Online Appendix for The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP

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A Data and Institutional Context

A.1 SNAP Sample Construction

We build off the sample in Ganong and Liebman (2018a), 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 (2018a), 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 (2018a), 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.

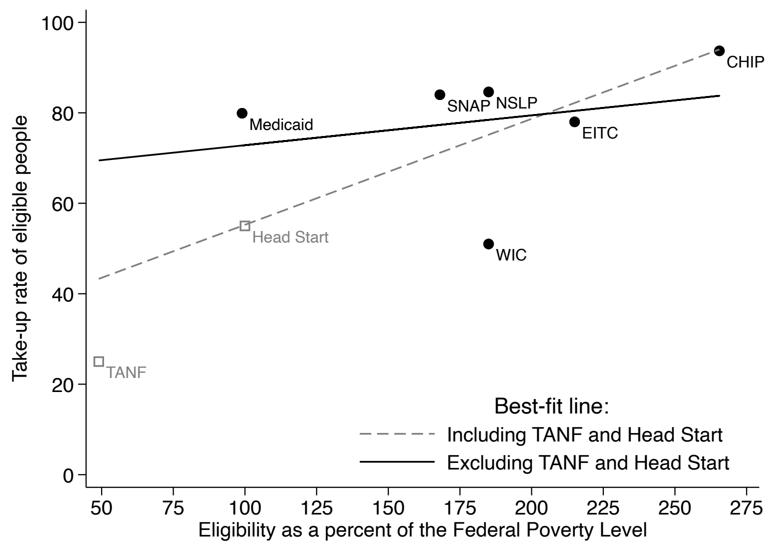
A.5 Figure 1 Details

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.[1] 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.

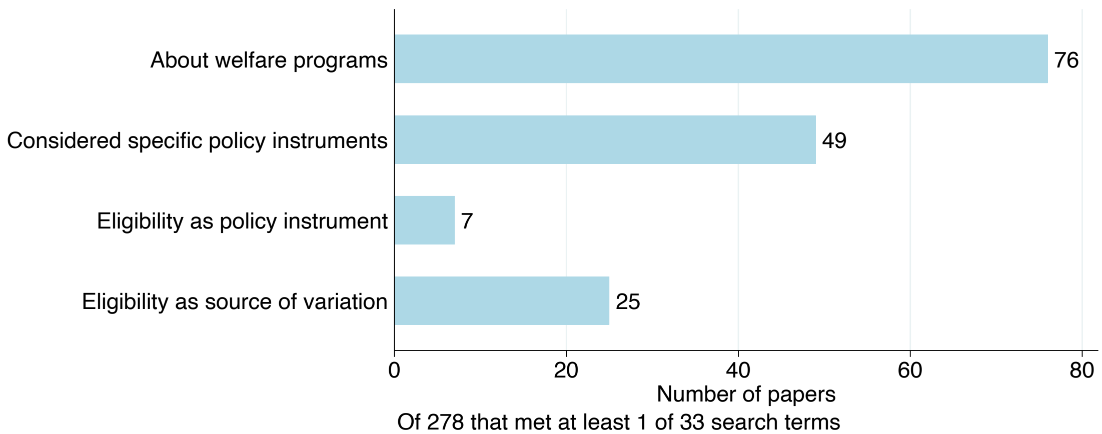
- We exclude papers that are principally about optimal income or capital taxation.
- We exclude transfers that are not intended to alleviate poverty (e.g., the effects of giving people computers).
- We exclude papers about credit market restrictions only, such as papers about mortgage deductions. We 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.

[1] 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

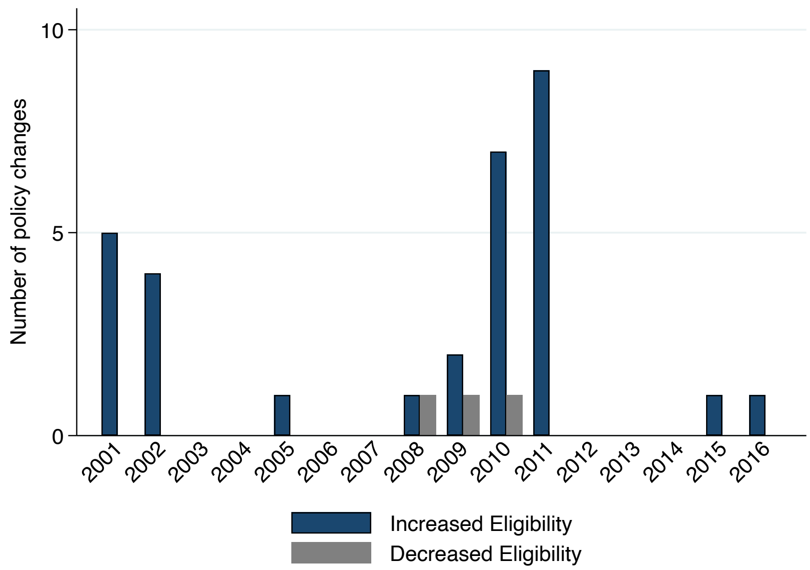
Figure A2: Literature Review: *AER* and *QJE* papers about Eligibility Criteria



