# Take-up and Targeting: Experimental Evidence from SNAP

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**Abstract**

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 also offered assistance in applying, or are in a "status quo" control group. Only 6 percent of the control group enrolls in SNAP over the next 9 months, compared to 11 percent of the Information Only group and 18 percent of the Information Plus Assistance group. The individuals who apply or enroll in response to either intervention receive lower benefits 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.

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# 1 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 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 United States Supplemental Assistance Nutrition 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 a given amount of public spending to be directed to individuals with higher marginal utility from enrollment, or higher social welfare weights (e.g., Nichols et al., 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 et al. 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 paper 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 non-participants in SNAP.

We focus - both conceptually and empirically - on interventions that inform individuals about their likely eligibility ("information interventions") or reduce the private costs of applying ("assistance interventions"). As described above, 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 (Barr and Turner forthcoming, Bhargava and Manoli 2015, Guyton et al. 2016, Manoli and Turner 2014), Social Security Disability Insurance (Armour forthcoming), post-secondary enrollment (Barr and Turner forthcoming, Bettinger at al. 2012, Dynarski et al.

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

2018 ), energy efficiency audits (Alcott and Greestone 2017) and SNAP (Daponte et al. 1999). Studies of assistance interventions have been conducted, for example, for Supplemental Security Income and Social Security Disability Insurance (Deshpandi and Li 2017), the Women, Infants and Children program (Rossin-Slater 2013), post-secondary 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 either private or social welfare. We extend the standard targeting literature in which adding ordeals to a means-tested transfer program can improve social welfare beyond what can be achieved through an optimal non-linear 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 therefore allow the government to transfer more to lower ability types for a given amount of government expenditure. 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. SNAP is 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, 2013). 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 percent of SNAP caseload, they have especially low take-up; in 2012, only 42 percent of eligible elderly enrolled in SNAP, compared to 83 percent overall (Eslami, 2016). And the stakes associated with non-participation are non-trivial for the elderly; average annual SNAP benefits are about $1,500, or about 15 percent 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

[2]US Department of Agriculture 2016, US Department of Health and Human Services 2016, US Internal Revenue Service 2016, US Social Security Administration (2016)

transforming how individuals 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 et al. 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 Pennsylvania's Department of Human Services (DHS), informing them of their likely eligibility for SNAP and providing them 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 with 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 sub-treatments that varied the content of the letter and, in one case, whether or not 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 both 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 pre-intervention from the study participants' Medicaid records.

The experiment produced two main empirical findings. 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 ($p < 0.001$). 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 a sub-treatment of the Information Only intervention, which omits the reminder postcard, reduces its impact by about 20 percent. 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 percent 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 percent 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 pre-intervention rates of hospital visits and chronic diseases). Additionally, 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 percent of individuals who did not call in response to our interventions and remain largely un-enrolled 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 welfare gains from each intervention, as well as potential social welfare gains. Our estimates also suggest that underestimation of expected benefits is greater for needier individuals, again consistent with leading behavioral theories (e.g., Bertrand et al. 2004, Mani et al. 2013, Mullainathan and Shafir 2013). However, in contrast to these models and consistent with neoclassical theory (e.g., Nichols et al., 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. While 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 paper proceeds as follows. Section 2 presents our framework. Section 3 provides background information on SNAP. Section 4 describes our experimental design and data. Section 5 presents the experimental results, and briefly compares them to the prior RCTs of Daponte et al. (1999) and Schanzenbach (2009), as well as quasi-experimental evidence on barriers to SNAP take-up. Section 6 uses the results to calibrate the model from Section 2 and perform welfare analysis of the interventions.

## 2 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 Appendix E.1.

## 2.1 Model setup

There are types of individuals $j \in \{L, H\}$. Each type has unobserved wage $\theta_j$, 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 or not to apply to a supplemental income program. There is a (potentially non-linear) income tax system $\tau(\theta_j h_j)$, which maps pre-tax 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 $\epsilon_j$, so that the *perceived* benefit of applying is $(1 + \epsilon_j)B$. With $\epsilon_j < 0$, misperception reduces the perceived benefit of applying. We refer to the special case of no misperceptions – i.e., $\epsilon_j = 0$ 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 to applying that is common across individuals (and is under control of the social planner or researcher), and $\kappa_j$ 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 et al. 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 $h_H^A = r^*/\theta_H$, so that they are at the income threshold. Intuitively, both types choose weakly fewer hours of work if they apply ($h_j^A \leq h_j^{\neg A}$) due to 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 for $c < c_j^*$, type $j$ chooses to apply.

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

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

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) as well as any fiscal externalities from individual's 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, the application decisions imposes a negative fiscal externality on the government via its 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:

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

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 non-zero ordeal utility cost ($\bar{\Lambda} > 0$) even in the presence of an arbitrary optimal nonlinear income tax. Intuitively, with unobserved ability $\theta_j$ and endogenous hours choices, the incentive compatibility constraint that high

[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

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 rather to characterize the *marginal* social welfare gain (or loss) from interventions that affect information about program eligibility or the private cost of application.

## 2.2 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 ($d\epsilon$). In the **Information Plus Assistance treatment**, the treatment increases the perceived benefits of applying and decreases the actual private cost of applying ($d\epsilon_j$,$-d\bar{\Lambda}$). For simplicity, we assume the interventions have zero marginal cost.

For notational ease we introduce the following two definitions:

**Definition.** Define $\mu_j \equiv u(y_j^A + B) - u(y_j^A + (1+\epsilon_j)B)$ and $\xi_j \equiv u'(y_j^A + B)$.

The $\mu_j$ term denotes the difference for type $j$ between the actual and perceived utility when applying; if individuals under-estimate the benefits of applying (i.e., $\epsilon_j < 0$) - which is the premise of the information interventions - then $\mu_j > 0$. The $\xi_j$ 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:*

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

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

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

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 Appendix E.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" ($\epsilon_j = 0$), the intervention has no effect on private welfare, since $\mu_L = \mu_H = 0$.[5] 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 under-estimate the benefit of applying (i.e., $\epsilon_j < 0$), the intervention increases private welfare for marginal applicants of each type by $\mu_j$, with the increase in private welfare increasing in the amount of under-estimation 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 *infra-marginal* 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 $\epsilon_j$.

We define **targeting** as the share of enrollees who are type $L$; i.e., $e = E_L/(E_H + E_L)$, where $E_j$ is the number of type $j$ enrollees. We say that a treatment $T$ **increases targeting** if $de/dT > 0$.[6] 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* ($de/dT > 0$) *from an Information Only (or Information Plus Assistance) treatment is given by the following expression:*

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

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 to an

[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.

[5] If individuals have accurate beliefs, and these beliefs do not change in response to the Information Only treatment, then $dA/dT = 0$ for both types (since beliefs are unchanged), and there is no effect on private welfare since application decisions are unchanged. Alternatively, the Information Only treatment could cause individuals to update their beliefs ($d\epsilon > 0$), even if they start from $\epsilon_j = 0$. In this case $dA/dT > 0$, and the Information Only treatment has distorted individuals' beliefs away from the truth, but there are still no first-order effects on private welfare (for both types) according to Proposition 1.

