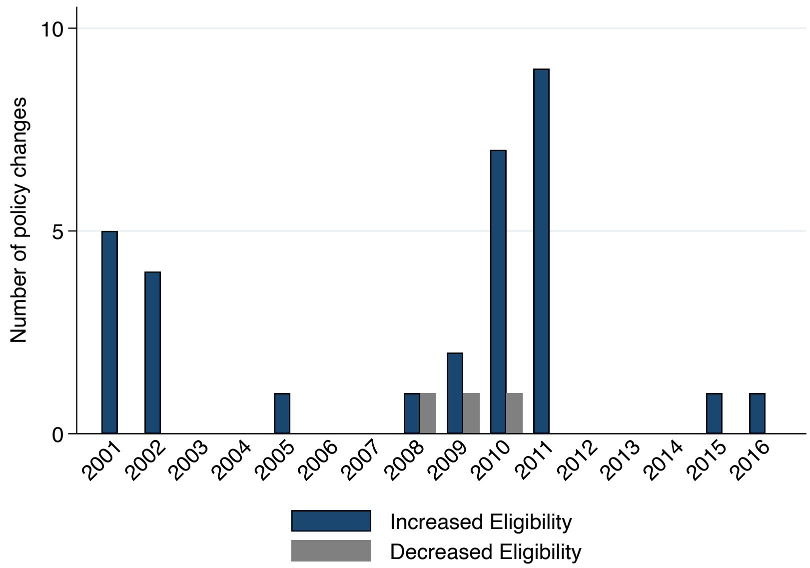
Figure A2: Literature Review: *AER* and *QJE* papers about Eligibility Criteria



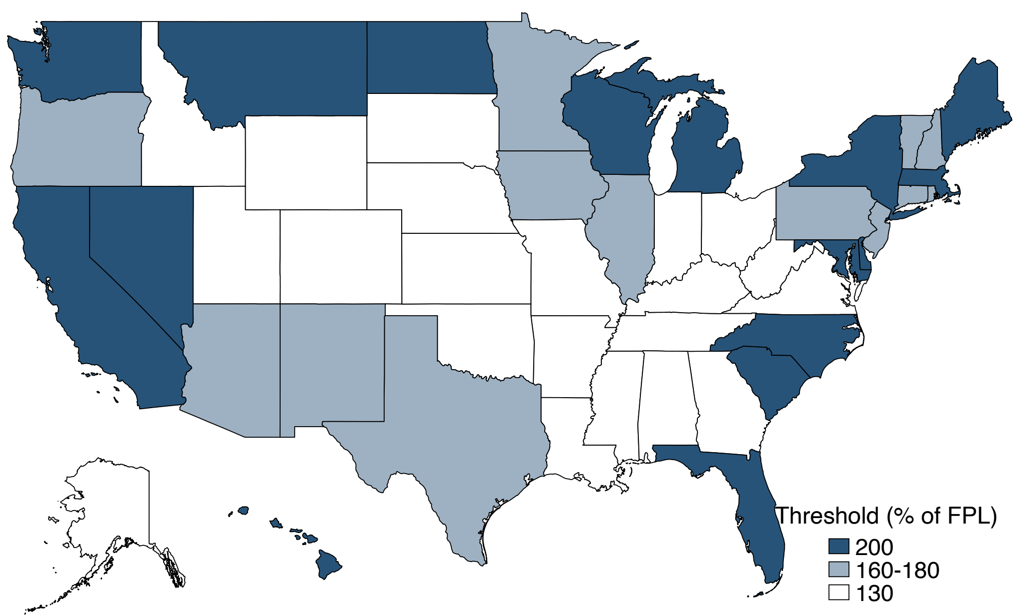
The figure presents the results from our literature review of papers in the *Quarterly Journal of Economics* (2010–2019) and the *American Economic Review* (2010–2018). Appendix A provides details about the sampling frame. The first row shows the total number of papers that we concluded were about welfare programs, after reading the abstract and introduction. The second row shows the number of papers that considered instruments with which the planner could enact optimal policy, e.g. the benefit size or duration. The third row shows the number of papers that considered the eligibility threshold as an instrument with which the planner could enact optimal policy. The fourth row shows the number of papers that use the eligibility threshold as a source of variation with which the authors estimated a treatment effect for the program.

Figure A3: BBCE implementation background

(A) Rollout of Eligibility Changes Per Year



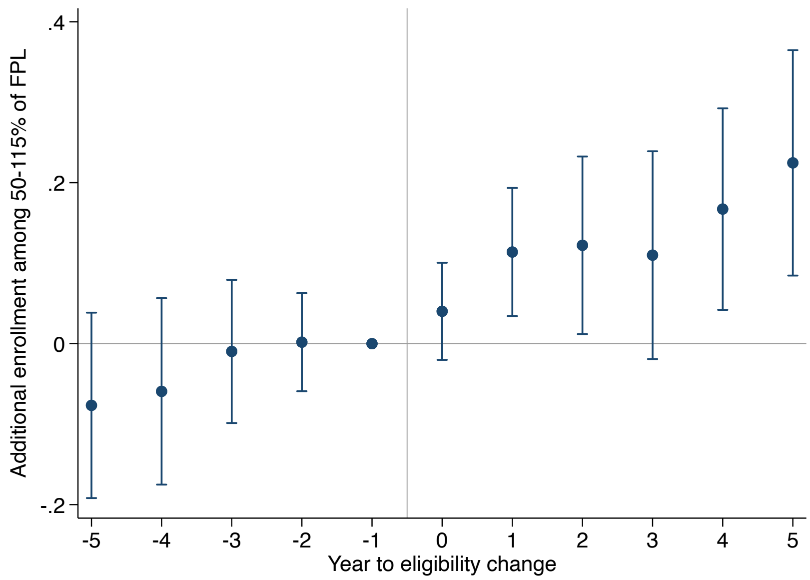
(B) Map of States that Implement Eligibility Expansions



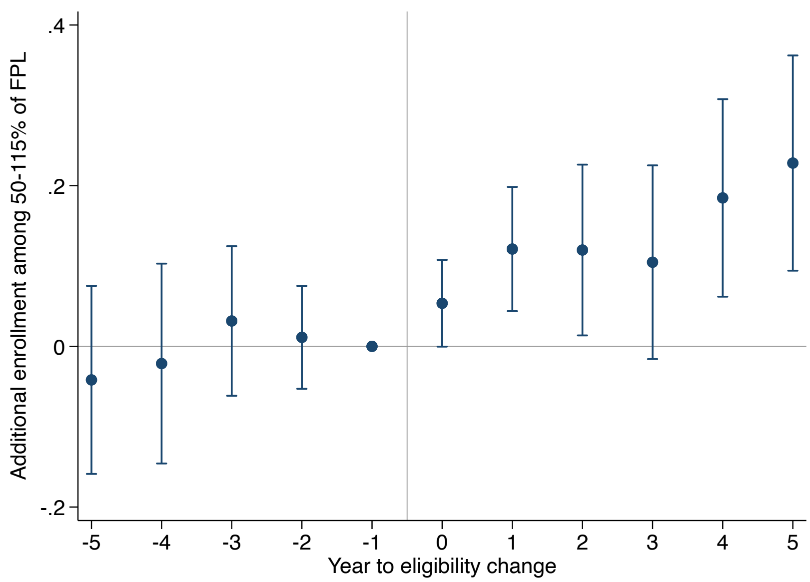
Panel A presents the number of states in each year that increased (blue bars) or decreased (gray bars) eligibility to the Supplemental Nutrition Assistance Program. Four states are counted twice, because they exhibit multiple changes. Panel B presents the maximum gross income eligibility threshold in a state from 1996–2016. The color coding refers to the maximum gross income eligibility threshold as a percent of the FPL; e.g., states colored in dark blue have maximum eligibility threshold of 200%. In two states that increase and then reduce the eligibility threshold, we present the largest eligibility threshold in the data. Source: SNAP Policy Database.

Figure A4: Event Study of Changes to Eligibility Threshold: Without Controls

(A) Sample: 50 to 115% of FPL, No Controls

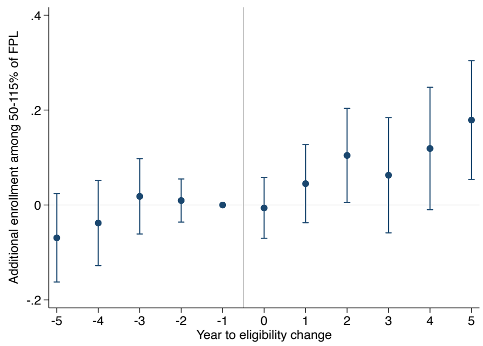


(B) Sample: 50 to 115% of FPL, Only Denominator Control

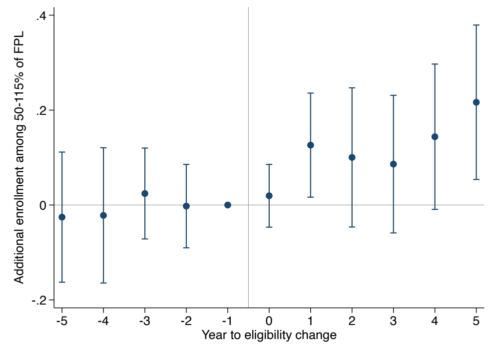


This figure is similar to Figure 3B, but Panel A presents the specification with no controls beyond state and year fixed effects. Panel B presents the specification with state and year fixed effects, only controlling for the log of the total number of people between 50 and 115% of the FPL (from the CPS).