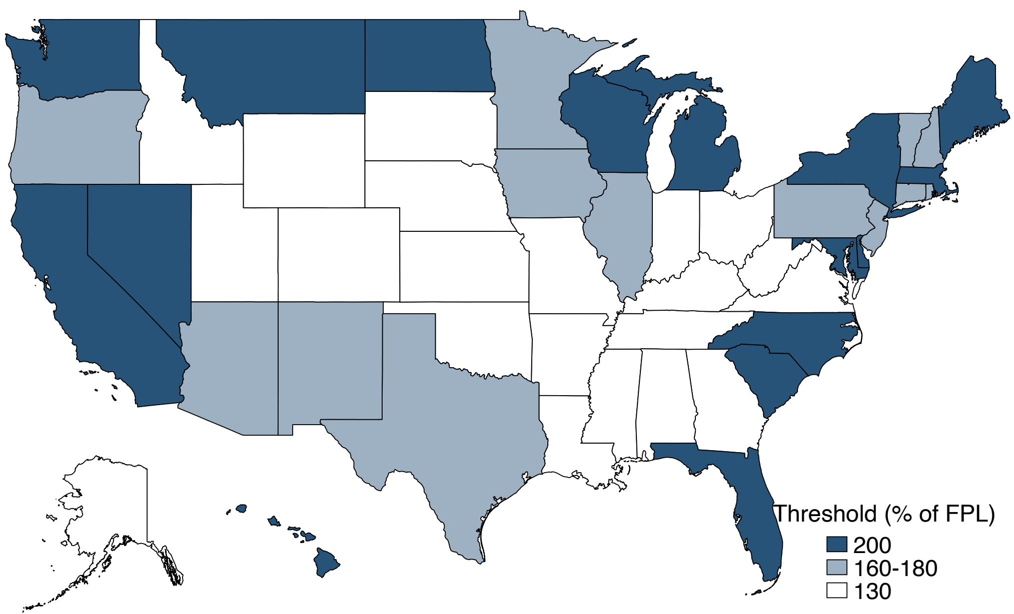
The figure presents the results from our literature review of papers in the *Quarterly Journal of Economics* (2010–2019) and the *American Economic Review* (2010–2018). Appendix A provides details about the sampling frame. The first row shows the total number of papers that we concluded were about welfare 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



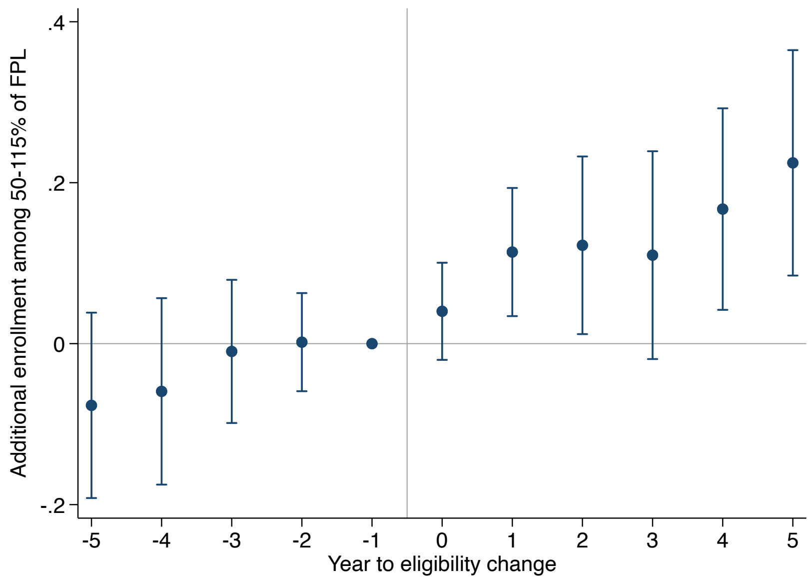
(B) Map of States that Implement Eligibility Expansions



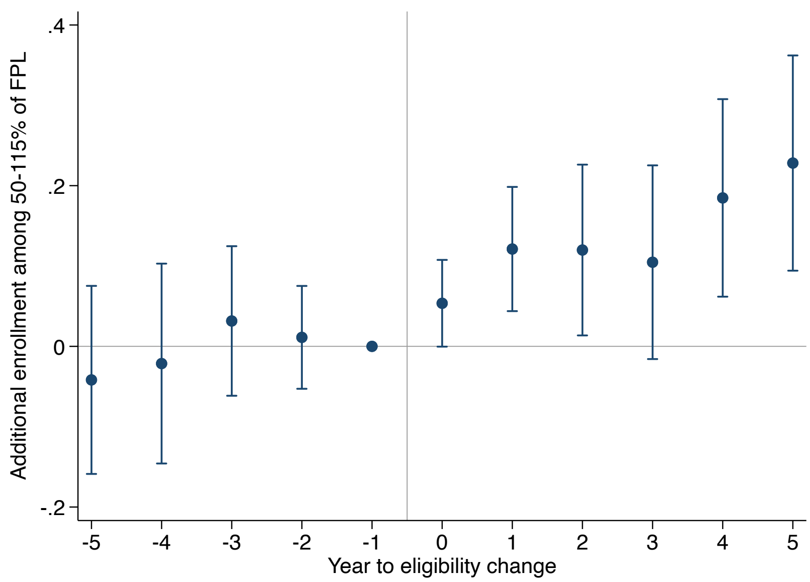
Panel A presents the number of states in each year that increased (blue bars) or decreased (gray bars) eligibility to the Supplemental Nutrition Assistance Program. Four states are counted twice, because they exhibit multiple changes. Panel B presents the maximum gross income eligibility threshold in 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

