TAKE-UP AND TARGETING: EXPERIMENTAL EVIDENCE FROM SNAP*

Amy Finkelstein and Matthew J. Notowidigdo

We develop a framework for welfare analysis of interventions designed to increase take-up of social safety net programs in the presence of potential behavioral biases. We calibrate the key parameters using a randomized field experiment in which 30,000 elderly individuals not enrolled in—but likely eligible for—the Supplemental Nutrition Assistance Program (SNAP) are either provided with information that they are likely eligible, provided with this information and offered assistance in applying, or are in a "status quo" control group. Only 6% of the control group enrolls in SNAP over the next nine months, compared to 11% of the Information Only group and 18% of the Information Plus Assistance group. The individuals who apply or enroll in response to either intervention have higher net income and are less sick than the average enrollee in the control group. We present evidence consistent with the existence of optimization frictions that are greater for needier individuals, which suggests that the poor targeting properties of the interventions reduce their welfare benefits. *JEL* Codes: C93, H53, I38.

I. Introduction

Enrollment in U.S. social safety net programs is not automatic: individuals must apply and demonstrate eligibility. Often, eligibility rules are complicated, application forms long, and documentation requirements substantial. Perhaps as a result, incomplete take-up is pervasive (Currie 2006). Two typical explanations

*We are grateful to Martin Aragoneses, Aileen Devlin, Carolyn Stein, John Tebes, and Ting Wang for excellent research assistance and Laura Feeney for superb research management. We thank our excellent partners at Benefits Data Trust, particularly Rachel Cahill and Matt Stevens, who worked tirelessly and patiently to address our innumerable requests and questions. We thank Abhijit Banerjee, Stefano DellaVigna, Manasi Deshpande, Paul Goldsmith-Pinkham, Colin Gray, Nathan Hendren, Richard Hornbeck, Henrik Kleven, Larry Katz, Kory Kroft, Elira Kuka, Ben Olken, Jesse Shapiro, Chris Udry, numerous seminar participants, and five anonymous referees for helpful comments. The experiment reported in this study is listed in the AEA RCT Registry (#0000902). We gratefully acknowledge financial support from the Alfred P. Sloan Foundation.

The Quarterly Journal of Economics (2019), 1505–1556. doi:10.1093/qje/qjz013.
Advance Access publication on May 2, 2019.

are lack of information about eligibility and transaction costs associated with enrollment.[1]

Numerous public policies try to increase take-up by increasing awareness of eligibility and simplifying application processes. For example, in the context of the U.S. Supplemental Nutrition Assistance Program (SNAP)—also known as food stamps—New York City Mayor Bill de Blasio proposed an enrollment campaign that contacted Medicare recipients about their SNAP eligibility and improved online services (Hu 2014), the state of Texas simplified the application process (Aaronson 2011), and Congress provided funding to study various models for facilitating access to SNAP among the elderly (Kauff et al. 2014).

Yet incomplete information or transaction costs that create barriers to enrollment may be part of a constrained social optimum. Indeed, neoclassical theory has long emphasized that such so-called ordeals may serve as useful screens, allowing for a given amount of redistribution to occur at lower public cost (e.g., Nichols, Smolensky, and Tideman 1971; Nichols and Zeckhauser 1982; Besley and Coate 1992). By contrast, recent work in behavioral economics has conjectured that these ordeals may have exactly the opposite targeting effect, discouraging precisely those applicants the social planner would most like to enroll (e.g., Bertrand, Mullainathan, and Shafir 2004; Mani et al. 2013; Mullainathan and Shafir 2013). For example, Mullainathan and Shafir (2013) argue that poverty imposes a "bandwidth tax" that makes poor individuals more likely to fail to undertake high-net-value activities, such as enrolling in a public benefit program for which one is eligible. Ultimately, the targeting properties of these barriers and their welfare implications are empirical questions.

This article formalizes a framework for analyzing the normative consequences of interventions that—by reducing ordeals—can affect take-up (the number of individuals who enroll in a social safety net program) and targeting (the types of individuals who enroll). We apply the framework to the results of a randomized evaluation of interventions aimed at elderly nonparticipants in SNAP.

We focus conceptually and empirically on interventions that inform people about their likely eligibility (information

1. A third common explanation—stigma associated with program participation—can be modeled as a form of a transaction cost (Moffit 1983; Currie 2006).

interventions) or reduce the private costs of applying (assistance interventions). As described already, such interventions are common forms of public policy. They are also the subject of an active empirical literature examining their impact on take-up and targeting. Studies of information interventions have been conducted, for example, for the Earned Income Tax Credit (Bhargava and Manoli 2012; Guyton et al. 2016; Manoli and Turner 2016; Barr and Turner 2018), Social Security Disability Insurance (Armour 2018), postsecondary enrollment (Bettinger et al. 2012; Barr and Turner 2018; Dynarski et al. 2018), energy efficiency audits (Allcott and Greenstone 2017), and SNAP (Daponte, Sanders, and Taylor 1999). Studies of assistance interventions have been conducted, for example, for Supplemental Security Income and Social Security Disability Insurance (Deshpande and Li forthcoming), the Women, Infants, and Children program (Rossin-Slater 2013), postsecondary enrollment (Bettinger et al. 2012), conditional cash transfers (Alatas et al. 2016), tax-subsidized savings plans (Madrian and Shea 2001), and SNAP (Schanzenbach 2009). This existing literature has been primarily descriptive, focusing on the number and observable characteristics of those who respond.

Our theoretical framework, however, shows that there is no general relationship between targeting on observables and the impact of the intervention on private or social welfare. We extend the standard targeting model in which adding ordeals to a means-tested transfer program can improve social welfare beyond what can be achieved through an optimal nonlinear income tax. In the standard framework (which is helpfully described by Currie and Gahvari 2008), individual types (i.e., abilities) are not observed, application decisions are privately optimal, and labor supply responds endogeneously to the income tax; in this case, ordeals that impose greater utility costs on higher-ability types can allow the government to redistribute a given amount to lower-ability types at lower public cost. Our key extension is—in the spirit of the behavioral literature—to allow for the possibility that individuals may not make privately optimal application decisions. The private welfare gains for marginal enrollees therefore depend on the size of their behavioral biases, which may vary (with unknown sign) by type. In addition, we allow for a flexible relationship between the individual's type and the fiscal externality from her enrollment on the government budget. Thus, for a given enrollment response to an intervention, the welfare implications of its targeting properties depend on the relative behavioral biases across types and the relative fiscal externalities across types. These are empirical questions.

To explore these issues empirically, we examine the impact of various interventions on the number and type of eligible elderly individuals who enroll in SNAP, one of the most important social safety net programs in the United States. It is the only benefit that is virtually universally available to low-income households. During the Great Recession, as many as one in seven individuals received SNAP (Ganong and Liebman 2018). In 2015, public expenditures on SNAP were about $70 billion, roughly the same amount as the Earned Income Tax Credit (EITC) and higher than the $60 billion spent on SSI or the $30 billion spent on cash welfare (TANF).[2] Although the elderly, who are the focus of our study, are only 10% of SNAP caseload, they have especially low take-up; in 2012, only 42% of eligible elderly enrolled in SNAP, compared with 83% overall (Eslami 2016). And the stakes associated with nonparticipation are nontrivial for the elderly; average annual SNAP benefits are about $1,500, or about 15% of household income among the eligible (Center on Budget and Policy Priorities 2017).

To explore barriers to enrollment and the types of individuals deterred by these barriers, we partnered with Benefits Data Trust (BDT), a national not-for-profit organization committed to transforming how people in need access public benefits. We constructed a study population of approximately 30,000 elderly individuals (age 60 and over) in Pennsylvania who are not enrolled in SNAP but are enrolled in Medicaid and therefore are likely eligible for SNAP. We randomized them into three equally sized groups: an Information Only treatment, an Information Plus Assistance treatment, and a status quo control group. Our interventions build on and significantly scale up two earlier randomized evaluations of interventions to increase SNAP take-up via the provision of information (Daponte, Sanders, and Taylor 1999) and assistance (Schanzenbach 2009).

The interventions took place in the first half of 2016. Study participants in the Information Only treatment received a mailing and a follow-up reminder postcard from the secretary of

2. U.S. Department of Agriculture (2016), U.S. Department of Health and Human Services (2016), U.S. Internal Revenue Service (2016), U.S. Social Security Administration (2016).

Pennsylvania's Department of Human Services (DHS), informing them of their likely eligibility for SNAP and providing a phone number at DHS to call to apply. Study participants in the Information Plus Assistance arm received a virtually identical letter and reminder postcard, with one key change: they were provided a phone number at the PA Benefits Center (the local name of BDT) to call to apply. Callers in this arm received phone-based application assistance from one of BDT's benefits outreach specialists; these BDT employees asked a series of questions that allowed them to inform the caller of their potential eligibility and likely benefit amount, to fill out the application, to assist the applicant in collecting necessary verification documents, to submit the application, and to assist with any follow-up questions that arose from DHS. Both intervention arms included subtreatments that varied the content of the letter and, in one case, whether the reminder postcard was sent; we describe these in more detail below, although we focus primarily on the main treatments. We tracked calls from study participants to BDT and DHS and received administrative data from DHS on SNAP applications, enrollments, and benefit amounts after the intervention; we obtained additional demographic and health data preintervention from the study participants' Medicaid records.

The experiment produced two main empirical results. First, information alone increases enrollment, while information combined with assistance increases enrollment even more, but at a higher cost per enrollee. Nine months after the intervention—at which point the initial impact appears to be fully in place—enrollment is 6 percentage points in the control arm compared to 11 percentage points in the Information Only arm and 18 percentage points in the Information Plus Assistance arm; these enrollment rates are all statistically distinguishable ($p < .001$). A rough calculation suggests the intervention cost per additional enrollee is lower in the Information Only treatment: about $20 per enrollee compared to about $60 per enrollee in the Information Plus Assistance treatment. We also find that a subtreatment of the Information Only intervention, which omits the reminder postcard, reduces its impact by about 20%. This suggests a role for inattention in explaining at least some of the impact of the Information Only intervention.

We observe intervention effects at several intermediate stages. About 30% of the participants in each intervention arm call in response to the outreach materials, suggesting a likely ceiling for the impact of the interventions on enrollment. Similar call-in rates in the two interventions also suggest that the larger enrollment effects of Information plus Assistance relative to Information Only are likely due to the assistance per se, rather than the anticipation of assistance. Each intervention increases applications proportionally to its effect on enrollment; the success rate of applications is about 75% in all three arms.

The second main empirical finding is that both interventions decrease targeting. We find that marginal applicants and enrollees in either intervention are less needy than average applicants or enrollees in the control group. They receive lower benefits if they enroll (from a benefit formula that decreases with net income) and are less sick (as measured by preintervention rates of hospital visits and chronic diseases). In addition, they are more likely to be white and more likely to have English as their primary language, suggesting that they may be less socioeconomically disadvantaged than the control group applicants and enrollees. These targeting results are similar across the intervention arms. Importantly, however, the 70% of individuals who did not call in response to our interventions and remain largely unenrolled look more needy than those who responded on all these dimensions; this suggests that other interventions that may reach different populations—such as those who do not even open their mail—may have different targeting effects.

We use the conceptual framework we developed to explore the normative implications of the experiment's findings. The evidence is consistent with the "behavioral" hypothesis that individuals underestimate their expected benefits from applying. This suggests potential private—as well as social—welfare gains from each intervention. Our estimates also suggest that underestimation of expected benefits is greater for needier individuals, again consistent with leading behavioral theories (e.g., Bertrand, Mullainathan, and Shafir 2004; Mani et al. 2013; Mullainathan and Shafir 2013). However, in contrast to these models and consistent with neoclassical theory (e.g., Nichols, Smolensky, and Tideman 1971; Nichols and Zeckhauser 1982; Besley and Coate 1992), we find that our interventions to reduce transaction costs or improve information target less needy individuals. This bodes poorly for their welfare effects. Indeed, our calibrated model suggests that if—counterfactually—our intervention had better targeting, the social welfare benefits would have been substantially higher. Although these particular findings are naturally specific to our setting and intervention, we believe the normative framework—which we illustrate in our specific context—may be usefully applied to other settings.

The rest of the article proceeds as follows. Section II presents our framework. Section III provides background information on SNAP. Section IV describes our experimental design and data. Section V presents the experimental results. Section VI uses the results to calibrate the model from Section II and perform welfare analysis of the interventions. All appendix material is presented in the Online Appendix.

II. Framework

We analyze the welfare impact of interventions that provide eligibility information and/or application assistance for a redistributive transfer program. We summarize the model and results here, emphasizing intuition; the proofs are in Online Appendix A.1.

II.A. Model Setup

There are two types of individuals $j \in \{L, H\}$. Each type has unobserved wage θ_j, with $\theta_H > \theta_L$. This is the key source of heterogeneity in the model. We assume throughout that there is a unit mass of each type.

Individuals choose hours of work h_j (which produces labor income $\theta_j h_j$) and whether to apply to a supplemental income program. There is a (potentially nonlinear) income tax system $\tau(\theta_j h_j)$, which maps pretax labor earnings to taxes owed to the government. We denote net of tax earnings by $y_j \equiv \theta_j h_j - \tau(\theta_j h_j)$.

Program application provides benefits B if income is below an earnings cutoff we denote by r^*. We allow each type to misperceive the benefit amount by ε_j, so that the perceived benefit of applying is $(1 + \varepsilon_j)B$. With $\varepsilon_j < 0$, misperception reduces the perceived benefit of applying. We refer to the special case of no misperceptions—that is, $\varepsilon_j = 0$ for $j \in \{L, H\}$—as the "neoclassical" benchmark case.