[6] Another type of targeting that would be natural to analyze is acceptance rate targeting -- i.e., the expected share of applicants who are accepted. We do not develop this aspect of the model since, as we will see, neither of our treatments has an effect on acceptance rate targeting. This is consistent with other recent findings of the (non-)impact of reductions in transaction costs on acceptance rates (e.g., Alatas et al., 2016; Deshpande and Li 2017, Armour forthcoming).

$H$ 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 which improve targeting (i.e., $de/dT > 0$) will be better for social welfare. This is most naturally seen in the "standard" setting (e.g. Nichols and Zeckahuser 1982) in which individuals do not make mistakes in their application decisions ($\epsilon_L = \epsilon_H = 0$) and the fiscal externality is via the impact on income tax revenue. Because individuals do not make mistakes, $(\mu_L - \mu_H)$ is zero; a change in targeting therefore has no effect on private welfare. The relationship between a change in social welfare and a change in targeting therefore depends only on how the change in targeting changes the fiscal externality from applying. In the "standard" setting, improved targeting - i.e. 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 2015, Bhargava et al. 2017, Alatas et al. 2016, Deshpande and Li (2017).

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 ($\epsilon_j \neq 0$), the change in social welfare from a change in targeting is increasing in $(\mu_L - \mu_H)$; $\mu_L$ enters positively while $\mu_H$ enters negatively because the thought experiment of increasing targeting "swaps" an $H$ applicant for an $L$ applicant. With $\epsilon_j < 0$, $\mu_j$ is increasing in two type-specific factors: the marginal utility of consumption ($\xi_j$) and the magnitude of the under-estimation ($-\epsilon_j$). 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 under-estimation is non-zero for at least one type and weakly higher (in absolute value) for the $L$ type (i.e., $\epsilon_L \leq \epsilon_H \leq 0$, 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 under-estimate the probability of application acceptance by same amount (in a proportional sense), so that $\epsilon_H = \epsilon_L < 0$.

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 impact the government budget, such as an impact of the program on health and hence public healthcare expenditures. These fiscal externalities may be either positive or negative and may be larger or smaller for $L$ types compared to $H$ types.[7] 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.

**Extensions**

In Appendix E.2 we show that the core propositions are robust to alternative modeling choices about the nature of misperceptions and "mistakes". We also consider non-marginal changes, similar in spirit to Kleven (2018). Our main analysis focuses on the marginal welfare gain (or loss) from "small" interventions, allowing us to make heavy use of the envelope theorem. Non-marginal 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 non-marginal case the increase in private welfare for a marginal enrollee 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.

# 3 Setting and Background

SNAP is the second-largest means-tested program in the United States (US 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 – i.e., households with an individual aged 60 or over - in Pennsylvania (PA) in 2016. Take-up of SNAP in Pennsylvania is similar to the nationwide estimates (Cunnyngham 2015). Appendix G 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

[7] 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 et al. 2017, section 5.2.

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 on line. The on-line 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 re-certification. 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 benefit distribution at the minimum and maximum. SNAP benefits are a substantial source of potential income for eligible households. For elderly households in PA, enrollment entitles the household to benefits equivalent to, on average, about 15 percent of annual income (Center on Budget and Policy Priorities, 2017).

The application imposes costs on both 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 percent of annual benefits, a substantially higher share of benefits than administrative costs for the Earned Income Tax Credit.

# 4 Empirical Design and Data

## 4.1 Design of Interventions

We partnered with Benefits Data Trust (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 likely 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 then 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.

**Information Plus Assistance**

BDT conducts a series of outreach services to inform individuals of their likely eligibility and assist them in applying 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 8 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 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 4th to 6th grade reading level and are sent from the Secretary of the Pennsylvania Department of Human Services. Appendix Figure A1 shows these standard outreach materials. In the framework of Section 2, we think of this intervention as increasing the perceived benefits from applying ($d\epsilon$).

The assistance component begins if, in response to these outreach materials, the individual 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 individual needs to submit, offering to review and submit documents on their behalf, and assisting with post-submission 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). Appendix A provides more detail on the nature of BDT's assistance. In the framework of Section 2, 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 percent 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.[8] 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.

**Information Only**

Our "Information Only" intervention contains only the letters and follow-up postcards to non-respondents 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 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. Appendix A provides more details, and Appendix Figure A2 shows the outreach materials in the Information Only arm.

**Sub-treatments**

Within each treatment, we created additional sub-treatments in the presentation and frequency with which the information was presented. In practice, most of these sub-treatments had little or no impact and therefore in most of our analysis we pool them. However, we also present results from the one sub-treatment where we found substantial effects: the elimination of the postcard follow-up in the standard Information Only intervention. Appendix A provides more detail of the sub-treatments and how they were distributed across arms.

## 4.2 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 Appendix G, exact assessment of SNAP eligibility requires non-income 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

[8]As we will see in the results below, 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.

only about 20 percent of individuals aged 60 and over who are not enrolled in SNAP but have income less than 200 percent 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 rather 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 2016 intervention.[9] Since only about three-quarters of our study population were enrolled in Medicaid for the entirety of 2015, we annualized all of the health care utilization measures by dividing by the number of days enrolled out of 365. This is an imperfect approach, since 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.

### 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 1 illustrates the construction of our study population and the pre-randomization 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) – since 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 percent who were enrolled in SNAP or living with someone enrolled in SNAP, while column 4 shows characteristics for the 40 percent (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 individual from each "household" (this excludes 1,842 individuals)[10], and excluded all individuals

[9] Medicaid in PA is provided either fee-for-service or managed care, determined in large part based on geography. Our "claims" data are therefore a mix of encounter data from Medicaid Managed Care and Fee for Service claims. Although there are well-known measurement issues with encounter data - and comparability issues with fee for service claims data (e.g., Lewin Group 2012) - such measurement issues should not bias our comparisons of these measures across randomly assigned arms.

[10] There is no household identifier in the Medicaid outreach file; BDT therefore created a "pseudo-household" ID to identify individuals on the outreach list sharing the same last name and address.

to whom BDT had previously sent any outreach materials (N=26,155).[11]

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).

### 4.3 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 sub-treatments 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, while 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"). See Appendix A for more detail.

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 following each mailing, with the last postcards scheduled to be sent on May 11, 2016.[12] 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 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 de-identified data that we received. 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 the

[11] BDT has comprehensive data on outreach efforts since 2012, and limited data on outreach back to 2007. BDT started conducting SNAP outreach in 2008.

[12] 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.

Department of Human Services (DHS) before the study was launched. MIT's Institutional review board (IRB) approved this research as well as the data sharing outlined in Appendix B (Protocol: 1506106206; FWA: 00004881).[13] The trial was registered on the AEA RCT Registry (AEA RCTR -0000902) in October 2015, prior to our launch - at which point we pre-specified our primary and secondary outcomes.[14] We updated the registry to specify additional detail - such as a 9 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.[15]

## 4.4 Outcomes data

**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 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 9 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.[16]

DHS also provided us with monthly benefit amounts for enrolled individuals. We measure the monthly benefit amount in months enrolled in the 9 months post outreach. The monthly benefit amount will serve as one of the key measures of enrollee characteristics.

