Self-Targeting in U.S. Transfer Programs*

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Abstract

Should transfers require take-up or go automatically to all eligible people? This paper evaluates a classic rationale for take-up, that it induces "self-targeting" on characteristics not observed in eligibility rules. Using a correlation test, we find self-targeting in eight U.S. transfers: on average, recipients have lower consumption and lifetime incomes than similar eligible non-recipients. Due to self-targeting, transfers provide 50–75 percent more to the consumption- and lifetime-poorest than if automatic and distributionally equivalent by income. With a new sufficient-statistics result, we value the benefits of self-targeting at six cents per transfer dollar, offsetting take-up costs and making automatic transfers undesirable.

Keywords: transfer programs, take-up, eligibility, self-targeting, consumption inequality
JEL Codes: H23, H53, I38

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1 Introduction

Enrollment in U.S. means-tested transfer programs is voluntary, and in some programs, as many as one in three eligible households does not receive benefits (Currie, 2006; Ko and Moffitt, 2024). Instead of these voluntary transfers, the government could help all eligible people automatically, adjusting benefit levels to hold expenditure fixed. Going automatic would avoid the costs that recipients incur to claim benefits, but it would also give up a potential advantage of voluntary transfers: self-targeting. A transfer induces self-targeting when selective take-up among the eligible implicitly reveals dimensions of need which the government cannot itself readily use in eligibility rules. How much self-targeting is there in U.S. transfer programs, and can it justify why they are not automatic?

Answers to these questions would inform contentious debates over the role of voluntary take-up in the American social safety net. Many critics of "administrative burdens" promote reforms to raise take-up, including automatic enrollment, over alternatives that would raise benefit levels or expand eligibility (e.g., Herd and Moynihan, 2019). Going automatic would make the U.S. safety net more like those in other developed countries.[1] It would also build on U.S. policy experiments in the Covid-19 pandemic, when two transfers—Medicaid and school meals—became essentially automatic as part of a temporary expansion of the safety net.[2] The issue of voluntary versus automatic redistribution bears further on other longstanding policy issues, such as the complexity of eligibility rules and fundamental welfare reforms like a negative income tax.

The classic theoretical rationale for voluntary transfers is that selection into take-up may be "advantageous." That is, a household's choice to take up a voluntary transfer in the face of costs or "ordeals" may reveal that it has a higher level of unobservable need (Nichols and Zeckhauser, 1982; Besley and Coate, 1992). However, there are also prominent arguments against voluntary transfers: Ordeals may instead perversely screen out the neediest households, who may face greater take-up costs or behavioral frictions (Currie and Gahvari, 2008; Kleven and Kopczuk, 2011; Mullainathan and Shafir, 2013). Even if selection is advantageous in direction, it may be too small in magnitude to offset take-up costs (Herd et al., 2023). Theory alone thus cannot say whether any benefits of self-targeting outweigh ordeal costs, and economists lack a way to connect empirical estimates of self-targeting to theory.

This paper studies self-targeting in U.S. transfer programs. First, we measure the extent of self-targeting in eight means-tested transfers that constitute together most of the U.S. safety net.

[1] This is a key conclusion in international comparisons of welfare states (e.g., Esping-Andersen, 1990; Bartels and Neumann, 2021). However, these countries also see incomplete take-up in voluntary programs (Eurofound, 2015).

[2] Before the end of Medicaid auto-enrollment, U.S. Department of Health and Human Services (2022) forecasted that 8.2 million people would lose benefits, of which 6.8 million (83 percent) were expected to be from non-take-up among the eligible. Some policy reforms have already occurred in the context of school meals: Ten states have extended the pandemic-era expansion as of Spring 2024 (see `https://frac.org/healthy-school-meals-for-all`).

Second, we show how self-targeting increases the progressivity of the safety net in ways that are not captured by standard methods of distributional analysis. Third, we derive a new sufficient-statistics formula for the welfare impact of moving from voluntary to automatic transfers, and we implement this formula using values for the sufficient statistics from our data and external estimates.

The first part of our analysis documents self-targeting through a correlation test. Using data from the Panel Study of Income Dynamics (PSID), we compare the consumption and lifetime income of transfer recipients to those of eligible nonrecipients with similar current annual income. This selection measure is inspired by a tradition in public finance dating to Vickrey (1947): We take as a premise that consumption and lifetime income are superior measures of living standards to current income, since households can save or dissave to smooth transient shocks.[3] However, these dimensions of need are rarely and imperfectly observed by the government in determining tax liabilities and transfer eligibility.

We find self-targeting on both consumption and lifetime income for all eight transfers we study, often of substantial magnitude. In the Supplemental Nutrition Assistance Program (SNAP), for instance, the average consumption rank of recipients is 19 percentiles lower than eligible nonrecipients with similar incomes, a difference of about $11,000 per person per year or 46 percent of their average per-capita consumption. These gaps are somewhat smaller for other transfers and for selection on lifetime income. Take-up appears to distinguish temporarily low-income households, who can smooth consumption, from persistently low-income ones who cannot. Our findings contrast sharply with recent research examining take-up responses to marginal changes in ordeals, whose mixed results have called into question the relevance of self-targeting.[4]

Our correlation test differs in three main ways from the approaches in this literature. First, it measures the welfare-relevant parameter for the comparison of voluntary and automatic transfers: average differences in need between recipients and eligible nonrecipients, rather than differences at the margin of specific ordeals.[5] Second, it distinguishes whether take-up reveals unobserved need rather than observed characteristics, a key distinction when eligibility rules or taxes can be adjusted in addition to ordeals. Prior work has instead examined heterogeneity in take-up responses according to characteristics commonly included in eligibility rules, like age or current income. Third, it is easy to implement via a cross-sectional descriptive regression (Chiappori and Salanie,

[3] Sullivan et al. (2008) find that the persistent component of income is far more predictive of measures of material hardship than the transient component.

[4] Recent papers studying the targeting properties of ordeals and information include Bhargava and Manoli (2015), Ganong and Liebman (2018), Deshpande and Li (2019), Finkelstein and Notowidigdo (2019), Gray (2019), Lieber and Lockwood (2019), Homonoff and Somerville (2021), Unrath (2021), Arbogast et al. (2022), Shepard and Wagner (2022), Wu and Meyer (2022), Ericson et al. (2023), and Giannella et al. (2023).

[5] Holding the budget fixed, an automatic transfer would redistribute from current recipients to all eligible nonrecipients, whether they are marginal to the ordeal or not. By contrast, differences in need on the margin are uninformative about need among inframarginal recipients and nonrecipients without strong assumptions (see Section 3).

2000; Jacobsen et al., 2020). By contrast, testing for selection on a given margin requires as-good-as-random variation in the relevant ordeal. Prior research has missed strongly advantageous self-targeting, as it manifests among inframarginal recipients on eligibility-rule unobservables rather than marginal recipients selecting on observables.

The second part of our analysis reconsiders the overall progressivity of transfer programs in light of self-targeted take-up. We highlight two implications that are particularly relevant for real-world policy analysis. First, we consider budget-neutral reforms that shift from voluntary to automatic transfers. In particular, we reallocate the value of benefits claimed by people at a given income to all people with the same income, including current nonrecipients. Due to self-targeting, households at the bottom of the distributions of consumption and lifetime income currently receive 50 to 75 percent more under voluntary transfers than they would under this reform. Voluntary transfers are thus systematically more progressive than naively-designed automatic alternatives.

The other policy insight follows from contrasting the distributional incidence of transfers by current income with their incidence by consumption and lifetime income. In the context of taxes, economists have argued that income-based analyses usually overstate the welfare-relevant notion of progressivity or regressivity due to year-to-year household income fluctuations (Poterba, 1989, 1991). We find that, in most transfers, self-targeting entirely offsets this "smoothing" effect of lifetime and consumption incidence. The view that redistributive policies struggle to reduce inequalities in consumption and lifetime income is thus far less true of transfers than it is of taxes. Eligible nonrecipients are the other side of the coin: In this population, low current incomes mask relatively high consumption and lifetime incomes, with smoothing and self-targeting operating in the same direction. Self-targeting thus raises new and significant issues when distributional analysis moves to transfers from its more-familiar domain of taxes.[6]

The third part of our analysis asks whether a transfer should be automatic, given the extent of self-targeting when it is voluntary.[7] We prove that, for a specific "voluntary-to-automatic" reform within a class of theoretical models, the social benefits of self-targeting are summarized by the regression coefficient from our correlation test. We also show that, in the same reform, the social costs of ordeals are summarized by a take-up elasticity with respect to the benefit level. This result holds for a reform that marginally cuts a voluntary transfer to fund an automatic transfer in a way

[6]These results are related to studies of the lifetime incidence of taxes and transfers (Blundell et al., 2015; Bengtsson et al., 2016; Roantree and Shaw, 2018; Fullerton and Rao, 2019; Levell et al., 2021; Auerbach et al., forthcoming). Our analysis highlights the implications of self-targeting to distributional incidence and shifts the focus to consumption incidence, which is relevant for policies serving a credit-constrained low-income population. This literature is further reviewed in Fullerton and Metcalf (2002) and often assumes away the take-up margin, treating transfers as equivalent to taxes (e.g., Davies et al., 1984; Fullerton and Lim Rogers, 1993; Guner et al., 2021). Another related paper is Blank and Ruggles (1996), which finds heterogeneous income dynamics that are correlated with take-up.

[7]We focus on welfarist rationales for transfers. Non-welfarist normative frameworks, such as specific egalitarianism, can also justify transfers—as can externalities, paternalism, or market imperfections.

that is uniquely budget-neutral, distribution-neutral with respect to current income, and entails a flat change in the voluntary transfer available at any income. However, the reform is generically not incentive-neutral in labor supply, and we derive two additional terms for the welfare impacts of labor-supply responses. The reform can be viewed as marginally shifting from a fully-voluntary transfer $\$b$ to a \$1 automatic transfer with a voluntary "top-up" of $\$(b-1)$, thus holding fixed other aspects of the transfer.[8] In particular, it leaves the ordeal unchanged, an exercise that differs from changes to ordeals as analyzed in Finkelstein and Notowidigdo (2019).

Calibrating the welfare formula, we draw three conclusions about the social costs and benefits of self-targeting. First, self-targeting yields quantitatively important social benefits: In our baseline calibration, we estimate that they are approximately six cents per transfer dollar, taking a dollar-weighted average across programs.

Second, overall across transfers, the social benefits of self-targeting likely exceed the social costs of ordeals. Our theoretical framework obtains upper-bound estimates of ordeal costs through the envelope theorem, as when take-up choices are made optimally, the welfare-relevant ordeal cost equals the fiscal cost of marginal recipients. These upper bounds are large: In annual per-recipient terms, we find ordeal costs could be as high as \$240 for SNAP or \$500 for Medicaid. Models in which incomplete take-up is, at least in part, a result of non-optimizing behavior imply smaller ordeal costs, as the marginal costs paid by non-optimizers must be less than their marginal fiscal costs. Behavioral frictions that reduce take-up would thus further weaken the case for automatic redistribution. Our results thus establish self-targeting as an empirically credible argument for existing U.S. transfers and cast doubt on the merits of going automatic.

The third conclusion from our welfare analysis is that programs vary greatly in the welfare effects of going automatic, stemming from differences in the magnitudes of self-targeting. In SNAP and housing assistance, self-targeting is strong and valued at more than ten cents per transfer dollar to society. This amount vastly exceeds upper-bound estimates of ordeal costs, so making these transfers automatic is unlikely to be socially desirable. By contrast, self-targeting is of less social value in Medicaid, two transfers that provide food to children (WIC and the National School Lunch Program), and one transfer for utility assistance (LIHEAP). The programs nevertheless inflict ordeal costs, and so it may be valuable on net to go automatic for some transfers.

Survey data like the PSID are subject to important concerns about measurement error in transfer receipt, income, and consumption (Meyer et al., 2009, 2015). We take this challenge seriously, as administrative data lack consumption and transfer receipt linked across programs. First, on misreporting of receipt, we adopt corrections from a recent literature that estimates how

[8]Holding fixed other aspects is important, given rationales for voluntary transfers related to their insurance value (Hoynes and Luttmer, 2011; Deshpande and Lockwood, 2022; Gadenne et al., 2024; Lockwood, 2024), in-kind nature (Cunha et al., 2019; Gadenne and Singhal, 2023), and treatment effects (e.g., Bailey et al., forthcoming).

misreporting probabilities vary with observable characteristics (Davern et al., 2019; Mittag, 2019; Meyer et al., 2020). These corrections actually strengthen our results. Second, on consumption misreporting, self-targeting holds for types of consumption thought to be well-measured and for durable goods ownership (Meyer and Sullivan, 2023). Third, on lifetime income, we extend methods in Haider and Solon (2006) to address potential bias from incomplete income histories. Our analysis contains several implicit replications: first, the complementary analysis of consumption and lifetime income (Caspersen and Metcalf, 1994) and second, the consistency across programs.

We also implement several tests to address measurement error in transfer eligibility. Eligibility imputation is a difficult and pervasive challenge in analyses of U.S. transfers, whether using surveys or administrative data, as both lack eligibility information about nonrecipients. Our results are robust to reclassifying simulated-ineligible recipients as eligible (Duclos, 1995), hold in subsets of the population that are almost certainly eligible, and persist after further controlling for any characteristic observed in any eligibility rule across our eight transfers. These sensitivity analyses suggest it is unlikely that measurement issues in survey data explain our findings.

2 Data and Measurement

Our main source of data is the Panel Study of Income Dynamics (PSID) in its eleven biennial survey waves from 1997 to 2019. In each PSID wave, we observe heads of household and spouses ages 18 to 65. Here we first review key aspects of the data, leaving further details to Appendix B. We then explain three imputation procedures that augment the PSID data: for cash-equivalent values of in-kind transfers, transfer eligibility, and lifetime income.

Our goal is to measure selection into transfers on consumption and lifetime income. The PSID data has several crucial features for this purpose, including its long panel dimension to estimate lifetime income, its consumption data, and its information on the receipt of all major U.S. transfer programs. Its major limitations are the reporting issues that we discuss in depth in Section 3.

2.1 Income, Consumption, and Transfer Receipt

Current Income. We define household income as total annual income of the head and spouse before taxes and transfers, excluding other household members. Income includes labor, business, and capital income. Following the National Academy of Sciences (Citro and Michael, 1995), we adjust for household size using the equivalence scale $e_h = (N_{h,\text{adult}} + 0.7N_{h,\text{child}})^{-0.7}$, where $N_{h,\text{adult}}$ and $N_{h,\text{child}}$ respectively denote the numbers of adults and of children in household h.[9] We compute

[9] Appendix A includes results not adjusted for household size and composition. These are typically quite similar to those with equivalized households. By implication, other equivalence scales are also likely to yield similar results.

income ranks within year, pooling across birth-year cohorts.[10]

Current Consumption. The PSID has had extensive coverage of consumption expenditures since 1999. Expenditure categories include food, housing, health, transportation, education, child care, and several smaller topics. We adjust the data in two ways to better reflect consumption rather than expenditure, following Meyer and Sullivan (2023). These adjustments aim to convert durable-goods ownership into consumption flows. First, for homeowners, we replace mortgage and property tax payments with equivalent rents based on reported home values. Second, for vehicle owners, we replace loan payments with estimates of lease-cost equivalents. Household consumption is then equivalized as above. Consumption ranks are also computed within year.

Transfer Receipt. We observe self-reported household-level receipt for ten transfers. These are the Supplemental Assistance Nutrition Program (SNAP); Medicaid; Section 8; public housing; Temporary Assistance for Needy Families (TANF); Supplemental Security Income (SSI); Women, Infants, and Children (WIC); the Low Income Home Energy Assistance Program (LIHEAP); and the National School Lunch Program and School Breakfast Program. We combine public housing and Section 8 into one program to which we refer as "housing assistance," and the lunch and breakfast programs into "school meals." Table 1 provides summary statistics on the transfers.

2.2 Imputation of Other Variables

Cash Equivalents of In-Kind Transfers. We measure the dollar value of transfers by combining information from the PSID and the Supplemental Poverty Measure module of the U.S. Current Population Survey (CPS). For SNAP, TANF, SSI, UI, and LIHEAP, the PSID records the nominal value of transfers over various time periods, which we rebase as the per-capita annualized amount in 2020 constant dollars. The PSID does not include cash-equivalent values for in-kind transfers, namely Medicaid, Section 8, public housing, and WIC. We impute these amounts with the average values by household size and year reported in the CPS for all but WIC, where we use the national average benefit. The CPS generally values in-kind transfers dollar-for-dollar with expenditure.[11]

Lifetime Income. We construct a lifetime concept of household income from incomplete income histories. To begin, we estimate a Poisson regression model with individual fixed effects, interacted with age-specific coefficients as recommended by Haider and Solon (2006). Letting i index

[10] We do not rank households within both year and cohort, as this would remove life-cycle effects of rising incomes and falling transfer receipt rates with age. Reranking within year and cohort would thus reduce estimated transfer progressivity in current income relative to lifetime income, so it would only strengthen our findings.

[11] Except for Medicaid, for which the Census produces household-level "fungible values" and individual-level "market values." We use fungible values, so as to remain at the household level.

individuals, t index calendar years, and a index age in years, the model takes the following form:

$$\mathrm{E}[y_{it} \mid X_{it}] = \exp(\alpha_i \lambda_a + X_{it}' \beta_a), \tag{1}$$

where α_i is an individual fixed effect, X_{it} is a matrix of time-varying demographic characteristics, and λ_a and β_a are vectors of age-specific coefficients. The outcome y_{it} is individual income.

We then perform several adjustments, explained in Appendix B, before using the regression results to impute lifetime income. These adjustments shrink the fixed effects to account for sampling variation and impute demographic characteristics to balance the panel. We calculate lifetime average income from ages 18 to 65, and then we account for spousal income in a way that permits changes in household composition over time. In particular, let $j(i,t)$ indicate i's spouse in year t. Our concept of lifetime household income follows each individual through the sequence of households they experience as adults, without discounting for time. That is, the lifetime household income of individual i is

$$\overline{y}_i^h = \sum_t e(\widehat{y}_{it}^h) = \sum_t e(\widehat{y}_{it} + \widehat{y}_{j(i,t),t}) \tag{2}$$

where t is again summed over the years in which i is between ages 18 and 65, $\widehat{y}$ is a predicted income, and $e(\cdot)$ is the equivalence-scale function. If we restrict our sample to stable households (as in, e.g., Fullerton and Lim Rogers, 1993), our definition of lifetime income would coincide with the standard concept. We compute lifetime-income ranks within birth-year cohorts.

Simulated Eligibility. Studying transfer take-up among the eligible requires measures of transfer eligibility, so that one can distinguish the ineligible from eligible nonrecipients. We simulate eligibility by compiling information on program rules, mainly from primary-source documents and research databases of such rules, similar to the Urban Institute's TRIM program (Zedlewski and Giannarelli, 2015). See Appendix B for details on these eligibility simulations.

Eligibility simulations cannot perfectly capture true eligibility, as information used in actual eligibility determinations differs from that recorded in surveys. In validation checks in Appendix B, our simulated-eligibility measure is strongly predictive of transfer receipt, though misclassification is apparent. Considerable fractions of recipients are simulated to be ineligible, and take-up rates are counterfactually low among the simulated-eligible. Both are routine issues in eligibility simulations (Duclos, 1995). Mismeasured eligibility would generally cause us to understate the importance of eligibility rules relative to self-targeting among the eligible, and so we consider this threat carefully.

Table 1: Means-Tested Transfer Programs in the U.S.

	SNAP	Medicaid	Housing Assistance	TANF	SSI	WIC	LIHEAP	School Meals	Any Transfer	U.S. Population
Budgetary Cost in 2019 (billions)	60.4	613.5	41.7	30.9	55.8	5.3	3.7	18.7	n.a.	n.a.
Receipt Rate, Households	14.1	20.0	6.5	1.0	6.2	7.5	5.1	18.9	33.8	n.a.
Mean Annual Benefit, Recipients	4,062	5,802	6,893	14,144	3,368	205	535	647	n.a.	n.a.
Characteristics of Households or Heads of Recipient Households										
Mean Age, Head	42.1	42.7	40.9	35.6	47.1	34.3	45.4	40.2	43.0	44.3
% Married	19.8	30.7	11.7	15.6	25.2	37.4	26.5	35.5	33.0	47.2
Mean Household Size	3.7	3.8	3.1	3.5	3.2	4.5	3.7	4.3	3.6	3.0
% Children at Home	51.6	58.9	40.3	90.9	27.7	92.0	48.2	94.1	50.5	30.9
% Nonwhite or Hispanic	64.6	62.4	73.0	74.8	60.2	70.3	58.7	69.0	62.2	40.7
% H.S. Graduate	70.6	73.4	74.3	60.0	73.8	71.4	70.6	71.9	76.2	88.6
Mean Household Income	16,263	27,992	17,829	11,735	20,501	32,091	17,223	34,860	31,741	78,506
% Employed	45.7	53.9	50.1	40.0	37.3	69.5	44.7	70.7	58.4	77.0
Mean Rank, Equivalized Households										
Current Income	16.7	22.3	19.8	12.6	18.6	24.1	17.2	25.5	25.2	50.0
Consumption	17.0	22.8	16.9	11.9	27.8	19.3	19.8	22.4	26.4	50.0
Lifetime Income	24.3	30.6	24.9	22.6	27.1	34.1	26.0	34.3	33.3	50.0

Notes: This table reports summary statistics on the eight means-tested transfer programs we study. See Appendix B for sources on budgetary costs. All other values are from the PSID. Monetary values are expressed in 2020 constant dollars.

3 Estimates of Self-Targeting in Transfers

This section quantifies advantageous self-targeting in transfers. First, we introduce and implement a correlation test to detect self-targeting. Second, we establish that selection into transfers is mostly explained by self-targeting among the eligible and not the direct effects of eligibility rules. Third, we show the implications of self-targeting for the distributional analysis of transfers. Finally, we assess the sensitivity of our results to measurement issues.

3.1 Empirical Definition

We take the following definition of *advantageous self-targeting* in transfers to the data. Self-targeting is advantageous on an outcome C_i (e.g., consumption) if transfer recipients are negatively selected on the outcome relative to eligible nonrecipients with the same current income Y_i. That is:

$$\mathrm{E}[C_i \mid D_i = E_i = 1, Y_i] \;\leq\; \mathrm{E}[C_i \mid D_i = 0, E_i = 1, Y_i], \tag{3}$$

where, for households indexed by i, D_i and E_i respectively indicate receipt and eligibility.

Equation 3 is motivated by the correlation test of Chiappori and Salanie (2000). Consider, in particular, the following joint semiparametric model of the outcome and transfer receipt:

$$C_i = \beta D_i + f(Y_i) + X_i\delta + \nu_i \tag{4}$$

$$D_i = \begin{cases} 1[g(Y_i) + X_i\gamma + \varepsilon_i \geq 0] & \text{if } E_i = 1 \\ 0 & \text{if } E_i = 0. \end{cases} \tag{5}$$

where X_i contains eligibility-rule observables, so that eligibility $E_i = E(X_i)$, and $f(\cdot)$ and $g(\cdot)$ are flexible functions of current income.

In this model, transfer receipt D_i may be correlated with the outcome C_i for two reasons, a causal relation (given by β) and a non-causal association ($\mathrm{corr}(\nu_i, \varepsilon_i) \neq 0$). The latter is self-targeting: Households which are unobservably more or less likely to take up the transfer when eligible may also be also positively or negatively selected on the unobservable component of the outcome. If $\beta \geq 0$ (transfer receipt has a weakly positive causal effect on the outcome), then Equation 3 implies advantageous self-targeting, that is, the errors ν_i and ε_i are negatively correlated.

To test for self-targeting, we always control for current income. In robustness checks, we also control for eligibility-rule observable characteristics. These specifications are motivated by what the government might adjust to redistribute in lieu of self-targeting. Current income is a sufficient control if the government adjusts the income tax, whereas controlling for eligibility-rule observables would be appropriate if these rules are also adjusted.

Connection to Prior Research. We discuss in Section 1 that scholars have examined the targeting properties of changes in ordeal costs, which may enter ε_i. This literature uses as-good-as-random variation in specific ordeals to identify the level of need and other characteristics among ordeal "compliers": those for whom $E_i = 1$ and $X_i\gamma + \varepsilon_i \approx 0$. By contrast, the correlation test includes "always-takers" and "never-takers" and is the appropriate measure of selection for studying automatic transfers, as formalized in Section 4. Intuitively, shifting toward an automatic transfer redistributes from transfer always-takers to never-takers, holding the budget fixed. The targeting of ordeal compliers is only indirectly informative about always- and never-takers.[12]

Both analyses are useful but answer distinct questions. We study "should transfers be automatic?" rather than "should ordeals change?" On the one hand, it may be easier for policymakers to change ordeals than to reallocate funds across programs or go fully automatic. On the other hand, the ordeals literature suggests that the complier population varies greatly with the setting and ordeal in question. This heterogeneity means this literature has yielded few general lessons about targeting, and it suggests the value of studying the voluntary-versus-automatic question. The varied evidence among compliers also sharply contrasts with our consistent finding of self-targeting on average. Another virtue of the correlation test is that it is easy to estimate with observational data, since it is fundamentally descriptive. Identifying valid complier groups for ordeals further requires identifying variation, such as through a randomized trial.

3.2 Who Gets Transfers?

We first document advantageous selection through simple two-way tabulations of transfer receipt by income and consumption. Table 2 reports the average annual per-capita value of benefits for households in each combination of income quintile and consumption quintile.

Moving across the income distribution, higher-income households unsurprisingly receive less in transfers. Yet, comparing households in the same income quintile but with different levels of consumption, lower-consumption households receive more in transfers than higher-consumption households. Thus, there is advantageous selection on consumption. For instance, among households in the bottom income quintile, those also in the bottom consumption quintile receive six times more in transfers as those also in the top consumption quintile. We find similar results for selection on lifetime income. The similarity is reassuring in that both consumption and lifetime income require assumptions to impute but are constructed entirely separately of each other.

These tabulations do not account for within-quintile income differences, and it is possible that apparent selection on consumption merely reflects that households with the lowest income within

[12] With sufficient variation in the ordeal, one could recover the average by integrating through the full distribution of complier characteristics. In practice, variation in ordeals rarely sends take-up rates to zero or one, so statements about average effects require parametric extrapolation to reach deeply inframarginal households (Heckman, 1990).