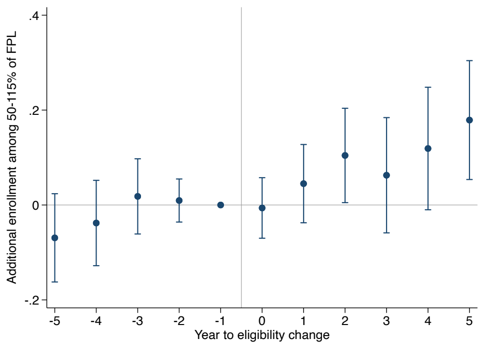


(B) Sample: 50 to 115% of FPL, Only Denominator Control

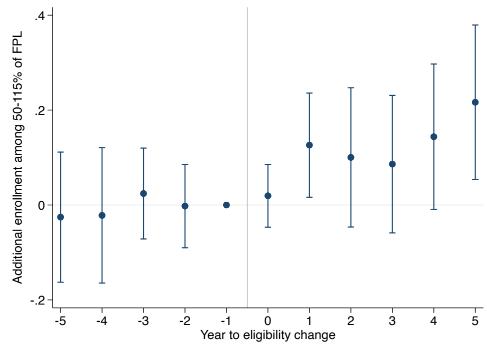


This figure is similar to Figure 3B, but Panel A presents the specification with no controls beyond state and year fixed effects. Panel B presents the specification with state and year fixed effects, only controlling for the log of the total number of people between 50 and 115% of the FPL (from the CPS).

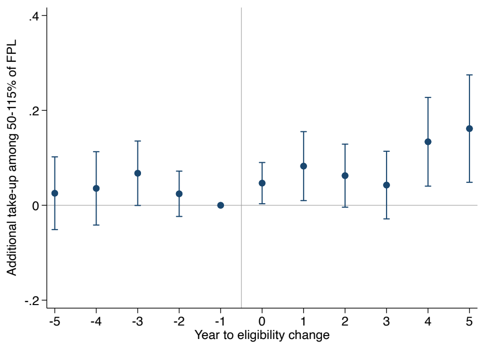
Figure A5: Extra robustness checks



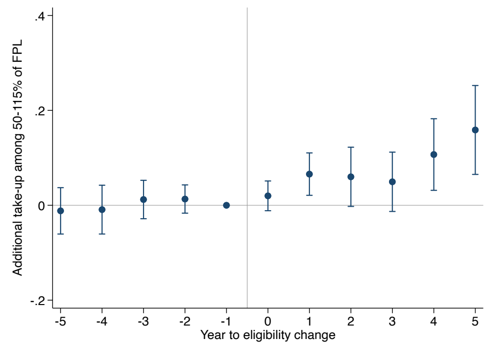
(A) Monthly data



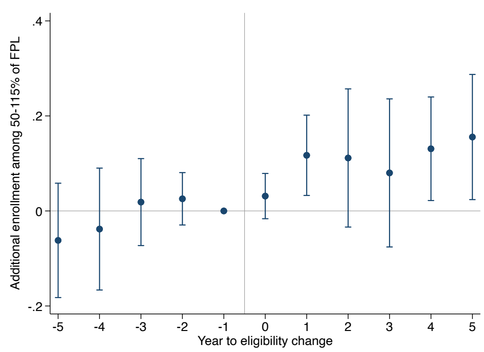
(B) Only Households with Dependents



(C) Take-up



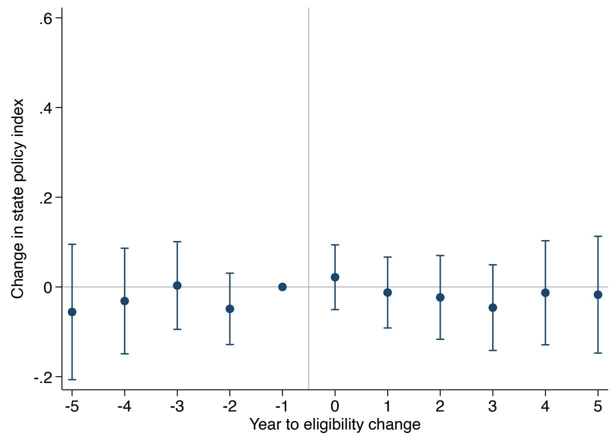
(D) Take-up, controlling for Ln(CPS count in 50-115%)