**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. In order to capture comparable information on which individuals call in to DHS in response to the

[13]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 (NBER) judged the protocol to be exempt (IRB Ref#15_129; FWA: 00003692).

[14]Specifically, at that time we wrote: "Primary outcome: number of SNAP enrollees. Secondary outcomes: baseline characteristics of enrollees (e.g., demographics, measures of economic well-being, measures of health etc); number of SNAP applications; baseline characteristics of applicants; number of responses to outreach letters (i.e., phone calls to the number listed on the outreach letter). Outcomes (explanation) We are interested in measuring characteristics of the enrollees, for example measures of economic well-being, demographics and health status. Which characteristics we measure and how we measure them will depend largely on the quality and availability of data."

[15]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 still left unspecified at that point due to uncertainty on data availability. However, we were unable to execute on our aspirations to analyze additional characteristics like earnings and credit report outcomes due to lack of the relevant data.

[16]Based on their prior outreach efforts, BDT estimated that initial responses (i.e., calls) if they are going to occur will happen within the first three months. We wanted to allow sufficient time for applying and for the state's 30-day decision time and erred on the side of a long time window to make sure we had the full impact.

Information Only treatment, we contracted with a call forwarding service, and the information-only outreach letters provided the 1-800 numbers of the call forwarding service, with a different call-in number in each sub-treatment 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 in response to the outreach. It also allowed BDT to send follow-up postcards to non-callers 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 non-trivial 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. Appendix C provides more details on the call-in data, the call forwarding service, the script for the receptionists, which was provided in English and in Spanish, and the construction of the "adjusted" call-in rate.

# 5 Results

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

## 5.1 Behavioral Responses to Intervention

### Enrollment, applications, and calls

Table 2 presents the main take-up results of the experiment by intervention arm. All outcomes are measured in the nine months after the initial mail date.

The first row shows results for our primary outcome: enrollment within 9 months. In the control group, about 6 percent enroll. The Information Only intervention increases enrollment by 5 percentage points. Information Plus Assistance increases enrollment by 12 percentage points, or 200 percent relative to the control; the impacts of the intervention are statistically different from the control and from each other ($p<0.001$).[17]

[17] 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

Figure 1 shows the time pattern of the interventions' impacts on enrollment by month through 23 months post intervention, which is as long as our current data allow. The time pattern is similar for both interventions: over 85 percent of the 9-month enrollment effect is present by 4 months, and the impact has clearly leveled off before 9 months (our baseline time window). The impacts of the intervention appear to largely persist, as least through the 23 months we can observe post-intervention; about 90 percent of the 9-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.[18]

The next two rows of Table 2 show that the the interventions' impacts on applications are roughly proportional to the increase in enrollment. About 22 percent of applications in each arm are rejected; differences across arms are substantively and statistically indistinguishable.[19] 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. Of course, since 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 Appendix Table A9 we briefly explored 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 - e.g., withdrew or didn't 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 2 examine call-in rates. A caller is defined as someone calling the number provided on the outreach material; the caller rate is therefore mechanically zero for those in the control arm.[20] The raw call-in rates are 30 percent for the Information Plus Assistance outreach letters, and 27 percent 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 4.4 above) is 29 percent, and statistically

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 percent relative to the control. In the context of informing low-income tax filers about their likely eligibility for the EITC, Bhargava and Manoli (2015) found that their average informational outreach increased EITC filing by 22 percentage points (or about 50 percent above baseline).

[18] Appendix Figure A7 shows similar monthly patterns for applications and calls.

[19] In other contexts, changes in transaction costs have similarly had a small effect on rejection rates of applicants. Deshpande and Li (2017) find that increasing transaction costs (via closings of Social Security field offices) results in about a two percentage point increase in rejection rates for SSDI applicants, while Alatas et al. (2016) find that increasing transaction costs (via requiring individuals to apply for the benefit as opposed to the government automatically screening potential eligibles) decreases rejection rates by about two percentage points in a conditional cash transfer program in Indonesia.

[20] 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 sub-treatment in the Information Only arms). There is virtually no cross-contamination.

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 (recall Appendix A and 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.[21] 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 ($p<0.001$).

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), and/or apply on-line 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 percent likely received the outreach materials, since less than 1 percent 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 by the materials to apply for SNAP benefits. Presumably some of the non-callers are actually ineligible for SNAP, given that some of the applications are rejected due to ineligibility; perhaps an even larger share of non-callers believe themselves (potentially correctly) to be ineligible. However, we show below that predicted enrollment is similar for callers and non-callers.

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 percent) than when only information is provided (about 30 percent). Likewise, enrollment rates (conditional on interest) are about 45 percent when information and assistance is provided compared to 23 percent when only information is provided.

All of the results shown in Table 2 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 Appendix Table A16 that all of the results in Table 2 are robust to controlling for baseline demographic and health characteristics of the individuals, as well as for the date of their mail batch.

[21] As described in more detail in Appendix A, callers receive assistance from a Benefit Outreach Specialist (BOS). BDT assigns callers to Benefit Outreach Specialists using a system that is similar to how cases are often assigned to judges (i.e., a rotation system). As a result, we can plausibly assume that the assignment of a BOS to a caller is quasi-random. Using this assumption, we estimate treatment effect heterogeneity across Benefit Outreach Specialists using a non-parametric Empirical Bayes approach, and (perhaps surprisingly) we do not find statistically significant evidence of treatment effect heterogeneity – i.e., we cannot reject that the BOS fixed effect estimates are all equal to each other. We also examined whether the characteristics of the BOS we can observe - age, gender, and experience - are associated with differential impacts on applications or enrollment. Appendix Table A19 summarizes the characteristics of the BOSs' and Appendix Table A20 shows that none of these characteristics are associated with differential impacts on applications or enrollment; it also shows that concordance between caller and BOS gender is not associated with differential impacts.

**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 individual who is sent outreach materials, and the marginal cost of the Information Only treatment was about $1 per individual who was sent outreach materials.[22] This suggests that the cost per additional enrollee is $20 in the Information Only treatment, compared to $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 enrollees receive, on average, about $1,300 per 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 as well as 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) as well as the administrative costs of processing the applications, these would still be less than 25 percent of the new federal benefits received by state residents, and presumably spent largely at local retail outlets. Interestingly, this conclusion would be different if virtually all of new enrollees received the minimum benefit ($16 per month or $192 per year); this would be similar to the state's average administrative costs per recipient. Additionally, since 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.

**Effects of reminders**

Table 3 shows results for two sub-treatments of the Information Only intervention: the "standard" treatment, which includes an initial letter and a reminder postcard 8 weeks later if the individual has not yet called in (see Appendix Figure A2), and a "no reminder postcard" sub-treatment in which the follow-up postcard is not sent.[23] Reminders matter: all behavioral responses decrease by about 20 percent without the reminder postcard. Specifically, the "standard" Information Only

[22] 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.