Table 2: Average Annual Per-Capita Total Transfer Benefits
by Quintile of Current Income, Lifetime Income, and Consumption

		Income Quintile					
		1	2	3	4	5	Avg.
Consumption Quintile	1	3,647	1,353	600	397	155	2,440
	2	1,745	719	296	134	80	666
	3	920	563	217	102	33	303
	4	572	403	168	60	33	153
	5	557	273	133	58	18	101
	Avg.	2,435	844	266	92	27	
		1	2	3	4	5	Avg.
Lifetime Income Quintile	1	3,346	1,243	498	253	28	2,208
	2	1,594	839	278	103	36	627
	3	1,272	664	230	88	36	349
	4	1,152	556	211	79	26	242
	5	1,344	522	189	66	23	239
	Avg.	2,435	844	266	92	27	

Notes: This table reports the average annual per-capita total value of transfer benefits, cash and in-kind, by quintiles of equivalized household current income, lifetime income, and consumption. Values are in constant 2020 dollars.

each quintile have the lowest consumption and also take up the transfer. A flexible rank–rank regression specification to control for income addresses this concern. We therefore estimate

$$\overline{R}_{it} = \beta D_{it} + f(R_{it}) + u_{it}, \tag{6}$$

where $\overline{R}_{it}$ is the consumption rank or lifetime-income rank for household i in year t, R_{it} is i's current-income rank, $f(R_{it})$ is a flexible function of this rank, and D_{it} indicates i's receipt status for a given transfer program. The coefficient β summarizes the extent of advantageous selection into a transfer. We parameterize $f(R_{it})$ using cubic splines with knots at the 10th, 25th, and 50th percentiles of the current-income distribution.

Panel A of Figure 1 shows the patterns in Table 2 hold up in the regression and apply broadly across programs. Overall, recipients of a given transfer rank about 15 percentiles lower in the consumption distribution than nonrecipients of that transfer with similar current incomes. Appendix Table A4 estimates the Poisson-regression equivalent of Equation 6, with levels of annual consumption and lifetime income per capita as outcomes. The rank differences are consistent with differences of approximately 30 to 60 percent in these outcomes, or around $7,500 to $14,000 per person per year in consumption.

The extent of selection into transfers on consumption and lifetime income varies considerably across programs. For instance, the receipt of SNAP and housing assistance is highly informative about consumption and lifetime income given income, whereas SSI receipt is less informative. Some "non-differences" are also interesting: for example, it is not the case that cash-like transfers (e.g., SNAP) are systematically more selective than the transfers most unlike cash (e.g., housing or Medicaid). Results for lifetime income are similar, although there are some notable differences (Panel B of Figure 1). One is that TANF recipients are highly negatively selected on consumption but are much less selected on lifetime income.

Panel C of Figure 1 estimates the extent of advantageous selection into transfers, distinguishing by the number of distinct transfers received. Households that receive multiple transfers are more advantageously selected on consumption and lifetime income than households receiving only one transfer. Such a pattern could arise if take-up costs were positively correlated across programs within household and negatively correlated with need, so that multiple receipt indicates deep inframarginality to transfers. This explanation would reconcile our finding (strong self-targeting on average) with the findings of prior research (mixed evidence of selection on the margin).

3.3 Self-Targeting or Eligibility Rules?

Our analysis has so far pooled the ineligible and eligible. The results may thus reflect not only self-targeting but also eligibility requirements which select on correlates of consumption and lifetime income, such as asset tests or categorical eligibility for some groups (e.g., people with disabilities). We now use our simulated-eligibility measures to disentangle the contributions of self-targeting and eligibility rules. We find self-targeting is the primary force, and eligibility rules distinctly secondary, in advantageous selection into transfers.

Table 3 shows the importance of self-targeting over eligibility rules, with SNAP as an example. Panel A shows that receipt rates of SNAP decline in both income rank and consumption rank given income rank. Panel B shows rates of simulated eligibility for SNAP by income and consumption quintile. Unsurprisingly, SNAP eligibility falls quickly in income; very few households above the second income quintile are SNAP-eligible. The rate of simulated SNAP eligibility also falls in consumption given income but less markedly than does receipt. These declines in eligibility with respect to consumption are driven by asset tests in SNAP, which existed until 2014, as well as details of the transfer's income-eligibility criteria. Take-up rates among simulated eligibles, as shown in Panel C, fall sharply in consumption given income and thus explain the difference between Panels A and B. Among eligible households in the bottom income quintile, the SNAP take-up rate among those also in the bottom consumption quintile is around 52 percent, as compared to approximately 8 percent among top-consumption-quintile households.[13]

[13] While take-up rates are sensitive in *levels* to the general expansiveness or conservativeness of any eligibility simulation, measurement issues can less easily explain the vast *differences* in take-up by consumption given income.

We generalize this analysis in Panel A of Figure 1, where we again estimate Equation 6 but with the sample restricted to the simulated-eligible. For programs like SNAP or housing assistance, advantageous selection on consumption or lifetime income into transfers therefore appears to be driven almost entirely by self-targeting among the eligible rather than eligibility. TANF, WIC, and school meals are three notable exceptions. For these programs, conditioning on eligibility more than halves the predictive effect of receipt on consumption rank, although it remains economically large. Eligibility adjustments matter less for selection on lifetime income, although for three programs (WIC, school meals and SSI), selection effects are not statistically distinguishable from zero.

Robustness of Eligibility Measures. Measurement error in simulated eligibility may lead our analysis to overstate self-targeting. We therefore provide three robustness checks for our eligibility measures. None of these three adjustments to simulated eligibility overturns the basic conclusion that selective take-up, rather than eligibility rules, is the primary force in the selection patterns we document. See Section 3.5 for analyses of measurement error in other variables, namely consumption, income, and transfer receipt.

First, when we reclassify simulated-ineligible recipients of a given program as eligible, we do not see a clearer role for eligibility in concentrating incidence among the consumption-poor and lifetime-poor (Appendix Figure A5). The transfer program where our results are most sensitive to this reclassification is SSI, likely due to challenges in imputing SSI eligibility.

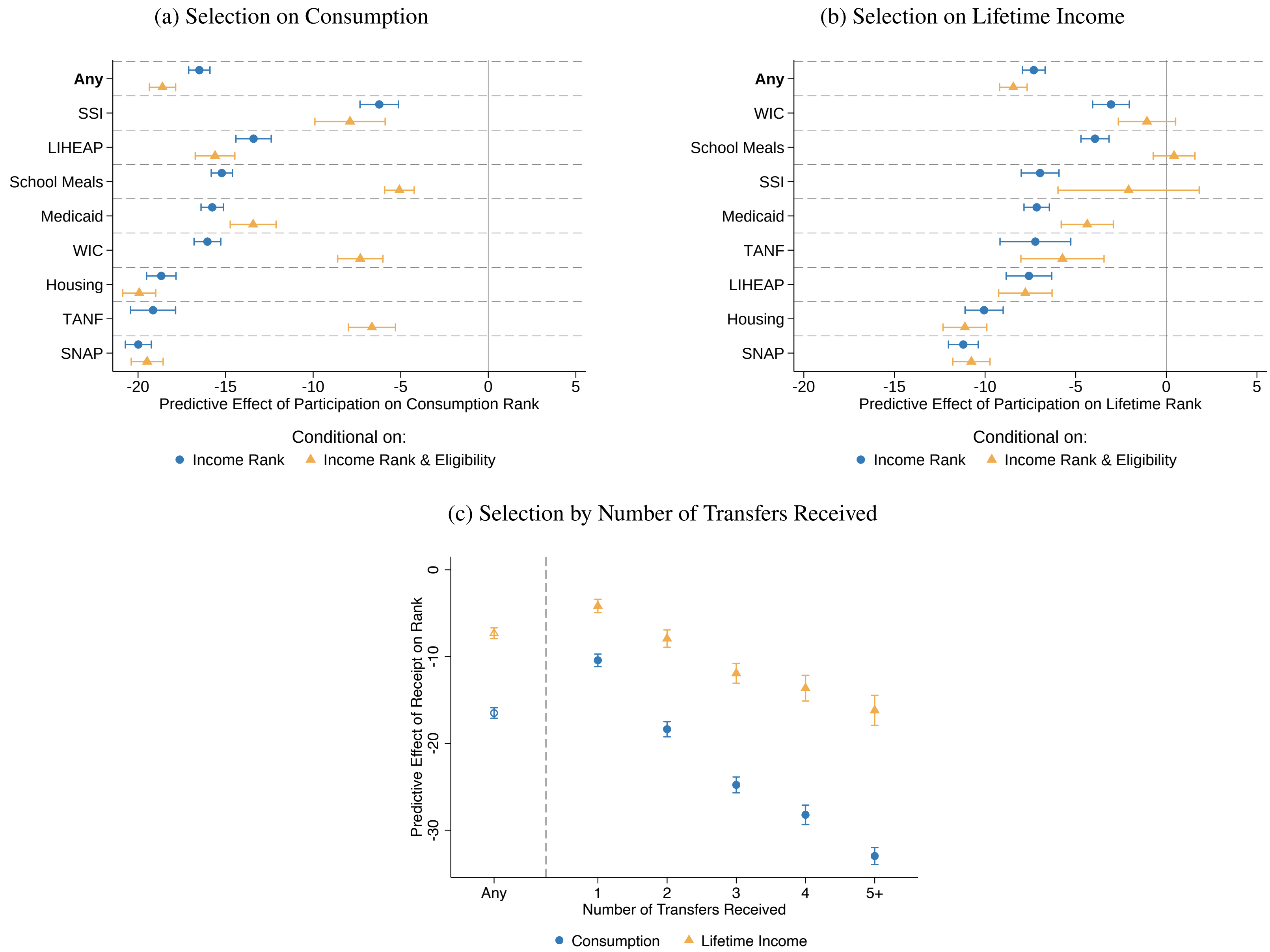
Second, we find similar estimates of self-targeting in demographic groups with near-certain eligibility as compared in our main results (Appendix Figure A7). In such "always-eligible" demographic groups, take-up directly reveals self-targeting.

Third, we test robustness to controlling for additional variables. We first augment Equation 6 with controls for any variable that enters into the eligibility simulation for any transfer we study.[14] We then also include controls for variables that do not enter any eligibility simulation but predict both consumption or lifetime income and transfer receipt: race, education, and marital status. These tests address concerns that we do not perfectly observe eligibility rules or their input variables.

Controlling for all eligibility-rule observables attenuates but does not eliminate selection into receipt for most transfers (Appendix Figure A4). Our further controls for "unused observables" make only a minor difference for our estimates. The coefficient stability suggests that mismeasurement in eligibility rules and their inputs is unlikely to reverse our conclusions, unless it is more relevant to take-up behavior than variables such as race or education.

[14] These are the household's state of residence by year, household size and composition, income, earnings, ages of household members, disability status, unemployment duration and reason, and basic measures of wealth (value of any automobiles and liquid assets).

Figure 1: Self-Targeting in Transfer Programs



Notes: This figure displays estimates of the predictive effect of transfer receipt on consumption rank (Panel A) or lifetime-income rank (Panel B), conditional on current-income rank (coefficient γ from Equation 6). For the yellow diamonds, we estimate the regression only on people whom we simulate to be eligible. The "any" row of Panel A is an indicator for receipt of at least one of the eight transfers. 95-percent confidence intervals reflect clustered standard errors by household. In Panel C, we adapt Equation 6 by replacing the transfer indicator with indicators for the number of transfers received in that year.

Table 3: SNAP Receipt, Eligibility, and Take-Up Rates by Income and Consumption Quintile

Panel A: Receipt Rate

		Income Quintile					
		1	2	3	4	5	Avg.
Consumption Quintile	1	51.2	22.3	7.8	4.9	4.9	35.3
	2	23.7	9.6	2.7	1.1	0.5	8.4
	3	12.3	5.9	2.3	0.5	0.3	3.3
	4	6.3	3.4	1.4	0.2	0.2	1.3
	5	5.5	2.9	1.7	0.3	0.1	0.9
	Avg.	33.6	12.2	2.8	0.6	0.2	

Panel B: Simulated Eligibility Rate

		Income Quintile					
		1	2	3	4	5	Avg.
Consumption Quintile	1	83.8	23.7	0.4	0.8	0.0	51.9
	2	75.5	15.8	0.4	0.2	0.0	19.0
	3	67.5	14.0	0.4	0.2	0.1	10.5
	4	61.3	13.9	0.4	0.3	0.1	7.2
	5	60.7	17.6	0.5	0.3	0.0	6.5
	Avg.	76.3	18.1	0.4	0.3	0.1	

Panel C: Take-Up Rate Among Simulated Eligibles

		Income Quintile					
		1	2	3	4	5	Avg.
Consumption Quintile	1	52.2	39.3	.	.	.	50.2
	2	26.8	23.7	.	.	.	25.9
	3	14.9	14.4	.	.	.	14.5
	4	8.3	8.2	.	.	.	8.1
	5	8.1	8.6	.	.	.	8.1
	Avg.	37.5	27.2	.	.	.	

Notes: This table reports the shares of households that receive SNAP (Panel A), are simulated to be eligible for SNAP (Panel B), and take up SNAP conditional on being simulated to eligible (Panel C). Households are split by quintiles of equivalized household consumption and income. Due to low rates of simulated eligibility, we do not report take-up rates for the top three income quintiles. See Appendix A for a tabulation by income and lifetime income.

3.4 Distributional Analysis and the Automatic Counterfactual

We now reassess the progressivity of the U.S. social safety net in light of self-targeting. This force would, all else equal, make transfers more progressive in consumption and lifetime income by comparison to current income. First, we document the distribution of existing transfers on both consumption and lifetime income. Second, we contrast these findings with the distribution of automatic transfers that are equivalent by income.

To compute distributional incidence, we first rank households by current income, current consumption, and lifetime income.[15] We then estimate the average dollar amounts of benefits per person per year as locally-linear functions of households' percentile rank in each distribution. Figure 2 plots the average total annual per-capita value of transfer benefits as functions of these ranks. In the left sub-panel, we combine transfers across the eight transfer programs we study. The right sub-panel excludes in-kind programs, for which we impute cash equivalents. Figure 3 plots the results separately for each program.

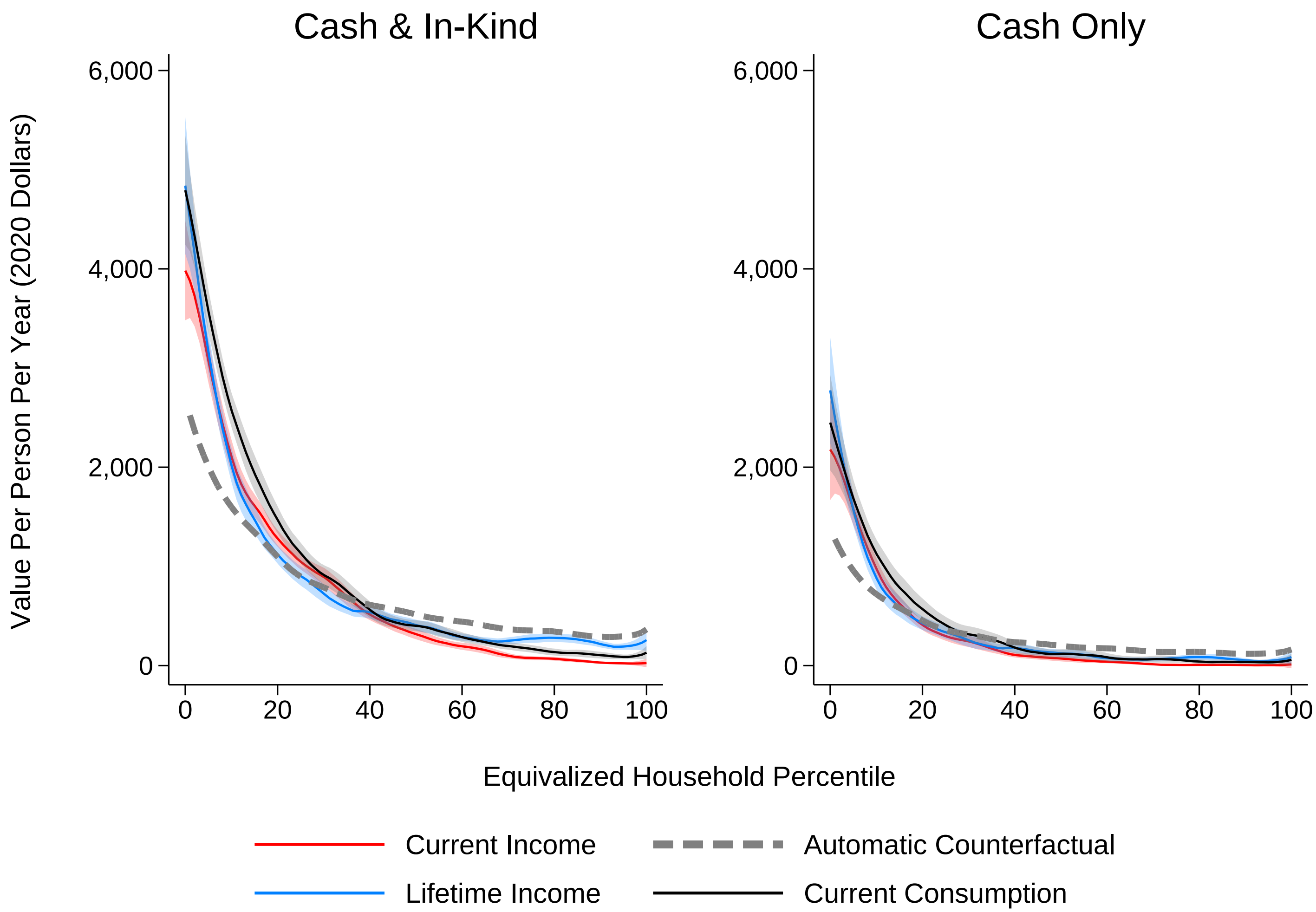
Both figures also show that transfer incidence with respect to consumption, lifetime income, and current income are remarkably similar, overall and for most individual transfer programs. That is, average annual per-capita benefits are about as high at the bottom of the distributions of consumption and lifetime income as they are at the bottom of the current-income distribution. Households in the bottom five percentiles of the income distribution, for instance, receive approximately $3,700 in transfers per person per year, as compared to $4,300 for the lowest-consumption households (Appendix Table A2). Furthermore, the similarity of consumption and lifetime incidence suggests transfer programs identify households with low consumption as a result of persistently low income, rather than a lesser ability to smooth consumption relative to income over time.

The results in Figure 2 should be surprising, because year-to-year income fluctuations should mechanically reduce the lifetime or consumption incidence of transfers at the bottom of the distribution. The apparent absence of this effect is not for a lack of year-to-year mobility at the bottom, as we show through a formal decomposition in Appendix A. Instead, the substantial compressive effect of income fluctuations is fully undone by selection into receipt.

Although eligible nonrecipients of most transfers are poor in current-income terms, many have significant consumption or lifetime resources, as shown in Appendix Figure A3. This result captures the two-sided implications of self-targeting: while existing transfers look more progressive, going automatic becomes less progressive. For instance, around one third of eligible nonrecipients of SNAP and Medicaid have above-median consumption. A similar fraction has above-median lifetime income. Rates of eligible non-receipt are therefore much lower among the consumption- and lifetime-poorest than at the bottom of the current-income distribution. In Medicaid, about one fifth of the consumption- and lifetime-poorest are eligible but do not take up, as compared to one third of current-poorest.

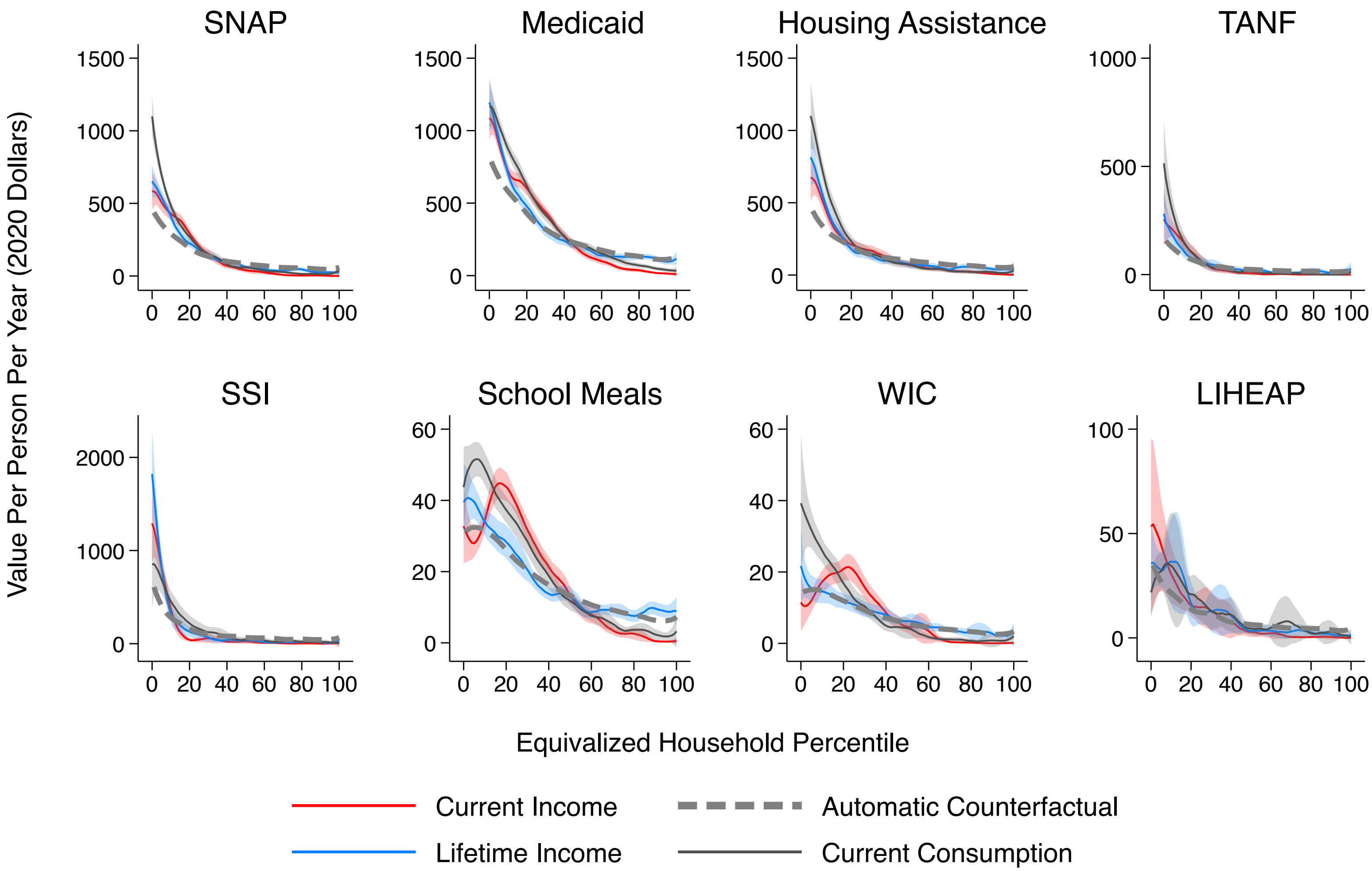
[15] Appendix A contains supplementary figures that probe the sensitivity of our results to definitional choices and provide detail on distributional incidence. There we also investigate unemployment insurance (UI), and we find that its distributional incidence is essentially uniform with respect to current income, lifetime income, and consumption (Appendix Figure A6). This is consistent with the distinct contributory structure of UI and pension systems.

Figure 2: Average Total Annual Per-Capita Benefits



Notes: This figure displays average total annual per-capita dollar amounts as functions of household rank in the distributions of equivalized current income, lifetime income, and consumption. The functions are estimated by local linear regressions with bandwidths of three percentiles. Dashed lines indicate counterfactual incidence in consumption for an automatic transfer that is distributionally neutral with respect to income. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

Figure 3: Average Annual Per-Capita Benefits by Program



Notes: This figure displays the average annual per-capita value of transfer benefits by program as functions of household rank in the distributions of equivalized current income, lifetime income, and current consumption. The functions are estimated by local linear regressions with bandwidths of three percentiles. Dashed lines indicate counterfactual incidence in consumption for an automatic transfer that is distributionally neutral with respect to income. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

Automatic Counterfactual. The role of self-targeting is made particularly clear by considering the consumption and lifetime incidence of automatic transfers, in which self-targeting is inherently impossible. We consider an automatic counterfactual in which the government pays the average unconditional value of a given transfer to all households at each income level. For instance, suppose households at a given income level receive \$1,000 if they take up a voluntary transfer, and half take up. In the counterfactual, the government would give \$500 to all the households with that income. Compared to a voluntary transfer, the automatic transfer gives less to the households who always take up and more to households who do not. We simulate this counterfactual by applying the empirical transition matrix from income ranks to consumption ranks.[16]

Relative to a voluntary transfer with the same distributional incidence, automatic transfers would give the lowest-consumption households about half as much on average (see the dashed lines in Figure 2). Voluntary transfers provide the lowest-consumption households about \$4,000 per person per year. By comparison, automatic transfers would provide \$2,300 annually to members of this group. Applying the same approach to lifetime income, the corresponding amounts are \$3,800 under the voluntary transfer and \$2,600 under the automatic transfer. In summary, voluntary transfers provide 50–75 percent more to the consumption- and lifetime-poorest than an automatic transfer that is distributionally equivalent in income.[17]

Figure 3 shows this result holds for most transfer programs individually. Holding fixed a transfer's distributional incidence with respect to income, going automatic generally reduces progressivity in consumption, often substantially so. In several transfers with strong self-targeting, the voluntary transfer provides more than twice as much to the lowest-consumption households than the counterfactual automatic transfer.

Appendix A further allows us to consider another automatic counterfactual: redistributing automatically to only the eligible, rather than all households, at the same income. We present there an accounting decomposition of the contributions of income mobility, eligibility, and take-up to the consumption incidence and lifetime incidence of transfers, starting from their current incidence. The decomposition builds on Brewer et al. (2020) by distinguishing between eligibility and take-up and uses our simulated eligibility measures.

To incorporate eligibility into the above counterfactuals, one adds the eligibility component of the decomposition (e.g., black line of Appendix Figure A13) to the automatic counterfactual. Consistent with Figure 1, we find these eligibility components are generally small, except for Medicaid and TANF. Eligible-only automatic counterfactuals therefore reach a similar conclusion:

[16] For the average benefit amount $s(y)$ at current-income rank y, this mobility counterfactual is $s(c) = \int s(y)\, dH(y|c)$, where $H(y|c)$ is the conditional distribution of income rank at consumption rank c.

[17] Appendix Table A2 reports point estimates and standard errors for transfer incidence in different parts of the distributions of current income and consumption, along with incidence in the automatic counterfactual. Appendix Table A3 provides the same analysis but for lifetime income.

self-targeting meaningfully concentrates incidence at the bottom of the distributions of consumption and lifetime income.

3.5 Sensitivity to Mismeasurement

Survey data are imperfect. Here we consider the potential for bias in our results due to known issues with self-reported transfer receipt and current income. We also address potential concerns with our measures of consumption and lifetime income. Overall, measurement issues seem unlikely to explain our results, given their magnitude and robustness.