(E) Enrollment, weighted by state-year population

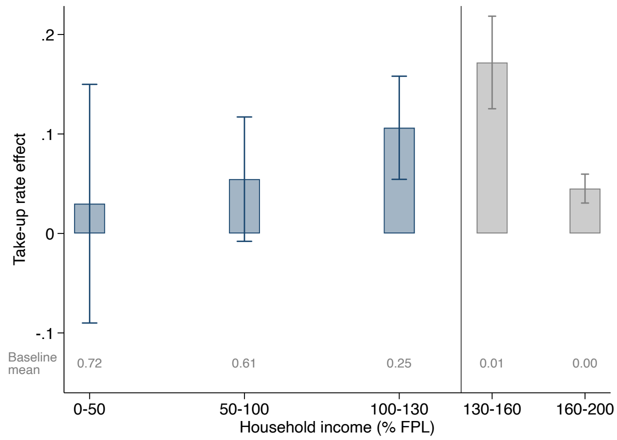
Panel A presents the results of estimating Equation (2) with monthly data instead of annual data. Panel B includes only SNAP recipients with any dependents—households that will not be affected by ABAWD work waivers. Panels C and D use the take-up share instead of the log of enrollment as the regressand, 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.

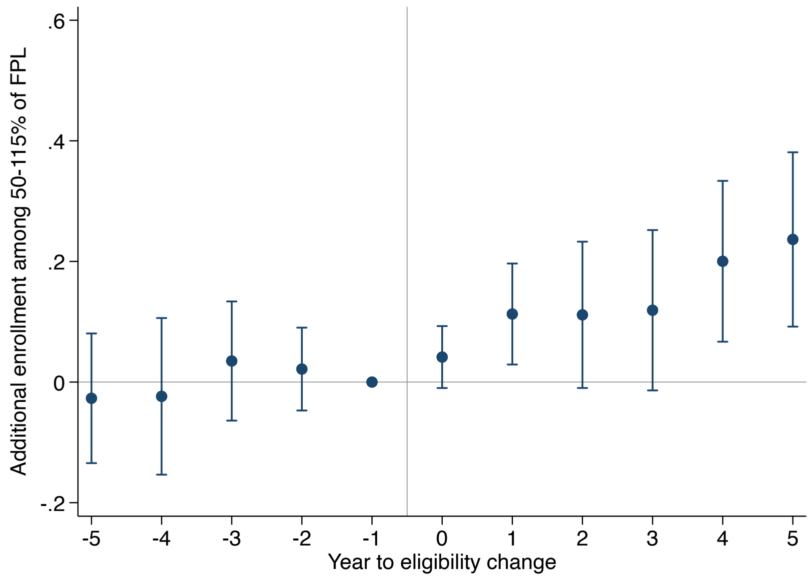
Figure A7: Effect on Take-Up Rates by Income Group



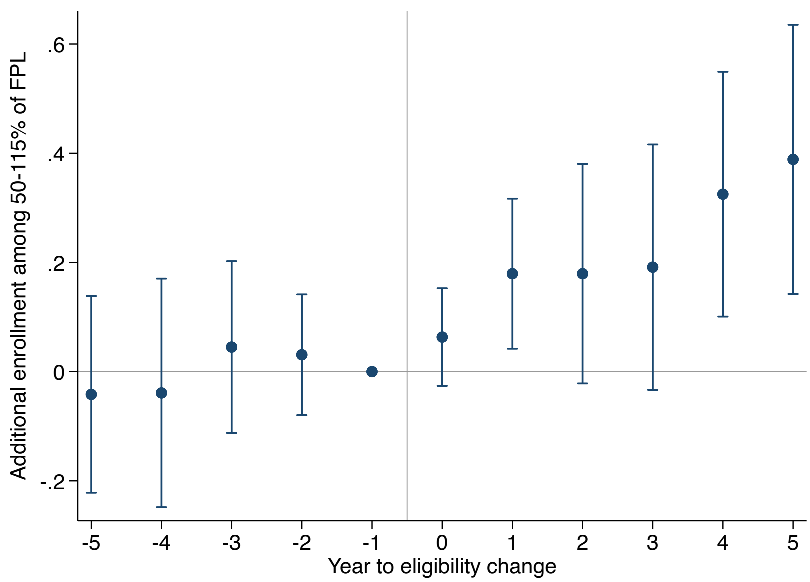
This figure presents estimates of Equation (2) using take-up rates as the outcome variable. The bars show the effect of the eligibility threshold on SNAP take-up by income group, and the whiskers show the 95% confidence intervals. While the regression specification is the same for all bars (with only 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)