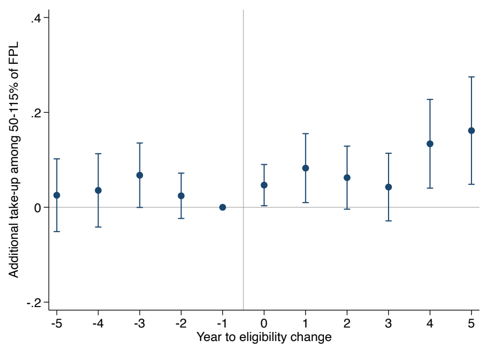
Figure A5: Extra robustness checks



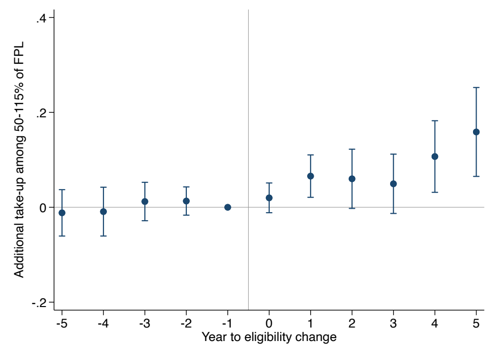
(A) Monthly data



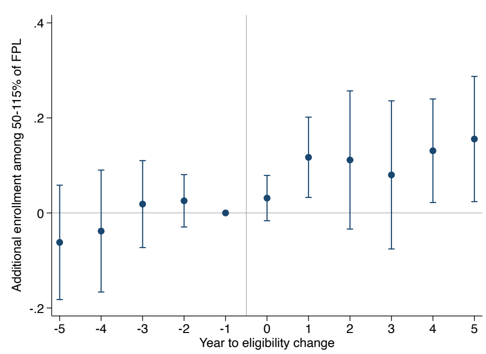
(B) Only Households with Dependents



(C) Take-up



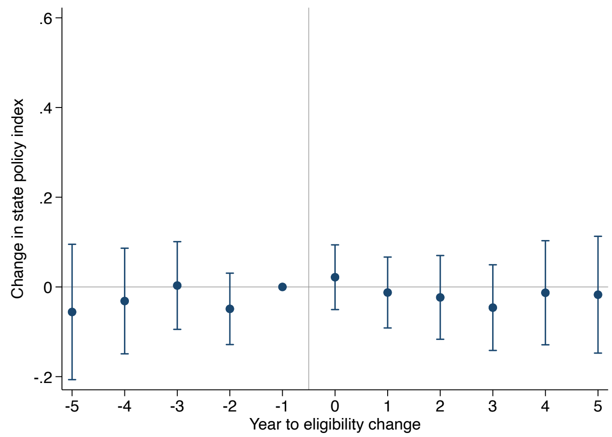
(D) Take-up, controlling for Ln(CPS count in 50-115%)



(E) Enrollment, weighted by state-year population

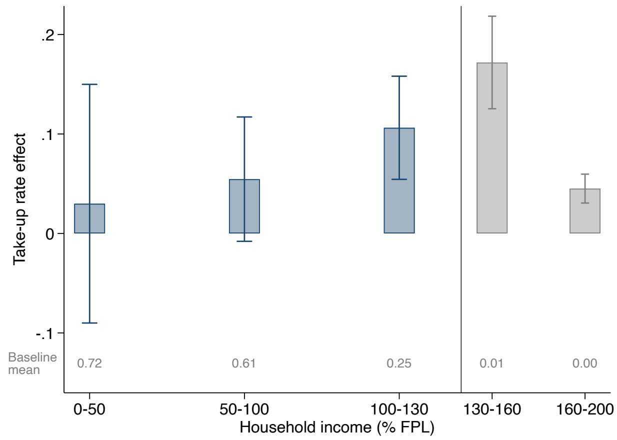
Panel A presents the results of estimating Equation (2) with monthly data instead of annual data. Panel B includes only SNAP recipients with any dependents—households that will not be affected by ABAWD work waivers. Panels C and D use the take-up share instead of the log of enrollment as the regressand, where the numerator in the take-up share comes from the USDA Quality Control data and the denominator uses the CPS. Panel C has no controls for state-year CPS population, while Panel D controls for the log of count of individuals in the CPS with household income in 50-115% FPL. Panel E uses the main specification and weights by population size in each state-year. Standard errors are robust to heteroskedasticity and clustered by state.

Figure A6: Balance: SNAP Policy Index



This figure presents a placebo event study with the main specification from Equation (1) but replacing the outcome with one of our controls: the "Ganong-Liebman" index of SNAP policies, which are found in the USDA's SNAP Policy Database, as the outcome. Standard errors are robust to heteroskedasticity and clustered by state. Additional balance tests can be found in Figure A10.

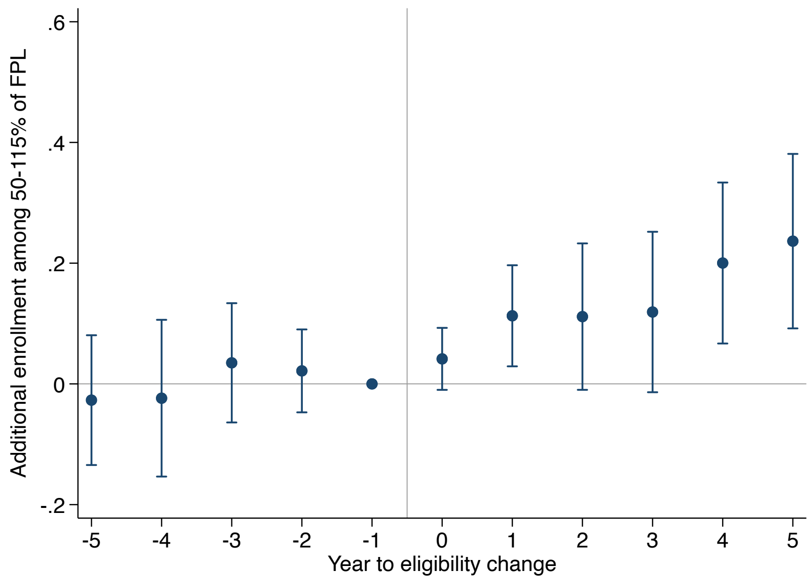
Figure A7: Effect on Take-Up Rates by Income Group



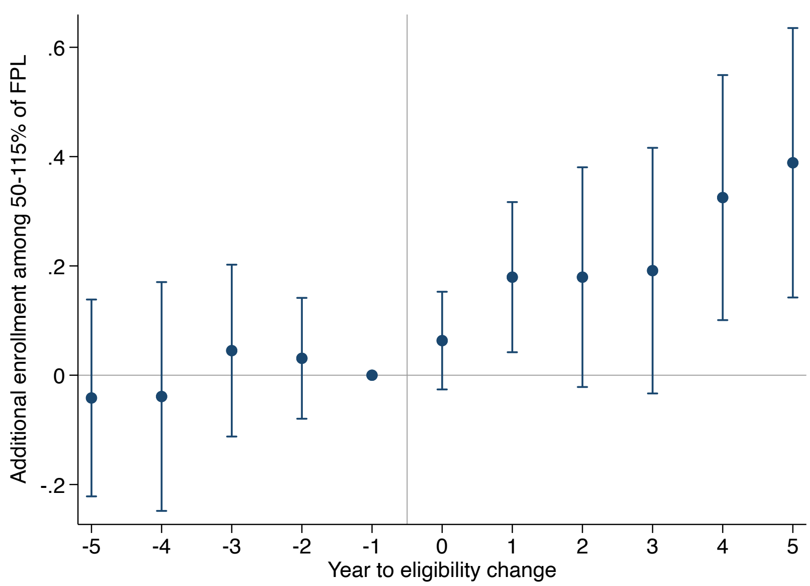
This figure presents estimates of Equation (2) using take-up rates as the outcome variable. The bars show the effect of the eligibility threshold on SNAP take-up by income group, and the whiskers show the 95% confidence intervals. While the regression specification is the same for all bars (with only the reference group changing), they are colored blue and gray to distinguish the effects on the already-eligible population versus the effects on the newly eligible population. Take-up rates are calculated using the enrollment counts from the USDA Quality Control (QC) data in the numerator and total counts of individuals within the income group from the Current Population Survey (CPS) in the denominator. Standard errors are robust to heteroskedasticity and clustered by state.

Figure A8: Two-Way Fixed Effects Robustness

(A) Stacked Estimator (Cengiz et al., 2019)



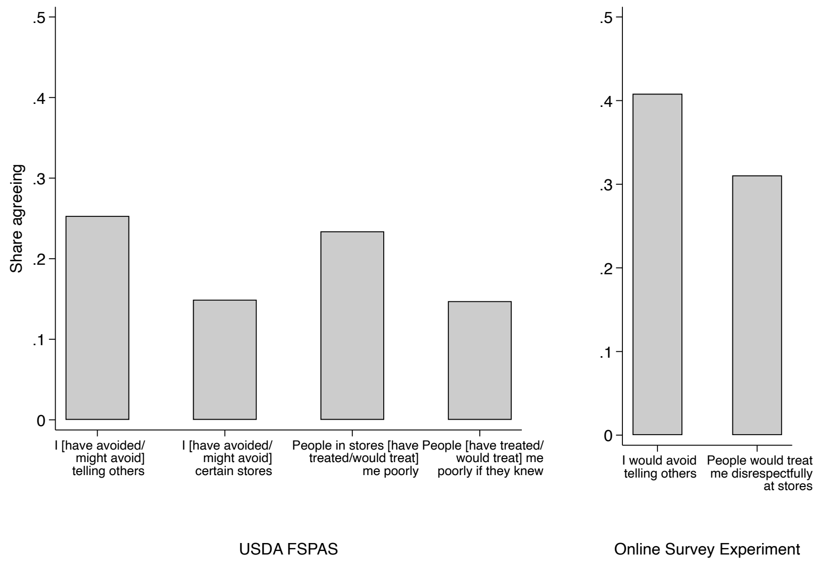
(B) Sun and Abraham (2021) Estimator



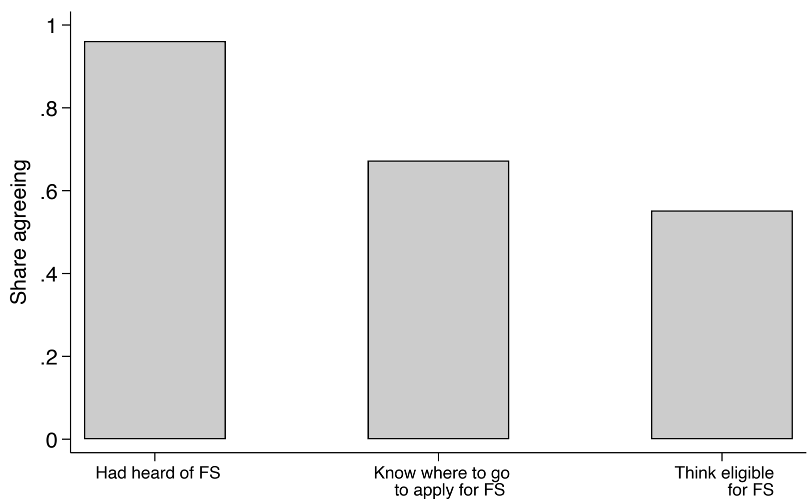
This figure presents heterogeneity-robust event study estimates using the 50–115% sample. Panel A presents the "stacked estimator" developed in Cengiz et al. (2019). For each treated state, we form a dataset keeping just one treated state and all never-treated states. We then stack all datasets and estimate a version of Equation (1), controlling for dataset-state fixed effects. We employ two-way clustering by dataset and state. Panel B presents the results from the estimator in Sun and Abraham (2021), using never-treated states as a comparison group.