We are especially careful with the survey data because our analysis is mostly infeasible in administrative data. First, such datasets lack appropriate measures of consumption. Second, they rarely link information across transfer programs. Third, while administrative data would improve measurement for some inputs to the analysis (e.g., income and transfer receipt), it would be harder to impute eligibility. Administrative data largely does not record eligibility information for nonrecipients, lacks the detailed covariates of survey data useful for imputing it, and may not capture some eligible nonrecipients who may appear in surveys (e.g., income-tax nonfilers).

Transfer Receipt. Using linked survey and administrative data, Mittag (2019) and Davern et al. (2019) estimate statistical models of household survey reporting behavior for SNAP and Medicaid receipt respectively. Their models, intended for use as misreporting corrections, predict the probability of true transfer receipt given survey-reported receipt and demographic characteristics. These models allow researchers to replace assumptions of constant misreporting rates with misreporting probabilities that are functions of demographic observables.

Their corrections consistently increase our estimates of advantageous selection (Appendix Table A5). There are two reasons why. First, under constant misreporting rates, our estimates are attenuated. Consider misreporting probabilities $p_0 = \Pr(\widetilde{D}_i = 0|D_i = 1)$ and $p_1 = \Pr(\widetilde{D}_i = 1|D_i = 0)$, where D_i indicates true receipt and $\widetilde{D}_i$ indicates reported receipt. Comparing the feasible regression of $y_i = \widetilde{\beta}\widetilde{D}_i + u_i$ to the infeasible regression $y_i = \beta D_i + u_i$, one can show that $\beta = \widetilde{\beta}/(1 - p_0 - p_1)$.[18] Second, the parameter estimates in Mittag (2019) and Davern et al. (2019) both imply that underreporting of transfer receipt is somewhat more common among households with low consumption and lifetime income, holding income constant. Thus, their adjustments amplify the increase in the selection that we would find under constant misreporting rates.

We also compute the rates of transfer underreporting at the top of the consumption distribution that would be necessary to yield zero selection among the eligible (Appendix B). Overturning our conclusions requires a degree of underreporting that we view as implausible, such as

[18] Meyer et al. (2009) finds rates of under-reporting rates in the PSID in the range of 7 to 27 percent across programs we study. This suggests a presumption that our main estimates in Figure 2 are understated, even for transfer programs where heterogeneous-misreporting corrections have not yet been estimated.

"false-negative" rates of 50 percent in the top quarter of the consumption distribution. Though misreporting of transfer receipt is an important phenomenon, it is unlikely to explain our results.

Income. Income is often said to be poorly measured at the bottom of the distribution, and while there is also mismeasurement in consumption, it appears less severe than for income (Meyer and Sullivan, 2003; Brewer et al., 2017). How is this mismeasurement likely to affect our results?

One story of intentional misreporting likely pushes in the opposite direction of our results. In this story, transfer recipients have incentives to underreport income so as to maintain eligibility, and they may do so in any quasi-official setting, including in surveys. Such incentives could apply less strongly to consumption, and to nonrecipients. All else equal, transfer recipients would thus appear *positively* selected on consumption given income. This is opposite to what we find.

We further explore concerns about income misreporting by predicting household income using other labor variables, such as weekly hours and occupation.[19] This approach provides an external check against issues in reporting, assuming accurate reporting of these variables. Appendix Figure A10 show that controlling for predicted income, in addition to reported income, has a modest impact on our results. Even this modest attenuation may reflect that occupation and education account for much of the year-to-year persistence in income.

Consumption. We show in Appendix Table A6 that similar selection patterns appear when only looking at categories of consumption deemed "well-measured" in Meyer and Sullivan (2023). For instance, on average, Medicaid recipients consume 36 percent less in housing, 25 percent less in vehicles, and 24 percent less in food at home than similar-income people who are not on Medicaid. Similar patterns also manifest in PSID data on durable-goods ownership, such as whether the household owns a home, car, computer, and the number of rooms or presence of air conditioning in the home (Meyer and Sullivan, 2012), as we show in Appendix Table A7. SNAP recipients are 16 percentage points less likely to own a home, 13 percentage points less likely to own a car, and 12 percentage points less likely to own a computer than similar-income people not on SNAP. The consistency of selection across measures, some of which seem truly unlikely to suffer from meaningful mismeasurement, bolsters our findings.

Lifetime Income. Inferring lifetime income from "snapshots" is challenging (Haider and Solon, 2006). If mismeasurement of lifetime income for people with fewer years-in-sample were to impart a systematic bias in our results, then the estimated extent of selection into transfers would "drift" up or down as one examines selection for households with more or fewer years-in-sample. We re-estimate the predictive effects of transfer receipt on lifetime rank as in Equation 6, retaining only

[19] Using the March Supplements to the Current Population Survey that match our PSID data years, we estimate Poisson regression models of individual income using occupation, industry, weeks per year, weekly hours, self-employment, in addition to basic demographic information. We then apply these predicted incomes to our PSID data.

individuals with progressively more years-in-sample.

Appendix Figure A8 does show selection on lifetime income is modestly weaker among households with many years-in-sample. Yet this "drift" effect also appears as strong for consumption, which has no across-year imputation step. Thus, the phenomenon likely reflects considerations other than a bias in lifetime-income estimation, such as sample attrition.[20]

3.6 Other Extensions and Robustness Checks

Behavioral Responses and Impacts of Transfers. We have interpreted our correlation-test results as showing that transfer receipt acts as a "tag" of exogenous earnings ability. Yet they do not directly distinguish between selection and potential behavioral responses to transfers. For instance, some households may reduce their consumption or lifetime income so as to receive transfers. Appendix Figure A9 presents evidence that selection predominates. We show that, among *current* nonrecipients of a given transfer, *future* recipients have on average a lower current-consumption rank than *future* nonrecipients with similar current incomes.[21]

Such patterns hold even when one performs this comparison in the distant future, so this finding cannot be explained by households that may strategically reduce their consumption just before an eligibility determination. By comparison, advantageous selection in distant-future transfer receipt is quite consistent with the view of current receipt as a tag of permanent earnings ability. The welfare analysis adjusts for behavioral responses through labor supply. This figure also addresses the symmetric concern that transfers raise lifetime income, e.g., by improving productivity.

Transfers also mechanically raise consumption of transfer recipients relative to their incomes, as consumption is measured in the PSID as inclusive of transfers. For instance, food consumption includes food purchased with SNAP dollars. This force clearly works against our conclusions, reducing both the estimated extent of self-targeting and the progressivity of transfers.

Selection Over Time. Our data span 1997 to 2019, allowing us to address how the U.S. safety net has evolved over this period. We estimate a version of Equation 6 that allows for year-specific coefficients on transfer receipt. To allow us to describe broad trends, we "stack" the data over programs and include program-specific controls for current income. Across our eight transfer programs, we see little change in selection on consumption over time, but a considerable intensification of selection on lifetime income (Appendix Figure A12).

[20]While attrition from the PSID is another potential source of concern, it is not obvious that households that remain in the sample for 20 years are a more representative sample than those who remain for 10 years, say, before attriting.

[21]The regression of interest is $\overline{R}_{it} = \alpha_{ct} + \beta D_{i,t+k} + f(R_{it}) + u_{it}$ within the subsample such that $D_{it} = 0$, for $k > 0$.

4 Welfare Analysis of Automatic Transfers

This section conducts a welfare analysis of the social costs and benefits of using voluntary take-up to target transfers. We develop a new sufficient-statistics formula for a specific reform that shifts resources from voluntary to automatic transfers. We then calibrate the formula using our data and external estimates. Proofs of all theoretical results are in Appendix C.

4.1 Basic Environment

To make the economics of the sufficient-statistics result especially clear, we first illustrate it in a basic environment without labor supply. Suppose there is a unit mass of people indexed by $i \in [0, 1]$. Of these, a share M choose to take up a voluntary transfer, receiving benefit b and paying a hassle cost $\kappa(i)$ to do so. The complementary share $1 - M$ do not take up because their ordeal cost strictly exceeds the benefit, $\kappa(i) > b$.

We evaluate the welfare impact of the following budget-neutral "voluntary-to-automatic" benefit reform. In the reform, the government cuts the voluntary benefit by an amount db. Cutting the benefit causes a share $\frac{M}{b}\varepsilon_b \cdot db$ to drop out of the transfer, where the take-up elasticity with respect to the benefit amount is $\varepsilon_b = d \log M / d \log b$. The fiscal savings from this behavioral response is $M\varepsilon_b \cdot db$. The government gives all people $M(1 + \varepsilon_b) \cdot db$ dollars automatically, spending the fiscal savings. We want to evaluate whether this reform made society better or worse off overall.

Let $\alpha(i)$ be the marginal social welfare weight of person i, and let $\beta = E[\alpha(i) \,|\, \kappa(i) > b] - E[\alpha(i) \,|\, \kappa(i) \leq b]$ be the average difference in these weights between nonrecipients and recipients of the transfer. In this environment, the welfare effect of this reform is

$$\begin{aligned} \frac{dW}{db} = & \underbrace{M\mathrm{E}[\alpha(i) \,|\, \kappa(i) \leq b] \cdot (M(1 + \varepsilon_b) - 1) \cdot db}_{\text{recipients}} \\ & + \underbrace{(1 - M)\mathrm{E}[\alpha(i) \,|\, \kappa(i) > b] \cdot M(1 + \varepsilon_b) \cdot db}_{\text{nonrecipients}}. \end{aligned} \tag{7}$$

This expression follows from weighting the net changes in payments to recipients and nonrecipients by these groups' respective welfare weights. Ordeal costs do not appear, as they only change for a marginal type who was exactly indifferent to taking up. Equation 7 simplifies to

$$\frac{dW}{db} = \beta\sigma_M^2 + M\varepsilon_b, \tag{8}$$

where the variance of take-up is $\sigma_M^2 = M(1 - M)$.

Equation 8 captures our main theoretical result. The first term represents the social benefits of self-targeting. By moving toward automatic transfers, the government diminishes its ability to target. If take-up identifies higher-welfare-weight people on average, then making transfers automatic has undesirable redistributive properties. The targeting is more socially valuable when take-up is more informative about welfare weights $\alpha(i)$ and thus when β is larger. When all or none take up the transfer, targeting is impossible, explaining why benefits depend on the variance.

The second term represents the reduction in the social costs of ordeals. In response to the transfer cut, some people no longer take up, since the benefit no longer exceeds their ordeal cost. We apply the envelope theorem to infer these costs. With privately-optimal take-up choices, the fiscal savings from people who no longer take up equals the change in the social cost of ordeals.

Example. Suppose there is a voluntary transfer with a 50-percent take-up rate. Imagine that on average, recipients consume 71 percent as much as non-recipients, and the take-up elasticity is 0.5. Society has constant-relative-risk-aversion (CRRA) preferences over consumption with parameter $\gamma = 2$. On the margin, should funds shift from this transfer to an automatic one?

The answer is no. Each dollar taken from the recipients is worth about twice ($1/0.71^2 \approx 2$) that of a dollar given to nonrecipients. Normalizing the average welfare weight to one, given 50-percent take-up, yields a regression coefficient $\beta = 0.75 - 1.5 = -0.75$. The variance of take-up is $\sigma_m^2 = (0.5)(1 - 0.5) = 0.25$. Then $dW/db = (-0.75)(0.25) + (0.5)(0.5) = -0.0625$, so each transfer dollar db moved reduces money-metric social welfare by about six cents.

4.2 Setup of Full Model

We next derive a similar formula in a Mirrleesian model of optimal redistribution with endogenous labor supply. The reform is budget-neutral and distribution-neutral with respect to income but not generically incentive-neutral for labor supply. Labor-supply responses therefore need to be accounted for in the sufficient-statistics formula.

Households. Each household has a multidimensional type $\theta = (w, \kappa)$ distributed according to the density μ. The parameter $w \in \mathbb{R}^+$ is the household's wage, encoding their productivity, and $\kappa \in \mathbb{R}^+$ is their cost of taking up the transfer. Households choose how much labor $l \in \mathbb{R}^+$ to supply to generate pre-tax income $z = wl$.

Pre-tax income is taxed according to the nonlinear schedule $T(z) : \mathbb{R}^+ \to \mathbb{R}$, and the remainder is consumed. Households value a dollar of automatic transfer as equivalent to a cash dollar, so there is no distinction with income taxes.[22] Negative taxes are possible. There is also a voluntary transfer with non-linear schedule $S(z) : \mathbb{R}^+ \to \mathbb{R}^+$. Households choose to take up this benefit after observing their private cost κ.[23]

[22]Appendix C discusses the implications of valuing in-kind transfers differently than cash.

[23]We do not explicitly model eligibility rules, treating them as infinite take-up costs. Households therefore do not

We assume households choose their labor supply before they see their realization of κ.[24] We use $\mathbb{1}_S = 1[S(z) \geq \kappa]$ as an indicator for whether, after drawing their κ, the household takes up the transfer. To rule out income effects, we assume households have quasi-linear utility in cash and cash-equivalent transfer dollars and disutility of work hours $v(l) = v(z/w)$. We also assume no income effects are present with respect to the transfer $S(z)$. For each household, the choice of l is one-to-one with income z. We therefore model the household's labor supply choice as a direct choice of z, with each household solving the program:

$$\max_z \left\{ z - T(z) - v(z/w) + \int_0^{S(z)} (S(z) - \kappa)\mu(\kappa \mid w)\, d\kappa \right\}. \tag{9}$$

Suppressing the dependence on the wage w for clarity, the household's optimal choice $z^* = z^*(w)$ leads to ex-post consumption $c^* = z^* - T(z^*) + \mathbb{1}_S(S(z^*) - \kappa)$. Ex-post household utility is

$$V(\theta) = z^* - T(z^*) - v(z^*/w) + \mathbb{1}_S(S(z^*) - \kappa)\,. \tag{10}$$

Government. The government chooses tax and transfer schedules $T(\cdot)$ and $S(\cdot)$ to maximize utility summed across households according to type-specific Pareto weights ($\alpha(\theta)$):

$$\max_{T,S} \int_\Theta \alpha(\theta)V(\theta)\, d\mu(\theta),$$

subject to a balanced-budget constraint:

$$\int_\Theta [T\,(z(\theta)) - \mathbb{1}_S S\,(z(\theta))]\, d\mu(\theta) = 0 \tag{11}$$

and to household optimization. The welfare weights may capture, for instance, a higher social value of transferring to people with low consumption. Since utility is quasilinear in income, any redistributive motives enter through welfare weights. As is usual in this setting, we assume that social marginal welfare weights decrease with wages, all else equal: $\frac{\partial}{\partial w}\alpha(\theta) < 0$. Because $\frac{\partial z^*(w)}{\partial w} > 0$, this assumption is equivalent to assuming that welfare weights are higher for those with lower pre-tax income z^*. We normalize the population-average welfare weight to one.

Our results are delineated by the following relationship between social welfare weights and underlying type parameters, consistent with our empirical definition of self-targeting (Equation 3).

know more than the eligibility rate at their income level. We could introduce eligibility by dividing households into eligibles and ineligibles before they draw their take-up cost.

[24]We do so to circumvent challenging issues of multidimensional screening. The choice also rules out a distinct rationale for transfer programs: self-targeting on "slopes" and not on "levels." That is, if labor supply may depend on the realization of the take-up cost κ, then transfers may sort households on their labor-supply elasticities. An example of this rationale is that disability benefits may target inelastic adults instead of those with low lifetime income.

Definition 1. *We say that take-up is* advantageously self-targeted *when* $\frac{\partial}{\partial \kappa}\alpha(\theta) < 0$.

In the model, advantageous self-targeting means that, all else equal, households with low take-up costs have higher marginal welfare weights. This definition follows Saez and Stantcheva (2016) in abstracting away from any specific rationale for these weights. That is, the households first to take up transfers at any income level have high social marginal welfare weights. If recipients are negatively selected on consumption given income (as in Section 3), advantageous self-targeting holds, assuming social welfare weights that decrease in consumption. On the other hand, if ordeals perversely screen out needier people, advantageous self-targeting would not hold.

Let $M(z) = \Pr(S(z) \geq \kappa)$ denote the take-up rate for households at income z. We assume the take-up cost distribution is continuous on $\kappa \geq 0$, that is, there are no mass points. Take-up rates are endogenous to tax and transfer schedules, which our notation omits for brevity. Moreover, we define $h(z)$ as the mass of types that choose pre-reform income z, which is one-to-one with the primitive w.

4.3 "Voluntary-to-Automatic" Reform

We study a reform that shifts a transfer on the margin from voluntary to automatic. We choose the only reform that is budget-neutral, distributionally neutral with respect to income, and imposes a flat reduction in the voluntary transfer.

The reform is defined precisely as follows. The transfer schedule $S(z)$ is cut by ds at all incomes. At income z, taxes are cut by $\tau(z) = M(z)ds$. This is equivalent to providing an automatic transfer of $M(z)ds$, as it is valued at par with cash. That is, people at each income level are compensated on average for the transfer cut. Marginal rates thus change by $\tau'(z) = \frac{d}{dz}M(z)ds$ at z. Fiscal savings from marginal transfer recipients are redistributed as a lump sum $E_z\left[(S(z) + ds)m(z)\right]$. The revenue cost of any labor supply response is then paid for via lump-sum taxes.[25]

This specific reform is natural to study for two reasons. First, as in Kaplow (2011), requiring neutrality with respect to the income distribution removes incidental impacts of the policy change on the overall progressivity of taxes and transfers. Our welfare calculations thus do not reflect changes in progressivity that could, in principle, be achieved by income taxes alone. Second, a flat change in the transfer has an intuitive real-world analog: changing a fully-voluntary transfer into one with a small automatic transfer with a large top-up provided upon application.

The next proposition, proven using a perturbation-based argument (Jacquet and Lehmann, 2014), presents welfare formulas for this marginal shift toward automatic transfers.

[25]The reform redistributes to all households at each income, without conditioning on eligibility. We could instead redistribute only to the eligible. Such a change would be inconsequential, given the relative importance of self-targeting and eligibility rules in Section 3.

Proposition 1. *The welfare effect of the reform is*

$$\frac{dW}{ds} = \underbrace{\beta\sigma_M^2}_{\text{lost value of self-targeting}} + \underbrace{\bar{M}\bar{\varepsilon}_b}_{\text{fiscal savings from marginals}}$$
$$+ \underbrace{\int_z \frac{M'(z)z\varepsilon_\tau(z)}{1-T'(z)}\left(\frac{d}{dz}(S(z)M(z)) - T'(z)\right)dH(z)}_{\text{labor-supply effect (i)}}$$
$$+ \underbrace{\int_z \frac{M'(z)z\varepsilon_\tau(z)}{1-T'(z)}S'(z)\beta(z)M(z)(1-M(z))dH(z),}_{\text{labor-supply effect (ii)}} \tag{12}$$

where β is the coefficient on take-up from a regression of welfare weights on take-up controlling for income, $\beta(z)$ is this coefficient locally around income z, $\bar{M} = \int_z M(z)dH(z)$ is the overall take-up rate, $\sigma_M^2 = \int_z M(z)(1-M(z))dH(z)$ is the variance of take-up rates by income, $\bar{\varepsilon}_b$ is an average take-up elasticity with respect to benefit size, and $\varepsilon_\tau(z) = -\frac{\partial z}{\partial \tau'}\frac{1-T'(z)}{z}$ is the elasticity of income with respect to a small change τ' in the marginal tax rate of those with initial income z.[26]

Equation 12 shows the welfare result from the basic environment carries over into the Mirrleesian setting with a labor-supply choice: targeting benefits remain summarized by a regression coefficient, and ordeal costs are still summarized by a take-up elasticity. We show formally in Appendix C that, when self-targeting is advantageous and $S(z)$ is positive, the first term in Equation 12 is negative. That is, society loses some benefits of self-targeting to move toward automatic transfers.[27]

Yet there are some differences with the basic environment. Our reform is generically not incentive-neutral in labor supply, so the welfare formula adds two more terms to account for these responses. The first term captures the fiscal impact of behavioral responses to changes in marginal "keep" rates, viewing taxes and transfers as a consolidated system. The second term comes from our timing assumption that households choose labor supply before observing their take-up cost. As we prove in Appendix C, the sum of these terms is negative when the tax system is optimal and take-up decreases in income ($M'(z) < 0$). Under these assumptions, the automatic reform requires higher marginal tax rates to offset the cut to the voluntary transfer, reducing labor supply.

Suppose households value each transfer dollar at the willingness-to-pay λ, perhaps because transfers are in-kind ($\lambda < 1$) or have insurance value ($\lambda > 1$). Our expression for dW/ds from Proposition 1 would then be multiplied by λ, as we show in Proposition 2 in Appendix C. The

[26] In Appendix C, we give conditions under which this elasticity of income is properly defined. To compute the take-up elasticity, the appropriate weight on income level z is take-up: $h(z)M(z)/\bar{M}$.

[27] This is anticipated by Diamond and Sheshinski (1995). In their analysis of disability insurance, they observe that having the receipt rate rising in the level of disability "is sufficient to make a disability program desirable, provided the marginal utility of consumption of non-workers exceeds that of workers at the optimum without a disability program."

welfare effect dW/ds would still be correct in units of households' willingness-to-pay for the transfer, rather than in dollars. The parameter λ thus simply rescales the welfare effect and cannot reverse its sign.

The reform considered here differs importantly from reforms to ordeals. We instead take the ordeal as given and reallocate resources between voluntary and automatic transfers. The welfare analysis of ordeal reforms weighs the change in ordeal costs to *inframarginal* recipients against the fiscal externalities from changes in take-up (e.g., Finkelstein and Notowidigdo, 2019). By comparison, the welfare analysis of reallocating resources contrasts the value of transfers to inframarginal recipients against ordeal costs to *marginal* recipients. A virtue of our reform is that the welfare-relevant measure of ordeal costs is obtained by the envelope theorem. These are otherwise difficult to measure.[28]

Using the envelope theorem to infer ordeal costs assumes that households make transfer take-up decisions optimally. However, research has found non-optimizing behavior in take-up (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019; Anders and Rafkin, 2022). Importantly, the optimizing assumption works against our conclusion, as it yields upper bounds on ordeal costs. If households do not take-up because of mis-optimization or a lack of information, then ordeal costs would be smaller than what is implied by equating them to marginal benefits. Transfers would then achieve advantageous self-targeting in ways that do not spend real resources on ordeals.

In Appendix C, we provide a formula for the welfare effects of a more general class of transfer reforms. This formula accounts for redistribution both between and across incomes, fiscal savings from marginal recipients, and labor-supply effects. We also consider non-marginal changes to voluntary transfers, in which case we cannot apply the envelope theorem to reveal ordeal costs.

4.4 Quantification

Due to the trade-off between self-targeting and ordeal costs, the welfare effect of shifting between voluntary and automatic transfers is ambiguous. To estimate the welfare effect, we calibrate Equation 12 using our results and several external inputs. We discuss where our welfare calculations are more and less sensitive to assumptions, as there is the room for reasonable disagreement in the calibration of the external inputs and some simplifications.

Calibration. From the PSID, we obtain receipt rates $M(z)$, average benefits $S(z)$, and the income distribution $h(z)$. We compute the first sufficient statistic for targeting, the receipt-rate variance

[28] Shepard and Wagner (2022) evaluate ordeals in a complementary setting to ours, with interesting differences from most transfer programs: markets for subsidized health insurance. They show ordeals can intensify adverse selection in insurance markets, which affects their welfare implications. However, there is no zero-profit condition in non-contributory transfers, so market unraveling is not relevant in programs like SNAP or LIHEAP. It may be relevant for Medicaid, depending on the government's constraints for that program.

σ_M^2, using our estimates of $M(z)$.

The second sufficient statistic, the regression coefficient β, requires us to set welfare weights. We again use CRRA preferences over consumption. In our primary estimates, we calibrate the CRRA parameter $\gamma = 2$, as our model lacks household risk aversion (Chetty and Finkelstein, 2013). A higher γ would give the society a stronger redistributive motive, favoring voluntary transfers if they improve targeting. Fixing the parameter γ, we use the joint distribution of income and consumption to compute the welfare weight for each household. We then estimate differences in welfare weights between transfer recipients and non-recipients conditional on income, similar to the self-targeting analysis in Section 3.

We set the take-up elasticity $\bar{\varepsilon}_b$ to 0.6, the upper end of the range in Krueger and Meyer (2002)'s review of take-up elasticities for unemployment insurance.[29] We also assume a constant elasticity of taxable income $\varepsilon_\tau(z) = 0.3$, following Saez et al. (2012). We calibrate a piecewise-linear tax schedule $T(z)$ using effective average marginal tax rates that incorporate federal and state taxes on income and payroll (Congressional Budget Office, 2015).

Results. Panel A of Table 4 reports our primary estimates of the welfare effects of reallocating resources from a given voluntary transfer to an automatic one. Column 1 shows the social costs from giving up some self-targeting (the first term in Equation 12), while Column 2 shows the fiscal savings on marginal households who exit the transfer (the second term in Equation 12). Due to the envelope argument explained above, Column 2 can also be interpreted as the social savings on ordeal costs among marginal households. Column 3 shows the labor-supply effect (the sum of the third and fourth terms in Equation 12). Column 4 shows the total effect of the reform.

To take SNAP as an example, we find that making SNAP more automatic forgoes some of the social benefits of self-targeting. On the margin, self-targeting is worth about 10.5 cents per dollar of SNAP. In per-recipient terms, self-targeting yields a social benefit of about \$500, an amount that we see as obviously unlikely to be offset by ordeal costs. By comparison, the government saves 5.9 cents per SNAP dollar from marginal households who exit when the voluntary benefit is cut. Finally, the automatic transfer increases marginal tax rates, which reduces labor supply and imposes a one-cent fiscal externality. Together, the net effect of making SNAP more automatic on the margin is a net social loss of 6.7 cents per SNAP dollar. The magnitudes of welfare effects seem consequential, especially for a budget-neutral reform.

Overall, we find a stark trade-off between the social benefits of self-targeting and the social costs of ordeals. Looking across transfers, social benefits are often equal to or greater than our upper-bound estimates of social costs. By consequence, the net social gains from making transfers

[29] The upper end of this range is conservative for our analysis in the sense that it favors automatic transfers. McGarry (1996) likewise finds a take-up elasticity for SSI benefits of 0.5. We are not aware of estimates of take-up elasticities with respect to benefit level for other U.S. transfers, or newer estimates for the same transfers.

automatic tend to be be negative or small, and they are not well-approximated by the social savings on ordeal costs alone. That result is reflected in the dollar-weighted average, which shows that on the margin the forgone social benefits of self-targeting actually exceed the social costs of ordeals. In summary, the value of self-targeting appears to be a credible argument for the status quo of voluntary transfers and against automatic transfers.