Individuals share a common utility function: $u(x_j) - v(h_j)$ if they do not apply and $u(x_j) - v(h_j) - (\bar{\Lambda}\kappa_j + c)$ if they apply.

Individuals get utilty from consumption (x_j), disutility from hours worked (h_j), and disutility from applying ($\bar{\Lambda}\kappa_j + c$).

Disutility from applying can include the time and effort spent compiling documents, filling out forms, and participating in an interview, as well as any associated stigma. This disutility depends on three terms: c is an individual-specific utility cost of applying and is distributed according to a type-specific distribution $f_j(c)$, $\bar{\Lambda}$ is a parameter that affects the utility cost of applying that is common across individuals (and is under the control of the social planner or researcher), and κ_j is how the utility cost varies with $\bar{\Lambda}$ for individuals of type j. This formulation nests ordeals that impose a greater utility cost on H types ($\kappa_H > \kappa_L$) or on L types ($\kappa_L > \kappa_H$). The former includes utility costs $\kappa_j = \theta_j$, which might correspond to a common time cost that has higher utility costs for H types due to higher wages (e.g., Nichols and Zeckhauser 1982). The latter includes L types having more difficulties filling out forms (e.g., Bertrand, Mullainathan, and Shafir 2004).

Individuals make application and labor supply choices to maximize private utility, given their (possibly incorrect) perceptions. We denote type j's hours choice by h_j^A if they apply and by $h_j^{\neg A}$ if they do not apply; we denote their corresponding after-tax income by y_j^A and $y_j^{\neg A}$. For low-ability individuals, we assume that either hours choice would leave them with labor earnings at or below the income threshold r^* needed to qualify for the supplemental income program. For high-ability individuals, we assume that the hours choice if they do not apply puts their income above the eligibility threshold r^*; therefore, if they do apply, their hours choice is given by $h_H^A = \frac{r^*}{\theta_H}$, so that they are at the income threshold. Intuitively, both types choose weakly fewer hours of work if they apply ($h_j^A \leqslant h_j^{\neg A}$) due to the potential income effect from benefits; for H types there is an added reduction in hours from applying because of the need to reduce hours to meet the income eligibility threshold.

Type j individuals apply if their expected utility from applying (given their optimal hours choice) exceeds their expected utility from not applying (again given their optimal hours choices). We define c_j^* to be the threshold level of c such that for $c < c_j^*$, type j chooses to apply.

Total private welfare of type j, V_j, can therefore be written:

$$\begin{aligned}V_j &= Pr(apply) * E[u()|apply] + Pr(\neg apply) * E[u()|\neg apply]\\ &= \int_0^{c_j^*} (u(y_j^A + B) - v(h_j^A) - (\bar{\Lambda}\kappa_j + c))dF_j(c)\\ &\quad + \int_{c_j^*}^{\infty} [u(y_j^{\neg A}) - v(h_j^{\neg A})]dF_j(c).\end{aligned}$$

We assume a utilitarian social welfare function. Social welfare is therefore the sum of private welfare minus social costs. The social costs of the program include the "mechanical" program costs (B per applicant) and any fiscal externalities from individuals' application choices on the government budget. In the presence of fiscal externalities, privately optimal application decisions may not be socially optimal.

We explicitly model the "standard" fiscal externality: if individuals choose fewer hours of work as a result of applying for benefits, application decisions impose a negative fiscal externality on the government via their impact on income tax revenue; application decisions impose a social cost—above and beyond the mechanical program cost (i.e., transfer) B—due to their impact on labor supply decisions and hence net tax revenue. As a result, when individuals privately optimize with accurate beliefs, too many people apply relative to the social optimum. For expositional ease we use G_j^A (respectively, $G_j^{\neg A}$) to denote the net fiscal externality when a type j individual does (does not) apply. In our set-up $G_j^A = \tau(h_j^A\theta_j)$ and $G_j^{\neg A} = \tau(h_j^{\neg A}\theta_j)$; we later discuss how the model is easily generalized to allow for other possible fiscal externalities.

Total social welfare, W, can therefore be written:

$$\begin{aligned}W = &\underbrace{V_L + V_H}_{\text{Private Welfare}} - \underbrace{\left[B(A_L + A_H)\right]}_{\text{Program Cost}}\\ &+ \underbrace{[A_L G_L^A + (1 - A_L)G_L^{\neg A} + A_H G_H^A + (1 - A_H)G_H^{\neg A}]}_{\text{Fiscal Externality}},\end{aligned}$$

where $A_j = F_j(c_j^*)$ is the expected number of applications from type j individuals.[3]

The social planner chooses the income tax system $\tau(\theta_j h_j)$ and the income transfer program (including the "ordeal" parameter $\bar{\Lambda}$) to maximize social welfare. As has been shown (see, e.g., Currie and Gahvari 2008), if $\kappa_H > \kappa_L$, the social optimum will involve a nonzero ordeal utility cost ($\bar{\Lambda} > 0$) even in the presence of an arbitrary optimal nonlinear income tax. Intuitively, with unobserved ability θ_j and endogenous hours choices, the incentive compatibility constraint that high-ability types do not want to "mimic" low-ability types prevents the government from achieving the first-best amount of redistribution (i.e., equal consumption across types). Adding ordeals that are more costly for the high-ability types (i.e., $\kappa_H > \kappa_L$) can relax the incentive compatibility constraint on the H type and thus allow for more redistribution. Our goal, however, is not to characterize the globally optimal system of taxes, transfers, and ordeals, but to characterize the marginal social welfare gain (or loss) from interventions that affect information about program eligibility or the private cost of application.

II.B. Social Welfare Effects of Interventions and Targeting

We model two alternative interventions corresponding to the two main treatment arms in the experiment. In the Information Only treatment, the treatment increases the perceived benefits of applying ($d\varepsilon_j$). In the Information Plus Assistance treatment, the treatment increases the perceived benefits of applying and decreases the actual private cost of applying ($d\varepsilon_j, -d\bar{\Lambda}$). For simplicity, we assume the interventions have zero marginal cost.

For notational ease we introduce the following definitions.

DEFINITION 1. Define $\mu_j \equiv u(y_j^A + B) - u(y_j^A + (1+\varepsilon_j)B)$ and $\xi_j \equiv u'(y_j^A + B)$.

The μ_j term denotes the difference for type j between the actual and perceived utility when applying; if individuals under-

3. Note that rather than add mechanical program costs and fiscal externalities to the social welfare function, we could instead "close" the government budget by having these "paid for" out of individual consumption. Our approach assumes that the costs of the government budget are borne by someone with the average marginal utility of consumption in society; implicitly, our W expression is thus a "money metric" social welfare expression, normalized by the average marginal utility of consumption in the population.

estimate the benefits of applying (i.e., $\varepsilon_j < 0$), which is the premise of the information interventions, then $\mu_j > 0$. The ξ_j term denotes the marginal utility of consumption for type j individuals who choose to apply.

PROPOSITION 1. The effect of the Information Only treatment on welfare is given by:

$$\begin{aligned} \frac{dW}{dT}^{Information\ Only} &= \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT}}_{\text{Change in Private Welfare}} \\ &- \underbrace{\left[B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT} \right) \right]}_{\text{Change in Mechanical Program Costs}} \\ &+ \underbrace{\left[[G_L^A - G_L^{\neg A}] \frac{dA_L}{dT} + [G_H^A - G_H^{\neg A}] \frac{dA_H}{dT} \right]}_{\text{Change in Fiscal Externality}}. \end{aligned} \tag{1}$$

The effect of the Information Plus Assistance treatment on welfare is given by:

$$\begin{aligned} &\frac{dW}{dT}^{Information\ Plus\ Assistance} \\ &= \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT} + \kappa_L A_L + \kappa_H A_H}_{\text{Change in Private Welfare}} \\ &- \underbrace{\left[B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT} \right) \right]}_{\text{Change in Mechanical Program Costs}} \\ &+ \underbrace{\left[[G_L^A - G_L^{\neg A}] \frac{dA_L}{dT} + [G_H^A - G_H^{\neg A}] \frac{dA_H}{dT} \right]}_{\text{Change in Fiscal Externality}}. \end{aligned} \tag{2}$$

Note that we abstract away from potential income effects of the interventions on inframarginal applicants. These generate additional terms without qualitatively changing the main insights of the model; for completeness, we provide the terms in Online Appendix A.1.1. We also implicitly assume in our discussion that

$u'(x_j) > 1$ for $j \in \{H, L\}$, so that the marginal utility of consumption from B exceeds the mechanical program cost for both types.[4]

For the Information Only intervention, the above expressions indicate that in the "neoclassical benchmark" ($\varepsilon_j = 0$), the intervention has no effect on private welfare, since $\mu_L = \mu_H = 0$. Intuitively, since individual decisions are already privately optimal, the marginal individual is indifferent between applying and not applying, and therefore a change in behavior has no first-order impact on their private welfare. If, however, individuals underestimate the benefit of applying (i.e., $\varepsilon_j < 0$), the intervention increases private welfare for marginal applicants of each type by μ_j, with the increase in private welfare increasing in the amount of underestimation of benefits. Private welfare analysis is similar for the Information Plus Assistance intervention, but with two additional terms that represent the increase in private welfare from reducing costs for *inframarginal* applicants of each type. The interventions also affect social welfare through their direct (mechanical) impact on program costs and their impact on the program's fiscal externalities. The expressions for these impacts are the same for both interventions, and do not directly depend on perceptions ε_j.

DEFINITION 2. We define targeting as the share of enrollees who are type L; that is, $e = \frac{E_L}{E_H+E_L}$, where E_j is the number of type j enrollees. We say that a treatment T increases targeting if $\frac{de}{dT} > 0$. We derive the following proposition summarizing the relationship between changes in social welfare and changes in targeting:

PROPOSITION 2. Holding constant the change in applications due to an intervention, the change in social welfare in response to an improvement in targeting $\left(\frac{de}{dT} > 0\right)$ from an Information Only (or Information Plus Assistance) treatment is given by:

$$\text{(3)} \qquad \frac{\partial}{\partial\left(\frac{de}{dT}\right)}\left(\frac{dW}{dT}\right)\Bigg|_{\frac{dA}{dT}} = \left[(\mu_L - \mu_H) + \left(G_L^A - G_L^{\neg A}\right) - \left(G_H^A - G_H^{\neg A}\right)\right](E_H + E_L).$$

4. This would follow if, for example, we normalized our expression for $\frac{dW}{dT}$ by the average marginal utility of the population, and both eligible types $j \in \{H, L\}$ have higher marginal utility of consumption than the average in the population, as would be expected in any means-tested benefit program.

This result shows that the ceteris paribus change in social welfare from a change in targeting is a function of two terms: the difference in private welfare from enrolling an L type compared with an H type (i.e., $(\mu_L - \mu_H)$) and the difference in the fiscal externality imposed from enrolling a low type compared to an H type (i.e., $(G_L^A - G_L^{\neg A}) - (G_H^A - G_H^{\neg A})$).

Our framework nests the "folk wisdom" that for a given change in applications, interventions that improve targeting (i.e., $\frac{de}{dT} > 0$) will be better for social welfare. This is most naturally seen in the "standard" setting (e.g., Nichols and Zeckhauser 1982) in which individuals do not make mistakes in their application decisions ($\varepsilon_L = \varepsilon_H = 0$) and the fiscal externality is via the impact on income tax revenue. Because individuals do not make mistakes, $(\mu_L - \mu_H)$ is 0; a change in targeting therefore has no effect on private welfare. The relationship between a change in social welfare and a change in targeting depends only on how the change in targeting changes the fiscal externality from applying. In the "standard" setting, improved targeting—that is, inducing an L to apply instead of an H—lowers the (negative) fiscal externality from applying, since reductions in earnings for H types induced to apply are larger than for L types induced to apply.

One aspect of the "debate" then between the neoclassical models (e.g., Nichols and Zeckhauser 1982) and the "behavioral models" (e.g., Mullainathan and Shafir 2013) is about whether interventions that reduce ordeals worsen targeting (i.e., $\kappa_H > \kappa_L$) or improve targeting (i.e., $\kappa_L < \kappa_H$), because these have different implications for the fiscal externalities generated by the program. This may explain why empirical research has focused on the targeting properties of interventions (e.g., Bhargava and Manoli 2012; Alatas et al. 2016; Bhargava, Loewenstein, and Sydnor 2017; Deshpande and Li forthcoming).

However, our framework shows that once we depart from the neoclassical benchmark, the relationship between the targeting properties of the intervention and the social welfare impact of the intervention breaks down. With misperceptions ($\varepsilon_j \neq 0$), the change in social welfare from a change in targeting is increasing in $(\mu_L - \mu_H)$; μ_L enters positively while μ_H enters negatively because the thought experiment of increasing targeting "swaps" an H applicant for an L applicant. With $\varepsilon_j < 0$, μ_j is increasing in two type-specific factors: the marginal utility of consumption (ξ_j) and the magnitude of the underestimation ($-\varepsilon_j$). Assuming that the L types have a higher marginal utility of income (or a higher social marginal utility of income), as would be the case when an optimal income tax cannot achieve the first-best level of redistribution, then a sufficient condition for an increase in targeting to increase private welfare is that underestimation is nonzero for at least one type and weakly higher (in absolute value) for the L type (i.e., $\varepsilon_L \leqslant \varepsilon_H \leqslant 0$, with at least one inequality strict). This includes, for example, the case assumed in much of the behavioral literature (e.g., Mullainathan and Shafir 2013) that behavioral frictions are larger for the L type, as well as the case where both types underestimate the probability of application acceptance by the same amount (in a proportional sense), so that $\varepsilon_H = \varepsilon_L < 0$.