[23] The results for the Information Only treatment results shown in Table 2 pool the results from the standard treatment and a "marketing" sub-treatment that varied the content of the outreach letters (see Appendix A and Appendix Figure A5 for more details); these two sub-treatments are pooled in the same proportions in the Information Plus Assistance treatment results shown in Table 2.

treatment (with the reminder postcard) had a 30 percent call rate, a 15 percent application rate and an 11 percent enrollment rate. The lack of a postcard reminder reduced the caller rate by 7 percentage points ($p < 0.001$), the application rate by 3 percentage points ($p = 0.001$) and the enrollment rate by 2 percentage points ($p = 0.016$). Given the 2 percentage point increase in enrollment with the reminder postcard, and its marginal cost of roughly $0.35, cost per additional enrollee is similar with and without the reminder postcard.

The non-trivial impact of a reminder postcard is similar to Bhargava and Manoli's (2015) 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 percent 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, non-participants in SNAP reported that they were not aware of their eligibility (Bartlett et al. 2004). In our framework in Section 2, this is modeled as under-estimating the benefits of applying (i.e., $\epsilon < 0$).

## 5.2 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 reveals how the characteristic of the marginal individual who apply or enroll due to a given intervention differs from the average applicant or enrollee who would enroll absent 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 et al. (1999) and Einav et al. (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 also show that characteristics tend to be similar between the two intervention arms, and within each intervention arm, between applicants and enrollees. However, callers and non-callers look quite different.

### Monthly benefits among enrollees

Table 4 shows monthly benefits for individuals who enrolled in the 9 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 percent lower for enrollees in either intervention than for control enrollees. Average monthly benefits are $146 in the control compared to $115 in the Information

Only intervention and \$101 in the Information Plus Assistance intervention; average benefits in each intervention arm are statistically different from those in the control ($p < 0.001$) as well as from each other ($p = 0.013$).[24]

There are clear modes in the distribution of benefits received, corresponding to minimum and maximum benefit amounts. Among the controls, 18 percent receive \$16 (the minimum monthly benefit for a household of size 1 or 2 who are categorically eligible) and another 19 percent receive \$194 (the maximum monthly benefit for a household of size 1); see also Appendix Figure A8. Table 4 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 percent of enrollees, presumably due to data errors. Importantly, Table 4 indicates that this missing rate is not balanced across arms. Such non-random attrition could bias our comparison of enrollee benefits across arms; however, we show in Appendix D 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 pre-randomization demographic and health characteristics shown in Table 1; Appendix D provides more detail on the prediction algorithm which follows a standard algorithm in machine learning (Rifkin and Klautau 2004). Table 4 shows that predicted benefits show the same pattern across arms as actual benefits, both among enrollees with non-missing 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 4 shows that the interventions increase the share of enrollees who are in a household size of 1. However, the bottom row of Table 4 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.

[24]Differences in the average characteristics of enrollees in an intervention arm relative to the control arm reflect differences between the average characteristics of infra-marginal enrollees (or "always takers") relative to marginal enrollees (or "compliers"). As another way of presenting the same information, Appendix Table A7 reports the average characteristics for always takers and compliers; estimation of these objects is standard (see, e.g., Abadie 2002, Abadie 2003, or Angrist and Pischke 2009) and we describe it in more detail in Appendix F. Note that comparing average characteristics of enrollees across treatment arms mixes both differences in average characteristics of compliers as well as 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 Appendix Table A7, which reports similar complier means across the two arms.

**Demographics and health of applicants and enrollees**

Table 5 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 ($p < 0.001$).

Panel B shows results for health and healthcare, measured in the calendar year prior to the intervention. We measure health care 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.[25] 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 both 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 a SNF day or a doctor visit. For all three measures, applicants and enrollees in the intervention arms use less health care pre-randomization than those in the control arm, although these differences are not always statistically different from the control.[26]

The final row of Panel B shows that the number of measured chronic conditions is also lower in both intervention arms relative to the control arm for both applicants and enrollees, with most of these differences statistically significant at conventional levels. A smaller number of chronic conditions could reflect better underlying health . It could also - partly or entirely - reflect lower health care utilization, since chronic conditions are only measured if the individuals use the relevant health care (Song et al., 2010; Finkelstein et al., 2016).

Panel C reports demographic characteristics. Relative to the control group, applicants and enrollees in either intervention arm are statistically significantly ($p < 0.001$) older, more likely to be white, and more likely to have their primary language be English. For example, 71 percent of control enrollees are white, compared to 78 percent in either intervention arm. In general, these results suggest that - consistent with the results for benefit amounts and health - the socio-economic status of marginal enrollees is higher than inframarginal enrollees; one exception, however, is age, since among the elderly older individuals tend to have higher poverty rates. Of course, as emphasized by the conceptual framework in Section 2, the observable socio-economic characteristics of those targeted by the intervention are neither necessary nor sufficient for normative analysis, a

[25]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 a SNF day,and $79 for a doctor visit.

[26]As discussed above, 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 Appendix Table A3). Therefore, not surprisingly, we find in 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).

point we return to when we explore the normative implications of our findings in Section 6 below.

### Out-of-sample implications

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

Table 6 therefore shows characteristics separately for callers and non-callers (pooled across interventions; the characteristics of callers look similar across the two interventions, as shown in Appendix Table A13). The 70 percent of individuals who did not call in response to our intervention look worse off on all dimensions: they have higher predicted benefits, higher health care spending and use, and more chronic conditions.[27] Consistent with this, 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.

This suggests that there may indeed be a sizable mass of individuals who - consistent with behavioral theories - are high need but deterred from enrolling. Mailing interventions aimed at informing such individuals of their eligibility and offering phone assistance with applying do not, however, appear to reach them. An open question is whether there are other interventions that would.

Another out-of-sample question is whether the impact of the interventions would be very different for younger individuals. This is hard to say since our experiment was limited to individuals 60 and older. But within that sample, Appendix Table A18 shows no differential impact of either intervention by individual age.

## 5.3 Comparison to impacts of other SNAP interventions

As noted in the Introduction, our information and assistance interventions are similar to those studied in two prior randomized evaluations. In an early and innovative small randomized trial in 1993 in Pennsylvania, Daponte et al (1999) found suggestive evidence that informing non-participating, eligible households about their SNAP eligibility affected SNAP applications: 11 out of 32 households in the treatment arm replied in a follow-up survey that they had subsequently signed up for SNAP, while no households in the control group reported signing up for SNAP.

This study also found suggestive evidence of what the authors interpret as an endogenous lack of information: eligible non-participants were eligible for lower benefits than their participating counterparts. Consistent with this, we found (see Table 4) that individuals who respond to the Information Only treatment have lower benefits than the control group. However, our larger sample size also allows us to find evidence of individuals who receive very benefit levels in response to the information treatment but would not have applied if they were in control group (again see Table

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

4). This is harder to square with an "endogenous information acquisition model" since it implies that individuals are leaving thousands of dollars of benefits on the table, but sign up shortly after receiving a letter and postcard. We instead interpret this as a substantial misperception or misunderstanding of SNAP eligibility. In our welfare calibrations below, these individuals drive much of the welfare impact.