Figure A9: FSPAS Descriptives

(A) Stigma



(B) Information



This figure shows the share of respondents agreeing with different statements presented in the USDA Food Stamp Program Access Study about the stigma around SNAP (in Panel A) and their access to information about SNAP (Panel B). Panel A includes both approved applicants and eligible nonparticipants, and Panel B includes only eligible nonparticipants. In Panel A, we compare results to those from the online experiment, limited to respondents earning under 130% FPL.

B.2 Additional Tables

Table A1: Estimates of the Woodwork Effect in Alternate Samples

	(1) Main estimate	(2) Extra controls	(3) Waivers, lag unemp.	(4) Excludes recession	(5) Weighted	(6) Avg of coefficients	(7) All data
Panel A. 0–115% FPL							
Income limit (% FPL) / 100	0.070	0.074	0.056	0.072	0.061	0.061	0.070
	(0.058)	(0.058)	(0.055)	(0.061)	(0.073)	(0.065)	(0.049)
Panel B. Any dependents							
Income limit (% FPL) / 100	0.121**	0.127**	0.107*	0.132**	0.120*	0.122*	0.134**
	(0.059)	(0.063)	(0.057)	(0.061)	(0.071)	(0.065)	(0.050)
Observations	705	705	680	628	705	705	1071
N states	45	45	45	45	45	45	51

This table presents Table 1 with different samples, using the specification in Equation (2). See notes to Table 1 for details. Panel A uses the sample of people at 0–115% of the Federal Poverty Line (FPL). Panel B presents estimates for the sample of households with dependents, who are not subject to ABAWDs rules, in households earning 50–115% FPL. The outcome is SNAP enrollment as estimated from the USDA Quality Control data. Standard errors are robust to heteroskedasticity and clustered by state. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A2: USDA FSPAS Characteristics

	(1) Info types	(2) Stigma types
Enrolled	0.38	0.43
Female	0.76	0.78
White	0.52	0.65
Has kids	0.47	0.53
Age	43.77	39.31
Observations	953	575

The table shows summary statistics for respondents categorized as affected by information or affected by stigma in the USDA Food Stamp Program Access Study (approved applicants and eligible nonparticipant samples only).

B.3 Robustness

Economic Conditions. To examine whether economic conditions drive eligibility expansions, we estimate Equation (1) with the log of the CPS counts of the people at 50–115% of the FPL as the dependent variable (Figure A10A). We find a slight pre-trend in the CPS populations three years before the event. We already include this control because it helps purge a moderate pre-trend in the main event study. Similarly, we estimate Equation (1) with the unemployment rate as the dependent variable (Figure A10B). Although the unemployment rate appears to grow in advance of the policy, the time-series pattern of the changes in the unemployment rate do not align with our main results: the unemployment rate returns to 0 after 5 years, whereas our main effects persist. That is why when we control for the unemployment rate, this control does not materially affect our results (Figure A4B versus Figure 3B). Two other tests further suggest robustness to concerns about economic conditions. We include a further control for lagged unemployment (Column 3, Table 1). We also exclude the Great Recession (Column 4), when unemployment rates had the greatest fluctuation.

Comparing States Which Did and Did Not Expand Eligibility. We compare states which did and did not ever change their eligibility threshold in Table A3. In Panel A, we see that states which ever changed their eligibility threshold have significantly higher average family incomes in the pre-period (measured in 2000, the last year before any state changed its eligibility threshold), and marginally significantly higher measures of SNAP access-related policy (the Ganong-Liebman index and SNAP outreach spending). Because our main results use an event study, imbalance in levels in the pre-period is not itself concerning. Toward this point, Panel B shows that these measures are not strongly associated with the size of the means-test change, which provides suggestive evidence that the policy decision is not driven by these measures.

Measurement Error. We study whether measurement error in reported income in the Current Population Survey (CPS) could explain our main results. Figure A10A shows that the count of people in the CPS earning below 130% FPL does not change discretely around the time of the policy implementation. The figure for people earning 50–115% FPL looks very similar. Especially given that we control for the denominator, it is implausible that state populations grow fast enough only in treated state-years, beginning exactly at the time of the eligibility increase, that this measurement error could explain our event study results. Any threat to identification requires that the mismeasured portion of the denominator grows in a way that is correlated with treatment, beginning precisely at the date of treatment.

To formalize this point, we obtain the following bound on the magnitude of measurement error in the denominator required to explain our results. In state-years with an eligibility threshold above 130% of the FPL, we simulate systematic measurement error in the denominator using an "inflated" denominator that we define as:

$$\text{simulated denominator} := \text{observed denominator} \times \text{inflation factor},$$

where the inflation factor represents the magnitude of simulated measurement error. For instance, an inflation factor of 1.05 represents the case where we replace the treated state-years' denominators as being 5% larger than what we observe in the CPS.

We then estimate Equation (2) with the simulated denominator in treated state-years. We find that the woodwork effect vanishes only if the denominator in treated state-years is inflated by more than 30% (Figure A11). Put another way, only when we add an additional 30% of the population to the denominator (and impose that this measurement error only exists in treated state-years) can we eliminate the woodwork effect. As a benchmark, we note that the average state population between 50 to 115% of the FPL (i.e., the denominator) grew by 26% between 2001 and 2016. Thus the measurement error required to reverse our result would need to be larger than the entire observed population growth in the sample period. It is implausible that *only* treated states are subject to measurement error that is this extreme.

Altogether, while our denominator obtained from the CPS may be subject to some measurement error, it would have to be systematically correlated with treatment to an implausible degree in order to explain our results.

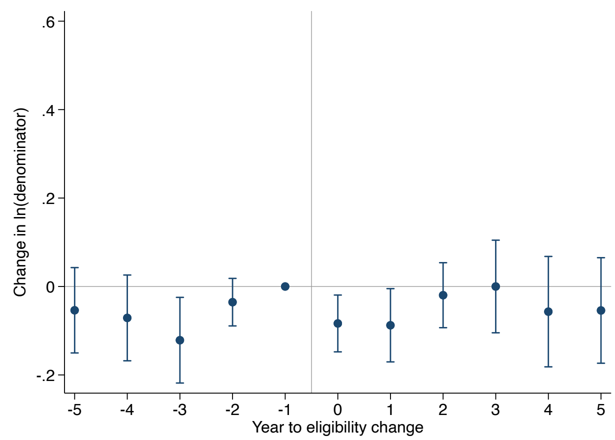
There may also be measurement error in the timing of the policy implementation.[43] We use annual data

[43]We follow the date of the policy implementation in the SNAP Policy Database. However, the precise implementation date may vary across sources, and the legal implementation date may not coincide with the date that the program actually began accepting

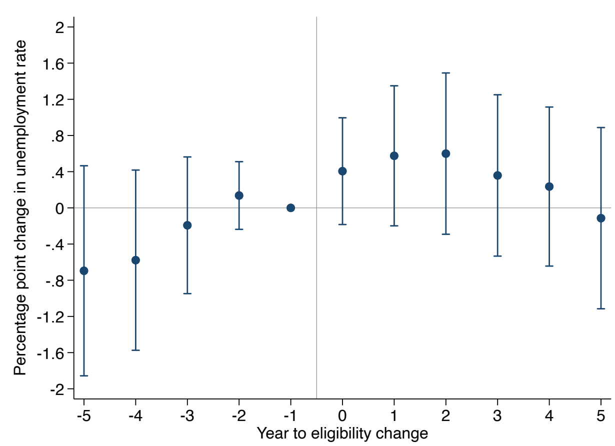
in our main specification because we measure the number of people who are eligible from the March CPS, which is only available annually. Moreover, the QC data contain relatively few people at the month-state-income group level. However, BBCE policies can be implemented mid-year. In Figure A5A, we show our event study using monthly data to estimate Equation (2). It looks similar, although the woodwork effect is slightly slower to appear. This reflects the fact that in our main specification, we index policy implementation to the beginning of the first fully treated year.

Figure A10: Additional Balance Tests

(A) CPS Population Count: 0 to 130% of FPL



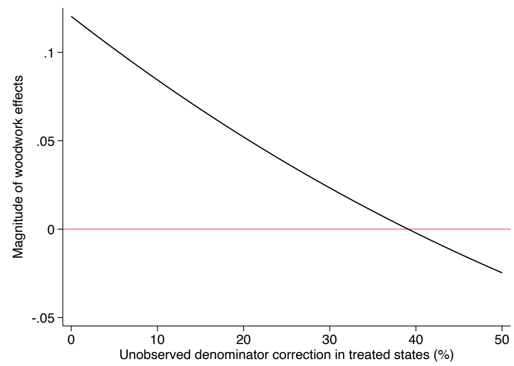
(B) Unemployment Rate



This figure presents placebo event studies with the main specification from Equation (1) but replacing the outcome with control variables. The event time is indexed around changes to state eligibility thresholds. Panel A uses the (ln of) the number of people in a state earning below 130% FPL (from the CPS) as the outcome. Panel B uses the state unemployment rate as the outcome. Standard errors are robust to heteroskedasticity and clustered by state.

people with incomes larger than 130% of the FPL (e.g., if program social workers need to be trained on the new procedures). In practice, measurement error along these lines would likely add noise to the event study.

Figure A11: Simulated Measurement Error



This figure presents a bound on the amount of measurement error in the denominator that would be required to reverse our results. In states where the eligibility threshold exceeds 130% of the FPL, we inflate the observed population between 50–115% of the FPL by the factor on the x-axis. We then present the estimate of the woodwork effect from Equation (2), estimated using the simulated denominator. Only if the population is inflated by 40% can we reverse the woodwork effect.