Finally, we note that in practice, G_j^A and $G_j^{\neg A}$ may include other sources of fiscal externalities instead of (or in addition to) the standard one modeled here. These could include the public costs of reviewing an application to determine eligibility and benefit amounts (Kleven and Kopczuk 2011) or other ways enrollment may affect the government budget, such as an impact of the program on health and hence public healthcare expenditures. These fiscal externalities may be positive or negative and may be larger or smaller for L types compared with H types.[5] As a result, even in the absence of behavioral frictions, interventions that improve targeting are not necessarily better for social welfare—it depends on the relative magnitudes of the fiscal externalities generated from enrollment by different types.

1. Extensions. In Online Appendix A.2 we show that the core propositions are robust to alternative modeling choices about the nature of misperceptions and "mistakes." We also consider nonmarginal changes, similar in spirit to Kleven (2018). Nonmarginal interventions can also undo the relationship between changes in targeting and changes in social welfare that is otherwise present in the "standard" setting (i.e., no mistakes and the only fiscal externality occurs via labor supply). Intuitively, in the nonmarginal case the increase in private welfare for enrollees who would not have enrolled absent the intervention can no longer be zeroed out by the envelope theorem. Instead, their increase in private

5. While the literature has tended to focus on fiscal externalities, any external impact from applying on the utility of other individuals in society also needs to be accounted for in normative welfare analysis. For an illustrative example of how this can be easily incorporated into our framework, see Finkelstein, Hendren, and Shepard (2019), section 5.2.

welfare now depends on the shape of the type-specific cost distribution $f_j(c)$; thus, the cost distribution functions introduce another "free parameter" that can affect the relationship between improvements in targeting and changes in social welfare, much as the misperception terms do when we depart from the neoclassical benchmark.

III. Setting and Background

SNAP is the second-largest means-tested program in the United States (U.S. Congressional Budget Office 2013). It is a household-level benefit designed to ensure a minimum level of food consumption for low-income families (Hoynes and Schanzenbach 2016). Our study focuses on elderly households—that is, households with an individual aged 60 or over—in Pennsylvania in 2016. Take-up of SNAP in Pennsylvania is similar to the nationwide estimates (Cunnyngham 2015). Online Appendix B provides more details on the program for our study participants; we summarize a few key features here.

Eligibility may be categorical—if the individual receives a qualifying benefit such as SSI or TANF—or based on means testing, which depends on gross income, assets, and, in some cases, information on particular types of income and expenditures. About two-thirds of elderly households in Pennsylvania receiving SNAP had household incomes below the federal poverty line (Center on Budget and Policy Priorities 2017).

To enroll in SNAP, an individual must complete an application, provide the necessary documents verifying household circumstances, and participate in an interview (phone or in person). The applicant must provide identifying information about herself and each household member, information on resources and income, and information on various household expenses such as medical expenses, rent, and utilities. She must provide documentation verifying identity, proof of residency, and proof of earnings, income, resources, and expenses. Applications can be submitted by mail, fax, in person at the County Assistance Office, or online. The online information and application system in Pennsylvania is considered one of the better state designs (Center on Budget and Policy Priorities 2016). In most cases, the state has 30 calendar days to process an application.

Once enrolled, an elderly household is certified to receive SNAP benefits for 36 months, although there are exceptions that require earlier recertification. The benefit formula is a decreasing function of net income—gross income minus certain exempt income and deductions for certain expenses—subject to a minimum and maximum. During our study period, the minimum monthly benefit was $0 or $16 depending on household type, and the maximum monthly benefit was $194 for a household size of 1, $357 for a household size of 2, and $511 for a household size of 3. In practice, as we will see in our data, there are distinct modes of the benefit distribution at the minimum and maximum. SNAP benefits are a substantial source of potential income for eligible households. For elderly households in Pennsylvania, enrollment entitles the household to benefits equivalent to, on average, about 15% of annual income (Center on Budget and Policy Priorities 2017).

The application imposes costs on the applicant and the government. Survey evidence from the late 1990s suggests that the average application takes about five hours to complete, including two trips to the SNAP office or other places, and average out-of-pocket costs were about $10, primarily for transportation (Ponza et al. 1999); however, regulatory changes enacted since the time of that survey were designed to reduce applicant costs by, for example, allowing a phone interview in lieu of an in person interview (Hoynes and Schanzenbach 2016). The state must process applications to determine eligibility, including verifying self-reported information in various available administrative data systems. Estimates from Isaacs (2008) suggest that annualized state administrative certification costs are about 10%–15% of annual benefits, a substantially higher share of benefits than administrative costs for the EITC.

IV. Empirical Design and Data

This section describes the interventions, empirical design, and data. Online Appendix C provides more details on the interventions, including subtreatments. More detail on the data is provided in Online Appendix D.

IV.A. Design of Interventions

We partnered with BDT, a national not-for-profit organization founded in 2005 and based in Philadelphia that strives to be a “one-stop shop” for benefits access, screening individuals for benefit eligibility and providing application assistance (Benefits Data Trust 2016). An observational study by Mathematica of six different SNAP outreach and enrollment approaches nationwide concluded that the BDT's intervention for the elderly in Pennsylvania was the lowest cost per enrollment of any of the methods studied (Kauff et al. 2014), although the 2009 program studied there was somewhat different than BDT's 2016 approach, which is what we study here.

For our study, as with past BDT SNAP enrollment efforts, the state of Pennsylvania provided BDT with administrative data on individuals aged 60 and older who were enrolled in Medicaid but not in SNAP. Such individuals are probably income-eligible for SNAP, since Medicaid tends to have income criteria similar to that of SNAP.

We randomized our study population of approximately 30,000 elderly individuals enrolled in Medicaid but not SNAP into three equally sized arms. Individuals in the control group received no intervention. Individuals in the Information Only intervention received outreach materials informing them of their likely eligibility for SNAP and the benefits they might receive and providing them with information on how to call the Department of Human Services to apply. Individuals in the Information Plus Assistance intervention received similar outreach materials but with information on how to call BDT to apply; if they called, they received application assistance. We did not design the Information Plus Assistance intervention; it follows BDT's current practices for helping to enroll individuals in SNAP.

1. Information Plus Assistance. BDT conducts a series of outreach services to inform individuals of their likely eligibility and help them apply for benefits. This outreach has two components: information and assistance. The information component consists of proactively reaching out by mail to individuals whom they have identified as likely eligible for SNAP and following up with a postcard after eight weeks if the individual has not called BDT. Letters and postcards inform individuals of their likely SNAP eligibility ("Good news! You may qualify for help paying for groceries through the Supplemental Nutrition Assistance Program (SNAP)") and typical benefits ("Thousands of older Pennsylvanians already get an average of $119 a month to buy healthy food") and provide information on how to apply ("We want to help you apply for SNAP!"), offering a number at BDT to call ("Please call the PA Benefits Center today. It could save you hundreds of dollars each year"). These materials are written in simple, clear language for a fourth- to sixth-grade reading level and are sent from the secretary of the Pennsylvania Department of Human Services. Online Appendix Figure A1 shows these standard outreach materials. In the framework of Section II, we think of this intervention as increasing the perceived benefits from applying ($d\varepsilon$).

The assistance component begins if, in response to these outreach materials, the person calls the BDT number. BDT then provides assistance with the application process. This includes asking questions so that BDT staff can populate an application and submit it on their behalf, advising on what documents the person needs to submit, offering to review and submit documents on their behalf, and assisting with postsubmission requests or questions from the state regarding the application. BDT also tries to ensure that the individual receives the maximum benefit for which they are eligible by collecting detailed information on income and expenses (the latter contributing to potential deductions). In the framework of Section II, we think of this intervention as reducing the private costs of applying ($-d\bar{\Lambda}$).

Data from our intervention indicate that BDT submitted about 70% of applications made by individuals in the Information Plus Assistance intervention and provided their full set of services (including document review) for about two-thirds of the applications it submitted.[6] For callers who end up applying, BDT spends on average 47 minutes on the phone with them; for callers who end up not applying, the average phone time is about 30 minutes.

2. Information Only. Our Information Only intervention contains only the letters and follow-up postcards to nonrespondents sent as part of the outreach materials. They are designed to be as similar as possible to the information content of the Information Plus Assistance intervention: both are sent from the secretary of the Pennsylvania Department of Human Services (DHS) and include virtually identical language and layout. Some

6. As we will see in the results, given that we estimate that about one-third of applicants are always takers, this suggests that BDT submits applications for the vast majority of compliers and provides their full set of services for about three-quarters of these compliers.

minor differences were naturally unavoidable. In particular, the Information Plus Assistance materials direct individuals to call the PA Benefits Center (the local name of BDT), while the Information Only materials direct them to call the Department of Human Services ("Please call the Department of Human Services today. It could save you hundreds of dollars each year"). In addition, the hours of operation for DHS (8:45am–4:45pm) listed on the Information Only outreach materials differed slightly from the BDT hours (9:00am–5:00pm) listed on the Information Plus Assistance outreach materials. Online Appendix Figure A2 shows the outreach materials in the Information Only arm.

3. Subtreatments. Within each treatment, we created subtreatments in the presentation and frequency with which the information was presented. In practice, most of these subtreatments had little or no impact, and therefore in most of our analysis we pool them. However, we also present results from the one subtreatment where we found substantial effects: the elimination of the postcard follow-up in the standard Information Only intervention.

IV.B. Study Population

Our study population consists of individuals aged 60 and older who are enrolled in Medicaid but not SNAP. They are considered likely income eligible for SNAP based on their enrollment (and hence eligibility) for Medicaid. This is, of course, an imperfect proxy of SNAP eligibility. This is by necessity; as described in detail in Online Appendix B, exact assessment of SNAP eligibility requires nonincome information that must be actively supplied on an application; eligibility cannot be passively determined through existing administrative data.

Our study population thus consists of individuals already enrolled in at least one public benefit program: Medicaid. This is a particular subset of people eligible for but not enrolled in SNAP. For example, our analysis in the pooled 2010–2015 Consumer Expenditure Survey (CEX) suggests that only about 20% of individuals aged 60 and over who are not enrolled in SNAP but have income less than 200% of FPL (a rough proxy for potential SNAP eligibility) are enrolled in Medicaid. Caution is always warranted in generalizing findings beyond the specific study population. In this particular case, one might be concerned that enrollment in another public benefit program could be indicative of the study population's general knowledge about benefit eligibility or interest and ability to sign up for government services. This particular issue, however, may not be a major concern. Many individuals do not actively choose to enroll in Medicaid themselves but are enrolled in Medicaid by social workers at hospitals when they arrive uninsured and ill—a fact that has led researchers to refer to many of those eligible for Medicaid but not currently enrolled as "conditionally covered" (Cutler and Gruber 1996).

A benefit of using Medicaid enrollment as a proxy for likely eligibility is that we can use their Medicaid data to measure healthcare utilization and health in 2015, the year prior to the intervention. Since only about three-quarters of our study population were enrolled in Medicaid for the entirety of 2015, we annualized all of the healthcare utilization measures by dividing by the number of days enrolled out of 365. This is an imperfect approach, because utilization during a partial coverage year may be disproportionately higher (or lower) than it would be if coverage existed for the full year. However, we are not unduly concerned given that this adjustment will affect enrollees in randomly assigned arms equivalently, and we confirm this in sensitivity analysis.

1. Summary Statistics. To construct the study population, DHS supplied BDT with a list of approximately 230,000 individuals aged 60 and older who were enrolled in Medicaid as of October 31, 2015; DHS also merged on a flag for whether the individual was currently enrolled in SNAP. Table I illustrates the construction of our study population and the prerandomization characteristics of the sample. Column (1) shows the initial outreach list of 229,584 individuals aged 60 and over enrolled in Medicaid as of October 31, 2015. In column (2) we exclude individuals enrolled in the Long-Term Care Medicaid program ($N = 47{,}729$)—because they almost always have meals provided and are therefore not eligible for SNAP—and individuals with an address in Philadelphia ($N = 37{,}932$), since they were subject to prior outreach efforts by BDT. Of the remaining individuals, column (3) shows characteristics for the 60% who were enrolled in SNAP or living with someone enrolled in SNAP, and column (4) shows characteristics for the 40% ($N = 59{,}885$) who were not enrolled in SNAP and not living with anyone in SNAP; recall that SNAP is a household-level benefit. Our final study population, shown in column (5) ($N = 31{,}188$) is a subset of column (4). From column (4), we randomly select one person from each household (this excludes 1,842 individuals),

TABLE I
DESCRIPTION OF STUDY POPULATION

		After exclusions			
	Original outreach list (1)	List, after exclusions (2)	Receving SNAP (3)	Not receiving SNAP (4)	Study population (5)
Observations (N)	229,584	143,923	84,038	59,885	31,888
Panel A: Demographics					
Age (as of October 31, 2015)	72.91	70.45	69.77	71.42	68.83
Share age above median = 65	0.72	0.66	0.66	0.66	0.50
Share age 80+	0.27	0.18	0.15	0.23	0.16
Male	0.35	0.36	0.36	0.36	0.38
Share white[a]	0.71	0.79	0.79	0.79	0.75
Share black[a]	0.17	0.10	0.11	0.07	0.08
Share primary language not English	0.04	0.03	0.03	0.03	0.04
Share living in Philadelphia	0.18	0.00	0.00	0.00	0.00
Share living in Pittsburgh	0.05	0.07	0.07	0.06	0.06
Share last Medicaid spell starting before 2011	0.45	0.47	0.55	0.36	0.33
Share enrolled in Medicaid for 2015 full year	0.83	0.84	0.89	0.77	0.73
Panel B: (Annual) healthcare measures, 2015					
Total healthcare spending ($)[b]	18,347	7,683	6,036	9,995	11,838
Number of hospital days	5.41	1.51	1.24	1.88	2.16
Number of ER visits	0.41	0.41	0.41	0.40	0.50
Number of doctor visits	6.25	5.87	5.97	5.74	7.11
Number of SNF days	66.23	1.57	0.85	2.58	2.67
Number of chronic conditions	6.50	4.93	5.08	4.70	5.45

Notes. Observations correspond to a sample of Medicaid enrollees using data from Pennsylvania Dept. of Human Services (DHS). Column (1) shows the initial outreach list of individuals aged 60 and over enrolled in Medicaid as of October 31, 2015. In column (2) we make two exclusions from this list: all individuals enrolled in the Long-Term Care Medicaid program and individuals with an address in Philadelphia city. Columns (3) and (4) partition the resulting sample in column (2) into those in "households" enrolled in SNAP and those not, respectively, where a household is defined as individuals on the outreach list sharing the same last name and address; recall that SNAP is a household-level benefit. Column (5) shows the final study population, which is a subset of the individuals not enrolled in SNAP in column (4); we excluded all individuals in column (4) to whom BDT had previously sent outreach materials and randomly selected one individual from each household . All data come from Medicaid administrative data; healthcare spending and utilization data come from the 2015 Medicaid claims files and all measures are annualized for individuals with less than a full year of Medicaid enrollment; see Online Appendix D for more details.
[a]Omitted category is other or missing race.
[b]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.

and excluded all individuals to whom BDT had previously sent any outreach materials ($N = 26{,}155$).