In another entrepreneurial SNAP RCT, Schanzenbach (2009) provides evidence from one California county that those provided with full assistance (in which a tax preparer went through a detailed interview with the client and then filled out and filed the application on the client's behalf) were more likely to file an application than those who received help filling out the application (but had to file it themselves), or those who only received a blank application (which might be viewed as analogous to our "Information Only" intervention). Consistent with these findings and ours of impacts of application assistance, a number of quasi-experimental studies suggest a role for transaction costs in reducing SNAP participation rates (e.g. Kabbani and Wilde 2003, Hanratty 2006, Ratcliffe 2008, Klerman and Danielson 2011); we describe these in more detail in Appendix H.

We also provide more detail in Appendix H on the active empirical literature (referenced in the Introduction) studying take up and targeting in other programs. This literature, which has been primarily descriptive, has found mixed evidence on 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 2 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 in order 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 on-line SNAP recertification tools (Gray 2018).

# 6 Normative implications

## 6.1 Conceptual framework mapped to our context

We tailor the framework from Section 2 in two minor ways in order to apply it to our empirical setting. First, to facilitate our subsequent calibration, we allow for an exogenous probability $\pi_j$ that the application is accepted. Ex-ante uncertainty about acceptance comes from a 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 4, 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 non-elderly 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 since, as discussed, in Section 2, 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: $e = E_L/(E_L + E_H)$ . 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., $de/dT < 0$).

With these modifications, we can re-state Propositions 1 and 2 as follows:

**Proposition 1a:** *The effect of the Information Only treatment on welfare is given*:

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

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

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

**Proof:** See Appendix E.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* $(de/dT > 0)$ *from an Information Only (or Information Plus Assistant) treatment is given by the following expression*:

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

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

**Proof:** See Appendix E.3

Note that while we interpret our two interventions in the context of our framework as "information only" ($d\epsilon$) and "information plus assistance" ($d\epsilon$,$-d\bar{\Lambda}$), 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 in so far as assistance interventions also reduce costs for infra-marginal applicants (see Proposition 1a); our calibrations below make clear that this reduction in costs for infra-marginal applicants is not what is driving our normative results.

### 6.2 Parameterizing the model

We use the statutory minimum benefit level for $B_{min}$ (\$16 per month) and set $\overline{B}$ to \$178 per month (the mean benefit for the approximately 80 percent of control group enrollees who do not receive the minimum). As described above, 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, while type $H$ enrollees receive \$576 over 36 months. After 36 months, individuals must re-certify 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 2). 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.[28]

Our baseline parameterization assumes that 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 5.1). We will explore below how the results vary under alternative assumptions about fiscal externalities.

In the neoclassical benchmark case ($\epsilon_j = 0$ 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

[28]While Hastings and Shapiro (2018) calls into question the standard assumption that SNAP benefits are fungible with cash for 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.

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 per hour, this implies the private (time) cost of applying is about \$75. With no misperceptions, rationalizing the decision not to apply therefore requires a non-time 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 the decision of non-applicants is to say that they experience stigma costs of participation that are about sixty times larger than their transactional costs of applying. For an $H$ type with no misperception of the probability an application is accepted, the implied non-time cost of applying is roughly \$350.

However, our reading of the evidence suggests that individuals under-estimate the probability their application is accepted (i.e., $\epsilon < 0$) and hence expected benefits from applying. As noted previously, existing survey evidence suggests that lack of awareness of expected benefits - e.g., under-estimating expected benefits - is a primary barrier to participation among eligible non-participants (Bartlett et al., 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; i.e., 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 also use a first-order Taylor approxmation to calculate the expected utility of applying, which ignores the role of risk aversion in the application decision (we will relax this below). With these assumptions, rationalizing non-participation with the time cost estimates above requires $\epsilon_L = -0.98$ and $\epsilon_H = -0.83$. Thus, $\epsilon_L < \epsilon_H < 0$, and for a type $L$ individual with a 75 percent 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 percent) of enrolling in 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 percent of individuals 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 individuals than those who already enrolled.

### 6.3 Normative Findings

Proposition 2a indicates that with $\epsilon_L < \epsilon_H < 0$, 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 $\epsilon_L < \epsilon_H < 0$, 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 - i.e., $\frac{dW}{dT}$ - we need estimates of $G_j^A - G_j^{\neg A}$, $\pi_j B_j$, $\frac{dA_j}{dT}$ and $\mu_j \equiv u(y_j^A + B) - u(y_j^A + (1+\epsilon_j)B)$ (for $j = \{L, H\}$). Recall our baseline assumption (which we will 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 $\epsilon$ (which we will also relax below) that allows us to approximate $\mu_H$ as $\xi_H \pi_H \epsilon_H B_{min}$, and $\mu_L$ as $\xi_L \pi_L \epsilon_L \overline{B}$.

To ease interpretation, we make two changes to the $\frac{dW}{dT}$ expression in Proposition 1a. First, we translate the changes in private utility $\mu_j$ into a change in dollars of surplus for each type (rather than the dollar surplus for a typical individual in the population - see footnote 3), by dividing by the marginal utility of consumption for each type ($\xi_j \equiv u'(y_j^A + B)$). Second, since quantitative welfare statements are more easily interpreted as a ratio of private welfare changes to changes in costs, we follow Hendren (2016) and re-write 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 individual's willingness to pay rather than society's (see, e.g., Finkelstein et al. 2017 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:

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

Appendix Section E.3.1 provides the derivation.

We previously parameterized $g \sim \$267$, $\pi_L \overline{B} \sim \$4,806$, $\pi_H B_{min} \sim \$432$, $\epsilon_L \sim -0.98$, and $\epsilon_H \sim -0.83$. The impact of the intervention on applications of each type $\frac{dA_j}{dT}$ comes directly from the experiment. Table 2 shows directly the increase in applications - for the Information Only intervention, $\frac{dA}{dT} = 0.07$ and for the Information Plus Assistance intervention, $\frac{dA}{dT} = 0.16$. Appendix Table A7 shows that, for each intervention, 44 percent of the marginal enrollees are $H$ types (i.e., 44 percent 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 2 shows, only about 20 percent are type $H$ individuals. Given our assumption of a common, 75 percent acceptance rate for both types, this suggests that for the Information Only intervention, $\frac{dA_L}{dT} = .03$ and $\frac{dA_H}{dT} = .04$, and for the Information Plus Assistance intervention, $\frac{dA_L}{dT} = .07$ and $\frac{dA_H}{dT} = .09$. We ).