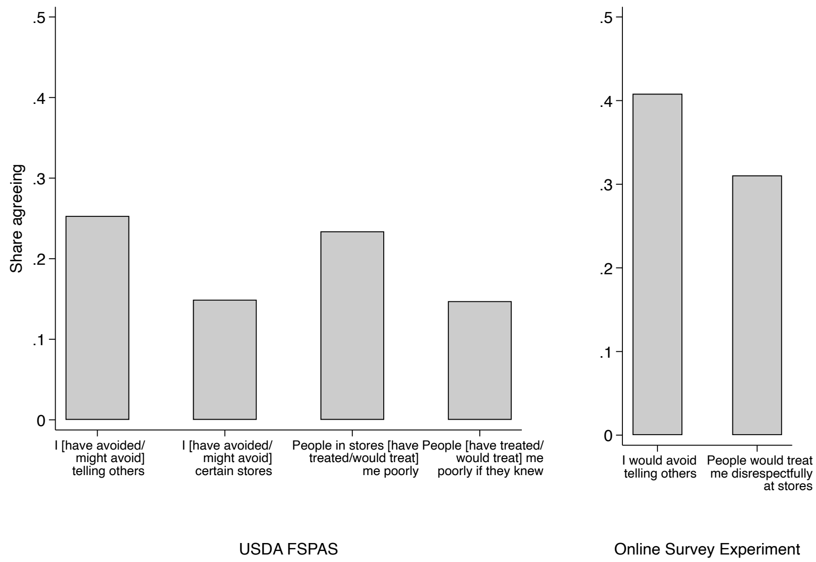
(B) Sun and Abraham (2021) Estimator



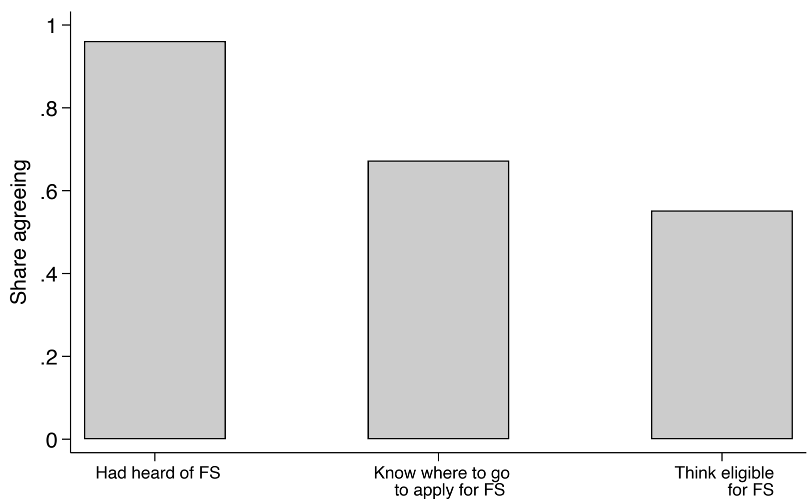
This figure presents heterogeneity-robust event study estimates using the 50–115% sample. Panel A presents the "stacked estimator" developed in Cengiz et al. (2019). For each treated state, we form a dataset keeping just one treated state and all never-treated states. We then stack all datasets and estimate 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.

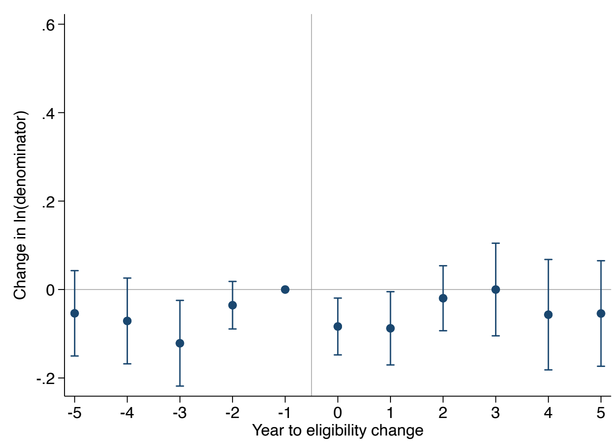
There may also be measurement error in the timing of the policy implementation.[2] We use annual data in

[2] 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

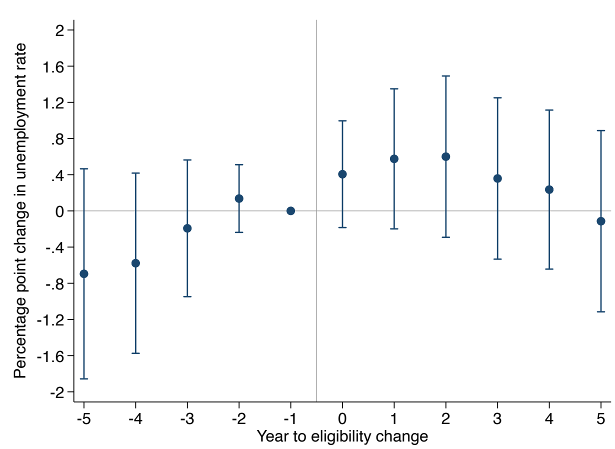
our main specification because we measure the number of people who are eligible from the March CPS, which is only available annually. Moreover, the QC data contain relatively few people at the month-state-income group level. However, BBCE policies can be implemented mid-year. In Figure A5A, we show our 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



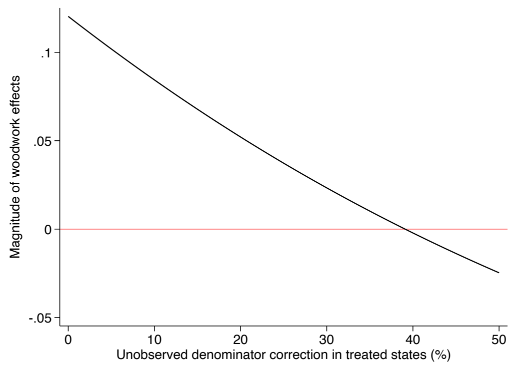
(B) Unemployment Rate



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.[3]

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%).