Table A3: Pre-Policy State Characteristics

Panel A. By Ever Changed Threshold			
	No	Yes	p-value
Share of state pop enrolled	0.08	0.07	0.29
Unemployment rate	3.93	3.88	0.85
Average family income in state	51.87	57.42	0.01
Ganong-Liebman Index	0.06	0.10	0.09
Outreach spending	1.64	16.92	0.09
Observations	30	21	
Panel B. By New Eligibility Threshold			
	$<$ 200% FPL	200% FPL	p-value
Share of state pop enrolled	0.09	0.09	0.79
Unemployment rate	6.04	5.89	0.86
Average family income in state	73.57	64.79	0.08
Ganong-Liebman Index	0.52	0.43	0.48
Outreach spending	54.57	48.82	0.85
Observations	13	17	

In Panel A, we compare states which did and did not ever change their SNAP eligibility threshold in their pre-policy characteristics, as measured in the year 2000 (before any states implemented policy changes). In Panel B, we limit the sample to states which did increase their eligibility threshold and compare those which raised it to 200% FPL to those which raised it to a value below 200% FPL, where the pre-policy characteristics are measured two years before their policy change. The first row shows the share of the state population enrolled in SNAP in the given year. The second row shows the state unemployment rate. The third row shows the average family income (in thousands of dollars, from the CPS). The fourth shows the Ganong-Liebman Index, excluding the BBCE indicator. The final row shows spending on SNAP outreach in the state (also in thousands of dollars), where the value is winsorized.

C Mechanisms Appendix

C.1 Belief-Correction Experiment

Table A6 shows the belief-correction experiment is balanced between treatment and control. The results of this second experiment are mixed (Table A9). We find no evidence for an effect of a belief-correction exercise on first-order beliefs. We find a *positive* effect of the belief correction on second-order beliefs: for people whose priors were below the truth, correcting beliefs *raises* the stigma they report (point estimate: 0.069, SE: 0.041, $p = 0.091$).

We note that the treatment effect is positive for people whose beliefs are corrected down (point estimate: 0.018, SE: 0.035). This point estimate is consistent with the results from the high-state treatment. Alternatively, it may suggest that any belief correction may simply cause participants to report more stigma, e.g. because they do not like being corrected after receiving an initial hint. We also present effects with demographic controls (Table A10), which are similar. In this case, the results are similar, although the positive effect on second-order stigma for correcting beliefs upward is very slightly attenuated.

We are more cautious about interpreting the results from the belief-correction experiment for the following reasons. First, people who are shown multiple pieces of information might simply end up confused, which could attenuate or undo its effects. Because we did not elicit beliefs after being shown the belief correction, we do not have a way of checking how the correction actually shifted posteriors. The inconclusive results suggest that providing the second piece of information might have had an unintended consequence of causing participants to tune out the second piece of information, perhaps because it was perceived as contradicting the first piece of information. We show effects of the high-share treatment separately by belief-correction treatment in Table A11.

Second, the belief-correction belief correction only operates on people *after* they have been shown a hint. As a result, because it is cross-randomized, it affects the group of people that do or do not comply with the high or low treatment. The staggered nature of the design complicates this interpretation: people who have low prior beliefs after treatment are a selected group, since they have been exposed to a hint that causes them to update.[44]

Third, the belief-correction treatment, when paired with the high-share treatment, affects people's beliefs about the distribution of eligibility thresholds across states. If stigma is linked to people's beliefs about the distribution of eligibility thresholds, it is not clear how the combination of experiments affects stigma.

Altogether, the high- and low-share experiment provides a somewhat cleaner test of the null hypothesis that stigma plays no role in woodwork effects. Nevertheless, the inconclusive results from the belief-correction experiment lead us to interpret the experiment with some caution.

C.2 FSPAS Data

- We use surveys of (1) eligible nonparticipants and (2) successful SNAP applicants from the FSPAS, a study conducted by the USDA in the year 2000. The USDA considered someone an eligible nonparticipant if their household income was beneath 130% FPL and they were not currently enrolled in SNAP. There are 421 successful SNAP applicants and 1,323 eligible nonparticipants.
- **Stigma.** Respondents were considered affected by stigma if they answered "yes" to (agreed with) at least one of the following questions (statements).
  - If they'd ever been enrolled in SNAP:
    * Have you ever avoided telling people you got food stamps?
    * Did you ever go out of your way to shop at a store where no one knew you?
    * Have you ever been treated disrespectfully when using food stamps in a store?
    * Were you ever treated disrespectfully when you told people that you received food stamps?
  - If they'd never been enrolled in SNAP:
    * "If I got food stamps, I might go out of my way so people would not find out."
    * "I might not shop in certain stores because I don't want people there to know I use food stamps."
    * "People in stores would treat me disrespectfully when I use food stamps."
    * "People would treat me disrespectfully if they found out that I got food stamps."
- **Information.** Respondents were considered affected by information barriers if they (a) were in the eligible nonparticipant sample and (b) said "no" to any of the following questions:
  - Had you heard of food stamps or the Food Stamp Program before today's interview?
  - Do you know where you would have to go to apply for food stamps or other assistance?
  - Do you think you may be eligible to receive food stamp benefits?
- **Survey weights.** Each survey in the FSPAS is weighted to be representative of the population the respondents were sampled from. When we combine participants and eligible nonparticipants, we adjust these weights according to the share of Americans who participated in SNAP conditional on being eligible in the year 2000 (estimated in the QC data to be 40%).

[44] Consistent with this point, the positive treatment effect on second-order stigma from correcting beliefs upward attenuates once we add demographic controls (Table A10).

C.3 Additional details

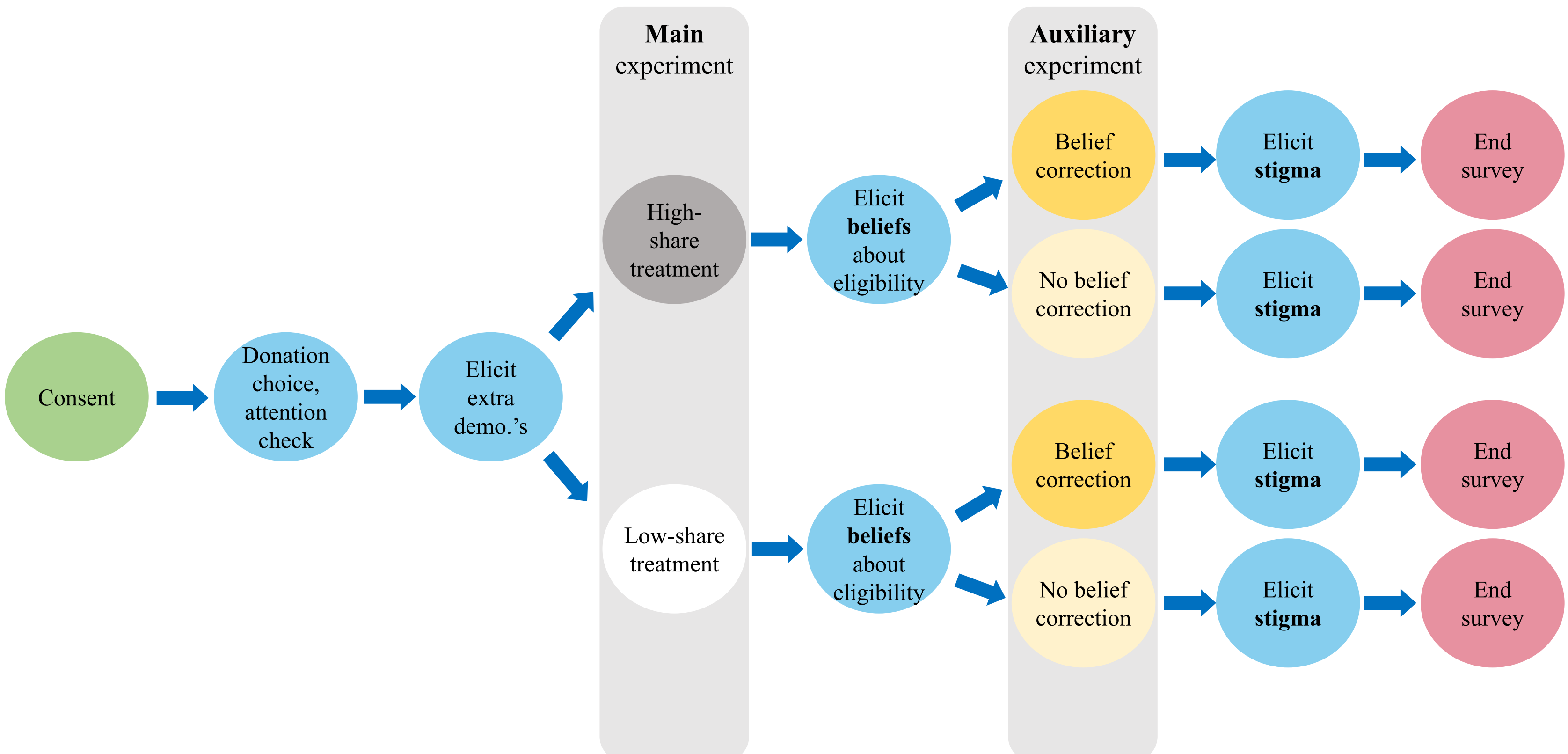
Demographics. We focus on the following demographic variables: female/non-female, white/non-white, age group (0–18, 19–30, 31–65, 66+), household size (1, 2, 3, or 4), and income decile (grouping deciles 40–70 and 70–100). To construct demographic *cells*, we fully interact each variable. For instance, "single white women ages 19–30 in income decile 10" is an example of a demographic cell.

To focus on the group that is most affected by woodwork effects, our estimates of $\frac{\partial \ln(\text{N enrolled})}{\partial m}_d$ use the population between 50–115% FPL. We cannot precisely limit to this group in the experiment, but we limit that to less than 130% of the FPL.

Precision Weighting. The regression used for Figure 5 uses previously estimated demographic subgroup effects. Because we estimate these effects with noise, the dispersion in the effects — and thus in the data used to estimate the relationships between effects — will be larger than the true variation. Moreover, effects estimated in small cells will be estimated less precisely than effects estimated in larger cells. To adjust for this, in the binned scatterplots, we weight by the inverse of the product of the variance of the estimates; i.e., we give more weight to cells that are more precisely estimated.