There is also considerable heterogeneity in welfare effects across programs. Ordeals in some transfers seem ineffectual: that is, they have social costs but do not induce socially valuable self-targeting. For example, our results suggest potential welfare gains from universal free school meals or making WIC automatic. Automatic benefits, by contrast, appear most costly in housing-assistance programs. Importantly, these programs have severe ordeals: low-quality and constrained choices, as well as long waiting lists. Our framework is thus not uniformly favorable towards ordeals but makes finer distinctions according to how effective an ordeal is in causing self-targeting.

Table 4: Welfare Effects of Making Transfers Automatic (Cents per Transfer Dollar)

	(1) Self-Targeting	(2) Upper Bound on Ordeals	(3) Labor-Supply Effects	(4) Total
Panel A: Primary Estimates				
Dollar-Weighted Average	**-6.1**	**5.7**	**-0.9**	**-1.4**
SNAP	-10.5	5.9	-1.0	-5.6
Medicaid	-4.7	8.6	-1.4	2.5
Housing Assistance	-11.0	3.1	-0.5	-8.4
TANF	-1.5	0.6	-0.1	-1.0
SSI	-2.7	3.5	-0.4	0.3
School Lunch	2.5	5.7	-0.9	7.4
WIC	-0.1	2.3	-0.4	1.8
LIHEAP	-0.7	2.3	-0.3	1.3
Panel B: Sensitivity (Dollar-Weighted Average)				
SWF curvature $\gamma = \frac{1}{2} \times$ primary estimate	-2.3	5.7	-0.9	2.5
SWF curvature $\gamma = 2 \times$ primary estimate	-10.6	5.7	-0.9	-5.8
SWF over lifetime income	-5.8	5.7	-0.9	-1.0
Take-up elasticity $\bar{\varepsilon}_b = \frac{1}{2} \times$ primary estimate	-6.1	2.8	-0.9	-4.2
Take-up elasticity $\bar{\varepsilon}_b = 2 \times$ primary estimate	-6.1	11.4	-0.9	4.3
Elasticity of taxable income $\varepsilon_\tau = \frac{1}{2} \times$ primary estimate	-6.1	5.7	-0.4	-0.9
Elasticity of taxable income $\varepsilon_\tau = 2 \times$ primary estimate	-6.1	5.7	-1.8	-2.2

Notes: This table reports estimates of the welfare effects of the reform, which marginally reduces the voluntary transfer to make it automatic. We calibrate the welfare weights by assuming a CES social welfare function with curvature parameter $\gamma = 2$. We calibrate the fiscal cost of marginals by assuming the takeup elasticity is $\bar{\varepsilon}_b = 0.6$. We calibrate the elasticity of taxable income at $\varepsilon_\tau = 0.3$. All columns report the money-metric welfare gains in cents per transfer dollar. Columns correspond to the terms of Equation 12, where we divide each term by the average welfare weight to yield a money-metric interpretation.

Sensitivity Analysis and Discussion. Panel B examines the sensitivity of our results to the calibrated parameters.

The more society cares more about redistribution, the larger are the welfare losses from forgoing self-targeting in transfers. Put another way, automating transfers is likely to be socially desirable only when society cares relatively less about the poor (i.e., it has a lower γ).[30] Meanwhile, redefining the social welfare function to be in terms of lifetime income rather than consumption does not much affect the conclusions of the analysis.

The take-up elasticity is critically important to fiscal costs and thus the implied ordeal costs. If take-up is more highly responsive to the benefit level than we expect, this would imply larger ordeal costs on the margin and thus could motivate automatic transfers. Results are less sensitive to the elasticity of taxable income. Across these permutations of our analysis, self-targeting remains a quantitatively important advantage of voluntary transfers. Indeed, self-targeting typically eliminates most if not all of the social savings on ordeals, even at upper-bound values for ordeal costs.

Valuing transfer dollars differently from cash, because of in-kind distortions or insurance value, would scale all entries in Table 4 by the willingness to pay for the transfer per dollar cost (Appendix C). For example, if people value a dollar of Section 8 housing vouchers at 80 cents (as in Reeder (1985)), then the overall welfare loss per transfer dollar in our reform is 6.7 cents (= 8.4 × 0.8).

This welfare analysis has several limitations. First, it does not account for differences in the government's administrative costs between voluntary and automatic transfers. Little is known about the appropriate values for these costs (Isaacs, 2008), but it is reasonable to suspect they favor automatic transfers. Second, we ignore behavioral responses to transfers beyond take-up and labor supply, such as cross-program enrollment spillovers or dynamic incentives for human-capital investment. Third, we assume homogeneous labor supply elasticities. Heterogeneity by income could shift our conclusions in either direction.

5 Conclusion

A large body of empirical research has studied many specific ordeals in transfer programs. It finds mixed evidence that, on the margin, these ordeals have favorable selection properties. Taken as a whole, this literature would seem to have radical implications for the design of social safety nets: Why do governments hassle people by making them ask for help, if those who do not ask are no less in need? Why not just send help automatically?

What is true among the complier population for the studied ordeals does not, we find, generalize to always- and never-takers of transfer programs. We show transfer recipients are, relative to non-

[30] This comparative static runs counter to current U.S. political debates, as progressive policy advocates often support making reforms automatic.

recipients, strongly and consistently negatively selected on consumption and lifetime income given income. This selection mostly reflects take-up among the eligible rather than eligibility rules.

Such self-targeting might rescue the case for voluntary take-up. To determine if it does, we quantify the social trade-off between self-targeting and ordeal costs. In particular, we examine reforms that incrementally shift redistribution from voluntary to automatic transfers. Calibrating a welfare-effect formula using our empirical estimates, we find that the social benefits from self-targeting generally equal or exceed upper-bound estimtes of the social costs of ordeals. There would be, by consequence, social losses from making transfers automatic overall. However, some transfers inflict ordeal costs but achieve minimal self-targeting, and in these programs, the U.S. could indeed achieve considerable welfare gains by eliminating the need to sign up.

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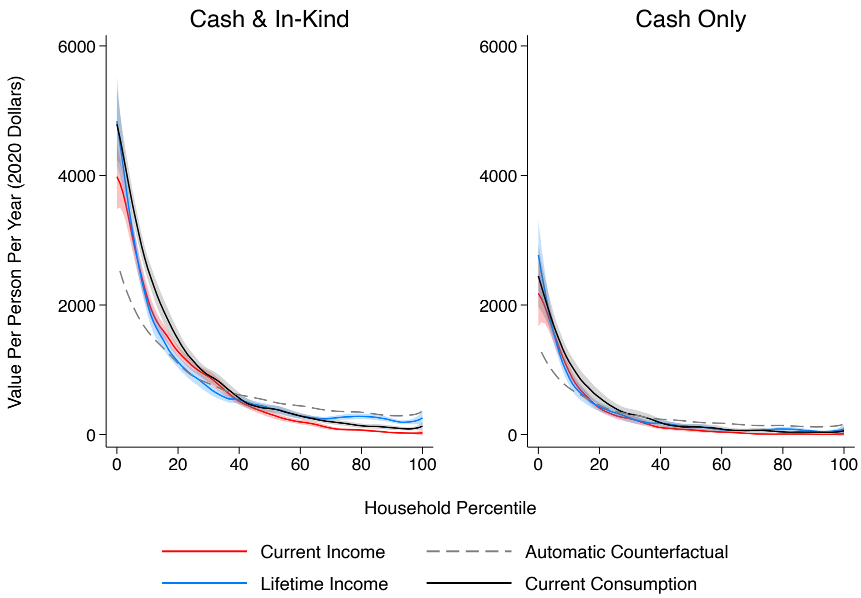
Appendices for Online Publication

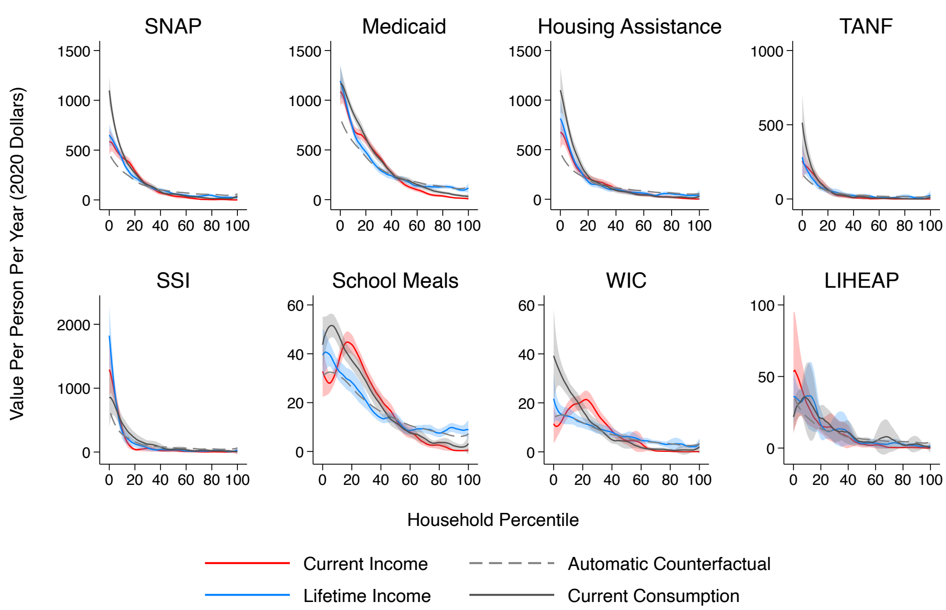
A Additional Tables and Figures

Figure A1: Receipt and Value of Transfer Benefits as a Function of Household Rank

Panel A: Average Total Annual Per-Capita Transfer



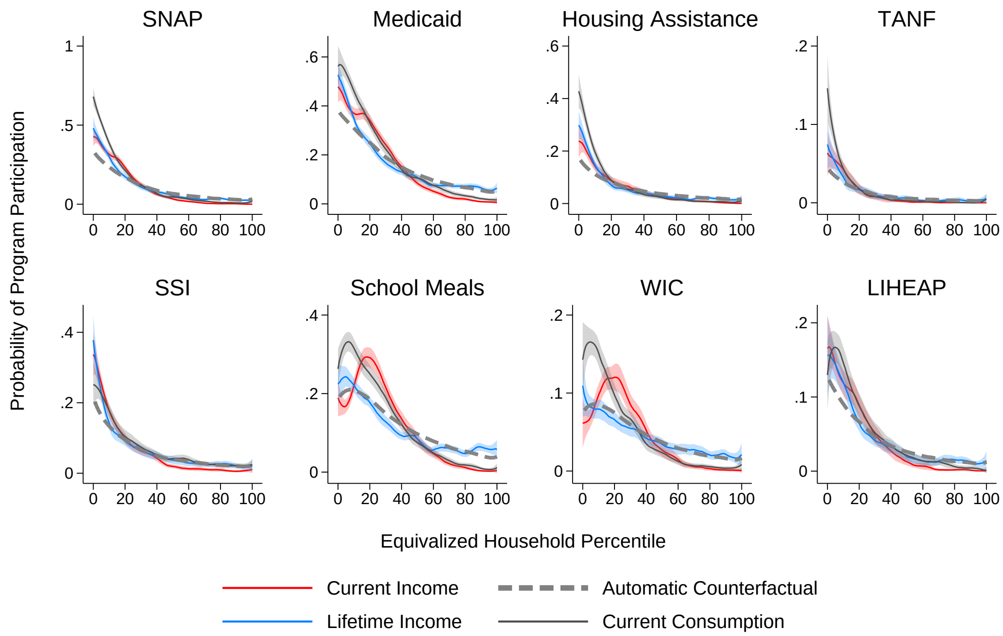
Panel B: Average Annual Per-Capita Transfer by Program



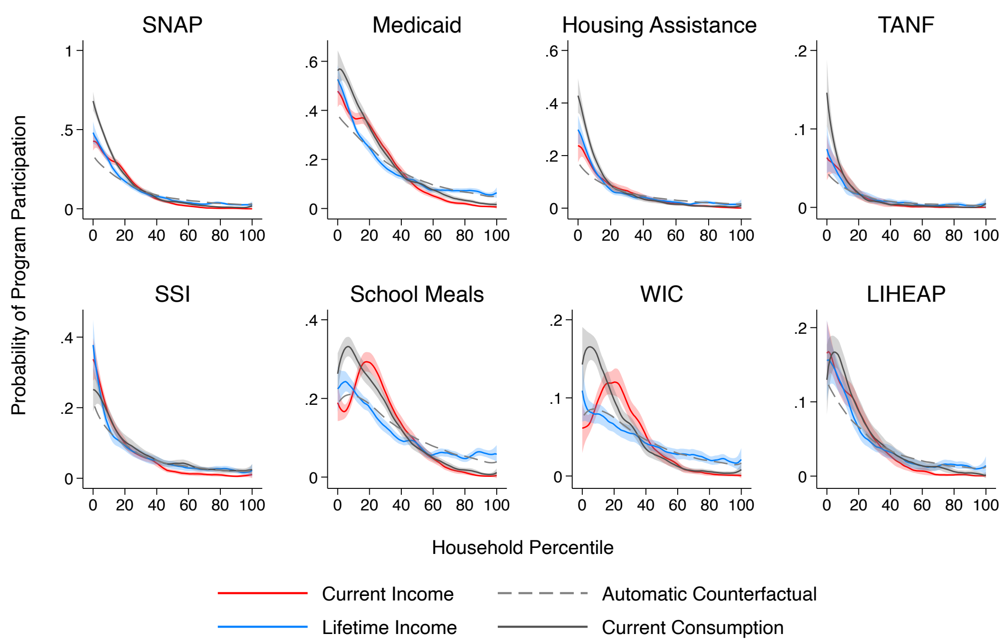
Notes: This figure displays average annual per-capita values of benefits, in total and by program, as functions of household ranks, in the distributions of household current income, lifetime income, and current consumption. There is no equivalence scale applied to household income. The functions are estimated by local linear regressions with bandwidths of three percentiles. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

Figure A2: Receipt Rates by Program

Panel A: Equivalized Household Rank

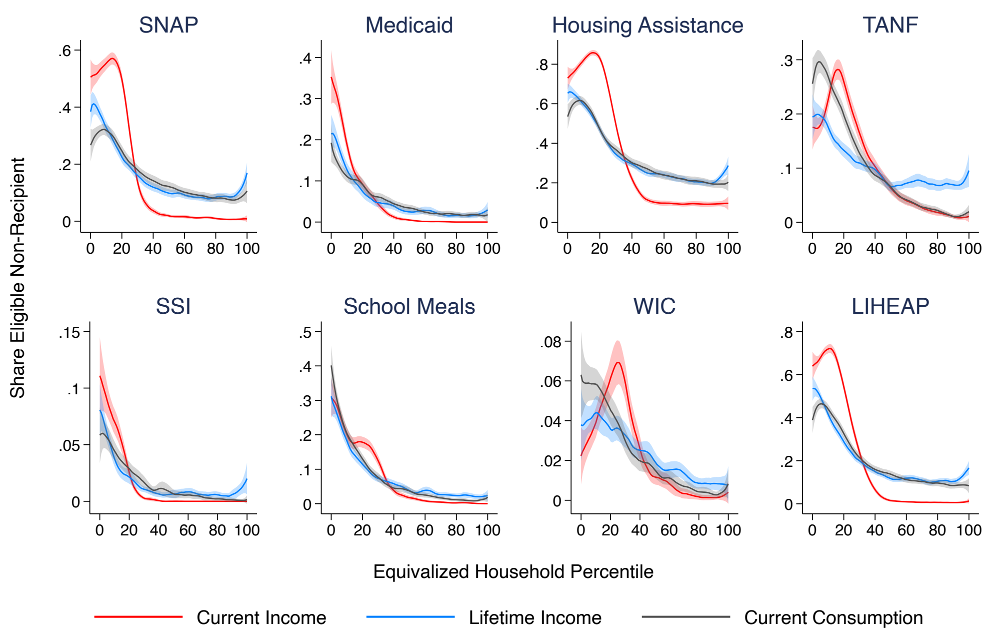


Panel B: Household Rank



Notes: This figure displays the shares of households that are transfer recipients as functions of ranks in the distributions of current income, lifetime income, and current consumption. Panel A shows the results using the equivalence scale of Citro and Michael (1995). Panel B does not adjust for differences in household size and composition. For comparability to figures in the main text, the dashed lines ("automatic counterfactual") compute the receipt rates at each consumption rank in a counterfactual where receipt rates are solely functions of income. The functions are estimated by local linear regressions with bandwidths of three percentiles. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

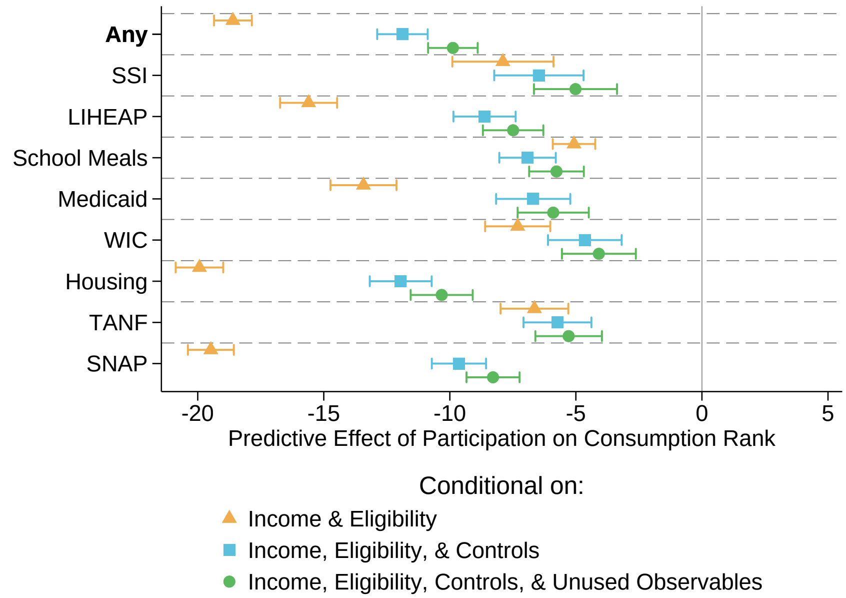
Figure A3: Eligible Non-Receipt Rates by Program



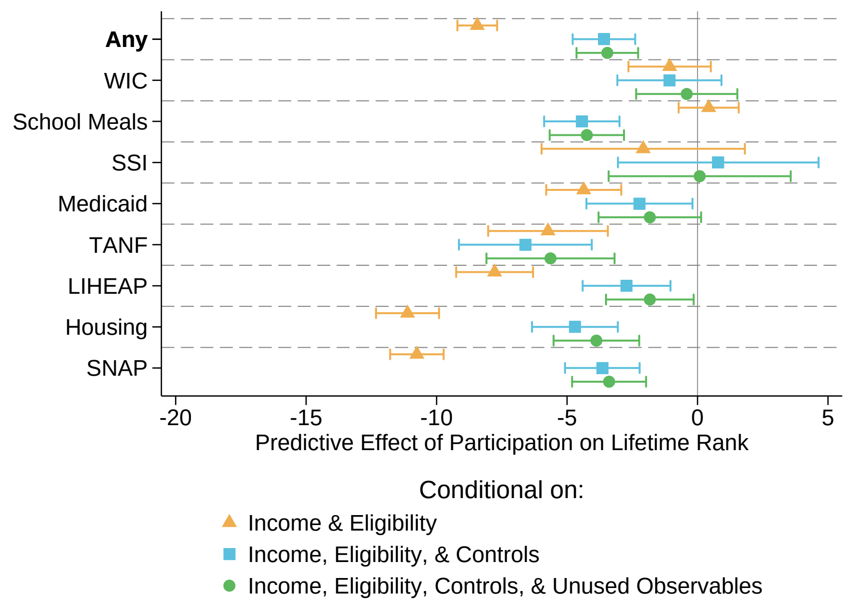
Notes: This figure displays the shares of households that are eligible nonrecipients of a transfer as functions of their ranks in the distributions of equivalized current income, lifetime income, and consumption. The functions are estimated by local linear regressions with abandwidth of three percentiles. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, with clustering by household.

Figure A4: What Explains Selection into Transfer Receipt? With Controls

Panel A: Selection on Consumption



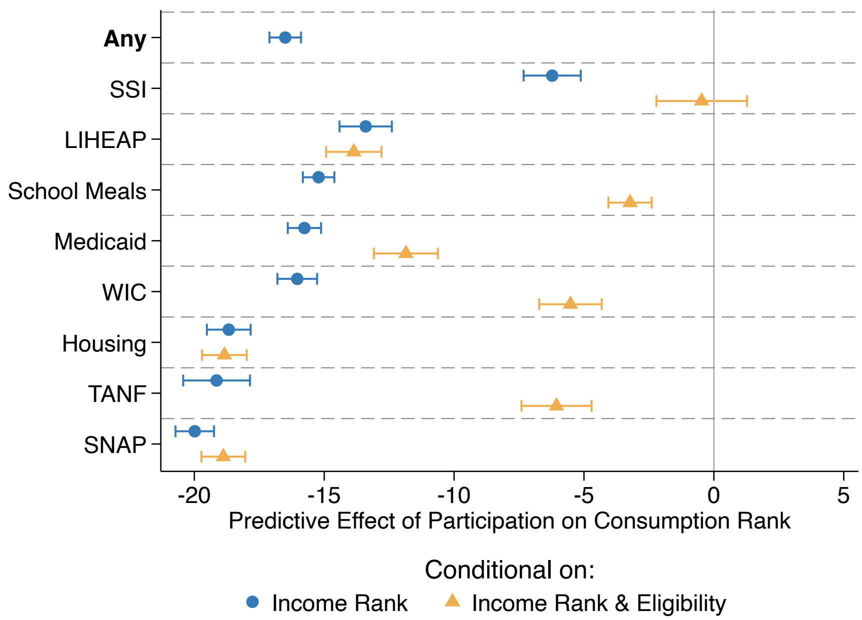
Panel B: Selection on Lifetime Income



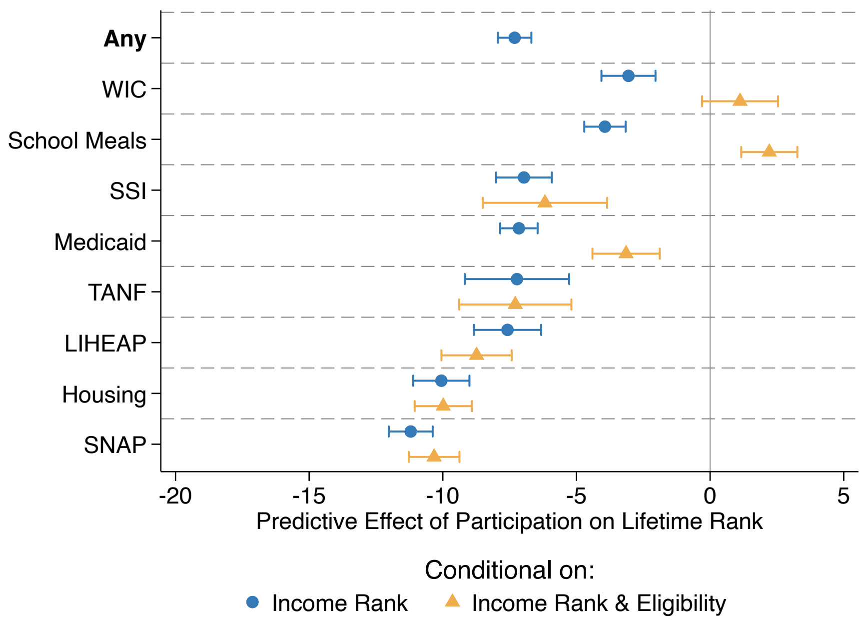
Notes: This figure displays estimates of the predictive effect of transfer receipt on consumption rank (Panel A) or lifetime-income rank (Panel B), conditional on current-income rank (coefficient β from Equation 6). For estimates represented by the yellow diamonds, we estimate the regression only on people whom we simulate to be eligible. For estimates represented by the teal squares, we condition on eligibility and characteristics that enter any eligibility rule. For estimates represented by green circles, we condition on eligibility, eligibility characteristics, and several demographic characteristics (race, education, and marital status). The "any" row is an indicator for receipt of at least one of the eight transfers. The confidence intervals are for the 95-percent level and reflect clustered standard errors by household.

Figure A5: What Explains Selection into Transfer Receipt?
Reclassifying Simulated-In Eligible Recipients

Panel A: Selection on Consumption



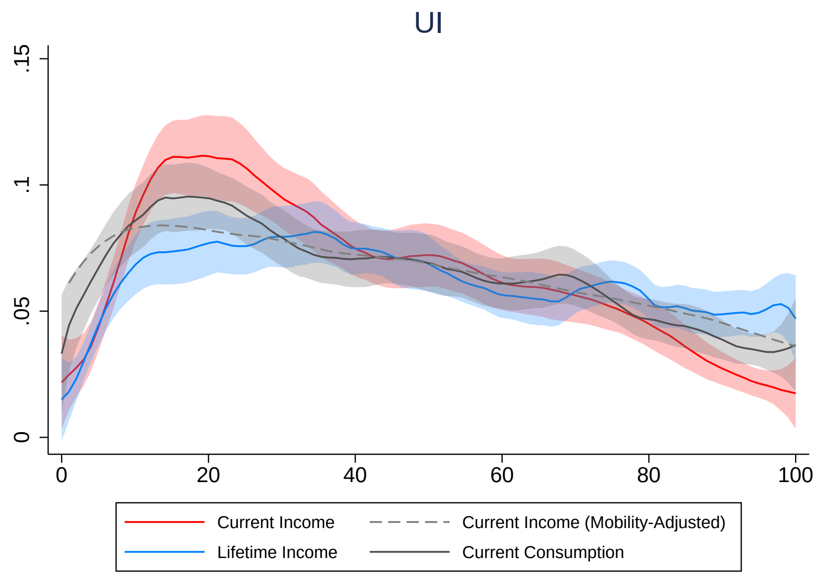
Panel B: Selection on Lifetime Income



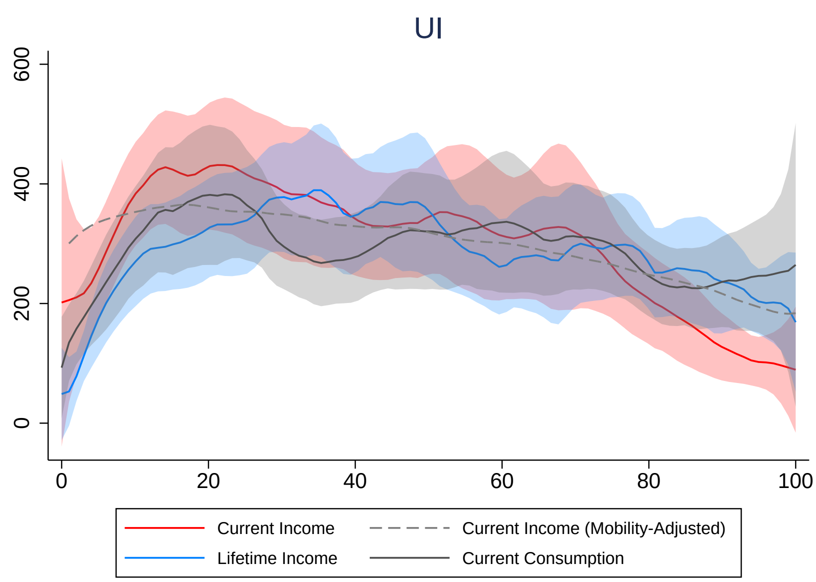
Notes: This figure displays estimates of the predictive effect of transfer benefit receipt on consumption rank or lifetime-income rank, conditional on current-income rank (coefficient β from Equation 6). For estimates represented by blue circles, we add no additional control variables to the specification, whereas for the yellow diamonds, we add program eligibility. The confidence intervals are at the 95-percent level with clustering by household. In Panel B, we adapt Equation 6 by replacing the transfer indicator with indicators for the number of unique transfers received. Eligibility samples in both panels reclassify all recipients as eligible, even if we initially simulate them to be ineligible.