A comparison of columns (3) and (4) shows no clear demographic gradient between Medicaid enrollees who do and do not enroll in SNAP. Those not on SNAP (column (4)) are older, with similar gender, racial, and language makeup to those on SNAP (column (3)). On some dimensions those not on SNAP (column (4)) appear sicker—they have more hospital days and skilled nursing facility (SNF) days—than those on SNAP (column (3)) but on other dimensions they appear less sick, such as fewer chronic conditions. One notable difference is that those not on SNAP have been on Medicaid for less time (i.e., only one-third had their last enrollment spell starting before 2011, compared to about one-half of those on SNAP).

IV.C. Randomization

We randomly assigned the 31,888 individuals in our study population to one of three equally sized groups: Information Only treatment, Information Plus Assistance treatment, and control (no intervention). There were separate subtreatments within each treatment: one-quarter of each treatment was randomized into an arm with a variant of the outreach letters and postcards designed to attract clients by using a marketing approach that borrowed language and graphics from credit card solicitations; in the Information Plus Assistance treatment the remaining three-quarters received the standard outreach ("standard"); in the Information Only treatment, one-quarter received the standard outreach, and another one-quarter received the standard letter but no follow-up postcard ("no postcard") and another one-quarter received a letter that varied the description of the expected benefit amounts ("framing").

For practical reasons, the outreach letters were randomly distributed across 11 separate, equally sized weekly mailing batches. The first batch was sent on January 6, 2016, and the last on March 16, 2016; follow-up postcards were sent eight weeks after each mailing, with the last postcards scheduled to be sent on May 11, 2016.[7] Online Appendix Figure A4 provides more detail on the timing of the mailings.

We wrote the computer code that assigned individuals to these different treatments and treatment mailing batches by simple random assignment according to the share we wanted in each arm; this code also randomly assigned the control individuals to (non) mailing weekly batches, so that outcomes for all individuals

7. Due to an implementation error, postcards for the January 27 and February 3 batches were not mailed when scheduled and instead were sent on May 26 and May 27, respectively.

in our study can be measured relative to an initial "mail date." Implementation of the code on the actual, identified data was done by our partner BDT, who had access to these data and oversaw the physical mailings. BDT staff also performed a series of quality assurance tests that we programmed to ensure fidelity of the randomization protocol and the quality of the deidentified data that we received. Online Appendix Table A3 shows balance of the characteristics of our study population across the arms, as would be expected based on our randomized design.

All study materials, including letters, postcards, and envelopes, were approved by BDT and DHS before the study was launched. MIT's Institutional Review Board (IRB) approved this research (Protocol: 1506106206; FWA: 00004881).[8] The trial was registered on the AEA RCT Registry (AEA RCTR -0000902) in October 2015, prior to our launch, at which point we prespecified our primary and secondary outcomes. We updated the registry to specify additional detail, such as a nine-month time frame for the outcomes, and to post the more detailed analysis plan in March 2016, prior to receiving any data on applications or enrollment.[9]

IV.D. Outcomes Data

1. Applications, Enrollment, and Benefit Amounts. DHS provided data on SNAP applications from March 2008 through February 2018. The application data also include disposition codes and dates, which enable us to determine if and when the application was approved; we use this to measure enrollment. Our enrollment measure is therefore a flow measure ("was the individual's application approved within n months after the initial mail date") rather than a stock measure of whether the individual

8. Northwestern University's IRB (FWA: 00001549) ceded approval to MIT's IRB through an IRB Authorization Agreement. The IRB of the National Bureau of Economics Research judged the protocol to be exempt (IRB Ref#15_129; FWA: 00003692).

9. Our analysis hews closely to the analysis plan in terms of the take-up outcomes analyzed (calls, applications, and enrollment) and the analysis of enrollee benefits and enrollee and applicant demographic and health characteristics. The exact analysis of study participant characteristics was not fully specified at that point due to uncertainty on data availability. We were unable to execute on our aspirations to analyze additional characteristics like earnings and credit report outcomes due to our inability to obtain the relevant data.

is enrolled as of a given date. We also observe whether and when an application was rejected, as well as the reason for rejection. Our main analysis focuses on application and enrollment within nine months after the mail date. As a result, our outcomes data span the period January 6, 2016 (the date of the first mailing), through December 16, 2016 (nine months after our last mailing). This was chosen to be a sufficiently long window to capture the full impact of the intervention on these outcomes.

DHS also provided us with monthly benefit amounts for enrolled individuals. We measure the monthly benefit amount in months enrolled in the nine months after outreach. The monthly benefit amount will serve as one of the key measures of enrollee characteristics.

2. Call-in Data. BDT tracks all calls it receives, which allows us to measure call-ins to the BDT number in response to the outreach letters in the Information Plus Assistance treatment. To capture comparable information on which people call in to DHS in response to the Information Only treatment, we contracted with a call forwarding service, and the information-only outreach letters provided the toll-free numbers of the call forwarding service, with a different call-in number in each subtreatment arm. Call receptionists were asked to record the individual's unique identification number (printed on the outreach materials) before forwarding the call to DHS. The use of the call forwarding service allows us to measure for each individual in the Information Only treatment whether (and when) they called in response to the outreach. It also allowed BDT to send follow-up postcards to noncallers in the Information Only intervention, as in the Information Plus Assistance intervention.

We have caller data from January 7, 2016, through October 14, 2016. We use these data to measure calls in the seven months after the initial mail date. We report the "raw" call-in rates in each study arm. Because the call forwarding service was not as good at determining the identity of callers as our BDT partner, the Information Only treatment has a nontrivial number of callers without a valid study ID. We therefore also report an "adjusted" call-in rate for the Information Only treatment, which adjusts the measured call-in rate to account for our estimate of the rate of unrecorded callers.

TABLE II

Behavioral Responses to "Information Only" and "Information Plus Assistance"

	Control (1)	Information Only (2)	Information Plus Assistance (3)	p-value of difference (column (2) versus (3)) (4)
SNAP enrollees	0.058	0.105	0.176	
		[.000]	[.000]	[.000]
SNAP applicants	0.077	0.147	0.238	
		[.000]	[.000]	[.000]
SNAP rejections among applicants	0.233	0.266	0.255	
		[.119]	[.202]	[.557]
Callers	0.000	0.267	0.301	
		[.000]	[.000]	[.000]
Adjusted callers	0.000	0.289	0.301	
		[.000]	[.000]	[.156]
SNAP applicants among noncallers	0.077	0.086	0.081	
		[.063]	[.324]	[.363]
SNAP applicants among callers	0.000	0.313	0.602	
		[.000]	[.000]	[.000]
SNAP enrollees among noncallers	0.058	0.061	0.059	
		[.442]	[.713]	[.688]
SNAP enrollees among callers	0.000	0.226	0.450	
		[.000]	[.000]	[.000]
Observations (N)	10,630	5,314	10,629	

Notes. Columns (1)–(3) show means by intervention arm with the p-value relative to the control arm [in square brackets]. Column (1) shows the control. Column (2) shows the Information Only arm (for the two equally sized pooled subtreatments). Column (3) shows the Information Plus Assistance arms (weighted so that the two pooled subtreatments received equal weight). Column (4) reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. All outcomes are binary rates measured during the nine months from the initial mail date. All p-values are based on heteroskedasticity-robust standard errors. Callers are measured for the relevant call number and are therefore mechanically 0 for the control; see text for a description of the adjusted caller rate.

V. Results

Our main analysis compares three groups: the (pooled, equally weighted) "standard" and "marketing" subtreatments in the Information Only arm (5,314), the (pooled, equally weighted) "standard" and "marketing" subtreatments in the Information Plus Assistance arm (10,629), and the control (10,630). In Online Appendix Tables A5, A6, A14, and A15 we present the full set of results separately for each subtreatment; in general these subtreatments had little or no impact, except the "no reminder postcard" subtreatment, which we discuss below.

V.A. *Behavioral Responses to Intervention*

1. Enrollment, Applications, and Calls. Table II presents the main take-up results of the experiment by intervention arm.

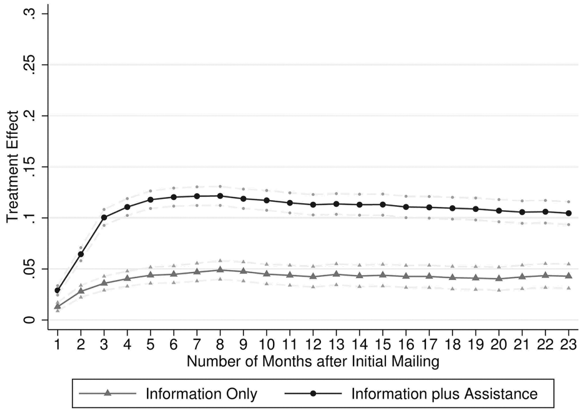

FIGURE I

Time Pattern of Enrollment Responses

Figure shows, by month, the (cumulative) estimated treatment effects on enrollment (relative to the control) for the Information Only arm and the Information Plus Assistance arm. Ninety-five percent confidence intervals on these estimates are shown in the dashed light gray lines.

All outcomes are measured in the nine months after the initial mail date.

The first row shows results for our primary outcome: enrollment within nine months. In the control group, about 6% enroll. The Information Only intervention increases enrollment by 5 percentage points. Information Plus Assistance increases enrollment by 12 percentage points, or 200% relative to the control; the impacts of the intervention are statistically different from the control and from each other ($p < .001$).[10]

Figure I shows the time pattern of the interventions' impacts on enrollment by month through 23 months after intervention,

10. For some perspective on these numbers, we considered how they compared to other take-up interventions, bearing in mind that these were different interventions conducted on different programs and populations. In the context of encouraging low-income high school seniors to apply for aid and attend college, Bettinger et al. (2012) found that providing information about aid eligibility and nearby colleges had no detectable effect, but combining the information with assistance in completing a streamlined application process increased college enrollment by 8 percentage points or about 25% relative to the control. In the context of informing low-income tax filers about their likely eligibility for the EITC, Bhargava and Manoli (2012) found that their average informational outreach increased EITC filing by 22 percentage points (or about 50% above baseline).

which is as long as our current data allow. The time pattern is similar for both interventions: over 85% of the nine-month enrollment effect is present by four months, and the impact has clearly leveled off before nine months (our baseline time window). The impacts of the intervention appear to largely persist, at least through the 23 months we can observe post-intervention; about 90% of the nine-month enrollment effect is present by 23 months. This suggests that the interventions are primarily generating new enrollment, as opposed to merely "moving forward" in time enrollment that would otherwise happen; Online Appendix Figure A7 shows similar monthly patterns for applications and calls.

The next two rows of Table II show that the interventions' impacts on applications are roughly proportional to the increase in enrollment. About 22% of applications in each arm are rejected; differences across arms are substantively and statistically indistinguishable. This suggests that assistance affects enrollment (over and above information alone) primarily by affecting individuals' willingness to apply, rather than by increasing the success (i.e., approval) rate of a given application. This is consistent with other studies that have found that changes in transaction costs have no or small effects on rejection rates of applicants (Alatas et al. 2016; Deshpande and Li forthcoming). Of course, because assistance may also change the composition of applicants (including their latent success probability), it is not possible to directly identify these two separate channels.

In Online Appendix Table A9, we briefly explore the nature of the "reasons" given by DHS for the rejections. Naturally these are not always straightforward to interpret. Nonetheless, it appears that relative to the control, the share of rejections in the Information Plus Assistance arm is higher for reasons that looks like "insufficient interest" on the part of the applicant—for example, withdrew or did not show up for an appointment—and lower for reasons that look like ineligibility after review (e.g., failure to meet citizenship or residency requirement). This is consistent with assistance reducing the error rate on applications, but also pushing marginally motivated individuals to start the application process.