We therefore have rough estimates of all the elements we need to evaluation 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 dollar spent on the intervention (in the form of benefits and processing costs), low-income recipients receive about 89 cents of benefits.[29] An MVPF below 1 is to be expected for a redistributive policy such as SNAP; redistribution inevitably involves some resource cost (Okun 1975). A more natural benchmark is to compare this estimate to the MVPF of other redistributive programs. Although we know of no elderly-specific estimates, it is interesting to see that this estimate is comparable to the estimate of the MVPF for the Earned Income Tax Credit, which Hendren (2016) estimates to be about 0.9; this is higher than the likely MVPF of public subsidies for health insurance for low-income adults (Finkelstein et al., forthcoming), as well as the MVPF of the SNAP program for the non-elderly, which Hendren (2016) estimates has an MVPF of roughly 0.5-0.7. In other words, an information intervention about likely eligibility for SNAP among an elderly population transfers more resources to low-income beneficiaries per dollar of public expenditure than the SNAP program for the non-elderly or subsidies for public health insurance for low-income adults. Of course, this calculation assumes away some potential behavioral responses to the intervention (such as decreased labor force participation) which could increase the fiscal externality on the government. This assumption seems empirically reasonable in the context of our experimental interventions, but might differ if these interventions were implemented at scale.

We can perform a similar analysis for the Information Plus Assistance intervention using the following extended formula:

$$MVPF^{Information\,Plus\,Assistance} = \frac{-\epsilon_L(\pi_L\overline{B})\frac{dA_L}{dT} - \epsilon_H(\pi_H B_{min})\frac{dA_H}{dT} - (A_H - A_L + \frac{dA_H}{dT} + \frac{dA_L}{dT})\frac{dc}{dT}}{(\pi_L\overline{B} + g)\frac{dA_L}{dT} + (\pi_H B_{min} + g)\frac{dA_H}{dT}}.$$

The MVPF for the Information Plus Assistance intervention is the same as for the Information Only intervention, plus one additional term in the numerator, representing the welfare gain from reducing application costs for both the infra-marginal and marginal applicants. The term $dc/dT$ is the (money-metric) change in application costs from the intervention, and it is scaled by the number of total applicants (both infra-marginal and marginal) of either type (i.e., this is the overall application rate in this treatment arm). The money metric term $dc/dT$ replaces the $\kappa_j$ terms multiplying the infra-marginal applicants in the expression for $\frac{dW}{dT}^{Information\,Plus\,Assistance}$ (see equation (1)).

Assuming that the application costs are costlessly reduced - which would correspond to removing some pre-existing barrier or ordeal - the MVPF is unambiguously higher for the Information Plus intervention than the Information Only one. If the intervention costlessly eliminated private application costs (i.e., reducing them from \$75 per application to zero), this would increase the

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

MVPF from 0.89 in the Information Only intervention to 0.93. If we allow for BDT's cost per application estimate of $45 ($60 per enrollee, adjusted for the acceptance rate), then the MVPF for the Information Plus Assistance Intervention would fall to 0.91.

To see the role that targeting plays in affecting the MVPF, we calculate the MVPF in the Information Only intervention separately for each type:

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

As Proposition 2a predicts, given our estimate of $\epsilon_L < \epsilon_H < 0$, 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 (~0.5). In other words, if those deterred by barriers were exclusively the less needy, our interventions would have looked substantially worse.

**Some alternative assumptions**

Many of the assumptions we made in the above calibration exercise are heroic. We briefly explore sensitivity to alternative assumptions in three areas, focusing on the magnitudes of the fiscal externalities, the degree of risk aversion, and the private costs of applying. The goals are the same as the baseline calibration: to provide insight into the determinants of the welfare impacts of the intervention.

In our baseline analysis we assumed the only fiscal externalities were from public processing costs and that these were constant across type. There may of course be other (unmeasured by us) fiscal externalities from enrolling in the program - such as impacts of SNAP on health and hence public (Medicaid and Medicare) health care expenditures; these of course may well vary by type.[30] In addition, interventions aimed at younger populations (or, in general equilibrium, even interventions for the elderly) may generate endogenous labor supply responses, which represent an additional fiscal externality. The conceptual framework emphasized that the social welfare impacts of targeting depend on how fiscal externalities vary with type. We therefore ask: how large would any additional fiscal externality for the $H$ types need to be to equalize the MVPFs across types in the Information Only intervention? The answer is $4,113, which means that the treatment would have to generate additional negative fiscal externalities of $4,113 from transfering $4,806 to high-benefit enrollees.

Our baseline analysis also used a first-order Taylor approximation to utility. We therefore

[30]For example, Almond et al. (2011) find evidence that the roll out of the Food Stamp Program (the precursor to SNAP) increased birth weight. Closer to our sample population, Berkowitz et al. (2017) report an association between SNAP participation and lower Medicaid and Medicare costs for low-income adults.

explored sensitivity of our results to the assumed degree of risk aversion. The results of these calculations are presented in Appendix Section E.3.2 and show very little sensitivity to allowing for risk aversion, which ultimately leads to similar MVPFs.

Lastly, we examined sensitivity to our assumptions about the private costs of enrolling. These are critical for our calibration of misperceptions, since greater private costs reduce the magnitude of misperceptions needed to rationalize the observed application decisions. In our baseline analysis we allowed for only time costs of applying. We now consider allowing for a non-time cost (which could reflect stigma, for example). We assume that this non-time cost is four times the time cost for each type of individual, so the money-metric stigma cost is $300 for each type.[31] As a result, the magnitude of implied "misperceptions" for each group are reduced to $\epsilon_L = -0.93$ and $\epsilon_H = -0.13$; no other inputs to the MVPF are affected. The MVPFs are therefore reduced to 0.80 for the Information Only intervention and 0.84 for the Information Plus Assistance. The type-specific MVPFs for the Information Only interventions are reduced to 0.87 for the type $L$ individuals and 0.08 for the type $H$ individuals. The very low MVPF for type $H$ individuals in this scenario reflects the fact that, with the assumed stigma costs, the $H$ types do not greatly misperceive their expected benefit of applying, so that the marginal $H$ type enrollees have limited private benefit of applying. This illustrates a key point of the framework: the relationship between the targeting properties of interventions and their welfare impacts depends on the distribution of misperceptions across the population. In this paper we have inferred misperceptions indirectly, but future work could combine the experimental methods in this paper with more direct measures of beliefs.[32]

# 7 Conclusion

Policymakers 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

[31] There is substantial uncertainty regarding the average stimga costs associated with applying for and enrolling in SNAP. Manchester and Mumford (2012) estimate the average psychological (stigma) costs of SNAP participation to be four times as large as the average time cost using a structural model of program participation, so we use this for our calibration.