[3]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

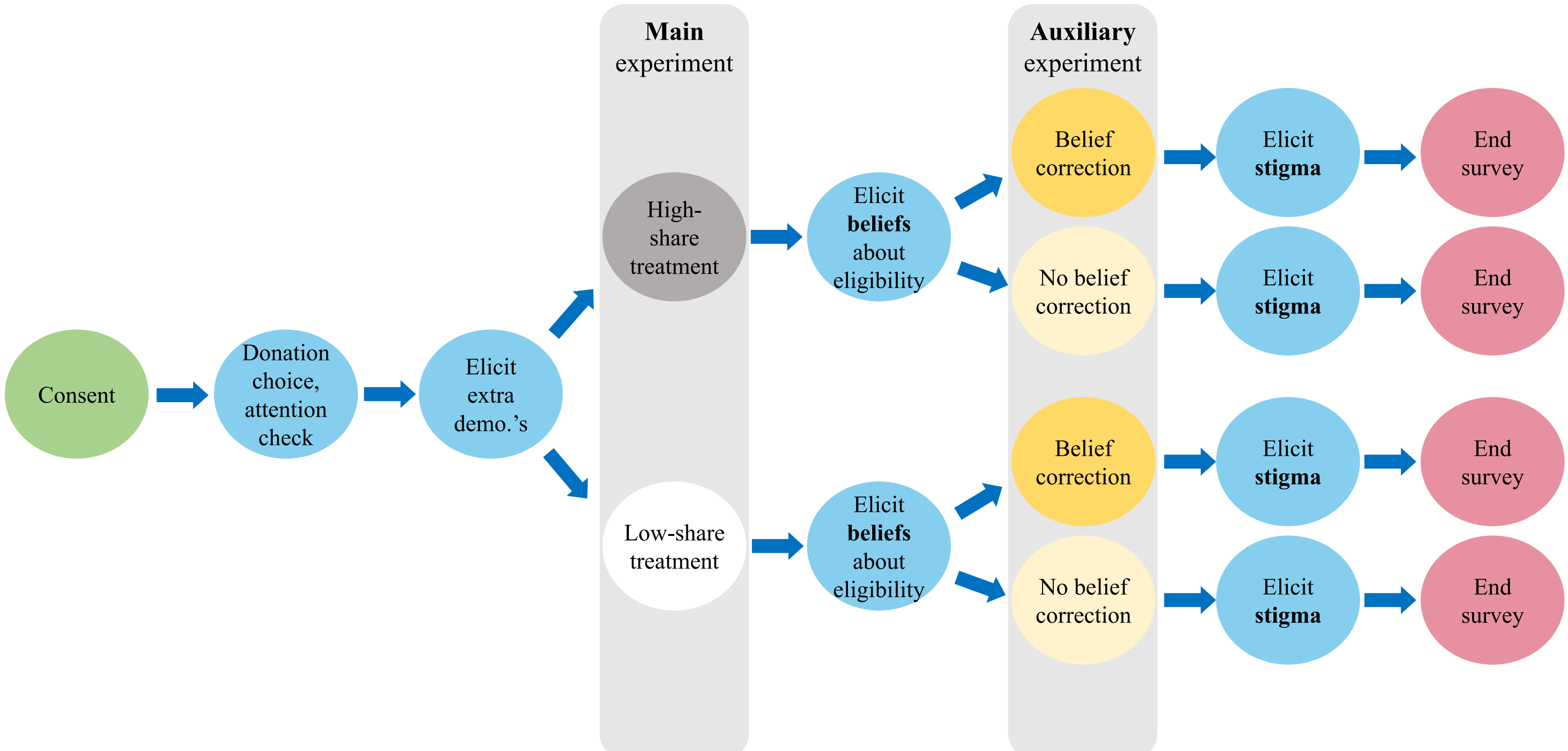
Demographics. We focus on the following demographic variables: female/non-female, white/non-white, age group (0–18, 19–30, 31–65, 66+), household size (1, 2, 3, or 4), and income decile (grouping deciles 40–70 and 70–100). To construct demographic *cells*, we fully interact each variable. For instance,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