Figure A6: Receipt and Value of Unemployment Insurance
as a Function of Equivalized Household Rank

Panel A: Receipt Rate

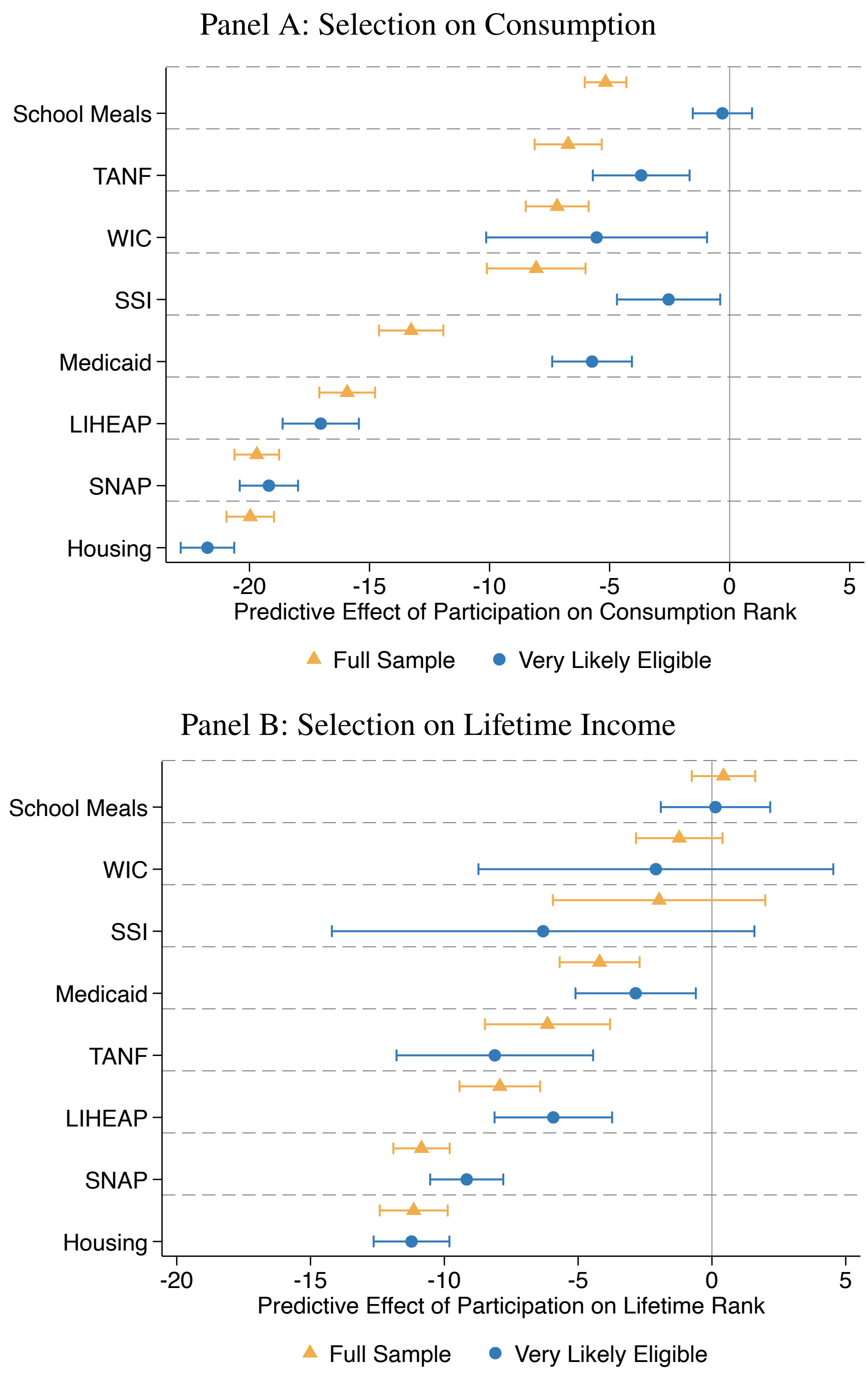


Panel B: Average Annual Per-Capita Transfer



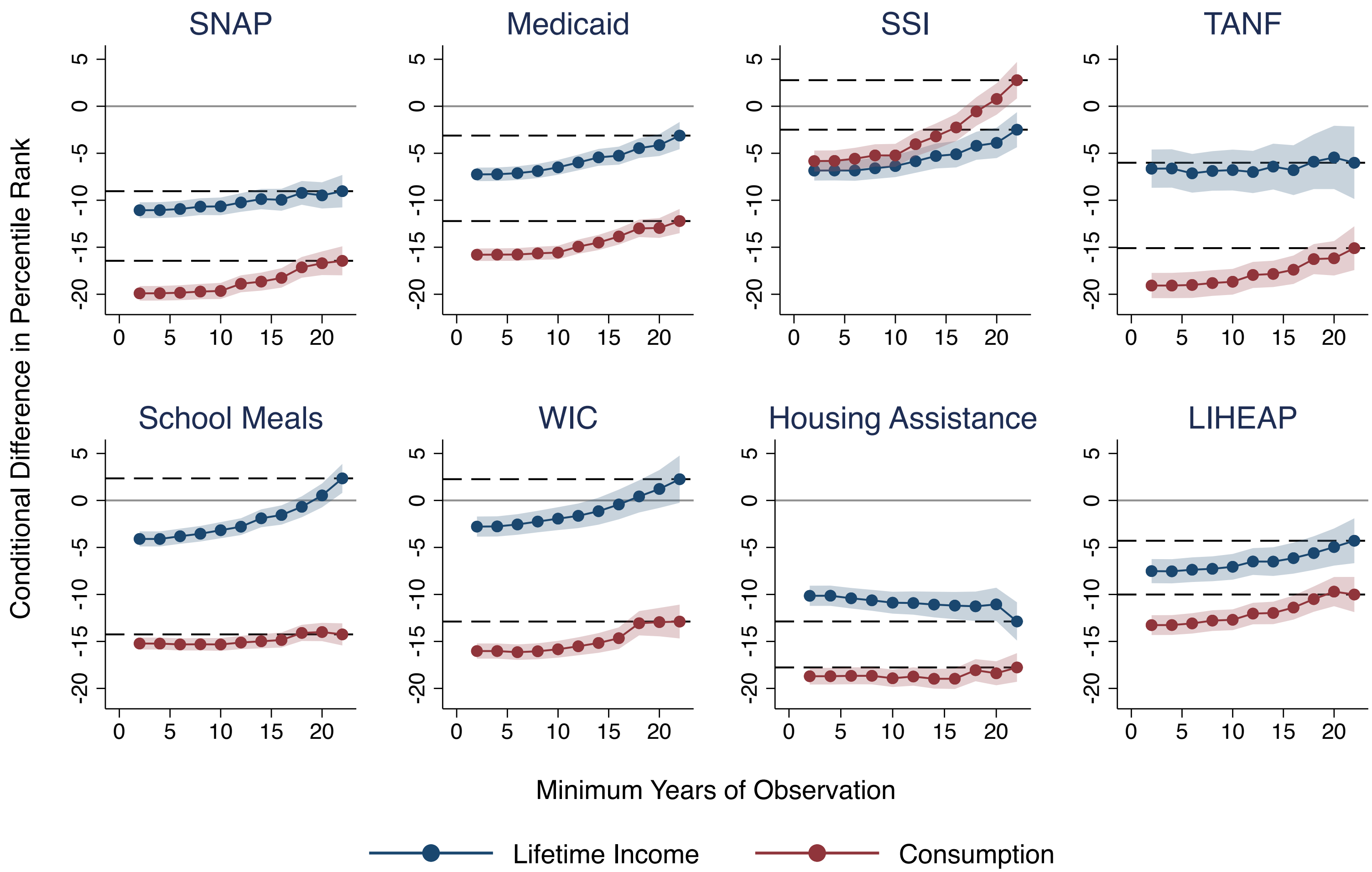
Notes: This figure displays average annual per-capita values of benefits and receipt rates for unemployment insurance as functions of household ranks in the distributions of equivalized current income, lifetime income, and current consumption. The functions are estimated by local linear regressions with bandwidths of three percentiles. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

Figure A7: Selection into Transfer Receipt: Very Likely Eligible Subsample



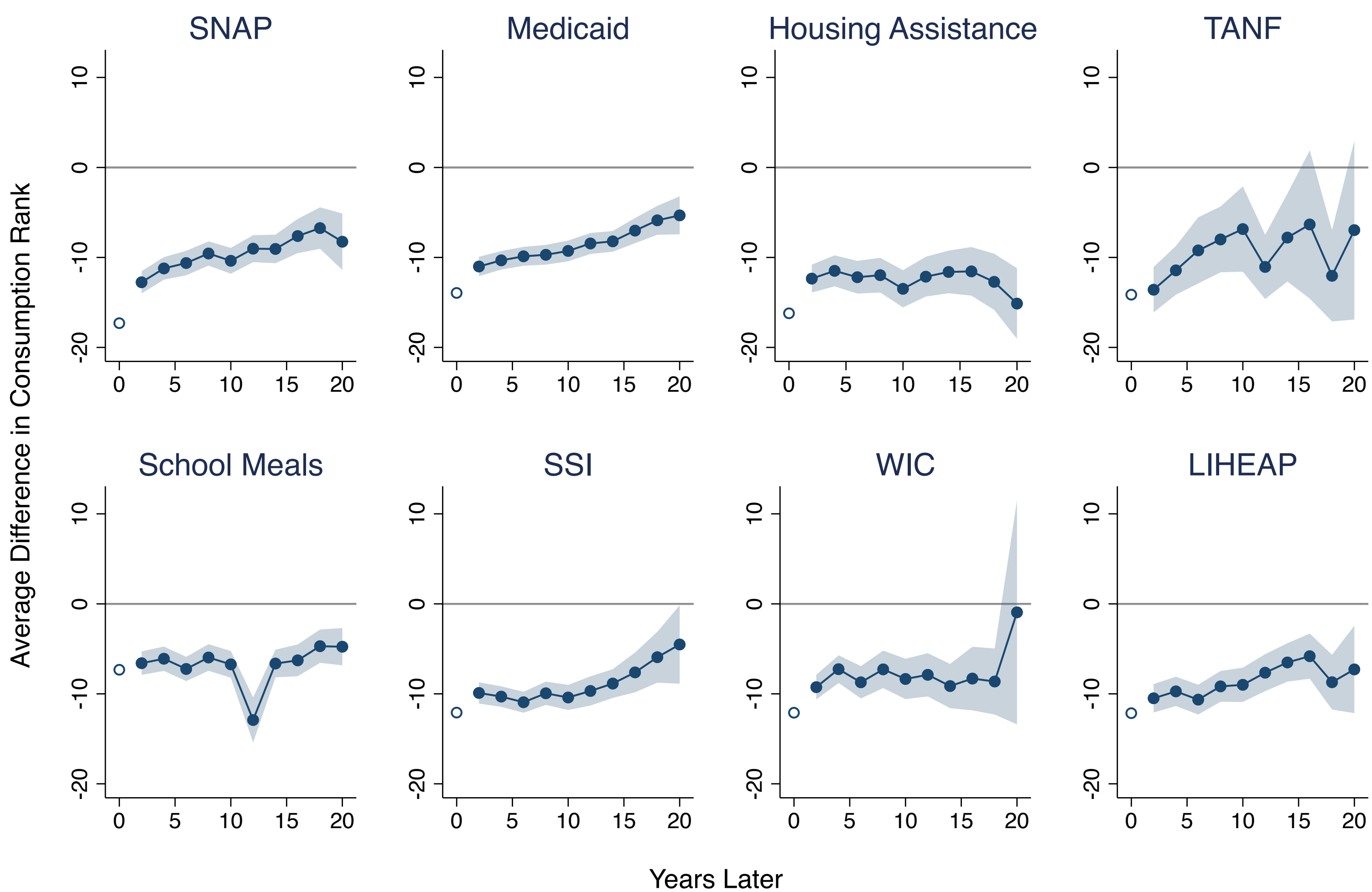
Notes: This figure displays estimates of the predictive effect of transfer benefit receipt on consumption rank or lifetime-income rank, conditional on current-income rank (coefficient γ from Equation 6). Both yellow diamonds and blue circles restrict the sample to simulated eligibles. For estimates represented by blue circles, we further limit the sample to people who, in a logistic regression of simulated eligibility status on demographic observables, have a predicted probability of eligibility above 0.8. The eligibility logit uses the following demographic variables: age (in ten bins), sex, marital status, race/ethnicity (white, black, Hispanic, other), education (less than high school, high school, some college, BA, more than BA), household size, homeownership, disability, and rank-transformed current income, lifetime income, and consumption. The confidence intervals are for the 95-percent level and clustering by household. In Panel B, we adapt Equation 6 by replacing the transfer indicator with indicators for the number of unique transfers received.

Figure A8: Transfer Receipt as a Function of Rank



Notes: This figure displays program receipt rates as functions of income percentile ranks for current or lifetime income. The functions are estimated using a local linear regression with a bandwidth of three percentiles. Shaded regions reflect bootstrapped 95-percent simultaneous confidence bands, as in Chernozhukov et al. (2013), with clustering by household.

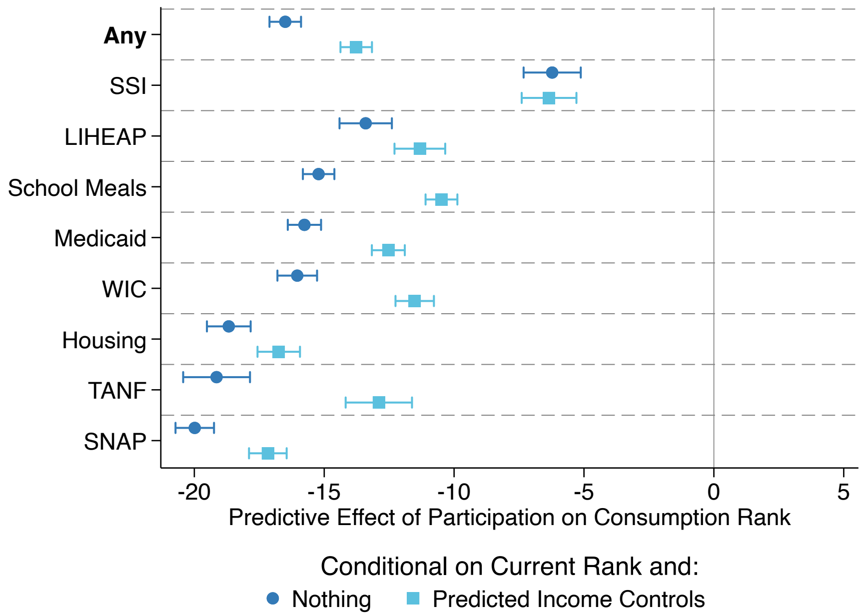
Figure A9: Advantageous Selection on Transfer Receipt in the Distant Future



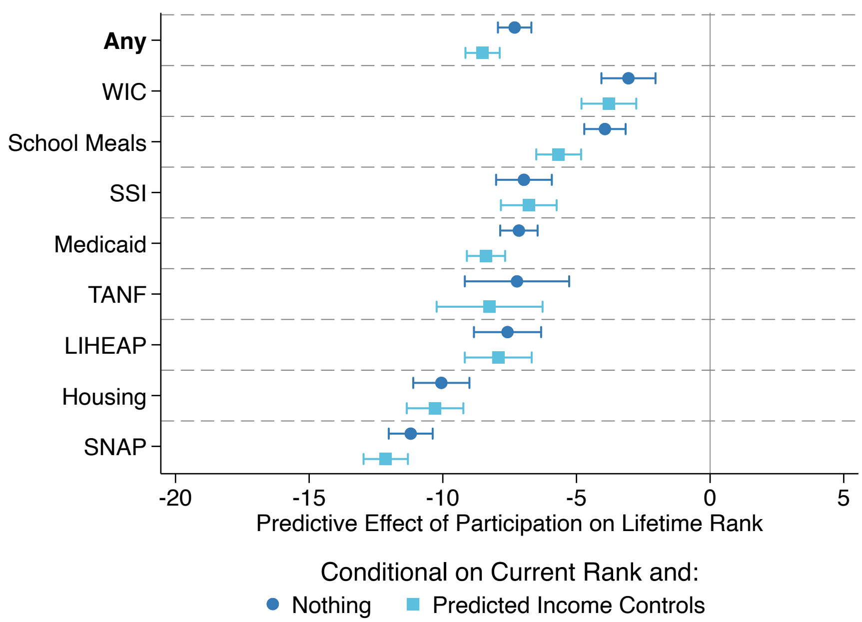
Notes: This figure displays the predictive effect of transfer receipt k years ahead on consumption rank this year conditional on current income rank. The regression equation is $\overline{R}_{it} = \alpha_{ct} + \beta D_{i,t+k} + f(R_{it}) + u_{it}$, where we plot β for each horizon k. The estimation sample is always restricted to current nonrecipients, $D_{it} = 0$. Shaded regions reflect bootstrapped 95-percent pointwise confidence intervals, with clustering by household.

Figure A10: Selection into Transfers, with Predicted-Income Control

Panel A: Consumption

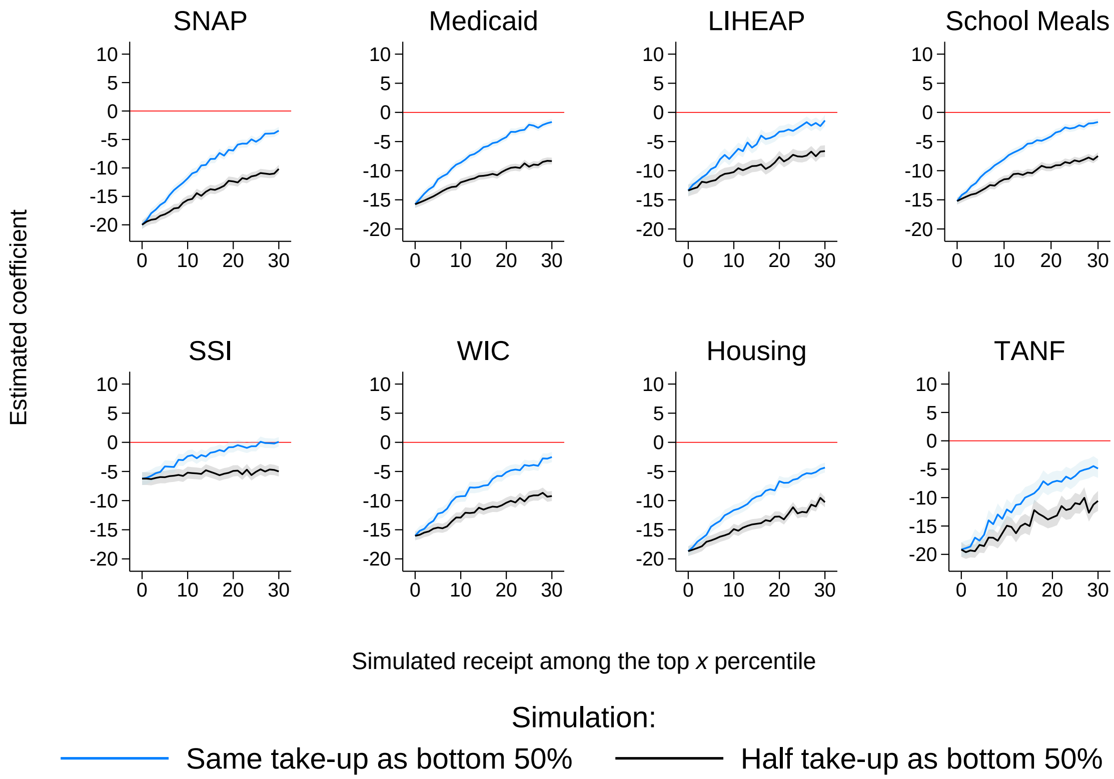


Panel B: Lifetime Income



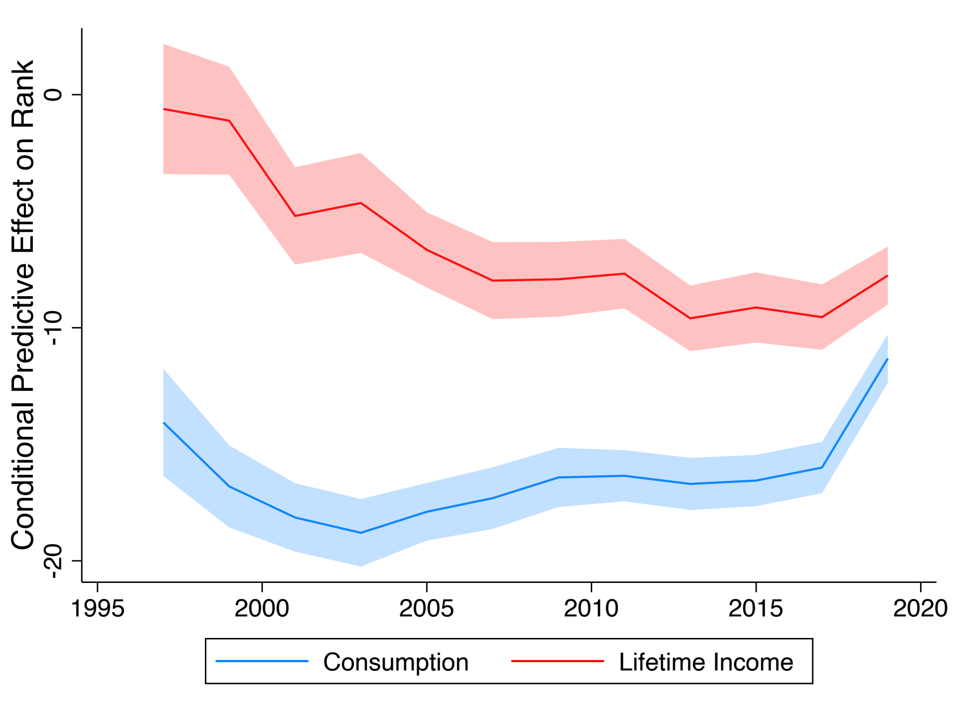
Notes: This figure displays estimates of the predictive effect of transfer receipt on equivalized household consumption rank, conditional on current-income rank as well as predicted-income rank. Income prediction uses a Poisson regression as explained in Section 3. For estimates represented by blue circles, we do not add additional control variables to the specification, whereas for the teal squares, we estimate the regression only on people whom we simulate to be eligible. The "any" row of Panel A is an indicator for receipt of at least one of the eight transfers. The confidence intervals are at the 95-percent level with clustering by household.

Figure A11: Measurement Error Simulations



Notes: This figure displays the predictive effect of transfer receipt on consumption rank given income rank (Equation 6) when we assume that take-up is underreported for the top x consumption percentiles. In blue, we assume that the top x percentiles actually have the same take-up rate as the bottom half of the consumption distribution. In black we assume that top take-up rate is half that of the bottom half of the consumption distribution. Shaded regions reflect 95-percent pointwise confidence intervals, with clustering by household.

Figure A12: Selection into Transfer Receipt Over Time



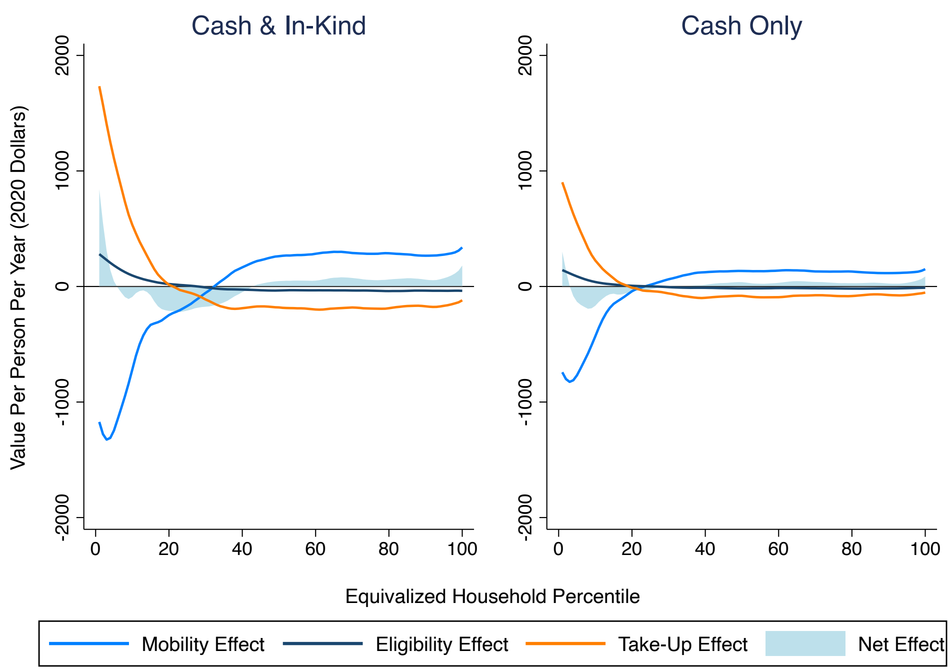
Notes: This figure displays coefficients from the following regression specification:

$$\overline{R}_{its} = \alpha_{cts} + \beta_t D_{its} + f_s(R_{it}) + u_{its},$$

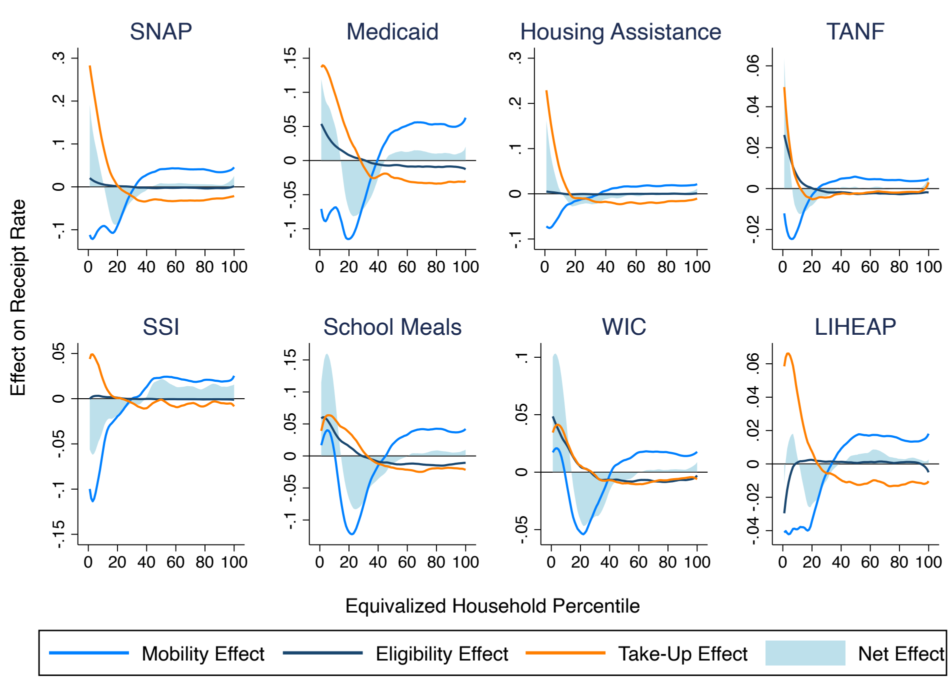
where i denotes households, t denotes years, and s denotes transfer programs. The outcome $\overline{R}_{its}$ is equivalized household consumption rank in the blue line and equivalized household lifetime income rank in red. The data are stacked across programs, so that each individual–year appears eight times, once for each transfer program s. We thus include cohort-year effects α_{cts} specific to each transfer, as well as transfer-specific spline controls $f_s(\cdot)$ for current-income rank R_{it}. The coefficients β_t thus report an average selection effect across transfer programs in a given year t. Shaded regions reflect 95-percent pointwise confidence intervals, with clustering by household. All regressions use PSID sample weights but are not otherwise adjust to account for variation in transfer program size.