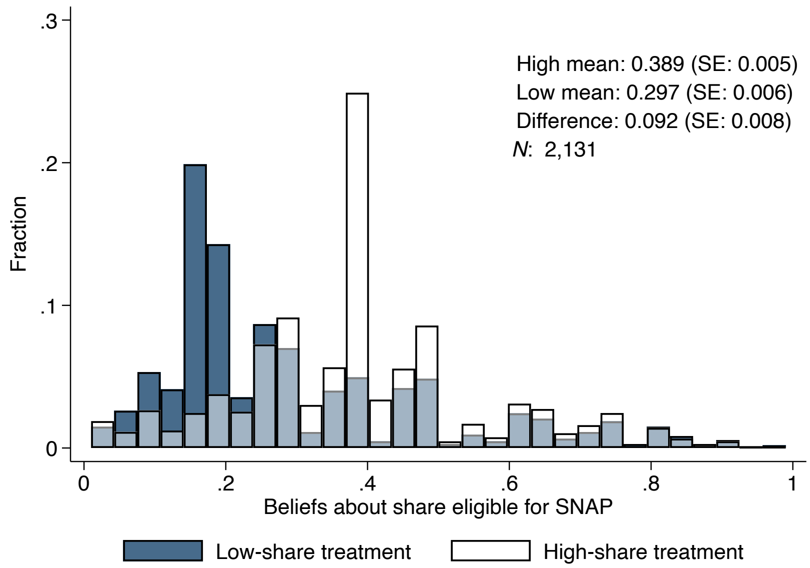
C.4 Additional Figures

Figure A12: Visual Depiction of Experiment Design



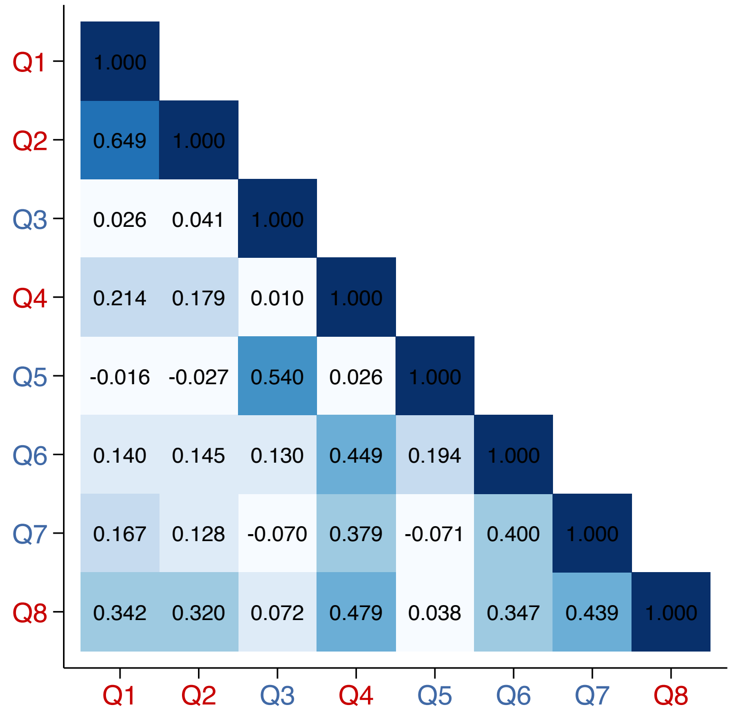
This figure presents the experiment design. The donation choice was to one of four charities (used to incentivize belief elicitation). We elicited several demographics (in addition to those provided by Lucid).

Figure A13: Effect of High-Share Treatment on Beliefs about Eligibility



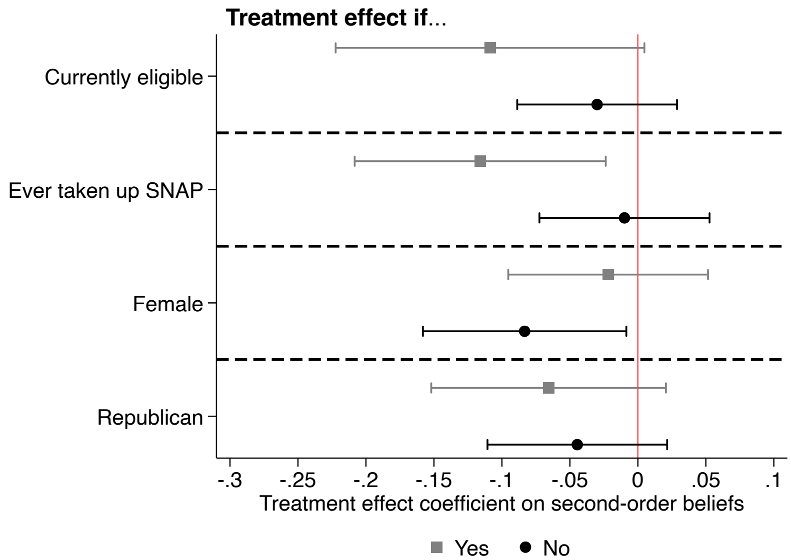
This figure presents the distribution of beliefs from the online experiment, split by treatment group, about the fraction of people who are eligible for SNAP. The *y*-axis shows the share of people within each treatment group who report a given fraction are eligible for SNAP. The blue bars show the values for the low-share treatment. The white bars show the values for the high-share treatment. The light blue shaded area shows the overlap.

Figure A14: Correlations Between Stigma Questions



This figure presents correlations between the stigma questions in the order they were elicited. Section 3 provides the question texts. We classify questions 1, 2, 4, and 8 (labeled in red) as first-order stigma. We classify questions 3, 5, 6, and 7 (labeled in blue) as second-order stigma.

Figure A15: Treatment Effect Heterogeneity



This figure presents treatment effects and 95% confidence intervals of the high-share treatment on the second-order stigma index (Equation (4)), split by demographic group.

C.5 Additional Tables

Table A4: Online Experiment: Attrition Balance

	Total N	High-share treatment		Beliefs correction	
		All	<= 130% FPL	All	<= 130% FPL
1. Any attrition or drops	**567**	**0.009**	**0.018**	**−0.001**	**0.005**
		(**0.016**)	(**0.033**)	(**0.015**)	(**0.033**)
2. Bad priors	237	0.002	0.008	-0.000	0.003
		(0.011)	(0.024)	(0.011)	(0.025)
3. Attrited before share treatment	49	0.004	-0.004		
		(0.005)	(0.010)		
4. Attrited at or after treatment	126	0.006	0.007	-0.001	0.013
		(0.008)	(0.019)	(0.008)	(0.019)
5. Omitted any stigma answers	107	0.002	0.005	-0.006	-0.013
		(0.008)	(0.016)	(0.008)	(0.017)
6. Inattentive	106	0.000	0.021	0.005	0.003
		(0.008)	(0.013)	(0.008)	(0.014)
Observations		2,698	689	2,698	689

This table shows that attrition and drops were balanced across treatment and control. Each row tests for balance between treatment and control on a different dummy outcome. The first column gives the total number of respondents who were dropped for the reason indicated by the row. Note that respondents could be dropped for multiple reasons. The next two columns show balance for the high- and low-share experiment, where respondents were provided a random hint about the share of Americans eligible for SNAP. The last two columns show balance for the secondary experiment, where respondents beliefs were corrected with the true share. Row 1's outcome is a dummy for attriting or being dropped from the sample. Row 2's outcome is a dummy for providing prior beliefs about the share of Americans eligible for SNAP that were below 1% or above 99%, or skipping this question entirely. Row 3's outcome is a dummy for dropping out of the survey before the treatment screen. The second two columns of this row are empty because individuals who attrited before the treatment screen were not randomized into treatment or control for the beliefs correction. Row 4's outcome is a dummy for attriting at or after the share treatment screen. Row 5's outcome is a dummy for not answering any of the stigma questions. Row 6's outcome is a dummy for failing an attention check. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A5: Experiment Sample Composition and Balance for High vs. Low Treatment

	CPS Sample	Full Sample			Below 130% FPL		
		Low-share	High-share	*p*-value	Low-share	High-share	*p*-value
Female	0.517	0.531	0.522	0.658	0.647	0.597	0.248
White	0.776	0.727	0.737	0.623	0.707	0.684	0.583
Hispanic	0.165	0.109	0.112	0.824	0.120	0.160	0.203
At least some college	0.611	0.778	0.772	0.737	0.606	0.612	0.894
Age	47.714	45.679	46.145	0.526	45.036	45.042	0.997
Any Children	0.254	0.537	0.531	0.790	0.618	0.597	0.619
Single	0.291	0.366	0.368	0.927	0.418	0.441	0.594
Household Size	2.296	2.519	2.517	0.973	2.687	2.692	0.970
Democrat	-	0.541	0.517	0.275	0.522	0.490	0.476
On Food Stamps (Currently or Ever)	-	0.383	0.392	0.648	0.627	0.624	0.946
Household Income (000's)	-	59.007	59.941	0.680	15.331	13.431	0.021
Census regions							
Northeast	0.175	0.208	0.191	0.308	0.169	0.095	0.014
Midwest	0.207	0.190	0.198	0.617	0.189	0.209	0.565
South	0.379	0.344	0.372	0.176	0.369	0.441	0.100
West	0.238	0.259	0.240	0.311	0.273	0.255	0.639
Joint F-test *p*-value				0.941			0.018
Observations				2131			512

Income uses the midpoint of a set of bins and is top-coded at $250,000. Household size is top-coded at 6. The CPS sample uses the 2019 NBER MORGs.

Table A6: Online Experiment: Randomization Balance for Belief Correction

	CPS Sample	Full Sample			Below 130% FPL		
		No Correction	Belief Correction	*p*-value	No Correction	Belief Correction	*p*-value
Female	0.517	0.515	0.537	0.304	0.615	0.626	0.798
White	0.776	0.742	0.722	0.312	0.704	0.687	0.665
Hispanic	0.165	0.109	0.113	0.753	0.130	0.151	0.488
At least some college	0.611	0.783	0.767	0.378	0.615	0.604	0.788
Age	47.714	45.770	46.048	0.705	45.725	44.400	0.391
Any Children	0.254	0.528	0.540	0.559	0.623	0.592	0.473
Single	0.291	0.366	0.368	0.906	0.401	0.457	0.203
Household Size	2.296	2.530	2.507	0.724	2.757	2.626	0.350
Democrat	-	0.525	0.533	0.709	0.490	0.521	0.486
On Food Stamps (Currently or Ever)	-	0.377	0.398	0.329	0.615	0.634	0.665
Household Income (000's)	-	61.526	57.476	0.073	14.787	13.952	0.311
Census regions							
Northeast	0.175	0.199	0.200	0.965	0.162	0.102	0.044
Midwest	0.207	0.208	0.180	0.112	0.215	0.185	0.402
South	0.379	0.361	0.354	0.749	0.413	0.400	0.766
West	0.238	0.232	0.265	0.077	0.211	0.313	0.008
Joint F-test *p*-value				0.611			0.498
Observations				2131			512

Income uses the midpoint of a set of bins and is top-coded at $250,000. Household size is top-coded at 6. The CPS sample uses the 2019 NBER MORGs.