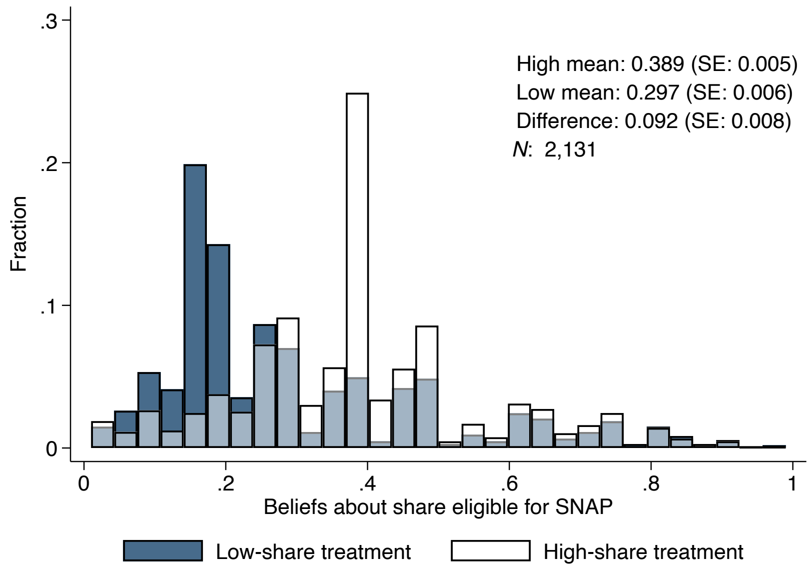
C.4 Additional Figures

Figure A12: Visual Depiction of Experiment Design



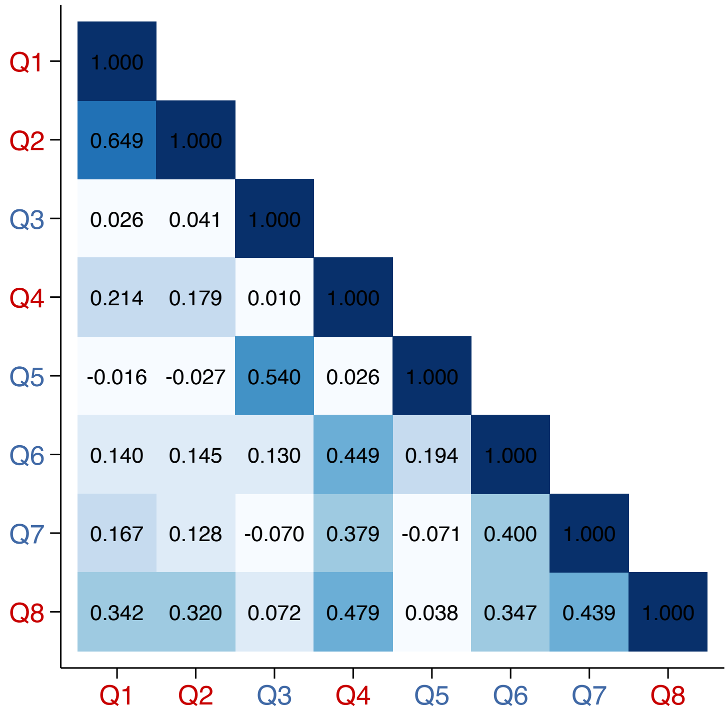
This figure presents the experiment design. The donation choice was to one of four charities (used to incentivize belief elicitation). We elicited several demographics (in addition to those provided by Lucid).

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



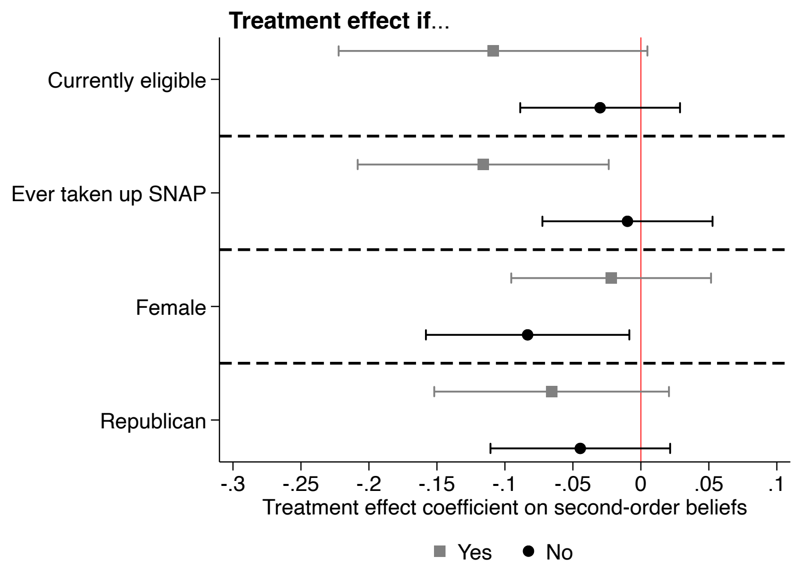
This figure presents the distribution of beliefs from the online experiment, split by treatment group, about the fraction of people who are eligible for SNAP. The *y*-axis shows the share of people within each treatment group who report a given fraction are eligible for SNAP. The blue bars show the values 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** (**0.016**)	**0.018** (**0.033**)	**−0.001** (**0.015**)	**0.005** (**0.033**)
2. Bad priors	237	0.002 (0.011)	0.008 (0.024)	-0.000 (0.011)	0.003 (0.025)
3. Attrited before share treatment	49	0.004 (0.005)	-0.004 (0.010)		
4. Attrited at or after treatment	126	0.006 (0.008)	0.007 (0.019)	-0.001 (0.008)	0.013 (0.019)
5. Omitted any stigma answers	107	0.002 (0.008)	0.005 (0.016)	-0.006 (0.008)	-0.013 (0.017)
6. Inattentive	106	0.000 (0.008)	0.021 (0.013)	0.005 (0.008)	0.003 (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 (U.S. Census Bureau and U.S. Bureau of Labor Statistics, 2019).