Figure A13: Decomposition of Difference Between Consumption and Current Incidence

Panel A: Average Total Annual Per-Capita Benefits

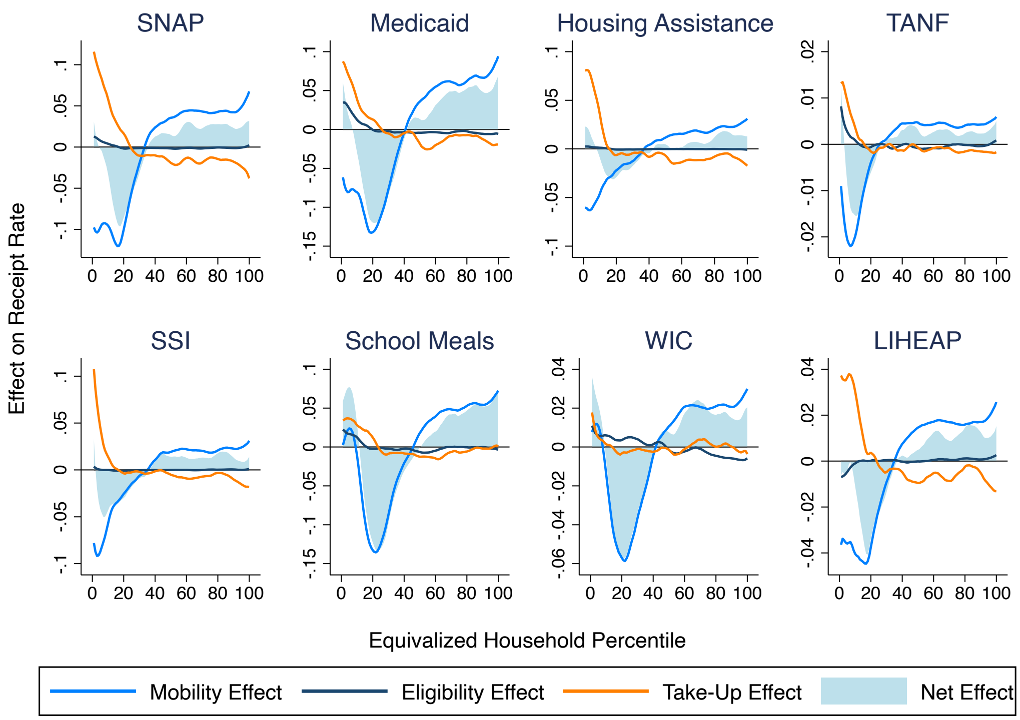


Panel B: Receipt Rate by Program



Notes: This figure displays decompositions of differences between current-income and consumption incidence into mobility, eligibility, and take-up effects. The underlying functions are estimated using local linear regressions with bandwidths of three percentiles.

Figure A14: Decomposition of Difference Between Lifetime and Current Incidence



Notes: This figure displays a decomposition of the difference between current and lifetime incidence into mobility and take-up effects. The original functions are estimated using a local linear regression with a bandwidth of three percentiles.

Table A1: SNAP Receipt, Eligibility, and Take-Up Rates by Income and Lifetime Income Quintile

Panel A: Receipt Rate

		Income Quintile					
		1	2	3	4	5	Avg.
Lifetime Income Quintile	1	43.2	19.9	6.4	1.7	0.1	35.3
	2	26.1	11.9	3.0	1.0	0.1	8.4
	3	20.5	9.5	2.0	0.4	0.2	3.3
	4	19.7	6.9	2.1	0.3	0.2	1.3
	5	20.2	4.6	1.9	0.4	0.2	0.9
	Avg.	33.6	12.2	2.8	0.6	0.2	

Panel B: Simulated Eligibility Rate

		Income Quintile					
		1	2	3	4	5	Avg.
Lifetime Income Quintile	1	80.5	24.0	0.0	0.0	0.0	49.7
	2	71.0	16.0	0.2	0.0	0.0	17.5
	3	70.8	13.7	0.0	0.0	0.0	10.0
	4	68.3	15.5	0.1	0.1	0.0	7.9
	5	76.3	19.0	2.3	1.0	0.1	10.0
	Avg.	76.3	18.1	0.4	0.3	0.1	

Panel C: Take-Up Rate Among Simulated Eligibles

		Income Quintile					
		1	2	3	4	5	Avg.
Lifetime Income Quintile	1	46.2	36.3	.	.	.	44.9
	2	30.1	27.5	.	.	.	29.4
	3	25.0	25.1	.	.	.	24.9
	4	23.9	17.8	.	.	.	22.3
	5	23.0	10.3	.	.	.	18.9
	Avg.	37.5	27.2	.	.	.	

Notes: This table reports the shares of households that receive SNAP (Panel A), are simulated to be eligible for SNAP (Panel B), and take up SNAP conditional on being simulated eligible (Panel C). Households are split by quintiles of equivalized household current and lifetime income. Due to low rates of simulated eligibility, we do not report take-up rates for the top three income quintiles.

Table A2: Transfer Amounts at the Bottom of the Distributions of Income and Consumption

	Income				Consumption				Counterfactual for Consumption			
Percentiles	0–5	0–10	0–25	0–50	0–5	0–10	0–25	0–50	0–5	0–10	0–25	0–50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Average Total Dollars Per Person Per Year												
Total	3,634	3,216	2,184	1,380	3,973	3,294	2,136	1,316	2,271	2,015	1,530	1,099
	(97)	(67)	(33)	(18)	(88)	(58)	(31)	(18)	(25)	(18)	(11)	(8)
Cash	1,928	1,676	1,014	577	1,917	1,545	927	545	1,086	931	663	454
	(72)	(49)	(23)	(12)	(62)	(41)	(21)	(12)	(16)	(11)	(7)	(4)
Panel B: Average Amounts by Program												
SNAP	568	511	409	247	788	642	397	236	404	363	276	195
	(19)	(13)	(7)	(4)	(19)	(13)	(7)	(4)	(4)	(3)	(2)	(1)
Medicaid	1,023	941	746	518	1,095	979	739	488	740	683	554	415
	(28)	(19)	(11)	(6)	(25)	(17)	(10)	(6)	(6)	(4)	(3)	(2)
Housing Assistance	645	562	374	245	882	695	411	245	401	357	273	199
	(29)	(19)	(10)	(6)	(28)	(18)	(10)	(5)	(5)	(4)	(2)	(1)
TANF	230	219	138	75	343	253	132	73	141	122	86	58
	(18)	(13)	(7)	(3)	(22)	(14)	(6)	(3)	(2)	(2)	(1)	(1)
SSI	1,087	909	441	238	772	628	378	222	514	423	283	188
	(62)	(42)	(20)	(10)	(57)	(36)	(18)	(10)	(11)	(7)	(4)	(3)
School Meals	31	28	36	29	50	50	41	28	32	32	29	24
	(2)	(1)	(1)	(0)	(2)	(1)	(1)	(0)	(0)	(0)	(0)	(0)
WIC	10	12	17	13	35	31	22	13	15	15	14	11
	(1)	(1)	(1)	(0)	(2)	(1)	(1)	(0)	(0)	(0)	(0)	(0)
LIHEAP	50	45	30	19	30	34	25	16	31	27	20	15
	(10)	(6)	(3)	(1)	(2)	(5)	(2)	(1)	(1)	(1)	(0)	(0)

Notes: This table reports sample means of average transfer payments per person per year (in 2020 constant dollars), in total and by transfer program. Each column is for a different range of percentiles in a distribution. Columns 1–4 are with respect to household equivalized current income, and Columns 5–8 are with respect to household equivalized consumption. Columns 9–12 calculate the consumption incidence under the counterfactual in which receipt is a function of income rank. Parentheses report standard errors clustered by household.

Table A3: Transfer Amounts at the Bottom of the Distributions of Current and Lifetime Income

	Current Income				Lifetime Income				Counterfactual for Lifetime Income			
Percentiles	0–5	0–10	0–25	0–50	0–5	0–10	0–25	0–50	0–5	0–10	0–25	0–50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Average Dollars Per Person Per Year												
Total	3,634	3,216	2,184	1,380	3,815	3,099	1,927	1,220	2,581	2,196	1,551	1,080
	(97)	(67)	(33)	(18)	(102)	(63)	(32)	(18)	(25)	(18)	(12)	(8)
Cash	1,928	1,676	1,014	577	2,011	1,544	881	521	1,293	1,056	693	456
	(72)	(49)	(23)	(12)	(77)	(46)	(22)	(12)	(16)	(11)	(7)	(4)
Panel B: Average Amounts by Program												
SNAP	568	511	409	247	606	525	352	218	434	381	275	191
	(19)	(13)	(7)	(4)	(19)	(13)	(7)	(4)	(4)	(3)	(2)	(1)
Medicaid	1,023	941	746	518	1,070	931	654	443	796	713	546	400
	(28)	(19)	(11)	(6)	(27)	(18)	(10)	(6)	(6)	(5)	(3)	(2)
Housing Assistance	645	562	374	245	677	572	347	223	452	386	276	195
	(29)	(19)	(10)	(6)	(28)	(18)	(9)	(5)	(5)	(4)	(2)	(1)
TANF	230	219	138	75	216	172	102	64	165	137	90	59
	(18)	(13)	(7)	(3)	(16)	(11)	(5)	(3)	(3)	(2)	(1)	(1)
SSI	1,087	909	441	238	1,166	822	404	223	664	513	309	194
	(62)	(42)	(20)	(10)	(72)	(42)	(19)	(10)	(11)	(8)	(4)	(3)
School Meals	31	28	36	29	42	39	33	24	30	30	27	22
	(2)	(1)	(1)	(0)	(2)	(1)	(1)	(0)	(0)	(0)	(0)	(0)
WIC	10	12	17	13	18	17	14	11	13	14	13	10
	(1)	(1)	(1)	(0)	(1)	(1)	(1)	(0)	(0)	(0)	(0)	(0)
LIHEAP	50	45	30	19	33	31	26	17	35	30	21	15
	(10)	(6)	(3)	(1)	(3)	(2)	(2)	(1)	(1)	(1)	(0)	(0)

Notes: This table reports sample means of average transfer payments per person per year (in 2020 constant dollars), in total and by transfer program. Each column is for a different range of percentiles in a distribution. Columns 1–4 are with respect to household equivalized current income, and Columns 5–8 are with respect to household equivalized lifetime income. Columns 9–12 calculate the lifetime incidence under the counterfactual in which receipt is a function of current-income rank. Parentheses report standard errors, clustered by household.

Table A4: Dollar and Percentage Differences Between Recipients and Similar-Income Non-Recipients

	Proportion Difference		Difference in 2020 Constant Dollars	
	Consumption (1)	Lifetime Income (2)	Consumption (3)	Lifetime Income (4)
SNAP	-0.461***	-0.440***	-10,843***	-23,648***
	(0.017)	(0.077)	(413)	(4,262)
Medicaid	-0.405***	-0.495***	-9,516***	-26,614***
	(0.013)	(0.053)	(300)	(3,058)
Housing Assistance	-0.367***	-0.205**	-8,626***	-11,029**
	(0.022)	(0.099)	(516)	(5,359)
TANF	-0.589***	-0.839***	-13,848***	-47,394***
	(0.079)	(0.168)	(1,848)	(9,855)
SSI	-0.084***	-0.353***	-1,976***	-18,979***
	(0.018)	(0.081)	(413)	(4,409)
School Meals	-0.487***	-0.420***	-12,080***	-22,385***
	(0.012)	(0.038)	(310)	(2,113)
WIC	-0.506***	-0.481***	-12,572***	-25,721***
	(0.023)	(0.053)	(585)	(2,939)
LIHEAP	-0.321***	-0.405***	-7,541***	-21,810***
	(0.021)	(0.075)	(501)	(4,152)

Notes: This table reports estimates of differences in consumption and lifetime income between transfer receipients and nonrecipients, conditional on current income. All columns report estimates obtained via Poisson regression. Columns 1 and 2 report exponentiated coefficients ($\exp(\beta) - 1$) from these regressions. Columns 3 and 4 report the dollar effects. Each cell is its own regression. All specifications control flexibly for the logarithm of equivalized current household income using cubic basis splines. Standard errors are clustered by household. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A5: Selection into Transfers, Adjusted for Misreporting of Transfer Receipt

	Baseline		Adjusted for Misreporting	
	Consumption (1)	Lifetime Income (2)	Consumption (3)	Lifetime Income (4)
Panel A: SNAP				
Receives Transfer	-17.6***	-11.1***	-26.4***	-14.3***
	(0.6)	(0.6)	(0.8)	(0.9)
Panel B: Medicaid				
Receives Transfer	-14.4***	-7.0***	-23.4***	-12.2***
	(0.5)	(0.5)	(0.7)	(0.8)

Notes: This table examines the effect of corrections for misreporting of transfer receipt on estimates of selection into transfers by consumption rank and lifetime-income rank, conditional on current-income rank. The estimating equation is Equation 6. In Columns 3 and 4, we replace reported receipt with the adjusted measures from Mittag (2019) for SNAP and Davern et al. (2019) for Medicaid. All specifications control flexibly for current-income rank using cubic basis splines. Standard errors are clustered by household. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A6: Well-Measured Consumption and Transfer Receipt

	SNAP (1)	Medicaid (2)	Housing Assistance (3)	TANF (4)	SSI (5)	School Meals (6)	WIC (7)	LIHEAP (8)
Panel A: Rent and Owner's Equivalent Rent (1997–2019)								
Receives Transfer	-0.431***	-0.333***	-0.670***	-0.615***	-0.137***	-0.374***	-0.370***	-0.320***
	(0.014)	(0.011)	(0.021)	(0.046)	(0.018)	(0.011)	(0.016)	(0.021)
Panel B: Vehicle Lease Cost and Equivalent Lease Cost (1999–2019)								
Receives Transfer	-0.216***	-0.242***	-0.113***	-0.372***	-0.026*	-0.296***	-0.291***	-0.167***
	(0.010)	(0.007)	(0.012)	(0.023)	(0.014)	(0.008)	(0.010)	(0.014)
Panel C: Food at Home Expenditure (1999–2019)								
Receives Transfer	-0.531***	-0.245***	-0.243***	-0.507***	-0.153***	-0.160***	-0.261***	-0.328***
	(0.014)	(0.011)	(0.017)	(0.040)	(0.018)	(0.011)	(0.016)	(0.021)
Panel D: Utility Expenditure (1999–2019)								
Receives Transfer	-0.045***	-0.095***	-0.266***	-0.215***	0.016	-0.113***	-0.124***	0.027
	(0.014)	(0.010)	(0.021)	(0.036)	(0.019)	(0.011)	(0.014)	(0.018)
Panel E: Gasoline Expenditure (1999–2019)								
Receives Transfer	-0.155***	-0.127***	-0.129***	-0.278***	-0.115***	-0.130***	-0.143***	-0.179***
	(0.016)	(0.013)	(0.022)	(0.045)	(0.023)	(0.013)	(0.017)	(0.024)

Notes: This table reports estimates of the predictive effect of transfer receipt on (log) levels of reported consumption, conditional on current-income rank. Each panel row is for a different consumption outcome, and each column is for a different transfer. The year ranges in parentheses indicate data coverage for the outcome of interest. All specifications control flexibly for current-income rank using cubic basis splines. Standard errors are clustered by household. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A7: Durable-Goods Ownership and Transfer Receipt

	SNAP (1)	Medicaid (2)	Housing Assistance (3)	TANF (4)	SSI (5)	School Meals (6)	WIC (7)	LIHEAP (8)
Panel A: HH Owns Primary Residence (1997–2019)								
Receives Transfer	-0.161***	-0.070***	-0.422***	-0.138***	-0.074***	0.001	0.032***	-0.064***
	(0.008)	(0.007)	(0.006)	(0.015)	(0.011)	(0.007)	(0.009)	(0.012)
Panel B: Number of Rooms in Home (1997–2019)								
Receives Transfer	-0.522***	-0.554***	-0.668***	-0.603***	-0.080*	-0.763***	-0.667***	0.000
	(0.032)	(0.029)	(0.034)	(0.072)	(0.048)	(0.031)	(0.042)	(0.046)
Panel C: Central Air Conditioning at Home (1997–2009)								
Receives Transfer	-0.044***	-0.020**	-0.002	-0.144***	0.025	-0.014	-0.033**	-0.068***
	(0.012)	(0.010)	(0.015)	(0.025)	(0.020)	(0.010)	(0.014)	(0.018)
Panel D: HH Owns a Car (1999–2019)								
Receives Transfer	-0.121***	-0.038***	-0.224***	-0.113***	-0.091***	0.035***	0.050***	-0.038***
	(0.008)	(0.006)	(0.011)	(0.022)	(0.011)	(0.006)	(0.009)	(0.012)
Panel D: HH Owns a Computer (2003–2019)								
Receives Transfer	-0.115***	-0.042***	-0.130***	-0.082***	-0.033***	-0.011	-0.056***	-0.083***
	(0.009)	(0.007)	(0.013)	(0.027)	(0.012)	(0.008)	(0.011)	(0.014)
Panel E: HH Owns a Smartphone (2015–2019)								
Receives Transfer	-0.004	0.006	-0.052***	0.056	0.007	0.043***	0.056***	-0.040*
	(0.012)	(0.009)	(0.017)	(0.035)	(0.012)	(0.008)	(0.011)	(0.021)

Notes: This table reports estimates of the predictive effect of transfer receipt on measures of household durable-goods ownership, conditional on current-income rank. Each panel row is for a different consumption outcome, and each column is for a different transfer. The year ranges in parentheses indicate data coverage for the outcome of interest. All specifications control flexibly for current-income rank using cubic basis splines. Standard errors are clustered by household. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A8: Simulated Eligibility and Receipt Rates

Program	P(Receive | Simulated Eligible) (1)	P(Receive | Not Sim. Elig.) (2)	P(Sim. Elig. | Receive) (3)	P(Sim. Elig. | Not Receive) (4)
Housing Assistance	0.11	0.02	0.80	0.35
LIHEAP	0.14	0.01	0.80	0.20
Medicaid	0.54	0.09	0.49	0.07
SNAP	0.34	0.03	0.80	0.17
SSI	0.30	0.05	0.12	0.02
TANF	0.07	0.00	0.77	0.10
School Meals	0.46	0.04	0.64	0.09
WIC	0.46	0.02	0.50	0.03

Notes: This table presents the share of households who do or do not receive transfers, conditional on our simulated eligibility measures. See Appendix B for details on forming the measures of simulated eligibility.

B Data Appendix

This appendix first explains measurement details for consumption, lifetime income, and simulated eligibility. Next, it presents the extensions we mention in Section 3. These are the decomposition of differences in incidence into mobility, eligibility, and take-up effects, as well as the inverse approach of finding the income-tax reform that would replicate the consumption or lifetime incidence of transfers.

B.1 Consumption Ranks

We compute households' equivalized consumption ranks using the expenditure data available in the PSID in a given year. Not all consumption categories are available in each year. In particular, we observe expenditures on clothing, furniture, travel, and recreation starting in 2005 and computer expenditures starting in 2017. We observe housing rents (actual and imputed) starting in 1997, and all other expenditures starting in 1999. These expenditure categories are childcare, education, food, health, transportation, and utilities (energy and water starting in 1999, phone/cable/internet starting in 2005).

As noted in Section 2, we follow Meyer and Sullivan (2023) in making two adjustments so that we more closely measure consumption rather than expenditure. Broadly, these adjustments estimate consumption flows from households' two key durable goods, homes and vehicles.

For renters, we take their paid rents as their housing consumption. For owner-occupiers, we obtain imputed rents in several steps. In 2017 and 2019, owner-occupier households were asked "If someone were to rent this (apartment/mobile home/home) today, how much do you think it would rent for per month, unfurnished and without utilities?" We take these values as housing consumption for such households. For all years in our sample period, households who report that their housing is free are asked "How much would it rent for if it were rented?", which we use as their housing consumption. Finally, we construct a mapping from home values to owners' equivalent rents using the cross-sectional relationship in 2017 and 2019 between households' estimates of their home's value and its equivalent rent.[31]

Transportation consumption is constructed as follows. We count any expenditures on gasoline, parking, public transportation, taxis, other transportation toward the household's transportation consumption. Due to PSID data limitations, we also count as consumption any expenditures on vehicles other than the household's three reported primary vehicles. For households that lease any of their three primary vehicles, we count their lease costs toward transportation consumption. For

[31] For households that do not report an exact home value, we use the midpoint of the elicited range. For households who say their homes are worth more than $400,000, but do not report an exact value, we impute it as the sample mean conditional on exceeding $400,000 among households who report exact home values.

households that own any of their three primary vehicles, we impute the equivalent lease cost from a hedonic regression.

To estimate this hedonic regression, we restructure our data into a vehicle-level dataset. Households that lease or own a vehicle report the vehicle's manufacturer (e.g., Toyota), its make (e.g., Lexus), its age at acquisition (year of purchase or lease minus model year), and its "type" (car, pickup/truck, van, utility, or motor home). We estimate Poisson regression models of all two-way interactions of these variables, along with indicator variables for calendar year and the rank (1/2/3) of the vehicle in the household's list, The outcomes are purchase price or lease cost, winsorizing values at the first and 99th percentiles. We then collapse these predicted values for purchase price and lease cost to the level of manufacturer, make, age, and type. This procedure yields an estimated lease cost equivalent for owned vehicles.[32]

B.2 Lifetime Income Ranks

Step 1: Estimate lifecycle regression parameters. Letting i index individuals, t index calendar years, and a index age in years, we estimate Poisson regression models of the following form:

$$\mathrm{E}[y_{it} \mid X_{it}] = \exp(\alpha_i \lambda_a + X_{it}' \beta_a), \tag{13}$$

where α_i is an individual fixed effect, α_t is a calendar-year fixed effect, X_{it} is a matrix of time-varying demographic characteristics, and λ_a and β_a are vectors of age-specific coefficients. The outcome y_{it} is individual income. For individuals with zero income in all observed years, we impute a constant annual income of \$100.[33] The age-specific coefficients are initialized to $\lambda_a = 1$ for all a but will be estimated in an outer loop discussed below. We make several adjustments before using the regression results to estimate lifetime-income ranks.

Step 2: Shrink fixed effects. First, we apply the empirical Bayes methods in Morris (1983) to shrink the estimated individual fixed effects $\widehat{\alpha_i}$ toward a conditional expectation fit from several time-invariant individual characteristics.[34] These methods accommodate both unequal individual means and unequal sampling variances in the fixed effects by iteratively re-estimating the extent of true heterogeneity among individuals and the conditional expectation function using weighted least

[32]For missing values, we impute using the cross-sectional relationship between fitted purchase prices and fitted lease values from these Poisson regressions.

[33]In an unadjusted Poisson regression, estimates of the individual fixed effects α_i diverge to negative infinity for any individual i who earns $y_{it} = 0$ for all observed periods t. By setting their y_{it} to a very low positive value, we obtain convergent fixed effects and rank these individuals at the bottom of the lifetime-income distribution. Importantly, this procedure does affect our estimates of β, as the fixed effects α_i perfectly explain the income of these individuals.

[34]Recent applications of these methods in economics include Chandra et al. (2016) and Sorkin (2018). We refer interested readers to their appendices for detailed expositions. One key modification we make to their approach is to use a within-individual Bayesian bootstrap (Rubin, 1981) instead of actual resampling.

squares. Our baseline specification uses sex, race, and ethnicity to fit this conditional expectation.

Step 3: Outer loop. Haider and Solon (2006) emphasize that the "error-in-variables" model of lifetime income is misspecified, as the predictive effect of individual fixed effects grows over the lifecycle. To account for this, we estimate the λ_a terms in Equation 1 through the following outer loop. Consider the first loop, in which we have initialized $\lambda_a = 1$ and have shrunken estimates of α_i. We can estimate the following Poisson model:

$$\mathrm{E}[y_{it} \mid X_{it}] = \exp(\widehat{\alpha}_i \lambda_a + X'_{it}\beta_a), \tag{14}$$

importantly treating $\{\widehat{\alpha}_i\}$ as data rather than as parameters. We then obtain coefficient estimates $\{\widehat{\lambda}_a\}$, and with these in hand, we return to step 1 and iterate until convergence of $\{\widehat{\alpha}_i, \widehat{\lambda}_a, \widehat{\beta}_a\}$. In practice, we find that convergence is fast; three runs of the outer loop are sufficient.