The last six rows of Table II examine call-in rates. A caller is defined as someone calling the number provided on the outreach material; the caller rate is therefore mechanically 0 for those in the control arm.[11] The raw call-in rates are 30% for the Information Plus Assistance outreach letters, and 27% for the Information Only outreach letters; the adjusted caller rate for the Information Only intervention (designed to account for the lower measurement of callers in the Information Only arm as explained in Section IV.D) is 29%, and statistically indistinguishable from the call-in rate for Information Plus Assistance. The similar call-in rate is not surprising given the (deliberate) similarity of the outreach materials (see Online Appendix Figures A1 and A2). It suggests that any difference in applications and enrollment between the Information Only and Information Plus Assistance interventions is attributable to the assistance itself, rather than to the expectation of assistance. Conditional on calling, we find the average caller made 1.8 calls in the Information Plus Assistance arm and 1.6 calls in the Information Only arm (results not shown); these differences are statistically distinguishable ($p < .001$). BDT employees are assigned to callers using a rotation system that is plausibly quasi-random. Using a nonparametric empirical Bayes approach, we found no evidence of statistically significant differences in treatment effects on applications or enrollment across employees. We also found no evidence of differential impact based on employee observable characteristics (see Online Appendix Tables A18 and A19).

The table also shows that the the share of people who apply or enroll without calling is the same in all three arms. This suggests that all marginal applicants affected by the interventions call in response to the outreach materials: such individuals presumably call the state directly (without being routed through BDT or our tracking service) or apply online or in person. Caller rates therefore provide a likely ceiling for the impact of the interventions: less than one-third of individuals appear to notice and respond to the outreach materials. The other 70% probably received the outreach materials, since less than 1% were returned to sender due to bad addresses. It is possible that they did not open or read the materials or did so but were not moved to apply for SNAP benefits. Presumably some of the noncallers are actually ineligible for SNAP, given that some of the applications are rejected due

11. Online Appendix Table A8 shows callers from each intervention arm into each possible call-in number (there was a different number for the Information Plus Assistance arm and for each subtreatment in the Information Only arms). There is virtually no cross-contamination.

to ineligibility; perhaps an even larger share of noncallers believe themselves (potentially correctly) to be ineligible. However, we show below that predicted enrollment is similar for callers and noncallers.

If we interpret calling as a sign of interest, the results show that, conditional on interest, the application rate is twice as high when assistance is provided (about 60%) than when only information is provided (about 30%). Likewise, enrollment rates (conditional on interest) are about 45% when information and assistance is provided compared to 23% when only information is provided.

All of the results shown in Table II are based on comparisons of mean outcomes by intervention arm. No covariates are needed given the simple random assignment. For completeness however, we show in Online Appendix Table A16 that all of the results in Table II are robust to controlling for baseline demographic and health characteristics of the individuals, as well as for the date of their mail batch.

2. Cost-effectiveness Approximation. A rough, back-of-the-envelope calculation suggests that the Information Only intervention was about two-thirds cheaper per additional enrollee than the Information Plus Assistance intervention. Separating out fixed and marginal costs of the intervention is difficult, but BDT has estimated the marginal cost of the Information Plus Assistance intervention at about $7 per person who is sent outreach materials, and the marginal cost of the Information Only treatment was about $1 per person who was sent outreach materials.[12] This suggests that the cost per additional enrollee is $20 in the Information Only treatment, compared with $60 in the Information Plus Assistance treatment. Naturally there are additional costs to the applicants from the time spent applying and to the government from processing applications and paying benefits.

Our results suggest that the state benefits financially from encouraging SNAP take-up, even if it bears the whole intervention cost as well as the processing costs. As we will see below, new

12. The cost of the Information Only intervention is primarily composed of the cost of mailing a first-class letter ($0.49 at the time of our intervention) plus the cost of the follow-up postcard ($0.34 at the time of our intervention), plus the costs of printing and assembling the mailings. The higher costs for the Information Plus Assistance intervention reflect the additional labor costs of the BDT staff who provide the assistance.

enrollees receive, on average, about $1,300 a year in annual SNAP benefits. This is paid for by the federal government. Isaacs (2008) estimated that the annualized administrative costs of the SNAP program (including certification costs and subsequent administrative costs) are about $178 per recipient, or about $134 per application given our estimate of a 75% acceptance rate; this is paid for by the state government. Thus, were the state to finance the marginal costs of either the Information Only intervention ($20 per enrollee) or the Information Plus Assistance intervention that BDT currently undertakes ($60 per enrollee) and the administrative costs of processing the applications, these would still be less than 25% of the new federal benefits received by state residents, presumably spent largely at local retail outlets. Interestingly, this conclusion would be different if virtually all of new enrollees received the minimum benefit ($16 a month or $192 a year); this would be similar to the state's average administrative costs per recipient. Additionally, because a meaningful share of the administrative costs come from the costs of processing applications, a different intervention that generated many applications—but few enrollments—would also not pass a simple cost-benefit test.

3. Effects of Reminders. Table III shows results for two subtreatments of the Information Only intervention: the "standard" treatment, which includes an initial letter and a reminder postcard eight weeks later if the individual has not yet called in (see Online Appendix Figure A2), and a "no reminder postcard" subtreatment in which the follow-up postcard is not sent.[13] Reminders matter: all behavioral responses decrease by about 20% without the reminder postcard. Specifically, the "standard" Information Only treatment (with the reminder postcard) had a 30% call rate, a 15% application rate and an 11% enrollment rate. The lack of a postcard reminder reduced the caller rate by 7 percentage points ($p < .001$), the application rate by 3 percentage points ($p = .001$), and the enrollment rate by 2 percentage points ($p = .016$). Given the 2 percentage point increase in enrollment with

13. The results for the Information Only treatment results shown in Table II pool the results from the standard treatment and a "marketing" subtreatment that varied the content of the outreach letters (see Online Appendix Figure A5 for more details); these two subtreatments are pooled in the same proportions in the Information Plus Assistance treatment results shown in Table II.

TABLE III

BEHAVIORAL RESPONSES TO INFORMATION ONLY INTERVENTION WITH AND WITHOUT REMINDERS

	Control (1)	Information Only standard (2)	Information Only no-postcard (3)	p-value of difference (column (2) versus (3)) (4)
SNAP enrollees	0.058	0.112	0.092	
		[.000]	[.000]	[.016]
SNAP applicants	0.077	0.151	0.120	
		[.000]	[.000]	[.001]
SNAP rejections among applicants	0.233	0.224	0.216	
		[.751]	[.536]	[.777]
Callers	0.000	0.278	0.212	
		[.000]	[.000]	[.000]
Adjusted callers	0.000	0.300	0.234	
		[.000]	[.000]	[.000]
SNAP applicants among noncallers	0.077	0.089	0.074	
		[.079]	[.593]	[.071]
SNAP applicants among callers	0.000	0.311	0.295	
		[.000]	[.000]	[.524]
SNAP enrollees among noncallers	0.058	0.064	0.054	
		[.284]	[.492]	[.172]
SNAP enrollees among callers	0.000	0.237	0.234	
		[.000]	[.000]	[.921]
Observations (N)	10,630	2,657	2,658	

Notes. Columns (1)–(3) show means by intervention arm with the p-value relative to the control arm [in square brackets]. Column (1) shows the control. Column (2) shows the standard Information Only intervention (see Online Appendix Figure A2; this standard intervention is half of the sample shown in Table II, column (3) for the pooled Information Only analysis). Column (3) shows the results of the Information Only intervention without the reminder postcard; the outreach materials are otherwise identical to those in Online Appendix Figure A2. Column (4) reports the p-value of the difference between the standard Information Only intervention and the Information Only intervention without the reminder postcard. All outcomes are binary rates measured during the nine months from the initial mail date. All p-values are based on heteroskedasticity-robust standard errors. Callers are measured for the relevant call number and are therefore mechanically 0 for the control; see text for a description of the adjusted caller rate.

the reminder postcard, and its marginal cost of roughly \$0.35, cost per additional enrollee is similar with and without the reminder postcard.

The nontrivial impact of a reminder postcard is similar to Bhargava and Manoli's (2012) finding that a similar second reminder letter, sent just months after the first, increased EITC take-up. They interpret the effect of the reminder as operating by combating low program awareness, inattention, or forgetfulness. A similar interpretation seems warranted in our context, where we estimate that less than 3% of our study population had applied for or enrolled in SNAP in the 10 years prior to our intervention. In addition, surveys suggest that about half of likely eligible nonparticipants in SNAP reported that they were not aware of their eligibility (Bartlett, Burstein, and Hamilton 2004). In our framework in Section II, this is modeled as underestimating the benefits of applying (i.e., $\varepsilon < 0$).

V.B. Characteristics of Marginal Applicants and Enrollees

To examine the characteristics of the marginal applicant or enrollee whose behavior is affected by the intervention, we define the outcome in each arm to be the average of a specific characteristic among those who apply or enroll. For example, we compare the average monthly benefits among those who enroll in each arm. Differences in the average characteristics of enrollees or applicants in a given treatment arm relative to the control group reveal how the characteristics of the marginal individual who applies or enrolls due to a given intervention differ from the average applicant or enrollee who would enroll without the intervention. This approach to analyzing the characteristics of the marginal person affected by an intervention is analogous to approaches taken in prior work by Gruber, Levine, and Staiger (1999) and Einav, Finkelstein, and Cullen (2010).

The results suggest that marginal applicants and enrollees in either intervention arm are less needy than the average applicants and enrollees who apply in the absence of the intervention. For brevity, we focus the discussion on a comparison of characteristics of enrollees in the control group relative to enrollees in either intervention. The tables show that characteristics tend to be similar between the two intervention arms, and, within each intervention arm, between applicants and enrollees. However, callers and noncallers look quite different.

1. Monthly Benefits among Enrollees. Table IV shows monthly benefits for individuals who enrolled in the nine months after the initial mail date, by study arm. Because the SNAP benefit formula provides lower benefits to those with higher net income, a lower benefit amount implies an enrollee with higher net resources. Average monthly benefits are 20% to 30% lower for enrollees in either intervention than for control enrollees. Average monthly benefits are \$146 in the control compared with \$115 in the Information Only intervention and \$101 in the Information Plus Assistance intervention; average benefits in each

TABLE IV
ENROLLEE MONTHLY BENEFITS AND PREDICTED BENEFITS

	Control (1)	Information Only (2)	Information Plus Assistance (3)	p-value of difference (column (2) versus (3)) (4)
Benefit amount ($)	145.94	115.38	101.32	
		[.000]	[.000]	[.013]
Share $16 benefit	0.192	0.312	0.367	
		[.000]	[.000]	[.021]
Share $194 benefit	0.206	0.164	0.147	
		[.076]	[.003]	[.352]
Share $357 benefit	0.060	0.052	0.040	
		[.587]	[.077]	[.259]
Share missing benefit	0.073	0.043	0.028	
		[.025]	[.000]	[.139]
Predicted benefit for enrollees w/ actual benefit	140.20	112.49	102.93	
		[.000]	[.000]	[.086]
Predicted benefit for all enrollees	138.65	114.01	104.03	
		[.000]	[.000]	[.068]
Share of enrollees in household size of 1	0.657	0.714	0.760	
		[.038]	[.000]	[.036]
Benefit amount for enrollees in household size of 1	116.97	93.35	85.82	
		[.000]	[.000]	[.134]
Observations (N)	613	559	1,861	

Notes. Sample is individuals who enrolled in the nine months after their initial mailing. Columns (1)–(3) show means by intervention arm with the p-value relative to the control arm [in square brackets] for SNAP enrollees. Column (1) shows the control. Column (2) shows the Information Only arm (with the two equally sized subtreatments pooled). Column (3) shows the Information Plus Assistance arms (weighted so that the two pooled subtreatments received equal weight). Column (4) reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. See text for a description of the predicted benefits. All p-values are based on heteroskedasticity-robust standard errors. N reports the sample size of enrollees.

intervention arm are statistically different from those in the control ($p < .001$) as well as from each other ($p = .013$).[14]

14. Differences in the average characteristics of enrollees in an intervention arm relative to the control arm reflect differences between the average characteristics of inframarginal enrollees (or "always takers") relative to marginal enrollees (or "compliers"). As another way of presenting the same information, Online Appendix Table A7 reports the average characteristics for always takers and compliers; estimation of these objects is straightforward (see, e.g., Abadie 2002, 2003; Angrist and Pischke 2009). Note that comparing average characteristics of enrollees across treatment arms mixes both differences in average characteristics of compliers as well as the complier share of enrollees. In our case, the fact that average benefits in each intervention arm are statistically different from each other is virtually all driven by differences in complier share (rather than differences in average characteristics for compliers in each arm). This is shown in Online Appendix Table A7, which reports similar complier means across the two arms.

There are clear modes in the distribution of benefits received, corresponding to minimum and maximum benefit amounts. Among the controls, 18% receive $16 (the minimum monthly benefit for a household of size 1 or 2 who are categorically eligible) and another 19% receive $194 (the maximum monthly benefit for a household of size 1); see also Online Appendix Figure A8. Table IV shows that the interventions increased the share of enrollees receiving the minimum benefit and decreased the share of enrollees receiving the maximum benefit.

We explored two potential concerns with these results. First, we are missing benefit information for about 4% of enrollees, presumably due to data errors. Importantly, Table IV indicates that this missing rate is not balanced across arms. Such nonrandom attrition could bias our comparison of enrollee benefits across arms; however, we show in Online Appendix E that the differences in benefits across the arms is robust to using the fairly conservative procedure of Lee (2009) to bound the potential bias arising from differential missing benefit rates. We also generated a predicted benefit measure in which we predict the benefit amounts based on the relationship between benefits and the prerandomization demographic and health characteristics shown in Table I; Online Appendix E provides more detail on the prediction algorithm which follows a standard algorithm in machine learning (Rifkin and Klautau 2004). Table IV shows that predicted benefits show the same pattern across arms as actual benefits, among enrollees with nonmissing benefit amounts (second to last row) and among all enrollees (last row).