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 (U.S. Census Bureau and U.S. Bureau of Labor Statistics, 2019).

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 ≥ 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:

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

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

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 $\eta = \frac{d(p^i_\theta p^s_\theta)}{dm}\frac{m}{p^i_\theta p^s_\theta}$, 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.[4] 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 if enrolled is $u^A_\theta(B_\theta, y_\theta) = \frac{(B_\theta + y_\theta)^{1-\gamma}}{1-\gamma}$. Utility if not enrolled is $u^N_\theta(B_\theta, y_\theta) = \frac{y_\theta^{1-\gamma}}{1-\gamma}$. 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:

$$\text{WelfareImpact}_{\text{StigmaReduction}} = p^i_\theta p^s_\theta \times \frac{\rho}{\rho + 1} \times (u^A_\theta - E[c_\theta \mid c_\theta < u^A_\theta - u^N_\theta] - u^N_\theta) \times \psi, \tag{D.6}$$

for a constant factor $\psi \in \mathbb{R}^+$ to be calibrated. The take-up rate $(p^i_\theta p^s_\theta)$ is a natural scaling factor, since this is

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

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

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

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

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

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

$$\begin{aligned}\text{Term A} &= p^i_m \left(u^A_m - \bar{c}_m\right) H_m\left(\Delta u_m \mid m\right) + \left(1 - p^i_m\right) u^N_m + p^i_m u^N_m - p^i_m u^N_m H_m\left(\Delta u_m \mid m\right) - u^N_m && \text{(E.4)}\\ &= p^i_m \left(u^A_m - \bar{c}_m - u^N_m\right) H_m\left(\Delta u_m \mid m\right) && \text{(E.5)}\\ &= p^i_m p^s_m \left(u^A_m - \bar{c}_m - u^N_m\right). && \text{(E.6)}\end{aligned}$$

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

$$\begin{aligned}\text{Term B} = \int_0^m \Bigg[& p^i_\theta(u^A_\theta - (u^A_\theta - u^N_\theta))\frac{dH_\theta(\Delta u_\theta \mid m)}{dm} - \lim_{c \to c^{\text{lb}}_\theta}\left[p^i_\theta(u^A_\theta - c)\frac{dH_\theta(c \mid m)}{dm}\right] \\ & + \int_{c<\Delta u_\theta} p^i_\theta \frac{dH_\theta(c \mid m)}{dm} dc + p^i_\theta u^N_\theta \left(\lim_{c \to c^{\text{ub}}_\theta}\left[\frac{dH_\theta(c \mid m)}{dm}\right] - \frac{dH_\theta(\Delta u_\theta \mid m)}{dm}\right)\Bigg] d\theta, \qquad \text{(E.7)}\end{aligned}$$

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

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

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

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

$$\text{Term C} = \int_0^m \frac{dp^i_\theta}{dm}[u^A_\theta - \bar{c}_\theta] H_\theta(\Delta u_\theta \mid m) d\theta + \int_0^m \frac{dp^i}{dm} u^N_\theta [1 - H_\theta(\Delta u_\theta \mid m)] d\theta - \int_0^m \frac{dp^i_\theta}{dm} u^N_\theta d\theta. \qquad \text{(E.9)}$$

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

$$\text{Term C} = \int_0^m \frac{dp^i_\theta}{dm}[u^A_\theta - \bar{c}_\theta - u^N_\theta] p^s_\theta d\theta. \qquad \text{(E.10)}$$

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, $\lim_{c \to c^{\text{lb}}_\theta}\left[c\frac{dH_\theta(c|m)}{dm}\right] = 0$.

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 $\frac{dp^i_\theta}{dm} \geq 0$.

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. $\int_0^m p^i_\theta p^s_\theta \frac{dG_\theta}{dm} d\theta = 0$.

Proposition 1. *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 1

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:

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

$$-\underbrace{\int_0^m \left[\frac{d\left(p^i_\theta p^s_\theta\right)}{dm} G_\theta + p^i_\theta p^s_\theta \frac{dG_\theta}{dm}\right] d\theta}_{\text{(iv) woodwork effect (costs)}} . \tag{E.12}$$

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. $\square$

E.4 Proof of Equation (D.5)

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

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

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

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

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

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

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. □

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