[32] Future work can also allow for richer heterogeneity in beliefs and heterogeneity in the impact of the information interventions on these beliefs. In the NBER Working Paper version of this paper, we present an extended version of the model (mapped to our context) that allows for heterogeneity in beliefs, and we model the information interventions as either reducing the bias or the variance in beliefs, with both types of reductions potentially affecting private welfare (and thus the implied MVPFs). We speculate that these effects may provide an additional reason why the marginal $H$ type enrollees have a limited private benefit of applying, since these individuals could have heterogeneous – but approximately unbiased – beliefs, and the information intervention could have increased the accuracy of their beliefs. For $L$ type enrollees, however, we speculate that the large estimated MVPFs are likely to be fairly robust to allowing for this additional source of heterogeneity. Another interesting aspect that can arise with belief heterogeneity is the existence of "defiers" – individuals who would have applied, but do not apply because of the information intervention. The NBER Working Paper version provides conditions under which the "net impact" of the interventions on targeting remains informative for welfare (in the presence of both compliers and defiers), but of course characterizing both the compliers and defiers would be more challenging than the empirical analysis in this paper.

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 as well as 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 9 months following the intervention, the Information Only intervention increased enrollment by 5 percentage points (or 83 percent relative to the enrollment rate among controls), while the Information Plus Assistance increased enrollment by 12 percentage points (a 200 percent 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 to 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 percent 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 et al., 1971, Nichols and Zeckhauser 1982, Besley and Coate 1992). However, consistent with behavioral models (e.g., Bertrand et al. 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 individuals 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 under-estimate expected benefits from applying, with this under-estimation greater among needier individuals. 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 as well as the size of the fiscal externality from enrolling across these individuals - in order 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.

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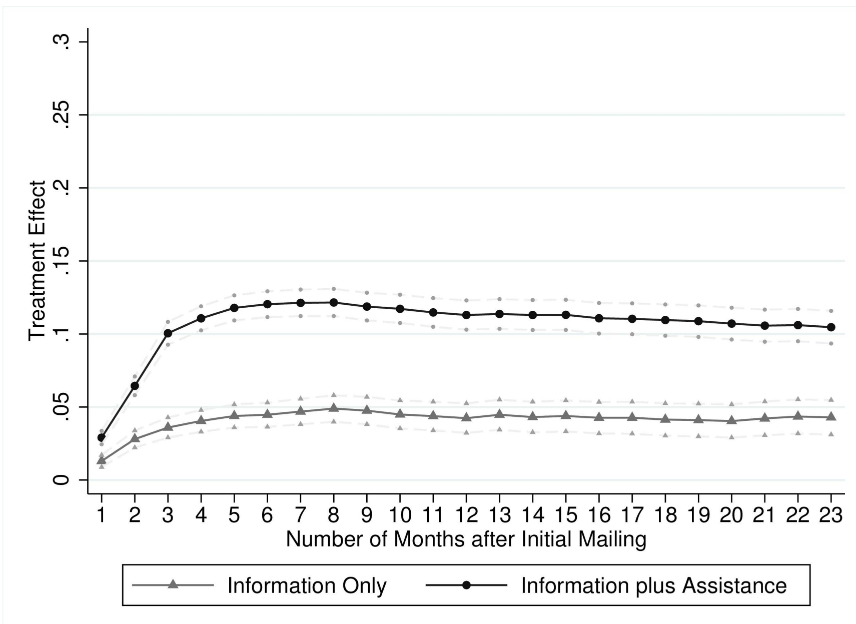
Figure 1: Time pattern of enrollment responses




NOTE: 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. 95 percent confidence intervals on these estimates are shown in the dashed light gray lines.

Table 1: Description of Study Population

| | Original Outreach List | List, After Exclusions | After Exclusions: Receving SNAP | After Exclusions: Not Receiving SNAP | Study Population |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (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) Health Care Measures, 2015 | | | | | |
| Total Health Care 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: we exclude 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; health care 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 Appendix B 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.

Table 2: Behavioral Responses to "Information Only" and "Information Plus Assistance"

| | Control | Information Only | Information Plus Assistance | P Value of Difference (Column 2 vs 3) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| SNAP Enrollees | 0.058 | 0.105 | 0.176 | |
| | | [0.000] | [0.000] | [0.000] |
| SNAP Applicants | 0.077 | 0.147 | 0.238 | |
| | | [0.000] | [0.000] | [0.000] |
| SNAP Rejections among Applicants | 0.233 | 0.266 | 0.255 | |
| | | [0.119] | [0.202] | [0.557] |
| Callers | 0.000 | 0.267 | 0.301 | |
| | | [0.000] | [0.000] | [0.000] |
| Adjusted Callers | 0.000 | 0.289 | 0.301 | |
| | | [0.000] | [0.000] | [0.156] |
| SNAP Applicants among Non-Callers | 0.077 | 0.086 | 0.081 | |
| | | [0.063] | [0.324] | [0.363] |
| SNAP Applicants among Callers | 0.000 | 0.313 | 0.602 | |
| | | [0.000] | [0.000] | [0.000] |
| SNAP Enrollees among Non-Callers | 0.058 | 0.061 | 0.059 | |
| | | [0.442] | [0.713] | [0.688] |
| SNAP Enrollees among Callers | 0.000 | 0.226 | 0.450 | |
| | | [0.000] | [0.000] | [0.000] |
| Observations (N) | 10,630 | 5,314 | 10,629 | |

Notes: Columns 1 through 3 shows 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 sub-treatments). Column 3 shows the Information Plus Assistance arms (weighted so that the two pooled sub-treatments 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 zero for the control; see text for a description of the adjusted caller rate.

Table 3: Behavioral Responses to "Information Only" Intervention with and without reminders

| | Control<br><br>(1) | Information Only Standard<br><br>(2) | Information Only No-Postcard<br><br>(3) | P Value of Difference (Column 2 vs 3)<br><br>(4) |
|---|---|---|---|---|
| SNAP Enrollees | 0.058 | 0.112 | 0.092 | |
| | | [0.000] | [0.000] | [0.016] |
| SNAP Applicants | 0.077 | 0.151 | 0.120 | |
| | | [0.000] | [0.000] | [0.001] |
| SNAP Rejections among Applicants | 0.233 | 0.224 | 0.216 | |
| | | [0.751] | [0.536] | [0.777] |
| Callers | 0.000 | 0.278 | 0.212 | |
| | | [0.000] | [0.000] | [0.000] |
| Adjusted Callers | 0.000 | 0.300 | 0.234 | |
| | | [0.000] | [0.000] | [0.000] |
| SNAP Applicants among Non-Callers | 0.077 | 0.089 | 0.074 | |
| | | [0.079] | [0.593] | [0.071] |
| SNAP Applicants among Callers | 0.000 | 0.311 | 0.295 | |
| | | [0.000] | [0.000] | [0.524] |
| SNAP Enrollees among Non-Callers | 0.058 | 0.064 | 0.054 | |
| | | [0.284] | [0.492] | [0.172] |
| SNAP Enrollees among Callers | 0.000 | 0.237 | 0.234 | |
| | | [0.000] | [0.000] | [0.921] |
| Observations (N) | 10,630 | 2,657 | 2,658 | |

Notes: Columns 1 through 3 shows 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 Appendix Figure A2; this "standard" intervention is half of the sample shown in Table 2 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 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 zero for the control; see text for a description of the adjusted caller rate.