Second, benefits increase in household size. If the interventions disproportionately encourage smaller households to apply, this will lower enrollee benefits without necessarily reflecting higher per capita resources. Indeed, the penultimate row of Table IV shows that the interventions increase the share of enrollees in a household size of 1. However, the bottom row of Table IV shows that if we limit our analysis to households of size 1, average benefits for these households are still statistically significantly lower in each intervention arm relative to the control. An additional attraction of limiting to households with only a single individual is that we have essentially no missing benefits for such households.

2. Demographics and Health of Applicants and Enrollees. Table V shows the demographic and health characteristics of applicants and enrollees. On a variety of dimensions, marginal applicants and enrollees from the intervention appear less needy than the average applicant or enrollee in the control group. Panel A shows that applicants and enrollees in either intervention have lower predicted benefits (i.e., have higher predicted net resources) than applicants in the control arm ($p < .001$).

Table V, Panel B shows results for health and healthcare, measured in the calendar year prior to the intervention. We measure healthcare utilization in three different ways: total medical spending, total number of visits or days (summed across emergency room (ER) visits, doctor visits, hospital days, and skilled nursing facility (SNF) days), and weighted number of visits or days, where the weights are set based on the average cost per encounter.[15] Total medical spending is noisy due to the well-known high variance of medical spending and conflates variation in utilization with variation in recorded prices. Our total number of days or visits measures attempt to circumvent these problems by creating a utilization-based measure. The weighted utilization measure is designed to account for the fact that a hospital day is substantially more expensive than an SNF day or a doctor visit. For all three measures, applicants and enrollees in the intervention arms use less healthcare before randomization than those in the control arm, although these differences are not always statistically different from the control.[16]

The final row of Table V, Panel B shows that the number of measured chronic conditions is also lower in both intervention arms relative to the control arm for applicants and enrollees, with most of these differences statistically significant at conventional

15. Specifically, we sum up the total number of encounters of a given type and the total spending on those encounters across our study population and divide total spending by total encounters to get a per encounter average "cost." The results are: \$1,607 for a hospital day, \$197 for an ED visit, \$147 for an SNF day, and \$79 for a doctor visit.

16. As discussed already, many of these health measures are annualized to account for the fact that not everyone was enrolled in Medicaid for the full year in 2015. The share enrolled for the full year is (as expected) balanced across control and intervention arms (see Online Appendix Table A3). Therefore, not surprisingly, we find in Online Appendix Table A17 that if we limit the analysis to the subset of study participants enrolled in Medicaid for the full year in 2015, the results remain qualitatively the same (although precision worsens).

TABLE V

DEMOGRAPHIC AND HEALTH CHARACTERISTICS: APPLICANTS AND ENROLLEES

	Applicants				Enrollees			
	Means			p-value	Means			p-value
	Control (1)	Info Only (2)	Info Plus Assistance (3)	Info Plus Assistance versus Info Only (4)	Control (5)	Info Only (6)	Info Plus Assistance (7)	Info Plus Assistance versus Info Only (8)
Panel A: Predicted benefits								
Predicted benefits	148.26	125.65	115.36		138.65	114.01	104.03	
		[.000]	[.000]	[.037]		[.000]	[.000]	[.068]
Panel B: (Annual) healthcare measures, 2015								
Total healthcare	9,424	8,605	8,334		10,238	9,532	8,603	
spending ($)[a]		[.517]	[.300]	[.781]		[.661]	[.208]	[.459]
Total number of visits	13.33	11.67	9.92		14.79	10.90	9.92	
and days		[.331]	[.018]	[.166]		[.058]	[.008]	[.467]
Weighted total number of	4,661	3,273	2,818		5,407	3,288	2,779	
visits and days		[.128]	[.022]	[.442]		[.064]	[.011]	[.461]
Number of chronic	6.21	5.55	5.27		6.54	5.43	5.37	
conditions		[.094]	[.006]	[.383]		[.019]	[.005]	[.875]
Panel C: Demographics								
Share age above	0.41	0.46	0.46		0.39	0.43	0.46	
median = 65		[.072]	[.014]	[.764]		[.282]	[.006]	[.159]
Share age 80+	0.06	0.11	0.14		0.07	0.12	0.14	
		[.001]	[.000]	[.042]		[.005]	[.000]	[.085]
Male	0.41	0.40	0.38		0.39	0.42	0.38	
		[.983]	[.232]	[.250]		[.446]	[.444]	[.104]

TABLE V
CONTINUED

	Applicants				Enrollees			
	Means			p-value Info Plus Assistance versus Info Only	Means			p-value Info Plus Assistance versus Info Only
	Control (1)	Info Only (2)	Info Plus Assistance (3)	(4)	Control (5)	Info Only (6)	Info Plus Assistance (7)	(8)
Share white[b]	0.67	0.73	0.74		0.71	0.78	0.78	
		[.005]	[.000]	[.554]		[.004]	[.001]	[.958]
Share black[b]	0.10	0.08	0.11		0.11	0.07	0.10	
		[.103]	[.577]	[.011]		[.011]	[.833]	[.004]
Share primary language not English	0.08	0.06	0.04		0.06	0.05	0.03	
		[.141]	[.000]	[.012]		[.242]	[.002]	[.067]
Share living in Pittsburgh	0.05	0.06	0.07		0.05	0.06	0.07	
		[.385]	[.066]	[.459]		[.374]	[.028]	[.310]
Share last Medicaid spell starting before 2011	0.25	0.30	0.29		0.26	0.33	0.31	
		[.022]	[.017]	[.704]		[.009]	[.026]	[.348]
Observations (N)	817	781	2,519		613	559	1,861	

Notes. Columns (1)–(3) and (5)–(7) show means by intervention arm with the p-value relative to the control arm [in square brackets] for SNAP applicants who applied within nine months of their initial mailing, and SNAP enrollees who enrolled within nine months of their initial mailing, respectively. Columns (1) and (5) show the control. Columns (2) and (6) show the Information Only arms (with the two equally sized subtreatments pooled); columns (3) and (7) show the Information Plus Assistance arms (weighted so that the two pooled subtreatments received equal weight). Columns (4) and (8) report the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. All p-values are based on heteroskedasticity-robust standard errors.

[a]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.

[b]Omitted category is other or missing race.

levels. A smaller number of chronic conditions could reflect better underlying health. It could also—partly or entirely—reflect lower healthcare utilization, since chronic conditions are only measured if the individuals use the relevant healthcare (Song et al. 2010; Finkelstein, Gentzkow, and Williams 2016).

Table V, Panel C reports demographic characteristics. Relative to the control group, applicants and enrollees in either intervention arm are statistically significantly ($p < .001$) older, more likely to be white, and more likely to have their primary language be English. For example, 71% of control enrollees are white, compared with 78% in either intervention arm. In general, these results suggest that (consistent with the results for benefit amounts and health) the socioeconomic status of marginal enrollees is higher than inframarginal enrollees; one exception, however, is age because, among the elderly, older individuals tend to have higher poverty rates. Of course, as emphasized by the conceptual framework in Section II, the observable socioeconomic characteristics of those targeted by the intervention are neither necessary nor sufficient for normative analysis, a point we return to when we explore the normative implications of our findings in Section VI.

3. Out-of-Sample Implications: Noncaller Characteristics. Both interventions attracted enrollees who looked less needy than control enrollees on a variety of dimensions. Of course, other types of interventions might attract different enrollees. The interventions studied here required that an individual open and read mailed communications and then decide to call the help line.

Table VI shows characteristics separately for callers and noncallers (pooled across interventions; the characteristics of callers look similar across the two interventions, as shown in Online Appendix Table A13). The 70% of individuals who did not call in response to our intervention look more needy on all dimensions: they have higher predicted benefits, higher healthcare spending and use, and more chronic conditions.[17] Consistent with this, Online Appendix Tables A11 and A12 show that never takers are worse off on these dimensions than always takers, who in turn are worse off than compliers.

17. Although interestingly they have similar predicted enrollment, suggesting that the decision to call is not informed by expected eligibility. Online Appendix E provides more detail on how we calculate predicted enrollment.

TABLE VI

DEMOGRAPHIC AND HEALTH CHARACTERISTICS: CALLERS AND NONCALLERS

	Callers (1)	Noncallers (2)	p-value of difference (3)
Panel A: Predicted benefits			
Predicted benefits	106.99	114.68	[.000]
Predicted enrollment	0.05	0.05	[.752]
Panel B: (Annual) Healthcare measures, 2015			
Total healthcare spending ($)[a]	7,316	13,656	[.000]
Total number of visits and days	9.52	13.50	[.000]
Weighted total number of visits and days	2,853	5,064	[.000]
Number of chronic conditions	5.16	5.48	[.024]
Panel C: Demographics			
Share age 80+	0.16	0.17	[.190]
Male	0.38	0.38	[.977]
Share white[b]	0.77	0.74	[.000]
Share black[b]	0.09	0.07	[.006]
Share primary language not English	0.03	0.05	[.000]
Share living in Pittsburgh	0.06	0.06	[.658]
Share last Medicaid spell starting before 2011	0.32	0.34	[.044]
Observations (N)	4,597	11,346	

Notes. Sample is those in the Information Only and Information Plus Assistance Intervention analyzed in Table II. Callers from Information Plus Assistance arms are weighted so that the two pooled subtreatments received equal weight. Column (1) shows means for callers (defined without any adjustment), and column (2) shows means for noncallers. Column (3) reports the p-value of the difference between callers and noncallers. All p-values are based on heteroskedasticity-robust standard errors.

[a]Total spending is truncated at twice 99.5th percentile of study population, which is 371,620 (99.5th percentile in study population is 185,810). Amounts greater than the threshold are set to missing.

[b]Omitted category is other or missing race.

This suggests that there may indeed be a sizable mass of individuals who, consistent with behavioral theories, are high need but deterred from enrolling. Our interventions do not, however, appear to affect their behavior. An open question is whether there are other interventions that would.

VI. NORMATIVE IMPLICATIONS

As noted in the introduction, there is an active empirical literature studying take-up and targeting in other programs. We discuss this literaure in more detail in Online Appendix F. Importantly, it has been primarily descriptive, examining whether interventions designed to increase enrollment tend to attract individuals who are observably worse off than those who would enroll in the absence of the intervention.

However, our framework in Section II emphasized that there is no general relationship between targeting on observables and the normative implications of the interventions. It also provided additional conditions that need to be examined empirically for an intervention's targeting properties to yield normative implications. We now demonstrate how this framework can be implemented in the context of our specific intervention and empirical results to assess their normative implications. We suspect it could be used more broadly for normative analysis of other information and assistance interventions, as well as normative analysis of other interventions, such as shorter SNAP recertification periods (Kabbani and Wilde 2003) or online SNAP recertification tools (Gray 2018).

VI.A. Conceptual Framework Mapped to Our Context

We tailor the framework from Section II in two minor ways to apply it to our empirical setting. First, to facilitate our subsequent calibration, we allow for an exogenous probability π_j that the application is accepted. Ex ante uncertainty about acceptance comes from several potential sources, including uncertainty about eligibility rules and the potential for implementation errors (by the individual or the government) in the application process. Second, we allow for two different benefit levels: individuals may receive either $\overline{B}$ or B_{min}, with $\overline{B} > B_{min}$. In practice, as seen in Table IV, a mass of individuals with sufficiently high net resources receive the minimum benefit B_{min}, and others with lower net resources receive higher benefits (which for simplicity we average together).

In addition, given the partial equilibrium nature of the intervention and the elderly study population, we assume that earnings do not respond endogenously to our intervention, although of course in nonelderly populations the evidence suggests that SNAP may well affect labor supply (Hoynes and Schanzenbach 2012). This does not constrain the fiscal externalities from the intervention because, as discussed in Section II, the framework and propositions developed apply generally to any fiscal externality. Importantly, however, without endogenous earnings, the level of benefits that individuals receive is determined by their type, with low-ability types receiving higher benefits $\overline{B}$ and high-ability types receiving the minimum level of benefits B_{min}. This suggests a natural empirical definition of targeting based on the level of benefits received: $e = \frac{E_L}{E_L+E_H}$. We thus interpret benefit level as a proxy for type in our setting. Our empirical results therefore indicate that both interventions decrease targeting (i.e., $\frac{de}{dT} < 0$).

With these modifications, we can restate Propositions 1 and 2 as follows:

PROPOSITION 1A. *The effect of the Information Only treatment on welfare is given by*:

$$\frac{dW}{dT}^{\textit{Information Only}} = \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT}}_{\text{Change in Private Welfare}} - \underbrace{\left[(\pi_H B_{min})\frac{dA_H}{dT} + (\pi_L \overline{B})\frac{dA_L}{dT}\right]}_{\text{Change in Mechanical Program Costs}} + \underbrace{\left[[G_L^A - G_L^{\neg A}]\frac{dA_L}{dT} + [G_H^A - G_H^{\neg A}]\frac{dA_H}{dT}\right]}_{\text{Change in Fiscal Externality}}. \tag{4}$$

The effect of the Information Plus Assistance treatment on welfare is given by:

$$\frac{dW}{dT}^{\textit{Info. + Assistance}} = \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT} + \kappa_L A_L + \kappa_H A_H}_{\text{Change in Private Welfare}} - \underbrace{\left[(\pi_H B_{min})\frac{dA_H}{dT} + (\pi_L \overline{B})\frac{dA_L}{dT}\right]}_{\text{Change in Mechanical Program Costs}} + \underbrace{\left[(G_L^A - G_L^{\neg A})\frac{dA_L}{dT} + (G_H^A - G_H^{\neg A})\frac{dA_H}{dT}\right]}_{\text{Change in Fiscal Externality}}. \tag{5}$$

Proof. See Online Appendix A.3

PROPOSITION 2A. *Holding constant the change in applications due to an intervention, the change in social welfare in response to an improvement in targeting* $\left(\frac{de}{dT} > 0\right)$ *from an Information Only (or Information Plus Assistance) treatment is given by the following expression*:

$$\frac{\partial}{\left(\frac{de}{dT}\right)}\left(\frac{dW}{dT}\right)\bigg|_{\frac{dA}{dT}} = \Big[(\mu_L - \mu_H) - (\pi_L \overline{B} - \pi_H B_{min}) + \left(G_L^A - G_L^{\neg A}\right) - \left(G_H^A - G_H^{\neg A}\right)\Big] * \Gamma. \tag{6}$$

where $\Gamma = \frac{(E_L+E_H)^2}{\pi_L E_L + \pi_H E_H} > 0.$

Proof. See Online Appendix A.3

Note that although we interpret our two interventions in the context of our framework as "information only" ($d\varepsilon$) and "information plus assistance" ($d\varepsilon$, $-d\bar{\Lambda}$), in practice the distinction between "information" and "assistance" is not always clear. Offering assistance may cause individuals to update their beliefs about the probability of acceptance; the Information Only intervention may reduce the costs of applying by highlighting the number to call to apply. For our purposes, this is not a critical distinction. As our welfare framework clarifies, the distinction between the two interventions is only relevant insofar as assistance interventions also reduce costs for inframarginal applicants (see Proposition 1a); our calibrations below make clear that this reduction in costs for inframarginal applicants is not what is driving our normative results.