Step 4: Balance the panel. Having estimated the model in Equation 1, we use it to predict income from ages 18 to 65, irrespective of the years in which we observe an individual's actual income. An individual's predicted income in year t is $\widehat{y}_{it} = \exp(\widehat{\alpha}_i^* \widehat{\lambda}_a + X'_{it}\widehat{\beta}_a)$, where $\widehat{\alpha}_i^*$ are the shrunken estimates of the individual fixed effects. Lifetime income are then

$$\overline{y}_i = \sum_t \widehat{y}_{it}, \tag{15}$$

where the summation over t is for the years $\{\underline{T}_i, \ldots, \overline{T}_i\}$ in which individual i is between the ages of 18 and 65. Importantly, however, we do not observe individual characteristics X_{it} in all years and therefore must impute them. In our baseline specification, we assume these characteristics are unchanged from the nearest period of observation, except for age.

Step 5: Construct ranks. We define an individual's lifetime income percentile rank as $\Pr(y \leq \overline{y}_i \mid c_i = c)$, where $\overline{y}_i$ is their estimated lifetime income and c_i is their birth-year cohort. We define an individual's current income percentile rank as $\Pr(y \leq y_{it} \mid c_i = c)$, again ranking individuals each year within their birth cohorts. Appendix A presents figures of our main results when we do not rank current income within cohorts.

We construct current and lifetime household income percentile ranks as follows. Let $j(i,t)$ indicate i's spouse in year t, and let $h(i,t)$ indicate the household of which i is a part at t. As explained above, current household income is the sum of the head's and spouse's individual current income: $y^h_{i,t} = y_{it} + y_{j(i,t),t}$. Our lifetime concept of household income follows each individual through the sequence of households during their adult life, again using individuals' income fitted from Equation 1 and the subsequent adjustments. That is, the lifetime household income of individual i is

$$\bar{y}_i^h = \sum_t e(\hat{y}_{it}^h) = \sum_t e(\hat{y}_{it} + \hat{y}_{j(i,t),t}) \tag{16}$$

where t is again summed over the years in which i is between ages 18 and 65. The function $e(\cdot)$ equivalizes household income for differences in household size in each year. If we were to restrict our sample to stable households over time (as in, e.g., Fullerton and Lim Rogers, 1993), our definition of household income would coincide exactly with the natural concept. However, it accommodates unstable households in a way that is meaningful as a measure of living standards.

B.3 Simulated Eligibility

Supplemental Nutrition Assistance Program (SNAP). SNAP eligibility is determined on the basis of three tests: (1) a gross-income test, (2) a net-income test, and (3) an asset test. Recipients of TANF and SSI are always categorically SNAP-eligible.

We use state-level gross-income tests from 1996 to 2016 from SNAP Policy Database, maintained by the Economic Research Service of the U.S. Department of Agriculture.[35] We assume these thresholds are unchanged from 2016 through 2019. Until 2000, all U.S. states had a SNAP gross-income test at 130 percent of the Federal Poverty Level (FPL). Under "broad-based categorical eligibility" (BBCE), states raised gross-income limits.

The net-income test requires that income net of specific deductions is less than 100 percent of the FPL. Starting from gross income, all households take a standard deduction as a function of their household size; they also deduct 20 percent of household earnings from gross income. There are four further deductions that may be applied to gross income. We focus on the most important, the "excess shelter deduction." This deduction subtracts housing costs, inclusive of utilities, that exceed half of net income after accounting for all other deductions. The excess-shelter deduction is capped at a level that depends on household size. Standard deductions and excess-shelter deduction caps vary by year but are different for Alaska and Hawaii; we collected these policy parameters from Federal Register notices. The three other deductions—for child support, medical expenses, and dependent care—appear rarely used in eligibility determinations, and we ignore them.[36]

We use asset-test thresholds from the SNAP Policy Database. We apply the asset test rules to household liquid savings, due to the exemption of most relevant other categories of wealth. The asset limit for nonelderly households was $2,000 from the 1980s until 2014, when it was raised to $2,250. The asset test is eliminated under BBCE.

There are special eligibility rules covering households with elderly or disabled adults. In particular, these households are only subject to the net-income test (no gross-income test). They

[35]See https://www.ers.usda.gov/data-products/snap-policy-data-sets/.

[36]For further details, see Center on Budget and Policy Priorities, "A Quick Guide to SNAP Eligibility and Benefits."

also face higher asset-test threshold of $4,250, unless the threshold has been raised under BBCE. We assume the asset-test threshold for such households is the maximum of $4,250 and their BBCE asset-test threshold for all other households.

Medicaid. Medicaid eligibility is determined by income and asset tests that vary by state and with household characteristics. In most states, SSI recipients are categorically Medicaid-eligible; we apply this to states which, under the "209(b)" rules, in principle have some Medicaid eligibility rules that are more stringent than for SSI.

Income eligibility thresholds come primarily from the Kaiser Family Foundation (KFF), with our supplementation to fill gaps in the data. We imputed that thresholds did not change when there are data gaps but we know thresholds on both ends of the gap were the same. Different income tests apply to non-disabled adults, parents, and pregnant women. Income eligibility is most complicated for disabled adults, who may become eligible under a number of pathways, including Medicaid buy-in and being "medically needy." We determine whether a household qualifies as medically needy using reported health expenditures.

We hand-collected Medicaid asset-test thresholds from state-agency websites and policy reports that will be included in our replication files. The thresholds vary for singles and couples, and for the Medicaid buy-in and medically-needy pathways. When we were unable to find state asset-test thresholds in a given year, we imputed it from surrounding years or used the federal thresholds.

Housing Assistance. Eligibility for housing assistance (Section 8 and public housing) is determined by income. Income is measured relative to Area Median Income (AMI) at the level of metropolitan area or non-metropolitan county. As we do not have sub-state geographic identifiers, we use state-level AMIs by household size. Public housing authorities may set their income thresholds between 50 percent and 80 percent of AMI. We assume an eligibility threshold of 50 percent of AMI, as large-city public housing authorities typically impose this threshold at voucher take-up or occupancy of the public-housing unit.

There is no asset test for housing assistance. Until 2014, however, households with no actual asset income but significant wealth could be excluded from housing assistance on the basis of imputed asset income. This imputation used a "passbook savings rate" of two percent until 2014. In 2014, HUD Notice H 2014-15 set this rate to almost zero, essentially eliminating the treatment of assets as income.

Supplemental Security Income (SSI). SSI eligibility is determined by disability of an adult or child member of the household, an income test, and an asset test.

Households are ineligible if their income exceeds a federal "substantial gainful activity" (SGA) threshold. This SGA threshold rose gradually from $500 per month in 1997 to $1,220 in 2019. We also label households ineligible if their countable income exceeds the Federal Benefit Rate (FBR), which implies they would not be eligible for a positive SSI benefit amount. Monthly countable income for SSI is defined by the following formula:

$$y_{\text{countable}} = \max\{0, y_{\text{earned}} + y_{\text{unearned}} - 0.5 \cdot \max\{0, y_{\text{earned}} - 65\} - 20\},$$

where y_{earned} and y_{unearned} are monthly earned and monthly unearned income respectively.

Single-adult households are ineligible for SSI if they possess more than $2,000 in countable assets. The asset threshold is $3,000 for couples. Countable assets are financial assets only after 2005 and financial assets plus the excess of vehicle wealth above $4,500 before 2005.

Women, Infants, and Children (WIC). WIC eligibility is determined by the presence of a child under age five in the household and an income test. The income test is that their income is no greater than 185 percent of the FPL. Households are also categorically WIC-eligible if they have such a child and receive SNAP, TANF, or Medicaid.

Low-Income Heating and Energy Assistance Program (LIHEAP). A household is LIHEAP-eligible if they pay utilities, satisfy an income test, and satisfy an asset test. We determine whether a household pays utilities based on reported utility expenditures.

States set their own income-test thresholds, and these differ by LIHEAP sub-program. Our eligibility simulation focuses on non-crisis heating assistance, the largest sub-program. For 1997–2007 and 2015–2019, we obtain these from the LIHEAP Clearinghouse website, using Internet Archive to obtain the first interval. We obtained the intermediate years from LIHEAP Reports to Congress.

Information was more limited on LIHEAP asset tests. From the Clearinghouse, Reports to Congress, and state-agency websites, we were able to determine whether states had asset tests for all years. The levels of the asset threshold, however, we have only beginning in 2015. We assume these thresholds were unchanged from 1997 to 2015 if the state always had an asset test. For states that had an asset test but eliminated it before 2015, we impute a limit of $5,000. We assume the assets covered by the test are liquid savings, although definitions appear to vary somewhat by state.

States may also make SNAP, SSI, and TANF recipients categorically eligible for LIHEAP. We obtained states' categorical-eligibility rules for fiscal year 2019 from the "Detailed Model Plan" submissions included in their SF-424 grant applications for federal LIHEAP funds. We assume that categorical-eligibility rules are unchanged over the entire period.

School Meals. A household is eligible for the National School Lunch Program and the School Breakfast Program if they have a school-age child (ages 5 to 18) and have an income less than 185 percent of the FPL. We use the threshold to qualify for reduced-price meals. The threshold is 150 percent of the FPL for free meals. Households can also be categorically eligible if they receive

SNAP, TANF, or other means-tested transfers.[37]

The Healthy Hunger-Free Kids Act of 2010 established the Community Eligibility Provision (CEP), which offers free school meals universally in high-poverty areas. We do not account for school-meals eligibility via the CEP, as we lack sub-state geographic identifiers.

Temporary Aid for Needy Families (TANF). We heavily rely on data and eligibility simulations from the Urban Institute's TRIM3 model. We use the following variables from TRIM3: state-year-household size gross income eligibility thresholds; state-year "standard of need" data, which scale the income eligibility thresholds; state-year "adjustment variables," which adjust the standard of need; state-year TANF asset thresholds; state-year earnings disregards (fixed levels and shares of income). Some states use a net asset test; in those cases, we impute their gross threshold as the median non-missing gross standard of need.

A household's income is below the TANF eligibility threshold if their income, less the disregard, is less than the standard of need times the eligibility threshold. A household is eligible for TANF if they have a child, are below the eligibility threshold, and are below the asset threshold.

This eligibility simulation neglects the following forces. First, we do not incorporate the TANF net income thresholds. Second, we assume that the vehicle asset test deducts the full value of the vehicle, which occurs in 39 states of 50 states. Third, some states do not require children in the household to be eligible.

B.4 Data Sources on Budgetary Cost

- *SNAP*: Laura Tiehen, "The Food Assistance Landscape: Fiscal Year 2019 Annual Report," Economic Research Service, U.S. Department of Agriculture, July 2020.
- *Medicaid*: U.S. Centers for Medicare & Medicaid Services, "CMS Office of the Actuary Releases 2019 National Health Expenditures," 16 December 2020.
- *Housing Assistance*: Donna Kimura, "Fiscal 2019 HUD Budget Approved," *Affordable Housing Finance*, 20 February 2019.
- *SSI*: Office of Research, Evaluation, and Statistics and Office of Retirement and Disability Policy, Social Security Administration, "SSI Annual Statistical Report, 2019," SSA Publication No. 13-11827, August 2020.
- *TANF*: Office of Family Assistance, Administration for Children & Families, U.S. Department of Health and Human Services, "TANF and MOE Spending and Transfers by Activity, FY 2019," 22 October 2020.

[37] See Rebecca R. Skinner and Randy Alison Aussenberg, "Overview of ESEA Title I-A and the School Meals' Community Eligibility Provision," Congressional Research Service Report R44568, 2016.

- *WIC*: Food and Nutrition Service, U.S. Department of Agriculture, "WIC Program Participation and Costs," 10 February 2023.
- *LIHEAP*: Office of Community Services, Administration for Children & Families, U.S. Department of Health and Human Services, "LIHEAP DCL Funding Release FY 2019," 26 October 2018.
- *School Meals*: Economic Research Service, U.S. Department of Agriculture, "National School Lunch Program" and "School Breakfast Program," 3 August 2020.

B.5 A Decomposition of Distributional Incidence

We have argued informally through regressions that take-up, more so than eligibility, explains why transfer receipt identifies those with low consumption given income. Here we extend the decomposition approach of Brewer et al. (2020) to carefully distinguish between selection via eligibility rules and selective take-up among the eligible.

Mobility Effect. The mobility effect is the component of the difference between incidence with respect to consumption and incidence with respect to income that results from year-to-year income mobility. For instance, because college graduates have significant debt early in their careers but later greatly out-earn non-graduates, the lifetime incidence of student loan forgiveness is likely to be less progressive than its incidence with respect to current income.

To measure the mobility effect, we estimate the share $q(r)$ of people with consumption rank $\overline{R}_i = r$ who would have received a given transfer in a given year if, counterfactually, transfer receipt were only a function of current-income rank. That is, our mobility-only counterfactual is

$$q(r) = \int \Pr(D_{it} = 1 \mid R_{it})\, dF(R_{it} \mid \overline{R}_i = r), \tag{17}$$

where D_{it} indicates transfer receipt and $F(R_{it} \mid \overline{R}_i = r)$ is the conditional distribution of current-income ranks R_{it} at consumption rank r. The mobility effect at rank r is the difference between $q(r)$ and the empirical receipt rate at a current-income rank r, $p(r) = \Pr(D_{it} = 1 \mid R_{it} = 1)$. Intuitively, the mobility effect applies the consumption–income "transition matrix" to the receipt rate by income rank, yielding the counterfactual receipt rates at each consumption rank.

Eligibility Effect. The eligibility effect is the component of the difference in incidence due to eligibility rules that, among low-income households, tag those with low consumption and low lifetime income. For instance, asset tests and categorical eligibility for single parents, people with disabilities, and similar groups tend to target benefits to the persistently poor, not people with merely low current income. By contrast, eligibility rules for contributory social insurance programs such as unemployment insurance operate in the opposite direction, restricting transfers to the persistently poor.

To measure the eligibility effect, we define the receipt rate at consumption rank r under a second counterfactual, $s(r)$. Here the probability of take-up conditional on eligibility is a function of current income alone, whereas we let eligibility depend on both current income and consumption (or current income and lifetime income). This counterfactual receipt rate $s(r)$ thus depends only on eligibility and current-income rank. The difference between these two counterfactuals, $q(r)$ and $s(r)$, is the eligibility effect.

When eligibility can be measured perfectly—that is, $E_i = 0$ implies $D_i = 0$—our second counterfactual is defined as

$$s(r) = \int \Pr(D_{it} = 1 \mid R_{it},\, E_{it} = 1) \Pr(E_{it} = 1 \mid R_{it},\, \overline{R}_i = r)\, dF(R_{it} \mid \overline{R}_i = r),$$

where E_{it} indicates eligibility. This expression emerges from the following reasoning. The probability $\Pr(D_{it} = 1 \mid R_{it},\, E_{it} = 1)$ is the transfer take-up rate among the eligible at current-income rank R_{it}. The probability $\Pr(E_{it} = 1 \mid R_{it},\, \overline{R}_i = r)$ is the eligibility rate at current-income rank R_{it} and consumption rank r. Multiplying these two probabilities yields a predicted receipt rate for people with current-income rank R_{it} that embeds the desired independence assumption: Given current income, take-up among the eligible is uninformative about consumption or lifetime income.[38] Integrating over the conditional distribution of current-income ranks given consumption, $F(R_{it} \mid \overline{R}_i = r)$, we have a counterfactual receipt rate $s(r)$ at consumption rank r that permits a role for eligibility rules while shutting down selective take-up.

Imperfect measurement of eligibility requires a more complex expression for the counterfactual. We now take the counterfactual to be:

$$\begin{aligned} s(r) = \int \Big[& \Pr(D_{it} = 1 \mid R_{it},\, E_{it} = 0) \Pr(E_{it} = 0 \mid R_{it},\, \overline{R}_i = r) \\ & + \Pr(D_{it} = 1 \mid R_{it},\, E_{it} = 1) \Pr(E_{it} = 1 \mid R_{it},\, \overline{R}_i = r) \Big]\, dF(R_{it} \mid \overline{R}_i = r), \end{aligned}$$

where all terms are as defined above. We use this counterfactual in our analysis. As a robustness check, we also use the earlier counterfactual that requires perfect measurement of eligibility and reclassify simulated-ineligible recipients as eligible.

Take-Up Effect. The take-up effect is the residual component after accounting for mobility and

[38] To better understand this counterfactual, note that the law of conditional probability implies that: $\Pr(D_{it} = 1 \mid \overline{R}_i = r) = \int \Pr(D_{it} = 1 \mid R_{it},\, \overline{R}_i = r)\, dF(R_{it} \mid \overline{R}_i = r)$. By consequence, the counterfactual receipt rate $s(r)$ equals the empirical receipt rate at a given consumption rank if and only if take-up among eligibles is conditionally independent of consumption given current income at all consumption ranks.

eligibility. Intuitively, there is some take-up effect when consumption and lifetime income matter for take-up rates among the eligible. For instance, procedural complexity (Deshpande and Li, 2019; Gray, 2019), "ordeal" costs (Nichols and Zeckhauser, 1982), and information frictions (Finkelstein and Notowidigdo, 2019) could select for or against low lifetime income and consumption among eligible households with similar current incomes. We define the take-up effect as the residual difference between the counterfactual receipt rate $s(r)$ and the empirical receipt rate at the same consumption rank, $\overline{p}(r) = \Pr(D_{it} = 1 \mid \overline{R}_i = 1)$.

B.6 Results of Decomposition

This decomposition enables us to interpret differences between the consumption incidence and the income incidence of transfer programs. For instance, why do a greater or smaller share of people at consumption rank r receive a transfer than do people at current-income rank r? Our decomposition is

$$\overline{p}(r) - p(r) = \underbrace{[\overline{p}(r) - s(r)]}_{\text{take-up effect}} + \underbrace{[s(r) - q(r)]}_{\text{eligibility effect}} + \underbrace{[q(r) - p(r)]}_{\text{mobility effect}}, \tag{18}$$

using the objects defined above. Appendix Figure A13 displays decomposition results for the difference between current-income and consumption incidence. Panel A displays results for the average total annual per-capita value of benefits, and Panel B displays results for receipt rates by program. The blue, black, and yellow lines plot the mobility, eligibility, and take-up effects, respectively; each represents the difference in consumption incidence with respect to current-income incidence if only this effect were present. The blue shaded region indicates the net effect, equal to the difference between the consumption and current-income incidence at the indicated rank; this region integrates to zero.

Absent eligibility and take-up effects, the incidence of transfer programs would be markedly less progressive at the bottom of the distribution. Consistent with our prior results, we find a central role for selective take-up among eligible households, and a modest role for eligibility rules, in shaping the consumption incidence of transfer programs.

In total over transfer programs, selective take-up increases the average annual value of benefits received by the consumption-poorest people by about \$750 per person relative to a counterfactual in which transfer receipt is a function of current income and eligibility alone. Eligibility rules contribute about \$250 per person at the bottom of the consumption distribution. Together these amounts represent about one quarter of the average transfer per capita per year to consumption-poorest people. Mobility tends to shift the incidence of transfers up the consumption distribution, but bograms, the eligibility and take-up effects largely offset the mobility effect.

The primacy of take-up applies broadly across the eight transfer programs we study. Eligibility rules appear most important in Medicaid, TANF, and WIC, whereas they appear essentially irrelevant or operating in the opposite direction for SNAP, LIHEAP, and housing assistance. While mobility alone would reduce the receipt rate among the consumption poor by about 10 percentage points for SNAP, Medicaid, and housing assistance, selective take-up increases receipt rates in these programs at the bottom of the consumption distribution by 10 to 20 percentage points. These are economically large differences relative to underlying rates of transfer receipt as well as relative to the effects of mobility.

B.7 Measurement Error: Simulation

Method. How much measurement error is required to overturn our results? We conduct an adversarial simulation to probe the robustness of Figure 1 to extreme amounts of measurement error. We consider the coefficient γ in Equation 6, which represents the marginal effect of take up of a given transfer program on lifetime rank, controlling for current rank.[39] We simulate measurement error as follows:

1. Obtain the take-up rate among the bottom 50% of households ranked in consumption terms, $\hat{D} \in [0, 1]$.

2. Assign the top x% in consumption ranks to have some constant $c \in [0, 1]$ times the take-up rate of the bottom 50%: $c\hat{D}$.

3. Estimate Equation 6 using the simulated data.

This exercise generates a large amount of measurement error at the top of the distribution. The take-up rate in the bottom 50% of the current consumption distribution is a natural bound on the take-up rate of the top x% of the consumption distribution, unless the programs' targeting properties are very perverse. The parameter c governs whether the measurement error is as severe as possible ($c = 1$).

Results. Measurement error would need to be very severe to overturn our results. Figure A11 shows the estimated coefficient $\hat{\beta}$ as a function of the share of the top of the consumption distribution that has severe measurement error. In black, we present the estimates if the true take-up rate is half the bottom 50%'s take-up rate. The blue lines show the estimates if the true take-up rate is the same as the bottom 50%'s take-up rate. When $x = 0$, the estimates coincide with Figure 1. As long as true take-up at the top is half the poorest's take-up rate, we continue to reject $\beta = 0$. If true take-up

[39] We do not condition on simulated eligibility in these specifications, to isolate the magnitude of take-up measurement error without controlling for a potentially contaminated confound.

is equal, then we can no longer reject $\beta = 0$ for $x \geq 15$ or so. These results are logical: if we impute take-up rates that are the same as at the bottom of the distribution for much of the top of the distribution, we no longer find evidence of selection. But as long as measurement error does not exceed half the take-up rate, we decisively reject the null.

C Theory Appendix

C.1 Valuing Transfers Differently from Cash

In the main paper, we assumed that a dollar of automatic transfer was worth a dollar of cash, and as result modelled changes in the automatic transfer as occurring through the tax system. Here, we explicitly distinguish between an automatic and a voluntary transfer program, and derive an analogue of Proposition 1.

Suppose the planner is now setting a tax schedule $T(z)$, a voluntary transfer $S_V(z)$ and an automatic transfer $S_A(z)$. The voluntary transfer is what we labelled as $S(z)$ in the main paper: households must pay a cost κ to enroll. Taxes and the automatic transfer are received automatically by households at income z (without a cost being paid), except a dollar of the latter is not valued equally to the a dollar of the former. Write λ for the marginal utility for a dollar of S_A or S_V relative to a dollar of cash.

The household's program is then to:

$$\max_z \left\{ z - T(z) - v(z/w) + \lambda S_A(z) + \int_0^{S_V(z)} \lambda(S_V(z) - \kappa)\mu(w \mid \kappa)d\kappa \right\}. \tag{19}$$

Suppressing the dependence on the wage w for clarity, the household's optimal choice $z^* = z^*(w)$ leads to ex-post consumption is $c^* = z^* - T(z^*) + \lambda S_A(z*) + \mathbb{1}_S \lambda(S(z^*) - \kappa)$. The planner maximizes the weighted sum of ex-post households utilities, which are given by

$$V(\theta) = z^* - T(z^*) - v(z^*/w) + \lambda S_A(z*) + \mathbb{1}_S \lambda(S(z^*) - \kappa)\,. \tag{20}$$

Government. The government chooses tax and transfer schedules $T(\cdot)$ and $S(\cdot)$ to maximize utility summed across households according to type-specific Pareto weights $(\alpha(\theta))$:

$$\max_{T,S} \int_\Theta \alpha(\theta)V(\theta)d\mu(\theta),$$

subject to a balanced-budget constraint:

$$\int_\Theta [T(z(\theta)) - S_A(z(\theta)) - \mathbb{1}_S S_V(z(\theta))]\, d\mu(\theta) = 0 \tag{21}$$

and to household optimization. Note that the budget constraint is denominated in dollars, even though a dollar of S_A or S_V is only valued at λ dollars by the household.

Reform. We define the reform analogously to the primary reform in Section 4. The voluntary transfer amount is cut by ds_V at all incomes. With the savings, at each income z, automatic transfers are increased by $s_A(z) = M(z)ds_V$, so that people at each income level are compensated on average for the voluntary transfer cut. The slope of S_A rates (analogous to marginal tax rates) thus change by $S'_A(z) = \frac{d}{dz}M(z)ds_V$ at z. Fiscal savings from marginal transfer recipients are redistributed as a lump sum automatic transfer: $E_z\left[(S_V(z) + ds_V)m(z)\right]$. The revenue cost of any labor supply response is then paid for via lump-sum taxes (i.e. as cash, not in-kind).

We calculate the welfare effects of this reform, analogously to Proposition 1.

Proposition 2. *The welfare effect of the reform is*

$$\begin{aligned}\frac{1}{\lambda}\frac{dW}{ds} = & \underbrace{\beta\sigma_M^2}_{\text{lost value of self-targeting}} + \underbrace{\bar{M}\bar{\varepsilon}_b}_{\text{fiscal savings from marginals}} \\ & + \underbrace{\int_z \frac{M'(z)z\varepsilon_\tau(z)}{1-T'(z)}\left(\frac{d}{dz}(S_V(z)M(z)) + S'_A(z) - T'(z)\right)dH(z)}_{\text{labor-supply effect (i)}} \\ & + \underbrace{\int_z \frac{M'(z)z\varepsilon_\tau u(z)}{1-T'(z)} S'_V(z)\beta(z)M(z)(1-M(z))dH(z)}_{\text{labor-supply effect (ii)}},\end{aligned} \tag{22}$$

where all other terms are as in Proposition 1.

While Proposition 1 moves money from from S to T, Proposition 2 moves money from S_V to S_A. In both cases, the dollar being moved has the same ex-post marginal utility (1 in S and T, λ in S_V and S_A). Moving a dollar from the those who take up the voluntary transfer to everyone decreases marginal utility by λ times the lost value of self-targeting from Proposition 1. Similarly, all the dollars saved from the marginals not taking up S_V are redistributed in-kind through S_A and so the utility value is λ times the fiscal savings from marginals term in Proposition 1.

As for the labor-supply effects, the first term is the change in the government budget due to labor supply changes that is then redistributed as a cash transfer to everyone. But unlike Proposition 1, where marginal tax rates changed, here the slope of the S_A schedule changes, and so the intrinsically relevant elasticity is ε_{S_A} not ε_z. However, per the consumers problem, $\frac{dz}{ds_A} = \lambda\frac{dz}{d\tau}$. Similarly the final term, which is the utility wedge between the consumer's choice of z (they maximize utility ex-ante of κ) and the planner's choice (they maximize utility ex-post of κ), is denominated in utility units and depends again on $\frac{dz}{ds_A}$. Again using $\frac{dz}{ds_A} = \lambda\frac{dz}{d\tau}$ and dividing by λ denominates this in in-kind dollars, as with the preceding three terms.

The intuition is that the costs and benefits of this reform are all scaled by λ relative to the reform in Proposition 1. Utility benefits/costs are λ lower since they are paid in in-kind dollars rather than cash dollars, and impacts on the budget constraint, although intrinsically denominated in cash, are convertible to in-kind dollars since labor supply response to a dollar change in S_A is λ times the labor supply response to a dollar change in T.