Table 4: Enrollee Monthly Benefits and Predicted Benefits

| | Control | Information Only | Information Plus Assistance | P Value of Difference (Column 2 vs 3) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Benefit Amount | 145.94 | 115.38 | 101.32 | |
| | | [0.000] | [0.000] | [0.013] |
| Share $16 Benefit | 0.192 | 0.312 | 0.367 | |
| | | [0.000] | [0.000] | [0.021] |
| Share $194 Benefit | 0.206 | 0.164 | 0.147 | |
| | | [0.076] | [0.003] | [0.352] |
| Share $357 Benefit | 0.060 | 0.052 | 0.040 | |
| | | [0.587] | [0.077] | [0.259] |
| Share Missing Benefit | 0.073 | 0.043 | 0.028 | |
| | | [0.025] | [0.000] | [0.139] |
| Predicted Benefit for Enrollees w/ Actual Benefit | 140.20 | 112.49 | 102.93 | |
| | | [0.000] | [0.000] | [0.086] |
| Predicted Benefit for All Enrollees | 138.65 | 114.01 | 104.03 | |
| | | [0.000] | [0.000] | [0.068] |
| Share of Enrollees in Household Size of 1 | 0.657 | 0.714 | 0.760 | |
| | | [0.038] | [0.000] | [0.036] |
| Benefit Amount for Enrollees in Household Size of 1 | 116.97 | 93.35 | 85.82 | |
| | | [0.000] | [0.000] | [0.134] |
| Observations (N) | 613 | 559 | 1,861 | |

Notes: Sample is individuals who enrolled in the 9 months after their initial mailing. Columns 1 through 3 shows 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 sub-treatments pooled). Column 3 shows the Information Plus Assistance arms (weighted so that the two pooled sub-treatments 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.

Table 5: Demographic and Health Characteristics: Applicants and Enrollees

| | Applicants | | | | Enrollees | | | |
|---|---|---|---|---|---|---|---|---|
| | Means | | | P Value | Means | | | P Value |
| | Control | Info Only | Info Plus Assistance | Info Plus Assistance vs Info Only | Control | Info Only | Info Plus Assistance | Info Plus Assistance vs Info Only |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A - Predicted Benefits | | | | | | | | |
| Predicted Benefits | 148.26 | 125.65 | 115.36 | | 138.65 | 114.01 | 104.03 | |
| | | [0.000] | [0.000] | [0.037] | | [0.000] | [0.000] | [0.068] |
| Panel B - (Annual) Health Care Measures, 2015 | | | | | | | | |
| Total Health Care Spending ($) [a] | 9,424 | 8,605 | 8,334 | | 10,238 | 9,532 | 8,603 | |
| | | [0.517] | [0.300] | [0.781] | | [0.661] | [0.208] | [0.459] |
| Total Number of Visits and Days | 13.33 | 11.67 | 9.92 | | 14.79 | 10.90 | 9.92 | |
| | | [0.331] | [0.018] | [0.166] | | [0.058] | [0.008] | [0.467] |
| Weighted Total Number of Visits and Days | 4,661 | 3,273 | 2,818 | | 5,407 | 3,288 | 2,779 | |
| | | [0.128] | [0.022] | [0.442] | | [0.064] | [0.011] | [0.461] |
| Number of Chronic Conditions | 6.21 | 5.55 | 5.27 | | 6.54 | 5.43 | 5.37 | |
| | | [0.094] | [0.006] | [0.383] | | [0.019] | [0.005] | [0.875] |
| Panel C - Demographics | | | | | | | | |
| Share Age above Median = 65 | 0.41 | 0.46 | 0.46 | | 0.39 | 0.43 | 0.46 | |
| | | [0.072] | [0.014] | [0.764] | | [0.282] | [0.006] | [0.159] |
| Share Age 80+ | 0.06 | 0.11 | 0.14 | | 0.07 | 0.12 | 0.14 | |
| | | [0.001] | [0.000] | [0.042] | | [0.005] | [0.000] | [0.085] |
| Male | 0.41 | 0.40 | 0.38 | | 0.39 | 0.42 | 0.38 | |
| | | [0.983] | [0.232] | [0.250] | | [0.446] | [0.444] | [0.104] |
| Share White [b] | 0.67 | 0.73 | 0.74 | | 0.71 | 0.78 | 0.78 | |
| | | [0.005] | [0.000] | [0.554] | | [0.004] | [0.001] | [0.958] |
| Share Black [b] | 0.10 | 0.08 | 0.11 | | 0.11 | 0.07 | 0.10 | |
| | | [0.103] | [0.577] | [0.011] | | [0.011] | [0.833] | [0.004] |
| Share Primary Language not English | 0.08 | 0.06 | 0.04 | | 0.06 | 0.05 | 0.03 | |
| | | [0.141] | [0.000] | [0.012] | | [0.242] | [0.002] | [0.067] |
| Share Living in Pittsburgh | 0.05 | 0.06 | 0.07 | | 0.05 | 0.06 | 0.07 | |
| | | [0.385] | [0.066] | [0.459] | | [0.374] | [0.028] | [0.310] |
| Share Last Medicaid Spell Starting before 2011 | 0.25 | 0.30 | 0.29 | | 0.26 | 0.33 | 0.31 | |
| | | [0.022] | [0.017] | [0.704] | | [0.009] | [0.026] | [0.348] |
| 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 9 months of their initial mailing, and SNAP enrollees who enrolled within 9 months of their initial mailing, respectively. Column 1 and 5 show the control. Column 2 and 6 show the Information Only arms (with the two equally-sized sub-treatments pooled); columns 3 and 7 show the Information Plus Assistance arms (weighted so that the two pooled sub-treatments 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]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.

Table 6: Demographic and Health Characteristics: Callers and non-Callers

| | Callers | Non-callers | P Value of Difference |
|---|---|---|---|
| | (1) | (2) | (3) |
| Panel A - Predicted Benefits | | | |
| Predicted Benefits | 106.99 | 114.68 | [0.000] |
| Predicted Enrollment | 0.05 | 0.05 | [0.752] |
| Panel B - (Annual) Health Care Measures, 2015 | | | |
| Total Health Care Spending ($) [a] | 7,316 | 13,656 | [0.000] |
| Total Number of Visits and Days | 9.52 | 13.50 | [0.000] |
| Weighted Total Number of Visits and Days | 2,853 | 5,064 | [0.000] |
| Number of Chronic Conditions | 5.16 | 5.48 | [0.024] |
| Panel C - Demographics | | | |
| Share Age above Median = 65 | 0.49 | 0.51 | [0.014] |
| Share Age 80+ | 0.16 | 0.17 | [0.190] |
| Male | 0.38 | 0.38 | [0.977] |
| Share White [b] | 0.77 | 0.74 | [0.000] |
| Share Black [b] | 0.09 | 0.07 | [0.006] |
| Share Primary Language not English | 0.03 | 0.05 | [0.000] |
| Share Living in Pittsburgh | 0.06 | 0.06 | [0.658] |
| Share Last Medicaid Spell Starting before 2011 | 0.32 | 0.34 | [0.044] |
| Observations (N) | 4,597 | 11,346 | |

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

[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.