VI.B. Parameterizing the Model

We use the statutory minimum benefit level for B_{min} (\$16 a month) and set $\overline{B}$ to \$178 a month (the mean benefit for the approximately 80% of control group enrollees who do not receive the minimum). As described already, we assume these two benefit levels correspond to the H and L types, respectively, in the model. These assumptions imply that type L enrollees receive \$6,408 during the first 36 months of enrollment, and type H enrollees receive \$576 over 36 months. After 36 months, individuals must recertify their eligibility; average lifetime benefits are therefore presumably greater than the 36-month amount but may not extend indefinitely; moreover, additional private costs must be incurred to maintain them. For simplicity, we assume benefits last only 36 months; this is a conservative assumption since, as we will see, higher expected benefits among enrollees imply larger misperceptions about the probability of successfully enrolling.

We assume that the probability an application is approved is 0.75 for both types (the empirical acceptance rate for the control group in Table II). Thus, expected benefits conditional on applying ($\pi_j B_j$) are \$4,806 for the L types and \$432 for H types. This calculation assumes that SNAP benefits are valued dollar-for-dollar by recipients.[18] We assume the fiscal externalities from applying come entirely from the public costs of processing applications and are constant across type; in other words, $G_L^A = G_H^A \equiv -g$, and $G_L^{\neg A} = G_H^{\neg A} = 0$. Using Isaacs (2008), we estimate $g \sim \$267$ (see Section V.A).

In the neoclassical benchmark case ($\varepsilon_j = 0$ and thus $\mu_j = 0$ for $j \in \{H, L\}$), an improvement in targeting does nothing for private welfare (due to the envelope theorem). Given that benefits decline with net income ($\overline{B} > B_{min}$) and we have assumed constant fiscal externalities across types, an improvement in targeting in the neoclassical benchmark reduces social welfare. This is the exact opposite of the standard intuition that social welfare increases from an intervention that increases targeting on observables that are correlated with the marginal utility of consumption. Another way of interpreting this result is that with constant fiscal externalities across types (which does not occur in a model with endogeneous labor supply) and with rational beliefs, the “folk wisdom” regarding the mapping from the targeting properties of interventions to social welfare does not apply.

Of course, a key factor in normative analysis is whether the neoclassical benchmark is a reasonable assumption. It is difficult to definitively reject it. Given that applying takes an estimated five hours (Ponza et al. 1999), if we (generously) assume the value of time for this low-income elderly population is roughly twice the minimum wage of \$7.25 a hour, this implies the private (time) cost of applying is about \$75. With no misperceptions, rationalizing the decision not to apply therefore requires a nontime cost of applying of roughly \$4,700 for an L type. If we model stigma as a participation cost (Moffitt 1983), one way to rationalize

18. While Hastings and Shapiro (2018) call into question the standard assumption that SNAP benefits are fungible with cash for a large majority of SNAP-eligible households, it is not immediately clear whether this implies that SNAP benefits are valued more or less than cash at the margin.

the decision of nonapplicants is to say that they experience stigma costs of participation that are about 60 times larger than their transactional costs of applying. For an H type with no misperception of the probability an application is accepted, the implied nontime cost of applying is roughly \$350.

However, our reading of the evidence suggests that individuals underestimate the probability their application is accepted (i.e., $\varepsilon < 0$) and hence expected benefits from applying. As noted previously, existing survey evidence suggests that lack of awareness of expected benefits, (e.g., underestimating expected benefits) is a primary barrier to participation among eligible nonparticipants (Bartlett, Burstein, and Hamilton 2004); one interpretation of our Information Only intervention is that it reduces such misperceptions. In addition, the substantial increase in applications and enrollment from a reminder postcard in the Information Only intervention suggests some form of inattention, lack of awareness, or forgetfulness; that is, individual application decisions may not be privately optimal, as implied by the neoclassical benchmark.

To calibrate the magnitude of the misperceptions, we assume that the time cost is the only cost of application. We use a first-order Taylor approximation to calculate the expected utility of applying, which ignores the role of risk aversion in the application decision (we relax this below). With these assumptions, rationalizing nonparticipation with the time cost estimates above requires $\varepsilon_L = -0.98$ and $\varepsilon_H = -0.83$. Thus, $\varepsilon_L < \varepsilon_H < 0$, and for a type L individual with a 75% chance of enrolling after applying, the only way to rationalize their not applying for benefits is that their misperceptions are so great that they perceive virtually no chance (less than 2%) of enrolling in the program, or alternatively that they are completely ignorant of the program. This calibration that misperceptions are larger in magnitude for the low type is consistent with the hypotheses of the behavioral literature, as well as our finding that the 70% of people who did not respond at all to our interventions look more needy than enrollees on many dimensions; interestingly, however, our particular interventions seem to have attracted relatively less needy people than those who already enrolled.

VI.C. Normative Findings

Proposition 2a indicates that with $\varepsilon_L < \varepsilon_H < 0$, a benefit formula that pays higher benefits to L types, and constant fiscal externalities g across types, our finding that the interventions decrease targeting bodes poorly for their welfare impacts. However, this is merely a qualitative comparative static result. Even with $\varepsilon_L < \varepsilon_H < 0$, the targeting effects of the intervention are neither necessary nor sufficient to sign the overall social welfare impact of the intervention. The overall social welfare effect may be positive, if private welfare gains to individuals with misperceptions outweigh the negative externality from the public application processing costs and expenditures on benefits.

Proposition 1a tells us that to make quantitative statements about the social welfare impact of the intervention—that is, $\frac{dW}{dT}$—we need estimates of $G_j^A - G_j^{\neg A}$, $\pi_j B_j$, $\frac{dA_j}{dT}$, and $\mu_j \equiv u(y_j^A + B) - u(y_j^A + (1+\varepsilon_j)B)$ (for $j = \{L, H\}$). Recall our baseline assumption (which we relax below) that $G_j^A - G_j^{\neg A} = -g$ and our use of a first-order Taylor approximation around actual utility to calibrate ε (which we also relax below) that allows us to approximate μ_H as $\xi_H \pi_H \varepsilon_H B_{min}$ and μ_L as $\xi_L \pi_L \varepsilon_L \overline{B}$.

To ease interpretation, we make two changes to the $\frac{dW}{dT}$ expression in Proposition 1a. First, we translate the changes in private utility μ_j into a change in dollars of surplus for each type (rather than the dollar surplus for a typical individual in the population; see note 3), by dividing by the marginal utility of consumption for each type ($\xi_j \equiv u'(y_j^A + B)$). Second, because quantitative welfare statements are more easily interpreted as a ratio of private welfare changes to changes in costs, we follow Hendren (2016) and rewrite the $\frac{dW}{dT}$ terms of Proposition 1a as a ratio rather than a difference; Hendren (2016) refers to this as the marginal value of public funds (*MVPF*) of our intervention. The *MVPF* is the ratio of marginal benefits to marginal costs, where marginal benefits are measured in terms of an individual's willingness to pay rather than society's (see, e.g., Finkelstein, Hendren, and Shepard 2019 for more discussion). Given our normalization, the MVPF represents the dollars of surplus transferred to each type (measured in that type's own money metric), divided by the total fiscal cost (in dollars) of the intervention. With these changes, we can write:

$$MVPF^{Information\ Only} = \frac{-\varepsilon_L(\pi_L \overline{B})\frac{dA_L}{dT} - \varepsilon_H(\pi_H B_{min})\frac{dA_H}{dT}}{(\pi_L \overline{B} + g)\frac{dA_L}{dT} + (\pi_H B_{min} + g)\frac{dA_H}{dT}}.$$

Online Appendix Section A.3.1 provides the derivation.

We previously parameterized $g \sim \$267$, $\pi_L\overline{B} \sim \$4{,}806$, $\pi_H B_{min} \sim \$432$, $\varepsilon_L \sim -0.98$, and $\varepsilon_H \sim -0.83$. The impact of the intervention on applications of each type $\frac{dA_j}{dT}$ comes directly from the experiment. Table II shows directly the increase in applications for the Information Only intervention, $\frac{dA}{dT} = 0.07$, and for the Information Plus Assistance intervention, $\frac{dA}{dT} = 0.16$. Online Appendix Table A7 shows that for each intervention, 44% of the marginal enrollees are H types (i.e., 44% of the compliers receive the minimum benefit level of \$16); this represents a decrease in targeting relative to the inframarginal enrollees (i.e., the always takers), for whom Table II shows only about 20% are type H individuals. Given our assumption of a common, 75% acceptance rate for both types, this suggests that for the Information Only intervention, $\frac{dA_L}{dT} = 0.03$ and $\frac{dA_H}{dT} = 0.04$, and for the Information Plus Assistance intervention, $\frac{dA_L}{dT} = 0.07$ and $\frac{dA_H}{dT} = 0.09$.

We therefore have rough estimates of all the elements we need to evaluate this expression:

$$MVPF^{Information\ Only} = \frac{0.98(\$4{,}806)0.04 + 0.83(\$432)0.03}{(\$4{,}806 + \$267)0.04 + (\$432 + \$267)0.03} = 0.89.$$

An MVPF estimate of 0.89 suggests that for every \$1 spent on the intervention (in the form of benefits and processing costs), low-income recipients receive about 89 cents of benefits.[19] An MVPF below 1 is to be expected for a redistributive policy such as SNAP; redistribution inevitably involves some resource cost (Okun 1975).

To see the role that targeting plays in affecting the MVPF, we calculate the MVPF in the Information Only intervention

19. This calculation assumes that the information intervention is itself costless. Accounting for the intervention costs (\$1 per outreach, or approximately \$7 for the 15% of the intervention arm who applied) in the denominator, however, has very little effect on the calculation.

separately for each type:

$$MVPF_L^{Information\ Only} = \frac{-\varepsilon_L(\pi_L\overline{B})\frac{dA_L}{dT}}{(\pi_L\overline{B}+g)\frac{dA_L}{dT}}$$
$$= \frac{0.98(\$4{,}806)0.04}{(\$4{,}806+\$267)0.04} = 0.93,$$
$$MVPF_H^{Information\ Only} = \frac{-\varepsilon_H(\pi_H B_{min})\frac{dA_H}{dT}}{(\pi_H B_{min}+g)\frac{dA_H}{dT}}$$
$$= \frac{0.83(\$432)0.03}{(\$432+\$267)0.03} = 0.52.$$

As Proposition 2a predicts, given our estimate of $\varepsilon_L < \varepsilon_H < 0$, the MVPF of the intervention is larger for L types. The difference is substantial, highlighting the potential welfare gains in our setting from policies that are especially effective at targeting high-benefit types. Policies that primarily enroll low-benefit types appear to have quite low MVPF ($\sim$0.5). In other words, if those deterred by barriers were exclusively the less needy, our interventions would have looked substantially worse.

Online Appendix A.3.2 describes an analogous calculation for the Information Plus Assistance Intervention. Assuming that the application costs are costlessly reduced—which would correspond to removing some preexisting barrier or ordeal—the MVPF is unambiguously higher for the Information Plus intervention than the Information Only one; indeed, we calculate that costlessly eliminating private application costs (i.e., reducing them from \$75 per application to 0) would increase the MVPF from 0.89 in the Information Only intervention to 0.93. Naturally, the specific numbers we calculate will be sensitive to the assumptions we have made. Online Appendix A.3.3 therefore briefly explores sensitivity of our results to some key alternative assumptions, with the goal of providing insight into the determinants of the welfare impacts of the intervention.

VII. Conclusion

Policy makers often advocate—and academics often study—interventions to increase take-up of public benefits. We provide a framework for analyzing the welfare impacts of such interventions and the welfare impacts of their targeting properties. The framework emphasizes that in the presence of potential behavioral frictions, a finding that interventions target relatively more needy individuals is neither necessary nor sufficient for inferring whether the intervention is more likely to improve welfare. We apply this framework to the results of a randomized field experiment of interventions designed to increase SNAP take-up. The interventions were designed to reduce potential information barriers to enrollment and potential transaction cost barriers. They were applied to a population of elderly individuals in Pennsylvania who are on Medicaid, and therefore likely eligible for SNAP, but not currently enrolled in SNAP.