Table A7: Online Experiment: High-Share Effect on Reported Stigma, without Demographic Controls

	Overall	Subindices	
		First-Order	Second-Order
Under 130% FPL			
High-share treatment	-0.032	0.046	-0.109*
	(0.050)	(0.065)	(0.058)
p-value	0.530	0.485	0.061
Observations	512	512	512
Full Sample			
High-share treatment	-0.013	0.025	-0.050*
	(0.024)	(0.031)	(0.027)
p-value	0.598	0.421	0.061
Observations	2,131	2,131	2,131

The table shows the effect of the "high-share" hint on individuals' level of agreement to statements measuring stigma around food stamps and welfare for individuals under 130% FPL (top panel) and the full sample (bottom panel) (Equation (4)). The estimates are identical to Figure 4. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A8: Online Experiment: High-Share Effect on Reported Stigma, with Demographic Controls

	Overall	Subindices	
		First-Order	Second-Order
Under 130% FPL			
High-share treatment	-0.023	0.049	-0.096*
	(0.050)	(0.064)	(0.058)
p-value	0.640	0.448	0.099
Observations	512	512	512
Full Sample			
High-share treatment	-0.016	0.016	-0.048*
	(0.023)	(0.029)	(0.026)
p-value	0.489	0.580	0.072
Observations	2,131	2,131	2,131

The table shows the effect of the "high-share" hint on individuals' level of agreement to statements measuring stigma around food stamps and welfare for individuals under 130% FPL (top panel) and the full sample (bottom panel) (Equation (4)). It is identical to Table A7 and Figure 4 except we include demographic controls for: an age quadratic, income, political party, gender, region, household size, marital status, having children, being on or ever having been on food stamps, and education and race/ethnicity fixed effects. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A9: Online Experiment: Belief Correction, No Demographic Controls

	Overall	Subindices	
		First-Order	Second-Order
Panel A. Priors < Truth			
Beliefs Correction Treatment	0.044	0.020	0.069*
	(0.036)	(0.048)	(0.041)
Observations	868	868	868
p-value	0.218	0.680	0.091
Panel B. Priors ≥ Truth			
Beliefs Correction Treatment	0.008	-0.002	0.018
	(0.031)	(0.041)	(0.035)
Observations	1,263	1,263	1,263
p-value	0.800	0.964	0.615

This table shows results from the second experiment embedded in our online survey, where respondents were informed of the true share of Americans eligible for SNAP after previously being asked to report their beliefs (and given a hint, which is the primary experiment discussed in the text). It presents treatment effect estimates from Equation (4). Panel A restricts the sample to those who initially underestimated the eligibility share, so that the treatment should have led them to revise upwards. Panel B restricts the sample to those who initially overestimated the eligibility share, so that the treatment should have decreased their beliefs. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A10: Online Experiment: Belief Correction, With Demographic Controls

	Overall	Subindices	
		First-Order	Second-Order
Panel A. Priors $<$ Truth			
Beliefs Correction Treatment	0.036	0.006	0.066
	(0.035)	(0.045)	(0.040)
Observations	868	868	868
p-value	0.301	0.900	0.103
Panel B. Priors $\geq$ Truth			
Beliefs Correction Treatment	0.032	0.034	0.030
	(0.030)	(0.038)	(0.035)
Observations	1,263	1,263	1,263
p-value	0.290	0.375	0.389

This table shows results from the second experiment embedded in our online survey, where respondents were informed of the true share of Americans eligible for SNAP after previously being asked to report their beliefs (and given a hint, which is the primary experiment discussed in the text). It presents treatment effect estimates from Equation (4). Panel A restricts the sample to those who initially underestimated the eligibility share, so that the treatment should have led them to revise upwards. Panel B restricts the sample to those who initially overestimated the eligibility share, so that the treatment should have decreased their beliefs. This table is identical to Table A9 except we additionally include demographic controls for: an age quadratic, income, political party, gender, region, household size, marital status, having children, being on or ever having been on food stamps, and education and race/ethnicity fixed effects. *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

Table A11: Online Experiment: Treatment Effect by Belief-Correction Randomization

	(1) First-order index	(2) Second-order index	(3) First-order index	(4) Second-order index
High-share treatment	0.048	-0.019	0.003	-0.081**
	(0.044)	(0.037)	(0.044)	(0.038)
Observations	1050	1050	1081	1081
Sample	Not shown truth	Not shown truth	Shown truth	Shown truth

Standard errors in parentheses

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

This table presents treatment effects on first- and second-order stigma from Equation (4) the sample by whether the sample's beliefs were not truthfully corrected (Columns 1 and 2) or were truthfully corrected (Columns 3 and 4). *, **, and *** indicate $p < 0.1$, 0.05, and 0.01, respectively.

D Calibration Appendix

Empirical Version of Welfare Impact Equation. We use a specialized version of Equation (6) expressed in terms of elasticities:

$$\frac{dW}{dm} = \underbrace{\int_0^m \eta^i \frac{p^i_\theta p^s_\theta}{m} \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta\right] d\theta}_{\text{(i) woodwork effect (information channel only)}} + \underbrace{\int_0^m \left[\int_{c<u^A_\theta - u^N_\theta} p^i_\theta \frac{dH_\theta(c \mid m)}{dm} dc + p^i_\theta \lim_{c \to c^{\text{lb}}_\theta} \left[c \frac{dH_\theta(c \mid m)}{dm}\right]\right] d\theta}_{\text{(ii) stigma reduction}}$$

$$+ \underbrace{p^i_m p^s_m \left[u^A_m - E[c_m \mid c_m < u^A_m - u^N_m] - u^N_m\right]}_{\text{(iii) newly eligible and enrolled (benefits)}} - \underbrace{\int_0^m \left[\eta \frac{p^i_\theta p^s_\theta}{m} G_\theta\right] d\theta}_{\text{(iv) woodwork effect (costs)}} - \underbrace{p^i_m p^s_m G_m}_{\text{(v) newly eligible (costs)}} \tag{D.1}$$

for woodwork-effect elasticities $\eta^s = \frac{dp^s_\theta}{dm}\frac{m}{p^s_\theta}$, $\eta^i = \frac{dp^i_\theta}{dm}\frac{m}{p^i_\theta}$ and $\eta = \frac{d\left(p^i_\theta p^s_\theta\right)}{dm}\frac{m}{p^i_\theta p^s_\theta}$, which we also assume are constant in θ. This expression implies that we want to calibrate the following parameters: $\{B_\theta, u^A_\theta, u^N_\theta, p^i_\theta, p^s_\theta, \eta^s, \eta^i, \frac{dH_\theta}{dm}, E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta]\}$. We operationalize our calibrations by forming 100 types θ, for each quantile of the income distribution. For each θ, we observe θ-specific take-up probability ($p^i_\theta p^s_\theta$) and average benefit size (B_θ). We take $m = 0.27$, roughly the average eligibility threshold in the CPS in 2016.

While specializing in several ways, Equation (D.1) does include a limit term in term (ii) that does not appear in Equation (6), which accounts for possibly unbounded cost distributions. See discussion of this term in Appendix E.1. Throughout, we assume this term is finite.

Calibrating Income. Our household income measure comes from the CPS, limited to state-years included in our main event study. We then calculate the average household income per percentile of the household income distribution. We equivalize household income y_θ by dividing by numbers of household members $\bar{e}_\theta := (N^{\text{adult}}_\theta + 0.7N^{\text{children}}_\theta)^{0.7}$ (Michael and Citro, 1995), where both are estimated for each θ from the CPS.

We impose a minimum household income, due to well-known sensitivity of CRRA utility with low values of income. Hoynes and Luttmer (2011) impose a floor of \$1,000 per *individual*. Finkelstein et al. (2019) impose a consumption floor of \$1,977 per individual, based on uninsured individuals in the Consumer Expenditure Surveys. We use \$2,000 per individual, assume that the household-equivalized minimum is $\$2000 \times \bar{e}$, and show sensitivity to halving or doubling it.

Benefit Size. In the QC data, we assign individuals their FPL percentile according to the CPS FPL distribution (i.e., their percentile in the overall population, not within the QC data). We then take averages of benefit size within these FPL percentile bins. This approach requires two caveats. The first caveat is that benefit sizes are based on net income and not gross income, while FPL is calculated using gross income; the population in the QC data might be selected on having lower net income (and therefore higher benefit amount) than a representative person in their FPL percentile bin. The second caveat is that the QC data presents benefit amounts monthly, and we multiply by 12 to present annual numbers, even though some households may not remain on SNAP for 12 months.

We equivalize household benefits by household size when they enter household utility. We do not equivalize when we consider fiscal costs. Intuitively, it costs the government B to provide the benefit, and the household gets a household-size-adjusted B_{adj} which enters u.

Fiscal Costs. Each household's fiscal cost is $G_\theta = p^i_\theta p^s_\theta B_\theta \tau$, the take-up probability times the benefit times a parameter $\tau \geq 1$. The parameter τ captures additional administrative costs, which we assume scale with the benefit size. We select $\tau = 1.189$, based on estimates in Isaacs (2008) (Figure 2A).

Calibrating Take-up Probabilities and Elasticities. To determine the overall takeup rate ($p^i p^s$), we count (weighted) individuals in the CPS and the QC data in 5-percentile bins of the FPL distribution and calculate the ratio of the CPS population to the QC population in each bin. We then interpolate between 5-percentile bins to get takeup rates at each percentile of the FPL distribution. Take-up rates are for individuals ranked by household income, to be consistent with Section 2, but is a slight discrepancy from the rest of the household analysis.

The FSPAS asks non-enrolled households whether they (a) have heard of SNAP; (b) think they are eligible for SNAP; and (c) are informed about how to enroll in SNAP. 36.3% of unenrolled households meet all three of these criteria. We start by estimating $\tilde{p}^i = 0.363(1-\bar{t}) + \bar{t}$, for mean enrollment rate $\bar{t}$. We then solve for the p^s_θ from the overall type-varying take-up rate $p^i p^s_\theta$.

As the ordeal costs never exceed welfare gains from enrollment, the logit assumption (discussed below) implies that $p^s_\theta \in (0.5, 1)$. We assume residual differences in take-up rates are due to stigma costs, such that p^s_θ is the observed take-up rate for quantile θ divided by $\tilde{p}^i$ but imposing that $p^s_\theta \in (0.55, 0.95)$. If p^s_θ lies at a corner, we then vary p^i_θ so that the $p^s_\theta \times p^i_\theta$ equals the empirical take-up rate. A limitation of this exercise is thus that p^s_θ is always larger than 0.5, which, given the small ordeal costs below, imposes strong restrictions on p^i_θ and the scale parameter.