C.2 Alternative Reforms

In this section we consider other reforms and analyze their welfare effects.

Alternative "UBI" Reform. We marginally reduce the voluntary transfer schedule by ds at all incomes. This finances an expansion of a universal basic income, i.e. a lump sum tax decrease of $\tau = \int_z [dsM(z) + S(z)m(z)]\, h(z)dz$. The decrease offsets the static cost of the decreased benefit to the inframarginals and fiscal cost from marginal recipients. Because this reform imposes lump-sum changes to both the transfer and income tax, it has no efficiency effects via labor-supply:

$$\begin{aligned}\frac{dW_{\text{UBI}}}{ds} = & \underbrace{\beta\sigma_M^2}_{\text{reduced redistribution within } z} \\ & + \underbrace{\int_z E_\kappa\left[\alpha(z,\kappa)\right]\left(M(z) - E_{z'}\left[M(z')\right]\right) h(z)dz}_{\text{reduced redistribution between } z} \\ & - \underbrace{E_z\left[(S(z))m(z)\right].}_{\text{fiscal saving from marginals}}\end{aligned} \tag{23}$$

The only new term in Equation 23 is the redistribution between z term. To speak to this, we define an alternative notion of advantageous selection.

Definition 2. *We say households are* advantageously selected between income *when the distribution of costs κ conditional on income z increases in z in the sense of first-order stochastic dominance.*

Between-income advantageous selection evokes Nichols and Zeckhauser's (1982) targeting argument for transfers. If take-up costs increase in z, then take-up rates are declining in income, and hence transfers will have positive redistributive effects between incomes. With this definition, we can sign the redistribution between term.

Proposition 3. *Assuming the transfer $S(z)$ is positive and weakly decreasing, the welfare effect of between-income redistribution is negative if there exists advantageous selection as well as between-income advantageous selection.*

This proposition shows that a voluntary transfer program has an advantage over a UBI. A dollar of voluntary transfer flows in expectation to income levels with lower application costs. Assuming advantageous selection between incomes, the voluntary transfer is taken-up relatively more often by lower incomes. The UBI reform therefore redistributes regressively: A dollar is taken disproportionately from lower incomes and given to the average household.

This suggests an efficiency motive for voluntary transfers. For any tax change that is redistributed as an automatic transfer, one could instead redistribute as a conditional transfer and the recipients would be strictly better targeted. The price of targeting precision is the real take-up cost that must be incurred to enroll. The following proposition shows that as long as some people have zero cost, voluntary transfers should exist in the planner's optimum.

Proposition 4. *Suppose there is a mass point of households with cost zero at each income z. If we have advantageous selection and between advantageous selection starting at $S(z) = 0$, then optimally there is a non-zero transfer schedule.*

This proposition echoes Nichols and Zeckhauser's (1982) rationale. So long as the first dollar of voluntary transfer has infinitesimally improved targeting properties compared to costs, then the planner should utilize this tool to relax screening constraints that frustrate redistribution.

Non-Marginal Reform. We now consider a non-marginal reform, that is, a reform that replaces the voluntary transfer wholesale. We remove the entire voluntary transfer program $S(z)$ and expand the automatic transfer program by $M(z)S(z)$ in its place.

We calculate the welfare impacts of this reforms below, as before assuming no income effects,

$$
\begin{aligned}
\Delta W_{\text{NonMarginal}} = & \underbrace{\int_z S(z)M(z)\left(E_\kappa\left[g(z,\kappa)\right] - E_{\kappa\leq S(z)}\left[g(z,\kappa)\right]\right)h(z)dz}_{\text{reduced redistribution within } z} \\
& + \underbrace{E_{z,\kappa\leq S(z)}\left[\kappa g(z,\kappa)\right]}_{\text{fiscal savings/ordeal costs}} \\
& + \mathscr{L},
\end{aligned} \tag{24}
$$

where $\mathscr{L}$ contains all labor-supply effects, which are additively separable. The perturbation approach does not admit a simple closed-form solution for $\mathscr{L}$ since changes in the slopes of the voluntary transfer and tax schedules are not infinitesimal.

If the voluntary transfer scheme is entirely replaced, the primary benefit is that ordeal costs paid by everyone in the program are removed. The main cost is that there is a redistribution from always-takers (i.e. those inframarginal to the old voluntary transfer schedule) to never-takers.

When the always-takers have higher social marginal welfare weights than never-takers — as our empirical analysis demonstrates is the case — this redistributive effect is bad for welfare. As noted in the text, the ordeal costs term requires estimating the *average* ordeal cost among inframarginal always-takers, not just the ordeal cost among compliers.

C.3 Formal Analysis of the Consumer's Problem

In this section, we provide details to set up the main proposition. To ensure that there is no bunching and that labor supply elasticities are well defined, we assume every consumer of type w chooses their pre-tax income z before their realization of κ is drawn. With the quasi-linearity assumptions, this means that labor supply will be chosen according to expected amount of social program dollars, $M(z)S(z)$ that the consumer expects to accrue.

Each household maximizes

$$\max_z z - T(z) + \int_0^{S(z)} (S(z) - \kappa)\mu(w \mid \kappa) d\kappa - v(z/w). \tag{25}$$

Hence the consumer's First Order Condition (FOC) is

$$0 = 1 - T'(z) + S'(z)M(z) - v'(z/w). \tag{26}$$

We use the notation $M(z)$ as take-up of the transfer, post income choice z is of primary interest. But for an household with type (w), there is no causal effect of z on $M(z)$ beyond $S(z)$ changing. The distriubition of that κ is fixed by w, and hence even if type w expands their labor supply choice, absent any $S(z)$ change, there will be no change to the probability of $\kappa \geq S(z)$.

Next, we calculate elasticities of labor supply with respect to tax and SNAP changes in terms of primitives. Following Jacquet and Lehmann (2014), we apply perturbations to the tax and transfer system about z_0 of the form $\hat{T} = T + \tau(z - z_0) - \nu$ and $\hat{S} = S + \varsigma(s - s_0) - \vartheta$. In each case, the marginal tax (transfer program) rate has increased by τ (ς) but the level has decreased by ν (ϑ).

Now the consumer's problem is

$$\max_z z - T(z) - \tau(z - z_o) - \nu + \int_0^{S(z)+\varsigma(z-z_0)-\vartheta} (S(z) + \varsigma(z - z_0) - \vartheta) - \kappa)\mu(w \mid \kappa) d\kappa - v(z/w). \tag{27}$$

The new FOC is

$$\mathcal{F} = 1 - T'(z) - \tau + \int_0^{S(z)+\varsigma(z-z_0)-\vartheta} (S'(z) + \varsigma)\mu(w \mid \kappa)d\kappa - v'(z/w) \tag{28}$$

$$= 1 - T'(z) - \tau + (S'(z) + \varsigma)Pr\,(z + \varsigma(z - z_0) - \vartheta \leq \kappa \mid w) - v'(z/w). \tag{29}$$

To use the implicit function theorem, we calculate the derivatives:

$$\mathcal{F}_z \mid_{\tau=\varsigma=0} = -T''(z) - \frac{v''(z/w_i)}{w_i^2} + S''(z)M(z) + S'(z)^2 m(z) \tag{30}$$

$$\mathcal{F}_\tau \mid_{\tau=\varsigma=\vartheta=0} = -1 \tag{31}$$

$$\mathcal{F}_\varsigma \mid_{\tau=\varsigma=\vartheta=0} = M(z), \tag{32}$$

noting that $z \longrightarrow z_0$ as $\tau, \varsigma, \vartheta \longrightarrow 0$. Hence by the implicit function theorem we have that

$$\frac{\partial z}{\partial \tau} = \frac{-1}{\mathcal{F}_z} \tag{33}$$

$$\frac{\partial z}{\partial \varsigma} = \frac{M(z)}{\mathcal{F}_z}, \tag{34}$$

where we have assumed $S(z)$ income effects are small: $\frac{\partial z}{\partial \vartheta} = 0$.

And the (compensated) elasticities needed are then defined as:

$$\epsilon^z = -\frac{\partial z}{\partial \tau} \mid_{\tau=0} \frac{1 - T'(z)}{z} \tag{35}$$

$$\epsilon^s = \frac{\partial z}{\partial \varsigma} \mid_{=0} \frac{S'(z)}{z}. \tag{36}$$

C.4 Signing Terms in Proposition 1

We now establish conditions under which there is a social benefit from self-targeting.

Proposition 5. *Assuming the transfer $S(z)$ is positive, the first term in Equation 12 is negative if there exists advantageous selection, implying lost value from self-targeting.*

We now establish conditions under which the policy reform we consider in the main text would raise or lower labor supply.

Proposition 6. *Suppose (1) the tax system is optimal and (2) take-up decreases in income (i.e., $M'(z) < 0$). Then the sum of the labor supply effects (i) and (ii) in Equation 12 is negative.*

C.5 Proofs

C.5.1 Proof of Proposition 1

Proof. As in Section C.3 the reform is composed of perturbations to the tax and transfer system about z_0 of the form $\hat{T} = T + \tau(z - z_0) - \nu$ and $\hat{S} = S + \varsigma(s - s_0) - \vartheta$. In particular, the reform is composed of a level shift in $S(z)$ of $\vartheta = ds$, a change in the marginal tax rate of $\tau = \frac{d}{dz}M(z)ds$,[40] and decreases in everyone's level of taxes $\nu = E_z\left[S(z)m(z)\right] + M(z)ds$ due to the reduced voluntary transfer expenditure. Finally, any changes in revenue due to changes in labor supply are redistributed lump sum. We analyse these in turn. Throughout, for convenience, we integrate over z instead of w, where z is the pre-reform income that is one to one with w, per the consumer's FOC in Section C.3.

Writing $V^* = V^*(z^*, w, \kappa)$ for the consumers pre-reform optimized utility function, since $W = \int_z \int_\kappa \alpha(w,\kappa)V^*(z^*, w, \kappa)d\mu$, it remains to calculate $\frac{\partial W}{\partial \tau} = \int_z \int_\kappa \alpha(w,\kappa)\frac{\partial}{\partial \tau}V^*(z^*, w)d\mu$ and similarly for ς and ϑ.

This total derivative is generally of the form

$$\frac{dV^*}{d\tau} = \frac{\partial V^*}{\partial \tau} + \frac{\partial V^*}{\partial z}\frac{\partial z}{\partial \tau},$$

where we use d to denote total derivatives and ∂ for partial derivatives in which all other channels are held fixed.

For ν we have

$$\frac{dV^*}{d\nu} = \frac{\partial V^*}{\partial \nu} + \frac{\partial V^*}{\partial z}\frac{\partial z}{\partial \nu} = \frac{\partial V^*}{\partial \nu}$$

since $\frac{\partial z}{\partial \nu} = 0$ by the assumption of no income effects. Hence we need only calculate the direct effect. Identically for $\frac{dV^*}{d\vartheta} = \frac{\partial V^*}{\partial \vartheta}$.

However, in the case of τ, we have $\frac{\partial V^*}{\partial \tau} = 0$: slope changes do not have a direct utility effect, yet $\frac{\partial V^*}{\partial z}\frac{\partial z}{\partial \tau} \neq 0$ as there are substitution effects, and the model does not admit an envelope theorem.

In particular, note that $\frac{\partial V^*}{\partial z} = 1 - T'(z) - v'(z/w) + \mathbb{1}(\kappa \leq S(z)) \cdot S'(z)$ whereas $\frac{\partial U^*}{\partial z} = 1 - T'(z) - v'(z/w) + M(z)S'(z)$. No envelope theorem applies since when the household averages over κ they do so without weights, whereas the planner averages over κ according to weights $\alpha(w,\kappa)$. Put another way, the household maximizes ex-ante to the realization κ but the planner ex-post. To evaluate the change in utility, for each z, we take the inner integral in the welfare function: $\frac{\partial W}{\partial \tau} = \int_z \int_\kappa \alpha(w,\kappa)\frac{\partial}{\partial \tau}V^*(z^*, w)d\mu$ and subtract off the household's FOC from Section C.3: $\frac{\partial U^*}{\partial z} = 1 - T'(z) - v'(z/w) + M(z)S'(z) = 0$, multiplied by $E_\kappa\left[\alpha(w,\kappa)\right]$. Since the household

[40] Note that the derivative $\frac{d}{dz}M(z)$ is a total derivative from the planner's perspective, i.e. shifting between people at different z's. It includes changes the $M(z)$ in z due to both the the schedule $S(z)$ varying in z and the distribution of $\kappa \mid w$ varying in z.

is optimizing, this FOC is zero and subtracting it does not change the expression. Hence we have:

$$\int_\kappa \alpha(w,\kappa)\frac{\partial}{\partial\tau}V^*(z^*,w)d\mu = \int_\kappa \alpha(w,\kappa)\frac{\partial z}{\partial\tau}\left(\frac{\partial V^*}{\partial z}\right)d\mu \tag{37}$$

$$= \frac{\partial z}{\partial\tau}\int_\kappa\left(\alpha(w,\kappa)\frac{\partial V^*}{\partial z} - E_\kappa\left[\alpha(w,\kappa)\right]\frac{\partial U^*}{\partial z}\right)d\mu \tag{38}$$

$$= \frac{\partial z}{\partial\tau}\int_\kappa\left(\alpha(w,\kappa)\mathbb{1}(\kappa\le S(z))\cdot S'(z) - E_\kappa\left[\alpha(w,\kappa)\right]M(z)S'(z)\right)d\mu \tag{39}$$

$$= \frac{\partial z}{\partial\tau}S'(z)E_\kappa\left[(\alpha(w,\kappa)\mathbb{1}(\kappa\le S(z)) - E_\kappa\left[\alpha(w,\kappa)\right]M(z))\right] \tag{40}$$

Noting that

$$E_\kappa\left[\alpha(z,k)\right] = M(z)E_{\kappa\le S(z)}\left[\alpha(z,k)\right] + (1-M(z))E_{\kappa>S(z)}\left[\alpha(z,k)\right],$$

and the definition of the regression coefficient

$$\beta(z) = M(z)(1-M(z))\left(E_{\kappa>S(z)}\left[\alpha(z,k)\right] - E_{\kappa\le S(z)}\left[\alpha(z,k)\right]\right) \tag{41}$$

and some manipulation yields the final term in the proposition.

It remains to count the direct effects of changes T and S levels, and any fiscal consequences of labor supply changes.

The total direct tax changes that accrue to pre-reform income level z are a reduction of tax of $M(z)ds$. This has a welfare effect of $E_{w,\kappa}\left[g(w,\kappa)M(z)\right]ds$, where implicitly the FOC defines a unique $z = z(w)$.

This is counterbalanced with an reduction of $S(z)$ by ds at all incomes. Write welfare as

$$W = \int_z\left(\int_0^{S(z)}\alpha(w,\kappa)(S(z)-\kappa)\mu d\kappa + \int_\kappa \alpha(w,\kappa)\left(z - T(z) - v(z/w)\right)\mu d\kappa\right)dw.$$

Hence, by the Leibniz rule, an expansion of $S(z)$ by ds has a direct effect of $\int_z\int_0^{S(z)}\alpha(w,\kappa)d\mu = \int_z E_{\kappa\le S(z)}\left[\alpha(w,\kappa)\right]M(z)$. Since w and z are in one to one correspondence, we switch the integration label, combine the direct effects of the tax increase and the $S(z)$ expansion to arrive at:

$$\Delta W_1 = \int_z M(z)\left(E_\kappa\left[\alpha(z,k)\right] - E_{\kappa\le S(z)}\left[\alpha(z,k)\right]\right)dH(z). \tag{42}$$

Again using the law ot total expectation on $E_\kappa\left[\alpha(z,k)\right]$ and the definition of the regression coefficient $\beta(z)$ yields the first term in the proposition.

In addition there is a lump-sum transfer to all z due to the fiscal savings from marginal recipients. As $S(z)$ declines by ds, the fiscal saving is $E_z\left[S(z)m(z)\right]$ which accrues to everyone, at welfare gain of

$$E_z\left[S(z)m(z)\right]\int_z\int_\kappa g(w,\alpha)d\mu.$$

Recalling that we calibrated the average welfare weight $E_{\kappa,z}\left[g(\kappa,z)\right]=1$ and writing $\bar{\varepsilon}_b=\int_z\frac{M(z)}{\bar{M}}\varepsilon_b(z)dz$ yields the second term in the proposition.

Finally, since tax has declined by $M(z)ds$ at each z, marginal tax rates have changed by $M'(z)ds$ (when we assume $M'(z)<0$, this means the marginal rates have increased), labor supply contracts by $dsM'(z)\frac{\partial z}{\partial\tau}$ at each income z. The fiscal cost per $d\tau$ is $T'(z)-\frac{d}{dz}\left(S(z)M(z)\right)$. We assume this is paid for lump-sum, which means it is paid by the household with average welfare weight, which we calibrated to be unity. Plugging in the definition of the elasticity yields the third term in proposition 1. This completes the proof.

□

C.6 Proof of Proposition 2

Proof. The proof is very similar to the proof of Proposition 1. The first term, the lost value of self-targeting, is λ multiplied by the term in Proposition 1. The same is true for the second term, since the fiscal savings are redistributed in-kind. The primary differences are in the labor supply terms.

Since the slope of the automatic transfer schedule has risen (as $M'(z)<0$), labor supply contracts by $dsM'(z)\frac{\partial z}{\partial s_A}$ at each income z. The fiscal cost per $d\tau$ is $T'(z)-\frac{d}{dz}\left(S(z)M(z)\right)-S_A'(z)$. However, examining the first-order condition of the household, we have that $\frac{\partial z}{\partial s_A}=\lambda\frac{\partial z}{\partial\tau}$. Thus, $dsM'(z)\frac{\partial z}{\partial s_A}=dsM'(z)\lambda\frac{\partial z}{\partial\tau}$ and plugging in the definition of the elasticity: $\epsilon^z=-\frac{\partial z}{\partial\tau}\frac{1-T'(z)}{z}$ and by assumption all fiscal costs are paid lump sum, i.e. by the average welfare weight $E_{w,\kappa}\left(\alpha(g,w)\right)=1$, yields the first labor supply term after rearrangements similar to Proposition 1.

Finally, we analyze the final term of the proposition, which arises from the wedge between welfare (which uses ex-post utility) and the utility that the agent optimizes (which is ex-ante to κ).

In particular, note that $\frac{\partial V^*}{\partial z}=1-T'(z)-v'(z/w)+S_A'(z)+\mathbb{1}(\kappa\leq S_V(z))\cdot S_V('(z)$ whereas $\frac{\partial U^*}{\partial z}=1-T'(z)-v'(z/w)+S_A'(z)+M(z)S_V'(z)$. No envelope theorem applies since when the household averages over κ they do so without weights, whereas the planner averages over κ according to weights $\alpha(w,\kappa)$ To evaluate the change in utility, we subtract off $\frac{\partial U^*}{\partial z}=1-T'(z)-v'(z/w)+S_A'(z)+M(z)S_V'(z)=0$ by an analogue of the household's FOC in Section C.3. Hence we have, noting again that $\frac{\partial z}{\partial s_a}=\lambda\frac{\partial z}{\partial\tau}$, :

$$\int_\kappa \alpha(w,\kappa)\frac{\partial}{\partial s_a}V^*(z^*,w)d\mu = \int_\kappa \alpha(w,\kappa)\frac{\partial z}{\partial s_a}\left(\frac{\partial V^*}{\partial z}\right)d\mu \tag{43}$$

$$= \lambda\frac{\partial z}{\partial \tau}\int_\kappa \left(\alpha(w,\kappa)\frac{\partial V^*}{\partial z} - E_\kappa\left[\alpha(w,\kappa)\right]\frac{\partial U^*}{\partial z}\right)d\mu \tag{44}$$

$$= \lambda\frac{\partial z}{\partial \tau}\int_\kappa \left(\alpha(w,\kappa)\mathbb{1}(\kappa \leq S(z))\cdot S'(z) - E_\kappa\left[\alpha(w,\kappa)\right]M(z)S'(z)\right)d\mu \tag{45}$$

$$= \lambda\frac{\partial z}{\partial \tau}S'(z)E_\kappa\left[\left(\alpha(w,\kappa)\mathbb{1}(\kappa \leq S(z)) - E_\kappa\left[\alpha(w,\kappa)\right]M(z)\right)\right] \tag{46}$$

All the terms are scaled by λ, which we divide by. This completes the proof.

□

C.6.1 Proof of Proposition 3

Proof. The positivity of the redistribution within z term follows directly from the definition of within-advantageous selection (Definition 1).

It remains to show the positivity of the redistribution between z term under within- and between-advantageous selection. Write

$$f_1(z) = -E_\kappa(\alpha(z,\kappa)) \tag{47}$$

and

$$f_2(z) = -\left(M(z) - E_{z'}(M(z'))\right) \tag{48}$$

such that the redistribution between z term can be written as

$$\int_z E_\kappa\left[\alpha(z,\kappa)\right]\left\{M(z) - E_{z'}\left[M(z')\right]\right\}h(z)dz = \int_z f_1(z)f_2(z)h(z)dz = E_z\left[f_1(z)f_2(z)\right]. \tag{49}$$

By the assumption of between-adverse selection, we have that the distribution of $\kappa \mid z$ increases in z in the FOSD sense. Since $M(z)$ is precisely the CDF of $\kappa \mid z$ evaluated at $S(z)$, and $S(z)$ is weakly decreasing, immediately we have that $M(z)$ decreases in z and hence $f_2(z)$ increases in z.

By within-adverse selection, $\alpha(z,\kappa)$ decreases in κ, hence $-\alpha(z,\kappa)$ increases in κ. Then again by FOSD, we have that $E_\kappa(-\alpha(z,\kappa)) = f_1(z)$ increases in z.

Since $f_1(z)$ and $f_2(z)$ are both increasing functions of z, it follows from Schmidt (2003) that $Cov(f_1(z), f_2(z)) \geq 0$. Noting that $E_z f_2(z) = 0$ we have

$$E_z\left[f_1(z)f_2(z)\right] = E_z\left[f_1\right]E_z\left[f_2(z)\right] + \mathrm{Cov}(f_1,f_2) = \mathrm{Cov}(f_1,f_2) \geq 0 \tag{50}$$

as required. $\square$

C.6.2 Proof of Proposition 4

Proof. Suppose, toward a contradiction, that optimally $S(z) = 0$ for all z. We implement the opposite of the reform described in the main text. That is, we increase the transfer to $S(z) = ds$ for all z, and taxes rise by $\tau(z) = M(z)ds$ at income z.

For now ignore labor supply changes. By assumption, there are no take-up costs for this mass of households. Thus each $\kappa = 0$ household receives a dollar, paid for by everyone at their income level z. The net welfare effect of this is $\int_z M(z) \{\alpha(z, \kappa = 0) - E_\kappa [\alpha(z, \kappa)]\} h(z)dz$, the redistribution within term. Assuming advantageous selection within income, this term is positive.

Next, consider the effects of the altered labor supply choices in response to the tax changes. The marginal rates have changed by $dsM'(z)$ which is negative due to advatageous selection between incomes. Hence labor supply everywhere increases. By the envelope theorem, there are no first order utility effects to this. There is a positive fiscal externality, which further increases the welfare effect of this reform.

In sum, the reform has a strictly positive effect. Hence $S(z) = 0$ cannot have been optimal. $\square$

C.6.3 Proof of Proposition 5

Proof. Directly from the definition of advantageous selection we get that $E_{z,\kappa\leq S(z)} [\alpha(z, \kappa)] > E_{z,\kappa} [\alpha(w, \kappa)]$. This implies the integrand is positive for all values of z, and hence positive overall. $\square$

C.6.4 Proof of Proposition 6

Proof. We derive a necessary condition from the optimal tax schedule that ensures the labor supply effect is signed as proposed. Suppose the planner increases the tax rate at income z by $d\tau$, and that the net fiscal gain/loss from this change is redistributed as a lump sum transfer/tax.

Breaking down the effect into fiscal and behavioural responses, we have the following changes to welfare:

1. Direct effect (fiscal and welfare):

$$d\tau \int_{x\geq z} (E_{z,\kappa}(\alpha(z, \kappa)) - E_\kappa(\alpha(x, \kappa))))dH(x) = E_{x\geq z} \left[E_{z,\kappa}(\alpha(z, \kappa) - E_\kappa(g(x, \kappa))\right] (1 - H(z)). \tag{51}$$

2. Compensated price effect (effect on tax receipts):

$$d\tau\frac{\partial z}{\partial \tau}\cdot[T'(z)]\cdot h(z)E_{z,\kappa}(\alpha(z,\kappa)=d\tau\left(-\epsilon^{z}\cdot z\cdot\frac{T'(z)}{1-T'(z)}h(z)E_{z,\kappa}(\alpha(z,\kappa))\right). \tag{52}$$

3. Compensated price effect (effect on social program payments):

$$d\tau\frac{\partial z}{\partial \tau}\frac{d}{dz}\left[-S(z)M(z)\right]h(z)E_{z,\kappa}(\alpha(z,\kappa))=d\tau\frac{\epsilon^{z}\cdot z}{1-T'(z)}\frac{d}{dz}\left[S(z)M(z)\right]h(z)E_{z,\kappa}(\alpha(z,\kappa)). \tag{53}$$

4. Non-envelope effect (welfare):

$$d\tau\int_{\kappa}\alpha(w,\kappa)\frac{\partial z}{\partial \tau}\left(\frac{\partial V^{*}}{\partial z}\right)d\mu=-d\tau\frac{\epsilon^{z}\cdot z}{1-T'(z)}S'(z)Cov_{k}\left[\alpha(w,k),\mathbb{1}(\kappa\leq S(z))\right]. \tag{54}$$

The equality of term 54 follows from the working in the proof of Proposition 1.

A necessary condition for the optimality of the tax system is that the sum of these welfare effects weakly negative. In particular, to convert to utility units, suppose the net fiscal gain/loss from this MTR change was redistributed as a lump sum. This need not be the optimal way to redistribute, but for optimality it cannot deliver a positive welfare benefit. Consequently,

$$\begin{aligned}E_{x\geq z}\left[E_{z,\kappa}(\alpha(z,\kappa))-E_{\kappa}(g(x,\kappa))\right](1-H(z))\leq d\tau\Big[E_{z,\kappa}(\alpha(z,\kappa))\left(T'(z)-\frac{d}{dz}\left[S(z)M(z)\right]\right)\\+S'(z)Cov_{k}\alpha(w,k),\mathbb{1}(\kappa\leq S(z))\Big]\left(\frac{h(z)\epsilon^{z}z}{1-T'(z)}\right).\end{aligned} \tag{55}$$

The left hand side is positive, and therefore so must the right hand side be positive. Immediately we have that the sum of the labor supply terms is negative, as hypothesized. □

References for Appendices

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