We found that both information and transaction costs are barriers to take-up. In the nine months following the intervention, the Information Only intervention increased enrollment by 5 percentage points (or 83% relative to the enrollment rate among controls), while the Information Plus Assistance increased enrollment by 12 percentage points (a 200% increase relative to the controls). The impact of the treatments appears to be fully present by about 6 months; the time pattern of effects out to 23 months suggests that the treatments primarily generate new enrollment, rather than merely moving forward in time enrollment that would have happened anyway. A back-of-the-envelope calculation suggests that the Information Only treatment may be more cost-effective, with an intervention cost of about \$20 per new enrollee, compared with about \$60 per new enrollee for the Information Plus Assistance intervention.

We also find that reducing informational or transactional barriers decreases targeting: the marginal applicants and enrollees from either intervention are less needy than the average enrollees in the control group. The average monthly SNAP benefit (which declines with net income) is 20% to 30% lower among enrollees in either intervention arm relative to enrollees in the control group. In addition, relative to the control group, applicants and enrollees in either intervention arm are in better health, more likely to be white, and more likely to have English as their primary language. The finding that barriers to take-up deter relatively less needy individuals from enrolling is consistent with neoclassical theories of ordeal mechanisms (e.g., Nichols, Smolensky, and Tideman 1971; Nichols and Zeckhauser 1982; Besley and Coate 1992). However, consistent with behavioral models (e.g., Bertrand, Mullainathan, and Shafir 2004; Mani et al. 2013; Mullainathan and Shafir 2013) we find that the set of individuals who do not enroll even with the interventions looks worse off than those who enroll with or without the interventions, suggesting that other interventions might potentially have very different targeting properties.

The framework we developed highlights that normative implications depend critically on whether individuals have accurate beliefs about the expected benefits from applying, as well as what types of people have greater misperceptions. We present several pieces of evidence that are consistent with standard behavioral models (e.g., Mullainathan and Shafir 2013) in which individuals underestimate expected benefits from applying, with this underestimation greater among needier people. Under the assumptions in our setting, this is a sufficient condition for a decrease in targeting to decrease the social welfare gains from intervention.

The framework we developed also clarifies conditions under which the targeting properties of an intervention based on observable characteristics such as poverty may be informative about the likely welfare impact of the intervention. These conditions suggest the importance of measuring additional empirical objects—specifically, the size of any misperceptions across individuals with different observable characteristics and the size of the fiscal externality from enrolling across these individuals—to draw normative inferences from targeting results. This should hopefully be useful for analyzing the welfare impacts of other interventions designed to increase take-up of social benefits.

Massachusetts Institute of Technology, National Bureau of Economic Research, and J-PAL North America
Northwestern, National Bureau of Economic Research, and J-PAL North America

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online. Code replicating tables and figures in this article can be found in Finkelstein and Notowidigdo (2019), in the Harvard Dataverse, doi:10.7910/DVN/8AWKIL.

References

Aaronson, Becca, “Number of Texans Receiving Food Stamps Up Sharply amid Recession,” *Texas Tribune*, November 10, 2011, http://www.texastribune.org/library/data/texas-supplemental-nutrition-assistance-program/.

Abadie, Alberto, "Bootstrap Tests for Distributional Treatment Effects in Instrumental Variable Models," *Journal of the American Statistical Association*, 97 (2002), 284–292.

———, "Semiparametric Instrumental Variable Estimation of Treatment Response Models," *Journal of Econometrics*, 113 (2003), 231–263.

Alatas, Vivi, Abhijit Banerjee, Rema Hanna, Benjamin Olken, Matthew Wai-Poi, and Ririn Pernamasari, "Self-Targeting: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, 124 (2016), 371–427.

Allcott, Hunt, and Michael Greenstone, "Measuring the Welfare Effects of Residential Energy Efficiency Programs," NBER Working Paper No. 23386, 2017.

Angrist, Joshua D., and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton, NJ: Princeton University Press, 2009).

Armour, Philip, "The Role of Information in Disability Insurance Application: An Analysis of the Social Security Statement Phase-In," *American Economic Journal: Economic Policy*, 10 (2018), 1–41.

Barr, Andrew, and Sarah Turner, "A Letter and Encouragement: Does Information Increase Postsecondary Enrollment of UI Recipients?," *American Economic Journal: Economic Policy*, 10 (2018), 42–68.

Bartlett, Susan, Nancy Burstein, and William Hamilton, "Food Stamp Access Study: Final Report," U.S. Department of Agriculture, Economic Research Service, November, 2004, https://www.ers.usda.gov/publications/pub-details/?pubid=43407.

Benefits Data Trust, "About: Our Model," 2016, https://web.archive.org/web/20160117032627/http://bdtrust.org/about/our_model.

Bertrand, Marianne, Sendhil Mullainathan, and Eldar Shafir, "A Behavioral-Economics View of Poverty," *American Economic Review*, 94 (2004), 419–423.

Besley, Timothy, and Stephen Coate, "Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs," *American Economic Review*, 82 (1992), 249–261.

Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu, "The Role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA Experiment," *Quarterly Journal of Economics*, 127 (2012), 1205–1242.

Bhargava, Saurabh, George Loewenstein, and Justin Sydnor, "Choose to Lose: Health Plan Choices from a Menu with Dominated Options," *Quarterly Journal of Economics*, 132 (2017), 1319–1372.

Bhargava, Saurabh, and Dayanand Manoli, "Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment," *American Economic Review*, 105 (2012), 3489–3529.

Center on Budget and Policy Priorities, "SNAP Online: A Review of State Government SNAP Websites," 2016, http://www.cbpp.org/research/food-assistance/snap-online-a-review-of-state-government-snap-websites.

———, "SNAP Helps Millions of Low-Income Seniors," 2017, https://www.cbpp.org/research/food-assistance/snap-helps-millions-of-low-income-seniors.

Cunnyngham, Karen, "Estimates of State Supplemental Nutrition Assistance Program Participation Rates in 2012," *Mathematical Policy Research*, 2015, https://fns-prod.azureedge.net/sites/default/files/ops/Reaching2012.pdf.

Currie, Janet, "The Take Up of Social Benefits," in *Poverty, the Distribution of Income, and Public Policy*, Alan Auerbach, David Card, and John Quigley, eds. (New York: Russell Sage Foundation, 2006).

Currie, Janet, and Firouz Gahvari, "Transfers in Cash and In-Kind: Theory Meets the Data," *Journal of Economic Literature*, 46 (2008), 333–383.

Cutler, David, and Jonathan Gruber, "Does Public Insurance Crowd Out Private Insurance?," *Quarterly Journal of Economics*, 111 (1996), 391–430.

Daponte, Beth O., Seth Sanders, and Lowell Taylor, "Why Do Low-Income Households Not Use Food Stamps? Evidence from an Experiment," *Journal of Human Resources*, 34 (1999), 612–628.

Deshpande, Manasi, and Yue Li, “Who Is Screened Out? Application Costs and the Targeting of Disability Programs,” *American Economic Journal: Economic Policy*, forthcoming.
Dynarski, Susan, C. J. Libassi, Katherine Michelmore, and Stephanie Owen, “Closing the Gap: The Effect of a Targeted, Tuition-Free Promise on College Choices of High-Achieving, Low-Income Students,” NBER Working Paper no. 25349, 2018.
Einav, Liran, Amy Finkelstein, and Mark R. Cullen, “Estimating Welfare in Insurance Markets using Variation in Prices,” *Quarterly Journal of Economics*, 125 (2010), 877–921.
Eslami, Esa, “Characteristics of Elderly Individuals Participating in and Eligible for SNAP,” Mathematica Policy Research, 2016, https://www.mathematica-mpr.com/download-media?MediaItemId=DB17EFFA-E155-48C0-9E2D-CF90C59BCDDD.
Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams, “Sources of Geographic Variation in Health Care: Evidence from Patient Migration,” *Quarterly Journal of Economics*, 131 (2016), 1681–1726.
Finkelstein, Amy, Nathaniel Hendren, and Mark Shepard, “Subsidizing Health Insurance for Low-Income Adults: Evidence from Massachusetts,” *American Economic Review*, 109 (2019), 1530–1567.
Finkelstein, Amy, and Matthew J. Notowidigdo, “Replication Data for: ‘Take-up and Targeting: Experimental Evidence from SNAP’,” Harvard Dataverse, (2019), doi:10.7910/DVN/8AWKIL.
Ganong, Peter, and Jeffrey B. Liebman, “The Decline, Rebound, and Further Rise in SNAP Enrollment: Disentangling Business Cycle Fluctuations and Policy Changes,” *American Economic Journal: Economic Policy*, 10 (2018), 153–176.
Gray, Colin, “Why Leave Benefits on the Table? Evidence from SNAP,” Upjohn Institute Working Paper, 2018.
Gruber, Jonathan, Philip B. Levine, and Douglas Staiger, “Abortion Legalization and Child Living Circumstances: Who Is the Marginal Child?,” *Quarterly Journal of Economics*, 114 (1999), 263–292.
Guyton, John, Dayanand Manoli, Brenda Schafer, and Michael Sebastiani, “Reminders & Recidivism: Evidence from Tax Filing & EITC Participation among Low-Income Non-Filers,” NBER Working Paper no. 21904, 2016.
Hastings, Justine, and Jesse Shapiro, “How Are SNAP Benefits Spent? Evidence from a Retail Panel,” *American Economic Review*, 108 (2018), 3493–3540.
Hendren, Nathaniel, “The Policy Elasticity,” *Tax Policy and the Economy*, 30 (2016), 51–89.
Hoynes, Hilary W., and Diane Whitmore Schanzenbach, “Work Incentives and the Food Stamp Program,” *Journal of Public Economics*, 96 (2012), 151–162.
———, “U.S. Food and Nutrition Programs,” in *Means Tested Transfer Programs in the United States, Volume II*, Robert Moffitt, ed. (Chicago: University of Chicago Press, 2016).
Hu, Winnie, “Rations Reduced as Demand Grows for Soup Kitchens,” *New York Times*, June 27, 2014.
Isaacs, Julia, “The Costs of Benefit Delivery in the Food Stamp Program,” USDA Contractor and Cooperator Report no. 39, 2008, https://www.brookings.edu/wp-content/uploads/2016/06/03_food_stamp_isaacs.pdf.
Kabbani, Nader S., and Parke E. Wilde, “Short Recertification Periods in the U.S. Food Stamp Program,” *Journal of Human Resources*, 38 (2003), 1112–1138.
Kauff, Jaqueline, Lisa Dragoset, Elizabeth Clary, Elizabeth Laird, Libby Makowsky, and Emma Samaa-Miller, “Reaching the Underserved Elderly and Working Poor in SNAP: Evaluation Findings from the Fiscal Year 2009 Pilots,” Mathematica Policy Research Working Paper, 2014.
Kleven, Henrik J., “Sufficeint Statistics Revisited,” Working Paper, 2018, https://www.henrikkleven.com/uploads/3/7/3/1/37310663/kleven_sufficientstats_march2018.pdf.

Kleven, Henrik J., and Wojiciech Kopczuk, “Transfer Program Complexity and the Take Up of Social Benefits.” *American Economic Journal: Economic Policy*, 3 (2011), 54–90.
Lee, David, “Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects,” *Review of Economic Studies*, 76 (2009), 1071–1102.
Madrian, Brigitte, and Dennis Shea, “The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior,” *Quarterly Journal of Economics*, 116 (2001), 1149–1187.
Mani, Anandi, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao, “Poverty Impedes Cognitive Function,” *Science*, 341 (2013), 976–980.
Manoli, Dayand, and Nicholas Turner, “Nudges and Learning: Evidence from Informational Interventions for Low-Income Taxpayers,” NBER Working Paper no. 20718, 2016.
Moffitt, Robert, “An Economic Model of Welfare Stigma,” *American Economic Review*, 73 (1983), 1023–1035.
Mullainathan, Sendhil, and Eldar Shafir, *Scarcity: Why Having Too Little Means So Much* (New York: Holt, 2013).
Nichols, Donald A., Eugene Smolensky, and T. Nicolaus Tideman, “Discrimination by Waiting Time in Merit Goods,” *American Economic Review*, 61 (1971), 312–323.
Nichols, Albert L., and Richard J. Zeckhauser, “Targeting Transfers through Restrictions on Recipients,” *American Economic Review*, 72 (1982), 372–377.
Okun, Arthur M., “Equality and Efficiency,” *The Big Tradeoff* (Washington, DC: Brookings Institution Press, 1975).
Ponza, Michael, James C. Ohls, Lorenzo Moreno, Amy Zambrowski, and Rhoda Cohen, “Customer Service in the Food Stamp Program,” Mathematica Policy Research no. 8243–140, 1999.
Rifkin, Ryan, and Aldebaro Klautau, “In Defense of One-vs-All Classification,” *Journal of Machine Learning Research*, 5 (2004), 101–141.
Rossin-Slater, Maya, “WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics,” *Journal of Public Economics*, 102 (2013), 51–69.
Schanzenbach, Diane, “Experimental Estimates of the Barriers to Food Stamp Enrollment,” Institute for Research on Poverty, University of Wisconsin-Madison, Discussion Paper 1367-09, 2009.
Song, Yunjie, Jonathan Skinner, Julie Bynum, Jason Sutherland, John E. Wennberg, and Elliott S. Fisher, “Regional Variations in Diagnostic Practices,” *New England Journal of Medicine*, 363 (2010), 45–53.
U.S. Congressional Budget Office, “Growth in Means-Tested Programs and Tax Credits for Low-Income Households,” 2013.
U.S. Department of Agriculture, “The Food Assistance Landscape: FY 2015 Annual Report,” 2016.
U.S. Department of Health and Human Services, “FY 2015 Federal TANF & State MOE financial data,” 2016.
U.S. Internal Revenue Service, “EITC Calendar Year Report,” 2016.
U.S. Social Security Administration, “Social Security Income Program 2016 Technical Materials,” 2016.