To obtain the woodwork-effect elasticities, we use that $\eta = \eta^s + \eta^i$, and we further write that $\eta^s = \rho\eta^i$ for $\rho \in \mathbb{R}$. Based on the empirical findings in Section 3, we assume that most of the woodwork effect operates through an information channel, such that $\rho = 1/2$ and $2/3$ of the woodwork effect comes from information. We show sensitivity to this choice.

Take-up Costs. Take-up costs depend on the combination of ordeal costs and the role of the eligibility threshold:

$$c_\theta = \underbrace{o_\theta}_{=\text{WTP to avoid ordeal}} + \varepsilon \tag{D.2}$$

assuming that ε is drawn from a mean-zero logit distribution with scale parameter σ_θ: $\varepsilon \sim \text{Logit}(0, \sigma_\theta)$. Changes in the eligibility threshold affect σ_θ.

Following Finkelstein and Notowidigdo (2019), we assume the ordeal would cost households \$75 (= 5 hours of foregone earnings, from Ponza et al. (1999), at \$15 per hour). Because utility is nonlinear, we convert the ordeal into utils by defining the ordeal for an individual of type θ as:

$$o_\theta = u(y_\theta + B_\theta + tc) - u(y_\theta + B_\theta), \tag{D.3}$$

where tc is the time cost, \$75. That is, o_θ is the utility difference between having an additional \$75 on top of current income, versus only having current income (without benefits). When we obtain our money-metric welfare expression, we divide each type's utility by the average marginal utility of society. We interpret this as society's willingness to pay for the policy change. Accordingly, society's preference to avoid ordeals is higher for low-income individuals than for high-income individuals.

The logit functional form yields a parametric expression for the expected cost term given p^s_θ and a parametric expression for utility.[45] We start by observing:

$$H_\theta(u^A_\theta - u^N_\theta - o_\theta \mid m) = \frac{\exp\left(\frac{u^A_\theta - u^N_\theta - o_\theta}{\sigma_\theta}\right)}{\exp\left(\frac{u^A_\theta - u^N_\theta - o_\theta}{\sigma_\theta}\right) + 1}. \tag{D.4}$$

Given the assumptions above, we observe $H_\theta(u^A_\theta - u^N_\theta - o|m) = p^s_\theta$. Then we can solve for σ_θ. With σ_θ in hand, the expected cost term E_θ has a parametric expression, via logit manipulations. In particular, we use the following fact about a logit random variable $\varepsilon \sim \text{Logit}(0, \sigma)$:

$$E[\varepsilon \mid \varepsilon < \kappa] = \kappa - \left(\frac{1 + \exp\left(\frac{\kappa}{\sigma}\right)}{\exp\left(\frac{\kappa}{\sigma}\right)}\right)\left(\sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right)\right), \tag{D.5}$$

which we prove in Appendix E as it is not easy to find in online references. We then put $\kappa \equiv u^A_\theta - u^N_\theta - o_\theta$.

Utility. We posit constant relative risk aversion over income y_θ with risk aversion parameter γ. The utility

[45] Our expression does not directly incorporate how costs depend on m. We explore the consequences of this limitation with an alternative uniformity assumption.

if enrolled is $u_\theta^A(B_\theta, y_\theta) = \frac{(B_\theta+y_\theta)^{1-\gamma}}{1-\gamma}$. Utility if not enrolled is $u_\theta^N(B_\theta, y_\theta) = \frac{y_\theta^{1-\gamma}}{1-\gamma}$. Both B_θ and y_θ are equivalized as above. We obtain average household income y_θ from the CPS, imposing a minimum income floor of $2,000 since CRRA utility is sensitive to very low values (Finkelstein et al., 2019).

To calibrate risk aversion γ, we select $\gamma = 2.1866$ so that terms (iii) plus term (v) of Equation (D.1) equal zero. We motivate this choice with the inverse-optimum assumption (see discussion in text).

Welfare Impact of Stigma Reductions. We parameterize the welfare effects of stigma reduction, term (ii) in Equation (D.1), as follows:

$$\text{WelfareImpact}_{\text{StigmaReduction}} = p_\theta^i p_\theta^s \times \frac{\rho}{\rho+1} \times (u_\theta^A - E[c_\theta \mid c_\theta < u_\theta^A - u_\theta^N] - u_\theta^N) \times \psi, \tag{D.6}$$

for a constant factor $\psi \in \mathbb{R}^+$ to be calibrated. The take-up rate ($p_\theta^i p_\theta^s$) is a natural scaling factor, since this is the share of the population that gets to enjoy stigma reductions. The share of the woodwork effect elasticity that depends on stigma ($\rho/\rho+1$) reflects the extent to which stigma costs change with a change in the means test. Finally, we include a constant factor (ψ) for the share of utility gains $(u_\theta^A - E[c_\theta \mid c_\theta < u_\theta^A - u_\theta^N] - u_\theta^N)$ from enrollment that are equivalent to a decrease in stigma. As an example, if $\psi = 0.5$, that implies that the change in stigma is valued at half the welfare gains from enrollment.

Of course, there always exists a ψ such that Equation (D.6) holds, but we do not observe it. We choose ψ to be small, to be adversarial for our conclusion that woodwork effects confer utility. In particular, we select a ψ such that the willingness to pay for a stigma reduction, if the eligibility threshold moved from $m = 0.27$ to $m = 0.37$, would be 2% of the welfare gains from enrolling for an uninformed person. Uncertainty about how much people value reductions in stigma suggests that future research to measure this parameter would be valuable.

As noted, we assume that the welfare impact of stigma reductions is finite, that is, that the limit term in Equation (D.1) is non-infinite. For instance, it is sufficient to assume that $dH(c \mid m)/dm = 0$ for all $c < \underline{c}$ with $\underline{c} \in \mathbb{R}$. Intuitively, if the eligibility threshold only affects stigma for ordeal costs that are not too negative (i.e., for households who do not draw too positive idiosyncratic shocks), the condition is met. As we calibrate the expression in a highly reduced-form manner, we posit that the overall willingness to pay (net of the limit term, which may go to zero or be finite) is given as in Equation (D.6).

E Proofs and Additional Theory

E.1 Proof of Proposition 1

Proof. We can write society's willingness to pay as

$$W = \int_0^m \left[\underbrace{\int_{c < u_\theta^A - u_\theta^N} p_\theta^i [u_\theta^A - c] h_\theta(c \mid m) dc}_{\text{Enrollees}} + \underbrace{\int_{u_\theta^A - u_\theta^N \le c} p_\theta^i u_\theta^N h_\theta(c \mid m) dc}_{\text{Non-enrollees due to costs}} + \underbrace{(1 - p_\theta^i) u_\theta^N}_{\text{Non-enrollees due to lack of information}} \right] d\theta$$

$$+ \underbrace{\int_m^1 u_\theta^N d\theta}_{\text{Non-enrollees because ineligible}} - \text{Fiscal Cost.} \tag{E.1}$$

The bounds on integrals with c reflect that the cdf H may be have finite or infinite support.
We show how welfare of SNAP enrollees changes with a change in m.
Write the fiscal costs as:

$$\text{Fiscal Cost} = \underbrace{\int_0^m p_\theta^i p_\theta^s G_\theta d\theta}_{\text{Additional fiscal cost of enrollees}} + \underbrace{\underline{G}_\theta}_{\text{Fiscal cost of non-enrollees}} \tag{E.2}$$

where $\underline{G}_\theta$ is the fiscal costs of non-enrollees, and G_θ is the additional fiscal costs of enrollees. We assume that $\frac{d\underline{G}_\theta}{dm} = 0$, that is, non-enrollees' fiscal costs are unchanged from the reform.
Using Leibniz's rule and writing $\Delta u_\theta := u_\theta^A - u_\theta^N$, the derivative of Equation (5) is

$$\frac{dW}{dm} = \underbrace{\int_{c<\Delta u_\theta} p_m^i [u_m^A - c] h_m(c \mid m) dc + (1 - p_m^i) u_m^N + \int_{c \ge \Delta u_\theta} p_m^i u_m^N h_m(c \mid m) dc - u_m^N}_{\equiv \text{Term A (newly eligible)}}$$
$$+ \underbrace{\int_0^m \int_{c<\Delta u_\theta} p_\theta^i [u_\theta^A - c] \frac{dh_\theta}{dm} dc + \int_0^m \int_{c \ge \Delta u_\theta} p_\theta^i u_\theta^N \frac{dh_\theta}{dm} dc}_{\equiv \text{Term B (change in stigma)}}$$
$$+ \underbrace{\int_0^m \int_{c<\Delta u_\theta} \frac{dp_\theta^i}{dm} [u_\theta^A - c] h_\theta(c \mid m) dc + \int_0^m \int_{c \ge \Delta u_\theta} \frac{dp^i}{dm} u_\theta^N h_\theta(c \mid m) dc d\theta - \int_0^m \frac{dp_\theta^i}{dm} u_\theta^N d\theta}_{\equiv \text{Term C (change in information)}}$$
$$+ \underbrace{\int_0^m \left[\frac{d(p_\theta^i p_\theta^s)}{dm} G_\theta + p_\theta^i p_\theta^s \frac{dG_\theta}{dm} \right] d\theta + p_m^i p_m^s G_m}_{\equiv \text{Term D (fiscal costs)}}. \tag{E.3}$$

Write $\bar{c}_\theta := E[c_\theta \mid c_\theta < \Delta u_\theta]$. Observe that $\int_{c<\Delta u_\theta} c h_\theta(c|m) dc = \bar{c}_\theta H_\theta(\Delta u_\theta \mid m)$.
We simplify Terms A, B, and C as follows. Evaluating the integral in Term A gives

$$\text{Term A} = p_m^i \left(u_m^A - \bar{c}_m \right) H_m (\Delta u_m \mid m) + \left(1 - p_m^i \right) u_m^N + p_m^i u_m^N - p_m^i u_m^N H_m (\Delta u_m \mid m) - u_m^N \tag{E.4}$$
$$= p_m^i \left(u_m^A - \bar{c}_m - u_m^N \right) H_m (\Delta u_m \mid m) \tag{E.5}$$

$$= p_m^i p_m^s \left(u_m^A - \bar{c}_m - u_m^N \right). \tag{E.6}$$

Toward simplifying Term B, evaluating the integrals over stigma costs (using integration by parts for the first term) gives:

$$\begin{aligned} \text{Term B} = \int_0^m \Bigg[& p_\theta^i (u_\theta^A - (u_\theta^A - u_\theta^N)) \frac{dH_\theta(\Delta u_\theta \mid m)}{dm} - \lim_{c \to c_\theta^{\text{lb}}} \left[p_\theta^i (u_\theta^A - c) \frac{dH_\theta(c \mid m)}{dm} \right] \\ & + \int_{c < \Delta u_\theta} p_\theta^i \frac{dH_\theta(c \mid m)}{dm} dc + p_\theta^i u_\theta^N \left(\lim_{c \to c_\theta^{\text{ub}}} \left[\frac{dH_\theta(c \mid m)}{dm} \right] - \frac{dH_\theta(\Delta u_\theta \mid m)}{dm} \right) \Bigg] d\theta, \end{aligned} \tag{E.7}$$

where $c_\theta^{\text{lb}} \in \mathbb{R} \cup \{-\infty\}$ and $c_\theta^{\text{ub}} \in \mathbb{R} \cup \{\infty\}$ are lower and upper bounds of H_θ's support.

Noticing that $\lim_{c \to c_\theta^{\text{lb}}} \frac{dH_\theta(c|m)}{dm} = \lim_{c \to c_\theta^{\text{ub}}} \frac{dH_\theta(c|m)}{dm} = 0$, Term B simplifies to:

$$\text{Term B} = \int_0^m \left[\int_{c < \Delta u_\theta} p_\theta^i \frac{dH_\theta(c \mid m)}{dm} dc + p_\theta^i \lim_{c \to c_\theta^{\text{lb}}} \left[c \frac{dH_\theta(c \mid m)}{dm} \right] \right] d\theta. \tag{E.8}$$

For Term C, we first evaluate the integral over stigma costs:

$$\text{Term C} = \int_0^m \frac{dp_\theta^i}{dm} [u_\theta^A - \bar{c}_\theta] H_\theta(\Delta u_\theta \mid m) d\theta + \int_0^m \frac{dp^i}{dm} u_\theta^N [1 - H_\theta(\Delta u_\theta \mid m)] d\theta - \int_0^m \frac{dp_\theta^i}{dm} u_\theta^N d\theta. \tag{E.9}$$

Collecting terms and substituting $H_\theta(\Delta u_\theta \mid m) = p_\theta^s$, gives

$$\text{Term C} = \int_0^m \frac{dp_\theta^i}{dm} [u_\theta^A - \bar{c}_\theta - u_\theta^N] p_\theta^s d\theta. \tag{E.10}$$

Term D needs no simplification. Adding Terms A, B, C, and D delivers the desired equation, augmented with the limit term in Term B.

This limit term accounts for issues at infinity. To recover Proposition 1 without the limit term, notice that with bounded support, $\lim_{c \to c_\theta^{\text{lb}}} \left[c \frac{dH_\theta(c|m)}{dm} \right] = 0$.

Returning to an H_θ with unbounded support, another way of eliminating the term is to assume that there exists a $\underline{c} \in \mathbb{R}$ such that $\frac{dH_\theta(c|m)}{dm} = 0$ for all $c < \underline{c}$. That is, under this assumption, the eligibility threshold affects only ordeal costs and not hugely positive idiosyncratic benefits from participation. More generally, the limit term disappears for plausible dH_θ / dm that vanish faster than c grows. □

E.2 A case where $dW^u/dm = 0$ implies that $dW/dm > 0$

Suppose the unaware social planner has optimized, such that $dW^u/dm \approx 0$. We provide sufficient conditions under which the existence of woodwork effects imply that the eligibility threshold should rise. Here we maintain the restriction of H_θ to cases with bounded support, and focus on cases where $\frac{dp_\theta^i}{dm} \geq 0$.

Assumption 1. *For all types θ and θ', $\theta' < \theta$ if and only if $\Delta u_{\theta'} - \bar{c}_{\theta'} - G_{\theta'} > \Delta u_\theta - \bar{c}_\theta - G_\theta$.*

Assumption 2. $\int_0^m p_\theta^i p_\theta^s \frac{dG_\theta}{dm} d\theta = 0$.

Proposition 2. *Suppose Assumptions 1 and 2 holds. If the unaware planner sets the eligibility threshold optimally, ($\frac{dW^u}{dm} = 0$) and woodwork effects come from information ($\frac{dH_\theta(\Delta u_\theta|m)}{dm} = 0$ for all θ), then $\frac{dW}{dm} > 0$.*

Assumption 1 holds when the net social gain from new enrollment is decreasing in the type distribution. Put another way, the expected gain from take-up, less the social cost, is higher for low types. This condition holds if benefits B_θ and fiscal costs are constant in θ and lower types get more utility from consuming B than marginals. On the other hand, differences in labor supply responses across types could break this condition.

For example, if new already-eligible enrollees (types $\theta < m$) have larger labor supply responses, and thus impose a larger fiscal externality, than the newly eligible enrollees (types m), the assumption may not hold.

Assumption 2 holds when raising the threshold does not affect the social cost of enrollment of those already enrolled. For example, this assumption rules out complementarities between stigma reductions and labor supply.

This proposition gives plausible sufficient conditions under which detecting woodwork effects implies $dW/dm > 0$. In particular, if Assumptions 1–2 and if woodwork effects are driven entirely by information, the optimal eligibility threshold in the presence of woodwork effects is higher than the optimal threshold set without knowledge of woodwork effects. The intuition for the proposition is that, if a social planner is indifferent to spending G_m to enroll type m, she must have positive welfare from spending G_θ to enroll type $\theta < m$.

E.3 Proof of Proposition 2

Proof. The proposition supposes $\frac{dW^u}{dm} = 0$. That is,

$$p^i_m H_m(\Delta u_m \mid m) \left([u^A_m - \bar{c}_\theta - u^N_m] - G_m \right) = 0. \tag{E.11}$$

From Proposition 1, we then have:

$$\begin{aligned} \frac{dW}{dm} = & \underbrace{\int_0^m \frac{dp^i_\theta}{dm} p^s_\theta \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta \right] d\theta}_{\text{(i) woodwork effect (information channel only)}} + \underbrace{\int_0^m \int_{c<u^A_\theta - u^N_\theta} p^i_\theta \frac{dH_\theta(c \mid m)}{dm} dc d\theta}_{\text{(ii) stigma reduction}} \\ & \underbrace{- \int_0^m \left[\frac{d(p^i_\theta p^s_\theta)}{dm} G_\theta + p^i_\theta p^s_\theta \frac{dG_\theta}{dm} \right] d\theta}_{\text{(iv) woodwork effect (costs)}} . \end{aligned} \tag{E.12}$$

As we additionally assume that $\frac{dH_\theta(c|m)}{dm} = 0$, so $\frac{dp^s_\theta}{dm} = 0$, as well as $\int_0^m p^i_\theta p^s_\theta \frac{dG_\theta}{dm} d\theta = 0$, it is sufficient to show:

$$\frac{dW}{dm} = \underbrace{\int_0^m \frac{dp^i_\theta}{dm} p^s_\theta \left[u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta \right] d\theta}_{\text{(i) woodwork effect (information channel only)}} - \underbrace{\int_0^m \frac{dp^i_\theta}{dm} p^s_\theta G_\theta d\theta}_{\text{(iv) woodwork effect (costs)}} . \tag{E.13}$$

Collecting terms gives:

$$\int_0^m \frac{dp^i_\theta}{dm} \left([u^A_\theta - \bar{c}_\theta - u^N_\theta] - G_\theta \right) H_\theta(\Delta u_\theta \mid m) d\theta > 0. \tag{E.14}$$

But $H_\theta(\Delta u_\theta \mid m) \geq 0$ and $\frac{dp^i_\theta}{dm} \geq 0$. It is sufficient (but not necessary) to show that $[u^A_\theta - \bar{c}_\theta - u^N_\theta] - G_\theta > 0$ for all $\theta < m$.

Note that Equation (E.11) implies that $\left([u^A_m - \bar{c}_m - u^N_m] - G_m \right) = 0$. By Assumption 1, the social gain of enrolling a lower type is higher than the social gain of enrolling a high type:

$$[u^A_\theta - \bar{c}_\theta - u^N_\theta] - G_\theta > [u^A_m - \bar{c}_m - u^N_m] - G_m = 0. \tag{E.15}$$

Thus, $[u^A_\theta - \bar{c}_\theta - u^N_\theta] - G_\theta > 0$ implies $\frac{dW}{dm} > 0$, completing the proof. □

E.4 Proof of Equation (D.5)

Proof. Let $E_l(\kappa, \sigma) := E[\varepsilon \mid \varepsilon < \kappa]$ for $\varepsilon \sim \text{Logit}(0, \sigma)$ and $\kappa \in \mathbb{R}$. Observe that:

$$E[\min\{\varepsilon, \kappa\}] = -E[\max\{-\varepsilon, -\kappa\}] \tag{E.16}$$

$$\iff E[\varepsilon \mid \varepsilon < \kappa] \Pr(\varepsilon < \kappa) + \kappa \Pr(\varepsilon \geq \kappa) = -\sigma \ln\left(\exp\left(\frac{-\kappa}{\sigma}\right) + 1\right) \tag{E.17}$$

$$\iff E[\varepsilon \mid \varepsilon < \kappa] \Pr(\varepsilon < \kappa) + \kappa \Pr(\varepsilon \geq \kappa) = \kappa - \sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right) \tag{E.18}$$

$$\iff E_l(\kappa, \sigma) = \frac{1 + \exp\left(\frac{\kappa}{\sigma}\right)}{\exp\left(\frac{\kappa}{\sigma}\right)} \left(\kappa - \sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right) - \kappa \frac{1}{1 + \exp\left(\frac{\kappa}{\sigma}\right)}\right) \tag{E.19}$$

$$\iff E_l(\kappa, \sigma) = \kappa - \sigma \left(\frac{1 + \exp\left(\frac{\kappa}{\sigma}\right)}{\exp\left(\frac{\kappa}{\sigma}\right)}\right) \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right) \tag{E.20}$$

where the second line uses the expectation of the maximum of the logit, and the third line can be shown by using that:

$$-\sigma \ln\left(\exp\left(\frac{-\kappa}{\sigma}\right) + 1\right) = -\sigma \ln\left(\frac{1}{\exp\left(\frac{\kappa}{\sigma}\right)} + 1\right) \tag{E.21}$$

$$= -\left(\sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right) - \sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right)\right)\right) \tag{E.22}$$

$$= \kappa - \sigma \ln\left(\exp\left(\frac{\kappa}{\sigma}\right) + 1\right), \tag{E.23}$$

and the remaining lines algebraically manipulate and use the logit PDF. Equation (E.20) is what we want to show, concluding the proof. □